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HRM AND PERFORMANCE: WHAT DO WE KNOW AND WHERE SHOULD WE GO?

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'Based on four national surveys and observations on more than 2000 firms, our judgment is that the *effect* of a one standard deviation *change* in the HR system is 10–20% of a firm's market value.' (Huselid & Becker, 2000, p. 851; emphasis added)

'The existing evidence for a relationship between HRM and performance should be treated with caution.' (Wall & Wood, 2005, p. 454)

'After hundreds of research studies we are still in no position to assert with any confidence that good HRM has an impact on organization performance.' (Guest, 2011, p. xx)

Practitioners interested in human resources management (HRM) have long sought to convince others of its value. Drucker (1954) referred to 'personnel' managers as constantly worrying about 'their inability to prove that they are making a contribution to the enterprise' (p. 275). More recently Tom Stewart described HR leaders as being 'unable to describe their contribution to value added except in trendy, unquantifiable and wannabe terms' (Stewart, 1996, p. 105).

In response to this longstanding and often repeated criticism that HR does not add value to organizations, academic research has exploded over the past 20 years, seeking to show that HRM practices are related to firm performance. Huselid's (1995) groundbreaking study showed that a set of HR practices he called 'high-performance work systems' (HPWSs) were related to turnover, accounting profits and firm market value. This study served as the springboard for a significant body of research confirming empirical relationships between HR and performance.

However, in spite of the vast body of research that has emerged over the past two decades, as these quotes from some distinguished academic researchers suggest, divergence exists regarding what we can conclude about the relationship

between HRM practices and firm performance. Is the HRM–performance relationship one that is strong, universal and causal, or is it potentially weak, contingent and even spurious? More importantly, what is the underlying research base from which we can answer that question, and how can we improve that base in order to answer it in a way that is valid, reliable and practically important? The purpose of this book is to attempt to bring together some of the leading researchers in this area to provide insights into what we know, what we need to know, and how we can begin on a journey to improve our knowledge of the relationship between HR and firm performance.

In this chapter, we will first present an overview of what we seem to know about this relationship. We will trace some of the streams of this research in an effort to provide a foundation for how we have arrived at this point in our knowledge base. We will then lay out some of the unanswered questions that have emerged from the research to date on the relationship between HRM and performance. Within this context we will show how the authors have attempted to provide some answers and set out future directions as an overview to the rest of the book.

What do we know about the HRM–performance relationship?

The development of theory and research on the relationship between HRM and performance began in the 1980s. A series of articles and books by authors such as Fombrun et al. (1984) and Miles and Snow (1984) began to link business strategy to human resource management. The Harvard group (Beer et al., 1984) and Schuler and Jackson (1987) began to argue for a clear and systematic integration between the strategy of the firm and the HRM practices used to manage the workforce of that firm. In the UK, writers such as Guest (1987) and Storey (1992) took a normative perspective, suggesting the need for external and internal fit of HRM, contrasting it with the pluralism of an industrial relations perspective.

During this time, others such as Walton (1985), while less concerned with fitting HRM to strategy, highlighted the need for a shift from control to commitment as the basis for management of people at work. In parallel with this, authors such as Foulkes (1980) and Peters and Waterman (1982) provided glimpses of evidence about successful organizations that seemed to apply the ‘high commitment’ HRM principles. Thus, this early phase presented the foundational arguments that (a) HRM practices should be integrated with the strategy of the firm in order to be maximally effective, and (b) that certain ‘high commitment’ HRM practices were more effective for generating higher firm performance relative to control-oriented practices.

While these foundational arguments sparked thinking, they alone could not provide convincing evidence of the potential value of HRM. However, the 1990s served as the springboard for what would end up being a vast and growing empirical literature. Huselid (1995) provided the seminal work in his study of more than 800 corporations, revealing an empirical relationship between the HPWSs (similar to the ‘high commitment’ practices discussed above) and important corporate performance variables such as the gross rate of return on assets (a measure of accounting profits) and Tobin’s Q (a measure of the value of the firm). This study

has become the central node in research on the HRM–performance relationship, but was by no means alone. Arthur (1994) found an empirical link between strategy of steel mini-mills and HRM practices. Ichniowski et al. (1995) found a relationship between HRM practices and operating performance of steel mini-mill manufacturing lines. MacDuffie (1995) presented evidence of ‘bundles’ of HRM practices and measures of manufacturing performance within the automobile industry. Finally, Delery and Doty (1996) explored universalistic, contingency and configurational models of HRM as predictors of firm performance among a sample of banks. They found support for the universalistic (i.e. a similar set of practices consistently related to performance) but little support for either contingency or configurational approaches. In Europe, and particularly the UK, a group of authors reacted strongly against this emerging stream of research, arguing that it represented a new and more subtle form of exploitation of workers (Keenoy, 1990; Keenoy & Schwan, 1990; Blyton & Turnbull, 1992; Legge, 1995). Their analysis was conceptual rather than empirical and partly as a result, received only limited attention among researchers (Keegan & Boselie, 2006). Nevertheless, they raised the important question, largely neglected in the early research on HRM and performance, about the impact of HRM on employees.

Since these early studies, the empirical research has continued unabated and expanded globally. For instance, Guthrie (2001) replicated Huselid’s methodology in a sample of New Zealand firms and found a similar relationship between HPWSs and firm performance. Guest et al. (2003) related HR practices to both past and subsequent objective productivity and profitability data, as well as current subjective productivity and financial performance estimates, among a sample of 366 companies in the UK. Boselie et al. (2003) explored the role of sectoral/institutional factors in The Netherlands and showed that the effect of HRM on performance is lower in highly institutionalized sectors (like hospitals and local government) than in a less institutionalized sector like hotels. More recently Takeuchi et al. (2007) found a relationship between HPWSs and firm performance among a sample of Japanese firms. There is now a growing body of research on Chinese organizations that shows a similar pattern of results (e.g. Liao et al., 2009).

There is little doubt that in the past 20 years some progress has been made in the analysis of the relationship between HRM and performance. On balance, however, progress has been modest. This is reflected in the rather optimistic conclusions from some of the main overview articles that have appeared. Becker and Gerhart (1996), indicated that the conceptual and empirical work had progressed far enough to suggest that the role of human resources can be crucial. Similarly, Paauwe and Richardson (1997), based on an early review of 22 studies, concluded that HRM activities give rise to HRM outcomes that will influence the performance of the firm. More specifically and more positively, Huselid and Becker (2000) indicated that the effect of one standard deviation change in the HR system leads to a 10–20 per cent increase in a firm’s market value.

However, on a more cautious note, Wright and Gardner (2003a), reflecting on the available evidence, concluded that HR practices are at least weakly related to firm performance. By 2005, Boselie et al. (2005), drawing on a comprehensive sample of 104 studies, concluded that much (though by no means all) of the

empirical research shows that HRM is associated with organizational performance. Based on a selection of 25 mainly American so-called high-quality studies, Wall and Wood (2005) concluded – even more cautiously – that the evidence for an effect of HRM on performance is promising but only circumstantial due, for the most part, to inadequate research design. Thus, 19 of the 25 studies they examined reported statistically significant positive relationships between HR practices and performance, but the effect sizes are typically small and the majority of studies failed to consider whether it is the HRM system (the ‘gestalt’) generating the effects or just specific component/individual practices. Overall, therefore, they conclude that ‘The existing evidence for a relationship between HRM and performance should be treated with caution’ (Wall & Wood, 2005, p. 454).

On a more positive note, Combs et al. (2006) carried out a meta-analysis of 92 studies on the HR–firm performance relationship and found that an increase of one standard deviation in the use of high-performance work practices (HPWP) is associated with a 4.6 per cent increase in return on assets, and with a 4.4 percentage point decrease in turnover. Hence their conclusion that ‘HPWPs’ impact on organizational performance is not only statistically significant, but managerially relevant’ (p. 518).

Whether examining individual studies, the systematic reviews or the meta-analytic summaries of this literature, what we do know is that HRM practices seem to be consistently related to performance. However, the reviews reveal different levels of confidence about the strength of the association, about the quality of the research on which it is based and about the practical conclusions we can draw from it about the impact of HRM. Therefore, before concluding that we have found the truth and there is no more need for research in this area, we may first want to look at it with a more critical eye. Such a critical view reveals that while this empirical body of research has consistently demonstrated a relationship between HRM practices and performance, it has revealed a number of problems. These centre around two basic themes: theoretical ambiguity and empirical invalidity.

Theoretical ambiguity

Regarding the theoretical ambiguity, Guest (1997) neatly summarized the challenge when he stated this literature needed ‘a theory of HRM, a theory of performance, and a theory of how the two are linked.’

First, in attempts to articulate a theory of HRM, Guest (1997), along with others such as Becker et al. (1997) utilized expectancy theory (Vroom, 1964) to theorize about the core HRM practices. This basic framework was adopted and adapted to what has emerged as the AMO (ability, motivation, and opportunity) framework (Appelbaum et al., 2000; Purcell & Hutchinson, 2007). This framework suggests that HR practices can influence the skills, competencies and abilities of the workforce to provide a strong human capital base. Second, practices can affect the motivation and commitment of employees, engaging them to want to behave in ways that benefit the firm. Finally, HR practices such as job design and participative

processes can provide the opportunities for the skilled and motivated workforce to positively affect organizational outcomes. This has become the most well-accepted framework for understanding HRM practices. However, while the framework has been widely accepted, there is no consensus about the specific practices that should be considered. This is a major problem. After all, how can we ever make progress in this field if we do not agree on what constitutes one of the main independent variables, namely HR practices?

Regarding a theory of performance, since Huselid's (1995) study, most of the research has focused on performance from the standpoint of the firm. For instance, in their meta-analysis on HPWSs and performance, Combs et al. (2006) noted that of the 92 studies they included, '[a]ccounting returns were most frequently studied (35 effects), followed by productivity (32), retention (23), multidimensional (22), growth (16), and market returns (8)' (p. 510).

However, a somewhat separate line of research on HRM has explored employee-centred outcomes, assuming a need to look beyond firm performance. For instance, Ramsay et al. (2000) suggested that HRM might not be in the workers' best interests, and some studies critical of HRM based on workers responses supported this (e.g. Cappelli & Neumark, 2001; Godard, 2004).

Recently, a more nuanced view is emerging, suggesting that worker outcomes can be considered a central issue, and that it is possible that HRM can lead to both enhanced workers' well-being and higher performance (Peccei, 2004; Guest, 2011). For instance, Kehoe and Wright (2010) found that HRM practices were related to employees' affective commitment to the firm, their willingness to exhibit discretionary behaviour, and their intent to stay with the firm. In a more complex analysis, Gardner et al. (2011) found that motivation and opportunity enhancing practices were positively related to affective commitment and negatively related to turnover. However, ability-enhancing practices, consistent with labour market theory, were unrelated to affective commitment and positively related to turnover. Thus, theory and research regarding what constitutes the various aspects of performance affected by HRM has expanded beyond the pure financial measures. This view is further reinforced by research in public sector organizations where conventional private sector financial indicators are not relevant (see, for example, Messersmith et al., 2011). This supports the view that the outcomes of HRM are multi-faceted and that we need to look beyond financial performance (Boxall & Purcell, 2003; Paauwe, 2004).

Finally, regarding the theory linking HRM and performance, Wright and McMahan (1992) presented six theories that had been used in exploring the relationship between the two. However, by far the most popular theory in the 1990s was the resource-based view of the firm, which argues that when a firm's resources are valuable, rare, inimitable, and non-substitutable, they can be a source of sustainable competitive advantage. Wright et al. (1994) explored how a firm's human resources could meet these criteria and thus constitute a potential source of sustainable competitive advantage. While popular, this theory provides only a very generic argument for how HR practices might be related to performance, and does not help to understand the specific nature of this relationship.

One of the challenges, therefore, is how far it can be adapted to offer more specific proposals.

Becker et al.'s (1997) box and arrows model presented a more specific theoretical framework for explaining the mediating mechanisms between HRM and performance. They suggested that HRM practices have their most direct impact on the employee skills and motivation, which, in turn, results in creativity, productivity, and discretionary behaviour. Employees' behaviour influences the firm's operating performance, which leads to profitability, growth, and market value.

The review by Boselie et al. (2005), concluded that up until then, the three most popular theories being used in HRM and performance research were contingency theory, the resource-based view (RBV) and the AMO framework. If we include papers published since 2000, more than half use AMO theory (Paauwe, 2009). Both RBV and contingency theory focus on the organizational level, whereas AMO theory focuses on the importance of taking into account employee-level factors. Overall we can discern a lack of attention being paid to the institutional context (Paauwe & Boselie, 2003).

Thus a number of attempts have been made to develop a theory of HRM, a theory of performance, and a theory of how the two are linked. However, all are 'works in progress' and none have achieved consensus support among researchers. There is therefore still a need for more theoretical and conceptual development.

Empirical invalidity

Even if there was consensus about theory, as the opening quotes illustrate, a number of researchers have taken issue with the validity of the research base exploring the HRM–performance relationship. Gerhart et al. (2000b) first suggested that the single respondent measures of HR practices lacked reliability. They noted that while low reliability could diminish the observed relationship between HR and performance, it could also inflate it, if much of the error variance was systematically biased. They suggested that the traditional point estimates of the dollar value gains associated with increasing HR practices by a standard deviation, if corrected, would be far beyond what seems reasonable, and thus suggested that some systematic bias might exist.

Later researchers investigated some of the potential forms of systematic bias. Guest et al. (2003) were the first researchers to note that while the assumed direction of causality was from HR practices to performance, this should not be taken for granted. Their own data suggested that performance causing HR practices was an equally plausible explanation of the association. Wright et al. (2005) followed up on this research and showed that their HR practice measures were as strongly related to past performance as to future performance. In addition, they found that when controlling for past performance, the relationship between HR practices and future performance was substantially reduced. Neither set of researchers suggested that HR had NO impact on later performance, but raised the

question about how much of the observed relationship was directly causal. Wall and Wood (2005) argue that the lack of rigorous longitudinal studies on the HRM–performance relationship precludes any firm conclusions regarding the causal nature of this relationship. Some more recent studies have taken careful account of causality, using longitudinal research (van Veldhoven, 2005; van de Voorde et al., 2010b) and they also indicate reverse causation, that is, that performance influences subsequent HR practices and well-being.

Another issue that may call into question the strength of the HRM–performance relationship deals with response patterns of the people filling out surveys. Implicit performance theory, rooted in the leadership literature, has shown that respondents observing the same leader will report leadership behaviours consistent with what they are led to believe was the leader’s group’s performance (Rush et al., 1977). Gardner and Wright (2010) presented executives and students with descriptions of high and low-performing organizations and found that respondents reported greater use of HR practices in the high-performing firms. This opens up the possibility that in addition to actual performance causing HR practices, there might be an effect of performance on respondents’ reports of HR practices.

The result of all this led Paauwe (2009) in his overview to conclude that in spite of the fact that we have made progress in the area of HRM and performance, we still face significant methodological and theoretical challenges with regard to furthering our understanding of this relationship. In a similar vein, Guest (2011) concludes that: ‘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., 2000b). But it also leaves room for considerable doubt about the processes at play. We therefore need to recognize 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.’ (Wall and Wood (2005))

Thus, while research on the HRM–performance relationship continues to grow in popularity, a number of questions remain. The purpose of this book is to begin a conversation around some of these questions in the hope that future research might be able to answer a number of them. We would argue that some of the major unanswered (or at least not fully answered) questions must be addressed if this area of research is to lead to any firm conclusions. While more specific, each question deals with the three challenges laid out by Guest (1997) to develop a theory of HR, a theory of performance and a theory of how the two are linked.

Which HRM practices?

Returning to Guest’s admonition that this field needs a theory of HR, one of the striking problems revolves around a lack of consensus regarding what constitutes the correct set of HRM practices. In the 1996 special issue of the *Academy of Management Journal* on HRM and performance, Becker and Gerhart examined the practices that were assessed in the papers making up that issue. They found that not a single practice appeared across all of the studies, and that only one (hours of training) appeared across a majority of the studies. This led them to conclude that

'even when the same HR practices are included in different studies, researchers may still use different measures, further hindering efforts to cumulate findings' (Becker & Gerhart, 1996, p. 793).

The review by Boselie et al. (2005) reveals an enormous variety of different practices being used in the 104 studies they analysed. There is no single agreed, or fixed, list of HR practices or systems of practices used to define or measure HRM. In total, Boselie et al. (2005) identified 26 different practices that are used in different studies, of which the top four, in order of popularity, are training and development, contingent pay and reward schemes, performance management (including appraisal), and careful recruitment and selection. The meta-analysis conducted by Combs et al. (2006) also noted a lack of consistency in which practices are assessed across a range of studies. Their review identified 22 practices that researchers had described as HPWPs. However, they narrowed these down to the 13 that they believed consensus had emerged around (incentive compensation, training, compensation level, participation, selectivity, internal promotion, HR planning, flexible work, performance appraisal, grievance procedures, teams, information sharing and employment security). Yet, they found that studies had employed a range from 2–13 practices, with the average and median number of practices studied being 6.2 and 5, respectively.

This problem of consistency extends beyond the specific practices, but is even evident in defining the goal of the system of HRM practices. For instance, authors have focused variously on high involvement work systems (Lawler, 1986), high performance work systems (Huselid, 1995), or commitment-based HR systems (Collins & Smith, 2006; Boxall & Macky, 2009). Such lack of consistency led Kepes and Delery (2007) to suggest that 'nearly all the empirical studies have measured different HRM practices and constructed HRM strategy and system measures in different ways.' (p. 57). Thus, in spite of all the research conducted on the HRM–performance relationship, one is struck by the fact that the field has still not reached any consensus regarding what HRM is and which HR practices, arranged within which system, constitute the drivers of firm performance.

How should HRM practices be measured?

Even if consensus emerged regarding the specific set or system of HR practices that effectively drive higher performance, one next must address the question as to how to measure them. This entails making research design decisions regarding the source (who, individually or collectively, to ask about the HRM practices), the scale (what type of rating scale is used), and the scope (what employee group or groups are the focus) of the measurement.

For instance, in Huselid's classic study (1995) he sent surveys to the top HR person at corporate head offices (source). This respondent was asked to indicate the percent of employees (scale) covered by each practice. Then these ratings were made twice: once for managerial, professional and technical employees, and once for hourly and manual employees (scope). At the other extreme, Wright et al. (2005) surveyed between 20 and 100 per cent of employees (source), asking them

whether or not (yes/no/don't know) each practice was used (scale) in their particular job (scope).

These differing measurement strategies suggest different assumptions regarding who can provide the most accurate reports of HR practices, what dimensions of the practices provide the most valid descriptions (e.g.; use, coverage, effectiveness, etc.) and the unit of measurement over which one can provide an accurate report of these practices. These different assumptions may each be right, and simply point to different constructs that are being assessed. For instance, Becker and Huselid (2001) and Gerhart et al. (2000b) distinguished between the HR policies (i.e. what the organization has defined as the practices that should be used by managers/supervisors) and HR practices (those actually used by a manager/supervisor and their subordinates). Each of these are quite valid and interesting constructs, and each may have different sets of antecedents and consequences. However, researchers need to carefully plan the measurement strategy to be consistent with the construct they seek to assess. The questions of which HR practices and how to measure them deal specifically with the challenge to develop a theory of HRM. Next we turn to the theory of performance.

What is performance?

One of the significant contributions of Huselid's 1995 study was the demonstration of an empirical relationship between HRM and corporate financial performance. This sparked a spate of studies examining some form of organizational performance such as accounting profits, economic profits, productivity, customer satisfaction, and so on. Many studies relied on accounting/financial/market measures of performance because such measures were publicly available through corporate reporting databases. However, whether using public data or self-report data, the vast majority of studies has defined performance from a strongly managerialist perspective, using performance measures most important to shareholders.

Rogers and Wright (1998) suggested that the use of public data was unnecessarily pushing research at a corporate level, with a narrow range of outcomes. They suggested performance information markets (PIM) as a broader alternative approach to assessing firm or unit performance. This approach entails identifying the extent to which the firm is satisfying important stakeholders. They proposed four main information markets relevant to HRM and performance research: the financial market, the labour market, the consumer/product market, and the political/social market. Such a stakeholder-oriented approach has been advocated by others such as Paauwe (2004) and Boxall and Purcell (2008). In fact, Panayotopoulou et al. (2003) used a competing values framework (similar to the PIM) and found different relationships between different aspects of HR and financial versus market performance. For public sector organizations in areas such as health and education, the relevant indicators of performance are unlikely to be financial. Yet public sector organizations have received relatively little attention in research on HRM and performance.

In addition, a number of researchers have rightly noted that much of the existing research carries managerialist assumptions, particularly in the choice of

outcomes (e.g. Godard, 2004; Francis & Keegan, 2006; Keegan & Boselie, 2006; Delbridge & Keenoy, 2010). The question arises as to whether or not HRM practices might positively affect firm performance through exploiting workers in a way that has detrimental effects on their well-being. This suggests that a wider range of outcomes should be considered, including impacts on employees and customers, and, in public sector organizations such as hospitals, on patients. Thus the theory of performance seems to be an area ripe for conceptual and empirical analysis.

How are HRM practices implemented?

Related to the previously discussed difference between policies and practices is the question of implementation. Inherently assumed in much of the research on HRM practices and performance is that supervisors simply do whatever the organization policy requires them to do. If the policy says that supervisors give formal performance appraisal feedback once a year, they do. If hiring managers are supposed to use structured interviews, they use them. Such assumptions underlie the approach of asking the head of HR about the HRM practices that exist. However, these assumptions probably rarely, if ever, match reality. In some cases supervisors may be new or unaware of the plethora of HRM practice requirements. In other cases, they may be aware, but simply unwilling to implement them. For instance, many organizations track the percentage of completed performance appraisals, but these rarely reach 100 per cent, indicating that at least some supervisors are either unaware of or ignoring this job requirement.

In addition to the question of whether or not supervisors are actually implementing the practices comes the issue of how well they are implementing them. For instance, supervisors differ widely in their leadership and communication styles. So two supervisors conducting a performance appraisal session might vary in the effectiveness with which each communicates support, identifies development needs, and develops action plans. Such implementation issues have been largely ignored within the HRM–performance literature, yet they may be critical to developing a deeper understanding regarding this relationship. In fact, regarding the first three questions, Guest has concluded that ‘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’ (2011, pp. 10–11).

How do HRM practices impact performance?

Earlier in the chapter we discussed the basic elements of a theory of how HRM and performance are related. However, many of the existing models of this relationship focus purely at the organizational or unit level of analysis, ignoring the individuals who constitute the focal actors in the process (Wright & Haggerty, 2005).

For instance, the Becker et al. (1997) model posits that HRM practices impact the creativity, productivity and discretionary behaviour of the workforce. However, in the context of the previously discussed issues, each individual employee

may, in fact, experience a different HRM system. His or her supervisor may or may not implement the HRM policies, and when implemented, each may do so with different styles and different levels of effectiveness. In addition, each employee brings his/her own values and experiences as a lens through which to perceive, interpret and evaluate the practice. For instance, Nishii et al. (2008) found that it was employees' attributions about the HR practices that was the greater determinant of their reactions.

In other words, much of the earlier work on the HRM–performance relationship has been conducted at the unit level, simply assuming a set of individual level processes. More recent research and theorizing has begun to explore multi-level processes (e.g. van de Voorde et al., 2010b; Ployhart & Moliterno, 2011). In addition, van de Voorde et al. (2011) provide a systematic review of relationships between HRM, employee well-being and organizational performance. Empirical work by Kroon et al. (2009) compares two individual-level processes that might mediate the relationship between high-performance work practices implemented by organizations and employee burnout. Furthermore, Wood et al. (2012) utilize a multi-level model of well-being (including job satisfaction and job stress) as a mediator of the relationship between high-involvement management and organizational performance.

This line of research indicates that in order to clearly understand the relationship between HRM and performance, one must attempt to understand how practices impact individuals, who may then collectively impact performance.

How do we statistically model the HRM–performance relationship?

An additional challenge facing researchers in the HRM–performance arena revolves around the rigour of design and statistical modelling of the relationship. For instance, regarding design, Wall and Wood (2005) noted three generic types of designs. Cross-sectional designs assess all the variables at one time and, while efficient, provide a weak foundation for making causal inferences. Quasi-longitudinal designs assess HRM practices at one point and performance at some later point in time. Finally, most rigorous (for inferring cause) are 'authentic longitudinal' designs, which assess HRM and performance at multiple points in time, and are most valid for drawing causal inferences. However, they noted of the 25 studies they reviewed, 21 used cross-sectional designs. Two of the studies were quasi-longitudinal and two authentic longitudinal. However, the one authentic longitudinal design (Ichniowski et al., 2001) finding a positive relationship between HRM and performance suffers from a measurement issue in that it was what Wright et al. (2005) referred to as a 'retrospective' design where respondents were asked on a given date to recall and report the HR practices that existed at previous points in time. The remaining strong authentic longitudinal study (Cappelli & Neumark, 2001), as defined by Wall and Wood, found limited support for the efficacy of HRM in driving performance. More recently, van de Voorde et al. (2010a) studied longitudinal relationships between employee survey data on HRM-related change processes and objective business unit performance. Using two

data waves, this study showed that business unit profits could be predicted by employee survey information on factors driven by HRM-related interventions after correcting for prior profits.

In addition to design issues is the actual statistical modelling of the relationship. Statistical modeling entails understanding all the types and sources of variance that exist in the measures and how each can lead to biasing the observed relationship. For instance, Gerhart et al. (2000b) discussed the various sources of measurement error (error due to items, source and time) that exist in measures of HRM practices. By showing that significant error due to source can exist, they suggested that more reliable measures of HRM practices could be procured by relying on multiple, as opposed to single, respondents. In addition, they showed how such error variance could bias, either positively or negatively, the observed relationship between HR and performance.

The modelling issue is not limited to single variables, but also to the relationship itself. For instance, Becker and Huselid (2006) note that omitted variable bias can occur when a variable is correlated with both the independent and dependent variables, and that failing to account for it can impact the magnitude and the direction of the observed relationship. Clearly, the statistical modelling of the HRM–performance relationship deserves further attention.

Answering the unanswered questions

The questions posed earlier frame a set of theoretical and research issues that need to be addressed if the HRM–performance relationship is to be better understood. The subsequent chapters allow some of the leading researchers in this area to begin to unravel the issues and questions posed, and to provide a set of guidelines and recommendations for future work that could enlighten the field.

The rest of the book is structured around two themes. The first section focuses on the theories and/or conceptual models for the processes through which HRM impacts performance. It begins in Chapter 2 with a detailed analysis and review by Riccardo Peccei, Karina van de Voorde and Marc van Veldhoven of the extent to which HRM can result in positive outcomes for both the organization and its employees. They therefore expand the view of performance beyond the narrow definition of financial performance to explore the impact of HRM on employee well-being. In Chapter 3, Peter Boxall presents a detailed analysis of the concept of high performance work systems and the role of HR practices and HR systems. One of the recurring themes in research on HRM and performance concerns the concept of ‘fit’. This is central both to the link to business strategy but also to the analysis of the concept of an HR system. In Chapter 4, Jaap Paauwe, Corine Boon, Paul Boselie and Deanne den Hartog analyse in some detail the various ways in which fit has been used in the literature, and present an integrated framework to stimulate future research. In Chapter 5 the issue of HR processes is further explored by David Guest and Anna Bos-Nehles, who analyse the concept of implementation of HR and again present a framework within which future research might progress. Chapter 6, by Patrick Wright and Lisa Nishii, forms the last chapter of our first

section and focuses on theories and/or conceptual models. They present the case for the incorporation of multi-level processes in studying how HRM impacts individual employees and, through this, affects organizational performance.

The second section explores more deeply some of the challenges in measuring constructs within the HRM–performance relationship. In Chapter 7, Gary McMahan and Christopher Harris focus on the measurement of human capital. In Chapter 8, Angela Langevin, Susanne Beijer, Jessica Federman, Michel Hermans, Felice Klein, Elizabeth McClean and Brian Martinson review the vast volume of HRM practices that have been measured in this vein of research, provide guidance regarding the most important HRM practices that should be included in research on the HRM–performance relationship and discuss in detail, drawing on best practice research evidence, the ways in which HR practices should be measured. In Chapter 9, Barry Gerhart explores a number of the challenges in statistically modelling the HRM–performance relationship and outlines the steps that need to be taken to arrive at more confident conclusions from our research. Chapter 10, by Sophie de Winne and Luc Sels, explores the evidence about the relationship between HRM and performance in small and medium-sized enterprises. This is a necessary antidote to the typical focus on research in large organizations. It also serves to broaden the perspective and, if there had been a larger body of relevant research, we might also have wished to include a chapter on HRM and performance in public sector and not-for-profit organizations. Finally, in Chapter 11 the editors bring the many ideas together to provide a clear roadmap for theory, research and application in the HRM–performance relationship.

Research and theorizing on the relationship between HRM and performance has the potential to have a significant impact on organizational performance and employee well-being. For these reasons, it is an important topic for research. It has already significantly influenced the credibility of HRM within organizations, as reflected in influential and widely sold books such as Huselid et al.'s *The Workforce Scorecard* (2005). As this research expands in terms of the issues addressed, extends the theoretical understanding of how HRM can impact performance and continues to grow in rigour, there is the potential for HRM to become more firmly established both as an academic discipline and a valuable business function. Our hope is that the ideas and suggestions provided in this book can be a springboard to furthering this evolution.

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