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# Exploring the Road Less Travelled in HRM-Performance Research: A Critical Realist Alternative to 'Big Science'

BRIAN HARNEY  
*Dublin City University*

### Abstract

A review of key contributions in HRM-performance research indicates methodological frailties and an explanatory void concerning why or how HRM performance impacts take effect. This paper considers whether such limitations are attributable to the very scientific perspective that has informed research. In this case prospective solutions will go beyond methodological fine-tuning to involve explorations of the ontological assumptions that underpin research. In advancing this argument, I propose that HRM research would benefit from embracing a critical realist perspective. Specifically, by focusing attention on generative causal mechanisms, critical realism offers the potential to provide theoretically informed, contextually sensitive, and explanatory accounts of HRM interventions. In following this road less travelled, HRM researchers might be better placed to consider emergent processes, divergent outcomes, and new organizational forms.

### Introduction

In recent times research using a plethora of statistical techniques claims to have demonstrated that a strong link exists between human resource management (HRM) practices and business performance (Combs et al. 2006). Continuously absent, however, is a sound explanation of why or how HRM performance impacts take effect. The quest to redress this problem typically involves a version of “normal science puzzle solving” (Kuhn 1962) advocating the traditional fix of more rigorous research methods (e.g., Wright et al. 2005:432). Overall, the implicit assumption appears to be that the absent theory will somehow arise from the empirical data “like steam from a kettle” (Marsden 1982:234).

Having reviewed this tendency, I will follow others in questioning this logic and instead consider whether the theoretical and explanatory void is attributable to the very “scientific” perspective that underpins research in this area (Fleetwood and Hesketh 2006). In this case prospective solutions will involve more than methodological fine-tuning, sophistication, and the advancement of “big science” (cf Wall and Wood 2005). Instead, they will be found through an exploration of the ontological assumptions that underpin research. In this respect “the HR community can ill afford to avoid philosophical issues” (Fleetwood and Hesketh 2006:1977), emphasizing the point that “consideration of ‘ontology’ is fundamental, not optional” (Delbridge 2006:1210). In advancing this argument, I propose that HRM research would benefit from embracing a critical realist perspective as one means to address the key deficiencies associated with the HRM–performance “scientific paradigm.”

To support this proposal, I briefly explore the application of a critical realist perspective to empirical examples by drawing on Ragin's (1987) notion of illusory commonalities, then conclude by highlighting some of the potential pitfalls in applying a critical realist perspective. Despite these deficiencies, I argue that critical realism offers much potential in addressing the explanatory void that plagues extant HRM–performance research. The task at hand is an ambitious one; central to the argument is a criticism not of quantitative methods per se, but rather of the underlying assumptions that are associated with their application under the tenets of a “big science” research agenda.<sup>1</sup> First, it is necessary to locate the argument in the context of the emergence of HRM and the subsequent search for the holy grail of HRM–performance linkages.

### **HRM–Performance Research: The Quest for the Holy Grail**

Increasingly, research efforts have been characterized by an attempt to consolidate HRM as a (if not the) key means of generating competitive advantage (e.g., Huselid 1995). A recent meta-theoretical review of data from 92 studies covering a total of 19,319 organizations found that 20% of the utility available from predicting performance differences among organizations was attributable to high performance work systems (HPWS). The authors concluded that their results “lay to rest any doubt about the existence of a relationship, and offers researchers a baseline estimate of its size” (Combs et al. 2006:524). Likewise, according to Becker and Huselid (2006:911), the focus should now turn to “the size of HR's effect, and the magnitude of potential biases in estimates, rather than simply focusing on whether the effects exist at all.” Yet while these contributions have clearly elevated HRM from operational obscurity, there are a number of methodological challenges that still place limits on the HRM–performance project.

#### *The Romance of “Big Science” and the Realities of HRM–Performance Research*

Research on HRM and performance has been drawn from a rather limited base, pursuing a largely cross-sectional survey-based agenda directed mainly at manufacturing establishments (Arthur 1994, Applebaum et al. 2000). This raises difficulties in establishing the direction of causality and bring into question the relative generalizability of findings. In cross-sectional studies there is no reason to dismiss reciprocal or reverse causation (Edwards and Wright 2001). Indeed, 50 of the 70 study designs reviewed by Wright and colleagues were “postpredictive” (i.e., they measured HRM after the performance period), leading them to comment that “it does make one wonder how such studies can legitimately suggest that HR practices ‘cause’ performance” (Wright et al. 2005:415). Equally problematic, and illustrated by Wall and Wood's review of 25 studies (2005), is that research has typically been plagued by small samples (<300 in 18 of the 25 studies) and low response rates (ranging from 4–84% in the 25 studies).

Turning to measurement issues, work yields little consensus as to what actually constitutes best practice HRM or how it should be measured (e.g., indexes, scales, clusters; Blasi and Kruse 2006). Combs and colleagues (2006) found that the number of HR practices included can range from 2 to 13. Similarly, in an earlier review Becker and Gerhart (1996) noted that 27 practices were used as proxies for HPWS across just five different studies. There is also a dangerous tendency to blur or conflate levels of analysis from policy to practice (Arthur and Boyles 2007). Many studies also rely on a single managerial respondent (21 out of 25 of the studies reviewed by Wall and Wood 2005) and self-reported performance data (12 out of the 25), resulting in low interrater reliabilities (Gerhart 2007). Further, employee voice and outcomes have ironically remained dormant, being used in only one of the 25 studies reviewed by Wall and Wood. If all this suggests a methodological conundrum, it is not facilitated by the varying levels of analysis deployed, ranging from the effects for individual workers (Guest 1999) to plant/workplace studies (Arthur 1994) and firm-level studies (Huselid 1995). Organization-level studies in particular often fail to capture or even acknowledge the variety that can exist among organizations (Wright and Boswell 2002). Further, differing dependent variable measures, ranging from direct productivity considerations (e.g., Ichniowski, Shaw, and Prennushi 1997) to more removed firm-level performance data (e.g. Huselid 1995), make comparison and reaching conclusive assertions difficult.

This brief review indicates that the quest for conclusive proof of a link may not be on as secure a footing as key proponents would have us believe. This is before we might even consider that the publication process is highly skewed toward successful outcomes. In light of such inconsistencies, the cumulative

progress of research findings is far from obvious. Paauwe and Boselie go so far as to argue that “there is little or no convincing empirical evidence that coherent and consistent systems or bundles automatically lead to higher performance” (2005:74). Indeed, even those studies cited as exemplar have been subject to varying interpretations, as the following section illustrates by way of a brief review of research on steel finishing lines by Ichniowski, Shaw, and Prensushi (1997).

#### *Interpretations of Studies*

Ichniowski, Shaw, and Prensushi (1997) provide an example of one of the most detailed and sophisticated research projects into the linkages between the HRM practices and organizational performance. Their research, which focuses on 36 homogenous steel production lines, is cited as exemplary for a number of reasons: their concentration on one very specific type of manufacturing production process, informed knowledge of the process obtained through organizational visits and multiple interviews, testing for alternative explanations, and using a panel dataset of up to 2,190 monthly objective and proximate performance measures (e.g., adjusted lineup time). Overall, their results indicate that innovative work practices raise productivity. They also find support for the critical role of complementarities between HRM practices in achieving these gains.

Drawing on alternative assessments of this study, however, indicates some interesting nuances. Godard and Delaney (2000) note that arguments in favor of synergistic HPWS are troublesome if the full adoption of HRM practices is rare. Thus for them an important but overlooked limitation of the Ichniowski study is that “only two of their thirty-six lines appear to have adopted the high performance model, thus limiting confidence that can be placed in their finding of support for this argument” (Godard and Delaney 2000:489). Further, in the Ichniowski study, all observations conforming to the high performance category were in new production sites, which more than likely employed new hires (see Ichniowski, Shaw, and Prensushi 1997). Consequently, prohibitive worker orientations as might exist with an established workforce (Guest 1987) would have been largely absent. Again, it would appear Ichniowski’s results require some qualification.

Leaving aside the issue of how information from multiple respondents was combined or the extent to which it was consistent across sources (Wall and Wood 2005), other interpretations more seriously question the very basis of Ichniowski’s assertions. Sels and colleagues (2006) argue that a focus entirely on “positively related” performance measures is one-sided, as it provides no indication of whether the productivity gains obtained outweigh the costs of introducing practices. Of equal concern, Gerhart finds evidence of collinearity rather than the fit or complementarities between individual HR practices suggested by Ichniowski and colleagues. The implication is quite damning: Ichniowski et al. conclude that “the synergies created by building these practices together have a stronger effect on performance than do individual practices” (1997:142). My reexamination of their results, however, turns up nothing that really supports this claim (Gerhart 2007). That these findings may not hold up to scrutiny may also be indicative of a deeper underlying problem, exemplified in the notion of a “black box” or explanatory void between HRM and performance (Harney and Jordan 2008).

#### *The Explanatory Void*

Although critical for the lifeblood of the HRM–performance project, HRM research is at present subject to an explanatory void. Guest’s commentary that “statistical sophistication appears to have been emphasized at the expense of theoretical rigour” (1997:264) still rings true (Ferris et al. 2007). Where theoretical frameworks are deployed, this is typically as part of the study’s rationale, or they are used to lend weight to findings rather than to explicitly drive research (Boselie, Dietz, and Boon 2005). While we are on safe ground in assuming that HRM can impact performance, in some shape or form, the key questions that remain are how (Marchington and Zagelmeyer 2005) and why (Hesketh and Fleetwood 2006). Nonetheless, research tends to focus on the degree of investment in HR practices, rather than on explanatory mechanisms (see Evans and Davis 2005). Guest again captures the risk of putting the empirical cart before the theoretical horse: “the risk of following the empirical route is that we allow ourselves to be dominated by statistical convenience. Ideally we should be adhering more rigorously to a theoretical basis” (Guest 2001:1099–1100).

So given these methodological inconsistencies, problems with interpretation, and a “theoretical black hole” what recommendations are on offer to advance research in this area?

### **Typical Recommendations: “Normal Science Puzzle Solving”**

In the main, the solutions proffered revolve around calls for more robust data and better theory (Godard 1993, 1994). For example, Wall and Wood (2005:455–8) conclude their review stressing the imperative of “investment in big science,” advocating “stronger research methods” comprising large-scale surveys, multiple respondents, and more sophisticated research instruments. Notably, each of the recommendations they offer finds contradiction from other experts in the field (see Becker and Huselid 2006), indicating that even the best pathway to technical sophistication is far from obvious.

The implications of calls for better data and better theory is that the HRM–performance project should proceed as is under the authority of scientific analysis, and of the premise of a big science agenda in particular. Arguably, the evidence presented here suggests that HRM–performance research has become characterized by “normal science puzzle solving” (Kuhn 1962). Kuhn argues that the reception of a common paradigm frees the community from having to constantly reexamine its first principles (1962). The paradigm in turn sets the standards for the legitimate work within the science it governs, coordinating and directing the “puzzle solving activity” of those that work within it (Chalmers 1982:90). A consequence is that for those operating within a distinct paradigm, “a failure to solve the puzzle is seen as a failure of the scientist rather than as an inadequacy of a paradigm” (Chalmers 1982:92). As Kincaid (1996:9, 265) notes in his defense of logical positivism, “all failings are contingent factors,” with the only obstacles en route to science being “practical and eliminable ones.” Similarly, within the HRM field any problems in application are seen as merely technical ones, and a multitude of methodological advances, as evidenced by the extent of empirical reviews, are offered to solve them. Likewise, any method that deviates from the norm or questions the scientific approach is deemed heretical (Fleetwood and Hesketh 2006, Dipboye 2007). This is evidenced by a recent content review of 1,674 articles from nine HR-based journals that found remarkable dominance of a “consensus orientation,” with a reliance on prescriptive, managerial, and positivist methods (Keegan and Boselie 2006).

I wish with this paper to destabilize this romantic fixation with “big science” and the “normal science puzzle solving” that defines and drives progress in HRM–performance research. Very rarely have those within the field considered the methodological and theoretical problems encountered so damning as to demand an alternative. Yet as it stands, the HRM–performance project is laboring under a model of science “that succumbed to the criticism of philosophers of science at least 30 years ago” (Kane 1991:245). It follows that the issues identified may be symptomatic of the deeper underlying problems of a big science approach—that is, rooted in foundational assumptions upon which the scaffolding of the HRM–performance project has been erected. Hence, limitations in this area to date may not be reflective of the inability of scholars to develop good theory or to obtain good data per se but rather may be attributable “to the false epistemological and ontological assumptions underlying empiricism as a philosophy of science” (Godard 1994:3). In the next section we make such assumptions more explicit and highlight some of their inadequacies (especially their limited ability to capture causality) before offering an alternative in the form of critical realism. Three quotes help capture and sharpen the essence of the argument:

HR science falls short on all three of the hallmarks of successful science. This is not a failure in aspiration, because there are significant attempts to improve the rigor and relevance of HR research and to advance the field. But HR has not arrived and in some respects the attempts to improve rigor, relevance, and value added have taken the field in the wrong direction (Dipboye 2007:98).

The development and evaluation of constructs and theories has been subordinated to—and to some extent eclipsed by—the search for lawful relationships among empirically observed phenomena. The major reason for this derogation of things conceptual in these fields is the continuing adherence to outmoded models of the conduct of science (Kane 1991:245).

Those who still wait for a Newton are not only waiting for a train that won't arrive, they're in the wrong station altogether (Giddens 1975:13 in Godard 1994:6).

### Limitations of “Big Science”: Unearthing Key Assumptions

In this section we follow others in arguing that progress in this area has actually been hindered by strict adherence to a narrow scientific model (Buckley et al. 1988) and its associated assumptions. These assumptions can be crudely grouped as follows:

- *Empiricist ontology*: Research only concerns itself with observables. Related to this is the notion of value-free observation, separation of the subject and object of knowledge, instrumentalism, and neutral observational language.
- *Positivist epistemology*: The goal is to seek invariant laws or “constant conjunctions of events” that link two or more phenomena or link a conceptual/ theoretical mechanism to phenomenal events in a universal conjunction of the “if A, then B” sort. All knowledge claims and concepts that are not empirically observable are judged invalid; theories that do not yield clear empirically tested hypotheses are inadequate (Delanty and Strydom 2003).
- *Scientistic naturalism* (or some variant of this): holds that research in the social sciences should model itself on the natural sciences (also referred to as “scientism” or “methodological monism”). Implications of this ideal of unified science include the following:
  - a) Concept independence of mechanisms, which exist independently of the agent's conceptions of what they are doing in their activity
  - b) A preference for large quantitative data sets and statistical methods. In order to be able to generalize about regularities in human and social behavior, it is necessary to select samples of sufficient size from which inferences can be drawn about the entire population.
  - c) The task of science is to enable prediction and control of social and natural events. Orientation toward the manipulation of the world rather than understanding it.

These assumptions are usually deployed implicitly and without direct questioning within HRM research. Criticisms of the methodological frailties of positivist research in HRM are well rehearsed (Legge 2005). Instead, the goal here is to consider the philosophical problems of utilizing the assumptions of “big science” as a meta-theory for HRM–performance research. In the main, research proceeding under the tenets of “big science” does not have time for reflections on the ontological conditions of social reality but instead is driven by the need to remain scientifically true to natural sciences (see point a above), has confidence that this possible and desirable (see point c above), and is committed to quantitative methods (see point b above; Steinmetz 2004).

One consequence is that “big science” tends to encourage “technicism,” which means that techniques and methods and specifying the “correct model” take precedence over the development of knowledge (Ragin 1987, Delanty and Strydom 2003). This is exemplified in contemporary discourse of HRM, which instead of being problem- or theory-driven tends to place undue emphasis on methodology and is largely data-driven (Whetten 1989, Dipboye 2007)<sup>2</sup>. This hints at one of the key weaknesses of big science: its focus on prediction to the neglect of explanation. Quite simply, observed statistical associations do not constitute a theory while they can do little to resolve issues of explanation and interpretation of why a relationship might hold (Godard 1989, Hesketh and Fleetwood 2006). The problem is that big science presupposes a certain world order, and then maps reality back to fit this order. Moreover, increasingly complex analyses such as structural equation modeling and hierarchical regression cannot substitute for research methods and strategies that would allow causal inferences (Dipboye 2007). Thus the solution to the explanatory void so prevalent in HRM–performance research may lie not with technical elegance, but rather with an overhaul of the key assumptions that have driven much of this research. Such a view has been put forward in other fields. Halfpenny argues that research in sociology needs “to step away from” the Humean analysis of causality and commitment to empiricism to consider programs “which explain the observed features of the world by identifying the underlying mechanisms that are causally responsible for them”

(1987:36). Recognition of this requirement for “complex causality” and “robust explanation” (Hesketh and Fleetwood 2006:986) has not gone totally unnoticed in HRM research. Application of the resource-based view of the firm points to idiosyncrasies and “unique fit” as the basis of competitive advantage, thereby drawing attention to the causal mechanisms underpinning and conditioning these. The next section explores one way we can foster such an understanding.

### **Critical Realism: The Road Less Travelled in HRM Research**

This section argues that a critical realist meta-theory provides a feasible alternative to the inappropriate premises put forward by the dominant HRM–performance “scientific paradigm.”<sup>3</sup> Specifically, and elaborated in detail below, critical realism relies on an open as opposed to a closed system ontology, offers a way to overcome the epistemic fallacy of collapsing ontology onto epistemology that haunts both positivist and constructivist research, and provides a means to capture generative mechanisms and emergence (Sayer 2004). Consideration of ontology surfaces a range of philosophical concerns that have been muted within HRM, partly because the assumption of theory neutral observation militates against critical reflection (Lawson et al. 2006, Dipboye 2007). The argument put forward in favour of critical realism is by no means novel (Godard 1994, Mingers 2000, Lawson 2003, Fleetwood 2005). At its analytical heart lies “the dynamic relationship between the generative potential inherent in social structures and its contingent realization through corporate agency, as well a . . . form of causal explanation sensitive to contextual complexity” (Reed 2005:1637). Indeed if the relationship between HRM and performance is judged to be complex, uneven, and variable, critical realism would seem to offer one potential for contextually rich explanatory accounts of HRM interventions (Edwards 2006).

Critical realism eschews the positivist search for laws and seeks instead to identify “tendencies” associated with the operation of structural mechanisms in open systems (Thompson 2004). Critical realism charges big science with committing the “epistemic fallacy,” letting questions of what we can know/how we may have knowledge (epistemology) determine what can exist/is knowable (Bhaskar 1978, Cruickshank 2003). For the present purposes, I will consider five main “domain assumptions” that underpin critical realism’s potential.

1. *Realist versus empiricist ontology.* Realism denotes the assumption that an external reality exists of things not observable or measurable beyond our senses that can nevertheless be real and known (Outhwaite 1987). As Johnson and Duberley (2005) note, such intransitive objects exist and can act independently of their identification in human knowledge; this is Bhaskar’s (1978) “intransitive domain.” Purcell and Kinnie (2007:540) have recently hinted at the importance of such insights: “methodological considerations have determined what questions have been asked while factors beyond the reach of the chosen method, however important, have been ignored.” The “transitive domain,” on the other hand, consists of socially produced knowledge that facilitates us in making sense of the world, largely through theoretically determined descriptions. Moreover, to follow the big science agenda argument is to conclude that because an empirical association or regularity does not exist that there is no causal connection, which “is misleading in the extreme” (Hesketh and Fleetwood 2006:678). Critical realism, on the other hand, offers an alternative route that goes beyond surface appearances to establish a theoretical understanding of the causal powers of real structures and mechanisms that account for the reality we observe (Godard 1989, Tsang and Kwan 1999).
2. *Open versus closed systems.* The distinction between open and closed systems is central to critical realism. Critical realism takes issue with the actualist ontology underpinning of the “big science” agenda. An actualist ontology is defined in terms of a closed system logic made up of a fixed set of unchanging regularities that are judged to be manifestations of universal causal laws. This is the epistemic fallacy in action, “given an empiricist epistemology the ontology produced is an actualist closed systems model of being” (Cruickshank 2003:97). This logic is diffused into HRM research as “to suggest that some HRM practices are statistically associated with increased performance, is to assume regularity and hence closure” (Hesketh and Fleetwood 2006:685). However, the open systems of the social world mean that such regularity and closure are rare even in the natural realm (Lawson 2003). Explicating Lawson’s critique to the domain of HRM suggests that to assume that HRM can automatically be reduced to

productivity-enhancing or -damaging effects that are realized as intended is to commit to the fallacy of isolationism, while to assume that HRM operates in isolation from the institutional context and broader dynamics of the economy in which is embedded is to commit to the fallacy of atomism. It has long been noted that relations at the point of production are complex and variable (Edwards and Wright 2001).

3. *Layered versus flat ontology.* Critical realism proposes that there are deeper underlying structures and generative mechanisms “lying beneath the surface phenomena of direct experience” (Lawson 2003:229). It is this multilayered ontology that differentiates critical realism from extreme interpretative accounts that see the world in a constant state of flux (Edwards 2006). For Bhaskar (1998) a layered ontology is made up of three dimensions: (1) the real, consisting of causal powers, enduring structures, generative mechanisms and their tendencies. These “real” nonempirical structures and mechanisms can produce (2) the actual, consisting of events, entities, and/or states of affairs, only some of which are conceptually mediated as (3) the empirical, consisting of experiences, perceptions, and impressions of events. The stratification between the levels of the empirical and the real allows for a disjuncture between underlying causal mechanisms and observable phenomenon (Steinmetz 2004). Thus, critical realism offers a genuine attempt to understand institutional embeddedness and provide contextually rich accounts (Delanty and Strydom 2003). Possibilities can exist that are never actualized; such real yet unactualized potentialities remain as tendencies in the deeper layers of reality. In contrast to an actualist ontology, if correlations are not observed, this does not mean that causal mechanisms do not act in regular ways, only that their effects are on occasion suppressed by the action of countervailing mechanisms.
4. *Explanation versus prediction.* It follows from the domain assumptions of open systems and a layered ontology that for critical realism the purpose of social science is not prediction, but rather explanation. The mode of inference is retroductive questioning the conditions of possibility (i.e., what must the world be like for such conditions to exist?) and working backward to gain a theoretical understanding of the mechanisms of cause and effect that underlie empirical tendencies (Reed 2005). Although these mechanisms are not directly observable, they underlie and govern the events of experience and hence explain why regularities occur (Johnson and Duberly 2005). The switch is from Humean causality with its ability for mere emaciated explanation to complex causality underpinned by robust explanation (see Hesketh and Fleetwood 2006).

Critical realism’s argument that explanations have to identify underlying causal powers or mechanisms (Sayer 2004) would seem quite useful in addressing some of the dilemmas facing the HRM field. For example, the resource-based view (RBV) of the firm views the employment systems as a complex social structure (Becker and Gerhart 1996). Central concepts include the idea of causal ambiguity as subtle interconnections between contingent factors and path dependency that recognizes the emergent nature of strategy, and HR decisions based on what “feels” right and fits with an organization’s culture (Legge 2005). The irony is that while this complexity is continually put forward as the basis for making each organization’s experience unique and thereby offering a route to sustainable competitive advantage, it is “routinely overlooked” (Hesketh and Fleetwood, 2006:686). Further, and very much in keeping with the logic of the RBV, critical realism also helps capture the essence of entities existing independent of their articulable identification, including tacit knowledge and cultural effects (Fleetwood 2005). Unfortunately, however, HRM–performance research still craves the fixed regularities and closed systems of classical scientific approaches, and until this is redressed our understandings can only ever remain very narrow and partial (Dipboye 2007).

5. *Multiple methods.* Critical realism does not favor or privilege any one method for the study of organizations and how they actually work (Ackroyd 2004). While I have been quite critical of quantitative methods, this is only as a result of their application under the tenets of logical positivist/big science assumptions (Godard 1993). Critical realists instead embrace a multimethod approach, recognizing the potential that qualitative analysis can bring to bear in terms of surfacing complexities, processes, and contextual idiosyncrasies (Sayer 2004). Ragin’s thesis (1987) that it is possible to combine case- and variable-oriented research, in particular by identifying sets of causal conditions that may affect some outcome, may have particular value here. This argument for multiple methods for HRM–performance research is not novel, but critical realism ensures this call is underpinned by a robust and defensible line of argument that

suggests an epistemological and ontological break from both big science and social action analysis (Ackroyd 2004).

### **Critical Realism in Action: Implications for Research**

The domain assumptions identified help to map critical realisms' intellectual niche. Critical realism charges big science and interpretivism with reducing questions of ontology to epistemology (the epistemic fallacy), and in so doing with denying the existence of mind independent causal structures and mechanisms. The value of critical realism is that it adheres to a scientific agenda of rationalism and objectivism while rejecting the key assumptions central to a big science agenda (e.g., closed systems, flat ontology; Halfpenny 1987, Godard 1993). This is largely the task that now confronts the HRM–performance research project; specifically this means the following:

1. Quantitative attempts to find/demonstrate the holy grail of HRM–performance linkages have to be judged as flawed at best and destructive at worst. Such approaches provide only limited insight into surface empirical phenomena, which in practice require further explanation through consideration of the mechanisms and structures found at deeper ontological levels. This mandates consideration of multiple levels of analysis and sympathy to contextual enactment of practices as well as paying due attention to employee perceptions and outcomes.
2. The purpose of social inquiry should be to produce causal explanations developed via theoretical interpretations as opposed to predictions. The causal-explanatory method is more suited to the openness of the social world, capturing discontinuities, tendencies, and emergence and thereby avoiding mistaking the particular for the general (Lawson 2003). For example, although not requiring the activity of explicit identification, class and patriarchal structures will be transformed and reproduced through various types of activity (Fleetwood 2005). The employer–employee relation itself presupposes the existence of capitalist structures and activities (e.g., contracts, wages, hiring, and so forth; Tsang and Kwan 1999). Here theory is indispensable to facilitate our understanding and provide better causal accounts than are provided by existing knowledge (Ackroyd 2004).
3. Critical realism encourages critical reflection, suggesting that “much knowledge (and the data derived from research) is flawed because it is under-theorized or otherwise ill-founded and inadequate, often coming to conclusions that serve the interests of the powerful” (Ackroyd 2004:158). In acting as a philosophical underlaborer, critical realism suggests why this may be the case, illuminating the conditions for human inquiry rather than attempting to certify particular theoretical or substantive claims (Johnson and Duberly 2005). This contrasts with the theoretical logic of big science's collapsed ontology and value-free observation, which mitigates against exploration of values with the consequence that “critique and change are purposefully excluded from what constitutes legitimate enquiry” (Laughlin 1995:81).

This type of analysis also offers a rich potential for informing policy, such as explaining why and in what circumstances policies are likely to be effective (Ackroyd 2004). Given space constraints, the best this section can do is merely point to areas where a critical realist explanation might work in action; one useful concept here is the notion of “illusory commonalities.” For Ragin (1987), illusory commonalities exist whenever two features appear similar but have different effects. This suggests that the generative tendencies of practices are actualized in a different and indeterminate manner (see Edwards 2006). Ragin uses the example of employment tests, comparing their use by some employers as a gating mechanism to screen out illiterate workers, regardless of the level of literacy required on the job. On the other hand, employment tests are used by other employers to identify applicants with the greatest job-relevant skills. Thus it would be “a mistake to equate these two uses in an investigation of a firm's hiring practices even though they appear to be similar” (Ragin 1987:48). Accordingly, a more valid explanation would involve contextualizing of the causal importance of different conditions and interpretations of employment testing (Ragin 1987: 48-9).

A similar point is made by Delbridge (2004) in his study of working teams in two “high performance” workplaces. In an argument complementary to Ragin's “illusory commonalities” thesis, Delbridge found that although both firms had very similar HRM and working systems, workers' perceptions and experiences of these systems varied in the two cases. Delbridge notes that such nuances



and negotiation conditioning the application of practices highlight the limitations of positivist survey-based research as “such studies may obscure or omit more than they reveal” (Delbridge 2004:271). More contextualized accounts may provide a better route to capture and explain how relatively dissimilar cases experience the same outcome or how relatively similar cases experience difference outcomes (cf. Ragin 1987, Lawson 2003). As Edwards (2006) argues, the key point is that practices such as team working do not have predefined causal influences, they had sets of potential that can be released (actualized), sometimes in unintended ways. At the same time, recognition of structural layers and emergence leaves scope for more critical accounts (see Godard 1993).

### **Summary: Moving Beyond “Big Science”**

In this review I have suggested that the methodological inconsistencies and the explanatory void that haunt the HRM–performance project may be rooted in the very “scientific agenda” that informs research in this area. In this case, it is deeply flawed to proffer “methodological fine tuning” and the advancement of “big science” as the prospective solution. Instead, fruitful answers may lie “beyond big science” in considering a critical realist alternative. By explaining the observed features of the world through identifying the underlying mechanisms and emergent processes that are causally responsible for them, critical realism offers the potential for more contextually rich, explanatory accounts of HRM interventions (Godard 1993, Edwards 2006).

This task is by no means simple, nor without its critics. Nonetheless, the value of a critical realist perspective may be best judged by any innovative thinking and critical reflection that it prompts. Further, for some commentators critical realism provides a grounding for what many researchers have often tacitly done (Godard 1993, Reed 2005, Edwards 2006). At the very least, surfacing the ontological assumptions that underpin research can only lead to more dialogue and internal conversations and less dogma and prescription than is currently the case (Godard 1993, Delbridge 2006). Moreover, a more explicit explanatory focus may go some way in bridging the researcher–practitioner gap (Rynes et al. 2007), as practitioners will be equally (if not more) interested in establishing what practices are likely to be most effective in their organizations and under what circumstances (i.e., the conditions of possibility of practices). Here a correlational approach is much less adept (Fiss 2007). The achievement of this goal will be facilitated by the fact that critical realism does not advocate the superiority of putting all its eggs in the same research method basket, instead initiating a useful line of questioning, including why we still tend to consider surveys explanatory and case studies purely exploratory and illustrative (Sayer 2004). Ultimately, in pursuing this road less travelled HRM researchers might be better placed to consider the realities of emergent processes, divergent outcomes, and new organizational forms, all of which find little significance in present analysis. In this vein it may be useful for HRM–performance researchers to take heed of a timely reminder that “the fundamental mission of the academic management discipline is not to propagate perceptions of ‘best practice’ in ‘excellent companies’, but, first of all, to identify and explain what happens in practice” (Boxall, Purcell, and Wright 2007:1).

### **Endnotes**

1. This terminology draws directly from Wall and Wood (2005). Hesketh and Fleetwood (2006) similarly refer to the dominant “scientific” agenda in this area. In following Steinmetz (2004), by “big science” I mean work that adheres to the principles of methodological positivism, that is, advocating exclusive progress through the use of deductive methods and holding an epistemological position that considers knowledge claims that are not empirically observable to be invalid (see Godard 1994 for a similar argument in relation to industrial relations research).

2. This is not to imply that there is no theory behind empiricism; its implicit theory is surfaced in the next section. Indeed, the irony is that “the claim that empirical verification provides the surest form of knowledge is itself based upon a metaphysical assumption which is not empirically verifiable” (Chia 1997:689).

3. Following Bhaskar (1978, 1998), I use the most common terminology “critical realism” here. This meta-theory is elsewhere referred to as “theoretical realism” (Godard 1993, 1994), “social realism”

(Cruickshank 2003), “pragmatic-critical realism” (Johnson and Duberley 2005), or to indicate the variability of interpretation it can also be placed as part of the broader “social ontology” project (Lawson et al. 2006).

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