

**Industrial Relations and Critical Realism:
IR's Tacit Contribution**

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Editor's foreword

The Warwick Papers in Industrial Relations series publishes the work of members of the Industrial Relations Research Unit and people associated with it. The papers may be work of a topical interest or require presentation outside the normal conventions of a journal article. A formal editorial process ensures that standards of quality and objectivity are maintained.

There are perhaps two real or imagined, but no less fundamental, critiques of Industrial Relations as an academic going concern. One is that Industrial Relations is a largely atheoretical discipline. The second takes this further to say that it is no discipline at all, or rather the awkward and perhaps illegitimate offspring of labour economics and industrial sociology. In this paper Paul Edwards takes issue with both of these notions. Whilst acknowledging that its focus is often engaged with practical employment issues and concerns, he takes the example of labour productivity to demonstrate that Industrial Relations scholarship can make a strong theoretical contribution even when utilising this approach. He further argues that a more explicit acknowledgement of its institutionalist heritage, and a clearer recognition of the relevance of the critical realist approach, would help confirm Industrial Relations as a field of study in its own right. He constructively concludes with a research agenda to this end.

Jim Arrowsmith

Industrial Relations and Critical Realism: IR's Tacit Contribution

Paul Edwards

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Abstract

Industrial relations (IR) is often accused of being atheoretical. Yet there has been a tradition of tacit theorizing that links IR to an important strand of social science theory, institutional analysis. There are further connections to the philosophy of science laid out in critical realism (CR). This paper argues that these connections need to be made more explicit and that a programme of IR research based on them can then be developed. The connections are first identified by laying out the core principles of CR and institutional analysis and the implications for IR. Illustrations of IR research that are consistent with CR are then indicated. The work of the leading CR theorist, Tony Lawson, is addressed: Lawson offers a CR-based perspective on a key issue, the UK productivity record, but his view is in fact insufficiently based in CR, and use of IR research can suggest a richer account. Finally, a comparative research programme is sketched. This reflects the need for IR to pursue its links with sociology and political science, with rather less emphasis on the relationship with labour economics, which has traditionally defined the field.

Critical Realism (CR) has attracted considerable attention in management studies in general (Ackroyd and Fleetwood, 2000; Reed, 2005) and in specific fields such as operations research and systems (Mingers, 2000). It has enjoyed only the most passing attention in industrial relations (Godard, 1993; Fleetwood, 1999). This paper builds on an earlier paper (Edwards, 2005b) which did three things. It demonstrated that explicit reference to CR in IR was indeed very rare; it identified examples of IR research which were consistent with the claims of CR; and it suggested that more attention to the connections between the two were mutually beneficial. The present paper sets out to recover the theoretical contribution of IR and hence to suggest how it can contribute to a CR project. This leads in the conclusion to ideas for a new research programme.

The paper takes IR as a key case of the argument of Reed (2005: 1637) who cites such classic authors as Bendix, Braverman, Fox, and Dalton, and goes on as follows.

The overriding research task for a critical-realist inspired study of organization and management is to retrieve and renew this classical explanatory focus on changing organizational forms and discursive technologies – within the material conditions and social structures taking shape in contemporary capitalist economies.

IR nearly exemplifies this research strategy.

CR has tended to operate at a high level of theoretical abstraction. Though there have been clear illustrations of empirical implications, as in the work of Sayer (2000), these illustrations have been rather rare. The empirically minded economist or sociologist turning to CR texts might ask how her concrete research programme might be informed by CR. This paper attempts to deal with this issue.

In doing so, it addresses that stream of IR research labelled in the earlier paper contextualized comparison, a term borrowed from Locke and Thelen (1995) and explained below. This stream tries to understand social processes in context, and has much in common with sociology and political science. One could also identify a stream that is more driven by deductive methods and has more parallels with labour economics. Nothing is said directly about the latter, though some remarks on its relative importance are made in the conclusion.

The paper begins with a brief sketch of the core elements of CR, together with an indication of why the approach has value in IR. The second section outlines the main features of IR's theory and methods. These features include important shared origins, in the work of John R. Commons, with a major strand of social science analysis, institutionalist theory. That theory is pertinent because it also has close connections with CR. It acts as a bridge between CR's abstract ontological analysis and concrete research programmes, and IR should be seen as part of this bridge. Third, some strands of research are used to illustrate a research programme that is consistent with CR. Fourth, the work of the leading CR theorist, Tony Lawson, is addressed, to argue that some aspects of an empirical CR programme are insufficiently developed in his work, and that an IR perspective suggests ways in which a stronger programme can be pursued. The conclusion takes up the theme of a research programme and the ways in which it can be developed through comparative analysis.

1 Critical realism¹

CR is an approach to the philosophy of science that seeks an alternative to two dominant positions (for cogent outlines, see Sayer 1992 and 2000). These are positivism and an approach variously labelled as relativism and social constructionism. Positivism seeks explanation in terms of law-like empirical regularities. It is faulted for addressing only empirical regularities rather than the underlying mechanisms producing these regularities; its basis in deductive-nomological approaches prevents it from asking *why* things occur as they do. Social constructionism focuses on the social processes through which people create meaning. It tends to ignore the influences of structures that lie outside the processes of social construction.

There are many variants of constructionism, and indeed of CR. The present approach follows those CR writers who recognize the value of a constructionist position but wish to add to it. Consider the example of a performance appraisal scheme. A constructionist approach would argue that 'performance' is not an objective characteristic but is defined through political processes (see Edwards and Wajcman, 2005: 96-101). It would go on to address how different definitions of performance are created and sustained. This is very valuable; yet there are two linked weaknesses. The first is a tendency to focus on construction to the neglect of the structural context in which it takes place. Second, revealing generic processes is stressed over causal explanation. It is true that performance is politically defined, but the ways in which the relevant processes occur will vary, and it is important in addition to ask why construction takes a *particular* form under given conditions.

CR argues that there are real, if unobservable, forces with 'causal powers' and that it is the task of science to understand the relevant mechanisms. The social world is seen as being different from the natural because it requires human intervention, but it does not follow that society is wholly the product of human design or discourse: rules, norms and institutions develop with logics independent of the choices of individual actors. CR stresses that causal powers are not necessarily activated and is thus very sensitive to the importance of institutional context. It aims to move beyond the discovery of empirical regularities to understand the mechanisms that not only produce these regularities but also determine when they will occur and when they do not.

Sayer (2000) gives examples of realist research in practice. One with resonances with IR debates is drawn from Morgan and Sayer (1988).

Conventional, 'taxonomic', approaches use large data sets to seek invariable relationships between independent factors and performance. But such relationships rarely exist because of the 'openness of systems'. Morgan and Sayer switched to an intensive methodology, treating firms in causal rather than taxonomic categories. . . . [E]xplanations as to why firms behaved as they did were in fact easier to come by than would have been possible through seeking determinate statistical relationships (Sayer, 2000: 24).

A further IR example would be the link between unemployment and union membership. Much UK and US research finds an inverse link, but studies in other countries found no such link or even a positive one, for reasons to do with their systems of unemployment insurance; an alternative approach to the links between unions and the development of capitalist economies then addresses sets of

¹ Parts of this section are drawn from Edwards (2005).

institutional conditions including the unemployment insurance system (Western, 1997).

CR has a substantial philosophical underpinning to these ideas. These issues may be pursued through the works of Sayer, who refers in turn to the well-known CR scholars notably Roy Bhaskar and Margaret Archer (see Archer, 1998, also Danermark et al., 2002). For present purposes, these deeper aspects can be left to one side, and no necessary commitment is made here to any larger CR programme. The argument is simply that CR provides a grounding for what IR researchers have often tacitly done, and that making the links more explicit may help in two ways: to establish the social science credentials of IR research, and to demonstrate IR's contribution to a social science programme.

2 IR and its tacit theorizing

Any explicit discussion of CR is virtually absent in IR. Two volumes reviewing analytical development of the field make no reference to CR (Ackers and Wilkinson, 2003; Kaufman, 2004a), and a search of articles in three leading journals over a five-year period found no explicit use of the term CR (Edwards, 2005b). Yet, to use Lawson's (1997: 247) comment on economics, there are approaches 'which seem broadly consistent with critical realism'. Or, at a more concrete level, Fleetwood (1999: 474), in providing a CR-based critique of economic models of trade union behaviour, says that 'something akin to' his preferred approach can be identified in a small number of extant studies. The review of IR journals just mentioned examined 353 papers, and concluded that 27 were 'consistent' with CR in implicitly addressing the causal powers of various phenomena and the causal mechanisms connecting them. In the study of work organization more generally, CR also appears to feature little. Watson (2004), for example, in his call for 'critical social science analysis' makes no reference to CR.

The reasons for this state of affairs can be expressed in 'negative' and 'positive' terms. In negative terms, IR defines itself in part in relation to the limits of core disciplines. In their review of the field, Bain and Clegg (1974) noted that economists had produced poor explanations of pay determination, while sociologists' efforts to link trade unions to social class foundered on a failure to grasp the nature of unions. The solution was to offer a more integrated approach, that drew on several disciplines and that respected the context-bound nature of IR phenomena. An account of democracy in trade unions, say, would need to start from differing definitions of democracy, so that the meaning of democracy will be different between unions in different countries and even between unions in the same country that have different organizations and traditions. Yet such an analysis led to an emphasis on facts at the expense of theory, a tendency that has been highlighted in a string of commentaries (Winchester, 1983; Berridge and Goodman, 1988; Kelly, 1998; Towers, 2003). As Kaufman (2004b: xv) notes in his review of the international development of the field, it 'is largely an applied area of problem solving, and theoretical development has rather badly lagged behind'.

This is not to say that IR lacked all theory. There clearly were efforts to explain observed phenomena through analytical frameworks, and there were efforts to develop theories 'of' industrial relations, the most noted example being the work of Dunlop (1958). But theory building tended to be eclectic, and to look inwards rather than outwards. That is, theories were developed to address specific problems, but there was little attempt to take any insights and use them to advance theoretical issues

in 'home' disciplines. IR has lacked attention to 'fundamental issues concerning the nature and purpose of social science theory and research, as raised in the philosophy and sociology of science literature' (Godard, 1993: 284).

In positive terms, IR had a clear intellectual basis in institutional economics, one of whose key founders, John R. Commons, was a leader in IR (Kaufman, 2003). The depth of this basis is illustrated by Kaufman's (2004b: 95-116) analysis of the 'science building' component of IR in its country of first development, the US: Kaufman details in the space of 21 pages what the early institutionalists said in relation to theory and methodology and how this shaped IR. Yet, as he admits, theoretical development was often 'inchoate', and a leading work of institutional analysis, Commons's *Institutional Economics* (1934), had a 'negligible influence' on IR (pp. 101, 102). Hodgson (2006) similarly notes the lack of a 'systematic statement' of institutionalism, and the inability of the approach to address such key issues as the operation of markets and the nature of incentive structures. It was thus in a weak position to respond to more rigorous and mathematical models.

Several scholars have analysed 'old' institutionalist analysis, often stressing its strengths as against newer approaches using the same label (Jacoby, 1990; Hodgson, 2006). In the words of Stinchcombe (1997: 6), new approaches focus on what is easy to 'mathematize', to the neglect of the 'guts of the causal processes of institutional influence', that is, the complex and uncertain negotiation of order. The purpose here is not to repeat these analyses, but to use them as a bridge between empirical IR and the philosophy of CR.

Institutional economics, with its emphasis on historical variation and the embeddedness of economic processes in social institutions, is strongly compatible with both IR and CR. Jacoby's (1990) important essay on 'institutional labour economics' (ILE), which is contrasted with new efficiency oriented ILE (NEO-ILE), makes several key points. The older ILE tradition stressed four explanatory tenets: indeterminacy of the labour contract, and the importance of local custom or bargaining; endogeneity; behavioural realism (meaning realistic assumptions, not realism in a CR sense); and diachronic analysis. These features, says Jacoby, provide a better explanation than does NEO-ILE, which seeks an explanation of observed phenomena in terms of efficiency: American internal labour markets for example are seen in NEO-ILE as an efficient means to induce worker commitment. Yet this functional and synchronic analysis cannot explain some key facts, such as that the alleged efficient features emerged many years after the appearance of the large firms that apparently stood to gain from these features: why did it take so long for efficiency to be identified? And other national IR systems operate with very different forms of organization. An explanation sensitive to history and context works better although, as Jacoby admits, it lends itself less well to clear hypothesis testing than does work in the NEO-ILE style. In short, a search for the complex and embedded causal powers of institutions drives ILE, but there is also the problem, shared with CR, that explanations can appear to be idiosyncratic and *ad hoc*.

Hodgson (2006) has further crystallized some of the core tenets of old institutionalists.

- First, an institution is not necessarily anything as formal as a firm or a trade union; the term also embraces ways of thinking or acting that have a degree of permanence.

- Second, institutions do not merely constrain autonomous individuals; they actively shape the ways in which people think and act.
- Third, knowledge is a key resource of organizations, but it is embedded in customs and routines rather than being simply the property of any one group.

Such insights are strongly in tune with empirical IR, and illustrate what is meant by contextualized comparison. Let us for simplicity consider the three points from Hodgson. First, much of IR traditionally addressed the creation of rules governing the employment relationship, notably a series of classic studies of custom and practice rules (Brown, 1973). Such rules can be seen as institutions, and research demonstrated the varying degree of fixity of different kinds of rule (Edwards, 1988). For example, some are firmly grounded in workers' expectations, and they are defended if challenged; an example from the past would be demarcation rules governing which trades could do which jobs. Other rules are merely weak understandings that can readily be overturned.

Second, workplace institutions shape how people behave. Classic studies of occupational crime showed that the relevant practices were features of occupations, not individuals (Mars, 1982). A worker who entered an occupation with a strong set of norms would be shaped by those norms and come to accept them as natural. Lupton (1963) opens his account of workplace politics by describing his first day as an engineering apprentice; as he was labouring at a task at the end of the day, he noticed a group of his mates gathering around him, who silently conveyed the message as to when he should stop work. Individual preferences are embedded in institutional norms and assumptions.

Third, tacit knowledge has been a theme of workplace studies (Kusterer, 1978; Halle, 1984). Such knowledge means, first, that efforts at rationalization are bound to be incomplete at best, and, second, that knowledge can be contested. Workers can retain knowledge and bargain about its use. Even in modern and highly rationalized settings such as call centres, workers use their judgement as to how to perform tasks. In doing so, they sometimes make work proceed more smoothly, and indeed rescue management from its own rules, by for example responding flexibly to a customer rather than sticking to their instructions. But they can also use their tacit knowledge to create their own social space independent of management (Korczynski, 2002).

Such empirical results can, then, be readily expressed in institutionalist terms. The link the other way, from institutional analysis to CR, does not need spelling out, save to say that the emphases on history and context are clearly compatible with CR.

3 Illustrations of explanatory development

IR in particular and institutionalism in general are, as noted above, criticized for the absence of theoretical progress. They may be able to explain some of the complexities of workplace rules or trade union behaviour, but is this any more than descriptive detail and *ad hoc* explanation? Developments in two areas illustrate ways in which further progress has been made (for other illustrations, see Edwards, 1995).

Teamwork

Teamwork has been a major innovation in workplace relations in the past 20 years, though it has important historical precedents. Debates on it neatly illustrate different empirical and theoretical approaches in IR. Teams have often been promoted as means to increase efficiency while also meeting workers' interests in job autonomy. Some IR researchers demonstrated that this was far from being the inevitable result,

producing evidence that some teams generated stress and work intensification. Debates on teams, and also on the closely related issue of total quality management, often took on an essentialist character, with proponents singing their praises and critics exploring their negative features (Wilkinson et al., 1997). Further research refined the analysis, in three main respects (Edwards et al., 2002).

First, rather than assuming that there was a uniform phenomenon called teamwork, analysts identified different kinds of teams. A major distinction was between the teams characteristic of lean production and those based on ideas of autonomous work groups. In the former case, team autonomy was tightly restricted whereas in the latter it was much more extensive. The causal power of 'teams' thus varied according to the type of team involved. Other more detailed distinctions were also introduced, for example the degree to which managements displayed real commitment to team ideas. Such commitment was shown to be important in shaping the extent to which the potential for teams to deliver desired outcomes was realized in practice.

The second refinement was to address the context in which teams operated. The 'teamwork dimensions' model identified three aspects of the context, which were labelled the normative, governance, and technical dimensions (Thompson and Wallace, 1996). The point was to show that the 'effects' of teams were shaped by each of these dimensions, rather than existing independently. Empirical research in a single company in different countries showed that the 'same' teamwork ideas worked quite differently in different national contexts (Thompson et al., 1995). To take another example, Ortiz (1998) used case study methods to examine teamwork in General Motors in Spain. He framed the analysis in terms of largely critical views of teams from a union and worker viewpoint in the US and UK and more favourable responses in Germany and Sweden. He showed that Spain fell between these two extremes, which he explained in terms of the way in which the IR system allowed negative features to be contained and positive features to be developed. Hence, in the UK, teams are often seen as a threat to trade unions, because they can undermine unions' claims to be the sole legitimate voice of workers; teams set up alternative channels, and in the absence of strong legal support for unions the danger is that firms will decide to operate without unions. In Spain, by contrast, unions were institutionally entrenched, and teams were not so much of a threat. In CR terms, teamwork has causal powers of positive and negative kinds, and how they are actualized depends on specific conditions.

Third, researchers identified sets of conditions that made teamwork more or less successful. These conditions included the extent of job security and the structure of workplace relations into which team experiments were introduced. It should be stressed at this point that a factor such as job security is not to be seen as having necessary effects. It is true that studies have shown that an absence of security tends to undermine team experiments, because co-operation and *worker* commitment through teams is unlikely where the *employer's* commitment to maintaining jobs is weak. As Thompson (2003) has argued, there are important structural features of modern capitalism that make it hard for employers to keep their promises (even when these are genuine). Yet other studies have revealed cases where job security has been weak but where a degree of success of teams was achieved. One set of reasons concerns employee expectations: where there has been little history of employee involvement, even quite modest moves can be valued even though security remains uncertain (Marchington et al., 1994). A second set of reasons concerns the context of a team experiment. Where workers lack alternative jobs, and where they know that an

experiment offers a chance to retain jobs, they may co-operate with it. Such conditions are unlikely in themselves to be sufficient for this result. Thus if an experiment is one of a series of failed managerial promises, or if it changes little in reality, acceptance is likely to evaporate rapidly. But where there seems to be concrete change, acceptance can occur. For example, Wright and Edwards (1998) report a case in which managerial intent was demonstrated by the imposition of teams on unwilling line managers and supervisors; in addition, specific and meaningful rights were given to teams, which were thus far more than a rhetorical device.

An important part of this line of research was the demonstration that teams did not have pre-defined causal influences. They had sets of potentials that could be released, sometimes in unintended ways. One study for example found that the language of empowerment used by managers was taken up by workers, who began to hold managers to account against the new image of a democratic organization and who took the rhetoric of empowerment as a means to make their own demands (Rosenthal et al., 1997). Such findings are strongly consistent with the arguments of CR about the indeterminacy of causal powers.

Pay determination and the causal powers of legislation

If there is one issue that illustrates the strengths and weaknesses of institutional analyses in IR, it is the setting of pay. For over 100 years, institutionalists have noted the inability of conventional economic theory to explain the diversity of pay, notably why the 'same' occupation in the same labour market can be rewarded differently and why externally mandated pay rises, such as those associated with minimum wages, have not had the expected effects in terms of reduced numbers of jobs.

It is also true that conventional explanations are not always convincing. Consider the two results just noted. The first is commonly rationalized in terms of efficiency wage theory: paying more than a market rate helps to secure worker commitment and induces discretionary effort (see Groshen, 1991). The second is usually explained in terms of monopsony power: employers had this power, and the effect of a rise in wages was to cut the super-normal profits they were making (Card and Krueger, 1995). Yet such explanations are profoundly functionalist in character, assuming that an answer must lie in rational models: if employers pay above 'market rates' there must be a functional explanation. Rather than asking how employers in fact behave, they are constrained by extant economic models. Monopsony seems to fit the logic of these models, and because it is thus consistent the assumption seems to be that an explanation has been provided. But it has not. Independent evidence for efficiency wage behaviour or monopsony is not provided, and in the latter case at least it seems unlikely to be available: minimum wages cover low-wage and competitive industries, and it is not clear how such competitive firms have monopsony power. The explanation was simply the most plausible within economic theory, but that does not necessarily make it a good one.

As Rubery (1997) has argued, existing models of wage setting tend to assume that the process is orderly and rational, whereas in fact it is often chaotic and unplanned. An illustration is the study of small firms in competitive markets that were affected by the arrival of the UK National Minimum Wage (Gilman et al., 2002). Given that they were in competitive sectors, and also that institutionalized pay setting through collective bargaining was absent, these firms might be expected to be paying 'market rates'. Yet they do not have formal pay-setting arrangements such as structures that specify rates for a job. This absence of formal institutions means that pay is set by

rules of thumb and broad notions of fairness. It is this elasticity, and not monopsony, that explains the ability to absorb higher wage costs. Second, however, behaviour is institutionalized: many firms operate in labour markets circumscribed by ethnic exclusion, and managers and workers develop expectations within such specific contexts as to what is fair in the light of knowledge of other local firms and the state of the trade. Pay is not immune to market forces, but these forces are interpreted through structures that limit knowledge of the wider economy and that shape norms of fairness. Third, within the shared context of competitive markets, the elasticity of pay structures leaves space for choice and idiosyncrasy. Firms in the same market and geographically close to each other were found paying notably different wages. Some of these differences could be traced back to differences of product market context, so that a restaurant offering a 'quality' product might pay more than one in a more basic niche. Yet just how much more it needed to pay depended on the preferences of the manager, the history of the firm, and the degree of active bargaining by workers. Other studies (Cox, 2005; Holliday, 1995; Moule, 1998; Ram, 1994) have shown that workers engage in tacit bargaining over pay, hours, and work load, drawing on their labour market power and their own past experience. For example, Holliday (1995) makes a distinction between 'core' and 'transient' workers. 'Core' workers were adaptable, loyal, and able to 'fit in' with the prevailing pattern of social relations in the firm. These workers were regarded as 'one of the family', and exerted a degree of influence over work loads, promotion and even who was recruited. 'Transient' workers lacked this leeway, and rarely stayed with the firm for any length of time.

This kind of evidence helps us resolve puzzles in contemporary accounts. The standard view of legislation on such things as minimum wages is that this legislation has determinate effects or at least tendencies. The most commonly expected effect is that wages will rise. Other employment legislation, for example that which controls the right to dismiss, is expected to limit managerial decision-making freedom. A less commonly articulated argument is that there may be a stimulus to economic efficiency, because increased costs will 'shock' firms into improving labour utilization. Yet evidence suggests that effects are often weak (Arrowsmith et al., 2003; Edwards et al., 2004). Part of the explanation is wholly consistent with conventional views: legislation is often modest in its aims, and thus any effects are small. But how then do we explain situations where there was likely to be an effect, for example firms that have to raise their wages? The answer lies in the context of many such firms, notably the fact that pay structures are far less fixed and clear-cut than usual models of pay setting expect. External influences are thus moderated. The effects of legislation are also not necessarily direct. For example, it was found that care homes were affected by the national minimum wage even though many paid above it and were little affected by it directly. But they were constrained in the prices that they could charge and they found that the advent of the minimum made other jobs more attractive. They thus found staff recruitment and retention difficult. Similarly, it was possible to identify those conditions under which the efficiency-encouraging aspects of legislation were activated.

Developing causal accounts

Institutional analysis has thus offered a rich list of factors that might explain apparent anomalies. The forces of history, custom, ignorance, and chance may all help to explain patterns that conventional views cannot. It has been possible to generate intellectually coherent explanations, an achievement that should not be minimized when the limited amount of research in this vein, as compared to the massive

literature of labour economics, is taken into account. The difficulty and expense of generating relevant data through intensive methods also needs to be recognized.

But it has proved very hard to turn such insights into a clear set of propositions. What we would need is a statement about, say, different types of custom and how it is expected to influence pay, rather than explanations that are contingent on circumstance. To develop a research programme here one might need to develop a theoretical statement about different sorts of custom. Some customs, for example, have been reasonably strongly embedded in certain occupations while others are much weaker rules of thumb. One could then try to explain how different customs work when as many other factors as possible are held constant. Causal explanation might then be developed in a less inductive way than has hitherto been the case.

A note on method

In these two areas, in short, IR can demonstrate a research programme. But it has been inductive, and it has not lent itself to more formal and rigorous approaches that might speak to more conventional modes of analysis. The inductive approach reflects the established style of inquiry of old institutionalism: very detailed investigation of concrete cases. As Kaufman (2004b) shows, this approach lost influence in the US from the 1950s in favour of more formal model-building. It survived in the UK, for reasons including the relatively late institutionalization of the field but also the fact that concrete 'labour problems' at company and workplace level continued to suggest the relevance of detailed case study methods (see Edwards, 1995 and 2005a). The approach continues to thrive, but as argued in section 5 below it needs further development. To establish the content of this development, attention now turns to a key area in which IR research has developed important, but rather implicit, theoretical insights: the debate on UK productivity.

4 The debate on UK productivity

Tony Lawson (1997) takes as one of his central examples of realist analysis the case of Britain's relative productivity problem. This is an excellent choice, for the issue has continued to shape academic and public debate (e.g. ESRC, 2004). Yet it will be argued that his analysis misses the opportunity of pressing forward a realist explanation, and in some respects it retreats from such explanation. If realist analysis is to gain any hold then it must be able to show, in terms comprehensible by conventional approaches, how it advances understanding. Going further than Lawson can help in this endeavour. It is notable that conventional approaches continue to pay scant attention to CR, the just mentioned ESRC publication, which is a compilation of leading empirical studies, being a case in point. If a conventional economist were to engage with Lawson's analysis what would she find?

The conclusion in 1997 is much as it was when the analysis was first presented (Kilpatrick and Lawson, 1980): there is something in the character of trade unions in Britain that has retarded productivity. The explanation

focuses on a set of structures that empower workers . . . to exercise a significant effect on all manner of outcomes. The exercise of this power can be, and usually is, situationally quite rational and comprehensible, and is essentially defensive strength (Lawson, 1997: 257).

Conventional economics would have little trouble with this view. As the ESRC (2004: 10) has it, 'from about 1980, productivity began to rise in the UK, especially in manufacturing as large numbers of jobs were shed and the power of trade unions,

which sometimes militated against innovation, was reduced'. So a first issue is that it might be concluded that realism and conventional views are different in rhetoric but that their empirical claims are similar.² Such inferences need to be avoided.

Some points stressed by Lawson are valid and important. Worker resistance, to the extent that it occurred, was both rational and defensive. Workers' powers grew up at the level of the shop floor, and they were based in the specifics of the production process. This potentially gave workers leverage over the details of the process. But this leverage was not articulated with the powers of other workers, either within the politics of production or in relation to national politics (Fox, 1985). Faced, moreover, with managements that often insisted on their own right to manage, workers responded rationally by playing the same game, of seeking advantage in shop floor bargaining when conditions were favourable.

Yet this argument says that workers should not be blamed for doing what they did, while still accepting, indeed stressing, that they did in fact do it. A substantial amount of IR research has criticized both Kilpatrick and Lawson and the conventional view (Nichols, 1986; Nolan and Marginson, 1990; Nolan and O'Donnell, 2003). Some key points are the following.

- Even at the height of their power, trade unions organized no more than half the UK working population. Even more to the point, the kind of trade unionism associated with poor productivity (namely, a strongly organized group of workers using shop floor power to resist management) was in a minority. Its influence on productivity elsewhere is not at all clear.
- Such unionism flourished during the 1960s and 1970s, yet poor productivity preceded and followed this period. Most notably, a phase of union weakness since the 1980s has not seen productivity leap to the levels of other countries.
- Unions in many sectors where innovation was weak, such as steel and the railways, co-operated in changed work organization, and more generally there was welcome of, and not hostility towards, technological change (Hyman and Elger, 1981; Daniel, 1987). More recent research, exploring the links between new human resource management practices and trade unions, finds a positive, not a negative, association (Cully et al., 1999).
- The pattern of relative productivity does not correlate with union strength. The sectors accounting for most of the productivity gap with the US are wholesale and retail trade and financial intermediation (ESRC, 2004: 8), neither of which is a bastion of unionism.
- Periods of marked trade union defeat did not lead to productivity growth, a clear example being the coal mining industry during the 1920s and 1930s (Fine and Harris, 1985).

So how do we develop a more complex picture than the conventional story of trade union 'constraints' on management? In respect of unions themselves, it is the case that British unions had a more 'restrictivist' character than their counterparts in many other countries (Fox, 1985). This term refers to a focus on the specifics of work organization at shop floor level and a tendency to defend traditional, informal, work practices together with the customary rules regulating those practices.

² There is an interesting parallel here in IR. Hugh Clegg (1975) argued that Marxist critiques of conventional approaches were long on rhetoric but offered no new empirical analysis. There are in fact differences of both questions posed and answers given, as argued elsewhere in relation to workplace industrial relations (Edwards and Scullion, 1982: 277-84).

But we need to consider what was the causal power of this characteristic. First, and crucially, it was not a one-dimensional resistance to all managerial goals. Hence Lupton (1963) discovered, through ethnographic inquiry, a set of practices that workers called 'the fiddle'. Some of these certainly entailed resistance, for example bargaining with rate-fixers to achieve favourable piece rate prices. But others were productively neutral while yet others, notably efforts to find ways of doing jobs more quickly, directly contributed to production. More generally, workers were as likely to complain about managerial failures to introduce new technology as they were to express commitment to custom and practice.

Second, restrictivism had many variants. It is usually equated with work group custom and practice that was expressed and sustained by workplace trade union organizations. Such a situation existed in, for example, parts of the car industry. Yet, in other parts of the same industry, union organization was much less solidly entrenched, and the extent of restrictive practices was distinctly limited (Edwards and Scullion, 1982). Lupton's cases reflect situations where the trade union role in organizing workers' workplace practices seems to have been even more limited. Finally, there are many situations where any organized restrictivism was virtually absent. Consequences of these different patterns of relations for workplace practice have been outlined (Edwards, 1988). We can, in short, refine the conceptualization of a phenomenon initially labelled as restrictivism.

Third, nor was restrictivism a fixed, unchanging characteristic. During the Second World War, joint production committees emerged in the engineering industry, with the goal of productivity improvement through a shared commitment by management and workers (Croucher, 1982). These committees disappeared after the war. It would seem that it was largely managers rather than workers who wished to return to business as usual, spurred by a fear of having to share control of the workplace. This tendency continued later. The classic IR study of productivity bargaining during the 1960s was Flanders's (1964) study of the Fawley oil refinery. It underlined the central managerial role of building trust, and the structural impediments to their doing so.

Lawson acknowledges other influences on productivity, such as weaknesses in the education and training system and managerial complacency. Yet he sees these, in conventional terms, as additional to his favoured explanation. A stronger argument, and surely one more in line with CR, is to say that these factors in combination created a certain approach to shop floor relations and that workers' causal powers had their effects only because of these factors, and indeed even came into existence at all because of them.

Such an explanation helps avoid a key problem in comparative study. If we say that Feature A of Country P leads to Outcome X, how do we deal with the fact that Feature A in Country Q does not lead to X? The answer is that the feature operates only in the context of other factors. Many features of UK labour relations were shared by the USA, including an emphasis on settling issues at the point of production and a strong craft tradition. But these features had different effects because the context was different. As is well-known, the US context of large firms and mass markets encouraged firms to behave in different ways from their UK counterparts (Elbaum and Lazonick, 1986). Indeed, to the extent that the 'same' industry was configured differently, they were in important respects not the same at all.

There are various ways in which such a view can be expressed, for example:

- A leads to X where other features B and C are also present, but not otherwise; B and C may be causally prior to A, or exist alongside it.
- The effect of A on X is mediated by D and E, which operate causally as intervening variables.
- F and G create conditions which tend to lead to X, but they also encourage the development of A, whose existence makes X more likely.

An example of the first view would be an argument that shop floor restrictiveness (A) tended to lead to poor productivity (X) but only where managerial complacency (B) was also present. Its difficulty is that it does not explain the origins of A, and nor does it offer any historical (diachronic in Jacoby's terms) analysis of the interaction between different factors, including any feedback effects from X to the prior conditions.

The explanation suggested here is closest to the third example, but only in the following sense. The focus is the set of cases where restrictiveness can be identified, and the question concerns its role in the performance of the relevant firms. How far any of this contributed to productivity performance at the level of the economy is a separate question. Conditions such as F and G might include a structure of firms in which managerial re-organization was limited. A crucial historical episode was the celebrated engineering dispute of 1897 (not discussed by Kilpatrick and Lawson), in which managements won a significant victory but did not use this to root out shop floor customs or establish a more rationalized work organization on the lines of that in the US (Zeitlin, 1990). This managerial approach encouraged a style that tolerated workers' shop floor controls when these seemed either innocuous or too troublesome to address, together with militant attacks when conditions suited. Not surprisingly, workers distrusted managers, and informal job controls re-emerged. When, later, issues of international competitiveness grew pressing, managerial solutions tended to emphasize cost-cutting rather than longer term innovation. And such solutions were deeply distrusted by workers. Hence restrictiveness was created and reinforced by other factors, but it played some part in the troubles of certain celebrated sectors such as car manufacture.

A good deal of research has addressed the organizational conditions allowing a pattern of shop floor practice to emerge. As Kilpatrick and Lawson (1980) note, these conditions include the small size of UK firms and their focus on specialized, rather than mass, markets; the incentive to rationalize was thus weak. It is also well-established that the multi-divisional form appeared later in the UK than in the US. Such a structure militated against the early development of professional personnel managers with a remit to establish formal systems of workplace governance. A reflection of this style of management was the relatively late development of IR as an academic field, even though the founders of the subject, the Webbs, were British (Kaufman, 2004b).

This analysis is consistent with much of what Lawson says, notably in relation to the situationally rational character of worker behaviour and the fact that this behaviour was the product of social conditions rather than a simple constraint on managers. But it also suggests several features that could be made more explicit in his account. First, restrictiveness was only one part of a complex set of features of workplace organization. Second, it was activated and kept alive by managerial policies that emphasized short-term battles over longer-term re-organization. Third, how far it is the hidden secret of productivity performance at the level of the whole economy remains questionable.

5 Conclusions: developing comparative analysis

IR offers some concrete examples of ways in which an approach akin to CR has evolved. It may be that research is now open to the multi-level approach favoured by CR. Thompson (2003: 372) notes the development of new approaches using 'multi-level case studies' to address contemporary developments. Watson (2004: 454) similarly underlines the need to link a micro political study of events within firms to the 'political economy of employment relationships'. The following remarks suggest how we can develop such a perspective.

There is evidence of a progressive research programme, in the terms used by Lakatos (1978) and as discussed elsewhere in relation to aspects of IR not addressed here (Edwards, 1993). 'Progressive' means a developing theme of research that builds on what has gone before, to improve conceptualization of the phenomenon in question and to advance explanation of its causes and consequences. The study of team work would be an example. We can also argue that empirical IR has had, in institutionalist analysis, an approach and set of concepts which can frame empirical inquiry and act as a link to more abstract issues addressed in CR's contribution to the philosophy of social science.

Yet research programmes have often developed haphazardly. Though it is possible to identify clear progress, there are also limitations. Studies focus on particular issues and may thus not be directly comparable with each others. At one level, it is simply an issue of whether or not results are reported, but there are also deeper issues of the selection of specific issues to study and of research sites. We often lack the information to make systematic statements about certain features (such as types of teams) and their conditions and effects.

It is also the case that IR has rarely made explicit its institutionalist analytical base. As noted above, old institutionalism left little legacy. Kaufman's (2004b) analysis of the origins and development of the field makes clear that several factors militated against the creation of a distinct paradigm. These included an interest in addressing practical real-world issues, distrust of frameworks from economics or sociology, which were felt to lack empirical realism, and a preference for inductive methods.

IR might then be said to have some unrealized potential (or even, causal power). One way of building on it is through debate between empirical researchers, institutional theorists, and experts in CR. But such engagement also needs to be taken further. It has been stressed above that explanation has been limited by the rarity of comparative studies that allow the researcher to specify the causal powers of a phenomenon in one setting through careful comparison with other settings. This is far from easy. Not only may suitable comparisons not exist, or be hard to access. There are two more fundamental issues. First, it may not be possible to identify likely candidates for study. Causal mechanisms can be deeply embedded in social arrangements, and finding a site for comparative study without already having detailed knowledge may be very hard. Second, causal influences also come in 'bundles' that have to be treated as wholes. All that said, however, past experience does show how cumulative information can be built up. It may be possible in the future for researchers to share existing knowledge and then devise a research programme with comparative questions clearly in view.

It has been suggested elsewhere that Qualitative Comparative Analysis (QCA) may have some value here, with some uses in an IR context being cited (Edwards, 2005b).

The main proponent of the approach explains it through an example, which happens to be an IR one (Ragin, 1987). Suppose that an analyst wants to know whether strikes are successful or not. Three possible conditions are identified: a booming product market, labelled A; the threat of sympathy strikes, B; and a large strike fund, C. A 'truth table' is assembled showing the sixteen possible combinations of A, B and C and whether or not a strike succeeded. It can be shown whether, for example, success depends on both A and B being present but not C, or whether C alone is sufficient. This way of thinking encourages the analyst to identify logical possibilities and then go through the evidence systematically to see what combination of factors is necessary and sufficient for a given outcome. Some examples of the constructive use of QCA in workplace analysis can be identified (Roscigno and Hodson, 2004).

QCA is far from being a ready-made solution. Weaknesses include its need to simplify complex patterns into yes/no dichotomies and the difficulty of handling more than a few variables at any one time. There is also a tendency to determinism, with a condition always being associated with a given outcome. How far advocates of CR are sympathetic to it would also be a useful theme to debate. The point here is simply that it may help to think about the bundling of causal powers and about how to develop a programme of research. That is, QCA may encourage researchers to think through why certain results seem to occur in certain conditions. It may be more of a preliminary tool or means of posing new questions than an analytical device that is used directly in an inquiry. For example, the work of Mars (1982) was mentioned above. It is notable because its origin is very rich ethnographic accounts of workplace fiddles; yet it is able to provide a classificatory framework based on a simple two-by-two matrix and then to offer a set of five causal influences. Anyone developing this approach could seek out apparent anomalies or mixed cases in terms of the framework, or address situations where the causal factors seem not to operate as predicted, or both. Similarly, in relation to one of the examples given above, teamwork, if we find that certain conditions seem to lead to teams having certain effects, we may be in a position to seek cases where some conditions vary, and then to explore effects. For example, do teams thrive in apparently unhelpful conditions or fail under more propitious conditions, and if so why?

This is not the place to lay out a detailed substantive agenda. The objective has been to recover some underlying theoretical and methodological strengths in IR, not to propose specific ways in which they can be deployed. But it is not hard to see where they might be of use. In addition to the areas discussed above, IR research as recently made substantial progress in areas as different as the work-life balance, partnership, and the effects of globalization. In all of these areas, systematic comparative inquiry could be an important way forward, for example in identifying types of partnership and the conditions leading to various outcomes of the relevant practices.

Such inquiry is not easy. As mentioned above, it is rarely the case that the characteristics of potential research sites are known in advance. In international comparative research, the problem is attenuated to a degree, in that the basic characteristics of a national IR system can be readily established. But, even here, the more subtle features of how a system really works – Stinchcombe's 'guts of the causal processes of institutional influence' – may be hard to define. And if we were, say, comparing teamwork in the UK and Germany, how would we establish that any observed features were truly British or German? Yet these questions are not unanswerable, as Locke and Thelen (1995) showed. The need now is to try to be more

systematic in building on work that has already been done, in particular in relation to causal explanation, and not to reinvent wheels.

In short, contextualized comparisons can offer causal explanation, and can thus contribute to the 'scientific' goals of the field. This is where there is a link to more quantitative and hypothesis-driven stream of research. Following Kaufman, the issue is that this stream became defined, particularly in the US, as the only way to build scientific analysis. More institutionally oriented approaches can also do so, but they need to pursue their links with disciplines such as sociology if they are to establish the 'scientific' validity of what they do. Consider an example. The links between human resource management and organizational performance have been a central recent concern, with much research being firmly in the quantitative mould. A major review has identified substantial issues of theory and evidence, and it calls for 'big science' comprising large-scale surveys and the collection of detailed data using sophisticated instruments (Wall and Wood, 2005). Much of the argument is persuasive, but no space is left for contextualized inquiry. Yet the relationships between HRM and performance would seem, from the evidence reviewed, to be complex and variable. Detailed inquiry into the meanings of HRM practices and how they work surely remains a crucial complement to big science. It can, indeed, be part of big science, rather than an adjunct. If it can offer rich and causally plausible accounts, it can say at least as much about the relevant questions as can quantitative studies. Indeed, on Wall and Wood's own analysis, a mass of quantitative studies offers at best patchy evidence, and new methods are needed. Such methods embrace contextualized analysis.

Some clear developments, together with prospects for future advance, can be discerned in such analysis. It remains the case, however, that IR is a far from unified field and that there remain substantial challenges to its intellectual development. Whether or not its past doubts about theoretical engagement have been overcome is highly uncertain. Yet pursuing such engagement may be one route to restore its institutionalist tradition and to show that it has insights to offer to mainstream social science. It may also, as a result, have stronger things to say about 'practice', an issue that will be taken up elsewhere.

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