

ISSN 1466-1535

ISSUES OF METHOD IN RESEARCH ON EMPLOYMENT RELATIONS

SKOPE Research Paper No. 9, Autumn 2000

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Oxford and Warwick Universities**

Editor's Foreword

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The following is an abstract of Research Paper No. 9.

Abstract: This paper considers the potential and limitations of the case, survey and aggregate statistical approaches to empirical research in the field of employment relations. The use of the various methods in mapping the state of employment relations, in 'description', is first considered. As a prelude to an assessment of the leverage of these methods in the causal exploration central to social science, the paper then turns to consider the critical issue of the place of structure and of agency in interpretative frameworks. In this light, the contributions of the case and variable oriented approaches to explanation, or 'analysis', are assessed. Particular attention is given to the deficiencies of the 'hard science' often confidently presented as the proper vehicle of social research. Finally, a synthetic approach to research in the area is proposed, and the vital role of theoretical pluralism in advancing understanding is stressed.

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Acknowledgements

Numerous discussions over a number of years have stimulated my consideration of research method. I am particularly grateful to Tony Elger, Roberto Franzosi and Theo Nichols for sharing their reflections in conversation and on paper. The comments of the full-time SKOPE staff, and particularly of Jonathan Payne, at an internal seminar, were also very helpful. Thanks are also due to Ken Mayhew, Director of SKOPE, and to the ESRC, which funds the research centre, for helping to create the space vital for the exploration of these issues.

Contents

| | |
|--|-----------|
| Issues of method in research on employment relations..... | 1 |
| | |
| Established methods for description – distinctive potentials and practical limits.. | 5 |
| Survey or questionnaire based research..... | 5 |
| Case study research..... | 9 |
| The use of official statistics..... | 10 |
| Forms of interpretation..... | 13 |
| Distinctions between structure and agency | 13 |
| Notions of structure..... | 14 |
| Is the structure-agency dual useful?..... | 17 |
| Agency and understanding | 21 |
| The experimental logic of the case study..... | 23 |
| Assessing hard social scientific explanation..... | 27 |
| Hypothesis testing..... | 28 |
| Empiricism as a model..... | 29 |
| How hard is hard data? – from statistics to data. | 30 |
| What when there are no statistics? | 32 |
| From data to draws and series | 34 |
| Association without causation..... | 35 |
| Confronting causality in variable oriented work..... | 38 |
| Non-scientist social scientific enquiry | 45 |
| The value of efforts at hypothesis testing..... | 45 |
| Developing causal explanation - a multi-level strategy..... | 47 |
| Conclusion: multiple methods plus knowledge of alternative perspectives..... | 54 |

The pursuit of substantive issues in social research, of the character of some feature of social reality or of the relations between these phenomena of the social world, inevitably raises issues of research method. Whilst contemplation of such issues is considered a distraction or an indulgence by many researchers, in all those disciplines which deal with the field of employment relations, these issues seem nonetheless critical to the development of our understanding of social reality. If the objective of social science is to develop characterisations of social reality, and to advance understanding of it by establishing causal explanation, the manner in which we seek to do this cannot be considered other than vital. The centrality of the considerations of method to popular notions of science is well justified. Whilst reflections on research method can bring little in the way of mechanical prescription for the pursuit of social scientific enquiry, contemplation of these issues is still of value, in part precisely because the inadequacy of simple research formulae is inevitably highlighted.

With specific substantive research questions, or classes of questions, in mind, issues of research method can be pursued in a grounded fashion, with the danger of drifting into utter abstraction attenuated. The present argument draws on a great variety of studies bearing on workplace employment relations, studies centring on a variety of objects of study, with a variety of different emphases and contributing to a number of different research traditions. These studies are chosen for the light they can shed on the epistemological issues of how we might ask better questions in the analysis of workplace employment relations and how we might better consider the answers which existing studies furnish. The paper is thus intended to offer something to those who wonder how to better go about a dialogue between the ideas of theory and the ‘facts’ of evidence to develop understanding of the social reality of the field (see e.g. Ragin, 1987; Hyman, 1994a; Franzosi, 1995; 2000).

Description is a prerequisite for causal analysis. The paper begins with an assessment of the distinctive descriptive potentials and limits of the research methods typically employed in work on employment relations. The role of surveys, case studies, and official statistics in contributing to our knowledge of the ‘what?’ in the social realm is thus considered. The second section of the paper deals with key forms of interpretation

in the analysis of workplace employment relations. This discussion of the forms of explanation, of theory, common in the field, focuses on the critical issue of the manner in which human actors are conceived and located.

This treatment of forms of interpretation of employment relations then informs the succeeding sections, concerning causal inference. The third section of the paper deals briefly with the potential of case based analysis in establishing patterns of causation. The fourth section features a more extensive assessment of the potential of the 'hard', quantitative, model of social scientific enquiry to deliver the causal understanding it promises in the field of employment relations. Particular attention is given this approach to social inquiry as it is accorded a privileged status by many researchers in the area. The fifth section of the paper argues that this notion of science, whilst valuable in stimulating quantitative research, simultaneously acts as an impediment to the advancement of understanding. The concluding section of the paper draws together the various strands, arguing that since numbers are not the solution, but nor are they the problem, a multi-faceted approach to the understanding of workplace employment relations is warranted. Only an approach which stresses the pursuit of substantive issues, rather than insisting on particular research methods or particular theoretical traditions, can allow proper consideration of causal possibilities, and thus push back the metaphysical.

Established methods for description – distinctive potentials and practical limits

Three distinct research methods have dominated research on employment relations for many years. Survey research, case study and documentary work employing official statistics tend to be performed by researchers inhabiting fairly distinct communities, and communication between them can be problematic. Each has distinct potentials and limits in the establishment of knowledge of the social world.

Survey or questionnaire based research

Large-scale surveys have been employed extensively to investigate patterns of employment relations within national borders, with the British Workplace Industrial Relations Surveys (WIRS), and now the Workplace Employment Relation Survey

(WERS), prominent examples. Smaller scale applications of survey work, investigating the operations of multi-nationals in particular, are becoming increasingly common (e.g. Kochan et al, 1997).

The survey technique, particularly when executed on a large scale, promises an impression of facets of the employment relation which is representative of the situation in a wider class of workplaces or companies. The purported strength of the method, emphasised repeatedly by its proponents, is the generalisability of the research findings it generates (e.g. Millward and Hawes, 1995). Carefully employed, the technique can indeed afford major advances in knowledge of aspects of the broad state of employment relations, as did WIRS 3 (1990) in evidencing the general decline in the role of collective bargaining in Britain (Millward and Hawes, 1995, 71).

The principal problem of this approach is that responses which may be interpreted as indicators of the typical substance of employment relations may reflect rather the rhetoric or reference systems of respondents (Morris and Wood, 1991; Berggren, 1994, 188). This practical difficulty of the technique underlies the widely shared perception that surveys may be used most readily to identify the existence or absence of formal procedures and structures of decision making. It is now generally accepted that the WIRS1&2 data, showing continuity in the proportion of establishments in which personnel specialists were employed and the proportion overseen by a personnel function represented at board level, which Batstone (1988, 191) cites as indicative of continuity in the influence and status of IR within managerial hierarchies, was misleading. Such workplace infrastructures, along with others such as union representation or works councils, may in practice be by-passed, ritualised, or hollowed out (e.g. Morris and Wood, 1991; Pollert, 1996; Bacon, 1997; Hyman, 1997). Similarly, the labels pinned on practices may veil a very limited substantive content, as Sisson (1993) recognises in his careful commentary on the relatively greater preponderance of fragments of HRM in unionised workplaces apparent from WIRS3 (of 1990). On the other hand, informal agreements on the shopfloor may result in practices which may bely an immediate survey impression of unrestricted managerial prerogative, and indeed contradict not only the situation officially acknowledged but also statutory regulations, as Martin Wright (1996) showed has sometimes been the case with respect to the closed shop in the UK. Our

understanding of workplace practices, and of the discourse surrounding them, is obviously insufficient to justify an assumption that survey findings are subject only to some random error around the substance of workplace employment relations, implying as this would that the typical responses emerging from large samples should be treated as representative indicators of the content of employment relations.

The construct validity of the organising concepts is vital in survey work, with a survey being successful to the extent that it elicits responses which constitute meaningful indicators of phenomena which are of real significance (e.g. Yin, 1994, 34-5). Inasmuch as the aim of a survey employed within IR is to enhance appreciation of social reality on the ground, the precision of the questioning and the relation of the tightly drawn questions to the substance of working life are critical (May, 1997, 98-9; Berggren, 1994, 187-8; 264 n2). Respondents interpretation of the discourse employed by the researcher is obviously critical to their response, as Cicourel (1964) stresses (see also Baldamus, 1978). Research conducted on the significance of phrasing by the Swedish Central Statistical Office (SCB) has demonstrated the paucity of indicators derived from vague questioning, even where the object of the question is itself rather simple. Responses on a Likert scale to the question 'To what extent is noise a problem in your workplace?' were found to correspond very poorly to noise levels as gauged by instruments. Moreover, there were severe problems of reliability in the figures derived, with the same individual prone to give widely differing responses when asked the question on different occasions. More precise questioning, of the form 'Is it so noisy that you cannot conduct a conversation in a normal tone when the machines are on? If so, during what proportion of the day is this the case?: at most one-fourth, at most half?' was shown to elicit a more readily comprehensible response (Berggren, 1994, 187-188; 264n2).

These findings demonstrate the significance of a common understanding of the question posed for the results obtained. Research employing vague questioning in effect abdicates all responsibility for the meaning systems applied, passing this to the respondent. It is then left to the respondent to contemplate the object to which the question refers; the labelling conventions employed. The response is thus conditioned by the respondent's experience and aspirations, with the result thus depending critically on what she takes as given and what she regards as possible. Responses are thus framed by

the reference group of the respondent and by the nature of public debate. Survey questioning should recognise the implications of differing reference systems and labelling conventions amongst respondents, attempting to reduce the impression of what respondents take for granted in their work experience on the responses recorded.

In this context, the play allowed respondents by any looseness or ambiguity in questioning seems a particular problem where the objective is to track changes in employment relations over time, even where the same questions are asked in each round of the investigation and the respondents are those directly involved (Berggren, 1994, 187). It would seem likely that the role allowed differing reference systems, labelling conventions and rhetorics by imprecision in questioning would be likely to generate, if anything, a still greater problem in international comparative applications of the survey method, whether they are historical or not.

Whilst there are real dangers in survey questioning which is vague, there are dangers too of survey researchers seeking precision in their questioning through the use of technical terms, in ignorance of the manner in which this vocabulary may be interpreted or regarded by the respondents whose impressions are sought at arms length. This is one avenue by which the pre-conceptions of the researcher are manifested in the survey, one aspect of the critical issue of what is highlighted and what played down in investigation (May, 1997). This matter of variation in the labelling of workplace practice may cause respondents confusion, with uncertain consequences for their responses, but it may also intimidate the respondent, deepening latent feelings of alienation from the project represented by the survey questionnaire and having deleterious consequences for their effort to respond faithfully. Thus, precision in questioning is best pursued with sensitivity to the conceptual framework of the respondents, a task which may be very difficult at arms length and in ignorance of the nature of the discourses at the workplaces under study.

Moreover, since the requirement that the respondent should be able to comment knowledgeably on the issues pursued in the survey is fundamental to the technique (May, 1997, 99), the immediacy of their experience of the aspect of employment relations under study is vital. The identity of the respondent is thus of potential relevance, with the responses of managers detached from the everyday reality of the working life of lower

grade employees likely to be shaded more by established management discourse and rather less by daily practice on the shopfloor or in the office (Geary, 1996; Rees, 1996). The dangers of eliciting self-serving responses which reflect not so much the substance of employment relations as the self-perception and even identity of actors, and thus perhaps the ideology dominant in the milieu in which they operate, are obvious. Such considerations are of course of relevance when managers are questioned about company practices (e.g. Bacon, 1997).

In summary, the validity of a survey application is a critical consideration and far from unproblematic in the context of such social enquiry at arms length. The dangers of statistical artefacts are legion. The central claim of survey work to be representative is well founded, but the critical issue is what it is that survey responses represent.

Case study research

The in-depth case study, employing semi- or even un-structured interview techniques at workplace or company level, and perhaps drawing on the direct observation of work, offers a method for the detailed examinations of workplace employment relations which is more amenable to an assessment of the daily experience of employment relations. It presents the possibility of a flexible exploration of the instance, reducing the distortion implicit in researchers pre-conceptions through its sensitivity to the discourse employed, and the issues as problematised, by the actors. It thus presents possibilities of pursuing objects of study it is otherwise extremely difficult to explore, in large part because of the ignorance of the researcher about the considerations which are critical in the context and about the discourse which is common in it. This allows the case researcher to surmount the survey researchers' difficulties (which are often unacknowledged due to social distance) in enumerating at arms length the social phenomena which are the object of study. In these ways the method facilitates the discovery of the hidden, unappreciated, unofficial or less readily quantified in the particular instance which is the subject of the case study (Edwards, 1994; Rees, 1996; Franzosi, 2000).

The case technique therefore allows the researcher to delve well beneath the policy statement, nurturing a deeper, more rounded appreciation of the nature of work relations by facilitating an intensive consideration of the operation of professed policies and practices (Morris and Wood, 1991; Bélanger et al, 1994; Pollert, 1996; Bacon, 1997). The technique is likely to be particularly fruitful in furthering knowledge of the substance of working life on the floor if (lower grade) employees are interviewed (Geary, 1996; Rees, 1996; Bacon, 1997). It can also be used to explore in detail the disputed; for example the views of various actors of the distinctiveness of the employee as opposed to managerial groups (see e.g. Kelly, 1998). Case work thus promises a more subtle and nuanced representation of the ‘what’ in social research, to our knowledge of the state of social reality.

The potential of case work for detailed exploration thus allows it to make an indirect contribution to knowledge. In their exploration of the implementation of professed policy and of the meaning in practice of local institutional arrangements, case histories are of use in interpreting the representation of workplace employment relations offered by existing survey work, and thus in honing the construct validity of survey questioning (Millward and Hawes, 1995, 72; Bacon, 1997, 3). However, the typicality of the situation described by a company case study must necessarily be in doubt. To some extent, of course, this problem can be mitigated by the working through of a number of detailed studies.

The use of official statistics

Both survey and case methods have, then, alongside their strengths, certain specific difficulties in mapping and tracking the comparative natures of the workplace IR under different national systems. Moreover, to some extent the methods share the difficulties which arise from the reliance of researchers on the goodwill of employers in order to achieve access to their workforces. Purcell (1994, 213), for example, emphasises the temptation of employers to circumscribe the agenda of detailed employee surveys, even once they have consented to cooperate in principle with research. Furthermore, both case and survey methods are costly.

In the context of these difficulties, summary measures of facets of workplace employment relations which have as their unit of analysis the experience in the entire economy or society may play an important role. Indeed aggregated comparative analysis which focuses on the nature of employment relations in the workplace seems particularly pertinent at a time when public discussion of broad labour market conditions in Europe and more broadly increasingly focuses on aggregate measures of unemployment to the exclusion of all other indicators of the well-being of the labour force as a totality.

The use of official statistics, standardised to varying degrees, is well established in industrial relations, with extensive tables featuring in, for example, Ferner and Hyman (1992) and Bamber and Lansbury (1993). However, even leaving aside for a moment problems of comparability, which though often substantial are sometimes side-stepped, the quantitative data employed tend to constitute rather remote indicators of the nature of workplace employment relations. The data most commonly relate to industrial production (GDP, productivity, the capital-labour ratio), basic labour force aggregates (employment, unemployment), the structure of employment (by sector/ establishment size/ company size/ gender /status), wages or earnings and differentials thereof, to the institutional characteristics of collective bargaining and employee representation (union membership, confederal affiliation, works council election results) or to one very particular manifestation of worker dissatisfaction - strike activity. These are also the sorts of data commonly employed in labour economics (see e.g. Booth, 1995). Attention to other official statistics which relate more immediately to the nature of the experience engendered by the sale of labour power is very limited within comparative IR, with Wolfgang Streeck's (e.g. 1997) use of snapshots of wage differentials, annual hours and job tenure something of a pinnacle. Outside IR circles, the late David M. Gordon's (1996) comparative work on the extent of managerial hierarchies is a landmark in this regard.

Within IR, the most intense attention to aggregate statistics has been that afforded those on strike activity. With many commentators drawing on official strike records in comparative analysis of industrial conflict, issues of the international comparability of the available data became paramount. Edwards and Hyman (1994) note the problem caused by cross-country differences in the definition of strikes and in methods of compiling data,

whilst Shalev (1992) emphasises the changes in the definitions applied within countries over time. Shalev's (1978) detailed treatment of the problems of strike data explores the complexity of a consideration of data validity, commenting not only on the significance of the definition of strike action employed - the thresholds, scope and exclusions of the declared 'subject matter' - but on the relevance of the sources employed and the human resources committed to data collection. The considered use of a wide variety of sources by a responsible body committed at all levels to the task is vital for the reliability and completeness of the strike record, on the particular definition of strike action ostensibly employed.

Similar issues arise in the interpretation of the available official statistics pertaining to routinised workplace employment relations. The use of any official indicators requires detailed consideration of the conceptual basis and coverage of published statistics which shape the 'hard data' often exclusively emphasised in quantitative work. It is not enough to have collected some numbers - the 'metadata' underlying them should be given proper attention (see Eurostat, 1997). The considerations which arise in work with official statistics may thus be paralleled with those arising in the treatment of survey responses. Ideally, official statistics may constitute an exemplary survey of specific aspects of the employment relationship, reflecting consensus over the meaning of carefully employed terminology, a thoroughness in corroboration, and a massive coverage. The construct validity of official indicators is however critical, as of course is the execution of collation.

Moreover, there is the issue of the partiality of the representation of the employment relationship which is inherent in the selective focus of official statistics. Thus, for example, the collation of meaningful cross-national comparative data on work organisation seems impossible, whilst the collation of that on at least some aspects of working conditions is not prohibitively tricky. Finally, official statistics generally afford strictly limited opportunities to assess the unevenness of employees' experience within national borders.

Forms of interpretation

This section introduces interpretation, focusing on the critical issue of the attribution of causality. Any analysis of patterns apparent in social reality, whether taken from qualitative or quantitative investigations, involves some attribution, or at minimum suggestion, of causality, whether the implication of the discussion is acknowledged by the author or not. Such social theory may be evident in its most obvious form; as necessarily abstract causal explanation based on some comparison or counterfactual, and thus intended to be generalisable. But, outside labour economics (e.g. Booth, 1995), such explicit theorisation is rare. Nevertheless, in any contribution there is always some commentary which bears on the port of intervention by which change in the particular aspect of social reality which is the object of study may be effected. Interpretations of social phenomena and processes, in ordering muddled reality, feature narrative accounts which amount to causal explanations. In this sense, theory is indeed everywhere (e.g. Hyman, 1994a).

In this causal interpretation of patterns in social activity some distinction between the constraints on the activity of actors and the play of their own influence is common, no less so in empirically based work in the field of the employment relationship as pursued by researchers in labour economics, industrial relations and industrial sociology. Often this distinction is implied, as indeed it can be even in analysis which explicitly disowns such a distinction, such as Clegg (1976). Whilst pointedly denying the relevance of any distinction between 'structure' and 'behaviour' to the analysis of trade union activity, Clegg proceeds with a monograph premised on the shaping of various facets of industrial relations - such as union membership, strike activity and industrial democracy - by the pattern of collective bargaining, a facet supposed exogenous in some sense.

Distinctions between structure and agency

The nature of constraint-discretion distinctions of this kind thus seems a critical consideration in meta-theory, and thus in the nature of social science explanation itself. The following discussion seeks to explore the notions of structure present in labour

literature, to consider the broad nature of critiques of such notions, and finally to come to a view about the meaning, implications and heuristic value of the operationalisation of them.

Where the constraint-discretion distinction is explicitly highlighted it may be expressed in a variety of dualisms, such as structure-agency or structure-actor and material-ideological or materiality-discourse. Often, these dualisms are employed interchangeably. In certain contexts specific dualisms are used; thus the distinction base-superstructure is common in Marxist, or materialist, discussions of the operation of the capitalist social system as an entirety (see Edwards, 1986). The distinction business environment-company strategy is common in treatment of enterprise management in many traditions (e.g. Hyman, 1987; Andersen, 1997). The precise nature of the notions counterposed in the dualisms is often unclear, to the extent that further consideration of the meaning and purchase of the notions employed in the literature, or lurking beneath texts, seems worthwhile. The patterns of thought which these dualisms embody have profound implications for scholarly treatment of developments in social systems, whether they be specific national capitalisms or particular companies.

Notions of structure

Hyman (1972), Godard (1993) and Layder (1993) conceive structure as those enduring regularities and arrangements framing social action. The perils of reification inherent in this dualism are acknowledged by Hyman (1972; 1975). But the distinction made implies that what is critical is the degree of endurance of features of social relations. This view of structure as enduring social regularities suggests immediately the existence of distinct arenas of action, characterised by distinct ranges of agency, in which various groups of actors may shape what will serve as structure to other groups of agents in other arenas. It is thus that the objective structure in which the action of each agent is embedded is shaped.ⁱ

Layder (1993) suggests that it may be useful to attribute a hierarchy to the formative arenas of action, with some notion of levels of social life. The theoretical space for a consideration of proximate and remote causes, or of what Baldamus (1978) termed

'the determinants of the determinants' is thus emphasised (see Nichols, 1997). Craib (1992) notes the centrality of such a notion of 'ontological depth' to Roy Bhaskars' (e.g. 1978) critical realism. The notion of such a hierarchy of influences on social phenomena, of a deep structure, seems implicit in the political science notion of 'antecedent conditions' (e.g. Stephens, 1979; Van Evera, 1997). It is also present in a number of contributions in the field of study of industrial relations. It surfaces in the notion that there may be 'facilitating characteristics' which ease the process of introduction of innovative work practices (e.g. Edwards and Wright, 1998). It is present also in Pontusson's (1992) examination of work organisation at British Leyland and at Volvo, in his comments on the need for an assessment of the significance for job design of the external conditions in which a company operates relative to the conditions internal to the organisation. Sisson's (1993) comments on the implications for workplace employment relations of the short-termism which characterises British corporate governance structures raise similar issues.

A hierarchy of arenas of action is explicitly stressed in the seminal contribution of Korpi (1978), who distinguishes three levels of analysis – the macro (the societal distribution of power resources), the meso (the functioning and strategy of organised labour) and the micro (shopfloor activity). It is present also in the commentary of Ferner and Hyman (1992), who stress 'layers of variation' in national attributes. These layers are distinguished by their durability, with political institutions the most enduring, but the more persistent national features are not regarded as dictating developments in other layers. Such approaches suggest that the agency of distinct groups in distinct arenas may be usefully ordered into some sort of causal hierarchy involving the operation of agency at different levels of the social structure. Of course, the very notion of a structural hierarchy implies a partial subversion of it to the extent that action at deeper levels must necessarily be conditioned by the relevant actors' perceptions of attitudes and patterns of agency and activity of actors closer to the surface. But such approaches positing layers of variation suggest that the critical interventions, which it is the task of social science to trace, may be those embedded deeper in the structure.

Moreover, Layder (1993) writes of the overlaying of 'vertical' and 'horizontal' layers of social activity, stressing the location of agency in any one arena of action with

respect not only to other arenas currently active but with respect also to past activity in all arenas. Social agency should thus be located not only in space but in time. The common emphasis of scholars on historical moments at which there is an opportunity for the transformation of institutions which will then have an enduring impact is consistent with such a view of structure. The US multi-national General Motors explicitly stresses the role of 'significant emotional events' such as mass redundancy or the threat of closure in opening up possibilities of changing work organisation (MacDuffie & Pil, 1997). At the societal level, many authors stress the role of parallel conjunctures at which new institutions are shaped by the economic and political context of the time, and then take on a life of their own (e.g. Stephens, 1979; Pontusson, 1984; 1992; Fulcher, 1991; Ferner & Hyman, 1992; Visser & Hemerijck, 1997). It seems that such moments are generally precipitated by some form of societal crisis. The perception of failure may arise from a sudden deterioration of economic performance, from a realisation of undesirable unintended consequences of the path being pursued, or from some form of affront to the moral economy of subordinates which triggers a crisis of legitimacy of hitherto accepted authority. The perception of the failure of existing structures results in a partial ideological vacuum as a feeling of uncertainty and confusion takes hold. This weakening of the hold of the established patterns of thought on actors, encourages them to strike out in new directions.

Pontusson (1984; 1992) stresses both a politics of legislation and a politics of implementation in the arena of governmental policy making, with each involving conflict within the labour movement and within business and across the capital-labour divide, and argues that these various conflicts have been of differing significance in different policy contexts in Sweden. The emphasis, particularly in Pontusson's earlier work, on the role of structure, or social reality, in delimiting the range of outcomes realisable at the stage of implementation of a reform echoes Marx's suggestion that, ultimately, the purchase of a theoretical analysis can only be assessed by the fate of a practice, e.g. reform, grounded in it. Hyman (1975; 1994a) in particular has asserted the centrality of such a notion of 'praxis' to the Marxist approach. Pontusson (1984; 1992) does not thus simply assert a distinction between structure and agency, rather he implicitly founds this distinction on a notion that structure is that which is (inevitably) confronted when agency is exercised. It

is thus that the 'concrete reality' (Godard, 1993), the 'objective structure' (Delanty, 1997), enters, and is indeed defined. This view of structure as the conditions confronted by action itself suggests arenas or levels of social action or agency, with action in each arena contributing to the structure faced in other arenas. In this context, the identities or beliefs of others may be seen as an element of structure, so that, for example, the ideological resource of broader solidarity, the significance of which for labour movements is stressed by Goldthorpe (1984), could be characterised in this way.

Whilst it is common to conceive structure as the enduring regularities, it is also often viewed as the incomprehensible. Structure is seen as the complex historical interweaving of forces which it is difficult for an actor even to grasp, even without any attempt at redirection (see Hyman, 1972). This notion of structure seems quite compatible with, and perhaps underpins in part, a conception which stresses durability as a defining characteristic, as there seems no chance of forging a change in forces which one cannot even comprehend. Some authors have emphasised that an agent need not be aware of an influence for it to have implications for their behaviour (Hyman, 1972; Ramsay, 1993). In the context of the diffusion of practices within the multi-national company, Edwards et al (1996) contrast the thrust of deliberate corporate strategy with the drift of an unconscious reflex. The view that structure need not be acknowledged to work its effects contrasts with the hermeneutic view that structural phenomena which are not immediately observable, such as power, are simply fictive (see Layder, 1993).

Is the structure-agency dual useful?

Some of the social theorists who have informed recent contributions in the broad field of employment relations seem to question the relevance of the structure-agency dual, either by denying the space for agency or by disputing the existence of structure. The influential work of Louis Althusser (e.g. 1969) and Nicos Poulantzas (e.g. 1973) has been widely interpreted as amounting to a return to a deterministic and reductionist view of the operation of capitalism, in which ideas are a reflex of economic conditions (e.g. Giddens, 1982; Eagleton, 1994). These abstract contributions give particular attention to the role of the state and of state actors, as these are seen as the principal means by which

purposeful collective or social action might be imagined possible, but seem to have implications for other actors too. Althusser's (1969) notion of the individual's 'imaginary relation' with the social order has been taken to suggest that individuals constitute themselves with the coherence required to act in their specific social roles, whether actively or passively. Ideology, an unconscious product of lived experience, is then best considered not as an epi-phenomenon, but as an aspect of the material (Hirst, 1976; Eagleton, 1991), or of the objective structure of society, as it may have been by the later Marx (Eagleton, 1994). Poulantzas (1973) treatment of actors as 'bearers of modes of production' has been taken as similarly suggesting that capitalism is perpetuated through individuals' interpretation of social reality (see Giddens, 1982).

Both Althusser and Poulantzas have been characterised as 'super-determinists', interpreted as suggesting that the structures which endlessly perpetuate capitalism are still deeper than had previously been supposed, lodged in the consciousness of individuals (Jessop, 1982; Giddens, 1982). Regardless of its subsequent employment (see e.g. Edwards, 1986; Kelly, 1989), Poulantzas own notion of the 'relative autonomy' of the state seems, in as far as it is developed in his own work, severely circumscribed. It appears to amount only to the suggestion that state officials constitute a group distinct from the capitalist class who arbitrate intra-class conflicts and coordinate the action required to ensure the macroeconomic and social stability and economic infrastructure necessary for continued capitalist accumulation (Stephens, 1979; Giddens, 1982; Jessop, 1982). Aside from his discussion of Mediterranean military dictatorship, he presents no analysis of the development of specific national capitalisms (see Jessop, 1982).

In sharp contrast, post-structuralism or constructivism stresses the significance of individual perceptions and interactions, suggesting that the aim of social science should be micro-sociological analysis acknowledging the particularity of each event. Such hermeneutic approaches, such as ethno-methodology, stress the production and reproduction of rules, norms and understandings (Godard, 1993). Delanty (1997) comments on the 'under-theorisation' of agency not only in the structuralist Marxism discussed above, but in such constructivism, stressing as it does the role of culture and ideology in the perpetuation of patterns of behaviour. Here, there appear parallels with Maurice et al's (1986) famed 'societal effects' approach to comparative industrial

relations, an approach which highlights the mutual consistencies in the characteristics of systems, suggesting that these behave as a kind of organism, or, as Lane (1989) puts it, a 'syndrome'.

Such perspectives as these are plagued by difficulties in the location of agency. In general there does seem a danger that if the existence of a constraining structure is utterly denied, then 'agency', or ideology, becomes, in a sense, all structuring, allowing individuals no room for manoeuvre. This seems a particular danger to the extent that the reproduction of rules and norms may be unwitting. Authors favourably disposed to the distinction between structure and agency often allow a role for an agent in constituting and reproducing the structure she faces, in so doing emphasising their dialectical approach and deepening their response to the danger of reification. Giddens (1971) and Edwards (1986), dealing with issues of base and superstructure, stress the role of political and legal action in a simple legitimation and rendering acceptable of the existing structure, and thus in the buttressing of it. Hence legal and political superstructures may be critical in the preservation of the apparently analytically prior base through their validation of it. More generally, Paul Edwards presents materialism as an approach to the capitalist system stressing the economic base, conceived in the technicist fashion of Hirst (1976), whilst acknowledging the *conditioning* of it by the social relations and social consciousness of the superstructure (Edwards, 1986). As Giddens (1971) notes, such approaches suggest that the relation between power and values is not uni-directional.

With regard to employment relations more narrowly conceived, Turner (1998) argues that the maintenance of the substance of the German institutional framework is predicated upon the periodic renewal through the mobilisation of employees of structural arrangements prone to atrophy. The celebrated case work of Burawoy (1985) earlier stressed the significance of employees active engagement, with the 'game' of 'making out' under the piece system, in perpetuating their subordination under the relations of production, arguing that one cannot throw oneself into a game whilst questioning the rules under which it is conducted. There has thus been some acknowledgement, by prominent specialists, of the consolidation of structure through the exercise of agency in the practice of industrial relations.

The constitution of agents is explicit in the work of Skocpol (e.g. 1992) on the role of state officials in the shaping of welfare states. She emphasises the shaping of state traditions and state capacities in social policy, with implications for the nature of social knowledge pursued and comprehended. Indeed, such a process seems evidenced by the Swedish experience, with the initial reaction of the generation of labour leaders who had overseen the functioning of societal rationalisation to the grassroots discontent of the late 1960s being to tinker with existing institutions rather than pursue a new stage of reform (Fulcher, 1991). There is an obvious parallel with the shaping of strategic capacities or organisational capabilities at the level of the management of an enterprise. The distinct trajectory of Volvo with regard to its production systems perhaps provides the starkest example (Berggren, 1994). To some extent, the constitution of agents may have underpinnings in historical material conditions, but there remains room for agents to break free of established patterns of thought, a process which can be only nurtured by the pursuit of knowledge of alternatives, the centrality of which to ideology is stressed by Eagleton (1994).

As regards agents' constitution of the structures in which they operate, various authors suggest that levels of the structure may be gradually transformed through considered interventions by key actors. The so-called 'social democratic theorists' are an excellent example. Korpi (1978; 1983) and Stephens (1979) suggest that the macro structures of national capitalisms may be incrementally moulded in a way which favours labour by an accumulation of power resources which are mutually conditioning, principally through their role in reinforcing the solidarity and basis for action of the labour movement, but also in demonstrating the potential of organised labour. The suggestion is that such an incremental development of structures may be quite intentionally pursued by an enlightened actor, or series of actors. In this context, Craib's (1992) notion of structure as congealed social action seems apposite.

For Korpi (e.g. 1978) particularly, it is the belief systems of the population which are the critical constraint – faith, solidarity and social closure are critical to the immediate possibilities of reform but subject also to moulding by societal change. Interestingly, the closing comments of Pontusson (1992) on the prospects of social democracy have similar implications in this regard, although Pontusson's earlier work (1984) highlights the

dangers of teleology in the interpretation of national structural formation, stressing the relevance of crisis and the unintended consequences of initiatives with contradictory implications in the evolution of national capitalisms, and specifically questioning the foresight attributed to Swedish social democrats by some theorists. Of course, few if any scholars suggesting the possibility of an accumulation of a mutually supportive system of power resources would deny the vulnerability of such a configuration, as it has been demonstrated most strikingly in Sweden.

Several authors have stressed that the structure within which an agent operates can present possibilities as well as imposing limits – that it is in a sense enabling. Thus Layder (1993,) supports the position of Giddens (1982), expressed in structuration theory, that there is a ‘dialectic of control’ in power relations. In the field of industrial relations specifically, the emphasis of Streeck (1992) on the constraints and opportunities afforded employers by the institutional density of the (West) German system seems intended to advance this point. The significance of the sophistication of the understanding of the agents in question seems implicit in such arguments.

Agency and understanding

The notions of structure and agency do generally carry with them a presumption that there is a possibility of emancipatory knowledge. The notion of emancipatory knowledge is inherent in the Enlightenment belief in Reason, and to the modernist project of societal improvement through improved knowledge of the operation of social systems (Eagleton, 1994; Jenkins, 1995; Delanty, 1997). The notion presumes the possibility that there can be a loosening of historical associations which may be erroneously viewed as structural imperatives effecting agents in their arena of action. Thus, at the macro level of analysis, whilst particular societal characteristics may nurture the development of power resources, if the benefit of those power resources can be established by study it may be hoped that rational intervention in societies not featuring such antecedent conditions may weaken the relation between societal characteristics and power resources. This is implicit in Stephens (1979) and Korpi’s (1978; 1983) examinations of the influence of labour movements. Similarly, in the field of study of HRM, whilst it may be that some

circumstances nurture high performance work systems (HPWS) whilst others do not, it may yet be the case that companies not blessed with facilitating preconditions may benefit from HPWS - that they would benefit if a rational intervention could be made (see Godard, 1999). Whilst the port for such interventions may remain to be established, in principle such possibilities represent one means by which historical associations need not represent necessary associations.

The constructivist denial of the relevance of the structure-agency distinction seems to relate to the constructivist position on the possibility of emancipatory knowledge. Lyotard's post-modernism, an extreme constructivism, is characterised by a rejection of the view that knowledge can be emancipatory (Delanty, 1997). Keith Jenkins' (1995) comments on the failure of the modernist project of societal improvement through the advancement of knowledge suggest that this attitude is characteristic of post-modern approaches more broadly. Craib (1992) notes the appeal of a number of such post-modern authors to the non-existence of any 'transcendental signified' which can guarantee meaning in social research in their advancement of a relativist position. If emancipatory knowledge is granted as possible then it becomes meaningful to concede a role for structure – as that of which one is ignorant. Indeed, one might say that the sense in which constructivists neglect agency, trapping actors in a kind of cultural structure, is that they do not deal with the manner in which an actor may break free of reproducing social norms. If they were to, perhaps it would be helpful to employ the notion of structure, regarded simply as that of which one remains ignorant. The underlying issue seems to be that of the potential for deepening knowledge which can serve an emancipatory function (Delanty, 1997, 137), though this issue is itself intimately related to that of the possibility of causal explanation and of the existence of a 'concrete reality' (Godard, 1993) which can be better understood.

By denying the relevance of emancipatory knowledge to social phenomena, the possibility of constructivists achieving such knowledge in theorisation is of course also denied. This itself seems an impediment to the possibility of the achievement of such knowledge if it is indeed possible. To the extent that relativism and a stress on the rhetorical has taken hold in the social sciences, paralysing critical engagement and debate within academia and between it and the social world (see Delanty, 1997, 3), there does

seem less need for urgency in analysis, less meaning in human agency. If there is no objective reality to shape, there is space only for the management of meaning, and the responsibility of social scientists is limited by the implication that there are only individual, or community, 'takes' on phenomena. Feyerabend's 'radical relativism' suggests that 'anything goes'. Whilst some assert that such a deconstructivist attitude can ultimately only affirm the existing social order (e.g. Delanty, 1997, 107), relativists may take a partisan approach. Whether such relativism is deliberately politically imbued or not, a contrast may be drawn with the approach of critical realism, which whilst wary of simple universal causal laws, retains a commitment to the possibility of advancing understanding, by way of more contingent and elaborate causal explanation (Delanty, 1997).

To the extent that one's belief in the possibility of emancipatory knowledge survives constructivist assaults, the urgency of issues of research method remains. The existence of the various approaches to social theory outlined here give some idea of the demands which must be made of research methods if they are to allow the development of understanding of social reality. Attempts at the revelation of patterns of causation are rendered intensely problematic. Two paths are typically followed in developing such analyses, frequently mutually exclusively. One strand of work seeks to derive causal conclusions from case work on companies or workplaces, whilst the other pursues a sketch of statistical relationships across a relatively large number of such cases.

The experimental logic of the case study

Case work on particular companies or workplaces is widely used in the field of employment relations. By allowing the researcher to engage immediately with social processes, the case study technique promises a detailed exploration of the causal mechanisms underlying the associations between the various social phenomena observed as these influences operate within the boundaries of the unit of analysis (Edwards et al, 1994, 9). Proponents thus argue that exemplary case work involves considered analysis of what amounts to a natural experiment in the social sphere, upon which theoretical generalisation can be based, just as it is in the natural sciences (Yin, 1994, 10). The

parallel with research methods in evolutionary biology seems particularly close (see e.g. Gould, 1993).

More concretely, case work may contribute to causal analysis of developments by deepening our understanding of the play of social relations and human agency within the instance under study, of the role of the multiple subjects active at this level. It may do this because it allows the possibility of informed reflection on the significance of actors framing of issues. The possibility of exploring the role of subjectivity can develop appreciation of the meaning of actors actions to themselves, of their feelings and intentions (e.g. Franzosi, 2000; Frege and Toth, 1999). This can help the researcher to make sense of actions which might seem puzzling. The process of case work in the field of employment relations presents a researcher with the possibility to challenge preconceptions of the internal operation of companies, particularly those supposedly general propositions resulting from a priori abstract deduction about what 'must' happen at company level. Case work can thus contribute to the 'how' and indeed 'why' of social research, our understanding of influences and processes in the social sphere. This can inform our interpretation of relations between data categories apparent from surveys or other research methods, elaborating the mechanisms operating and thus nurturing more nuanced interpretations of those associations which are more general (Edwards, 1981; Elster, 1989).

Whilst case analysis has very real strengths, however, it is also subject to limitations. A case study of a particular workplace, or even company, can offer no broader overview of the more general situation with regard to the characteristics, or phenomena, which are the object of study. The typicality, or statistical representativeness, of the characteristics displayed is uncertain (see Hyman, 1994b, 783). Even if a critical case, in which a phenomenon is most likely to occur (see Edwards et al, 1994, 9), has been correctly identified - a process which is not unproblematic - then the study can provide only an indication of the most extreme instance of the phenomenon. Only very rarely is the number of studies accumulated great enough in relation to the relevant population for a general conclusion about the incidence of the target phenomena to be established from an agglomeration of cases. The costly and demanding nature of case work means that it is relatively scarce.

There are, moreover, practical difficulties in analytic generalisation - in theorisation about the processes which foster the characteristics displayed by the case - which can be traced to the local focus of case work. Geary (1996) comments on the contribution of the case study in 'placing context centre stage'. Blanger (1994, 49) comments on the necessity in theoretically orientated case work of a consideration of the 'particularities of the context' (quoting Mitchell, 1983), and that 'proper consideration is given to the historical context and the whole social structure'. Mitchell's (1983, 203) elaboration is as follows:

the extent to which generalization may be made from case studies depends upon the adequacy of the underlying theory and the whole corpus of related knowledge of which the case is analyzed rather than on the particular instance itself.

Whilst the importance of a sensitivity to context is thus emphasised in case work, it is clear that no amount of study of an instance can, in isolation, furnish the understanding necessary to appreciate the environment in which the workplace is embedded, and thus reveal the dimensions of generalisation which are warranted from the case study. Although it may be true that 'every particularity contains a generality' (Burawoy, 1985, 18), it is extremely difficult in practice to determine the dimensions along which, and level to which, generalisation is permissible (see Edwards et al, 1994, 12). The appreciation of the conditions framing the natural experiment which the instance constitutes, and describing the space within which local activity plays, can only be deepened outwith its confines. This is equally true if several case studies are available, even if they demonstrate a diversity of experience. The location of an instance requires a characterisation of the nuances of the context which necessarily involves stepping outside the social processes of the workplace or company.

The difficulties of case work in contributing to causal analysis are dealt with by a number of authors. Indeed, the notion of 'entire cause' elucidated by Thomas Hobbes, in his *Elementary Philosophy* of 1656, captures many of the issues involved in this context. Van Evera (1997) acknowledges the problem of inferring the 'antecedent conditions' - the general background structuring or heightening relationships - which gave rise to the particular relation evidenced in an instance. Lieberman (1991) stresses the difficulties in

isolating those particular influences which gave rise to the outcome of interest, emphasising in particular the problems posed by the possibilities that outcomes may emerge as a result of an interaction of influences, that influences may be unobserved or neglected, and indeed even by the possibility that there may be multiple routes to a single outcome.

Lieberson (1991) also suggests that the possibility that a relation may be probabilistic presents difficulties for the case approach. But the possibility of a non-deterministic relation might better be thought of as a general tendency impacted by intervening conditions, rather than as a relation subject to chance, with the possible problems raised for the case approach then subsumed by the other problems raised by Lieberson. Regardless of this detail of interpretation, these authors suggest, in Yin's (1994) terms, difficulties with not only the 'external validity' but the 'internal validity' of case work, and indeed implicitly question the value of such a distinction in the consideration of the potential of case work to contribute to causal understanding.

Moreover, since the profile of the context cannot be appreciated solely by study of the social processes of the workplace, the development of that context cannot be so understood. A case study allows only the local mechanisms of causation to be explored. Study of a particular company or workplace allows only a consideration of local influences on outcomes. There is a need to subject the context, the remote influences on local developments, to the same *analysis* of causal mechanisms (see Ramsay, 1993, 78).

Thus it is not simply that case studies of particular companies cannot on their own provide an overview of the situation with respect to the object of study or target phenomenon, a task which may be aided by more aggregated quantitative data. More than this, the validity of theoretical generalisation from it is predicated upon an appreciation of context which requires the sort of historical knowledge to which more aggregated quantitative data may contribute. For example, where aspects of workplace employment relations are the objects of case analysis, the nature of workplace conditions typical in companies sharing some or other of the case's characteristics may themselves constitute a critical influence on developments in the workplace under study, whether via the aspirations of employees and their representatives or the orientation of management.

In any event, the theoretical contribution of case histories of particular workplaces or companies is confined in its analysis to a treatment of local causal mechanisms. The company does not operate in a vacuum, and company management is not the only strategic actor (Purcell, 1994). There may indeed be certain innovations at company or workplace level which are simply not in the gift of management. More surely, though, whilst in the social world there may be no ‘iron laws’ of mechanical causation (Edwards et al, 1994, 9), developments at the level of the workplace and of the company (perhaps even the very nature of the causal mechanisms of local causation therein) may still be influenced decisively by the complexion of the capitalism in which they are located, and the appreciation of this complexion may be deepened with the use of aggregate quantitative data. Indeed, to the extent that actors may be unwittingly influenced by the broader context of their activity, there seem profound difficulties in seeking to gauge the importance of the political economic soup in which the company floats with either case or survey work at company or workplace level.

The protestation of, for example, Blanger (1994, 48-9) to the effect that case work does not have inherent weaknesses, is misleading. Kelly’s (1998, 133, en3) comment that the generalisability of results from a single case is a problem merely of detailed research design, not of case work itself, is overly sanguine. The validity of analytic generalisation is not unproblematic. Moreover, the penetration of the analysis of the causal structure is necessarily circumscribed.

Assessing hard social scientific explanation

It is not immediately clear that any of the methods common in employment relations are capable of addressing the differential empirical relevance of these various sorts of perspectives. Nevertheless, it is quite common in the field of employment relations to hear simple appeals for research to employ ‘hard data’ to discriminate between good and bad theory by explicitly testing hypotheses. The nature of this approach in practice will now be considered.

Hypothesis testing

The core objective of social scientific research, with its ambitions of emancipatory knowledge, is to establish patterns of causality. In the context of the present field, this involves identifying the critical interventions which can shape workplace employment relations. In this context, key figures in US industrial relations, such as Adams (1993), stress the importance of the explicit testing of hypotheses in the field, bemoaning the failure of Dunlop's delineation of industrial relations to inspire hypothesis testing research. They argue that research in the area has been predominantly descriptive, with little effort made to derive generalisable findings (see Kelly, 1998). In British industrial relations, the importance of self-consciously testing hypothesis with quantitative data is particularly stressed by those working at the London School of Economics. For example, Frege and Toth (1999) repeatedly refer to the testing of hypotheses in presenting their work on the attitude of members to their unions in Eastern Germany and central Europe.

Often, the testing of hypotheses is regarded as requiring quantitative data. As Edwards (1981) notes, scientific testing, even in the diverse field of industrial relations, is widely regarded as the bringing of 'hard', quantitative, evidence to bear to assess the validity of the implications of explicitly specified theoretical propositions. More generally amongst social scientists, and particularly in economics, theory testing is quite typically regarded as the quantitative and thus scientific alternative to qualitative research. Indeed, there is a common supposition amongst quantitative social scientists, who are most prone to refer to the testing of hypotheses, that there is a hierarchy of research methods, in which case oriented work is exploratory whilst variable oriented work is discriminating, allowing the scientific testing of hypotheses (see Yin, 1994, 3). Mention of hypothesis testing is often intended as a rebuke to those who pursue qualitative work.

Empiricism as a model

Methodological approaches underlying treatments of employment issues are quite commonly labelled ‘empiricist’ whether they emerge from industrial relations, sociology or economics departments. Empiricism is sometimes characterised as the collection of ‘facts’ outwith any theoretical framework. In the former disciplines, the application of the label is generally intended as a slight on the sophistication of interpretation demonstrated in the research in question, whilst in economics the term is most commonly applied as a badge of approval.

Within the usage common in economics, empiricism is regarded as relating closely to Friedman’s (1953) famed methodology of ‘positive’ economics. Friedman’s methodological prescription, that theory should be assessed by exposing the implications of a model to the harsh judgement of quantitative empirical testing, rests on a distinction between the assumptions of a theory and its predictions. Friedman argues that the assessment of the empirical relevance of a model should be based solely on the correspondence of the specified predictions with empirical reality, without regard to the apparent correspondence of the assumptions to empirical reality. However, the distinction between assumptions and predictions must be quite arbitrary within an internally consistent and fully specified theory. What are characterised as assumptions might just as well be characterised as predictions, and vice-versa. The denial of the relevance to its empirical validity of those facets of the model characterised as its assumptions afford established theory a formidable protection, effectively buttressing the theoretical status quo, and perhaps by implication also the societal status quo.

Nonetheless, Friedman’s prescription, and in particular his distinction between the assessment of the predictions and the assumptions of a model, remains influential in economics, and economists remain for the large part hostile to further methodological reflection (Hoover, 1988; 1990; 1994; 1995). The acceptance of Friedman’s prescription is related to the exclusive pursuit of a quantitative, statistical and econometric approach of the sort which dominates labour economics, but which has also a substantial presence also in some schools of sociology, political science, employment relations and human resource management. Ragin (1987) characterises such quantitative work, dealing with a

large number of units of observation, as ‘variable oriented’, stressing the attempt in such work to establish general (causal) associations between quantified variables. As Godard (1999) notes, in the context of such work in employment relations as that relating to unions and wages and so termed ‘high performance work systems’ and productivity, state of the art quantitative work then becomes focused on establishing some universally true relation between quantitative indicators.

How hard is hard data? – from statistics to data

Empiricism takes a simple view of statistics as data – the observations given by social reality. Yet, as is now commonly accepted in social science, theory is everywhere, whether its presence is acknowledged in a particular characterisation or interpretation of social reality or not. As Hyman (1994a) stresses, a personnel manager is only such if he is so termed. A contrast between empiricism and critical realism, with the latter’s acknowledgement of ‘structures’ not directly observable, cannot thus be sustained. Empiricism might be characterised as a belief that data categories are unproblematic, an approach reliant on ‘common sense’ in viewing the state of reality, which makes use of conventional data labels without question. Data categories are not problematised, their interpretation is not subject to critical reflection. Issues of ‘meta-data’ (Eurostat, 1997) are thus ignored.

However, empiricism consists of more than an uncritical attitude to existing data categories. The term ‘meta-theory’ is sometimes used to label the unacknowledged assumptions remaining after any theoretical elaboration or exchange. Empiricism cannot be considered but as an orientation characterised by a lack of attention to, and interest in, theoretical and meta-theoretical considerations at all levels. An empiricist researcher proceeds for the most part unconscious of theory, not only taking as unproblematic the conventional labels applied to series, but jumping immediately to a particular interpretation of the meaning of the relationships between data categories which is established. The broader theoretical basis of the study, the framework with which the data is interpreted, is left unacknowledged, as there is little or no awareness of alternative approaches which throw the particularity of that pursued into relief.

Franzosi (2000) argues that there is a tendency in quantitative work to gloss over the problematic nature of the enumeration of aspects of social reality. He notes that economists seem particularly prone to a presumption that the assignment of a number to a phenomena puts issues beyond reasoned debate. Economists commonly counterpose ethnographic and other qualitative research with 'hard data', asserting that numbers are needed if understanding is to be advanced. These attitudes are not confined to those working in economics departments, but are present for example amongst those who regard themselves as industrial relations researchers (e.g. Fernie, 1999). As Franzosi (2000) stresses, quantification gives the appearance of the scientific. It is certainly the case that numbers conceal doubts and have a veneer of certainty which can very easily seem to justify taking them as datum. The difficulties of the process of enumeration are obscured by the presentation of a precise figure.

The construct validity underlying figures is of course critical; a consideration that also encompasses the vital issue of the social significance of the phenomena quantified. The very history of the enumeration of the previously unquantified, as described by Franzosi (2000), is cause for a critical attitude to the quantitative and the consideration of the wealth of richly detailed qualitative material. Yet, as Franzosi (2000) argues, an engagement in quantitative work tends to blind researchers to such evidence.

Validity is a central issue not only in history, where it has been the object of much attention and discussion, but in social science, where the focus of discussion of the basis of quantitative data has been on issues of reliability, of random error, which are more easily dispensed with under a discourse which is dominated by statistical concepts (Franzosi, 2000). Consideration of the *nature* of the social production of numbers, for example of the likely direction of systematic distortions or biases in official statistics, is vital (Nichols, 1997, Franzosi, 2000). Barrington Moore (1964) stresses the inherent inability of numbers to represent qualitative change, highlighting the danger that the relative continuity of a quantitative indicator of one aspect of social reality blinds researchers to more substantial change in aspects which have not been enumerated or which it is meaningless to seek to enumerate. As Moore notes, enumeration requires that there be a given pattern within which counting is meaningful – it cannot cope with categorical social change except by glossing over it.

This is not to propose a cynical rejection of all attempts at enumeration, but rather a nurturing of a ‘critical habit’ in the treatment of statistical data (Franzosi, 2000). The use, in secondary analysis, of the more sophisticated statistical and econometric techniques seems inimical to this habit in practice (see Franzosi, 1995). The lack of attention to the shaping and significance of numbers may also encourage a failure to engage with social reality in the interpretation of results. More attention to issues of meta-data would seem likely to undermine the faith of quantitative social scientists in the numbers they celebrate, and encourage more engagement in pursuit of understanding.

What when there are no statistics?

Franzosi (1995; 2000) stresses the paucity of the available statistical data on employment relations. As he notes, where researchers are only interested in statistical relationships, there is a risk that the same issues are turned over and over with minimal innovation, and indeed even that attention is devoted to narrower and narrower questions, as more and more becomes known about less and less. Franzosi (2000) bemoans in particular the absence of quantitative indicators of shifts in power relations, political context and generalised political exchange which can be employed to further understanding of historical developments in Italian strike activity. This limits the potential of quantitative analysis in the area.

Eric Hobsbawm (1997) discusses the significance of quantitatively oriented economic historians’ desire to deal in numbers to their conception of the issues of the social world, echoing the concerns of Elton (1983). These eminent historians argue that the terrain of enquiry tends thus to be defined narrowly, one aspect of the social system partitioned, and rather little is problematised, in order to make the problem defined amenable to quantitative analysis. Hobsbawm also notes the confluent quantitative professional’s need to lay out a problem which is amenable to the conceptual framework available of which she has knowledge; in neo-classical economics a framework which deals in a limited range of essentially simple propositions supposed of universal relevance. In this context, there is then often difficulty in locating precisely what has been established by the resulting empirical work.

Hobsbawm (1997) cites research in economic history into the adequacy of the performance of the British economy in the late nineteenth century. The examination of this performance was reduced by some quantitatively oriented economic historians to an examination of whether British entrepreneurs, as individuals, were irrationally short-termist in their orientation in the period. The evidence that they were not was then taken as falsification of the thesis of under-performance in late nineteenth century British capitalism. As Hobsbawm notes, such an approach embodies a certain circularity of reasoning, since the absence of problems is inferred on the basis of an assumption that these problems can be of only one form. An aspect of the sphere of social relations is delimited according to available quantitative data and conceptual frameworks and then implicitly regarded as the social totality.

A similar practice is apparent in at least some strands of research in economics into gender and the labour market. The tendency of economists to attribute differences in the pay received by men and women to differences in the sectoral and occupational composition of male and female employment, typified by the comments of Metcalf (1983), deflects attention from inequalities in these aspects of the paid work experience of men and women, focussing attention on the relative pay received by women and men occupying identical job roles. Thus, only one aspect of the phenomenon at stake is dealt with. Paradoxically, it may be that these sorts of procedures can, through the very circumscription of the area problematised and the limits of the interpretative horizons of researchers, nurture a complacency about the power of statistical and econometric technique.

There is a related danger that researchers familiar only with quantitative technique rush to assign a numerical value to a politico-economic characteristic of the organisation or nation under study so as to make this feature amenable to quantitative analysis. This may involve rather little reflection on the significance and integrity of the concept they are attempting to quantify. The literature on corporatism which blossomed in the 1970s and 1980s seemed subject to this weakness. Economists showed a particular impatience to pin a numerical value on the nature of generalised political exchange, or, as they saw it, on the structure of collective bargaining. There followed a series of re-evaluations of the extent of effective centralisation of wage bargaining, as it became apparent that

existing enumerations could not adequately account for cross-national comparative difference in economic performance (see Therborn, 1986; Crouch, 1993; Dowrick, 1993). The dangers of circularity in the definition of ‘corporatism’ were increasingly obvious, as Germany and Japan in particular were reclassified (Therborn, 1992; Edwards & Elger, 1999).

From data to draws and series

Once variable-oriented researchers have collated statistics in a manner which satisfies them as to their status as data, they tend to treat their empirical material as if it were formed by random drawings from some kind of lottery. The data are discussed as realisations of a ‘data generating process’ (or DGP) (e.g. Baltagi, 1995), a process conceived as a system of equations with a deterministic component disturbed by the draw of a random error. Correspondingly, the data are from thence regarded not so much as representations of social reality but more as a numerical phenomenon in their own right. This is reflected, for example, in the description of historical statistics as (time) ‘series’.

The framing of the determination of a phenomena in terms of an equation featuring a number of variables may be critical in nurturing the variable oriented researchers’ focus on the general rule, the average statistical relation between the variables (see Godard, 1999). Variation around this mean relation is regarded as random variation which might occur for any number of reasons and thus is of little interest. As Franzosi (1995; 2000) argues, outliers are commonly disregarded as exceptions to the rule. This focus is reinforced in economics applications by the notion that the theoretical framework and conceptual tools underlying the model must have a universal applicability.

The treatment of the data as realisations of a well-defined DGP also has implications for the treatment of the dynamics of a variable. Against such a backdrop it is natural to regard, at least in the first instance, any serial correlation of a variable evident as a result, quite literally, of its own internal dynamic. The significance of this treatment is discussed in the forthcoming section on the practice of econometrics.

Moreover, the notion that there is some underlying DGP, an algebraic model of the causal process, seems to nurture a cavalier approach to the issue of causality more generally. Commonly, examination of the pattern of causation is reduced to the tracking of series. Theory becomes viewed as a matter of the variables to be entered in an estimating equation (Franzosi, 1995); a matter of the ‘true model’ in this sense. There seems a definite feeling amongst many variable oriented researchers that beyond the specification and testing of such equations nothing may be done to advance understanding of the patterns the data reveals – the role of interpretation is in this way devalued.

The efforts of econometricians to determine the properties of estimators under various data conditions is of course valuable. However, by exploring the performance of estimators by determining their biases and inefficiencies when used on data resulting from known data generating processes, data thrown up by pre-specified equations featuring deterministic and stochastic components and an implicit causal ordering, econometricians buttress the idea that equations are all that matter. It is implicit that econometrics holds *the* key to causality, if only the correct estimator can be identified. In all this, the status of regression analysis as a technique for uncovering statistical relations seems easily lost.

Association without causation

The difficulties of quantitative research extend beyond the issue of the meaning of the data categories employed. The multiple regression techniques which stand at the heart of econometric analysis of theory do not in general offer clear guidance as to the validity of theory. There are at least seven ways to interpret the statistical associations thrown up by multiple regression analysis.

Firstly, and most seriously, they may be absolutely spurious, not grounded in any causal relationship of any sort. Ramsay (1993) highlights the statistical association between the numbers of recorded stork flights and the birth rate in Denmark, an association which epitomises the possibility of statistical relationships which can express nothing of the determination of the phenomena under study. Similarly, Franzosi (1995)

highlights correlations between income and the occurrence of sunspots. Moreover, econometricians working to establish the properties of estimators used in regression analysis continue to elaborate the extent of the possibilities of bias in estimators and thus of mistaken inference of statistical associations.

Secondly, the associations established by econometrics may record a highly precarious relation quite non-robust to econometric specification. This may emerge as a result of the selection of certain proxies of concepts rather than others with an apparently equivalent claim to validity. A highly contextual relation which may be stressed by a researcher may appear scientifically established in regression results only because various 'controls', whether quantitative or qualitative dummy variables, feature in the estimating equation whose results are reported. Econometric estimation is often a very untidy process, involving hundreds of alternative specifications only a select few of which are reported. As Edward Leamer has noted, econometric results, like sausages, may be better not seen in the making (Franzosi, 2000). Throughout the econometric exercise, the researcher seeks to establish statistically significant relationships which she can make sense of within the theoretical approaches and concepts she is familiar with.

The need to be able to interpret the results, and indeed to offer an interpretation of them which colleagues would find meaningful, frames the practice of estimation and the emphases in the reporting of results. The danger of ad hoc reconciliation in case work highlighted by Korpi (1989) is not absent in statistical work, though it may be less apparent to audiences. Franzosi (1995) notes the tendency of quantitatively oriented researchers to turn back to the theory which is supposed to be under examination, in order to impose order on the chaos of a multitude of conflicting regression results. To the extent that researchers return to the theory they are assessing to render the results reported comprehensible to themselves and their audience, empirical estimation becomes an exercise in calibration.

Thirdly, the variables taken as dependent and independent may each be (equally immediate) expressions of some more deeply embedded characteristic or characteristics which do, quite generally, drive the measured variables. It is then mistaken to attribute the association between the measured variables as causal. Critically, statistical analysis cannot show whether a variable correlated with the proxy of the phenomena of interest is

epi-phenomenal. Rather than constituting a lever on the target variable, the regressand, the regressor may itself be levered by a neglected force (see e.g. Blalock, 1964).

Fourthly, and relatedly, the direction of any causal relation between the variables taken as dependent and independent may not be that supposed. There may be ‘reverse causation’ – the dependent variable may drive the independent. More generally, variables may be mutually conditioning (see e.g. Franzosi, 1995).

Fifthly, the independent variable may capture something of a generalised actual influence on the dependent variable which whilst acting via the independent variable is not reducible to it. Thus the independent variable may not itself constitute the port of intervention, being rather an expression of some underlying general influence.

Sixthly, the establishment of the independent variable may be of significance of itself for the dependent variable, but this establishment may depend critically on the occurrence of one possible sequence or combination of events or circumstances which it is impossible or meaningless to proxy quantitatively. If a statistical relation is predicated on a particular contingency, or set of antecedent conditions, the sense in which the independent variable causes the dependent is then in doubt.

Seventhly, and the best possible scenario in the application of multiple regression, it may be that the general historical association evidenced by a statistical relationship reflects a ‘probabilistic’ *causal* relation. Thus the independent variables, in general, afford leverage over the dependent variable of themselves, though this relation may not be universally evidenced in the sample. Indeed, this may be contradicted completely in particular circumstances within or without the sample. To the extent that the latter is the case there is obvious difficulty in describing the independent variables as causing the dependent variable. Even where there is no clear contradiction of a posited causal relation there remain to be explored issues of the differential salience of the factor. Certain underlying conditions may accentuate a general relationship (Van Evera, 1997). Ragin (1987) emphasises that influences tend to act in concert, so that one may best think in terms of a conjuncture of events, or, as Lieberman (1991) would have it, of events occurring in the context of other events (see also Franzosi, 2000).

Confronting causality in variable oriented work

Quite generally, regardless of the pedigree of data employed, in its efforts to assess theory, 'variable oriented' work runs into the problem of the attribution of causality. The statistical associations, probabilistic relations, between variables which are uncovered by multiple regression analysis may arise for a number of reasons which are glossed over by the common reference to the 'explaining' of the 'dependent' regressor by the 'independent' regressands.

In the UK at least it is in economics that multiple regression analysis is most intensively, and exclusively, applied in empirical work (see e.g. Booth, 1995). There is a tendency in applied economics to presume that the statistical relationships uncovered by multiple regression are indicative of particular patterns of causality. As Franzosi (1995) notes, there is often thus no attempt to confront the obvious chasm between a statistical correlation and a causal relationship. Those attempts which are made typically depend exclusively on econometricians efforts to come to terms with the issue. Although there has been some journal discussion of the issue (see Hoover, 1990; 1994), even advanced econometrics textbooks typically devote only a few pages to causation (e.g. Greene, 1998). The statistical assessment of the opportunities for predicting one variable with another are central to the treatment of the issue of causality in these academic circles. Three concepts seem central to economists' attempts to address doubts about the causal status of the statistical associations which multiple regression throws up. Although there are some inconsistencies in the usage of terminology, 'weak exogeneity', 'Granger causality' and 'strict (or strong) exogeneity' dominate the terminology used in econometrics to address causal uncertainties.

At its most general, the notion of weak exogeneity relates to the nature of the possibilities of the accurate, or 'efficient', estimation of multiple regression coefficients where allowance is made for the possibility that there may be a two way relationship between certain of the variables through the specification of a system of regression equations. In this context, a variable is said to be weakly exogenous if the parameter attached to it may be estimated efficiently without resort to the full system of simultaneous equations, but by block recursion. Usually, however, the term is employed

in a slightly more straightforward fashion. A variable X is said to be weakly exogenous with respect to a variable Y if the solution of the system of simultaneous equations shows that the coefficient on the current value of Y is not significantly different from zero in the equation where it is entered as a regressor with X as the regressand. Y may thus be treated as the sole regressand.

The variable X is then interpreted by applied economists as being ‘pre-determined’, in the sense that whilst the variable X may instantaneously ‘effect’ the variable Y , the variable Y does not ‘effect’ the variable X . Although the meaning of the weak exogeneity of a variable is very difficult to explain intuitively in any terms but these, which imply an inference about the pattern of causal relations between variables, theoretically oriented econometricians are careful to stress that the term merely denotes the nature of the possibilities of an efficient estimation of statistical relationships. Perhaps because of the difficulties of an intuitive interpretation of weak exogeneity, the term does not feature as prominently in the applied economics literature as do the other terms which are employed to comment on the causal status of relationships.

The notion of ‘Granger causality’ is central to the remainder of applied economics’ treatment of the issue of causation. The assessment of Granger causality is based upon the statistical analysis of the sequencing of events. This stress on sequential precedence is consistent with the approaches of philosophers of the Enlightenment and after to causality. Jevons refers to precedence in his *Elementary Logic*. John Stuart Mill argues that ‘We may define....the cause of a phenomena to be the antecedent of the concurrence of antecedents, on which it is invariably and unconditionally consequent’ in his *Logic*. Whilst he does not insist on its importance, David Hume, in his *Enquiry into Human Understanding*, writes as though temporal precedence, observed over many instances, were central in conceiving cause. He suggests that with such evidence one may conclude that ‘objects’, i.e. events, are more than in conjunction, but are rather ‘connected’.

Strictly, the assessment of Granger causality can only be made of ‘stationary’ data – time series which demonstrate statistical properties which are independent of time. A working, or ‘null’, hypothesis is adopted to the effect that information on the past values of a variable X cannot be employed to improve on the prediction of a variable Y which

can be made from Y's past values. There is thus a critical presumption in favour of the view that Y is driven by its own internal dynamic, rather than by any other force. A variable X is said to Granger cause a variable Y if it is shown that past values of the variable X *can* contribute to the prediction of the variable Y to an extent which is statistically significant. The null hypothesis that Y simply has an internal dynamic is under these circumstances rejected.

The concept of strict or strong exogeneity is intimately bound up with Granger causality, but also relates to the concept of weak exogeneity. A variable X is said to be strictly or strongly exogenous with respect to a variable Y if the variable X is not Granger caused by Y, *and* the variable X is weakly exogenous. Thus, X is strictly exogenous if the past values of Y cannot contribute statistically significantly to the prediction of the current value of X which is obtainable from X's own past values, and, in addition, X is pre-determined in the statistical sense described above. The most obvious example of a variable strictly exogenous in any context is age. A person's actual age at a particular moment can be perfectly predicted from their age in previous moments, without regard to any other factor, and moreover, this age cannot, at any moment, be effected by the political, economic or social conditions prevailing.

As should be obvious, there are severe difficulties in the treatment of issues of causation in the applied economics and econometrics literature. In the central concept of Granger causality, causation is conflated with temporal precedence. This notion that the sequencing of developments expresses the nature of the causal relation between them rests on the principle of 'post hoc ergo propter hoc' – a renowned fallacy (Hoover, 1988; 1990). Moreover, tests for Granger causation involve a presumption in favour of the internal dynamic of a variable. Unless there is a statistically significant indication to the contrary, the development of a social phenomena is assumed to be self-generated, rather than shaped by the context in which it occurs.

Many econometric studies show that the variable taken to measure the phenomena of interest has its own internal dynamic – that the observations shown in the series are statistically related to the observation which preceded them. To take a specific example, Pehkonen & Tanninen's (1997) study of the development of the density of union membership in Finland over the period 1962-92 finds that density in any year is closely

related to density in the previous year, even where many other variables thought to bear on union density are introduced in to the econometric specifications - in this sense controlled for.

Should this be taken to imply that the density of union membership was literally self-perpetuating in Finland in the period 1962-1992? Does density at any moment, of itself, have causal implications for density in the future, in the same way that, for example, the radiation emitted by a radioactive substance in one period has implications for the emission in the next? More to the point, does density at one moment have causal implications for density in the next moment just as an observation of a computer generated variable which follows a deterministic path subject to some periodic random 'shock' component (a 'random walk' say) has implications for the observation of this computer generated variable in the next period? It seems likely that rather than being causally self-perpetuating in this way, the extent of union density at any time is intimately bound up with a plethora of situational characteristics which are the actual influences on the future course of union density.

In effect, then, the internal dynamic apparent in union density in the study constitutes a gauge of the deficiency of the characterisation of the influences on density offered by the other variables which feature in the data set. It may be apparent from econometric analysis of a data set that a social phenomena has its own internal dynamic. This conclusion may, however, be a product of the paucity of the data set rather than the self-generating nature of the phenomena. The strict exogeneity of a variable does not imply that its trajectory may not be shaped by the conditions in which it evolves, only that these conditions are not proxied in the data set.

Our perception that there is an internal dynamic is an expression of our ignorance of the causal mechanisms which constitute the social process shaping density. Following from this, the apparent internal dynamic is an expression of the paucity of definition of the phenomena which the dependent variable, 'density', gauges. It is an expression of the extent to which the dependent variable constitutes a gauge not of one well-defined phenomena, but of the state of a process, an organism, a system. The power of the internal dynamic provides a gauge of the imprecision of the concept which the dependent variable expresses, predicated of course on the richness of the dataset. To say that a

phenomenon has its own dynamic is to say that we cannot even begin to identify the conditions which drive it. But crucially it is also to say that we are unsure of the nature of the phenomenon which we are seeking to understand. Interestingly, it may then be more appropriate to talk of a data series of interest as a syndrome or an organism rather than a variable, with its connotations of a well-defined and distinct single outcome. The precision of enumeration recedes.

Nevertheless, the practice in applied economics, as it stands, is to assess the causal status of relations according to the possibilities of prediction of one variable from another. Critically, all the conclusions which may be drawn with these statistical techniques are predicated on the data set employed, being valid only conditional on it. As Hoover (1990) notes, Granger causality would be better termed 'incremental predictability'. There seems to have been rather little development in the treatment of causation since the cross-lagging procedures developed by Donald Campbell in the early 1960s (see Blalock, 1964). As the discussions in Blalock (1964) and Persson and Tabellini (1995), writing three and a half decades apart, demonstrate, there are limits to the distance which statistical and econometric technique can, of themselves, carry us in exploring the causal meaning of the more or less general statistical associations which may be derived from historical experience.

Crucially, the very assessment of causation as it proceeds in applied economics and econometrics relies on the examination of patterns of correlation. Thus, the possibility that the social phenomena being explored is not in any meaningful sense caused by the variables entered as regressors with which it is related, but that this object of study and the indicators treated as regressors are in some way joint products of some unquantified underlying conditions, cannot be addressed. Econometric modelling and statistical assessment have very definite limits. It is not only that their validity depends on the variables reflecting substantial developments, nor is it that one can only hope to identify causal tendencies, and not necessary exact causal relationships. It is that econometric technique cannot of itself establish the point of intervention by which change in an aspect of social reality may be effected. The task of identifying ultimate cause is glossed over.

The determination of causation is not, as the most sophisticated approach to the issue current in econometrics implies, simply a matter of the temporal sequencing of shifts in the quantitative variables in a dataset. Chronological priority does not amount to causal status. It is not simply that the temporal precedence of a phenomenon is insufficient to establish causation. The chronological ordering of the most readily observable indicators may indicate nothing of the pattern of causation. As is often noted in explicit considerations of econometric methodology (e.g. Hoover, 1988), Christmas cards do not cause Christmas and the purchase of wedding rings does not cause marriage. Clegg (1985) throws up an interesting example in the field of employment relations in his early twentieth century history of British unions. He argues that the memberships of some unions held up in the difficult years of the early 1920s as a result of their strength of tradition, and of the intense loyalty of their members. He thus implies that later movements in union membership may provide some indication of the attachment of members to their union in earlier years. Later developments express something of earlier conditions. In this context, for example, a regression analysis establishing a relation between some indicator of the earlier efficacy of unions in collective bargaining and a later change in union density might be read as suggesting that more successful unions lose fewer members. But it might be better interpreted as showing that a more committed membership, which sustains organisation in difficult times, promotes improvements in terms and conditions. The sequencing of developments in indicators need not correspond with the structure of causal relations, an unfortunate reality bound up with the uncertainty around quite what it is that quantitative variables represent.

Approaches to the issue of causation relying on temporal sequencing cannot overcome the difficulty of inferring causal relations from the statistical associations thrown up in multiple regression, any more than can the use of longitudinal cross-sections, or panels, of itself. As is the case in all regression analysis, associations may express causal relationships, but they may well not. Such an approach to causation cannot address the problem of a conflation of association and causation, as it relies precisely on the assessment of patterns of statistical association. This conclusion may be particularly difficult for those variable oriented researchers working in the neo-classical tradition of economics to comprehend, implying as it does that their very discipline is characterised

by social norms, features which are not explicitly recognised in their conceptual framework, and indeed are antithetical to many, despite the efforts of Donald McCloskey (e.g. 1985). Economists' disciplinary norms may thus be all the more strongly held and vehemently defended as simply scientific.

Kevin D. Hoover (1988; 1990; 1994; 1995) develops a remarkable argument which seeks to offer an alternative approach to the drawing of causal inference in econometrics. He argues that the invariance of a system of equations to intervention is a critical test of causality, or what he terms the causal ordering. A causal relationship should not break down when an agent attempts to use it to control the phenomena of interest – Goodhart's Law should not hold. This approach to the assessment of theory bears a surprising relation to the Marxist notion that 'praxis', the application of ideas by actors, is the ultimate arbiter of the adequacy of our understanding (e.g. Hyman, 1975; 1994a). As Hoover recognises, however, his approach begs the question of how an intervention seeking to affect control may be identified. This identification must be based on historical or institutional knowledge – it must be qualitative. Indeed Hoover (1990) implies that something akin to an experimental logic could be pursued, with conclusions about causal orderings of universal relevance obtainable where a change in the institutional context of developments in a particular case is clear. Despite his suggestion of the validity of experimental logic, Hoover also stresses the possibility that a relation may be causal only in certain circumstances, circumstances which might be appreciated only by turning to qualitative analysis, and in any case require exploration in their own right. Moreover, as Hoover (1990; 1994) stresses, the truth claim of a system is limited by the extent to which the 'errors' or 'random shocks' of the equations which comprise it are explicable. A further role for qualitative evidence is thus suggested.

Interestingly, Hoover does not problematise control itself, being content with the econometric thought experiment 'if we could control X, then...'. An additional role for qualitative analysis, in identifying a feasible port of intervention for the manipulation of a process, is thus glossed over. Nonetheless, by locating its contribution, Hoover's (1990; 1994) considered arguments demonstrate the limits of econometric method.

Non-scientist social scientific enquiry

Explorations of the social world may be regarded as ‘scientist’; as having the form rather than the substance of scientific method, if they obscure the limits of statistical analysis of the social realm, and intimidate with their certainty those who bring to bear a distinct theoretical framework. It is clear that there is a deal of scientism in the field of employment relations. This is not to suggest the rejection of the entire edifice of ‘hard’ social scientific enquiry. But nor is it to suggest the rejection of the detailed case study. It seems rather that there is an opportunity for the fruitful combination of what are most usually competing ‘scientific’ research methods.

The value of efforts at hypothesis testing

As already demonstrated, the use of ‘hard data’ for hypothesis testing is insufficient to develop causal explanation. As Franzosi (2000) claims, citing Leamer, statistics and econometrics cannot bear the weight put on them; the falsificationism underlying their use has a slippery nature (Hyman, 1994a). Of course, where hypothesis testing is pursued in qualitative case study problems also arise. Case work is no solution to the difficulties of establishing cause, as an earlier section has indicated. Lieberman (1991) shows the difficulties which the detailed study of a small number of cases has in dealing with interactions and multiple influences on outcomes – there is no simple way to pull apart factors any of which may be shaping developments. Influences may well tend to act in concert, so that one may best think in terms of a conjuncture of events, or events occurring in the context of other events (Ragin, 1987; Lieberman, 1991; Franzosi, 2000). Case work, as is true of all types of empirical study to some degree, is confined to a certain unit of observation, in a certain period. With regard to case work, however, these reflections highlight the difficulty of the appreciation of the context of an instance, and thus the problems of establishing the dimensions in which generalisation from case work is valid. Yet, as Lieberman (1991) argues, there is a temptation in such qualitative work towards ‘small N and big conclusions.’ⁱⁱ

It is misleading to characterise some research methods as tools for exploration and others as the means for hypothesis testing. In practice, the results of all investigations are partial, provisional and uncertain, and raise new questions. The critical distinction seems that between efforts with the intention of assessing the relevance of some specific well established theory or theories and those intended rather to generating new means of understanding. There seems a related distinction between efforts which require absolute conclusions and those which do not demonstrate this need. Researchers may well attempt to draw out associations, attempt causal influences, and be oriented to generalisation to propositions, and thus seek to contribute to theory. But the task of identifying the antecedent conditions on which a phenomena is consequent is an extremely difficult one. In practice, the distinction between 'theory testing' and 'theory generation' seems more one of presentation and ambition than of achievement. In pursuing understanding, it appears that a more realistic notion of the scientific, and rather less preoccupation with an abstracted notion of it, which distorts even the practice of natural science (see e.g. Campbell, 1921; Gould, 1993), might be fruitful.

This is not to say that an orientation to the testing of well-defined theories is not valuable, whether in quantitative or qualitative work. The explicit specification of the model which this approach involves clearly demonstrates the view taken of the pattern of causation underlying associations. The problem is that researchers then move very readily from establishing association to causal inference. In contrast, the lack of clarity in the causal inferences often drawn in qualitative work seems likely an expression of qualitative researchers' appreciation that patterns of causality may not easily be taken from associations between events.

The middle ground between the approaches, where there is some explicit treatment of the causal relations underlying relationships which does not jump immediately to the conclusion that the associations evidenced demonstrate the adequacy of a theory, is very difficult terrain. Negotiating it involves some confrontation of the profoundly problematic nature of causal inference.

Developing causal explanation - a multi-level strategy

Quantitative hypothesis testing as it is usually pursued is not enough, despite the value of the analytic orientation it embodies. Large scale statistical work suffers certain profound weaknesses. Such variable oriented work needs complementing, for a number of reasons, with more detailed studies of particular ‘cases’. Case studies, dealing with developments more thoroughly than is possible with statistical work, are valuable in several respects. Case oriented work need not deal with a particular workplace or company, despite the common object of study in employment relations, but may of course concern some aspect of the historical experience of an industry, or an entire nation.

Case work allows the exploration of the possible qualitative, or at least unquantified, concomitants of the magnitudes expressed in the statistical series commonly available or employed. This sort of work facilitates a critical appreciation of what data series may be gauging. Thus, case work enables reflection upon the nature of the differences which may be picked up in quantitative indicators, and which may underlie statistical associations. For example, a quantitative cross-national comparative study of inequality may find that a series describing the coverage of collective bargaining is statistically related to wage dispersion, even when union density, for example, is statistically controlled for. Yet it may not be the extension of collective agreements, of itself, which is critical for wage inequality – rather the variable taken to gauge coverage may reflect the broader historic involvement of the state in the sphere of industrial relations, whether through peak level collective bargaining, statutory employment protection, statutory rights to co-determination, legislation on training provision, or through the implementation of minimum wages. Coverage may thus tend to be associated with a cluster of political-economic characteristics which shape the start point and content of collective bargaining, with the statistical relation between coverage and wage differentials expressing the pertinence of multi-faceted state action for inequality.

More detailed case work, pursued at the level of the experience of particular countries, can thus illuminate the relations between quantitative variables and the mass of unquantified conditions prevailing. Some at least of these may be unquantifiable in the

sense that in some dimensions cross-national variations may not be matters of degree but matters of kind, at least in the context of the prevailing conceptual categories.

Case work also deepens the possibilities of seeking evidence of the causal mechanisms reflected in statistical relations by examining developments in a number of dimensions of social reality and at a number of levels of aggregation or observation. The common empirical, or observational, equivalence of theories with regard to one specific phenomenon implies a need for a rather subtler means of assessment of relevance and validity than the comparison of the one dimensional predictions of a theory with some particular aspect of social reality. This accords with the modern concern in natural science that theories be able to account for phenomena other than that for which they were originally intended to account (e.g. Campbell, 1921; Gleick, 1992).

This represents a vital means of assessing the relative relevance of competing perspectives. Rather than seeking explanations of phenomena in isolation, theory should be at least consistent with, and hopefully illuminate, other aspects of social reality, with parsimony regarded more holistically. There is otherwise the danger that the status quo is effectively insulated from critical onslaught, with an implicit feeling that if a single target phenomena may be accounted for by existing theory then no more thought is needed on it. If some alternative perspective then emerges the onus is all on it to offer evidence to prove the irrelevance of the established perspective, a very difficult task in the context of a determined profession defending an established tradition by requiring of opponents evidence far transcending that ranged in its favour.

Variable oriented researchers, and most particularly applied economists, are prone to deal with observational equivalence by privileging one interpretation of data over another on a priori 'theoretical' grounds. Where systems of equations are involved, this privileging is very obviously embodied in the restrictions explicitly applied in econometric estimation (Hoover, 1988; 1994). In such contexts, the critical issue of the underlying causal mechanisms which generated the data manifests itself in the problem of 'identification' which arises in obtaining econometric estimates. In order to obtain parameter estimates, the problem of observational equivalence is confronted by imposing (identifying) restrictions on the form of the model estimated. These restrictions are derived from (a particular) a priori theory, rather than by looking to evidence of the actual

micro mechanisms, which may be qualitative in form, completely unfamiliar or inconvenient in its implications for the theory on which everything is predicated. In effect, presumptions about the mechanisms operating are used to organise the story told through the data. Abstract theory is preferred to what may be fragmentary, and qualitative, evidence about the mechanisms actually operating in the social world. In neo-classical economics, this practice is encouraged by the notion that the fundamentals of theory, at least, (must) have a universal applicability. The result of this approach to the problem of empirical equivalence, which Hoover (1994) terms 'a priorism', has much of the character of a calibration, rather than a test of theory (see also Hobsbawm, 1997).

More generally, the tendency is apparent amongst variable oriented researchers to truncate causal consideration according to the availability of what is regarded as valid quantitative data, with the assessment of data validity of course depending to some extent on the correspondence of the statistics with theory. In neo-classical economics, where only variable-orientation is acceptable, the opportunities for the demonstration of the misleading or incomplete understanding of social developments offered by the frequently severe 'economic realities' confidently propounded by what Thomas Carlyle characterised as the 'dismal science' are thus confined, despite the scientific panoply of mathematical formulation and statistical method.

Paul K. Edwards (1981), in his historical work on strikes in the US, argues that a critical standard according to which theory should be judged ought to be the extent to which the accounts of social phenomena are 'illuminating'. Accounts should demonstrate their validity by illuminating facets of social reality which have remained neglected or puzzling. The 'generally illuminating interpretation' is that consistent with findings of other studies dealing with different levels of aggregation, or with related issues whatever their units of observation or analysis. The application of this criteria involves a more rigorous test of the relevance of a theory. Moreover, it offers the prospect of a progressive development of theory in a way which an insistence on the relevance of some theoretical presumptions cannot (see Hoover, 1994). Edwards (1981) also argues that theory should also offer a 'satisfying account' of phenomena, in the sense that there should be a convincing elaboration of the links between the alleged influences and the

phenomena alleged to result. Elster (1989) stresses the importance of such elaboration to explanation, as opposed to prediction; an essentially descriptive activity.

In large part, case work may play a role in this context by allowing the study of the 'real dynamics of micro relations' (Ramsay, 1993). This knowledge can be applied in the interpretation of macro developments and relations. It can thus make a very real contribution to the assessment of causation, allowing as it does the elaboration of the bases for aggregate relationships. As Ramsay emphasises, echoing Edwards (1981), the combination of micro and macro knowledge is vital for the understanding of causal processes. Despite the claims of Friedman, the assumptions of a model, the meta-physical basis of it, are crucial for the assessment of its adequacy. The inevitability of observational or empirical equivalence of different theories with regard to one facet of reality considered in isolation implies that it could not be otherwise. The consideration of the tenability of facets of the model characterised as assumptions is critical in the assessment of whether the model should be accorded any more credence than a 'just so' story (see Elster, 1989). In the absence of this consideration, a theory may too readily be taken as that special fairy tale which renders the world intelligible (cf Campbell, 1921).

Case work also allows consideration of the limits of operation of tendencies, mechanisms, laws, and, critically, of the basis of these limits within the units of observation (see Ragin, 1987; Elster, 1989, Edwards, 1994). These possibilities of exploring the exceptions to tendencies, of the relative autonomy of, for example, company developments from the society in which the company is embedded, arise because case work allows an examination of the mechanisms, processes and human action within the organisation (Ragin, 1987, 77; Edwards, 1994). Similarly, detailed work can be pursued in the analysis of societal developments. Theda Skocpol's (e.g. 1992) studies of the development of social protection feature the nation state as the unit of analysis, emphasising the possibility of comparative historical analysis of a small number of cases with the method, drawing on J.S. Mill's method of agreement and method of difference (see Lieberson, 1991). Fulcher (1991) develops a comparative historical case study of industrial relations in the UK and Sweden over a period of a century.

Case work presents, at various levels, the possibility of the consideration of the losses of reductionism, reification, and the collapsing of levels of analysis (see Colomny,

1992). The danger of reductionism inherent in more aggregate research; the temptation to collapse levels of analysis via the assumption that there is a simple reflex reaction to external imperatives, can be attenuated. Case study guards against the squeezing out of the process of agency by allowing detailed exploration of the various arenas in which it is exercised. This nurtures a more nuanced view of the influences at play on phenomena. It allows a treatment of the organisation, which is often treated as a black box, a problematisation of the subjectivity which exists at this juncture. It allows a more general appreciation of the nature of the interactions of social actors, in whatever arena. Thus, some appreciation can be developed of the importance of actors' framing of the conditions facing them, of their orientation in confronting the complexities of their everyday working lives. Frege & Toth (1999), whilst insisting on the testing of hypotheses, stress the role of qualitative historical work in deepening understanding of the meanings held by and intentions of actors. Meanwhile, within industrial economics, Sutton's (1992) increasingly influential approach stresses the importance of case examination of the strategy emerging from management discussions. Some exploration of the role of discourse, ideology, and thus some recognition of its potential play at other levels, is allowed in case work. The notion that humans are bounded in their rationality can then be given some content.

Case work has a vital role too in exploring the basis of diversity, of an outlier, such as a strike wave in an analysis of the history of industrial conflict in a nation (e.g. Franzosi, 1995). The significance of variation around the mean in reality, and the importance of understanding this variation, is easily understated (Ragin, 1987; Lieberman, 1991; De Vera, 1997; Godard, 1997). Whilst the attention of statisticians is generally to the average, the average is an abstraction and the variation the essence of experience (Franzosi, 1995; Gould, 1992). As Franzosi (1995) stresses, if one found in an experiment that, on average, a new fertilizer brought a 20% improvement in crop yield, but in one instance doubled yield, it would surely be interesting to know why. Deviations from means should not be presumed a priori a matter of measurement error, though this presumption is convenient for the truth claims of statistical and econometric models.

Exceptions to the general rule promise much in the way of understanding if they are regarded as more than expressions of a random disturbance to a deterministic

numerical relationship. Exploring such issues in the social world requires a savouring of the detail of social reality. Case work may permit exploration which can aid appreciation of which of the historical associations, whether these are between events, arenas, or levels of the social structure, are necessary and which may be not. It has potential in this way to contribute to the development of understanding of the various points of intervention from which change might be pursued.

Furthermore, explanation ultimately requires an assessment of levels of causation, of immediate or proximate and contributing and remote causes. Thus, counterfactuals at a number of levels of analysis are critical. Cross-national comparative work has a vital role in providing benchmarks for the critical assessment of aggregate developments, necessitating as it does the confrontation of a broader range of experience and allowing as it does a more thorough contemplation of a diverse range of societal possibilities (Hyman, 1994). Whilst variable oriented work is of obvious use in such large scale analysis, case studies at various levels of aggregation can aid understanding of the conditions on which a phenomenon taken as a cause is itself consequent.

Thus, in the broad field of employment relations, we might seek to explore the basis of cross-national comparative variation in the density of union membership, particularly in the light of such cross-national comparative relationships as that between labour organisation and the development of welfare states (e.g. Stephens, 1979; Esping-Andersen, 1990), and indeed that between labour organisation and working conditions. In his study of developments in Sweden, Korpi (1978, 74-5) argues that the exceptional organisation of the Swedish working class reflected particular 'structural and historical circumstances', societal characteristics which facilitated the development of collective organisation and action. Korpi stresses in particular the relevance of an absence of racial or ethnic tensions in what was a relatively ethnically homogenous society, the weakness of religious divisions, and the influence of emergent socialist ideas as the basis of the labour movement was laid alongside Sweden's late industrialisation. Fulcher (1991) echoes Korpi in his argument that the timing of industrialisation relative to the currency of socialist ideas was critical in the comparative development of the Swedish and British labour movements. Stephens (1979), in his cross-national comparative study, noted the statistical relation of rates of union density in advanced capitalist countries in 1930 to

industrial concentration, arguing in a similar spirit to Ingham (1974) that a tight network of employers tends to force the union centralisation which nurtures an extension of organisation. Stephens also notes the roles of ethnic, linguistic and religious homogeneity to which Korpi (1978) accords much importance. But Stephens (1979, 200) stresses the limits of the statistical relation of union density in the opening decades of the twentieth century to these 'structural' factors, and moreover their lack of relation to the subsequent developments in membership density.

The comparative historical pattern of union density, which has been shown to be of statistical relevance to the development of welfare states, is thus itself not determined by identifiable quantified facets of societal structure. Stephens (1979) highlights the importance of qualitative historical investigation not only in the assessment of the contemporary conditions which may be expressed by quantitative variables, but in the examination of the critical events which shape the very quantitative variables which, on the basis of statistical associations, are often accorded causal status. Even to the extent that there are, at some level, general associations between data categories which reflect the influence of one quantifiable social characteristic on another, the issue of the forces which shaped the influencing variable remain to be explored. These latter issues are of course central to the question of the arena in which change was, and may be, forged, the manner in which a solution to difficulties identified may be effected, i.e. to causal explanation.

Relatedly, case work may be used to countenance the possibility of patterns of mutual conditioning. For example, Franzosi's (1995) analysis of Italian strike activity shows that political economic structure, including the pattern of collective bargaining, limits, or shapes, mobilisation, but also that mobilisations transform the political economy. Stephens (1979) attempts to use qualitative material to explore the co-development of labour movements and welfare states which his quantitative analysis reveals. Such nuances are much better amenable to case-oriented study.

In a myriad of ways, then, case work offers potential for a richer understanding of causal processes in a critical sense. It can contribute not only to the elaboration of the nature of a causal mechanism of universal relevance, detailing the manner in which a universal law operates. It can allow an exploration of the conditions under which a law

operates, the antecedent conditions on which its operation is predicated or which mute or amplify its operation. It allows exploration of the manner in which causal laws are contingent, and thus the sense in which they are generative mechanisms rather than causal laws. It can contribute also to the appreciation of causes located deeper in a causal structure. It is thus that case work of particular organisations or institutions can contribute to not only the 'how' but the 'why' of social research, and thus to the development of the pattern of causation and thus bring a richer causal appreciation. The enrichment of understanding which, for example, Edwards et al (1994) stress that case work can bring is thus very substantial.

Conclusion: multiple methods plus knowledge of alternative perspectives

Causal inference is a tortuous task whatever the research method. This however is no excuse for a lack of contemplation of the issues involved. As Hoover notes in his discussion of economists, there is a very real temptation to dismiss philosophical contemplation, to 'get on with the job', although 'resolutely looking the other way does not dissolve the problem' (1990, 208; 228). Whilst any single research method is insufficient for a thorough assessment of theory, the issue is how we might best move forward in advancing understanding of employment relations. In the terms of John Goldthorpe (1998), the aim is to gradually uncover the 'generative process' which underlies the phenomena of interest.

In assessing theoretical perspectives a ruthlessly discriminating attitude is an ideal. Researchers must be self-critical in their orientation, firstly to acknowledge the presence of, and then to begin an effort to push back, the metaphysical presumptions inherent in interpretations. This requires that we draw on an armoury of methods to assess the meta-theory implicit in the theory stressed for the purposes of empirical analysis. In this way we can hope to avoid the essence of an uncritical empiricism in practice. Social science requires a battery of research methods to eradicate the mystical, the unbased and unsubstantiated armchair conjecture, and the presumptions veiling prejudices often sustained by circular argumentation. With theory 'a kind of intellectual

sticking plaster', as Hyman (1994a) puts it, this sort of determined confrontation of our beliefs is necessarily painful.

The weaknesses of statistical work, regarded as it is by many as *the* scientific standard, are particularly worthy of emphasis. Determined insistence and effective intimidation are not confined to the community of variable-oriented researchers, but its members are particularly vociferous in their certainty. In the pursuit of research on the employment relation, numbers should not be afforded a mystical status, as if they were the product of divine intervention, the only indicators of the state of social reality. But this does not mean that numbers be reviled or feared. Numbers are not the solution but nor are they the problem. Rather, an approach to explanation which recognises the need to operate in various areas of social activity and at various levels of social structure seems vital. It is only in this way that the residual categories into which unexplained differences are washed up as flotsam in reductionist analysis can be investigated and elaborated.

In this way, the dangers of presuming too much can be attenuated. Otherwise the basis of social scientific enquiry is undercut by the hasty assignation of difference to some residual category. An approach to explanation which allows a number of levels of analysis, more than one 'middle range' in the terminology of Merton (1968), promises opportunities to unpack residual categories and explore black boxes, developing appreciation of causation both upstream and down, and indeed to confront the structure-agency dichotomy itself. Research which employs qualitative and quantitative material at various levels, seeking to understand variations across individuals, workplaces, companies, industries and nations seems to stand some chance of illuminating the causal processes which underlie the outcomes apparent in social reality. There is a clear relation of what is being suggested here to the realist approach of Bhaskar (see Craib, 1992; Hyman, 1994a). With such an orientation we may make better progress towards understanding how it is that the future will resemble the past (see e.g. Ayr, 1980).

Quantitative, variable oriented, analysis, seeking to deal with a large number of cases, can be extremely useful to social science. But such work may be pursued much more fruitfully if it is complemented with the insights which detailed case analysis may furnish. Case work can enrich appreciation of what that quantitative data which is available might express, and, moreover, nurture appreciation of what it does not express.

It can provide additional dimensions of evidence along which theory can be assessed. It promises a richer causal appreciation and a greater depth of explanation in recognising the potential relevance of a number of arenas of action, or levels of agency. It also allows the prospect of some unpicking of organisational or societal trajectories, of the patterns of mutual conditioning which can lend units of analysis the quality of organisms.

However, a combination of research methods is not sufficient on its own. More than a century ago, Sidney and Beatrice Webb (1897) commented on the bias introduced into the selection of facts by limited theoretical knowledge. A knowledge of alternative interpretations and of associations is critical in the context of observational equivalence:- to assess the significance of an alternative interpretation for the understanding of social reality, one needs to have knowledge of that alternative system of thought. In its absence, research communities' deeply held prior beliefs, infusing their social identities, straitjacket the sorts of analysis developed and the sorts of argument to which they are receptive. As Walter Korpi (1996) emphasises, an environment of theoretical pluralism is vital in attenuating prejudice.

Critically, the development of causal explanation requires that research is issue driven, rather than motivated, and thus circumscribed, by a particular research method (see e.g. Ragin, 1987; Franzosi, 1995; 2000). If we recognise the importance of establishing the causal mechanisms which give rise to particular phenomena without jumping too readily to conclusions about them, awareness is nurtured of differing means of theorising and interest provoked in developing research methods which reach beyond the conventional methods to draw on the strengths of differing research traditions.

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ⁱ Interestingly, this suggests the sense in which Hyman's (1987, 30) characterisation of employers' individual strategic choice as being a choice between 'different routes to partial failure' is misleading, or at least collapses together levels of analysis which might be better kept distinct. An agent's strategy cannot inescapably fail unless we regard the context in which it is forged, that left untouched by it which precipitates undesirable consequences, as being as readily subject to an agent's action as are the avenues which the agent's strategy pursues.

ⁱⁱ Franzosi (1995), in his monumental study of industrial conflict in Italy in the period 1950-78, concedes the weaknesses of his prediction of a global strike wave in the early twenty-first century from, as he puts it, 'one data point', the Italian 'autunno caldo' of 1969, which occurred some 50 years after the social conflicts of post-World War I Italy.