

Enterprise Bargaining: The Truth Revealed at Last

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Abstract

This paper is a selective review of recent Australian research upon enterprise bargaining and workplace industrial relations. It begins with a discussion of data collection methods, pointing out some strengths and weaknesses of survey, case study, and agreement text analysis methods. It then focusses upon two substantive issues to test the success of research. The first concerns the infrastructure for enterprise bargaining. Our conclusion is that research illuminates this topic, and reveals general unreadiness. The second issue is productivity performance and enterprise bargaining. We are far more sceptical that research has proved a relation between the two. We conclude with the observation that researchers may be on the wrong track if they try to quantify the effects of enterprise bargaining on productivity performance. The paper recommends that greater attention be given to change management programmes designed to increase competitiveness, and to the outcomes sought from these, rather than to productivity.

1. Introduction

Enterprise bargaining: is it an engine of productivity growth, or a threat to pay equity and macroeconomic wage stability? Since the late 1980s there has been a rising clamour of claims and counter-claims on these questions. Many of these claims are fuelled by the supposed merits of enterprise bargaining in other countries, or by deductions from economic theory. But

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these are imprecise guides. We cannot settle the important questions about enterprise bargaining sitting behind our desks.

Meanwhile the intensity of debate has been heightened by sustained reliance upon enterprise bargaining as a cog in the mechanism of official wage and industrial relations policies. Since the 'restructuring and efficiency' principle was introduced in March 1987, workplace negotiations to raise business efficiency have been at the centre of the industrial relations stage. Of course seven years of policy have witnessed a great deal of change – in the name, form and objects of wages principles, in federal and state industrial laws, in government and opposition policy platforms, and in the attitudes and policies of employers and unions. But consistent throughout the twists and turns of policy has been an underlying and widespread view that Australian enterprises can become more productive if they embrace enterprise bargaining.

Has this shift towards enterprise bargaining succeeded in achieving policy objectives? There has been considerable official and private curiosity on this point. As a result, seven years of policy have been accompanied by seven years of research. Those who were involved will recall the first tentative steps in this research – Jon Zappala's literature review for the Business Council of Australia (BCA) (which disclosed prior neglect by researchers); Ron Callus and Russell Lansbury running a seminar upon workplace research at Sydney University (following which Melbourne and Sydney Universities banded together to win an Australian Research Council grant to study workplace industrial relations); and Peter Brannen, Roy Green and Joe Isaac urging the need for a workplace survey akin to the British surveys (following which the Department of Industrial Relations (DIR) funded the first Australian Workplace Industrial Relations Survey (AWIRS)).

Research followed on a sufficient scale that it is now appropriate to take stock of its results. Should we accept or repudiate the claims of economic theorists? Has enterprise bargaining made workplaces more efficient? Has it yielded perverse results such as pay inequity or macroeconomic wage instability, or will it in the future? Are we any closer to knowing what makes managers and unionists tick if they take up the invitation to bargain at the enterprise level?

The purpose of this paper is to review some of these questions on the basis of a selective reading of workplace research conducted since 1987. We begin with a discussion of research methods, which limit, to a considerable degree, the conclusions that can be drawn from research. We then focus upon two substantive questions. First, does Australia possess the infrastructure to sustain a system of enterprise bargaining. Second, has such

bargaining delivered the results expected by policy makers? We close with an attempt to refine a question for future research. Was it false to expect enterprise bargaining to yield measurable productivity gains, and should its results be sought elsewhere?

2. Data Collection Methods

What we can claim to know about enterprise bargaining is limited by the type of research method we use and the quality of its use. Our concern here is with empirical methods for research, not a priori analysis. What methods have been applied by researchers in this field?

Textbooks on research methodology normally suggest that researchers should first formulate a question, and then select and apply an appropriate method to gather data capable of yielding an answer to that question. But most workplace research has not proceeded in this order. It has been inductive work. Data has been gathered first, and then scanned to unearth patterns which may, after the event, be fitted to questions significant to policy makers or theorists. Two significant examples of such retrospective analysis were the AWIRS research and the Melbourne/Sydney case study project (Lansbury & MacDonald, 1992). In neither case was the research design governed by any explicit hypotheses. Indeed the AWIRS study was designed under a formal prohibition against hypothesis formation and testing. Such is official disdain for academic theorising.

Inductive research has been governed by three principal methods of data collection – surveys, case studies, and ‘agreement text analysis’. Each type of data source has been mined extensively, and each has strengths and weaknesses which control the kinds of inference or legitimate research findings that are possible.

First are surveys. There have been two large sample surveys upon general workplace industrial relations. The first was conducted by the National Institute of Labour Studies (NILS) which surveyed managers in workplaces associated with the Business Council of Australia (BCA, 1989). The second was the AWIRS project (Callus, et.al., 1991). There have also been several more limited surveys examining, for example, the implementation of particular wages principles. Included here are the NILS/BCA survey of award restructuring (Sloan and Wooden, 1990) and the survey of enterprise bargaining undertaken by the Australian Centre for Industrial Relations Research and Teaching (ACIRRT) with the Department of Industrial Relations (DIR, 1993). There have also been regional surveys (Still and Mortimer, 1993) and other selective studies.

Surveys always seem dependable for gathering factual and attitudinal data which can yield generalisations about what managers and unionists do and think. But they have their limitations. One of these concerns timing. Few surveys offer scope for longitudinal analysis, and their findings may be coloured by special conditions at the moment they were conducted. Thus Sloan and Wooden's survey of the implementation of award restructuring in early 1990 probably recorded management optimism (because many managers were in the throes of negotiation at the time), rather than the delivery of results (which could only be anticipated) (Sloan & Wooden, 1990). Under those conditions the survey could not prove or disprove that award restructuring was working.

But there is a deeper problem to which industrial relations surveys are especially prone. This concerns the quality of factual data. Often it is polluted by response bias or attitudinal content without any disclosure (or perhaps even awareness) by the researcher. This problem is especially severe when surveys deal with a topic on which the respondent wishes to create a 'good impression'. An example might be the productivity performance of workplaces – an area where managers might seek to impress their inquisitor. It is extremely difficult to gather meaningful hard data upon workplace productivity. It is also difficult to extract subjective impressions upon productivity performance without being told what managers would like you to believe.

An example of this exact problem is to be found in some secondary analysis of AWIRS which purports to relate survey data on workplace productivity to a number of independent variables. One econometric study modelled the relationship between productivity (the dependent variable) and workplace unionism (several independent variables), while extracting the effects of several control variables that affect productivity (capital intensity and the like) (Crockett, Dawkins & Mulvey, 1993). But what is the quality of the productivity data on which all this is founded? Unfortunately, the data for the dependent variable (workplace productivity) was drawn from a subjective question. Managers were asked to rate the productivity of their workplace relative to the industry on a five point scale ranging from 'a lot lower' to 'a lot higher'. How reliable were responses to this question? First, no check was conducted to tell if managers had access to the productivity data for other firms which could inform this judgement. They may have been guessing. There was also strong evidence of response bias from management respondents. It is not possible for 87% of respondents to claim accurately that their workplaces' productivity was either the same or higher than for the industry as a whole. Clearly very few managers were prepared to confess to poor performance. The weakness of this

subjective data was such that it should not have been regarded as factual, and therefore should not have been correlated with other factual data. There was no acceptable productivity data that could be a basis for the study's conclusion that "... unions have a ... negative effect on productivity" (Crockett, et. al., 1993: 221).

Flora Gill's caution against trying to explain too much by quantitative analysis was well directed at AWIRS secondary analysis (Gill, 1993). Too few survey analysts allowed for the possibility of subjective impressions colouring responses to 'factual' questions. They showed little awareness of the divergence between the different parties' subjective assessments – a divergence clearly revealed in management and union qualitative ratings of employee/management relations (Callus, et.al., 1991: 293). They placed exclusive reliance upon management responses to questions that seemed purely factual in nature, when the 'facts' could be disputed. And if these researchers were quietly aware of the possibility of response bias, no effort was made to justify any assumptions that the effect of this bias would be trivial, evenly distributed, or not affect the reliability of correlation in some other way.

It is ironic that much AWIRS secondary analysis seeks to prise open key questions about the determinants of conflict or productivity in the workplace by assigning importance to practices (such as employee consultation) which enter the murky world of subjective impression, style, quality, and values at the workplace. That there are dangers in this terrain is amply proven by the self-imposed and clearly articulated reservations of some other econometricians. For example, Dawkins and Wooden say: "... with respect to obtaining an adequate assessment of managerial style and quality the AWIRS is deficient ... management surveys are not very good vehicles for obtaining information about management; instead this is better accessed via case studies or through data gathered from employees" (Dawkins & Wooden, 1993). Their study of industrial action is exemplary in confining its generalisations to the influence of workplace size, presence of unions and the like, while expressing regret that it was not possible to explore policy relevant variables like "grievance procedures, management – employee communications, employee involvement in decision-making, and the like" (Dawkins & Wooden, 1993: 183). It is probable that the main weaknesses of AWIRS (and other surveys) lies in probing the complex causal relationships behind improved workplace productivity, industrial conflict, and other such 'policy outcome' variables. These are, of course, the things we most want to understand. But that cannot excuse us overlooking the methodological difficulties connected with sole reliance on survey data.

Can case studies do any better? There has been a steady flow of major case study (observation/interview) projects since 1987, many of them directly concerned with probing enterprise bargaining (or award restructuring) as a policy tool to enhance productivity and efficiency.

Five of these projects (at least) involved multiple-case studies. These include six case studies of 'active bargainers' (Lansbury & MacDonald, 1992), twenty case studies of award restructuring at the workplace (Rimmer & Verevis, 1990), thirty three cases of workplace reform and award restructuring (Curtain, Gough & Rimmer, 1992), seven cases of retrenchment management and workplace reform (Buchanan, Campbell, Callus & Rimmer, 1992) and a further thirty plus cases of enterprise bargaining (DIR, unpublished, 1993). No review should neglect an early and insightful set of cases questioning the actual impact of second tier agreements (Frenkel & Shaw, 1989). Nor should we forget a large number of single-workplace studies (for example Frenkel & Shaw, 1991) or single-employer cases (such as Whyte, 1992). Finally, there has been an interesting secondary analysis of the first three 'multiple case' projects listed above (Nutku, 1993).

The main value of case studies is to generate new ideas whose general validity can then be tested by other means. To a degree the above mentioned studies have done this. However, case studies can rarely be used to support generalisations about the total population of workplaces. This limitation bedevils multiple case research as much as single case projects. As a result multiple case research tends to have a spurious breadth for which depth has often been sacrificed. Multiple cases do give the researcher a better chance to hit upon 'interesting' examples (for which more careful selection would have sufficed), and they do open up scope for inter-case comparisons from which new typologies can be founded. But the prospects for meaningful comparison are probably greatest when cases are carefully 'paired' (for control), and we have yet to see the results fully disclosed for such a project (Drago, Wooden & Sloan, 1992).

Despite these problems and shortfalls, the attraction of case study research remains. More than any other method, case studies offer two promises: to put the researcher in a position to discount bias (which infects much industrial relations data), and to unravel the complex 'core issues' of workplace industrial relations. What are the *actual* (rather than hypothesised) mainsprings of power, conflict, action and reform within the workplace? Is there really such a creature as workplace reform, or is it a mythical beast?

The case study worker's responsibility for answering these latter questions properly is enormous because primary data can rarely be checked. It is incumbent upon case study workers to use the method with profession-

alism and detachment – qualities not always evident in the past, and whose absence is not always disclosed in published results. Indeed the quality of Australian case study research has often been quite low, suggesting that earlier models of this kind of research could be rediscovered with profit (e.g., Batstone, Boraston & Frenkel, 1977).

A particular shortfall has been the failure of case studies to deliver conclusive findings on the ‘productivity’ question. Does enterprise bargaining contribute (in some complex fashion) to improved productivity? Most honest case study research cannot answer this question. First, workplace productivity data is often very poor. Second, even if good productivity data is collected at the workplace, it is still difficult to isolate or quantify the effects of the many independent variables which might cause changes in productivity. If we ask whether an enterprise agreement has a causal effect upon a productivity change, we cannot usually find out. Tidy control experiments, or realistic modelling (which might answer this question) are very hard to conduct in a dynamic workplace. Case studies sometimes substitute weaker claims of the axiomatic kind (if jobs are slashed and output remains stable then labour productivity increases), or the fragmentary kind (a specific reform produced a change in a single related performance indicator which in turn is linked to productivity in some way). But the best research on single performance indicators shows the complexity of realistic modelling (Deery & Hintz, 1989). Furthermore, such techniques have yet to be deployed convincingly by case study researchers on the wider question of the determinants of workplace productivity. To date, case study researchers have not thrown much light on the ‘productivity’ problem. Even a twenty-five year longitudinal analysis of one of the world’s most famous productivity agreements – Fawley refinery – failed to prove results (Ahlstrand, 1990).

The third data collection method we term ‘agreement text analysis’ – systematically recording and analysing the text of tribunal decisions, awards or agreements to describe (and then pattern) the formal rules governing the workplace. There have been several such projects in Australia beginning with an analysis of bargaining structure and the second tier (McDonald & Rimmer, 1988), labour market flexibility and the second tier (Rimmer & Zappala, 1988), the differences between enterprise and multi-employer awards (Rimmer, 1989) and the content of NSW agreements (Plowman & Favotto, 1992). More recently computer data-bases of enterprise agreements have been assembled by ACIRRT, the ACCI, and the ACTU in conjunction with the Monash Industrial Relations Key Centre. But the best working model of such a data-base lies across the Tasman (Harbridge, 1992).

Analysis of the text of agreements only records formal rules. It cannot tell you about their application, attitudes towards them, or even (except by inference) why these rules were made. Further, data bases are prisoners of their data sources. They will record only some formal rules. No data base (to my knowledge) incorporates rules set down in employee handbooks and the like. Data bases comprising registered or certified agreements will not include unregistered agreements (a problem overcome by Harbridge in New Zealand). Indeed, any tendencies towards informality in rule making tend to rot the foundations of a data base as a comprehensive source of information upon workplace employment regulation. Thus the ACIRRT/DIR survey of enterprise agreements tells us that certified agreements are only the tip of an iceberg which looks very different underwater (DIR, 1993).

Nevertheless, data bases have research value. Unlike surveys and case studies, they can be a dynamic indicator of changes in the extent and content of formal enterprise bargaining. Dynamic data of this kind should not be discarded as trivial by industrial relations researchers, especially when they remember to correlate formal bargaining with significant industrial relations variables such as union recognition and density (Harbridge, 1993; Clegg, 1976).

The main risk with decision analysis is to infer that 'words in agreements' imply workplace change (Rimmer & Zappala, 1988). As Frenkel and Shaw correctly pointed out, many written second tier agreements were not implemented. Similarly, formal award restructuring agreements were often not implemented, or were only implemented slowly and ineffectively (Curtain, Gough & Rimmer, 1992). Decision analysis needs to distinguish between the hard and soft tissue in productivity agreements. Most such agreements contain pay adjustments (which are honoured) and productivity enhancing clauses (which are implemented slowly, if at all). The soft tissue is this gap between words on paper and practice when translating productivity enhancement measures into action.

The truth about our data collection methods is that we probably know much less about enterprise bargaining and workplace industrial relations than we think we do. By inductive means we have gathered more facts than we could use and answered fewer questions than we ought. We have suffered from excessive reliance on case studies (especially multiple case studies). We have tried to correlate data that was not worth correlating (or at least was not designed for the purpose to which it was put). We have also placed too much reliance upon the text of agreements. At times we have drawn conclusions that were unfounded because of inappropriate data. But clearly seven years of research has not been wasted. What have we found out that was worth knowing?

3. Bargaining Infrastructure

Joe Isaac remarked that: “effective enterprise bargaining in pursuit of workplace reforms calls for a suitable infrastructure – management and union capacity to identify appropriate changes in work practices, technology, management and consequential training requirements; as well as the sophistication and skill to negotiate terms and conditions without undue industrial disruption” (Isaac, 1993: 23).

We use the term ‘bargaining infrastructure’ to refer to two things. First are the personnel, the union delegates and managers, who conduct workplace negotiations. Are they to be found in sufficient numbers, and do they have the appropriate skills and authority? Second we refer to bargaining units – the way unions and employers are grouped for bargaining. Do they negotiate nationally, for an industry or occupation, for a company, a workplace, or for part of a workplace? The two aspects of bargaining infrastructure are related. For example, in countries where negotiations are conducted at the industry level we are unlikely to find many skilled union and management personnel at the workplace (Clegg, 1976). Both elements of bargaining infrastructure can change, but it is unlikely that they will change very quickly. They seem to be characterised by a degree of institutional rigidity, founded in custom as much as rationale or resources. This rigidity has been a cause for complaint amongst those who like their policy changes to take effect quickly.

Our claim is that ‘bargaining infrastructure’ constitutes an important topic for workplace industrial relations research, and that we now know much more about this subject than we did seven years ago. However, not everyone agrees that this is an important topic. During the early days of AWIRS a theoretical paper was undertaken which expressed scepticism as to whether bargaining (and therefore bargaining infrastructure) were the proper focus of workplace research (Littler, Quinlan & Kitay, 1989). Such an approach smacked, it was claimed, of the now discredited pluralism of the Oxford School of the 1960’s, and was seen as too limiting to encompass the realities of the Australian workplace, especially non-bargaining phenomena or situations.

There are many possible rejoinders to this view (Zappala, 1990). First, it rests on a distortion of the views of the British pluralists, who were very interested in the tension between bargaining and non-bargaining. Second, it rests on a mistaken semantic quibble that ‘bargaining’ is a narrow, exclusive and limiting concept. But perhaps most important, it seeks to trivialise an important issue – whether unions can systematically sustain bargaining at the workplace. Harbridge’s work upon New Zealand shows what can be done when researchers explore the link between union strength

(membership) and a system of workplace bargaining. He draws a link between the adoption of a system based on enterprise and individual bargaining, the contraction of bargaining, and the decline of union membership (Harbridge, 1993). This link between union membership and bargaining level is postulated in Clegg (1976), has been noted by some Australian researchers (Peetz, 1990), and should now be recognised as an important policy problem in contemporary Australia.

The Australian union movement is now very concerned about bargaining infrastructure at the workplace, and in particular the capacity of unions to sustain joint regulation. This concern is based on a difficult dilemma. Seven years of centralised Accord policies show that it was a recipe for falling union density (Peetz, 1990; Crean & Rimmer, 1990). Yet the New Zealand experience suggests that a precipitate rush into decentralised bargaining only accelerates the decline of union density. Current ACTU policy is directed towards finding an alternative to these “damned if you do, damned if you don’t” scenarios. Can union reorganisation and the development of new services, skills and policies regenerate the union movement so that the effects of enterprise bargaining will be positive (wealth creation), rather than negative (destroying minimum award standards and bringing union density below ‘critical mass’ levels) (Ross, 1993; Watts, 1993 and Ogden, 1993).

Given the fashionable repudiation of pluralism, it is ironic that the best results of seven years workplace research lie in disclosing new information about bargaining infrastructure at the workplace. Before discussing these results, we will confess that they often elicit a long yawn from economists, management theorists, and sociologists who have no professional interest in unions and bargaining. Should industrial relations scholars show the same tendency to nod off, they have probably forgotten what their subject is.

So, if bargaining infrastructure is about (1) bargaining personnel, aptitudes and authority, and (2) bargaining units, what does the available evidence now tell us? The following points are not intended to be exhaustive.

- (1) The trend towards single-employer bargaining pre-dates 1987 (Brown & Zappala, 1993), which suggests (contrary to popular wisdom) that compulsory arbitration was not an impossible obstacle to its development (Rimmer, 1989), while leaving open the finer question of whether it created disincentives (BCA, 1989).
- (2) *Formal* bargaining units possessed structural characteristics that bore the imprint of distant origins and rationales – multi-employer awards fragmented along occupational lines (by occupational unions) (Plow-

man & Rimmer, 1992). Once more, there was evidence of scope for determinations to be customised to suit particular workplaces (Brown & Ferris, 1991).

- (3) While enterprise awards have evolved in some numbers (McDonald & Rimmer, 1988), they seem to be limited in employee coverage, concentrated in some industry sectors, fragmented along union lines, and are often confined to single issues (add-ons to awards) (Rimmer, 1989). This tendency towards 'add-on' bargaining at the enterprise is still evident in recent research on enterprise agreements (Labour Information Network, 1993).
- (4) Relatively few workplaces qualify as 'active bargainers' (14%), although they employ a significant proportion of workers (40%) (Callus, et.al., 1991). However, case study research suggests that 'active' bargainers may not be very active (Lansbury & Macdonald, 1992)
- (5) Union delegates are not thin on the ground (1:39 employees in unionised workplaces), but have neither the time, experience or training to undertake more than the marginal tasks that fall to them. Thus four fifths of delegates spend less than 3 hours a week on union duties, half had held office less than 2 years, three fifths had no formal training, and their major activities concerned individual grievances and problems.
- (6) Specialist workplace industrial relations management are no better equipped. Only 34% of workplaces have such a manager, their turnover rate is high (49% had held their current job less than 2 years), and only a minority are empowered to make decisions (40% of workplaces were deemed 'highly autonomous' in deciding workplace issues) (Callus, et.al., 1991).

Much more AWIRS evidence could be cited to substantiate the two key points: that bargaining units tend to be centralised and/or fragmented by occupation (despite formal scope to arrange things otherwise) *and* that there is a major shortfall in the incidence of trained shopfloor personnel (union and management) authorised and focussed to conduct enterprise bargaining on significant matters.

The obvious conclusion is that systemic enterprise bargaining is unlikely to be sustained in a meaningful way because many workplaces are not equipped to handle it. Every enterprise could, if necessary, handle its own employment matters. But 'bargaining' would be rare, and professionalism might be low. In this regard, it is worth noting that most managers in small

workplaces *don't want* the job of enterprise bargaining, and are content with existing multi-employer awards (Isaac, et.al., 1993).

One possible criticism of the data cited above is that much of it is out of date. There have been major developments since 1989/90 including award restructuring, union amalgamations, new legislation, a recession, and much else. Some of these were intended to impact on bargaining infrastructure, while others may have done so inadvertently. In any event, we can no longer assume that the conclusions drawn from AWIRS and other research of that time are quite valid today. However, there are strands of evidence to suggest that 'general unreadiness' is still a fair characterisation of most workplaces (Macklin, Goodwin & Docherty, 1993; Labour Information Network, 1993). In December 1993 the AMEU and FIMEE still accounted for 50% of a sample of 1,053 federal registered enterprise agreements – itself a very small number for two years of activity, despite the effects of 'framework agreements' in spreading the incidence of bargaining.

Another objection is that we have not directly answered the central questions about bargaining infrastructure. We have sketched patterns, but we have not analysed or demonstrated important causal relations. For example, we have done little to explore the link between *changes* towards a more decentralised bargaining system and union density. Our central question is whether the development of systemic enterprise bargaining reduces union density, or might be expected to do so? We don't yet know the answer to this for Australia. Indeed, it may be premature to research this question since we don't yet have systemic enterprise bargaining. Price's analysis of union growth distinguishes between periods of institutional upheaval and lengthy periods of consolidation, and remarks that different variables (and methods of analysis) are relevant to union growth in each of these phases (Price, 1991). Australia has not yet experienced institutional upheaval, at least on the scale that Britain and New Zealand have. We still have our old institutional framework founded on state recognition of unions, conciliation and arbitration, and multi-employer awards covering 80% of the workforce. For several years we have been in a state of nervous anticipation, waiting for change in our core institutional arrangements. But this has yet to happen. In the meantime, it is too soon to ask what effect major institutional change has had upon union membership, and it is less critical to explore the demographic and other variables that influence union density.

4. Results – Better Productivity or Something Else?

Perhaps the strongest claim made upon enterprise bargaining and productivity is to be found in a report of the BCA's Industrial Relations Study Commission which stated, "research suggested that the enterprise approach could yield average increases in the productivity of labour and capital in the order of 25%" (BCA, 1989: 8). This has been disputed (Frenkel & Peetz, 1990).

In 1989 the BCA was concerned with the performance gap between Australian enterprises and their overseas counterparts or competitors. By 1993 it was possible for the Study Commission to ask what had changed in Australia. What had been the impact of workplace reform upon Australian enterprises? Drawing from a range of case study findings and survey data (including surveys of BCA CEOs), the Study Commission concluded with some mixed messages:

- There is still a long way to go before Australian businesses match the best of their competition, despite significant recent improvements in productivity.
- Employee contribution and the management of employees are the factors most important to that goal.
- There has been a moderate improvement in those areas over the past four to five years.
- Award restructuring and other reforms have made a moderate difference but not necessarily in the areas most critical to the achievement of business objectives.

(Hilmer, et.al., 1993: 86-89)

The first and fourth of these propositions deserve some closer scrutiny.

The association between enterprise bargaining (under one of its several names), and productivity improvement *over the past few years* tends to be treated with some scepticism by industrial relations academics (Gardner, 1992; Brown & Zappala, 1993). As Brown and Zappala observe "the record on productivity is ... unclear" (Brown & Zappala, 1993: 81). Whether one explores the effects of enterprise bargaining or of unions upon productivity, the problems discussed above (in section 2.) recur. We do not have good survey data on productivity, and case studies encounter 'control' problems. It may be noted that the productivity outcomes of enterprise bargaining are no clearer in the USA or UK where there exists a much longer record of negotiation at this level (Bray, 1993; Brown & Zappala, 1993). We suspect that a good deal more research effort could be devoted to unlocking the secrets of the 'black box', but diminishing returns will soon be experienced.

The 'truth' upon this matter (at least in regard to systemic analysis based on large bodies of data) is that the institutional variable (centralism v decentralism) is hard to specify because of hybrid forms (a mix of the two) (McDonald & Rimmer, 1989; Buchanan, 1992; Gardner, 1992; Macklin, Goodwin & Doherty, 1993). Even if we had better productivity data, it is not clear we could make much sense of the 'enterprise bargaining side' of the equation.

There is a further reason why researchers may find it hard to quantify the relationship between enterprise bargaining and productivity outcomes. This is connected with the time needed for workplace reform initiatives to take effect. One clear finding from case studies upon award restructuring is that workplace reform takes a long time to produce results (Rimmer & Verevis, 1990; Curtain, Gough & Rimmer, 1992). Most productivity initiatives (with the exception of retrenchment) are heavy resource consumers (which means that management tend to implement in stages), are directed at slow change areas (e.g., retraining cannot be done overnight), depend upon attitude change (which further slows their impact, and must overcome resistance from many quarters (which makes results unpredictable). Given these facts, it is naive to expect demonstrable results overnight.

Turning to the BCA's fourth proposition, the interesting conclusion is that award restructuring and other reforms have not necessarily impacted on "the areas most critical to the achievement of business objectives". What are these areas, and what relationship (if any) do they have to productivity?

Our argument is as follows:

- (1) A purpose of enterprise bargaining or workplace reform is to improve competitiveness, rather than productivity (or labour productivity).
- (2) Survey evidence suggests that most large Australian firms seek to improve competitiveness in the product market by improving quality, production costs, and delivery time – variables which are contingently related to labour, and may not always be captured in simple input/output measures of productivity.
- (3) Enterprise bargaining is not always necessary, and is never sufficient to improve competitiveness.
- (4) Managers will (and do) prioritise other changes or techniques which impact more directly on competitiveness (for example, Total Quality Management, Benchmarking, Business Process Re-engineering, or Statistical Process Control) relative to enterprise bargaining – which has a contingent (often weak) relationship to these change management techniques.

- (5) Managers are not heavily constrained by unions, awards, or other industrial relations factors in the utilisation of such change management tools (or indeed in other kinds of change) (Callus, et.al., 1992: 339), but they may be relatively ineffective themselves in using these tools (MacNeil et.al., 1993).
- (6) The measures appropriate to any focus on 'competitiveness' are not productivity measures. They are more likely to be conventional business performance indicators (market share, market growth, return on investment, and so on) and particular measures related to change management initiatives (quality, service delivery, and the like).
- (7) Labour may be a partner in this focus upon competitiveness, a point that is pressed by the ACTU (and is applied in some Best Practice Program companies), but not all managers are ready to accept this.
- (8) At its best, enterprise bargaining can be a catalyst for the effective use of change management tools.

It is almost axiomatic in economics that national real wage levels or living standards only rise, in the long term, to the extent that productivity growth occurs. While this proposition holds force for national economies in the long run, it does not translate so easily into a comparable proposition for enterprises in the short run. Firms seek competitive advantage. They can do so in many ways, one of which may be to improve labour productivity in its conventional sense. However, we should not confuse competitiveness or competitive advantage with labour productivity by claiming they are the same thing when they are not. If enterprise bargaining is to help enterprises, it is probably appropriate for it to be directed towards improving competitiveness, a goal which need not entail productivity improvement. Conversely, if enterprise bargaining is intended only to improve labour productivity (labour units divided by output units), then it may not be very interesting to either management or unions. The major game lies somewhere else.

We are guessing of course. The truth is that we know very little at present about the *actual* relationships between competitiveness and productivity, or between enterprise bargaining, and change management (or indeed any activity of managers other than industrial relations managers). If industrial relations scholars are to open up what really goes on at workplaces, then it will become necessary to explore a lot of new terrain – strategy, marketing, accounting, information technology and the like.

5. Conclusion

Our review does not disclose many major truths about enterprise bargaining. Perhaps we can be accused of being too selective in the research we have examined and the research questions we have deemed important. It is clear that there are many aspects of enterprise bargaining we have not covered in our review. One significant area of neglect is the impact of enterprise bargaining upon equity – a subject on which a large body of high quality research is now being amassed. Another significant omission is payment systems. In this field the literature divides between normative discussion of performance related pay and reports of research findings on over-awards, overtime, and penalty rates, the latter having greatest value for those who are curious about Australian workplaces. But our omissions are justified on the grounds that our objectives are selective. The opportunity remains ripe for another annotated general bibliography upon workplace industrial relations because the literature has grown a great deal since Zappala (1988).

To summarise our main points, we are critical of the data collection methods used by researchers. While a range of techniques has been deployed, researchers have not always shown sufficient awareness of the limits and proper uses appropriate to each. The result is that the conclusions claimed by researchers probably often run beyond what is really justified by the data. One point deserves expansion here. It is widely thought that survey and case study data collection methods can be knitted together for optimal results. Several projects where this has been attempted come to mind (e.g., Sloan & Wooden, 1990; Rimmer & Verevis, 1990). In each case it is doubtful whether the two strands of research were properly integrated. There is room for development in this approach to data collection.

We hope that people will agree it is important to look at bargaining infrastructure. It will be odd if industrial relations scholars abandon interest in the relationship between unions and bargaining. We live in an era when institutional upheaval is anticipated, if not yet wholly real. The issues in that upheaval are very much to do with the role of unions in a decentralised system. They seem too important to ignore.

Our scepticism on the value of productivity measurement (by both macro and micro research) may overstate the difficulties. But one objective may have been served: to draw attention to the possibility that managers are not very interested in productivity (as economists define and measure it), and are chasing a lot of agendas which economists and industrial relations scholars have yet to grapple with. Unless these are understood, we may misunderstand the real dynamics of workplace change.

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