INDUSTRIAL CONFLICT IN BRITAIN

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Thesis submitted in fulfilment of the degree of Doctor of Philosophy

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May 1992

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Acknowledgements

I would primarily like to acknowledge the support, advice and encouragement of my two thesis supervisors Ben Knight and Mark Stewart over the last three years in particular, but also for their assistance more generally throughout the time that I have been associated with the Department of Economics at the University of Warwick. I would also like to thank a number of other individuals and institutions for the help they have given during the writing of this thesis. Pete Mitchell at Warwick provided practical assistance in accessing and interpreting the Workplace Industrial Relations Surveys. The ESRC Data Archive at the University of Essex supplied me with the Department of Employment Industrial Stoppages Data Tapes and Eric Roughley in particular deserves special thanks for his invaluable help in deciphering the tapes. In addition, the more recent efforts of John Elliott and Derek Bird of the Department of Employment in this respect have been particularly welcome. Thanks are due to them, and also to the Department of Employment for placing this information in the public domain. Lastly, I would like to thank Paul Geroski of the London Business School for the provision of the Census of Production data.

Financial support for the thesis was received in the form of a Rootes Scholarship for 1986/87 from the University of Warwick and from the Economic and Social Research Council under award number A00428724142 for 1988/90.

Finally, while I have received many suggestions during the compilation of this thesis from friends and colleagues too numerous to mention at the Universities of Warwick, York and Kent, I, alone, remain responsible for any errors.

Declaration

There are two chapters in this thesis that are the result of collaborative research. Chapter 5 was written jointly with my supervisor Mark Stewart and has benefited from comments received from participants at the EMRU Labour Economics Study Group Conference at the University of Kent, Canterbury in July 1991 and the European Association of Labour Economists 3rd Annual Conference at El Escorial, Madrid in September 1991. Chapter 6 is the product of work conducted jointly with my other supervisor, Ben Knight, together with Paul Geroski of the London Business School.

Summary

The economic analysis of conflict in Britain has previously concentrated on examining aggregate strike frequency. The thesis recognises the limitations of this approach and argues for the investigation of a broader definition of conflict and at a more disaggregated level. While weakly encompassing previous theoretical work, the principal objective is to establish the patterns and trends pertaining to wider set of measures of conflict in post-war Britain. The empirical investigation of these disaggregated dimensions of conflict and their inter-relationships appears to have previously received only very limited attention.

Following a critique of the extant theoretical and empirical literature, the first substantive chapter examines the traditional aggregate econometric models of strike frequency. These are shown to be unsatisfactory in a number of ways. The chapter then turns to the central issue of the procyclicality of strikes. It is shown that while the total number of strikes is only very loosely related to the cycle, strikes arising over the level of remuneration bear a much closer correspondence with the level of economic activity and this finding accords with many of the theoretical models that have been proposed for strike activity. The chapter concludes with an examination of a cyclical-political model of strikes within which the impact of the recent reforms in labour legislation is also investigated.

One of the central arguments of the thesis is that the emphasis on strike frequency is inappropriate. This is most clearly illustrated by the fact that while strike frequency fell by almost one quarter between 1980 and 1984, the incidence of strikes at the establishment level actually *increased* by 45%. An examination of the determinants of the incidence of conflict activity forms the basis of the second substantive chapter of the thesis. As a subsidiary theme, the complementary nature of strike and non-strike action is also explored.

The next chapter investigates the *ceteris paribus* differences in strike probabilities between the public and private sectors. While the levels of strike incidence and frequency appear to be much higher in the public sector, much of the divergence is found to be a consequence of differences in the characteristics of the two sectors. Additionally, when weighted by employment and/or union coverage, strike frequency is found to be lower in the public sector and, moreover, each of these strikes tends to be shorter and involve fewer workers.

The final substantive chapter looks at the impact of strikes on industry output and efficiency. The structure of the model is novel in that a production frontier is estimated without having recourse to an explicit functional form for the inefficiency component. This is due to the availability of a panel of data in which the fixed effects can be viewed as capturing both the inefficiency term as well as the industry fixed effect. A second stage estimation is then used to identify each industry's level of efficiency. While strikes do not appear to reduce output in aggregate, there is some evidence to suggest that those industries which incur a large number of short strikes do have their output significantly disrupted. This loss of output also serves to make these industries less efficient in general.

Thus a major conclusion is that a disaggregated approach is necessary in order that the multi-dimensional nature of conflict and the sectoral diversity in the incidence of industrial action can be investigated in a satisfactory manner. Any new theories of conflict will need to encompass the empirical findings of the thesis.

to my family and friends

CHAPTER 1

Introduction

1.1. Introduction.

The analysis of industrial conflict and its relationship to bargaining and wage setting is of major importance in developing an understanding of how labour markets operate. However, this area of study has been rather disregarded, particularly research pertaining to conflict activity in Britain; there is no generally accepted theory and extant empirical studies are of variable quality. That there have been significant changes in the patterns of industrial conflict in recent years can be in little doubt, but the nature of these transitions and their implications have not been studied in any depth. This thesis aims to redress these deficiencies in the literature.

Before the nature, causes and consequences of these changes can be revealed, an acceptable methodological framework within which conflict activity can be examined needs to be defined. Industrial conflict is multi-dimensional and yet the existing literature is dominated by studies of strike frequency. In part, this concentration on the number of strikes was due to limited sources of data. Such difficulties have been at least partially alleviated with the compilation of the Workplace Industrial Relations Surveys, and the provision by the Department of Employment of the Industrial Stoppages Data Tapes which contain information on every strike recorded in Britain. These two sources allow a more extensive set of measures of conflict to be defined. A number of different dimensions of industrial conflict are considered in the following chapters of the thesis and, where appropriate, these are compared and contrasted with the traditional measure of strike frequency. One further weakness in the literature is that it is mainly descriptive, focussing on when, and under what circumstances, strikes are more likely to occur. Thus the final part of the thesis examines the costs and consequences of strikes at a disaggregated level. A brief outline of each of the subsequent chapters of the thesis is provided in section 1.2.

1.2. An Outline of the Thesis.

Chapter 2 presents an overview of the theoretical literature on strike activity together with a selective review of the empirical studies. This review reveals several significant deficiencies in the body of research relating to industrial conflict; the adoption of models of strike activity which are derived under assumptions inappropriate for the system of industrial relations that exists in Britain is particular unwelcome, as is the dominance of aggregate studies of strike frequency. The remaining four substantive chapters of the thesis seek to address these issues in particular by investigating the determination of more disaggregated measures of strikes and other forms of industrial action.

Chapter 3 begins by examining the aggregate measures of stoppages published by the Department of Employment in the Employment Gazette since these have provided the basis for most of the previous studies of industrial conflict in Britain. However, there are several evident weaknesses in these data, in particular, in their extreme sensitivity to large strikes. The chapter then turns to a reassessment of the traditional econometric time-series models for post-war strike frequency in Britain. While, in general, these are unsatisfactory in their empirical performance, this does not preclude the claims for cyclicality in strike frequency. This issue is investigated explicitly in the chapter, while the importance of institutional considerations such as the recent changes in labour law is also highlighted.

Chapter 4 is a study of the incidence of conflict at the establishment level[1] since this

[1]. That is, the proportion of establishments incurring industrial action in the sample period.

seen to differ in important ways from strike frequency as a measure of the level of, and changes in, conflict activity. The importance of the evident transitions in the structural, legislative and economic environment in the 1980s are discussed, and the net changes in conflict incidence are apportioned to changes in industrial composition and changes in bargainers' behaviour. The relationship between strike and non-strike activity provides an subsidiary theme in this chapter.

Chapter 5 considers the principal sectoral differences in strikes in its examination of the public and private sectors. *Ceteris paribus* differences in the incidence of strikes are computed, together with employment weighted measures of strike frequency. Using a combination of strike frequency and establishment-level strike incidence reveals the importance of examining a range of dimensions of conflict rather than focusing on one specific measure.

The notion of 'incidence' being used in the thesis perhaps needs emphasising and clarifying at this juncture. Some researchers would argue that the measure of strike activity which is of primary interest is strike incidence expressed as the number of strikes per bargaining group[2]. Since this is unobservable in practice, one interpretation of the numerous published studies on strike frequency is that this represents an imperfect approximation to the 'ideal' measure of strikes. Thus, according to this view, the probability of a strike at the establishment in the previous year (as reported in chapters 4 and 5) could be interpreted as a further, but still imperfect, proxy for the measure of fundamental interest. However, the thesis also argues for a wider interpretation to be given than this affords, for both previous studies and for those presented here. Different measures of strikes are of greater or lesser interest according to the purpose in hand. Thus, while the performance of the bargainers is probably best illustrated by the measure proposed above, workplace managers may be more

2. Note that this cannot simply be interpreted as a probability as some have suggested since it is not constrained to lie in the (0,1) interval.

concerned with the proportion of establishments incurring strike action, while central government policy makers may only be concerned with the total number of strikes. The measures of strike 'incidence' investigated in the thesis should best be interpreted in this manner, that is, as complementary, but *different* measures of strike activity rather than purely as proxies for the number of strikes per bargaining group. Furthermore, the thesis is not only concerned with strike activity, but also with non-strike activity, and the relationship between these different manifestations of conflict.

In addition, it should be noted that the thesis is concerned almost entirely with the frequency-incidence axis rather than the size or duration of stoppages. The latter in particular deserves much greater attention than has been afforded to date. Some preliminary work has been done on the analysis of strike durations utilising the Department of Employment Industrial Stoppages Data Tapes. However, due to the unavailability of the 1981 tape and data for 1984 onwards, this material is excluded from the thesis. A summary is presented in Dickerson (1988, 1989).

While chapters 3, 4 and 5 investigate different measures of stoppage starts, chapter 6 provides a distinct contrast in its examination of the costs and consequences of strikes for industrial production and efficiency. A novel approach is utilised which extends the traditional stochastic production function framework to explicitly investigate relative efficiency as the distance 'inside' the production frontier and the extent to which this is affected by high levels of strike activity.

Finally, chapter 7 presents a summary of the major findings of the thesis together with some brief conclusions.

CHAPTER 2

Industrial Conflict in Britain: an Overview and Critique of the Theoretical and Empirical Literature

2.1. Introduction.

The purpose of this chapter is to provide both context and motivation to the thesis. The primary objective, which derives from the appraisal of the empirical evidence and the extant literature presented below, can be usefully stated at this juncture. The thesis aims to redress some of the limitations and omissions in the plethora of studies pertaining to industrial conflict in Britain. In particular, it seeks to identify and establish the relevant 'facts' about conflict activity that successful theoretical models must be able to encompass and explain. In so doing, it demonstrates that the historic dominance within the literature of studies which examine aggregate strike frequency is myopic and arguably inappropriate. More recent studies in the US and Canada in particular have recognised this limitation and have investigated strike incidence and strike durations. Other dimensions of strikes, as well as non-strike activity, are also important in Britain and certainly warrant much greater attention than has been afforded to date. Finally, the level of aggregation typically employed in most previous studies unfortunately serves to obscure many of the important distinctions in patterns of conflict activity between industrial sectors and industries, and, moreover, mitigates against the development of prescriptive and practical policy implications. Thus there is a need for a more disaggregated approach to the analysis of conflict activity.

The remainder of this chapter establishes these general conclusions and suggests several primary areas for research, some of which form the bases for subsequent chapters of the thesis. Section 2.2 reviews the principal theoretical models proposed, many of which originate in North America. Given the rather different system of collective bargaining and industrial relations which dominates the US and Canada (for example, and perhaps in particular, with regard to contract formulation), the appropriateness of many of these models to strikes in Britain is questionable. Partly due to this and additionally because of deficiencies in the data available on strike activity[1], many of the theoretical models do not have empirical counterparts for British strike activity. Thus while the overview of the empirical literature in section 2.3 concentrates principally on Britain, reference is made to other countries (notably North America) for an assessment of some of the models as well as for comparison and contrast. Several fundamentally important areas for new research are identified following the examination of the existing literature in sections 2.2 and 2.3 and these are highlighted within the discussion. Those on which the thesis concentrates are briefly summarised in the final section of the chapter.

2.2. An Overview of the Theoretical Literature.[2]

Models of strike activity have concentrated on attempting to explain why strikes might occur. At first sight, a strike must be a Pareto inefficient outcome since both parties could be made better off by choosing the same settlement position but without undergoing the strike. Kennan (1986, p.1091) calls this the 'Hicks Paradox', since Hicks was the first to state this basic result, albeit, only implicitly. Many models have attempted to explain this apparently non-rational behaviour by individuals and/or unions and firms. Much of the early theory was weak and fairly unsatisfactory, although more recent developments represent some considerable improvement. These later models consider a strike as a rational response to a lack of information by one of the parties involved in the bargaining process.

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1. The aggregate stoppages data for the UK is described and appraised in chapter 3.

2. There have been several recent reviews of this literature, including Hirsch & Addison (1986), Kennan (1986) and Sapsford (1990).

Given its origins, most discussions of strike behaviour begin with a reiteration of the well known Hicks (1963) model in which the rates at which the employer and union make concessions in wage bargaining are positively related to the expected duration of the strike. Hicks noted that if both parties were equally well informed about these 'concession curves' and the point at which they intersect, then a strike will not occur since this settlement could be reached without incurring the costs associated with strike action[3]. The existence of the majority of strikes in this model is thus attributed to,

> "... the result of faulty negotiations... Any means which enables either side to appreciate better the position of the other will make a settlement easier; adequate knowledge will always make a settlement possible." (Hicks (1963, pp.146-47))

The only time that a strike may be advantageous according to Hicks was if the union was concerned with establishing or maintaining the credibility of the strike threat;

> "Weapons grow rusty if unused, and a Union ... will embark on strikes occasionally ... in order to keep their weapon burnished for future use." (Hicks (1963, p.146))

The eventual settlement position and the way in which this might be attained was not examined by Hicks in any detail. However, a class of bargaining models has emerged which seeks to examine the way in which two (or more) parties might reach a settlement when there are net gains to be made by co-operative behaviour. Clearly, some consideration of the nature of the collective bargaining process is important for an understanding of why it sometimes fails to reach a settlement, but it is apparent that many of these game-theoretic models are highly abstracted and bear little relation to the 'real world'. Following the pioneering work of Nash (1950, 1953), such models have been developed to a high degree of sophistication to include notions of

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[3]. That the occurrence of strikes is sub-optimal is made more explicit in the reinterpretation given to Hicks' model by Comay & Subotnik (1977).

commitment, time and, in particular, imperfect or incomplete informatio:[4] Ultimately, however, these models have serious deficiencies as explanations of conflict activity. Although they are able to provide a determinate solution to the bargaining problem, this is at the cost of implausible axioms and unreasonable assumptions. Moreover, experimental tests conducted under strictly controlled conditions (a far cry from the practical realities of workplace negotiations) have shown that the models have some fundamental weaknesses in terms of predicting actual bargaining behaviour (see, for example, Roth & Malouf (1979), Malouf & Roth (1981) and Roth & Murnighan (1982)). Finally, and most importantly, these models typically examine the process by which the parties reach a *settlement*, and thus explicitly preclude strikes taking place. Even those models specific to union bargaining[5] employ particular solution concepts which fail to encompass the possibility that a settlement may not be reached. The role of threats and counterthreats in the process of reaching an agreement is also neglected.

In rather dismissive tones, Hamermesh (1973) suggests that

> "Bargaining theory contains few very interesting propositions that can be tested empirically." (p.1146)

While this is debatable, of greater significance is that those models which do permit a breakdown in bargaining and allow a strike to occur invariably have to assume non-rational behaviour by at least one of the parties. Such an assumption is clearly unsatisfactory. In short, it is simply not possible to construct a bargaining model in which both sides behave optimally but the result is a Pareto sub-optimal disagreement.

Thus it can perhaps be concluded that the primary importance of the pioneering work

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4. For selective surveys see, for example, Roth (1979, 1985), Myerson (1984) and Binmore & Dasgupta (1987).

5. For example, McDonald & Solow (1981), Oswald (1985) and, for a recent review of this class of models, Ulph & Ulph (1990).

of Hicks was to emphasise the role of *information* in bargaining; strikes in his model arise from imperfect or incomplete information which results in miscalculations by one or both parties. Almost all of the notable subsequent models have accentuated this informational aspect of Hicks' work, the best known of which is that of Ashenfelter & Johnson (1969).

2.2.1. The model of Ashenfelter & Johnson.

The publishing of the paper by Ashenfelter & Johnson (1969) marked a watershed in the economic analysis of strike activity for two principal reasons. Firstly, it made explicit the role of information in the bargaining process and thus can be seen to provide the link between the seminal work of Hicks and the more recent imperfect and asymmetric models discussed below. Secondly, it provided the first multivariate empirical analysis of the determinants of strike activity, while being grounded in microeconomic theory. It heralded a plethora of similar studies, many of which modified and extended Ashenfelter & Johnson's model, and thus it deserves special consideration here, especially given the volume of criticism that it has attracted.

Following Ross (1948), Ashenfelter & Johnson postulate the existence of three parties in collective bargaining; management, union leaders and union members. They assume that, although the union's leaders are better informed about the firm's ability to pay a wage increase than the union's members, there will be occasions when they will prefer to take a strike in order to maintain their credibility, rather than settle for a lower wage than the rank and file expect. Strikes in this model therefore serve to deflate the (unrealistic) expectations of union members. The firm, having set a target wage and a strike length which it is willing to accept (in part this is a function of the concession rate of the union), simply waits until the union concedes sufficiently. Presumably, workers' expectations fall during the duration of the strike because they believe that the firm does not engage in bluffing. Whatever the rationale (and none is provided by Ashenfelter & Johnson), the outcome is modelled as a downward sloping union concession schedule similar to that of Hicks.

The model can be easily summarised in the following fashion[6]. Let y_0 be the minimum wage increase acceptable to the union membership without striking, and y_* the lowest conceivable wage increase (which could be negative). Then the union's downward sloping concession curve can be written as

$$y_s = y_* + (y_0 - y_*)exp(-\alpha s) \tag{2.2}$$

where α is simply the rate at which the wage increase, y_s, decreases during the length of the strike s. The firm simply seeks to profit maximise subject to this concession schedule. Its optimal strategy can be shown to be to set a target wage, y_T, which will be a weighted average of the maximum increase the firm can pay (that is, the rate at which the firm just breaks even) and the minimum acceptable increase to the union y_*, the weights depending on the concession rate of the union and the cost of waiting to the firm.

If $y_0 \leq y_T$, then the firm grants the wage increase y_0 and no strike occurs. This is shown in the top panel of figure 2.1, which depicts the profit maximising position of the firm as being on the $s = 0$ axis with profitability π_0. However, if $y_0 > y_T$ then a strike occurs of length s_+, with an eventual wage increase of y_+. This combination yields a profit of π_+ which is greater than that obtainable without a strike, π_0, as shown in the bottom panel of the figure.

While a discussion of the difficulties in formulating an appropriate empirical specification from Ashenfelter & Johnsons' model is reserved for section 2.3, their theoretical framework warrants some attention given its prominence in the literature. The model has been extensively criticised in this particular aspect on a number of grounds by several authors, but perhaps most vehemently by Shalev (1980). Firstly,

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6. This paragraph draws heavily on Hirsch & Addison (1986) and Kennan (1986).

strikes are initiated and maintained exclusively by unions, in contrast to the models of Rabinovitch & Swary (1976) and Siebert *et al* (1985) who employ a symmetric argument to attribute this role exclusively to firms. In their models, unions wait (on strike) for intransigent management to increase their wage offers. Both of these extreme views of strikes are clearly untenable. Secondly, since the union's leadership is essentially passive, there are in fact only two active participants, management and workers. A genuine tripartite model with union leaders' interests differentiated from those of their members is developed by Swint & Nelson (1978, 1980). Thirdly, the 'bargaining process' is modelled as one in which the firm simply profit maximises subject to a given union concession schedule, and thus there is an absence of the strategic interaction which characterises the bargaining process. In this important sense, Ashenfelter & Johnson is not a bargaining model at all. Finally, and most crucially, there is an explicit asymmetry in the treatment of the parties; firms have perfect knowledge and engage in optimal maximising behaviour, while unions act irrationally and are faced with worker ignorance[7]. The notion that both sides may concede in reaching a settlement is not considered by Ashenfelter & Johnson.

Two central themes can be identified in the literature of the last two decades, both stemming from the informational considerations of Hicks and Ashenfelter & Johnson. The first has given rise to a class of models based on imperfect information, in that one or both sides are uninformed about the position of the other, and these models are discussed in the next sub-section. The second set of models is based on asymmetric or private information held by one party (typically the employer); these are examined in sub-section 2.2.3. An alternative view, which has a long history, is provided by the institutional models of strikes. These are briefly considered in sub-section 2.2.4.

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7. Note that the strike outcome (s_+, y_+) in figure 2.1 is Pareto sub-optimal since each party could reach a better position without making the other worse off. Of course, the optimal settlement entails a strike of length $s = 0$; Pareto optimal outcomes are all on the vertical axis.

2.2.2. Imperfect Information Models of Strike Activity.

In the most literal extension of Hicks' work, Mauro (1982) suggests that if different factors are considered by each party when deriving their own 'concession schedule' and their perception of the other party's schedule, then possibilities for miscalculation about the shape and location of these concession schedules can easily arise. As an example, Mauro proposes that firms may use product prices in determining labour demand, while unions will use consumer prices in evaluating the wage and labour supply. He explicitly models these potential misperceptions by each party to negotiations within the Hicksian concession curves framework and shows that strikes will be more common when this information is more costly to obtain. However, why established negotiators should continually and systematically utilise the wrong information in this manner is not made clear.

In a similar vein, some authors have attempted to explicitly model the occurrence of strikes as 'accidents' or mistakes, in that due to imperfect information, there is a non-zero probability that negotiations may fail. Siebert & Addison (1981) treat strikes as being analogous to road accidents, in the sense that,

> "... although any single accident is unforeseen, the probability of having an accident is foreseen and is a consequence of rational choice." (p.392)

Time costs are fundamental; accidents occur because information is not costless and the parties to the bargain cannot negotiate indefinitely. Where this information is cheaper and/or easier to obtain, the probability of a settlement within the negotiation time available should be higher. Addison & Siebert also show that higher potential losses from a strike have to affect both parties if they are to negotiate for a longer period and hence reduce the probability of a strike.

That the likelihood of a strike should be inversely related to the cost of the strike to both parties is made most explicit in the 'joint-cost' models formulated by Reder & Neumann (1980) and Kennan (1980). Strikes in these models arise from gaps in the 'protocol' which is designed to cover most potential, especially costly, conflict situations. Not all eventualities can be covered however, but essentially strikes should be less likely when they are most expensive. The theory has some attractive attributes; it avoids the assumption that workers or firms behave irrationally, and circumvents the difficulties associated with establishing how any settlement is reached. It also yields predictions as to the determinants of strike activity; any factor influencing its cost is likely to be important. However, as noted by McConnell (1987), this can easily be interpreted as a weakness of the joint-cost models:

> "While this theory can be tested empirically, the cost of the strike would presumably matter in almost any economic theory of strikes." (p.3)

In these joint-cost models, the bargaining process itself is assumed away[8]; rather, assumptions simply are made as to its outcome. Strikes are still sub-optimal, however, since even if a contingency arises which is not covered by the protocol, there is no need for there to be a strike; rational bargainers would still reach a settlement. A genuine attempt to develop a bargaining model under imperfect information is made by Kaufman (1981); however, he still requires myopic behaviour by at least one party and it is not clear why bargainers should systematically exaggerate the other side's position as he claims.

The papers by Mauro (1982) and Reder & Neumann (1980) and Kennan (1980) can be seen to be complementary; while Mauro suggests that strikes should be more common when information is costly to obtain, the joint-cost school posits that costly strikes should elicit better information flows. Both have found some support in the empirical literature; this is discussed in sub-section 2.3.3 below.

2.2.3. Private (or Asymmetric) Information Models of Strike Activity.

It is this relatively new class of models that has provided a consistent rationale for

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8. That is, it is treated as a 'black-box'.

strikes for the first time. These theories show that under asymmetric information[9], strikes can be *ex ante* Pareto optimal. The models are typically non-cooperative games, and include Hayes (1984), Morton (1983), Fudenberg & Tirole (1983) and Tracy (1984, 1987) among others[10].

Representative of this school, Hayes (1984) considers a situation in which a firm facing a downward sloping demand curve has more information about the state of the product market (and hence profitability) than the union. The union designs its wage-strike proposals to be incentive compatible; that is, strike activity is greater when the firm offers a low wage claiming that the value of the rent is low. There is no incentive for the firm to claim that the rent is high and offer a high wage if the rent is in fact low, and hence the union does not strike when offered a high wage. However, if the union does not impose costs on the firm when it is offered a low wage, the firm is always better off offering a low wage, irrespective of its true profitability. Thus unions use stoppages to reveal information about the state of the market and, in this manner, strikes can therefore be rationalised as an *ex ante* efficient bargaining tool.

There are two central predictions of such theories. Firstly, strike activity should be negatively related to (the unobserved component of) profits or rent since this will proxy the private information held by firms. Secondly, strike activity should be negatively correlated with the wage settlement given the form of the incentive compatible wage-strike proposals of the union[11]. Note that the first prediction is not incompatible with the apparently procyclical nature of strike activity, since it is the firm's position relative to comparable firms or the economy more widely that is the

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9. That is, unequal access to the <u>same</u> information, in contrast to the imperfect information models which consider the case in which bargainers utilise different information sets.

10. In contrast, Kennan (1986, pp.1105-1112) describes a cooperative model of bargaining.

11. In his signalling model, Card (1990b) also derives some comparative statics for the effects of changes in the mean and variance of the profitability of the firm.

relevant factor[12]. However, the most significant contribution of these models is the predicted negative correlation between negotiated wages and strike activity; if strikes were accidents, such a relationship would not exist. It is this *observable* phenomenon that provides the testable prediction on which several recent North American empirical studies have focussed. These are discussed in sub-section 2.3.4 below.

It should be noted, however, that this school of leader-follower models in which the union always leads has a fundamental weakness. As Kennan (1986) remarks,

> "... since the firm has an informational advantage, it would be more natural to let the firm lead. If the firm is allowed to lead, however, it will set the wage equal to the union's reservation wage (regardless of the state) and the union can do no better than to accept, so strikes will not occur." (p.1105)

Thus, once again, the models are flawed by their unequal treatment of the two parties. In particular, if the union possesses some private information (such as its minimum acceptable wage increase), then strikes could be used in a similar manner by the firm in order to infer this information. The model would then predict a *positive* correlation between strikes and wages, equivalent to Hicks' upward sloping employers resistance curve (McConnell (1989)). Unfortunately, models combining private information on both sides would therefore appear to give ambiguous predictions as to the relationship between strikes and wages.

2.2.4. Institutional Models of Strike Activity.

There are, of course, also a large number of largely 'non-economic' or institutional models of strike activity, the most notable of which include Ross & Hartman (1960), Snyder (1975, 1977) and Kerr & Siegel (1954). Earlier models of this school concentrated on examining the hypothesis that strikes, as an expression of conflict, reflect the subordination of workers within industry and within society more generally. Ross & Hartman attempt to explain the 'withering away' of the strike (as

12. If the union observes that the general economy is doing well then it may expect the firm to be similarly successful. If, in fact, the firm is doing relatively poorly, then a strike may result (Hayes (1984)).

they then saw it) in relation to the development and maturing of the collective bargaining process, unity and centralisation in the labour movement, and political stabilisation. Snyder stresses the importance of organisational and political factors. In particular, he finds that the institutional setting, especially regarding union membership, is important in explaining aggregate strike activity in the UK, and this is consistent with the findings of Shorter & Tilly (1974) for France. Kerr & Siegel, in their international comparison, find some support for their well known and much criticised 'isolated mass and integrated group' hypothesis. More recently, Naylor (1987) has developed a 'social custom'[13] model to explain strike support and the lack of free riding when a strike is called; even though it entails a loss of earnings, the individual worker may still strike if the loss of reputation from disobeying the custom is sufficiently high.

While these institutionally based models undoubtedly have features important to the successful interpretation of industrial conflict, their failure to ascribe any role to economic factors remains a major weakness and tends to limit their applicability to specific industries under particular systems of industrial relations. However, they do serve to highlight several of the relevant institutional considerations that have typically been neglected by economists, and therefore rather greater consideration of these models seems warranted.

Thus it has been seen that there are a large number of competing theories which purport to explain the occurrence of strike activity. More recent attempts which consider informational asymmetries between rational bargainers seem most satisfactory, although further modifications and developments are still required. While these models represent some advance in the study of strike activity, their scope

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13. See Akerlof (1980) and Romer (1984) for an exposition.

is often rather limited, and they clearly still omit several important aspects of the collective bargaining process. In addition, their emphasis (and dependence) on the role of fixed term contract negotiations, together with contract expirations, limits their applicability. Moreover, given the large differences between the system of collective bargaining that operates primarily in North America and that which is dominant in Britain, further modifications to the theory are necessary before its utilisation in studies of strikes in Britain. While American employees enter into bargaining only at the end of their (often three year) contracts, their British counterparts are more regularly involved in negotiations and are not faced with contract expirations. Additionally, it is clear that patterns of workplace bargaining and unionism differ substantially between the two countries. Finally, the notion that strikes might be used to develop a reputation for 'toughness' in subsequent bargaining rounds, or that management may be similarly intransigent as an investment for future negotiations, has yet to be formally incorporated in any of the models, and they would also benefit from being augmented by relevant institutional and structural considerations as suggested in sub-section 2.2.4. However, it is this new class of micro-level bargaining models that seems the most promising in that it provides, for the first time, credible theoretical models with empirically testable implications. As will be seen below, many of the earlier models have yet to be adequately assessed because their hypothesised determinants of strikes have extremely poor observable counterparts.

2.3. An Overview of the Empirical Literature.

In order to evaluate the large number of theories which have been proposed to explain strike activity, it is essential to discriminate between them empirically. Unfortunately, attempts to test the models in this manner have been largely unsatisfactory because many of the theoretical determinants of strikes have to be inferred from rather indirect proxies; concession rates, relative bargaining strengths, negotiators' perceptions, bargainers' expectations and strike costs are all unobservable phenomena to the econometrician. Consequently, one of the major weaknesses in the literature in this area is that the different theoretical models tend to yield very similar empirical specifications, and, as a result, model discrimination is poor. While not attempting a complete review, this section describes the general patterns that have been established. Certain key contributions serve to highlight the central findings and the difficulties that have been encountered. It is these studies that are emphasised in the following discussion.

Perhaps the main conclusion to emerge from the numerous studies is that strike frequency varies procyclically, although not all studies support this finding[14]. However, this pervading impression does serve to identify a major discrepancy between the theoretical models and their empirical implementations; many of the applications are little more than a descriptive relationship between strike frequency and a set of macroeconomic variables and thus fail to explicitly test the microeconomic models on which they are supposedly based. Indeed, the earliest studies of strike activity were simple descriptions of the relationship between strikes and the business cycle. These are examined in the next sub-section, while the large number of econometric models which followed the publication of the seminal study by Ashenfelter & Johnson (1969) are appraised in the following three sub-sections.

2.3.1. Cyclicality and Seasonality in Strikes.

The economic analysis of strike activity has a long history and originates in the late nineteenth century with Bevan (1880) who studied British strikes from 1870 to 1879. A useful summary of many of these early studies can be found in Kennan (1986). That strikes exhibit strong seasonal patterns has long been established. Knowles (1952) found similar patterns for both the UK and the US for the inter-war period, with UK strike frequency highest in May with a secondary peak in October and a

[14]. There is also an important methodological weakness in many of the studies which claim this to be their conclusion. This is discussed by Kennan (1986, p.1120) and in further detail in sub-section 2.3.2 below.

trough in December (a similar finding to that of Yoder (1938, 1940) and Tracy (1986) for the US, and Geare (1972) for New Zealand). Knowles' analysis has been extended by Sapsford (1975, 1982) who shows that the seasonal pattern in strike frequency is stable throughout the period 1893 to 1971. This seasonality in strikes undoubtedly reflects the seasonality in the determinants of strikes, and thus has been attributed to the structure of the wage round, the seasonality in production in certain industries, and the timing of holidays (during which, it is argued, it is in the best interests of workers to maintain their income flows). Somewhat curiously, despite the strong evidence for stable seasonal patterns in strikes, the Department of Employment has never published stoppages data on a seasonally adjusted basis.

The other major strand in the literature prior to Ashenfelter & Johnson (1969) was the examination of the cyclicality of strike frequency. This literature dates from Hansen (1921), and includes Griffen (1939) for the US and Gomberg (1944) and Knowles (1952) for the UK. These studies demonstrate a positive correlation between strike frequency and some measure of the business cycle such as the rate of change of prices or wages or, with a negative correlation, the rate of unemployment. Stronger results were obtained for the US using the National Bureau for Economic Research methodology for the analysis of business cycles developed and expounded by Burns & Mitchell (1946). These studies include Jurkat & Jurkat (1949), Rees (1952) and Weintraub (1966), and are summarised and extended through to 1980 by Kennan (1986). Taking the average over all the US business cycles from 1915 to 1980, the latter finds a 'perfect fit' between the nine phases of the business cycle and phases in the cycles of strike frequency. Although the Burns-Mitchell method has little statistical rigour, the degree of consistency in this result is still highly surprising, as Kennan notes.

A similar analysis of Canadian strike frequency from 1946 to 1983 has been performed by Harrison & Stewart (1990). They find that

> "[t]here is little evidence of any strong fluctuations in the number of strikes until about 1960, but thereafter there is a reasonably close correspondence between the frequency and the business cycle." (p.5)

However, when the strikes series is disaggregated by issue and contract status[15], Harrison & Stewart find that this evidence for pro-cyclicality in strike frequency derives principally from non-contract strikes. For contract strikes, there is no evidence of cyclicality in either wage strikes or non-wage strikes. The authors also consider the incidence of strikes, and again find no evidence for cyclicality in strike activity arising over the level of remuneration. These findings, together with their implications, are discussed more fully in chapter 3 of the thesis[16].

There would appear to have been no similar disaggregated analyses of British strike frequency using this kind of purely 'statistical' approach. Despite its lack of rigour, it would still be of interest to analyse the cyclicality of British strike frequency in this manner in order that broad comparisons can be made with the findings for the US and Canada. Thus the National Bureau for Economic Research methodology is utilised in chapter 3 to examine the correlation between strikes and the business cycle in the post-war period in Britain. Care is taken to distinguish the issue over which a strike is called and the industrial sector(s) affected by the conflict activity. However, while international comparisons are undoubtedly of interest, the principal motivation underlying this exercise is to establish whether British strike frequency displays any evidence of cyclicality. This issue arises following the poor performance of some standard aggregate macroeconomic models of strike frequency, an assessment of which is also presented in chapter 3. Previous evidence for these econometric models is examined in some detail below.

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15. That is, whether the strike began at the expiry of the contract ('contract strikes') or during the term of the contract ('non-contract strikes').

16. Additionally, Screpanti (1987) compares cycles in strike activity in France, Germany, Italy, the USA and the UK to the Kondratieff cycles, and reveals some limited support in favour of these long waves.

2.3.2. Aggregate Econometric Models of Strike Frequency.

The number of studies of strike frequency increased rapidly following the seminal article by Ashenfelter & Johnson (1969). As suggested above, several fairly heroic assumptions are needed to turn the theoretical model that Ashenfelter & Johnson derive into an empirically tractable specification. In particular, the authors assume that the union's rate of concession, the firm's discount rate and the horizontal asymptote y_* (see figure 2.1) should all be fairly constant over time. Thus variations in strikes are explained purely through variations in the workers' initial demand, y_0, and the maximum amount the firm can conceivably offer. The rate of unemployment and lagged real wages are hypothesised to determine the former, while the firm's profitability is the principal determinant of the latter, although this is also supposed to have an indirect impact on workers' demands y_0. Much of Ashenfelter & Johnson's model has therefore been abandoned with these simplifying assumptions; other authors have subsequently attempted to keep closer to the spirit of the original model, for example by hypothesising that the union's rate of concession can also be affected by the state of the labour market as in Farber (1978).

Despite its rather weak formulation, the empirical version of the Ashenfelter & Johnson model has received considerable attention in the literature. This derives principally from its ease of applicability and because of the fact that, for the first time, it offered testable empirical predictions from a rigourous economic model of strike activity. Many of the subsequent applications have been similarly concerned with relatively aggregated time-series studies, concentrating primarily on the impact of changes in the economic environment on strike frequency; these include Pencavel (1970), Hunter (1973), Bean & Peel (1974), Shorey (1977), Davies (1979) and Buck (1982) for Britain[17]; Vanderkamp (1970), Smith (1972), Walsh (1975) and Abbott

17. Although many of these specifically exclude coal mining, considering it a 'special case' (Turner (1963)), there have been some studies which have concentrated solely on this industry. These include Lynch (1978) and Winterton (1981).

(1984) for Canada; Turkington (1975) and Hazledine *et al* (1977) for New Zealand; Phipps (1977) for Australia; Sapsford (1979) for Eire while Paldam & Pedersen (1982) present a review of seventeen studies for various OECD countries[18]. One basic criticism of all of the studies which lay claim to the model of Ashenfelter & Johnson is that it is fundamentally a model of strike durations, or, conceivably, of strike incidence, and *not* of strike frequency. Thus it is implicitly assumed in many of these studies that patterns in incidence are identical to those in frequency over the relevant time period. As emphasised below and, in more detail in chapter 4, incidence and frequency are rather different dimensions of strikes and thus this additional assumption may be inappropriate. The choice of which dimension of conflict to use as the regressand is crucial since patterns in the various measures differ widely. To reiterate, most studies are of strike frequency, rather than of workers involved, duration or working days lost even though these other measures may be more indicative of the level of conflict activity in the economy as discussed in chapter 3. Most authors simply use the number of strikes with little or no justification for this choice. In many cases, it would seem that this is not the most appropriate statistic. For example, if an individual's decision to strike is supposedly based on some utility function consideration, then the number of workers involved would seem to be the most appropriate measure.

In general, the results of the studies of strike frequency for the UK are broadly in line with those that Ashenfelter & Johnson obtained for the US, although it should be noted that there are a number of rather conflicting findings as discussed below. Pencavel (1970) provides the first and perhaps most direct application of the model to UK strike frequency, justifying his choice of theoretical framework on the basis that shop-stewards or branch officials could be considered as fulfilling the union leadership role. He finds that quarterly non-coal strike frequency is negatively related

18. Note that not all of these models allude to Ashenfelter & Johnson specifically. Some are based on more *ad hoc* theorising although can still justifiably be grouped in this class given their final empirical specifications.

to real wage changes and the rate of unemployment and positively correlated with a profits variable defined as gross trading profits as a percentage of wage and salary compensation. As expected, given the trend in non-coal strike frequency over his estimation period 1950 to 1967, the time trend is also highly significant. But as Pencavel himself notes, this in itself has little explanatory power unless some interpretation can be given as to why strikes should be so highly trended. In addition, there is some evidence of instability in his results, perhaps arising from aggregating across heterogeneous industrial sectors as he suggests in his paper. However, it may equally arise from the model being misspecified, and this possibility is not really considered in depth by Pencavel[19]. Finally, such are the weaknesses underlying the empirical model that Pencavel estimates, his (reduced form) equation could be regarded as deriving from several of the theoretical (structural) specifications discussed in section 2.2 above[20].

Knight (1972) and Mayhew (1979), among others, have criticised the applicability of the Ashenfelter & Johnson model to the system of bargaining and industrial relations that pertains in the UK; most British strikes are unofficial and thus not called by a union leadership relatively separated from its members as in the Ashenfelter & Johnson model. Knight (1972) circumvents the problems associated with aggregating the utility function over individuals (as in the Ashenfelter & Johnson model) by specifying his utility function over group consciousness. He argues that this is more appropriate since it does not ascribe strike calls exclusively to trades unions but rather, and more realistically for Britain, to groups of workers. He concentrates on manufacturing strikes only, and controls for the size of the labour force by specifying

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19. He states that "To guard against the possibility that our inferences are jeopardized by serial correlation ... the equation was transformed according to the procedure suggested by Cochrane & Orcutt. ...[A]ll the independent variables have very similar estimated coefficients and standard errors" (Pencavel (1970), p.246, fn.1).

20. Note also that the model is only really appropriate for the occurrence of strikes at contract renegotiations. While wage strikes are probably the most equivalent class of stoppages in the UK, only around half of all strikes are over wage issues.

the dependent variable as strikes per capita. Moreover, his equation, using biannual data for 1950 to 1968, is formulated in terms of expectations, and is estimated in first differences. Expected money wage increases are very important, but prices and the rate of unemployment are insignificant, and the impact of profitability is not well defined. Certainly, while there is still no account for the issues over which strikes arise, Knight's analysis is more satisfactory than Pencavel's in that it focuses directly on the conflict between groups of workers and their employer.

A similar study to that of Pencavel (1970), but covering inter-war as well as post-war non-coal strike frequency, has been conducted by Shorey (1977). The theoretical model employed differs somewhat in that it is derived from *ad hoc* arguments as to the determinants of the probability of a strike at a single bargaining unit, rather than from a rigourously defined theoretical model. However, once aggregated across bargaining units and transformed into an operational model, Shorey's final empirical specification is very similar to that of Pencavel despite its rather different origins. Shorey estimates his model over two periods; 1920 to 1939 and 1950 to 1967 and, for comparative purposes, it is the latter period that is of interest here. In his final specification, he omits all variables which are not 'significant' (despite suspicions of multicollinearity) and, as one might expect given the broad similarities between their specifications, his results are quantitatively similar to those of Pencavel. He finds that non-coal strike frequency is positively correlated with real profits, (lagged) price changes and the price level and negatively correlated with (lagged) wage changes. Finally, he finds that lagged strike frequency, included to proxy the extent of external militancy in the (non-coal) economy, is strongly significant over the period 1950 to 1967.

The one substantive difference between the papers by Pencavel and Shorey is that the former includes a time trend whereas the latter includes a lagged dependent variable. Since strike frequency is highly (positively) trended over their post-war sample period, these two variables perform a similar function in terms of the empirical models' apparently high 'explanatory' power[21].

Davies (1979) attempts to extend the previous models to include the strike wave witnessed in 1969-71, and to include, in an appropriate fashion, the impact of incomes policies on strike activity. His operational equation, although derived from rather different theoretical reasoning to either Pencavel or Shorey, is again rather similar in terms of the variables included in the empirical specification. Davies states that his model assumes that all disputes occur over wages (or over issues with easily quantifiable parameters). However, he estimates three equations, using all strikes, wage strikes and non-wage strikes as the dependent variables for the period 1966 to 1975. Thus, while his analysis is important in that it is the first to distinguish between the issues over which disputes have arisen, his results are not strictly comparable to those of Pencavel or Shorey; firstly, he *includes* strikes in the coal industry despite the very distinctive differences between coal and non-coal strikes over the period; and, secondly, his investigation spans a different sample period during which the pattern in strike activity was clearly dissimilar from that in the earlier period covered by Pencavel and Shorey. The distinction between wage strikes and non-wage strikes is seen to be important in the results obtained; non-wage strike frequency would appear to increase significantly during periods when incomes policies are implemented, cancelling out the reduction in disputes over wages. Although there are some fundamental weaknesses in the formulation of his incomes policy variables, the overall impact when considered over their complete life-cycle is to significantly *increase* aggregate strike frequency. One disturbing feature of his paper is that he would appear to have transformed his equation in some way; while few details are provided he states that,

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21. The papers by Pencavel and Shorey are evaluated in much greater detail in chapter 3.

> "... in order to correct for the possibility of autocorrelation in the regressors the strike frequency equations were transformed according to the autoregressive scheme devised by Cochrane & Orcutt." (p.215)

No econometric justification is provided for this and his results may be both biased and inconsistent (and inference invalid) if the disturbances in the original specification are not serially correlated after all. Forrest (1990) presents evidence to suggest that his estimation strategy is indeed inappropriate. However, his analysis does serve to illustrate the need to take account of the issue(s) over which strikes occur; although many certainly derive from disagreements over levels of remuneration (as most of the models explicitly or implicitly presume), other factors should not be dismissed since it seems likely that the nature of the strike and the concession rate of each party will differ according to the issue over which conflict has arisen. In Britain, only about 50% of strikes now occur over disputes over pay, and thus models needs to be specifically developed to describe the occurrence of non-wage grievances[22].

Smith (1980) attempts to apply the specifications that Pencavel and Shorey utilised to the period 1967 to 1976. His results are much less satisfactory than those obtained for the earlier period, perhaps reflecting the increasing importance of institutional factors such as incomes policies in this period. These are not considered by Smith and there is little discussion of the quarterly results since his preference would appear to be for an alternative annual model. Sapsford (1982) estimates a slightly different model subject to AR(1) disturbances (despite the fact that the DW-statistic could indicate the existence of almost any misspecification), and then drops all 'insignificant' variables. Cronin (1979) attempts to improve on these economic models by including 'political' factors such as indicators of union strength, political party in power and lagged strike frequency to represent strike 'waves'. However, since he measures his improvement in terms of the Durbin-Watson statistic being closer to 2, his whole (econometric)

22. The value of the official statistics on the cause of stoppages is questionable however; not only are there likely to be multiple causes, but their relative importance may change over the course of the dispute, differ between strikers and there are also likely to be secondary motivations and latent or repressed grievances.

argument is fundamentally flawed since it will be biased in this direction by the inclusion of the lagged dependent variable. Finally, Cameron (1984) specifies an *ad hoc* model with which he claims to demonstrate structural instability in post-war annual strike frequency. But he provides little justification for the break at 1968, other than that most studies do not extend past this date.

Unfortunately, aggregate macroeconomic relationships of the type considered by Pencavel, Shorey, Knight and Davies among many others are largely unrelated to any *specific* theory. This gives rise to several ambiguities in the theoretical foundations preceding estimation, as emphasised by Mayhew (1979), and hence empirical support is given to a wide variety of conflicting *a priori* arguments. For example, workers may have more reason to strike in recession in order to protect their interests, while when demand is high, employers may be more willing to concede without a strike. Hence the rate of unemployment and strike frequency should be positively correlated, contrary to the prediction of Ashenfelter & Johnson. A similar argument can give rise to an ambiguous sign for the coefficient on the variable measuring profitability. Bean & Peel (1974) argue that real profits are an indication of the employer's ability to pay and thus should be associated with a decline in strike frequency, a result confirmed by their study, whereas Pencavel argues that high profits serve to increase wage demands and thus should be positively correlated with strike frequency as both he and Shorey subsequently found. Davies obtains a significantly negative coefficient on real profits for non-wage strikes only, while for all strikes and wage strikes, its impact is negative but insignificant.

This 'identification' problem is clearly important and is, at least partially, a consequence of 'observational equivalence' in many of the models. Any econometric estimation of these single equation models can only ever reveal a net effect which, if close to zero, may indicate a near balance between two competing hypotheses, or alternatively, that the variable has no influence in the determination of strike activity.

Two related criticisms of the literature are relevant here. Firstly, the conflicting results may also be a consequence of poor econometric techniques and uncorrected model misspecifications as suggested by Shalev (1980) and, in particular, by Abbott (1984) in his detailed re-estimation of several Canadian studies. Aggregation bias has been recognised as a potential source of problems by both Pencavel (1970) and Buck (1982), the latter estimating separate industry level equations by Zellner's (1962) seemingly unrelated regression (SUR) technique to circumvent Pencavel's assumption of independence between industries. However, both of these disaggregated studies are extremely partial in their industrial coverage, and both include the atypical example of coal-mining.

Parameter instability has also been recognised as a possible explanation for the diversity in the results obtained. Moore & Pearce (1982) report on the inability of the Ashenfelter & Johnson model to explain US strike frequency beyond the original sample period, while Hunter (1973) finds similar instability when the Pencavel model is extended to the end of 1972. Cameron (1984) provides some evidence for a structural break at 1968 in his study of annual UK strike frequency, although this finding could be a consequence of several other misspecifications not considered in his paper, since he measures model adequacy in terms of R^2 and the Durbin-Watson statistic. Hundley & Koreisha (1987) also note that structural instability seems likely in models of strike activity since,

> "... the costs of strikes provide incentives for bargainers to behave adaptively so that observable events which affected strike frequency in the past will not have similar effects in the future." (p.512)

Their solution is to employ atheoretical vector autoregressive moving average (VARMA) techniques. The results suggest that a stable macro-model of US strike activity does not exist; although in the immediate post-war period, economic variables had a significant influence on strike activity, more recently this relationship has not been maintained. This, they argue, is consistent with a process in which negotiators learn from their bargaining experience. These specification and estimation issues are reconsidered in some depth in the discussion relating to the replication of two selected models for the UK in chapter 3. Over the longer time period now available to researchers, such models are shown to have poor forecasting performance even when suitably 'corrected' for a variety of misspecifications to provide an econometrically satisfactory empirical macro-model of strike activity. While no similar VARMA analysis has yet been conducted, the adaptive behaviour suggested by Hundley & Koreisha may provide an explanation for this generally poor recent performance of aggregate econometric models in the UK.

A second major criticism, also related to model specification and estimation, is that the econometric technique typically employed by researchers is single equation ordinary least squares. The endogeneity of strikes within labour market behaviour implies that such single equation regression techniques are inappropriate. This has long been recognised and yet, with the exception of Knight (1972), Geroski *et al* (1982) and Geroski & Knight (1983), the simultaneous determination of wages, market structure and strike activity has remained unstudied. The three papers cited all indicate that such inter-relationships do exist, and thus have important implications for most of the single equation studies.

Several researchers have presented modifications to the Ashenfelter & Johnson model but, in general, these do little to circumvent the difficulties described above. An example of particular interest is that of Farber (1978). He hypothesises that the union's rate of concession α will be greater if unemployment is high, or if the union has only small strike funds. However, his model still suffers from the same basic criticism of Ashenfelter & Johnson that an outcome involving a strike is not Pareto optimal. Farber tests his model against a rather limited micro data set of 10 bargaining pairs in US manufacturing over 1954 to 1970, covering 80 contract negotiations. His results, while only weak, suggest that the concession schedule is downward-sloping at about 6% per annum. That he only obtains limited empirical support for his model may be a direct consequence of his lack of data, or due to the theoretical weaknesses in the framework he employs.

Although this econometric literature has been dominated by time-series studies, there have been a number of studies examining inter-industry strike activity including Shorey (1976), Creigh & Makeham (1980), Geroski *et al* (1982) and Geroski & Knight (1983). Shorey examines strike frequency for 33 manufacturing industries using annual averages for the period 1963-67. While his results are qualitatively similar to much of the time-series literature, there are also some additional revelations; in particular, strike frequency would appear to be positively related to industry plant or firm size and to the rate of change of wages, and negatively related to the proportion of women in the workforce and the rate of change of productivity. The existence of payment-by-results schemes seems detrimental to the state of industrial relations, a finding supported by the cross-section study by Creigh & Makeham (1980) using data for 120 MLHs averaged over 1971-75. Again, their study claims to support the hypothesis of procyclicality in strike activity (both frequency and incidence are modelled), although neither of these studies found the rate of unemployment in the industry to be significant. Although there would appear to be no extant pooled time-series cross-section analyses for the UK, Card (1988), Kaufman (1983) and Vroman (1989) employ such models for the US with some success.

There is also a large literature which emphasises the role of plant size on strike frequency, including Shorey (1975), George *et al* (1977), Holden (1978), Prais (1978), and Edwards (1980, 1981). The explanations proffered for the observation that large plants are more likely to experience industrial action differ widely; George *et al* (1977) summarise many of the arguments although it is clear that there is no commonly accepted causal link. Three studies concentrate solely on the effect of size on the effects of size on the likelihood of conflict. Prais (1978) predicts that the number of strikes should increase proportionately with employment and finds some support for his heirarchical model using data derived from Department of Employment sources. In contrast, Edwards (1980) observes a less than proportionate relationship using survey data and suggests that this may be because the number of bargaining groups does not increase linearly with plant size. Finally, Marginson (1984) extends the previous analysis to include the distinct effects of company size, which he argues may have an additional effect on strike activity due to management specialisation, standardisation and formalisation, and through these, unionisation. He finds both plant and company size to be important in his study using the Warwick Survey of Industrial Relations (Brown (1981)). One obvious criticism of these three studies is that size may be acting as a proxy for other excluded differences between plants, such as bureaucratisation, technology or workforce characteristics. Such differences need to be appropriately controlled for in order that the *ceteris paribus* effects of size can be identified.

The level of aggregation at which most of the macroeconomic studies have been conducted has been condemned most strongly by Wheeler (1984) in his extensive criticism of Kaufman (1982), Skeels (1982), Paldam & Pedersen (1982) and the macro-level strike literature in general. In part this criticism is related to the general structure of the empirical studies in that they fail to explicitly test the microeconomic theoretical models, and simply examine *ad hoc* macroeconomic specifications of strike activity. Relevant institutional considerations are also largely ignored at the high level of aggregation at which these studies are conducted. From all of these studies, few, if any, positive policy prescriptions have been obtained since it is improbable that policy-makers will attempt to manipulate inflation or unemployment, or restrict economic activity, simply to affect the number of strikes that occur. Indeed, policy is more likely to be directed and implemented at unions and individual firms or specific industries, and clearly the aggregate statistics cannot hope to identify the impact of any changes at this more disaggregated level[23]. On similar grounds, one might also object to the inclusion of a time trend in many of the studies. Clearly, the fact that strike activity may be increasing (or decreasing) over time is of relatively little interest unless some explanation can be proffered as to why this might be so. It is clear that there is a need to examine strikes at higher levels of disaggregation in order that the relationships at the bargaining level can be studied and so that relevant historical, institutional and political information can be incorporated.

In their defence, however, the macro-models do serve to emphasise some of the empirical regularities that a satisfactory economic theory of strikes should be able to explain. Nevertheless, they do not necessarily establish the procyclicality of strikes as claimed by many researchers. Even where the relevant variables enter into the specifications with the 'correct' sign, there are typically a number of measures of the cycle, not all of which will be concurrent cyclical indicators. As Kennan (1986) notes:

> "In principle, the advantage of regression models is that they allow the separate influences of several explanatory variables to be disentangled. If the question of interest is whether strikes are procyclical however, then estimates of regression coefficients miss the point... Thus for example, a negative regression coefficient associated with the unemployment rate does not necessarily mean that strikes are procyclical, if the regression also includes variables such as prices, wages and profits which may vary systematically with the cycle" (p.1120)

It is the response of strikes to the combination of these multifarious cyclical indicators that is important, and none of the multiple regression models attempt to amalgamate the cyclical explanatory variables in any way. Herein lies the advantage of the Burns-Mitchell method over these multiple regression studies; the former has only a single index of the cycle and thus avoids such difficulties.

As previously stated, one of the most unsatisfactory aspects of the studies of strike activity is their almost exclusive focus on strike frequency. If the number of bargains

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23. Note, however, that both Hunter (1973) and Davies (1979) distinguish carefully between various 'strengths' of incomes policies and their effects on strike activity and so may be considered a partial exception.

being made is changing, then changes in frequency may say little about changes in incidence. Stern (1978) highlights the problems arising over the choice of an appropriate 'population' measure with which to standardise the statistics for strike frequency. Such deflators are necessary in order that actual changes in the level of conflict can be distinguished from changes in the volume of bargaining. A variety of proxies have been utilised in the literature including the number of workers in the relevant labour force, the number of establishments, workers involved, union membership and so on. Other authors have made no correction at all claiming, for example, that the number of 'bargaining units' is constant over the relevant sample period despite the long time series often used. Given the abundance of union mergers and amalgamations, this assumption seems untenable. None of the divisors used are entirely suitable however; for example, simply deflating frequency by the number of establishments may provide an exaggeration of the amount of bargaining since collective agreements are less common in small establishments, while it may also underestimate bargaining activity since negotiations often occur more frequently than once each year. Stern therefore concludes that, rather than arbitrarily choosing a single denominator, it may be preferable to estimate equations with raw count data but using appropriate 'population' measures as explanatory variables.

The study of strike incidence or probability in Britain has been hampered by having no control group against which to compare those firms or establishments incurring strike action. The Department of Employment statistics fail to record even how many establishments are affected by each dispute and thus are not appropriate for a study of strike incidence in Britain. For the US and Canada, however, detailed results of contract negotiations have been recorded and thus provide a suitable source of data for the study of strike incidence at this level. Card (1987a, 1987b), Gunderson *et al* (1986), McConnell (1987, 1989), Swidinsky & Vanderkamp (1982) and Tracy (1986, 1987) all utilise data of this kind and their results show that bargaining-pair-specific, firm-specific and industry-specific factors are all important. These more recent empirical studies using micro-level data are typically much more closely related to the (microtheoretical) models from which they are derived, and thus come closer to explicitly testing the theories of strike activity. In that many of these studies are based on imperfections or asymmetries in information, they are discussed under their relevant sub-headings in the next two sub-sections. However, it should be noted that although these studies represent considerable advances in the investigation of strike activity, their scope is still often rather limited, and they clearly omit several important aspects of the collective bargaining process.

For the UK, the most appropriate source of data suitable for an analysis of the incidence of strike activity is the two Workplace Industrial Relations Surveys. These can be used to provide establishment-level strike incidence rates, and Millward & Stevens (1986) devote a chapter to the analysis of the industrial conflict information in the two surveys. Blanchflower & Cubbin (1986) and Booth & Cressy (1990) estimate equations for the probability of a strike using the 1980 and 1984 Workplace Industrial Relations Surveys respectively. Unfortunately, their specifications are very different and thus comparisons between the two years covered by the surveys are not possible. Following a comparison of strike frequency and strike incidence in the 1980s in Britain, chapter 4 reviews the studies by Blanchflower & Cubbin and Booth & Cressy in detail and shows them to be deficient in a number of respects, including the definition of conflict that they utilise as their dependent variables. The chapter then addresses the wider question of the determination of *conflict* incidence using the Workplace Industrial Relations Surveys within a suitable empirical framework. Chapter 4 also presents an analysis of the joint determination of strike action and non-strike action. The results show that, *ceteris paribus*, establishments which are characterised by high levels of strike activity also experience higher levels of non-strike activity; that is, certain establishments can be characterised as being conflict-prone.

While still not optimal (in that it still cannot perfectly reflect the proportion of bargains that fail to reach a settlement), the establishment-level measure of strike incidence provided by the Workplace Industrial Relations Surveys is much less open to criticism than aggregate strike frequency, since there is a clearly identifiable control group. Such disaggregated studies are still very uncommon for Britain and the chapter provides the first examination of the co-determination of strike and non-strike activity. Consistent specifications are estimated enabling comparisons to be made between 1980 and 1984, a period which saw many changes in the patterns of workplace bargaining, a huge rise in unemployment and many legislative changes governing union activity. The likely effects of these transitions in the bargaining environment are also discussed in detail in the chapter.

Chapter 5 attempts to combine the insights gained from chapters 3 and 4 with respect to strike frequency and incidence respectively to present an examination of the commonly voiced supposition that the public sector is strike prone. Both the Workplace Industrial Relations Surveys and the Department of Employment Stoppages Data Tapes are utilised; the results show that there are few significant differences between public and private sector strike propensities once adequate controls have been used to correct for the many differences between the public and private sectors. The tapes can also be used to provide alternative measures of the public-private differential in industrial action and reveal that public sector strikes tend to be smaller and shorter on average.

2.3.3. Joint-Cost and Imperfect Information Models of Strike Activity.

As described in sub-section 2.2.2 above, Mauro (1982) extends the concession schedules framework of Hicks to develop a model in which each party incorrectly estimates the other's concession curve because it utilises different variables when deriving the position of the curve. The misperceptions that this use of the incorrect information implies result in a divergence in the expected wage settlement, and thus a strike can occur, especially if the difference between the parties' expectations is large. Mauro finds some support for his model using a set of micro data derived from 14 US union-firm bargaining pairs over 30 years, and covering a total of 149 contract expirations. However, why systematic errors of this kind should continue to be made, or why particular contract negotiations should be characterised by this kind of sub-optimal behaviour (while other contract expirations are settled without strikes), is not really addressed fully in Mauro's paper.

The hypothesis that strikes are accidents, as emphasised by Siebert & Addison (1981), has received only limited attention in the literature perhaps because the notion that systematic accidents can and would continually be made has little appeal. However, in its emphasis on the role of information in bargaining, with the advocacy of written agreements and cooling off periods (during which the uncertainties associated with negotiations can hopefully be reduced), the theory raises some important issues. Discrimination between the 'political' model of Ashenfelter & Johnson and the accident theory is fraught with difficulties; Addison & Siebert are able to interpret the findings from several previous studies in terms of their theory but do not offer any particularly convincing reasons for preferring their model. Thus, once again, model discrimination is weak, in part due to the difficulty in identifying proxies for information shortages which might precipitate strikes. The only other study examining the accident theory is by Cousineau & Lacroix (1983) (reported in Hirsch & Addison (1986)) utilising data for Canadian manufacturing strikes for the period 1967-82. They emphasise the trade-off between the costs of a strike and its benefits in terms of the quantity and quality of information generated during the strike. While Cousineau & Lacroix find no relationship between strike probabilities and joint strike costs, they do find that strikes increase with increasing uncertainty as proxied by the retail price index, job vacancies and the coefficient of variation on capital utilisation. Thus their results arguably provide support for the accident theory of strikes, although once again, their findings could also be interpreted as endorsing a number of alternative models.

The joint-cost model, which posits that strikes should be less common when strike costs are higher, has seen a wide variety of applications in the literature. The origins of the theory lie in the papers by Reder & Neumann (1980) and Kennan (1980), while applications include Gunderson *et al* (1986), Sopher (1990) and some of the strike durations literature discussed below. According to this theory, the costs of a strike are weighed against the costs of attempting to specify a more detailed bargaining 'protocol'. Reder & Neumann proxy strike costs with shipments and inventories and find some evidence for an inverse relationship between strike activity and strike costs for 14 US manufacturing industries. In addition, experienced bargainers would appear to engage in fewer strikes, again lending support to the theory.

Gunderson *et al* (1986) provide a microeconomic study of Canadian contract strike incidence within a joint-cost framework. They emphasise that,

> "... strikes are more likely to occur in situations of uncertainty, imperfect and asymmetric information, and divergent expectations. In such circumstances, strikes can serve a number of functions pertaining to generating information, eliciting truth telling, establishing reputations, providing catharsis, and solving intraorganizational problems." (p.273)

Their results provide some evidence in favour of the joint-cost theory although much of the variation in strike activity is unexplained, even when industry dummies are included in the specification. A contrasting study by Sopher (1990) reports the findings from an experimental game under (notionally) complete information in which bargaining takes place over a 'shrinking pie'. Somewhat surprisingly, strikes were quite frequent and the bargaining outcomes provide some general support for the joint-cost theory.

The joint-cost model of Reder & Neumann (1980) and Kennan (1980) has received most attention in the study of strike durations. It is clear that such analyses are of considerable importance; strike durations can provide information on the likely determinants of the length of stoppages and the factors which might contribute to a settlement being reached while they may also be used to distinguish the form and nature of disputes. In addition, some authors have argued that the duration of a stoppage gives an indication of its cost to both parties (e.g. Creigh (1978)). For employees, the cost of a strike in terms of foregone wages is clearly directly related to its length while, for firms, production losses can often be compensated for once a dispute is settled. Thus (at least part of) the real cost of a strike lies in the delay that it causes in the productive process; the longer that a dispute lasts, the more likely it is that other firms will erode market share and that inventories will be exhausted with a subsequent loss of goodwill through failure to meet delivery schedules.

Thus the joint-cost theory would seem to offer a plausible framework within which the duration of strikes can be examined, and is a clear improvement to many of the previous studies of strike durations which typically lacked any structural hypothesis. In fact, early studies were simply statistical curve fitting exercises (e.g. Horvath (1968), Lancaster (1972)). In contrast, more recent analyses have allowed for observed and unobserved differences between strikes and can be seen as an attempt to bring a more formal modelling strategy to bear on the issue of strike durations. These include Harrison & Stewart (1989) for Canadian contract strikes, and Kennan (1985) and Tracy (1986, 1987) for the US. These models concentrate on trying to establish the econom(etr)ic regularities that any successful theory of strike durations must confront; Kennan (1985) and Harrison & Stewart (1989) both find that North American contract strike durations are countercyclical and this is in accordance with the joint-cost theory[24].

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24. Harrison & Stewart find that for their Canadian data, their chosen econometric specification dominates that chosen by Kennan for his US data. The latter fails to check the robustness of his results to alternative functional forms.

The analysis of strike durations has been deficient to date, especially for Britain, in that there has been little modelling of the process by which a settlement is reached once a strike has begun. The complex issue of identification of the structural parameters in these models has also yet to be adequately addressed in the literature, although both Kennan and Harrison & Stewart employ very flexible specifications to capture the duration dependence in the data to circumvent this problem, a solution suggested by Ridder (1986). In contrast, Tracy (1986) adopts a monotonic (Weibull) hazard and thus his results may be severely biased as shown by Heckman & Singer (1982, 1984a,b). A full discussion of the pertinent issues in the study of strike durations, together with some preliminary results for UK data derived from the Department of Employment Stoppages Data Tapes can be found in Dickerson (1988).

2.3.4. Private Information Models of Strike Activity.

There have been some recent attempts to test the private information theories of strike activity discussed in sub-section 2.2.3 above. The private, or asymmetric, information models typically assume that some component of profitability is unobserved by the union. Strikes are the mechanism by which workers obtain higher wages from more profitable employers; unions use wage-strike combinations to elicit information from the firm. While the result is observationally equivalent to the Ashenfelter & Johnson downward-sloping concession schedule, in this case the negative correlation between wages and strikes is deduced as a direct implication of incentive compatible bargaining by the union. Thus strikes can be seen to be *ex-post* Pareto-optimal.

The initial attempts to test such theories concentrated on their first prediction, that strikes should be positively related to the degree of uncertainty facing the union, and negatively related to the size of the rents to be shared. In his study utilising a micro data set of major US contract negotiations, Tracy (1986) finds that firm-specific uncertainty, as proxied by the volatility of its security returns, is particularly important while the rate of return on the firm's stock has no effect on the likelihood of a strike. Using the market model from finance theory, Tracy (1987) derives a stronger test of the asymmetric information model by decomposing the investor's uncertainty over the firm's future profitability into two components; the first resulting from economy-wide events and the second from firm-specific events. It is this latter measure of uncertainty that is seen to be particularly important for the incidence (and duration) of strikes, providing further support for the private information models. The predicted negative correlation between rents and strikes receives only weak support however.

McConnell (1989) extends Tracy's data set to include both manufacturing and non-manufacturing. She finds that the firm's private information, as proxied by both the level and variation in errors in future price expectations, has an insignificant impact on strike activity[25]. One major criticism of these studies is that if the relevant private information can be inferred by the researcher, it can surely be inferred by experienced union bargainers too, and thus is <u>not</u> unobservable! This dilemma might partially explain the generally poor econometric support for the theory found in these papers.

In response, some later studies have examined the second main prediction of the private information theories, that of a negative correlation between wage outcomes and strike activity. This *observable* prediction of the theories is clearly much more amenable to empirical investigation. Utilising data for Canadian manufacturing contract strikes, Card (1990b) finds no evidence of a relationship between the average real wage during the contract and strike activity, having controlled for alternative wage opportunities, unemployment, industry demand and fixed effects[26]. In contrast, McConnell (1989) shows that both the incidence and duration of strikes are

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[25]. However, uncertainty is still seen to play a role; the variation in past prices and unemployment both exert a positive effect on strikes.

[26]. However, while there is no evidence for a link from strikes to wages, he does find that lagged wage outcomes affect future strike probabilities.

negatively correlated with the residual component of real wage settlements in her broad cross-section of US labour contracts, thus providing some support for the theory. However, the estimated concession schedule is very flat, with the real wage decreasing by only 3% after a strike lasting 100 days. This implies that the cost of a long strike to the firm is negligible compared to the cost of conceding a small increase in the wage rate; this seems rather improbable.

The contradicting findings of the papers by Card and McConnell are discussed briefly by Card (1990a). He suggests that part of the explanation may lie in the differing industrial coverage of the studies; while Card (1990b) includes manufacturing industries only, McConnell's (1989) study also encompasses the non-manufacturing sector, and indeed it is this sector that displays greater sensitivity of wages to strike activity.

Some of these more recent microeconometric studies are reviewed in a recent article by Card (1990a), but, as yet, there are no such studies for UK strike activity. One primary difficulty is that even when information is available on individual bargaining pairs, as can be inferred from the Workplace Industrial Relations Surveys for example, it is still not possible to identify whether the prevailing wage at the establishment is the outcome of pre- or post-strike negotiations.

2.3.5. Costs and Consequences of Strike Activity.

There has been an obvious unwillingness to directly address the costs and consequences of industrial conflict. This may be related to the criticism noted above that most research has tended to concentrate on the circumstances in which conflict is more or less probable, rather than on why it occurs or how it is settled. Yet it is clearly important to assess the impact of conflict since it seems probable that there is at least some expectation as to the final outcome when strike action is undertaken or incurred. That the gains and losses from strike action have an important role to play in the analysis of conflict has been recognised implicitly by few authors, while only Reder & Neumann (1980) treat the (expected) costs of industrial action as paramount in the occurrence of strike action.

Conceptually, the cost of strikes should be measured as the sum of the producer and consumer surpluses lost as a result of strike action, but obviously no such measures exist. Days lost has been used most frequently for assessing the impact of strike action. However, it is not possible to tell from this statistic what combination of size and duration is involved. It also assumes that these days could have been usefully employed and cannot be compensated for at some future date. Therefore as a measure of the costs of strikes to either party or to the economy as a whole, days lost has some rather fundamental weaknesses. Furthermore, if the argument of Creigh (1978) is accepted, then the real cost of a strike results from the *delay* that it produces in the production process rather than any irrecoverable loss in output, and thus it is the duration of a stoppage that is the best indication of its cost.

The extent to which the costs of a dispute spread beyond the parties immediately concerned depends on the availability of stocks, supplies, the level of demand and the position of the firm in any production chain. Knight (1989) finds that only in those industries which hold relatively low levels of inventories do strikes adversely affect labour productivity in his cross-section of production industries in 1968. In the majority of industries, the effect of strike activity on productivity is actually positive (although small), lending some support to the Harvard School view[27] that unions can have beneficial effects.

The available evidence on the cost of strikes in Britain in terms of lost output is limited; Whittingham & Towers (1971) calculate the total loss to be only 0.2% of

27. The most forceful proponents of this school are Freeman & Medoff (1984).

GNP for 1970 (and this was an exceptional year in terms of days lost), while Turner (1969a) puts the figure at less than 0.1% in general, a negligible amount. However, these figures are based on the proportion of working days lost attributable to strike activity, and this is likely to be rather different from the loss to the economy; in part, this will depend upon how far any reduction in production is passed on to the workers in terms of lost wages, or made up for at overtime rates. In addition, other firms may gain at the expense of those firms incurring industrial action. Clearly the net (welfare) loss is immeasurable, but seems likely to be small in aggregate. An innovative attempt to analyse the relationship between strikes and output losses has been made by Neumann & Reder (1984). They use vector autoregressions (as described by Sims (1972)) to reveal that for US manufacturing 1958-77, industry losses due to strike activity are negligible or zero; shifts in production between firms and over time virtually offset all output losses. However, such an aggregated study, as with previous estimates quoted for the UK, misses the fundamental point that the costs to the actual participants will be high.. It is these individual costs which are important when attempting to explain the level and nature of conflict activity.

The consequences of strike action are also uncertain. At the macroeconomic level, the available evidence on strikes and inflation is limited and not conclusive[28], and strikes in this context are often being used as a proxy for union power which implies that their separate effect is not identifiable. Moreover, as shown by Stewart (1983, 1987) among others, the relative wage advantage of those covered by collective agreements, and thus those more able to take collective action, would appear to be small in practice. Note, however, the ability to take action does not necessarily imply that this threat effect has to be invoked; indeed, stronger unions may need to actually utilise their 'strike weapon' less frequently than weaker unions.

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28. The relevant literature includes Godfrey (1971), Taylor (1972), Purdy & Zis (1973), Johnson & Timbrell (1974) and Knight (1972), while Ward & Zis (1974) provide an international comparison.

Examining the consequences for inflation or wages fails to recognise that it is the *individuals'* costs which are important when attempting to explain the level and nature of conflict activity in terms of its costs. While sickness and industrial accidents may be responsible for many more days lost as previously noted, the significance of strikes is that their impact is concentrated, that they are more intensive and thus cause much greater disruption. Hence, although there are serious methodological problems, it would seem important to try to gauge the consequences of strikes and other industrial action.

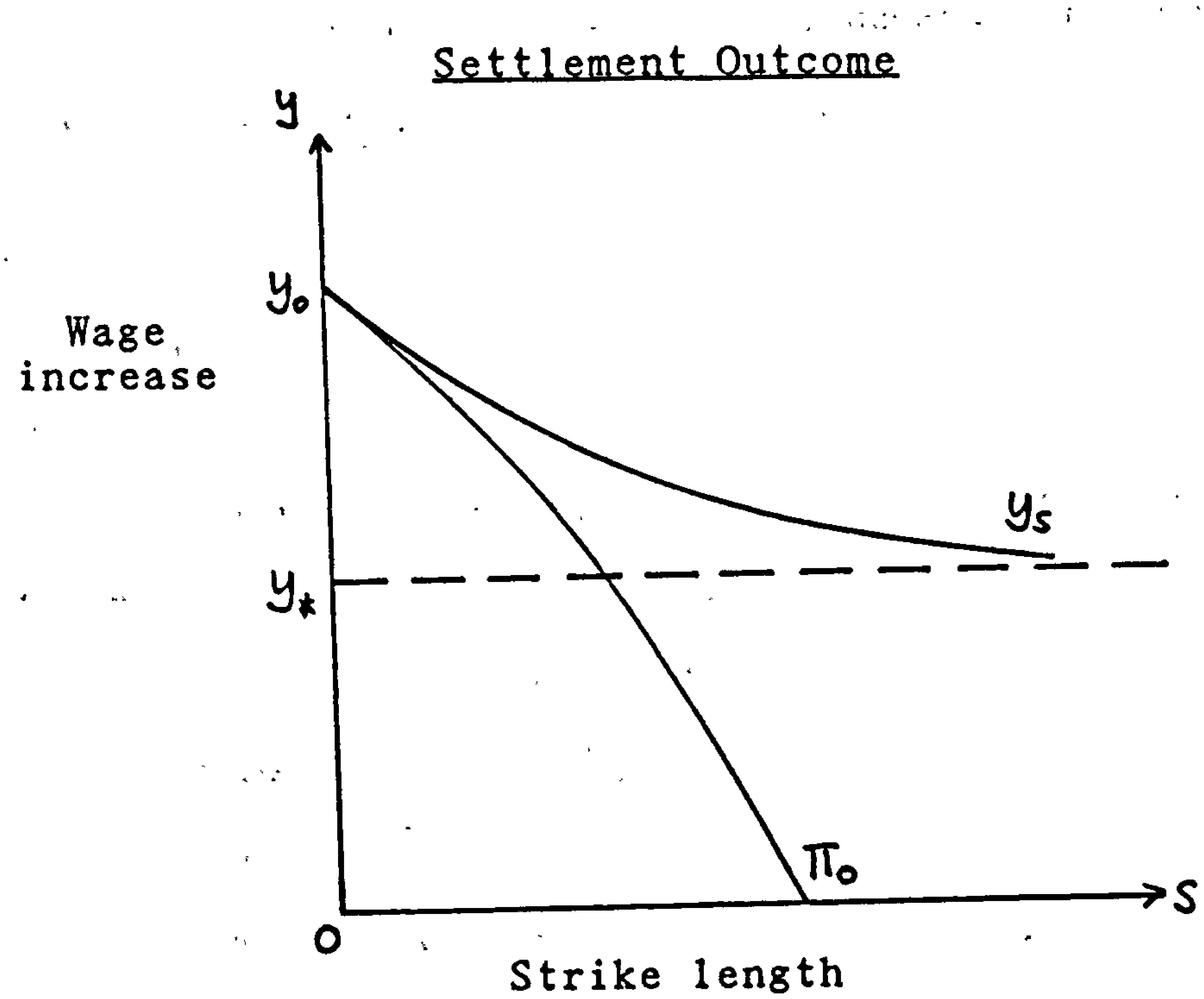
Chapter 6 of the thesis makes a preliminary investigation of the consequences of strike action for industrial output and efficiency. Production frontiers are estimated for a panel of 3-digit production industries for the 1970s. Even at this relatively high level of disaggregation, there are still no discernible output losses arising from either strike frequency or strike incidence; either lost output is made up within the plant or firm affected, or is compensated by increased production by other industry group members. However, decomposing the strikes variable reveals that there is a marginally significant negative impact on output of a high number of very short strikes. Thus industries which are dominated by frequent short stoppages would seem to incur some overall output losses.

The second half of chapter 6 analyses the consequences of strike activity for relative efficiency at the 3-digit level. Controlling for other relevant plant, workforce and workplace characteristics reveals that industries which are highly unionised are less efficient. However, this effect would seem to derive from union presence (or collective bargaining coverage) *per se*, rather than from the actions of unions in terms of high levels of strike activity. Indeed, strikes may even have a slightly mitigating effect.

2.4. Summary and Conclusions.

The appraisal and analysis of the extant studies presented above has revealed a number of important shortcomings in the literature pertaining to strikes and other forms of conflict activity. There are several criticisms of the models themselves and their empirical implementations have been rather weak. Research into industrial conflict in Britain has been particularly deficient to date. The utility of the macroeconomic aggregate studies in general has been called into question; there are a variety of extant models describing different time periods and, although theoretically quite different in origin, they tend to be empirically very similar and consequently fail to discriminate between the competing hypotheses that they purport to represent. No conclusions have been reached as to the cyclicality of strike frequency and thus the single 'stylised fact' of procyclicality has yet to be established for British strike activity. There are also very few studies of dimensions of strikes other than frequency, and those that examine British strike durations do not appropriately control for observed and unobserved differences between strikes. In addition, there are still very few studies at the disaggregated level for British strike activity and thus microeconomic evidence is also extremely scarce. Although there have been some recent micro-theoretical advances yielding directly testable predictions, as yet there have been no applications to strike activity in Britain. Furthermore, these models are arguably even less appropriate outside North American systems of industrial relations than the earlier, essentially macroeconomic model of Ashenfelter & Johnson. Finally, the causes and consequences of industrial action in Britain are still largely unknown.

The following chapters aim to address some of these issues as highlighted at the relevant junctures in the discussion above. The thesis does not pretend to be exhaustive however and concentrates in particular on the determination of the outbreak of conflict. Perhaps the most important omissions are an analysis of strike durations and the other fundamental dimensions of conflict, and of the specification of an appropriate formulation of an asymmetric bargaining model for the system of industrial relations that exists in Britain. Thus, while arguably eclectic in approach, the subsequent chapters concentrate on establishing the patterns in industrial conflict, in particular with regard to strike frequency, conflict incidence and the impact of strikes, that any successful theoretical model must be able to encompass. Chapter 3 evaluates the aggregate macroeconomic models of strike frequency and finds them to be seriously deficient. However, the cyclicality of strikes over wages is established. Chapter 4 examines the important distinction between strike frequency and strike incidence in the 1980s; thereafter, an empirical model is developed for the incidence of industrial conflict together with the co-determination of strike and non-strike activity. Chapter 5 concentrates exclusively on the public-private sector distinction, and finds few significant *ceteris paribus* differences in the incidence of strike action between the sectors while chapter 6 examines some of the consequences of strike activity for productivity and efficiency. Certainly each of these chapters reveal several interesting empirical conclusions that merit further investigation. Their central findings are appraised and analysed in the final and concluding chapter of the thesis.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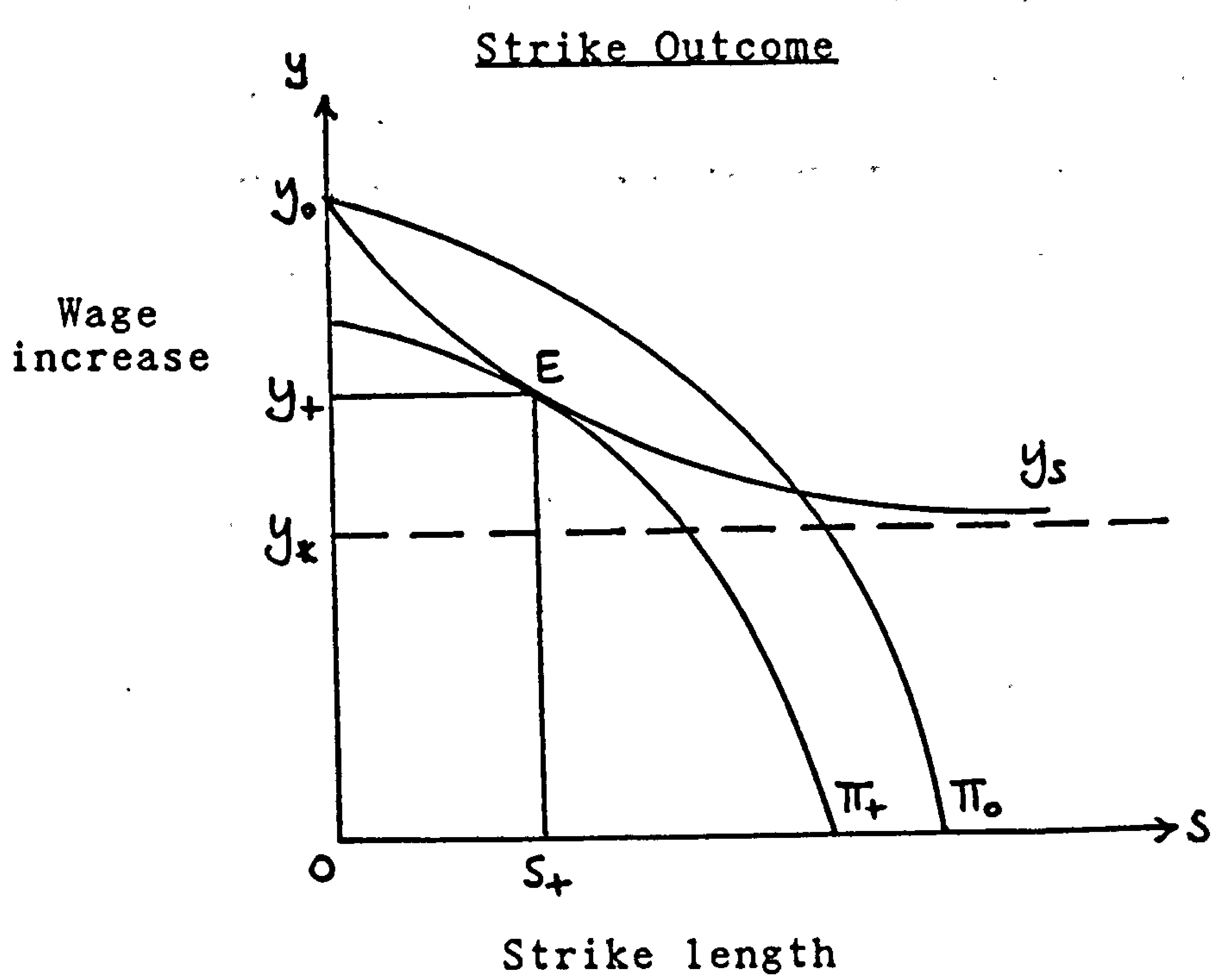
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CHAPTER 3

A Reassessment of British Post-War Aggregate Strike Frequency

3.1. Introduction.

The central purpose of this chapter is to reassess the aggregate strike statistics for the UK, with particular emphasis on strike frequency since this has been the dimension of industrial conflict that has received most attention in the literature. Section 3.2 presents a brief synopsis of the centrally collated statistics on industrial conflict since these have been the source for almost all of the investigation and commentary made with reference to strike activity in Britain to date. In general, researchers have claimed that their studies show strike frequency to be procyclical. However, for UK strike activity at least, the adequacy of these econometric models has yet to be established[1], and most do not cover strike activity in the 1970s or 1980s. Thus in section 3.3 the results obtained from re-estimating two popular macro-economic time-series studies of strike frequency are presented. Subjecting these models to rigourous econometric investigation reveals that they are fundamentally misspecified in a number of ways. Even satisfactory econometric specifications obtained from suitably transformed models still fail dramatically in post-sample predictions.

It has long been argued that strikes are a cyclical phenomenon and much of the earliest literature concentrated exclusively on this observation (see, for example, Hansen (1921), Knowles (1952), Rees (1952)). Many econometric studies also incorporate indicators of the business cycle such as unemployment and the rate of change of prices. Indeed, given their aggregated nature, it is possible to interpret

[1]. In contrast, Abbott (1984) presents a detailed examination of several models using Canadian data.

many of the time-series studies of strike frequency as purely descriptive models[2]. Although some are based on formal economic analysis, many simply resort to *ad hoc* theorising for their choice of explanatory variables and strike frequency is typically simply related linearly to a set of aggregate economic indicators of the state of the cycle.

Given the rather pessimistic findings from aggregate econometric models, section 3.4 provides a purely statistical analysis of the cyclicality of strike frequency, disaggregated by cause and broad industrial sector. In light of the results obtained, in particular that it is wage strikes in the non-coal sector that are most strongly cyclical, section 3.5 presents some preliminary estimates of a new empirical model for quarterly strike frequency in Britain. This takes into account the inappropriateness of the cyclical measures that have been employed in previous models. In addition, estimation spans the whole of the post-war period, including more recent times during which major developments have taken place in the environment in which collective bargaining is conducted. Some conclusions and prospects for further research into strike frequency are discussed in the final section of the chapter.

3.2. Industrial Conflict in Britain: A Summary of the Available Evidence.

Information about stoppages in the UK is collated by the Department of Employment (DE) on a voluntary basis, through local Unemployment Benefit Offices, returns from nationalised industries, public bodies and large firms, reports in the national and local press and, for some larger stoppages, directly from the organisations involved[3]. There obviously exist problems in ensuring complete coverage and partly for this reason, stoppages involving fewer than ten workers and those lasting less than one day are

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2. That is, rather than investigating the causes or consequences of strike action, they simply tend to describe when strikes are more or less likely to occur.

3. The origins of British strike statistics are discussed by Creigh (1982).

excluded from the statistics unless the total number of working days lost exceeds one hundred. It is not possible to quantify the under-recording that this implies; clearly it will be greatest for the number of strikes whereas the statistics for the total number of workers involved and total working days lost should be less adversely affected. The reporting method also leads to some other unfortunate biases; in that typically each party to any dispute may have a vested interest to misrepresent the size and scope of any strike action, then the fact that typically the DE is more reliant on the information yielded by employers may be of some concern when assessing the coverage of the official statistics. Perceptions of conflict can and do differ widely[4] and the portrayal of conflict by the media also has an important role (Philo (1990)). Even the responses of those actually involved can differ in the identification and definition of the characteristics of any strike. For example, in the 1984 Workplace Industrial Relations Survey (Millward & Stevens (1986)), while in 89.1% of establishments worker and management respondents agreed as to whether or not a strike had arisen among manual workers in the previous 12 month period, in the remaining 10.9% of establishments, they disagreed. For non-manual workers, the extent of disagreement was even greater, with either but not both of the respondents reporting that a strike had occurred in the previous year in 12.1% of all establishments. The divergences in responses between managers and workers are investigated more fully in chapter 4 and various suggestions are made for treating the discrepancies in measurement that these differing perceptions of conflict imply.

The three basic indices of stoppage activity recorded for each strike in Britain can be related in the identity:

$$\text{Days Lost} \equiv \text{Strike Size} \times \text{Duration}$$

where Strike Size is the average number of workers involved in the dispute and

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4. Compare, for example, MacGregor (1986), Reed & Adamson (1985), Samuel *et al* (1986) and Winterton & Winterton (1989) as conflicting accounts and interpretations of the miners' strike in 1984-5.

Duration is measured in days[5]. The total number of days lost is then obtained by summing over all disputes. This relationship can be applied at any level of aggregation, to any unit of observation and for any time period of interest. Each component of the identity expressed above reflects a different dimension of conflict, yet as will be seen below, there is a pronounced emphasis in the literature on the study of the number of strikes. Thus, before any inference or deductions can be drawn from the current studies, further justification needs to be made for this concentration on strike frequency since there is no guarantee that the characteristics of this series will be similar to those of the other dimensions of strikes. Indeed, as is shown below, there are some rather pronounced differences.

One suggestion made in response to such criticism is that efforts should be directed towards creating an index of strike activity which appropriately encapsulates all its various possible dimensions. The notion of a 'volume' of strike activity has already been used by Kerr & Siegal (1954), Britt & Galle (1972) and Shorter & Tilly (1974) among others[6]. Galambos & Evans (1966a, 1973), recognising that working days lost is a very crude measure of any reduction in output, compute a multi-dimensional 'index' of stoppage activity. However, they simply take the unweighted arithmetic mean of the frequency, days lost and number of workers involved (the latter two dimensions scaled by employment in the sector) and this seems inadequate, as noted by Knowles (1966). The problem is that the three indices need to be weighted in order to measure the (economic) loss accountable to the strike, but that there is no method of determining the appropriate weights to use. The relative importance of each of the components depends upon the structure of the industry, will differ according to the

5. Note that this differs from the identity presented by Hirsch & Addison (1986, p.76) in their equation (4.1). They claim that total days lost is the product of the number of stoppages, the average number of workers involved and average duration. This is clearly incorrect since $\sum s_i d_i \neq n(\sum s_i/n)(\sum d_i/n)$.

6. For example, Shorter & Tilly (1974) found that although days lost due to strikes in France was little changed over the period under consideration, the nature of strikes was very different. They viewed strike activity as a rectangular solid (frequency x size x duration) whose sides changed independently, but whose volume was unchanged.

context of the action and will change over time. Knowles (1966) contends that simply taking the average is a spurious convenience and probably says less than each component would in isolation[7]. Thus, rather than a single index, the alternative of investigating how each of the measures of conflict vary in response to external forces along the lines of the existing studies of strike frequency would seem to be a preferable methodology. Each component's contribution to the overall 'volume' of strike activity could then be separately ascertained. The merits and demerits of attempting to combine the measures in some manner can then be discussed in light of the findings from the separate studies of each dimension[8]. Details of the coverage of each of the official statistical series are provided separately for each dimension of strikes in sub-sections 3.2.1 to 3.2.4 below. Firstly however, some problems of definition and interpretation in the measurement of strikes warrant attention.

That UK strike statistics are not a particularly accurate representation or reflection of the extent and pattern of industrial conflict has long been recognised (see, for example, Shalev (1978b)). Much of the criticism has been reserved for the coverage and criteria that have been traditionally used to record conflict behaviour in particular, and that the official statistics ignore forms of conflict other than strikes[9]. There is some industry-specific evidence to suggest that the extent of under-recording of strikes is very serious, although it is impossible to make generalisations from this to the economy as a whole. Some estimates are presented in Turner *et al* (1967) and Kelly & Nicholson (1980) for the motor industry and in Brown (1981) for the

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[7]. In fairness, in their reply to Knowles, Galambos & Evans (1966b) argue that their index is only intended for comparison, not measurement, and thus only an algebraic system of combining the indices is required. In the update of their earlier analysis, Galambos & Evans (1973) go further and argue that since their index does not measure hardship, loss of profits, wages or output, but only shows changes in the level of strike activity (and draws no implications as a result of such variations), then no weighting system is required in *logical* terms.

[8]. However it should be recognised that although all the components in equation (3.1) are obviously interrelated, several important distinctions and relative changes are likely to be obscured by simple (or more complex) aggregation.

[9]. A comprehensive note describing the compilation of the strikes statistics is published most years along with the annual summaries in the <u>Employment Gazette</u>. The most recent of these is <u>Employment Gazette</u>, July 1990, p.346.

manufacturing sector as a whole. The latter suggests that, although direct contact was established in only a small minority of cases, it would appear that, together with other sources available to local Department of Employment officers, 62% of eligible strikes and 96% of working days lost in manufacturing in 1976-77 were recorded in the official statistics.

Perhaps of greater importance than the completeness of the coverage of the official statistics is their *consistency* in recording strikes over time. There seems no obvious way in which this can be assessed, and hence it has to be assumed that the efficiency with which strikes are recorded has been fairly constant over the period of interest. However, this is a potentially important caveat when utilising the data compiled by the Department of Employment, and thus the confirmation of any patterns and trends identified in the official Department of Employment statistics by comparison with other sources of data is clearly desirable.

In addition to the non-recording of short disputes described above (and the probable under-recording of stoppages near the margins of the definitions), strikes over issues not directly linked to terms and conditions of employment are not included in the official statistics. Thus disputes of a 'political' nature are excluded although, until comparatively recently, these probably accounted for very few working days being lost in the UK[10]. Strikes and lock-outs are not distinguished, nor are 'lawful' and 'unlawful' strikes most probably because of the difficulty in assigning such categories to any particular dispute. In light of the increasing 'juridification' of British industrial relations (see, for example, Clark (1985)) it would be interesting to have this classification[11].

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10. Thus protest strikes over the 1971 Industrial Relations Act and over the abolition of the Greater London Council are not included in the official statistics.

11. In particular, the implementation of the Trade Union Act in September 1984 made strikes unlawful if a ballot of workers has not been conducted, or if the ballot paper does not make it explicit that, by striking, the workers would be in breach of contract.

Whether a strike is 'official' or 'unofficial' was recorded up until 1981; this distinction has now been dropped from the annually published summaries in the Employment Gazette most probably because of the difficulty in categorising each dispute. It should be noted that the vast majority of strikes were (and still are) unofficial. In the period 1966 to 1984, less than 5% of all strikes were classified as 'official'[12], and this is probably symptomatic of the spontaneity with which industrial action is usually taken. As Hyman (1984) notes,

> "The common stereotype of the strike as a carefully planned confrontation between union and employer organisations is not characteristic of stoppages in general. Disputes have traditionally stemmed principally from the initiative of the union rank and file." (p.41)

Unfortunately, international comparisons of strike activity are severely hampered by the differing coverage of each country's statistics. For example, since 1981, the US Bureau of Labour Statistics has only recorded disputes involving more than 1,000 workers. Edwards (1983) calculates from data collected prior to 1981 that this will remove around 94% of stoppages from the figures and will therefore make it very difficult to perform many kinds of comparative analysis. It will also result in over 35% of workers involved and working days lost no longer being recorded in the official statistics, with the new size criterion for measurement exhibiting a wide coverage variation over previous years[13]. For other countries, the greatest differences

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12. Source: Department of Employment Industrial Stoppages Data Tapes, 1966-1984. For the period after 1981, these still record whether each strike was made official at any stage despite this distinction being dropped from the published statistics.

13. It should be noted that Garen & Krislov (1988) dissent from this view. They examine a semi-logarithmic strike equation separately for small and large US strikes prior to 1981. While they find that coefficient equality is rejected for the two series, they argue that this is "due to a minority of coefficients" (p.80) and hence conclude that research can continue to be done on long-term strike patterns with the new series comprising large strikes only. Their failure to recognise that their estimating equations may be unstable *over time* is fundamentally important (there are no tests for stability or any other misspecification), and suggests that their conclusion is overly optimistic. Hundley & Koreisha (1987) present evidence to suggest that a stable post-war strike equation for the US does not exist. This has the implication that large and small strikes series may behave somewhat differently, and thus the new BLS recording criterion will seriously jeopardise any new research into US strike activity.

are between the criteria used to determine whether any particular dispute will be included in the official statistics. Most, including the UK, exclude small stoppages, and detailed descriptions of these 'minimum' criteria are provided in the Employment Gazette, April 1985[14]. The treatment of political disputes also differs widely between countries as does the inclusion or exclusion of workers indirectly involved in the dispute. Clearly these differences prevent definitive comparisons between countries. At best, as Shalev (1978a) notes, comparison should be limited to evaluating similarity and diversity in trends, rather than directly comparing the statistical characteristics of strikes under different recording methods. He describes in some detail the problems that arise in international comparisons; further discussion can be found in Turner (1969a), McCarthy (1970) and Fisher (1973).

There are no attempts to provide an international comparative exercise in this thesis. This is not due to narrow parochialism but rather because it is first necessary to widen the scope of investigation relating to industrial conflict in Britain. Summaries of the levels of post-war strike activity in terms of each of the dimensions of strikes follow in the next four sub-sections together with a further critique of each series as a measure of conflict. For completeness and comparison, the available evidence on non-strike activity is described and discussed in the appendix to this chapter, section A3.1.

3.2.1. Number of Strikes.

As noted above, although the number of stoppages is the statistic most frequently used by economists in their studies of industrial conflict, there are a number of reasons why the official record may be severely under-recording, especially in small firms. In the literature this issue has typically been ignored despite the spurious results it may be generating and there seems to be no simple method of successfully

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[14]. See also Crouch & Pizzarno (1978, pp.328-332).

measuring or discounting the bias that exists. The extent of under-recording of eligible strikes has gone largely unexamined except for a few specific industries. However, this masks the more considerable bias resulting from the fact that many short strikes are not recorded by the official statistics because they do not meet the 'minimum size' requirement of the Department of Employment. Brown (1981, pp.99-100) estimates that for manufacturing, the two effects together may imply that the official statistics record only about one fifth of the total number of stoppages occurring[15]. Indeed, the size eligibility requirement may account for some of the positive correlation exhibited between the number of strikes and size of the firm (see, for example, Smith *et al* (1978)), since it implies a bias in favour of reporting strikes which occur in large firms.

The Department of Employment records every stoppage, regardless of its extent, as a single dispute. Thus, although the series for strike frequency may reflect the number of separate outbreaks of overt action, it is clear that the considerable heterogeneity that exists between strikes is not revealed by such measures. Thus the miners' strike of 1984-85 is given the same weight in the frequency statistics as a walk-out for a few hours by sufficient workers to meet the minimum size requirement of the recording criteria. The attraction of the series for researchers is that it displays considerably less volatility than the other measures available although this is, of course, insufficient reason for discounting the other dimensions of industrial conflict.

The total number of strikes beginning each year from 1945 to 1989 is shown in figure 3.1 along with the number of strikes outside the coal industry[16]. This clearly illustrates the generally increasing growth in stoppage frequency from 1950 through to the peak in 1970, and the decline witnessed thereafter. For non-coal stoppages, the

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15. The figures for days lost are, of course, little affected by such problems since they are typically dominated by a few large, lengthy disputes.

16. For convenience, tables and figures are gathered together at the end of each of the chapters of the thesis.

increasing trend through the 1950s and 1960s and the decline after 1970 are both very prominent, although there are still quite considerable year-on-year fluctuations. It is also evident that while the number of coal strikes dominated the aggregate statistics in the 1940s and 1950s, they have subsequently become a smaller and smaller fraction of the total. Indeed, the raw data reveal that coal strikes comprised some three-quarters of all disputes which met the Department of Employment recording criteria in the 1950s but, while 1970 marked the peak in strike frequency, the number of coal strikes was actually at its lowest post-war level and comprised only 4% of the total number of stoppages in that year. Subsequently, the number of disputes in the coal industry has remained comparatively stable while the number of stoppages elsewhere has fallen dramatically. Consequently the proportion of coal strikes has actually risen in the 1980s[17].

It is also of some interest to examine the recorded causes of strikes and how these have changed over the period under consideration. Figure 3.2 makes a preliminary comparison of stoppages arising from disputes over wages with all other disputes. It can be seen that the pattern in total strike frequency is quite similar to that of the number of wage strikes, while the number of non-wage strikes displays much less variation around its mean. This raises several important and interesting questions. In particular, it would appear that the overall volume of strikes is primarily driven by the occurrence or otherwise of disputes over wages. The number of strikes over all other issues, while not time-invariant, is much less sensitive to the changing economic, political and social environment. This general conclusion is most starkly illustrated in the post-1970 period. It is the dramatic fall in the number of disputes over wages that has been primarily responsible for the overall decrease in strike frequency. Therefore, in order to understand the changing overall level of strike activity it would appear that the first concern should be to explain the fluctuations in the number of strikes over

17. The exception, of course, is 1984, reflecting the miners' strike of that year; very few coal strikes were recorded because there was, simply, one very large dispute.

wage issues.

3.2.2. Number of Workers Involved.

Strikes vary considerably in size and it is clear that their economic, political and social impact will differ according to the number of workers involved. Clearly, it is often difficult to distinguish those simply laid-off (indirectly involved) from those actually participating in the dispute (especially given the reporting methods used) and thus the statistics published by the Department of Employment record those both directly and indirectly involved at the establishment(s) where the dispute occurred. The Department of Employment attempts to record the total number of workers involved at any time in the dispute, but this presents difficulties since the number taking industrial action for the first time will not usually be available; in such cases the statistics simply record the maximum number involved at any one time. There may therefore be an under-recording of the total number of workers involved in any dispute.

There are at least two additional problems with the official statistics for the number of workers involved. Firstly, because individual strikes are aggregated in the published statistics, it is not possible to draw conclusions about the willingness of individuals to take strike action in support of any dispute. This can also manifest itself in double counting; as noted by Smith *et al* (1978), if one hundred workers strike three times a year, the aggregate number of workers involved increases by three hundred, and thus the statistics can be a rather misleading indicator of workforce 'militancy'. Moreover, as recognised by Batstone *et al* (1978) among others, the figures for the number of workers involved only yield a limited amount of information about the nature of the strike. In particular, the organisational level at which the strike occurred (plant, trade union or particular skill category for example) is not revealed by the aggregated statistics and this feature of the strike seems likely to have ramifications as to the cause and likely consequences of strike action.

A second major problem is the inclusion of those workers only indirectly involved. This results in a further bias since bigger plants are likely to have more workers indirectly involved, especially if there is some form of continuous production process as in much of manufacturing for example. Thus strikes at larger workplaces tend to show up disproportionately in the statistics for workers involved, irrespective of the number of workers actually taking strike action[18]. Once again therefore, the conclusion that large plants incur greater levels of strike activity is at least partially an artefact of the recording methodology. Finally, however, the statistics do not include those laid-off in consequence of the dispute at other points in the production chain. Clearly the full repercussions of any single dispute cannot be traced throughout the whole economy but it should be noted that this does present an inconsistency in the construction of the statistics for the number of workers involved as a measure of the scope or intensity of the strike. At a minimum, the coverage of the statistics for the number of workers 'involved' in any particular dispute seems rather arbitrary and appears likely to differ between individual strikes.

Figure 3.3 plots the annual totals for workers involved in stoppages in progress in the post-war period. This clearly illustrates the considerable volatility of this particular dimension of strike activity. The graph is dominated by the two peaks in 1962 and in 1979; more than twice as many employees participated in industrial conflict in these particular years than in any other year in the post-war era. The peak in 1962 represents the two national one-day engineering and shipbuilding stoppages of that year, while that in 1979 is principally a reflection of the national dispute in engineering in the late summer of 1979, although the so called 'winter of discontent' also contributes to the overall total number of workers involved. However, as Whitehead (1985) notes with regard to the latter,

18. It is also possible that some of those recorded as indirectly involved may only be so as the consequence of sanctions imposed by management in order to indirectly pressurise those taking strike action to return to work.

> "... it was neither the extent of the strikes nor the economic damage caused by them which made an impact. The strikes were effective because they were visible and they hurt the public directly." (p.281)

It is apparent from the raw data that a small number of disputes typically dominates the composition of the statistics in each year, and it is this size concentration that leads to the considerable volatility exhibited by the series for the number of workers involved (and, consequently, the number of days lost as shown in sub-section 3.2.4). Extending the summary for 1960 to 1979 presented in the Employment Gazette September 1980 for the period up to 1989 demonstrates this phenomenon. The 91 stoppages that involved over 200,000 days lost in these three decades accounted for 31% of all workers involved and 55% of all working days lost, but only comprised 0.15% of the total number of strikes[19]. These large strikes tend to be official and have distinctive and rather specific causal factors. Thus there is a need to somehow remove these from the statistics in order that the underlying patterns and trends in the extensiveness of strike activity (as measured by the number of workers participating in strike action in any year) can be discerned.

3.2.3. Strike Durations.

There are several reasons why the duration of a strike is of particular interest. Firstly, it may yield information about the nature and significance of any particular stoppage; trials of strength may be compared with 'token demonstrations' (Hyman (1984)). While the former dominate the aggregate statistics, especially for days lost, most strikes are of the latter variety in which the decision to stop working is spontaneous. Secondly, Creigh (1978) among others has argued that the real cost of a strike results from the delay it produces in the production process, rather than an irrecoverable loss in output, and thus the duration of a strike is one potential indicator of its cost. Thirdly, the speed at which disputes are settled is indicative of the efficacy of the negotiation and bargaining mechanisms in operation.

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19. In fact, the concentration in large strikes is even more acute than this. Further analysis shows that the 35 stoppages involving more than 500,000 days lost still comprise 27% of all workers involved and almost half of all days lost in the period.

The Department of Employment aggregates strike durations into several banded measures for strikes longer than five days, currently completing the categorisation with an open-ended interval comprising all strikes of over fifty days in length[20]. This form of measurement implies that it is not possible to directly calculate the average duration of strikes unless strong distributional assumptions are made[21]. Thus figure 3.4 depicts the post-war patterns in the distribution of strike durations; the chart depicts the proportions of strikes lasting up to 2 days and up to 5 days. It is immediately apparent that most stoppages are of limited duration; on average, over half last no more than 2 days and more than three-quarters last 5 days or less. It is also important to note that these proportions are for recorded strikes only; given the minimum size criteria used in the compilation of the statistics, they would be somewhat higher if all stoppages were included.

It is also evident that the duration distribution of stoppages has not been invariant over the whole period. Following the relatively stable post-war decade, strike durations increased almost monotonically until the mid-1970s, since when they have decreased sharply (although are still longer than during the 1950s). This may at first suggest that UK strike durations are largely unrelated to the economic cycle, contrary to the findings of Kennan (1985) and Harrison & Stewart (1989), for the US and Canada respectively, who both find countercyclicality in (manufacturing) strike durations. However, the trends exhibited in figure 3.4 may also be due to the changing composition of the number of strikes throughout the period and/or any of the other numerous differences between them. Some evidence is provided by disaggregating strike durations by cause of stoppage and industrial sector using the Department of

20. The duration band-widths used have not been consistent over the whole of the post-war period.

21. Meaningful estimates of average durations cannot be reliably calculated from the ratio of days lost to workers involved as in Edwards (1983), since both these series are frequently dominated by one or two very large strikes in each year. The annual summary tables in the Employment Gazette show that median durations are typically 2 to 4 days.

Employment Stoppages Tapes. This exercise reveals that strikes in the non-coal sector are longer, on average, than those in the coal industry. Since the former increasingly replaced the latter as the dominant determinant of the overall level of strike activity from 1950 onwards, then average durations would be expected to increase, *ceteris paribus*, and this tendency is indeed apparent in figure 3.4. Similarly, strikes arising over the level of remuneration would appear to be longer than non-wage stoppages. Given the falling proportion of wage strikes in total strikes since the peak in 1970 as shown in figure 3.2, then average durations would be expected to fall again. These two 'composition' effects clearly explain at least some of the trends displayed by the duration distribution, but clearly much more detailed investigation is necessary before any firm conclusions can be drawn[22].

3.2.4. Number of Days Lost.

In the technical note attached to the annual summary of stoppages in the Employment Gazette, the Department of Employment argue that the figures for working days lost are the best indicator of the impact of industrial conflict. This assertion is based on the fact that the number of strikes is under-recorded due to the omission of strikes that are small; since the number of days lost in short strikes represents a tiny fraction of the total number of working days lost, then the latter is a better measure of the level of conflict than strike frequency. While there are undoubtedly problems arising from the unreserved use of the statistics as discussed below, days lost is clearly an important dimension of industrial conflict. Despite this, it has received scant attention in the literature to date.

The unit of reference for days lost is the basic working day, excluding overtime working. Where a strike lasts less than a complete number of days (or shifts), hours

[22]. It should perhaps be noted that the duration distributions for these disaggregated series display quite similar patterns to those for the total number of strikes (although they are only available for the period 1966-1984), and thus increasing durations followed by decreasing durations may have typified stoppages more generally over the period.

lost are converted to full-days equivalents as are any days lost by part-time workers. However, it is clear that comparisons between different sectors of the economy will be hampered by the extent to which the standard working day differs and thus the proportional impact on output, for example, will not be the same for any given percentage of days lost.

There are at least two major objections to the unreserved and unqualified use of these data. Firstly, as seen in sub-section 3.2.2, the figures for days lost are typically dominated by a few very large strikes, particularly when these occur at the national level. Hence the statistic can be a very misleading indicator on a year to year basis of the state of industrial relations in the economy. In 1984 for example, the miners' strike resulted in an almost unprecedented total for days lost, and yet this was but one strike in a year which saw the number of disputes at its lowest level for any year in the post-war period. Secondly, given the coverage problems noted above in relation to the number of workers involved, then the number of days lost may under- or over-emphasise the impact of strike action.

Figure 3.5 presents the annual aggregates for the number of days lost. The volatility of this series is evident, and reflects the different composition of some of the major disputes in the last four decades. The statistics are dominated by three peaks; the first corresponds to the Post Office and car workers disputes in 1971 and the two miners' strikes of January-February 1972 and February-March 1974; the second reflects the strike by public service and hospital ancillary workers and the national engineering workers' stoppage in the late summer of 1979 together with the national steel strike in 1980; finally, the third is exclusively due to the miners' strike of 1984-85. Of these, only 1979 is exceptional in terms of the number of workers involved as seen in figure 3.3 above, while the peaks around 1971-74 and 1984 are the result of strikes that were extraordinary because of their length.

An indication of the relative scope of strike action is more clearly illustrated by the series for the number of days lost per employee in employment. This series is presented in figure 3.6 and reveals that only in the three years 1972, 1979 and 1984 did UK employees lose, on average, more than a single day each in stoppage activity. For the whole period, workers spent less than half a day per annum involved in overt strike action. This helps to place strikes in context; the number of days lost is an order of magnitude smaller than the number lost through sickness or industrial accidents. Knowles (1952) goes further and claims that

> "... it is clear that the loss in working time from other causes has hitherto been very much greater than the loss from strikes. Again the loss of time and output from managerial inefficiency, for example, may sometimes be considerable; but while the time lost by workers is measureable, that lost by managers is not." (p.251)

However, this does not imply that the study of conflict is unimportant; the impact of industrial disputes is concentrated both over time and in dispersion and strikes are therefore of much greater significance, particularly to the active participants. Typically, there can be no allowance for a strike while an average level of sickness can be indemnified against by proportional over-employment to the extent to which workers are expected to be ill. Finally, strikes are likely to affect workplace morale and disrupt output and may also have an inflated impact in the wider economy through shock and spillover effects.

In summary, the above description of the centrally collated statistics relating to strikes in Britain demonstrates firstly, that strikes are uncommon, and secondly, mostly of limited duration and size. Moreover, non-strike action is seen to be at least as prevalent as overt strike action.

3.3. Replication and Re-estimation of Two Selected Models.

As seen in chapter 2 above, there are a large number and wide variety of models for strike frequency. Although often theoretically quite different in origin, they tend to be empirically very similar and consequently fail to discriminate between the competing hypotheses that they purport to represent. Moreover, they have yet to be rigourously tested or extended to the whole of the post-war period and it can therefore be argued that a satisfactory empirical macro-model of UK post-war strike activity has yet to be established. This section of the thesis addresses these issues in particular.

The exercise to be performed is one of replication and re-estimation. Utilising a wider range of econometric techniques than was available to previous authors, the primary purpose is to discover to what extent competing extant models adequately describe strike frequency for the period for which they were originally specified, and to investigate their ability to explain subsequent strike activity. The two models chosen for comparison are those of Pencavel (1970) and Shorey (1977), this choice being governed by a number of considerations. Pencavel's study was effectively the first application of the model of Ashenfelter & Johnson to UK data and is one of the few that have an underlying theoretical model[23] and many of the other models cited in section 2.3 are similar in specification. An obvious exception is the paper by Shorey (1977) which is also unusual in including a lagged dependent variable, and can be considered to be representative of the class of models that is based on more informal reasoning. Both of these models were originally estimated for all disputes excluding coal mining over the same period, 1950(1) to 1967(2), and this will facilitate non-nested comparisons. Finally, these papers by Pencavel and Shorey remain two of the most widely cited of all studies of post-war strike frequency in the UK. It would therefore seem prudent to test and evaluate their alternative specifications given the recent advances in econometric methodology and the longer time-series now

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23. It should be noted that several heroic assumptions are necessary to translate their theoretical model into an operational specification. Full details are given in Ashenfelter & Johnson (1969) and discussed in sub-section 2.2.1 above.

available[24].

3.3.1. Replication of Pencavel (1970).[25]

Pencavel postulates that the number of stoppages beginning in each quarter is linearly related to a set of seasonal dummies ($Q1_t$, $Q2_t$, $Q3_t$), the rate of unemployment (U_t), the ratio of profits to total compensation (D_t), a moving average of real wage changes ($\sum\beta_i\Delta R_{t-i}$), and a time trend ($T_t$). His preferred basic specification and results are presented in column 1 of table 3.1. The six lag coefficients (β_i) on real wage changes (ΔR_{t-i}, $i = 1,\ldots,6$) are restricted by imposing a second degree polynomial with the far endpoint constraint ($\beta_7 = 0$) imposed, by the method of Almon (1965). This implies that only two parameters are freely estimated, from which the β_i can be recovered. Somewhat curiously perhaps, real wage changes are defined as,

$$\Delta R_t = 100[(W_{t+2}-W_{t-2})/2W_t - (P_{t+2}-P_{t-2})/2P_t] \qquad (3.1)$$

where W_t is an index of weekly wage rates and P_t is the retail price index. This specification may be intended to 'centre' the rate of change at the current period, although this does not explain the divisor of 2.

Despite Pencavel detailing his sources, it has not been possible to replicate his results exactly as can be seen in column 2[26]. However, they are qualitatively and quantitatively very similar and again provide broad support for the model of Ashenfelter & Johnson[27]. The number of strikes tends to be lower when unemployment is high, reflecting the lack of alternative job opportunities available to striking workers. Higher relative profitability raises workers' aspirations and hence

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24. Forrest (1990) attempts a similar exercise to that described below for the model of Davies (1979). However, she meets with little success in either replication or respecification..

25. The software used for this section of the chapter was Datafit (Pesaran & Pesaran (1987)) and Microfit, version 3.0 (Pesaran & Pesaran (1991)).

26. In particular, the coefficient on D_t is larger in both magnitude and significance.

27. The appendix, table A3.2, details the data sources utilised here.

tends to increase strike activity. Employees are less militant the larger recent real wage increases have been, and hence the coefficient on lagged real wage changes is negative. One immediate criticism is that although Pencavel excludes the current real wage change in his specification in order to avoid any simultaneous equation bias, he still includes contemporaneous D_t. Since the latter represents the employers' bargaining gain, it is also simultaneously determined with the level of wages and thus should again be included as a lagged variable on similar grounds. There tend to be more strikes in the first half of the year and the trend term is highly significant. This reflects the fact that over the sample period, non-coal strike frequency increased by nearly 60% as seen in figure 3.1. Pencavel offers a variety of explanations of this latter result, including poor industrial morale and increases in labour's bargaining power within a fairly inflexible institutional setting. The corresponding estimated lag coefficients are also presented in table 3.1. Those in the first column are taken from Pencavel (1970) and can be seen to follow an exponential pattern, while those for the replication in column 2 describe a seemingly less plausible U-shape. One possible explanation for the latter finding is that wage increases in the previous quarter are not as relevant to wage claims as those received the previous year (given that most UK employees only negotiate annually). That the largest lag coefficients are at lags 3 and 4 lends support to such an argument.

The first task is to evaluate the chosen specification. The polynomial restriction on the β_i, including the endpoint constraint, is easily supported by the data in a comparison with the unrestricted alternative[28]. Of rather greater importance perhaps, table 3.1 also presents some common tests of misspecification for the replication in column 2. These are described in greater detail in Godfrey (1988), Maddala (1988) and the references cited therein. While the Durbin-Watson test (DW) for first order

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[28]. An F-test yields a value of 0.347, which is less than any reasonable critical value from an F(4,49) distribution. Estimated regression coefficients of the unrestricted equation are very similar to those in table 3.1, column 2, and are therefore not reported here.

autocorrelation is (marginally) significant at the 5% level but is indeterminate at the 1% level, it seems more appropriate to test for higher orders of autocorrelation with quarterly data as used here. Correspondingly, an LM-test for (up to) fourth order autocorrelation is computed, and this is insignificant at the 5% level[29].

While there is no evidence of non-normality (NN) or heteroskedasticity (HETERO), a RESET test for functional form misspecification clearly rejects the formulation that Pencavel has chosen. However, this test cannot indicate precisely where the error might lay and there are several likely candidates. There may exist a problem over the definition of real wage changes, the price and nominal wage change components may not act symmetrically with respect to the frequency of strikes, there may be an omitted variable problem or the functional form chosen may be inappropriate. Each of these four possibilities is examined in turn below[30].

Table 3.1, column 3 reports estimates of Pencavel's specification with real wage changes redefined to be,

$$\Delta R_t = 100[(W_t - W_{t-4})/W_{t-4} - (P_t - P_{t-4})/P_{t-4}] \qquad (3.2)$$

which is more appropriate if employees do base their expectations and hence demands on *past* changes in real wages[31]. The estimated regression coefficients appear to be comparable to those in column 2, with the exception of the overall magnitude of the impact of real wage changes which is much smaller due to the omission of the divisor of 2 included in the definition in equation (3.1). However, the major difference that results from this respecification is in the pattern of the lag coefficients. These do now

29. It should be noted that misspecification tests of this kind can be sensitive to a number of misspecifications other than that for which they were originally formulated. This non-robustness to other specification errors is generally ignored in what follows, and hence any conclusions reached on the basis of such tests should always be treated with caution.

30. A fifth possibility that is ignored at this juncture is that W_t may be endogenous (see, for example, Knight (1972) *inter alia* and the discussion in section 2.3.2).

31. This is not the only possible alternative definition. For example, Shalev (1980) uses $\Delta R_t = 100[ln(R_t/R_{t-1})]$ where $R_t = W_t/P_t$.

trace out an exponential decay as found by Pencavel, although take positive (but insignificant) values at the longer lags[32]. More importantly however, there would still appear to be evidence of some form of misspecification.

To determine whether this has resulted from incorrectly assuming that price and wage changes have a symmetric impact on strike frequency, real wage changes were decomposed into their separate price and nominal wage change components. Table 3.2, column 1 reports the estimates that Pencavel obtained, while column 2 presents those obtained from the replication. Once again, these are qualitatively and quantitatively similar to those that Pencavel derived. The Almon restrictions for this model are again supported by the data, and an F-test of the restrictions implied by the estimates in table 3.1, column 2 against table 3.2, column 2 yields a statistic of 1.71, which is less than the 5% critical value from an $F(2,51)$ distribution. Hence the hypothesis that price and wage changes act symmetrically on strike frequency is not rejected by the data, a result also found by Pencavel.

The corresponding values of the lag coefficients are also reported in table 3.2. For the replication in column 2, these are both U-shaped and this is in contrast to the estimates that Pencavel reports (column 1) where only those on wage changes (γ_i) are U-shaped while the coefficients on price changes (δ_i) follow an exponentially declining trend. Additionally, while the alternative definition of rates of change given in equation (3.2) above makes little difference to the coefficient estimates (as can be seen by comparing columns 2 and 3 in table 3.2), the effects of past changes in prices and wages are very different when defined in this manner as revealed by the estimated lag coefficients in column 3. Once again the restrictions implied by table 3.1 (that it is only changes in *real* wages that are important) are supported by the data. Therefore, rather as expected, dichotomising real wage changes into their separate price and

32. An F-test reveals that the Almon restrictions (a second order polynomial with the far endpoint constraint imposed) are again not rejected by the data.

nominal wage components does nothing to alleviate the misspecification as revealed by the diagnostics at the bottom of table 3.2.

The third possibility noted above for the significance of the RESET test is that there are important variables omitted from the specification. In his study, Pencavel estimates an additional equation which attempts to account for the effects of incomes policies and the party of government and these can be regarded as potential omitted variables. Institutional factors like these have been found to be important by Davies (1979) among others. Pencavel's results and those from the replication are reported in table 3.3. The variable L_t takes value one when the Labour Party was in power, while J_t is unity when an incomes policy was in effect. Pencavel argues that although individually insignificant, his two dummy variables are jointly significant and are therefore important. However, a comparison of table 3.1, column 1 with table 3.3, column 1, reveals that this conclusion is incorrect, and that his calculated F-statistic of 5.08 (Pencavel (1970, p.248)) is in error. That these crude indicators of institutional arrangements are not statistically significant is confirmed by the results reported for the replication in table 3.3, column 2. A similar comparison to that attempted by Pencavel yields an F-statistic of 0.387 which is less than any relevant critical value[33]. Hence it would appear that these additional variables have not been erroneously omitted from the selected specification, and indeed the equation is still misspecified as indicated by the diagnostics reported at the bottom of table 3.3. For completeness, the final column in the table reports estimates based on the alternative definition of real wage changes as specified in equation (3.2). Once again, the two additional variables are not significant either separately or jointly, and the specification is rejected by the misspecification tests at the bottom of the table.

The last possible source suggested above for the econometric inadequacy of the

[33]. It should also be noted that the estimated coefficients on L_t and J_t are very different from those that Pencavel reports.

equation is that the functional form is inappropriate. Econometricians often use logarithmic or semi-logarithmic formulations, especially in time-series work of this kind. While a full logarithmic model would allow interpretation of the estimated coefficients as elasticities, given the approximately exponential trend in the dependent variable over the sample period, it was decided to examine a semi-logarithmic specification in which the logarithm of non-coal strike frequency is regressed on the same set of explanatory variables used previously. The resulting estimates for the original sample period and the initial specification are reported in table 3.4A. Both the Bera & McAleer (1983) test and the PE test suggested by Mackinnon *et al* (1983) support the selection of this semi-logarithmic model over the levels formulation in table 3.1, column 2 and the diagnostic tests at the bottom of table 3.4A indicate that it is econometrically acceptable on all criteria. It is therefore this specification that is preferred for the period 1950(1) to 1967(2).

Table 3.4B, column 1 incorporates the institutional factors considered previously. A simple F-test indicates that these are now important in explaining strike frequency. In particular, it can be seen that in periods when a Labour Government is in power, strike frequency is significantly lower. This seemingly provides some support for the hypothesis advocated by Turner (1963) that the links between the unions and the Labour Party will mean that workers are more reluctant to see a Labour Government 'embarrassed' by industrial conflict. However, there are obvious exceptions to this rather naive argument, with the so-called 'Winter of Discontent' of 1978/79 perhaps being the most damaging politically (see, for example, Whitehead (1985)).

Forecasting with the Semi-logarithmic Pencavel Model.

Given the econometrically satisfactory semi-logarithmic empirical specification, this sub-section reports on its ability to describe strike activity after 1967(2). In the following years, strike frequency more than doubled to reach a new peak in 1970, although has subsequently declined in the 1980s to less than half of the 1967 level.

The period selected for forecasting is restricted to 1967(3) to 1979(1) however. In that the election of the Conservative Government in May 1979 heralded a radical reform of labour law affecting both collective and individual rights, it seems likely that there will have been major implications for the conduct of industrial relations. In addition, the beginning of the 1980s marked the start of an economy wide recession which substantially altered the balance of power between employers and employees. During the 1970s however (perhaps with the exception of a greater emphasis on both statutory and voluntary incomes policies), the patterns of industrial relations continued to change only gradually, reinforcing the trend away from national agreements towards workplace bargaining that had begun many years previously. Given this relatively stable background, if the chosen model does indeed represent an appropriate and adequate description of the true data generating process, then it should be able to explain strike frequency over the whole period to 1979.

At the bottom of column 1 in tables 3.4A and 3.4B, tests for the forecasting ability of the model are reported[34]. These are Chow tests as reported in Chow (1960); the test for structural stability (SSTAB) tests the hypothesis of coefficient equality across the two periods, while Chow's second test, the predictive failure test (PRED), effectively examines whether the mean prediction error is significantly different from zero. For both tables, these decisively reject the hypothesis that the estimated model is able to predict strike frequency for the period 1967(3) to 1979(1). A caveat is necessary however, since both tests are conditional on homogeneity in the disturbances across the estimation and forecast periods. Fortunately, this hypothesis can be tested explicitly; the ratio of estimated variances from the two sub-periods was calculated, and this has an $F(n_2\text{-}k,n_1\text{-}k)$ distribution under the null, where n_i is the number of observations in period i, i = 1,2. Homogeneity is marginally rejected at the 5% level for table 3.4A, (but not at 1%), although it is accepted for table 3.4B when the

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34. The incomes policies variable J_t is extended using information derived from Davies (1979) and Clegg (1979).

institutional factors are included. Hence there would seem little doubt that the results of the Chow tests are acceptable, and thus the conclusion that the chosen specification fails as an appropriate model for the 1970s is inescapable[35].

The generally poor predictive performance of the model is starkly illustrated in figure 3.7, which is based on the estimates presented in table 3.4B, column 1. Clearly the model fails to predict the peak in (log) strike frequency around 1970, and greatly over-predicts in the late 1970s when strike frequency no longer continued to increase. Undoubtedly, this failure is principally the consequence of the highly significant positive trend term which dominates the estimates for 1950(1) to 1967(2)[36].

The second column in tables 3.4A and 3.4B report separate estimates for the forecast period 1967(3) to 1979(1). Comparing these with the coefficients in the first column demonstrates more precisely the reason why the model fails to predict beyond the sample period; in particular, the previously highly significant trend term (T_t) has a much smaller coefficient and is no longer significant. Additionally, the coefficient on the ratio of profits to total compensation (D_t) is now small and insignificant, while the rate of unemployment U_t is an important 'explanatory' variable in this period, reflecting the increase in the rate of unemployment at a time when strike frequency was falling. Of course, this does not necessarily imply a causal relationship. Incomes policies would also appear to have had a suppressing effect on the number of strikes, perhaps by lowering the minimum acceptable wage increase as suggested by Ashenfelter & Johnson (1969). However, as indicated by the diagnostics at the bottom of both tables, that there is evidence of residual autocorrelation means that these estimates, though unbiased, are not efficient and the inference procedures are

35. While it has already been noted that Pencavel's specification is misspecified for his original sample period, his model was also subjected to the same analysis as performed for the semi-logarithmic respecification. As expected, its forecasting performance was even more disappointing.

36. A further analysis reveals that the model is not even able to forecast satisfactorily from 1967(3) through to the peak in 1970. Hence the problem does not appear to be a structural break at or around the peak in strike frequency.

invalid[37]. Hence these results must be viewed with some caution.

The third and final column in tables 3.4A and 3.4B reports the estimates from pooling the data and estimating the models over the whole period 1950(1) to 1979(1). The results serve to confirm the analysis above in that the model would not appear to be an adequate description of strike frequency for the whole of this period. While the coefficient estimates reflect the increase in strike frequency up until 1970 (in the positive trend term), and the subsequent decrease (in the negative unemployment coefficient), both specifications are decisively rejected by several of the misspecification tests employed. In particular perhaps, the dynamic behaviour of non-coal strike frequency has not been adequately captured by the specifications employed here.

It must therefore be concluded that neither Pencavel's chosen formulation nor the replication examined here can provide a satisfactory empirical model of non-coal strike frequency in Britain. Whether the rather different model suggested by Shorey (1977) is more successful is examined in the next sub-section.

3.3.2. Replication of Shorey (1977).

As reviewed in sub-section 2.3.2 above, Shorey (1977) discusses the relevant influences on the probability of strike action in a single bargaining unit. However, in aggregation, the operational specification requires the assumption that the number of bargaining units is constant through time. This then implies that strike probability is

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37. Note that this conclusion is only valid if the autocorrelation is 'genuine' serial correlation rather than the result of some form of model misspecification.

proportional to the number of strikes[38]. Shorey's final specification for the period 1950(1) to 1967(2) omits all 'insignificant' variables and for comparative purposes, his estimates are reported in table 3.5, column 1; the number of non-coal strikes is linearly related to a dummy variable for the first quarter ($Q1_t$), real profits ($(\pi/P)_t$), lagged changes in wages (ΔW_{t-1}) and prices (ΔP_{t-1}), the current price level (P_t) and non-coal strike frequency in the previous quarter (S_{t-1}).

Attempts to replicate these estimates have been fairly unsatisfactory. In part, this is because Shorey fails to give details of his data sources, base years used or the precise specification of the rate of change variables. Table 3.5, column 2 presents estimates when annual rates of change are used, that is

$$\Delta W_t = (W_t - W_{t-4})/W_{t-4} \quad \text{and} \quad \Delta P_t = (P_t - P_{t-4})/P_{t-4} \tag{3.3}$$

while column 3 utilises quarterly rates of change defined as

$$\Delta W_t = (W_t - W_{t-1})/W_{t-1} \quad \text{and} \quad \Delta P_t = (P_t - P_{t-1})/P_{t-1} \tag{3.4}$$

As can be seen, neither of these duplicates column 1 very precisely. While all of the coefficients are of the correct sign, many are of very different magnitudes and this may reflect differing normalisations. Several other rates of change specifications were also examined but none produced estimates that matched those that Shorey obtained.

The diagnostics at the bottom of table 3.5 indicate that the replications are apparently misspecified. Although there is no evidence of serial correlation in the disturbances[39], both the RESET test and the test for heteroskedasticity are rejected by the

38. This assumption is obviously unrealistic; over the period in question, the number of unions fell through amalgamations, and there was a growth in average firm and establishment size. Thus the number of bargaining groups cannot really be considered constant over time although the crucial issue is whether the assumption has important consequences for the interpretation that can be given to the results he obtains. This is difficult to assess. Moreover, the model is not really of strike probability since it is quite possible for there to be *more* than one strike at a bargaining unit in any time period. It would seem more appropriate to view the model as either an approximation to the incidence of strikes (at the level of the bargaining unit if these are assumed constant in number), or as a descriptive model of strike frequency, though the latter interpretation detracts rather from the original rationale that Shorey uses to specify his model.

39. Only the absolute value of Durbin's h-statistic (Durbin (1970)) is reported in the tables.

specification (although these may of course be interrelated). The results presented in sub-section 3.3.1 above for the replication of Pencavel's model most readily suggest that the semi-logarithmic transformation may be more appropriate. Therefore, table 3.6A, column 1 presents the estimates obtained when this alternative dependent variable is utilised together with annual rates of change for ΔW_{t-1} and ΔP_{t-1} as in equation (3.3)[40]. This specification can be seen to be accepted by all the criteria employed. Furthermore, when compared with the estimates presented in table 3.6B, column 1, an F-test for the omission of dummy variables for the second and third quarters and the institutional factors L_t (Labour party in Government) and J_t (incomes policies in operation) considered previously reveals that these additional regressors do not contribute significantly to the chosen model. All the coefficients have their expected signs and are almost identical when these supplementary variables are included. Thus this is the basic formulation employed for forecasting in the next sub-section.

Forecasting with the Semi-logarithmic Shorey model.

The results from forecasting over the period 1967(3) to 1979(1) are given at the bottom of table 3.6A, column 1. These Chow tests are identical to those discussed in relation to tables 3.4A and 3.4B above and they show that once again the model fails to predict over the latter period. One weakness of the tests is that the requirement of homoskedasticity would seem to be violated as revealed by the variance ratio test and hence they may have unacceptably low power. However, when the additional quarterly dummies and the institutional factors are included as in table 3.6B, column 1, this problem disappears while the model still fails to forecast satisfactorily. Moreover, that the model fails to describe the more recent trends in strike frequency is confirmed by examining figure 3.8, derived from the estimates in table 3.6B, column 1. Once again, the model is unable to predict the peak in (log) strike

40. The estimated coefficients are very similar when quarterly rates of change are used and hence these results are not reported here.

frequency around 1970 and its subsequent levelling-off and finally substantially over-predicts in the late 1970s when the levels of strike activity fell[41].

Estimates of the semi-logarithmic model for the period 1967(3) to 1979(1) are presented in the second column in table 3.6A. This indicates that the economic influences on strike frequency as captured by the respecification of Shorey's model are very slight for this period[42]. Most of the 'explanation' of the dependent variable is derived from its lagged value, and hence the model describes a simple first order autoregressive process with no cyclical variations[43]. However, as revealed in table 3.6B, column 2, the four additional variables are highly significant in this period, and the other parameter estimates are clearly not robust to their inclusion. Real profits and prices are now strongly significant, a Labour Government is associated with more strikes per quarter while incomes policies are seen to suppress strike activity. Finally, strike frequency is significantly higher in the first nine months of the year. These results clearly warrant further investigation although it is important to note that the rather strong evidence of autocorrelation in the disturbances together with the presence of the lagged dependent variable means that the OLS estimates are both biased and inconsistent, and their t-ratios are invalid. Hence the conclusions drawn above should be treated with some caution.

For completeness, estimates of the model for the whole period under consideration are presented in column 3 of tables 3.6A and 3.6B. As might be expected from the discussion above, most of the explanatory power rests with the lagged dependent variable, although the cyclical nature of real profits is also important. Over the whole

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41. Once again, the model is not even able to forecast through to the peak in strikes in 1970.

42. A test for zero-restrictions on all variables except the intercept and $ln(S_{t-1})$ is easily rejected by the data, and thus the problem does not appear to be one of multicollinearity.

43. Estimating the model in levels yields similar results and hence this finding is not a result of the respecification of the dependent variable.

period 1950(1) to 1979(1), the four additional variables in table 3.6B are not jointly significant at conventional levels and the dynamics of the model would again appear to be misspecified. The overall conclusion from the replication is that even though the semi-logarithmic respecification of Shorey's model is satisfactory for the original sample period 1950(1) to 1967(2), it fails to describe subsequent strike frequency and certainly cannot provide an acceptable model for the whole of the post-war period.

3.3.3. Commentary, Comparisons and Prospects.

The attempts at replicating the results of two popular quarterly models of strike frequency over the period 1950(1) to 1967(2) indicate that, at least for the data and formulations employed here, the original specifications are inappropriate. A semi-logarithmic transformation is found to be econometrically acceptable for both models although in several non-nested comparisons[44], all favoured the modification to Shorey's formulation over that of Pencavel. However, this finding is of little value in itself since both models fail to predict strike activity beyond the original sample period. Neither can predict the level of strikes in the period immediately following up to the peak in strikes in 1970, let alone beyond and through to 1979 as illustrated in the tables. Re-estimating both specifications over the whole of the post-war period is also found to be unsatisfactory even with sympathetic treatment of institutional factors such as incomes policies[45]. Much of the explanatory power of the models over this longer period lies with the trend term (Pencavel) or with the lagged dependent variable (Shorey) and therefore any description of strike frequency in terms of cyclical economic variables is rather ambiguous.

The prospects for estimating models of the type presented by Pencavel and Shorey are still uncertain however. Although the formulations examined here are rejected by the

[44]. See, for example, the surveys by MacKinnon (1983) and McAleer & Pesaran (1986).

[45]. Forrest (1990) observes a similarly unsatisfactory performance for her replication of Davies' (1979) model.

data, this does not necessarily imply that a satisfactory aggregate empirical macro-model of strike frequency does not exist since a more carefully chosen dynamic specification (based on a suitable model) could yield more satisfactory results than those discussed above. However, it seems prudent first to consider the central question as to whether strike frequency is a cyclical phenomenon since this has yet to be established by the investigation presented previously. Such information will clearly be relevant to any new theoretical model and empirical specification and thus section 3.4 presents a detailed analysis of this conjecture.

3.4. The Cyclicality of British Strike Frequency.[46]

Persuasive theoretical explanations for the cyclical behaviour of strike frequency have been provided by the more recent asymmetric- or private-information theories as described by Hayes (1984) and Tracy (1984, 1987) among others and reviewed in sections 2.2.3 and 2.3.4. Representative of this class of models, Hayes considers a situation in which the firm has more information about the state of the product market than the union. The union designs its wage-strike proposals such that in the 'bad state', the firm will prefer to take a strike and pay a lower wage, while in the 'good state', the firm will find it profitable to grant the union's higher demands immediately. Hence only 'bad state' firms incur strike action. However, if the union *perceives* that the firm is doing well, perhaps on the basis of information derived from some general economic indicator, it is more likely to propose a higher wage-strike schedule. Thus there will tend to be more strikes at the top of the cycle through the union incorrectly evaluating the firm's profitability. Counterfactually, in a slump when all firms appear to being doing poorly, the union will tend to offer a lower wage-strike schedule and thus there is a smaller probability of overestimating the firm's performance. Hence

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[46]. The software used in this section of the chapter was written by the author in VS-Fortran running on an IBM-4381.

these models predict that strike activity should be procyclical[47].

However the identification of any cyclicality in strikes is fundamentally flawed in the standard multiple regression models that have typically been estimated. As noted by Kennan (1986);

> "...a negative regression coefficient associated with the unemployment rate does not necessarily mean that strikes are procyclical, if the regression also includes variables such as prices, wages and profits which may vary systematically with the cycle." (pp.1119-1120)

As discussed in section 2.3.2 above, studies of strike frequency in Britain have utilised a variety of dependent variables and econometric specifications and cover many different time periods making any direct comparisons difficult. However, the realisation of the 'identification' dilemma when attempting to discern possible cyclical patterns in British strike frequency has been noted by Davies (1979) who observes that,

> "... the expected inverse relationship between unemployment, used as a proxy for the state of the labour market, and strike frequency has not been confirmed by all studies, while coefficients with both positive and negative signs have been found to be significant for real corporate profits." (p.205)

The attempted replications and extensions of the studies by Pencavel and Shorey in section 3.3 also reveal such problems (as well as yielding estimates which are clearly not robust to the estimation period under consideration). As can be seen from figure 3.7 and figure 3.8, even within the estimation period, there is only very weak evidence for any cyclicality in the predicted values, and these 'cycles' would not appear to correspond with the business cycle which has a periodicity of around five years[48]. Thus Kennan's critique would certainly appear to be pertinent and may be the underlying reason for the generally unsatisfactory performance of the aggregate

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47. Note that the role of employers in these models is minimal since they are presumed to hold the informational advantage in every case.

48. Of course, this finding could be interpreted to imply that aggregate strike frequency is NOT cyclical. This issue is discussed in much greater detail below.

econometric models.

One plausible solution to this difficulty would be to compare levels of strike activity with a *single* measure of the business cycle and such an exercise is conducted in this section of the chapter. As described in more detail in sub-section 3.4.1 below, the National Bureau of Economic Research (NBER) methodology for the analysis of business cycles can be used to examine the concordance of the changing level of strike activity with the timing of the cycle. For the US, this kind of analysis of strike frequency has a long history from Jurkat & Jurkat (1949) through Rees (1952), O'Brien (1965) and Weintraub (1966) to Kennan (1986) who also summarises the previous studies. Kennan demonstrates that while the timing within any single cycle is variable, an average taken over all fourteen business cycles from 1915 to 1980 reveals that strike frequency rises during the up-swing and falls during the down-swing of the cycle[49]; that is, strike frequency is procyclical.

A similar and rather more comprehensive exercise has been performed recently by Harrison & Stewart (1990) for Canadian strike activity over the period 1946 to 1983. Their results reveal that both the issue and whether the strike occurred at the end of a contract (termed contract strikes) are important factors in determining the correspondence between strike activity and business cycles[50]. There is some indication of cyclicality at the aggregate level, although little evidence of a stable cyclical effect. Moreover, the disaggregated series reveal that,

> "... among all strikes, the procyclical frequency ... derives predominantly from noncontract strikes, and that contract strikes over wage issues alone show no evidence of procyclical frequency." (p.12)

In addition, for their contracts data, Harrison & Stewart find that only strikes over non-wage issues demonstrate procyclical incidence. Thus, while strikes over wages

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49. However, this relationship seems rather stronger earlier in the period, in particular, prior to World War II.

50. The US studies cited above fail to disaggregate the total number of strikes either by issue or the point at which the strikes occurred during the contract period.

are the central concern of the theoretical bargaining models, these strikes do not appear to exhibit any systematic cyclicality for Canadian post-war data.

There are two related studies for British strike frequency. Firstly, Knowles (1952) presents a graphical analysis for the pre-war period and reveals a positive relationship between strikes and the level of wages and prices, and a (weakly) negative relationship between strikes and the unemployment rate. Secondly, Mayhew (1979) constructs the detrended seasonally adjusted logarithm of quarterly non-coal strike frequency for the period 1958 to 1974. This is regressed on a similarly detrended index of GDP and a positive coefficient is obtained. He concludes,

> "There is thus a significant, but not very strong, relationship between strike activity and cyclical movements in the economy." (p.12)

He attributes this finding to the fact that the cycle affects both the variables which determine the size of the initial offers and demands, and the determination with which these demands are pursued. This section concentrates on examining this result in more detail and investigates its robustness over a rather longer time horizon and for various disaggregated strike measures. Sub-section 3.4.1 briefly explains the NBER methodology used for the identification of British post-war business cycles and describes the disaggregated strikes series. Thereafter, a similar analysis of British strike frequency to the US and Canadian studies cited above is presented in sub-section 3.4.2.

3.4.1. National Bureau of Economic Research Methodology and Data Description.

Burns & Mitchell (1946) describe the construction of a variety of tables which can be used to compare the duration, timing and magnitude of cycles in economic time-series[51]. Here the intention is only to match the *timing* of the specific cycle of interest

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51. Although their analysis is primarily designed to be used with monthly data, it can be appropriately modified for series at lower frequencies.

(strike frequency) with that of some reference business cycle. Having identified the dates of the turning points of the business cycle, the Burns-Mitchell method proceeds as follows[52]. Each business cycle is divided into nine phases; phase 1 is the three months centred around the initial trough, phase 5 includes the three months around the peak and phase 9 is the three months around the terminal trough (and which therefore corresponds with phase 1 of the next cycle). The up-swing, identified by phases 2, 3 and 4, splits the months between initial trough and peak into three equal length periods, and the down-swing is similarly defined over phases 6, 7 and 8. Over each complete cycle, an index of the average monthly level of strike activity is constructed as a base and then the relative levels for each phase are computed[53]. If strikes are unrelated to the cycle, these nine indices should be randomly distributed, whereas if strikes are procyclical, the indices should increase over phases 1 to 4, reach a peak in phase 5 and decrease thereafter.

Thus the first task in the analysis is to identify the timing of the reference business cycles. Monthly data are required to define precisely the turning points and typically an index of industrial production is utilised since this is often the only monthly series available covering a sufficiently long time series. Figure 3.9 shows the strongly trended and highly seasonal Index of Production (IOP) series for the period January 1946 to December 1987[54]. Identifying any cycles and their turning points is extremely difficult and hence the 12-month centred moving average of the logarithm of IOP was constructed to remove the seasonality in the series. This was then detrended by taking the residuals from a regression on time and time-squared and the resultant series is

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52. The following is a description of a table of type R1 according to Burns & Mitchell (1946).

53. The first and last months are weighted by 0.5 to avoid the downward bias from including two trough values in the index (Burns & Mitchell (1946, p.131)). Of course, this adjustment will only affect the amplitude of the strikes cycles and not their timing.

54. Source: Monthly Digest of Statistics and unpublished data provided by the Central Statistical Office.

plotted in the top panel of figure 3.10[55]. Clearly identifiable cycles are apparent with a fairly regular frequency although no downturn is discernible after the trough in 1981.

Since the timing of the turning points is crucial to the Burns-Mitchell methodology, it is important to compare the correspondence of the Index of Production cycles with other available cyclical measures of economic activity. One strong candidate is the Concurrent Cyclical Indicator (CCI) series published by the Central Statistical Office (CSO) and available from 1957 onwards[56]. The CCI is a composite series based upon the three quarterly measures of GDP (output, expenditure and income) together with the monthly Index of Volume of Retail Sales and the Index of Production, plus information derived from the Confederation of British Industry's Capacity Utilisation Index which forms part of their quarterly Industrial Trends Survey. These are then amalgamated to yield the composite CCI index. The detrended moving average of this series is plotted in the lower panel of figure 3.10 and displays a very close correspondence with the IOP cycles shown in the top panel. A closer examination of the numerical values of the two residual series reveals that no turning points are more than one month apart and thus the seven post-war cycles identified from the longer IOP series were chosen as the reference cycles to be used in the Burns-Mitchell analysis below[57].

Reiterating the description given in section 2.2, strikes in Britain are recorded by the

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[55]. Higher order detrending terms were not significant at conventional levels and did not alter the timing or magnitude of the cycles identified in figure 3.10.

[56]. A full description of the compilation of this monthly series is given in Economic Trends, March 1975 and May 1976.

[57]. The turning points identified from the CCI are slightly different from those presented by the CSO for their reference cycle. This is a result of the degree of subjectivity introduced by the CSO who attempt to discount "(k)nown special economic or climatic factors . . . in judging the location of reference turning points" (Economic Trends, May 1976, p.70). However, there is still never more than three months disparity between their turning points and those utilised in the analysis below.

Department of Employment from information gathered through local Unemployment Benefit Offices, returns from Nationalised Industries, public bodies and large firms, statements in the press and, for some larger disputes, directly from the organisations involved. There are problems in ensuring complete coverage and, partly for this reason, stoppages involving fewer than ten workers and those lasting less than one day are excluded from the statistics unless the total number of working days lost exceeds one hundred. The annual aggregates for monthly strike frequencies disaggregated by cause and sector are summarised in table 3.7[58].

Several points need to be noted with respect to these data. Firstly, while the monthly data for all strikes (column 1) and coal strikes (column 4) are re-published with a 13 month lag following revisions, monthly data for strikes disaggregated by cause of stoppage (columns 2 and 3) are only available contemporaneously. Hence the numbers of pay and non-pay strikes tend to underestimate the actual totals, although this under-recording is likely to be fairly consistent and therefore should not affect the *timing* of any cycles in strike activity[59]. Secondly, given the dominance of strikes in the coal industry particularly in the immediate post war period, it would seem useful to investigate separate series for pay and non-pay strikes in the non-coal economy. Unfortunately such series are not available on a monthly basis but can be approximated if, for each month, the ratio of pay to non-pay strikes in the non-coal sector can be assumed to be the same as in the whole economy. The annual aggregates for the monthly series constructed under this assumption are summarised

58. Source: Employment Gazette (and its predecessors).

59. Annual totals for pay and non-pay strikes consistent with column 1 are available, of course, and form the basis of the discussion in chapter 2. The apparent falling efficacy in the recording of strikes (in that the sum of columns 2 and 3 is a falling proportion of the (revised) total in column 1) is closely correlated with the falling proportion of coal strikes in the economy. A possible explanation may lie in the more extensive data collation service in the coal industry as compared with the wider economy.

in columns 6 and 7 of table 3.7[60]. The coefficients of variation at the bottom of the table reveal that the number of pay strikes has been much more volatile than the number of non-pay strikes, particularly in the non-coal sector. This mirrors the findings for the annual series discussed in section 3.2 and illustrated in figure 3.2.

That there are strong seasonal patterns in strike frequency is evident from table 3.8 which illustrates the distinct troughs in December and July and peaks in October and March. It is also apparent that each of the seven strikes series exhibits approximately the same degree of seasonal variation. As noted by Sapsford (1975), this pattern reflects the seasonality of wage negotiations combined with that of production. He confirms this seasonal bimodality with peaks in spring and autumn for a series taken over the rather longer period 1893 to 1971.

3.4.2. An Assessment of the Cyclicality of British Strike Frequency.

To identify any cycles and their turning points, the strikes series need to be detrended and seasonally adjusted. The residuals from 12 month centred moving averages regressed on a cubic polynomial in time were computed for all of the disaggregated measures of strike frequency summarised in table 3.7[61]. These indicators of the cyclicality in strikes were then examined for any correspondence with the business cycle.

As an initial, although rather crude, comparison, the standardised residuals from the

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60. Some assessment of the reliability of this exercise has been conducted. Since 1959, the Employment Gazette has published *annual* totals for pay and non-pay strikes disaggregated by industrial sector and thus the constructed series for non-coal strikes can be directly compared to the official statistics on an annual basis. For both pay and non-pay strikes, the series are virtually indistinguishable from these published statistics, and hence the exercise conducted would appear to be legitimate. Any inappropriately induced monthly variations will be nullified since seasonally adjusted series are used throughout the Burns-Mitchell analysis presented below. However, as a further check on the validity of the method of constructing these non-coal strikes series, the ratio of the annual number of pay and non-pay strikes in the non-coal sector from 1959 was used to decompose the monthly non-coal strikes series from that date. None of the conclusions presented below are affected by this second decomposition and the detailed results are little changed.

61. Higher order detrending polynomials made little difference to the results obtained.

detrended moving average series for industrial production and total strike frequency are plotted together in figure 3.11[62]. Discounting the first business cycle from September 1946 to October 1952, this graph reveals a surprisingly close correspondence in the timing of the troughs and peaks of the two series[63]. Even for the period after the trough in 1981 when no complete cycle can yet be identified, it is apparent that the cyclical components of the two series move quite closely together. Figure 3.11 would therefore indicate that post-war strike frequency in Britain is procyclical. This is in contrast to the results that Harrison & Stewart (1990) present for their Canadian data; prior to the 1960s, there is no evidence of any cyclicality in their strike frequency series.

This finding is confirmed by the Burns-Mitchell analysis presented in the first panel of table 3.9. The trough-peak-trough dates of the business cycles identified in the IOP series in sub-section 3.4.1 are given in the left hand column, while the relative levels of strike activity in the nine divisions of each cycle are recorded in the body of the table[64]. Interpretation of the statistics is as follows; in line 2 for the business cycle running trough-to-trough from October 1952 to October 1958, strike frequency was 67.5% of the average for this cycle in the initial trough, reached a peak of 120.2% of the average halfway through the downswing and fell to 72.9% of the average by the end of the cycle. Except for the first cycle which would appear to be inverted[65], it can be seen that in general, peaks in strike activity do roughly correspond with peaks in industrial production, and thus strike frequency is procyclical. The mean taken over all seven cycles given at the bottom of the panel confirms this general conclusion,

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62. Standardised (or 'normalised') residuals are obtained by dividing the residuals by their standard deviations. This produces series with (a mean of zero and) a variance of one and thereby facilitates comparisons between series.

63. Note that the residuals have been normalised and thus no inference can be drawn from the relative magnitude of the cycles.

64. The relative peaks in strike frequency for each business cycle are given in italics.

65. That is, the peak in strike frequency corresponds to the trough in industrial production and *vice versa*.

with the peak in strike frequency falling on average in phase 6 of the cycle[66]. Of course, this result is strengthened when the first cycle is excluded from the calculations; the inverted U-shape becomes more pronounced and the averages for phases 4, 5 and 6 are then more than 50% greater than at the start and end of the mean cycle. Thus there would appear to be fairly strong support for the proposition that aggregate strike frequency is procyclical.

Whether this procyclicality is a general feature of strike activity can be determined by performing similar analyses for the component disaggregated series of strike frequency. These are presented in the subsequent panels of table 3.9. Panel (2) and panel (3) show that both pay and non-pay strikes exhibit a degree of procyclicality although this tendency is much stronger for pay strikes both in terms of the concordance with business cycles and the distinction between troughs and peaks of each cycle. For non-pay strikes, the relative peaks tend to occur before the peaks in the cycle in business activity and the cycles are less pronounced. Panel (4) illustrates that for coal strikes, there is no systematic pattern whatsoever, with peaks in strikes appearing randomly throughout the phases of the business cycles. Correspondingly, panel (5) reveals that it is indeed strikes outside the coal industry that are strongly procyclical. In particular, the inverted first cycle apparent in panel (1) for all strikes can now be seen to be entirely the consequence of the dominance of the pattern in coal strikes in this period. When these are excluded as in panel (5), the number of strikes is once again seen to be closely correlated with the corresponding business cycle.

This finding would appear to confirm the suspicion that coal strikes may dominate in the immediate post-war period and provides further justification for the construction of pay and non-pay strike series for the non-coal sector as detailed in sub-

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66. Harrison & Stewart (1990) find a similar slight lag in many of their disaggregated Canadian strikes series.

section 3.4.1. The Burns-Mitchell analyses for these series are presented in panels (6) and (7) and the results are very much as expected. It is cycles in *pay* strikes that are highly correlated with cycles in production and are particularly pronounced; non-pay strikes exhibit less cyclicality in general both in magnitude and in correspondence with business cycles. This is encouraging in that theories of strike activity tend to be more applicable to grievances arising over rates of remuneration than to those arising over more general disagreements. It is also starkly in contrast to the Harrison & Stewart (1990) results which find no relationship between strikes over wage disputes and Canadian business cycles.

The Burns-Mitchell analysis therefore reveals a remarkably close correspondence in the timing of cycles in production and strike frequency and thus provides circumstantial support for those theories that predict that (pay) strikes are procyclical[67]. Given the inconclusive evidence from the econometric models of strike frequency presented in section 3.3, this finding is perhaps surprising. However, as suggested above, undoubtedly part of the difficulty with the econometric models is their inclusion of so many cyclical indicators (some which lag and some which lead the cycle) and which, in combination, would therefore appear to obscure the evident cyclicality in strikes in Britain.

As guidance for future research, the evidence presented above suggests that strikes in the coal industry should continue to be treated separately from strikes elsewhere in the economy. Additionally, given the differences in the timing and magnitudes of their cycles, pay and non-pay strikes should also be distinguished. Finally, given that

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[67]. One possible criticism is that this may partially reflect the cyclicality in the number of negotiations taking place, and thus says little about any procyclicality in the *incidence* of strikes. However, worker-firm relationships in Britain are such that the number of bargaining groups changes fairly slowly over time and each bargaining pair tends to conduct negotiations at a similar time every year. In addition, each cycle is treated separately in the Burns-Mitchell analysis and on average is only five years long in the post-war period considered here. Hence any potential effects this problem could have should be minimised. However, the general issue of strike frequency *vs* strike incidence is returned to in section 3.6 and, in more detail, in chapter 4.

the general conclusion from this section is that there is strong evidence of procyclicality in strike frequency, then the inclusion of an *appropriate* concurrent indicator of the cycle in any future econometric study should serve to reveal this cyclicality in strikes. This would then leave the non-cyclical component of strikes to be explained by the other (non-cyclical) variables included in the model. The next section presents a preliminary investigation of the application of these proposals to a quarterly 'cyclical-political' model of post-war strike frequency.

3.5. A Cyclical-Political Model of Strike Frequency in Britain.

The results presented in the previous two sections indicate that while post-war aggregate (non-coal) strike frequency would appear to be poorly described by conventional econometric specifications, there is undoubtedly a strong cyclical element to the patterns in certain classes of strikes, in particular those that result from disputes over levels of remuneration. This section attempts to integrate these findings into a new empirical description of strikes which any satisfactory theory of strike frequency must embrace. While the results revealed in section 3.4 above are derived from monthly data, in order to facilitate comparisons with the previously published studies and the respecifications detailed in section 3.3, the estimates presented in this section utilise quarterly data. Sub-section 3.5.1 outlines the specification of the empirical model while sub-section 3.5.2 details the method of estimation and the results obtained. Finally, sub-section 3.5.3 presents some brief observations on recent related attempts to estimate models of strike frequency together with the consequences in the context of the cyclical-political specification considered in this section.

3.5.1. Description and Specification.

Broadly speaking, the chosen specification might be described as a 'cyclical-political' model. As noted in section 2.3.2 above, almost all empirical models of strike frequency include variables which can be construed as cyclical indicators, even though they are often acting as proxies for other information which is not available. These variables include price changes, variations in nominal or real wages, the level or rate of unemployment, measures of profitability and so on. For the current study however, only (the logarithm of) the quarterly seasonally unadjusted index of production series ($CYCLE_t$) has been included[68]. This is the quarterly counterpart of the monthly series utilised in section 3.4 above, although it is used here without prior filtering since interest is now focussed on the magnitude of the seasonal and trend responses as well as the timing of the cyclical variations in strike activity to general movements in the economy. Quarterly dummies and a quadratic in time are included to capture the systematic seasonal and trend variations in strikes over time.

The basic estimating equation can therefore be written as

$$lnS_t = \beta_0 + \beta_1 Q_{1t} + \beta_2 Q_{2t} + \beta_3 Q_{3t} + \beta_4 T_t + \beta_5 T_t^2 + \beta_6 CYCLE_t + u_t \qquad (3.5)$$

in obvious notation[69]. This should not be regarded as an explanatory model of strike frequency but as a decomposition of the number of strikes into seasonal, trend and cyclical components. The residuals u_t are thus the constituent part of strike frequency which is unrelated to the cycle. As noted by Harrison & Stewart (1990, p.6, fn.17), the coefficient estimates β_j from equation (3.5) will be unbiased despite these missing non-cyclical variables since, by definition, the latter are uncorrelated with the business cycle. Estimates for the simple decomposition represented by equation (3.5) are presented in the next sub-section and comparisons are made with the rather more 'sophisticated' respecifications of previous econometric studies considered in section 3.3 above.

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68. This variable should capture the *concurrent* changes in strike activity with the business cycle, including any impact arising from bargaining over a larger 'pie' or any increase in uncertainty over price and/or real wage levels at the peak of the cycle.

69. The close similarities with the work of Harrison & Stewart (1990) for Canada should be acknowledged at this juncture.

In addition, and as an extension to the basic formulation in equation (3.5), 'political' variables are subsequently included in order that their separate impact on strike frequency can be ascertained. While the issue of the possible endogeneity of the implementation of these policies is not treated in the analysis below, the estimates reported do serve to give some indication as to the influence of political factors that a satisfactory model of strike frequency would need to incorporate. Specifically. the 'political' side of the model represents an attempt to capture the broad effects of major policy decisions and enactments by parliament which seem likely to affect the level of industrial conflict. In particular, it is their likely effect on the drive and ability of workers to engage in direct and overt industrial action in support of any claim that the model attempts to identify.

Recent work by Freeman & Pelletier (1990) on the impact of industrial relations legislation on trade union density provides a readily accessible index of labour law. They construct four measures to represent the 'favourableness' of UK industrial relations laws towards unionisation, each coded from 1 (least favourable) to 5 (most favourable), and which are then aggregated to yield two indices of labour legislation. The first relates to union organisation *per se* and embodies those laws pertaining to recognition and bargaining rights and enactments relating to individual rights to associate/disassociate from trades unions. The second reflects the relative power of unions and management in collective bargaining and encompasses parliamentary acts giving immunities to unions engaging in industrial disputes together with a subjective index measuring the relative power of trades unions *vis a vis* employers.

There are undoubtedly problems with the necessarily *ad hoc* assignment of cardinal values to the two indices. Freeman & Pelletier (1990) state that they,

> "... tried to scale the indices across time ... in a consistent way so that one-unit differences in the indices reflect roughly comparable differences in the laws." (p.150)

In fact, the two indices are highly correlated[70], and thus both reflect rather generally the changing legal climate in which unions have been operating in the post-war period. However, despite the similarities, Freeman & Pelletier find that it is only the index of organisation (ORG_t) that has a significant (positive) impact on the level of trade union density in the UK in the period 1945-1986, while the index of collective bargaining (BAR_t) has a negligible and insignificant effect.

The changing legal climate in the post-war period and its likely impact on the level of industrial conflict is discussed in detail in chapter 4, section 2. At this juncture, it is sufficient to note that the *a priori* expected signs on the Freeman & Pelletier legal indices in the context of strike activity are indeterminate in general; an improvement in the ability of a union to collectively organise its members and/or a strengthening of its relative bargaining power may not necessarily lead to an increase in industrial action if this is counteracted by a more conciliatory attitude of employers in the face of such circumstances. In a similar fashion, the undoubtedly weakened position of unions in the 1980s may not necessarily imply a reduction in conflict activity if the employers' response is to become more confrontational in the light of its enhanced relative position. The two variables ORG_t and BAR_t are included linearly in equation (3.5) and, once again, it should be noted that the coefficient estimates will be unbiased as long as these policy variables are orthogonal to any variables excluded from the model.

In summary therefore, the empirical model estimated in the next sub-section relates the logarithm of strike frequency to seasonal and trend variables, a measure of the

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70. They are both roughly constant until 1974 and then rise steeply with the successive enactments of the Trade Union and Labour Relations Act (1974), the Employment Protection Act (1975) and the Trade Union and Labour Relations (Amendment) Act (1976). From 1980 onwards, they both fall rapidly following the passage of the Employment Acts of 1980, 1982 and 1988 and the Trade Union Act of 1984. For the period 1946-1988, the correlation between the two indices ORG_t and BAR_t is 0.748, while for the period of interest here (1950-1987), the correlation coefficient is 0.909. Simple interpolation of the Freeman and Pelletier series is used to derive quarterly series as used in sub-section 3.5.2.

business cycle together with the two political variables. While this is acknowledged to be a rather naive model (or perhaps 'eclectic'), it should be recalled that the intention here is only to establish the degree to which cyclical and political and/or institutional forces influence various disaggregated series of strike frequency. It also serves as a reference point against which to assess previous econometric models such as those considered in section 3.3. A fairly simple specification should therefore suffice.

3.5.2. Estimation and Results.[71]

The seven disaggregated measures of strikes described in section 3.4 are modelled separately since it is apparent from the results above that only certain classes of strikes are cyclical and therefore likely to be adequately described by the framework outlined in sub-section 3.5.1. Table 3.10 presents estimates for the period 1950(1) to 1979(1) in order that comparisons with the previous work in this chapter can be made. The basic cyclical specification (equation (3.5)) is considered in the first half of the table. The four misspecification tests presented for the OLS estimates are identical to those utilised in section 3.3 above. The diagnostics indicate the presence of (up to) fourth order autocorrelation in the disturbances for all seven measures of strike frequency, while the other tests for misspecification are generally accepted at conventional levels. As is well known, such a finding of serially correlated residuals could simply be indicating that the dynamic structure of the equation is misspecified (Sargan (1964) and Hendry & Mizon (1978)). The argument is familiar; in the simplest terms, suppose that

$$y_t = \beta x_t + u_t \quad \text{with} \quad u_t = \rho_1 u_{t-1} + \varepsilon_t, \quad |\rho_1| < 1 \quad \text{and} \quad \varepsilon_t \sim \mathrm{ID}(0,\sigma^2). \tag{3.6}$$

This can be rewritten as

$$y_t = \rho_1 y_{t-1} + \beta x_t - \rho_1 \beta x_{t-1} + \varepsilon_t \tag{3.7}$$

which is sometimes termed the restricted transformed equation (RTE). An alternative stable dynamic model given by

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71. The software utilised in this section of the chapter was a combination of Microfit, and Limdep, version 5.1 (Greene (1990)) and version 6.0 (Greene (1991)).

$$y_t = \beta_1 y_{t-1} + \beta_2 x_t + \beta_3 x_{t-1} + \varepsilon_t \quad |\beta_1| < 1, \tag{3.8}$$

is termed the unrestricted transformed equation (UTE). Clearly equation (3.7) is equivalent to equation (3.8) with the restriction

$$\beta_1\beta_2 + \beta_3 = 0. \tag{3.9}$$

The strategy Sargan suggests is to first test this non-linear restriction, and to only test for $\rho_1 = 0$ if (3.9) is not rejected. If it is rejected, then the problem is not one of serial correlation at all, but the omission of the variables y_{t-1} and x_{t-1} from the equation, i.e. 'misspecified dynamics', and the coefficient estimates will be biased.

The results from this testing strategy are reported in table 3.10 under the heading 'LR Tests for Misspecified Dynamics'. AR4 is a simple likelihood ratio test of the RTE against the UTE for the more general fourth order autoregressive scheme

$$u_t = \rho_1 u_{t-1} + \rho_2 u_{t-2} + \rho_3 u_{t-3} + \rho_4 u_{t-4} + \varepsilon_t \quad \text{and} \quad \varepsilon_t \sim \mathrm{ID}(0,\sigma^2) \tag{3.10}$$

which seems a reasonable hypothesis given that the data are quarterly. The test statistic has a $\chi^2(4)$ distribution under the null; while there is only one variable in the model to which the equivalent restriction to equation (3.9) is being applied ($CYCLE_t$), there are four such restrictions following the imposition of the fourth order autoregressive scheme (3.10). Clearly the restrictions are accepted for all seven measures of strikes with the implication that the error process is indeed autoregressive rather than the equation having an omitted variables problem resulting in misspecified dynamics.

The consequences of autocorrelated disturbances for OLS estimation are that although coefficient estimates are still unbiased given that all the right-hand side variables are exogeneous, the standard errors are incorrectly estimated and thus conventional inference procedures are invalid. While it is possible to estimate the model under an autoregressive error specification using maximum likelihood or Cochrane-Orcutt or some other iterative technique, the strategy invoked in the current study is to simply present coefficient standard errors that are robust to generally unspecified autocorrelation. Newey & West (1987) detail the construction of an autocorrelation consistent variance-covariance matrix which is a generalisation of the more familiar heteroskedastic-consistent version devised by White (1980). The estimates presented in table 3.10 allow for autocorrelation of up to order four, this lag length being determined by the insignificance of additional terms in higher order autoregressive error schemes (using simple likelihood ratio tests) and inspection of the residual correlograms. As expected, the autocorrelation consistent standard errors are larger than their OLS equivalents since both the errors and the explanatory variables tend to be positively correlated over time leading to the OLS standard errors underestimating the true standard errors (see, for example, Koutsoyiannis (1977, pp.208-210)). Only estimated coefficients for the cyclical variable are reported in the table; these represent the elasticity of the measure of strikes to the business cycle (since $CYCLE_t$ is the logarithm of the index of industrial production). The coefficient estimates confirm the results from the monthly data examined in section 3.4[72]; it is pay strikes that are most strongly procyclical and most responsive to the business cycle, especially when attention is restricted to the non-coal sector as can be seen from an examination of the final two columns of the table in particular. Coal strikes (column 4) are exceptional in their strong counter-cyclicality, and their presence would appear to adversely affect the results for columns 2 and 3 and also when strikes are taken in aggregate as in column 1.

The next section of table 3.10 considers the comparative performance of the empirical model over the period 1950(1) to 1979(1)[73]. Firstly the forecasting performance of the specification is examined by considering structural stability (SSTAB) and predictive failure (PRED) tests for the sample period divided at 1967(2). These are Chow (1960)

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72. The seasonal dummies again reveal that strikes are more prevalent in the first six months of the year while the coefficients on the quadratic in time yield an inverse U-shaped function for all seven measures of strikes.

73. Note that these tests are conducted under the assumption that the model is correctly specified. That there is evidence of autocorrelation may possibly invalidate the tests.

tests as described in sub-section 3.3.1 above, and are, of course, only appropriate to OLS estimated coefficients. While these will be unbiased for the specification considered here, they are not efficient, and hence perhaps do not accurately reflect the ability of the model to forecast beyond the estimating sample. The results show that for the basic specification represented by equation (3.5) the model does not perform particularly well, although, encouragingly, the results for non-coal strikes are rather better as revealed by the statistics in the final three columns of the table. It should be noted, however, that the variance ratio test (VRATIO) rejects homogeneity in a number of cases and thus the tests may have unacceptably low power[74].

The next two lines of the table report the results of non-nested comparisons against the most favourable semi-logarithmic respecifications of Pencavel (1970) and Shorey (1977) described in section 3.3. As with the forecasting tests reported in the previous paragraph, only OLS estimation is appropriate and hence the autoregressive nature of the error process cannot be incorporated into the cyclical specification. A wide variety of tests is available for non-nested models and these have been extensively reviewed by MacKinnon (1983) and McAleer & Pesaran (1986). The six tests used here are: the N-test (Cox (1961, 1962)); the NT-test (Godfrey & Pesaran (1983)); the W-test (Godfrey & Pesaran (1983)); the J-test (Davidson & MacKinnon (1981)); the JA-test (Fisher & McAleer (1981)); and the encompassing test (which is simply the standard F-test for the excluded regressors, see Mizon & Richard (1986)). Full details of each of these tests can be found in MacKinnon (1983) and McAleer & Pesaran (1986) while their relative small-sample performances are investigated by Godfrey & Pesaran (1983)[75].

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[74]. The structural stability test cannot be calculated for coal strikes due to the inclusion of a dummy variable for the national coal strike of 1974. Since this dummy takes value zero for all observations before this date, no estimated coefficient is available for the period prior to 1967(2). The test for predictive failure is however available (and is resoundingly rejected).

[75]. These are the test procedures programmed into Microfit (Pesaran & Pesaran (1991)).

The line labelled PENCAVEL reports the results of testing the cyclical model against the semi-logarithmic Pencavel specification as reported in tables 3.4A and 3.4B for each of the seven disaggregated strikes series. In every case, the cyclical model is preferred (Accept) and, moreover, is acceptable in its own right as a descriptive model for all measures of strikes (except coal strikes) in an encompassing test (results not reported). Testing the cyclical specification against the semi-logarithmic Shorey model (SHOREY) yields rather different results, and it is the econometric model reported in tables 3.6A and 3.6B that is chosen by the non-nested tests (Reject)[76]. (The sole exception is non-coal, non-pay strikes in column 7.) However, given that the semi-logarithmic Shorey model is *rejected* for the period as a whole for *all* seven measures of strikes in an encompassing test, then this finding is not particularly instructive, and indeed may be rather misleading.

These conclusions for the basic cyclical specification are little changed when the political variables are included in the model as shown in the second half of table 3.10. Once again, the evidence points to there being genuinely autocorrelated disturbances (with the exception of the aggregated strikes series in the first column), and hence autocorrelation-consistent standard errors are presented for the cyclical and political variables. The coefficients on the cyclical variables again confirm the general patterns established in section 3.4, although coal strikes are no longer significantly correlated with the cycle. The model still displays structural instability however, and is unable to forecast satisfactorily when the sample period is divided at 1967(2). The inclusion of the political variables does not affect the conclusions with respect to the comparisons with the Pencavel and Shorey respecifications. Indeed, the legal indices rarely achieve

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76. It is the dynamic structure allowed for by the lagged dependent variable in Shorey's specification which would seem to put it at a considerable advantage over the essentially static cyclical model. It is conceivable that the latter would be preferred if only the tests could allow for the dynamic residual component.

statistical significance at conventional levels[77].

Table 3.11 presents the results from estimating the cyclical-political model over the whole of the post-war period from 1950(1) to 1987(4). Once again, it is apparent that the disturbances are autocorrelated, although there is also evidence of other forms of misspecification in several categories of strikes and which may therefore be producing a misleading LM4 statistic. These diagnostics tests may be indicating that there has been a departure from the simple cyclical-political description of strikes in the 1980s. Despite this finding, testing for misspecified dynamics in a similar manner to that described above yields strong support for the hypothesis of autocorrelated errors with the single exception of the coal industry. Thus the lower half of table 3.11 presents OLS estimates with autocorrelation-consistent standard errors computed to 4th order given in parentheses. Strikes are found to be more frequent in the first three quarters of the year, most probably reflecting the pattern in the seasonality of negotiations. Moreover, over the longer time period, the cyclicality of pay strikes is still strongly evident and the coefficients on the political variables are again rather poorly determined in general[78]. The forecasting tests reported at the bottom of the table are for the sample period split at 1979(1) and the model then used to predict through to 1987(4). As can be seen, the cyclical-political model receives partial support on this criterion.

3.5.3. Consequences for some Recent Studies of Strike Frequency.

Further work clearly needs to be done before a satisfactory empirical description of post-war strike frequency can be said to have been established. However, some

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77. Since the two political variables are highly correlated, this finding may be a consequence of multicollinearity. This possibility was investigated in two ways. Firstly, the indices were combined to give a single legal index, and secondly, each of the legislative variables was dropped in turn from the model. Qualitatively, the results in table 3.10 were unchanged by either of these respecifications.

78. The inclusion of an additional dummy variable for the implementation of incomes policies had little impact on the results reported in table 3.11.

observations seem pertinent at this juncture in the light of the results obtained above and the evidence recently reported by others and which has been succinctly summarised by Metcalf (1990). The first issue to be addressed centres on the impact of recent legislative reforms on the incidence of industrial conflict. Takla (1988, reported in Metcalf (1990)) estimates an aggregate time-series econometric model similar to that of Pencavel (1970) and shows strike frequency to have been significantly reduced due to the labour legislation introduced since 1980 as recorded by a simple dummy variable. McConnell & Takla (1990) use an industry level panel to show a similar finding, although the negative effect of the legislation is stronger for days lost than for strike frequency. Thirdly, utilising the Confederation of British Industry's Pay Data Bank from 1979 to 1989, Ingram *et al* (1991) show that the labour legislation has reduced strike activity for manufacturing bargaining groups which comprise their unit of analysis. They use both the Freeman & Pelletier (1990) index and dummy variables for 1982 and 1984.

One basic criticism of all three studies is their failure to explain why strike activity also fell substantially in the 1970s when the legal climate was becoming increasingly *favourable* towards unions. There is an important asymmetry here; strike activity has decreased under both supportive and adversarial legislative regimes. The three papers only record and examine the impact of the law during the 'unfavourable' period and this is clearly unsatisfactory and insufficient. Certainly, the observed correlation between the 'anti-union' legislation and strike frequency in the 1980s cannot be used to infer that there is some causation from one to the other. In fact, the results reported in table 3.11 above for a longer time series which incorporates the reforms of the 1970s as well as those of the 1980s show that, overall, there would appear to be no significant legislation effect[79].

The second issue concerns the applicability of the dependent variable used in the analysis in this chapter. As noted in chapter 2 and by Stern (1978), it would seem appropriate to deflate the statistics for strike frequency by some measure of population size in order to distinguish changes in the level of conflict from changes in the volume of bargaining. Two possibilities are considered in the context of the empirical model estimated in this sub-section; firstly, strike frequency is calculated on a per employee basis, and secondly, on a per union member basis.

Deflating the seven measures of strike frequency by the number of employees in the sector of interest and re-estimating the cyclical specification of sub-section 3.5.2 yields results little different from those presented in table 3.11. In particular, the elasticities of strike frequency with respect to the business cycle are virtually unchanged compared to those estimated for the raw frequency measures. The second strategy of deflating strike frequency by the number of union members in the sector of interest is one that has recently been (re-)advocated by Milner & Metcalf (1991). On the basis of strikes per union member they conclude that it was the 1970s that were exceptional,

> "... whereas the 1980s simply saw a return to the underlying trend of strike activity apparent since 1930." (Milner & Metcalf (1991, p.5))

This conclusion would seem to contradict with the assessment presented in Metcalf

[79]. Further evidence is provided by replacing the ORG_t and BAR_t variables in the cyclical-political specification in table 3.11 by three dummy variables recording the passage of the 1980 and 1982 Employment Acts and the 1984 Trade Union Act. The Employment Acts would appear to have reduced the frequency of all categories of strikes, although only by a small and insignificant amount in general. In contrast, and most conspicuously, the 1984 Trade Union Act is associated with a large and significant reduction in the number of strikes, particularly pay strikes. This Act enforces trades unions to hold secret ballots of their members before a strike can be called. Three plausible and competing interpretations of this result are admissible; firstly, it may be that the legislation has established a delaying, or 'cooling-off' period in negotiations so that settlements are reached more often. Alternatively, this finding may reflect the strengthened position of unions *following* a ballot of their members; employers may then be forced to make a quick settlement before incurring costly strike action. Finally, the downward trend in strike frequency witnessed since 1970 may have accelerated post-1980 for some other reason such as compositional changes in the economy. Without further information, it is not possible to discriminate between these three competing hypotheses.

(1990), and discussed above, in which he concludes that the impact of labour legislation has been significant in seeing the decline in industrial conflict in the 1980s.

Estimating the cyclical-political model using union membership weighted measures of strike frequency once again does not change the estimated cyclical responsiveness of strikes[80]. However, the model now forecasts rather more successfully in the 1980s than when estimated using raw frequency measures of strikes. This would seem to give some support for the proposition that the acceleration apparent in the falling levels in strike frequency seen in the 1980s stems largely from the falling levels of union membership rather than a direct result of the changing legislative environment. Thus, failing to deflate strike frequency statistics for changes in union membership erroneously attributes this acceleration to the direct effect of the Conservatives' anti-union legislation. A more cautious analysis would seem to indicate that the legislation has had only an indirect effect, if any, and mainly through its impact on the level of union membership. It should be emphasised that this conclusion is necessarily tentative, and clearly much work remains to be done in this area. In particular, an investigation of the *simultaneous* determination of union density and strike activity, together with their relationship to the business cycle, would seem to be warranted.

Finally, it is important to reiterate that the cyclical-political model presented above is *not* an attempt to specify a new model of strike frequency in Britain. Rather it is an 'eclectic' description of strikes which serves to reveal the differing patterns of conflict across sectors and by cause and establishes a bench-mark against which a satisfactory 'economic' model of strike activity will need to compare.

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80. The single exception is the series for coal strikes which are now (marginally) insignificantly negatively correlated with the business cycle.

3.6. Summary and Conclusions.

It is apparent from the analysis presented above that strikes in Britain are procyclical. The extant models singularly fail to establish this result because they are econometrically misspecified and incorporate several indicators of the business cycle, some leading, some concurrent with and some lagging the cycle and which together serve to obscure the cyclicality in strikes. These weaknesses are revealed in section 3.3 when two popular models are subjected to a more rigourous econometric examination than was previously possible. Moreover, even suitably transformed, these models cannot account for the downturn in strike activity after 1970.

The chapter then addresses the proposition that strikes are a cyclical phenomenon in a more direct manner. While the Burns-Mitchell analysis is purely statistical in nature, it does serve to highlight the differences in the cyclical behaviour of various disaggregated measures of strike frequency. It also provides pointers for future research of this kind. Not only should coal and non-coal strikes continue to be distinguished, but so also should pay-strikes and non-pay strikes within the non-coal sector.

In an attempt to put these findings into context, section 3.5 presents some preliminary results for a 'cyclical-political' empirical specification using a similar framework to that employed for the previous econometric models of strike frequency. The inclusion of a single index of the cycle does serve to reveal the cyclicality noted in section 3.4 in certain disaggregated categories of strikes. Moreover, this simple cyclical model is seen to perform relatively favourably against the more sophisticated econometric specifications examined in section 3.3. Finally, some tentative estimates are made of the impact of the changing legislative environment. While others have suggested that the reforms of the 1980s have led to a decrease in strike activity, they universally fail explain why there was no corresponding upturn in strikes in the 1970s. Further analysis shows that there would appear to be no overall effect of changes in labour law in the post-war period.

There are several implications of the results presented in this chapter for any further analysis of aggregate strike frequency. Firstly, it is evident that important distinctions exist in the behaviour of different component series of aggregate strike frequency. Not only does the coal sector remain rather an enigma, but there are strong differences in strikes over pay and non-pay issues and thus these should be treated separately in order to avoid the aggregation bias that has plagued many previous studies. Moreover, the results from the cycle-trend-seasonal decomposition in section 3.4 indicate that further work along the lines of a structural time-series approach may be fruitful.

Some final remarks with respect to the changing patterns of unionisation and its likely impact on the level of industrial conflict seem merited. A crude examination shows that at least part of the apparent further downturn in strikes in the 1980s is a consequence of the decline in union density in the period. But there has also been an increasing concentration among unions through amalgamations, so that while there were some 513 unions in 1970, this had fallen to only 438 in 1980, and by 1988 (the latest date for which statistics are available), unions numbered only 314[81]. During the same period, union density rose substantially (in particular between 1970 and 1980) and reached an historically high level of over 55% in 1979. Since then, as is well documented, union density has been in sharp decline and a variety of hypotheses have been offered in explanation (see, for example, Carruth & Disney (1988) and Disney (1990)). Undoubtedly, these changes in unionisation will have had an impact on the recorded levels of strike activity and must account for at least part of the substantial fall in strike frequency since 1970. The decreasing number of unions means that fewer strikes are recorded for any given level of grievance activity (and that each strike will tend to involve more workers) purely as an artefact of the way in which

81. Source: Employment Gazette, May 1990.

strikes are measured. Moreover, the decline in density since 1980 will compound this effect since unions will find it increasingly difficult to organise strike action if they comprise a smaller and smaller proportion of the workforce. Indeed there may be a minimum feasible density below which it is simply not possible to call a strike.

Together therefore, it seems likely that these changes in the patterns of unionisation will serve to reduce recorded strike frequency and may obscure any real changes in the level of conflict activity. A more suitable approach may be to examine strike *incidence*, (that is, strikes per worker or strikes per plant etc.) to complement the study of strike frequency. However, the central difficulty lies in selecting an appropriate deflator for the number of strikes, and it is clear that simply deflating strike frequency by employment or union membership as in sub-section 3.5.3 does not adequately capture the influence of union amalgamation, or of the trend towards decentralised bargaining, or of the compositional changes in the economy witnessed in the early 1980s in particular.

While there are a number of possibilities, given the changes in workplace unionism, a natural unit of analysis to choose for further investigation is the plant or establishment, with the measure of conflict suitably adjusted for the degree of unionism. Thus the next chapter presents an investigation of the incidence of strikes (and non-strike activity) at the establishment level utilising the Workplace Industrial Relations Surveys of 1980 and 1984.

Table 3.1

Replication of Pencavel (1970): Basic Specification

Dependent Variable: Non-Coal Strike Frequency

	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1950(1)-1967(2)		1950(1)-1967(2)	
Mean	-		237.7		240.9	
S.D.	-		109.9		110.3	
n	-		62		60	
Regression Coefficients						
CNST	-147.88	(1.68)	-264.01	(2.47)	-331.10	(2.87)
$Q1_t$	74.93	(4.99)	82.90	(5.15)	91.34	(5.42)
$Q2_t$	38.00	(2.67)	29.00	(1.86)	24.47	(1.60)
$Q3_t$	7.01	(0.48)	1.75	(0.11)	0.51	(0.03)
U_t	-41.29	(2.89)	-42.07	(2.42)	-42.92	(2.43)
D_t	1.87	(2.60)	3.74	(3.54)	4.53	(3.73)
$\sum\beta_i\Delta R_{t-i}$	-42.46	(4.89)	-41.16	(3.62)	-20.15	(2.92)
T_t	5.50	(15.28)	5.98	(15.90)	5.97	(16.49)
Lag Coefficients						
β_1	-10.27	(1.97)	-6.04	(1.02)	-10.91	(3.78)
β_2	-9.49	(4.06)	-7.89	(2.75)	-6.52	(3.85)
β_3	-8.34	(4.28)	-8.60	(3.43)	-3.15	(2.22)
β_4	-6.81	(2.52)	-8.17	(2.52)	-0.82	(0.53)
β_5	-4.92	(1.77)	-6.59	(2.03)	0.48	(0.33)
β_6	-2.64	(1.39)	-3.87	(1.76)	0.76	(0.78)
$\sum\beta_i$	-42.46	(4.89)	-41.16	(3.62)	-20.15	(2.92)
Diagnostics						
R^2	0.869		0.873		0.880	
SEE	41.9		42.1		41.1	
*ln*L	-		-314.93		-303.25	
DW	1.36	(1.89)	1.31	(1.89)	1.37	(1.89)
LM	-	-	9.09	(9.49)	11.12	(9.49)
RESET	-	-	9.45	(3.84)	10.54	(3.84)
NN	-	-	0.94	(5.99)	0.35	(5.99)
HETERO	-	-	2.14	(3.84)	0.64	(3.84)

Notes:
1. Absolute t-ratios for the estimated coefficients are given in parentheses.
2. The figures in parentheses beside the diagnostic test statistics are the relevant 5% critical values. For DW, the *upper* bound is given.

Table 3.2

Replication of Pencavel (1970): Wage-Price Specification

Dependent Variable: Non-Coal Strike Frequency						
	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1950(1)-1967(2)		1950(1)-1967(2)	
Mean	-		237.7		240.9	
S.D.	-		109.9		110.3	
n	-		62		60	
Regression Coefficients						
CNST	-74.46	(0.72)	-172.60	(1.43)	-263.14	(1.75)
$Q1_t$	70.55	(4.60)	84.42	(5.18)	88.30	(5.04)
$Q2_t$	38.45	(2.70)	29.81	(1.94)	24.72	(1.59)
$Q3_t$	5.44	(0.37)	2.02	(0.13)	0.65	(0.04)
U_t	-38.73	(2.43)	-64.47	(2.74)	-52.09	(2.48)
D_t	1.27	(1.49)	3.81	(3.32)	4.14	(3.08)
$\sum\gamma_i\Delta W_{t-i}$	-44.20	(3.89)	-68.66	(3.68)	-22.99	(2.66)
$\sum\delta_i\Delta P_{t-i}$	39.34	(4.34)	49.52	(4.09)	19.98	(2.83)
T_t	5.24	(13.10)	5.65	(12.09)	5.87	(12.32)
Lag Coefficients						
γ_1	-6.52	(1.02)	-10.24	(1.34)	-12.81	(3.36)
γ_2	-8.49	(2.73)	-13.22	(3.03)	-7.55	(3.57)
γ_3	-9.23	(3.80)	-14.33	(3.62)	-3.55	(1.95)
γ_4	-8.78	(2.81)	-13.56	(3.00)	-0.79	(0.38)
γ_5	-7.06	(2.21)	-10.92	(2.54)	0.72	(0.36)
γ_6	-4.14	(1.91)	-6.40	(2.25)	0.99	(0.74)
$\sum\gamma_i$	-44.20	(3.89)	-68.66	(3.68)	-22.99	(2.66)
δ_1	11.12	(2.05)	4.23	(0.63)	10.29	(3.39)
δ_2	9.33	(3.94)	8.48	(2.79)	6.28	(3.58)
δ_3	7.51	(3.58)	10.76	(3.90)	3.20	(2.21)
δ_4	5.67	(1.92)	11.04	(2.96)	1.02	(0.64)
δ_5	3.80	(1.25)	9.35	(2.46)	-0.24	(0.16)
δ_6	1.91	(0.92)	5.67	(2.20)	-0.57	(0.57)
$\sum\delta_i$	39.34	(4.34)	49.52	(4.09)	19.98	(2.83)
Diagnostics						
R^2	0.873		0.881		0.882	
SEE	41.9		41.5		41.7	
*ln*L	-		-312.88		-302.83	
DW	1.35	(1.98)	1.39	(1.98)	1.38	(1.98)
LM	-	-	7.66	(9.49)	11.25	(9.49)
RESET	-	-	15.15	(3.84)	14.64	(3.84)
NN	-	-	0.30	(5.99)	0.39	(5.99)
HETERO	-	-	1.85	(3.84)	0.73	(3.84)

Notes:
1. See table 3.1.

Table 3.3

Replication of Pencavel (1970): Institutional Factors

Dependent Variable: Non-Coal Strike Frequency

	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1950(1)-1967(2)		1950(1)-1967(2)	
Mean	-		237.7		240.9	
S.D.	-		109.9		110.3	
n	-		62		60	
Regression Coefficients						
CNST	-142.25	(1.47)	-256.77	(2.11)	-351.74	(2.67)
$Q1_t$	75.56	(4.91)	82.78	(5.01)	93.04	(5.33)
$Q2_t$	37.99	(2.64)	28.31	(1.78)	22.78	(1.45)
$Q3_t$	6.71	(0.46)	0.48	(0.03)	-0.72	(0.05)
U_t	-50.19	(2.95)	-51.09	(2.29)	-47.74	(2.04)
D_t	1.97	(2.35)	3.76	(3.24)	4.81	(3.59)
$\sum\beta_i\Delta R_{t-i}$	-52.52	(3.85)	-45.22	(3.31)	-21.81	(2.80)
T_t	5.71	(13.28)	6.29	(11.30)	6.16	(11.01)
L_t	-11.10	(0.54)	-21.42	(0.88)	-21.25	(0.88)
J_t	-10.27	(0.56)	2.53	(0.11)	12.93	(0.56)
Lag Coefficients						
β_1	-	-	5.39	(0.88)	-10.98	(3.60)
β_2	-	-	8.26	(2.61)	-6.78	(3.61)
β_3	-	-	9.62	(3.18)	-3.52	(2.21)
β_4	-	-	9.47	(2.53)	-1.22	(0.73)
β_5	-	-	7.82	(2.14)	0.14	(0.09)
β_6	-	-	4.67	(1.91)	0.54	(0.54)
$\sum\beta_i$	-52.52	(3.85)	-45.22	(3.31)	-21.81	(2.80)
Diagnostics						
R^2	0.871		0.875		0.882	
SEE	42.3		42.5		41.6	
*ln*L	-		-314.45		-302.70	
DW	1.41	(1.98)	1.33	(1.98)	1.42	(1.98)
LM	-	-	8.52	(9.49)	9.96	(9.49)
RESET	-	-	19.81	(3.84)	19.47	(3.84)
NN	-	-	0.52	(5.99)	0.39	(5.99)
HETERO	-	-	1.87	(3.84)	0.77	(3.84)

Notes:
1. See table 3.1.

Table 3.4A

Replication of Pencavel (1970): Semi-Logarithmic Specification

Dependent Variable: *ln*(Non-Coal Strike Frequency)						
	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1967(3)-1979(1)		1950(1)-1979(1)	
Mean	5.364		6.366		5.796	
S.D.	0.472		0.253		0.634	
n	62		47		109	
Regression Coefficients						
CNST	3.013	(7.09)	6.144	(15.42)	4.992	(18.65)
$Q1_t$	0.324	(5.07)	0.096	(1.02)	0.157	(2.34)
$Q2_t$	0.058	(0.94)	0.180	(1.68)	0.119	(1.78)
$Q3_t$	-0.004	(0.06)	0.064	(0.57)	0.003	(0.04)
U_t	-0.098	(1.42)	-0.195	(3.18)	-0.287	(7.90)
D_t	0.016	(3.93)	0.002	(0.41)	-0.002	(0.76)
$\sum\beta_i\Delta R_{t-i}$	-0.124	(2.59)	-0.047	(1.38)	-0.043	(1.94)
T_t	0.026	(17.56)	0.007	(1.52)	0.026	(19.10)
Lag Coefficients						
β_1	-0.028	(1.19)	0.001	(0.07)	-0.001	(0.08)
β_2	-0.027	(2.28)	-0.006	(0.84)	-0.006	(1.23)
β_3	-0.025	(2.36)	-0.011	(1.54)	-0.010	(2.04)
β_4	-0.021	(1.58)	-0.012	(1.74)	-0.011	(1.89)
β_5	-0.015	(1.17)	-0.011	(1.76)	-0.010	(1.72)
β_6	-0.008	(0.95)	-0.007	(1.73)	-0.006	(1.61)
$\sum\beta_i$	-0.124	(2.59)	-0.047	(1.38)	-0.043	(1.94)
Diagnostics						
R^2	0.891		0.343		0.864	
SEE	0.167		0.225		0.243	
*ln*L	27.84		8.35		4.15	
DW	1.38	(1.89)	0.97	(1.95)	0.75	(1.85)
LM	7.06	(9.49)	17.54	(9.49)	45.05	(9.49)
RESET	0.22	(3.84)	1.78	(3.84)	3.78	(3.84)
NN	1.16	(5.99)	3.02	(5.99)	6.29	(5.99)
HETERO	0.01	(3.84)	1.64	(3.84)	4.69	(3.84)
SSTAB	7.44	(1.98)				
PRED	3.38	(1.60)				
VRATIO	1.82	(1.62)				

Notes:
1. See table 3.1.

Table 3.4B

Replication of Pencavel (1970): Semi-Logarithmic Specification

Dependent Variable: *ln*(Non-Coal Strike Frequency)

	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1967(3)-1979(1)		1950(1)-1979(1)	
Mean	5.364		6.366		5.796	
S.D.	0.472		0.253		0.634	
n	62		47		109	
Regression Coefficients						
CNST	3.067	(6.80)	5.980	(15.95)	5.110	(18.97)
$Q1_t$	0.324	(5.28)	0.109	(1.38)	0.150	(2.31)
$Q2_t$	0.049	(0.82)	0.230	(2.49)	0.124	(1.92)
$Q3_t$	-0.019	(0.32)	0.098	(0.99)	-0.011	(0.16)
U_t	-0.200	(2.42)	-0.198	(3.84)	-0.317	(8.62)
D_t	0.017	(3.93)	0.006	(1.34)	-0.003	(1.16)
$\sum\beta_i\Delta R_{t-i}$	-0.170	(3.34)	-0.062	(3.25)	-0.078	(3.98)
T_t	0.030	(14.43)	0.007	(1.57)	0.029	(16.61)
L_t	-0.257	(2.86)	0.117	(1.52)	-0.001	(0.01)
J_t	0.044	(0.50)	-0.274	(4.11)	-0.194	(2.95)
Lag Coefficients						
β_1	-0.020	(0.90)	-0.015	(1.86)	-0.014	(1.51)
β_2	-0.031	(2.75)	-0.014	(3.33)	-0.016	(3.35)
β_3	-0.036	(3.33)	-0.012	(2.85)	-0.016	(3.69)
β_4	-0.035	(2.60)	-0.010	(1.90)	-0.014	(2.65)
β_5	-0.029	(2.18)	-0.007	(1.43)	-0.011	(2.09)
β_6	-0.018	(1.95)	-0.004	(1.17)	-0.006	(1.78)
$\sum\beta_i$	-0.170	(3.34)	-0.062	(3.25)	-0.078	(3.98)
Diagnostics						
R^2	0.906		0.559		0.875	
SEE	0.158		0.190		0.235	
*ln*L	32.48		17.7		18.96	
DW	1.63	(1.98)	1.55	(2.07)	0.85	(1.89)
LM	2.99	(9.49)	14.38	(9.49)	41.91	(9.49)
RESET	2.54	(3.84)	1.16	(3.84)	1.91	(3.84)
NN	0.93	(5.99)	0.57	(5.99)	0.91	(5.99)
HETERO	0.57	(3.84)	1.61	(3.84)	4.25	(3.84)
SSTAB	8.76	(1.89)				
PRED	3.53	(1.60)				
VRATIO	1.44	(1.66)				

Notes:
1. See table 3.1.

Table 3.5

Replication of Shorey (1977): Basic Specification

Dependent Variable: Non-Coal Strike Frequency

	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1950(1)-1967(2)		1950(1)-1967(2)	
Mean	231		234.3		229.4	
S.D.	107		108.6		108.8	
n	-		65		68	
Regression Coefficients						
CNST	-291.7	(5.2)	-328.25	(4.89)	-320.98	(5.10)
$Q1_t$	81.8	(7.3)	86.83	(7.53)	83.37	(7.00)
$(\pi/P)_t$	158.1	(5.4)	45.63	(5.84)	37.42	(5.22)
ΔW_{t-1}	-18.2	(2.3)	-13.50	(3.27)	-9.41	(1.75)
ΔP_{t-1}	12.9	(1.8)	10.01	(3.27)	3.67	(0.78)
P_t	1.2	(2.2)	1.19	(2.06)	1.31	(2.36)
S_{t-1}	0.48	(5.3)	0.38	(4.22)	0.48	(5.17)
Diagnostics						
R^2	0.89		0.897		0.887	
SEE	36.3		36.7		38.4	
*ln*L	-		-322.67		-340.85	
Dh	-	-	0.33	(1.96)	0.26	(1.96)
LM	-	-	8.77	(9.49)	7.53	(9.49)
RESET	-	-	10.72	(3.84)	8.60	(3.84)
NN	-	-	0.76	(5.99)	0.68	(5.99)
HETERO	-	-	4.36	(3.84)	7.03	(3.84)

Notes:
1. See table 3.1.

Table 3.6A.

Replication of Shorey (1977): Semi-Logarithmic Specification

Dependent Variable: *ln*(Non-Coal Strike Frequency)

	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1967(3)-1979(1)		1950(1)-1979(1)	
Mean	5.351		6.366		5.777	
S.D.	0.465		0.253		0.636	
n	65		47		112	
Regression Coefficients						
CNST	1.370	(5.05)	2.585	(3.19)	0.378	(1.89)
$Q1_t$	0.349	(7.87)	0.071	(1.04)	0.213	(4.74)
$(\pi/P)_t$	0.197	(6.26)	0.009	(0.41)	0.043	(2.14)
ΔW_{t-1}	-0.060	(3.73)	-0.003	(0.50)	-0.002	(0.39)
ΔP_{t-1}	0.039	(3.25)	-0.004	(0.44)	-0.003	(0.44)
P_t	0.005	(2.27)	-0.000	(0.44)	-0.000	(0.12)
$ln(S_{t-1})$	0.369	(4.35)	0.596	(4.91)	0.869	(19.06)
Diagnostics						
R^2	0.916		0.450		0.902	
SEE	0.141		0.201		0.205	
*ln*L	38.64		12.53		22.30	
Dh	0.75	(1.96)	2.61	(1.96)	3.02	(1.96)
LM	6.43	(9.49)	8.81	(9.49)	16.34	(9.49)
RESET	0.32	(3.84)	2.90	(3.84)	0.80	(3.84)
NN	1.01	(5.99)	2.33	(5.99)	0.17	(5.99)
HETERO	1.42	(3.84)	0.74	(3.84)	0.48	(3.84)
SSTAB	8.23	(2.10)				
PRED	3.45	(1.58)				
VRATIO	2.02	(1.60)				

Notes:
1. See table 3.1.

Table 3.6B

Replication of Shorey (1977): Semi-Logarithmic Specification

Dependent Variable: *ln*(Non-Coal Strike Frequency)						
	Column 1		Column 2		Column 3	
Period	1950(1)-1967(2)		1967(3)-1979(1)		1950(1)-1979(1)	
Mean	5.351		6.366		5.777	
S.D.	0.465		0.253		0.636	
n	65		47		112	
Regression Coefficients						
CNST	1.420	(4.55)	2.797	(3.47)	0.549	(2.58)
$Q1_t$	0.356	(6.46)	0.183	(2.45)	0.224	(3.99)
$(\pi/P)_t$	0.221	(5.60)	0.093	(2.91)	0.052	(2.33)
ΔW_{t-1}	-0.057	(3.45)	-0.005	(0.81)	0.001	(0.21)
ΔP_{t-1}	0.038	(2.87)	0.004	(0.48)	-0.008	(0.98)
P_t	0.004	(1.34)	-0.002	(2.85)	-0.000	(0.75)
$ln(S_{t-1})$	0.353	(3.81)	0.436	(3.50)	0.823	(16.05)
$Q2_t$	-0.014	(0.27)	0.291	(3.26)	0.035	(0.62)
$Q3_t$	-0.004	(0.08)	0.170	(1.78)	-0.007	(0.12)
L_t	-0.062	(0.96)	0.233	(3.04)	0.125	(2.34)
J_t	0.107	(1.47)	-0.152	(2.24)	-0.003	(0.06)
Diagnostics						
R^2	0.920		0.632		0.908	
SEE	0.143		0.173		0.202	
*ln*L	40.01		21.98		25.99	
Dh	1.53	(1.96)	4.78	(1.96)	3.01	(1.96)
LM	11.04	(9.49)	15.45	(9.49)	16.61	(9.49)
RESET	0.60	(3.84)	2.10	(3.84)	0.09	(3.84)
NN	0.71	(5.99)	1.05	(5.99)	0.14	(5.99)
HETERO	1.66	(3.84)	0.00	(3.84)	0.03	(3.84)
SSTAB	7.21	(1.90)				
PRED	3.11	(1.59)				
VRATIO	1.46	(1.64)				

Notes:
1. See table 3.1.

Table 3.7

Strikes Series Annual Aggregates 1946-1987

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
Year	All Strikes	Pay Strikes	Non-Pay Strikes	Coal Strikes	Non-Coal Strikes	Non-Coal Strikes Pay	Non-Pay
1946	2205	912	1147	1333	872	385	487
1947	1721	764	854	1053	668	316	352
1948	1759	723	1004	1116	643	271	372
1949	1426	590	794	874	552	236	316
1950	1339	561	749	860	479	205	274
1951	1719	790	867	1058	661	314	347
1952	1714	697	948	1222	492	209	283
1953	1746	767	964	1307	439	196	243
1954	1989	936	1095	1464	525	244	281
1955	2418	1213	1238	1783	635	312	323
1956	2648	1190	1450	2076	572	258	314
1957	2858	1257	1630	2224	634	276	358
1958	2629	1228	1453	1963	666	305	361
1959	2093	956	1137	1306	787	357	430
1960	2832	1336	1402	1666	1166	569	597
1961	2686	1228	1312	1458	1228	593	635
1962	2449	1009	1201	1105	1344	614	730
1963	2068	861	1037	987	1081	495	586
1964	2524	1135	1230	1058	1466	704	762
1965	2354	1053	1073	740	1614	799	815
1966	1937	794	958	563	1374	624	750
1967	2116	885	1002	394	1722	810	912
1968	2378	1073	957	221	2157	1141	1016
1969	3116	1419	1058	186	2930	1680	1250
1970	3906	2130	1193	160	3746	2403	1343
1971	2228	961	875	135	2093	1096	997
1972	2489	1259	815	224	2265	1377	888
1973	2873	1232	1111	301	2572	1354	1218
1974	2922	1569	818	186	2736	1800	936
1975	2282	1089	737	212	2070	1231	839
1976	2016	720	849	276	1740	795	945
1977	2703	1231	847	262	2441	1449	992
1978	2471	1082	652	338	2133	1337	796
1979	2080	870	574	298	1782	1077	705
1980	1330	453	484	302	1028	500	528
1981	1338	444	441	302	1036	520	516
1982	1528	446	490	403	1125	548	577
1983	1352	369	453	355	997	447	550
1984	1206	386	387	78	1128	566	562
1985	887	249	270	160	727	346	381
1986	1053	320	484	351	702	281	421
1987	1004	283	629	299	705	223	482
Mean	2104.6	915.9	920.7	777.6	1327.0	696.7	630.2
S.D.	648.6	387.6	318.6	611.7	797.1	519.4	294.5
CV	0.308	0.423	0.346	0.787	0.601	0.746	0.467

Table 3.8

Strikes Series Seasonality 1946-1987

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
Month	All Strikes	Pay Strikes	Non-Pay Strikes	Coal Strikes	Non-Coal Strikes	Non-Coal Strikes Pay	Non-Coal Strikes Non-Pay
Jan	176.1	72.0	81.7	61.2	114.9	56.7	58.2
Feb	185.4	78.7	86.5	67.7	117.7	59.5	58.2
Mar	202.0	88.6	86.8	75.5	126.5	67.5	59.0
Apr	192.7	85.5	83.0	74.4	118.3	64.9	53.4
May	192.0	87.1	83.0	72.1	119.8	64.1	55.7
Jun	183.9	81.6	80.5	69.8	114.1	60.8	53.4
Jul	136.2	60.6	57.0	45.5	90.7	49.0	41.8
Aug	162.9	73.7	69.1	57.7	105.2	57.1	48.2
Sep	184.6	81.4	79.8	68.1	116.5	61.9	54.6
Oct	212.3	93.7	91.4	77.0	135.2	71.1	64.1
Nov	182.8	76.8	78.4	68.3	114.5	59.3	55.2
Dec	93.9	36.3	43.5	40.3	53.5	24.9	28.6
Mean	175.4	76.3	76.7	64.8	110.6	58.1	52.5
S.D.	32.0	15.3	13.8	11.7	20.9	11.9	9.4
CV	0.183	0.201	0.180	0.180	0.189	0.205	0.179

Table 3.9

Burns-Mitchell Results

Panel (1): All Strikes

Reference Cycle	TROUGH 1	2	3	4	PEAK 5	6	7	8	TROUGH 9
9/46--6/50--10/52	*131.4*	124.5	100.4	82.3	85.5	97.5	98.6	92.6	86.9
10/52--6/55--10/58	67.5	77.1	85.2	102.7	111.2	114.3	*120.2*	95.0	72.9
10/58--6/60--1/63	83.5	87.0	111.4	*124.8*	123.5	116.4	101.2	67.4	69.2
1/63--10/64--5/67	93.2	110.5	126.1	*130.9*	120.9	104.0	70.2	79.9	89.9
5/67--3/69--11/71	58.5	65.8	83.8	109.6	138.4	*162.7*	99.2	65.9	79.5
11/71--6/73--10/75	83.0	93.8	105.9	103.9	105.2	*116.5*	113.0	70.4	64.0
10/75--8/79--3/81	72.0	107.0	*126.2*	109.4	81.4	67.0	62.5	71.1	81.5
Mean	84.2	95.1	105.6	109.1	109.4	*111.2*	95.0	77.5	77.7

Panel (2): Pay Strikes

Reference Cycle	TROUGH 1	2	3	4	PEAK 5	6	7	8	TROUGH 9
9/46--6/50--10/52	129.9	*133.1*	95.1	78.4	87.8	106.2	96.9	85.9	85.3
10/52--6/55--10/58	62.3	72.5	87.9	115.0	*121.1*	112.6	114.0	94.4	70.5
10/58--6/60--1/63	89.1	94.3	125.6	*136.3*	132.3	123.5	91.0	48.8	55.9
1/63--10/64--5/67	87.0	118.9	144.0	*145.5*	133.9	121.4	46.1	59.0	87.3
5/67--3/69--11/71	42.6	54.0	73.1	98.3	141.3	*191.5*	101.7	58.1	86.5
11/71--6/73--10/75	89.2	97.7	92.5	91.4	107.8	*133.8*	127.9	53.8	32.2
10/75--8/79--3/81	42.3	99.6	*141.8*	115.7	72.6	56.4	50.2	62.4	75.6
Mean	77.5	95.7	108.6	111.5	113.8	*120.8*	89.7	66.1	70.5

Panel (3): Non-Pay Strikes

Reference Cycle	TROUGH 1	2	3	4	PEAK 5	6	7	8	TROUGH 9
9/46--6/50--10/52	*147.8*	136.7	110.9	80.9	75.0	80.8	87.5	86.7	78.3
10/52--6/55--10/58	57.4	69.6	79.2	91.1	102.9	117.2	*132.2*	100.8	75.9
10/58--6/60--1/63	87.1	88.3	110.5	*119.7*	117.3	112.6	103.0	72.5	75.9
1/63--10/64--5/67	86.6	102.0	*117.4*	115.7	108.4	95.0	85.6	96.4	96.5
5/67--3/69--11/71	86.2	85.0	89.6	102.0	114.7	*126.8*	106.0	82.9	84.8
11/71--6/73--10/75	78.7	93.7	*118.1*	114.3	102.3	95.5	93.6	94.3	102.1
10/75--8/79--3/81	109.0	*122.7*	102.1	93.8	94.5	88.1	84.2	82.5	86.0
Mean	93.2	99.7	*104.0*	102.5	102.1	102.3	98.9	88.0	85.6

Table 3.9 (continued)

Burns-Mitchell Results

Panel (4): Coal Strikes

Reference Cycle	TROUGH 1	2	3	4	PEAK 5	6	7	8	TROUGH 9
9/46--6/50--10/52	*184.0*	156.5	103.4	61.4	60.9	74.7	89.7	100.4	95.5
10/52--6/55--10/58	47.8	58.3	69.9	95.2	109.0	123.0	*138.4*	102.5	65.6
10/58--6/60--1/63	85.3	85.9	110.3	*129.5*	127.2	118.2	91.7	73.0	77.1
1/63--10/64--5/67	112.5	121.8	*134.5*	125.9	107.6	94.1	80.9	71.4	62.7
5/67--3/69--11/71	84.0	76.9	83.2	88.8	88.9	96.8	110.6	126.8	*147.7*
11/71--6/73--10/75	78.8	89.1	101.7	97.8	93.9	95.1	105.8	107.8	*113.6*
10/75--8/79--3/81	90.9	95.6	103.5	102.4	104.7	101.7	97.8	96.8	98.1
Mean	97.6	97.7	100.9	100.1	98.9	100.5	*102.1*	97.0	94.3

Panel (5): Non-Coal Strikes

Reference Cycle	TROUGH 1	2	3	4	PEAK 5	6	7	8	TROUGH 9
9/46--6/50--10/52	71.1	84.2	94.0	101.4	108.3	*116.6*	113.5	104.0	100.5
10/52--6/55--10/58	105.0	109.2	109.7	*110.7*	109.4	100.1	91.6	84.5	83.9
10/58--6/60--1/63	95.8	99.2	112.4	*114.3*	113.2	108.6	105.1	70.0	68.2
1/63--10/64--5/67	86.7	102.2	112.8	*123.8*	122.3	108.5	72.3	91.3	110.6
5/67--3/69--11/71	57.9	67.6	85.4	111.8	142.5	*166.8*	97.6	59.8	69.8
11/71--6/73--10/75	85.8	93.8	102.7	104.1	109.0	*123.6*	115.1	63.2	53.0
10/75--8/79--3/81	64.4	109.3	*131.5*	112.3	73.3	56.7	54.1	65.8	78.2
Mean	80.9	95.1	106.9	111.2	111.4	*111.6*	92.8	77.0	80.6

Table 3.9 (continued)

Burns-Mitchell Results

Panel (6): Non-Coal Pay Strikes

Reference Cycle	TROUGH 1	2	3	4	PEAK 5	6	7	8	TROUGH 9
9/46--6/50--10/52	65.8	82.2	90.7	100.8	109.9	*120.6*	116.3	106.6	104.5
10/52--6/55--10/58	106.3	110.1	111.2	*114.0*	112.0	99.9	89.5	82.2	79.9
10/58--6/60--1/63	99.8	104.1	118.8	*120.0*	118.0	111.8	100.0	60.8	59.7
1/63--10/64--5/67	92.3	113.0	125.8	*140.2*	137.9	123.6	49.7	70.3	107.6
5/67--3/69--11/71	39.6	53.7	77.2	109.5	153.8	*194.0*	95.9	51.1	72.4
11/71--6/73--10/75	92.4	95.4	86.7	90.5	112.4	*146.3*	135.0	40.6	13.6
10/75--8/79--3/81	18.6	92.6	*158.8*	129.0	57.4	34.4	30.2	51.1	70.3
Mean	73.5	93.0	109.9	114.9	114.5	*118.7*	88.1	66.1	72.6

Panel (7): Non-Coal Non-Pay Strikes

Reference Cycle	TROUGH 1	2	3	4	PEAK 5	6	7	8	TROUGH 9
9/46--6/50--10/52	76.7	86.2	97.4	102.0	106.7	*112.5*	110.7	101.2	96.3
10/52--6/55--10/58	103.5	*108.2*	108.1	107.1	106.5	100.3	94.0	86.9	88.3
10/58--6/60--1/63	92.1	94.8	106.5	109.1	108.8	105.7	*109.7*	78.5	76.0
1/63--10/64--5/67	83.1	95.2	104.4	*113.1*	112.2	98.6	86.9	104.9	112.6
5/67--3/69--11/71	81.1	85.1	95.8	114.6	128.2	*132.3*	99.7	70.9	66.4
11/71--6/73--10/75	78.1	91.9	*121.4*	119.9	104.9	97.1	91.8	89.8	99.0
10/75--8/79--3/81	106.8	*124.8*	106.2	96.8	88.0	77.4	76.2	79.4	85.5
Mean	88.8	98.0	105.7	*109.0*	107.9	103.4	95.6	87.4	89.1

Table 3.10

A Cyclical-Political Model of Quarterly Strike Frequency

Period: 1950(1)-1979(1)
Dependent Variable: *ln*(Strike Frequency)

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
Strikes Series	All Strikes	Pay Strikes	Non-Pay Strikes	Coal Strikes	Non-Coal Strikes	Non-Coal Strikes Pay	Non-Coal Strikes Non-Pay
Mean	6.365	5.556	5.541	5.004	5.743	5.045	5.029
S.D.	0.252	0.328	0.259	1.025	0.651	0.763	0.548
n	117	117	117	117	117	117	117
Basic Cyclical Specification							
OLS Diagnostics							
LM4 (9.49)	48.67	48.10	38.66	85.43	31.19	33.65	27.62
FF (3.84)	3.26	0.04	5.07	2.81	2.78	2.00	3.81
NN (5.99)	0.43	4.65	0.10	6.26	3.90	4.52	3.10
HET (3.84)	0.25	0.38	0.76	3.91	0.03	3.02	7.12
LR Test for Misspecified Dynamics							
AR4 (9.49)	4.50	2.94	2.92	5.78	3.20	4.06	0.78
Regression Coefficient on Cyclical Variable *(Autocorrelation consistent SEs in parentheses, computed to 4th order)*							
$CYCLE_t$	0.898 *(0.662)*	1.783 *(0.920)*	-0.083 *(0.594)*	-5.585 *(1.592)*	3.788 *(0.662)*	4.717 *(0.892)*	2.861 *(0.516)*
Forecasting Performance for sample divided at 1967(2)							
SSTAB (2.10)	10.93	9.52	10.99	N.A.	2.09	1.87	2.57
PRED (1.56)	4.45	6.11	2.63	20.80	1.25	1.93	0.75
VRATI (1.59)	2.32	3.67	1.10	N.A.	1.09	1.84	1.94
Non-Nested Tests							
PENCAVEL	Accept	Accept	Accept	Accept	Accept	Accept	Accept
SHOREY	Reject	Reject	Reject	Reject	Reject	Reject	Accept

Table 3.10 (continued)

A Cyclical-Political Model of Quarterly Strike Frequency

Political-Cyclical Specification							
	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
Strikes Series	All Strikes	Pay Strikes	Non-Pay Strikes	Coal Strikes	Non-Coal Strikes	Non-Coal Strikes Pay	Non-Coal Strikes Non-Pay
OLS Diagnostics							
LM4 (9.49)	48.40	48.47	33.16	77.38	28.47	33.03	22.39
FF (3.84)	4.81	0.72	2.15	3.10	4.05	2.68	5.86
NN (5.99)	1.13	7.37	0.58	2.29	2.32	3.76	2.92
HET (3.84)	1.58	0.80	1.29	12.84	0.17	2.88	10.89
LR Test for Misspecified Dynamics							
AR4 (21.03)	26.22	20.37	17.92	16.32	15.36	18.20	10.72
Regression Coefficients on Cyclical and Political Variables *(Autocorrelation consistent SEs in parentheses, computed to 4th order)*							
$CYCLE_t$	1.715	2.682	0.971	-1.548	3.437	4.304	2.608
	(0.621)	*(0.865)*	*(0.623)*	*(1.632)*	*(0.669)*	*(0.810)*	*(0.672)*
ORG_t	-0.022	-0.039	-0.012	-0.174	0.033	0.020	0.052
	(0.054)	*(0.072)*	*(0.038)*	*(0.148)*	*(0.067)*	*(0.089)*	*(0.049)*
BAR_t	0.089	0.109	0.104	0.485	-0.056	-0.052	-0.061
	(0.052)	*(0.067)*	*(0.040)*	*(0.142)*	*(0.056)*	*(0.074)*	*(0.045)*
Forecasting Performance for sample divided at 1967(2)							
SSTAB (1.98)	9.05	7.23	8.15	N.A.	2.07	1.78	2.56
PRED (1.57)	4.54	6.24	2.36	33.44	1.22	1.91	0.68
VRATI (1.59)	2.34	3.99	1.00	N.A.	1.01	1.81	2.60
Non-Nested Tests							
PENCAVEL	Accept	Accept	Accept	Accept	Accept	Accept	Accept
SHOREY	Reject	Reject	Reject	Reject	Reject	Reject	Accept

Notes:
1. See text for details of specification and estimation methodology.
2. Each of the specifications also include quarterly dummies and a quadratic in time.
3. The figures in parentheses beside the diagnostic tests SSTAB, PRED and VRATI are the relevant 5% critical values.
4. The regression specification for coal strikes additionally includes dummies for the national strikes in 1974.
5. The non-nested test procedures utilised are: the N-test; the NT-test; the W-test; the J-test; the JA-test; and the encompassing principal.

Table 3.11

A Cyclical-Political Model of Quarterly Strike Frequency

Period: 1950(1)-1987(4)
Dependent Variable: *ln*(Strike Frequency)

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7
Strikes Series	All Strikes	Pay Strikes	Non-Pay Strikes	Coal Strikes	Non-Coal Strikes	Non-Coal Strikes Pay	Non-Coal Strikes Non-Pay
Mean	6.218	5.321	5.349	4.772	5.678	4.958	4.984
S.D.	0.372	0.551	0.441	1.112	0.601	0.724	0.497
n	152	152	152	152	152	152	152
Political-Cyclical Specification							
OLS Diagnostics							
LM4 (9.49)	54.63	49.05	64.71	88.01	46.42	56.07	29.86
FF (3.84)	1.40	0.79	11.64	2.25	18.83	17.53	12.14
NN (5.99)	0.60	4.55	6.99	2.62	2.55	1.01	0.99
HET (3.84)	2.05	1.27	12.81	0.36	0.62	0.34	9.52
LR Test for Misspecified Dynamics							
AR4 (21.03)	15.93	17.07	7.22	50.88	11.83	15.91	6.99
Regression Coefficients on Cyclical and Political Variables *(Autocorrelation consistent SEs in parentheses, computed to 4th order)*							
$CYCLE_t$	1.374	2.480	1.083	-3.250	3.076	3.651	2.256
	(0.329)	*(0.447)*	*(0.550)*	*(0.911)*	*(0.458)*	*(0.726)*	*(0.349)*
ORG_t	-0.054	-0.047	-0.060	-0.290	0.022	0.028	0.021
	(0.041)	*(0.054)*	*(0.046)*	*(0.097)*	*(0.047)*	*(0.065)*	*(0.035)*
BAR_t	0.083	0.098	0.041	0.261	0.005	0.033	-0.029
	(0.040)	*(0.052)*	*(0.046)*	*(0.111)*	*(0.048)*	*(0.065)*	*(0.037)*
Forecasting Performance for sample divided at 1979(1)							
SSTAB (1.95)	0.88	0.81	7.89	N.A.	3.40	5.38	2.08
PRED (1.54)	0.80	0.73	3.82	6.50	1.42	1.97	1.21
VRATI (1.61)	1.23	1.41	1.92	N.A.	1.27	1.19	1.09

Notes:
1. See table 3.10.

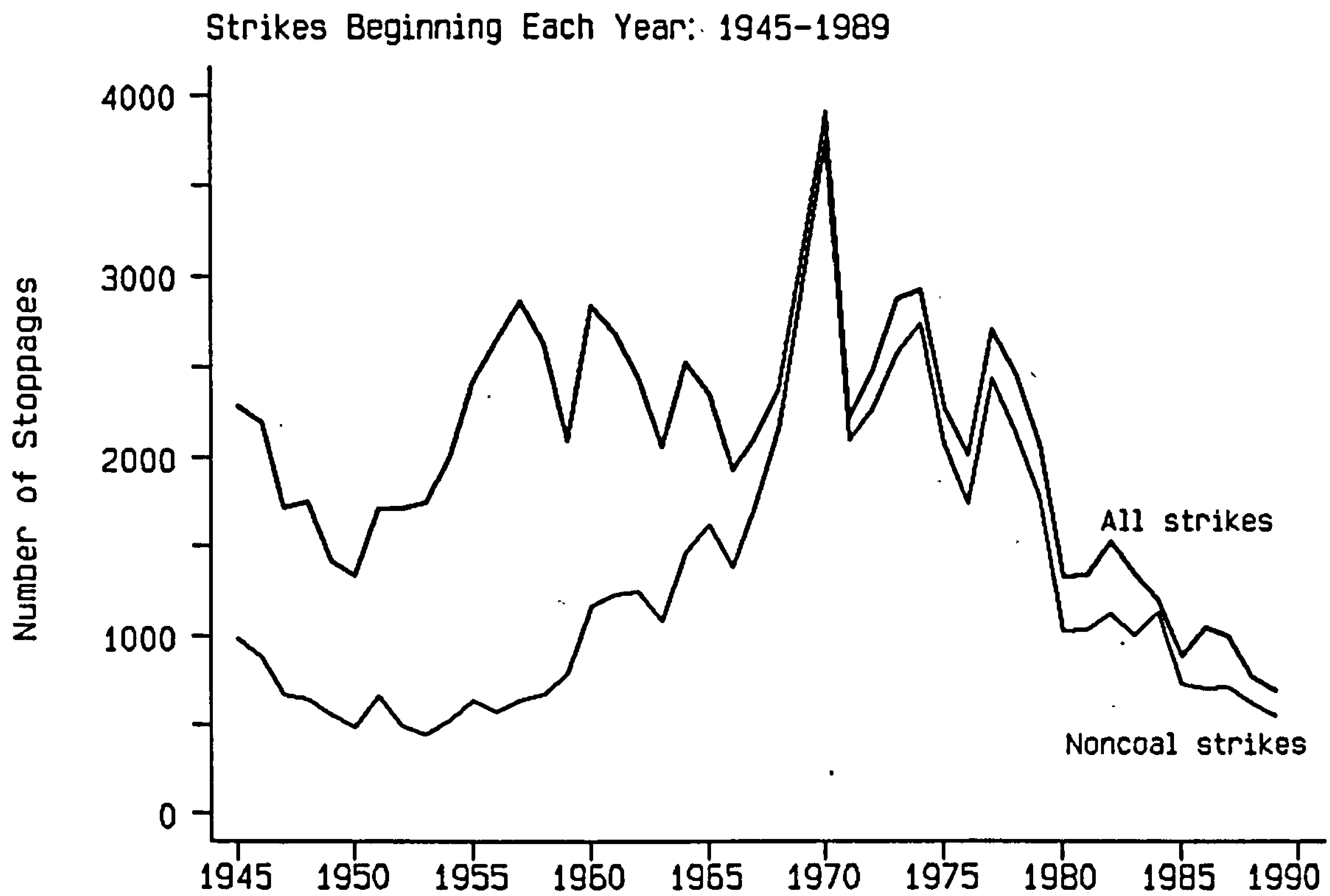

Strikes Beginning Each Year: 1945-1989
Number of Stoppages
4000
3000
2000
1000
0
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990
All strikes
Noncoal strikes

Figure 3.1

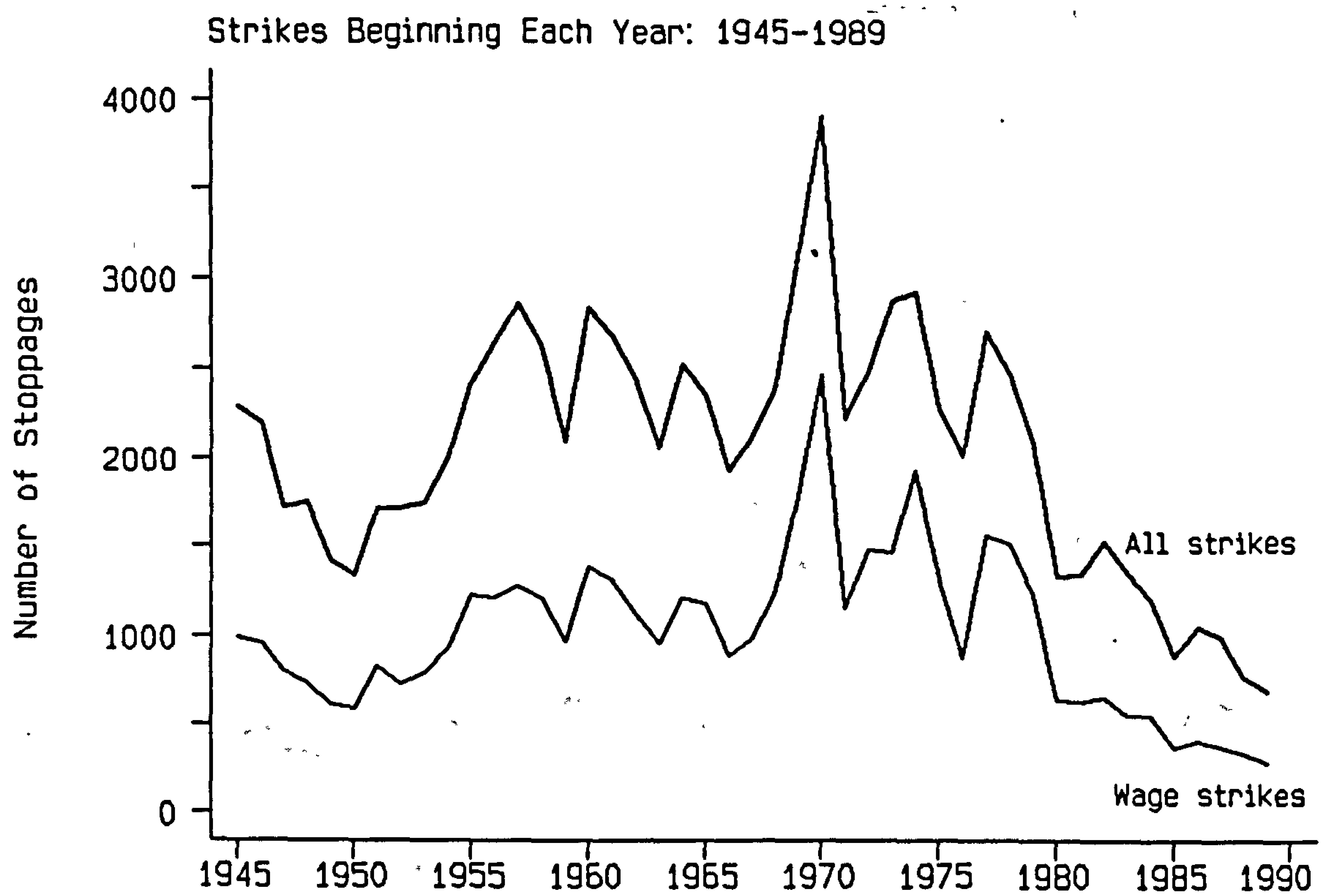

Strikes Beginning Each Year: 1945-1989
Number of Stoppages
4000
3000
2000
1000
0
All strikes
Wage strikes
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990

Figure 3.2

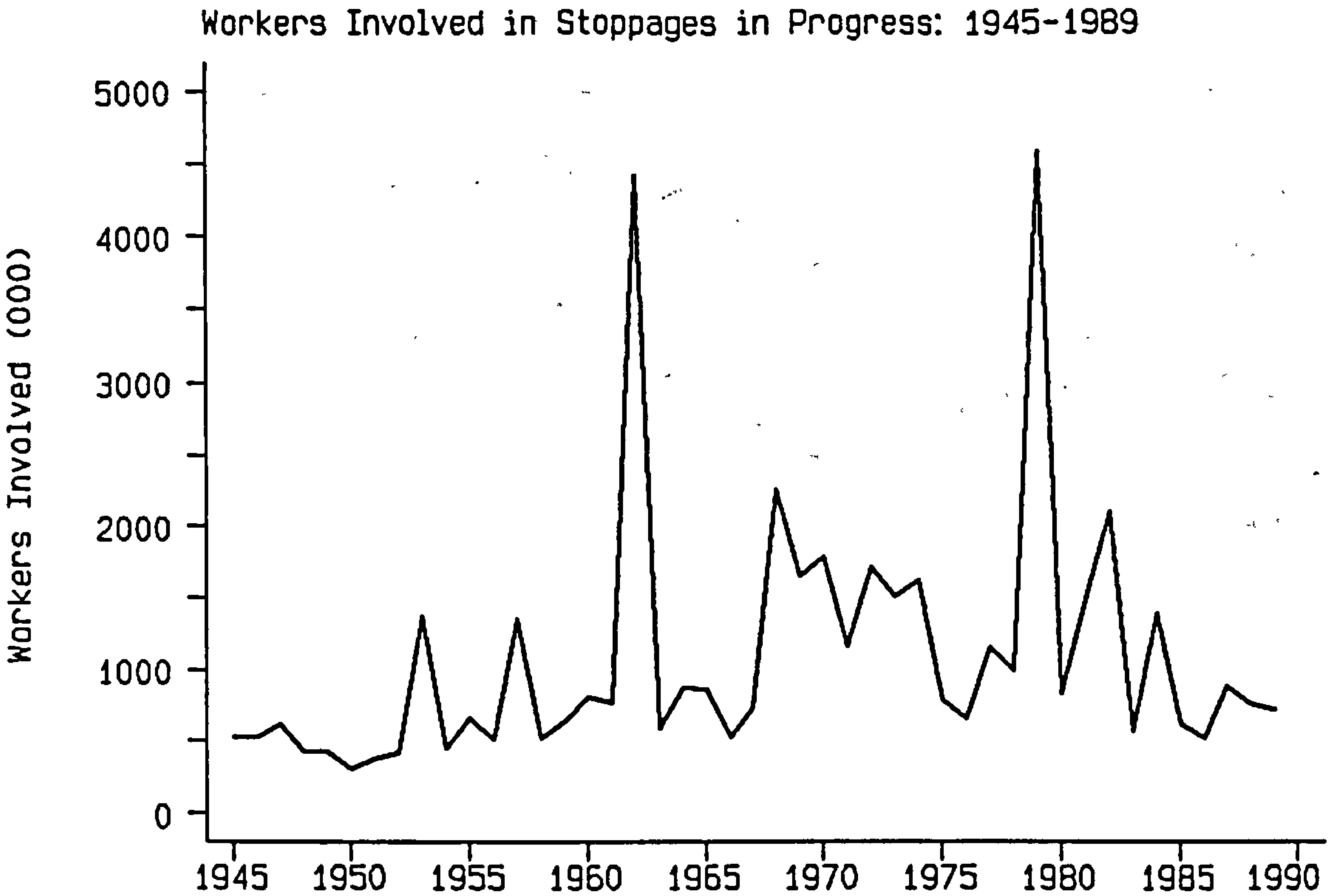

Workers Involved in Stoppages in Progress: 1945-1989
Workers Involved (000)
5000
4000
3000
2000
1000
0
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990

Figure 3.3

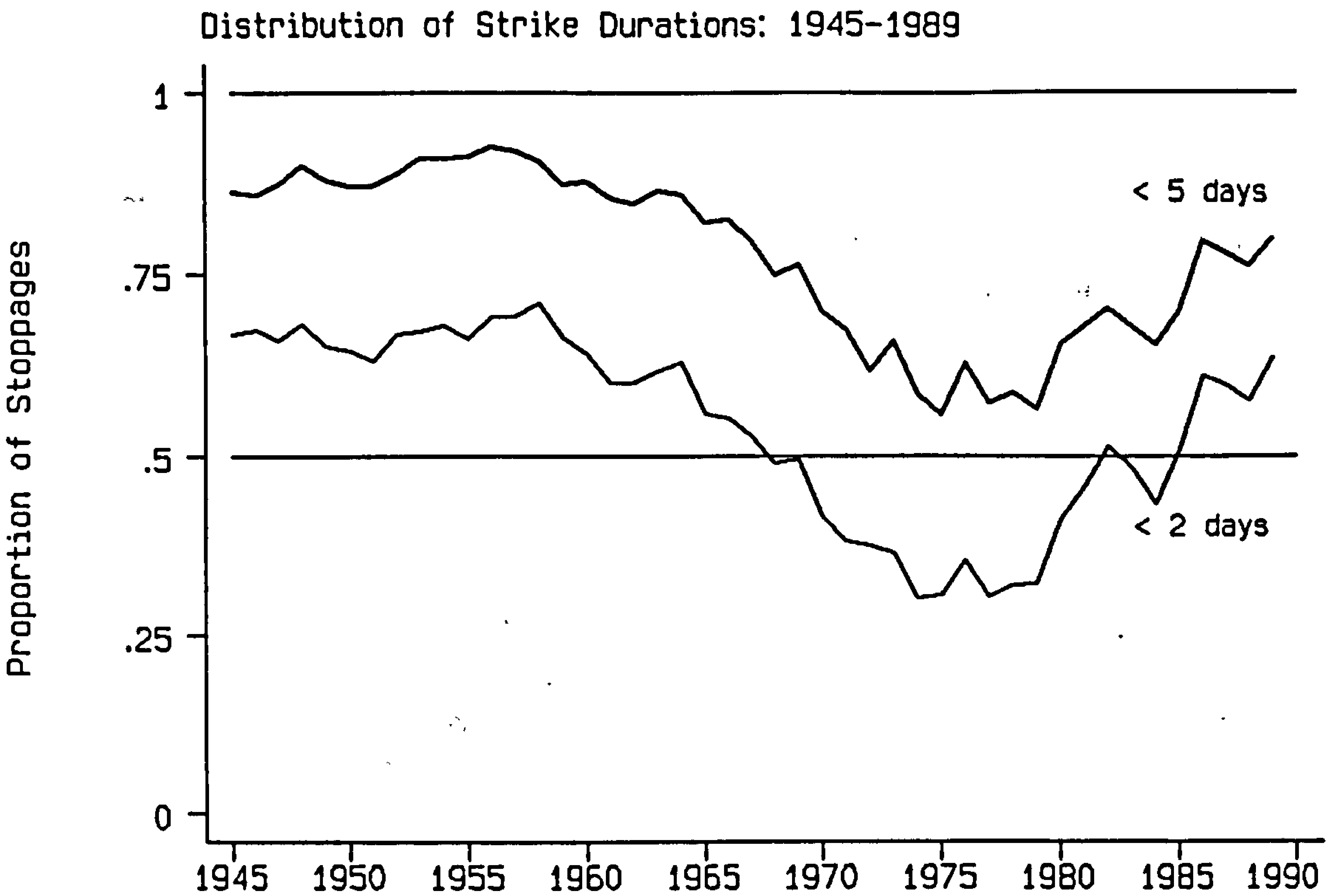

Distribution of Strike Durations: 1945-1989
Proportion of Stoppages
1
.75
.5
.25
0
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990
< 5 days
< 2 days

Figure 3.4

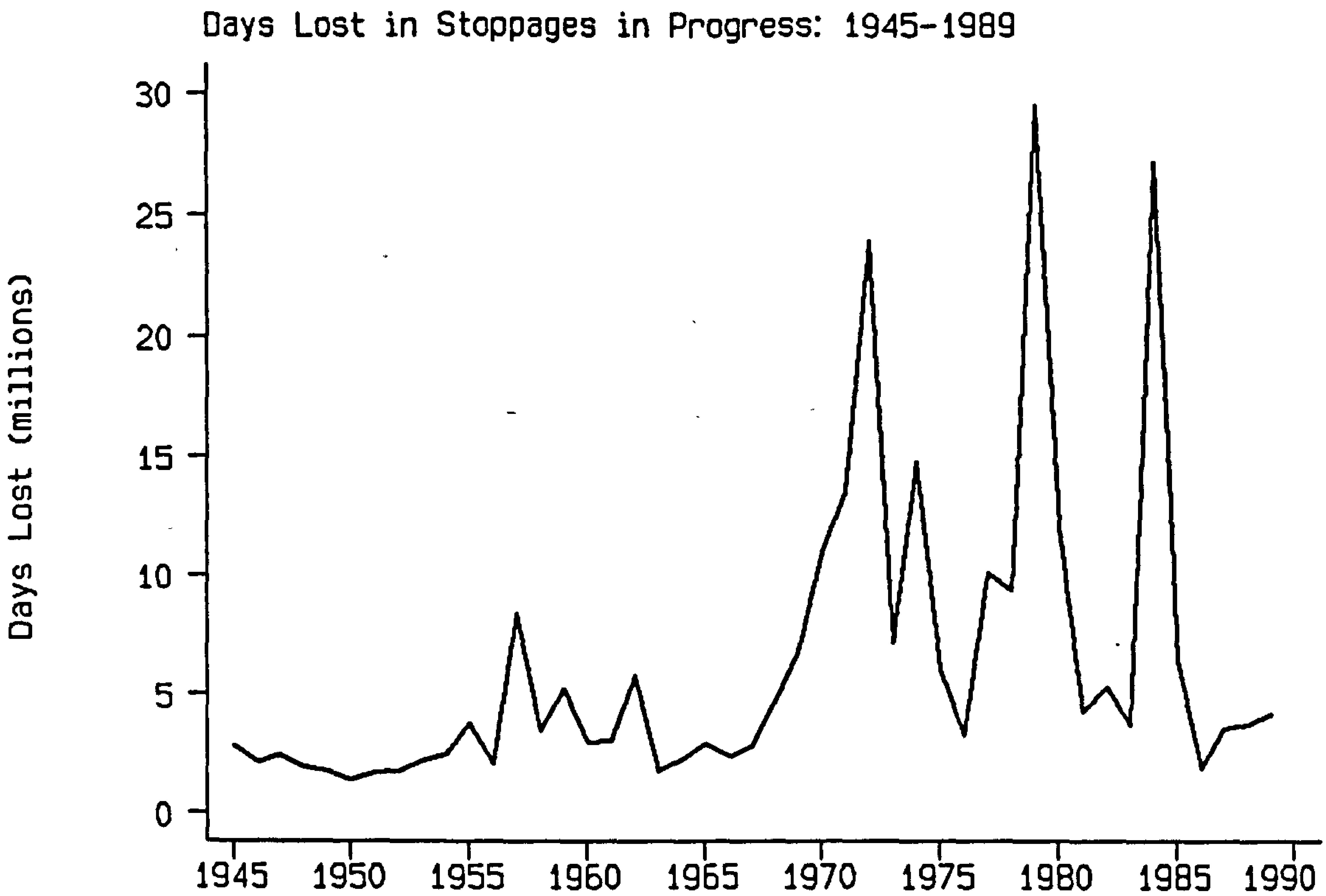

Days Lost in Stoppages in Progress: 1945-1989
Days Lost (millions)
30
25
20
15
10
5
0
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990

Figure 3.5

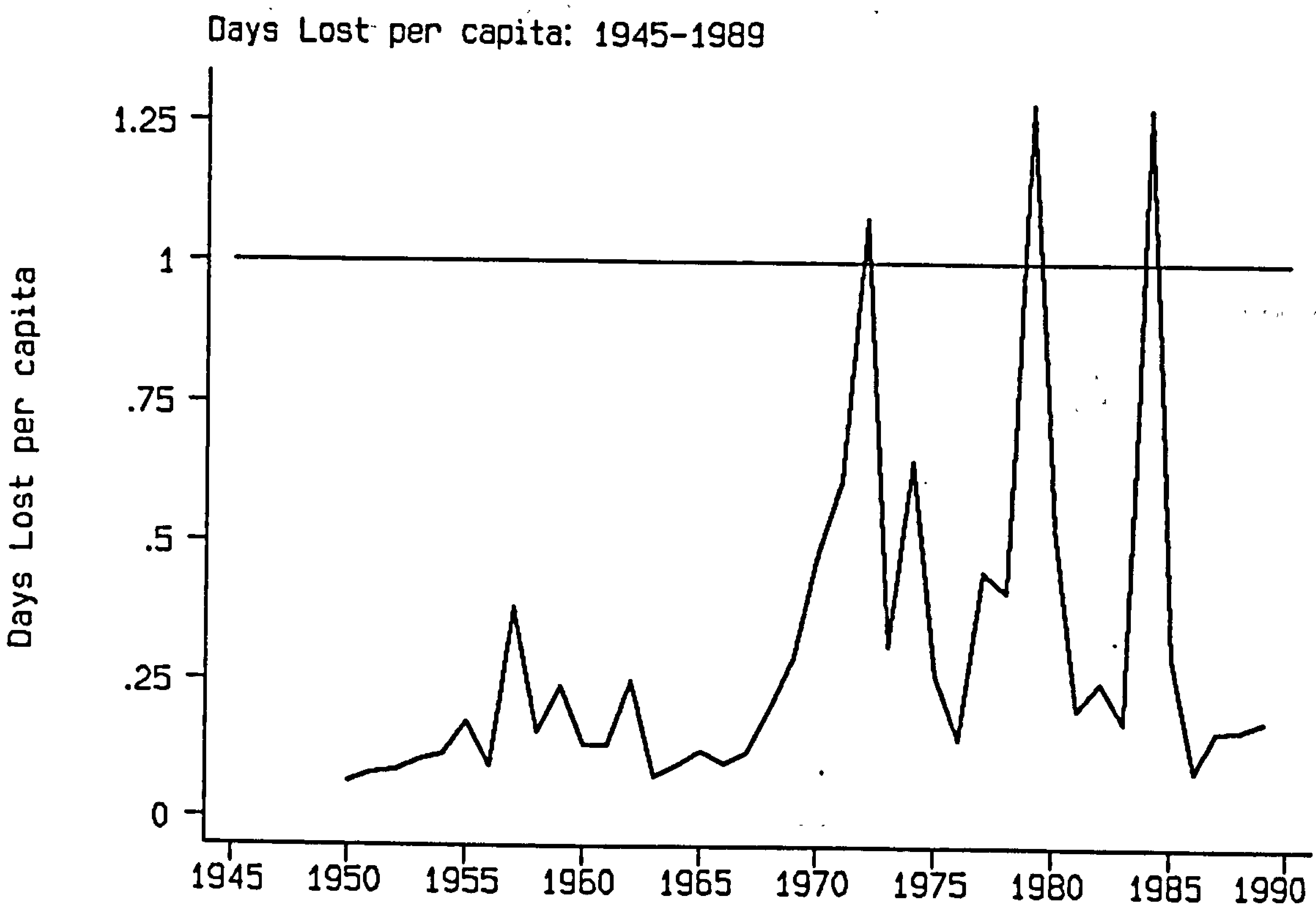

Days Lost per capita: 1945-1989
Days Lost per capita
1.25
1
.75
.5
.25
0
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990

Figure 3.6

Predictive Power of Pencavel: Semi-logarithmic Specification

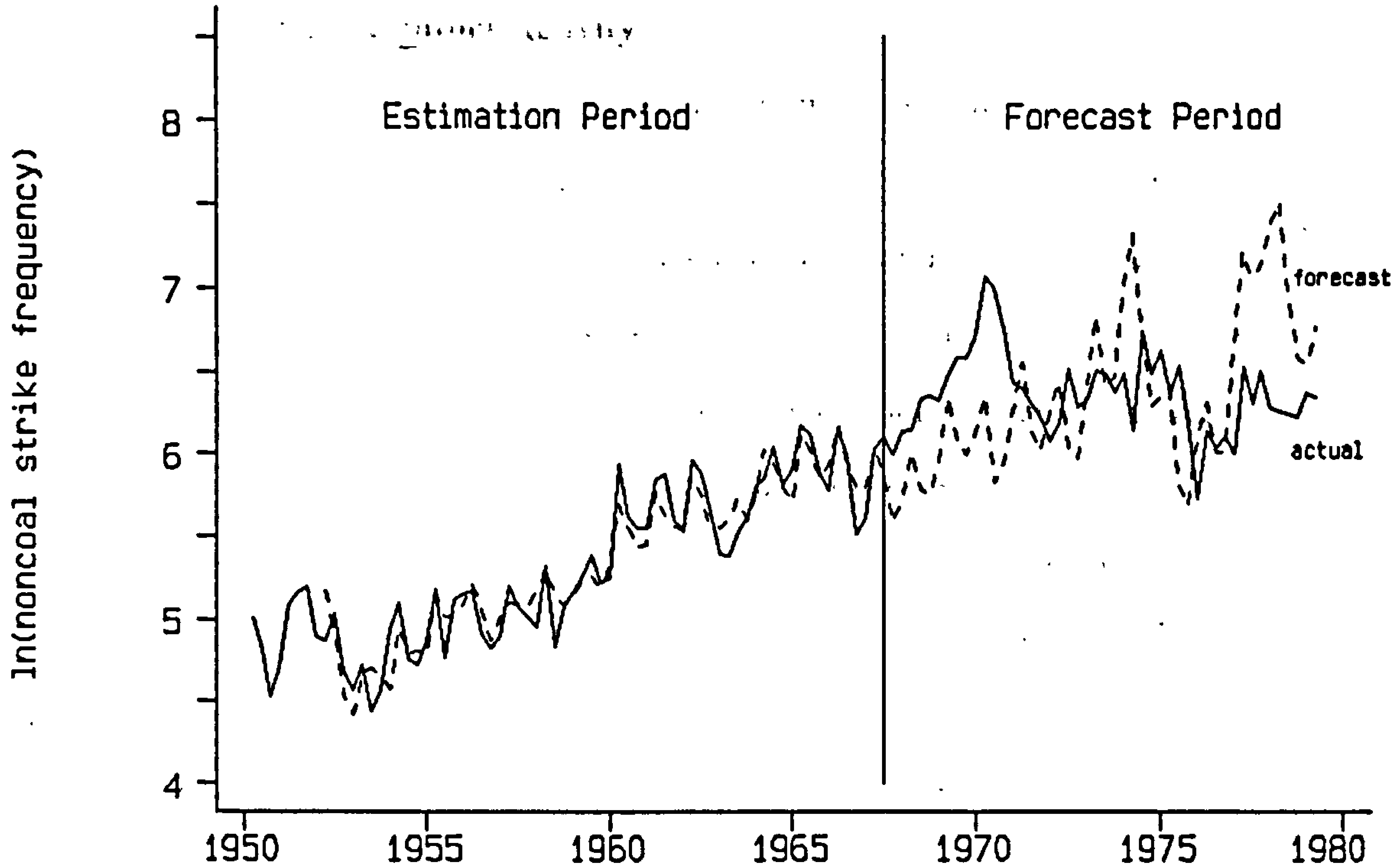
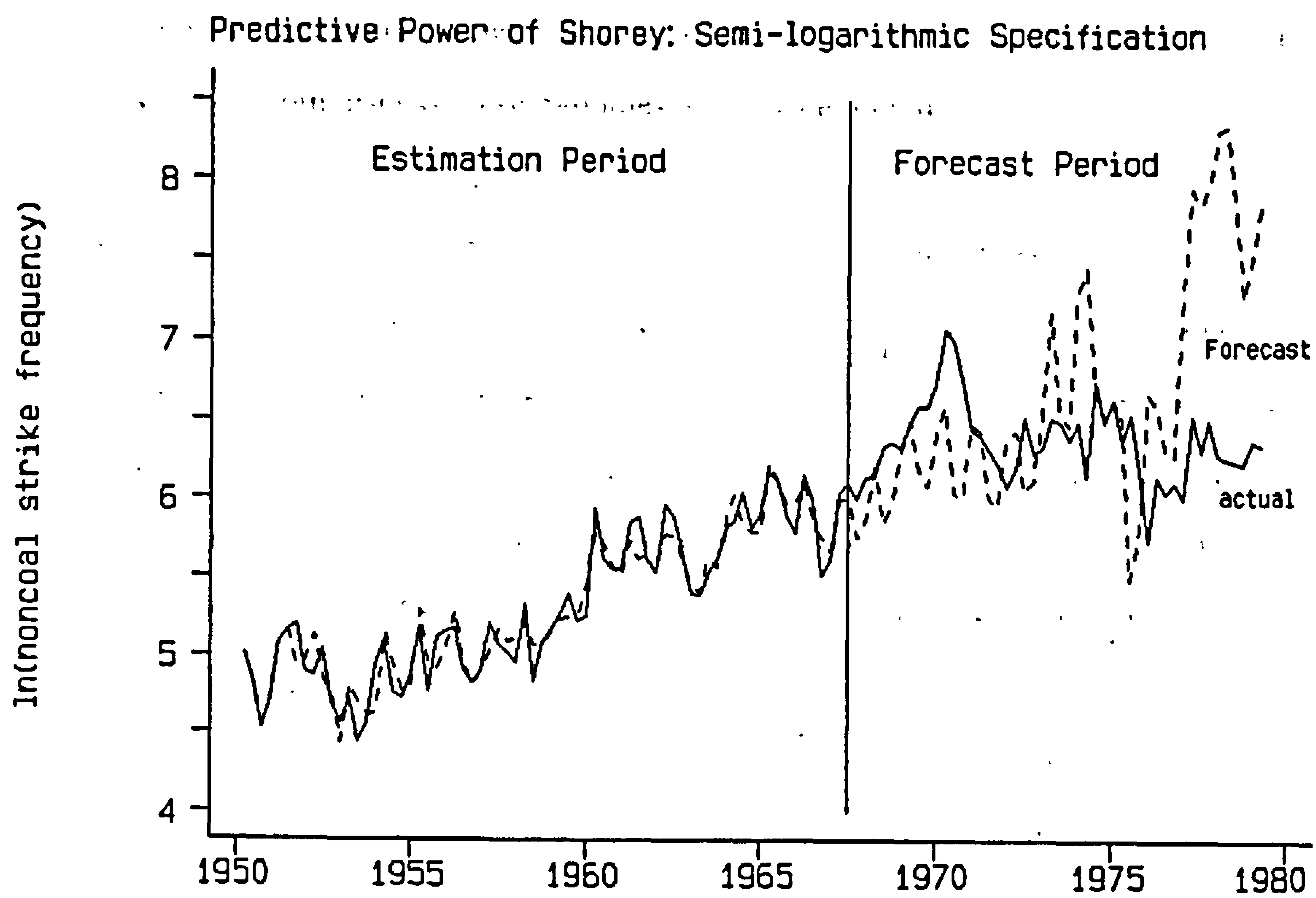


Figure 3.7



Predictive Power of Shorey: Semi-logarithmic Specification
Estimation Period
Forecast Period
Forecast
actual
ln(noncoal strike frequency)
8
7
6
5
4
1950
1955
1960
1965
1970
1975
1980

Figure 3.8

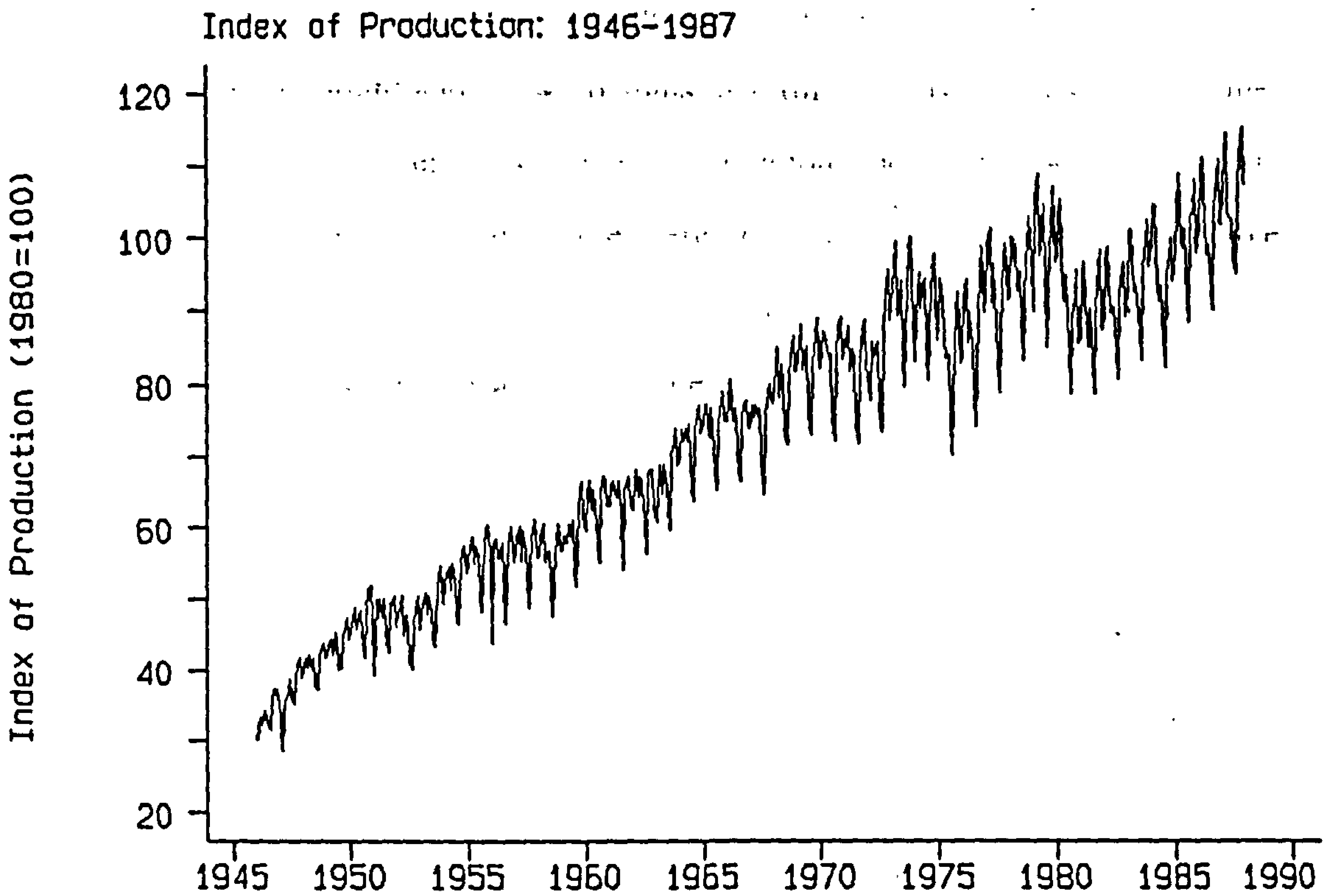
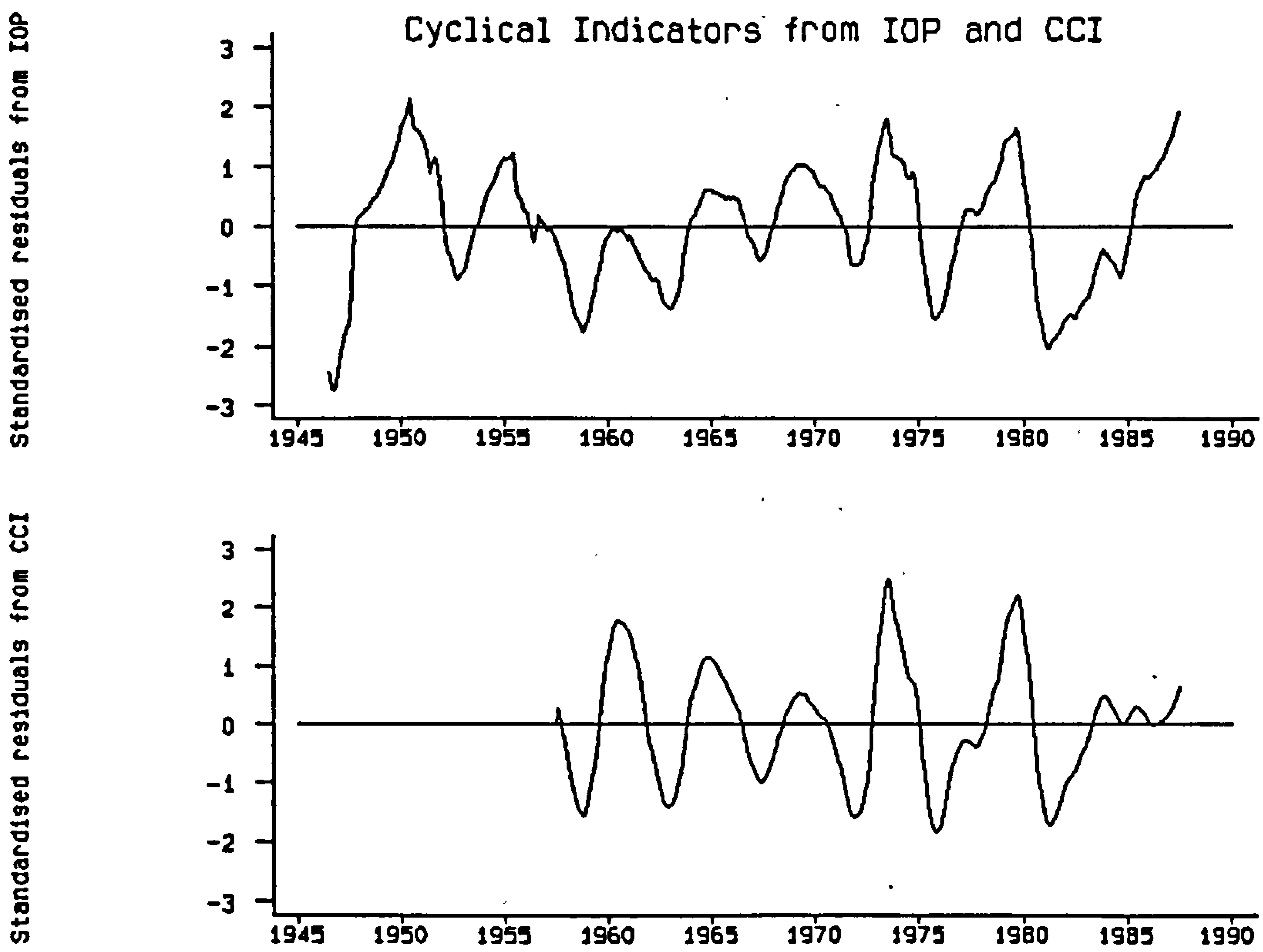
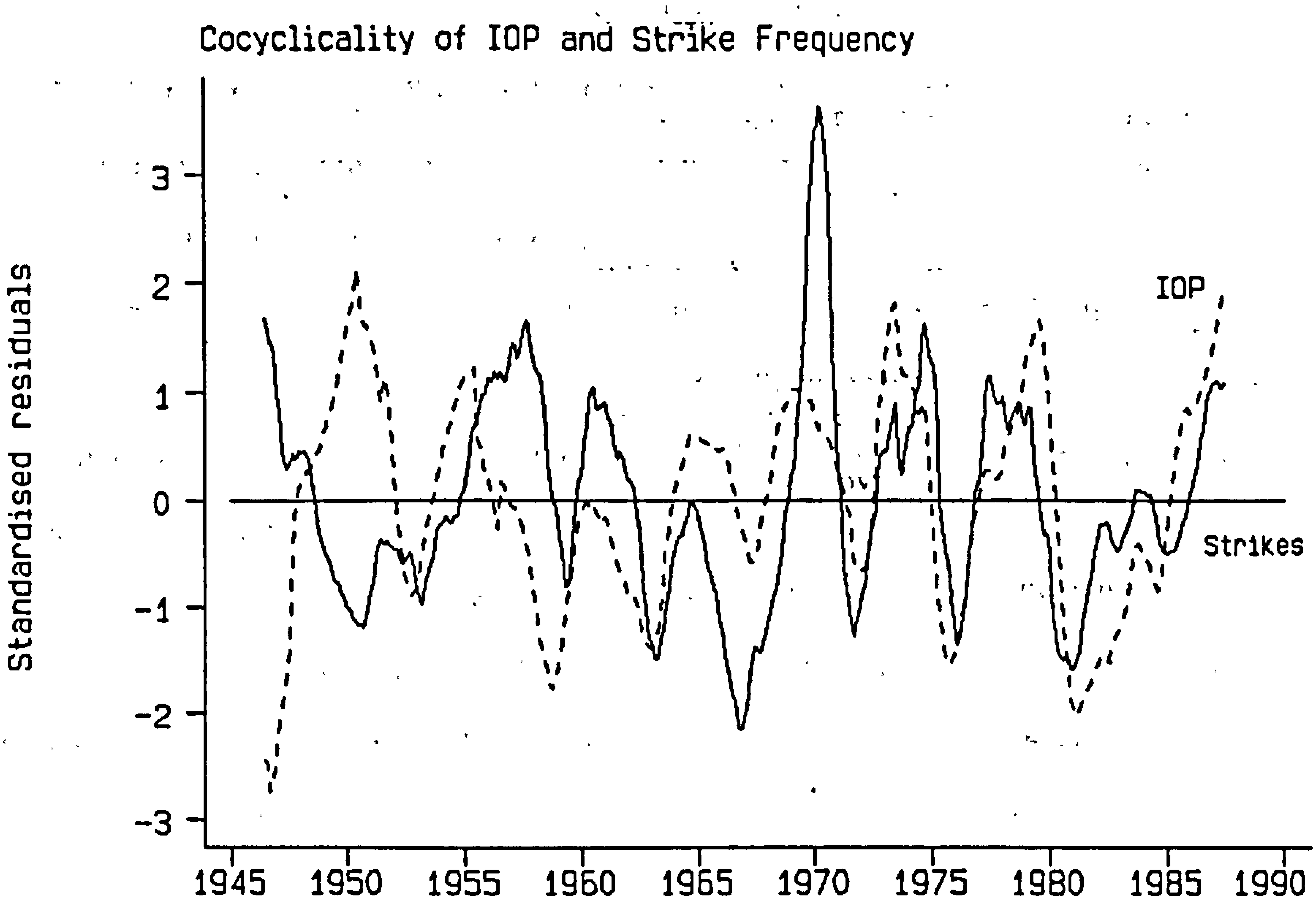

Index of Production: 1946-1987
Index of Production (1980=100)
120
100
80
60
40
20
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990

Figure 3.9



Cyclical Indicators from IOP and CCI
Standardised residuals from IOP
3
2
1
0
-1
-2
-3
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990
Standardised residuals from CCI
3
2
1
0
-1
-2
-3
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990

Figure 3.10



Cocyclicality of IOP and Strike Frequency
Standardised residuals
3
2
1
0
-1
-2
-3
1945
1950
1955
1960
1965
1970
1975
1980
1985
1990
IOP
Strikes

Figure 3.11

Appendix to Chapter 3

A3.1. Other Forms of Conflict Activity.

It is apparent that other forms of conflict activity[1] are important factors in the collective bargaining process. Indeed, strike action is frequently seen to be a derivative of situations in which such other sanctions have previously been employed. Yet the study of non-strike action has been seriously neglected in the literature. This is undoubtedly due to the fact that official statistics only cover stoppages (albeit imperfectly as noted above) and not these other indicators of industrial unrest. In addition, non-strike action is typically less explicit than overt strike action and thus attracts only secondary public and media attention. Hence, while the patterns and trends have been described in the measured realisations of conflict, these unrecorded manifestations may yet be of greater economic and political significance.

A few commentators have previously noted the importance of non-strike sanctions; Clegg (1979) goes as far as to argue that,

> "... there can be little doubt that the overtime ban is now the most common form of industrial action." (p.258)

Despite such claims, the investigation and analysis of non-strike action, together with its association with overt strike action, is still extremely limited. Within the industrial relations literature, there is some debate as to whether the 'alternative' or the 'additive' hypothesis is appropriate to describe the relationship between strikes and non-strike action. The former argues that industrial action arises as a response to work-related discontentment which can be expressed through a variety of channels such as strikes (organised) through to absenteeism and quitting (unorganised), and that these are substitutes for each other. In contrast, the 'additive' hypothesis asserts that where there is a high incidence of one form of conflict, other forms will also be in evidence, a result perhaps of generally poor industrial relations within the workplace

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1. These include working-to-rule, overtime bans and restrictions, go-slows, and blacking of work.

environment. According to this school, non-strike sanctions are likely to be an accompaniment preceding, rather than an alternative to, a strike. Bean (1975) and Edwards (1979) both discuss these hypotheses in further detail.

There is a minimum of published evidence on non-strike sanctions and it has only been with the advent of detailed surveys of industrial relations that information pertaining to collective non-strike action has become available. The evidence gathered by these surveys for some selected forms of non-strike action is chronologically summarised in table A3.1[2]. It is apparent that non-strike action is at least as prevalent as strike action. Unfortunately comparisons over time are not feasible because of the differing industrial coverage of the surveys and the time span to which the questions regarding the incidence of conflict activity refer (the 'referral period'). However, for each survey, the ratio of the incidence of non-strike action to strike action can be used to gain an impression of the relative importance of non-strike sanctions, and how this may have changed over time. This methodology is also suggested by Brown (1981), who concludes that,

> "... the relative importance of action short of a strike has fallen."(p.83)

Further support for this observation would appear to be provided by the two Workplace Industrial Relations Surveys; approximately equal numbers of establishments experience non-strike and strike action, in sharp contrast to the earlier surveys in which the ratio was much higher. Brown suggests that a possible explanation lies in the increase in unemployment in his reference period; since non-strike action is less costly at such times (to both employer and employee), employers may be more prepared to hold out against such sanctions, thus reducing their effectiveness. Strike action may then be the only potent collective sanction available. However, given the substantial increases in unemployment in the early 1980s,

2. The only known omission is the survey collated for the Donovan Commission (Government Social Survey (1968)). In this case, the respondents were simply asked whether industrial conflict had taken place "since they took up their present position". Thus the referral period is unknown and not constant for the different respondents (union stewards and works managers) who each had rather different average tenures.

augmenting the evidence he presents with that provided by the Workplace Industrial Relations Surveys would seem to contradict this hypothesis. Non-strike sanctions are clearly still very important and show no signs of further relative or absolute decline. They certainly merit further substantive investigation and chapter 4 of the thesis provides such an analysis. Finally, while there now exists survey data on collective non-strike sanctions, it would appear that an analysis of individual expressions of discontent, especially absenteeism, is still some way off[3].

[3]. Some additional observations are noted by Kelly & Nicholson (1980) who also discuss the problem of providing a suitable definition of 'conflict' and in particular, whether absenteeism and labour turnover can be classified as such. Their attempted survey of South Yorkshire firms met with little success; a response rate of only 21% drew 51 replies, and therefore generalisations beyond this tiny sample seem unwise.

Table A3.1

The Incidence of Strike and Non-Strike Activity: A Summary of Survey Evidence

Percentages of Respondents Reporting Industrial Action

Survey Source	OPCS 1972 Parker (1974)		OPCS 1973 Parker (1975)		PEP Daniel (1976)	IRRU Brown (1981)		WIRS 1980 Daniel & Millward (1984)			WIRS 1984 Millward & Stevens (1986)		
Compilation Date	Jan - Mar 1972		Apr - Jun 1973		Oct - Dec 1975	Winter 1977-78		Apr - Sep 1980			Mar - Oct 1984		
Referral Period	Previous 2 Years		Previous 2 Years		Previous 12 Months	Previous 2 Years		Previous 12 Months			Previous 12 Months		
Industrial Coverage	'Industrial Relations Setting'		'Industrial Relations Setting'		Manufacturing Estab. with > 200 employees	Manufacturing Estab. with > 50 employees		All Establishments with > 25 employees			All Establishments with > 25 employees		
Respondent	Stewards	Senior Managers	Stewards	Senior Managers	Manager Negotiators	Senior Managers		Either Manager or Worker Respondent Reported Action			Either Manager or Worker Respondent Reported Action		
Worker Coverage	All Workers		All Workers		All Workers	Manual	Non-Man	Manual	Non-Man	Either	Manual	Non-Man	Either
No Conflict Activity	n/a	n/a	n/a	n/a	60	54	91	84	89	88	87	82	75
Strike Action	35	32	23	18	24	33	5	11	4	13	8	14	19
Non-Strike Action including:	60	38	53	40	n/a	29	6	10	8	16	8	12	18
Overtime Ban	38	24	36	25	17	23	4	7	5	10	6	6	11
Work-to-rule	30	19	25	17	16	12	3	4	4	7	2	6	8
Go-Slow	7	9	9	9	7	4	1	1	--	1	--	--	--

Notes:
1. Both of the WIRS surveys exclude the coal industry and the agricultural sector.
2. n/a denotes not available.
3. -- denotes negligible.

Table A3.2 Data Sources and Description	
Strike Frequency S_t	Employment Gazette, various issues Stoppages involving fewer than ten workers and those lasting less than one day are excluded from the statistics unless the nuumber of days lost exceeds one hundred.
Unemployment Rate U_t	Employment Gazette, various issues.
Profits Rate D_t	Defined as gross trading profits as a percentage of wage and salary compensation. Klein *et al* (1961) plus Monthly Digest of Statistics. Base: 1948=100.
Weekly Wage Rate W_t	Klein *et al* (1961) and Employment Gazette. Base: 1956=100.
Retail Price Index P_t	Klein *et al* (1961) and Employment Gazette. Base: 1956=100.
Real Profits π_t	National Accounts

CHAPTER 4

The Incidence of Industrial Action in Britain

4.1. Introduction.

That there have been substantial changes in the collective bargaining arena in the 1980s can be in little doubt. Transitions in the structural, legislative and macroeconomic environment have led to unions and firms facing very different circumstances from those that prevailed in the 1970s. While the implications have yet to be fully realised, it seems prudent to begin to try to assess the effects that these changes may have had. Stewart (1991) examines the variation in the union wage differential between 1980 and 1984 utilising the Workplace Industrial Relations Surveys[1] and finds that the mean differential altered very little. What change there has been is attributed to the decline in manufacturing industry and a downward shift in the size distribution of establishments. In particular, industry union density and local unemployment rates are seen to have negligible net impacts on the average union wage differential. This chapter can be considered as complementary to Stewart's paper in that it is concerned with the impact of these changes on another major area of union activity, that of industrial action.

The last ten years have seen substantial reforms in the traditional non-interventionist approach to industrial relations in Britain against a background of fundamental economic and structural upheaval. The activity of trades unions may be supposed to have been curtailed as a consequence of both falling union membership together with the labour legislation introduced by the Conservative administration since 1979. These institutional changes in particular may have weakened the relative strength of

[1]. See Daniel & Millward (1983) and Millward & Stevens (1986) and the discussion in section 4.3 below.

unions thus reducing their ability to organise and induce their members to take industrial action. Additionally, the generally poor economic outlook in the first half of the 1980s may have led unions to alter the balance of their relative preferences from wages to employment protection for their members. Finally, it seems probable that there will be differences in the (net) impact of these changes on strike and non-strike activity; much of the legislation has been concerned with overt strike action and, in the face of high unemployment and weak domestic demand, sanctions such as go-slows and working-to-rule may have become relatively more attractive grievance activities for both employers and employees.

Utilising the Workplace Industrial relations Survey data, this chapter documents the changes in conflict activity in the 1980s and attempts to provide plausible explanations for the observed patterns in industrial conflict at the establishment level in light of the various transitions in the structural, economic and legislative background. The chapter therefore provides an alternative measure of conflict activity to the aggregate and disaggregate measures of strike frequency as investigated in chapter 3, while also providing an opportunity to extend the analysis of the determinants of strikes to include a wider range of institutional factors than is possible in the aggregate studies.

The remainder of the chapter is organised as follows. The major changes in the bargaining environment in the 1980s and some likely implications for industrial conflict are described briefly in section 4.2[2]. The data source is described in section 4.3 and the evidence it contains pertaining to industrial conflict is summarised. The divergence between strike frequency as described in section 3.2 and strike incidence is also highlighted in this section. Results from estimating conflict activity equations for 1980 and 1984 utilising the Workplace Industrial Relations

[2]. The emphasis is on the early 1980s since this is the period on which the remainder of the chapter concentrates.

Surveys are reported in section 4.4 and appropriate comparisons between strike and non-strike activity are made. Finally section 4.5 presents some concluding comments.

4.2. Structural, Economic and Legislative Developments in the 1980s.

Structural Changes in the 1980s.

With reference to industrial conflict, arguably the most significant structural change that took place in the early 1980s was the dramatic fall in union membership both in absolute number and relative to total employment. Union membership fell from over 13 million in 1979 to around 10 million by 1988 while union density (as a proportion of employment) fell from 58% in 1979 to just over 50% by 1984, and to around only 40% by the end of the decade, a rate rather lower than that which prevailed in the 1950s and 1960s. These movements have been well documented by Disney & Mudambi (1987), Carruth & Disney (1988), Disney (1990) and Freeman & Pelletier (1990) among several others. The hypotheses suggested in the literature for the decline in union density are diverse and are difficult to separately identify empirically in a precise manner. The 1980s have seen a major restructuring of UK labour markets and, undoubtedly, some of the fall in union density can be accounted for by the compositional transitions that have taken place, in particular, the shake-out in manufacturing industry in the early 1980s. As Towers (1989) notes, the relevant structural factors that have contributed to the fall in union membership include:

> "... the shift in the balance of the economy towards services; the increasing number of women and white-collar workers; the growing incidence of temporary, part-time and peripheral employment; and the decline in the number of workplaces employing large groups of people." (p.180)

However, such arguments fail to account for the rise in density in the 1970s. In addition, as Disney (1990) notes, much of the decline in 'heavy' manufacturing occurred in the 1970s before the downturn in unionisation, while the recession particularly affected industries such as clothing and textiles which were not highly unionised. Hence others attribute the trends in union density primarily to cyclical economic factors and/or legislative changes.

Millward & Stevens (1986, p.52) attribute the decline in the proportion of private manufacturing workplaces with manual trade union members from 76% to 66% as being almost entirely due to the disproportionate closure of large (and therefore generally unionised) establishments between 1980 and 1984. Alternative explanations, such as newer establishments being less likely to be unionised or significant de-recognition of unions cannot account for such a substantial fall; indeed, union recognition in unionised establishments was unchanged between the two surveys at about 85%. It is also clear that, although aggregate density and coverage may have fallen significantly, union density in unionised establishments changed little. The implication is that there was a large increase in the proportion of establishments with no union representation.

Whatever the explanation for the decline in unionisation in the 1980s, the consequences for union bargaining strength are also not straightforward and seem likely to differ by industrial sector. Where bargaining takes place at the firm or establishment level, the increased competition from non-unionised firms may act as a moderating influence in negotiations, and thus reduce the likelihood of overt action being undertaken in support of any wage claim. Alternatively, increased resistance by employers in the face of this stronger competition may result in unions having to press harder for any given gain. In contrast, however, where bargaining is conducted at the regional or national level, the collective strength of the union may still be sufficient that such considerations are unnecessary. For the UK, while company level bargaining has certainly increased in the last decade, there are still industrial sectors which negotiate mainly at the national level and thus these may be less affected by the increase in non-unionised establishments. However, on balance, the aggregate effect would seem likely to have reduced the likelihood of industrial action across all establishments, although there may be an offsetting increase among some highly unionised establishments and industries. This may be particularly true for the public sector, which having faced the general incomes policies in the 1970s was then faced with strict cash-limits in the early 1980s in order to restrict public spending. The resulting erosion of their position relative to the private sector is well documented. Chapter 5 examines the distinctions between public and private sector conflict activity in much greater detail.

Economic Changes in the 1980s.

The most important macroeconomic change with respect to conflict activity was undoubtedly the more than doubling of the rate of unemployment from around 5% in 1979 to over 10% by 1983. Unemployment finally peaked at about 12% in 1985, before declining continuously until 1990. In addition, although the early 1980s was a period of world-wide recession, the UK suffered relatively severely under the generally deflationary stance and tight monetary policy which characterised the early Thatcher government. Since 1985, Britain's growth rate has finally reached the levels attained by the other OECD countries following a period of comparatively poor performance in the early 1980s. In summary therefore, there were severe pressures on the labour market for much of the decade and even after the downturn in unemployment, extreme regional disparities still exist.

With high unemployment, the threat of a strike becomes less credible since it represents a more costly action for the union given the lack of alternative employment prospects, and is less costly to the firm given the general slackness in demand. Reinforcing this effect is the probable shift in union preferences from wage increases to employment protection for union members. However, this lower pressure for wage increases in recession will only result in fewer recorded incidences of industrial conflict if the reduced likelihood of action over wage increases is not simply offset by the increase in industrial action in relation to protecting employment levels. There may also be a corresponding switch from strike to non-strike action at such times, the latter being a lower-cost form of action to both employee and employer. Any associated decrease in the risk of workers being dismissed may also be a factor in unions substituting non-strike sanctions for overt strike activity.

Stewart (1991) also highlights the improvement in international competitiveness in the period resulting from the fall in exchange rates against most major currencies and in particular those of our trading partners. This served to increase the profitability of those firms operating in international markets, although these tend to be the least unionised firms and hence those experiencing little industrial action. Although the aggregate impact is likely to be small, this would serve to further reduce conflict activity since these firms could more easily meet demands for wage increases.

Legislative Changes in the 1980s.

Perhaps the most significant changes in industrial conflict would be expected to result from the impact of the legislative reforms introduced since 1979. It is clear that the tradition of 'voluntarism' or 'abstentionism' in British industrial relations in the post-war period has now been largely superseded, firstly by a more interventionist role for the state in collective bargaining and secondly, by an individualistic legal framework which it is difficult to reconcile with the notion of collective industrial relations. The increasing juridification of British post-war industrial relations has been quantified by Hepple (1983) who notes,

> "Compared with some five general Acts ... regulating employment passed from 1950 to 1959, and sixteen from 1960 to 1969, there were thirty from 1970 to 1979 and a further eight from 1980 to 1982." (p.393)

However, while there were fundamental changes in labour law throughout the 1970s, the incoming Conservative administration in 1979 introduced a radically different approach, encapsulated in the Employment Acts of 1980, 1982 and 1988 and the Trade Union Act of 1984. There were new restrictions on employment protection;

much of the legislation regarding minimum conditions for less well organised or low paid workers was dismantled in pursuit of the free operation of market forces; with respect to industrial action, restrictions were placed on picketing, especially secondary action which dominated much of the debate regarding the winter of discontent of 1978/79; changes were made in the laws relating to the dismissal of strikers, closed shops, election of union officials, political subscriptions and so on. While purporting to support individual rights in the employment relationship, it is clear that this has been largely at the expense of collective rights as acclaimed by trades unions. Most significantly perhaps, compulsory ballots for official strikes were introduced in the 1984 Trade Union Act. The question prescribed by the Act is whether the worker *is prepared to take part in a strike involving him in a breach of his contract of employment.* Wedderburn (1986) and others have argued that the question is hardly neutral, and does not include a statement to the effect that strikes are usually a breach. The new changes and those yet proposed (with respect to closed shops and unofficial action for example) mark a significant departure from preceding legislation supporting collectivism and many of the clauses would seem to reflect a rejection of the legitimacy of collective action.

The effect of this body of legislation depends on the extent to which individuals, unions and employers actually utilise the new Acts, or whether they retain much of the 'custom and practice' in their collective bargaining behaviour. Evans (1985, 1987) records 34 applications for injunctions in industrial disputes between September 1980 and September 1984, half of which related to picketing, whereas between May 1984 and April 1987, he records a total of 77. Batstone (1988) notes only 70 uses of post-1979 labour legislation which resulted in legal action being taken in the period 1980-85. Thus it is clear that employers have not rushed to take legal action in any circumstances and most disputes are still settled without recourse to the courts[3].

The clauses in the various Acts relating to strike action would appear to substantially increase its potential cost and therefore decrease its probability of occurring. Moreover, the unions' effective power is diminished once a strike has begun by restrictions imposed on secondary action and hence the likelihood that strike action is effective is correspondingly reduced. Together, these effects would serve to decrease the probability of direct action, although whether grievances are simply translated into non-strike action or somehow dissipated is unclear. For the period of interest between 1980 and 1984, it should be noted that the full impact of the 1984 Trade Union Act will not have been felt, although its ramifications certainly dominated much of the debate with respect to industrial action at that time. Of greater importance therefore were the two Employment Acts (1980, 1982) and perhaps in particular, their impact on closed shops and secondary action. Millward & Stevens (1986, chapter 4) document these changes in full and reveal that the proportion of manual workers in a closed shop fell from 40% to 30% between 1980 and 1984. This seems likely to have reduced the bargaining strength of unions at the establishment level. With respect to picketing, the Workplace Industrial Relations Surveys reveal that the overall extent declined by almost one half, from 11% of all establishments in the twelve months preceding the 1980 survey, to only 6% in 1984 (Millward & Stevens (1986, pp.282-294))[4].

The net impact of these structural, economic and legislative changes in the early

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3. However, it should be noted that the *threat* of such action has probably been a sufficient deterrent in many more incidences since the law has been rigidly applied in the vast majority of cases that have reached the courts, with some notable cases of large fines and sequestration of union funds.

4. Manning (1989) considers a theoretical model of the effects of the restrictions on secondary action on employment, wages and investment. However, his model is one of complete information and hence effectively precludes industrial action taking place. It is therefore inappropriate for the analysis of the realisations of conflict investigated in this paper.

1980s would appear to have severely weakened trades unions and thus diminished their ability to organise collective industrial action in support of their claims. Moreover, the discussion above indicates the potential advantages of a shift away from overt strike action in favour of non-strike sanctions; these are less costly, less risky in terms of employment, easier to organise, and less likely to invoke legal action against the unions. Against these factors, the weakened trades unions may have had to resort to taking strike action more frequently in order to legitimise their claims if their threat effect has been significantly eroded. Whether these expected transitions in industrial conflict have indeed taken place is examined in the remainder of the chapter.

4.3. The Workplace Industrial Relations Surveys and Conflict Activity.

This section of the chapter briefly outlines the structure and composition of the Workplace Industrial Relations Surveys and summarises the evidence that the surveys yield on the incidence of conflict activity at the establishment level. The differences in the recorded levels of strike frequency and (establishment level) strike incidence are also highlighted.

4.3.1. The Workplace Industrial Relations Surveys.

The Workplace Industrial Relations Surveys of 1980 (WIRS80) and 1984 (WIRS84) are described in detail by Daniel & Millward (1983) and Millward & Stevens (1986) respectively. The sampling frame in each case was the Census of Employment (1977 and 1981 respectively) but both surveys exclude agricultural, farming and coalmining establishments and all those with less than 25 employees. The sampling design incorporates varying sampling fractions according to the number of employees at a 'census unit' to ensure that large establishments were adequately represented in the sample. Hence the data needs to be weighted to adjust for the sampling stratification. Over 2000 establishments were surveyed in both 1980 and 1984, and responses were obtained from both manager and worker representatives where possible[5]. One major advantage of the WIRS is that the broad consistency of the two surveys permits comparisons between 1980 and 1984.

There is a wealth of information about workplace industrial relations contained in the two surveys and a number of authors have already utilised various aspects of the data, in particular that on wages (Blanchflower (1984), Stewart (1987, 1990)), share ownership and other profit sharing schemes (Blanchflower & Oswald (1986), Gregg & Machin (1987)), financial performance (Machin & Stewart (1990)) and the impact of unions on economic performance (Blanchflower *et al* (1991), Machin & Wadhwani (1991a, 1991b)).

The WIRS contains several questions relating to the nature and incidence of industral conflict including the number, duration, cause of strikes and non-strike sanctions at the unit of observation which is the establishment. The two surveys have several advantages over simple deflation of the statistics for strike frequency as a measure of strike incidence as suggested by Stern (1978) and described in section 3.2. Firstly, it is possible to calculate directly the incidence of conflict activity at this level of disaggregation. Secondly, the important effects of establishment and firm size that have been noted by Shorey (1975), George *et al* (1977), Prais (1978), Edwards (1980) and many other researchers in this field can be identified. Thirdly, the Workplace Industrial Relations Surveys yield information on the incidence of non-strike sanctions, specifically overtime restrictions, work-to-rules and go-slows. The study of realisations of conflict other than strikes has been largely neglected in the literature, most probably because non-strike sanctions have a lower profile than strike action and

5. There is also a small panel of establishments at which interviews were conducted in both surveys but this subset proved too small for useful analysis.

because data have previously been less readily available[6]. But these other forms of collective action are undoubtedly of considerable importance, either as an accompaniment preceding, or as an alternative to a strike and they certainly deserve much greater consideration than has been afforded to date. There has been some limited discussion of unorganised and individual manifestations of conflict such as absenteeism and labour turnover, and these have been found to be positively correlated with strike activity across plants and between industries and regions (Bean (1975), Edwards (1979)). However it remains to be seen whether a similar relationship exists between strike action and collectively organised non-strike activity.

Finally, in an important sense strike *frequency* and strike *incidence* comprise different dimensions of strike activity; Millward & Stevens (1986, p.265) suggest that the latter is a measure of the *extensiveness* of strikes. In the past however, frequency and incidence have often been used interchangeably and models of strike incidence estimated using statistics for strike frequency. Although clearly untenable, such bold assumptions may ultimately not be significant. Some legitimacy for the procedure may be therefore provided by the current study if the conclusions reached with regard to the incidence of strikes are qualitatively similar to those obtained for strike frequency.

4.3.2. Conflict Incidence as Recorded by the Workplace Industrial Relations Surveys.[7]

This section concentrates exclusively on the responses to and problems associated with the questions regarding the incidence of conflict activity in the two Workplace Industrial Relations Surveys. At approximately half of all establishments at which interviews were conducted with management representatives in the two surveys, there

[6]. Certainly it has only been with the advent of detailed surveys of industrial relations that information relating to non-strike sanctions has become available. These are discussed in the appendix to chapter 3.

[7]. Summary statistics are generated using SPSSx.

was also a secondary questionnaire conducted with a representative of the manual and/or non-manual workers. For both management and worker representatives, the questions referred to the twelve months prior to interview, and thus cover the periods mid-1979 to mid-1980 for the 1980 survey and mid-1983 to mid-1984 for the 1984 survey.

As would be expected, incomplete and imperfect recall and differing perceptions of industrial action and its various forms imply that workers and managers will often record the incidence of industrial action differently. This is clearly illustrated in table 4.1 which records for those establishments where *both* manager and worker representatives were interviewed, their responses to questions regarding the incidence of strike and non-strike activity[8,9].

These 2 x 2 contingency tables clearly demonstrate the degree of disagreement. In particular, it is apparent that with the exception of manual workers strike activity in 1980, workers' representatives more frequently report both strike and non-strike activity. Of perhaps greater significance is that there are a large number of establishments where one but not both of the respondents reported industrial action of some kind. For example, the cross-tabulation for manual workers in 1980 reveals that while in 9.0% of establishments both respondents reported strike action in the previous twelve months, in an additional 12.8% either the management or the worker representative (but not both) also reported a strike. And it is clear that this does not simply represent systematic over-reporting by worker representatives since there are a large number of establishments for which it is only the management representative who reports industrial action, particularly for manual workers.

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8. All the statistics are weighted to correct for the stratified sampling scheme used in the compilation of the Workplace Industrial Relations Surveys (see Millward & Stevens (1986, pp.329-332)).

9. 'Any Action' is defined as either strike and/or non-strike action.

The problem is thus to somehow reconcile these differing rates of conflict activity as reported by the two types of respondent. Given that there are a substantial number of establishments where industrial action was reported by only one of the respondents, it would appear that the most significant problem is one of incomplete recall. This supposition accords with the finding of Daniel & Millward (1983) who conclude,

> "In consequence, the balance of possible error is in the direction of under-reporting industrial action ... (and) reliance on a single respondent's account will almost certainly lead to underestimation of the extent of industrial action." (p.216)

Therefore, following Daniel & Millward (1983) and Millward & Stevens (1986), the strategy adopted here is to integrate the reports of both management and worker representatives where possible. Thus establishments are assumed to have incurred conflict activity if *either* respondent reports industrial action. Note, however, that for those workplaces where no interview was conducted with a worker representative, the only information available is that from the management respondent and hence the possibility of under-recording still exists in this subsample[10].

Table 4.2 presents incidence rates of various manifestations of industrial action as computed in the manner described above[11]. It is apparent that there have been important changes in the incidence of conflict at the establishment level between 1980 and 1984, although these have not affected manual and non-manual workers identically. Conflict activity among manual workers actually fell by about one fifth while the substantial increase in non-manual industrial action was sufficient to yield an overall increase in the rate of conflict activity for all establishments from 22% to over 25%. Much of this can be seen to derive from the increase in strike activity among non-manual workers and, in particular, short strikes of one day or less. The nearly 50% *increase* in the proportion of establishments incurring strike action

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10. Full consideration of the possible bias arising from combining the responses in this manner is given in the estimates reported in section 4.4 below.

11. Table 4.2 can be seen to partially replicate table 10.1 from Millward & Stevens (1986, p.264).

contrasts quite starkly with the finding in section 3.2 above that strike frequency continued to *fall* quite rapidly throughout the 1980s. This distinction is discussed further below.

In part, these changes in conflict incidence undoubtedly reflect the particular disputes that occurred in the twelve months prior to each of the survey's interviews; the 1980 figures are therefore affected by the 1979 engineering workers' dispute while the statistics for 1984 are heavily influenced by the widespread disruptions in education and the public service sector more generally. Clearly, these period-specific sectoral differences need to be taken into account when attempting to identify and explain the more general patterns in the incidence of industrial action. Table 4.3 presents the broad measures of conflict broken down by the industrial sector of the establishment in order to investigate this sectoral specificity in more detail.

It is immediately apparent that the overall increase in conflict is entirely accounted for by public service establishments, and in particular strike action among their non-manual employees. This highly unionised sector has therefore failed to respond to the multiple pressures mitigating against strike activity as outlined in section 4.2. As noted previously, the public sector received the additional burden of government imposed limits on pay increases, culminating in widespread dissent among certain service sector unions and their members. It is this, and in particular the dispute in education, that the statistics for this sector therefore reflect.

Conflict activity in the other sectors perhaps more closely accords with the thoughts expressed in section 4.2. This is particularly true for private manufacturing; the overall rate of conflict activity fell by one third in this sector but, while strike activity was halved, non-strike activity only fell by one quarter[12]. Hence there is some

12. Note that for non-manual workers in private manufacturing, strike incidence falls while non-strike incidence actually increases.

evidence for the predicted 'switching' into non-strike activity (within the overall decrease in conflict). That this sector should appear to have been more responsive to the forces that were prevalent in the early 1980s is of little surprise since it was the highly unionised manufacturing sector that was particularly affected by the economic recession of the early 1980s, and which faced the most severe domestic and international competitive pressures.

There is one final important distinction that the survey data reveal. A prerequisite for industrial action would seem to be that workers cooperate in a formal and explicit manner and unionisation will be the form of organisation utilised in the vast majority of cases. Indeed, it is difficult to conceptualise what might be implied by a 'strike' among non-organised workers. 'Unionised' establishments were therefore defined as those in which any union members present were recognised for the purpose of bargaining and negotiation; table 4.4 disaggregates the broad measures of conflict incidence from table 4.2 according to the unionisation status of the establishment. It can be seen that virtually all of the recorded conflict incidence is for 'unionised' establishments while 'non-unionised' establishments report very few occurrences of any type of industrial action as surmised above[13]. When further disaggregated by sector as in table 4.3, similar patterns are revealed, with evidence of switching between strike and non-strike activity in the unionised private sector.

It is important to note that the recorded overall increase in strike *incidence* in table 4.2 from 13.2% in 1980 to 19.1% in 1984 is not incompatible with the decrease in strike *frequency* as reported by the Department of Employment in its statistics published in the Employment Gazette. These reveal a fall in the number of strikes from 1,871 in

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13. Note that for most (all) of the 'non-unionised' establishments in 1980 (1984), the only respondent was the manager representative; few (no) worker representatives were interviewed at establishments which did not have recognised union members simply because a worker 'representative' could not be identified. Thus there remains the possibility that these statistics may underestimate the true proportion of establishments incurring industrial action.

the year to May 1980 to 1,416 in the year to May 1984, a decrease of one quarter[14]. The implication is that although there may have been fewer strikes in 1984, they were more widespread and thus affected a higher *proportion* of establishments than in 1980. This difference serves to highlight the fact that these alternative data sources are measuring very different dimensions of strike activity and further emphasises the important distinction between strike frequency and (establishment level) strike incidence.

The analysis above indicates that while models for strike frequency abound in the literature, they fail to describe the distribution of strike incidence since this differs substantially from strike frequency. In particular, large disputes such as that in education in 1983/4 are recorded as a single strike in the frequency statistics despite the wide-spread disruption caused. The measures of strike incidence therefore more appropriately reflect disputes of this kind. A further advantage is that statistics for incidence will only record conflict a single time where action at the establishment is intermittent but essentially concerned with a continuing grievance; frequency statistics would indicate that several strikes have occurred.

Of course, there are also inherent weaknesses in survey data such as the Workplace Industrial Relations Surveys. In particular, they tend to reflect the dominant disputes in progress at the time that they are compiled. Additionally, intertemporal comparisons are limited to changes occurring between survey dates, while it would be of greater interest to compare the impact of the transitions identified in section 4.2 on industrial relations and conflict activity over a rather longer period than that allowed by the two survey dates. Within these limitations, however, the surveys provide some interesting and important evidence pertaining to the changing incidence of conflict

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[14]. The median interview date for both of the Workplace Industrial Relations Surveys was sometime in May (Millward & Stevens (1986, pp.323-324)) and hence the periods chosen are those relevant for comparison. These statistics exclude stoppages in the coal industry in order to be comparable to the two WIRS.

activity in Britain in the 1980s. Section 4.4 therefore presents an analysis of the determinants of conflict activity as identified by the Workplace Industrial Relations Surveys.

4.4. The Determinants of Conflict Incidence: Evidence from the Workplace Industrial Relations Surveys.

There have been two previous studies of the incidence of industrial action utilising the Workplace Industrial Relations Surveys and hence some rationale must be provided to justify yet another. Blanchflower & Cubbin (1986) (hereafter B&CUB) who utilise the 1980 survey and Booth & Cressy (1990) (hereafter B&CRE) who examine the second survey both consider manual workers only. Neither incorporate the reports of worker representatives where these were available and hence both are liable to underestimate the incidence of conflict. Additionally, B&CUB argue that

> "... there is a high degree of correspondence between the pattern of responses of the two groups for our dependent variables." (p.23)

This is clearly at variance with the analysis reported in sub-section 4.3.2 above which shows that there are large discrepancies between the responses of the two sets of representatives. In particular, the mean of B&CUB's dependent variable for any industrial action among manual workers in 1980 (STK1) is 12.1% using management respondents only. This should be contrasted with the figure in table 4.2 of 16.0% using the combined reports of management and worker representatives and thus they understate the extent of industrial action by around one quarter[15]. Similarly, B&CRE calculate the mean for manual strike incidence in 1984 as 6.4% compared with the figure in table 4.2 of 8.4% utilising both sets of respondents and consequently their results may also be biased.

Although there is no formal theoretical structure, B&CUB base the selection of their

[15]. A similar underestimation of the mean is apparent for both 'short strikes' (STK2) and 'long strikes' (STK3) which B&CUB give as 3.3% and 5.6% respectively, in comparison with 4.4% and 7.5% from table 4.2.

large number of 'relevant' variables in their model on ideas borrowed from game-theory. Some apparently spurious results are obtained; the coefficient on the union recognition dummy is significantly negative in the short strike equation, and significantly positive in the long strike equation. Additionally, predicted strike probabilities for unionised and non-unionised establishments are presented, yet as seen in table 4.4, industrial action is very seldom defined for those establishments which do not recognise unions and hence there are too few observations on which to base predictions of this kind.

In contrast, B&CRE test a formal game-theoretic bargaining model which presupposes that the union has an informational disadvantage relative to the firm, and is similar in many respects to the work of Tracy (1984, 1987). However, their empirical estimation is rather poor. It is not clear what criteria have been used to eliminate 'irrelevant' variables since many of those retained are still insignificant at conventional levels; improvements are claimed to be made in terms of "goodness of fit" (B&CRE, p.284) although the only diagnostic reported is McFadden's (1974) pseudo-R^2 which has dubious merit (Maddala (1988)). Furthermore, their inclusion of union density separately for where unions are recognised (together with its squared value) and where they are not recognised presents problems in interpreting their results. In particular, and as clearly illustrated in table 4.4, very few establishments in which unions are not recognised experience any form of industrial action. Finally, their conclusions show only rather weak support for the theoretical specification outlined. This may be a consequence of the unsuitability of the Workplace Industrial Relations Survey data for several of the more important parameters of their game-theoretic, asymmetric information model.

The empirical specifications chosen by B&CUB and B&CRE are very different with respect to both dependent and explanatory variables, and thus no comparisons are possible between the two years. Moreover, neither consider non-strike action and no attempts are made to assess in an appropriate manner the validity of the specifications chosen. The investigation presented below circumvents these shortcomings in the previous studies and also examines a wider set of issues. Firstly, a more consistent measure of conflict incidence is utilised, based on both management and worker representative responses. Secondly, the incidence of any industrial action and of strike and non-strike activity are estimated separately within a uniform framework and this allows the similarities and differences in the determinants of conflict incidence between the two years to be revealed. Thirdly, the dichotomy that exists between strike and non-strike activity is explicitly incorporated in an appropriate empirical model. Finally, suitable diagnostic tests for misspecification are employed in order to assess the statistical validity of the model.

The subsample of the Workplace Industrial Relations Surveys chosen for the current study is restricted to private sector manual workers. There are several reasons for this limitation on the scope of the chapter. Firstly, it is apparent that most of the theoretical models of conflict activity refer to production technologies and workplace organisations that are most characteristic of this sector. Additionally, the intention here is to describe and explain not only the incidence of conflict, but also of the inter-relationship between strike and non-strike activity and this task is facilitated by concentrating on a single, relatively homogeneous sector of the economy[16]. Finally, chapter 5 compares and contrasts strike activity in the public and private sectors for both manual and non-manual workers and thus provides a complementary study of wider sectoral coverage, even if more restricted in terms of the nature of conflict considered.

An empirical model for the incidence of conflict behaviour at the establishment level is formulated in sub-section 4.4.1 and estimates are presented for the occurrence of

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16. Additionally, given that both of the previous studies are limited to a study of manual workers, then an attempt can be made to partially encompass their findings if a similarly restricted sample is chosen.

any form of industrial action in sub-section 4.4.2. This measure perhaps represents a more general indicator of employees' grievances than captured simply by the number of strikes as used in other studies, especially given the more recent legislative restrictions placed on overt strike activity[17]. In addition, it may be expected that the incidence of strike and non-strike activity will be correlated, either positively or negatively according to whether they are complements or substitutes. However, there have been no previous studies measuring the strength of this inter-relationship and thus a model for their joint determination is estimated and the results presented in sub-section 4.4.3. This serves to reveal their similarities and differences and enables the postulated hypothesis of switching between strike and non-strike activity to be examined in detail[18].

4.4.1. The Determinants of Industrial Conflict

Numerous theories have been proposed in the literature to explain the incidence and/or frequency of strike action and these have been reviewed in chapter 2 of this thesis. Here, however, an empirical model is specified which is only loosely based on extant theory since there is little consensus as to what comprises an adequate model. In this way, it is hoped to be able to identify those factors that are pertinent to the determination of *conflict* activity more generally since all the existing theories concentrate solely on *strike* action. This should also allow the data to reveal the relative support for competing theories of conflict activity, rather than imposing a particular model and associated specification on the data. In this manner, the models can perhaps be empirically discriminated in an informal fashion.

17. Admittedly, only some of these had been implemented by the date of the second survey.

18. Note that any correlation between strike and non-strike activity at the establishment level is not an indicator of switching from one to the other over time. For example, establishments which are conflict prone are likely to have incidents of both strike and non-strike action and thus these should be positively related in both 1980 and 1984. Switching is revealed by the *relative* use of strike and non-strike sanctions and, as shown in table 4.3, this would appear to have changed somewhat between 1980 and 1984.

The first control included in the equations is a dummy variable indicating whether a manual worker representative was interviewed at the establishment (MWREP). As described in sub-section 4.3.2, the construction of the measures of conflict activity imply that in establishments where only management representatives were interviewed, the incidence of conflict is likely to be under-stated due to incomplete recall. The MWREP dummy is included to capture this effect and would therefore be expected to have a positive coefficient.

The explanatory variables chosen for the basic specification can be subdivided into four main groups, with the selection of variables being conditioned by their availablility in both WIRS80 and WIRS84 in order that comparisons can be made[19]. The first set incorporates measures of establishment size (ESIZE) since there is a large literature that indicates that there is a relationship between plant size and workplace industrial action. The explanations proffered for this empirical observation differ widely; while George *et al* (1977) summarise many of the arguments, it is clear that there is no commonly accepted causal link and plant size may be acting as a proxy for some other phenomenon, such as bureaucratisation or technology. Prais (1978), Edwards (1980) and Marginson (1984) concentrate solely on the intrinsic effect of size in the determination of strike activity. Prais predicts that the number of strikes should increase proportionately with employment and finds some support for his hierarchical model using data from the Department of Employment. In contrast, Edwards observes a less than proportionate relationship using survey data and suggests that this may be the result of the number of bargaining groups increasing less than proportionately with plant size. Finally, Marginson extends the analysis to include the distinct effects of company size, which he argues may have an additional effect on strike activity due to management specialisation, standardisation and formalisation and through these, on unionisation. Both plant and company size are

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19. A full description of the variables together with their weighted means is presented in the appendix to this chapter, table A4.2.

found to be important in his study utilising the Warwick Workplace Survey of Industrial Relations (see Brown (1981)).

An obvious problem with these three studies is that they are not *ceteris paribus* comparisons, and thus size could be acting as a proxy for any number of other excluded differences between plants. However, existing multiple regression models controlling for these additional distinctions between establishments have typically had too few observations on which to base any robust conclusions. In the current study however, the effects of establishment and company size can both be identified more precisely. Banded measures of establishment size are used since these can accommodate any non-linearities in the probability of conflict across establishment size. Any additional effects of company size on conflict incidence are tested for explicitly in a similar fashion.

The second set of variables relate to differences in union presence and organisation at the establishment level. The first variable records whether the establishment is unionised in that it has recognised manual union members (UNION). As seen in table 4.4, few non-unionised establishments report industrial action of any kind. Indeed there is a strong case for estimating the equations conditional on the unionisation status of the establishment given the paucity of conflict activity in establishments that are not unionised[20]. However the results would not then be comparable to those obtained by B&CUB and B&CRE and thus separate estimates for all establishments and for unionised establishments only (i.e. UNION=1) are *both* presented below. Next, banded measures of union density among manual workers are included to capture the impact of the extent of union organisation at the establishment (DENSM). At low levels of coverage, the union may simply be unable to organise,

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20. Additionally, Naylor & Gregg (1989) demonstrate that the determinants of union membership differ according to the unionisation status of the establishment. By estimating separate equations therefore, the differential impact on industrial conflict can also be identified.

and indeed may not even be recognised by the management. In contrast, at very high levels the union's threat effect may be more effective and thus overt action may be unnecessary. At intermediate levels of union density, the balance between (positive) coverage and (negative) threat effects is unclear and hence the profile of conflict activity across the full range of union density is uncertain. However, it does seem likely that level of membership is likely to be important in determining the probability of success in any action taken.

The next variable, CSHOP, indicates whether there is either a pre-entry or post-entry closed shop for any *group* of manual workers at the establishment. This contrasts with DENSM5 which takes a value of unity if *all* manual workers at the establishment are union members. Clearly unions have potentially more power where they are able to organise all the workers within a particular group, and more so if this covers all workers at the establishment. Of course, this does not imply that they necessarily need to exercise this power. Finally in this group, MUNION records whether there are multiple manual trades unions at the establishment. This is included to act both as a proxy for the number of negotiating groups (which, although recorded in 1984, was not reported in the 1980 survey) and any inefficiencies arising from inter-union disagreements (demarcation) which may lead to delays or breakdowns in bargaining and subsequent conflict activity. Thus its expected effect on the level of conflict activity is positive.

The third set of explanatory variables control for characteristics of the firm and its organisation which may contribute to the likelihood of conflict activity. That the British manufacturing sector is particularly strike prone is well established in the literature, and some studies have concentrated exclusively on this sector (e.g. Edwards (1981)). Certainly table 4.3 reveals that manufacturing establishments were between three and five times more likely to incur industrial action than those in the service sector. Of course, this is not a *ceteris paribus* comparison, and may be related to differences in establishment size, the level of unionisation and so on. The inclusion of a dummy can perhaps be justified on the grounds that there are remaining workplace characteristics in manufacturing establishments that are not accounted for by the other controls included in the equations, such as the 'nature' of the work. However, it is possible that conflict activity is more heterogeneous by industry than can be adequately captured by this simple dummy, and thus it may be necessary to include a more extensive range of controls to compensate for the differing propensities to incur industrial action across industrial sectors. This possibility is also investigated.

Secondly in this group, SINGLE takes a value of one if the firm comprises just a single establishment. This is likely to be an important determinant of the propensity to incur industrial action for a number of reasons. Employees may have greater loyalty to a single establishment enterprise than to a multi-establishment organisation with distant parent company. Additionally, the workforce may be more aware of the financial position of the firm where it exists as a single establishment, thus leading to fewer incorrect assessments of its ability to meet any wage claims, and hence a lower probability of conflict. Next, a variable indicating whether the establishment is a member of an employers' association (EMPASS) is included to capture the effect of solidarity and benefits that the management may derive from its cooperation with other employers. Fourthly, LEVEL takes a value of unity when the most important level of negotiation is at the plant level. This is intended to capture any effects deriving from profit-centre accountability and the closeness of any management-employee relationship at the local level. Finally, a variable which records whether the management's own assessment of the relative financial performance of the establishment is above average (FINPERF) is included as a (rather imperfect) proxy for the ability to pay any wage claim. If this information is widely available, it may affect the workers' assessment of the firm too and thus may be an important consideration in determining their bargaining behaviour.

It is clearly important to consider the nature of the product market that the firm is facing. In firms faced with a high degree of competition, there are stronger incentives for both management and unions to avoid strike activity. For the firm, any disruption to production is liable to lead to a rapid erosion of market share since other firms are easily able to provide similar products. For the union, potential closure presents a greater threat to their job security than in firms facing less competitive conditions, and thus they too should be more willing to concede demands and reach a settlement[21]. The 1984 Workplace Industrial Relations Survey contains some limited information on the degree of competition in the product market, but the corresponding questions were not asked in 1980. In order to be able to make direct comparisons between the 1980 and 1984 surveys, it is important that the same controls are included for each year. Thus a full analysis of this particular issue for the 1984 survey is relegated to the appendix to this chapter.

The fourth and final set of controls encompass various descriptive measures of the workforce and (implicitly) the workplace and the nature of employment. Women have historically been less well organised and less militant than men and the proportion of the manual workforce who are female (P_FE) is therefore included to capture this effect. On similar grounds, the proportion of the workforce who are part-time (P_PT) is included to control for the more general difficulties in organising among flexibly employed persons and subsequently mobilising them into taking industrial action in support of any claim. Inclusion of the proportion of the workforce who are skilled (P_SK) is an attempt to capture the notion that certain groups of workers are essential to the running of the enterprise, and thus these employees may be prepared to take strike action more readily in support of any claim or grievance since there is little

21. However note that private information theories of strike behaviour predict a negative correlation between the size of the rents for division and the probability of a strike. Given that the rents are greater in firms facing less competitive conditions, this suggests the opposite.

chance of them being dismissed. However, skilled workers with high human capital may be able to command a premium in any negotiations and hence need to strike less in support of any claim.. Thus the sign on this variable is uncertain. Finally, whether there are payment by results schemes (PBR) or shift work at the establishment (SHIFT) are included in an attempt to capture additional workplace environment considerations; the presence of PBR schemes can lead to a higher frequency of negotiations (because of changes in products and production processes) and thus to a greater chance of any conflict at the establishment while shift working can lead to difficulties in communication and/or organisation, although may also be associated with lower worker satisfaction[22].

As can be seen from the sectoral disaggregation of conflict activity in table 4.3, the incidence of industrial action among private sector manual workers decreased quite substantially between 1980 and 1984[23]. The decline was most marked for manufacturing establishments in which the incidence of strike action fell by more than 50%. However, whether this represents any change in collective bargaining behaviour, or is simply due to the changing composition of the private sector between 1980 and 1984 can only be resolved empirically. The analysis in section 4.2 suggests that both of these factors could have served to reduce conflict activity between 1980 and 1984, and may also have resulted in 'switching' from strike to non-strike activity. The changing composition of the private sector is revealed by the weighted means presented in the appendix to this chapter, table A4.2. There has been a noticeable downward shift in the size distribution of establishments together with a decline in the

22. It may be considered that some measure of the level of remuneration should be included in the specification. However, there are two problems with such a strategy. Firstly, it is not possible to assess whether wage rates are at pre- or post-strike levels, and this is clearly essential in order to gauge the responsiveness of conflict to pay. A second, and closely related, problem is that it is not apparent that wages are exogenous in a model of conflict activity.

23. Figures in the appendix, table A4.2, show that the incidence of any form of action by manual workers in all private sector establishments decreased from 15.1% to 10.7%. This was composed of a fall from 10.6% to 6.0% in strike action and from 9.8% to 8.0% in non-strike action.

representation and coverage of unions, especially the closed shop. Given the clear evidence of a positive correlation between the level of industrial conflict and both size and unionism, this would imply a decrease in conflict, *ceteris paribus*. The relative importance of this composition effect *vis a vis* any actual change in behaviour in response to the changed economic and legislative environment (as detailed in section 4.2) is considered in detail in sub-sections 4.4.2 and 4.4.3 below.

In any empirically based model such as this, it is essential to rigourously test the adequacy of the selected specification. Yet as noted above, neither of B&CUB or B&CRE make any attempt to assess the econometric validity of the model on which their conclusions are based[24]. However, Chesher & Irish (1987) develop score tests for limited dependent variable models using the generalised residuals devised by Gourieroux *et al* (1987) and these are used here to test the estimated Probit equations for various forms of misspecification. As reported at the bottom of each table along with their critical values (at 5%), they thereby provide an assessment of the models' empirical adequacy.

Firstly, any remaining heterogeneity by establishment size (HETSIZE) is tested for explicitly to ensure that all size effects have been correctly incorporated[25]. Secondly, a RESET type test (Ramsey (1969), Ramsey & Schmidt (1976)) based on the square, cube and fourth power of the fitted values is constructed for general functional form misspecification (FF). Thirdly, NN is a test for non-normality constructed from the third and fourth sample moments of the residuals. The final two tests are tests for

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24. In that B&CRE claim that their specification is empirically determined, this is a cause of particular concern. In contrast, B&CUB adopt the 'kitchen sink' approach and probably err on the side of over-inclusion of variables thus obtaining inefficient estimates.

25. Given the stratified sampling used in the compilation of the WIRS, some researchers have estimated under a weighting scheme to correct for the over-sampling of large establishments. However, as noted by DuMouchel & Duncan (1983) for the linear case, as long as the model is correctly specified, the unweighted estimator is MVUE and thus would be preferred given that the weighted estimator is only consistent. Since suitable diagnostic checks are being used here to ensure that the specification is acceptable, then unweighted estimation is appropriate.

omitted variables; OSIZE tests for omitted organisation (or company) size effects since these were found to be important by Marginson (1984) in addition to the impact of increasing establishment size, while INDY tests for omitted one-digit industry dummies in comparison to simply dichotomising the sample by manufacturing status[26]. Clearly for inference to be valid, the estimated equations should simultaneously satisfy all of these various misspecification tests.

4.4.2. The Incidence of Industrial Conflict[27]

Given that the incidence of conflict is recorded as a 1-0 dummy, appropriate limited dependent variable estimation techniques must be applied (see, for example, Maddala (1983)). Accordingly, Probit estimates of the empirical model described in sub-section 4.4.1 are presented in table 4.5A, columns 1 and 2 for the incidence of any industrial action in private sector manual establishments. There would appear to be at least partial support for the proposed determinants of conflict activity. The coefficient on MWREP is positive as expected, although only significant in 1984. This accords with the suspicion that incomplete recall leads to the incidence of conflict being underestimated in establishments in which workers representatives were not interviewed. More importantly, as found in many previous studies, conflict incidence increases with establishment size although not linearly. An alternative specification of the size effects is to include a polynomial in establishment size such as the quadratic utilised by B&CUB. However, the set of banded dummies utilised here dominate (in likelihood) all such continuous functions and thus are to be preferred. For 1984 this comes as no surprise since the estimated coefficients reveal a sharp increase in the propensity to incur industrial action for establishments with more than 500 workers. In contrast B&CRE retain establishment size linearly in their final specification,

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[26]. At higher levels of disaggregation than 1-digit industries, several of the cell sizes become too small for meaningful estimates to be made and the results are liable to be sensitive to any arbitrary grouping of the observations.

[27]. All estimation in this section of the chapter is conducted using Limdep (Greene (1990, 1991)).

together with two banded dummies of organisation size, neither of which is significant (although theirs is purely a strike activity equation rather than the more general measure of industrial conflict being used here). The test here for omitted organisation size effects (OSIZE) yields a similar finding to that of B&CRE; for the specification selected, there would appear to be no grounds for including measures of firm/organisation size[28]. This contrasts with the results of Marginson (1984) who found such effects to be significant determinant of industrial conflict although his model was one in which *only* size matters. Naylor & Gregg (1989) find that organisation size is not a significant determinant of union density and argue instead that the company size effect is simply a proxy for union recognition which is typically determined at the company level. Since recognition is controlled for explicitly in their equations, there is no separate company size effect. Its insignificance in the equations for conflict incidence presented here can be explained in a similar fashion; the unionisation status of the establishment is certainly important as table 4.4 clearly demonstrates, but since this is included as a control (or estimates are made conditional upon the union status of the establishment as in columns 3 and 4 of table 4.5A), then there is no additional identifiable organisational size effect.

In an important sense however, controlling for size is only a scale effect (whether linear or otherwise) and the main interest is in the other parameters of the model. Somewhat surprisingly, the unionisation variable UNION, though positive, is insignificant for both 1980 and 1984 despite the evident dichotomy illustrated in table 4.4. The reason for this would appear to lie in the inclusion of measures of union density at the establishment. Having any recognised union members is not important *per se*; rather it is the coverage that the union has at the establishment which is significant in the determination of the likelihood of conflict. For 1984, the profile of conflict across union density is an inverted U-shape which accords well with the

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28. In contrast, B&CUB obtain a negative coefficient on organisation size in their 'any action' equation.

hypotheses outlined above; at low levels of coverage, it is difficult to organise collective industrial action, while at very high levels, such action is less necessary since the threat of any action is much more effective. Unfortunately, for 1980 the pattern is less clear and no regularities can be discerned from the estimated coefficients; in particular, there is a sharp fall in the propensity to take industrial action in establishments which are between 50% and 75% unionised (DENSM3), although this coefficient is not significant at conventional levels. B&CUB include the number of union members at the establishment linearly in their equation together with a union recognition variable (which is highly correlated with the UNION variable being used here) and report positive and significant coefficients on both variables. B&CRE include union membership separately for recognised and unrecognised unions which means that it is not possible to distinguish a union recognition effect from a union density effect. The coefficients they report for recognised union membership imply a U-shaped quadratic with a minimum well above the variable's mean. Hence, they find that strike probability *falls* with increasing union density for the vast majority of establishments and this contrasts strongly with the results reported here.

The closed shop variable is insignificant in both years, although the presence of multiple unions at the establishment led to a significantly higher probability of conflict in 1980. A similar finding was obtained by B&CUB. The coefficient on MANUF is positive for 1980 indicating the heterogeneity in conflict that exists across broad sectors as found by Smith *et al* (1978) and others, and the apparent greater propensity for workers in this sector to take industrial action. Manufacturing status was found to be similarly important by B&CUB for 1980, but B&CRE only include two 1-digit industry dummies and hence no comparisons are possible for 1984 when this result would not appear to hold. That the impact of the manufacturing status of the establishment should be smaller in 1984 than in 1980 accords well with the ideas expressed in section 4.2 above. It was the manufacturing sector that was particularly hard hit by the recession in the early 1980s and the generally poor state of the much-reduced manufacturing base by 1984 would have had implications for its workers in terms of the probability of their taking industrial action.

As hypothesised above, single establishment firms do indeed incur less industrial action than multi-establishment enterprises, although this effect is not significant for either year. Both EMPASS and LEVEL have positive coefficients, although they are only significantly different from zero for 1980. This may reflect the underlying bargaining structure for the particular disputes that were prevalent at the time that the surveys were compiled, especially the engineering workers' stoppages for the 1980 survey. These findings are analogous to those of B&CUB and B&CRE where comparisons are possible. B&CRE hypothesise a positive sign on LEVEL since if bargaining occurs at plant level in a multi-establishment enterprise, the union may have poor information of the firm's true profitability. Firms who have above average financial performance have a lower probability of conflict at the establishment level as predicted above although there is some doubt over the direction of causation; whether this is because they can more easily meet wage claims without conflict, or because they have not incurred industrial action that they are relatively more profitable cannot be ascertained from survey data of this kind.

Finally, it can be seen that workforce and workplace characteristics are also relevant. The proportion female (1980) and the proportion part-time (1984) are particularly important; the higher are these proportions, the lower the probability of conflict at the establishment. Skilled workers are more likely to take industrial action and a similarly positive coefficient was obtained by both B&CUB and B&CRE. Both PBR and SHIFT take their expected signs, although the only significant coefficient is for PBR in 1980.

In general, the results would appear to indicate that the determination of conflict activity among private sector manual workers differed somewhat between 1980 and 1984; while the signs on the coefficients are much the same in both years, their significance is much reduced in 1984 as compared with 1980[29]. In particular, only one of the coefficients on the 'firm' and 'worker' control variables is significant at conventional levels for the 1984 survey. One possible explanation lies in the changing pattern of unionisation between the two years since, as revealed in table 4.4, it is almost exclusively unionised establishments that incur strike action. Columns 3 and 4 of table 4.5A therefore present the results of estimating the same empirical model over the unionised establishments only. For this subsample of establishments the results are qualitatively very similar to those obtained for all private sector manual workers and the same variables are significant as before when estimating over both unionised and non-unionised establishments. Hence some other explanation is required to account for the difference between the two years and the rather poor performance of the equation in 1984.

While the tests for misspecification indicate that the empirical model presented in table 4.5A is generally acceptable (at a significance level of 5%), it is apparent that there are omitted industry effects in both the estimated equations for 1980. The results detailed above may therefore be a reflection of uncorrected differences between industrial sectors in their propensity to strike rather than in any dissimilarities in the determinants of conflict incidence *per se*; the union, firm and worker variables are then simply acting as proxies for these differences. Table 4.5B investigates this hypothesis further by including eight 1-digit industry dummies in the equations rather than differentiating only by manufacturing status. As noted by Stewart (1991) in the context of union wage differentials, one weakness of such a strategy is that the inclusion of industry dummies,

"... merely formalize[s] our ignorance" (p.170)

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[29]. While fifteen (nineteen) of the coefficients are significant at 5% (10%) in the 1980 equation, only ten (eleven) are significant in the 1984 equation.

That is, attributing remaining differences in the incidence of conflict to industry dummy variables does not actually contribute to the explanation of *why* certain industries should be more prone to conflict[30].

As would be expected from the INDY test at the bottom of table 4.5A, only for the 1980 equations does the inclusion of these dummies lead to a significant improvement in the log-likelihood. However, the results obtained are qualitatively and quantitatively very similar to those in table 4.5A and hence the comments made above are appropriate to these estimates too. In conclusion therefore, it would appear that the determinants of conflict activity are rather more difficult to identify in 1984 than in 1980. Although the estimates are econometrically satisfactory, the general performance of the equation for 1984 is quite poor[31].

It is of some interest to decompose the net change in the incidence of conflict into its 'compositional' and 'behavioural' components in order that some empirical weight can be apportioned to the various hypotheses outlined in section 4.2. Compositional changes between 1980 and 1984 are reflected in changes in the (means of the) variables, while changes in collective bargaining behaviour are represented by changes in the estimated coefficients. One way in which the relative weight of these two separate effects can be measured is as follows; define:

$$P_{ij} = \Phi(X_i'\beta_j) \quad i,j = 1,2 \tag{4.1}$$

where P is the predicted probability of industrial action at any given establishment given a vector of characteristics X and estimated parameter coefficients β, Φ is the

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30. However, an important defence of this strategy in the context of conflict activity is that of the 'time-specificity' of survey data of this kind. Because of the discontinuous nature of conflict, the compilation date of the survey is critical. One way of minimising the risk of bias arising from the dominance of any particular dispute that occurred in the sample period is to include industry dummies. This may be particularly important for the 1980 Workplace Industrial Relations Survey since it included the 1979 national Engineering workers stoppage. For wages (which are continuous) time-specificity is not really an issue, and thus Stewart's (1991) criticism is valid.

31. However, note that for both years and both specifications, the equations 'predict' over 80% of the observations correctly (using a cutoff of 0.5).

cumulative distribution function of the standard normal distribution, and subscripts i,j = 1,2 refer to 1980 and 1984 respectively. Four expected values can therefore be calculated and compared; P_{00} and P_{11}, which are, respectively, the 1980 and 1984 mean predicted probabilities of industrial action; P_{01}, which is the mean expected probability of conflict in 1984 for a fixed set of (1980) characteristics, and, finally, P_{10} which is the mean expected probability of industrial action in 1984 for given (1980) behavioural coefficients.

Thus the 'composition' effect is measured by comparing P_{00} with P_{10} (for given 1980 coefficients) or P_{01} with P_{11} (for given 1984 coefficients). Similarly, the 'behavioural' effect is calculated by comparing P_{00} with P_{01} (for given 1980 characteristics) or P_{10} with P_{11} (for given 1984 characteristics)[32].

Performing these decompositions on the specifications in table 4.5A indicates that, for all establishments, approximately one-quarter of the decrease in conflict can be attributed to changes in the composition of establishments, while three-quarters is a result of changes in behaviour. When attention is restricted to unionised establishments only (that is, essentially removing the impact of changes in union recognition), the composition effect drops to less than 10%. Thus, while the greater part of the composition effect is due to a pure union effect, most of the overall decrease in conflict is a consequence of changes in collective bargaining behaviour between 1980 and 1984. This finding is in contrast to that found by Stewart (1991) who concludes that the small net change in the mean union wage differential between 1980 and 1984 is entirely the result of compositional changes between 1980 and 1984.

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[32]. Given the nonlinearity of the Probit model, it is important to note that this decomposition is only approximate. However, for the relatively small changes in the means of the dependent variable (for all establishments (unionised establishments), a decrease in the unweighted mean from 0.339 (0.501) in 1980 to 0.289 (0.443) in 1984), these decompositions are linearly additive to three decimal places in this case.

For the specification in table 4.5B in which industry dummies are included in the equation, the role of compositional changes in the decline in the incidence of conflict is slightly higher (one third for all establishments and 15% for unionised establishments). However, the central finding above, that the majority of the decrease in conflict activity is due to changes in the values of the coefficients rather than changes in establishment composition, is confirmed. In an attempt to ascertain whether this behavioural explanation for the decline in conflict activity can be equally applied to the changing relative incidence of strike and non-strike sanctions, the next sub-section examines the joint determination of these forms of industrial action.

4.4.3. The Incidence of Strike and Non-Strike Activity.

Incidents of conflict activity at the establishment can be dichotomised into strike and non-strike actions. Clearly there may exist a correlation between the incidence of the overt and costly sanction of a strike and the incidence of less costly non-strike sanctions, although whether these are alternatives or complementary is unclear. Action short of a strike may be used to exert pressure during bargaining thus providing a threat effect and negating the need for strike action. Alternatively, non-strike action may act as a prelude to strike action if the (expected) negotiation period is exceeded[33]. To assess this co-determination of strike and non-strike sanctions, Bivariate Probit models are estimated as follows; define

$$y_s^* = X'\beta_s + \varepsilon_s \qquad (4.2)$$

$$y_n^* = X'\beta_n + \varepsilon_n \qquad (4.3)$$

where y_s^* and y_n^* are the underlying propensities to incur strike and non-strike action respectively given a vector of establishment characteristics X, and $(\varepsilon_s, \varepsilon_n)$ have a bivariate standard normal distribution with correlation ρ. The sign on the estimated correlation parameter (RHO) in the bivariate error structure can then be interpreted as

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33. Moene (1988) suggests that strike threats and non-strike threats (and thus presumably action) are mutually exclusive regimes whereby the existence of each depends upon the parameters of the bargaining relationship between the negotiating parties. He therefore precludes the 'complementary' hypothesis suggested here.

indicating the direction of the *ceteris paribus* correlation between strike and non-strike sanctions.

It is possible to estimate equations (4.2) and (4.3) (although not ρ) consistently by single equation Probit techniques. This corresponds to LIML and would, however, be inefficient in that it would fail to take into account the correlation between the disturbances. In addition, the correlation is of particular interest in this case; a positive sign on this parameter would indicate that strike and non-strike sanctions are complementary, whereas a negative estimate for the parameter would signify that they are being used as alternative forms of action. Thus FIML estimation which appropriately encapsulates the bivariate error structure is preferred.

In practise, of course, the latent variables y_s^* and y_n^* are not observable and instead dummy variables y_s and y_n are recorded. These are defined by

$$\begin{aligned} y_s &= 1 \quad \text{if } y_s^* > 0 \\ y_s &= 0 \quad \text{otherwise} \end{aligned} \tag{4.4}$$

and similarly

$$\begin{aligned} y_n &= 1 \quad \text{if } y_n^* > 0 \\ y_n &= 0 \quad \text{otherwise} \end{aligned} \tag{4.5}$$

Thus the zero categories in the bivariate model comprise establishments at which there is no incidence of action of the relevant type. Note that this is <u>not</u> a bivariate model with selectivity (i.e. conditional on any action having occurred). The choice is not between strike or non-strike action since it is quite possible for *both* or *neither* forms of action to be undertaken.

The results from estimating the model defined by equations (4.2) and (4.3) for this model are reported in tables 4.6A and 4.6B for all establishments and for unionised establishments only respectively[34]. The coefficient on MWREP is larger for non-strike action than for strike action. This reflects the greater degree of disagreement over the incidence of non-strike sanctions as can be seen from a examination of table 4.1. The implication is that establishments in which a worker representative was not interviewed are relatively less likely to report the occurrence of non-strike action as compared to strike action, and thus the MWREP coefficient is larger for the former than for the latter. Once again, size is seen to be particularly important; conflict incidence increases with establishment size, although at a decreasing rate. A contrast between strike and non-strike activity is also evident, with a higher but flatter profile across establishment size for non-strike sanctions. This implies that smaller workplaces are relatively more likely to incur non-strike action rather than strike action, although the relative incidence rates for both are approximately equal for larger establishments. The size of the organisation to which the plant belongs (OSIZE) appears to have no effect on either strike or non-strike action.

High union density is again a significant determinant of conflict activity, and, moreover, the hypothesised switching from strike to non-strike sanctions is detectable in the results presented. For table 4.6A (all establishments), while plants with high union density in 1980 were more likely to take strike action, by 1984, their relative preference was for non-strike action[35]. The presence of multiple unions at the establishment (MUNION) results in a significantly higher propensity to incur strike action, although it has a smaller and insignificant impact on non-strike activity. For firm and worker/workplace characteristics, the patterns are less clear, particularly for 1984, and this is consistent with the results obtained for the incidence of any industrial action as reported in sub-section 4.4.2 above.

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34. Once again, the test for omitted 1-digit industry effects indicates that these are required by the model and hence they are included in the specification reported in tables 4.6A and 4.6B.

35. The coefficient estimates are only poorly determined however, and this result is not replicated when the sample is restricted to unionised establishments only as in table 4.6B.

Of particular interest is the parameter RHO. For both 1980 and 1984, this is significantly positive, with the implication that strike and non-strike action are used in a complementary manner rather than as alternatives, conditional on the included explanatory variables[36]. This may (at least in part) reflect the typical sequential nature of disputes which have periods of non-strike action *and* of overt strike activity, rather than only having either form of action in isolation. The strength of this relationship indicates just how characteristic this dual use of sanctions is within establishments; that is, certain establishments can be characterised as being *conflict* prone in that they incur both strike and non-strike industrial conflict. Finally, the size of the coefficient suggests that the complementary use of strike and non-strike sanctions was even more common in 1984 than in 1980.

The decomposition of the changes in the incidence of strike and non-strike activities into their compositional and behavioural components is also of interest and reveals some interesting conclusions. For strikes, the large net decrease is principally due to changes in the behavioural actions of agents to the collective bargaining process. Indeed, for unionised establishments, over 90% of the decline is due to such factors. This is in sharp contrast to the rather smaller decrease in the proportion of establishments reporting the incidence of non-strike action. The reduction in this case is primarily the result of compositional changes and around three-quarters of the net change can be attributed to differences between establishments in 1980 and 1984. These findings lend further, though tentative, support to the 'switching' hypothesis from strike to non-strike activity. While changes in behaviour have resulted in a decrease in strike activity, conditional on establishment characteristics, there has been little corresponding change in the incidence of non-strike action; in the latter case, most of the small decrease is simply due to changes in composition. Hence there has

36. The (unweighted) raw correlations between strike and non-strike activity for 1980 and 1984 are 0.502 and 0.471 respectively for all establishments, and 0.416 and 0.398 for unionised establishments.

been a relative swing towards the use of non-strike sanctions.

4.5. Summary and Conclusions.

This chapter has compared and contrasted the frequency and incidence of industrial action in the early 1980s using statistics on the number of strikes compiled by the Department of Employment and published in the Employment Gazette together with data from the two Workplace Industrial Relations Surveys. That strike frequency and incidence are rather different dimensions of strikes and measure different aspects of conflict activity is apparent from the analysis presented in section 4.3. While the number of strikes continued to fall throughout the 1980s following a trend that began in the early 1970s, the incidence of stoppages at the establishment level actually increased between 1980 and 1984. The implication is that strikes, while fewer in number in 1984, were more widespread and thus affected a higher proportion of establishments. However, in relation to the changes in the bargaining environment in the 1980s, and in particular the structural, economic and legislative transitions outlined in section 4.2, the incidence of conflict activity[37] is of greater interest than simply examining the number of strikes that occur. Thus the empirical analysis in section 4.4 is concerned with this more general and alternative measure of industrial conflict together with how the relative use of strike and non-strike sanctions has changed in the 1980s.

Unlike the previous two studies that have utilised the Workplace Industrial Relations Surveys, the responses of *both* management and worker representatives are used in this thesis. This is in an attempt to account for the effects of incomplete recall in the responses to the questions regarding the incidence of various forms of conflict activity. It is apparent that establishments that are not unionised (in the sense that they have no recognised union members) incur very few incidents of industrial action of

[37]. That is, both strike and non-strike activity (and their inter-relationship).

any form and thus it is important to control appropriately for this distinction in the sample.

In order that comparisons can be drawn, an identical specification is estimated for both years, and appropriate tests are carried out to check the econometric adequacy of the empirical model. The results confirm the importance of plant size found by numerous other studies of conflict activity and also reveal that the size of the organisation to which the establishment belongs is not a significant determinant of the likelihood of conflict. Many of the other suggested hypotheses pertaining to conflict incidence in Britain are seen to receive at least partial support from the data and, for 1980 in particular, several important determinants of the incidence of industrial action are identified. Overall, establishment size and union density would appear to dominate however, even when sectoral identifiers have been explicitly included to account for the time specificity of industrial action in cross-section survey data of this kind. Most of the net decrease in the incidence of industrial conflict among private sector manual workers is seen to result from changes in the behaviour of agents, rather than changes in the composition of establishments between 1980 and 1984. This lends support to the hypotheses discussed in section 4.2, especially the view that the net impact of the economic and legislative changes would serve to reduce the level of conflict activity at the establishment, over and above any decrease resulting from the changing structural composition of establishments.

Estimates for the joint determination of strike and non-strike activity reveal similar patterns as for the incidence of any form of conflict activity as would be expected. In addition, these two forms of sanctions tend to be used together at any establishment which incurs industrial action, rather than as substitute forms of grievance activity. This most probably reflects the nature of disputes in the early 1980s. In addition, there is some evidence to support the 'switching' hypothesis whereby, in times of recession, both employers and employees have a relative preference for less costly non-strike sanctions if breakdowns in bargaining cannot be avoided. While the incidence of strikes has fallen due to changes in behaviour for a given set of characteristics, the proportion of establishments incurring non-strike sanctions has only fallen slightly, and, moreover, this decrease is seen to be primarily due to changes in composition.

In general, the estimates for 1984 are less satisfactory than those for 1980 and, with the exception of establishment size and union density, most variables are not significant at conventional levels. The generally poor results for 1984 may indicate a fundamental change in the way in which conflict is organised, or may perhaps be a reflection of the fact that by 1983/84, with high and increasing unemployment, conflict among private sector manual workers was potentially much more costly to both employees and employers than was perceived in 1979/80. In such circumstances, any systematic occurrence of conflict activity would be carefully eliminated where ever possible. An alternative explanation may lie in the uncertainty created by the new industrial relations legislation. This may have led to ambiguity on the part of unions as to the type of action that they could organise effectively without incurring severe penalties, and thereby shifting the 'balance of power' in the employers' favour. Together with the other transitions outlined in section 4.2, this would be likely to destabilise any previously established regularities in union behaviour and consequent conflict activity. It will therefore be of some interest to replicate the current study when the next Workplace Industrial Relations Survey is published since, by 1990, the full impact of much of the early 1980s labour legislation should be apparent.

Finally, it should be recalled that the sample considered in the empirical estimation presented in section 4.4 is confined to private sector manual workers. It was shown in table 4.3 that the incidence of industrial action differed considerably across sector and worker type. The next chapter therefore presents a complementary study, in that it considers, separately for manual and non-manual workers, the relative strike proneness of the public sector.

Table 4.1

The Incidence of Industrial Conflict as Reported by Various Respondents

Manual Workers

1980: *Base (Weighted) 713*

Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	78.1	5.6	83.7
YES	7.2	9.0	16.3
	85.4	14.6	100.0

Non-Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	78.9	7.4	86.3
YES	6.8	6.9	13.7
	85.7	14.3	100.0

Any Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	67.8	8.0	75.8
YES	10.4	13.8	24.2
	78.2	21.8	100.0

1984: *Base (Weighted) 554*

Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	80.1	6.3	86.4
YES	4.6	9.0	13.6
	84.7	15.3	100.0

Non-Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	78.5	9.1	87.7
YES	5.0	7.3	12.3
	83.6	16.4	100.0

Any Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	68.7	10.3	78.9
YES	6.7	14.4	21.1
	75.3	24.7	100.0

Non-Manual Workers

1980: *Base (Weighted) 643*

Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	89.3	2.9	92.2
YES	2.6	5.1	7.8
	91.9	8.1	100.0

Non-Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	79.1	9.3	88.4
YES	4.4	7.2	11.6
	83.5	16.5	100.0

Any Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	73.7	9.8	83.4
YES	5.1	11.5	16.6
	78.8	21.2	100.0

1984: *Base (Weighted) 560*

Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	62.5	9.9	72.3
YES	2.2	25.5	27.7
	64.7	35.3	100.0

Non-Strike Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	69.0	12.3	81.3
YES	4.1	14.6	18.7
	73.1	26.9	100.0

Any Action

Management Rep.	Worker Rep. NO	Worker Rep. YES	
NO	55.7	12.1	67.8
YES	2.9	29.3	32.2
	58.6	41.4	100.0

Notes:
1. All the statistics are weighted to correct for the stratified sampling scheme used in the compilation of the Workplace Industrial Relations Suveys.

Table 4.2

The Incidence of Industrial Conflict as Reported by Either Respondent

	Establishments with Worker types					
	Manual or Non-manual		Manual		Non-Manual	
Type of Action	1980	1984	1980	1984	1980	1984
Any Industrial Action	22.0	25.5	16.0	13.1	10.9	17.8
Strike Action	13.2	19.1	11.0	8.4	4.5	14.4
Non-Strike Action	16.1	18.1	10.0	8.3	8.7	12.1
Strike Action:						
Less than one day	5.9	13.7	4.4	5.2	2.2	10.4
More than one day	9.0	11.7	7.5	5.1	3.1	8.8
One day - one week	-	11.2	-	4.6	-	8.8
More than one week	-	0.9	-	0.9	-	0.1
Non-Strike Action:						
Over-time ban	10.5	11.2	7.0	6.3	5.1	6.3
Work-to-rule	6.8	8.2	4.0	2.3	3.6	6.4
Blacking of work	5.2	3.5	2.4	2.1	3.2	2.0
Other	2.7	2.8	2.1	1.2	0.9	1.9
Base: Weighted	*2000*	*2000*	*1823*	*1749*	*1988*	*1985*
Unweighted	*2040*	*2019*	*1898*	*1853*	*2034*	*2010*

Notes:
1. See table 4.1.

Table 4.3

The Incidence of Industrial Conflict by Industrial Sector

Manual and Non-Manual Workers

	Private Manufacturing		Nationalised Industries		Private Services		Public Services	
Type of Action	1980	1984	1980	1984	1980	1984	1980	1984
Any Industrial Action	29.8	19.9	48.1	44.5	8.0	8.2	33.3	49.2
Strike Action	21.1	10.4	33.7	33.2	4.4	5.4	17.1	40.9
Non-Strike Action	21.0	15.9	31.0	25.5	5.1	4.7	26.9	36.4
Base: Weighted	*498*	*424*	*69*	*106*	*865*	*843*	*568*	*627*
Unweighted	*746*	*592*	*134*	*196*	*584*	*597*	*576*	*634*

Manual Workers

	Private Manufacturing		Nationalised Industries		Private Services		Public Services	
Type of Action	1980	1984	1980	1984	1980	1984	1980	1984
Any Industrial Action	29.3	18.7	41.2	37.6	6.2	5.8	14.5	13.3
Strike Action	20.5	10.1	24.0	30.5	4.1	3.5	10.3	9.1
Non-Strike Action	20.5	14.3	24.7	20.4	3.3	4.1	7.8	6.8
Base: Weighted	*492*	*412*	*66*	*103*	*761*	*689*	*504*	*545*
Unweighted	*736*	*580*	*130*	*191*	*519*	*515*	*513*	*567*

Non-Manual Workers

	Private Manufacturing		Nationalised Industries		Private Services		Public Services	
Type of Action	1980	1984	1980	1984	1980	1984	1980	1984
Any Industrial Action	4.4	4.6	27.0	19.0	3.6	4.8	26.0	44.2
Strike Action	2.7	1.4	20.3	10.1	1.4	3.7	9.0	38.2
Non-Strike Action	2.9	3.9	14.2	10.8	2.5	1.7	22.7	32.0
Base: Weighted	*498*	*424*	*68*	*105*	*860*	*836*	*562*	*621*
Unweighted	*746*	*592*	*133*	*194*	*582*	*593*	*573*	*631*

Notes:
1. See table 4.1.
2. The four-way sectoral decomposition is mutually exclusive and exhaustive.

Table 4.4

The Incidence of Industrial Conflict by Union Status

All Unionised Establishments

	Establishments with Worker types					
	Manual or Non-manual		Manual		Non-Manual	
Type of Action	1980	1984	1980	1984	1980	1984
Any Industrial Action	32.7	38.1	26.7	20.8	21.8	33.1
Strike Action	19.5	28.7	18.1	13.4	8.8	26.7
Non-Strike Action	24.1	27.1	16.8	13.3	17.4	22.5
Base: Weighted	*1277*	*1327*	*1009*	*1077*	*943*	*1069*
Unweighted	*1574*	*1593*	*1363*	*1405*	*1277*	*1397*

All Non-Unionised Establishments

	Establishments with Worker types					
	Manual or Non-manual		Manual		Non-Manual	
Type of Action	1980	1984	1980	1984	1980	1984
Any Industrial Action	3.1	0.5	2.8	0.7	1.2	0.0
Strike Action	2.0	0.2	2.1	0.4	0.6	0.0
Non-Strike Action	2.0	0.4	1.6	0.4	0.9	0.0
Base: Weighted	*723*	*673*	*814*	*672*	*1045*	*917*
Unweighted	*466*	*426*	*535*	*448*	*757*	*613*

Notes:
1. See table 4.1.

Table 4.5A

Probit Estimates: Any Industrial Action

Private Sector Manual Workers

	All Establishments				Unionised Establishments			
	1980		1984		1980		1984	
CNST	-2.729	(0.248)*	-2.852	(0.303)*	-2.081	(0.347)*	-2.233	(0.422)*
MWREP	0.068	(0.117)	0.468	(0.125)*	0.012	(0.126)	0.469	(0.126)*
size:								
ESIZE2	0.382	(0.201)	0.791	(0.247)*	0.276	(0.225)	0.764	(0.261)*
ESIZE3	0.534	(0.206)*	0.884	(0.250)*	0.431	(0.226)	0.763	(0.264)*
ESIZE4	0.854	(0.213)*	0.808	(0.249)*	0.754	(0.233)*	0.761	(0.260)*
ESIZE5	0.937	(0.226)*	1.174	(0.262)*	0.857	(0.244)*	1.114	(0.273)*
ESIZE6	1.227	(0.250)*	1.260	(0.268)*	1.169	(0.272)*	1.207	(0.279)*
union:								
UNION	0.354	(0.208)	0.443	(0.284)	-	-	-	-
DENSM2	0.753	(0.233)*	0.258	(0.319)	0.741	(0.289)*	0.133	(0.386)
DENSM3	0.405	(0.238)	0.435	(0.296)	0.352	(0.279)	0.400	(0.353)
DENSM4	0.769	(0.216)*	0.827	(0.278)*	0.695	(0.261)*	0.759	(0.341)*
DENSM5	0.942	(0.224)*	0.758	(0.287)*	0.833	(0.271)*	0.686	(0.349)*
CSHOP	-0.017	(0.117)	0.185	(0.123)	0.019	(0.119)	0.187	(0.124)
MUNION	0.359	(0.112)*	0.094	(0.125)	0.291	(0.120)*	0.113	(0.129)
firm:								
MANUF	0.272	(0.127)*	0.071	(0.135)	0.321	(0.140)*	-0.008	(0.145)
SINGLE	-0.084	(0.135)	-0.076	(0.167)	-0.052	(0.150)	-0.200	(0.186)
EMPASS	0.273	(0.100)*	0.062	(0.112)	0.217	(0.106)*	-0.001	(0.117)
LEVEL	0.522	(0.111)*	0.134	(0.116)	0.515	(0.112)*	0.153	(0.117)
FINPERF	-0.157	(0.099)	-0.067	(0.105)	-0.127	(0.105)	-0.083	(0.111)
workers:								
P_FE	-0.879	(0.253)*	-0.246	(0.255)	-0.951	(0.268)*	-0.238	(0.269)
P_PT	0.148	(0.467)	-1.396	(0.447)*	-0.262	(0.587)	-1.500	(0.483)*
P_SK	0.624	(0.247)*	0.402	(0.290)	0.540	(0.269)*	0.430	(0.303)
PBR	0.316	(0.099)*	0.094	(0.114)	0.251	(0.106)*	0.116	(0.118)
SHIFT	0.217	(0.116)	0.222	(0.131)	0.193	(0.126)	0.249	(0.139)
Industry Effects:	NO		NO		NO		NO	
	Diagnostics							
n	1197		1048		777		668	
*ln*L	-459.45		-402.72		-409.55		-371.99	
HETSIZE	7.41	(11.07)	0.75	(11.07)	1.15	(11.07)	5.08	(11.07)
FF	1.62	(7.81)	2.12	(7.81)	3.27	(7.81)	7.08	(7.81)
NN	1.28	(5.99)	2.01	(5.99)	1.27	(5.99)	4.12	(5.99)
OSIZE	1.19	(11.07)	7.63	(11.07)	0.40	(11.07)	8.56	(11.07)
INDY	36.06	(14.07)	9.28	(14.07)	32.67	(14.07)	12.12	(14.07)

Notes:
1. Asymptotic standard errors are in parentheses.
2. * denotes significant at 5% or better.
3. The figures in parentheses beside the diagnostic test statistics are the relevant 5% critical values.

Table 4.5B

Probit Estimates: Any Industrial Action

Private Sector Manual Workers

	All Establishments				Unionised Establishments			
	1980		1984		1980		1984	
CNST	-2.588	(0.294)*	-2.633	(0.391)*	-1.815	(0.408)*	-1.926	(0.513)*
MWREP	0.053	(0.120)	0.453	(0.126)*	-0.015	(0.129)	0.452	(0.127)*
size:								
ESIZE2	0.425	(0.205)*	0.836	(0.252)*	0.291	(0.231)	0.823	(0.273)*
ESIZE3	0.498	(0.211)*	0.945	(0.257)*	0.386	(0.233)	0.847	(0.278)*
ESIZE4	0.842	(0.218)*	0.867	(0.257)*	0.722	(0.240)*	0.833	(0.276)*
ESIZE5	0.904	(0.230)*	1.228	(0.272)*	0.810	(0.250)*	1.170	(0.291)*
ESIZE6	1.131	(0.256)*	1.305	(0.281)*	1.050	(0.280)*	1.246	(0.299)*
union:								
UNION	0.406	(0.215)	0.428	(0.290)	-	-	-	-
DENSM2	0.777	(0.241)*	0.236	(0.325)	0.730	(0.298)*	0.157	(0.403)
DENSM3	0.495	(0.245)*	0.426	(0.302)	0.395	(0.287)	0.435	(0.367)
DENSM4	0.855	(0.224)*	0.822	(0.286)*	0.754	(0.269)*	0.811	(0.357)*
DENSM5	1.037	(0.232)*	0.742	(0.298)*	0.891	(0.278)*	0.720	(0.366)*
CSHOP	0.043	(0.122)	0.179	(0.125)	0.085	(0.124)	0.179	(0.126)
MUNION	0.355	(0.116)*	0.078	(0.127)	0.295	(0.124)*	0.085	(0.133)
firm:								
MANUF	-	-	-	-	-	-	-	-
SINGLE	-0.086	(0.137)	-0.128	(0.177)	-0.052	(0.152)	-0.292	(0.202)
EMPASS	0.263	(0.105)*	0.081	(0.117)	0.197	(0.110)	0.007	(0.122)
LEVEL	0.413	(0.116)*	0.099	(0.122)	0.414	(0.118)*	0.108	(0.124)
FINPERF	-0.151	(0.101)	-0.042	(0.106)	-0.132	(0.108)	-0.051	(0.113)
workers:								
P_FE	-0.738	(0.267)*	-0.239	(0.261)	-0.793	(0.285)*	-0.223	(0.277)
P_PT	0.259	(0.486)	-1.427	(0.465)*	-0.078	(0.630)	-1.534	(0.511)*
P_SK	0.619	(0.260)*	0.382	(0.299)	0.564	(0.282)*	0.424	(0.314)
PBR	0.319	(0.103)*	0.093	(0.116)	0.249	(0.110)*	0.111	(0.120)
SHIFT	0.284	(0.123)*	0.224	(0.134)	0.280	(0.134)*	0.274	(0.143)
Industry Effects:	YES		YES		YES		YES	
	Diagnostics							
n	1197		1048		777		668	
*ln*L	-443.07		-398.32		-394.05		-365.49	
HETSIZE	8.49	(11.07)	0.88	(11.07)	6.95	(11.07)	3.15	(11.07)
FF	6.41	(7.81)	3.05	(7.81)	7.66	(7.81)	5.39	(7.81)
NN	5.21	(5.99)	3.04	(5.99)	4.29	(5.99)	2.67	(5.99)
OSIZE	1.52	(11.07)	7.48	(11.07)	0.55	(11.07)	8.32	(11.07)
INDY	N/A		N/A		N/A		N/A	

Notes:
1. See table 4.5A.

Table 4.6A

Bivariate Probit Estimates: Strike and Non-Strike Action

Private Sector Manual Workers

All Establishments

	1980 Strike Action		1980 Non-Strike Action		1984 Strike Action		1984 Non-Strike Action	
CNST	-2.645	(0.402)*	-3.130	(0.423)*	-2.625	(0.527)*	-3.106	(0.582)*
MWREP	0.078	(0.139)	0.167	(0.131)	0.312	(0.152)*	0.499	(0.140)*
size:								
ESIZE2	0.361	(0.258)	0.541	(0.268)*	0.483	(0.344)	0.845	(0.320)*
ESIZE3	0.547	(0.269)*	0.577	(0.285)*	0.571	(0.329)	1.045	(0.323)*
ESIZE4	0.897	(0.268)*	0.751	(0.284)*	0.585	(0.347)	0.905	(0.326)*
ESIZE5	0.849	(0.274)*	0.936	(0.292)*	1.028	(0.354)*	1.203	(0.340)*
ESIZE6	1.172	(0.302)*	1.135	(0.325)*	1.093	(0.363)*	1.325	(0.352)*
union:								
UNION	0.240	(0.277)	0.240	(0.268)	0.640	(0.702)	0.359	(0.366)
DENSM2	0.785	(0.331)*	0.675	(0.306)*	-0.086	(0.713)	0.266	(0.381)
DENSM3	0.460	(0.335)	0.696	(0.294)*	0.200	(0.684)	0.464	(0.369)
DENSM4	0.933	(0.307)*	0.789	(0.272)*	0.611	(0.686)	0.688	(0.335)*
DENSM5	1.136	(0.308)*	0.930	(0.270)*	0.559	(0.687)	0.702	(0.355)*
CSHOP	-0.120	(0.128)	0.192	(0.128)	0.003	(0.133)	0.095	(0.137)
MUNION	0.358	(0.133)*	0.201	(0.125)	0.321	(0.154)*	-0.073	(0.144)
firm:								
MANUF	-	-	-	-	-	-	-	-
SINGLE	-0.061	(0.157)	0.024	(0.154)	0.085	(0.222)	-0.282	(0.226)
EMPASS	0.321	(0.115)*	0.165	(0.115)	0.032	(0.134)	0.019	(0.128)
LEVEL	0.457	(0.121)*	0.198	(0.116)	0.001	(0.133)	0.161	(0.128)
FINPERF	-0.181	(0.112)	-0.165	(0.111)	-0.033	(0.123)	-0.029	(0.117)
workers:								
P_FE	-0.147	(0.328)	-1.210	(0.338)*	0.082	(0.298)	-0.250	(0.302)
P_PT	-0.200	(0.682)	1.026	(0.630)	-0.994	(0.619)	-1.147	(0.597)
P_SK	0.300	(0.308)	0.668	(0.319)*	0.345	(0.379)	0.491	(0.347)
PBR	0.322	(0.111)*	0.315	(0.111)*	0.110	(0.134)	0.014	(0.124)
SHIFT	0.082	(0.140)	0.319	(0.141)*	0.020	(0.176)	0.282	(0.161)
Industry Effects:	YES		YES		YES		YES	
RHO	0.462 (0.059)*				0.512 (0.059)*			
Diagnostics								
n	1197				1048			
*ln*L	-828.33				-704.31			
HETSIZE	5.71	(11.07)	3.22	(11.07)	7.24	(11.07)	3.47	(11.07)
FF	5.51	(7.81)	4.93	(7.81)	5.06	(7.81)	3.63	(7.81)
NN	2.89	(5.99)	2.99	(5.99)	4.17	(5.99)	2.73	(5.99)
OSIZE	1.45	(11.07)	3.95	(11.07)	8.65	(11.07)	8.30	(11.07)
INDY	N/A		N/A		N/A		N/A	

Notes:
1. See table 4.5A.

Table 4.6B

Bivariate Probit Estimates: Strike and Non-Strike Action

Private Sector Manual Workers

	Unionised Establishments							
	1980				1984			
	Strike Action		Non-Strike Action		Strike Action		Non-Strike Action	
CNST	-2.153	(0.535)*	-2.629	(0.557)*	-1.989	(0.789)*	-2.284	(0.680)*
MWREP	0.034	(0.148)	0.146	(0.139)	0.291	(0.154)	0.509	(0.141)*
size:								
ESIZE2	0.245	(0.276)	0.468	(0.291)	0.424	(0.348)	0.807	(0.325)*
ESIZE3	0.435	(0.295)	0.554	(0.303)	0.480	(0.336)	0.893	(0.330)*
ESIZE4	0.814	(0.287)*	0.684	(0.301)*	0.495	(0.350)	0.858	(0.326)*
ESIZE5	0.805	(0.293)*	0.878	(0.309)*	0.941	(0.357)*	1.114	(0.340)*
ESIZE6	1.099	(0.324)*	1.122	(0.344)*	0.991	(0.368)*	1.243	(0.353)*
union:								
UNION	-	-	-	-	-	-	-	-
DENSM2	0.840	(0.433)	0.555	(0.378)	0.120	(0.789)	-0.019	(0.572)
DENSM3	0.443	(0.430)	0.549	(0.358)	0.431	(0.766)	0.283	(0.531)
DENSM4	0.938	(0.399)*	0.578	(0.341)	0.848	(0.770)	0.475	(0.515)
DENSM5	1.109	(0.403)*	0.687	(0.344)*	0.797	(0.771)	0.483	(0.523)
CSHOP	-0.089	(0.130)	0.231	(0.129)	-0.007	(0.134)	0.098	(0.138)
MUNION	0.326	(0.138)*	0.153	(0.135)	0.307	(0.158)	-0.048	(0.148)
firm:								
MANUF	-	-	-	-	-	-	-	-
SINGLE	0.022	(0.163)	0.019	(0.167)	0.025	(0.234)	-0.529	(0.250)*
EMPASS	0.259	(0.119)*	0.138	(0.118)	-0.033	(0.137)	-0.042	(0.130)
LEVEL	0.450	(0.125)*	0.196	(0.118)	-0.008	(0.134)	0.177	(0.129)
FINPERF	-0.186	(0.116)	-0.132	(0.115)	-0.013	(0.126)	-0.078	(0.122)
workers:								
P_FE	-0.184	(0.334)	-1.276	(0.349)*	0.139	(0.301)	-0.260	(0.315)
P_PT	-0.460	(0.816)	0.694	(0.920)	-0.925	(0.623)	-1.220	(0.620)*
P_SK	0.162	(0.337)	0.653	(0.343)	0.322	(0.384)	0.546	(0.360)
PBR	0.235	(0.116)*	0.309	(0.115)*	0.132	(0.135)	0.016	(0.128)
SHIFT	0.054	(0.155)	0.308	(0.148)*	0.072	(0.177)	0.305	(0.166)
Industry Effects:	YES		YES		YES		YES	
RHO	0.434 (0.064)*				0.505 (0.061)*			
	Diagnostics							
n	777				668			
*ln*L	-757.57				-664.86			
HETSIZE	7.22	(11.07)	2.07	(11.07)	8.61	(11.07)	3.58	(11.07)
FF	4.43	(7.81)	4.78	(7.81)	5.94	(7.81)	4.97	(7.81)
NN	4.40	(5.99)	3.66	(5.99)	2.93	(5.99)	2.21	(5.99)
OSIZE	2.29	(11.07)	3.17	(11.07)	8.93	(11.07)	8.65	(11.07)
INDY	N/A		N/A		N/A		N/A	

Notes:
1. See table 4.5A.

Appendix to Chapter 4

A4.1 The Impact of Product Market Conditions in 1984.

The 1984 Workplace Industrial Relations Survey contains some limited information on the degree of product market conditions facing the firm. As discussed in sub-section 4.4.1., this is likely to be important for both the firm and the union since each will have incentives to avoid industrial action if the firm faces competitive market conditions. The purpose of this section is to investigate the extent to which such influences are apparent in the 1984 survey responses.

The information provided is in three categories; whether the market is dominated by the organisation, whether there were 'few' competitors or whether there were 'many'. The question is asked with respect to the establishment's sole, main or range of products as determined by other questions to be appropriate. For the purposes of the analysis in this section, the first two categories are amalgamated as in Stewart (1990) and referred to as facing 'non-competitive' (FEW) conditions, while those facing many competitors are labelled 'competitive' (MANY)[1].

This distinction was incorporated into the equations estimated in the body of the chapter by augmenting the specifications with the MANY variable (so that the omitted category is firms facing few competitors or head offices); the expected sign on this variable is therefore negative. However, in every case, the conclusions reached in sub-sections 4.4.2 and 4.4.3 are unchanged by the addition of this variable; it never achieves statistical significance in any equation and the estimated coefficients on the remaining variables are robust to its inclusion.

To illustrate this finding, a subset of the results for unionised establishments only is presented in table A4.1. The first column for the incidence of any industrial action

[1]. The relevant questions were not asked of head offices or other administrative establishments.

corresponds to table 4.5A, column 4, and a comparison reveals that the estimated coefficients are little changed by the inclusion of MANY and that the variable is itself insignificant at conventional levels. Column 2 includes the industry dummies and conforms with table 4.5B, column 4. Once again, MANY is statistically insignificant. Finally, the results in columns 3 and 4 of table A4.1 mirror those in table 4.6B, columns 3 and 4 for the incidence of strike and non-strike activity. Not only are the same variables significant in each equation in the bivariate probit model, but the estimated value for RHO is the same to three decimal places.

This exercise would therefore appear to suggest that the incidence of industrial action is not affected by the product market conditions facing the firm. One interpretation is that such influences have already been accounted for by the other variables present in the model. However, it should be recalled that the equations for 1984 in general do not perform well statistically since many variables are insignificant even though they are econometrically satisfactory. Thus any conclusions about the impact of the degree of competition on the incidence of industrial action among private sector manual workers can only be very tentative.

Table A4.1

The Impact of Competitive Market Conditions: 1984

	Private Sector Manual Workers							
	Unionised Establishments							
	Any Action		Any Action		Strike Action		Non-Strike Action	
CNST	-2.310	(0.431)*	-1.985	(0.518)*	-2.033	(0.805)*	-2.363	(0.685)*
MWREP	0.465	(0.126)*	0.447	(0.128)*	0.286	(0.154)	0.501	(0.141)*
size:								
ESIZE2	0.792	(0.263)*	0.852	(0.275)*	0.452	(0.356)	0.851	(0.333)*
ESIZE3	0.790	(0.266)*	0.874	(0.281)*	0.502	(0.338)	0.932	(0.336)*
ESIZE4	0.795	(0.264)*	0.867	(0.279)*	0.525	(0.354)	0.908	(0.334)*
ESIZE5	1.144	(0.276)*	1.199	(0.294)*	0.967	(0.360)*	1.157	(0.346)*
ESIZE6	1.236	(0.282)*	1.276	(0.302)*	1.019	(0.370)*	1.289	(0.360)*
union:								
UNION	-	-	-	-	-	-	-	-
DENSM2	0.154	(0.390)	0.176	(0.403)	0.132	(0.794)	0.010	(0.570)
DENSM3	0.422	(0.354)	0.457	(0.368)	0.445	(0.776)	0.310	(0.528)
DENSM4	0.784	(0.343)*	0.835	(0.358)*	0.866	(0.783)	0.504	(0.513)
DENSM5	0.708	(0.350)*	0.743	(0.367)*	0.812	(0.784)	0.507	(0.523)
CSHOP	0.185	(0.124)	0.177	(0.126)	-0.007	(0.135)	0.097	(0.140)
MUNION	0.112	(0.130)	0.084	(0.133)	0.304	(0.159)	-0.053	(0.149)
firm:								
MANUF	-0.017	(0.146)	-	-	-	-	-	-
SINGLE	-0.180	(0.188)	-0.277	(0.202)	0.038	(0.236)	-0.510	(0.252)*
EMPASS	-0.001	(0.117)	0.008	(0.122)	-0.032	(0.137)	-0.042	(0.131)
LEVEL	0.147	(0.117)	0.104	(0.124)	-0.011	(0.135)	0.171	(0.129)
FINPERF	-0.079	(0.112)	-0.048	(0.113)	-0.009	(0.126)	-0.073	(0.123)
MANY	0.102	(0.113)	0.100	(0.115)	0.089	(0.132)	0.141	(0.125)
workers:								
P_FE	-0.243	(0.269)	-0.231	(0.277)	0.131	(0.302)	-0.272	(0.314)
P_PT	-1.521	(0.483)*	-1.560	(0.512)*	-0.951	(0.624)	-1.264	(0.630)*
P_SK	0.403	(0.305)	0.399	(0.316)	0.297	(0.389)	0.509	(0.363)
PBR	0.124	(0.119)	0.120	(0.120)	0.141	(0.136)	0.029	(0.129)
SHIFT	0.252	(0.140)	0.275	(0.144)*	0.073	(0.177)	0.308	(0.166)
Industry Effects:	NO		YES		YES		YES	
RHO					0.505 (0.061)*			
	Diagnostics							
n	668		668		668			
*ln*L	-371.58		-365.11		-664.86			
HETSIZE	5.20	(11.07)	2.92	(11.07)	8.08	(11.07)	3.14	(11.07)
FF	6.68	(7.81)	4.73	(7.81)	5.86	(7.81)	2.63	(7.81)
NN	4.90	(5.99)	2.92	(5.99)	2.32	(5.99)	1.89	(5.99)
OSIZE	8.37	(11.07)	8.13	(11.07)	8.83	(11.07)	8.59	(11.07)
INDY	12.04	(14.07)	N/A		N/A		N/A	

Notes:
1. See table 4.5A.

Table A4.2

Description of Variables and Weighted Means

Private Sector Manual Workers

		All Establishments		Unionised Establishments	
Key	Description of Variable	1980	1984	1980	1984
	Any Industrial Action	0.151	0.107	0.311	0.232
	Any Strike Action	0.106	0.060	0.216	0.131
	Any Non-Strike Action	0.098	0.080	0.200	0.176
MWREP	Manual Worker Representative Interviewed	0.377	0.277	0.714	0.630
size:					
ESIZE1	Establishment size 25 - 49	0.520	0.510	0.385	0.402
ESIZE2	Establishment size 50 - 99	0.259	0.270	0.278	0.280
ESIZE3	Establishment size 100 - 199	0.120	0.126	0.162	0.164
ESIZE4	Establishment size 200 - 499	0.068	0.067	0.111	0.102
ESIZE5	Establishment size 500 - 999	0.019	0.017	0.037	0.033
ESIZE6	Establishment size1000 +	0.013	0.009	0.026	0.019
union:					
UNION	Manual Union Members Recognised	0.452	0.441	1.000	1.000
DENSM1	Manual Union Density0% - 24%	0.532	0.561	0.077	0.112
DENSM2	Manual Union Density25% -49%	0.077	0.083	0.117	0.130
DENSM3	Manual Union Density50% -74%	0.071	0.080	0.144	0.160
DENSM4	Manual Union Density75% -99%	0.140	0.159	0.294	0.338
DENSM5	Manual Union Density100%	0.180	0.117	0.368	0.260
CSHOP	Closed Shop for Some Workers	0.183	0.128	0.405	0.291
MUNION	Multiple Unions at Establishment	0.174	0.166	0.319	0.342
firm:					
MANUF	Manufacturing Establishment	0.396	0.377	0.560	0.476
SINGLE	Single Establishment Enterprise	0.315	0.297	0.192	0.207
EMPASS	Member of Employers' Association	0.303	0.244	0.450	0.377
LEVEL	Plant Level Bargaining	0.125	0.105	0.275	0.239
FINPERF	Above Average Financial Performance	0.389	0.405	0.365	0.372
workers:					
P_FE	Proportion Female	0.207	0.145	0.181	0.140
P_PT	Proportion Part Time	0.185	0.166	0.114	0.116
P_SK	Proportion Skilled	0.202	0.197	0.241	0.249
PBR	Payment by Results for Manuals	0.243	0.197	0.317	0.253
SHIFT	Shift Work at Establishment	0.343	0.361	0.405	0.418
n	Number of Observations	1197	1048	777	668

CHAPTER 5

Is the Public Sector Strike Prone?[1]

5.1. Introduction.

During the rail dispute of summer 1989 the Prime Minister of the day suggested in the House of Commons that such things would not happen in the private sector and agreed with a questioner that privatisation would reduce the likelihood of strikes, declaring that,

"... privatised services are less likely to strike than public ones."[2]

While it is not directly stated, the implicit assertion behind such statements is that, *ceteris paribus*, an establishment is less likely to experience a strike if it is in the private sector than if it is in the public sector. There is however little empirical evidence available on which to judge the veracity of this hypothesis. This chapter provides an empirical analysis based on two very different types of data and examines what support there is for the proposition.

The data utilised in the analysis presented below corresponds to that used in the previous two chapters of the thesis; strike frequency and establishment-level strike incidence. The first approach is based on establishment-level data from the 1980 and 1984 Workplace Industrial Relations Surveys as utilised in chapter 4. The raw frequencies given by Millward and Stevens (1986, table 10.2) indicate that the proportion of establishments reporting at least one strike in the twelve months prior to interview was higher in the public sector than in the private sector for both the

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1. The majority of the work in this chapter represents joint research with my thesis supervisor Mark Stewart.

2. 29 June 1989. Hansard, Sixth Series, Volume 155, p.1105.

services and manufacturing sectors in both 1980 and 1984[3]. It is important to recall that the Workplace Industrial Relations Surveys *do not* include establishments in coal-mining, and thus the potential distortion in the statistics arising from the high stoppage activity in this sector, and described in chapter 3 of the thesis, is avoided. However the figures given by Millward and Stevens are proportions of all establishments including non-union ones while strikes are extremely rare where there are no union members as seen in table 4.4. As is well known, union presence is much greater in the public sector, both in terms of membership and in terms of recognition and this potentially distorts the comparison between the sectors. The focus here will be on the probability of a unionised establishment experiencing a strike.

The first panel of table 5.1 presents the raw strike incidence figures from the two surveys for the public and private sectors with attention restricted to those establishments in which there are union members and the union or unions are recognised by management[4]. While the broad picture is the same as reported by Millward and Stevens, the differences between the public and private sector are much reduced, and in 1980 for manual workers, strikes were more likely in private manufacturing than in the nationalised industries.

However this comparison still does not address the *ceteris paribus* question posed above, since public and private sector establishments differ systematically in a number of important ways which might reasonably be expected to be related to the probability of a strike occurring. One covariate serves to clearly illustrate this point. There is considerable evidence that large plants are more likely to experience strikes than small ones and plants are on average larger in the public sector. The remaining

3. Millward & Stevens record a strike having occured if either the management or worker representative reports a strike. This is the same strategy that has been adopted in the previous chapter of the thesis.

4. The responses of both management and worker representatives are combined as advocated by Millward & Stevens (1986, p.263) while the statistics in this and the following tables in this section are weighted to account for the stratified sampling used in the two surveys (see Millward & Stevens (1986, pp.329-332)).

panels of table 5.1 present corresponding figures separately for establishments with less than 100, 100 to 499, and 500 or more employees. Further reductions in the differences between the public and private sectors are evident when attention in restricted to larger establishments. It is clearly important to control for establishment size and other factors when comparing strike probabilities between the public and private sectors.

A second important covariate is also worth mentioning at this stage. The conceptual experiment under consideration in this chapter is the transfer of a public sector establishment into the private sector, *ceteris paribus*. In this hypothetical comparison it is important to control for the product market conditions faced by the establishment. These are of course typically very different for public and private sector establishments. As discussed below, there are likely to be greater incentives for both management and unions to avoid strike activity in firms facing a high degree of competition in the product market. The 1984 Workplace Industrial Relations Survey contains some limited information on this, but the corresponding questions were not asked in 1980. In 1984 there is a marked tendency for private sector unionised establishments which face many competitors to be less prone to strike action. For manual workers 10% of private sector establishments that faced many competitors experienced strike activity compared with 17% of those who were the dominant organisation in the market or faced few competitors. The corresponding figures for non-manual workers were 5% and 15% respectively. The impact of the extent of product market competition clearly also needs to be considered in any comparison.

The first objective of this chapter is to consider the *ceteris paribus* difference in the probability of a strike between otherwise comparable establishments in the public and private sectors using the Workplace Industrial Relations Survey data, controlling for as many relevant and appropriate covariates as possible. There are, however, a number of problems with this establishment-based approach to the question.

Therefore comparisons are also made between the sectors on the basis of strike frequency measures constructed from the official Department of Employment industrial stoppages data as used in chapter 3. This provides the second approach to the question posed in the title to the chapter.

The remainder of the chapter is structured as follows. Section 5.2.1 presents a brief discussion of the previously published evidence pertaining to differences in the strike propensities of the public and private sectors. This leads to the formulation of an empirical model for the determination of the probability of strike action at the establishment level which allows the required *ceteris paribus* comparisons between the private and public sectors to be made. Essentially this is a modified version of the specification considered in chapter 4. The empirical results of this analysis are presented in section 5.2.2. The results of the alternative approach based on adjusted strike frequency measures are reported in section 5.3 and some concluding comments presented in section 5.4.

5.2. Public-Private Strike Differentials: Evidence from the WIRS.

5.2.1. Evidence from Previous Studies of Strike Activity.

The majority of empirical studies for the UK have been time-series econometric analyses of aggregate strike frequency. However, none of them have addressed directly, and few throw any light on, the question of interest here. In particular, no comparisons are made between public and private sector strikes. There have also been a number of studies examining inter-industry differences in strike activity, but again little attention has been paid to differences between the private and public sectors.

The two published studies which utilise the Workplace Industrial Relations Surveys discussed in section 4.4 above (Blanchflower & Cubbin (1986) (hereafter B&CUB)

who utilise WIRS80 and Booth & Cressy (1990) (hereafter B&CRE) who use WIRS84) also fail to provide satisfactory information on which to consider the question being posed in this chapter. Both papers use a measure of strike activity based solely on the management questionnaire, taking no account of the responses from worker representatives and the discrepancies between the two. However, Daniel & Millward (1983) suggest that,

> "... reliance upon a single respondent's account will almost certainly lead to underestimation of the extent of industrial action" (p.216).

They therefore advise integrating the reports of managers and workers wherever possible. The differences are indeed quite large and both B&CUB and B&CRE understate the extent of industrial action by around one quarter.

A public sector dummy variable is included by B&CUB to control for the fact that this sector had

> "... experienced considerable industrial unrest over the relevant period" (p.27).

However, a <u>negative</u> coefficient is then estimated in all three specifications used[5], in contrast to this justification for the variable's inclusion. But since the commentary then reports that

> "... as expected, the manufacturing sector, public sector and engineering industry dummies are also statistically significant with a positive coefficient" (p.33),

the picture is somewhat unclear.

Non-union establishments are included in the B&CUB sample and the coefficient on the union recognition dummy found to be significantly negative in the short strike equation, and significantly positive in the long strike equation. They suggest that this may reflect

> "... the use of short strikes in attempts to gain recognition." (p.35),

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5. It is significant in the 'any action' and 'longer strike' equations. It has a similar numerical value in the equation for shorter strikes, but is insignificantly different from zero.

However, on the basis of the definition of strike action used (management respondents only) the 1980 Workplace Industrial Relations Survey data indicate almost no strikes in establishments without recognised unions[6]. At a conceptual level, the comparison required for the focus of attention in this chapter is between unionised establishments.

B&CRE test a private information model similar in many respects to the work of Tracy (1987). They exclude much of the public sector from their study, arguing that their model in which organisation profitability is a prime concern is only strictly relevant to the private sector. However, they do include bodies such as government-owned limited companies and the nationalised industries along with the private sector establishments, arguing that for these 'quasi-public' bodies the

> "... profitability of the organisation is arguably relevant to the union's wage demands" (p.280).

The coefficient on the dummy variable for these 'quasi-public' bodies is negative, but insignificantly different from zero. However two relevant 1-digit industry dummies are also included. Both have significant positive effects. One is for transport, post and telecommunications and the other is for an amalgam of industries dominated by electricity, gas and water. They too include non-union establishments in their sample, and include measures of union density separately for establishments where unions are recognised and for where they are not. This makes interpretation somewhat difficult. In addition, as noted above, there are very few strikes in establishments which are not unionised.

Neither study seems to provide satisfactory information to form a judgement on the question being posed in this chapter. In addition both papers include a number of variables that would not seem suitable as exogenous controls; for example, an index

6. In establishments without recognised unions, only 2% of management respondents reported strike action (compared to over 14% in unionised establishments) and furthermore, a mere 0.6% reported the incidence of 'long' strikes.

of capital utilisation, changes in the value of sales (included in B&CRE) and the existence of short-time working in the previous year (in B&CUB). In the next sub-section a model is formulated and estimated for the probability of strike activity at an establishment explicitly to estimate the *ceteris paribus* difference in this probability between the public and private sectors.

5.2.2. Establishment-level Analysis of the Probability of a Strike.[7]

In this section Probit models for the probability of a strike at an establishment in the previous twelve months are estimated using data from the 1980 and 1984 Workplace Industrial Relations Surveys. Although very similar in nature, the questionnaires used in the two years were not identical. For example the information on the number of competitors faced referred to in the introduction is only available in 1984. However for purposes of comparison a consistent specification is required. Two alternative strategies are adopted. Initially the control variables selected are restricted to those which appeared in *both* surveys. Subsequently, for 1984 only, we investigate the incorporation of product market competition effects into the model. As in chapter 4 and section 5.1, strike action is deemed to have taken place if *either* the management or the worker representative reported strike action to have occurred in the previous twelve months in order to minimise the effects of incomplete recall by respondents. The robustness of the results to this particular course of action is considered below. Given the differences illustrated in table 5.1, it is evident that separate equations should be estimated for manual and non-manual workers. In addition the samples are restricted to those establishments that recognised unions for the purposes of bargaining.

A description and summary statistics of the variables used is given in the appendix to this chapter, table A5.1. Two specifications of the control vector are used. The first

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7. The estimation in this section of the chapter is conducted using Limdep (Greene (1990, 1991)).

contains only establishment size variables, while the second incorporates the additional variables reflecting other major differences between the establishments in each sector as outlined above[8]. Controls for the size of the organisation as opposed to the establishment are not included. This is because the focus of attention here is on the public-private difference and in this case it is not clear that one would wish to control for organisational size if the conceptual experiment under consideration is the transfer of a public sector plant into the private sector. Related to this, there are worries about how the questions on organisational size are interpreted and answered by establishments in the public sector. This difficulty of interpretation is supported by the fact that in 1984 among non-single establishments, 25.5% of those in local or central government replied 'don't know' when asked roughly how many employees the organisation of which they were a part employed in the UK as compared with 9.2% of private sector limited companies. Therefore, controls for this are not included in our basic model, although diagnostic checks are conducted for the possibility of it being a relevant omitted factor.

To each of the vectors of controls is added a set of dummy variables to capture the sectoral effects of interest. Again two versions are employed. Variant A uses one dummy variable to indicate whether the establishment is in the production sector and another to indicate whether it is in the public sector. The base group is thus private sector service establishments and there is a constraint that the production effect is the same in the public and private sectors, or equivalently that the public sector effect is the same in the production and service sectors. The differential implied by the coefficient on the public sector dummy will be the one of interest here. Variant B lifts this restriction by including dummy variables for establishments in the nationalised industries, those in the services sector and those in the public services sector. In this

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8. In essence, this is because size is a 'scale effect' and thus controlling for size enables the focus to be concentrated on 'scale-free' differences in strike propensities. However, other differences between establishments also warrant consideration since these would presumably also remain essentially unchanged whatever sector the establishment is in. Hence we are also interested in the differences derived from the second vector of control variables.

case the base group consists of private manufacturing establishments and the two public sector differentials implied by the coefficients on the nationalised industries and public services dummies are the focus of attention.

The central focus in this chapter is on the *ceteris paribus* differences in strike activity between comparable public and private sector establishments. The probabilities of strike action in the two sectors for an establishment with a given set of characteristics can be written

$$\Pr(\text{Strike}|D=0,X^*) = \Phi(k) \tag{5.1}$$

$$\Pr(\text{Strike}|D=1,X^*) = \Phi(k+\beta) \tag{5.2}$$

where β is the coefficient on D, the public sector dummy variable of interest, X^* is a particular valuation of the control vector and $k = X^*\gamma$, with γ the vector of coefficients on the Xs. Then the proportionate *ceteris paribus* difference in the probability of a strike is given by

$$\Delta = \frac{\Phi(k+\beta) - \Phi(k)}{\Phi(k)} \tag{5.3}$$

Clearly the differential varies with the value of k. For summary purposes a representative value is chosen. While there are several possibilities, one of the easiest to implement and interpret is that given as the solution to $\Phi(k) = p$, where p is the sample proportion of establishments in the base sector which incur strike action. Then

$$\Delta = \{[\Phi(\Phi^{-1}(p)+\beta)]/p\} - 1 \tag{5.4}$$

Thus Δ is the proportionate increase in the probability of a strike for an establishment with characteristics which give it a strike probability equal to the sample proportion of establishments in the base sector experiencing a strike.

The estimated Probit coefficients and implied differentials for both variants of the model and both specifications of the control vector are given in table 5.2. The table also gives the raw (unweighted) differentials. These indicate that for manual workers in 1980, the public sector was 42% *less* strike prone, while for non-manual workers in

1984, it was 167% *more* strike prone than the private sector. For the other two cases (non-manual workers in 1980 and manual workers in 1984) the raw differentials are small in comparison. A further decomposition into production and service sectors indicates that the 1980 manual workers result is driven by the relatively low level of strike action in the nationalised industries. In contrast, the apparent strike proneness of non-manual workers in the public sector in 1984 results from a very high relative level of strike activity in the public services sector; almost 40% of such establishments had experienced at least one strike in the previous twelve month period, while the comparative figure for private service establishments was only 16%. This difference is likely to be partly due to the teachers' disputes of that year.

For non-manual workers in 1980, the proportionate public-private differences for the production and service sectors are actually quite high at 34% and 45% respectively. However, these statistics are derived from low group averages and, in aggregate, the public and private sector means are quite similar. As a result, the proportionate difference is fairly small at under 12%. For manual workers in 1984, although over 40% of establishments in the nationalised industries experienced strike action in the previous twelve months, this comprises a relatively small proportion of the public sector total, while the 33% of private manufacturing establishments reporting strike action is based on a much larger proportion of the private sector. Thus in aggregate, the public and private sector probabilities are once again similar, with a proportionate difference of only 3%.

The introduction of controls for establishment size (specification 1) reduces the magnitude of the two large raw differentials and increases that of the other two. Manual workers in 1980 have a significant negative differential even after controlling for establishment size, while in the other three cases the differential is significantly positive. The introduction of the further controls in specification 2 renders both differentials for 1980 insignificantly different from zero. Those for 1984 are both significant and positive. That for non-manuals is further reduced, but is still very large. Likelihood-ratio tests indicate that in all cases a single equation with a public sector dummy is acceptable against separate equations for the two sectors.

The results for variant B are also presented in table 5.2. In this case the base group differs for the two public sector differentials. For the nationalised industry effect, the differential is calculated relative to private manufacturing while the public service differential is measured relative to the average private sector service establishment. For the nationalised industries for specification 1, the proportionate probability effects are all significant and greater than their comparable raw differences in the top of the table, and this is a consequence of the rather perverse size distribution in the sample[9]. However, once the additional controls are included as in specification 2, the Δs are all smaller than those derived under specification 1 and there are no significant differences in strike proneness between the nationalised industries and the base of private manufacturing, although for non-manuals in 1980 the effect is quite large.

For service sector employees in 1980, there were no significant differences in strike propensities between public and private service establishments in either specification 1 or specification 2. In 1984, however, public sector service establishments were significantly more strike prone than their private sector counterparts. The effect is very large for non-manual workers, and while this is smaller once all the controls are included in the equation as in specification 2, it is not reduced much below the raw proportionate difference of 149%. Again the strategy of estimating a single equation rather than separate public and private equations is not rejected for either specification.

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9. This rather unexpected result is a consequence of the fact that for the unionised sample under consideration, the private sector establishments are, on average, larger than the public sector establishments. Thus controlling for establishment size increases the public sector relative strike probability and hence the proportionate differences in the probability of a strike are greater than the raw differentials.

The final section of table 5.2 presents likelihood ratio tests of the restrictions implied by variant A against variant B. Somewhat surprisingly perhaps, for most of the groups of workers and for both specifications, distinguishing the production and services effects is not required by the data in general, and thus Variant A with just the production and public sector dummies is data sufficient. The one exception is non-manual workers in 1984, and thus this grouping requires separate production and services dummies as in Variant B.

Diagnostic tests for the models presented in table 5.2 are given in table 5.3. It is important in empirically based models of the kind used here to test for various forms of potential misspecification of the chosen econometric specification. Table 5.3 presents score tests for heteroskedasticity, functional form misspecification, non-normality and omitted variables in the two specifications of the two variants of the model presented in table 5.2[10].

For the models for manual workers in both 1980 and 1984, the diagnostic test statistics are satisfactory, both with and without the additional controls of specification 2. The model for non-manual workers in 1980 has satisfactory diagnostics once the additional explanatory variables are introduced in specification 2. Without them there is considerable evidence of misspecification. The diagnostics for the 1984 non-manual workers models indicate serious misspecification and in this case the problems are not solved by the introduction of the extra controls of specification 2. As seen by the likelihood-ratio test in table 5.2, variant B of the model is required for this group but there is still strong evidence of heteroscedasticity with respect to establishment size and of omitted organisation size effects (although earlier caveats with regard to this variable need to be kept in mind). Thus some caution should be applied to the results for this group. One might suspect that the

10. The tests, based on the concept of generalised residuals (Gourieroux *et al*, (1987)), are described in Chesher and Irish (1987).

wide-spread disputes in education in the particular year covered may dominate the results for this group. This possibility is investigated further below.

First however it seems useful to summarise the results for this basic set of specifications. Once adequate controls have been introduced for the dissimilarities between sectors, most of the raw differences are found not to be significant at conventional levels. This conclusion is applicable to both production and service sectors. Despite this, the differentials in some cases, though statistically insignificant, are quite large. For example for non-manual workers in the nationalised industries in 1980 the probability differential is 51%. Significant differentials are found for the public services sector in 1984. In the case of non-manual workers, such establishments were more than twice as likely to incur strike action as their equivalent private service sector workplaces. There are however doubts about the specification of the model for this group.

The conceptual experiment under consideration in this chapter is the transfer of a public sector establishment into the private sector, *ceteris paribus*. It might reasonably be argued that the estimated models presented so far do not correspond to such an experiment, since they do not control for the product market conditions faced by the establishment, which are typically very different for public and private sector establishments. As indicated in the introduction, the 1984 survey contains some limited information on this for private sector establishments, but the corresponding questions were not asked in 1980. The relevant questions were also not asked of head offices or other administrative establishments. The information provided is in three categories; whether the market is dominated by the organisation, whether there were 'few' competitors or whether there were 'many'. The question is asked with respect to the establishment's sole, main or range of products as determined by other questions to be appropriate. For the purposes of the analysis in this chapter, the first two categories are amalgamated as in Stewart (1990) and referred to as facing 'non-competitive' conditions, while those facing many competitors are labelled 'competitive'.

In firms faced with a high degree of competition, there are stronger incentives for both management and unions to avoid strike activity. For the firm, any disruption to production is liable to lead to a rapid erosion of market share since other firms are easily able to provide similar products. For the union, potential closure presents a greater threat to their job security than in firms facing less competitive conditions, and thus they too should be more willing to concede demands and reach a settlement. However note that private information theories of strike behaviour predict a negative correlation between the size of the rents for division and the probability of a strike. Given that the rents are greater in firms facing less competitive conditions, this suggests the opposite. Public sector establishments typically face little or no (domestic) competition and therefore the impact of this factor requires some investigation.

This distinction is incorporated into the strike probability model by dividing private sector establishments into those facing competitive and non-competitive product market conditions; interactive dummies for head-offices and missing information are also included. The results are given in table 5.4. For variant A, the base group for comparison now becomes private sector establishments facing non-competitive conditions. The public sector *ceteris paribus* differentials are smaller than those given in table 5.2 and are all no longer significantly different from zero (although for non-manuals this occurs only when the full set of controls is included as in specification 2). However, as can be seen from the likelihood ratio test statistics at the bottom of the table, variant B of the model in which the production and service sectors are distinguished is required for both manual and non-manual workers. For the nationalised industries, the *ceteris paribus* difference in strike probability is now measured relative to private manufacturing establishments facing non-competitive conditions, while for the public service sector, the Δs are calculated relative to non-competitive private sector service establishments. For manual workers, the differentials are insignificant, while those for non-manuals are substantially reduced compared to those in table 5.2, and are again insignificant once a full vector of controls is included in the model. However, the diagnostics for table 5.4 presented in table 5.5 indicate that there are still problems with the econometric specification of the equation for non-manual workers in this year.

Further investigation of this issue seems warranted, and in particular, the role played by the disputes in the education sector. For variant A, public sector establishments were dichotomised into schools and non-schools, and separate differentials calculated for each sub-sector, both measured relative to the non-competitive private sector. For variant B, the public service sector establishments were partitioned, and differentials calculated relative to private service establishments facing non-competitive conditions. The proportionate probability differences relative to private services for the schools are much higher than for non-school establishments and the latter are small and insignificant[11]. The diagnostics indicate a more satisfactory specification although the statistic for the test for non-normality is now (marginally) significant and there remains evidence of omitted organisation size effects. In conclusion it would appear that much of the public sector effect for non-manual workers in 1984 is attributable to the educational disputes of that year, and for the remainder of the public sector, the *ceteris paribus* difference is again negligible and insignificant.

Another issue requiring further consideration is the definition of the dependent variable being used. This has been based on the combined responses of management and worker respondents to minimise the impact of imperfect and incomplete recall. The degree of disagreement between the survey participants in reporting strike action

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[11]. Variant B, specification 2 gives Δs of 1.573 for schools and 0.098 for non-school service establishments.

is considerable. Interestingly, there would appear to be greater disagreement between management and worker respondents in the public sector than in the private sector (with the exception of non-manuals in 1984). The impact of this disagreement on the estimated *ceteris paribus* differentials is investigated in a number of ways. A potential problem with the approach adopted above is that in some establishments no worker representative was interviewed. There were two main reasons for this in establishments that recognised unions. The first was that the management respondent could identify no worker representative at the establishment; and the second was refusal by management to allow an interview with the worker representative. In these establishments the dependent variable used so far is therefore constructed on the basis of the management questionnaire only. As a result there is likely to be an under-reporting of strikes in these establishments. This may distort the estimated effects.

As a first method of examining this a dummy variable indicating these establishments is included in the model. This makes very little difference to the Δs reported in table 5.2. An alternative is to rely solely on the management questionnaire to indicate strike activity. This results in about 20% of the strikes captured by the variable used so far being lost. To the extent that the differences are systematic, this will induce a bias in the estimated coefficients. The results from re-estimating the models described above for this strike variable show the estimated Δs to generally have the same sign and similar magnitude. One exception is manual workers in 1984 where management respondents reported substantially fewer strikes especially in the public sector. As a result the public sector differential is cut by about a third. As a final alternative, the models were re-estimated with the establishments in which there was no worker representative interview excluded. Again the magnitudes of the estimated differentials are similar. A major problem with both these latter methods is that management refusal to allow an interview with the worker representative may not be exogenous with respect to the recent strike record at the establishment.

The above analysis focuses solely on whether an establishment experienced any strike action during the twelve month period prior to the interview. It does not take account of the fact that many of the establishments that did had more than one strike during the period or that, in some cases, strikes involve more than one establishment. On the first of these points, the Workplace Industrial Relations Survey provide some information on the frequency of strikes at a given establishment. A Poisson model for this count variable produced qualitatively similar estimates of the public-private differential to those presented above. The second of the above points cannot be addressed using the Workplace Industrial Relations Survey but is taken account of in the next section where a frequency-based approach is adopted.

5.3. A Comparison of Adjusted Strike Frequency Measures.[12]

For a number of reasons, the analysis based on the Workplace Industrial Relations Surveys in the previous section cannot provide a full picture of the differences between public and private sector strike activity. Firstly, it is establishment based. This means that figures can be dominated by a single strike that covered many establishments as noted above with reference to the national engineering and teaching disputes. This raises the interesting question of whether the comparison that should be made should be based, as in the previous section, on the probability of an establishment having at least one strike in a twelve month period or on some other probability. Potential alternatives include the probability of a given bargaining pair producing a strike in a given period, the probability of a given round of contract negotiations leading to a strike, the number of strikes per establishment during a given period, the number of strikes per employee during the period, the number of strikes per union member during the period and so on. Secondly, the Workplace Industrial Relations Survey data is a sample and although designed to be representative inevitably has rather small numbers of observations in certain cells. Thirdly, the

12. The analysis in this section utilises the Stata package, version 2 (Stata (1990)).

Workplace Industrial Relations Survey only provide survey information for two particular years and this does not permit the identification of any trends in the relative strike proneness of the two sectors.

This section presents an alternative approach to the public *vs* private sector strike question based on the Industrial Stoppages data files compiled by the Department of Employment. This source alleviates many of the weaknesses described above; it comprises all strikes[13], and records stoppages with a common 'cause' as a single stoppage, regardless of the number of workplaces affected. In addition it covers a fairly long time period and hence can be used to identify any trends.

Table 5.6 presents summary data for strikes in the public and private sectors for 1974 to 1984[14]. In aggregate, the declining total number of strikes throughout the 1970s and into the 1980s (from the unprecedented peak of 3,906 stoppages in 1970) can be seen to result entirely from the fall in the number of strikes in the private sector. The frequency of public sector strikes shows no discernible trend and thus movements in total strike frequency are dominated by those for the private sector[15]. The latter comprised over 80% of the total at the start of the period, but only aound 60% ten years later. In addition the number of strikes in the public sector displays much less variability around its mean as shown by the coefficients of variation at the bottom of the table. The figures deflated by number of employees indicate that strikes per

13. The Department of Employment collates information from local Unemployment Benefit Offices, returns from Nationalised Industries, public bodies and large firms, reports in the national and local press and, for some larger stoppages, directly from the organisations involved. There obviously exist problems in ensuring complete coverage and partly for this reason, stoppages involving fewer than ten workers and those lasting less than one day are excluded from the statistics unless the total number of working days lost exceeds one hundred.

14. Although the tapes are also available for 1966-1973, there is no public sector coding for these years. One possibility would be to (subjectively) allocate strikes to the public and private sectors according to their SIC classification since this information is given on the tapes. However, it was considered preferable to work with a consistently compiled series, and hence the analysis is restricted to 1974 onwards. The omission of statistics for 1981 is due to current data deficiencies.

15. The correlation between the series for total and public sector strikes is only 0.07, while that for total and private sector strikes is 0.99.

employee in the public sector were about one third lower than in the private sector in the 1970s, but almost 50% higher than in the private sector in the 1980s. However as before this turn-around is due to changes in the private sector, with public sector strikes per employee showing no discernible trend.

If union membership is taken as the deflator a rather different picture emerges for the early 1980s, since union density is considerably greater in the public sector than in the private sector. As a rough calculation, the density by sector figures from Bailey and Kelly (1990) is applied to the years 1982-84. They estimate union density to be 29.6% in the private sector and 69.0% in the public sector. The next panel for each sector in table 5.6 gives estimated figures per union member. Strikes per union member are found to be higher in the private sector than the public sector in the early 1980s. Clearly the same will be true of the 1970s.

Table 5.6 also indicates that typically strikes in the private sector involve more workers than those in the public sector. Hence if one wants to adopt an individual-based measure of strike activity, the figures for strike frequency will overstate the relative activity in the public sector. An estimate of the number of 'strikers' per employee for each sector is also presented in table 5.6 and reveals that while the private sector was comparatively more strike prone in the 1970s, the individual-level strike intensity of the two sectors was not substantially different in the 1980s.

Several explanations can be advanced for these important findings. The relatively high levels of unionisation found in the public sector may imply that, although there is a greater tendency for strike action, there also exists well-defined and established negotiation and bargaining structures which can be invoked to generate earlier settlements and prevent the spread of the dispute beyond the immediate group of employees involved. Alternatively, or additionally, public sector employers may conceed more easily to workers' claims since they do not face the same competitive pressures as private sector employers. Certainly, product market conditions were seen to be important in the analysis of strike incidence in section 5.2. Unfortunately, further details of the strike or terms of settlement are not included on the Department of Employment tapes, and hence these (or any other) rationales cannot be discriminated in any way. In particular, the kind of *ceteris paribus* comparisons made in the previous section utilising the Workplace Industrial Relations Survey data cannot be repeated since there is no base group against which to compare those situations which resulted in strike action.

As an alternative to the number of strikes per head one might wish to compare the number of strikes per establishment. To do this, the measures of strike frequency from the Department of Employment tapes is combined with the size distribution of establishments taken from the Workplace Industrial Relations Survey. One immediate problem is that the Department of Employment tapes do not identify the exact starting date of each of the stoppages (only the year) and hence the strikes series cannot be matched to the Workplace Industrial Relations Survey compilation period which had a median survey date of sometime in May for both survey years (Millward & Stevens (1986, pp.323-324)). However, it seems likely that the size distribution of establishments changes very slowly over time, and hence strike frequency for both 1979 and 1980 is related to Workplace Industrial Relations Survey 1980, while the number of strikes in both 1983 and 1984 are matched with Workplace Industrial Relations Survey 1984.

The distribution of establishments by sector[16] and the total number of strikes together with the ratio, θ, of the proportion of strikes to the proportion of establishments are shown in table 5.7. It can be seen that the public sector comprised a rather higher proportion of all establishments in 1984 than in 1980, principally due to the

16. These are weighted to account for the stratified sampling scheme used to compile the Workplace Industrial Relations Survey.

widespread closure of private sector manufacturing establishments in the recession of 1980/81. The index of inter-sector strike proneness, θ, takes a value of unity if the distribution of strikes exactly matches the distribution of establishments between sectors. Thus it can be seen that for 1979, the private sector was relatively more strike prone, while for 1980 and 1983, the public sector had a higher strike propensity. Finally, for 1984, the relative propensities per establishment can be seen to be approximately equal.

To examine how these aggregate changes in strike propensities have been distributed across establishment size is a slightly more difficult exercise. Several qualifications need to be noted before the results are presented. Firstly, the analysis only includes strikes which occurred at a single establishment since the concept of 'establishment size' obviously has no meaning for multi-establishment strikes[17]. This affects a disproportionately large number of the public sector strikes in 1979 and 1980 and thus the results in table 5.8 are biased to the extent that these more extensive strikes are omitted from the analysis. The exact coverage proportions for each year are given in the table for reference. Secondly, the sample of strikes chosen has been correctly selected to include only those which occurred at establishments with at least 25 workers in order that the comparisons with Workplace Industrial Relations Survey are valid. Thirdly, the establishment size information for 1983 and 1984 is already grouped on the Department of Employment tapes, and the size bands chosen necessitate amalgamating the bottom two size divisions. This group, labelled Esize1 & 2, therefore incorporates strikes at establishments with between 25 and 99 workers.

As expected public sector establishments are larger than private sector establishments, with higher proportions of public sector establishments in size categories 3 to 6 for

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[17]. Establishment size is not recorded for such strikes in the Department of Employment statistics.

both of the Workplace Industrial Relations Surveys. Secondly, the size distribution of strikes is highly skewed; as noted above, strikes are much more common at large establishments and this is illustrated very clearly in table 5.8. Public sector strikes are more heavily skewed to the left (i.e. concentrated to the right) than private sector ones. However, this may simply reflect the skewness of the size distribution of establishments and thus whether the *relative* size distribution differs between public and private sectors can only be ascertained by comparing the inter-establishment relative strike propensities. These are also presented in the table. A value of $\theta = 1$ would indicate an exactly proportionate relationship between strikes and establishments and given the previously noted impact of size on strike propensity, it is expected that θ will increase sharply with size and this is indeed evident from the table. More importantly in the current context, the θs for the private sector are almost without exception larger than those for the public sector in every size category. The interpretation is that while there are more strikes at larger establishments in both sectors yielding θs greater than unity, the private sector incurs single establishment strikes disproportionately more in every size category when compared to the public sector. That is, the private sector distribution of strikes is even more highly skewed to the left when compared to that of the public sector, having controlled for the relative skewness of the distribution of establishment size. This would indicate that single establishment strikes are larger, on average, in the private sector.

Although the principal focus of this chapter is on the incidence of strikes and/or the number of strike starts, it is of some interest to briefly compare the differences between the public and private sectors in some of the other dimensions of stoppages as recorded by the two sources of data utilised here. Firstly, in an identical manner to the investigation of the incidence of strikes using the Workplace Industrial Relations Surveys in section 5.2, proportionate probability differences for the relative incidence of non-strike action were calculated. Once again, the combined responses of management and worker representatives were used in the construction of the dependent variables. The raw incidence statistics reveal that while manual workers in public sector establishments are less likely to take non-strike action than those in private sector establishments, for non-manual workers this pattern is reversed. This lower incidence of non-strike action among manual workers is seen to be confined mainly to the production sector, while the higher incidence among non-manual workers is principally a characteristic of the service sector. These observations are valid for both the 1980 and the 1984 Workplace Industrial Relations Surveys. However, once again, when a full vector of control vaiables is added to the equations, few of these differences are significant at conventioanl levels.

In addition to the strike frequency statistics, the Department of Employment Industrial Stoppages Data Tapes also contain information on the size and duration of each stoppage, as well as the total number of days lost. Much of these data are only recorded as grouped measures. Table 5.9 presents summary statistics separately for the public and private sector for each of these other dimensions of strikes. The top panel disaggregates strikes by the number of workers involved and shows that strikes in the public sector tend to be smaller on average, with over one fifth involving less than 25 employees[18]. The second panel shows that public sector strikes are also typically of shorter duration; 45% last one day or less compared with a figure of only 14% in this category for the private sector. At the other end of the scale, less than one fifth of public sector disputes last six days or more compared with almost one half of all private sector stoppages[19]. The final panel of the table reflects the combined impact of these observations since the mean number of days lost per strike is identically equal to the product of average strike size and duration. It can be seen that almost 60% of public sector stoppages result in fewer than 250 days lost, while 35%

18. The figures for the median number of workers involved in table 5.6 are derived from these data.

19. While the last of the duration categories encompasses nearly 20% of public sector strikes and over 45% of private sector strikes, more disaggregated statistics are actually available on the tapes for strikes in excess of 6 days. However the grouping band intervals were changed from 1979 (from 12 groups to 11), and in consequence, a better consistent comparison across all eleven years is not available.

of private sector disputes lead to at least 1,000 days lost. Clearly further research into these rather marked distinctions between public and private sector stoppages is needed.

5.4. Summary and Conclusions.

This chapter has examined differentials between public and private sector strike activity. The empirical evidence from the Workplace Industrial Relations Survey data in section 5.2 reveals that the apparent greater propensity for strike action in the public sector is largely due to the characteristics of public sector establishments, which tend to be larger, more highly unionised and face less competitive product market conditions. The differentials between the sectors are very different for manual and non-manual workers. For manual workers the estimated differential in 1980 is roughly zero, while in 1984 a public sector establishment is 37% more likely to have had a strike than a comparable private sector one at the mean without control for product market conditions. It is 21% more likely than a comparable private sector establishment facing few competitors in the product market (but not statistically significant).

The pattern of non-manual strikes is completely different. In 1980 there is a differential at the mean without control for product market factors of 21%. This is entirely due to the differential in the manufacturing sector, that in the services sector being zero. In 1984 the differential at the mean without control for product market factors is 89%. Incorporation of product market effects reduces this to 45%. Unlike in 1980 this is found to be entirely due to the effect in the services sector. This in turn is entirely the result of the series of strikes in the school sector. The differential in the remainder of the services sector is less than 10% and statistically insignificant.

One of the problems of using survey data to investigate such issues is apparent in the analysis using the Workplace Industrial Relations Surveys; single widespread disputes in the survey sampling period can dominate the statistics. The analysis in section 5.3 based on aggregate strike frequency data is not able to distinguish manual and non-manual strikes. These aggregate strike frequency figures deflated by employment indicate that strikes per employee were lower in the public sector in the late 1970s and higher in the early 1980s. However this turn-around was seen to be due to frequency changes in the private sector. When frequency is deflated by an estimate of union membership in the sector, strikes per union member are found to also be lower in the early 1980s in the public sector than in the private sector. Strikes in the private sector are found on average to involve more workers and to be of longer duration. Thus when a measure taking account of this is used the relative strike proneness of the public sector is further reduced.

Table 5.1

Percentage of Establishments Experiencing Strikes During the Year by Sector

All establishments with union members and recognition

Sector	Manual 1980	Non-Man 1980	Manual 1984	Non-Man 1984
Private Manufacturing	30	10	18	5
Nationalised Industries	24	17	31	10
Private Services	10	4	9	13
Public Services	12	10	10	39
Total	18	9	13	27

Establishments with less than 100 employees

Sector	Manual 1980	Non-Man 1980	Manual 1984	Non-Man 1984
Private Manufacturing	21	0	10	2
Nationalised Industries	25	21	29	8
Private Services	9	1	6	12
Public Services	9	8	7	38
Total	13	6	9	27

Establishments with 100-499 employees

Sector	Manual 1980	Non-Man 1980	Manual 1984	Non-Man 1984
Private Manufacturing	37	11	23	6
Nationalised Industries	19	10	31	12
Private Services	11	14	15	13
Public Services	17	13	17	41
Total	24	12	20	26

Establishments with more than 500 employees

Sector	Manual 1980	Non-Man 1980	Manual 1984	Non-Man 1984
Private Manufacturing	56	23	46	10
Nationalised Industries	44	23	45	18
Private Services	53	31	32	25
Public Services	24	28	30	40
Total	44	25	38	26

Table 5.2

Ceteris Paribus Comparisons of the Probability of Strike Action

			Manual 1980	Non-Manual 1980	Manual 1984	Non-Manual 1984
Number of Observations			1363	1277	1405	1397
Mean Strike Probability			0.311 (0.463)	0.154 (0.361)	0.283 (0.451)	0.252 (0.434)
Raw Proportionate Probability differences:						
PUBLIC *vs* PRIVATE			-0.416	0.117	0.034	1.667
NATINDY *vs* PRIMANF			-0.309	0.338	0.225	0.619
PUBSERV *vs* PRISERV			0.101	0.445	0.384	1.491
Variant A: Single Public dummy						
Spec 1:	PROD	coeff	0.480 (.090)	0.022 (.108)	0.346 (.083)	-0.506 (.091)
	PUBLIC	coeff	-0.223 (.090)	0.237 (.103)	0.208 (.082)	0.594 (.086)
		Δ	*-0.217*	*0.420*	*0.265*	*1.299*
Spec 2:	PROD	coeff	0.144 (.102)	-0.035 (.129)	-0.047 (.102)	-0.454 (.116)
	PUBLIC	coeff	0.047 (.105)	0.124 (.119)	0.283 (.097)	0.434 (.108)
		Δ	*0.048*	*0.209*	*0.366*	*0.888*
LR tests for single equation *vs* separate Public and Private equations:						
	Specification 1		6.74	11.44	5.94	11.41
	Specification 2		18.10	21.76	23.38	25.58
Variant B: Separate Production and Services effects						
Spec 1:	NATINDY	coeff	-0.352 (.131)	0.362 (.148)	0.276 (.113)	0.342 (.141)
		Δ	*-0.297*	*0.635*	*0.318*	*0.715*
	SERVICE	coeff	-0.579 (.118)	0.075 (.152)	-0.258 (.121)	0.303 (.141)
	PUBSERV	coeff	-0.096 (.128)	0.139 (.142)	0.130 (.120)	0.733 (.113)
		Δ	*-0.136*	*0.266*	*0.202*	*1.496*
Spec 2:	NATINDY	coeff	-0.124 (.152)	0.297 (.173)	0.211 (.131)	0.022 (.174)
		Δ	*-0.108*	*0.508*	*0.241*	*0.039*
	SERVICE	coeff	-0.278 (.132)	0.167 (.176)	-0.007 (.137)	0.124 (.171)
	PUBSERV	coeff	0.203 (.144)	0.002 (.153)	0.350 (.135)	0.627 (.130)
		Δ	*0.331*	*0.004*	*0.591*	*1.241*
LR tests for single equation *vs* separate Public and Private equations:						
	Specification 1		5.52	10.16	6.37	3.67
	Specification 2		15.54	20.10	22.69	14.23
LR tests on Variant A *vs* Variant B						
	Specification 1		1.22	1.27	0.43	7.74*
	Specification 2		2.55	1.66	0.68	11.35*

Table 5.2 (continued)

Definitions and Notes

Variant A	Sectoral dummies: PROD and PUBLIC
Variant B	Sectoral dummies: NATINDY, SERVICE and PUBSERV
Specification 1	Vector of controls: ESIZE2-ESIZE6
Specification 2	Vector of controls: ESIZE2-ESIZE6, PROP, P_SK, P_PT, P_FE, DENS, MUNION, C_SHOP, EMPASS, LEVEL, SINGLE
Δ	Proportionate change in probability of strike measured relative to the appropriate base (see text for detail)
*	Test statistic significant at 5%
	Standard errors are given in parentheses

Table 5.2

Ceteris Paribus Comparisons of the Probability of Strike Action

	Manual 1980	Non-Manual 1980	Manual 1984	Non-Manual 1984
Number of Observations	1363	1277	1405	1397
Mean Strike Probability	0.311 (0.463)	0.154 (0.361)	0.283 (0.451)	0.252 (0.434)
Raw Proportionate Probability differences:				
PUBLIC *vs* PRIVATE	-0.416	0.117	0.034	1.667
NATINDY *vs* PRIMANF	-0.309	0.338	0.225	0.619
PUBSERV *vs* PRISERV	0.101	0.445	0.384	1.491
Variant A: Single Public dummy				
Spec 1: PROD coeff	0.480 (.090)	0.022 (.108)	0.346 (.083)	-0.506 (.091)
PUBLIC coeff	-0.223 (.090)	0.237 (.103)	0.208 (.082)	0.594 (.086)
Δ	*-0.217*	*0.420*	*0.265*	*1.299*
Spec 2: PROD coeff	0.144 (.102)	-0.035 (.129)	-0.047 (.102)	-0.454 (.116)
PUBLIC coeff	0.047 (.105)	0.124 (.119)	0.283 (.097)	0.434 (.108)
Δ	*0.048*	*0.209*	*0.366*	*0.888*
LR tests for single equation *vs* separate Public and Private equations:				
Specification 1	6.74	11.44	5.94	11.41
Specification 2	18.10	21.76	23.38	25.58
Variant B: Separate Production and Services effects				
Spec 1: NATINDY coeff	-0.352 (.131)	0.362 (.148)	0.276 (.113)	0.342 (.141)
Δ	*-0.297*	*0.635*	*0.318*	*0.715*
SERVICE coeff	-0.579 (.118)	0.075 (.152)	-0.258 (.121)	0.303 (.141)
PUBSERV coeff	-0.096 (.128)	0.139 (.142)	0.130 (.120)	0.733 (.113)
Δ	*-0.136*	*0.266*	*0.202*	*1.496*
Spec 2: NATINDY coeff	-0.124 (.152)	0.297 (.173)	0.211 (.131)	0.022 (.174)
Δ	*-0.108*	*0.508*	*0.241*	*0.039*
SERVICE coeff	-0.278 (.132)	0.167 (.176)	-0.007 (.137)	0.124 (.171)
PUBSERV coeff	0.203 (.144)	0.002 (.153)	0.350 (.135)	0.627 (.130)
Δ	*0.331*	*0.004*	*0.591*	*1.241*
LR tests for single equation *vs* separate Public and Private equations:				
Specification 1	5.52	10.16	6.37	3.67
Specification 2	15.54	20.10	22.69	14.23
LR tests on Variant A *vs* Variant B				
Specification 1	1.22	1.27	0.43	7.74*
Specification 2	2.55	1.66	0.68	11.35*

Table 5.3

Misspecification Diagnostics and Omitted Variable Tests

Variant A: Single Public dummy

Test Stat	1980				1984			
	Specification 1		Specification 2		Specification 1		Specification 2	
	Manual	Non-Man	Manual	Non-Man	Manual	Non-Man	Manual	Non-Man
n	1363	1277	1363	1277	1405	1397	1405	1397
*log*L	-735.74	-511.52	-672.51	-495.81	-751.18	-721.57	-726.43	-686.17
Hetsize(5)	7.02	11.32*	10.34	3.84	0.79	17.15*	3.11	21.07*
Hetsec(2)	0.64	0.24	2.42	0.26	2.21	0.29	1.38	2.70
Fform(3)	0.33	8.94*	4.34	3.31	0.99	11.43*	2.66	5.69
Norm(2)	0.32	8.92*	4.14	3.14	0.76	9.41*	2.49	5.68
Orgsize(5)	2.24	4.14	1.66	1.48	7.47	19.68*	7.20	15.97*
Omits(5)	7.39	2.77	1.58	1.30	10.36	41.45*	7.07	27.67*

Variant B: Separate Production and Services effects

Test Stat	1980				1984			
	Specification 1		Specification 2		Specification 1		Specification 2	
	Manual	Non-Man	Manual	Non-Man	Manual	Non-Man	Manual	Non-Man
n	1363	1277	1363	1277	1405	1397	1405	1397
*log*L	-735.12	-510.88	-671.24	-495.02	-751.39	-717.70	-726.30	-680.93
Hetsize(5)	6.84	12.23*	9.59	3.06	1.64	13.89*	3.24	14.45*
Hetsec(3)	0.31	0.69	4.13	0.24	4.37	4.42	1.41	3.44
Fform(3)	1.12	13.52*	3.94	4.00	2.05	7.02	2.48	3.26
Norm(2)	0.32	11.61*	3.91	2.80	1.71	5.41	2.28	3.16
Orgsize(5)	2.24	3.64	1.81	1.34	6.97	23.14*	7.92	18.83*
Omits(5)	8.02	2.52	1.90	1.08	10.21	45.35*	7.27	32.15*

Definitions and Notes

	See notes to table 5.2
Hetsize	Any heteroskedasticity by establishment size
Hetsec	Any heteroskedasticity by sector
Fform	RESET test of general functional form misspecification
Norm	Test for non-normality (higher moments of residuals)
Orgsize	Test for the exclusion of 5 organisation size dummies
Omits	Test for the omission of FOREIGN, SHIFT, PAY, DIS, IND
*	Test statistic significant at 5% Degrees of freedom for score tests are given in parentheses

Table 5.4

The Impact of Competitive and Non-Competitive Market Conditions in 1984

		Manual Workers		Non-Manual Workers	
Number of Observations		1405		1397	
Mean Strike Probability		0.283	(0.451)	0.252	(0.434)
Raw Proportionate Probability differences:					
PUBLIC *vs* PRIVATE		-0.126		1.272	
NATINDY *vs* PRIMANF		0.151		0.764	
PUBSERV *vs* PRISERV		-0.103		0.567	
Variant A					
		Spec. 1	Spec. 2	Spec. 1	Spec. 2
PROD	coeff	0.330 (.084)	-0.050 (.103)	-0.491 (.092)	-0.452 (.116)
PUBLIC	coeff	0.105 (.105)	0.188 (.119)	0.478 (.118)	0.255 (.139)
	Δ	*0.118*	*0.214*	*0.922*	*0.450*
LR tests for single equation *vs* separate Public and Private equations:					
		6.15	22.40	11.10	23.95
Variant B					
		Spec. 1	Spec. 2	Spec. 1	Spec. 2
NATINDY	coeff	0.250 (.134)	0.200 (.154)	0.393 (.178)	0.039 (.211)
	Δ	*0.274*	*0.218*	*0.554*	*0.070*
PUBSERV	coeff	-0.195 (.178)	0.055 (.194)	0.398 (.165)	0.212 (.182)
	Δ	*-0.222*	*0.068*	*0.564*	*0.287*
LR tests for single equation *vs* separate Public and Private equations:					
		6.36	23.92	3.64	14.69
LR tests on Variant A *vs* Variant B					
		11.60*	12.97*	18.65*	20.96*

Definitions and Notes

	See notes to table 5.2
Variant A Variant B	Sectoral dummies: PROD, PUBLIC, COMP, HEAD, MISS Sectoral dummies: NATINDY, PRIMANF*COMP, PRIMANF*HEAD, PRIMANF*MISS, SERVICE*(NONCOMP or PUBLIC), SERVICE*MANY, SERVICE*HEAD, SERVICE*MISS, PUBSERV

Table 5.5

Misspecification Diagnostics and Omitted Variable Tests

	Manual Workers		Non-Manual Workers	
Variant A				
	Spec. 1	Spec. 2	Spec. 1	Spec. 2
*log*L	-747.43	-717.71	-718.28	-676.63
Hetsize(5)	0.30	2.98	28.90*	21.80*
Hetsec(5)	4.99	5.29	6.41	12.48*
Fform(3)	1.56	3.18	13.70*	7.66
Norm(2)	1.46	2.83	12.93*	7.52*
Orgsize(5)	6.57	6.47	19.33*	15.26*
Omits(5)	11.35*	6.82	32.44*	27.88*
Variant B				
	Spec. 1	Spec. 2	Spec. 1	Spec. 2
*log*L	-741.63	-711.23	-708.95	-666.15
Hetsize(5)	5.04	2.90	19.61*	13.79*
Hetsec(9)	20.72*	11.35	9.69	17.77*
Fform(3)	16.27*	3.26	6.14	6.91
Norm(2)	9.91*	3.03	5.62	6.62*
Orgsize(5)	7.05	8.12	25.81*	20.36*
Omits(5)	10.45	6.34	36.44*	32.37*

Definitions and Notes

See notes to table 5.3

Table 5.6

Strike Frequency and Incidence 1974-1984 Disaggregated by Broad Sector

	Public Sector							Private Sector						
Year	Strike Frequency Total	%	Employees (000)	Strikes per million: employees	Union Members	Median Workers Involved	Strikers per 10^8 employees	Strike Frequency Total	%	Employees (000)	Strikes per million: employees	Union Members	Median Workers Involved	Strikers per 10^8 employees
1974	544	*19*	6,907	78.8		96.3	75.9	2378	*81*	16,245	146.4		121.3	177.6
1975	466	*20*	7,253	64.3		99.0	63.6	1816	*80*	15,809	114.9		106.5	122.4
1976	538	*27*	7,300	73.7		69.0	50.1	1478	*73*	15,592	94.8		113.5	107.6
1977	557	*21*	7,368	75.6		85.0	64.3	2146	*79*	15,590	137.7		124.8	171.8
1978	665	*27*	7,357	90.4		96.1	86.9	1806	*73*	15,750	114.7		123.8	142.0
1979	563	*27*	7,449	75.6		99.0	74.8	1517	*73*	16,038	94.6		143.4	135.6
1980	534	*40*	7,387	72.3		85.0	61.4	796	*60*	15,927	50.0		106.8	53.4
1981			7,185							15,040				
1982	683	*45*	7,021	97.3	141.0	82.7	80.5	845	*55*	14,717	57.4	193.9	135.5	77.8
1983	614	*45*	6,952	88.3	128.0	53.8	47.5	738	*55*	14,437	51.1	172.6	135.8	69.4
1984	433	*36*	6,911	62.7	90.9	159.6	100.1	773	*64*	14,653	52.8	178.4	174.5	92.1
Mean	560	*31*				92.6		1429	*69*				128.6	
SD	78							612						
CV	14%							43%						

Sources: Strike frequency and size: Department of Employment Industrial Stoppages data files.
Employment: Economic Trends
Union Density: Bailey & Kelly (1990)

Note: 1981 figures missing due to coding errors on DE tape.

Table 5.7

Relative Strike Incidence by Broad Sector

WIRS80 and DE Tapes 1979 & 1980

Sector	Estab. 1979/80		Strikes 1979			Strikes 1980		
	Number	%	Number	%	θ	Number	%	θ
Public	650	31.9	563	27.1	*0.85*	534	40.1	*1.26*
Private	1390	68.1	1517	72.9	*1.07*	796	59.9	*0.88*
Total	2040	100.0	2080	100.0		1330	100.0	

WIRS84 and DE Tapes 1983 & 1984

Sector	Estab. 1983/84		Strikes 1983			Strikes 1984		
	Number	%	Number	%	θ	Number	%	θ
Public	740	36.6	614	45.4	*1.24*	433	35.9	*0.98*
Private	1297	63.4	738	54.6	*0.86*	773	64.1	*1.01*
Total	2019	100.0	1352	100.0		1206	100.0	

Table 5.8

Relative Strike Incidence by Broad Sector and Establishment Size

(Single Establishment Strikes Only)

WIRS80 and DE Tapes 1979 & 1980

Sector	Esize1	Esize2	Esize3	Esize4	Esize5	Esize6	Total	Coverage %	
Size Distribution of Establishments 1979/80 (weighted):									
Public	0.469	0.232	0.163	0.093	0.028	0.015	1.00	650	100.0
Private	0.519	0.263	0.121	0.068	0.018	0.012	1.00	1390	100.0
Size Distribution of Single Establishment Strikes:									
1979 Public	0.055	0.075	0.104	0.254	0.114	0.398	1.00	201	35.7
Private	0.060	0.102	0.139	0.219	0.160	0.320	1.00	1328	87.5
1980 Public	0.072	0.087	0.159	0.196	0.109	0.377	1.00	138	25.8
Private	0.063	0.130	0.164	0.250	0.144	0.250	1.00	653	82.0
Inter-Establishment Relative Strike Propensities:									
θ_{79} Public	0.12	0.32	0.64	2.73	4.07	26.53			
θ_{79} Private	0.12	0.39	1.15	3.22	8.89	26.67			
θ_{80} Public	0.15	0.38	0.98	2.11	3.89	25.13			
θ_{80} Private	0.12	0.49	1.36	3.68	8.00	20.83			

WIRS84 and DE Tapes 1983 & 1984

Sector	Esize1 & 2	Esize3	Esize4	Esize5	Esize6	Total	Coverage %	
Size Distribution of Establishments 1983/84 (weighted):								
Public	0.726	0.142	0.088	0.027	0.018	1.00	740	100.0
Private	0.791	0.123	0.063	0.016	0.008	1.00	1297	100.0
Size Distribution of Single Establishment Strikes:								
1983 Public	0.033	0.053	0.115	0.254	0.544	1.00	511	83.2
Private	0.181	0.169	0.218	0.132	0.299	1.00	568	77.0
1984 Public	0.065	0.097	0.136	0.190	0.513	1.00	279	64.4
Private	0.131	0.131	0.252	0.136	0.350	1.00	611	79.0
Inter-Establishment Relative Strike Propensities:								
θ_{83} Public	0.04	0.37	1.31	9.41	30.22			
θ_{83} Private	0.23	1.37	3.46	8.25	37.38			
θ_{84} Public	0.09	0.68	1.54	7.04	28.50			
θ_{84} Private	0.17	1.06	4.00	8.50	43.75			

Note: See text for details of results.

Table 5.9

Strike Size, Duration and Days Lost 1974-1984 Disaggregated by Broad Sector

Number of Workers Involved

Year	Public Sector						Private Sector					
	Workers Involved (% of public sector)						Workers Involved (% of private sector)					
	1	2	3	4	5	6	1	2	3	4	5	6
1974	16	17	18	21	14	14	13	16	18	21	14	18
1975	19	16	15	26	11	13	14	16	19	23	12	16
1976	22	21	18	18	14	8	14	18	16	22	13	16
1977	21	14	21	20	13	12	12	15	19	24	14	17
1978	17	17	17	23	13	14	13	15	18	25	13	16
1979	20	17	13	21	11	17	11	16	16	24	15	19
1980	24	16	14	19	11	17	13	18	18	22	12	17
1981												
1982	28	12	15	18	11	17	15	14	16	21	14	20
1983	33	16	13	15	10	14	13	15	16	25	13	18
1984	14	11	17	20	13	25	12	12	15	22	16	22
Mean	21	16	16	20	12	15	13	16	17	23	14	18

Durations of Stoppages

Year	Public Sector						Private Sector					
	Strike Duration (% of public sector)						Strike Duration (% of private sector)					
	1	2	3	4	5	6	1	2	3	4	5	6
1974	32	20	11	7	6	24	11	14	13	8	9	45
1975	44	18	9	5	5	20	11	12	10	8	9	50
1976	43	21	10	6	5	15	11	14	12	9	9	45
1977	42	15	12	5	5	22	11	13	11	10	9	48
1978	39	19	10	7	6	20	11	12	11	9	9	49
1979	41	18	12	5	6	18	12	10	9	8	8	53
1980	50	16	10	6	2	16	11	13	11	8	10	47
1981												
1982	58	15	6	3	3	14	21	14	11	8	5	42
1983	54	14	5	5	4	17	18	13	9	8	7	45
1984	46	13	8	6	4	23	21	14	9	9	7	41
Mean	45	17	9	6	5	19	14	13	11	9	8	47

Table 5.9 (continued)

Strike Size, Duration and Days Lost 1974-1984 Disaggregated by Broad Sector

Working Days Lost

Year	Public Sector						Private Sector					
	Working Days Lost (% of public sector)						Working Days Lost (% of private sector)					
	1	2	3	4	5	6	1	2	3	4	5	6
1974	52	13	12	12	5	7	34	16	16	16	8	10
1975	58	13	11	10	3	5	33	17	16	17	8	9
1976	64	12	10	9	2	4	35	17	17	16	8	8
1977	59	11	11	10	3	6	33	16	16	16	8	11
1978	55	15	11	10	5	5	31	18	16	16	9	10
1979	56	13	11	9	4	7	30	15	16	16	9	14
1980	62	10	10	8	4	6	32	17	16	20	7	8
1981												
1982	65	9	9	8	3	6	38	15	14	17	9	8
1983	65	10	9	8	3	5	37	15	15	16	8	9
1984	51	10	10	12	7	10	34	15	15	16	9	10
Mean	59	12	10	10	4	6	34	16	16	17	8	10

Source: Department of Employment Industrial Stoppages Data Tapes

Notes and Definitions:

1. The workers involved categorisation used in the table is as follows:-
   - 1 : less than 25 workers involved
   - 2 : 25 - 49 workers involved
   - 3 : 50 - 99 workers involved
   - 4 : 100 - 249 workers involved
   - 5 : 250 - 499 workers involved
   - 6 : 500 or more workers involved

2. The strike duration categorisation used in the table is as follows:-
   - 1 : 1 day
   - 2 : 2 days
   - 3 : 3 days
   - 4 : 4 days
   - 5 : 5 days
   - 6 : 6 days or longer

3. The working days lost categorisation used in the table is as follows:-
   - 1 : less than 250 days lost
   - 2 : 250 - 499 days lost
   - 3 : 500 - 999 days lost
   - 4 : 1000 - 2499 days lost
   - 5 : 2500 - 4999 days lost
   - 6 : 5000 or more days lost

Appendix to Chapter 5

Table A5.1

Description of Variables and Weighted Means

Key	Variable Description	1980 Manual	1980 Non-Man	1984 Manual	1984 Non-Man
	Dependent Variable				
STRIKE	Proportion incurring strike action	0.181	0.088	0.134	0.267
	Sectoral Dummies: Sample Proportions				
PUBLIC	Public Sector	0.429	0.607	0.547	0.667
PRIVATE	Private Sector	0.571	0.393	0.453	0.333
COMP	Private Sector - Many competitors	n/a	n/a	0.215	0.144
NONCOMP	Private Sector - Few competitors	n/a	n/a	0.155	0.113
PROD	Production Sector	0.380	0.205	0.308	0.201
NATINDY	Nationalised Industries	0.065	0.067	0.096	0.098
PRIMANF	Private Manufacturing	0.315	0.137	0.212	0.102
SERVICE	Service Sector	0.620	0.796	0.692	0.800
PUBSERV	Public Services	0.364	0.540	0.451	0.569
PRISERV	Private Services	0.256	0.256	0.241	0.231
	Control Variables				
ESIZE1	Establishment size 25- 49	0.402	0.430	0.444	0.470
ESIZE2	Establishment size 50- 99	0.252	0.234	0.245	0.230
ESIZE3	Establishment size 100- 199	0.168	0.158	0.162	0.152
ESIZE4	Establishment size 200- 499	0.115	0.116	0.098	0.097
ESIZE5	Establishment size 500- 999	0.039	0.036	0.032	0.031
ESIZE6	Establishment size 1000+	0.023	0.025	0.020	0.020
PROP	Proportion in total employment	0.620	0.535	0.574	0.571
P_SK	Proportion Skilled	0.181	0.085	0.168	0.092
P_PT	Proportion Part-time	0.172	0.196	0.205	0.220
P_FE	Proportion Female	0.211	0.270	0.131	0.410
DENS	Union Density	0.644	0.666	0.570	0.684
MUNION	Multiple Unions at Establishment	0.387	0.598	0.367	0.633
C_SHOP	Closed Shop	0.376	0.142	0.271	0.110
EMPASS	Member of Employers' Association	0.333	0.248	0.238	0.192
LEVEL	Plant Level Bargaining	0.154	0.089	0.111	0.073
SINGLE	Single Establishment Firm	0.118	0.058	0.098	0.044
n	Number of Observations	1363	1277	1405	1397

CHAPTER 6

Unions, Conflict Activity and Efficiency[1]

6.1. Introduction.

While the previous three chapters have concentrated solely on determining the patterns that exist in the incidence and number of stoppage starts, this chapter examines the repercussions, rather than the causes, of strikes. The evaluation of both the costs and consequences of conflict activity has been much neglected in the literature on strike activity. The joint-cost models attributed to Reder & Neumann (1980) and Kennan (1980) postulate that where the establishment of suitable strike-preventing institutional arrangements or 'protocols' is costly, relative to the cost of incurring a strike, there is a greater likelihood of observing strike activity. Thus differences in the costs of strikes can be expected to be related to the observed levels of strike activity and hence strike costs are of much importance.

Despite this observation, apart from the two papers cited above and a recent experimental study by Sopher (1990) there has been little analysis of the costs of strike activity. In part, this omission is the result of the high level of aggregation typically employed in studies of strike activity. In total, the number of days lost in Britain due to strikes is a fraction of 1% of all working days as shown by Whittingham & Towers (1971) and Turner (1969a). However, such calculations are largely inappropriate. Firstly, they fail to discriminate between individual industries which differ quite considerably in their strike proneness as found by Smith *et al* (1978) among others. Secondly, aggregating days lost over the duration of a stoppage

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1. Some of the work in this chapter represents collaborative research with my supervisor, Ben Knight, together with Paul Geroski of the London Business School.

does not recognise that output lost during a strike may be compensated for by increases in production before and/or after the strike, or by other firms in the same industry. Finally, counting the number of days lost fails to identify the costs to the individual participants which may be substantial.

Related to this inappropriateness of days lost as a measure of strike costs, in situations where strikes are precipitated by employers, the concept of 'days lost' may be ambiguous if strikes are used by management as output or inventory reduction schemes. Thus while there may be losses to the actual participants, aggregate output may be unchanged either because these losses are small, or because there is substitution in production between firms and/or industries. Therefore the first objective of this chapter is to assess the effects of strikes on output in Britain at a higher level of disaggregation than has previously been employed.

The consequences of strike activity have also received little attention to date. Apart from a series of aggregate time-series studies investigating the implications of strike activity for inflation (with strikes typically being used as a proxy of union power)[2], there has been little evaluation of the direct impact of strikes on economic performance. The more recent analyses of the effects of unions have typically not been able to identify the channels through which unionism has its claimed impact on performance. However, without such information, the findings of union productivity differentials, for example, cannot be attributed to any particular functional mechanisms and hence competing theories cannot be adequately discriminated[3]. A positive productivity differential may be the result of unionised establishments hiring higher quality workers within a traditional monopoly union model to offset the higher

2. These include Godfrey (1971), Taylor (1972), Purdy & Zis (1973), Knight (1972) while Ward & Zis (1974) provide an international comparison.

3. In a similar vein, a recent paper by Cable & Machin (1991) presents evidence to suggest that the negative impact of unions on profitability, as shown by Machin (1989) and Machin & Stewart (1990) for example, is no more than a further manifestation of the positive union wage effect.

wages they face. However, such an observation is also consistent with improved morale and motivation among the workforce together with a decreased quit rate if the 'collective voice' or 'institutional response' view of unions is the appropriate model.

One easily identifiable manifestation of unionisation is collective industrial sanctions. Their effect on output and productivity will differ according to which of the competing models of unions is appropriate and thus strikes can perhaps be used as an indicator to discriminate between the theories. In a monopoly union model, such actions will reflect poor relationships between management and workers and lead to low workforce morale, little cooperation at the workplace, and will therefore adversely affect the performance of the enterprise. However, under the Harvard school view of unions as beneficial institutions, to the extent that conflict activity reflects the expression of (collective) 'voice' rather than (individual) 'exit' such as shirking and absenteeism, it can actually lead to improvements in productivity by providing an outlet for frustration and discontentment and by grievances being identified and thus more probably settled. Clearly, discrimination between these two views is an empirical issue.

A further aspect of economic performance which has yet to receive adequate attention in the literature is the impact of unions on efficiency. While there have been several recent studies examining the consequences of unionisation for productivity, productivity growth, profitability, employment and investment, the issue of the efficiency in which factors of production are utilised (sometimes called total factor productivity or TFP) has yet to be addressed. Once again, it is important to postulate the route by which unions might have an impact. While union presence *per se* might be fairly unimportant as far as efficiency is concerned, the impact of <u>active</u> unionisation in the form of overt conflict activity may be the significant factor. Thus, the second empirical section of the chapter therefore concentrates on the possible role of strike activity in the determination of (technical) efficiency[4], having corrected for differences in union presence.

The remainder of the chapter is structured as follows. The next section briefly reviews the relevant extant literature, and in particular, notes the recent work examining the impact of unions on economic performance in Britain. Section 6.3 then outlines an appropriate model in which the consequences of strike activity for both output and efficiency can be explicitly investigated. The data to be utilised in the chapter is also described in this section. The empirical results are detailed in sections 6.4, 6.5 and 6.6, while the final section presents some brief conclusions.

6.2. The Economic Impact of Unions: A Brief Synopsis of the Evidence.

The traditional monopoly view of unions as argued by Simons (1944) and Hayek (1959) is that they simply raise wages above their competitive level and thus cause a misallocation of resources with consequent reduction in economic efficiency. This has been forcefully challenged by the alternative view of unionism grounded in Hirschman's (1970) exit-voice paradigm and most frequently associated with Freeman and Medoff's (1984) seminal work "What do Unions Do?". The latter argue that unions can have a number of beneficial effects which may possibly outweigh the adverse effects of the wage premium and consequent reallocation of resources that unionisation implies. The relevant theoretical considerations are outlined in sub-section 6.2.1 while the empirical evidence for Britain is examined in sub-section 6.2.2.

6.2.1. Theoretical Considerations.

The orthodox view considers unions to be detrimental institutions for a variety of reasons. Most obviously, the higher wages they extract through their monopolistic

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4. Throughout this chapter, by 'efficiency' is meant *relative* efficiency.

power causes firms to substitute capital for labour and hire higher quality workers. Higher productivity may be observed in unionised firms as a result, but this is obviously not a consequence of higher productive effort by unionised workers. Unions also lower employment since the wage-employment outcome is further up the labour demand curve[5]. The misallocation of resources they thereby induce has an unfavourable effect on welfare. In addition, unions are sometimes argued to reduce managerial efficiency through the introduction of restrictive practices. They may also limit managerial flexibility though seniority rules, and may oppose technological change thus adversely affecting economic efficiency.

The alternative view of unions, while acknowledging the wage differential accruing to unionised workers, emphasises a number of directions in which unions can have beneficial effects. In particular, in order to remain competitive, management will be 'shocked' into reducing organisational slack and technical inefficiency by the presence of unions[6]. Unions thereby perform a monitoring role for the owners of capital. Additionally, the public goods dimension of the workplace necessitates collective organisation which will be unionism in the majority of cases. This is discussed at length by Hirsch & Addison (1986). The union provides a means of expressing discontent without quitting and thus unionism can decrease exit behaviour. The lower labour turnover, absenteeism and shirking that unionism can imply will increase productivity. Furthermore, the presence of unions can lead to better communications and can encourage cooperation and improve morale with further gains for productive efficiency. Note, however, that such gains depend crucially on management's response to collective workforce organisations and collective bargaining. Ultimately,

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[5]. This assumes that the outcome is on the labour demand curve, which if unions set wages and employers then set employment, will be optimal. However, such an outcome is Pareto inefficient as shown by McDonald & Solow (1981). If unions and employers bargain simultaneously over both wages and employment, efficient contracts are no longer on the labour demand schedule, and can yield both higher wages and employment in unionised firms.

[6]. The higher X-efficiency in unionised firms presumes pre-union or non-union X-inefficiency however, and this may not be appropriate.

the impact of unions on productivity and other aspects of economic performance is thus an empirical issue.

6.2.2. Empirical Evidence.

Much of the evidence pertaining to the two views of unionism described above has been for the US and it is only comparatively recently that studies have examined the economic impact of unions in Britain. The seminal American study is that of Brown & Medoff (1978) who estimate an augmented Cobb-Douglas production function, distinguishing between union and non-union labour. This may be written as

$$Y = AK^{\alpha}(L_n+cL_u)^{1-\alpha} \tag{6.1}$$

where Y is output, K is capital, L_n and L_u are non-union and union labour respectively, A is a constant of proportionality, and α and $(1-\alpha)$ are the elasticities of capital and labour respectively. The parameter c reflects productivity differences between union and non-union labour; if $c > 1$ $(c < 1)$, then union labour is more (less) productive as suggested by the Harvard (monopoly) view of unions. Rearrangement and manipulation of equation (6.1) yields

$$ln(Y/L) \approx lnA + \alpha lnK/L + (1-\alpha)(c-1)D \tag{6.2}$$

where $D = L_u/L$ is union density. This provides the estimating equation once suitably augmented by a vector of control variables to correct for the other differences between union and non-union establishments[7]. The coefficient on D is the (log) productivity differential of unionised firms, and thus the union *labour* productivity effect is calculated by dividing this coefficient by $(1-\alpha)$.

Similar studies to that of Brown & Medoff include Allen (1984, 1986a,b), Clark (1980a,b, 1984), Ichniowski (1984a,b, 1986), Maki (1983), Noam (1983) and Warren (1985) and many of these are reviewed by Hirsch & Addison (1986) and Machin (1986). Several of the studies lend at least some support to the view that unions can

[7]. The assumption of constant returns to scale in equations (6.1) and (6.2) can be relaxed by simply including *ln*L as a measure of size.

enhance the productive process, although others dissent from this view (e.g. Bemmels (1987)). However, most fail to identify precisely the mechanism that links unionism to increases (or decreases) in productivity[8], and none can adequately account for the impact of unions on managerial efficiency. The most recent US evidence is reviewed by Addison & Hirsch (1989) who report that these newer studies collectively indicate that unions inhibit both investment and productivity growth. They also suggest that their impact on productivity levels throws some doubt on the Harvard school view.

The direct aggregate costs of unions in terms of conflict activity are easily calculated from the statistics for days lost, but as argued above, the real costs of strike activity to the participants will depend on the extent of substitution in production between firms and industries. Neumann & Reder (1984) present the only analysis of the direct impact of strikes on output. Their study covers 63 industry groups in US manufacturing, and using vector autoregression techniques (as described by Sims (1972) for example), they demonstrate that in 38 of these industries, strikes had no net effect on output while in a further 6 industries, strikes actually increased output. For the 19 industries in which strikes had a significantly negative effect on output, this was shown to be very small.

For Britain, evidence is more scarce. Pencavel (1977) presents a historical study of the coalmining industry while the work of Caves (1980), and Davies & Caves (1987) are similar comparative studies of British and American productivity and produce conflicting conclusions regarding the impact of unions. Knight (1989) presents an analysis using a cross-section of industries for the historically important period around 1968. He finds that the impact of strikes on productivity is weakly positive in a majority of industries in his sample (supporting the alternative view of unions) but also finds negative effects in a significant minority of industries. Machin's (1988)

8. One exception is Brown & Medoff (1978) who attribute 20% of their positive differential to the lower quit rate in unionised establishments.

study of 52 British engineering firms is also cross-sectional in nature due to the structural characteristics of the firms only being recorded in 1982. He concludes that the overall effect of unionisation is insignificant, although significantly negative in large firms (especially those with closed shops) perhaps resulting from increased X-inefficiency or the greater power of unions in such firms. However, his study is specific to a single industry which has a number of unique characteristics and thus it seems unlikely his results can be safely generalised to other industrial sectors.

While the precise causes and mechanisms of the celebrated growth in productivity (in manufacturing in particular) in the 1980s are not clear, many hypotheses have been advanced to explain the phenomenon. Popular and media opinion has often attributed Britain's past weak industrial performance to poor industrial relations, high rates of strike activity and an over-powerful trades union movement. According to this school of thought, recent increases in productivity result from management regaining control of the productive process. Metcalf (1988, 1989) reviews a number of other studies as well as those cited above and suggests that this literature enables the adverse effects of unions on performance, the economy and on welfare to be stated with confidence. His conclusion is that a combination of fear, competition and the decentralisation of collective bargaining has led to major changes in the industrial relations climate and this is the primary factor responsible for the reversal of Britain's poor productivity record.

This conclusion is challenged forcefully by Nolan & Marginson (1990) who argue that the alternative view cannot be dismissed on the weight of current evidence. In particular, they argue that unions still may be a source of dynamism and greater productivity. Daniel (1987), for example, concludes that unions have not inhibited the introduction of new technology in general (although there are obvious exceptions, notably printing). Even Metcalf (1988, p.21) admits that, despite there being greater opportunities to increase productivity in highly unionised sectors under the new industrial relations climate, productivity has in fact grown just as fast in industries where union density is low. Thus the evidence to date is not at all conclusive, and certainly is not as clear cut as Metcalf implies.

A review of the most recent studies of the effects of unions on economic performance in Britain has been presented by Wadhwani (1990). His summary is based upon his joint work reported in Machin & Wadhwani (1989), Wadhwani & Wall (1989) and Nickell *et al* (1989). The first study is based on the 1984 Workplace Industrial Relations Survey (Millward & Stevens (1986)). The results indicate that managerial discretion is restricted in larger, especially unionised establishments. This confirms some traditional notions of the effects of unions although the same study also shows that organisational change[9] was more prevalent in unionised than in non-unionised establishments. However, this latter result may be a consequence of the introduction of legislation to inhibit the power of unions to resist such change together with the removal of restrictive practices over the relevant sample period.

The impact of unions on investment is also examined using this data source. Having controlled for differences between establishments by including variables capturing financial performance, demand, and the extent of organisational change, Machin & Wadhwani (1989) find that there is no association between unions and investment in new technology. Daniel's (1987) finding that unionised establishments invested more is thus attributed to the removal of restrictive practices in the early 1980s and differences between industries together with his failure to control for the fact that unionised establishments experienced faster TFP growth (and thus invested more) over the period. Finally, the authors find that unionisation does not depress employment in those establishments which did not experience organisational change, while for those in which change was evident, this did lead to reductions in

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9. Organisational change is defined as 'Substantial changes in work organisation or work practices not involving new plant, machinery or equipment'.

employment principally through the elimination of restrictive practices.

The other two papers referenced above use company accounts data covering large manufacturing firms over the period 1972-1986 supplemented by two questionnaire surveys. The results show that unions do not consistently reduce productivity growth, contrary to most of the US findings. During 1980-1984, unionised firms actually experienced faster productivity growth, while by 1985-1986, there was no differential between union and non-union firms. If this is again argued to be a consequence of the introduction of 'anti-union' legislation in the 1980s leading to a boost in productivity growth, there is an important asymmetry which remains unexplained. For the 1970s, given the rather more favourable union position which existed for much of the decade, together with the Labour government's generally cooperative stance towards unions for much of the period, there should be evidence of correspondingly poor economic performance in unionised firms and industries. However, there is no such evidence of a negative productivity growth effect for the 1970s and hence such interpretations do not seem to be supported by these data.

There is also no evidence that unions affect investment in these company accounts data; although this was higher for 1980-1984 (as in the Workplace Industrial Relations Survey data), investment in unionised firms may have grown more quickly following the recession in the early 1980s which hit unionised firms relatively hard. Finally, these data give no evidence for a relationship between unionism and employment growth over the period covered.

On the basis of this evidence, Wadhwani (1990) concludes that some of the previous criticism of the union movement has been inappropriate. However, one weakness of these studies is that they typically fail to identify the instruments by which unions may have an impact on economic performance. The sequence of papers jointly authored by Wadhwani hypothesise several likely vehicles but data limitations prevent further investigation. However, until such transmission mechanisms from unionisation to performance are identified, the finding of no net effect of unionism on economic performance is not an adequate test of either the monopoly or Harvard view of unions. This chapter investigates whether overt action by unions is the key to unravelling any union impact on productivity and efficiency. A suitable framework for such an investigation is outlined in the next section.

6.3. Modelling Strategy and Data Description.

Sub-section 6.3.1 presents an extension to the conventional framework in which estimates of the impact of unions on economic performance have been traditionally conducted. The approach adopted here is arguably more appropriate and provides for greater flexibility. In particular, it exploits the dual time-series cross-section dimensions of the data which are described in sub-section 6.3.2.

6.3.1. Frontier Production Frontiers and Efficiency.

Conventionally, and as in the seminal work of Brown & Medoff (1978) and described in sub-section 6.2.2 above, a stochastic Cobb-Douglas production function has been considered the most appropriate framework for the analysis of the impact of unions on productivity. Reiterating, if industry output, Y, is a function of k factor inputs X_j, $j = 1,\ldots,k$, then this can be written as

$$Y = A\Pi_j X^{\beta} e^{\varepsilon} \tag{6.3}$$

where e^{ε} is the stochastic (multiplicative) error component. Union and non-union labour are then distinguished and the production function is augmented by a vector of controls to account for other differences between industries[10]. Estimation then reveals whether the mean production frontier for unionised industries is inside or outside that of non-unionised industries according to the sign of the coefficient on the union

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10. The description is in terms of 'industries' since this is the unit of observation for the current study. However, as noted above, some of the previous studies have been at the firm level and Warren (1985) is a time series study.

variable.

Here, however, a more tenable version of this model is utilised which takes into account the 'maximal' nature of production functions. Whereas conventional regression techniques applied to equation (6.3) estimates the <u>mean</u> output as a function of the various inputs, in the literature describing frontier production functions, it is the relevance of the <u>maximum</u> possible output that is emphasised. This permits consideration of more interesting economic questions, particularly with respect to efficiency[11]. More importantly, the concept of a frontier production function is clearly also in accord with the theoretical definition of a production function as the maximum of the distribution of output.

The development of frontier production functions has a long history, originating in the initial specification of the problem by Farrell (1957), through the computation of deterministic frontiers (Afrait (1972), Aigner & Chu (1968), Richmond (1974)), to probabilistic frontiers as advocated by Timmer (1971), and, more recently, stochastic frontier models, for which the seminal papers are Aigner *et al* (1977) and Meeusen & van den Broeck (1977). Useful surveys of the relevant literature are presented by Forsund *et al* (1980) and Schmidt (1985).

Following Aigner *et al* (1977), the basic model for the Cobb-Douglas case can be written as follows

$$y_i \leq \alpha + \beta' x_i + v_i \tag{6.4}$$

where $i = 1,\ldots,N$ indexes industries, output $y_i = ln Y_i$ and the k-vector of inputs $x_i = ln X_i$ are in logarithms (so that the β_js are elasticities), $\alpha = ln A$ and v_i represents a random (stochastic) error term. Equation (6.4) derives from equation (6.3) taking

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11. Note that the dual of production maximisation is cost minimisation which can (and should) obviously be treated in an analogous fashion. There have been a number of applications of the framework described below in the context of cost minimisation. These include Schmidt & Lovell (1979, 1980).

account of the fact that output Y is constrained from above by the efficiency in resource utilisation. Letting $u_i \geq 0$ represent the shortfall of output from the frontier, then equation (6.4) can be rewritten as

$$y_i = \alpha + \beta' x_i + v_i - u_i \tag{6.5}$$

Thus the disturbance term $\varepsilon_i = v_i - u_i$ has two parts; a stochastic component reflecting the general randomness inherent in economic data ('noise'), and a second element representing technical inefficiency in the utilisation of the inputs[12]. This represents a generalisation of the traditional production frontier models; if $\sigma_v^2 = 0$ then equation (6.5) is a deterministic frontier as utilised by Aigner & Chu (1968), whereas if $\sigma_u^2 = 0$ then (6.5) becomes a stochastic production function model as first specified by Zellner *et al* (1966) and utilised in many of the previous studies reviewed in sub-section 6.2.2 above.

Estimation of the stochastic frontier production function (6.4) for a single cross-section of industries requires the explicit specification of distributions for v and u. Typically, the v_i are assumed to be NID$(0,\sigma_v^2)$ and independent of $u_i \geq 0$, while the inputs X_j are assumed exogenous, perhaps following the behavioural justification suggested by Zellner *et al* (1966) of expected profit maximisation. Several distributions for u have been considered in the literature, although by far the most commonly employed are the positive half normal and the exponential[13]. Stevenson (1980) disputes the merits of these however since they both have modes of zero; he argues that this is tantamount to assuming that the likelihood of efficient behaviour declines monotonically with increasing levels of inefficiency[14]. Clearly this need not necessarily be the case, and thus he suggests using direct generalisations of the half

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12. Note that the estimation of production frontiers of this kind can only yield information on *technical* efficiency (as opposed to *allocative* efficiency) since it only uses data on input quantities and not on input prices.

13. Other possibilities include gamma (Richmond (1974)) and log-normal (Greene (1980a)).

14. That is, most industries are very efficient, and the number at increasing 'distance' from the production frontier declines monotonically.

normal and the exponential (i.e. truncated normal and gamma respectively) and then explicitly testing for a zero mode of the distribution.

Whatever distributions are chosen, given parameter estimates β, the technical efficiency of industry i is defined as

$$exp(-u_i) = Y_i / A\Pi_j X_i^{\beta} exp(v_i) \tag{6.6}$$

since $A\Pi_j X_i^{\beta} exp(v_i)$ represents the stochastic Cobb-Douglas production level. At the individual industry level however, estimates are only obtained for the total error term $\varepsilon_i = v_i - u_i$ and the separation of ε_i into its constituent components is not immediately apparent. The *average* technical inefficiency can be simply calculated from the moments of the distributions and this estimate will be consistent[15]. However, to compare efficiencies across individuals requires an estimate of $E(u_i)$; Jondrow *et al* (1982) suggest using the mean or mode of the conditional distribution $E(u_i \mid \varepsilon_i)$ and give explicit formulae for both the half normal and exponential cases[16]. Of course this cannot provide a consistent estimate of u_i since the variability of v_i is still present.

Much of the criticism surrounding previous single cross-section estimates of frontier production functions has been concerned with the strength of the distributional assumptions that must be made for the parameters of the model to be identified, particularly with regard to the measurement of technical efficiency. Although tests of these assumptions have been proposed in the literature (see, for example, Lee (1983a) and Schmidt & Lin (1984)), these are often very complex. In addition, although a more flexible specification for the distribution of u has been advocated by a number of authors as noted above, they still require u to be independent of v and this may be

15. For the half normal, this is given by $E(exp(-u)) = 2exp(\sigma_u^2/2)[1-\Phi(\sigma_u)]$ where $\Phi(.)$ is the standard normal distribution function, whereas if u is exponential with parameter θ, then it is easily seen that $E(exp(-u)) = (1 + \theta)^{-1}$.

16. Waldman (1984) considers two alternative unbiased linear estimators of $E(u_i)$ and compares them with the conditional expectation suggested by Jondrow *et al*. He demonstrates that although the conditional expectation estimator is to be preferred (given that it utilises information about the form of the distribution function), its advantage is very small over his much simpler linear estimators. All three estimators considered by Waldman (1984) give the same ranking of individual level efficiency for his sample.

inappropriate as argued below. Furthermore, it is apparent that alternative distributional assumptions about the efficiency disturbance term can lead to rather different results in empirical applications, especially for the estimates of inefficiency. Fortunately, such strong assumptions are not required when a time-series of cross-sections is available, and the use of a panel of industries in the current chapter represents an advance on almost all previous studies. Only Schmidt & Sickles (1984) have formulated a similar model, while Schmidt (1985, pp.312-315) reiterates some of the findings from this previous joint work.

Before describing in detail the new framework and its inherent advantages, it should first be noted that the important question is whether some component of the industry's level of inefficiency can be regarded as constant over time. If not, and thus inefficiency is considered to be both independent over time *and* across industries, then the panel nature of the data is irrelevant and the appropriate estimation methodology is that presented above in the context of a single cross-section. However, if at least part of the industry inefficiency can be considered time invariant, then the use of a panel of data can yield substantial advantages over a single cross-section.

The assumption of time-invariant technical inefficiency is clearly a very strong hypothesis, and is required for the identification of the model as will be seen below. However, following recent work by Kumbhakar (1990) and Cornwell *et al* (1990), this assumption can be partially relaxed by allowing the degree of inefficiency in each individual industry to vary over some flexible function of time whose parameterisation depends on the industry. Thus the sensitivity of the results to the assumption of time-invariant inefficiency can be investigated and this is the approach that is considered below.

There are three principal benefits accruing to panel data in the context of frontier production functions. Following the notation of Schmidt (1985), consider the following extension of equation (6.5), where again i indexes industries, and now additionally, t indexes time. The production frontier can be written

$$y_{it} = \alpha + \beta' x_{it} + v_{it} - u_i \tag{6.7}$$

for $i = 1,...,N$, $t = 1,...,T$ and with $u_i \geq 0$. This is again a simple Cobb-Douglas production frontier, with 'noise' v_{it} and technical inefficiency u_i that is constant over time but varies across industries. Defining $\delta_i = \alpha - u_i$, then equation (6.7) becomes

$$y_{it} = \delta_i + \beta' x_{it} + v_{it} \tag{6.8}$$

and this can be seen to be a standard fixed-effects panel model (since u_i is non-stochastic), with industry fixed-effects δ_i[17]. Unlike for the single cross-section model as in equation (6.4), no specific distributional specification of either of the disturbance terms in equation (6.8) is necessary to obtain consistent estimation of the βs and the δ_is[18].

The second problem that panel frontier production functions can alleviate is that associated with the assumption that inefficiency and factor input levels are independent. This may be unrealistic, since

> "... if a firm can foresee its level of technical inefficiency at all, it must be expected to affect its decisions." (Schmidt (1985, p.314)).

Any correlation between the regressors X and the level of technical inefficiency u will imply that the standard estimates of equation (6.5) are inappropriate. However, the fixed-effects model does not require this assumption of independence and, given the potential endogeneity illustrated by the above quotation, this is clearly an important advantage of estimates based on panel data over those based on single cross-sections.

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17. References to the specification and estimation of panel models include Chamberlain (1984), Hsiao (1985, 1986) and Mundlak (1978).

18. Clearly it is still possible within this framework to make specific assumptions about the distribution of v_{it} and u_i, and Pitt & Lee (1981) use the normal and half normal combination as previously utilised in many of the single cross-section studies. If these distributional assumptions are valid, then the estimates obtained will be more efficient as compared to simply estimating equation (6.8) as a fixed-effects model. Hence the assumptions can be tested explicitly by comparing the difference between the estimators in the usual fashion (see, for example, Hausman (1978) and Ruud (1984)).

Finally, panel models can more easily identify at the level of the individual that component of the disturbance which is technical inefficiency, separate from that which is noise[19]. Although proposals have been made in the single cross-section equation to identify $E(u_i)$, or more preferably $E(u_i | v_i - u_i)$, these estimates are still contaminated by the noise v and are again heavily dependent on the distributional assumptions made. In particular, the level of individual technical inefficiency cannot be consistently estimated in a single cross-section (see Jondrow *et al* (1982)). With a panel of data, this deficiency is resolved essentially because the noise is being averaged in the overall residual. This is clearly not possible in the one dimensional case but in the panel becomes feasible because u_i is observed T times rather than only once[20]. At the most basic level, technical inefficiency can then be identified from equation (6.8) as

$$u_i = D - \delta_i \quad \text{where} \quad D = \max(\delta_i) \tag{6.9}$$

and this will yield consistent estimates provided N is large enough (see, Schmidt (1985, p.314), and for the single cross-section case with deterministic frontiers, Gabrielson (1975) and Greene (1980a))[21].

Much of commentary above has been with reference to the fixed-effects model. One disadvantage with this estimator is that it cannot accommodate regressors which are time invariant while varying across industries[22]. Any industry specific heterogeneity is thus labelled as inefficiency and this interpretation clearly may not be appropriate; there are clearly likely to be other unobserved differences between industries which

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19. Note that the identification is achieved by virtue of the assumption that the u_i are fixed over time.

20. Note that T must be large enough for the estimation of the δ_i to be precise.

21. This is equivalent to counting one industry as 100% efficient. Schmidt & Sickles (1984, p.368) provide a rationalisation for this, similar to that of Greene (1980a) in the single cross-section case.

22. Alternatively, the Hausman & Taylor (1981) estimator allows for only some of the regressors to be correlated with the effects and thus is a hybrid of the fixed- and random-effects specifications. Again, consistent estimation of the individual effects requires T large (for separation from the residuals) and N large (for separation from the intercept), conditions identical to those required under the random effects model.

affect productivity and which cannot be included in the estimating equation. The problem could be avoided by assuming that the u_i are uncorrelated with the regressors and can be treated as random across industries. Then equation (6.7) can be estimated as a random effects model by generalised least squares. For the panel utilised in this study (see sub-section 6.3.2 below), the fact that N is large and T is small will mean that such an assumption will yield estimators which are more efficient than in the fixed-effects specification, although, as demonstrated by Schmidt & Sickles (1984, p.369), consistent estimation of industry level technical inefficiency still requires both N and T large just as for the fixed-effects specification.

Thus the main advantage of the random effects (GLS) specification is that it can accommodate time-invariant regressors. The assumption that the u_i are uncorrelated with the regressors (and thus the random effects model is appropriate) can of course be tested against the fixed-effects specification which does not make this presumption. However, as already noted, it seems improbable that such an assumption will be accepted by the data used in the current study.

An alternative approach is to estimate the fixed-effects model (6.8) and recognise that the δ_is include unmeasured industry heterogeneity as well as the inefficiency of interest. The separate determination of these individual industry intercepts can then be ascertained by suitable specification of an industry level equation including controls to correct for the remaining industry heterogeneity. In this fashion, the direct impact of conflict on efficiency can also be identified. This is therefore the methodological approach adopted in this chapter.

Thus estimation takes place in two distinct stages. The first involves estimating the production frontier (6.8) suitably augmented by time-varying variables to correct for the observed differences between industries, together with various indicators of strike activity[23]. The positive (negative) sign of the coefficient on the stoppages variables indicates whether the position of the frontier for industries which are relatively strike prone is outside (inside) that for industries which incur relatively little strike action and thus reveals that strike prone industries have higher (lower) productivity. The analysis of this issue forms the basis of the results reported in section 6.4.

The individual industry intercepts $\delta_i = \alpha - u_i$ thereby obtained include unobserved industry heterogeneity together with the estimate of the inefficiency of the industry in terms of its distance from the production frontier. Hence, while equations (6.9) and subsequently (6.6) could be used to generate estimates of the absolute efficiency as $exp(-u_i)$, the preferred strategy is to interpret the δ_is as indicators of relative efficiency only. The impact of conflict activity in the determination of relative efficiency can then be identified by regressing these δ_is on a vector of industry characteristics which seem likely to be important factors in determining industry productivity and efficiency, including measures of strike activity. Section 6.5 reports the results of this exercise.

Having described at some length the advantages inherent in the frontier approach, particularly when a panel of industries is available, the next sub-section describes in detail the data to be used in this study.

6.3.2. Data Description.

The data used in this chapter are compiled from a variety of sources. Firstly data on employment, output and industrial concentration are drawn from the Census of Production for 3-digit manufacturing industries[24]. This source is supplemented by various measures of industrial stoppages taken from the Department of Employment's

23. This represents a direct extension of Knight (1989) who performs a similar analysis but with a single cross section and estimating a productivity equation.

24. I would like to thank Paul Geroski for making these data available.

Industrial Stoppages Data Tapes. Presently, these tapes are not available for 1981 or beyond 1984[25] and thus for the purposes of the thesis, it was decided to restrict attention to the 1970s for which consistent information is obtainable on the 1968 Standard Industrial Classification for both sources. This decade is important historically and forms rather a distinct period when compared to the preceding and subsequent decades. The 1970s are often claimed to have laid many of the foundations for Britain's poor economic performance and the depth of recession witnessed in the early 1980s. In addition, the 1970s began with a peak in strike frequency, and was a period in which strike activity was at historically high levels; if conflict is important in determining either productivity or efficiency, then it seems most likely to be a factor in a period when it was so prevalent.

Figure 6.1 graphs the time-series for average productivity[26] and total strike frequency for the 114 3-digit manufacturing industries for which information is available from the Census of Production for the period 1970 to 1979[27]. The general improvement in labour productivity over the early part of the decade is marred by the downturn following the 1973 oil price shock, and a full recovery to 1973 levels is only just made by the end of the decade. The trend in strikes in the manufacturing sector closely follows the trend in the series for all industries and services seen in chapter 2, and displays a decrease of over 50% from the peak year of 1970. The cross-section relationship between output and strike activity at the industry level is depicted in figure 6.2, which shows (log) aggregate productivity (scaled to have a mean of 100) against per capita strike frequency, both averaged over the decade for each industry. Clearly there is no evidence of any direct association between the two measures at

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25. There has been some recent progress with respect to the provision of the tapes for 1981 and it is hoped that data for this year and the further provision of data beyond 1984 will soon be made available.

26. This is computed as output per head deflated by the implicit price index, and indexed to 1970 = 100.

27. There are in fact 122 3-digit industries but 8 industries are not covered (or not separately identified) in the Census of Production.

this level of aggregation, although this is not the *ceteris paribus* comparison between strikes and output that is of central interest.

This panel of 114 industries over 10 years is used in section 6.4 below to investigate the impact of strikes on output and productivity at the industry level. For the evaluation of the determinants of efficiency a number of additional sources have been used to generate variables to control for other differences in industry characteristics. Data on male, female, full-time and part-time employment was derived from the June 1973 Census of Employment, while information relating to the occupational breakdown of employment and wages comes from the 1973 New Earnings Survey. These are used in section 6.5 together with industry-level summary statistics derived from the Census of Production in order to identify the determinants of industry efficiency.

6.4. Output, Productivity and Conflict Activity.[28]

The dependent variable for the stochastic production frontiers estimated in this section is (the logarithm of) net output deflated by the implicit price index[29]. Factor inputs are the capital stock (KS) and level of employment (EMP) both entered in logarithms. Full data are only available for 98 of the 114 3-digit manufacturing industries covered by the Census of Production[30]. Finally, the central question of interest, whether output is adversely affected in industries with high levels of conflict activity, is examined by including a vector of measures of strike activity. These are defined in the Appendix, table A6.2 which also includes some summary statistics.

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28. The analysis of this section of the chapter was carried out using Limdep (Greene (1990, 1991)).

29. An immediate departure from previous analyses is made at this juncture since typically a *productivity* equation is estimated. Here, emphasis is focussed on production (output) frontiers; the concept of a productivity frontier has little intuitive appeal.

30. The reasons for the 16 omissions are detailed in the Appendix to this chapter, table A6.1.

Estimates for the basic fixed-effects production frontier as specified by equation (6.8) are presented in table 6.1, column 1. The first concern is over the functional specification and this has been tested in a number of ways. Kmenta (1967) suggests a simple (approximate) test of the Cobb-Douglas specification over the more general constant elasticity of substitution (CES) formulation. To second order, the latter can be written as the basic Cobb-Douglas production function plus an additional term $(ln\text{KS} - ln\text{EMP})^2$, the coefficient of which will be zero when the elasticity of substitution is unity as in the Cobb-Douglas case (see, for example, Heathfield (1971)). A test of this hypothesis cannot be rejected by the data ($\chi^2(1) = 2.08$) and thus a Cobb-Douglas production frontier would seem to be an appropriate description of production technologies in the manufacturing sector in Britain in the 1970s. Furthermore, and as shown in the table, the hypothesis of constant returns to scale cannot be rejected either, and the relative sizes of the employment and capital elasticities do not seem unreasonable for manufacturing as a whole. Finally, a Hausman (1978) test of the random effects specification over this fixed-effects model is soundly rejected by the data. This is of little surprise; there are excluded systematic differences between industries which affect productivity and thus the assumption of independence of regressors and disturbances is unlikely to be satisfied for these data. Hence the fixed-effects model is the appropriate specification.

The estimates in column 2 incorporate time-effects to control for any systematic fixed-effects across time including economy-wide cyclical fluctuations. These are clearly collectively significant (as seen by a LR or, equivalently, an F-test against column 1). However, the estimated elasticities are fairly robust to their inclusion, and, once again, the assumption of Cobb-Douglas technology is not rejected by the data against a CES production function. Finally, the hypothesis of constant returns to scale is again not rejected.

It seems probable that the production frontier might not be stable over the whole decade given the important economic and institutional changes in the period. A full treatment of this consideration is presented at the bottom of table 6.1. The most obvious candidate for a structural break is around the oil price shock of 1973/4 and this possibility has been investigated in two ways. Firstly, to examine whether there were changes in the elasticities of factor inputs, a dummy variable taking value one for observations from 1974-1979 was constructed. For column 1, this was included along with its interactions with the capital and labour variables, while for column 2 which includes time-effects, only the interactions were included to avoid multicollinearity. These additional terms are collectively significant for both columns; the relevant F-statistic is given in the table as ELASTICITY together with its 5% critical value (CV) in parentheses immediately below. More specifically, it is apparent that the elasticity of output with respect to capital fell after 1973, while that for labour was insignificantly different from its value over the early part of the decade (as was the overall intercept in column 1 which reflects the mean efficiency in production). This may reflect the fact that a proportion of the capital stock became obsolete following the oil crisis or alternatively, may be a consequence of more general shifts in production across industries (and/or time, when considering column 2).

This latter possibility was investigated by estimating the model specified by equation (6.8) separately over 1970-73 and 1974-79 thus allowing the intercepts as well as the elasticities of factor inputs to differ. Formal (Chow (1960)) tests for structural stability are presented at the bottom of table 6.1 (CHOW) and clearly these reject the null hypothesis of parameter stability over the decade[31]. Finally, estimating separate equations over the two time periods can be compared with only allowing the elasticities to vary as in the ELASTICITY test described above. This is equivalent to

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[31]. One caveat is that this test is conditional on homoskedasticity of the disturbances across the two sample periods. However a variance-ratio test (also included in the table as V-RATIO) rejects this hypothesis and thus the conclusion with regard to structural change should be treated with caution.

holding the industry-effects (and, for column 2, time-effects) constant. This is recorded in the table as INTERCEPT and, once again, the null hypothesis of stability is resoundingly rejected by the data. (The final combination would be to allow the intercepts to vary but test whether the elasticities are constant over time. Since this would involve increasing the number of parameters in the model by over one hundred, this strategy was considered to be less efficient than the modification suggested below).

The other potential candidate for a structural break arises from previous work on the cyclicality of strikes reported in section 3.4 above. A discernable trough in detrended monthly industrial production can be identified in October 1975, and consequently the 1970s fall fairly evenly into two distinct cycles; the first from November 1971 to October 1975 with a peak in June 1973 and the second from October 1975 to March 1981 with a peak in August 1979. Thus the sample period was split at 1975/76 and a similar analysis was performed to that described above for the structural break at 1973/74. Parameter stability is again rejected by the data on all tests, and the significant change in the elasticity of output with respect to the capital stock input is still evident.

The finding of non-stability in the production function estimates is perhaps not very surprising; industrial production tends to be strongly cyclical and there were several major economic fluctuations during the decade. Moreover, not all industries will be equally affected, nor will they react to movements in the cycle at the same time. Thus it should be expected that both the industry and time fixed-effects will differ over the decade. That the elasticities also differ is of more concern since it suggests that there were fundamental changes in the production technology over the period of interest. However, it should be noted that the Cobb-Douglas specification is not rejected against the CES formulation for any of the sub-periods considered, and that, despite the change in the elasticity of capital (which, although significant, is typically less than a 5% fall in numerical value); constant returns to scale is never rejected in any sub-period.

One possible correction to minimise the effects of any (cyclical) instability in the parameters of the production function is to control for cyclical changes at the industry level. A cyclical variable (CYCLE) was constructed as the rate of growth of gross output (or equivalently, the change in the logarithm of the index of production), and included the estimating equation[32]. The results are reported in column 3 of table 6.1; clearly CYCLE is strongly significant although the magnitudes of the elasticities are reasonably robust to the inclusion of this extra variable. However, the tests for structural change reported at the bottom of the table still indicate structural instability for the factor elasticities and the intercepts across both of the sample divisions investigated above. In particular, there is evidence of a statistically significant fall in the capital elasticity in the second sub-period (although this is again of negligible magnitude). The hypothesis of a constant returns to scale Cobb-Douglas production function cannot be rejected however, and this provides further support for the basic functional specification selected and represented by the estimates in column 3.

The principal purpose of this section of the chapter is to examine the effect of strikes on output. Column 4 of table 6.1 includes a variable recording the number of strikes in each industry in each year (STRIKE), scaled by a factor of 10^{-3} to facilitate presentation of the results. This term is insignificant, and remains so when a non-linear formulation is considered as in column 5 which includes a quadratic term ($STRIKE^2$). A similar finding is obtained in a crude dynamic specification presented in column 6 which additionally includes the level of strike activity in the previous year ($STRIKE_{-1}$). Thus, at first sight, that these measures of strikes are never separately or collectively significant would indicate that there is no discernable net

32. Note that this should not be endogenous; the dependent variable is (the log of) *net* output, suitably deflated. Obviously, one year of data is 'lost' in the construction of this variable.

effect of strikes on output in British manufacturing industry[33]. This finding is unaffected by whether or not the time-effects are included in the specification, and thus only the estimates which include the time fixed-effects (which are always collectively significant) are reported in table 6.1. The production frontier parameters are robust to the inclusion of the strikes variables and the hypothesis of constant returns to scale still cannot be rejected by the data.

It is of some interest to consider the trend in the mean growth of TFP throughout the period under consideration. This can be investigated in two ways; firstly by examining the time fixed-effects in a two-way model as considered above, and secondly, in one-way model, to include an appropriate polynomial in time and calculate the derivative at the median time period, say. For the specification in table 6.1, column 4, the estimated time-effects for each year are as follows:

1971	1972	1973	1974	1975	1976	1977	1978	1979
-0.0184	0.0289	0.0554	0.0173	0.0031	-0.0240	-0.0549	-0.0222	0.0150

One problem with the interpretation of these coefficients is that they are constrained to sum to zero (to avoid perfect multicollinearity with the industry fixed-effects). Thus, only 'relative' TFP growth rather than an 'absolute' growth interpretation is really possible. It can be seen that, in general, TFP was increasing in the early 1970s (relative to the mean), but that its rate of growth fell from 1973, and was negative from 1976 to 1978 inclusive. This accords roughly with what might be expected given the 1973 oil shock and other major trends in economic activity in the period.

The alternative method of estimating TFP growth is to omit the time fixed-effects and include a quadratic in time. The coefficient estimates for the remaining variables in the model are fairly robust to this strategy and are therefore not reported. The

33. However, this result may be due to substitution in production between firms as discussed in section 6.2, and thus there may still be significant effects at the plant or establishment level. However, such effects cannot be identified with the data utilised here.

coefficients on the quadratic yield a time-path for TFP which is monotonically decreasing for the period 1971 to 1979, and is almost linear. The median time is t = 1975, and this yields an average TFP growth for production industries through the 1970s of —0.42%, i.e. just under one half of one percentage point per year. The range is from —0.32% in 1971 to —0.54% in 1979. Note, however, that the coefficients on the time variables are rather poorly determined, and thus these estimates should be treated with some caution. In particular, the inverted U-shape temporal pattern in TFP growth evident in the time-effects presented above is not apparent, and a test of the restrictions implied by the quadratic against the freely estimated time-effects as above is soundly rejected by the data ($\chi^2(7) = 69.46$). Thus the two-way model remains the favoured specification.

As discussed in chapter 2, strike frequency is but one dimension of strike activity. With regard to their impact on industrial production, the number of strikes as considered above may not be the relevant measure of conflict activity. Thus table 6.2 presents estimates of the cyclically adjusted production frontier augmented by grouped measures of strike frequency disaggregated by the number of workers involved (SIZE), by strike duration (DUR), and by the number of days lost (DLOST). These variables are grouped because the exact size, duration and days lost for each strike are not recorded on the Department of Employment Industrial Stoppages Data Tapes, but are categorised into several (typically 10-12) divisions. The variables included are broad amalgamations of these classes to reduce the sparseness of the data matrix when disaggregated by 3-digit industries. Hence, for each dimension of strikes, three categories have been chosen for inclusion (Small, Medium, and Large, or Short, Medium and Long, arbitrarily defined), the value that each takes therefore being the number of strikes of that size, duration or days lost which were recorded for each industry in each year. A full description of the variables is provided in the Appendix, table A6.2.

The results from incorporating these three additional dimensions of strikes are reported in table 6.2, columns 1 to 3. It can be seen that the inclusion of measures of strike size, duration and days lost are collectively insignificant in every case when compared to table 6.1, column 3[34,35]. Each of the specifications considered is data acceptable against the more general alternative as represented by the CES production frontier, and the finding of constant returns to scale is robust to the addition of these extra variables. Finally, the random effects model is also rejected against the fixed-effects estimates presented in the table. Of course, the insignificance of these additional strike dimensions is consistent with both a negligible impact of conflict at the firm level (and in aggregate at the industry level), or large disruptions at the firm level which are 'smoothed out' by other firms in the same industry through substitutions in production.

This second possibility is investigated in the following manner. If strikes are prevalent in the general industrial category to which the 3-digit industry belongs, then the opportunities for substitution in production may be curtailed. Furthermore, there may also be indirect negative spillover effects due to interruptions elsewhere in the production chain. Thus a variable recording the number of strikes elsewhere in the 2-digit industry group to which the 3-digit industry belongs was constructed (SPILL) and included in the production frontier specifications. The results are reported in columns 4 to 6 of table 6.2. As can be clearly seen, SPILL is never significant at conventional levels, either in current or lagged form, with or without the inclusion of lagged strike frequency in the 3-digit industry. In addition, the position of the production frontier is hardly changed by these alternative specifications of the vector

34. Although a high frequency of short duration strikes (DUR_S) has a significantly negative impact on output, when considered together with the other measures of duration, they are again collectively insignificant ($\chi^2(3) = 6.72$).

35. In addition, disaggregating the total number of strikes by cause into pay and non-pay strikes had little effect; neither variable was individually significant, and collectively their inclusion was easily rejected by the data in a variable addition test.

of measures of strikes. One obvious weakness here is the omission of some proxy for the degree of competition in the industry.

The strike variables used in the specifications above are all aggregate measures of strike frequency. As argued in chapter 4 in particular, this may not be the appropriate criterion and the level of strike incidence may be more suitable. In particular, the number of strikes may be relatively unimportant for output if each of these strikes affects relatively few workers; a more relevant measure of the level of conflict may be the number of strikes per employee. Thus for each of the specifications reported in tables 6.1 and 6.2 which include measures of strikes, the model was reestimated using strike incidence rather than strike frequency. The results of this exercise are reported in table 6.3. The only column for which the strikes variables are significant is column 5 which disaggregates the number of strikes according to their durations. Those industries which incur a large number of short strikes (of 1 day or less) per employee (DUR_S) have their output significantly reduced. This result seems intuitively plausible; frequent short interruptions to production are likely to be disruptive and may indicate generally poor industrial relations within the industry. Moreover, if startup costs are high, short strikes may be as costly for output as more lengthy disputes. The mean of DUR_S is small however, and thus the magnitude of this effect is fairly negligible.

Taking these findings together with the results presented in tables 6.1 and 6.2 for strike frequency, the central conclusion of this section of the chapter is that strikes have little or no net impact on output at the 3-digit industry level. Whether they affect the efficiency at which each individual industry operates is investigated in the next section.

6.5. Efficiency and Conflict Activity.[36]

The industry intercepts from the production frontier estimates presented in section 6.4 can be recovered from equation (6.8) as

$$\delta_i = \sum_t (y_{it} - \beta' x_{it})/T, \quad i = 1,\ldots,N \tag{6.10}$$

These δ_is represent the unobserved differences between industries together with a measure of their inefficiency in the utilisation of factor inputs as the 'distance' u_i inside the production frontier. If industries were homogeneous in terms of the quality of labour and capital inputs, the degree of competition they faced, the scale of production and so on, then the δ_is would be pure inefficiency parameters, and the level of (technical) efficiency could be calculated using equations (6.9) and (6.6). However, there are clearly industry level differences which have not been accounted for in the simple production frontier estimates presented in section 6.4 and hence these need to be adequately controlled for before an investigation of the relative efficiency of each industry can be conducted. While a wide variety of industry characteristics have been utilised in the specifications reported below to correct for the unobserved heterogeneity, it should be noted that some of these factors will also be important determinants of efficiency. However the primary objective of this section is the investigation of the *ceteris paribus* impact of the level of conflict activity on industry level efficiency.

The postulated relationship between conflict and efficiency can be represented as

$$\delta_i = \theta' Z_i + \gamma' S_i + w_i \tag{6.11}$$

where Z_i is a vector of industry characteristics and S_i is a vector of measures of strike activity[37], with possible interactions between the Z_i and S_i variables. Positive

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36. The analysis of this section of the chapter utilises Microfit (Pesaran & Pesaran (1991)) and Limdep (Greene (1990, 1991)).

37. Note that equation (6.9) has not first been used to derive measures of absolute efficiency from the δ_i since this would then entail estimating an equation whose dependent variable was constrained to lie in the interval (0,1]. Although this is feasible, it necessarily implies further assumptions about the distribution of the disturbance term w_i and this was considered undesirable in the present context. The model represented by equation (6.11) is, in fact, a model of *ln*(efficiency), and in this sense is not linear, but log-linear despite its apparent functional specification.

coefficients in the vectors of parameters θ and γ indicate a positive impact on (log) efficiency since each of the δ_i is negatively correlated with the industry's inefficiency.

The first set of Z variables control for various plant and company level factors. Differences in the size of plants are captured by SIZE, which measures the average size of plants above the median size, as a proportion of domestic production[38]. Its impact on efficiency is indeterminate *a priori* since returns to scale may be increasing or decreasing in large plants. Differences in the labour input between industries are measured by the proportion part-time (P_PT), the proportion female (P_FE) and the proportion of operatives (P_OPS). A more flexibly employed workforce may yield opportunities for greater efficiency in the use of factor inputs and labour hours can more easily be adjusted where part-time employment is commonplace. The proportion of operatives is included as a proxy for the degree of supervision and bureaucracy. Where most employees are engaged in the productive process, this may be because supervision is remote, or because those in management are sufficiently competent that few supervisors are needed. Thus its net impact on efficiency is indeterminate.

The second set of variables control for wider industry-level differences in performance and the degree of competition within the industry. The average (logarithm of the) capital to labour ratio (MKL) over the decade is included to capture any capital-labour substitution made in response to union wage and strike effects. The average 5-firm concentration ratio (MCONC) reflects the degree of competition in the market facing the industry and thus acts as a proxy for the pressure on the industry to perform efficiently, while AS records the advertising intensity in the industry, commonly used as a proxy for barriers to entry. Finally, a variable measuring average growth over the decade (MCYCLE) is included to capture the differences in

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38. Five industries have no data available for this variable and hence are dropped from the subsequent analysis.

behaviour that can be expected from industries which are expanding compared with those which are in decline.

The final set of variables of interest measure differences in unionisation and strike activity at the industry level. The proportion covered by collective agreements (P_CA) may be expected to have a negative impact if unions are detrimental to efficiency as suggested by much of the US literature, while if unions are a spur to greater X-efficiency, this variable should enter with a positive coefficient. The mean number of strikes over the decade (MSTR) is also hypothesised to have an effect; while in section 6.4, strikes were seen to have a negligible impact on industry output, they may still affect industry efficiency. The direction of the effect will depend crucially on which of the two alternative models of union behaviour described in section 6.2 above is most appropriate for these data.

Table 6.4 presents estimates of the model represented by equation (6.11)[39]. The dependent variable is the industry fixed-effects derived from the results in table 6.1, column 6. This represents the estimate of the remaining industry heterogeneity plus inefficiency parameter u_i, corrected for cyclical deviations and differences in the level of strike activity[40]. Column 1 controls for the various plant and company level factors which are likely to be important determinants of output and efficiency. As hypothesised above, larger plants do tend to be less efficient, while those employing more part time workers are relatively more efficient[41]. There would appear to be no net impact of the proportion of the workforce who are operatives. These results are robust to the inclusion of the second set of variables as shown in column 2, and the latter are not individually nor collectively significant at conventional levels

39. Summary statistics for the explanatory variables are presented in the Appendix, table A6.3.

40. The results reported below were little changed by utilising δ_is derived from other columns of table 6.1.

41. P_PT is highly correlated with the proportion female P_FE (r = 0.825) and so the latter was dropped from the estimating equation.

($\chi^2(4) = 4.58$). Column 3 includes the unionisation and strike variables. The estimated coefficients on these variables are statistically significant and have their expected signs. Highly unionised industries tend to be less efficient as do those with high levels of strike activity.

In order to test the hypothesis that union presence has its impact through collective action, column 4 considers interactions of the level of strikes with the proportion of the workforce covered by collective agreements and plant size. The importance of size for union productivity differentials was emphasised by Machin (1988) while the relationship between size and strike activity is well established in the preceding chapters of the thesis. While these four additional variables are not collectively significant ($\chi^2(4) = 6.74$), it can be seen that high levels of strike activity in industries with high union density serves to detract from the lower levels of efficiency that tend to result from the presence of either of these two indicators of unionism. Unions also tend to generate less negative efficiency effects in large plants (contrary to the finding of Machin (1988) for his sample of engineering firms), although these joint terms are not individually significant. These findings are consistent with the Harvard school view of unions as beneficial institutions which alleviate grievances through the 'voice' rather than the 'quit' mechanism and can shock management into achieving lower levels of X-inefficiency[42].

6.6. Time-Varying Technical Inefficiency.

In the approach considered in section 6.5 above, the estimated fixed-effect for each industry is treated as an indicator of the industry's inefficiency. However, this assumes that technical inefficiency is time-invariant and that relative inefficiencies

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42. For completeness, the model was also estimated using measures of average strike incidence (i.e. strikes per employee) rather than average strike frequency. The results are qualitatively similar to those presented in table 6.4 although they are not as statistically significant and the interactions effects between strikes and unionism are not identified.

between industries are unchanging. Both of these assumptions are far from innocuous and are clearly rather unsatisfactory. However, two recent papers have taken the panel frontier production literature a stage further by specifying models which, for the first time, allow for *time-varying* inefficiency. These are Kumbhakar (1990) and Cornwell *et al* (1990). The former depends rather heavily on distributional assumptions for technical and allocative inefficiency in a manner similar to that utilised in the single cross-section frontier literature. The latter, however, follows a similar methodology to that of Schmidt & Sickles (1984) and thus complements the results in section 6.5 above. While retaining the advantages of the panel nature of the data (in particular, once again not requiring the assumption of independence between factor inputs and inefficiency), Cornwell *et al* allow for technical inefficiency to be time-variant by specifying it as a polynomial in time. Thus output and efficiency can now vary over both industries and time. Efficiency measurement focuses on the cross-sectional variation and the model allows efficiency levels to vary over time. Conversely, the measurement of productivity growth focuses on the temporal variation, and the model allows the rate of productivity growth to vary across industries. Once again, the measures of efficiency are calculated relative to an industry which is deemed to be 100% efficient. However, in this new specification, the relatively most efficient industry can differ for each time period under consideration.

Thus while the elasticity estimates are identical to those in table 6.1 and table 6.2, the estimates of efficiency for each industry are allowed to change over time. The mechanics of this exercise are quite simple; essentially the residuals from the panel estimates for each industry are regressed on a constant, time and time-squared. The fitted values from this regression form a consistent estimate of δ_{it} that is consistent as T tends to infinity (see Cornwell *et al* (1990), p.192)). Finally, the frontier intercept at time t, D_t, is given by

$$D_t = \max(\delta_{it}) \quad \text{for } t = 1,\ldots,T \tag{6.12}$$

where maximisation takes place over $i = 1,\ldots,N$, and the industry-specific estimate of technical inefficiency for industry i at time t is

$$u_{it} = D_t \cdot \delta_{it} \tag{6.13}$$

Full details are provided by Cornwell *et al* (1990).

The results from this exercise are quite interesting. For the basic specification in column 4 of table 6.1, for almost all industries, the estimated efficiency is broadly increasing and then decreasing through the 1970s, despite the presence of the time fixed-effects[43]. Considering first the temporal variation (that is, making comparisons within industries through time), for most industries, efficiency peaks in 1973 or 1974, although there are a few exceptions in which efficiency is monotonically decreasing throughout the 1970s[44].

The industries which were assigned to be 100% efficient according to equation (6.12) form an extremely disparate group, and include SIC 216 (Sugar) for both 1973 and 1974 and SIC 366 (Electronic Computers) for 1977-1979. When compared to these industries, the average *relative* efficiency level in each year varies between 54% and 93%, with an average for the nine years of around 80%. The temporal variation in average relative efficiencies is as follows:

1971	1972	1973	1974	1975	1976	1977	1978	1979
75.05	81.93	88.95	90.75	90.78	92.62	80.74	67.07	54.48

Note that the relative peak appears in 1976. This is not inconsistent with the remark above that, when comparisons are made *within* industries, the relative peak for most industries is in 1973 or 1974. The implication is that efficiencies are most similar *between* industries in 1976 and hence relative efficiencies are greatest in this year.

43. That is, over and above the aggregate trends discussed in section 6.5, individual industries also display the same inverted U-shaped movement in TFP.

44. This finding is even more pronounced if a cubic polynomial is used to describe the time-varying inefficiency, although estimating 4 regression coefficients from only 9 observations for each industry is perhaps rather ambitious, especially given that the dependent variable is only a residual.

6.7. Summary and Conclusions.

This chapter has examined two very distinct effects of unions. Firstly, using a panel of 3-digit manufacturing industries for the 1970s, strikes are seen to have a negligible net impact on output. A crude attempt was made to investigate whether this was the result of output substitution by other firms in the industry not affected by strike action or was an indicator that strikes have little impact at the firm or establishment level. The spillover effects were not significant, although the level of aggregation used is probably rather high to identify such microeconomic effects. Indeed, while this is the most disaggregated study undertaken to date for the UK, the analysis may yet be flawed by being at too high a level of aggregation.

The second empirical part of the chapter investigates the impact of unions on economic efficiency, using strikes as a proxy for active unionisation, rather than passive unionisation as recorded by collective bargaining coverage. The impact of unions on (technical) efficiency is unambiguously negative. Firstly, there is an effect through union presence as recorded by the proportion covered by collective agreements, and secondly, through the use of strike action. These both serve to reduce the level of efficiency at which the industry operates. Interactive effects are also considered, and the detrimental impact of unions on efficiency is seen to be smaller in large unionised plants. This may possibly be an economies of scale effect. Finally, in industries where both union coverage and strike activity is high, the negative impact of unions on efficiency is again ameliorated. One possible interpretation of this finding is that only in situations in which there is overt union activity is management sufficiently 'shocked' into improving managerial slackness and X-efficiency.

In conclusion, the costs of strikes would seem to be negligible at the 3-digit level for British production industries in the 1970s. However, there is some evidence to suggest that the consequences of high levels of strike activity are experienced in terms of (technical) inefficiency and that this impact is not simply a result of high union presence at the industry level. Given the severity of the recession in the early 1980s and the changing patterns in unionisation, it will be of some considerable interest to replicate the current study for the 1980s when the data become fully available.

Table 6.1

Panel Data Estimates: Basic Production Frontier

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6
CONST	-	-0.9631 *(0.2968)*	-0.9734 *(0.3120)*	-0.9736 *(0.3122)*	-0.9584 *(0.3128)*	-0.9580 *(0.3130)*
ln(EMP)	0.5854 *(0.0474)*	0.6166 *(0.0584)*	0.5461 *(0.0597)*	0.5463 *(0.0598)*	0.5365 *(0.0609)*	0.5366 *(0.0610)*
ln(KS)	0.4066 *(0.0371)*	0.4496 *(0.0544)*	0.4967 *(0.0574)*	0.4967 *(0.0575)*	0.4993 *(0.0575)*	0.4992 *(0.0576)*
CYCLE	-	-	0.5492 *(0.0525)*	0.5491 *(0.0526)*	0.5511 *(0.0526)*	0.5509 *(0.0527)*
STRIKE *x10^{-3}*	-	-	-	-0.0722 *(0.8218)*	0.9680 *(1.4548)*	0.9620 *(1.4595)*
STRIKE^2 *x10^{-3}*	-	-	-	-	-0.0144 *(0.0166)*	-0.0142 *(0.0170)*
STRIKE$_{-1}$ *x10^{-3}*	-	-	-	-	-	-0.0419 *(0.7242)*
Time Dummies	NO	YES	YES	YES	YES	YES
Diagnostics						
N x T	98 x 10	98 x 10	98 x 9	98 x 9	98 x 9	98 x 9
*ln*L	218.337	274.188	351.097	351.098	351.603	351.605
R^2	0.8741	0.8877	0.9099	0.9099	0.9100	0.9100
Test for Constant Returns to Scale						
$\alpha + \beta$	0.9920 (0.0655)	1.0662 (0.0635)	1.0429 (0.0660)	1.0431 (0.0661)	1.0358 (0.0666)	1.0358 (0.0667)
Tests for Structural Change						
Structural Break at 1973/74						
ELASTICITY	9.15	5.38	3.93			
CV	*(2.60)*	*(3.00)*	*(3.00)*			
CHOW	5.37	4.93	3.81			
CV	*(1.24)*	*(1.24)*	*(1.24)*			
V-RATIO	1.20	1.37	1.71			
CV	*(1.19)*	*(1.19)*	*(1.23)*			
INTERCEPT	5.02	4.42	3.50			
CV	*(1.24)*	*(1.23)*	*(1.23)*			
Structural Break at 1975/76						
ELASTICITY	4.41	3.88	5.18			
CV	*(2.60)*	*(3.00)*	*(3.00)*			
CHOW	4.35	4.87	4.90			
CV	*(1.24)*	*(1.24)*	*(1.24)*			
V-RATIO	1.47	1.30	1.20			
CV	*(1.19)*	*(1.19)*	*(1.20)*			
INTERCEPT	4.21	4.41	4.39			
CV	*(1.24)*	*(1.23)*	*(1.23)*			

Notes:

1. Only fixed effects estimates are presented. The random effects specification is rejected by Hausman tests for every column in the table (results not reported).

2. Standard errors are given in italicised parentheses.

Table 6.2

Panel Data Estimates: Other Dimensions of Strike Activity

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6
CONST	-0.9962	-0.9025	-0.9704	-0.9536	-0.9518	-0.9440
	(0.3128)	*(0.3130)*	*(0.3134)*	*(0.3132)*	*(0.3135)*	*(0.3150)*
ln(EMP)	0.5539	0.5394	0.5450	0.5432	0.5430	0.5413
	(0.0601)	*(0.0598)*	*(0.0602)*	*(0.0600)*	*(0.0600)*	*(0.0604)*
ln(KS)	0.4959	0.4884	0.4969	0.4972	0.4972	0.4974
	(0.0574)	*(0.0574)*	*(0.0575)*	*(0.0575)*	*(0.0575)*	*(0.0576)*
CYCLE	0.5527	0.5530	0.5479	0.5542	0.5537	0.5528
	(0.0526)	*(0.0525)*	*(0.0527)*	*(0.0529)*	*(0.0530)*	*(0.0531)*
SIZE_S	-0.6632	-	-	-	-	-
$x10^{-3}$	*(1.5145)*					
SIZE_M	3.4505	-	-	-	-	-
$x10^{-3}$	*(2.4020)*					
SIZE_L	-1.7948	-	-	-	-	-
$x10^{-3}$	*(1.9070)*					
DUR_S	-	-7.2043	-	-	-	-
$x10^{-3}$		*(3.3012)*				
DUR_M	-	1.9150	-	-	-	-
$x10^{-3}$		*(1.7410)*				
DUR_L	-	0.1248	-	-	-	-
$x10^{-3}$		*(1.2692)*				
DLOST_S	-	-	-0.4528	-	-	-
$x10^{-3}$			*(1.3424)*			
DLOST_M	-	-	0.7650	-	-	-
$x10^{-3}$			*(2.0280)*			
DLOST_L	-	-	-0.2558	-	-	-
$x10^{-3}$			*(2.4647)*			
STRIKE	-	-	-	0.0091	0.0377	0.0288
$x10^{-3}$				*(0.8274)*	*(0.8400)*	*(0.8411)*
$STRIKE_{-1}$	-	-	-	-	-0.1428	-0.1135
$x10^{-3}$					*(0.7092)*	*(0.7178)*
SPILL	-	-	-	-0.2632	-0.2608	-0.2519
$x10^{-3}$				*(0.3085)*	*(0.3089)*	*(0.3108)*
$SPILL_{-1}$	-	-	-	-	-	-0.0690
$x10^{-3}$						*(0.2528)*
Time Dummies	YES	YES	YES	YES	YES	YES
Diagnostics						
N x T	98 x 9	98 x 9	98 x 9	98 x 9	98 x 9	98 x 9
*ln*L	352.533	354.458	351.236	351.549	351.576	351.638
R^2	0.9102	0.9106	0.9099	0.9100	0.9100	0.9100
Test for Constant Returns to Scale						
$\alpha + \beta$	1.0498	1.0278	1.0420	1.0404	1.0402	1.0387
	(0.0663)	(0.0662)	(0.0664)	(0.0662)	(0.0662)	(0.0665)

Notes:
1. See table 6.1.

Table 6.3

Panel Data Estimates: Per Capita Strike Measures

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7	Column 8	Column 9
CONST	-0.9738	-0.9801	-0.9839	-0.9746	-0.9090	-0.9704	-0.9962	-0.9981	-0.9047
	(0.3122)	*(0.3119)*	*(0.3125)*	*(0.3131)*	*(0.3122)*	*(0.3124)*	*(0.3161)*	*(0.3166)*	*(0.3215)*
ln(EMP)	0.5464	0.5438	0.5440	0.5463	0.5387	0.5425	0.5522	0.5522	0.5313
	(0.0598)	*(0.0598)*	*(0.0598)*	*(0.0599)*	*(0.0597)*	*(0.0599)*	*(0.0611)*	*(0.0612)*	*(0.0625)*
ln(KS)	0.4965	0.4976	0.4979	0.4966	0.4896	0.4982	0.4960	0.4962	0.4956
	(0.0575)	*(0.0574)*	*(0.0575)*	*(0.0576)*	*(0.0574)*	*(0.0575)*	*(0.0576)*	*(0.0576)*	*(0.0575)*
CYCLE	0.5494	0.5512	0.5518	0.5502	0.5538	0.5520	0.5483	0.5487	0.5463
	(0.0526)	*(0.0526)*	*(0.0526)*	*(0.0527)*	*(0.0525)*	*(0.0527)*	*(0.0527)*	*(0.0528)*	*(0.0527)*
STRIKE	0.0042	0.1169	0.1169	-	-	-	0.0043	0.0040	0.0032
	(0.0398)	*(0.0783)*	*(0.0783)*				*(0.0398)*	*(0.0398)*	*(0.0398)*
$STRIKE^2$	-	-0.1872	-0.1880	-	-	-	-	-	-
		(0.1121)	*(0.1122)*						
$STRIKE_{-1}$	-	-	0.0088	-	-	-	-	0.0058	0.0084
			(0.0366)					*(0.0367)*	*(0.0367)*
SIZE_S	-	-	-	-0.0303	-	-	-	-	-
				(0.0626)					
SIZE_M	-	-	-	0.0142	-	-	-	-	-
				(0.0842)					
SIZE_L	-	-	-	0.0494	-	-	-	-	-
				(0.0801)					
DUR_S	-	-	-	-	-0.3289	-	-	-	-
					(0.1351)				
DUR_M	-	-	-	-	0.1027	-	-	-	-
					(0.0730)				
DUR_L	-	-	-	-	0.0161	-	-	-	-
					(0.0574)				
DLOST_S	-	-	-	-	-	-0.0448	-	-	-
						(0.0564)			
DLOST_M	-	-	-	-	-	0.0351	-	-	-
						(0.0800)			
DLOST_L	-	-	-	-	-	0.1004	-	-	-
						(0.1110)			

Table 6.3 (continued)

Panel Data Estimates: Per Capita Strike Measures

	Column 1	Column 2	Column 3	Column 4	Column 5	Column 6	Column 7	Column 8	Column 9
SPILL	-	-	-	-	-	-	0.0028 *(0.0060)*	0.0027 *(0.0061)*	0.0030 *(0.0061)*
$SPILL_{-1}$	-	-	-	-	-	-	-	-	-0.0092 *(0.0058)*
Time Dummies	YES	YES	YES	YES	YES	YES	YES	YES	YES
Diagnostics									
N x T	98 x 9	98 x 9	98 x 9	98 x 9	98 x 9	98 x 9	98 x 9	98 x 9	98 x 9
*ln*L	351.104	352.687	352.706	351.432	355.471	352.034	351.151	351.158	352.951
R^2	0.9099	0.9102	0.9102	0.9100	0.9108	0.9101	0.9100	0.9100	0.9103
Test for Constant Returns to Scale									
$\alpha + \beta$	1.0429 (0.0660)	1.0414 (0.0660)	1.0419 (0.0660)	1.0429 (0.0662)	1.0283 (0.0660)	1.0408 (0.0661)	1.0482 (0.0670)	1.0484 (0.0671)	1.0269 (0.0683)

Notes:
1. See table 6.1.

Table 6.4

The Impact of Unionisation and Strike Activity on Efficiency

	Column 1	Column 2	Column 3	Column 4
CONST	-0.186 *(0.690)*	-0.073 *(0.684)*	1.678 *(0.738)*	3.399 *(1.074)*
SIZE	-0.647 *(0.319)*	-0.872 *(0.325)*	-0.302 *(0.261)*	-5.778 *(3.286)*
P_PT	3.620 *(1.582)*	3.989 *(1.784)*	1.902 *(1.468)*	1.505 *(1.402)*
P_OPS	0.050 *(0.852)*	-0.340 *(0.872)*	-1.125 *(0.855)*	-1.517 *(0.831)*
MKL	-	0.006 *(0.077)*	-0.079 *(0.069)*	-0.099 *(0.071)*
MCONC	-	0.353 *(0.386)*	0.070 *(0.331)*	0.128 *(0.351)*
AS	-	2.651 *(2.727)*	1.484 *(2.615)*	2.062 *(2.394)*
MCYCLE	-	-4.329 *(2.728)*	-5.949 *(2.504)*	-5.835 *(2.408)*
P_CA	-	-	-0.700 *(0.310)*	-2.342 *(0.786)*
MSTR	-	-	-0.028 *(0.007)*	-0.176 *(0.069)*
P_CA*SIZE	-	-	-	6.196 *(3.499)*
MSTR*SIZE	-	-	-	0.211 *(0.248)*
P_CA*MSTR	-	-	-	0.174 *(0.081)*
P_CA*MSTR*SIZE	-	-	-	-0.246 *(0.273)*
Diagnostics				
N	93	93	93	93
*ln*L	-97.10	-94.81	-82.26	-78.89
R^2	0.128	0.170	0.366	0.410

Notes:
1. Heteroskedastic consistent standard errors in parentheses.

Appendix to Chapter 6

Table A6.1

Omitted 3-digit Industries
1968 Standard Industrial Classification

SIC	Classification	Key
213	Biscuits	+
311	Iron and Steel (General)	+ #
312	Steel Tubes	#
313	Iron Castings etc	#
333	Pumps, Valves and Compressors	%
334	Industrial Engines	+
335	Textile Machinery and Accessories	+
338	Office Machinery	+
349	Other Mechanical Engineering n.e.s.	*
365	Broadcasting & Sound Reproduction Equipment	*
369	Other Electrical Goods	*
370	Shipbuilding and Marine Engineering	#
381	Motor Vehicle Manufacturing	*
385	Railway Carriages and Wagons and Trams	*
395	Cans and Metal Boxes	%
396	Jewellery and Precious Metals	%
399	Metal Industries n.e.s.	%
417	Hosiery and Knitted Goods	*
461	Bricks, Fireclay and Refractory Goods	*
484	Manufacturers of Paper and Board n.e.s.	#
485	Printing, Publishing of Newspapers	#
486	Printing, Publishing of Periodicals	*
494	Toys, Games, Prams and Sports Equipment	#
499	Miscellaneous Manufacturing Enterprises	#

	Key Identification
*	not available from Census of Production
+	Index of Output figures not available
#	Capital Stock figures not available
%	Net Output figures not available

Thus 8 industries have no data at all from the Census of Production (coded * in table A6.1), and a further 16 are missing vital series for the current analysis. This leaves a total of 98 industries from the 122 SIC 1968 3-digit manufacturing industries observed over the 10 years 1970 to 1979, a total of 980 observations.

Table A6.2

Panel Data: Variable Description and Summary Statistics
98 3-digit Industries 1970-1979

Variable	Measures of Strike Activity: Levels	Mean	S.D.
STRIKE	Strike Frequency	9.42	13.36
STRIKE²	Strike Frequency squared	266.94	997.62
SIZE_S	99 > Number of Workers Involved	4.10	6.68
SIZE_M	100 < Number of Workers Involved < 249	2.26	3.45
SIZE_L	250 < Number of Workers Involved	3.06	4.57
DUR_S	Duration 1 day or less	1.19	2.26
DUR_M	Duration 2, 3 or 4 days	3.29	4.69
DUR_L	Duration 5 or more days	4.93	7.53
DLOST_S	499 > Days Lost	4.69	7.35
DLOST_M	500 < Days Lost < 2499	3.03	4.34
DLOST_L	2500 < Days Lost	1.70	2.83
PAY	Number of Pay Strikes	6.09	8.85
NONPAY	Number of Non-pay Strikes	3.33	5.10
SPILL	Nunber of Strikes elsewhere in 2-digit Parent Industry	48.64	45.31
	Measures of Strike Activity: Per Capita		
STR_PH	Strikes per head	0.173	0.158
STR_PH²	Strikes per head squared	0.055	0.110
SIZE_S	99 > Number of Workers Involved	0.074	0.082
SIZE_M	100 < Number of Workers Involved < 249	0.042	0.054
SIZE_L	250 < Number of Workers Involved	0.057	0.070
DUR_S	Duration 1 day or less	0.021	0.038
DUR_M	Duration 2, 3 or 4 days	0.062	0.068
DUR_L	Duration 5 or more days	0.089	0.093
DLOST_S	499 > Days Lost	0.085	0.094
DLOST_M	500 < Days Lost < 2499	0.057	0.062
DLOST_L	2500 < Days Lost	0.031	0.044
PAY	Number of Pay Strikes	0.113	0.116
NONPAY	Number of Non-pay Strikes	0.060	0.067
SPILL	Number of Strikes elsewhere in 2-digit Parent Industry	1.719	2.168

Table A6.3

Industry Data: Variable Description and Summary Statistics

93 3-digit Industries 1970-1979

Variable	Description	Mean	S.D.
SIZE	Average plant size above median size	0.217	0.216
P_PT	Proportion part-time	0.084	0.052
P_F	Proportion female	0.358	0.214
P_OPS	Proportion operatives	0.733	0.096
MKL	Mean (log) capital:labour ratio	1.884	1.017
MCONC	Mean (5-firm) concentration ratio	0.478	0.240
AS	Advertising:sales ratio	0.016	0.023
MCYCLE	Mean growth rate of output	0.007	0.032
P_CA	Proportion covered by collective agreements	0.748	0.185
MSTR	Mean strike frequency	9.772	12.404

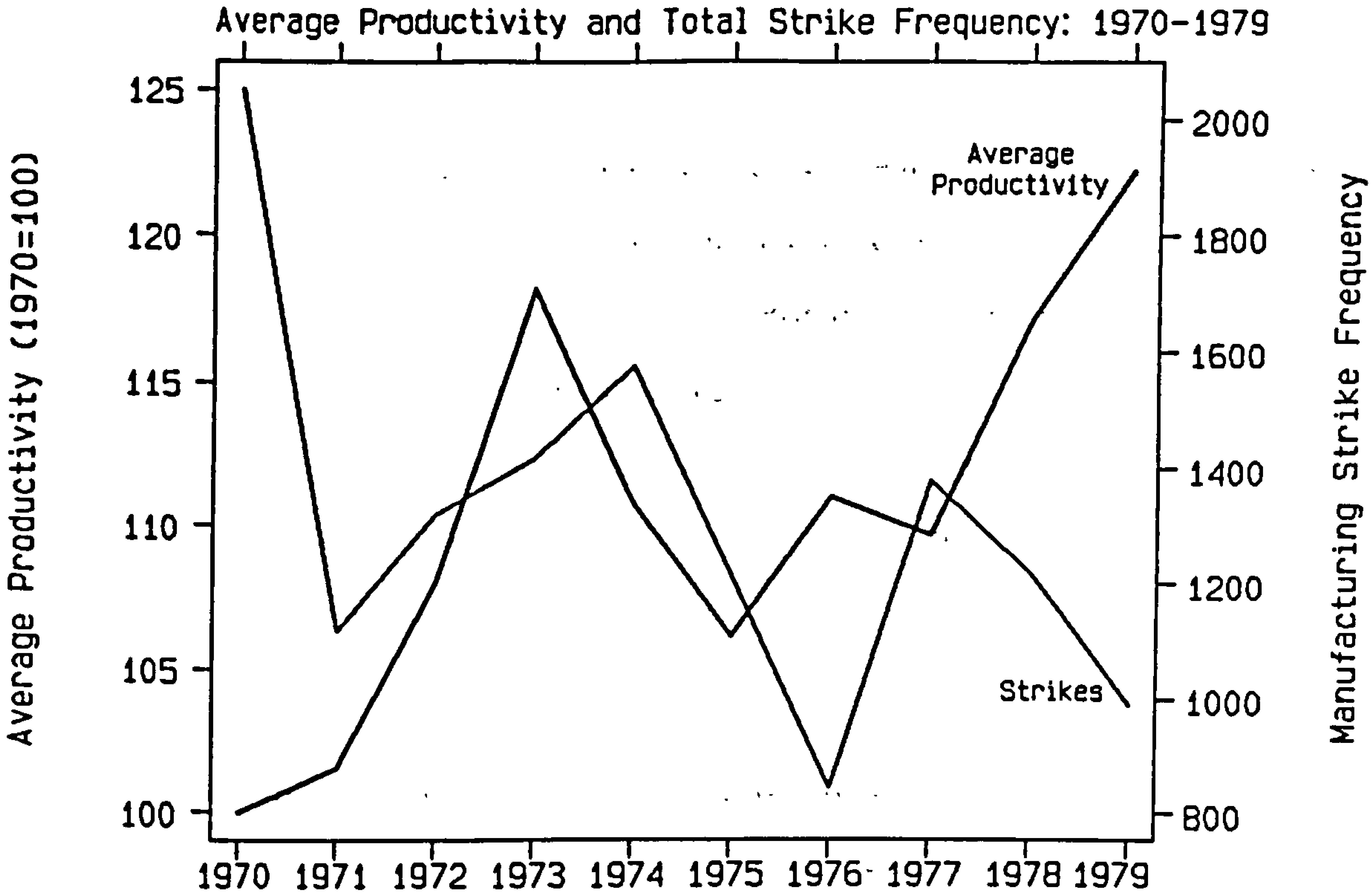
Average Productivity and Total Strike Frequency: 1970-1979
Average Productivity (1970=100)
Manufacturing Strike Frequency
125
120
115
110
105
100
2000
1800
1600
1400
1200
1000
800
Average
Productivity
Strikes
1970
1971
1972
1973
1974
1975
1976
1977
1978
1979

Figure 6.1

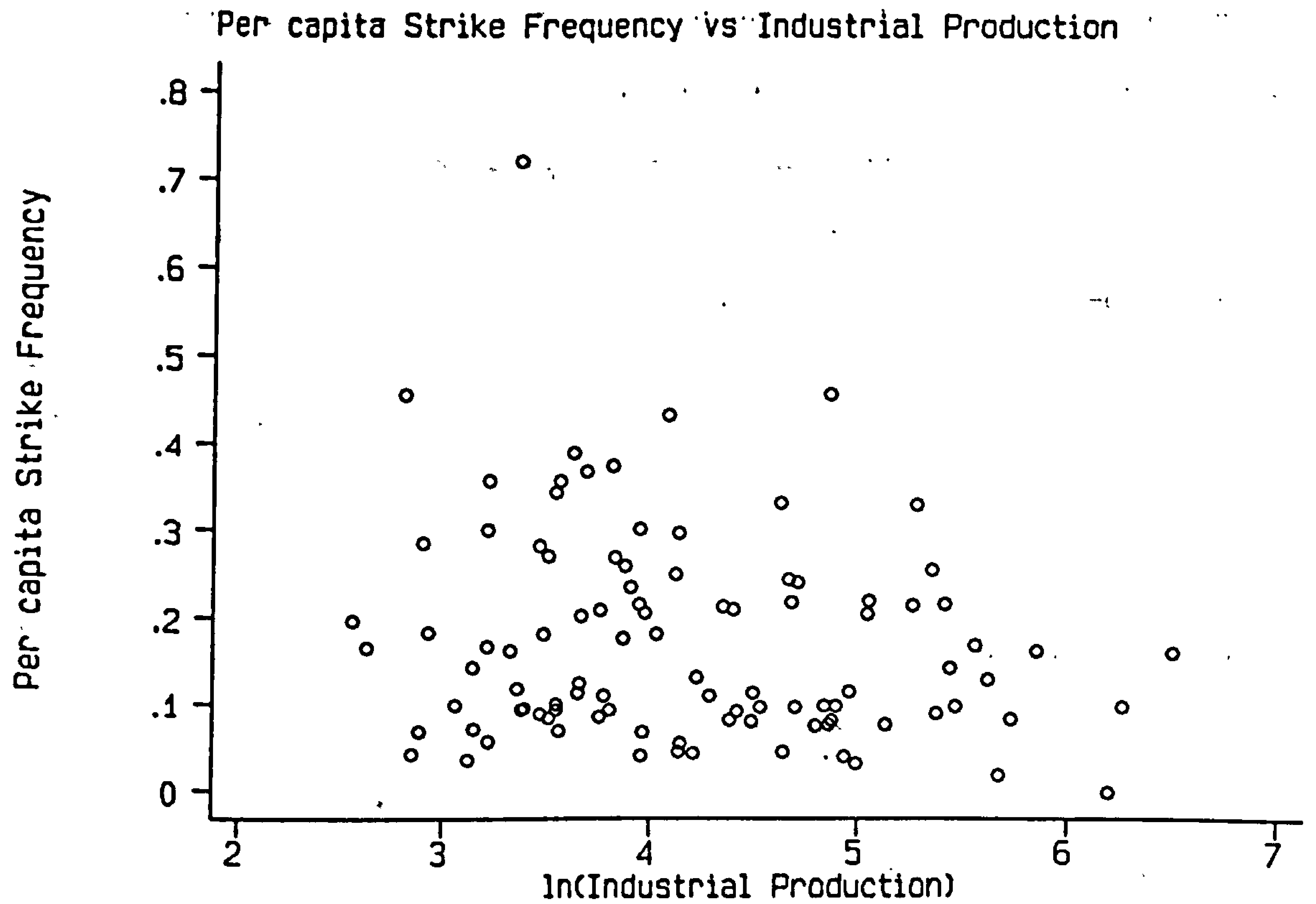
Per capita Strike Frequency vs Industrial Production
Per capita Strike Frequency
.8
.7
.6
.5
.4
.3
.2
.1
0
2
3
4
5
6
7
ln(Industrial Production)

Figure 6.2

CHAPTER 7

Summary and Conclusions

7.1. Introduction.

This final chapter of the thesis presents a summary of the main findings and brings together the results from the preceding analyses in section 7.2. The chapter briefly concludes in section 7.3 with a discussion of some of the implications for any future work in the economic analysis of industrial conflict in Britain.

7.2. A Summary of the Major Findings.

The overview of the theoretical and empirical literature presented in chapter 2 highlights the central issues that provide the motivation for much of the work in the thesis. Many of the issues raised have long frustrated economists and researchers in industrial relations in their endeavours to describe and explain industrial conflict. Given the recent advances in the quality and quantity of micro-level data, it is the improved *response* to these difficulties that provides justification for the thesis. While acknowledging the weaknesses in the aggregate strike statistics published by the Department of Employment, it should still be emphasised that these provide the only consistent source of information on industrial conflict in Britain over any reasonable time period. On examination, the various dimensions of strikes measured by the Department of Employment are shown to be either overly sensitive or insensitive to particularly large strikes; while the numbers of workers involved and the number of days lost tend to reflect only the major disputes in any year, the statistics for strike frequency give such strikes equal weight as minor stoppages which just satisfy the minimum size criteria used. There seems no satisfactory solution to this dilemma with these data. However, the recent surveys of workplace industrial relations can provide an alternative source of data on industrial conflict, and one which records rather different dimensions of conflict activity. Clearly, when used in conjunction with the strikes data from the Department of Employment, these micro-level surveys can provide much needed additional breadth to the study of conflict behaviour.

The overview of the many theories of strike activity shows that most are derived from North American studies, and are therefore arguably inappropriate for the system of industrial relations that pertains in Britain. In particular, notions of fixed-term contracts have only very weak counterparts in employment arrangements in Britain. Despite this and several other important differences in bargaining structures, many of the studies of British strike activity have used these models as the basis for their empirical work. Time-series studies of aggregate strike frequency dominate the literature, despite, or perhaps because of the relative insensitivity of this dimension of conflict to major disputes. Many studies date from the 1970s, and there is therefore typically little econometric evaluation of the results obtained. Moreover, most models fail to take account of the changing volume of bargaining activity by neglecting to deflate the statistics for the number of strikes by a suitable measure of the 'population' size.

The thesis recognises these limitations and argues for a more appropriate methodological approach which encompasses broader definitions of conflict than simply the number of strikes, and provides for more disaggregated studies in which the sectoral diversity in conflict can be investigated. Thus the central objective of the thesis is to investigate the various dimensions of conflict and their interrelationships, while only loosely attempting to encompass previous theoretical work. There would appear to have been little previous study in this area, and much of it is rather unsatisfactory.

In order to reinforce the argument for such an approach, the first substantive empirical chapter, chapter 3, reexamines two of the most often cited aggregate econometric models of strike frequency in Britain. It is shown that these are fundamentally misspecified in a number of ways and thus the conclusions that are drawn in these and similar studies should be viewed with some caution. In particular, they are unable to forecast satisfactorily, even when given sympathetic treatment in terms of model specification. This would appear to be primarily due to the reversal in the trend in strikes after 1970. The chapter then turns to the central issue of the procyclicality of strikes that these aggregate econometric models often claim to demonstrate. Using the Burns-Mitchell NBER methodology for analysing business cycles, it is shown that while the total number of strikes is only very loosely related to the cycle, strikes arising over the level of remuneration bear a much closer correspondence with the level of economic activity. Of course, this finding accords with many of the theoretical models that have been proposed for strike activity. The number of strikes in the coal industry is seen to follow a completely different pattern however. The implications are that future aggregate econometric models should focus on non-coal wage strikes while concentrating on specifications that include only a single concurrent measure of the cycle; the proliferation of cyclical indicators in many of the previous studies has served to occlude the evident cyclicality in strike frequency. The chapter concludes with an examination of a 'cyclical-political' model of strikes which is seen to perform reasonably satisfactorily over the whole of the post-war period, a result that the 'econometric' models have been unable to achieve. The impact of the reforms in labour legislation which have been introduced in the 1980s, and which have had particular relevance for the ability of unions to call for strike action, is also examined at this juncture.

One of the central conclusions drawn from the examination of the literature in this area in chapter 2 is that the emphasis on strike *frequency* is rather inappropriate. This is most starkly illustrated by the fact that while aggregate strike frequency fell by almost 25% between 1980 and 1984, the incidence of strikes at the establishment level as recorded by the Workplace Industrial Relations Surveys actually *increased* by 45%. Thus the substantial fall in strike frequency witnessed since 1970 does not necessarily imply that conflict is less prevalent; the increasing concentration of unions by successive amalgamations and the fall in union density since 1980 will have both served to reduce the number of strikes recorded by the Department of Employment even if the underlying level of conflict in the economy was unchanged. Thus it may be more appropriate to examine the determinants of strike incidence rather than strike frequency, and it is this issue that forms the basis of the second substantive empirical chapter, chapter 4. Models for the incidence of conflict activity among manual workers in private sector establishments are estimated using the Workplace Industrial Relations Surveys of 1980 and 1984. Changes in the incidence of conflict are seen to be predominantly the result of changes in behaviour between the two years rather than changes in the composition of establishments, although the time scale is admittedly rather short for compositional changes to have impacted on the level of industrial conflict. However, this finding does contrast with that of Stewart (1991) who concludes that the small change in the union wage differential between the two years is attributable entirely to compositional changes, in particular, the shift away from the manufacturing sector and the downward shift in the size distribution of establishments. As a subsidiary theme, the relationship between overt conflict activity (strikes) and lesser forms of grievance activity (go-slows, overtime bans, work-to-rules) is also investigated explicitly in this chapter. Strike action and non-strike activity are shown to be complementary forms of expression of industrial unrest at the establishment level, rather than being substitutes as some have argued. There is also some weak evidence in support of some 'switching' from (relatively costly) strike action to (cheaper) non-strike activity between the two survey dates.

The next chapter, chapter 5, investigates the distinctions between public and private sector strike activity. While the raw incidence and frequency levels appear to be much higher in the public sector, there are a number of differences between the public and private sector that act to make strikes more likely in the public sector. The most obvious influential characteristic is plant size which has been shown in many previous studies to be positively related to the likelihood of strike action. Given that public sector establishments tend to be bigger, on average, than private sector establishments, then one would expect the public sector to experience higher levels of strike activity. Thus the primary purpose of chapter 5 is to estimate the *ceteris paribus* differences in strike probabilities between the public and private sectors utilising both the Workplace Industrial Relations Surveys and the Department of Employment's Industrial Stoppages Data Tapes. The findings indicate that much of the divergence in recorded strike activity between the two sectors is a consequence of differences in their characteristics, and that variations in the incidence of conflict are typically not significant at conventional levels. Moreover, while it is apparent that the public sector exhibits a higher incidence of strikes per establishment, each of these strikes tends to be shorter and involve fewer workers and therefore results in fewer days being lost than during strikes in the private sector. This chapter highlights the need to take account of the sectoral diversity in strike activity and, moreover, to suitably adjust raw incidence and frequency statistics for the differences in employment and union membership in order that appropriate comparisons can be made.

Chapters 3, 4 and 5 focus attention on the significance of considering disaggregated measures of strikes (such as pay strikes) together with the importance of the relationship between strike frequency and strike incidence and the related issue of non-strike activity. The final substantive chapter, chapter 6, looks at some of the effects of strikes and, in particular, at their impact on industrial output and efficiency. The data used are a 3-digit panel of production industries in the 1970s drawn from the Census of Production[1]. The structure of the model is novel in that a production frontier is estimated without having to assume an explicit functional form for the

1. The intention is to extend this analysis to the 1980s once the 1981 Industrial Stoppages Data Tape becomes available.

(one-sided) inefficiency component. This formulation has a number of benefits and is due to the availability of a panel of data in which the fixed effects can be viewed as capturing both the inefficiency term as well as the industry specific component. A second stage estimation is then used to examine the determination of the magnitude of the inefficiency parameter. While strikes do not appear to significantly reduce output in aggregate, there is some evidence to suggest that those industries which incur a large number of short strikes do have their output significantly disrupted. This is a similar result to that found by Knight (1989) in his examination of a single cross-section of production industries in the late 1960s. The loss of output also serves to make these industries less efficient in general, over and above their lower efficiency resulting from higher levels of unionisation. Ultimately, it would be of interest to conduct a similar study at even finer levels of disaggregation since there is some suspicion that the real costs of strikes cannot be identified even at the 3-digit level.

In sum, the findings of this essentially empirical work clearly illustrate the need for a wider perspective to be taken in the analysis of industrial conflict than has previously been the case for Britain. One major weakness of the extant studies is their estimation of models which are largely inappropriate given the system of industrial relations that exists outside North America. While the thesis does not address this problem directly by developing suitable alternative models, it does serve to establish further details of the patterns in industrial conflict that more appropriate theoretical models will need to encompass; the strong procyclicality in the number of wage strikes; the importance of institutional considerations; the trend towards fewer, but more extensive, stoppages; the sectoral diversity in conflict; the relationship between different forms of conflict activity; the costs and consequences of industrial action and the impact of the changing patterns of unionism are all clearly issues that a satisfactory model of industrial conflict in Britain will need to address.

7.3. Conclusions.

The collective bargaining process is fundamental to the operation of labour markets and the use of collective sanctions is an important factor in bargaining behaviour and, consequentially, in settlement outcomes. But conflict is costly, especially to the parties involved, and hence it is important to assess the motivations that lay behind both strike and non-strike action. Previous attempts to investigate industrial conflict in Britain have been marred by poor econometric models which concentrate mostly on strike frequency. The thesis argues for a change in the methodological approach to the study of conflict in Britain. The investigation of a number of different dimensions of industrial action using a variety of information sources would provide a much more comprehensive and satisfactory picture of the nature and scope of conflict in Britain.

The conclusions of the research conducted in the thesis demonstrate that such an approach is now both feasible and beneficial. The results discussed in section 7.2 above illustrate the importance of a more disaggregated, multi-dimensional approach to industrial conflict in Britain.

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