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ONE HUNDRED YEARS OF STRIKE STATISTICS:  
DATA, METHODOLOGY, AND THEORETICAL ISSUES IN QUANTITATIVE STRIKE RESEARCH

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## Introduction

The bulk of strike research has used quantitative methodologies, largely as a result of the ready availability of government data collected for over one hundred years. Even a cursory reading of the literature reveals a concern with numbers from as early as the turn of the century. Yet, to date, no systematic attempt has been made to evaluate the empirical basis of this rapidly growing body of literature.

The central purpose of this paper is to assess both the reliability of available official strike material, and the methodological adequacy of quantitative strike analysis. The focus is on the ways data and methodological problems may have affected theoretically relevant conclusions. All too often, sound theories are rejected and bad ones put forward on the basis of poor empirical work, either in terms of unreliable data or methodological pitfalls. More broadly, the paper also critically evaluates a set of relevant references, highlights the central issues and problems in the field, offers some suggestions on how to overcome existing discrepancies in findings, and explores lines for future research. It draws especially on my experience in analyzing Italian strikes since 1945, but also ranges widely across other countries and times.

The first three sections of the paper (sections 2 through 4) appraise the quality of official data on which most empirical works are based. Sections 5 and 6 survey the major findings and the methodological weaknesses and strengths of quantitative strike research, concentrating on the study of both economic and political/organizational determinants of strikes. The lack of integration and the discrepancies in empirical findings between these two lines of inquiry will be addressed in section 7. In section 8 an attempt is made at clearly spelling out the limitations of the quantitative approach to the study of strikes and at exploring profitable alternatives. The conclusions bring together the focal points of the paper, highlighting unexplored lines of research.

## About Strikes and Strike Statistics

To say that strikes are as old as work itself is to repeat the first line of many studies of strikes. The withholding of labor is known to have occurred during antiquity, the Middle Ages and the Renaissance (see Crouzel, 1887). It was not, however, until the emergence of wage labor under industrial capitalism that strikes became a routine expression of workers' claims. "Out of the incessant struggles among claimants, objects of their claims, and intervening authorities grew a series of half-stated and compelling definitions and rules": the forms of work stoppages that would be acceptable, the actions allowed during a strike, the kind of demands that would be appropriate, where and when police would step in, and so on (see Tilly, 1981:5).

Sometime during the last century--around 1830 in England, toward the middle of the century in France and later yet in Germany and Italy--strikes came into being together with a distinctive new repertoire of collective action, largely brought about by the formation of national states and "nationalization of power and politics". Demonstrations, electoral rallies, public meetings, etc. gradually came to replace routines that had been familiar to the Western World for centuries: food riots, tax rebellions, collective invasions of enclosed lands, and so on (for an introduction to the changes in repertoire of collective action during the nineteenth century see Tilly, 1981).

Another half a century, though, was to go by before systematic collecting and publishing of strike data would begin. In most western countries this occurred sometime between 1870 and 1900. Until then, strikes were mainly a matter of police recording (see Perrot, 1974). They were viewed, in fact, as crimes under Combination Acts. Data were kept secret in the hands of Ministries of the Interior.

Only when the conception of strikes shifted from one of crime to one of 'social illness', did strikes become the concern of specialized Labor Ministries. It is these Ministries that initiated systematic recording and publishing of strike data and that

standardized definitions and collecting procedures (see Perrot, 1974). These early statistics were very rich in information on the immediate causes of strikes, their outcomes, their organizational bases, the occurrences of violent incidents and the number of establishments affected by any given strike.

Unfortunately, the times when official statistics published such detailed information for each individual strike are long gone. The figures currently published in most western countries are highly aggregated (on data availability across countries, see Fisher, 1973).

The purpose of this section is to assess the reliability of official strike data and, in particular, of the three measures most commonly used to quantify strikes: number of strikes, number of workers involved and number of hours lost (on the general issue, see Shalev, 1978).

The first problem concerns the official definition of a strike. What most quantitative researchers study, in fact, is what they find in national statistical yearbooks. The way a strike is defined by the data collecting agencies does make a difference. For instance, in Italy, until 1954, a strike according to ISTAT (the Italian Central Statistical Office) was any labor dispute--strike or lockout--between employers and employees that would lead to an interruption of normal working activities of at least one working day of duration, or, if less, for several consecutive days. Starting in 1955, labor conflicts include all work stoppages of any duration, even of some fraction of an hour. In both periods, all conflicts that originate for reasons that go beyond the strict employer-employee relation, such as demonstrative strikes connected with measures of political economy, with national or international events, etc., are not recorded (after 1975 these types of strikes have been recorded separately).

Considering that most Italian strikes are of a short duration, official data probably underestimated strike frequency in the years immediately following the Second World War. Furthermore, until 1975, strike size and volume are biased

downward. Demonstrative strikes, in fact, usually are national, inter-industry strikes that involve, for however short a time, millions of workers throughout the country. The exclusion of this type of strike cannot but bias the number of strikers and hours lost indicators, and the others derived from them: size, volume and duration. The extent of the bias depends upon the frequency and average duration of disputes.

What is more important is that such biases in the data can distort relationships among variables. For instance, large scale, inter-industry strikes clearly require a higher degree of workers' organizational capacity than do small, shop-level disputes. The loss of information about the large-scale, 'political', demonstrative strikes, may be telling when assessing the impact of unions' organizational capacity on strike activity (see Stearns, 1974:10). By the same token, one could say very little about unions' ability to influence, by their action, national, central-state politics (1). Definitions do make a difference, and therefore more attention should be paid to the problem than is normally done in the literature (2).

Second, there are the problems involved in the data collecting procedures. To continue with the Italian example, although the Central Statistical Office (ISTAT) is charged with data collection, the actual work is carried out by local police offices (questura). They will record a strike only if they come to know about it, either directly from the employer or the unions, or indirectly through their information network. Under this data collecting procedure the number of strikers and hours lost are likely to be quite distorted.

Guessing the magnitude or the direction (upward or downward) of the bias is not an easy matter. If employers report the strike, they may be more inclined, from time to time, to play down the strike to embarrass the unions or to portray a public image of peaceful labor relations, they may also magnify it in order to justify future retaliatory actions and/or police or government intervention. Union reported figures are more likely to be overestimated for obvious reasons. If police offices themselves

estimate the size of the strike it may be no more than pure guess-work (3) (4).

Strike frequency, on the other hand, is likely to be always greatly underestimated since small, short, shop-level spontaneous work stoppages, that represent the bulk of all strike occurrences, probably escape recording, as scores of field investigations have shown (see Turner, Clack and Roberts, 1967; Korpi, 1978, 1981; Batstone et al., 1978) (5). But it is precisely this type of strike, rather than large and centrally directed displays of union strength, that provides a good indication of the existence of diffused, grass-root discontent--often directed against official unions as much as against employers.

Finally, postwar official statistics, as we have seen, are highly aggregated. As a consequence, there is no way of telling, for instance, which combination of strike frequency and duration produced any given figure of number of hours lost (Batstone et al., 1978:23). Furthermore, official statistics do not tell us how many different workers went on strike in any given year. As Batstone et al. argue, a yearly total of 100,000 could refer to 100,000 different workers or to the same 10,000 going on strike 10 different times (Batstone et al., 1978:21; see also Knowles, 1968:502).

Although it is always possible to get the average worker involvement per strike, the fact that the average is among strikes very different in size may blur things even further. At the very minimum, it prevents any serious assessment of the impact of different organizational forms on strike activity. The type of organization required by a large, nation-wide and economy-wide strike must be very different from the one required by a small, shop-level strike.

Further problems arise when what are essentially single strikes across a city or an industry get recorded separately (6). The number of strikes is inflated but the average strike size is reduced. Again, this would misrepresent the organizational basis of strikes (Batstone et al., 1978:22). As Stearns clearly points out: "The large strike that does not exceed the bounds of a single company probably does not reflect the



same level of class or group consciousness as a smaller strike that spread more widely, at least before World War I" (Stearns, 1974:11).

No less significant may be the loss of information involved in spatial aggregation. And this is so not simply because official geographical aggregation may have more to do with bureaucratic jurisdictions and local governments, than with culturally and historically homogeneous populations. Historical geography has, in fact, shown that the spatial spreading of various forms of protest movements follows traceable and predictable patterns. We may not be able to map over space and time the diffusion of strikes--in particular during strike waves--when data are too highly aggregated (see Abler, Adams and Gould, 1971; for an example, see Charlesworth, 1979) (7).

#### About Strike Indicators and Their Graphical Representations

On the basis of the three indicators collected and reported by official government statistics--number of strikes (S), number of workers involved (W) and number of hours lost (H)--several different measures have been constructed in the literature:

Frequency or number of strikes per thousand employed workers

Size or average number of strikers per strike (W/S)

Duration or average number of hours lost per striker (H/W)

Volume defined as frequency x size x duration

$$= (S/\text{Thousand employed workers}) \times (W/S) \times (H/W)$$

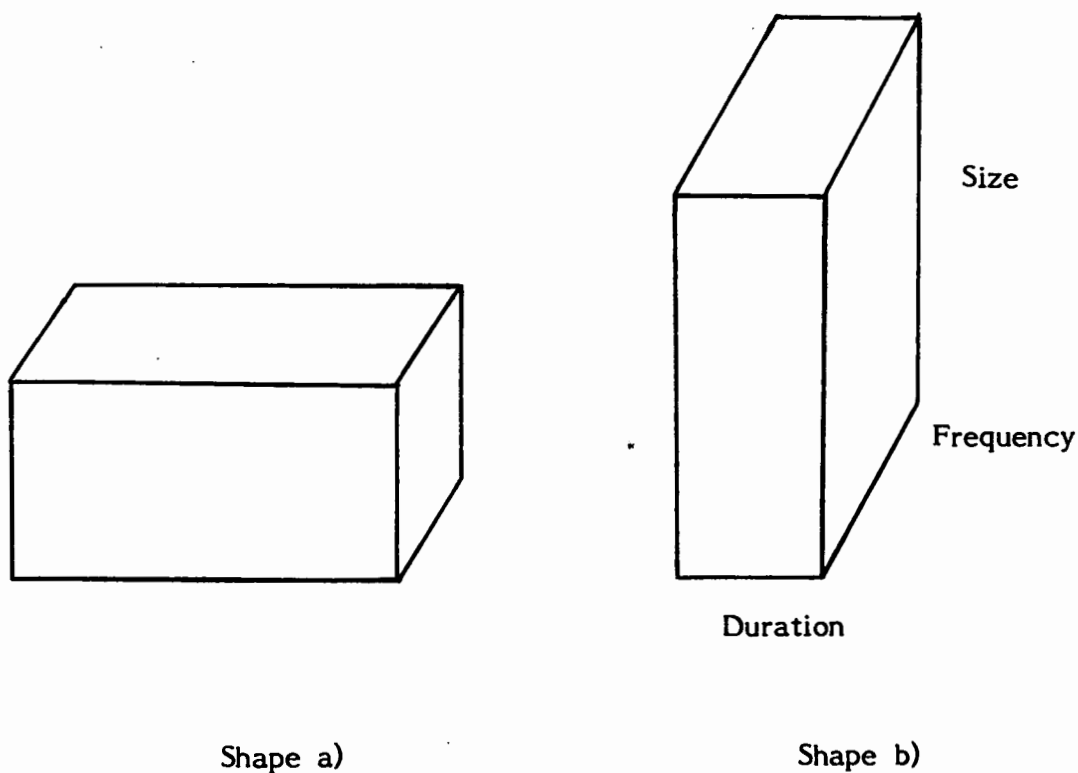
$$= (H/\text{Thousand employed workers})$$

(for these definitions, see Forcheimer, 1948; Knowles, 1952; Shorter and Tilly, 1974; Hibbs, 1976).

The following graphical representations have been proposed for the above measures: the two-dimensional, rectangular strike 'profiles' suggested by Spielmans (1944); the time plots proposed by Knowles (1952), where each component of the

additive expression obtained by taking the log of the volume of strikes is plotted in the same graph; and Shorter and Tilly's three-dimensional 'shape' of strikes (see Shorter and Tilly, 1971, 1974; see also Britt and Galle, 1974; Hibbs, 1976). In a three-dimensional space, the shape of strikes refers to the rectangular solid whose sides are given by the size, duration, and frequency of strikes and whose volume corresponds, of course, to the volume of strikes previously defined (8).

Fig. 1 - Strike Shapes



The most interesting finding about the shape of strikes is the dramatic change it has undergone over the last one hundred years. The expansion of industrial establishments, in terms of both number and size, and the growth of labor organizations, have led to a long-run increase in strike frequency and size (9). This upward trend has not been abated even by the coming of 'mature prosperity'. In fact,

the 1960s waves of industrial conflict across the Western world largely disproved theses on the "withering away of strikes" in advanced capitalist societies (see Ross and Hartman, 1960) (10). Only strike duration has been declining, underscoring the characteristic of strikes as a limited form of warfare, where neither party throws all power resources into the battle until the opponent succumbs (see Batstone et al., 1978).

But shapes a) and b) in Fig. 1 do not only capture temporal differences. There is in fact considerable cross-country variation that can be approximated by shapes a) and b). For instance, even modern American and Canadian strike shapes more nearly resemble shape a). What makes a country's or a period's strike pattern so distinct is the particular combination of these three strike dimensions, as one finds in the shape of strikes, rather than the volume which can mask such marked differences in its constituent components. In light of this finding, works that purportedly explain inter-temporal and inter-national differences in industrial conflict on the basis of volume alone appear to have little justification (11).

Another problem with standardized indicators is that different deflating procedures have been used--total civilian labor force, non-agricultural labor force, unionized labor force, employment figures, etc.--leading to an unnecessary proliferation of strike indicators that stand in the way of comparability of findings (see Stern, 1978). Furthermore, standardized measures, such as volume, where labor force figures appear in the denominator, have been used in the context of multivariate regression analysis as 'dependent' variables. The appearance among the regressors of unemployment rate, which also contains by definition labor force figures in the denominator, builds into the model artificial correlations between 'dependent' and 'independent' variables (see Stern, 1978).

It is in the long run (or across countries) that deflating procedures have most appeal, since they can account for structural changes (or differences) in the size and

composition of the labor force (see Douglas, 1923:869). Clearly, there is a big difference between one million strikers in a total labor force of five million, or the same one million workers on strike in a labor force of twenty million. In the short run, deflating is not only unwarranted by the lack of major structural changes, but it is also inappropriate. In fact, one of the aims of econometric models is to estimate labor market effects on strike activity. Deflating procedures may actually defeat this purpose, given that part of the effect of unemployment on strikes would have been 'taken out' through the employment deflator (when employment rather than labor force figures are used).

Even in the long run, however, deflating procedures may not be the most appropriate way to cope with varying sizes of "population at risk". In fact, if long-run labor force movements are believed to be significant factors behind the trend of strikes, then why not explicitly include them among the regressors, just like any other strike determinant, instead of using deflating procedures? (see Stern, 1978; for an example of the use of such deflating procedures, see Hibbs, 1976).

Second, 'average' measures, such as size and duration, may mask wide differences in the distribution of actual strike sizes and durations, in particular when used for international comparisons. The same 'average' size, in fact, could be the result of a few, large and official strikes for the renewal of industry-wide, collective

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Finally, in the attempt to come up with a 'catch-all' indicator that would synthetically combine the three basic ones (S, W and H), Galambos and Evans (1966) have proposed a composite strike index based on an unweighted average of S, W and H. The problem with this type of indicator is that it is based on the assumption that

S, W and H contribute equally, with the same weight, to total strike variance. This is clearly faulty, though, since, as quantitative research has shown, S, W and H are three different and partly independent measures (for a critique of the Galambos-Evans index see Knowles, 1966; see also Franzosi, 1980).

### The Social Definition of a Strike and Other Definitions

Even under the unrealistic assumption that the problems of data reliability could be overcome, official strike data would still present serious limitations. In fact, "the process of defining a strike is a social process" (Batstone et al., 1978:20), involving workers as well as management. In particular, it is usually management's decision "to take the workers off the clock" (that is, suspending payment for idle time) that primarily defines a strike as such. Thus, even if the administrative bodies charged with data collection made no errors, many work stoppages would go unrecorded simply because they would not have been socially defined as strikes.

Batstone et al. (1978) have argued that, to the extent that management is more likely to accept as legitimate--and therefore not define as strikes--work stoppages over safety and working conditions, official strike statistics underestimate the number of work stoppages occurring over such issues. This implies that the traditional view that most strikes occur over wage issues may be quite exaggerated (12).

Even the relationship between strike activity and the business cycle may be distorted as a consequence of the social definition of a strike. During prosperity, management, concerned with an uninterrupted flow of production, may be less willing to classify work stoppages as strikes in fear of antagonizing workers. In the downswing phases of the cycle, when orders slacken, such fears vanish and management may actually seek confrontation in an attempt to reduce non-fixed costs. Such levelling off between bad and good times in the frequency of work stoppages defined as strikes artificially reduces the dependency of strike frequency upon the business

cycle.

The issue of the social definition of a strike brings out yet another central methodological problem in quantitative strike analysis. To the extent that strikes are a social product, i.e., the result of the interaction between at least two parties--workers and employers--strike research has given an inadequate, if not misleading, representation of social reality. In fact, most works have looked at strikes as working class actions exclusively, using strike data to assess the behavior of just one of the parties involved: the workers.

Very few studies, for instance, have concentrated on employers' reactions to labor unrest (for an example, see Stearns, 1968), on their vulnerability to strike threats, or on the strategies pursued by the different parties. To be sure, such problems have not been totally ignored in the literature. Ashenfelter and Johnson (1969)--and most econometric works thereafter--have argued, for instance, that the sign of a profit variable in a strike frequency model is undetermined, since, when profits are booming employers are more likely to meet workers' demands and thwart strike threats, while workers, at the same time, are likely to be more strike prone.

Failure of the data to provide sufficient information should not, however, be used as an excuse to avoid the problem. Alternative routes should be actively sought. Perrone, for instance, has recently argued that input/output techniques could be profitably used to gauge the structural--and not just cyclical--vulnerability to strikes of different industrial sectors and/or regions (see Perrone, 1980; see also Conell, 1980).

Within a more general perspective, Korpi has argued that the power imbalance between workers and employers, stemming from their relation to the means of production, is the key variable to understanding industrial conflict (see Korpi, 1974, 1978). In Korpi's power difference model, changes in the power position of one party with respect to the other affect the probability that the party favored by the change

will mobilize resources and engage in collective action. The implications of the model are that the actions and strategies of one party cannot be understood in isolation, without reference to the actions and strategy of the opposing party(s). Within this perspective, the range of actions available to employers is much broader than what is implied by the social definition of a strike.

Management's broader strategy will in general be one of trying to lower the probability of industrial conflict by raising the cost of workers' collective action through such tactics as: discriminating among workers in terms of pay and career opportunities, fining, isolating, and dismissing 'troublesome' workers, setting up blacklists of undesirable workers to be circulated among employers, rotating and transferring workers from one plant and one shop to another, and banning union activities from inside the plant (13).

In this respect, no less significant is the role played by a third party of industrial relations systems: the state (see Crouch, 1978). Government intervention in labor disputes, both in repressive or conciliatory terms, has a long and extensive history, ranging from use of police force, to legislative measures, labor court hearings, forced arbitration, etc. The importance of such strategies, and of repression in particular, is not only central to Marxist tradition. Repression, in fact, is a key variable in many power models, where the probability of collective action is postulated to be inversely proportional to the amount of repression applied against it (see Tilly, 1973; see also Feierabend and Feierabend, 1966). Yet no attempt has been made in the literature to find satisfying measures for the concept, or at least to provide evidence of another kind (14).

Perhaps, what is needed is a different definition of a strike, one that could account for these shortcomings. In this respect, Batstone's concept of work stoppage would be more useful than that of strike, since it takes employers' behavior partially into account. The concept retains, in fact, the central elements of the definition of



'strike'--that workers intentionally and temporarily refuse to work, pending resolution of a dispute with management, for which a grievance and/or a demand has been expressed (see also Edwards, 1981:287)--yet also offers a measure of employers' reactions, in so far as management is more or less prone to define a strike depending upon the issue.

Management, however, may be also more or less prone to make concessions and eschew even a work stoppage, depending upon cyclical and structural factors (see Conell, 1980). So, even work stoppages do not tell us the whole story. Many voiced labor grievances, in fact, never develop into either work stoppages or strikes. As Batstone et al. write: "if we want to understand strikes we have also to understand non-strikes" (Batstone et al., 1978:26) (15).

Only separate and detailed information on labor grievances, work stoppages, and strikes would account fully for the dialectic process involved in industrial relations, for the conditions that affect both workers' mobilization processes and management's willingness to give in to strikers, bargain or resist demands.

Inferring the extent of workers' discontent and class conflict from strike activity alone could be quite misleading (see Korpi, 1978; Edwards, 1981:297-301). Not only because a great deal of work stoppages never make it into the statistics, or because many grievances never even develop into work stoppages, but also because many other forms of labor protest are used, both collectively--work to the rule, i.e., rigorously applying the norms contained in the collective contract, slow-downs, non-cooperation, sabotage--and individually--absenteeism, turnover (16).

Unfortunately, the problems involved in collecting information on these other forms of industrial conflict are even more complex (except for labor turn-over) than those involved in strike statistics. As a consequence, there exists very little reliable evidence that could be used to assess labor strategy in choosing among alternative forms of industrial conflict.

## Determinants of Strikes: State of the Economy and Strike Activity

The importance of reliable data is never emphasized enough. Theory building and empirical testing, in fact, are complementary processes, with empirical testing being comprised of both data and methodology. Up to this point I have discussed at length the problems involved in relying upon official data. In the next sections I turn to focus on the methodological aspects of empirical testing. The discussion will center on two of the most successful areas of application of the quantitative approach as they have historically developed: the study of the relationship between strike activity and the business cycle, and the study of political/organizational determinants of strikes.

A fruitful area of application of the quantitative approach, and one that has enjoyed a long tradition of inquiry, has been the study of economic determinants of strikes, in particular frequency. Quantitative research has shown beyond doubt, across different institutional settings (sample periods and countries) that strike frequency follows the business cycle and the movement of unemployment in particular--the higher the level of unemployment, the lower the number of strikes (17).

The reasons for this inverse relationship were succinctly laid out by Rees (1952). According to Rees, in times of rising employment workers can more easily find a new job in case of retaliatory dismissal, and/or find part-time jobs during a strike. Employers, on the other hand, during a strike can less easily replace strikers with outsiders and in any case are less willing to interrupt production, being afraid of losing their share of an expanding market. Also, demonstration effects of rising wages in other firms are likely to induce employers to give in to strikers. Under conditions of full employment, workers are thus in a more favorable position than employers, and are more willing to go on strike.

In times of falling employment, the situation is reversed. Workers are afraid to

form new unions in non-unionized establishments, since troublesome workers can be replaced easily. Employers can fill orders out of inventories. The general downward stickiness of wages, fixed term union contracts, and falling consumer prices help to further explain the unions' more cautious approach in the bargaining process and their reluctance to strike.

This interpretation, however, has not gone unchallenged. A scholarly controversy, between those who claim that strike activity is negatively and significantly related to the level of unemployment, or more generally to the business cycle, and those who oppose such a view, can be traced back to the seminal works by Rist and March in France at the beginning of the century.

The controversy partly stems from a tendency, among all involved, to identify strike activity with just one of its components, frequency. In fact, while it is true that strike frequency follows the business cycle, as supported by much research, it is also true that frequency represents only one measure of strike activity. The other components depend much less on the fluctuations of economic activity (March, 1911, 1913; Forcheimer, 1948; Knowles, 1952:146; Andreani, 1968; Skeels, 1971; Walsh, 1975). Furthermore, while strike frequency is negatively related to the level of unemployment, there is evidence supporting a positive relationship for duration (e.g., see Turner, Clack and Roberts, 1967).

Several arguments have been advanced against the view that relates strikes to the state of the economy. We have already seen how the relationship between strike frequency and prosperity is a priori ambiguous, since during prosperity employers are more likely to meet workers' demands, but workers are likely to be more strike prone (see Ashenfelter and Johnson, 1969; Cousineau and Lacroix, 1976; Conell, 1980). Edwards has argued that this combination of forces affecting strike activity during prosperity is likely to vary from cycle to cycle. As a result, the overall effect of the business cycle on strikes is limited (see Edwards, 1981:67). In fact, according to

Edwards "the view that strike activity will depend on the business cycle has won rather too easy acceptance" (Edwards, 1981:67). In any case, economic determinants of strikes may be indeed statistically significant without, however, being the main or exclusive influence (Vanderkamp, 1970). Given the high explanatory power achieved in regressing strike frequency upon a set of seasonal dummy variables alone and/or upon a time trend, it has been contended that there is "a strong suspicion that such equations might be saying a little more than that strikes have increased over time, and that they have a seasonal pattern" (Mayhew, 1979:10; see also Franzosi, 1981).

Some scholars have even challenged the dependence of strike frequency upon the business cycle in institutional settings different from the post World War II Anglo-American ones, where strike frequency models have most successfully been tested (see Snyder, 1975, 1977). Although such arguments have been damagingly criticized both on methodological and theoretical grounds (18), there is evidence that the relationship between economic and strike variables is not stable over long sample periods (19). As a consequence, Cronin suggested that "long-term models, . . . which specify fixed and stable interactions and a constant pattern of causation, should not work" (Cronin, 1979:74). The proper temporal unit of analysis would rather consist in sets of social and economic homogeneous relationships within specific periods (Cronin, 1979; see also, Boyer, 1979).

On an even more general theoretical level, several arguments have been advanced against the view that relates prosperity to increases in the frequency of strikes.

First, Levitt argued that during prosperity, in particular after several uninterrupted years of prosperity, workers are likely to incur long-term financial obligations for housing and durable consumer goods, thereby becoming more dependent on weekly paychecks. "The prospect of a strike of even two weeks duration can be frightening to a debt-burdened worker" (Levitt, 1953:225). During depressions, on the

other hand, in a general climate of job insecurity and negative expectations about the future, workers are less likely to incur these fixed-term debt payments; credit, also, is less readily available.

Even broader social changes--according to Levitt--have contributed to making workers more dependent upon a continuous cash flow. The increasing number of young nuclear families, the higher birth rates that keep young mothers out of the job market, the diminishing number of unmarried income producing relatives living with the family, all point to the more common situation in which family well-being is dependent on one income-earning individual. The broader scope of strikes in recent times and the greater prevalence of one-industry towns also makes it harder for strikers to find temporary employment elsewhere. On the basis of these observations, Levitt concluded that "the number, frequency, and effectiveness of strikes during periods of mature prosperity may be expected to decline rather than to increase" (Levitt, 1953:226) (20).

Second, Pizzorno has argued that industrial relations are less and less limited to collective bargaining, where labor market mechanisms can significantly affect the short-term bargaining power of the parties. Political factors have come to play an increasing role everywhere. While under collective bargaining workers exchange continuity in production against greater resources (i.e., strikes vs. non-strikes), under political exchange consensus and social order are at stake. To the extent that under political exchange bargaining power is less dependent upon labor market conditions, strike activity can be expected to become less sensitive to fluctuations in the level of economic activity (Pizzorno, 1978; see also, Boyer, 1979; Franzosi, 1981:90-102). Furthermore, under political exchange not only the lower dependence upon the cycle, but the very way consensus is obtained may affect long-term movements and shapes of strikes (see Lange, 1979).

Nonetheless, despite these criticisms, the dependence of strike frequency upon

the business cycle is one of the clearest findings of econometric research. Even those authors opposed to such a view have conceded that there exists a short-run positive correlation between strikes and prosperity (see Levitt, 1953:581) or, even more strongly, that "during the 1930s as much as at other times, strike activity was related to economic conditions" (Edwards, 1981:148). What is much more debatable, and indeed very unclear, is the effect of the business cycle upon all other strike components, besides frequency.

### Beyond Economic Models of Strikes

If economic determinants play a significant role only on frequency, and each strike component measures a different aspect of strike activity what then are the determinants of size, duration and all other strike components? Economists have played a major role in pinning down the effect of the business cycle upon strike frequency. In recent years sociologists and political scientists have concentrated their attention on the other strike components, in particular size.

This is not to say that economists ever denied the existence of other forces at work. However, even the most critical and thorough among them have not gone beyond some vague notion about social and political correlates of strikes--union strength, in particular--in works otherwise wholly centered on prices, wages and unemployment (e.g., see Griffin, 1939; Knowles, 1952). The search for non-economic determinants of strikes has taken various routes.

First, long-run movements of strikes have been investigated in relation to changes in the political economy of the distribution of resources--national income, in particular. According to Hibbs, the high participation of labor forces in the management and control of the distribution of resources has shifted the locus of industrial conflict from the labor market and the private sector, where strike activity is the typical means of pressure, to the public one, where bargaining and political

exchange prevail (Hibbs, 1976; see also Korpi and Shalev, 1979, 1980).

Comparative research has shown that strike activity has gone down whenever and wherever labor oriented, social democratic parties have acquired government responsibilities. Direct access to political power has provided labor with alternative, less costly means to its ends, such as the welfare policies directed at the redistribution of national income (see Shorter and Tilly, 1974; Hibbs, 1976; Korpi and Shalev, 1979, 1980).

Second, it has been argued that major outbursts of labor unrest, or strike waves, are related to shifts in the political position of labor in the national power structure (see Shorter and Tilly, 1974) (21). During strike waves the shape of strikes changes drastically, along with the whole repertoire of labor actions. During strike waves workers face up against capital and bargain their position as a class, with implications that go far beyond the system of industrial relations where strikes normally tend to be confined. During strike waves the separation of the economic and political spheres of conflict--"a fundamental basis of the capitalist state" (Giddens, 1973:206; see also Dahrendorf, 1959:277)--breaks down and workers bring their weight to bear upon the legitimacy of the existing power structure. Also, while normally a great deal of industrial conflict occurs over the structure of wages--i.e., wage differentials among industrial sectors, among firms within a sector, among workers within a firm--during strike waves it is the distribution of income among classes, and not just within the working class, that is challenged (on the revolutionary potential of strike waves--or mass strikes as the author defines them--see Brecher, 1972).

In view of their importance, it is surprising that so few monographs exist on individual strike waves (e.g., see Pizzorno, 1974-1978), and that so little attention has been paid in the literature to the concomitant occurrence of outbreaks of industrial conflict across countries otherwise characterized by wide differences in levels of industrialization, composition of the labor force, political regimes and industrial

relations systems (e.g., see Crouch and Pizzorno, 1978).

A third line of inquiry has concentrated on the organizational basis of strikes with resource-management models of collective actions (for such models, see Obershall, 1973; Tilly, 1974; Gamson, 1975). Shorter and Tilly (1974) have shown that strike size and organization, as measured by unionization, go together (see also Andreani, 1968). It is the organizational ability of trade unions that enables them to call on strike a larger portion of the work force. In the long run, strikes are less and less the result of spontaneous outbreaks of workers' hanger. Although such angry protests are not at all uncommon, particularly in early days (see Stearns, 1974, 1975; Hobsbawm, 1964), workers have learned fast "the common sense of demanding concessions when conditions are favourable, not when hunger suggests" (Hobsbawm, 1964:144). Furthermore, strike activity is not the result of an "isolated mass" (see Kerr and Siegel, 1954) or of the least integrated members of a working class community.

Taken together, research on non-economic determinants of strikes has met with a great deal of success. Nevertheless, many problems still remain. To begin with, the empirical testing of long-term political hypotheses (Skeels, 1971; Hibbs, 1976, 1978; Korpi and Shalev, 1980) has been based almost exclusively, although to some extent successfully, on dummy variables (cabinet change, election year, party in power). Given the inadequacy of such variables, much more work is needed both in terms of model specification and variable operationalization (22). Even at the theoretical level Edwards has recently systematically challenged the view that relates strikes to shifts in the political position of labor (see Edwards, 1981; see also Smith, 1979).

Second, unionization and strike activity go together mostly during strike waves, as these are often accompanied by high jumps in union membership. This fact substantially reduces the number of available observations, since there are no more



than a handful of strike waves even over the time span of a century (see Shorter and Tilly, 1973; Edwards, 1981:28-9; Cronin, 1979:39). Thus, although the sample period on which the regression is being performed may be quite long, the effective number of observations significant for the relationship under consideration may well fall below minimum requirements.

Furthermore, unionization variables usually perform well in a regression model of number of strikers. This, however, can hardly be taken as an explanation in a causal sense. Econometricians have in fact just as successfully reversed the causal order in union growth models, where unionization becomes the 'dependent' variable and number of strikers the 'independent' one (23). Joining a union is likely to represent a further step in workers' commitment and class consciousness with respect to strike participation implying that unionization is a consequence of strike participation, rather than a cause (see Perrot, 1973; see also Edwards, 1981:29). It is only data aggregation over time that makes these two aspects of mobilization processes appear contemporaneous.

Third, it is not always easy to separate organizational effects from background effects. Do unionized firms pay higher wages because workers are organized and can strike more effectively? Or, are some firms and industrial sectors better organized because of some inherent structural features conducive to organization, collective actions and higher strike returns (see Ashenfelter, Johnson and Pencavel, 1972)? There is evidence that the same structural features lie behind low strike proneness, low strike returns and organization weaknesses (see Conell, 1980:264). In other words, there is some truth to the statement: "workers' ability to obtain concessions depended heavily on where they worked and very little on how they were organized" (Conell, 1980:83).

In conclusion, then, strike size models based on unionization may yield significant and highly explicative results. Yet, we may have done no more than

observe a strong correlation, leaving unresolved the problem of pinning down the common causes of these highly interrelated phenomena. Goetz-Girey, for instance, has claimed indeed that "the fundamental movements that originate strikes are often the same that originate union growth. When we observe a correlation between union growth and strike activity, that does not necessarily mean that union growth is at the origin of increases in strike activity. It is the same causes that affect at the same time union growth and strike activity. In particular, industrialization and economic cycles have an effect on both strike activity and union growth" (Goetz-Girey, 1965:146-7; see also Edwards, 1981:9).

Exclusive reliance on highly aggregated unionization figures also appears to have exaggerated the role of formal organization at the expense of informal shop-floor leadership (see Batstone et al., 1978).

To complicate matters even further, in most countries the number of workers involved and the number of hours lost are very sensitive to outliers, since just one or two strikes can account for as much as 70 to 90 per cent of all strikers and hours lost in a year (24). The explanation of such one or two strikes per year is probably beyond econometric testing and current data availability. It may be more fruitful to isolate large strikes and study them in a more systematic way over time, with a combination of quantitative and qualitative techniques (e.g., for a qualitative study see Brecher, 1972).

Finally, Stern (1978) has pointed out that most of these works have done little more than add political/organizational measures to economic strike models, or extend the analysis to other strike components besides frequency (e.g., see Vanderkamp, 1970; Snyder, 1975; Bordogna and Provasi, 1979). From an empirical standpoint Stern's criticisms address a severe shortcoming of this body of literature. To be fair, though, one should add that this literature has laid the theoretical foundations for an approach to strikes that would go beyond the restricted view of strikes as "the result

of faulty negotiations" (Hicks, 1957:146). Furthermore, some attempts at specifying alternative models have been made, although, admittedly, their empirical basis is not very strong (e.g., see Snyder and Tilly, 1972; Snyder and Kelly, 1976).

#### Economic Versus Political/Organizational Strike Models: Striving for Integration

Two different sets of questions will be addressed in this section: First, why has there been so little integration of the economic and political/organizational streams of research? Second, why has there been so much disparity in empirical findings?

There now exists a great deal of empirical evidence independently developed by economists on the economic determinants of strikes, and by sociologists and political scientists on the organizational and political correlates. Yet little integration of all this material has been attempted. As Waldman has rightly pointed out, such a separation between political and economic variables does not belong to historical reality and by seeking one or the other the connection is missed (Waldman, 1976:159). To a large degree, the academic division of labor and our needs as model builders may have led to this unnecessary dichotomy. More serious obstacles, however, have stood in the way of integration.

A first obstacle stems from the fact that not all strike determinants operate in the same time frame: the business cycle, for instance, affects strike activity in the short and medium run, while technological change or organization are likely to operate in the very long run. Correct model specification would require including all relevant variables in the model, and testing would have to be carried out on a time frame long enough to encompass short, medium, and long term effects.

This in itself should not be too much of a problem, but it is likely to become one in the context of regression analysis applied to a time-series model 1) where variables are likely to be trended, 2) which is likely to suffer from model misspecification, at least to some extent, since only a minor portion of all factors

involved are usually considered in econometric models. Either problem may result in autocorrelated errors (see Johnston, 1972:243-65; Granger and Newbold, 1974). The choice then becomes one of accepting unbiased but inefficient and inconsistent estimates for the coefficients in the model, or detrending in order to eliminate autocorrelation (see Granger and Newbold, 1974). However, detrending, i.e., taking the long-term component out of a series, may distort some of the relationships in the model. What if, as in our case, some of the variables are in fact related precisely in the long run?

On a more general level, one could question the very appropriateness of regression techniques applied to time series data. Indeed, from econometric outputs there is no way of telling to which time frames--frequency bands as they would be called in time series spectral analysis--estimated coefficients refer to. Such knowledge must necessarily come from a priori information. One would feel more comfortable if such a priori assumptions could be tested empirically. Techniques specifically conceived to handle time dependent data, such as spectral analysis or ARIMA models, would be more adequate (25).

Another consequence of the various time frames in which different factors operate is that testing alternative sets of hypotheses will require different degrees of involvement in data collection. For instance, business cycle effects, that operate in the short and medium run, can be easily tested with relatively short series and minor data collection efforts (considering also that most economic data are readily available). Organizational and political factors, which are more likely to operate in the long run, will need longer series. Considering that generally non-economic data are not readily available, the task confronting social scientists working on quantitative strike research is not an easy one, or one that could lead to 'fast and ready' answers. Data collecting work is in itself of little reward, but in the end it may prove very fruitful, as has been suggested on several occasions in the course of this paper.

A common practice, followed by researchers in recent years as a start toward integration, has consisted in regressing various strike measures upon the same set of key independent variables: economic, political and organizational (e.g., see Vanderkamp, 1970; Skeels, 1971; Snyder, 1975; Bordogna and Provasi, 1979; Franzosi, 1981). Edwards writes "it . . . seems useful to explore the role of the same model in explaining different features of strikes, while bearing in mind that strike frequency and worker involvement may be influenced by different factors" (Edwards, 1981:73).

However, if such a solution may have some intuitive appeal, it has serious methodological pitfalls. In fact, if the models are indeed misspecified, and strike frequency and worker involvement are influenced by other factors besides the ones accounted for in the model, then the regression estimates will be inconsistent, i.e., with very large variances. This fact rules out standard tests of significance as a criterion of acceptance or rejection of the null hypothesis and casts serious doubt on the overall value of such procedures. To the extent that the statistical consequences of model misspecification are not kept in mind, such procedures may easily lead to incorrect substantive interpretations.

Integration, therefore, is not likely to come from these kinds of mechanical procedures. A more promising solution seems to come, instead, from those theories that can incorporate all relevant factors--be these economic or political/-organizational--in a coherent and comprehensive framework. Korpi, for instance, has brought together relative deprivation and economic hardship in an essentially political power model, but where changes in economic conditions can alter a group's stock of power resources.

The argument sounds very much like arguments underlying bargaining models. Korpi's model, though, is much broader. It can account for both short-term changes in the power position of a group, as affected by such factors as the business cycle, and long-term changes in the locus of conflict, as affected by shifts in the political

position of labor. Economic variables are central to bargaining models. Differences in power, as perhaps affected by economic conditions, but by no means by these ones alone, constitute the core of Korpi's model (see Korpi, 1974, 1978).

Turning to the second question, there are several reasons why there should be so much disparity in empirical findings, given that most studies have differed in their selection of the basic observation period (month, quarter, year), of the various measures of strike activity (frequency, size, duration, volume), of the sample periods analyzed, and of the specification of the overall model and/or of the functional form of a particular relationship.

We would expect the choice of temporal aggregation to make a difference, in view of the fact that the more highly aggregated the data, the more will be missed in terms of high frequency variation. Furthermore, overtime aggregation is likely to distort relationships among variables. For instance, one of the main foci of strike research has been the price expectation-wage relationship (see Ashenfelter and Johnson, 1969; Pencavel, 1970; Snyder, 1975; Hibbs, 1976). Empirical tests of this hypothesis have often been based on yearly data (see Snyder, 1975; Hibbs, 1976; see also Smith, 1979). Yet the difficulties of satisfactorily estimating short lag-structures with highly aggregated data are well known (see, Mundlak, 1961) (26) (27).

Second, several different strike measures, deflated in several different ways, have been used (28). All in all, there seems to be agreement among scholars in the field that frequency is negatively related to the business cycle, and that the fewer strike occurrences during recessions tend to last longer. Findings are also consistent in showing much less dependency of other strike components upon the cycle. Yet, these last findings may be more the result of failure on the part of researchers to recognize first, and then deal with, difficulties involved in properly estimating the relationship between level of economic activity and strike size and time lost.

One of the problems is that the distribution of strikes by size and time lost is

highly skewed, making such measures very sensitive to a handful of exceptionally large and long strikes. As it has been suggested already, it may be more appropriate to take out such strikes for separate analysis, either quantitative or qualitative. There is evidence that a series of number of strikers deflated in the way proposed here may be as sensitive to the business cycle as the number of strikes itself (see Franzosi, 1981).

Another source of difficulties is that strike size and duration, may have a considerable impact upon production and the economy in general (see de Wasseige, 1952; Franzosi, 1981). Thus, when production or GNP figures are used as indicators of economic activity in models of strike size, duration, or volume, we are likely to get results distorted by reciprocal causation (29). Model specification, in other words, is likely to make a difference, and the various economic indicators cannot be used indifferently with the expectation that they will perform the same way.

There is another aspect of model specification that can further complicate comparability of results. In fact, even models that are identically specified and that use the same level of aggregation can still differ in terms of the functional form of a particular relationship. For instance, Edwards, in re-estimating the Ashenfelter and Johnson model, makes use of an inadequately justified "difference between the rates of change of wages and prices" instead of a distributed lag form (see Edwards, 1981:72). Although such differences may sound like details, it should be remembered that incorrect specification of the functional form of the relationship between variables is as serious a problem, with as undesirable statistical consequences as omission of variables itself (see Johnston, 1972:243-4).

Finally, no two works in this literature have analyzed the same sample period. We already saw above that the strength and even the sign of the relationship between strikes and their determinants is likely to change over time across homogeneous historical periods. It is no surprise, then, that works that have addressed different and

relatively short sample periods have obtained different results. What is more surprising is that a clear chronology of all such homogeneous periods for various countries is still lacking.

Because of such differences, it is hard to get a sense of cumulative evidence from this literature. It is not, however, fortuitous that works have differed so much in terms of sample period, level of aggregation and model specification. A number of these works were, in fact, aimed at showing that economic models do not work for certain countries, certain periods or certain strike measures. Despite their obvious inadequacies, economic models had dominated the field for many decades. It is not surprising that a critical stance developed.

One can now see a similar reaction developing vis a vis political/organizational models, Shorter and Tilly's in particular. Edwards, for instance, on the basis of regression results for the United States, concludes: "The failure of union density to operate significantly in combination with the more general economic variables casts very great doubt on the view that organizational factors must be given an independent role in the determination of strike activity" (Edwards, 1981:77). Yet, against Edwards' own recommendation (see Edwards, 1981:73), only one measure of strike activity was used. Furthermore, a measure well known to be poorly related to unionization figures, i.e., strike frequency, was chosen.

It is time that such discrepancies be addressed directly, with an eye toward isolating homogeneous periods during which some given set of correlates seems to work, and toward explaining differences in performance between alternative specifications (30). We should not be content with simply showing that differences exist. We ought to explain such differences. For instance, we cannot reject economic models simply because GNP does not perform as well as unemployment in a regression equation (e.g., see Vanderkamp, 1970; Snyder, 1975). In fact, not all economic indicators can be expected to perform the same way. Yet, no attempt has been made



to go beyond reporting the mere findings (see, however, Franzosi, 1981).

In view of these considerations there is a growing need for replication of previous works, in order to directly address discrepancies in the field. Failure to do so would perpetuate the indefinite testing of models which are based on different levels of aggregation, sample periods and model specifications, and would not allow accumulation or integration of knowledge.

Discrepancies in empirical findings, though, may actually hide much more fundamental problems. Because, as every practitioner knows all too well, empirical evidence is never clear cut. There is always a choice involved which will bring about the researcher's own construction of reality. The guiding criterion in selecting among contrasting bits of information--e.g., magnitude, sign, significance of regression coefficients--may well ultimately depend upon the researcher's personal and theoretical biases, upon his belonging to a shared scientific paradigm. So, by and large, our theories guide us in the process of reconstructing social reality. Yet, this reality is used to uphold theories that we so independently set out to verify.

#### Further Issues and Alternative Approaches

These critical considerations bring out yet another set of problems that have not been addressed up to this point. In fact, the data and methodological issues considered in the previous sections have not challenged the value of the quantitative approach. More fundamental criticisms, however, can be and have been brought against it, uncovering inadequacies that directly bear upon the approach as such, rather than on its applications.

In the present section, I will consider some such charges as the consistency of aggregate data with different causal structures and different behavioral, individual-level hypotheses, the lack of integration between micro and macro levels of analysis, and the phenomenological critique.

It has been argued that, although quantitative strike research purportedly endeavours to disentangle causal structures, in reality it is merely correlational rather than causal (see Durand, 1977) (31). The criticism seems to be empirically grounded. As I have mentioned earlier, in fact, wage determination and union growth models have just as successfully reversed the causal order, treating the rate of change of money wages and unionization as the 'dependent' variables with a measure of strike activity among the regressors. The data, in other words, are indeed consistent with different causal structures, as Durand would argue. Although statistical causality tests are available (e.g., see Granger, 1969; Sims, 1972), they are not easily applicable to short series.

The problem is that if Ordinary Least Squares are used to estimate models where there is more than one current endogenous variable in the relation, whichever variable one selects as the 'dependent' variable, the remaining endogenous variable(s), in general, will be correlated with the error term. Under these circumstances, OLS estimates will be biased and inconsistent (see Johnston, 1972:376). For proper estimation, simultaneous equation techniques should be applied (either single equation--Two Stage Least Squares (2SLS), Limited Information Maximum Likelihood--or system of equations--Three Stage Least Squares and Full Information Maximum Likelihood).

The high degree of interdependence and reciprocal causation among the main variables usually has been recognized and acknowledged (e.g., see Ashenfelter and Johnson, 1969). Yet, available econometric strike models, are all Ordinary Least Squares (OLS) single-equation models, in which all independent variables are treated as exogenous, i.e., determined outside the model, independent of all other variables considered. Only in a few cases have 2SLS estimating methods been applied (see Pencavel, 1970; Snyder, 1977; Franzosi, 1981).

Even simultaneous single equation estimating techniques, such as 2SLS, however, do not directly address the complexity of the relationships involved, although they

provide unbiased and consistent estimates. Only with multiple equation models is such complexity built into the model, rather than being a matter of statistical estimation.

There is yet another reason why system of equations models could prove fruitful. In fact, such multiple input/multiple output simultaneous equation models could deal more easily with the reciprocal interdependencies of all strike indicators, accounting for all of them simultaneously. However, there are several reasons why such an ambitious project may not as yet be feasible.

First, the fact that authors who have separately tackled all three lines of research--strike frequency, union growth and wage determination models--have used systems of equations for all models, except for strike frequency, should make us aware of the possible difficulties involved (e.g., see Ashenfelter and Johnson, 1969; Ashenfelter and Pencavel, 1969; Ashenfelter, Johnson and Pencavel, 1972). If one considers that strike frequency models, on which these authors have concentrated their attention, have usually achieved the best statistical results, little hope can be held of being able to specify multi-equation strike models--which would include correct specification of the other strike components as well.

Second, although there now exists a great deal of empirical evidence on the way institutional and organizational forces shape the pattern of strikes, most of this literature does not lend itself to quantitative analysis (e.g., see Clegg, 1976; Batstone et al., 1978). Thus, while we now know a great deal about how certain mobilization processes occur, very little of that knowledge can be used in the context of econometric analysis. Furthermore, many of the phenomena that are well known to affect strikes on a theoretical level (e.g., repression) have received little attention at the empirical level, even in very simple quantitative analyses. Again, much more work would be needed in terms of data collection and operationalization of theoretically relevant variables before one could hope to specify non-trivial, multi-equation strike models.

A second argument, parallel to the one just considered, has charged that quantitative aggregate strike models may be consistent with many underlying hypotheses at the individual level (see Mayhew, 1979; see also Edwards, 1981:64). The argument does not challenge the value of quantitative strike research as such. It does suggest, however, that little is known about the behavioral micro-level hypotheses underlying aggregate models (32) and that there is a need for further micro-level research and integration.

Going from the macro to the micro level of analysis, though, is not an easy matter since it will usually involve working with an entirely different data set (see Stern, 1978). Considering that official strike data rarely have been gathered at the micro level, hypothesis testing at this level will also require primary data collecting (33). In any case, the difficulties involved in specifying econometric models which combined micro and macro information, with even the simplest kind of linear aggregation functions, may be insurmountable at the present state of the art.

It is qualitative case studies of strikes that have provided much of the detailed micro-level information (34). In fact, while quantitative works have centered on macro-level variables, case studies have focused on community characteristics, plant-level labor relations, workers' consciousness and the like, i.e., on the micro correlates of strikes.

Given the richness of the case-study literature, it is regrettable that no systematic use or even mention of its findings has been made in quantitative works (for an exception, see Edwards, 1981). Such neglect may depend upon the fact that it is hard to incorporate such qualitative evidence in econometric models or to generalize from case studies. Furthermore, although such detailed knowledge is very helpful within a more general theoretical framework, by itself it is of limited use.

One way of bridging the gulf and making the contribution of this vast body of literature both available to quantitative researchers and statistically more

representative would be to compare all this information in a more systematic way, and quantify it, as if it were coming from a sample survey--a non random sample of the case-study strikes. Unfortunately, no such task has been undertaken. Another way would be to integrate qualitative and quantitative evidence, using the depth and richness of case studies to illustrate the micro-level implications of statistical findings (e.g., see Edwards, 1981), or to verify the micro-level assumptions upon which aggregate studies rest (e.g., see Batstone et al., 1978).

These suggestions should not be taken to imply that statistical extrapolation is the only way of going from a case to its universe, the only way of understanding the relationship of the part to the whole (Burawoy, 1979:XIV). Neither should they be taken to imply that qualitative case-study research only plays a secondary role, subordinate to quantitative analysis. They should only be seen as ways of bringing micro-level information within aggregate quantitative analyses, as ways of covering the most patent deficiencies.

Neither the case study nor the quantitative literature have addressed each other's issues, each keeping within its own research tradition, and thus preventing comparison and integration of findings at various levels of analysis. Rare exceptions indicate that there would be a great deal to be learned from their systematic integration (see Batstone et al., 1978; Edwards, 1981) (35).

Finally, a more radical critique of the quantitative approach has come from phenomenologists who have argued that quantitative evidence is of no value for sociological inquiry. They have challenged the Durkheimian view of the very existence of immutable social facts (see Durkheim, 1938, 1951) (36). As a result, they have also questioned the feasibility of standardizing measurement across individuals, of constructing statistical models and of making inferences (see Douglas, 1967). Finally, phenomenologists have regarded official records mainly as the result of bureaucratic caprice (see Cicourel, 1968, 1974). Reliance on official data would miss out on

people's thoughts and perceptions, specifically on what strikes really mean to workers and on the micro-level processes that lead them to take such course of action.

Without altogether eschewing the use of quantitative methodologies in the social sciences, it is true that quantitative research--of the econometric type in particular--has often focused too narrowly on what is quantifiable, thus presenting a view that even in the authors' opinion is unsatisfactory (37). All too frequently it is forgotten that "there are people behind numbers" (Perrot, 1968:120).

As we have seen time and again in the course of this paper, exclusive reliance on quantitative evidence neglects some fundamental aspects of strike activity, such as the role of informal leadership, management's actions, workers' strategies in terms of demands (38), repertoire of actions and strike techniques (see Shalev, 1978:8), and of work groups' changing power as affected by their position in the production process at the plant level (see Batstone et al., 1978:32).

Given the difficulties involved in studying the relationship between class conflict and the organization of the labor process at the macro level, many scholars in the Marxist tradition argue that macro-level works miss the central characteristic of class conflict: that the capitalist organization of the labor process in the factory and the changes unilaterally introduced by management are the prime source of conflict at the point of production (see Ignazio and Costa, 1975; see also Regini and Reyneri, 1971; Burawoy, 1979; on the macro-level characteristics of the relationship between technological change and strike activity, see Shorter and Tilly, 1974).

Furthermore, as working class actions get crunched in regression coefficients, tests of significance and the like, in most quantitative works much of the historical embedment of strikes is lost, together with the background of workers' everyday life and conditions, their thoughts and aspirations, and the deep sense of injustice and moral outrage that at times motivates them to act collectively (see Barrington Moore, 1978; see also Brecher, 1972; Crew, 1979; Procacci, 1971).

It is through the filters of workers' consciousness that macro-level factors affect strike activity. "Because strikes are not an abstract phenomenon: they involve a decision, more or less thought out, more or less spontaneous, that sets in motion the motivations, secret or avowed, the representations, the hopes, the beliefs of the actors. Strikes are a beautiful historic object: there one finds men. And time." (Perrot, 1968:123).

### Summary and Conclusions

A few summary considerations deserve to be brought together.

First, the data upon which quantitative works are based present shortcomings that have been shown to bias our understanding of strikes. Errors in measurement (underreporting, etc.) distort relationships among variables. Moreover, official statistics fail to provide information on strike techniques (walk-outs, sit-downs, checker-board, etc.), on other relevant features of strikes (demonstrations through the shop, marches to city hall, etc.), and on the broader spectrum of working class actions (such as slowdowns, work to the rule, overtime ban, etc.). The lack of this type of data has dangerously led to the 'tout court' identification of class conflict with just one of its manifestations—strikes. Explaining how and why workers choose among alternatives within their repertoire of actions is as interesting—and as important—as discerning how and why strike decisions are taken. Furthermore, what is the distribution of actions in this repertoire and how does it change over time?

Second, the presence of methodological problems is likely to undermine the empirical basis of many of the theoretical conclusions reached: the various standardizing procedures used to account for long-term structural changes in the labor force have led to an unnecessary proliferation of strike indicators; furthermore, they have introduced artificial correlations when employment or unemployment figures appear among the regressors; OLS techniques have been used to estimate relationships

that are likely to be affected by reciprocal causation, yielding estimates affected by simultaneous equation bias; model misspecification, finally, has had serious consequences on the estimates obtained.

To the extent that data and methodological shortcomings undermine the empirical basis of much strike research, our confidence in some of the theoretical conclusions may be very much weakened. All too often such problems are at best simply acknowledged in statistical appendices without further concern of "how the problems raised affect the conclusions reached" (Edwards, 1981:281).

Theoretical weaknesses, though, do not only stem from questionable empirical testing. Quantitative works have also failed to incorporate economic and political/-organizational models and micro and macro models within unified theoretical frameworks. Furthermore, attention has rarely been paid to the role of strategies on the part of employers or the state in fostering or damping labor militancy, thus failing to deal with all parties involved (workers, employers and increasingly so the state).

Fourth, a set of issues has emerged that require further investigation, either because of neglect (e.g., strike waves, employers' behavior), or because of unresolved discrepancies. There is an increasing need for replication of previous work, with the aim of directly addressing discrepancies stemming from differences in the level of aggregation, model specification or sample period. Furthermore, Edwards' systematic challenge of Shorter and Tilly's hypotheses indicates that there may be differences in national patterns of strikes that cannot be easily explained in terms of current theories. In depth comparative research may help overcoming also such discrepancies.

Finally, our discussion has shown that exclusive reliance on official strike data neglects fundamental aspects of strike activity, such as the role of informal leadership, the dialectic between workers and employers that leads to strikes, the ways technological change may lead to social unrest through the disruption of



customary social relations within the plant. Although these processes are well documented by case studies, by and large, quantitative analysis has failed to incorporate their findings.

In conclusion, quantitative methods cannot tell the whole story. They should be viewed as one among a number of tools of inquiry. As Stearns put it "I have found it impossible to develop a sufficiently subtle interpretive framework relying on statistical materials alone; other evidence, derived from more detailed study of individual strikes, is essential, and while it is potentially quantifiable the practical difficulties are considerable" (Stearns, 1974:9).

## Notes

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(1)

New bills are passed and old ones revoked as a consequence of large-scale demonstrative strikes. Even governments fall on the wake of labor "political" strikes (e.g., see the right-wing Tambroni government in Italy, brought down by labor disorders in Genoa). Clearly if information on this type of demonstrative, "political" strike is not recorded, the short-run relationship between strikes and politics is missed. This does not exclude, however, the existence of a second, more pervasive type of relationship between strikes and politics. This second type can be captured by the data even if large-scale demonstrative strikes are not recorded, either because they do not meet the definitional criteria of the collecting agencies, like in the Italian case, or because such strikes are not common labor tactics, like in the United States. Democratic administrations in the U.S. and labor governments in England have been shown to be positively related to the frequency of strikes (see Ashenfelter and Johnson, 1969; Pencavel, 1970). The shape of strikes in Italy has been shown to change drastically during the years of the "historic compromise" strategy pursued by the Italian Communist Party after 1975 (see Franzosi, 1980). On the relationship between strikes and politics see the later section "Beyond Economic Models of Strikes".

(2)

Korpi, for instance, argues that British definitions leave out the great majority of strikes, indeed something of the order of 90% of all strike occurrences (Korpi, 1981:75).

(3)

It should also be pointed out that there may be intrinsic difficulties in reporting the number of workers involved. Depending upon their position in the production process, the strike of key work groups may leave idle many more workers than the ones actually on strike. Whether these workers not directly striking but not working either are included in the statistics may again depend upon management's decision to take all idle workers "off the clock", as much as upon the power relationships existing within the plant. Many of the arguments on the alleged cost of strikes are based on such extended concepts of workers' involvement in strikes, directly and indirectly, on the producer and consumer side; see for all Chamberlain and Metzger Schilling, 1954.

(4)

Data collecting on the part of highly trained bureaucratic technicians at the dependence of the Ministry of Labor appears to minimize these problems for the French case (see Perrot, 1968). Systematic double-checking of information from

various sources, and accurate scanning of local press can further reduce problems of under-reporting (for the United States see Peterson, 1937:170-2; see also Edwards, 1981). In my discussion, I have preferred to rely on the Italian example exclusively, in order to be more specific about the ways definitions and collecting procedures may affect our understanding of strikes.

(5)

Turner reports that in England in the motor industry and coalmining approximately 80% and 30% respectively of all work stoppages, as recorded by the firms themselves, never make into the official statistics (Turner, 1969). For the United States, Kuhn argues that "short strikes are much more likely to go unreported" and quotes from the U.S. Labor Department that a change in the collecting procedures in the mid-1950 increased the number of strikes by an estimated 5% (Kuhn, 1961:194; see also Edwards, 1981:306-13). On the basis of a sample survey among metalworking firms in Sweden, Korpi estimates an annual average of 1.8 unreported strikes per 10,000 workers, and a figure at least three times higher if all collective incidences are included, and not just strikes (Korpi, 1981:73).

It should be noted, however, that from a purely methodological point of view the discontinuities that are introduced in a time series as a result of changes in the collecting procedures, definitions or other, may be less of a problem than systematic underestimation. Changes in the mean at given points in time can in fact be dealt with in the general linear model through covariance analysis (see Johnston, 1972:192-207) or in time series ARIMA models through intervention analysis (e.g., see Box and Tiao, 1975). Systematic bias, on the other hand, may be potentially a more serious problem, unless one could assume that recording biases do not change overtime (in which case the Least Squares estimates of the coefficients would only differ by an unknown transformation constant). Such an assumption, however, is not likely to be realistic in most cases and in strike statistics in particular. As we have seen, Korpi has shown that the frequency of short unofficial work stoppages, that most often go unrecorded, varies overtime as a result of both economic and political conditions.

(6)

The decision involved often being 'political' rather than merely technical (see Stearns, 1974:9).

(7)

Again, too highly aggregated spatial data would make less firm any conclusion about the effect of urbanization (see Stern, 1976; Shorter and Tilly, 1974; Edwards, 1981). No less serious is the problem of data aggregation across industrial sectors. In fact, the more highly aggregated the figures provided at the industry level, the less one can say about the effect on strike activity of the level of technology, the composition of the work force by sex and age, the average plant size, the degree of monopolistic concentration in an industry, the skill mix, etc. (see Shorey, 1975; Conell, 1980:112-57). This paper has primarily focused on the temporal aspects of strike activity, mostly because the distribution of quantitative strike research works themselves is highly skewed toward time series, rather than cross sectional issues. However, in particular in recent decades, a few studies have dealt with inter-regional and inter-industry propensities to strike (see Knowles, 1952; Kerr and Siegel, 1954; Britt and Galle, 1972; Shorter and Tilly, 1974; Shorey, 1976; Stern, 1976; on the relationship between plant size and strike activity, see Cleland, 1955; Cass, 1957; Ingham, 1970; Shorey, 1975; Prais, 1978; Churnside and Creigh, 1981).

(8)

On the shape of strikes see Shorter and Tilly, 1971, 1974; for Italy, see also,

Bordogna and Provasi, 1979; Franzosi, 1980; the United States seem to be an exception to international trends, see Edwards, 1981:23; on the long-run processes that have affected the shape of strikes, see Shorter and Tilly, 1971, 1974; for the short-run aspects see also, Britt and Galle, 1974.

(9)

On the trend of strikes see Douglas, 1923; Douty, 1932; Gomberg, 1944; Jurkat and Jurkat, 1949; Turner, 1963; Goodman, 1967; Silver, 1973; Sapsford, 1975; Franzosi, 1980.

(10)

For a criticism of the "withering away of strikes" thesis, see Elridge, 1968:23-35 and Ingham, 1974:11-23.

(11)

For an example of the use of volume, see Hibbs, 1976; for a critique, see Shalev, 1978:482-3; Korpi and Shalev, 1979:186.

(12)

For a detailed analysis of the immediate causes of strikes and the importance of wage issues, see Durand and Dubois, 1975; Durand, 1977; Edwards, 1981:36-41.

(13)

For an account of such employers' practices see Stearns, 1968; Della Rocca, 1976; Treu, 1976; Conell, 1980.

(14)

For attempts at quantifying employers' behavior through the number of layoffs and transfers in the context of econometric analysis, see Franzosi, 1981; for an example of how to construct quantitative indices of labor repression through labor court sentences, see Melucci and Rositi, 1975.

(15)

For a detailed account of the processes leading to a strike that never occurred, see Batstone et al., 1978.

(16)

Korpi estimates that collective actions which do not involve work stoppages (what Korpi calls collective demonstrations, such as slow-downs, overtime bans, etc.) are by far more frequent than work stoppages and much less likely to go beyond the factories' gates and come to the attention of the press or even of unions or employers organizations' headquarters. Their number is perhaps three times as high as the number of unofficial strikes, as recorded by the firms themselves (Korpi, 1981:72-5).

(17)

There exists an abundant production of empirical works focusing on the relationship between the business cycle and strike activity--frequency in particular. Several different economic indicators have been used as measures of the cycle: price and wage indices, import values of raw materials, industrial production, composite cyclical indicators, and GNP. After World War II, when reliable labor force survey data have become available, unemployment rate has been the most commonly used cyclical indicator.

For the United States, see Hansen, 1921; Griffin, 1939; Yoder, 1940; Jurkat and

Jurkat, 1949; Rees, 1952; O'Brien, 1965. Ashenfelter and Johnson's work (1969) has become a landmark in quantitative strike research dealing with economic determinants. For more recent work, see Skeels, 1971; Snyder, 1975, 1977; Lipsky and Farber, 1976; Conell, 1980; Edwards, 1981.

For the United Kingdom, see Gomberg, 1944; Knowles, 1952; Pencavel, 1970; Bean and Peel, 1976; Shorey, 1976; Mayhew, 1979; Cronin, 1979. For a collection of quantitative works on U.K. strike activity, see Evans and Creigh, 1977.

For Canada, see Vanderkamp, 1970; Smith, 1972; Walsh, 1975; Snyder, 1977; Cousineau and Lacroix, 1976; Smith, 1979.

For France, see Rist, 1907, 1912; March, 1911, 1913. For a discussion of subsequent French literature, see Bouvier, 1964. For later works, see Goetz-Girey, 1965; Andreani, 1968; Perrot, 1974; Shorter and Tilly, 1974; Snyder, 1975. See also Perrot, 1968.

For Italy, see Snyder, 1975; Bordogna and Provasi, 1979; Franzosi, 1981, 1981b. For a review of the Italian literature, most of which is qualitative, see Franzosi, 1981, 1981b.

(18)

For a critique of Snyder's work, see, Smith, 1979; Edwards, 1981:52-4,67,79-81; Franzosi, 1981:190-6.

(19)

For France, see Perrot, 1974; Andreani, 1968; Goetz-Girey, 1965; for the United States see Snyder, 1975, 1977; for the United Kingdom see Cronin, 1979.

(20)

Levitt's view has not gone uncriticized. First, his argument primarily deals with long-term movements of strikes (see Goldner, 1953). Levitt himself, in fact, concedes to Rees that in the short period the correlation between strike frequency and the business cycle is positive (Levitt, 1953b). In the second place, strikes in most countries are typically short, and therefore unlikely to put workers in serious financial difficulties. In any case, it is always possible for workers on strike to postpone monthly installment payments or purchase subsistence goods on credit from local shop keepers (see Goldner, 1953; Blitz, 1954; see also Levitt, 1954). The extent of the phenomenon of one industry towns would also appear to have been exaggerated. In historical perspective, even broader issues raised by Levitt, such as increasing birth rates, have lost some of their relevance, reflecting a situation typical of the fifties, and disproved by later trends.

(21)

For a more systematic treatment of strike waves see Shorter and Tilly, 1973, 1974; see also, Cronin, 1979; Edwards, 1981.

(22)

On the inadequacy of the political indicators commonly used in econometric works, see also Edwards, 1981:81.

(23)

There exists a vast literature on the subject; see for all Hines, 1964; Ashenfelter and Pencavel, 1969; Bain and Elsheik, 1976; Romagnoli and Rossi, 1980. For a survey, see Parsley, 1980.

(24)

This is particularly true for those countries, where collective bargaining is carried out

both at the plant level, and at the industry or even economy level (on the relationship between bargaining structure and strike activity, see Clegg, 1976). As a result, outliers in strike size could be less severe for the United States, where plant level bargaining is predominant. Outliers for the United States are more likely to occur for strike duration. There, in fact, much more so than anywhere else, strikes tend to be quite long (see Clegg, 1976; Edwards, 1981).

(25)

For an introduction to spectral analysis, see Jenkins and Watts, 1968; for ARIMA models, see Box and Jenkins, 1976; for an application of spectral analysis using strike data, see Franzosi, 1980.

(26)

On over time aggregation see also, Moriguchi, 1970; Zellner and Montmarquette, 1971.

(27)

There is yet another way in which temporal aggregation may seriously affect empirical results. In the last decade, for instance, there has been much debate as to whether trade unions cause inflation through collective bargaining. The question is whether trade union militancy, as measured by unionization and/or strike activity, can push wages up independently of labor market conditions. What comparative econometric work has failed to recognize is that over time aggregation has significantly affected their answer to this question. In fact, when collective agreements are signed at year end, wage increases will be effective as of the beginning of the following year. Strike activity, though, would have occurred prior to the signing of the agreement. Using yearly data, a lagged relationship would have to be specified. But at other times, for those cases where collective agreements are renewed at mid year, strike activity and wage increases fall in the same year, therefore making the relationship contemporaneous. As a consequence of aggregation problems, the sign of the relationship is thus highly dependent upon the type of specification adopted--contemporaneous or lagged. Furthermore, in these cases, where relatively short series are used (usually 20 to 25 years for the postwar period) and where the effective number of observations may be further reduced by the fact that collective agreements usually last between two to three years, an outlier can make a significant difference, tilting the relationship one way or the other (for the problems involved in estimating wage determination models for the Italian case, see de Caprariis and Franzosi, 1981).

(28)

For a list of which measures have been used by which scholars, see Stern, 1978.

(29)

For the use of such variables, see Vanderkamp, 1970; Snyder, 1975.

(30)

For a similar consideration in relation to Snyder's failure to justify results that run counter prevailing findings and to confront the discrepancy, see Smith, 1979.

(31)

Even partially accepting Durand's critique, in my opinion, having isolated a set of social, political and economic variables related to strikes is in itself no little result. The issue, though, is more than just academic, as it can have both ideological relevance and policy implications. Claiming, for instance, that labor militancy is a cause of inflation, on the basis of regression results, means assuming a causality that

runs from strike activity to the rate of change of money wages. As we have seen, though, econometric models have just as successfully reversed the causal order. Since econometric results often have policy implications with ideological overtones, too narrowly focused empirical investigations may be not simply faulty but even dangerous.

(32)

Indeed, at the lowest level of analysis very little is known about people's reasons for joining social movements. Olson has argued that, unless differential rewards and sanctions to particular individuals are applied, it would actually run counter individual self-interest to apply resources in the group's interest, in particular for large, latent groups (see Olson, 1965). Groups, however, do form and individuals do engage in collective actions (see Smith, 1974), although at the present state of the art very little is known about the individual level mechanisms that lead to such processes.

(33)

Except for early periods when information on individual strikes were provided. As a result, research works that have analyzed last century data have been much more specific and insightful than works based on recent, highly aggregated data (see Perrot, 1974; Conell, 1980).

(34)

The case study literature is enormous. For some examples, see Warner and Low, 1947; Gouldner, 1954; Turner, Clack and Roberts, 1967; Elridge, 1968; Batstone et al., 1978.

(35)

Recently, there have appeared investigations that combined the micro and macro approaches in quantitative--although non-econometric--analyses of large samples of individual strikes and their characteristics (see Durand and Dubois, 1975; Durand, 1977). Such works are attempts at retaining much of the micro-level information available in case studies, and at the same time achieving the generality and representativeness of quantitative studies. While the main emphasis of quantitative studies based on official data is usually longitudinal, works based on sample surveys are almost of necessity cross-sectional, given the intrinsic difficulties involved in collecting a large sample of strikes over time.

As a consequence, the issues addressed are different. Studies based on official time-series data deal with the temporal components of strikes--trend, cycle, seasonality--and their determinants, while cross-section works primarily focus on intra- and inter-plant characteristics with no time dimension (if one excludes comparisons between few points in time when previous surveys happen to be available; see Durand and Dubois, 1975; Durand, 1977). To the extent that strike activity shows significant differences over time, one difficulty research works based on sample surveys encounter is that it is hard to generalize their findings over time.

Additional problems arise if the survey is taken from a sample of strikes or plants across several industrial sectors in countries where collective bargaining is characterized by both industry and plant level bargaining (see Clegg, 1976; Franzosi, 1981, 1981b). The level of mobilization in each sector may in fact differ primarily as a consequence of the particular bargaining situation of a sector or plant at the time of the survey. Although, ideally, one would like to see both levels of analysis performed within the same work, the data collecting task confronting such a research design is enormous.

(36)

For an answer to the phenomenological critique, at least for quantitative strike

research, see Edwards, 1981:284-91; see also McCarthy, 1959.

(37)

In expressions of the type, "We would have liked to submit this hypothesis to empirical testing. Unfortunately the data were not available." or "The statistical techniques did not allow us to test this hypothesis more satisfactorily." And the problems thus get pushed aside.

(38)

The distribution of strikes by their immediate cause, in terms of the reasons given for striking, i.e., expressed demands, has long been available for most countries. On the basis of such figures, analyses of strikers' demands have been conducted (see Knowles, 1952; McCarthy, 1954; Edwards, 1981). However, given that only a handful of categories of causes are usually reported, the information provided are necessarily limited. As a consequence only gross generalizations about the over time distribution of strikes by cause, and their meaning, in terms of working class changing strategies in the face of changing identifiable external conditions, can be afforded (see McCarthy, 1954).



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