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Unemployment Insurance and Strikes

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Abstract

In several states workers who are unemployed because of a labor dispute can collect unemployment benefits. Due to imperfect experience rating, such policies can create a public subsidy to strikes. This study examines whether these policies affect strike activity. In particular, both cross-sectional and fixed effects models are employed to test whether an increase in the public subsidy inherent in unemployment insurance leads to an increase in strike frequency.

I. *Introduction*

Since the turn of the century, all levels of government in the United States have experimented with providing welfare and unemployment benefits to strikers. Such experiments are always controversial, with the controversy in part focused on whether the benefits increase strike activity. For example, in *Grinnell Corp.* v. *Hackett,* a case involving the payment of unemployment compensation to strikers in Rhode Island, the First Circuit Court said:

> [The] present record suffers from a fundamental defect. It provides no support for a causal relationship between the receipt of benefits, which unions obviously desire and often actively seek, and longer, costlier strikes. . . . [The] record lacks even a crude form of what we assume would be the most relevant and probative type of evidence—statistical comparisons of the length and cost of strikes in states granting unemployment benefits (Rhode Island and New York) and the length and cost of strikes of similar size in similar industries in other states not granting such benefits.[1]

This paper presents empirical evidence on whether the provision of unemployment insurance to strikers affects strike activity.

Some argue that because government transfers make it easier for strikers to support themselves, it is a truism that such transfers increase strike activity.[2] There is, however, a logical problem. Although government transfers will almost certainly make it easier for strikers to support themselves, that in itself may not lead to increased strike activity. If government transfers strengthen the bargaining position of the union, then one might expect a rational employer to be *more* willing to settle without a strike, or, failing that, to settle sooner after a strike has begun rather than later. Arguing this way, one could claim that government transfers will reduce strike activity. Thus, it is not obvious how government transfers affect strike activity and empirical evidence must be used to resolve the issue.

Much of the available evidence is anecdotal and taken from case studies. For example, Thieblot and Cowin (1972) used a case study approach to investigate U.S. strikes in which workers received government transfer payments; Gennard (1977) did the same for Great Britain. More recently, Kennan (1980) used modem statistical methods to analyze the relationship between strike duration and unemployment insurance policies in New York and Rhode Island. Finally, our monograph (Hutchens, Lipsky, and Stern, 1989) analyzed links between strike activity and a broad range of government welfare and unemployment insurance policies. In that work, however, we did not test hypotheses on the effect of experience rating, under which a firm pays taxes that reflect the cost of the unemployment insurance benefits received by its employees. This study summarizes and extends that work by presenting new results on links between experience rating and strike activity.

The second section of this paper describes current policies for paying unemployment benefits to strikers. There is considerable confusion surrounding this issue. It is widely believed that only two states—New York and Rhode Island—routinely permit strikers to collect unemployment benefits. Although these two states do allow strikers to collect benefits (in New York after an eight-week waiting period and in Rhode Island after a seven-week period), a majority of other states allow workers unemployed because of a labor dispute to collect unemployment benefits under specific (but not unusual) conditions.

In the third section we use a joint cost theory of strikes to develop testable hypotheses linking unemployment insurance policies to strike activity. The fundamental proposition of joint cost theory is that strike activity is a decreasing function of the combined (union plus management) cost of strikes. To the extent that the provisions of state unemployment insurance laws reduce this combined cost, they will increase strike activity.

The fourth section tests hypotheses using state level data on strike activity. Since unemployment insurance laws vary across—but not within—states, their effects should be revealed through interstate differences in the level of strike activity. Using both cross-sectional and fixed-effects models, we find solid evidence of a link between the provisions of state unemployment insurance laws and strike activity.

II. *Current Practice*

The Social Security Act of 1935, which established the unemployment insurance system, grants the states considerable autonomy in establishing rules governing claimant eligibility for unemployment benefits. For example, each state can determine whether, and under what conditions, workers unemployed because of a labor dispute can collect unemployment insurance benefits. Federal tolerance of state autonomy on this issue, reinforced by several key Supreme Court decisions, results in considerable diversity in the unemployment insurance eligibility rules that affect strikers. We focus on three rules that are of particular empirical importance: the stoppage of work rule, the innocent bystander rule, and the New York-Rhode Island rule.[3]

The Stoppage of Work Rule. In 1988, 24 states permitted strikers to collect unemployment benefits during a labor dispute if their employer continues to operate at or near normal levels. An eligible striker can collect benefits after the normal waiting period, which in most states is one week after the claimant files for benefits. Thus, in stoppage of work states strikers receive benefits virtually from the outset of the strike.

In a sense this provision provides strikers with insurance against a failed strike. In work stoppage states, if a strike succeeds in forcing employers to close down or reduce the scale of their operations significantly, the strikers cannot collect unemployment benefits (unless the state has other provisions permitting strikers to receive benefits). But if a strike fails in the sense that employers hire replacements (or strikebreakers) or are able to use supervisors or other nonstriking employees to continue to operate at or near normal levels, the strikers can collect benefits.

This work stoppage provision has become more important with time. In the immediate post-World War II period the practice of operating during a strike was largely confined to advanced technology industries, such as oil refining, telephones, and broadcasting. In recent years, however, the practice spread to more labor-intensive industries such as newspapers, hotels, paper, and shipbuilding. There are several possible explanations for this development. Automation and other forms of new technology give many employers the technical capacity to operate during strikes. Moreover, as product markets become increasingly competitive, employers are .probably more likely to protect their sales, revenues, and profits by operating during a strike. Finally, erosion in union strength and solidarity as well as the high unemployment rates of the 1970s and early 1980s may have made it easier for struck employers to hire replacements. Whatever the reason, the work stoppage provision becomes more important as firms become more likely to operate during strikes.

The Innocent Bystander Rule. In 1988, 44 states had an “innocent bystander” provision that permits workers who are unemployed because of a labor dispute but not affiliated with the strikers to obtain unemployment insurance benefits. More precisely, an innocent bystander must not (1) participate in the labor dispute (e.g., by picketing or refusing to cross a picket line), (2) finance the dispute (e.g., through the payment of union dues that are used to finance strike benefits), or (3) have a direct “interest” in the dispute in the sense of benefitting from its settlement. Suppose, for example, that a unionized group of production workers strike their employer, causing the employer to lay off his nonunion office personnel: can the office workers collect unemployment benefits? If the state UI program has an innocent bystander provision, and the office workers satisfy the above three conditions, then they would collect benefits after the normal (usually one week) waiting period. If the state UI program does not distinguish innocent bystanders from actual strikers, both the production and office workers would be disqualified.

The New York-Rhode Island Rule. Strikers in New York and Rhode Island can obtain UI benefits after a waiting period of eight weeks in New York and seven weeks in Rhode Island. New York not only disqualifies strikers but also innocent bystanders during the first eight weeks. Thus, New York uses a “no fault” approach, disqualifying all workers unemployed because of a labor dispute in its early stages, and qualifying them for benefits thereafter. This approach avoids the complicated problem of interpreting and administering the innocent bystander and work stoppage provisions. In contrast, Rhode Island’s UI program contains both an innocent bystander and a work stoppage provision. Rhode Island workers who qualify under these provisions wait only one week before obtaining benefits. In a sense then, Rhode Island’s law is both more “liberal” and more complicated to administer than New York’s.

Do such provisions influence strike activity? In the course of collecting our data, we asked this question of all state unemployment insurance administrators. Although none had hard evidence, several speculated that prohibitions against striker receipt of unemployment insurance affect behavior. For example, the Commissioner of the Department of Economic Security in Minnesota wrote, “I believe that there can be little doubt that total disqualification from benefits during a strike, as provided by our law, is a significant consideration for employees faced with a decision whether or not to strike.”[4] Similarly, the Department of Employment and Training

Commissioner in Vermont wrote, "I believe that the work stoppage portion of the labor dispute disqualification provision is the most significant in affecting behavior of the parties in collective bargaining or industrial relations."[5] Thus, there is an anecdotal basis for the proposition that the payment of unemployment insurance benefits to strikers increases strike activity.

III. *Theory*

As noted in Kennan's (1986) literature review, although there are many economic theories of strike behavior, no single theory dominates. This paper adopts the "joint cost theory" developed by Reder and Neumann (1980) and Kennan (1980) to derive hypotheses about links between unemployment insurance and strike activity. This theory argues that strike activity is a function of the combined (employer plus employee) cost of a strike, with greater combined cost reducing strike activity. We adopt this perspective not because we believe joint cost theory is dominant—it is largely untested and confronts many worthy competitors—but rather because it yields sharp, testable hypotheses.

Reder and Neumann argue that as the combined cost of strikes rise, bargainers develop protocols that make it easier to reach an agreement. Protocols are "the rules or conventions governing the procedure for negotiating collective-bargaining agreements." These rules or conventions specify the procedures for negotiations, what topics will be covered, and how to know when a settlement is reached; they may include provisions for mediation or arbitration; and they may deal with rates and methods of compensation, work rules, and fringe benefits. For example, many municipal fire departments and firefighter unions abide by the protocol that their salary settlements should exactly equal the salary settlements reached by the municipalities with their police unions.

Although more elaborate protocols facilitate agreements, they are also costly to negotiate. As such, not all contingencies and procedures will be covered by protocols. “In specifying a protocol, bargainers balance the cost reduction from reduced strike activity against the increased cost of specifying a more detailed protocol…” (Reder and Neumann, 1980, p. 871). It follows that as the combined cost of strikes rise, bargainers will tend to negotiate more elaborate protocols and thereby reduce both the frequency and duration of strikes.

This logic applies irrespective of the division of strike costs. If strike costs are large and equally divided between the parties, then both will seek to negotiate protocols that minimize strikes. If strike activity imposes large costs on one party but not on the other, then the party that bears the larger costs will tend to make concessions that yield elaborate protocols. Either way, as combined strike costs rise, strike activity should fall, *ceteris paribus.*

This theory yields clear hypotheses on how government transfer payments affect strike activity. If the transfer payments are wholly financed out of taxes on the struck employer, they will not alter the combined cost of strikes. In this case, although the transfer payments reduce the cost of strikes to strikers, they increase the cost of strikes to the employer by an equal amount. Since the combined cost of strikes is not altered, strike activity remains unchanged. Strike activity is only affected if the parties to the strike do not bear the full cost of the transfer payment.

In the case of unemployment insurance, employers pay “experience-rated” taxes; when a worker receives $1.00 in unemployment insurance benefits, the employer should pay $1.00 in taxes. But experience rating is not perfect. In all states there are firms that pay taxes that are not commensurate with payments to their employees. For example, according to Topel (1984), in 1973-1976 a California firm with an insured unemployment rate of 4 percent would pay a tax of 62 cents on each additional dollar of UI benefits. If that firm's insured unemployment rate went above 4.6 percent, its tax on additional UI benefits was zero. Moreover, prior to 1979 unemployment insurance benefits were not subject to the federal income tax. Such "tax preferences" are a form of subsidy to the recipient from the rest of society. Although experience rating usually insures that the struck employer bears some of the cost of unemployment insurance benefits to strikers, imperfect experience rating and tax preferences insure that the employer will generally not pay the full cost. Under these conditions a joint cost model implies that when unemployment insurance benefits are paid to strikers, strike activity will increase.

In particular, one would expect that, because of imperfect experience rating, states with "work stoppage" provisions in their unemployment insurance laws will have greater strike activity, *ceteris paribus.* In addition, in these states a greater "subsidy" to strikes should cause a higher level of strike activity. More precisely, let B represent the weekly UI benefit received by a striker, let t be the worker's marginal tax rate, and assume that, due to imperfect experience rating, a firm usually pays taxes equal to $m\%$ of the UI benefits received by its employees. Then in the event of a strike, the subsidy per striker equals $B[1 - m + t/(1 - t)]$.[6] According to joint cost theory, an increase in this subsidy results in greater strike activity, *ceteris paribus.* Of course, this hypothesis not only applies to work stoppage states, but also to New York and Rhode Island where strikers receive benefits after a period of disqualification.

The "innocent bystander" rule raises an interesting problem in this regard. Since strikers cannot benefit from this provision, and since the firm pays for it through experience rated taxes, one could argue that the innocent bystander rule increases the combined cost of strikes and thereby reduces strike activity. But this logic may be flawed; it is only valid if in the absence of the rule, firms do not compensate innocent bystanders.[7] Suppose that, in the absence of the rule, the firm bears the full cost of compensating innocent bystanders for their income loss. (This could occur through pre-strike inventory build up, through post-strike catch up, or through strike prone firms paying a compensating wage differential to potentially innocent bystanders.) In this case, an innocent bystander rule could conceivably *reduce* the cost of strikes to the firm; given imperfect experience rating, under this rule the firm would bear only part of the cost of compensating innocent bystanders. There is then good reason to hypothesize that the innocent bystander rule reduces the combined cost of strikes.

IV. *Evidence*

In order to test these hypotheses, we collected data on strike activity, control variables, and UI program parameters in 50 states and the District of Columbia over the period 1960-1974. Data availability dictate this time period. Although strike activity occurs at the bargaining unit level, for testing purposes it is sensible to use states as the unit of analysis—unemployment insurance policies vary across, but not within, states. If transfer policies affect strike activity, then, holding other factors constant, one should observe predictable patterns of strike activity across states and over time.[8]

The analysis uses several measures of state unemployment insurance policy. Three binary (0-1) variables indicate whether or not a state has a work-stoppage rule, an innocent bystander rule, or a New York-Rhode Island rule. As shown in Tables 1 and 2, the set of states using the work stoppage and innocent bystander rule changed with time, while those using the New York-

Rhode Island rule did not. The lack of intertemporal variation in the New York-Rhode Island rule means that the effects of this rule can only be analyzed in a cross-section.

We use the maximum UI benefit in a state as a proxy for the unemployment insurance subsidy per striker. Ideally, the subsidy would be measured as *Bk*, where *B* is the weekly UI benefit received by a striker, and $k = [1 - m + t/(1 - t)]$. The maximum UI benefit is a good measure of *B* for three reasons. First, the maximum is strictly a function of state unemployment insurance policy.[9] Second, given their comparatively high earnings, union members are likely to receive unemployment benefits that are at or near the maximum. Third, data on the maximum are available for all states over the 1960-1974 time period.

Insert Table 1 Here

Insert Table 2 Here

To compute the subsidy, one would ideally obtain information on *k*, multiply *k* by the maximum UI benefit, and thereby obtain a proxy for *Bk.* That is not, however, possible; there do not exist data on experience rating for all states over the period of analysis. There are, however, a handful of states (referred to as the “reserve-ratio” states) for which experience rating can be measured over most of the time period. For these states one can compute *k* and, using the maximum UI benefit, obtain a second measure of the UI subsidy per striker. These states can then be used to examine whether the maximum UI benefit is a good proxy for the subsidy per striker.

Our measure of *k* in these reserve ratio states is derived from Topel (1983, 1984). Since $k = [1 - m + t/(1 - t)]$, *k* can be computed from data on *t* and *m.* Topel used information on state and federal income taxes to compute *t* and developed methods for using information on experience rating to compute the marginal cost of a dollar of UI benefits for a specific firm in a specific state. Denote this marginal cost as *e(u,s),* where *u* is the firm's insured unemployment rate (the steady state fraction of the firm's employees that are unemployed and receiving UI), and *s* indicates jurisdiction VI We computed *e(u,s)* for firms within 27 reserve-ratio states for the period 1960-1971. Data were not available for other states or years. Our state specific measure of m, denoted as *m(s),* is the average of *e(u,s)* over a fixed distribution of firms with different insured unemployment rates. Thus, letting *f(u)* denote the distribution of firm-specific insured unemployment rates,

$$m(s) = \int e(u,s)f(u)du.$$

Note that since *f(u)* is the same for all states, *m(s)* has the desirable quality of being strictly a function of state experience rating policies; it does not depend on the industrial composition of a state. An appendix is available from the authors with further details on the computation of *k.*

We use data on strike frequency as a measure of strike activity. More precisely, our dependent variable is the natural logarithm of (# strikes in state *i* in year *j*)/ (# labor force participants in state *i* and year *j*). The numerator is computed from the U S. Department of Labor Work Stoppage Historical File, and the denominator is computed from Census data.[10] Although one can conceive of other state-level measures of strike activity (e.g., the average size or duration of strikes in a state), these measures are either theoretically inappropriate or potentially flawed by sample selection bias.[11] Strike frequency yields a clean test of the hypotheses.

Finally, since it is only after holding other factors constant that we expect a relationship between transfers and strike activity, the analysis requires a vector of control variables. That raises difficult conceptual problems. Whereas theories of strike activity focus on the bargaining unit, this research uses state level data. Variables that are appropriate controls for an analysis of strike activity at the level of the bargaining unit (e.g., a measure of the capacity to buffer input shocks through inventory fluctuations) may not be appropriate or, if appropriate, may not be available for an analysis of strike activity at the state level. How does one specify a parsimonious vector of control variables in a state-level analysis of strike activity?

We resolved this quandary by collecting data on a long list of variables and examining whether results on UI program parameters were robust to variation in the vector of controls. Our list of 27 control variables included economic variables (e.g., industry composition), political variables (e.g., whether or not the state is a right-to- work state), and demographic variables (e.g., percent female in the state).[12] The empirical literature on strikes links several of these variables to strike activity.

Because specification analyses raise questions about a model's statistical properties, we restricted our examination of alternative control variables to the 1970 cross-section. This permitted us to take advantage of the 1970 Census which provides a wealth of state-level data. Our strategy was first to use the 1970 data to arrive at a parsimonious vector of control variables. We then took the resulting specification as given and examined whether results could be replicated in other cross-sections and over time.

Table 3 displays key results for 1970. The model in the first column includes dummy variables indicating that the state has a work stoppage (*WS*) rule, a New York-Rhode Island (*NYRI*) rule, or an innocent bystander *(IB)* rule. Our theory predicts positive coefficients on each of these rules. As indicated in column 1, although the coefficient on *WS* and *NYRI* are positive, neither is statistically significant at conventional levels.

The models in columns 2-4 of Table 3 test whether the effect of these UI rules depend on the maximum benefit. Given imperfect experience rating and the tax treatment of unemployment benefits, a higher maximum in states with any of the three rules should lead to lower strike costs, less comprehensive protocols, and increased strike activity. The column 2 model interacts the WS rule with the maximum UI benefit. Since the coefficient on this interaction is positive and statistically significant, the results indicate that a higher maximum benefit in states with the WS rule is associated with a higher strike frequency.

Although similar results obtain when the IB rule is interacted with the maximum benefit, if both the WS rule interaction and the IB rule interaction are included in the regression, then neither is statistically significant (column 3).[13] This is because the two interaction terms are highly correlated. Accordingly, this evidence indicates that *either* the WS rule interaction *or* the IB rule interaction is associated with higher strike frequencies. But there is no basis for a claim that one of the interactions is the principle source of the association. Thus, column 4 presents a fourth regression with an interaction between the maximum UI benefit and a variable indicating states that use either the WS rule or the IB rule. The coefficient on this interaction term is positive and statistically significant, a plausible result indicating that for states with either the WS rule or the IB rule, an increase in program generosity, as proxied by the maximum benefit, is associated with more strikes.

Insert Table 3 Here

Such results in part depend on the other independent variables in the model. The Table 3 models include six control variables that are proxies for the complex web of social and economic forces that shape strike activity within a geographic unit. In addition to these, we tested a long list of alternative independent variables. Results on the interaction variable were remarkably insensitive to such changes in specification (for example, see Appendix Table A.1).

The temptation to seek "explanations" for the coefficients on the control variables in Table 3 should be resisted. Consider, for example, the statistically insignificant coefficient on "% establishments with 100+ employees." This coefficient should not be interpreted as revealing that establishment size has little to do with strike activity. That hypothesis is best tested with plant level data. When the state is the unit of observation, this variable acts as a proxy for a network of forces that influence strike activity in the state.

Table 4 provides estimates which show that the 1970 results can be replicated in other years. The relationship between the interaction variable and strike frequency is thoroughly robust across different years.[14] These estimates indicate that the 1970 result is not simply a statistical artifact.

Insert Table 4 Here

Such results could, however, be challenged as a consequence of unobserved variables. Suppose there exist unobserved state-specific determinants of strike frequency that are positively correlated with the interaction variable. Then the cross-section results do not address the issue of whether the unemployment insurance system actually leads to more strikes; the results may simply reveal that the interaction variable is correlated with some unobserved state-specific determinant of strike frequency that persists over time.

In order to examine this issue, we ran a fixed-effects version of the model. In essence we pooled together 15 years of data on the 51 jurisdictions and estimated the Table 3, column 4 model with a separate intercept for each jurisdiction. Since separate intercepts control for time-invariant (fixed) unobserved state characteristics, the fixed-effects model allows us to test whether the interaction result is due to state-specific fixed effects that are unrelated to the unemployment insurance system.

Two data issues are notable. First, because the *NYRI* variable equals "1" throughout the 15-year sample period, it is perfectly correlated with the state-specific intercepts and must be excluded to avoid collinearity. Second, although we had data on strike frequency for all states and all years, that was not the case for the independent variables. For example, data on *WS* and *IB* are not available for 1961, 1963, 1965, and 1969. In such cases we interpolated the missing data.

Table 5 presents the fixed-effects results which are quite similar to the cross-section results. The coefficient on the interaction variable is positive and statistically significant, implying that the cross-section results are not simply a product of unobserved, state-specific, fixed effects. Moreover, the magnitude of this coefficient is similar to that in the cross-section. All regressions indicate that in states that use an innocent bystander or stoppage of work" disqualification rule, a one percent increase in the maximum benefit is associated with a .5 percent increase in strike frequency, *ceteris paribus.* Since we only tested one model with this pooled data set, the Table 5 results are statistically meaningful in the sense that they are not a consequence of testing numerous models with the same data. Thus, this table offers strong evidence of a link between the payment of UI benefits to strikers and strike frequency.[15]

Joint cost theory yields a clear interpretation of the above result: a greater subsidy per striker results in greater strike activity. Yet this interpretation is only tenable if the maximum UI benefit is a good proxy for the subsidy per striker. Since this subsidy not only depends upon the maximum but also experience rating, there are grounds for doubt. For the joint cost interpretation to be fully convincing, better information on the subsidy per striker must be obtained.

We obtained such information by using data on experience rating and federal and state income taxes to compute the subsidy per striker, *Bk,* where *B* is the maximum UI benefit and $k = [1 - m + t/(1 - t)]$. Due to data limitations, the subsidy could only be computed for 27 reserve-ratio states over the period 1960-1971. (An Appendix available on request presents details on computation of *k.)* In this sample the correlation between *B* and *Bk* was .93, suggesting that the maximum is a good proxy for the subsidy.

Insert Table 5 Here

Table 6 presents fixed-effect results. The model in column 1 uses the previous interaction variable (the product of the maximum UI benefit and a variable indicating whether a state uses either the WS or the IB rule). Although the coefficient on this variable remains positive, in the smaller sample it ceases to be statistically significant at conventional levels.[16] In column 2 we replace the maximum benefit with our measure of the subsidy per striker. The key finding is that the coefficient on the new interaction variable is positive and attains a higher level of statistical significance than the coefficient on the column 1 interaction variable. As such, a joint cost interpretation of the earlier results is tenable. The Table 6 estimates indicate that replacement of the maximum with the subsidy per striker strengthens the statistical relationship between the unemployment insurance system and strike activity. The evidence is consistent with the hypothesis that a greater UI subsidy per striker results in more strikes.

V. *Conclusion*

This study establishes a link between the unemployment insurance system and strike frequency. A higher maximum UI benefit is related to a higher strike frequency in states that use "innocent bystander" or "stoppage of work" disqualification rules. The relationship not only arises in cross-sectional models for 1960, 1966, 1970, and 1974, but also in a fixed-effects model for the period 1960-1974. Moreover, this relationship is not trivial: In states with these disqualification rules, a one percent increase in the maximum benefit is associated with a .5 percent increase in strike frequency, *ceteris paribus.* There is good reason to believe that this is a causal relationship; not only are the results consistent with a joint cost theory of strikes, but also, when it is possible to regress strike frequency on a measure of the subsidy per striker, an even stronger relationship obtains. Providing unemployment insurance to strikers does indeed increase strike activity.

Notes

[1]*Grinnell Corp* v. *Hackett*, 475 F. 2d 449 at 459 (1st Cir., 1973).

[2]In the Circuit Court's decision in the New York Telephone case, Judge Owen wrote, "I regard it as a fundamental truism that the availability to, or expectation of a substantial weekly, tax-free payment of money by a striker is a substantial factor affecting his willingness to strike or, once on strike, to remain on strike, in the pursuit of desired goals. This being a truism, one therefore would expect to find confirmation of it everywhere. One does." *New York Telephone Co.* v. *New York State Department of Labor*, 556 F. 2d 388 (2d Cir. 1977), 96 LRRM 2921 at 2926.

[3]Hutchens, Lipsky, and Stem (1989, Chapters 2 and 3) discuss other less important rules.

[4]Letter written by Rolf Middleton, Commissioner, Minnesota Department of Economic Security, 390 North Robert Street, St. Paul Minnesota, January 6, 1982.

[5]Letter from Sandra D. Dragon, Commissioner, Department of Employment and Training, State of Vermont, P.O. Box 488, Montpelier, Vermont, dated December 28, 1981.

[6]When the benefits are not taxed, strikers receive a benefit *(B)* that is equivalent to X in taxable income, where $X(1-t)=B$. That implies a subsidy of $X-B=B(1/(1-t)=B[t/(1-t)]$. Since employers only pay mB, the total subsidy is $B[1-m+t/(1-t)]$.

[7]The literature contains no evidence on whether innocent bystanders are in fact compensated. This is a topic for future research.

[8]We did look for data on strikes by bargaining units. None of the available data sets had necessary information on key control variables or on the distribution of bargaining unit membership across states. A goal for future research would be to collect and analyze such data.

[9]In contrast the average benefit in the state depends not only on state policy but also on environmental variables like the average wage in the state or the state's ratio of part time to full-time workers. Since the maximum is an instrument of and depends only on state policy, empirical results on the maximum yield information on the effects of state policy.

[10]Consistent with a theory of joint costs, the numerator includes all strikes. Alternative measures (e.g., "number of strikes during the negotiation of new contracts" or "number of strikes over economic issues") are highly correlated with this measure, and yield results similar to those presented here. The denominator is interpolated from data on the resident civilian population in the state in a given year (U.S. Bureau of the Census, Series P-25 No. 460, Table 2 and No. 876, Table 4), and from 1960 and 1970 Census data on labor force participation in the state. Although the ideal denominator would be the number of bargaining units in the state, such data do not exist. We also estimated models with number of union members and number of establishments in the denominator. Results were thoroughly insensitive to such changes.

[11]A joint cost theory yields no hypotheses on the determinants of the average size of strikes (the number of workers involved per strike) in a state. Moreover, to analyze the average size of strikes one should control for the average size of bargaining units in a state, and such data are not available. Although a joint cost theory does yield hypotheses on strike duration, our state level data on the average duration of strikes are inappropriate for testing these hypotheses. Our duration data come from a self-selected sample of bargaining units that actually experienced a strike, and there is no way to control for selection bias. See Hutchens, Lipsky, and Stem (1989) for the full argument.

[12]More specifically, in addition to the control variables in Table 3, the following control variables were tested: percent prime age male, net migration rate, value added per employee in

manufacturing, percent of establishments with 20+ employees, average size of establishment, average hourly earnings of production workers in manufacturing, percent change in average earnings between 1969 and 1970, median income of families, unemployment rate, index of industrialization, index of affluence, dummy variable for state located in the south, dummy variable for states with right-to-work laws, and fraction of employees in mining, construction, manufacturing, trade, finance, and services. Our final vector of control variables was primarily selected on the basis of individual t-statistics.

[13]Since only two states use the NYRI rule, a similar interaction is not feasible for this rule. The residuals for New York and Rhode Island would be forced to zero, implying an implausible model that perfectly explains strike frequency in New York and Rhode Island.

[14]Note that the coefficient on the interaction variables tends to decline over time. This is in part because the models were run with the maximum benefit measured in nominal dollars. When the maximums were deflated by a price index, these differences nearly disappeared. Signs and t-statistics would not be affected by deflation.

[15]As a further check, we estimated a fixed-effects model with the maximum UI benefit as an exogenous variable in the six states that used neither the “innocent bystander” nor “work stoppage” rule throughout the 1960-1974 period. Consistent with expectations, the coefficient on the maximum benefit was small and statistically insignificant.

[16]We tested whether the column 1 coefficients differ from those in the other 24 jurisdictions over the same period. Based on an F-statistic of 1.8562, with 8 and 543 degrees of freedom, we cannot reject the null hypothesis of no difference at a .05 level.

Table 1

Table 1

Existence of Work Stoppage and Innocent Bystander Provisions of State Unemployment Insurance Laws Regarding Strikers, 1961

	Yes	No
Work Stoppage Provision	AK, CO, DE, GA, HA, ID, IL, IN, IW, KN, ME, MD, MA, MI, MS, MO, MT, NE, NH, NJ, NM, NC, ND, OH, OK, PA, RI, SD, TX, UT, VT, VA, WA, WV, WY	AL, AZ, AR, CA, CT, DC, FL, KT, LA, MN, NV, NY, OR, SC, TN, WI
Innocent Bystander Provision	AK, AZ, AR, CO, CT, DC, FL, GA, HA, ID, IL, IN, IA, KN, LA, ME, MD, MA, MI, MS, MO, MT, NE, NV, NH, NJ, NM, NC, ND, OH, OK, OR, PA, RI, SC, SD, TN, TX, VT, VA, WA, WV, WY	AL, CA, DE, KT, MN, NY UT, WI

Table 2

Table 2

Changes in Unemployment Disqualification of Strikers, 1961–1974

Year	Start Work Stoppage	Stop Work Stoppage	Start Innocent Bystanders	Stop Innocent Bystanders
1961				
1962		NC		NC
1963		MI, OH		OH
1964		CO		
1965				
1966		ID		
1967		NJ		NJ
1968	NJ		NJ	
1969				
1970				
1971		VA		
1972				
1973				
1974	MN			

Table 3

Table 3

1970 Strike Frequency[a] Regressions That Include Interaction Variables (t-statistics in parentheses)

Independent Variable	1	2	3	4
WS = 1	.228 (1.7)	−.651 (1.6)	−.190 (.4)	.192 (1.5)
NYRI = 1	.455 (1.4)	.522 (1.7)	.494 (1.7)	.508 (1.7)
IB = 1	−.008 (.1)	−.034 (.2)	−.668 (1.6)	−.470 (1.9)
WS × Max. UI Benefit		.015 (2.3)	.008 (1.0)	
IB × Max. UI Benefit			.010 (1.6)	
[Either *WS* = 1 *or* *IB* = 1] × Max. UI Benefit				.010 (2.4)
Unemployment Rate	−.031 (.7)	−.064 (1.4)	−.079 (1.7)	−.074 (1.6)
% Establishment with 100+ Employees	16.221 (1.2)	5.261 (.4)	2.488 (.2)	10.502 (.8)
% of Labor Force in Unions	4.188 (5.4)	4.363 (5.9)	4.355 (6.0)	4.349 (5.8)
% Females in Labor Force	−10.502 (3.0)	−11.150 (3.3)	−11.589 (3.5)	−11.940 (3.5)
% Urban in Labor Force	−1.165 (2.2)	−1.240 (2.5)	−1.254 (2.5)	−1.287 (2.4)
% of Population in Poverty	−.002 (.1)	.009 (.6)	.017 (1.2)	.014 (1.0)
Intercept	.896 (.7)	1.453 (1.2)	1.655 (1.4)	1.540 (1.3)
N	51	51	51	50
R-Square	.563	.616	.641	.618
F	5.875	6.420	6.332	6.303

[a]Frequency = ln (# strikes in a state in 1970/labor force size in state in 1970).

Table 4

Table 4

Strike Frequency Regressions for Different Years
(t-statistics in parentheses)

Independent Variable	1960	1966	1970	1974
WS = 1	.247	.018	.192	.008
	(1.2)	(.1)	(1.5)	(.1)
NYRI = 1	.106	.390	.508	−.074
	(.2)	(1.4)	(1.7)	(.2)
IB = 1	−.790	−.350	−.470	−.583
	(2.3)	(1.5)	(1.9)	(2.3)
[Either *WS* = 1 *or*	.019	.017	.010	.008
IB = 1] × Max. UI Benefit	(2.3)	(2.4)	(2.4)	(2.6)
Unemployment Rate	.112	.056	−.074	−.014
	(1.8)	(1.1)	(1.6)	(.3)
% Establishment With	−32.509	19.187	10.502	−.909
100+ Employees	(1.4)	(1.4)	(.8)	(.1)
% of Labor Force in Unions	1.977	2.576	4.349	5.711
	(1.7)	(3.7)	(5.8)	(7.2)
% Females in Labor Force	−2.262	−10.832	−11.940	−14.298
	(.6)	(3.9)	(3.5)	(4.3)
% Urban in Labor Force	.550	−.606	−1.287	−.586
	(.6)	(1.2)	(2.4)	(1.1)
% of Population in Poverty	−.003	.002	.014	.037
	(.2)	(.2)	(1.0)	(2.5)
Intercept	−3.359	.114	1.540	1.434
	(2.5)	(.1)	(1.3)	(1.3)
N	50	50	50	50
R-Square	.475	.584	.618	.690
F	3.533	5.471	6.303	8.678

See bottom of Table 3 for the definition of strike frequency.

Table 5

Table 5

Fixed Effects Regressions on the Frequency of Strikes, 1960–1974 (t-statistics in parentheses)

Independent Variable	
WS = 1	–.055 (0.6)
IB = 1	–.653 (3.3)
[Either *WS* = 1 *or* *IB* = 1] x Max. UI Benefit	.010 (4.3)
Unemployment Rate	–.072 (6.7)
% Establishments with 100+ Employees	15.210 (1.8)
% Labor Force in Unions	1.410 (0.4)
% Females in Labor Force	–4.742 (3.3)
% Urban in Labor Force	1.514 (1.4)
% of Population in Poverty	–.019 (3.7)
Intercept	–2.586 (2.8)
N	763
R-Square	.782
F	42.781

See bottom of Table 3 for the definition of the strike frequency variable. The estimated model includes state dummy variables with New York the excluded state.

Table 6

Table 6

Fixed Effects Regressions on the Frequency of Strikes in 27 States, 1960-1971 (t-statistics in parentheses)

Independent Variable	1	2
WS = 1	.205	.141
	(1.4)	(1.0)
IB = 1	–.400	–.495
	(1.5)	(2.2)
[Either *WS* = 1 *or* *IB* = 1 x Max. UI Benefit	.002 (0.6)	—
[Either *WS* = 1 *or* *IB* = 1] x Subsidy	—	.005 (1.7)
Unemployment Rate	–.066	–.068
	(3.9)	(4.0)
% Establishments with 100+ Employees	24.135 (1.4)	17.075 (1.0)
% Labor Force in Unions	2.116	2.280
	(1.8)	(1.9)
% Females in Labor Force	–1.801	–1.764
	(0.6)	(0.6)
% Urban in Labor Force	–3.924	–3.610
	(1.8)	(1.7)
% of Population in Poverty	–.029	–.029
	(3.1)	(3.2)
Intercept	.365	.306
	(0.3)	(0.2)
N	324	324
R-Square	.819	.820
F	37.122	37.531

See bottom of Table 3 for the definition of strike frequency.

Appendix Table A.1

Appendix Table A.1

Strike Frequency Regressions with Alternative Control Variables
(t-statistics in parentheses)

Independent Variable		
WS = 1	.222 (1.6)	.204 (1.4)
NYRI = 1	.663 (2.1)	.587 (1.7)
IB = 1	−.494 (2.0)	−.461 (1.8)
[Either *WS* = 1 *or* *IB* = 1] × Max. UI Benefit	.009 (2.1)	.010 (2.4)
Unemployment Rate	−.131 (2.1)	−.090 (1.5)
% Establishment with 100+ Employees	−1.078 (1.1)	10.506 (.6)
% of Labor Force in Unions	5.098 (4.4)	3.885 (3.3)
% Females in Labor Force	−11.187 (2.4)	−11.952 (3.4)
% Urban in Labor Force	−.446 (.6)	−1.309 (2.3)
% Population in Poverty	.010 (.7)	.015 (.6)
South = 1		.087 (.4)
Right−to-Work Law = 1		−.063 (.4)
Average Hourly Earnings		.129 (.4)
% of Labor Force in Mining	.141 (.0)	
% of Labor Force in Construction	16.221 (2.0)	
% of Labor Force in Transportation	−1.444 (.2)	
% of Labor Force in Trade	−5.928 (.9)	
% of Labor Force in Finance	−11.463 (1.1)	
% of Labor Force in Services	.807 (.5)	
Intercept	1.914 (1.0)	11.270 (.8)
N	50	50
R-Square	.703	.624
F	4.872	4.597

See bottom of Table 3 for the definition of strike frequency.

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