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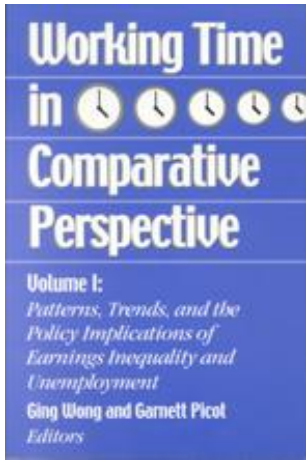
Measuring the Effects of Short-Time Compensation on Workforce Dynamics



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9
Measuring the Effects of Short-Time Compensation on Workforce Dynamics

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This chapter examines existing research on the relationship between short-time compensation (STC) use and patterns of labor market adjustment by firms. We note that the research often fails to model the actual choices available to firms and their workers in which both reductions in compensated hours and layoffs are feasible. We show that the strategies used previously to calculate the extent to which time on STC affects time on layoff have yielded a wide range of estimates. We also explore new data from a recently completed evaluation of STC programs in the United States. These data confirm the need to study labor force adjustment strategies along multiple dimensions, but they also illustrate the difficulties involved in assessing the precise impact of STC use on workforce adjustment.

North American employers—particularly those in the United States—have often been alleged to prefer layoffs to hours reductions when responding to output or cost shocks (Burdett and Wright 1989; Feldstein 1976, 1978). Feldstein, for example, highlighted two features of the U.S. system that encouraged firms to opt for layoffs during cyclical downturns: 1) unemployment insurance (UI) benefits were, at that time, nontaxable to workers who received them, and 2) the UI payroll tax was not fully "experience rated" so that firms did not incur the full costs of benefits paid to their workers. According to Feldstein, these features created a strong incentive for firms to use layoffs to reduce the workforce. The author's empirical estimates, together with

subsequent estimates by others (for example, Topel 1983), tended to support the notion that layoffs, especially temporary ones, were correlated with the level of the "layoff subsidy" the UI system provided.

In an effort to identify other ways in which the U.S. system of unemployment compensation may encourage layoffs, several other authors have focused on the way the UI systems treat hours reductions. In many European countries, workers who are placed on reduced hours are eligible for a prorated share of their unemployment benefits. Until recently, however, the availability of such STC was severely limited in both the United States and Canada.

In this chapter, we critically examine the methods that have been used to assess the effects of STC on total workforce reductions. We begin by briefly discussing the existing theoretical approaches to this topic. We then describe a number of previous attempts to develop empirical evidence on conversions between STC and layoffs. We devote considerable attention to evaluating methodologies that have been used for this purpose, because they have yielded widely differing results. In the next section, we present some new empirical evidence on the relationship between STC use and layoffs from our recently completed evaluation of STC programs in the United States. This research further confirms the importance of this relationship and illustrates the difficulties in measuring it precisely. Finally, in the last section, we offer some general conclusions about our state of knowledge on the ability of STC-type programs to influence firms' workforce adjustment patterns.

THEORETICAL MODELING

To understand how employers' and workers' preferences interact when labor input is reduced during periods of changing demand, the development of an employment contract model is required. The approach usually taken in the literature draws on the early implicit contracts model developed by Azariadis (1975). This model views workers and employers as engaged in a bilateral bargaining process. An efficient outcome from the process is a set of choices that maximizes each party's well-being (that is, profits for firms and utility for work-

ers), given the choice of the other party. In its most general form, this model predicts that risk-averse workers will generally prefer hours reductions to layoffs during economic downturns. This preference can be altered by technical aspects of a firm's production and cost functions. For example, if the firm's adjustment costs are asymmetric with respect to changes in hours and employment, different patterns may be optimal. High fringe benefit costs, especially those that are "quasi-fixed," may deter downward adjustments in hours. On the other hand, hiring costs, such as those related to the search for workers or to training and acquiring job-specific human capital, may deter layoffs. Choices may also be affected by imperfect substitutability between hours and employment in the production process.

A few theoretical papers (Wright and Hotchkiss 1988; Burdett and Wright 1989; Jehle and Lieberman 1992) have attempted to explore how the availability of various UI and STC options may affect hours–employment choices. Under a stylized "American" system, UI benefits are assumed to be payable only if the worker is fully separated from the firm. Alternatively, a stylized "European" system of compensation is assumed to provide benefits only for reductions in normal working hours. Models adopting this approach suggest, not surprisingly, that the American system encourages firms to opt for layoffs during downturns in demand, whereas firms operating under a European system, *ceteris paribus*, favor shortened workweeks. In both cases, the work reduction incentives derive primarily from the incomplete experience rating of UI benefit payments. Hence, some authors favor a move toward more complete experience rating as the primary way to ameliorate inefficient labor input choices encouraged by UI benefits (see, for example, Burdett and Wright 1989).

The all-or-nothing nature of the stylized UI systems in the theoretical literature makes it difficult to apply these models directly to actual data. In Europe, the bulk of UI benefits goes to workers who are fully laid off rather than on reduced hours, and in North America, Canada has a national STC program, as do 18 U.S. states.[1] Furthermore, most examinations of firms' adjustment patterns on the microeconomic level, especially in North America, have found that individual firms use both layoffs and work-week reductions to reduce labor utilization (Kerachsky et al. 1986; Employment and Immigration Canada [EIC]1993). Hence, a clear first step in the development of theoretical models that

can be estimated from real world data is to generalize the nature of adjustment options faced by the firm.

Adapting these theoretical models to allow UI eligibility for both hours reductions and layoffs seems straightforward, although the published literature has not attempted this adaptation in its full generality. Presumably, such a generalized model would predict that both types of reductions would be encouraged by the availability of benefits, with firms choosing the type of reduction or combination of reductions on the basis of their own cost and productivity considerations.

Differences in the generosity of benefits available under the two unemployment options could also affect observed workforce adjustments, a point made forcefully in the recent paper by Van Audenrode (1994). Indeed, the increased flexibility of a system that compensates both hours reductions and layoffs might encourage additional compensated unemployment, relative to a system that provides UI eligibility for only one type of workforce reduction.

Aggregate studies of U.S. labor market dynamics over the business cycle provide evidence on this issue. These studies suggest that labor hoarding may have accounted for between 4 and 9 percent of total employment during downturns in demand, probably because of high adjustment costs associated with layoffs (Hamermesh 1993, p. 185). Some portion of this excess labor would probably find compensated hours reductions attractive after STC becomes available.[2] The extent of these incentives depends on variations in fringe benefit costs available from hours reductions and the degree to which STC benefits are effectively experience rated, among other things. Much of the existing empirical research on STC has not, however, addressed the increasing flexibility suggested by theoretical predictions.

EMPIRICAL STUDIES MEASURING THE EFFECT OF STC USE ON WORKFORCE DYNAMICS

Because one of the primary goals of STC is to reduce the number of laid-off workers, most previous researchers have tried to estimate differences in the dynamics of workforce reductions that result from STC usage. Although many different approaches have been taken to

analyzing such reductions, we summarize these approaches in what we call the "layoff conversion rate." This measure reflects the degree to which unemployment compensation under an STC-type program substitutes for unemployment compensation under a regular UI program.[3] A conversion rate of 1.0 (a value frequently assumed in the literature) implies perfect substitution: each hour of STC substitutes for precisely an hour of layoff. Layoff conversion rates greater than 1.0 imply that the compensated unemployment from layoffs avoided because of STC exceeds the compensated unemployment from STC itself. Conversion rates less than 1.0 imply that firms had greater total workforce reductions with STC than they would have if STC had been unavailable.

In reviewing previous research, we focus narrowly on ways in which conversion rates have been treated. Many of the studies we discuss here have contributed significantly to an understanding of the other important issues such as 1) the effects of STC on employer and employee satisfaction, achievement of affirmative action goals, and worker productivity; 2) the effects of program legislation and administration on STC participation rates; 3) the relative costs and benefits of STC usage to employees, firms, the UI trust fund, and society; 4) seasonal, cyclical, and repeat use of STC; and 5) the use of STC by firms undergoing structural change. We do not summarize research on these issues here, however.[4] Rather, we concentrate solely on the methodology researchers have used to examine layoff conversion rates. Hence, we are not attempting to explore the full social costs and benefits of STC compared to layoffs. For many issues involving STC desirability, however, estimates of the layoff conversion rate play an important, even central, role. For example, measuring any potential social benefit from STC use requires some way of estimating how many layoffs would have occurred in the absence of the program so that the analysis can be conducted on a "per layoff equivalent" basis. Much of the prior research on STC has not been especially careful in adopting such a consistent basis. Our research focus on measurement of workforce dynamics therefore serves to highlight a primary source of the differences in conclusions from previous research.

Experimental studies to determine the layoff conversion rate have not been feasible, so researchers have typically used one of three nonexperimental approaches to estimate the workforce reduction that would have occurred if firms had not used STC: self-reporting, the

explicit or implicit assumption that the conversion rate is precisely 1.0, and estimation using matched samples of firms that did and did not use STC during some period. In the next three subsections, we discuss these approaches and some of the research based on them. For ease of comparison among the studies, those that we shall discuss are briefly summarized in Table 1.

Studies Based on Self-Reporting

One way of estimating the number of layoffs avoided because of STC usage is based on firms' self-reports. This method uses two main data sources: firms' plan applications and surveys of employers. When a firm applies for an STC plan approval, it typically has to specify the

Table 1 Studies of the Effects of Short-Time Compensation on Workforce Dynamics

Study	Data	Method	Conversion rate
New York Department of Labor (1994)	New York STC Firms	Self-reporting	>1
Best (1988)	Canadian and California STC firms, administrative data, and simulations	Self-reporting	1.0
Vroman (1992)	Germany, 1970–1991, administrative data	Assumed	1.0
Abraham and Houseman (1994)	France, aggregate manufacturing data	Assumed	1.0
Van Audenrode (1994)	Five European countries, aggregate employment data	NA[a]	NA
Kerachsky et al. (1986)	Arizona, California, and Oregon	Comparison matching	0 to 1.0
Employment and Immigration Canada (1993)	Canada, worksharing and comparison firms in 1989 and 1990	Comparison matching, self-reporting	>1.0
Berkeley Planning Associates and Mathematica Policy Research, Inc. (1997)	Five states	Comparison matching	See text

[a] NA = not applicable.

number of layoffs that its use of the plan would avoid. Because a firm needs administrative approval for its STC plan to become effective, it may have an incentive to overstate the number of layoffs that would occur if its STC plan is not approved. The validity of this counterfactual cannot be tested directly.[5] EIC (1993, pp. 58–59) found that firms typically overstate their planned workforce reductions; that is, the actual reduction is less than the planned reduction. This study also found that a significant number of post-STC layoffs occurred. These factors suggest that using firms' self-reported statements on the number of layoffs that will be averted if an STC plan is approved overestimates the effects of STC on layoffs. EIC (1984, pp. 116–118) found that retrospective interview responses on layoffs that would have occurred had STC not been available were 23 percent lower than firms' self-reports of planned layoffs from STC plan applications.

A second source of self-reported information on averted layoffs is survey data from firms that have used STC, such as those reported in EIC (1984). These data may suffer from the same problem as self-reported data on STC plan application forms—firms are asked to hypothesize about how many employees they would have laid off had STC been unavailable. A firm might never have laid off any employees (as the "labor-hoarding" literature suggests), and it might also have laid off significantly fewer or more full-time employees than the full-time equivalent (FTE) value of the STC reduction implemented.

Most states with STC legislation have not conducted explicit studies to estimate the number of layoffs averted by STC usage. Their research interest has focused on other aspects of the STC adoption and financing processes. The New York Department of Labor, however, has estimated the number of layoffs averted in each year since 1988. In 1994, for example, 445 New York firms had a total of 9,284 employees on STC plans, which paid out $3.6 million in benefits. These firms reported that, because of STC use, almost 4,000 layoffs were averted. Using average benefit levels and unemployment durations for laid-off workers, the state estimated that about $10.8 million was saved in UI benefits in 1994 alone. If we assume perfect experience rating, these results suggest that, on average, firms saved three dollars in potential UI taxes for every dollar paid out in taxes to support STC benefits. At face value, this calculation seems implausible and is inconsistent with the low observed utilization rates for STC in New York and elsewhere.

As we shall see, the result also conflicts with most other empirical evidence on STC.

Studies Assuming a Conversion Rate of 1.0

A second way to estimate the number of layoffs avoided because of STC usage is based on calculating the FTE workforce reduction directly from administrative records (or survey data) on the number of employees collecting STC and their workweek percentage reductions using an assumed conversion rate of 1.0. For example, if a firm has 10 employees on a 40 percent workweek reduction for five weeks, the researcher assumes that the STC plan averted four layoffs, each of a five-week duration.

It is important to recognize that several assumptions are inherent in the "one-for-one" conversion rate assumed in this type of estimation. Most important, the calculation assumes a linearity that often does not exist in either production technologies or employment policies. In standard economic theory, the firm maximizes profits by adjusting labor and other inputs. Because STC may change many factors in the profit function—such as productivity, labor costs, and logistical constraints—it is unlikely that firms would choose the same person-hours of labor input under both shortened workweeks and full-time layoffs. Assuming a one-for-one conversion rate suggests that firms are not responsive to the theoretical advantages and disadvantages the researcher is trying to estimate (or that the advantages and disadvantages cancel each other out). In many situations, however, researchers assume a one-for-one conversion rate primarily because data limitations prevent estimation of the rate directly.

For example, Best (1988) uses a variety of data at the firm level to conduct a comprehensive and innovative evaluation of California's STC program and the old Canadian program. This study presents a good discussion of the factors that can affect the layoff conversion rate, such as firms' ability to resume production more quickly after a downturn if STC is used, laid-off workers' tendencies to leave unemployment for new jobs, and workers' tendencies to oppose STC less than layoffs. Because he had no administrative data on nonparticipating employers, Best relied heavily on employer and employee survey data and simulations to derive his estimates. Although he presented infor-

mation on employers' and employees' perceptions of the work loss from STC relative to work loss under layoffs, he suggested that these data may be invalid because of inconsistencies between the perceptions of the two groups within firms (p. 76). He concluded that using a one-for-one conversion rate is the most reasonable approach because the data on actual conversion rates are mixed, and these rates may vary significantly over time and by other factors unique to an individual firm.

Best also simulated different estimates of the cost of STC relative to the cost of layoffs for the UI system (including both benefits paid and administrative costs). These estimates, ranging from 1.2 to 3.7, depend on the duration of work-sharing plans, the magnitude of the workweek reduction, and the percentage of STC participants laid off after STC.[6] Best acknowledged that most of the scenarios he presents are uncommon, and it appears that the simulation closest to the average workweek reduction, average duration of the reduction, and average post-STC layoffs provides a ratio of the cost of STC to the costs of equivalent layoffs for the UI system of around 1.6. Because this estimate is based on an underlying assumed conversion rate of 1.0, the extra costs from STC arise from such factors as the higher weekly UI benefits of STC recipients, differential treatment of the waiting week under the two programs, and the additional administrative costs of STC. The result shows that there can be a considerable difference between conversion rate estimates based on equivalent hours of layoff and estimates based on costs to the UI system.

Studies based on aggregate data have tended to use an assumed conversion rate of 1.0 when attempting to estimate the impact of STC usage.[7] This is especially true for studies that have sought to evaluate STC in the European context. For example, Vroman (1992) used administrative data on STC usage in Germany to estimate what employment would have been in the absence of the program during the period 1970 to 1991. To make that calculation, he simply subtracted "full-time equivalent layoffs" experienced by workers on STC from actual employment data, thereby implicitly assuming a conversion rate of 1.0. He found that the cyclical behavior of his adjusted employment series has a closer relationship to the cyclical behavior of U.S. employment than the unadjusted series. Hence, Vroman concluded that the greater availability of STC is an important reason for observed Euro-

pean/American differences. The author presents no empirical evidence to support his conversion rate assumption.

The study by Abraham and Houseman (1994) examined the ways in which job security regulations in Belgium, France, and Germany affect labor force adjustments in response to output shocks. The authors devoted some discussion to the possible influence of STC programs on this adjustment process, although this was not a primary focus of their research. Using aggregate data on hours worked and hours on STC, they show that compensated short-time hours play an important role in hours adjustments in Belgium and Germany. They did not explicitly consider the layoff conversion issue for these countries. Because aggregate data on hours worked were not available for France, however, the authors studied only aggregate employment trends. For France, they constructed a hypothetical employment series "assuming that layoffs were used in lieu of short time." This construction required the authors to assume that layoffs and short-time could be substituted on a one-for-one basis. In common with most of the other literature on European STC programs that we reviewed, the authors offer no empirical support for this assumption. Hence, the issue of precisely how widespread STC availability in Europe affects use of layoffs remains open.

Studies Using Matching Methods

A third way of estimating the numbers of layoffs avoided because of STC use is based on pairing firms that used STC with firms that did not. Difference-in-differences analysis is used to compare the FTE workforce reductions of the STC firms and non-STC firms over time. The critical assumption in this approach is that non-STC firms do not differ systematically from STC firms; that is, unobserved differences between the two groups are independent of treatment status.[8]

A growing set of economic literature has evaluated such non-experimental evaluation (matching) methods (see, for example, Friedlander and Robins 1995; LaLonde 1986; and Fraker and Maynard 1987). Selecting the pool of potential matches on the basis of similarities in time, geographic area, and observation-specific characteristics is one of the most difficult aspects of matching, and the appropriateness of various criteria for restricting the pool has been debated.[9] For exam-

ple, researchers may consider limiting the pool of potential comparison firms to those with certain observed levels of compensated unemployment, even though firms that used STC may have chosen not to lay off workers. Thus, researchers who use matching procedures to generate comparison samples may have to make many decisions about what constitutes a good match, without being able to draw on much economic theory as a guideline.

Matching procedures have several practical limitations. First, although matching attempts to control firm-specific differences at the outset of the research design, the variables used for matching may not adequately represent all factors affecting workforce adjustment strategies. The financial health of firms, their labor/management relations, the demand for their products, and their production technologies, as well as trends in these factors, may affect whether firms consider workforce reductions.[10] Data on these factors, however, are extremely hard to obtain; most likely, the variables firms use to make their production (and labor input demand) decisions are known only to the firms themselves. Second, the treatment variable in matching studies must be defined carefully. Because firms change their STC status over time, and enrollment can begin at any time, construction of this variable requires focusing on a particular period (the study period) during which the firm "uses" the program. But that definition must invariably involve some ambiguity when intensity of usage varies. Finally, because comparison firms are chosen to be as similar as possible to STC firms, they are also likely to have participated in the STC program at times outside the study period. Such prior participation may bias the estimated treatment effects.

A 1986 study conducted by Mathematica Policy Research, Inc., (MPR) matched STC firms in Arizona, California, and Oregon to non-STC comparison firms on the basis of their size, UI tax rates, and Standard Industrial Classification (SIC) code (Kerachsky et al. 1986). The comparison firms in each state were chosen from among all firms in the state not using STC during the study period. Empirical estimates from this study found widely varying layoff conversion rates across the three states: California's STC program did not appear to avert any layoffs; Arizona's STC program averted some layoffs, although total unemployment increased for firms using the program; and Oregon's program

appeared to have a layoff conversion rate that approximated the 1.0 value assumed in many studies.

Although a matching process was used, STC firms seemed to be in greater economic distress because they had somewhat higher levels of pre-STC compensated unemployment. Although the researchers controlled for this factor in most of their analysis, concerns about their ability to control for pre-STC differences in layoff propensities between the STC and comparison groups resulted in some criticism of their findings. These criticisms focused especially on the finding of no STC impact on layoffs in California and on the possibility that the state of California extracted the data incorrectly (Best 1988, pp. 75–76). Still, the MPR study helped to emphasize the importance of measuring rather than assuming the extent of layoff conversions that STC provides.

The recent Canadian evaluation used a very different methodological approach to matching (Employment and Immigration Canada 1993). In constructing the comparison group, the researchers chose a random sample from administrative records of employees who had been laid off in 1989 or 1990. The firms from which the employees were laid off were screened to ensure that the comparison firms chosen had been in existence for at least two years and that they had "considered" laying off 20 percent or more of the full-time employees in a business unit for nonseasonal reasons.[11] Eligible comparison firms could not have used the STC program in the past, and laid-off employees had to have been recalled within 26 weeks. The analysis included a total of 1,080 firms.

Because firms in the STC and comparison samples were not matched according to specific characteristics (except the screening requirement that comparison firms had to have considered laying off employees), the two samples differed markedly along many of the dimensions that the earlier MPR study used for matching, such as geographic location and industry. Most notably, STC firms were only about one-third the size of comparison firms, on average. Possibly because of these and other differences, many of the results from the analysis of raw data were not supported when regression adjustment techniques were used. In contrast to the MPR study, STC firms in Canada appeared to have been in less economic distress than were firms in the comparison group because employees in these firms had signifi-

cantly lower pre-STC compensated unemployment than comparison firms' employees.

Despite using a comparison group for some parts of the analyses, the Canadian evaluation did not directly compare layoffs or total compensated unemployment for STC and comparison firms.[12] To determine the number of layoffs averted because of STC, the ratio of STC participants to plan-reported hypothetical layoffs was calculated for 1989 and 1990. Because the firms included had 177,800 employees using STC during these years, and the firms reported that STC averted 67,500 layoffs, the overall ratio was set at 2.6, although for some simulations the 1990 rate of 2.31 was used instead. These figures, together with information from the comparison sample, provided the basic input into the Canadian evaluation's estimates of UI costs.

Overall, the Canadian evaluation found that STC cost the UI system approximately 35 percent more than an equivalent layoff alternative would have.[13,14] The researchers stated that the differences probably resulted primarily because 1) 29 percent of participating employees were laid off after their period of STC collection; 2) STC participation does not require the two-week waiting period, while regular UI does; 3) STC recipients were eligible for higher weekly benefit amounts than laid-off employees; and 4) UI is not collected by all eligible laid-off employees.

In summary, the Canadian study represents a hybrid in terms of the methodology used to measure the layoff conversion rate. The basic conversion rate used was primarily self-reported, but many adjustments were made to this rate with information from the study's relatively imperfectly matched control sample. It is interesting that the study yielded cost comparisons that are similar to those reported in the Best and MPR studies.

RESULTS FROM THE BPA/MPR STUDY

Our recently completed study of the STC program in the United States sheds additional light on the layoff conversion question, although it fails to provide a convincing numerical estimate of this parameter (Berkeley Planning Associates and Mathematica Policy

Research, Inc. 1997). As part of that project, we collected administrative data from a relatively large sample of firms that used STC during 1992 in five states (California, Florida, Kansas, New York, and Washington).[15] A comparison sample of equal size was also selected from these states, with firms being matched to the STC firms using three variables: firm size, three-digit industry, and UI tax rate. Because of the large number of non-STC firms in the states, we were able to match quite closely along these dimensions. Table 2 provides some quantitative measures of these matches. In general, the availability of a very large universe of firms that did not use STC made it possible to achieve a comparison sample that was virtually identical to the STC sample as measured by the available data. Still, as for all nonrandom evaluation methodologies, we were concerned that STC participants may have differed from nonparticipants along unmeasured dimensions—a concern that proved to be of crucial significance in the analysis of the data we collected.

Table 3 summarizes our data on regular UI and STC benefits charged to firms during the three years 1991, 1992, and 1993. To control for the large variation in firms' sizes in our sample, these data have been normalized by firms' total 1991 "full-time-equivalent payrolls" and then stated as a percent. Hence, a total normalized charges of 1.00 means that the firms' workers collected total chargeable benefits that amounted to 1 percent of the total 1991 payroll.[16]

Three general conclusions are immediately apparent from the table. First, although overall charges to the UI system varied among the three years, they were substantial in all three years, being somewhat larger in 1992 than in either 1991 or 1993. Second, regular UI was the predominate form of charges incurred. That finding was, of course, expected for firms in the comparison sample who, by definition, had no STC charges in 1992. The findings for firms in the STC sample are important, however. They show that, in 1992, between 62 and 78 percent of all UI system charges were for regular benefits; that is, despite their participation in the STC program, firms in 1992 appear to have used layoffs as their primary workforce reduction strategy. Thus, models that assume that firms follow an "either/or" approach to such strategies clearly are inappropriate.

Finally, and most important, the values in Table 3 suggests the possible difficulties in relying on a comparison methodology for address-

Table 2 Descriptive Statistics for Match Quality

Match characteristic	California[a]	Florida[b]	Kansas	New York[c]	Washington[d]
Correlation between number of employees in matched firms	0.98	0.84	0.70	0.9	0.79
Correlation between tax rates in matched firms	0.99	0.98	0.98	0.99	0.98
Number (percentage) of matches at the three-digit standard industrial classification (SIC) level	474 (93.5)	174 (82.1)	65 (63.7)	477 (94.5)	326 (87.2)
Number (percentage) of matches at the two-digit SIC level	27 (5.3)	20 (9.4)	28 (27.5)	27 (5.3)	44 (11.8)
Number of matched pairs of firms	507	212	102	505	374

SOURCE State administrative records.

[a] The 507 firms in California were selected from among 5,143 firms with STC plans in 1992 using sampling stratified by the number of employees and one-digit SIC code. There are 100 matches in California that are excluded from this table since the STC and comparison firms did not have complete information upon which a match could be made; that is, they were missing information on the number of employees or the tax rate.

[b] There are eight matches in Florida excluded from this table since the STC and comparison firms did not have complete information upon which a match could be made; that is, they were missing information on the number of employees or the tax rate.

[c] The 505 firms in New York were selected from among 737 firms with STC plans in 1992 using sampling stratified by the number of employees and one-digit SIC code. Only firms with at least five employees are eligible for participation in STC; comparison firms with fewer than five employees, therefore, were excluded from the pool of potential comparison employers.

[d] There are nine matches in Washington excluded from this table since the STC and comparison firms did not have complete information upon which a match could be made; that is, they were missing information on the number of employees or the tax rate.

Table 3 Average Compensated Unemployment Charges, STC and Non-STC Firms[a,b]

Characteristics	California		Florida		Kansas		New York		Washington	
	STC	Non-STC	STC	Non-STC	STC	Non-STC	STC	Non-STC	STC	Non-STC
1991										
Normalized UI charges	0.871	0.791	1.426	1.284	0.915	0.762	1.335	1.501	3.543	3.470
Normalized STC charges	0.277	0.007***	0.359	0.036***	0.343	0.027***	0.400	0.009***	0.368	0.039***
Normalized total charges	1.149	0.798**	1.785	1.320**	1.258	0.788***	1.735	1.510*	3.911	3.508
Percentage of total charges that are UI charges	79.424	98.531	84.060	98.773	77.536	98.387	71.393	99.433	90.275	98.519
1992										
Normalized UI charges	0.936	0.964	1.825	1.153***	1.681	1.206**	2.339	2.297	3.695	3.907
Normalized STC charges	0.561	0.000***	0.847	0.000***	0.759	0.000***	0.878	0.000***	1.022	0.000***
Normalized total charges	1.497	0.965***	2.672	1.153***	2.440	1.206***	3.217	2.297***	4.717	3.907***
Percentage of total charges that are UI charges	63.304	100.034[c]	62.078	100.000	69.030	100.000	65.146	100.000	78.456	100.000

1993										
Normalized UI charges	0.788	1.009***	0.935	1.088	1.366	1.137	1.783	1.810	3.633	4.298*
Normalized STC charges	0.331	0.131***	0.181	0.021***	0.258	0.040***	0.639	0.016***	0.893	0.084***
Normalized total charges	1.120	1.140	1.116	1.108	1.624	1.177*	2.421	1.826***	4.359	4.383
Percentage of total charges that are UI charges	74.999	92.423	79.442	99.564	85.169	98.894	69.354	98.438	81.856	97.039
Sample size	431	721	191	231	90	106	441	559	314	378

SOURCE: State administrative records.

[a] * = Significantly different from STC firms at the 10% level, two-tailed test.
** = Significantly different from STC firms at the 5% level, two-tailed test.
*** = Significantly different from STC firms at the 1% level, two-tailed test.

[b] Samples restricted to firms in business throughout 1991 and 1992. Because sample sizes vary slightly per charges measured, and because of rounding, the sum of normalized UI charges and normalized STC charges in a year may not equal normalized total charges in a year. All charges variables are normalized by an approximation of payroll at full employment in 1991. See text for further details.

[c] Firms occasionally have negative STC charges for a year. In these instances, the percentage of total charges that are UI charges may appear greater than 100%.

ing the layoff conversion question. In two of the states (Florida and Kansas), mean UI charges in 1992 were significantly larger in STC firms than in comparison firms. Because no current theory suggests that layoffs should be greater in otherwise similar firms that use STC, the most likely explanation of this result is that STC and comparison firms differ along dimensions that were not adequately controlled for in the matching process—for example, those firms which opt to use STC may have faced systematically worse economic prospects than did similar firms in the comparison group. Further support for this supposition is reflected in the figures for total charges in 1992, which were significantly larger for STC firms than for comparison firms in all of the states by amounts ranging from 0.53 percent of total payroll (California) to 1.52 percent (Florida).

We examined a number of statistical procedures for controlling for these unmeasured differences between the STC and comparison firms in models that seek to estimate the effect of STC on layoffs. For ease of comparison we present a variety of results for one simple specification:

$$\textbf{(1)} \quad Y_t = \alpha + \beta X_t + \gamma STC + \delta Y_{t-1} + u_t,$$

where Y is our measure of layoffs (normalized UI charges), X is a set of individual firm characteristics, STC is a dummy variable representing participation in the STC program, u is a random error term, and the lagged value of Y is included as a proxy for the general economic health of the firm in the previous year.[17,18] This specification was chosen both for its overall simplicity and because it was believed that estimates of γ, if they were unbiased, would permit a direct measure of what we have called the "layoff conversion rate."[19]

Table 4 reports results for four alternative approaches to the estimation of γ. The first specification used all observations in the sample for which we had complete data. As is immediately apparent, these estimates take on values that cannot reasonably be interpreted in the layoff conversion context. In three of the states, γ is estimated to be positive; in the other two, its value is very close to zero. One interpretation of such estimates is that they arise through selectivity bias—that is, unmeasurable characteristics that differ systematically between STC participants and firms in the comparison group cause estimates of γ to

Table 4 Estimated Effect of STC Participation on Normalized UI Charges in 1992 under Alternative Specifications[a]

Specification[b]	California	Florida	Kansas	New York	Washington
(1) Full sample					
STC dummy	–0.094	0.554***	0.487**	0.223	–0.075
Sample size	1,152	421	196	1,000	692
(2) Omit zero UI charges					
STC dummy	–0.301***	0.279	0.363	–0.101	–0.670**
Sample size	993	375	183	907	631
(3) Include zero STC charges					
STC dummy	–0.203	0.387**	0.546*	0.322*	–0.110
Sample size	1,152	416	194	1,000	692
(4) Matched pairs					
STC dummy	–0.073	0.574***	0.487*	0.233	–0.306
Sample size	502	336	132	814	444

SOURCE: U.S. Department of Labor Evaluation of Short-Time Compensation Programs. Administrative data.

[a] *Significantly different from zero at the 0.10 level, two-tailed test.
**Significantly different from zero at the 0.05 level, two-tailed test.
***Significantly different from zero at the 0.01 level, two-tailed test.

[b] All regressions contained an identical set of control variables for firm size and its square, UI tax rate, industry, and normalized UI and STC charges in 1991.

be biased in a positive direction. However, the first specification does control for some variables that might plausibly serve as proxies for the firms' economic health, such as their UI tax rates or their UI charges in the prior year. Hence, such selectivity must be based on current economic factors that are not adequately controlled for by these variables.

Because we did not have extensive information on other potential control variables, we employed a variety of sample restrictions to determine whether the suspected biases in specification (1) might be mitigated.[20] Our first approach focused on a possible asymmetry in the

treatment definition. The STC treatment variable requires that some STC benefits were paid to a firm's workers in 1992, but no similar requirement was imposed for the comparison firms.[21] As an attempt to control for this asymmetry, we omitted from the comparison group all firms without UI charges in 1992. Results for this omission are shown in Table 4, as specification (2). Although this approach is admittedly ad hoc and runs the danger of incorporating biases of its own (an STC firm might not have made any layoffs in the absence of the program, and that decision would not be represented in such a sample), it did have a substantial effect on the estimates of γ. Both Kansas and Florida samples continued to exhibit (insignificantly) positive estimates, but estimates for the other three states were negative. In California and Washington, they were significantly different from zero.[22] These results clearly implied that selectivity is biasing parameter estimates in the full sample, but we had little confidence that our sample redefinition corrected for those biases in any systematic way.

Our final two specifications yielded similar ambiguous results. For specification (3), we redefined our STC variable to indicate only the filing of plans in 1992—it was not necessary for the firm to have experienced any benefit charges under the program. The rationale here was the converse of that employed for specification (2)—firms filing for STC were obviously aware of the program, but this specification required that neither STC firms nor comparison firms had actually incurred any charges in 1992. Again, however, this procedure yielded several significant positive estimates of γ. Finally, in specification (4), we returned to our original STC indicator variable but required that only pairs of STC firms and comparison firms enter the sample together. That procedure was intended to ensure that sample attrition because of insufficient data was not influencing our results.[23] Again, many of the estimates for γ were implausibly positive.

Our statistical examination of the layoff conversion issue therefore reached two primary conclusions. First, firms that use STC also make extensive use of layoffs. Even though participation in the STC program may offer significant advantages to these firms, they still appear to rely on compensated reductions in hours for much less than half of their total workforce adjustments. Further analysis of this outcome confirmed that STC use often accompanied widespread, "massive" layoffs (see Berkeley Planning Associates and Mathematica Policy

Research 1997, Chapter 6). Thus, it seems clear that modeling of the impact of STC-type programs on labor demand over the business cycle must take into account the full complexities of firms' actual workforce reduction strategies. Considerably more empirical work is required if we are to have reliable estimates of layoff conversion rates from STC use in various circumstances.

Our second primary conclusion is that adoption of a comparison methodology for the measurement of layoff conversions may be inadequate for obtaining unbiased estimates. Selectivity effects in program participation, at least for the low levels of participation experienced in the United States, pose major, perhaps insurmountable, problems in statistical inference. In our experimentation with alternative specifications, we obtained a wide variety of layoff conversion rate estimates, many of which seemed implausible on theoretical grounds. Indeed, our results suggest that the variation in earlier estimates of layoff conversion rates using matching methodologies (Kerachsky et al. 1986) may also be in part explained by selectivity biases. Further progress on estimation of this parameter may well require the development of alternative statistical approaches.

CONCLUSIONS

In theory, the availability of STC benefits in addition to benefits provided through regular unemployment compensation should affect how firms make cyclical workforce adjustments. Understanding the quantitative magnitude of these effects is an important component in any evaluation of a program's overall desirability. Given the centrality of this issue, we find it surprising that relatively little attention has been paid to the specification of a clear model for estimating these effects. In our view, estimation of this type of model should be in the forefront of economic research on STC programs.

Our review of the empirical literature on STC suggests that how program availability affects firms' workforce dynamics is far from clear. Consistent with other evaluations of their type, studies based on self-reporting have produced widely varying estimates, some of which are implausible. Many other studies merely assume the size of an

effect that should, in principle, be estimated. Matching methodologies seemed to offer the best promise of obtaining estimates of the effect of STC from microdata. Previous experiences with that methodology in other contexts, however, suggest that its greatest drawback is the lack of assurance that STC users and nonusers face similar economic prospects despite major efforts to assure that the firms are closely matched on measurable variables. This possibility was confirmed by our experiences in trying to model the impact of firm use of STC in the United States during the 1992 recession, for which we obtained a wide variety of layoff conversion rate estimates. Hence, the suitability of other research designs (such as innovative uses of aggregate data, random-assignment experiments, or carefully designed case studies) needs to be considered before attempting any overall assessment of the general desirability of STC-type programs.

Notes

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1. Utilization rates for STC have been low under both programs, however. UI programs in the United States also have provisions for the payment of partial benefits, but these benefits are generally unavailable to workers suffering relatively modest workweek reductions.
2. Studies of the increase in short-time work during recessions provide additional support for the possibility that total compensation might increase when STC becomes available.
3. In principle, one might want to measure changes in total hours and employment in response to STC use, not simply unemployed hours that are compensated. But the data requirements for a more complete measurement at the level of the firm are quite onerous, and no researcher has attempted such an evaluation. Rather, the existing research has focused on more readily measured compensated hours, usu-

ally by drawing data from administrative sources. Although use of the compensated data sheds light on a number of important policy questions (such as the effect of STC adoption on overall expenditures under the UI system), the extent to which these data accurately reflect changes in total hours and employment is not known.

4. For summaries of many of the other issues that have been addressed in the STC research, see Best (1988) or Cook et al. (1995).
5. A firm may choose not to use its approved plan. State agencies that approve plans do not typically monitor whether plans are used and what happens if they are not used.
6. Other assumptions pertain to the UI take-up rate of STC participants, the hazard rate to reemployment for laid-off employees, the wages and benefit levels of STC participants, and the costs of processing STC claims. The estimate assumes firms operate at the average values of workweek reductions, STC durations, and post-STC layoffs.
7. In some cases, effects of STC on aggregate fluctuations make no use of the STC data and therefore need no assumption about layoff conversions. For example, Van Audenrode (1994) finds that total hours exhibit much greater flexibility in European countries with generous STC systems (Belgium, Italy, and Sweden) than in countries with only modest levels of compensation (France and Germany). Although this finding does not provide a direct estimate of the extent to which STC use deters layoffs, it suggests that the trade-off may be significant in certain situations.
8. Of course, the reason firms choose not to use STC is of critical importance. Firms that were not aware of STC might be more suitable matches than firms that knew about it and chose not to use it.
9. Economic or operational criteria, such as legislative restrictions on firm characteristics that limit eligibility for STC, may suggest the need to exclude certain firms. In addition, there may be no credible matches for a particular firm (see, for example, Kerachsky et al. 1986; and Schiff 1986).
10. Even if additional data were available for matching, the matching procedure is computationally burdensome and extremely slow.
11. The authors made no attempt to reweight the sample to adjust for the higher probability of sampling large firms.
12. The analysis on pp. 148–155 indicates that STC claimants collected benefits for fewer weeks than UI claimants, but it appears that adjustments were not made to account for the increased number of claimants under an STC program. Furthermore, compensation under STC was only for the workweek reduction and not for the full weekly benefit amount.
13. Canada's UI system is not experience rated, so higher compensated unemployment charges are not charged to a firm. Because the U.S. system is experience rated, albeit imperfectly and with a lag, firms with higher charges typically bear responsibility for them. Hence, computations of the U.S. program's "cost" to the UI system must, of necessity, be more complex.

14. For firms, the evaluation's reported favorable benefit–cost estimates for STC compared to layoffs resulted largely from the significant savings derived from reduced training and hiring costs. For society, the favorable estimates resulted largely from the much lower stress-related costs of STC compared to layoffs. These values were calculated on a per-layoff equivalent basis using the self-reported figures described earlier.
15. Our definition of having "used" STC was that some benefits were paid to the firm's workers during 1992 under this program. Implementing this definition posed some difficulties because the only information available was on which firms had filed STC plans at the time of sample selection. Hence, we selected our STC sample on the basis of having filed a plan in 1992, although (as discussed later) we primarily used a definition stressing actual use in most of our analysis.
16. 1991 full-time-equivalent payrolls were estimated by adding total wages paid during 1991 to an estimate of wages that would have been paid to the firms' workers who collected UI or STC during 1991 if these workers had instead been fully employed during these periods.
17. The vector $\boldsymbol{X}$ includes firm size and its square, dummy variables for one-digit SIC industry, and a measure of the 1991 UI tax rate.
18. Prior year normalized STC charges were also included in all regressions as a further control on the firm's health in 1991.
19. Specifically, the average layoff conversion rate can be estimated as γ/k, where k represents mean normalized STC charges during 1992. In this specification, the layoff conversion rate would be in terms of dollars—STC dollars substituting for UI dollars. We obtained substantially similar results from estimates of Y (and γ) in terms of hours—perhaps a more natural, if less accurately measured, conversion concept.
20. We also experimented with a number of statistical methods for controlling for sample selectivity, but these were largely unsuccessful because of our inability to develop clear ways of identifying the selectivity equation given the limited set of control variables we had and the fact that firms in the sample had been matched on these variables.
21. This asymmetry was also pointed out in connection with the earlier MPR study (Morand 1990).
22. The point estimates for California and Washington imply layoff conversion rates of –0.55 and –0.66, respectively.
23. To ensure the integrity of the matching, pairs in which a comparison firm experienced STC charges (because of a plan filed prior to 1992) were also omitted from the sample in some specifications. We also used this sample to implement a "paired" regression analysis, but this did not substantially change the results.

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Working Time in Comparative Perspective

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