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A QUASI-EXPERIMENTAL APPROACH TO THE EFFECTS OF UNEMPLOYMENT INSURANCE

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ABSTRACT

This paper uses the natural experiment provided by periodic increases in state benefit levels to estimate the effects of higher unemployment insurance benefits. Individuals who filed just before and just after sixteen benefit increases are compared using data from five states during 1979-1984. The increases, which average about 9 percent, are found to increase the period of unemployment insurance receipt by about one week. This effect is precisely estimated and found using several approaches. The incidence of layoffs resulting in unemployment insurance claims is unaffected by the increases. The evidence does not suggest that higher benefits lead to better jobs. In fact, the post-unemployment earnings of individuals receiving higher benefits are estimated to fall slightly, but the estimates are imprecise.

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1. Introduction

There are many unresolved issues about the effects of unemployment insurance (UI). Many studies have examined the effects of the level of UI benefits on the length of unemployment spells. However, Welch (1977) and others have criticized these studies, arguing that it is difficult to distinguish effects of UI from effects of previous earnings since the level of benefits is a nonlinear function of previous earnings. In this paper, the natural experiment provided by periodic increases in state benefit levels is used to obtain estimates of the effects of higher UI benefits. While higher UI benefits may increase the length of unemployment spells, they may also lead to higher reemployment earnings. This hypothesis has not been extensively tested. I compare both the spell lengths and reemployment earnings of people beginning UI spells just before and after benefit increases.

There are numerous papers on the effects of UI on the length of unemployment spells. Excellent surveys can be found in Danziger, Haveman and Plotnick (1981), Gustman (1983), Hamermesh (1977), and Welch (1977). Welch (1977) criticizes the conventional methodology by pointing out that within a given state at a point in time, the weekly UI benefit is a constant fraction of previous earnings except when an individual receives the minimum or maximum weekly benefit.[1] Thus, in a regression of spell length on weekly benefits and previous earnings, it is difficult to distinguish between the effects of UI and possible nonlinear effects of previous earnings.

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[1]The benefit structure of state workers' compensation programs also has this form. Ehrenberg (1988) makes the same criticism of studies of the effects of workers' compensation benefits on the time until return to work.

This paper uses only the variation in UI benefit levels that comes from periodic increases in the maximum weekly benefit amount to estimate the effects of the level of UI benefits on the length of unemployment and reemployment earnings. This new methodology allows one to examine the effect of exogenous variation in benefits on unemployment duration and incidence. In the typical study of spell lengths, the variation in UI benefits comes from some combination of different replacement rates in different states, different minima and maxima, and maybe some variation in these parameters over time. This paper uses one component of this variation which can be separated out and used to identify the effects of UI.

The spirit of the approach taken here is similar to that of Classen (1979) and Solon (1985). Classen uses data from Arizona and Pennsylvania around the time of two benefit increases. In a regression analysis, she allows a kink at the earnings necessary for the maximum benefit in an otherwise linear relationship between previous earnings and spell length. Solon examines the length of UI receipt in Georgia just before and after the introduction of federal income taxation of UI for high income individuals in 1979. He compares the spell lengths of individuals with high income last year who are likely to pay taxes on their UI benefits, to individuals with low income last year who are unlikely to pay taxes. The present study has the advantage that the assignment of an individual to the high or low benefit group is precise, and does not rely on the imputation of tax rates. Additionally, sixteen events which change the level of benefits are examined, rather than one or two.

Some early work by Ehrenberg and Oaxaca (1976) and others examined the effects of UI on reemployment earnings, but this topic has not been pursued

much recently. Many theoretical search models such as Mortensen (1970) predict that higher UI benefits will be associated with longer unemployment spells, and a higher wage conditional on finding a job. This paper tests these hypotheses using a source of variation in UI benefits that is unrelated to the individual's earnings history.

This paper is novel in its disaggregation of the effects of UI by recall status, expectations about recall, and UI payroll tax status. Various authors have emphasized the importance of these variables in determining the effects of UI. Feldstein (1975, 1978) emphasizes the quantitative importance of temporary layoffs. He concludes that among "unemployed job losers" temporary layoffs account for about half of the spells. He further argues that conventional models of search unemployment are inappropriate for this group. In his empirical work using the Current Population Survey (CPS) he finds that higher UI benefits are associated with much higher levels of temporary layoff unemployment.

Topel (1983, 1984) also uses the CPS and distinguishes between temporary and permanent layoffs, and the incidence and duration of unemployment. Also, for state/industry cells he imputes the average UI subsidy due to incomplete experience rating. He finds that this subsidy greatly increases temporary layoff unemployment and slightly increases permanent layoff unemployment. Incidence of unemployment is found to be particularly affected by incomplete experience rating.

Corson and Nicholson (1983) find that about two-thirds of UI recipients in a small two-state sample return to their previous employer. Using the same data, Katz and Meyer (1988) find the two alternatives of being recalled or taking a new job have very different time patterns and are affected in very

different ways by UI and other variables. The present study provides separate results for those that expect to be recalled and those who do not.

It is important to know whether higher benefits affect spell lengths primarily through individual search as opposed to firm layoff and recall policies. Brechling (1981) and Topel (1983, 1984) have empirically examined the effect of incomplete experience rating on unemployment using state level measures of experience rating. Surprisingly, no one has looked at firm level tax rates even though the incentive effects of incomplete experience rating depend on where a firm lies on a nonlinear and nonmonotonic marginal tax schedule. This paper examines the effect of firm level tax rates on layoff and recall patterns.

2. Data

The source of data for this study is a large sample of UI claimants taken from the Continuous Wage and Benefit History (CWBH) project. Individuals are randomly selected for the sample using the last two digits of their Social Security number. Data on 392,000 unemployment spells[2] in eight states[3] from January 1979 to early 1984 are available. The data include accurate administrative records on the key UI parameters: the weekly benefit amount,

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[2]The unit of observation is really a benefit year. The benefit year is a year long period starting when an individual files for UI benefits. Some information on the length of spells within the benefit year is available, but the best information is on cumulative benefits received during the benefit year.

[3]The eight states are Georgia, Idaho, Louisiana, Missouri, New Mexico, Pennsylvania, South Carolina, and Washington. CWBH data were collected for several other states, but these states have intermittent data, cover a shorter time period, or have other drawbacks.

the potential duration of benefits, and weeks of benefits received. Some demographic information including age, sex, race and education is in most cases administratively collected, along with additional survey information on expectations about recall to a previous employer, marital status, dependents, occupation, family earnings, and other variables. These individual data are matched to administrative data for each of an individual's covered employers during a 21 quarter period. These additional data allow the calculation of previous and subsequent earnings, and an indicator for whether or not an individual is recalled by a previous employer. The employer data also include UI payroll tax rates, 4-digit SIC codes, and the number of employees each quarter.

3. Methods

The main idea for the experimental design for this study can be seen in Figure 1. Figure 1 displays a typical state schedule relating the weekly UI benefit amount to previous earnings. The solid line is the schedule prior to a change in the state law which raises the minimum and maximum weekly benefit amount (WBA). The dashed line is the schedule after the benefit increase. Between the minimum and the maximum, the weekly benefit amount is a constant fraction of earnings during the highest calendar quarter of the base period (the first four of the last five calendar quarters prior to the date of filing for UI).

For people with high quarter earnings of at least E_4 (the H group), I compare the mean weeks of UI received and reemployment earnings of people who filed for UI benefits just prior to and just after the change in the benefit

schedule.[4] Those who file before the increase receive $B^{o}max$ while those filing afterwards receive $B^{n}max$. An individual's filing date determines his UI benefit amount for his entire benefit year (the one year period following date of claim). Thus, two individuals with high quarter earnings greater than E_4 will receive different weekly benefits for their entire benefit year if one filed a few days before and the other a few days after the effective date of the benefit increase. This is the main idea of the paper. Most of the remaining methodological problems involve correcting for possible differences between the individuals filing just before and just after the benefit increase. In much of what follows, I will use as a comparison group those with earnings between E_2 and E_3 (the L group) who file just before and just after the benefit increase. The benefits these individuals receive are unaffected by the increase in the maximum benefit amount.

The analysis uses sixteen benefit increases which are listed in Table 1. Table 1 summarizes some key characteristics of the state UI laws.[5] I exclude increases which straddle any of the following changes: the partial taxation of benefits in January 1979 and January 1982, changes in the replacement rate (on the linear part of the benefit schedule), and changes in the potential duration of benefits. I also use only benefit increases far enough from the beginning and end of the sampling frame that a complete benefit year of data is available for each person. Lastly, I exclude South Carolina because the

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[4]In principle, one could also examine the effects of increases in the minimum weekly benefit amount. Unfortunately, few people receive the minimum benefit and it is raised infrequently.

[5]The information needed for this table was obtained from the Department of Labor publications Comparison of State Unemployment Insurance Laws, and *Significant Provisions of State Unemployment Insurance Laws*, and numerous conversations with officials of state employment security agencies.

benefit increases are very small (they total $7 over three years). An extremely large benefit increase in Georgia is excluded because it is three times as large as the others. Results for this increase are discussed in Section 6.

The sixteen benefit increases are fairly evenly spread across three times of the year: January, July and September. The increases are automatic annual increases because the maximum benefit is indexed to state average weekly earnings in the five states examined. This research is aided by the high rate of nominal wage growth during the late seventies and the early eighties, which leads to large benefit increases averaging between 9 and 10 percent. I also present results for sixteen "placebo periods" below. These are periods around the same day and year as actual benefit increases but the data come from states where the UI law did not change. These periods serve as an additional comparison group for the results found using the benefit increases. A list of the placebo periods is provided in Table 2. The selection criteria for placebo periods was similar to that for benefit increases in that the periods could not overlap any other change in the UI law. In 13 of 16 cases this reduced the set to a single state. In the other 3 cases I picked a state that had not been already used for many placebo periods.

To make the before group (B group) and after group (A group) as comparable as possible, the upper and lower limits (E2 and E3) on previous earnings for the L group, and the lower earnings limit (E4) for the H group, were indexed using state level average weekly earnings in UI covered employment.[6] The period before and after each benefit increase that was used

[6]These data are unpublished, but were provided on a quarterly basis by Cindy Ambler of the Department of Labor.

for the analysis is diagrammed in Figure 2. The B group is the two month long period ending one-half month before the benefit increase, and the A group is the two month period beginning one-half month after the increase. The one month period surrounding the increase was not analyzed in case there was strategic behavior by claimants who waited until after the increase to file for benefits in order to receive a higher weekly payment.[7] The sample only includes males, since in most cases there are less than 20 women in the high earnings groups. In all, I use 18,370 observations around benefit increases, and 24,642 around placebo periods. A breakdown of these totals by subgroups is given in the notes to Table 4.

Figure 3 shows the timing of the earnings periods that are used to measure the effects of UI on post-unemployment earnings. Two earnings periods are examined. The first period is the 4 quarters beginning with the quarter of claim (this is approximately the benefit year). The second period is the 4 quarters after the first period, so it is approximately the year after the benefit year. Notice that the mean claim date (assuming a uniform distribution) for the B group is exactly one quarter before the mean claim date for the A group. This sample design was used to keep the earnings measures comparable for the A and B groups.

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[7]Results excluding a two month period around the benefit increases are reported in Table 13. They do not differ appreciably from the results with the one month exclusion, except for the expected larger standard errors.

4. Results for Weeks of UI Received and Change in Reemployment Earnings

Table 4 reports summary measures of the effects of each of the sixteen increases in state maximum benefits on spell length, earnings, and the relative number of UI claims. The spell length measure for increase i, ΔL_i equals $L_{AH_i} - L_{BH_i} - (L_{AL_i} - L_{BL_i})$, where L_{AH_i} is the mean weeks of UI received for the AH group. The AH group is just the intersection of the A group and the H group, i.e. those claimants with high quarter earnings above those needed for the new maximum benefit amount who filed between one-half month and two and one-half months after the benefit increase. L_{BH_i}, L_{AL_i} and L_{BL_i} are defined analogously. The statistic ΔL_i is the change in mean spell length for those subject to the change in the maximum benefit compared to the change over the same period for those unaffected by the increase. Subtracting out the change for those unaffected by the increase should eliminate the potential bias caused by changes in durations (and variables affecting them) over the three month period between the claim dates of those in the B group and those in the A group. Results with and without this adjustment are generally reported below. Table 4 and the following results also exclude individuals that were rehired as part of a recall that was a large fraction of the state's data because these observation cannot be treated as independent. If a recall accounts for more than 3 percent of the observations, it is dropped from the sample.[8]

The earnings measures used are the natural log of mean earnings. Let El_{AH_i} be the log of the average value of earnings during the benefit year for

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[8]Results including these observations are reported in Table 13 for comparison.

the AH group. Define El_{BH_i}, El_{AL_i} and El_{BL_i} analogously for the BH, AL and BL groups respectively. Then $\Delta El_i = El_{AH_i} - El_{BH_i} - (El_{AL_i} - El_{BL_i})$ is the measure of change in earnings during the benefit year associated with the increase in UI benefits. The measure of change in earnings during the year after the benefit year, $\Delta E2_i$, is defined analogously.

The measure of incidence of UI claims for increase i is $\Delta N_i = N_{AH_i}/N_{BH_i} - N_{AL_i}/N_{BL_i}$. This statistic measures the change in incidence of claims for high earnings individuals compared to low earnings individuals. The separate halves of ΔN_i, N_{AH_i}/N_{BH_i} and N_{AL_i}/N_{BL_i} are also generally reported.

The change in weeks of UI benefits, reemployment earnings and incidence of UI claims can be seen for each of the 16 increases in Table 4. The weeks of benefits measure is the number of weeks of full UI benefits[9] received during the benefit year. There is evidence that higher benefits tend to increase the number of weeks of benefits received. Of the 16 increases, 12 show longer unemployment spells. There is no clear pattern to the change in relative incidence of UI claims. There is some pattern of a decline in earnings during the year following the benefit year.

Table 5 reports the results of optimally combining the numbers from the 16 increase of Table 4 into a single number. The statistics from each benefit increase are summed, weighting by the variance of the statistic. For example, the mean change in weeks of benefits received, ΔL is calculated as

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[9]Weeks of full benefits do not include partial UI. Partial benefits are paid when an individual does some work for pay after being laid off from his primary job. Tabulations from the CWBH data indicate that about one-third of UI claimants receive some weeks of partial UI benefits. Most of these people receive partial UI for only a few weeks.

$(\sum 1/var(\Delta L_i))^{-1} \sum \Delta L_i/var(\Delta L_i)$. This formula is just the weighted least squares regression of ΔL_i on a constant, with the weights equal to $1/var(\Delta L_i)$. $var(\Delta L_i) = var(L_{AH_i}) + var(L_{BH_i}) + var(L_{AL_i}) + var(L_{BL_i})$, where $var(L_{AH_i})$ and the other terms are calculated using the usual formula for the variance of a sample mean. The earnings and incidence statistics are averaged in the same manner.[10]

Table 5 reports several estimates of duration, earnings and incidence in addition to those in Table 4. Means and standard deviations for the duration as well as other key sample variables are reported in Table 3. The duration measure, "Weeks of Full Benefits, Only Positive Weeks" excludes those claims that do not result in any benefits being paid. In the CWBH data about 18 percent of claims do not result in the payment of benefits. Roughly half of these claims without benefits are individuals who are disqualified, while the other half are people who find a job before the waiting week expires.[11] The last duration measure is weeks of regular UI benefits. This measure excludes federal and state extended benefits and federal supplemental benefits and is calculated using dollars paid so that it accounts for partial UI. Weeks of regular benefits provide a check against the other results. The changes in

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[10]I have also tried weighted least squares (WLS) regressions using the 16 observations on the change in weeks of benefits or earnings regressed on the corresponding percentage increase in the maximum benefit amount. I do not put much weight on these WLS results since they are not very robust and depend on the treatment of the constant. An example is provided by the weeks of full benefits results. If a constant is included, the coefficient (standard error) for the constant and the percentage increase are -1.505 (1.621) and .294 (.179) respectively. If a constant is not included the coefficient (standard error) for the percentage increase is .133 (.040).

[11]This statement is based on the average initial claim disqualification rate which is 8.5 percent in 1982 as reported in unpublished Unemployment Insurance Service data, and the fraction of spells lasting one week which is 9.4 percent in the CWBH data.

regular benefits are expected to be smaller than the other estimates because they are more likely to be censored at exhaustion of benefits. The additional earning measure is for the period of the benefit year as indicated in Figure 3. The second incidence measure excludes claims that do not result in any benefits being paid.

Table 5 indicates large and significant increases in all measures of the number of weeks of UI received for the average of the sixteen benefit increases. Weeks of full benefits in the benefit year, with or without those with zero weeks, are estimated to rise over one week. Using the means from Table 3, the implied elasticities of weeks of full benefits, including and excluding those with zero weeks, are .698 and .549 respectively. If one takes the average percentage increase in the maximum to be 7.28 (the 9.32 average nominal percentage increase minus the 2.04 average percentage increase in wages) the elasticities are a bit larger, .894 and .703 respectively. In comparison, the average of each duration measure for the placebo periods is small, negative and insignificant. Thus large increases in duration occur after benefit increases, while no significant change is seen for placebo periods.

Table 5 also decomposes the benefit increase and placebo period statistics into changes for the high and low earnings groups separately. The decomposition shows that most of the changes in duration for the benefit increases are due to large increases in spell length for those with high earnings. Those with low earnings experience a small and in most cases insignificant increase in duration. These last results suggest that the true effects of higher benefits are being measured. The results are also consistent with the change in duration for the low earnings group being an

unnecessary control, so that the results for the change in duration for those with high earnings could be used directly.

For the benefit increases, earnings show an appreciable decline during the benefit year. The decline is consistent with more weeks of UI receipt and fewer weeks of work during the benefit year. There is an estimated decline also during the year after the benefit year, but the estimated decline is smaller than that found for the placebo periods. There is essentially no change in the relative incidence of layoffs of high earnings individuals after the benefit increases and the placebo periods. The change in the number of claims is very close to zero and insignificant.

Separate results for those expecting recall and those not expecting recall are reported in Table 7. The exact question is "do you expect to be called back to work by any of your past employers?" The question is part of the CWBH questionnaire which is administered when an individual files for UI. This is the ex ante concept of temporary layoffs discussed in Katz and Meyer (1988). This measure should be preferred to the CPS measure since it is asked at the start of spell for all individuals. The CPS question asks people with in-progress spells of varying length if they are "on layoff awaiting recall by their employers." The CPS recall expectation concept is likely correlated with unexpected changes in spell length if recall expectations change in the course of spells.[12] As indicated in Table 6, about half of the individuals in the benefit increase and placebo period sample expect to be recalled.

Large increases in duration are found for both those who expect recall and those who do not. Comparisons of the two groups are difficult however

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[12]See Katz (1985) for a model where a person updates his recall expectations as a spell continues.

because of the larger standard errors and because the sign of the difference in the point estimates depend on the statistic examined. If one compares the high-low results, then those who expect recall have bigger responses. The reverse is true if one compares the high statistics. The high-low placebos for those expecting recall are surprisingly large and negative. In general, the disaggregated results seem much more variable and are hard to compare because their standard errors are larger.

Similar results are found when the sample is divided by actual recall status rather than expectations. An individual is classified as recalled if his employer during any of the three quarters after his last UI payment matches his employer during either of the two quarters prior to claim. The frequency of recall for each of the 7 states is reported in Table 6.

The last division of the sample is by the marginal tax cost of layoffs. It is difficult to find variation in experience rating across firms that is exogenous to the process of layoffs and recall rates since a firm's tax rate is a function of previous layoff and recall rates. A possible solution is to interact tax status with an exogenous change in benefits, such as an increase in the maximum benefit amount. One can then examine whether tax status affects the response of different firms. This strategy is taken here.

In four of the five benefit increase states[13], I determine the firm which laid off each individual, and then match that firm's UI payroll tax rate for the year to that individual's spell. Given the firm's tax rate, I can determine the firm's location on the state's tax schedule. I then treat the

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[13]The tax cost of unemployment has not been calculated for Pennsylvania since the combination of Benefit Ratio and Reserve Ratio experience rating that is used there is much more difficult to summarize.

schedule as being locally linear and determine how the firm's tax rate would change in response to a small increase in unemployment by its former employees.[14] Then, following the approach of Topel (1983), I calculate the present value of future taxes that the firm would pay if it slightly increased the number or duration of its layoffs. The derivation of the formula for the marginal tax cost of layoffs is reported in Appendix A. The calculations modify Topel's formula for reserve ratio states to account for differing state growth rates in the taxable wage base. The tax cost is the fraction of a dollar that the firm would pay in future taxes if one dollar were paid to former employees by the UI system.

I have divided the sample into UI recipients from low (< .5) and high (> .5) tax cost firms and calculated the average change in duration and earnings for each group. The results can be seen in Table 8. Most theories would suggest a larger increase in duration or incidence of layoffs for low tax cost firms, since they are paying a smaller fraction of the UI costs of the layoff. Surprisingly, the point estimates suggest a greater change in weeks of UI received for those with a high tax cost of unemployment.[15] Most of the differences between high and low tax cost firms are statistically insignificant though. The incidence number for the placebo periods are also

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[14]The determination of states' tax schedules was done mostly by contacting the individual states. Some information was obtained from the Department of Labor's Comparison of State Unemployment Insurance Laws and Commerce Clearing House's Unemployment Insurance Reports. Government entities, reimbursable employers, and employers that are charged the standard rate are excluded from the sample. Changes in tax rates cannot be determined for standard rated firms, since the changes depend on the firms' age and reserve ratio which are unavailable.

[15]This is true for the entire sample as reported in Table 8, and when Idaho and Louisiana are examined separately. These two states have a fairly even split between UI recipients from high and low tax cost firms.

puzzling as there are large changes in incidence in opposite directions for the high and low tax cost firms.

5. Regression Estimates

Regression estimates of duration and earnings effects that control for differences across individuals are reported in Tables 9 through 12. The estimates confirm the average change results reported earlier in Table 5. Tables 9 and 10 report equations for the log of weeks of full benefits received. The Table 9 sample includes both high and low earnings individuals, while Table 10 includes only high previous earnings individuals, since the results of Table 5 suggest that the low earnings individuals are an unnecessary control that adds imprecision to the estimates.

In Table 9, the estimate of the effect of the benefit increases is the coefficient on "After Increase, High Earnings Group (AH Group)". The AH Group dummy variable equals one if an observation is in both the A and H groups. A set of dummy variables has been included in the specifications in Table 9 so that the AH group coefficient has an interpretation similar to the average change estimates in Table 5. All specifications include 15 dummy variables for benefit increases, 16 dummy variables for increases interacted with being in the high earnings group, and 16 dummy variables for increases interacted with being in the group after the benefit increase. In Table 10, the estimate of the effect of the benefit increases is the coefficient on "After Increase," and I have included in the specifications 15 dummy variables for benefit increases.

In both Table 9 and 10, I progressively add more explanatory variables to check if the AH group coefficient is biased by omitted differences between the individuals who file before and after the benefit increases. The controls are fairly extensive; they include race dummy variables, 5 age dummy variables, 6 education dummy variables, 45 industry dummy variables, 4 firm size variables, and additional variables described in Appendix B. The sample sizes decrease as additional variables are added because more observations are excluded due to missing values.

The benefit increase coefficients in Table 9 are large and statistically significant with a value of .07 to .08 through specification (4). This specification already includes controls for previous earnings, the duration of benefits, age, race, education, firm size and 2-digit industry. In specifications (5) and (6) the coefficients are smaller and less precisely measured as one-half to two-thirds of the observations are excluded due to missing variables. In Table 10 the benefit increase coefficients tend to be somewhat more stable, ranging from about .06 to .09 in the six specifications.

The benefit increase coefficients from the Table 9 and 10 regressions imply somewhat larger elasticities than implied by the differences in means of Table 5. Since the dependent variable is in logs, the coefficients are directly interpretable as implying a 7 to 8 percent increase in duration after the benefit increases. Since the increases average 9.32 percent, the implied elasticity is approximately .8, or about 1.0 if one adjusts for the three months of inflation between the before and after groups.

Tables 11 and 12 are analogous to Tables 9 and 10, but the regressions estimate the change in earnings in response to the benefit increases. The dependent variable is (earnings during the 4 quarters after the benefit year -

base period earnings)/base period earnings. The earnings coefficients in Table 11 have relatively large standard errors and are not very stable; the last two specifications have coefficients of the opposite sign from the rest. Those in Table 12 imply a decline in earnings in response to the benefit increases, and three of the coefficients are significantly different from zero at the .05 level. These coefficients provide some evidence for an earnings decline after the benefit increases.

6. Robustness Checks and the July 1981 Georgia Increase

Several alternative samples to the one analyzed in Table 5 are tried as a check on the robustness of the earlier results. Table 13 reports four alternative samples which (1) exclude the month before and after the increases (2) include the Georgia July 1, 1981 increase as a 17th increase (3) include large reemployers, and (4) cover the entire year before and after the Georgia 1981 increase. The first alternative sample which excludes those filing one month before and one month after the benefit increases gives estimates which differ little from those in Table 5 except for slightly larger duration effects and larger standard errors. The sample which includes the Georgia 1981 increase along with the others gives duration estimates which are sixty to seventy percent as large as the ones from the 16 increases. This difference results from a large estimated decline in duration after the Georgia increase.[16] The sample which includes large reemployers gives estimates which are thirty to fifty percent larger for duration effects than

[16]The point estimate (standard error) for the change in weeks of benefits for the Georgia increase is -1.991 (.996).

those in Table 5 and which suggest a slight decline in the incidence of layoffs and a slightly greater decline in earnings than indicated in Table 5.

The last subsample, the year before and after the Georgia July 1, 1981 27.8 percent increase in the maximum benefit amount, was analyzed extensively. I compared the people who filed in a given calendar quarter to people who filed in that same calendar quarter one year later. I excluded the one-half month period just before and after the increase, and the corresponding calendar periods one year earlier or later. These four groups were then averaged in exactly the same way as the 16 increases were above.

The results reported in the last two columns of Table 13 indicate that there are severe problems with using the Georgia increase to determine the effects of higher benefits. During the year following the increase, layoffs resulting in UI receipt rose dramatically, with the increase unevenly split between high and low earnings individuals. While the 16 increases of Table 5 show changes in the two relative incidence measures of -.009 and .000 respectively, the Georgia increase shows changes of -.649 and -.609. These large declines in the relative incidence of high earnings individuals result from enormous increases in incidence for low earnings individuals combined with moderate increases for high earnings individuals.

Furthermore, the implied duration elasticities for Georgia depend greatly on whether the High-Low estimates or the High estimates are used. Since the mean duration of UI receipt in Georgia is about half that for the 16 increases, the elasticities are larger than one might expect from Table 13.[17]

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[17]The mean duration of UI receipt for the Before High group in Georgia with and without individuals with zero weeks is 9.91 and 11.74 respectively, compared to 16.79 and 20.07 respectively for the average of the 16 increases.

For the High-Low statistics, the elasticities for the two duration measures (with and without zeros) are .241 and .086 respectively. For the High statistics the corresponding elasticities are .518 and .371 respectively. Adjusting for the 10.2 percent increase in average wages over the year, the elasticities are .818 and .585 respectively. These last elasticities are similar to those found for the 16 increases above, while the High-Low elasticities are considerable lower than the above elasticities.[18]

Regression equations analogous to those in Table 9 were estimated for the Georgia sample. The coefficient estimates, which depend quite dramatically on whether explanatory variables are included, evidence large changes in the composition of unemployment during this period. In the specification analogous to (4) in Table 9 the coefficient (standard error) on the "AH group" dummy variable is .090 (.056). When specification (1) is tried with this same sample the coefficient (standard error) is .014 (.054). In the specification analogous to (5) in Table 9 the coefficient (standard error) on the "AH group" dummy variable is .105 (.056). When specification (1) is estimated with the same sample the coefficient (standard error) is -.019 (.056). Specification (6) cannot be estimated since occupation is not known for Georgia UI claimants. The specification (4) and (5) coefficients imply benefit elasticities in the .3 to .6 range, with estimates at the higher end if one adjusts for inflation.

A final check on the estimates of Table 5 was performed using information on weeks worked in each calendar quarter which is available for individuals

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[18]This analysis of the July 1, 1981 Georgia increase ignores the effects of taxation of unemployment benefits which was extended to middle income households and individuals on January 1, 1982.

from Pennsylvania and Washington for 5 benefit increases. While the estimates are only suggestive because of large standard errors, the increase in weeks of UI seems to be associated with a roughly comparable decline in weeks worked. The estimate (standard error) for the change in weeks of UI is 1.13 (.76) and change in weeks worked is -.96 (.72) .

7. Conclusions and Extensions

The estimates of duration effects found in this paper are somewhat larger than most previous estimates. The elasticity of weeks of UI benefits with respect to the weekly benefit amount is estimated to be about .8 to 1.0. Previous elasticity estimates have clustered in the .2 to .5 range (see Hamermesh (1977) or Danziger, Haveman and Plotnick (1981)). However, the estimates are comparable to those found by Classen (1979) and are only slightly larger than those found in Meyer (1988a). Classen found an elasticity of .6 in linear equations and about 1.0 in logarithms. When she tried modifications of the OLS results such as splines or Tobits to account for censoring, her estimates always rose.

One might argue that the duration results given here apply to weeks of benefits and may say little about the number of weeks without work. One line of argument is that higher UI benefits induce people to claim earlier. This has been examined by Katz and Meyer (1988) and Solon (1981), who find no support for this hypothesis. A second hypothesis is that filing for UI may increase. There is no evidence in the incidence measures that the number of initial claims increases. One could argue that people file for additional weeks at the end of spells, but do not work less. The evidence on weeks

worked from Pennsylvania and Washington given above is not consistent with this last argument.

The estimates show some evidence of a decline in reemployment earnings following the increases in benefits. Because the confidence intervals around many of these estimates are large, the evidence in less conclusive about effects on earnings. The estimates are, however, consistent with findings in other studies which examine earnings responses. A decline in earnings in response to higher UI benefits was generally found by Classen, but the evidence was not strong. The UI experiments in Illinois and New Jersey which offered payments to people who found a job quickly seemed to reduce the length of UI receipt, but they also resulted in no change in earnings or a slight increase in reemployment earnings.[19]

There are several unsatisfactory aspects of this paper which suggest that the approach should be taken as a complement, rather than a substitute for more conventional regression approaches. Large standard errors prevent the comparison of UI responses for different groups. The reemployment earnings results are somewhat inconclusive. Lastly, the incidence results are puzzling and suggest that a more direct approach like the examination of firm histories of layoffs and recalls may be useful. Other alternative ways of estimating UI effects are estimation using flexible function of previous earnings and weekly benefits, and comparisons of states with the same replacement rate but different maximum benefit amounts.

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[19]See Woodbury and Spiegelman (1987) and Meyer (1988b) for the Illinois experiments, and Corson et al (1989) for the New Jersey results.

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Figure 1

High Quarter Earnings Groups and Weekly Benefit Amounts

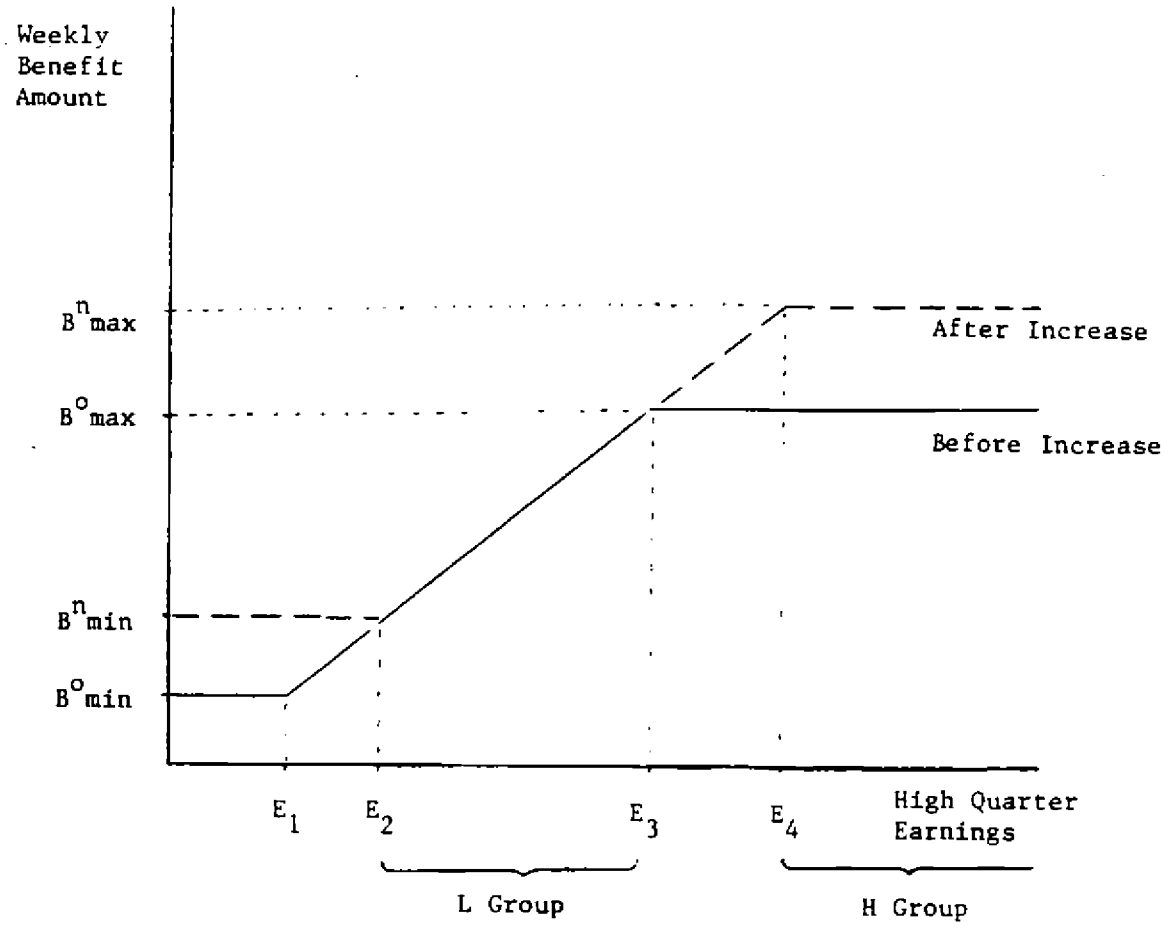

Figure 2
Before and After Groups

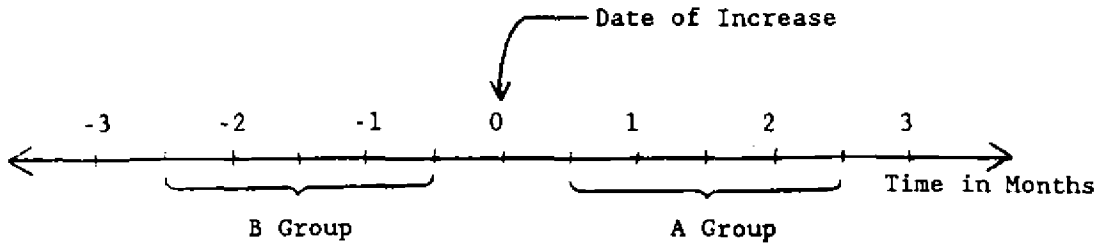

Figure 3
Earnings Periods

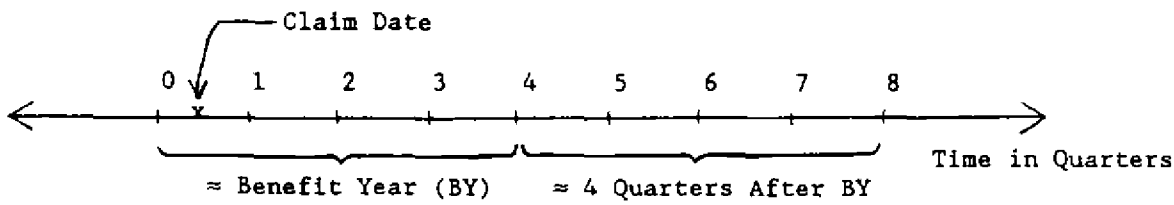

Table 1

Benefit Amounts and Qualifying Earnings Before and After Sixteen Benefit Increases

Date of Increase	Old Max. WBA	New Max. WBA	Percentage Increase in WBA	New Min. WBA	HQE for Old Max. WBA	HQE for New Max. WBA	HQE for New Min. WBA
Idaho							
7/1/79	116	121	4.3	---	3016	3146	-----
7/1/80	121	132	9.1	36	3146	3432	910.01[a]
7/1/81	132	145	9.8	---	3432	3770	-----
7/1/82	145	159	9.7	---	3770	4056	-----
Louisiana							
9/1/79	141	149	5.7	---	3525	3725	-----
9/1/80	149	164	10.1	---	3725	4100	-----
9/1/81	164	183	11.6	---	4100	4575	-----
9/1/82	183	205	12.0	---	4575	5125	-----
New Mexico							
1/4/81	106	117	10.4	24	2756	3042	624[a]
1/2/83	130	142	9.2	29	3380	3692	754[a]

(continued)

Table 1 (continued)

Benefit Amounts and Qualifying Earnings Before and After Sixteen Benefit Increases

Date of Increase	Old Max. WBA	New Max. WBA	Percentage Increase in WBA	New Min. WBA	HQE for Old Max. WBA	HQE for New Max. WBA	HQE for New Min. WBA
Pennsylvania							
1/6/80	152[b]	162[b]	6.6	---	3763	4013	-----
1/4/81	162	175	8.0	35[b]	4013	4338	800
1/2/83	190	205	7.9	---	4713	5088	-----
Washington							
7/6/80	137	150	9.5	41	3425[c]	3750[c]	1025[c]
7/5/81	150	163	8.7	45	3750	4075	1125
7/4/82	163	178	9.2	49	4075	4450	1225

Note: The WBA is the weekly benefit amount and HQE is high quarter earnings.

[a]Qualifying Base Period Earnings were also increased in Idaho to $910.01 and in New Mexico to $780.00 in 1981 and $921.15 in 1983.

[b]The WBAs for Pennsylvania do not include dependents' allowances.

[c]All qualifying high quarter earnings in Washington are actually the average of earnings in the two highest quarters.

Table 2

Placebo Periods

State and Date of Increase		Placebo State
Idaho	7/1/80	Missouri
Idaho	7/1/81	New Mexico
Idaho	7/1/82	Georgia and Missouri
Louisiana	9/1/79	Georgia
Louisiana	9/1/80	Georgia
Louisiana	9/1/81	Missouri
Louisiana	9/1/82	New Mexico
New Mexico	1/4/81	Idaho
New Mexico	1/2/83	Louisiana
Pennsylvania	1/6/80	Washington
Pennsylvania	1/4/81	Louisiana
Pennsylvania	1/2/83	Idaho
Washington	7/6/80	none available
Washington	7/5/81	Pennsylvania
Washington	7/4/82	Pennsylvania

Notes: Both Georgia and Washington are used for 7/1/82 to keep the times of year of the increases and placebos as close as possible since no placebo was available for the 7/6/80 increase in Washington.

Table 3

Means and Standard Deviations of Variables,
Before High Sample, 16 Increases

Variable	Mean	Standard Deviation	Sample Size
Weeks of Full Benefits	16.79	16.08	4901
Weeks of Full Benefits, Only Positive Weeks	20.07	15.59	4102
Weeks of Regular Benefits	16.53	10.15	4171
Percent Increase in Maximum	9.32	1.74	4901
Percent Change in Earnings in State During Period	2.04	.56	4901
Potential Duration of Regular Benefits (weeks)	27.67	3.61	4900
Weekly Benefit Amount ($)	155.32	23.35	4901
Base Period Earnings ($)	19766.62	7679.14	4879
High Quarter Earnings ($)	6612.26	2613.23	4879
Age	36.78	12.10	4816
Years of Education	11.98	2.28	4499
1 if White	.87	---	4787
1 if Expect Recall	.58	---	2959
1 if Have Definite Recall Date	.16	---	2806
1 if Married	.74	---	3498
1 if Industry Construction	.31	---	4901
1 if Industry Metals or Equipment	.19	---	4901
1 if Industry Other Manufacturing	.15	---	4901

Table 4

Change in Duration, Incidence and Earnings
After Sixteen Benefit Increases

State and Date of Change	Percent Increase in Maximum	Change in Weeks of Benefits Received		Change in Number of Claims	Change in Log Earnings Four Quarters After Benefit Year	Sample Sizes, After/Before	
		High-Low	High			High	Low
Idaho							
7/1/79	4.3	-1.380 (2.030)	-0.261 (1.657)	-.603 (.247)	.199 (.184)	78/87	135/90
7/1/80	9.1	2.396 (1.731)	3.043 (1.309)	-.258 (.105)	-.340 (.161)	116/226	138/179
7/1/81	9.8	3.234 (1.785)	2.662 (1.322)	-.041 (.163)	-.013 (.170)	167/165	160/152
7/1/82	9.7	2.363 (2.161)	-2.376 (1.619)	-.166 (.157)	--	191/192	202/174
Louisiana							
9/1/79	5.7	-0.170 (1.276)	2.972 (1.015)	.212 (.138)	.013 (.099)	273/216	384/365
9/1/80	10.1	1.173 (1.588)	1.804 (1.194)	.076 (.074)	-.209 (.134)	123/232	157/346
9/1/81	11.6	1.369 (1.434)	4.034 (1.070)	.066 (.159)	-.093 (.117)	367/250	443/316
9/1/82	12.0	1.056 (1.658)	3.581 (1.181)	.011 (.044)	--	337/709	321/691
New Mexico							
1/4/81	10.4	2.249 (1.546)	1.599 (1.081)	-.193 (.146)	-.286 (.160)	162/186	184/173
1/2/83	9.2	-2.384 (2.131)	-2.622 (1.290)	-.519 (.416)	--	302/138	222/82

(continued)

Table 4 (continued)

Change in Duration, Incidence and Earnings
After Sixteen Benefit Increases

State and Date of Change	Percentage Increase in Maximum	Change in Weeks of Benefits Received		Change in Number of Claims	Change in Log Earnings Four Quarters After Benefit Year	Sample Sizes, After/Before	
		High-Low	High			High	Low
Pennsylvania							
1/6/80	6.6	1.283 (1.595)	0.223 (1.221)	.509 (.159)	.002 (.087)	356/221	410/372
1/4/81	8.0	1.460 (1.455)	.704 (1.067)	.113 (.172)	-.218 (.098)	291/200	361/269
1/2/83	7.9	0.471 (1.632)	-2.174 (1.171)	-.020 (.097)	--	403/421	382/391
Washington							
7/6/80	9.5	-0.728 (1.716)	1.733 (1.109)	-.094 (.074)	-.026 (.104)	327/533	240/339
7/5/81	8.7	0.039 (2.110)	2.697 (1.347)	.252 (.112)	.192 (.105)	470/396	286/306
7/4/82	9.2	3.987 (1.719)	2.234 (1.039)	-.117 (.079)	--	547/729	320/369

Notes: (1) The numbers in parentheses are standard errors. (2) Earnings during the 4 quarters after the benefit year are not available for benefit increases close to the end of the sample period. (3) If one sums the number of observations from each of the benefit increases or placebo periods one obtains:

Group	Benefit Increases	Placebo Periods
After Increase, High Earnings	4510	7936
After Increase, Low Earnings	4345	5061
Before Increase, High Earnings	4901	7242
Before Increase, Low Earnings	4614	4403
Total	18370	24642

Table 5

Average Change in Duration, Earnings and Incidence Measures

	Benefit Increases			Placebo Periods		
	High-Low	High	Low	High-Low	High	Low
Statistics of Form After - Before						
1. Weeks of Full Benefits	1.095	1.473	.503	-.165	.164	.036
	(.417)	(.299)	(.283)	(.312)	(.195)	(.235)
2. Weeks of Full Benefits, Only Positive Weeks	1.029	1.133	.167	-.309	.290	.318
	(.436)	(.307)	(.301)	(.326)	(.202)	(.246)
3. Weeks of Regular Benefits	.674	.765	.158	-.362	-.039	.268
	(.307)	(.218)	(.212)	(.240)	(.155)	(.178)
4. Log of Mean Earnings in Benefit Year	-.109	-.051	.064	.017	.031	.015
	(.025)	(.016)	(.020)	(.022)	(.011)	(.018)
5. Log of Mean Earnings in Year After Benefit Year	-.055	-.015	.041	-.071	-.037	.041
	(.036)	(.023)	(.027)	(.035)	(.018)	(.029)
Statistics of Form After / Before						
6. Number of Claims	-.009	.722	.743	-.004	.934	.977
	(.025)	(.016)	(.017)	(.028)	(.016)	(.022)
7. Number of Claims with Positive Weeks	.000	.759	.784	-.005	.905	.992
	(.029)	(.019)	(.021)	(.032)	(.017)	(.024)

Notes: (1) The numbers in parentheses are standard errors. (2) The numbers in this table are averages of the numbers in Table 4, where the individual benefit increase statistics are averaged weighting by the inverse of the variance of the individual statistics. (3) The average of the High-Low statistics will not in general equal the average High minus the average Low since the weights for the two series differ.

Table 6

Percentage Expecting Recall, Percentage Recalled, and Tax Cost for the Benefit Increase and Placebo Period Samples, by State

State	Percentage Expecting Recall	Percentage Recalled	Percentage with Marginal Tax Cost of Unemployment < .50 in 1981
Georgia	36.3 (6)	41.9 (4)	11.8
Idaho	61.3 (2)	47.8 (2)	50.4
Louisiana	43.6 (5)	30.9 (6)	49.3
Missouri	54.7 (4)	43.9 (3)	
New Mexico	35.5 (7)	25.9 (7)	21.5
Pennsylvania	70.3 (1)	56.2 (1)	
Washington	56.2 (3)	41.1 (5)	100.0

Notes: (1) The numbers in parentheses are ranks. (2) All numbers are calculated using the sample from periods around benefit increases and placebo periods. In each column, missing values are excluded from the calculations, i.e., those whose recall expectations are unknown are dropped from the sample when calculating the percentage expecting recall.
(3) Since the sample exludes those who were part of a large recall, the recall and expect recall fractions are below those from the full sample.

Table 7

Average Change in Duration, Earnings and Incidence Measures
with Separate Results for those who Expect Recall,
and those who Do Not Expect Recall

	Expect Recall				Do Not Expect Recall			
	Benefit Increases		Placebo Periods		Benefit Increases		Placebo Periods	
	High-Low	High	High-Low	High	High-Low	High	High-Low	High
Statistics of Form After - Before								
1. Weeks of Full Benefits	1.527	.984	-.903	-.299	.049	1.446	-.127	.420
	(.688)	(.472)	(.519)	(.294)	(.796)	(.581)	(.532)	(.354)
2. Weeks of Full Benefits, Only Positive Weeks	.959	.382	-1.046	-.207	.969	1.745	-.054	.382
	(.700)	(.481)	(.526)	(.299)	(.825)	(.586)	(.546)	(.357)
3. Weeks of Regular Benefits	.805	.490	-.982	-.563	.686	1.051	-.385	-.015
	(.512)	(.354)	(.390)	(.233)	(.569)	(.405)	(.403)	(.271)
4. Log of Mean Earnings in Benefit Year	-.121	-.029	.046	.051	-.062	-.052	-.070	-.012
	(.037)	(.022)	(.033)	(.016)	(.057)	(.037)	(.043)	(.026)
5. Log of Mean Earnings in Year After Benefit Year	-.081	-.013	-.075	-.041	.027	-.025	-.151	-.049
	(.055)	(.033)	(.055)	(.025)	(.073)	(.051)	(.063)	(.036)
Statistics of Form After / Before								
6. Number of Claims	-.126	.719	.057	.937	.001	.785	-.038	.855
	(.053)	(.028)	(.047)	(.026)	(.049)	(.035)	(.046)	(.027)
7. Number of Claims with Positive Weeks	-.110	.745	.045	.898	-.016	.791	-.049	-.836
	(.057)	(.031)	(.050)	(.027)	(.057)	(.038)	(.056)	(.029)

Notes: (1) See the notes to Table 5. (2) Recall expectations come from the answer to the question "do you expect to be called back to work by any of your past employers?".

Table 8

Average Change in Duration, Earnings and Incidence Measures
with Separate Results for those from Low Tax Cost and High Tax Cost Firms

	Low Tax Cost (< .5)				High Tax Cost (> .5)			
	Benefit Increases		Placebo Periods		Benefit Increases		Placebo Periods	
	High-Low	High	High-Low	High	High-Low	High	High-Low	High
Statistics of Form After - Before								
1. Weeks of Full Benefits	1.207	1.990	-.247	-.726	1.292	2.293	-.150	-.337
	(.744)	(.497)	(.706)	(.431)	(.924)	(.645)	(.577)	(.377)
2. Weeks of Full Benefits, Only Positive Weeks	.299	1.287	-.439	-.419	1.882	2.180	.011	-.197
	(.766)	(.509)	(.703)	(.433)	(1.013)	(.681)	(.609)	(.398)
3. Weeks of Regular Benefits	.547	.977	-.285	.405	1.141	1.199	-.034	-.215
	(.474)	(.327)	(.509)	(.338)	(.720)	(.488)	(.475)	(.326)
4. Log of Mean Earnings in Benefit Year	-.156	-.086	.054	.058	-.111	-.090	.033	.031
	(.040)	(.023)	(.035)	(.020)	(.062)	(.040)	(.044)	(.025)
5. Log of Mean Earnings in Year After Benefit Year	-.033	-.006	-.006	.030	-.056	-.157	-.073	-.046
	(.062)	(.036)	(.062)	(.032)	(.096)	(.064)	(.080)	(.040)
Statistics of Form After / Before								
6. Number of Claims	-.097	.657	.137	.990	-.044	.650	-.134	.793
	(.039)	(.022)	(.067)	(.048)	(.053)	(.036)	(.056)	(.029)
7. Number of Claims with Positive Weeks	-.057	.710	.139	.967	-.085	.669	-.190	.764
	(.044)	(.025)	(.070)	(.049)	(.066)	(.041)	(.066)	(.031)

Notes: (1) See the notes to Table 5. (2) Appendix A describes the construction of the tax cost measure. (3) The low tax cost sample includes observations from Idaho, Louisiana and Washington, for a total of 10 benefit increases and 5 placebo periods. The high tax cost sample includes observations from Georgia, Idaho, Louisiana, and New Mexico, for a total of 9 benefit increases and 9 placebo periods.

Table 9

Regression Equations for Log of Weeks of Full Benefits Received

Variable	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
After Increase, High Earnings Group (AH Group)	.0647 (.0326)	.0739 (.0344)	.0785 (.0343)	.0827 (.0362)	.0503 (.0442)	.0315 (.0525)
Ln(Base Period Earnings)			-.4309 (.0449)	-.4077 (.0482)	-.3559 (.0570)	-.1160 (.0733)
Ln(High Quarter Earnings)			.1707 (.0548)	.0921 (.0595)	.0908 (.0719)	-.1283 (.0873)
Ln(Weekly Benefit Amount) interacted with L Group			.4255 (.0549)	.4711 (.0593)	.3907 (.0726)	.4059 (.0866)
Potential Duration of Regular Benefits in Weeks			.0241 (.0038)	.0231 (.0041)	.0221 (.0005)	.0103 (.0056)
Age, Race and Education Included		yes	yes	yes	yes	yes
Indicators for Extended Benefits and FSC			yes	yes	yes	yes
2-digit Industry and Firm Size Included				yes	yes	yes
Expect Recall and Definite Recall Date Indicators					yes	yes
2-digit Occupation, Marriage and Dependents Included						yes
Sample Size	16,049	13,955	13,955	12,504	8,109	5,659
R-squared	.0398	.0511	.0601	.0822	.1589	.1809

Notes: (1) The dependent variable is the natural logarithm of weeks of full benefits received, omitting observations with zero weeks. (2) Standard errors are in parentheses. (3) All equations include dummy variables for each increase, the increase dummies interacted with being in the high earnings group (H group), and the increase dummies interacted with being in the group after the benefit increase (A group). (4) The complete list of explanatory variables is given in Appendix B.

Table 10

Regression Equations for Log of Weeks of Full Benefits Received,
High Earnings Groups Only

Variable	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
After Increase	.0798	.0849	.0892	.0683	.0579	.0818
	(.0226)	(.0235)	(.0253)	(.0276)	(.0337)	(.0413)
Ln(Base Period Earnings)			-.5217	-.5093	-.4506	-.2267
			(.0574)	(.0626)	(.0769)	(.0936)
Ln(High Quarter Earnings)			.2502	.1959	.1982	-.0103
			(.0639)	(.0706)	(.0876)	(.1051)
Potential Duration of Regular Benefits in Weeks			.0243	.0228	.0156	.0071
			(.0066)	(.0070)	(.0083)	(.0092)
Age, Race and Education Included		yes	yes	yes	yes	yes
Indicators for Extended Benefits and FSC			yes	yes	yes	yes
2-digit Industry and Firm Size Included				yes	yes	yes
Expect Recall and Definite Recall Date Indicators					yes	yes
2-digit Occupation, Marriage and Dependents Included						yes
Sample Size	8,609	7,632	7,632	7,042	4,567	3,343
R-squared	.0322	.0423	.0510	.0892	.1726	.2045

Notes: (1) The dependent variable is the natural logarithm of weeks of full benefits received, omitting observations with zero weeks. (2) Standard errors are in parentheses. (3) All equations include dummy variables for each increase. (4) The complete list of explanatory variables is given in Appendix B.

Table 11

Regression Equations for Earnings During Four Quarters After Benefit Year

Variable	Specification				
	(1)	(2)	(3)	(4)	(5)
After Increase, High Earnings Group (AH Group)	-.0157 (.0408)	-.0630 (.0399)	-.0291 (.0406)	.0129 (.0492)	.0344 (.0612)
Ln(Base Period Earnings)		-.5824 (.0532)	-.6522 (.0544)	-.7081 (.0647)	-.7284 (.0935)
Ln(High Quarter Earnings)		.3411 (.0643)	.4030 (.0667)	.4656 (.0817)	.4782 (.1083)
Ln(Weekly Benefit Amount) interacted with L Group		-.2576 (.0621)	-.2424 (.0648)	-.2864 (.0796)	-.3153 (.1012)
Potential Duration of Regular Benefits in Weeks		-.0025 (.0042)	.0072 (.0043)	.0154 (.0052)	.0196 (.0064)
Age, Sex, Race and Education Included		yes	yes	yes	yes
Indicators for Extended Benefits and FSC		yes	yes	yes	yes
2-digit Industry and Firm Size Included			yes	yes	yes
Expect Recall and Definite Recall Date Indicators				yes	yes
2-digit Occupation, Marriage Dependents Included					yes
Sample Size	11,243	9,616	7,963	5,390	3,431
R-squared	.0432	.1163	.1273	.1477	.1595

Notes: (1) The dependent variable is the increase in earnings during the 4 quarters after the benefit year when compared to earnings during the base period, i.e. (Earnings 4 Quarters - Earnings Base Period)/(Earnings Base Period). (2) The numbers in parentheses are standard errors. (3) All equations include dummy variables for each increase, the increase dummies interacted with being in the high earnings group (H group), and the increase dummies interacted with being in the group after the benefit increase (A group). (4) The sample sizes are smaller than those in the weeks of benefits equations because earnings are not available for benefit increases near the end of the sample period. (5) The complete list of explanatory variables is given in Appendix B.

Table 12

Regression Equations for Earnings During Four Quarters After Benefit Year, High Earnings Groups Only

Variable	Specification				
	(1)	(2)	(3)	(4)	(5)
After Increase, High Earnings Group (AH Group)	-.0256 (.0171)	-.0366 (.0202)	-.0479 (.0236)	-.0477 (.0270)	-.0451 (.0380)
Ln(Base Period Earnings)		-.3503 (.0451)	-.4014 (.0488)	-.4200 (.0598)	-.3680 (.0833)
Ln(High Quarter Earnings)		.2165 (.0487)	.2508 (.0536)	.2520 (.0671)	.2040 (.0898)
Potential Duration of Regular Benefits in Weeks		.0020 (.0047)	.0069 (.0050)	.0101 (.0058)	.0069 (.0069)
Age, Sex, Race and Education Included		yes	yes	yes	yes
Indicators for Extended Benefits and FSC		yes	yes	yes	yes
2-digit Industry and Firm Size Included			yes	yes	yes
Expect Recall and Definite Recall Date Indicators				yes	yes
2-digit Occupation, Marriage Dependents Included					yes
Sample Size	5,440	4,742	4,137	2,847	1,925
R-squared	.0365	.0839	.1118	.1408	.1535

Notes: (1) The dependent variable is the increase in earnings during the 4 quarters after the benefit year when compared to earnings during the base period, i.e. (Earnings 4 Quarters - Earnings Base Period)/(Earnings Base Period). (2) The numbers in parentheses are standard errors. (3) All equations include dummy variables for each increase. (4) The sample sizes are smaller than those in the weeks of benefits equations because earnings are not available for benefit increases near the end of the sample period. (5) The complete list of explanatory variables is given in Appendix B.

Table 13

Average Change in Duration, Earnings and Incidence Measures, Several Alternative Samples

	Excluding 1 Month Before and After Increases		Including Georgia 7/81 Increase		Including Large Reemployers		Year Before and After Georgia 7/81 Increase	
	High-Low	High	High-Low	High	High-Low	High	High-Low	High
Statistics of Form After - Before								
1. Weeks of Full Benefits	1.440	1.555	.733	1.041	1.579	1.835	.665	1.427
	(.562)	(.401)	(.387)	(.276)	(.410)	(.292)	(.384)	(.264)
2. Weeks of Full Benefits, Only Positive Weeks	1.120	1.475	.632	.774	1.640	1.560	.279	1.213
	(.586)	(.413)	(.404)	(.284)	(.429)	(.301)	(.398)	(.277)
3. Weeks of Regular Benefits	.769	1.010	.503	.563	1.081	1.069	.276	.389
	(.414)	(.294)	(.292)	(.208)	(.303)	(.214)	(.346)	(.250)
4. Log of Mean Earnings in Benefit Year	-.107	-.050	-.095	-.039	-.148	-.077	-.123	.046
	(.034)	(.021)	(.025)	(.015)	(.025)	(.015)	(.033)	(.018)
5. Log of Mean Earnings in Year After Benefit Year	-.038	-.004	-.046	.002	-.105	-.053	-.123	.163
	(.049)	(.031)	(.034)	(.021)	(.035)	(.022)	(.038)	(.020)
Statistics of Form After / Before								
6. Number of Claims	.016	.871	-.007	.740	-.045	.686	-.649	1.565
	(.041)	(.026)	(.025)	(.016)	(.024)	(.015)	(.091)	(.043)
7. Number of Claims with Positive Weeks	.042	.912	.003	.778	-.036	.733	-.609	1.594
	(.046)	(.029)	(.028)	(.018)	(.028)	(.018)	(.103)	(.048)

Notes: See the notes to Table 5.

Appendix A: Reserve Ratio Experience Rating

This appendix derives a formula for the amount paid by a firm in future benefits if one dollar is paid to the firm's recent former employees by the UI system. The formula applies to reserve ratio experience rating systems which are is use in most states. The derivation below extends the work of Brechling (1977a, 1977b) and Topel (1983). The notation follows that of Topel.

Some useful definitions are:

μ_t = fraction of employees receiving UI on average during year t,

B_t = UI benefits on an annual basis in year t, i.e. B_t is the average weekly benefit amount times 52,

R_t = reserves credited to employer's acount in year t,

W_t = taxable wage base per employee in year t,

N_t = number of employees in year t,

τ_t = UI tax rate in year t,

θ = geometric growth rate of firm's employment, i.e. $N_{t+1}=\theta N_t$,

γ = geometric growth rate of the nominal taxable wage base, i.e. $W_{t+1}=\gamma W_t$,

i = nominal interest rate, and

r_t = reserve ratio in year t.

The reserve ratio is the ratio of reserves to taxable payroll averaged over the last three years,

$$(1) \quad r_t = \frac{R_t}{\left(\sum_{i=0}^{2} W_{t-i}N_{t-i}\right)/3} \approx \frac{R_t}{W_{t-1}N_{t-1}}$$

for θ and γ close to 1. The change in reserves is the difference between taxes paid and benefits paid to former employees

(2) $R_t = R_{t-1} + \tau_t W_t N_t - \mu_t B_t N_t$. In terms of the reserve ratio

$$(3) \quad r_t \approx \frac{r_{t-1}}{\theta\gamma} + \theta\gamma\tau_t - \frac{\theta\gamma\mu_t B_t}{W_t} .$$

Let the tax schedule be approximated by the linear relationship

(4) $r_{t+1} = \eta_0 + \eta_1 r_t$, or

(5) $$r_t = \frac{\eta_0 - r_{t+1}}{\eta_1} .$$

Substituting (5) in (3) yields

(6) $$\frac{\eta_0 - r_{t+1}}{\eta_1} \approx \frac{\eta_0 - r_t}{\theta\gamma\eta_1} + \theta\gamma r_t - \theta\gamma\mu_t B_t/W_t , \text{ or}$$

(7) $$r_{t+1} \approx (\eta_0 - \eta_0/(\theta\gamma)) + (1/(\theta\gamma) - \theta\gamma\eta_1) r_t + \theta\gamma\eta_1\mu_t B_t/W_t .$$

If one multiplies (7) by the wage base and employment one obtains the total tax bill for year t+1

(8) $$N_{t+1}W_{t+1}r_{t+1} \approx (\eta_0\theta\gamma - \eta_0)N_tW_t + (1 - \theta^2\gamma^2\eta_1)N_tW_tr_t + \theta^2\gamma^2\eta_1N_t\mu_tB_t .$$

Now, if $N_tB_t\mu_t$ increases by 1 dollar, the present value of the implied increase in future taxes is

(9) $$\Delta PV \text{ taxes} \approx \frac{\theta^2\gamma^2\eta_1}{(1+i)} + \frac{\theta^2\gamma^2\eta_1(1-\theta^2\gamma^2\eta_1)}{(1+i)^2} + \frac{\theta^2\gamma^2\eta_1(1-\theta^2\gamma^2\eta_1)^2}{(1+i)^3} + \ldots, \text{ or}$$

$$= \frac{\theta^2\gamma^2\eta_1}{(1+i) - (1 - \theta^2\gamma^2\eta_1)} .$$

The extent to which (9) is less than one is a measure of the degree of subsidy to layoffs or prolonged unemployment spells.

Appendix B: Explanatory Variables Used in Regressions

Base Period Earnings are earnings during the first four of the five calendar quarters prior to the claim date for UI.

High Quarter Earnings are earnings during the quarter of highest earnings in the base period.

The Weekly Benefit Amount is the weekly UI benefit received, which includes dependents' allowances in Pennsylvania.

The Potential Duration of Regular Benefits is the number of weeks of state regular benefits to which an individual was entitled during his benefit year.

Age: dummy variables for age 25-34, 35-44, 45-54, 55-64, and 65 or older.

Race: dummy variables for black, and other races.

Education: dummy variables for years of education equal to 8, 9-11, 12, 13-15, 16, and 17 or more.

The indicators for extended benefits (EB) and Federal Supplemental Compensation (FSC) take the value 1 if any time during the individuals benefit year EB or FSC respectively is available, and 0 otherwise.

Industry: 45 dummy variables that are mostly 2-digit Standard Industrial Classification industries, with some grouping of small industries together.

Firm Size: dummy variables for average number of employees between 20 and 99, 100-499, 500-1999, and 2000 or more.

Recall expectations: dummy variables for whether or not the individual expects to be called back to any former employer, and whether or not he has a definite recall date.

Occupation: 41 dummy variables for occupations that are mostly 2-digit occupations from the Dictionary of Occupation Titles, with some grouping of small occupations together.

Marriage: dummy variables for currently married, and never married.
Dependents: number of dependents.