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SPILLOVER EFFECTS BETWEEN THE INSURED AND UNINSURED UNEMPLOYED

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ABSTRACT

In this paper, I consider the effect of changing the level of unemployment insurance (UI) benefits on workers who do not receive UI. I hypothesize that a spillover effect between insured and uninsured workers exists so that an increase in the UI benefits, which leads to longer durations of unemployment for insured workers, will lead to a reduction in the duration of unemployment for the uninsured. This prediction is tested using data from several March Current Population Surveys and the National Longitudinal Survey of Youth. In both samples I find that an increase in UI benefits leads to a reduction in the duration of unemployment for uninsured workers. Furthermore, using several years of state level data, I show that the estimated effect on unemployment for the entire labor force is roughly zero when I allow for the spillover effect.

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The disincentive effects of the Unemployment Insurance (UI) system have been extensively studied by economists over the last two decades. Both theoretical (e.g. Mortensen, 1970; Feldstein, 1976; Baily, 1977) and empirical analyses (e.g. Ehrenberg and Oaxaca, 1976; Feldstein, 1978; Topel, 1983) have concluded that an increase in the level of UI benefits would cause more unemployment. At this point, the only debate in the literature concerns the magnitude of the additional unemployment created by an increase in UI benefits, not its existence (Feldstein, 1972; Marston, 1975; Hamermesh, 1977).

Of all the workers unemployed at a point in time, however, only about one-third receive UI (see Blank and Card, forthcoming). Most of the uninsured are ineligible for benefits. Workers who have newly entered the labor market, or re-entered after an absence of over a year, workers with unstable work histories, and those who have exhausted their benefits are all ineligible to receive UI.[1] These groups comprise close to 60% of the unemployed. In addition over one-quarter of those eligible for UI payments do not take up their benefits. In a general equilibrium framework, this institutional feature of the UI system has significant implications since a change in the behavior of insured workers may alter the outcome for uninsured workers.[2]

The purpose of this paper is to examine the effects of the UI system on unemployed workers who do not receive benefits. I hypothesize that a spillover effect exists between the insured and uninsured unemployed. If one considers insured and uninsured workers as two types of substitutable labor,

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[1]Workers who quit their jobs have been ineligible for UI in all states since 1986. Before then, some states allowed these workers to collect benefits typically after a substantial waiting period.

[2]This point has been made by Mortensen (1977), Hamermesh (1979), Albrecht and Axell (1984), Atkinson (1987) and Burtless (1990).

then one might expect that changes in the unemployment behavior of insured workers would have a spillover effect on those who are uninsured.[3] In particular, if unemployment increases among insured workers in response to an increase in UI benefits, one might also expect unemployment among the uninsured to decrease.

Ignoring this spillover effect could have significant implications for an analysis of the effects of UI on total unemployment. For example, in an influential paper, Martin Feldstein (1978) provided evidence that 50% of all layoff unemployment could be attributed to the UI system. Citing his previous theoretical work (Feldstein, 1976), Feldstein argued that since the financing methods for the UI system provide subsidies to many firms to layoff an additional worker, firms respond to this incentive by increasing the layoff unemployment rate.

However, Feldstein analyzed the effect of an increase in benefits on layoff unemployment exclusively; the effect on total unemployment may be, in fact, quite different. Applying Feldstein's methodology, Table 1 presents estimates of the effect of an increase in the ratio of UI benefits to pre-unemployment wages (the replacement ratio) on unemployment spells reported in the March 1979 and March 1982 Current Population Survey.[4] The specifications

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[3]The spillover effect that I refer to here is designed to reflect the substitution between workers in two separate labor markets. Other researchers (i.e. Mortensen, 1977; and Hamermesh, 1979) have discussed "entitlement effects" to explain similar behavior. This concept refers to uninsured workers taking jobs more quickly in order to receive UI benefits at some point in the future. For empirical purposes, however, I feel that the two effects are indistinguishable.

[4]These years were chosen for two reasons. First, they represent the highest and lowest unemployment rates between 1978 and 1987, the years for which the remainder of this paper will consider. Second, the taxation of UI benefits were different in these two years. In 1979, UI benefits were treated as taxable income only for the wealthy and in 1982 benefits were taxed for

reported here are essentially identical to those reported by Feldstein. Unlike Feldstein, however, I report results for all groups of unemployed workers; those on layoff and those who entered unemployment for other reasons.[5] The dependent variable in these regressions is unity if the worker is unemployed for the indicated reason and zero for all other workers in the labor market (those employed and those unemployed for other reasons). The top row of Table 1 indicates the fraction of unemployed workers who receive UI by reason unemployed.

The results of this analysis confirm Feldstein's findings that layoff unemployment increases as UI benefits increase (see column 1). However, they also suggest that uninsured workers exhibit behavior unanticipated by Feldstein. Among job quitters and the "other" group (mainly comprised of reentrants), both of which have very low rates of UI receipt, an increase in benefits leads to a reduction in unemployment.

This paper provides a more rigorous test of the spillover hypothesis. In the first part of the analysis, I focus on the effects of changes in the level of UI benefits on the duration of unemployment for uninsured workers using several years of data from the March Current Population Survey (CPS) and the National Longitudinal Survey of Youth (NLSY). In the CPS sample the duration is proxied by the fraction of the calendar year spent looking for work. Uninsured workers are identified as those who report experiencing some

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most workers. Hence, choosing these two years also allows a test of the sensitivity of the results to tax treatment.

[5]The only other difference is that the replacement rate is computed as the mean over all insured workers in the state and year (its derivation is detailed both below and in Appendix 2) and is assigned to each worker in that state and year, regardless of their insurance status. Assigning workers estimated to be ineligible a zero replacement rate, as in Feldstein's analysis, does not qualitatively change the results reported here.

unemployment and receiving no income from unemployment insurance during the calendar year. The format of the NLSY allows me to measure completed unemployment spell durations directly and to segregate the sample according to UI recipiency by observing UI receipt between jobs. In each case, I find support for the hypothesis that an increase in the UI replacement rate leads to a reduction in the duration of unemployment for uninsured workers.

I then consider the effect of changes in the level of UI benefits on the aggregate unemployment rate, explicitly allowing for spillover. Several years of data aggregated to the state level are used in this part of the analysis. Here I find that an increase in UI benefits leads to an increase in insured unemployment and a decrease in uninsured unemployment, resulting in no net effect on the aggregate unemployment rate.

The paper proceeds as follows: section I discusses theoretical considerations regarding the possible spillover between the insured and uninsured. Sections II through IV focus on the effect of a change in UI benefits on the duration of unemployment using the two micro level data sets. The second section describes the estimation strategy, the third discusses how the data sets were created, and the fourth presents the results. Section V describes the data and reports the results of the analysis estimating the effect of a change in the UI benefit level on the aggregate unemployment rate. Section VI offers concluding comments.

I. THEORETICAL CONSIDERATIONS

This section will discuss two relatively simple models of the labor market which incorporate both insured and uninsured spells of unemployment and which predict a spillover effect. While, clearly, other models could provide the same prediction, those presented here are chosen to simply demonstrate the intuition behind the spillover hypothesis and are not rigorously proven.[6]

The first model considers a simple supply and demand framework in which the labor market is perfectly segmented into an insured and uninsured sector, although workers in the two sectors are perfectly substitutable. This last assumption forces the wage rate to be equal in the two sectors because if they were not, employers would hire workers from the lower-paying sector. The wage rate is determined by the equilibrium in the aggregate labor market, equating supply with demand. Unemployment in this model is simply leisure, so any increase in employment is a reduction in unemployment.

The effects of an increase in UI benefits can be seen graphically in figure 1. In the insured sector, an increase in UI benefits results in the labor supply curve shifting back.[7] Workers in this sector require a higher wage to supply the same amount of labor since their leisure is being "subsidized" by the UI system. Since the labor supply curve in the uninsured sector remains unchanged, the aggregate supply curve shifts back as well. The equilibrium wage rises from W_1 to W_2 and aggregate unemployment increases by $L_1 - L_2$. Unemployment in the insured sector increases as well, by the amount $L^i{}_1 - L^i{}_3$. However, the wage increase leads to higher employment (lower unemployment) in the uninsured sector as more workers choose to work at the higher wage. In this simple two-sector model, the mechanism creating the spillover is the change in the wage rate.

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[6]Appendix 1 more formally develops a search-theoretic model of unemployment which predicts the possibility of a spillover effect.

[7]See Moffitt and Nicholson, (1982) for a formal derivation.

A second model depicts unemployed workers searching for job openings among firms who all pay the same wage.[8] The number of offers a worker receives in a given period increases with the intensity of that worker's search effort and decreases with the search intensity of other workers.[9] Workers are assumed to be risk-neutral and identical except that the insured receive a periodic payment while unemployed. The level of search intensity is chosen to maximize the expected value of income. The solution to this problem is to equate the marginal costs of search with the marginal benefit, which is the product of the probability that an offer arrives and the return to employment (the wage rate less the value of leisure).

A standard result in the search literature is that if UI benefits increase, then workers who receive UI will reduce the intensity of their search. This follows directly from the fact that higher UI benefits increase the value of leisure, reducing the marginal benefit of search. As a result, the average duration of unemployment spells will increase for UI recipients.

However, in a model with both insured and uninsured workers, this change will lead to spillover. Since insured workers do not look for work as hard as benefits go up, the offer arrival rate to uninsured workers will increase, ceteris paribus. In addition, the marginal benefit of additional search for the uninsured will increase and, since the marginal costs of search have

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[8]This model is a direct extension of the search on the job model in Mortensen (1986) where, in this case, the wage distribution is degenerate.

[9]While I assume this proposition it is straightforward to prove it under certain circumstances. As a simple example, suppose the number of job openings are fixed, that employers make an offer to the first worker who applies for the job, and that firms can only make one offer per period. Then if, say, insured workers search harder, they will have a higher probability of applying for any given job first and will therefore have a higher offer arrival rate. Since the number of openings are fixed, the number of offers that uninsured workers receive necessarily decreases.

remained unchanged, these workers will search harder increasing their offer arrival rate even more. In the end, uninsured workers will be more likely to receive an offer, and since no offers are rejected in this model (due to the fixed wage), will experience shorter spells of unemployment on average.

II. ESTIMATION STRATEGY

The prediction of spillover between the insured and uninsured unemployed is empirically tested in the following three sections of the paper. This section discusses the general form of the data available and the methods applied to test the spillover hypotheses. More specific details regarding the data sets used in estimation are presented in the following section.

The empirical analysis uses several March Current Population Surveys (CPS) and the National Longitudinal Survey of Youth (NLSY). In the CPS March supplement respondents supply retrospective information on labor market activity and income recipiency for the preceding calendar year. Individuals are asked how many weeks they were unemployed last year, if any, and whether or not they received any unemployment compensation.[10] The NLSY offers a work history section which carefully documents periods of employment and offers the opportunity to estimate completed unemployment spell lengths.[11] UI receipt during periods of nonemployment can also be determined easily.

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[10]While I use self-reported UI receipt to segregate the sample, others have pointed out that recipiency is underreported in the CPS data (see, for example, Hutchens; 1981). Given the retrospective nature of the UI recipiency questions in the NLSY, this source of data probably suffers from a similar shortcoming. Unfortunately, to obtain information on UI nonrecipients, no better data sets exist.

[11]See Appendix 3 for a full description of the way completed spell lengths are estimated from the NLSY. To limit the problems associated with right censoring inherent in this approach, I only include unemployment spells that begin before 1986 even though individual work histories in the NLSY continue through all of 1986 and into 1987.

Applying these data to test the spillover hypothesis, I estimate a linear model of the form:

$$D_{ist} = \alpha + \beta * RR_{st} + \gamma X_{ist} + \epsilon_{ist}$$

where: D = Measure of duration (fraction of the year unemployed in the CPS sample; the log of completed spell length in the NLSY)
RR = average replacement rate of UI benefits to preunemployment earnings for insured workers in state s and year t
X = vector of demographic variables
i = indexes individuals
s = " states
t = " time

The replacement rate (RR) is measured as the mean replacement rate for all individuals receiving UI in a given state and year; it is not indexed over individuals.[12] It is assigned to all workers in that state and year, regardless of their insurance status. For uninsured workers, this specification is designed to test the effect of a change in the replacement rate for the typical insured worker, who acts as a representative substitute for the uninsured. For insured workers, data limitations and the desire for consistency leads to this specification.

The implications of the models presented in the previous section are that β should be positive for the insured and may be negative for uninsured workers. While I estimate this model for both insured and uninsured workers, the strategy and data employed here is not particularly well-suited for analyzing the behavior of insured workers. Using the state/year mean replacement rate ignores individual variation that could provide more precise estimates. In addition, the group of insured workers is poorly defined in both samples (as described in the following section). Finally, due to its age

[12]The creation of this variable is described briefly in the next section and, in detail, in Appendix 4.

composition, the NLSY includes relatively few insured unemployed workers and these workers are hardly typical of the insured population. As a result, insured workers mainly act as a "control group" in this analysis to determine whether some spurious correlation is driving the results obtained for the uninsured group.

Ordinary least squares estimation of this model could lead to biased estimates of the coefficient, β, if there is an omitted variable, if RR is endogenous, or if selection into the insured or uninsured sample is related to RR. To eliminate, or at least reduce, the first form of bias I include state and year fixed effects along with standard demographic variables and the employment growth rate in the state and year, a proxy for local labor market conditions. With both state and year effects included in the model, β is essentially a difference in difference estimator since it can only pick up changes across both states and years. Endogeneity bias could occur if, say, a downturn in the economy led to a change in the composition of the insured unemployed which in turn led to a change in RR. To test the extent of this form of bias, I apply two stage least squares using the maximum weekly benefit amount (WBA) in the state/year as an instrument for the replacement rate. Finally, I also estimate the model applying a Heckman correction to test for bias caused by nonrandom selection into the insured and uninsured samples.

Although the fraction of the year unemployed among CPS respondents is not a direct measure of the duration of unemployment spells, under reasonable conditions the two measures are highly correlated. In fact, in a two state model of employment and unemployment, if unemployment spells are distributed exponentially and a change in RR does not affect the incidence of unemployment (as might be expected for uninsured workers), a simple mapping exists between the two measures. Under these conditions, I show in Appendix 2 that $\beta_{CPS} = E(F)\beta_{NLSY}$, where β_{CPS} and β_{NLSY} are the estimated coefficients on RR from the CPS and NLSY samples, respectively, and E(F) is the mean fraction of the year unemployed conditional on experiencing at least one spell of unemployment (available from the CPS data).

III. CREATION AND CHARACTERISTICS OF THE DATA

The key right hand side variable in the models presented above is the mean replacement ratio of UI benefits to preunemployment wages in a worker's state and year. Ideally, one would calculate this using information on each insured individual's WBA and average weekly wage prior to his/her spell of unemployment. The UI replacement rate for each insured individual could then be determined simply by taking the ratio of the former to the latter. Unfortunately data of this form is unavailable in either data set.

However, the structure of the March CPS surveys provides the means to create this variable. In the CPS workers are surveyed for four consecutive months and then again for four consecutive months after an eight month absence. In March, workers are asked questions regarding their labor market activity in the preceding calendar year. For 1/2 of the sample in any month, an individual's responses can be matched between one year and the next to provide three years of information for each person surveyed.[13] In year t, the first year the individual is in the survey, s/he answers questions for the current year and, retrospectively, for year t - 1. Similarly, in year t + 1 s/he provides current information for year t + 1 and retrospective information

[13]While, theoretically half of the sample can be matched, since the CPS identifies households and not individuals, the matching process is inexact. Only about 70% of those eligible (35% of the entire sample) can be reliably matched between one year and the next.

for year t. Several key questions are included in the retrospective part of the survey which are crucial for the analysis to follow. First, workers are asked if they spent any time looking for work last year and, if so, how many weeks did they look. Workers are also asked how much money they earned and how many weeks they worked in the preceding year. Finally workers are asked whether or not they received <u>any</u> unemployment compensation during the preceding year.

The ability to match two successive surveys allows me to simulate a measure of the state/year mean replacement rate that follows the desired principles as closely as possible for the years 1978 through 1984, 1986, and 1987 (see Appendix 4 for a more detailed discussion of the simulation method).[14] To do this, I identify a sample of workers who received UI in year t. Computing a worker's weekly wage in year t - 1 as the quotient of earnings and weeks worked information in t - 1, I apply this wage measure to state unemployment insurance benefit schedules (which indicate a WBA for any given dollar amount of the appropriate earnings measure) to simulate what each worker's WBA would be.[15] Then, dividing the simulated WBA by the worker's average weekly earnings in the preceding year (annual earnings divided by

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[14]Replacement rates for 1985 are omitted because in 1985 the sampling structure of the CPS changed and responses of those individuals first surveyed in March of 1985 cannot be matched to their responses in March of 1986 (see Creighton and Wilkinson, 1984). In addition, replacement rates for Michigan cannot be computed after 1980 because the state changed its benefit formula to an after-tax computation. Since the difficulty in computing each worker's after-tax earnings would be immense, all workers in Michigan after 1980 were dropped from the sample.

[15]This approach is technically not the correct method for computing worker's UI benefits. Benefit formulas are typically a function of a worker's high quarter wage in his/her "base period", which is the first four of the last five calendar quarters preceding the worker's unemployment spell. Unfortunately, the detailed wage records to more precisely simulate the benefit computation are not available in the CPS.

weeks worked), provides an estimate of each worker's individual replacement rate. Taking the mean over all workers in each state/year cell simulates the replacement rate measure desired.

As a crude test of the success of the simulation method, the predicted state/year means of the WBA can be compared to the reported values from administrative records.[16] The unweighted mean simulated WBA over all state/year cells is $1.59 higher than that reported. While this difference is statistically significant, it is economically negligible. Furthermore, the correlation between the simulated and reported state/year cell means is 0.94. These results indicate that the simulation method sufficiently replicates the benefit determination process and can be used to measure RR.

The CPS data used in the remaining analysis comes from the regular March Current Population Surveys (not matched), for the years that the replacement rate measure can be simulated (i.e. the 1979-85, 1987, and 1988 surveys providing retrospective information for 1978-84, 1986, and 1987). Since RR is simply assigned to workers according to their state of residence and year of unemployment, the full CPS can be utilized, extracting data on the fraction of the year spent unemployed and UI recipiency from the retrospective information obtained in the March sample. Receiving some UI in the past year does not guarantee that UI was received during all weeks spent looking for work last year. Hence splitting the sample according to this variable creates a poorly

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[16]Administrative records data is available from the U.S. Department of Labor, Employment and Training Administration, "Employment and Training Handbook 394" and its annual updates which are circulated as Unemployment Insurance Program Letters.

defined group of insured workers since this group is likely to include a combination of insured and uninsured unemployment spells.[17]

Descriptive statistics for the CPS sample can be found in Table 1. Over the nine sample years, 123,923 unemployed workers are identified, of which 29% reported receiving some UI.[18] On average, workers who experienced a spell of unemployment were unemployed for roughly 1/3 of the year; insured workers were unemployed two weeks longer than uninsured. The mean replacement rate over states and years in the sample is about 45% of preunemployment earnings. Demographically, the uninsured sample is younger, more female, more nonwhite, and less likely to be married than the insured. The level of education and the number of children is essentially identical between the two groups.

I also test the implications of the model using data from the 1987 NLSY, a nine year panel (1979-87) of 6,111 individual's born between 1957 and 1964.[19] The replacement rate variable is merged onto the NLSY from the CPS according to the year that the spell began and the state that the worker lived

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[17]One can also argue that measurement error in the UI recipiency variable will lead to nonuniform samples of both insured and uninsured workers. However, the point to be made here is that even in the absence of measurement error, the insured sample is poorly defined.

[18]Determining the accuracy of this statistic is difficult because the form of this data is different than anything available in published statistics. While UI administrative records can provide either the average number of claimants in a given week or the number of first payments made in a calendar year, neither of these numbers is exactly analogous to the number of UI recipients in a calendar year. In addition, the definition of unemployment that I am using (essentially self-reported) is stricter than the official definition of unemployment (see Levine, 1990). Hence, comparisons with the frequently reported ratio of insured to total unemployment are next to impossible.

[19]The sample utilized in this analysis consists of the nationally representative half of the full NLSY. The other half oversamples blacks, hispanics, and poor whites. A major strength of this survey is its very small attrition rate. Of those who first responded to the survey in 1979, roughly 95% were still in the sample by 1987.

at the interview date in the year the spell began. The unit of observation in this data set is a completed spell of unemployment, not an individual.

For the present purposes, this source of data provides both strong advantages and disadvantages. On the positive side, the NLSY is available for nine consecutive years and, since the respondents to the survey are young, one should expect to observe many periods of unemployment. Furthermore, in most of these periods the unemployed worker did not receive UI.

However, the form of the unemployment data in the NLSY has many shortcomings. If the focus of the analysis is to measure the effect of UI on unemployment as opposed to nonemployment, then the main problem is that the specific timing of unemployment spells cannot be precisely determined. The only information that exists is the respondent's labor force status at each survey date and the number of weeks spent looking for work either between the end of a job and the preceding survey date/previous job or between the survey date and the preceding survey date/previous job. In Appendix 3 I describe how I use this data to estimate completed spells of unemployment and to determine the timing of their occurrence. However, the potential for measurement error is large.[20]

[20]To counteract this problem some researchers have analyzed spells of nonemployment instead since the exact starting and ending dates can be determined directly from the data (c.f. Gritz and MaCurdy, 1990 and Lynch, 1989). While this strategy avoids the measurement error problem, it does not test the same hypothesis. One might suspect that in a three state model of employment, unemployment, and out of the labor force (OLF), a worker may employ strategic behavior in choosing between unemployment and OLF which would counteract the implications of the spillover hypothesis. For instance, a nonemployed worker may have a general idea of when s/he wants to return to work. If this worker lived in a high benefit state/year and understood that the offer arrival rate would be higher, s/he might start looking for work later than otherwise. In this way fewer weeks may be spent looking for work, as suggested by the model, but the nonemployment spell length may be unchanged. Again the results will be biased towards zero. Therefore, estimation of this model will be set aside for future research.

In the analysis below, unemployment spells beginning in 1978-84 are used. Spells starting in 1985 are dropped from the sample since no measure of RR can be created from the CPS for that year. Due to the uncertainties of perfectly dating spells of unemployment, a spell is assigned to the UI recipient group if <u>any</u> UI was received between the job preceding and following the unemployment spell. This strategy causes the same problems that are observed from the CPS sample in defining an insured sample of spells.

Descriptive statistics for the NLSY sample are also found in Table 2. Only 11.5% of the unemployment spells are included in the insured group, reflecting the age of the sample and the resulting high degree of UI ineligibility. Among NLSY respondents, unemployment spell durations are considerably longer for insured workers relative to the uninsured. An insured spell lasted roughly 15 weeks, on average, while an average uninsured spell lasted only nine weeks. As in the CPS sample, the uninsured sample is younger and comprised of more women and nonwhites. Again, fewer of the uninsured are married and they have fewer children although there is little difference between the insured and uninsured groups in the amount of education. However, the uninsured are considerably more likely to still be enrolled in school. Relative to the CPS sample, the NLSY sample is younger (by sample design) and most of the differences between the two samples are consistent with this fact. The larger percentage of nonwhites in the NLSY sample illustrates the magnitude of teenage unemployment among blacks.

IV. EMPIRICAL RESULTS

Table 3 presents the results of the analysis for UI nonrecipients using the CPS data. Column (1) reports the results for the simplest form of the model estimated for the uninsured unemployed. The results confirm the hypothesis of the model; an increase in the UI replacement rate leads to a decrease in the fraction of the year unemployed for uninsured workers. The coefficient on RR is negative and significant at conventional levels. To provide some intuition to the magnitude of the coefficient, a 10% increase in RR is estimated to lead to a 2% decrease in the fraction of the year unemployed, or a 1 week decrease in the number of weeks unemployed in the calendar year.

Demographic characteristics are shown to have the expected effect on the fraction of the year unemployed. Nonwhites, men, older people, the less educated and single workers with fewer children are predicted to have longer spells of unemployment. These findings are consistent with other studies in the literature (c.f. Ehrenberg and Oaxaca, 1976).

The remainder of Table 3 is designed to test for omitted variable bias and endogeneity bias. Year and state fixed effects are added separately in columns (3) and (5), respectively and then together in column (7). F-tests indicate both sets of dummy variables need to be included in the regression. The coefficient on RR goes down when year dummy variables are included in the model. This is consistent with the view that over the decade, durations of unemployment have increased and replacement rates have fallen. On the other hand, the replacement rate coefficient increases when state dummy variables are excluded. This finding is compatible with the fact that in large, northern industrial states (i.e. the rust belt) unemployment durations are longer and replacement rates are higher. All in all, these results indicate that to control for omitted variable bias, both state and year fixed effects need to be included in the model.

The effect of instrumenting RR measure with the maximum weekly benefit amount is presented in column (8). A Hausman test indicates that the coefficient on RR is significantly different between the two stage least squares and OLS estimates. The need to instrument RR appears to be necessary due to measurement error rather than endogeneity bias. A measurement error explanation is consistent with the finding that the RR coefficient is heavily biased towards zero in the OLS results when year and state fixed effects are controlled for. Since replacement rates are simulated using state/year benefit formulas, controlling for year and state fixed effects will absorb much of the signal in RR and will lead to greater attenuation bias.

When RR is instrumented and both state and year fixed effects are included, a 10% increase in the UI replacement rate is predicted to lead to a 5.45% reduction in the fraction of the year unemployed, or a 2.8 week reduction in the weeks unemployed in a given year. This effect is very large considering a "large" predicted increase in the duration of a spell of unemployment for insured workers for the same change in RR in the existing literature is 1.5 weeks (c.f. Meyer, 1990, for a recent example). While I do not mean to minimize the magnitude of this effect, one must remember that the dependent variable is not spell length, as typically estimated in the literature, but fraction of the year unemployed, which could potentially include more than one spell throughout the year.[21]

When I estimate the model for the insured group, F-tests and Hausman tests indicate that both state and year fixed effects need to be included and RR needs to be instrumented. The estimates of this specification are reported

[21]The CPS also asks workers how many spells of unemployment they experienced in the previous year where the choices are one, two, or three or more. The mean of this variable for the uninsured sample is 1.27.

in Table 4, column (8). While the coefficient on RR is hypothesized to be positive, and a substantial literature exists confirming this hypothesis, here I find the coefficient to be negative, but insignificant.[22] The failure to find a positive relationship can likely be accounted for by the poor definition of an insured worker in this sample, as discussed in section III. However, because the predicted effect of RR on duration is not significantly negative, as in the uninsured sample, the estimated effect for the uninsured is unlikely to be caused by some spurious correlation.

Table 5 tests the sensitivity of the CPS results for both uninsured and insured workers to additional model specifications. First I apply both OLS and two stage least squares estimation using the administratively reported ratio of the mean weekly UI benefit to the mean average weekly wage in a state and year instead of the replacement rate variable which I have created.[23] Next, I test the sensitivity of the results to the employment growth rate variable, which may be endogenous to the extent that unemployment durations and the employment growth rate are both cyclical indicators. I also estimate the models excluding the recessionary years of 1982-1984. To see whether a second difference in second difference estimator allows for stronger identification of the model, I also include only even numbered years between 1978 and 1987.

The final specification in Table 5 tests the sensitivity of the estimates to the use of the maximum benefit as an instrument. One might be

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[22]A Chow test indicates that the coefficients for the insured group are significantly different than those for the uninsured group.

[23]Since the desired measure would be E(WBA/AWW) where AWW = average weekly wage, the administratively reported replacement rate, E(WBA)/E(AWW), is biased to the extent that WBA and AWW are correlated.

concerned that this variable is not a valid instrument since a state UI system under financial strain during a recession will not increase the maximum benefit, resulting in a negative bias to the replacement rate coefficient. To test the sensitivity of the results to this sort of effect, I use a dummy variable equal to unity if the state did not increase its maximum benefit in a given year and zero otherwise. This variable has exactly the opposite bias in that a state experiencing a recession may be more likely to keep its maximum benefit constant. If the estimated effect of a change in the replacement rate was solely driven by the invalid instrument, it will be reversed by using the "no adjustment" instrument.

The results presented in Table 5 indicate that the support for the spillover hypothesis is indeed robust to alternative specifications. Estimates in the first five columns are quite comparable to the those reported in Tables 3 and 4. While for uninsured workers, the replacement rate is no longer statistically significant in the specification using the "no adjustment" variable as an instrument (column 6), the coefficient estimate is still negative despite its inherent positive bias.

It is possible that the above results are observed because in high benefit states and years, workers who are anticipating a longer spell of unemployment are more likely to take up their benefits. Hence, insured workers would have longer spells and uninsured workers would have shorter spells in these high benefits states and years. Therefore, I test the sensitivity of the reported results to this sort of selection bias. Instead of instrumenting for RR, I estimate the reduced form by replacing this variable with the maximum weekly benefit amount on the right hand side. Based on the results above, I also include both year and state dummy variables.

The first stage Probit for UI recipiency includes three measures of state/year UI disqualification rates to identify the model.[24] These variables are chosen because it has been argued that take-up rates of state UI programs are negatively correlated with the extent to which the state cracks down on alleged abuse (Englander and Director, 1986).

The results of the Probit are reported in Table 6. The disqualification rate due to misconduct has the negative, significant coefficient hypothesized, while the coefficients on the other two measures are statistically indistinguishable from zero. Surprisingly, the maximum weekly benefit amount also is estimated to have no effect on UI recipiency. This result contradicts the findings in Blank and Card (1989) although the discrepancy may again be due to the measure of UI status. Regarding the demographic variables, older, more educated, married men with fewer children are more likely to receive UI.

The results of the second stage estimation for both the insured and uninsured group are reported in the second and third columns in Table 6. They indicate that sample selection is not a problem here, particularly for the uninsured group.[25] Oddly, for UI recipients the coefficient on lambda, the inverse Mills ratio, indicates a negative selection into the insured group indicating that people with shorter durations "select in" to the UI system. However, the coefficient on the maximum weekly benefit amount is still not

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[24]The three measures are the percentage of workers disqualified for reasons of misconduct, refusal of suitable work, and failure to remain able and available or to actively search for work. These data were graciously provided by Julie Ho and Bruce Meyer.

The first stage Probit is estimated using a 10% random sample of the data while the coefficient estimates are used to correct the regression for the other 90%.

[25]This finding is not surprising since the vast majority of UI nonrecipients are ineligible for UI (see Blank and Card, 1989).

significantly different from zero. There is no evidence of selection problems for the uninsured group as the coefficient on lambda is insignificant.

The support found for the spillover hypothesis using the CPS is confirmed using the NLSY. In estimating the model with this sample, RR is interacted with birth cohort dummy variables to allow the effect to vary with age.[26] One might not expect very young workers (as young as age 14) who almost never receive UI, to be "as substitutable" with older, insured workers as the older workers in the sample. Hence the predicted effect for younger workers is smaller.[27]

Table 7 presents the estimation results for the uninsured sample. Without adding state or year effects, estimates obtained from OLS support the spillover hypothesis, that an increase in RR reduces spell durations, but controlling for endogeneity in RR by two stage least squares turns this result around. A Hausman test clearly indicates the need to instrument for RR in this case. However, adding state effects changes these findings dramatically. A negative, significant relationship is found between log duration and RR using OLS and a Hausman test strongly rejects inconsistency of the OLS results. F-tests indicate that adding year effects to control for the business cycle in this sample is superfluous given that the state/year

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[26]The three cohorts are defined to be those born in 1957-58, in 1959-61, and in 1962-64. Descriptive statistics for these cohorts are presented in Table 1.

[27]If the spillover effect is indeed smaller for younger workers, then interacting cohort dummy variables with RR will provide a stronger test of the model. This is because of the panel nature of the data. As I hypothesize, given the young age of these workers, the spillover effect should increase as workers in this sample age. However, because the data is in panel form, in each ensuing year the sample is one year older. Hence, without controlling for this interaction, year fixed effects will pick up some of the spillover effect.

employment growth rate is already included as an explanatory variable. Therefore, column (5) presents the most efficient, consistent estimates among the models estimated. The demographic effects are mostly consistent with those obtained in the CPS sample.[28]

Interpreting the coefficients in column (5), an increase in RR from 40% to 50% would lead to a reduction in the duration of an unemployment spell of between 10% and 13%, where the larger changes are for cohorts born earlier, as expected. Evaluated at the mean durations, this translates into roughly a one week reduction in unemployment spell length for all cohorts.

This effect can be compared to that found using the CPS data according to the relationship, $\beta_{NLSY} = \beta_{CPS}/E(F)$, derived in Appendix 2. From Table 2, E(F) = .315 for uninsured workers in the CPS and from column (8) in Table 3, $\beta_{CPS} = -0.545$. Hence the comparable estimated coefficient on RR in a regression on the log of completed spell lengths from the CPS data would be -1.730, implying an increase in the replacement rate from 40% to 50% would reduce the duration of a completed spell by 17% for uninsured workers. The effect is somewhat larger than those estimated from the NLSY, but is consistent with the fact that, on average, the NLSY sample is considerably younger than the general population. As discussed above, a larger effect is expected for prime-age versus younger workers because these workers are better substitutes for the insured unemployed.

Estimation results for insured workers are found in Table 8. For the most part, little information can be obtained from these results as estimated coefficients are unstable and extremely imprecise. These problems are

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[28]The only variable which changes sign is the number of children. This might represent a selection problem in that very young workers with more children may be more prone to experience longer spells of unemployment.

probably due to the small size of the insured sample. Given that the replacement rate measure is a state/year aggregate over seven years and 50 states, the insured sample of only 1,741 workers leaves very few workers in each state/year cell.

However, for the purposes of testing for spurious correlations, something can still be learned from these results. Once state effects are added to the model, the estimated effect of RR on log duration is consistently positive, in contrast to the findings from the uninsured sample.[29] Hence, there appears to be no evidence of a spurious correlation masquerading as support for the spillover hypothesis.

Given the wealth of information on each respondent's work history in the NLSY, the sensitivity of the estimated results to selection bias can be easily addressed.[30] To do this, I apply a definition of an uninsured spell that is independent of a worker's choice to take up benefits. Uninsured spells are chosen to be those encountered by new entrants, reentrants after a long absence (defined here as longer than 65 weeks), and those who have quit their jobs. Workers unemployed for these reasons will be almost uniformly uninsured given UI eligibility rules. "Insured" spells are defined as all spells originated by those who have been laid off, fired, or discharged. I then

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[29]Again, F-tests reject the need to include year fixed effects in the model. A Chow test indicates that the coefficients obtained for the insured sample are significantly different than those estimated for the uninsured sample.

[30]Using the NLSY data, I also attempted to control for selection bias by applying a Heckman correction as I did with the CPS data. However the variables included in the first stage Probit but excluded in the second stage selection corrected regression were very imprecisely estimated (t-statistics well below unity). Hence the power of the procedure to identify selection bias is extremely small and is not reported here.

reestimate the model for both insured and uninsured workers using this new definition of insurance status.

The results of this analysis are found in column 7 of Table 9. These results indicate that the earlier support for the spillover hypothesis is not caused by selection bias. Estimated coefficients on the replacement rate variables for "uninsured" spells are negative and significantly different from zero, but not significantly different from those presented in Table 7.

The remainder of table 9 tests the sensitivity of the NLSY results to the same alternative specifications as described above using the CPS data. The estimated effects for insured workers are extremely sensitive, as might have been expected from the earlier results. For uninsured workers, while an increase in the replacement rate still leads to a predicted decrease in spell durations in most of the specifications, many of the results are no longer statistically significant and the size of the effect is fairly unstable. Hence the strength of the support for the spillover hypothesis is not quite as strong in the NLSY data as was found in the CPS data. Again, this might have been anticipated given the age composition of the sample.

V. EFFECT ON THE AGGREGATE UNEMPLOYMENT RATE

Given the overall strength of the estimated spillover effect, it may be the case than any increase in unemployment experienced by the insured unemployed due to an increase in UI benefits may be completely counterbalanced by the decrease in unemployment among the uninsured. This section will examine the effect of a change in UI benefits on the aggregate unemployment rate incorporating possible spillover effects.

To do this, I apply a similar estimation strategy to the above using both aggregate unemployment rate data by state and year for the 1978-87 period (again, excluding 1985). The replacement rate variable is the same as described above while the other explanatory variables are computed as the state/year mean obtained from the relevant March CPS. I also use aggregate data to further test the spillover hypothesis by estimating the effect on the state/year insured and uninsured unemployment rates.[31] In all cases I apply two stage least squares, instrument the replacement rate with the maximum weekly UI benefit, and control for both state and year effects.[32]

The results of this analysis are reported in Table 10. The first three columns indicate results using aggregate data. The control variables in these specifications represent the means within each state and year. Columns (1) and (2) provide even more support for the spillover hypothesis. According to these results, a 10% increase in the replacement rate from, say 40% to 50%, will increase the insured unemployment rate by 1% and reduce the uninsured unemployment rate by 1.2%. Both of these effects are statistically significant. Column (3) presents the results for the aggregate unemployment rate. While the point estimate indicates a slight negative effect (the 10%

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[31]State UI agencies report both their annual insured unemployment rate (defined as the fraction of covered employees receiving UI) and the average weekly number of insured unemployed. I estimate the average weekly number of uninsured unemployed as the difference between the average weekly total number of unemployed and the number insured unemployed. The annual uninsured unemployment rate is then defined as the ratio of the average weekly number of uninsured unemployed to the number of workers in the labor force.

[32]This specification is chosen based on the results above using CPS data. Since the aggregate unemployment rate is derived from the CPS, I continue to use this specification. However, here I omit the employment growth rate as an explanatory variable. I do this because the unemployment rate is defined as U/(U + E) where U is the number unemployed and E is the number employed. Since the employment growth rate is also a function of E, including it as a control variable would mean placing the same variable on both the right and left hand sides of the equation, causing serious endogeneity bias. This form of bias is not present in the earlier analysis, where the sample is restricted to those experiencing some unemployment.

increase in RR would lead to a .4% reduction in aggregate unemployment), this effect is not different from zero at reasonable significance levels.

Column (4) of Table 10 reports the results of the analysis using micro level data. The dependent variable in this regression is the ratio of the number of weeks a worker spends unemployed to the number of weeks s/he spent in the labor force.[33] The regression is weighted by the product of the CPS sampling weight and the number of weeks the worker spent in the labor force. The results are quite similar to those using aggregate data in that a 10% increase in the replacement rate is predicted to reduce the aggregate unemployment rate by -.37%. This effect is significant at the 10% level, but not at the 5% level. Given the weak negative effect predicted from both sources of data, one can at least conclude that the aggregate unemployment rate will not increase in response to an increase in UI benefits.

V. SUMMARY AND CONCLUSIONS

Using data from both the CPS and the NLSY, this paper has presented evidence indicating that a spillover effect exists between insured and uninsured unemployed workers. The estimated effect is relatively large. Results from the CPS sample indicate that an increase in the replacement rate of UI benefits to preunemployment wages from 40% to 50% reduces weeks unemployed in the calendar year by over two and one half weeks. From the NLSY sample, the same change in the replacement rate is estimated to reduce completed unemployment spells by roughly one week.

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[33]The mean of this variable, weighted by the number of weeks in the labor force, is the retrospective unemployment rate. See Levine (1990) for more detail.

What do these results tell us about the effects of an increase in UI benefits on overall unemployment? While previous research has consistently estimated a positive effect, these studies typically predict the increase in unemployment among the insured and then multiply by the fraction insured to get the aggregate effect. The results of this paper indicate that this is inappropriate. Allowing for spillover, I show that there is certainly no positive effect on the aggregate unemployment rate in response to an increase in UI benefits.

This finding has substantial implications for public policy beyond the unemployment insurance system. It is possible that any social program targeting a certain segment of the population may have potentially unintended side effects for others who are substitutable with the target group. For instance, providing job search assistance or job training for unskilled mothers on welfare may improve this group's job finding prospects, but may make it more difficult for unskilled men or childless women to find a job. Research examining the extent of spillover in other social programs would certainly be desirable.

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Figure 1: Effect of an Increase in UI Benefits in a Labor Market where Insured and Uninsured Workers are Perfect Substitutes

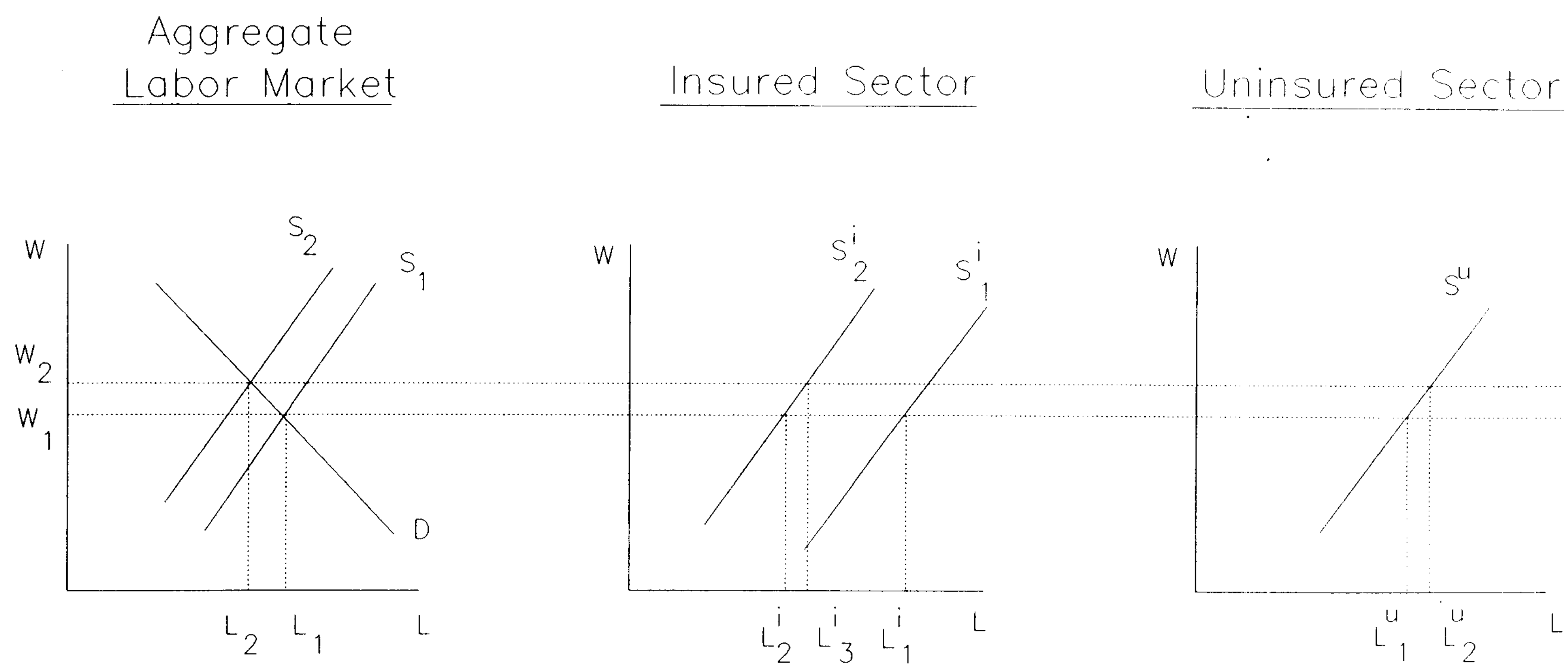

TABLE 1: EFFECT OF UNEMPLOYMENT INSURANCE ON UNEMPLOYMENT, BY REASON UNEMPLOYED - 1979 AND 1982[1]

	On Layoff	Lost Job	Quit Job	Left School	Wanted Temp. Work	Other	All Reasons
% Insured[2]	58.0	33.6	12.9	2.5	2.6	6.8	26.1

1979

	On Layoff	Lost Job	Quit Job	Left School	Wanted Temp. Work	Other	All Reasons
Intercept	-0.094 (0.355)	5.056 (0.553)	1.653 (0.356)	0.054 (0.176)	0.735 (0.326)	2.934 (0.539)	10.136 (1.001)
Replacement Ratio[3]	2.054 (0.732)	-4.902 (1.100)	-2.247 (0.725)	0.394 (0.385)	-0.134 (0.623)	-3.984 (1.039)	-8.594 (1.892)
age 16-19	0.332 (0.159)	0.490 (0.252)	1.076 (0.189)	1.579 (0.165)	4.816 (0.293)	2.484 (0.283)	10.810 (0.540)
age 20-29	0.471 (0.105)	0.991 (0.142)	0.913 (0.093)	0.244 (0.033)	0.030 (0.072)	0.771 (0.125)	3.421 (0.242)
age 30-39	0.117 (0.100)	0.064 (0.126)	0.445 (0.082)	0.043 (0.017)	-0.126 (0.062)	-0.023 (0.107)	0.051 (0.215)
age 40-49	-0.004 (0.104)	0.138 (0.137)	0.077 (0.073)	0.030 (0.012)	-0.157 (0.063)	0.125 (0.121)	0.195 (0.228)
Female	-0.410 (0.074)	-0.939 (0.108)	0.286 (0.079)	-0.066 (0.043)	0.162 (0.079)	0.805 (0.116)	-0.133 (0.229)
Married	0.015 (0.089)	-1.429 (0.134)	-0.383 (0.093)	-0.264 (0.035)	-0.292 (0.067)	-0.278 (0.116)	-2.598 (0.223)
Nonwhite	0.174 (0.122)	1.538 (0.207)	0.337 (0.132)	0.473 (0.095)	0.721 (0.140)	1.908 (0.206)	5.155 (0.359)
Average Weekly Wage	0.013 (0.013)	-0.080 (0.026)	-0.056 (0.017)	-0.025 (0.008)	-0.095 (0.028)	-0.198 (0.057)	-0.439 (0.123)
% of workforce	0.93	1.83	0.88	0.27	0.83	1.51	6.20

1982

	On Layoff	Lost Job	Quit Job	Left School	Wanted Temp. Work	Other	All Reasons
Intercept	0.795	8.127	1.107	0.586	1.047	2.683	14.292
	(0.511)	(0.668)	(0.326)	(0.215)	(0.336)	(0.447)	(1.025)
Replacement Ratio	2.680	-5.645	-0.560	-0.310	-0.655	-2.310	-7.037
	(1.063)	(1.352)	(0.683)	(0.454)	(0.697)	(0.915)	(2.106)
age 16-19	-0.101	0.074	1.255	2.551	5.545	3.266	12.606
	(0.212)	(0.333)	(0.205)	(0.224)	(0.332)	(0.321)	(0.616)
age 20-29	0.920	2.053	0.730	0.310	0.343	0.907	5.242
	(0.149)	(0.194)	(0.085)	(0.039)	(0.074)	(0.124)	(0.286)
age 30-39	0.480	0.648	0.417	0.045	0.133	0.423	2.152
	(0.141)	(0.173)	(0.075)	(0.021)	(0.063)	(0.107)	(0.816)
age 40-49	0.216	0.539	0.265	0.026	0.046	0.252	1.356
	(0.149)	(0.188)	(0.078)	(0.150)	(0.063)	(0.113)	(0.276)
Female	-0.110	-2.493	-0.074	-0.236	0.132	0.558	-3.153
	(0.100)	(0.147)	(0.075)	(0.050)	(0.076)	(0.101)	(0.224)
Married	-0.222	-2.048	-0.486	-0.261	-0.149	-0.339	-3.472
	(0.122)	(0.169)	(0.082)	(0.038)	(0.068)	(0.112)	(0.249)
Nonwhite	0.225	2.744	0.447	0.534	0.773	1.957	6.657
	(0.159)	(0.213)	(0.106)	(0.069)	(0.109)	(0.146)	(0.328)
Average Weekly Wage	0.031	-0.316	-0.072	-0.067	-0.171	-0.349	-0.934
	(0.019)	(0.032)	(0.013)	(0.007)	(0.011)	(0.019)	(0.047)
% of workforce	1.92	3.54	0.83	0.36	0.91	1.63	9.10

[1]The dependent variable in these regressions = 1 if the worker is unemployed for the specified reason, and 0 otherwise. The sample consists of all workers in the March CPS in the labor market (71,610 workers in 1979 and 74,555 in 1982). Coefficients and standard errors have been multiplied by 100 and standard errors are in parentheses.

[2]The percent insured by reason unemployed is computed using data from March CPS tapes, matched between one year and the next, from 1978/79 to 1987/88. (see section III for a description of this data). It is computed as the fraction of workers unemployed in March of year t who report in year t+1 that in the previous year they received some UI benefits.

[3]This variable is the average ratio of estimated UI benefits to preunemployment wages in each state for a random sample of insured unemployed workers. Appendix 2 describes the creation of this variable.

TABLE 2:
SAMPLE MEANS AND STANDARD DEVIATIONS*

	CPS Sample		NLSY Sample	
	UI	No UI	UI	No UI
Duration (Cohort 1)			16.082 (16.110)	8.673 (14.768)
Duration (Cohort 2)			14.022 (15.373)	9.147 (14.970)
Duration (Cohort 3)			15.613 (18.730)	9.706 (14.826)
Fraction of the year unemployed	0.353 (0.237)	0.315 (0.277)		
RR	0.448 (0.052)	0.446 (0.049)	0.443 (0.046)	0.448 (0.048)
Age	35.043 (12.010)	28.957 (12.373)	21.947 (2.161)	19.847 (2.517)
Female	0.341 (0.474)	0.491 (0.500)	0.346 (0.476)	0.499 (0.500)
Nonwhite	0.120 (0.325)	0.182 (0.386)	0.173 (0.379)	0.238 (0.426)
Educ	11.685 (2.656)	11.744 (2.723)	11.596 (1.534)	11.260 (1.971)
In School			0.092 (0.290)	0.493 (0.500)
Married	0.643 (0.479)	0.414 (0.493)	0.314 (0.464)	0.123 (0.328)
# of Kids	0.999 (1.229)	1.073 (1.309)	0.364 (0.721)	0.207 (0.582)
growth	0.013 (0.031)	0.019 (0.031)	0.006 (0.031)	0.018 (0.032)
N	35,888	88,035	1,741	13,457

*Variable definitions:

Fracyrue = fraction of the year unemployed (CPS sample only)
Duration = completed spell duration (NLSY sample only); by cohort
RR = state/year replacement rate of UI benefits to preunemployment wages
Age = age in years
Female = 1 if female, 0 otherwise
Nonwhite = 1 if nonwhite, 0 otherwise
Educ = years of education
In School = Still enrolled in school at survey date in year spell began (NLSY sample only)
Married = married with spouse present or absent
# of kids = number of dependents under the age of 18
Growth = state\year employment growth rate
Current spell length = length of the in progress spell at the survey date
N = sample size
Y19xx = 1 if year = xx, 0 otherwise
state DV's = state dummy variables
Maximum WBA = maximum weekly benefit amount in state/year

	Number in Cohort	
	UI	No UI
Cohort 1 = born in 1957-58 (NLSY sample only)	306	939
Cohort 2 = born in 1959-61 (NLSY sample only)	851	4,465
Cohort 3 = born in 1962-64 (NLSY sample only)	584	8,053

TABLE 3:
MODELS OF FRACTION OF THE YEAR UNEMPLOYED - CPS SAMPLE, UI NONRECIPIENTS*
(Standard Errors in Parentheses)

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Intercept	.477	.441	.445	.445	.649	0.717	.425	.639
	(.010)	(.016)	(.010)	(.016)	(.019)	(.041)	(.024)	(.037)
RR	-.201	-.120	-.116	-.116	-.577	-.740	-.027	-.545
	(.019)	(.034)	(.019)	(.034)	(.044)	(.098)	(.057)	(.088)
Nonwhite	.076	.076	.076	.076	.078	.078	.078	.078
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Female	-.063	-.063	-.062	-.062	-.063	-.063	-.062	-.062
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
Age	.003	.003	.003	.003	.003	.003	.003	.003
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Educ	-.009	-.009	-.009	-.009	-.009	-.009	-.009	-.009
	(.0003)	(.0003)	(.0003)	(.0003)	(.0003)	(.0003)	(.0003)	(.0003)
Married	-.029	-.029	-.029	-.029	-.027	-.027	-.027	-.027
	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)	(.002)
# Kids	-.002	-.002	-.0014	-.0014	-.002	-.002	-.0014	-.0014
	(.001)	(.001)	(.0007)	(.0007)	(.001)	(.001)	(.0007)	(.0007)
growth	-.976	-.960	-.879	-.879	-.904	-.912	-.573	-.693
	(.029)	(.030)	(.040)	(.042)	(.032)	(.032)	(.052)	(.054)
Year DV's	NO	NO	YES	YES	NO	NO	YES	YES
State DV's	NO	NO	NO	NO	YES	YES	YES	YES
Adj. R^2	.063	.062	.069	.069	.070	.068	.075	.075

*See table 1 for variable definitions. In the two stage least squares models, the replacement ratio is instrumented by the maximum weekly benefit amount in a worker's state and year.

TABLE 4:
MODELS OF FRACTION OF THE YEAR UNEMPLOYED - CPS SAMPLE, UI RECIPIENTS*
(Standard Errors in Parentheses)

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Intercept	.488	.324	.440	.331	.656	.405	.365	.434
	(.013)	(.022)	(.014)	(.022)	(.027)	(.068)	(.035)	(.059)
RR	-.226	.140	-.137	.110	-.629	-.027	.051	-.118
	(.024)	(.045)	(.025)	(.044)	(.061)	(.163)	(.082)	(.142)
Nonwhite	.036	.038	.036	.038	.039	.039	.039	.039
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Female	-.012	-.012	-.011	-.011	-.010	-.010	-.009	-.009
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
Age	.001	.001	.001	.001	.001	.001	.001	.001
	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)	(.0001)
Educ	-.004	-.004	-.004	-.004	-.004	-.004	-.004	-.004
	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)	(.0005)
Married	-.019	-.019	-.019	-.019	-.018	-.018	-.018	-.018
	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)	(.003)
# of Kids	.001	.001	.001	.001	.001	.001	.001	.001
	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)	(.001)
growth	-.883	-.819	-.802	-.722	-.918	-.896	-.760	-.784
	(.040)	(.041)	(.056)	(.057)	(.044)	(.045)	(.071)	(.073)
Year DV's	NO	NO	YES	YES	NO	NO	YES	YES
State DV's	NO	NO	NO	NO	YES	YES	YES	YES
Adj. R^2	.023	.021	.033	.032	.033	.030	.043	.043

*See table 1 for variable definitions. In the two stage least squares models, the replacement ratio is instrumented by the maximum weekly benefit amount in a worker's state and year.

TABLE 5: COEFFICIENTS ON THE REPLACEMENT RATE IN ALTERNATIVE SPECIFICATIONS USING CPS DATA
(Standard Errors in Parentheses)

UI Nonrecipients

	(1)	(2)	(3)	(4)	(5)	(6)
RR	-0.042	-0.415	-0.344	-0.564	-0.621	-0.239
	(0.045)	(0.067)	(0.084)	(0.101)	(0.115)	(0.216)

UI Recipients

	(1)	(2)	(3)	(4)	(5)	(6)
RR	0.051	-0.090	0.034	-0.059	-0.149	-0.026
	(0.064)	(0.109)	(0.139)	(0.166)	(0.179)	(0.314)

*All specifications include state and year fixed effects and are estimated using two stage least squares, except where noted otherwise. Alternative models are as follows:

(1): uses ratio of average WBA to average weekly wage as measure of RR (OLS)
(2): uses ratio of average WBA to average weekly wage as measure of RR (2SLS)
(3): excludes the state/year employment growth rate as a control variable
(4): excludes years 1982-1984 (eliminates recessionary years)
(5): excludes odd numbered years (2nd difference in 2nd difference estimator)
(6): uses the variable "noadjust" (equals 1 if state did not increase its maximum benefit in year and zero otherwise) as instrument (2SLS)

TABLE 6:
CORRECTION FOR SAMPLE SELECTION BIAS
IN REDUCED FORM FRACTION OF THE YEAR UNEMPLOYED MODELS[1,2]
(Standard Errors in Parentheses)

	(1) Probit for UI Recipiency	(2) UI Recipients: Corrected for Selection Bias	(3) UI Nonrecipients: Corrected for Selection Bias
Intercept	-1.190 (0.242)	0.598 (0.071)	0.491 (0.017)
Maximum WBA	-0.0001 (0.001)	-0.00016 (0.00014)	-0.0005 (0.00009)
Nonwhite	-0.170 (0.379)	0.053 (0.006)	0.080 (0.003)
Female	-0.396 (0.026)	0.021 (0.011)	-0.059 (0.004)
Age	0.018 (0.001)	-0.0005 (0.0005)	0.002 (0.0002)
Education	0.009 (0.005)	-0.005 (0.001)	-0.009 (0.0004)
Married	0.415 (0.028)	-0.052 (0.012)	-0.030 (0.005)
Kids	-0.024 (0.011)	0.002 (0.001)	-0.0013 (0.0008)
Growth	-2.448 (0.780)	-0.533 (0.112)	-0.562 (0.063)
Lambda		-0.112 (0.037)	0.016 (0.022)
% Disqualified for Misconduct	-0.005 (0.002)		
% Disqualified for AAA issues[3]	0.001 (0.007)		
% Disqualified for Refusal of Suitable Work	-0.038 (0.086)		
-2*(log likelihood)	1553.69		
Adj. R^2		0.042	0.075

[1]All models reported include state and year dummy variables. Variable definitions are reported in table 1. Standard errors have been corrected to account for heteroskedasticity, but not for the fact that the lambda entered in the second state equation is estimated.

[2]To convert the reduced form estimates into structural estimates comparable to tables 1 and 2, note that an OLS regression of the replacement rate on the maximum weekly benefit amount gives:

$$RR = 0.318 + \underset{(0.000067)}{0.0087} * Maxwba.$$

Hence, the coefficients on the replacement rate in the structural model are:

	UI Recipients	UI Nonrecipients
RR	-0.184	-0.575
	(0.214)	(0.113)

where the standard errors are derived by applying the delta method assuming zero covariance between the coefficients on maxwba in the reduced form regressions and in the replacement rate regression.

[3]This variable indicates disqualifications for failure to remain able and available or to actively search for work.

TABLE 7:
LOG DURATION MODELS - NLSY DATA, UI NONRECIPIENTS
(Standard Errors in Parentheses)

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Int.	2.063	1.149	2.535	1.883	2.198	2.100	2.508	2.444
	(.151)	(.200)	(.235)	(.262)	(.304)	(.717)	(.365)	(.591)
RR*	-.450	1.239	-.404	1.088	-1.277	-1.126	-.776	-.728
Cohort1	(.213)	(.326)	(.235)	(.344)	(.546)	(1.371)	(.701)	(1.325)
RR*	-.345	1.392	-.348	1.156	-1.157	-.982	-.739	-.665
Cohort2	(.200)	(.319)	(.210)	(.324)	(.552)	(1.396)	(.693)	(1.319)
RR*	-.246	1.537	-.325	1.373	-1.024	-.843	-.732	-.650
Cohort3	(.204)	(.327)	(.207)	(.320)	(.567)	(1.434)	(.692)	(1.314)
Nonwhite	.329	.349	.340	.356	.359	.359	.360	.360
	(.023)	(.023)	(.023)	(.023)	(.024)	(.024)	(.024)	(.024)
Female	-.073	-.075	-.072	-.074	-.073	-.073	-.073	-.073
	(.019)	(.019)	(.019)	(.019)	(.019)	(.019)	(.019)	(.019)
Age	.022	.029	.010	.009	.025	.027	.007	.008
	(.006)	(.006)	(.010)	(.010)	(.007)	(.008)	(.010)	(.010)
Educ	-.061	-.063	-.060	-.062	-.060	-.060	-.059	-.059
	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)	(.006)
In	-.156	-.158	-.159	-.159	-.141	-.141	-.142	-.142
School	(.023)	(.023)	(.023)	(.023)	(.023)	(.022)	(.023)	(.023)
Married	-.139	-.139	-.134	-.126	-.126	-.126	-.124	-.124
	(.033)	(.033)	(.033)	(.033)	(.033)	(.033)	(.033)	(.033)
# Kids	.013	.009	.010	.007	.009	.009	.010	.010
	(.020)	(.020)	(.020)	(.020)	(.020)	(.020)	(.020)	(.020)
growth	-2.828	-2.684	-4.564	-4.256	-1.308	-1.301	-1.581	-1.568
	(.299)	(.301)	(.429)	(.433)	(.355)	(.355)	(.721)	(.752)
Year DV's	NO	NO	YES	YES	NO	NO	YES	YES
State DV's	NO	NO	NO	NO	YES	YES	YES	YES
Adj. R^2	.044	.045	.046	.047	.065	.065	.065	.065

*See table 1 for variable definitions. In the two stage least squares models, the replacement ratio is instrumented by the maximum weekly benefit amount in a worker's state and year.

TABLE 8:
LOG DURATION MODELS - NLSY DATA, UI RECIPIENTS
(Standard Errors in Parentheses)

	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS	(7) OLS	(8) 2SLS
Int.	1.978	2.294	1.215	1.332	0.808	-0.296	1.018	-0.961
	(.467)	(.602)	(.744)	(.828)	(.920)	(2.917)	(1.146)	(2.383)
RR*	-.202	-.824	-.677	-1.063	2.526	4.679	1.196	6.244
Cohort1	(.579)	(.924)	(.625)	(.924)	(1.604)	(5.584)	(2.216)	(5.794)
RR*	-.432	-1.064	-.732	-1.112	2.207	4.401	.920	5.950
Cohort2	(.573)	(.931)	(.591)	(.901)	(1.636)	(5.710)	(2.207)	(5.778)
RR*	-.205	-.854	-.235	-.602	2.544	4.797	1.328	6.324
Cohort3	(.595)	(.965)	(.609)	(.922)	(1.683)	(5.866)	(2.201)	(5.730)
Nonwhite	.350	.341	.358	.352	.315	.314	.317	.313
	(.071)	(.072)	(.072)	(.073)	(.075)	(.075)	(.075)	(.075)
Female	-.022	-.020	-.013	-.012	-.019	-.018	-.017	-.019
	(.055)	(.055)	(.055)	(.055)	(.056)	(.056)	(.056)	(.056)
Age	.028	.026	.066	.068	.028	.037	.038	.034
	(.016)	(.016)	(.028)	(.030)	(.018)	(.029)	(.029)	(.030)
Educ	-.026	-.025	-.025	-.025	-.031	-.030	-.030	-.031
	(.018)	(.018)	(.018)	(.018)	(.018)	(.018)	(.018)	(.018)
In	-.001	-.008	-.009	-.013	-.015	-.015	-.020	-.015
School	(.093)	(.093)	(.092)	(.093)	(.092)	(.092)	(.092)	(.093)
Married	-.087	-.090	-.079	-.080	-.029	-.028	-.026	-.026
	(.064)	(.064)	(.064)	(.064)	(.066)	(.066)	(.066)	(.066)
# Kids	.058	.058	.051	.050	.026	.026	.023	.026
	(.041)	(.041)	(.041)	(.041)	(.042)	(.042)	(.042)	(.042)
growth	-5.336	-5.312	-5.277	-5.270	-4.725	-4.648	-3.890	-2.696
	(.848)	(.849)	(1.342)	(1.343)	(.952)	(.972)	(2.349)	(2.672)
Year DV's	NO	NO	YES	YES	NO	NO	YES	YES
State DV's	NO	NO	NO	NO	YES	YES	YES	YES
Adj. R^2	.035	.035	.036	.036	.071	.070	.068	.069

*See table 1 for variable definitions. In the two stage least squares models, the replacement ratio is instrumented by the maximum weekly benefit amount in a worker's state and year.

TABLE 9: COEFFICIENTS ON THE REPLACEMENT RATE IN ALTERNATIVE SPECIFICATIONS USING NLSY DATA
(Standard Errors in Parentheses)

NLSY DATA - UI Nonrecipients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RR*	-0.287	-0.852	-1.326	-0.599	-1.205	-2.580	-2.219
Cohort1	(0.544)	(0.854)	(0.546)	(0.956)	(0.854)	(3.018)	(0.736)
RR*	-0.103	-0.650	-1.188	-0.632	-1.124	-2.789	-2.066
Cohort2	(0.536)	(0.850)	(0.552)	(0.955)	(0.856)	(3.093)	(0.743)
RR*	0.093	-0.449	-1.018	-0.548	-0.945	-2.838	-1.851
Cohort3	(0.537)	(0.850)	(0.567)	(0.962)	(0.867)	(3.245)	(0.765)

NLSY DATA - UI Recipients

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
RR*	1.393	2.715	3.000	-2.879	-0.408	-3.182	1.168
Cohort1	(1.761)	(3.053)	(1.612)	(3.406)	(2.677)	(6.855)	(1.065)
RR*	0.908	2.253	2.717	-3.039	-0.891	-3.019	1.065
Cohort2	(1.756)	(3.052)	(1.644)	(3.437)	(2.707)	(7.120)	(0.748)
RR*	1.239	2.584	3.055	-2.817	-0.429	-2.817	1.181
Cohort3	(1.761)	(3.056)	(1.691)	(3.509)	(2.750)	(7.530)	(0.768)

*All specifications include state fixed effects and are estimated using OLS, except where noted otherwise. Alternative models are as follows (models 1-6 are identical to those in Table 5):

(1): uses ratio of average WBA to average weekly wage as measure of RR (OLS)
(2): uses ratio of average WBA to average weekly wage as measure of RR (2SLS)
(3): excludes the state/year employment growth rate as a control variable
(4): excludes years 1982-1984 (eliminates recessionary years)
(5): excludes odd numbered years (2nd difference in 2nd difference estimator)
(6): uses the variable "noadjust" (equals 1 if state did not increase its maximum benefit in year and zero otherwise) as instrument (2SLS)
(7): uses an exogenous measure of UI recipiency. The nonrecipient sample is defined to be those who were not employed in the previous 65 weeks or quit their last job (these workers are almost uniformly ineligible for benefits). The recipient sample is defined as all others.

TABLE 10: EFFECT OF UNEMPLOYMENT INSURANCE ON UNEMPLOYMENT RATES[1]
(Standard Errors in Parentheses)

	Aggregate Data			
	Insured Unemployment Rate[2]	Uninsured Unemployment Rate[3]	Overall Unemployment Rate	Micro Data From CPS[4]
Intercept	-5.369	-2.620	-6.807	24.457
	(5.344)	(8.870)	(11.063)	(0.871)
RR	10.334	-11.904	-4.223	-3.690
	(2.707)	(4.451)	(5.552)	(2.111)
Nonwhite	-1.435	-7.164	-7.881	4.459
	(2.290)	(3.812)	(4.754)	(0.067)
Female	9.761	14.417	20.651	-1.312
	(3.361)	(5.612)	(7.000)	(0.043)
Age	0.067	-0.197	-0.146	-0.147
	(0.075)	(0.124)	(0.155)	(0.002)
Educ	-0.285	0.799	0.651	-0.856
	(0.317)	(0.527)	(0.658)	(0.008)
Married	0.700	4.382	5.049	-0.3130
	(2.173)	(3.586)	(4.473)	(0.049)
# kids	0.321	0.602	0.867	0.218
	(0.708)	(1.165)	(1.452)	(0.020)
Adj. R^2	0.807	0.674	0.712	0.057

[1]All specifications include both state and year effects and are estimated using two stage least squares, where the replacement ratio is instrumented by the maximum weekly benefit amount in the state and year. Aggregate data were obtained from annual editions of The Statistical Abstract of the United States.

[2]The insured unemployment rate is the fraction of workers in covered employment receiving UI benefits, multiplied by 100. In estimation, I weight the state/year observations by the number of workers in covered employment.

[3]The uninsured unemployment rate is the fraction of the labor force that is unemployed, but not receiving UI benefits, multiplied by 100. The number of unemployed nonrecipients is obtained by subtracting the number of UI recipients from the total number of unemployed. In estimation, I weight the state/year observations by the size of the labor force.

[4]The dependent variable is the number of weeks looking for work in the calendar year, where full-year employees are coded with zero weeks looking. I use the product of the CPS sampling weight and the number of weeks in the labor force in the calender year to weight the observations in estimation. Coefficients and standard errors have been multiplied by 100.

APPENDIX 1: A FORMAL MODEL OF THE SPILLOVER EFFECT

In the past, models of search have largely ignored the existence of the uninsured pool of unemployed and have derived the result that an increase in the weekly benefit leads to a decrease in the intensity of search and an increase in the reservation wage, leading to a predicted increase in the duration of unemployment (c.f. Lippman and McCall, 1976). The search-theoretic model I present incorporates both groups of unemployed workers. I consider the effect of an increase in the weekly UI benefit on the insured group <u>and</u> the potential for spillover effects to the uninsured group.

The interaction between the two groups of workers is derived from the employer-side of the market. An employer with a job opening is assumed indifferent between making a job offer to any worker, regardless of their insured status (for simplicity of exposition, each firm is assumed to be comprised of one position). Hence, if more than one worker applies for a vacant position within the relevant decision-making period (say, one day), then the employer randomly makes an offer to one of them at the end of the period. The number of applicants, n, a firm gets is assumed to be distributed Poisson with applicant arrival rate, δ:

$$P(x = n) = \delta^{n} e^{-\delta}/n!$$

where: $\delta = n_i\delta_i + n_u\delta_u$
$n = n_i + n_u$
n_i = number of insured unemployed
n_u = number of uninsured unemployed
$\delta_i = \delta_i(s_i)$ = applicant arrival rate of insured unemployed; $\partial\delta_i/\partial s_i > 0$
$\delta_u = \delta_u(s_u)$ = applicant arrival rate of uninsured unemployed; $\partial\delta_u/\partial s_u > 0$
s_i = search intensity of insured unemployed
s_u = search intensity of uninsured unemployed

While search intensity is assumed to be continuous, an unemployed worker is assumed to file at most one application per period. Furthermore, an increase in search intensity is assumed to affect δ only in the current period.

Given these assumptions, an increase in search intensity by the insured group holding the uninsured group's intensity constant will have the following consequences. First, the number of new applications filed, equal to the number of job vacancies, n_v, multiplied by the change in the number of applicants per vacancy:

$$n_v * \frac{\partial \delta}{\partial s_i} = n_v * n_i * \frac{\partial \delta_i}{\partial s_i} \tag{1}$$

will increase. In addition, new job offers will be made that otherwise would not have been made since some firms with vacancies will now receive applications. The number of new job offers is represented by the number of vacancies multiplied by the reduction in the probability that no applicants arrive at a given firm:

$$-n_v * \frac{\partial [P(x=0)]}{\partial s_i} = n_v * e^{-\delta} * n_i * \frac{\partial \delta_i}{\partial s_i}. \tag{2}$$

However, at least some of the new applicants will go to firms that already have received applications. Subtracting (1) from (2), it is clear given $\delta > 0$ that the number of new applicants is greater than the number of new job offers:

$$(1) - (2) = n_v * n_i * \frac{\partial \delta_i}{\partial s_i} * (1 - e^{-\delta}) > 0.$$

Changes in application rates can be used to translate changes in one group's search intensity into changes in the arrival rate of job offers to workers in both groups. The probability that a particular worker receives an offer from a certain firm is given by the joint probability that an offer is made by the firm (i.e. receives at least one applicant) and that the worker receives the offer. Define δ_0 as the applicant arrival rate just prior to the change in the UI system. Then from above, a given firm makes an offer with probability $[1 - \exp(-\delta_0)]$. Since employers who make offers make them to a randomly selected applicant, the probability that an individual worker receives an offer from a firm is the expected number of applications filed by that worker at that firm during the period divided by the expected number of total applicants. Evaluating the joint probability at a given firm and multiplying by the number of firms provides the arrival rate of job offers for uninsured workers, λ_u, and insured workers, λ_i:

$$\lambda_u = \frac{n_v (1 - e^{\delta_0}) \delta_u}{n_i \delta_i + n_u \delta_u} \quad ; \qquad \lambda_i = \frac{n_v (1 - e^{\delta_0}) \delta_i}{n_i \delta_i + n_u \delta_u}.$$

Initially holding constant the number of firms that give offers, consider first the effects of an increase in the search intensity of the insured unemployed. As their search intensity increases, the offer arrival rate of an uninsured worker decreases:

$$\frac{\partial \lambda_u^0}{\partial s_i} = -\frac{n_v(1 - e^{\delta_0})\,\delta_u n_i}{(n_i\delta_i + n_u\delta_u)^2}\frac{\partial \delta_i}{\partial s_i} < 0,$$

and the offer arrival rate of an insured worker increases:

$$\frac{\partial \lambda_i^0}{\partial s_i} = \frac{n_v(1 - e^{\delta_0})\,\delta_u n_u}{(n_i\delta_i + n_u\delta_u)^2}\frac{\partial \delta_u}{\partial s_u} < 0.$$

Note that $n_i(\partial\lambda_i^0/\partial s_i) + n_u(\partial\lambda_u^0/\partial s_i) = 0$ as the number of offers made is held constant.

But the number of offers made does not stay constant. It increases and all the new offers go to insured workers. Hence the offer arrival rate for insured workers goes up by the number of new offers made per insured worker in addition to the change indicated above:

$$\frac{\partial \lambda_i}{\partial s_i} = \frac{\partial \lambda_i^0}{\partial s_i} + \frac{n_v}{n_i}\frac{\partial[p(x=0)]}{\partial s_i} = \frac{\partial \lambda_i^0}{\partial s_i} + n_v e^{-\delta}\frac{\partial \delta_i}{\partial s_i} > 0.$$

Since uninsured workers get no new offers, $\partial\lambda_u/\partial s_i = \partial\lambda_u^0/\partial s_i < 0$.

Differentiating again with respect to s_i indicates that the own second derivative, $\partial^2\lambda_i/\partial s_i^2$, is negative. A necessary condition for the cross second derivative of λ_i, $\partial^2\lambda_i/\partial s_i\partial s_u$, to be negative is the share of the total number of applications filed by the uninsured group, $n_u\delta_u/(n_i\delta_i + n_u\delta_u)$, must be greater than 1/2. In other words, it is easier for insured workers to displace the uninsured group if the uninsured group is small.[1] The analysis

[1]An example will help illustrate why this derivative is not necessarily negative. First, suppose the λ is large enough so that all firms receive applicants prior to any change in the system. Next, suppose the two groups search equally as hard, but the insured group is much larger. Finally, suppose both groups increase their search intensities by the same amount. As a result, the relative number of new applications filed by the insured group would be greater and, on average, insured workers would have a greater probability of receiving an offer.

of an increase in uninsured worker's search intensity is completely symmetric: $\partial\lambda_i/\partial s_u < 0$, $\partial\lambda_u/\partial s_u > 0$, $\partial^2\lambda_u/\partial s_u^2 < 0$, and $\partial^2\lambda_u/\partial s_i \partial s_u < 0$ if $n_i\delta_i/(n_i\delta_i + n_u\delta_u) < 1/2$.

In summary, as the insured group's search intensity increases, they receive additional job offers that would otherwise not have been made. However, some of the new applications go to firms that already had received some applicants prior to the change. Some of these new applicants displace some of the old applicants and, on average, more of the insured applicants receive offers. The same reasoning holds in reverse for an increase in the search intensity of the uninsured group holding the insured group's intensity constant.

These results are applied to the worker-side of the model to derive the relevant comparative static results.[2] Workers are assumed to be infinitely-lived, risk-neutral maximizers of the expected value of income. By assumption, once a new job is obtained a worker never quits, although there is a constant probability that s/he will be laid-off by the firm. If a worker is laid-off, then s/he is automatically eligible for UI, which is assumed to have an unlimited duration. Unemployed individuals are assumed to choose their search intensity and reservation wage to maximize the following value function:[3]

$$V_k = \frac{b_k - c(s_k)}{1 + r} + \left[\frac{q_k\lambda_k}{1 + r}\right]U_k + \left[\frac{1 - q_k\lambda_k}{1 + r}\right]V_k \tag{3}$$

where: k = i for an insured worker or u for an uninsured worker

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[2]The worker side of the model is a combination of the models in Mortensen (1986) and Burdett (1978).

[3]Note that due to the assumptions of infinitely-lived workers and a stationary environment, time subscripts are not needed on V_k and U_k.

$V_{t,k}$ = expected value of being unemployed for workers in group k given optimal behavior from week t forward
b_k = weekly unemployment compensation (= 0 if k = u)
s_k = search intensity for workers in group k (ranging from 0 to 1; 1 = full, 0 = none)
$\lambda_k = \lambda_k(s_i, s_u)$ = offer arrival rate for workers in group k
$c(s_k)$ = cost of search function, $c' > 0$, $c'' > 0$
r = rate of time preference
q_k = probability that new job offer is acceptable to workers in group k

$$= \int_{w_k^r}^{\bar{w}} f_k(w)\,dw$$

$\bar{w}$ = highest possible job value
w^r_k = reservation job value for workers in group k
$f_k(w)$ = p.d.f. of job values for workers in group k
U_k = expected return from working conditional on an acceptable wage offer

$$= \frac{w_k^e + \alpha V_i + (1 - \alpha) U_k}{1 + r}$$

α = separation rate
w^e_k = expected wage per period conditional on an acceptable wage offer

$$= \frac{1}{q_k} \int_{w_k^r}^{\bar{w}} w\, f(w)\,dw$$

Relevant derivatives of the offer arrival rate function were derived above.

The first term on the right hand side of equation (3) represents the value of unemployment insurance paid less the cost of search in the current week. The second term consists of the product of the probability that a job offer arrives and is accepted (starting in week t+1) and the expected value of accepting a new job conditional on receiving an acceptable wage offer. This expected value, U_k, is the sum of the conditional mean wage in the current period, the expected value of lifetime income if laid-off and the expected value if still employed in the subsequent period. Hence, the second term of (3) represents the discounted expected value of accepting a new job. Similarly, the third term is the product of the probability that a job is not obtained in the current week and the value of being unemployed starting in week t+1. This term is interpreted as the discounted expected value of remaining unemployed.

The optimal values of w^r and s are obtained from the following first order conditions:

$$w_i^r = rV_i \tag{4}$$

$$c'(s_i) = q_i \frac{\partial \lambda_i}{\partial s_i}(U_i - V_i) \tag{5}$$

$$\frac{w_u^r + \alpha V_i}{\alpha + r} = V_u \tag{6}$$

$$c'(s_u) = q_u \frac{\partial \lambda_i}{\partial s_u}(U_u - V_u) \tag{7}$$

The market will be in steady state equilibrium when the number of separations equals the number of new hires:

$$\alpha n_e = q_i \lambda_i n_i + q_u \lambda_u n_u$$

where n_e equals employment and q_k and λ_k are measured at the optimal reservation wage and search intensity.

To provide some intuition behind the optimal reservation wage conditions, define $U(w^r{}_k)$ as the value of holding a job at the reservation wage:

$$U(w_k^r) = \frac{w_k^r + \alpha V_i + (1 - \alpha) U(w_k^r)}{1 + r}$$

$$\Rightarrow \; U(w_k^r) = \frac{w_k^r + \alpha V_i}{\alpha + r}$$

Hence, the first order conditions imply that at the optimal reservation wage, the value of taking a job at that wage is just equal to the value of remaining unemployed. Similarly, the optimal levels of search intensity are such that marginal benefits of additional search equal the marginal costs.

To consider the effects of an increase in the weekly unemployment insurance benefit on the optimal levels of $w^r{}_i$, $w^r{}_u$, s_i, and s_u, consider the total derivatives of equations (4) - (7):

$$\left[1 + \frac{q_i \lambda_i}{\alpha + r}\right] dw_i^r = q_i (U_i - V_i) \frac{\partial \lambda_i}{\partial s_u} ds_u + db \tag{4'}$$

$$\left[c_i'' - q_i \frac{\partial^2 \lambda_i}{\partial s_i^2} (U_i - V_i)\right] ds_i =$$

$$q_i \frac{\partial^2 \lambda_i}{\partial s_i \partial s_u} (U_i - V_i) ds_u - \frac{q_i}{\alpha + r} \frac{\partial \lambda_i}{\partial s_i} dw_i^r \tag{5'}$$

$$\left[\frac{r + q_u \lambda_u}{\alpha + r}\right] dw_u^r = q_u (U_u - V_u) \frac{\partial \lambda_u}{\partial s_i} ds_i - \frac{\alpha}{\alpha + r} dw_i^r \tag{6'}$$

$$\left[c_u'' - q_u \frac{\partial^2 \lambda_u}{\partial s_u^2}(U_u - V_u)\right] ds_u =$$

$$q_u \frac{\partial^2 \lambda_u}{\partial s_i \partial s_u}(U_u - V_u) ds_i - \frac{q_u}{\alpha + r}\frac{\partial \lambda_u}{\partial s_u} dw_u^r \tag{7'}$$

Because the insured worker can always choose the identical values of w^r_i and s_i after an increase in the weekly benefit as before, it must be the case that his/her value of remaining unemployed increases leading to an increase in his/her reservation wage ($dV_i/db > 0$, which implies $dw^r_i/db > 0$). Using this result and (4'), it must be true that the search intensity of the uninsured unemployed also increases, $ds_u/db > 0$. Applying these results to (5'), it is evident that insured workers search less hard for a job ($ds_i/db < 0$) if $\partial^2\lambda_i/\partial s_i \partial s_u < 0$. Given the fact that many more of the unemployed are uninsured this condition is assumed to hold. From (6'), the effect on the reservation wage of uninsured workers, dw^r_u/db, is ambiguous.

Upon inspection, these results make intuitive sense. For the insured group, the increased benefit increases the value of remaining unemployed and must be met by an increase in the value of taking a job at the reservation wage. Hence, the optimal reservation wage must increase. With a higher reservation wage, the marginal benefit of search goes down and search intensity is reduced.to lower the marginal cost of search. Both of these effects lead to the conclusion that the duration of unemployment should increase for the insured unemployed.

For the uninsured group, the marginal benefit of search increases because the insured group searches less hard and accepts fewer offers. Hence, this group will increase its search intensity. However, two countervailing forces lead to an indeterminant change in the reservation wage. First, as

Mortensen (1977) points out, an increase in the UI weekly benefit amount increases the return of getting a job because UI payments if laid off will be higher. On the other hand, since the insured group searches less hard, even without altering its behavior the uninsured group will receive more offers, leading to an increase in the value of remaining unemployed. Overall, the effect on the reservation wage, and therefore the duration of unemployment, for uninsured workers is theoretically ambiguous. Other than this last effect, though, all other factors indicate that the duration of unemployment should go down for the insured group.

APPENDIX 2: COMPARING COEFFICIENT ESTIMATES FROM THE CPS AND NLSY SAMPLES

Although the fraction of the year unemployed among CPS respondents is not a direct measure of the duration of unemployment spells, under reasonable conditions the two measures are highly correlated. In fact, in a two state model of employment and unemployment, if unemployment spells are distributed exponentially and a change in RR does not affect the incidence of unemployment (as might be expected for uninsured workers), a simple mapping exists between the two measures. Since this mapping provides a useful means to interpret the difference between the estimated coefficients on RR using the CPS data (call it β_{cps}) and NLSY data (call it β_{NLSY}) it will be derived here.

First, denote F as the fraction of the year unemployed conditional on experiencing at least one spell of unemployment (as in the CPS data), P_u as the instantaneous probability of being unemployed, and P_o is the probability of experiencing no unemployment in the year. Then:

$$\beta_{cps} = \frac{\partial[E(F)]}{\partial RR} = \frac{\partial[E(F)]}{\partial P_u}\frac{\partial P_u}{\partial RR} \tag{26}$$

where E is the expectation operator. Note that by the laws of conditional probability, $E(F) = P_u/(1 - P_o)$. If the transition rate between employment and unemployment, denoted by λ_e, is constant over time for all workers then $(1 - P_o)$ is the product of the probability of starting the year employed (equal to $[1 - P_u]$) and the probability of surviving the year without losing a job (equal to $\exp[-\lambda_e]$ by integrating an exponential density from 0 to 1 where 1 is the normalized length of a year). Hence:

$$E(F) = \frac{P_u}{(1-P_u)e^{-\lambda_e}} \quad \rightarrow \quad \frac{\partial E(F)}{\partial P_u} = \frac{E(F)}{P_u(1-P_u)} \tag{27}$$

Furthermore, note that P_u can be characterized as the sum of the probability of being employed at time $t - \Delta$ (where Δ is some small time interval) and becoming unemployed at time t plus the probability of being unemployed at time $t - \Delta$ and not finding a job by time t: $P_u = P_u(1 - \lambda_u\Delta) + (1 - P_u)(\lambda_e\Delta) \rightarrow P_u = \lambda_e/(\lambda_e + \lambda_u)$ where λ_u is the instantaneous transition rate from unemployment to employment. If the incidence into unemployment is unaffected by a change in RR then:

$$\frac{\partial P_u}{\partial RR} = \frac{\partial P_u}{\partial \lambda_u}\frac{\partial \lambda_u}{\partial RR} = -\frac{\lambda_e}{(\lambda_e + \lambda_u)^2}\frac{\partial \lambda_u}{\partial RR} \tag{28}$$

In addition, assume that $\lambda_u = \exp(\alpha + \beta RR + \gamma X)$, as in Jones (1988), so that $\partial\lambda_u/\partial RR = \beta\lambda_u$. Substituting in:

$$\frac{\partial P_u}{\partial RR} = -P_u(1 - P_u)\beta \quad \rightarrow \quad \beta_{CPS} = -E(F)\beta \tag{29}$$

An extension provides the means for comparing the results obtained from the CPS and the NLSY sample. The value of β can be obtained by estimating the linear model (where S = completed spell duration):

$$\log S = \alpha + \beta_{NLSY}RR + \gamma X + \epsilon$$

as I estimate in the NLSY sample. Therefore,

$$\beta_{NLSY} = \frac{\partial[E(\log S)]}{\partial RR} = \frac{\partial[\log E(S)]}{\partial RR} \tag{30}$$

where the last equality is shown in Jones (1988). Note that by the assumption that unemployment spells are distributed exponentially, $E(S) = 1/\lambda_u$ so that:

$$\beta_{NLSY} = \frac{\partial\left[\log\left(\frac{1}{\lambda_u}\right)\right]}{\partial RR} = -\frac{\partial[\log(\lambda_u)]}{\partial RR} = -\beta \quad \rightarrow \quad \beta_{CPS} = E(F)\beta_{NLSY} \tag{31}$$

Under the strict assumptions made above, this formula provides a way to compare estimated parameter values obtained from the CPS and the NLSY samples. Given the assumption that incidence of unemployment is unaffected by a change in RR, such a comparison is more reasonable for uninsured workers.

APPENDIX 3: CREATING COMPLETED SPELLS OF UNEMPLOYMENT FROM THE NLSY

Unemployment data in the work history section of the NLSY is created in the following way. At the interview date, the respondent is asked to specify exactly the dates the s/he was employed. Periods of nonemployment since the last interview are therefore observed to be of four kinds: (1) between a job that has ended since the last interview and a new job obtained before the current interview; (2) between the current interview date and the last job (if held after the last interview date); (3) between the last interview date and the first job since the last interview; or (4) between the current interview date and the last interview date if no job was held during this period. Unemployment is measured by asking respondent's how many of the weeks during the nonemployment spell since the last interview were spent looking for work. The specific dates of the weeks spent looking are never asked. In the public use tapes, the weeks spent looking for work are arbitrarily assigned to the middle of the periods of nonemployment since the last interview.[1]

Even if the weeks reported looking for work occurred contiguously within one of the above types of nonemployment spells (an assumption which is maintained throughout the remaining discussion), they do not necessarily represent a completed spell of unemployment. A simple example can help illustrate this point. Suppose interview dates were 52 weeks apart with the first at week 0 and that a time line of a respondent's true work history showed a completed unemployment spell of 39 weeks beginning in week 39:

[1]The work history tape has a different code for a week out of the labor force, unemployed, or unable to make the distinction between the two. In my analysis, I only consider a week to be unemployed if it is explicitly coded that way.

```
        |---OLF--|------U------||---E------>
        ----------------+----------------+----------------+
week #:        39      52         78     104              156
```

At the second interview date, week 52, the respondent will report 13 weeks of unemployment since the last interview at week 0. Therefore, the survey will record 13 weeks of unemployment arbitrarily placed between weeks 0 and 52, say between weeks 20 and 33. At the third interview date, the survey will correctly pick up the new job beginning in week 78, will note that 26 weeks were spent looking for work between weeks 52 and 78 and will record all these weeks as unemployed. The resulting work history as coded by the NLSY would look like the following:

```
        |-OLF-|-U-|-OLF|---U---|-----E------>
        ----------------+----------------+----------------+
week #:      20   33   52       78       104              156
```

The one completed spell of 39 weeks will be coded to look like two spells, the first one 13 weeks long and the second one 26 weeks long. The timing of the spells is also incorrect since the respondent was not unemployed in, say, week 20.

Given the form of the data in the NLSY work history section, I apply information on the respondent's labor force status at the survey date and a few simple assumptions to estimate the completed unemployment spells and when they occurred. If an unemployment spell is observed between an interview date and a new job, then the spell is assumed to have ended immediately before the new job began. If a respondent is unemployed on a specific survey date and some weeks of unemployment are observed before the survey date from the NLSY work history, then the spell is assumed to include the survey date. Finally, a respondent whose work experience is coded as above will have the two coded spells of unemployment merged and placed immediately before the new job. A full description of how these assumptions are applied to estimate completed spells of unemployment is found at the end of this appendix.

The problems of right and left censoring also arise in creating completed spells of unemployment from the work history data. Right censoring would occur when an spell of unemployment is ongoing at the last interview date, prohibiting observation of the completed spell duration. To avoid this problem I only consider spells of unemployment that begin by the end of 1985, ignoring all spells that begin after that point. In this way, a spell can only be censored if it continues through 1986, into 1987, and is ongoing at the last interview date. This did not occur for any spell in my sample.

Left censoring, on the other hand, is a problem that cannot be easily dealt with given the form of the data. A spell is left censored if it is in progress at the initial date for which information is collected. Given the problems of dating spells of unemployment from the NLSY work histories and no additional information regarding labor force status at the first day in the history (January 1, 1978), it is impossible to determine whether a spell that occurred before the first survey date is left censored. However, the problems created by this should be small. Since I have collected spells beginning in eight consecutive years, even if the unemployment rate on January 1, 1978 was 10-15% for this age group, as a proportion of all spells in my sample potentially left censored spells will be very small.

Logic of Program to Create Completed Spells of Unemployment
Data from the NLSY

I. Person only has one group of weeks unemployed after old job ends or before accepting first job

A. Person found a new job before the end of the survey

1. the new job starts before the next interview date
   - put the spell immediately before the new job begins (RULE 1)
2. by the next interview date, the new job has not yet begun
   a. person is unemployed at the next interview date
      - put the spell just before the next interview date (RULE 2)
   b. person is out of the labor force at the next interview date
      - leave the spell where it has been placed on the work history tape and code the date as uncertain (RULE 3)

B. Person did not find a new job by the end of the survey

1. person is unemployed at the next interview date
   - put the spell just before the next interview date (RULE 4); note that if the next interview date is the end of the survey, then the spell is truncated
2. person is out of the labor force at the next interview date
   - leave the spell where it has been placed on the work history tape and code the date as uncertain (RULE 5)

II. Person has more than one group of weeks unemployed after an old job ends or before accepting first job

A. the group of weeks is not the last before new job begins or survey ends

1. person is out of the labor force at the next interview date
   - leave the spell where it has been placed on the work history tape and code the date as uncertain
2. person is unemployed at the next interview date
   a. an entire interview period passes with no unemployed weeks
      - place the spell just before the next interview date (RULE 7)
   b. some unemployment is observed within the following interview period
      (1) the weeks unemployed in the next interview period is followed by the new job also in the next interview period
         (a) more than four weeks of out of the labor force are observed between the next interview date and the start of the new job
            - place the spell just before the next interview date (RULE 8)
         (b) less than four weeks of out of the labor force are observed between the next interview date and the start of the new job

- join the two groups of weeks together and place the combined spell just prior to the date the new job begins (RULE 9)

(2) after the unemployment in the next interview period, another interview date passes before the new job begins

(a) at the second interview date following the group of weeks unemployed, the worker is out of the labor force

- join the two groups of weeks unemployed so that it begins before the next interview date by the amount of weeks of the first group of weeks unemployed (RULE 10)

(b) at the second interview date following the group of weeks unemployed, the worker is recorded as unemployed

- place the group of weeks just before the next interview date and record the spell is "split" in the sense that part of the weeks unemployed in the next group of weeks might be combined with this group of weeks (RULE 11)

B. the group of weeks is the last before the new job or the end of the survey

1. new job is found before the end of the survey

a. a new job is found before the next interview date

- place the spell just before the new job (RULE 11)

b. the new job isn't found until after the next interview date

(1) the worker is unemployed at the next interview date

- move the spell to just before the next interview date (RULE 13)

(2) the worker is out of the labor force at the next interview date

- leave the spell where it has been placed on the work history tape and record the date as uncertain (RULE 14)

2. no new job is found before the end of the survey

a. the worker is out of the labor force at the next survey date

- leave the spell where it has been placed on the work history tape and record the date as uncertain (RULE 15)

b. the worker is unemployed at the next interview date

- move the spell to just before the next interview date (RULE 16); note that if the next survey date is the end of the survey, then the spell is right censored

The rules are illustrated below using time lines showing spells of unemployment as coded by the NLSY work history and as I have recoded them. Interview dates are observed below the lines with the labor force status at the interview date following them, where necessary. In rules 1-5, only one group of weeks unemployed are observed between jobs or before the first job. In rules 5 through 11, only the first group of weeks is recoded and in rules 12 through 16, only the last group of weeks is recoded.

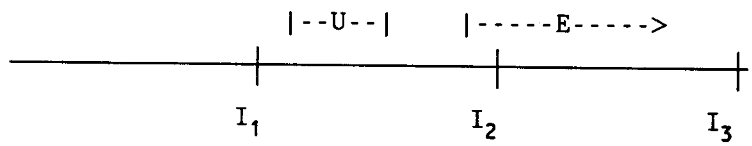
Key:
- U = unemployed
- E = employed
- O = out of the labor force
- I_j = interview date j

Frequency of occurrence

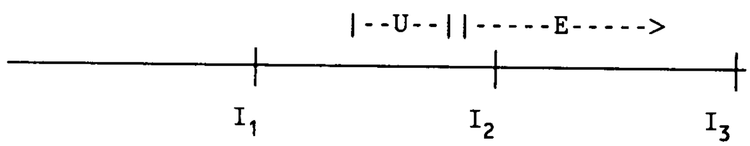
	UI	No UI

Rule 1

as coded:



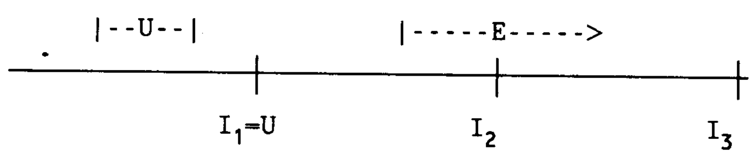
as recoded:



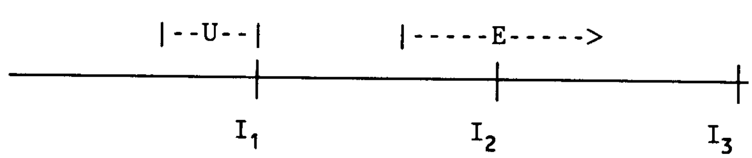
	UI	No UI
as recoded	73.4%	62.1%

Rule 2

as coded:



as recoded:

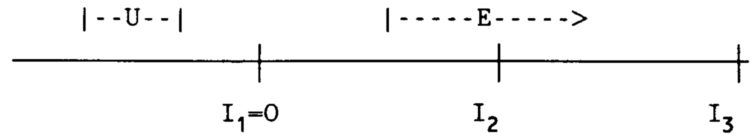


	UI	No UI
as recoded	6.3%	4.9%

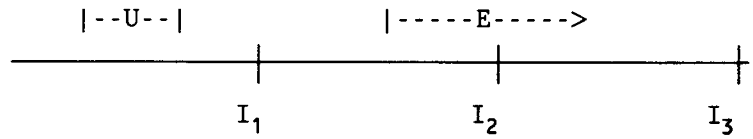
UI | No UI

Rule 3

as coded:



as recoded:

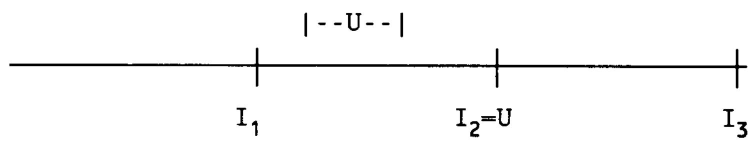


2.9% 4.9%

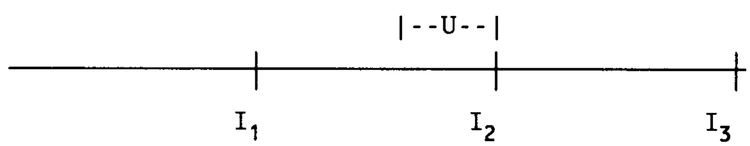
(note: code the spell's date as uncertain)

Rule 4

as coded:



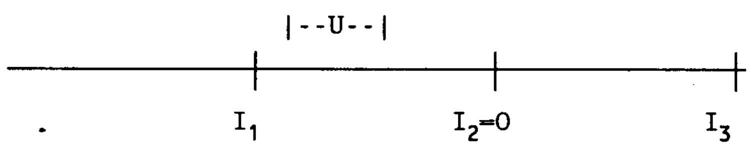
as recoded:



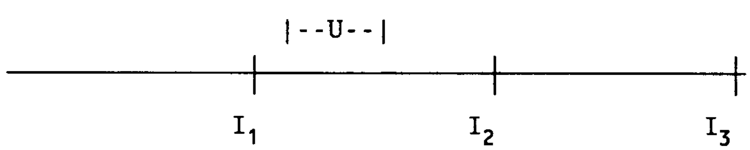
0.0% 0.4%

Rule 5

as coded:



as recoded:



0.0% 0.5%

(note: code the spell's date as uncertain)

	UI	No UI

<u>Rule 6</u>

```
                |--U1--|      |--U2--|    |-----E----->
as coded:      -----------------+-----------+-----------+
                              I1=0          I2          I3

                |--U1--|      |--U2--|    |-----E----->
as recoded:    -----------------+-----------+-----------+       2.7%     6.5%
                                I1          I2          I3
```

(note: code the spell's date as uncertain)

<u>Rule 7</u>

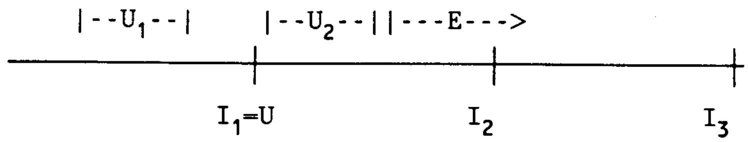
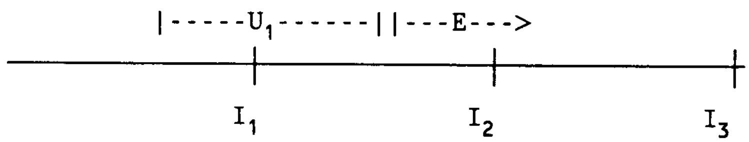
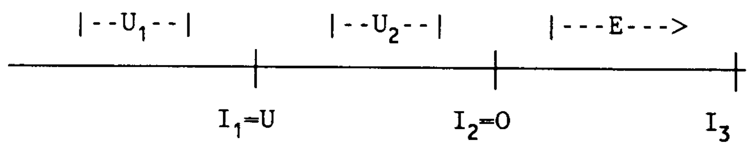
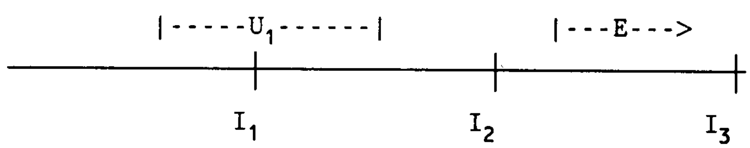
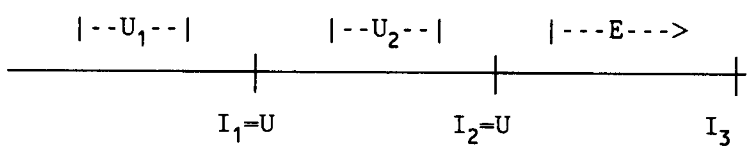
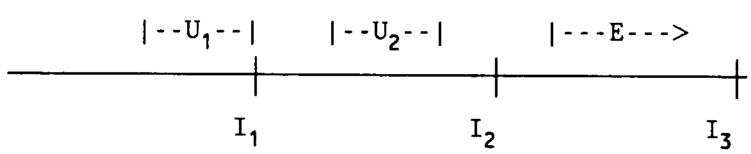
```
                |--U1--|                       |--U2--|
as coded:      -----------------+-----------+-----------+
                              I1=U          I2          I3

                     |--U1--|                  |--U2--|
as recoded:    -----------------+-----------+-----------+       0.4%     1.1%
                                I1          I2          I3
```

<u>Rule 8</u>

```
              |--U1--|          |--U2--|  |---E--->
as coded:      -----------------+-----------+-----------+
                              I1=U          I2          I3

                     |--U1--|  |--U2--|  |---E--->
as recoded:    -----------------+-----------+-----------+       3.6%     3.8%
                                I1          I2          I3
```

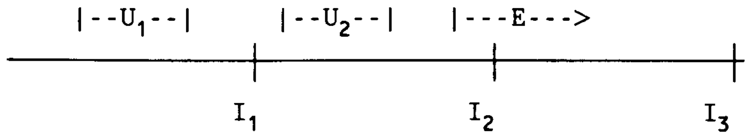
	UI	No UI
Rule 9		
as coded: 		
as recoded: 	2.9%	3.5%
Rule 10		
as coded: 		
as recoded: 	1.0%	1.2%
Rule 11		
as coded: 		
as recoded: 	0.9%	1.6%

(note: part of the second spell should potentially need to be joined to the first spell, but it isn't clear how to do this. I leave the spell where it is and code the first spell as potentially "split".

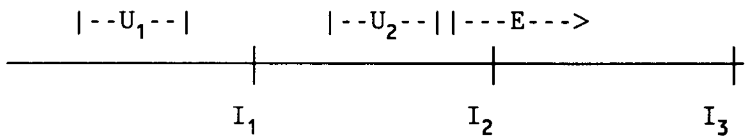
UI   No UI

Rule 12

as coded:



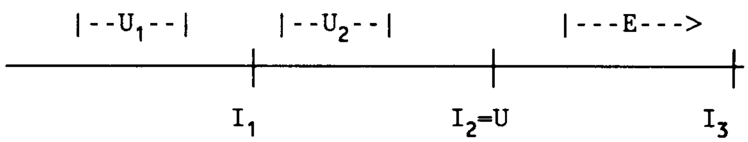
as recoded:



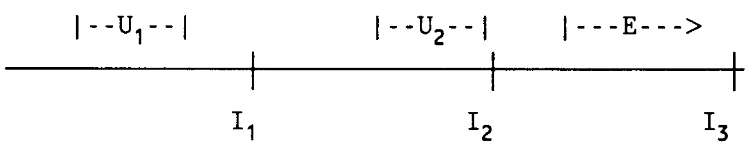
5.7%   8.4%

Rule 13

as coded:



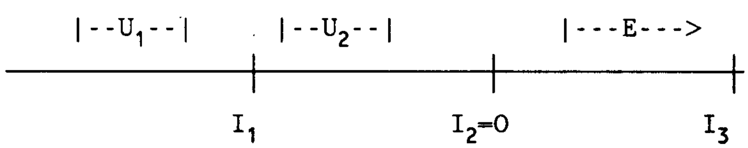
as recoded:



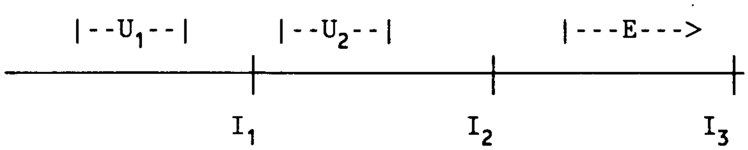
0.1%   0.5%

Rule 14

as coded:



as recoded:



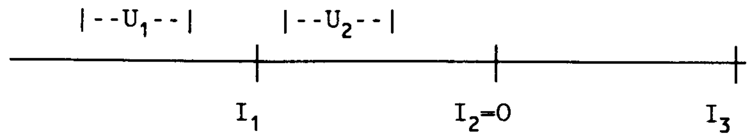
0.1%   0.2%

(note: code the spell's date as uncertain)

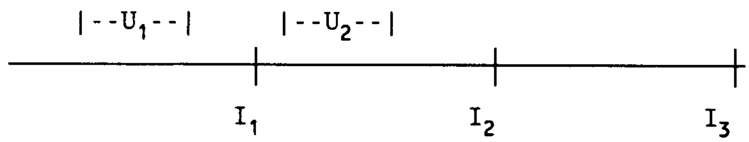
UI   No UI

Rule 15

as coded:



as recoded:

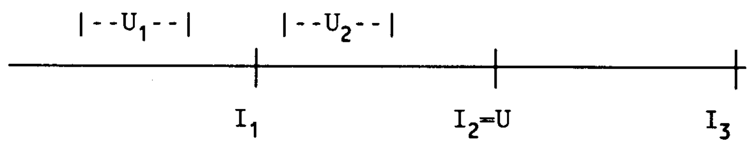


0.0%   0.1%

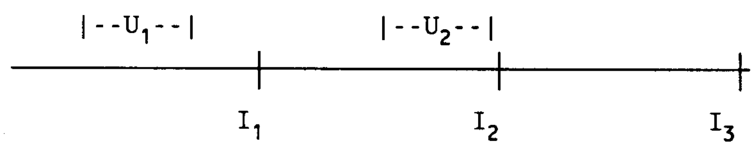
(note: code the spell's date as uncertain)

<u>Rule 16</u>

as coded:



as recoded:



0.0%   0.1%

APPENDIX 4: SIMULATING THE REPLACEMENT RATE

Typically, unemployment insurance benefits are computed as some function of earnings in the "base period" (the first four of the last five calendar quarters prior to filing a claim). The crucial assumption in my simulation is that earnings in the preceding calendar year prior to the year in which benefits were received are an appropriate proxy for earnings in the base period.

The most common functions of earnings in the base period that are used to estimate WBAs are high quarter earnings in the base period, average weekly earnings in the base period, and total base period earnings.[1] Again, total base period earnings are proxied by earnings in the preceding calendar year. Average weekly earnings in the base period are proxied by earnings in the preceding calendar year divided by weeks worked in the preceding calendar year. To proxy high quarter earnings, I apply outside data obtained from the Continuous Wage and Benefit History survey (courtesy of Bruce Meyer), an administrative records data base of unemployment insurance recipients in eight states. A consistent finding in each of the states in every year of this data set is that high quarter earnings are equal to approximately one-third base period earnings. Therefore, I proxy high quarter earnings with one-third of earnings in the preceding calendar year.

In performing this simulation, a problem emerges: using the matched CPS data sets, the sample of workers reporting some unemployment in the preceding calendar year who also report receiving UI is relatively small, especially in smaller states. For instance, in 1978 only 745 people fall into this category with only one person in Oklahoma. Therefore, only using the people who

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[1]An extensive data appendix detailing the WBA formulas for each state in each year is available from the author upon request.

actually reside in a given state in a given year to estimate the replacement rate in that state and year would lead to inefficient estimates.

To address this issue, for each individual identified as unemployed and receiving UI in a given year, I compute his/her replacement rate in every state in that year. Differences in labor quality, the price level, etc. between workers in different states are controlled for by normalizing earnings by the average weekly earnings of all workers in the state as reported in the Employment and Training Handbook 394 and annual updates (which are circulated as Unemployment Insurance Program Letters). For instance, consider a state, k, whose benefit formula is one-half of the average weekly wage subject to some minimum and maximum. Then worker i from state j is assigned a replacement rate in state k equal to (assume, for simplicity, his/her WBA is between the minimum and maximum benefit):

$$RR_{i,j,k} = \frac{1}{2} * \frac{HQE_i}{\overline{AWE}_j} * \frac{\overline{AWE}_k}{AWE_i}$$

where: $RR_{i,j,k}$ = replacement rate of person i from state j in state k

HQE_i = high quarter earnings of individual i

$\overline{AWE}_j$ = average weekly earnings for all workers in state j

$\overline{AWE}_k$ = average weekly earnings for all workers in state k

AWE_i = average weekly earnings of individual i

The mean replacement rate for state s is then computed as the mean of the simulated replacement rate in state s for all workers, regardless of their state of residence.

This strategy not only solves the small sample size problem in some states, but it actually provides a measure of the replacement rate more consistent with the hypothesis I am trying to test. Since the replacement rate is actually used as a proxy for the generosity of each state's UI system, using a uniform distribution of workers (displaced by the mean) across states actually better represents this concept. This method reduces differences in the average replacement rate across states that might be caused by differences in the population of the insured unemployed.