National Tax Journal, March 2014, 67 (1), 219–252

A SHORT REVIEW OF RECENT EVIDENCE ON THE DISINCENTIVE EFFECTS OF UNEMPLOYMENT INSURANCE AND NEW EVIDENCE FROM NEW YORK STATE

Bruce D. Meyer and Wallace K. C. Mok

This paper examines two sets of evidence on the effects of unemployment insurance (UI). First, we discuss two recent lines of research on the effects of UI, one of which argues that UI is more welfare enhancing than previously thought, and a second that suggests that its distortions are often larger than previously argued. We point out limitations in each research program, but conclude that both significantly advance our knowledge. Second, we summarize the evidence on the effect of UI on claim duration from a 36 percent increase in the maximum weekly benefit in New York State. This policy change sharply increased benefits for a large group of claimants, while leaving them unchanged for a large share of claimants who provide a natural comparison group. The New York benefit increase has the special features that it was unexpected and applied to in-progress spells. These features allow the effects on duration to be more convincingly separated from effects on incidence. The results show a fall in the hazard of leaving UI that coincides with the increase in benefits. The estimated unemployment duration elasticities with respect to the UI benefit range from 0.1–0.2, towards the low end of past estimates. We do not find larger effects for those who are more likely to be liquidity constrained. We also examine the extent of bias in standard methods that identify duration effects through nonlinearities in the benefit schedule, finding mixed results.

Keywords: unemployment insurance, unemployment, unemployment durations, unemployment incidence

JEL Codes: J65, J60, J64

I. INTRODUCTION

The effects of unemployment insurance (UI) on unemployment is of interest for two main reasons. First, many authors have argued that UI is a major determinant of differences in unemployment across countries and over time (Layard, Nickell, and Jackman, 1991; Ljungqvist and Sargent, 1998). Second, the magnitude of the effect of

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UI on unemployment is a key input into optimal UI benefit calculations.[1] With recent extensions of UI benefits in the recession, UI has become an especially important policy issue. Spending on UI topped $150 billion in fiscal year 2010 as the unemployment rate rose to 10 percent. These benefits were one of the most clearly countercyclical parts of the stimulus. The disincentive effects of UI have also been of concern to many as the length of unemployment benefits was extended to 99 weeks, unprecedented in U.S. history (though not out of line with the practice in many European countries). In light of these extensions some have argued that a substantial share of the increase in unemployment may be due to UI itself.[2] There has also been intense debate on how long benefits should last, as each extension during the recession has led to hard fought political battles.

This paper has two parts. We first summarize and critique two important recent lines of research in the UI literature. One line of research uses theory and clever empirical analyses to argue that unemployment insurance may have less of a distortionary effect than previously thought because it provides cash to liquidity constrained consumers (Card, Chetty, and Weber, 2007; Chetty, 2008). This line of research shows that UI is more benign than previously thought. A second line of research has used exogenous variation in the length of UI (in Germany) to show that unemployment extensions have substantial disincentive effects that are similar in good and bad times. Furthermore, benefit extensions do not aid the finding of better jobs, rather they lead workers to take worse jobs after a longer time out of the labor market (Schmieder, von Wachter, and Bender, 2012, 2013). This line of research indicates that the stakes of countercyclical stimulus are higher than previously thought and that what was thought to be one of the key benefits of UI, the provision of ample time for productive search, may often be a subsidy to unproductive skill decay.

Turning to the new evidence in this paper, we note that a large literature has examined the effects of UI on unemployment.[3] However, the validity of the sources of identification used in much of the literature has not been carefully examined. Most work on UI identifies its effects through cross-state variation in benefits or by assuming a linear relationship between earnings and duration. Thus, the work requires the comparability of different states or requires strong functional form assumptions. Sometimes changes over time within a state are used as an additional source of variation (Moffitt, 1985; Gritz and MaCurdy, 1989; Meyer, 1990). This paper continues an approach to identification that examines data from before and after sharp changes in the generosity of UI payments. This quasi-experiment or natural experiment approach follows the methods used initially by Classen (1979), Solon (1985), and Meyer (1989).[4]

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[1] Baily (1977), Gruber (1997), and Chetty (2006) pursue this approach.

[2] A range of estimates is provided by the research of Aaronson, Mazumder and Schechter (2010), Rothstein (2011), Mulligan (2012), Farber and Valletta (2013), and Hagedorn, et al. (2013), some of whom attribute a large share of recent unemployment to UI.

[3] Detailed surveys can be found in Krueger and Meyer (2002), Meyer (2002), Holmlund (1998), Atkinson and Micklewright (1991), Danziger, Haveman, and Plotnick (1981), Gustman (1983), Hamermesh (1977), and Welch (1977).

[4] Subsequent papers using a "natural experiment" or "quasi-experimental" method include Hunt (1995), Card and Levine (2000), Carling, Holmlund and Vejsiu (2001), and Røed and Zhang (2003, 2005).

This paper examines the effects of a 36 percent increase in the maximum UI benefit in New York State on the incidence and duration of UI claims. Several aspects of the New York reform make it particularly suitable for examination. First, the benefit increase was unexpected. A benefit increase had been prevented in the past because of a procedural deadlock in the Legislature.[5] After the announcement of an agreement between the legislative leaders and the governor, the reform was passed in a few hours and took effect six days later. Second, because the procedural deadlock had previously prevented a benefit increase, the increase is unlikely to have been caused by economic conditions in the state. Third, unlike most UI benefit increases, the higher benefits were available to those who had started their claims before the increase became effective. Thus, we can examine the effect of the higher benefits on the durations of a pool of claimants whose decision to file could not have been influenced by the higher benefits. Finally, unemployment was fairly stable in New York State during this time period, though macroeconomic changes will still be a concern that we will attempt to minimize with our methods.

The difficulty of identifying UI effects occurs in its most extreme form within a single state at a point in time. The weekly UI benefit is a constant fraction of previous earnings except when an individual receives the minimum or maximum weekly benefit. Since previous earnings strongly influence the payoff from returning to work, the economic benefits of returning to work and the economic gains from receiving benefits are each largely influenced by a common variable, previous earnings. Regressions of spell length on weekly benefits and previous earnings consequently cannot easily distinguish between the effect of UI and the highly correlated influence of previous earnings. Identification is impossible without a functional form assumption on the relationship between previous earnings and spell length.[6]

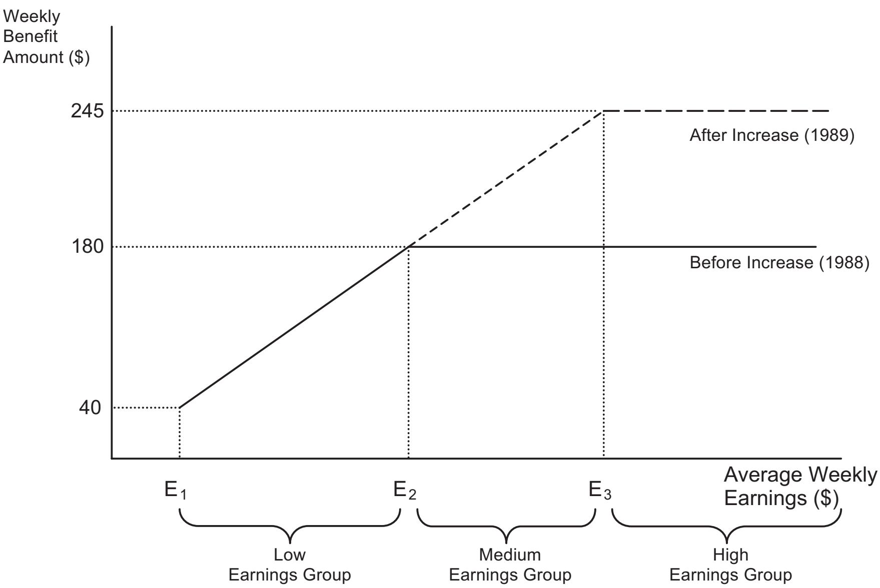
A key idea behind this study is illustrated by Figure 1 which displays the schedule relating the UI weekly benefit amount (WBA) in New York to previous average weekly earnings. The schedule is typical of those in the other states. The solid line is the schedule prior to the April 17, 1989 increase in the maximum weekly benefit amount. The dashed line is the revision to the schedule due to the benefit increase. Between the minimum and the maximum, the weekly benefit amount is one-half of previous weekly earnings. The UI reform increased the benefits received by the High Earnings group with previous weekly earnings greater than E_3, and increased the benefits of the Medium Earnings group with earnings between E_2 and E_3. But the Low Earnings group, with earnings between E_1 and E_2, was unaffected by the change. These Low Earnings individuals provide a natural comparison group to capture changes over time common to all individuals in the state.

[5] Verhovek (1989) reported in *The New York Times* that: "The New York increase, however, was held up because negotiators in the Legislature had until recently insisted on tying that issue to discussion of increases in workers compensation benefits. It was only after the two were severed that the way to a vote was cleared."

[6] This identification problem created by the dependence of program generosity on an individual's previous earnings is common to many social insurance programs and is emphasized in Krueger and Meyer (2002). Meyer, Viscusi and Durbin (1995) provide a similar paper on workers' compensation.

Figure 1

New York State UI Benefit Schedule
Before and After April 17, 1989 Increase in Benefits



While our methods will in the end be more sophisticated than differences in differences techniques, we initially compare changes in spell lengths and the number of claims before and after the benefit increase for the three groups in Figure 1 to estimate the effects of higher benefits. However, concerns about seasonality and macroeconomic changes move us to focus on hazard model estimates of in-progress spells immediately around the increase in benefits. This approach is aided by the fact that the New York benefit increase was unexpected and applicable to all weeks claimed after April 17, 1989 regardless of when an individual filed for benefits. This unusual aspect of the change allows us to examine the effect of benefits on those who had filed just prior to the increase and for whom the increase was unexpected. Thus, we can separate the effect of higher benefits on duration from its effect on the composition of the pool of claimants who start spells. There is only a small literature on the effect of benefit generosity on UI take-up, but researchers have often found a strong effect of benefits on take-up (Corson and Nicholson, 1988; Blank and Card, 1991; McCall, 1995; Anderson and Meyer, 1997).

In a working paper version of this paper (Meyer and Mok, 2007), we show that endogenous take-up that alters the pool of UI recipients can lead to an understatement or overstatement of the effect of UI on unemployment durations. Overstatement of UI effects on duration could occur if those drawn to apply when benefits are higher have shorter expected durations than average. On the other hand, those among a pool of initial non-applicants who are most likely to be induced to apply by a benefit increase might be those who would receive the most from the increase, i.e., those who expect to have a long duration and thus receive that higher benefit for many weeks. Thus, the bias in the duration elasticity could be upward as well.

The results show a fall in the hazard of leaving UI that coincides with the increase in benefits. The estimated unemployment duration elasticities with respect to the UI benefit range from 0.1–0.2, towards the low end of past estimates. We do not find larger effects for those who are more likely to be liquidity constrained. We also examine the extent of bias in standard methods that identify duration effects through nonlinearities in the benefit schedule, finding mixed results.

II. RECENT EVIDENCE ON THE DISINCENTIVE EFFECTS OF UI

This section discusses two lines of recent research that have important implications for the welfare effects of UI. While there is much other recent high quality research, these two lines of work have the potential to change the conventional wisdom on the effects of UI. The first line of research uses theory and clever empirical analyses to argue that unemployment insurance may have less of a distortionary effect than previously thought because it provides cash to liquidity constrained consumers. The key paper in this line of research is Chetty (2008), which shows that the effect of UI on time out of work can be split into income and substitution effects. Past work has argued that unemployment insurance increases durations primarily through a substitution effect (Krueger and Meyer, 2002). Chetty argues that this result is correct for unconstrained individuals. However, for those who face liquidity constraints, he argues the unemployment response is mostly an income effect. Based on empirical evidence he concludes that 60 percent of the increase in durations due to UI is a "liquidity effect." This distinction is crucial. If the increases in unemployment duration are due to a substitution effect, then it is distortionary. Unemployment is a socially suboptimal response to a wedge between private and social marginal costs. On the other hand, if the increase reflects a liquidity effect, then it is a socially beneficial response to credit and insurance market failures.

Empirically, the conclusion is based on hazard models of spell length with measures of UI benefit generosity and wealth and their interactions as the key explanatory variables. Chetty (2008) uses the Survey of Income and Program Participation (SIPP) data for prime-age males who have a work history and are UI recipients. He finds that the state level average benefit has a much larger effect on the job finding hazard for households with low liquid wealth. This interaction is found with liquid wealth, as well as with proxies for liquidity constraints including the lack of a second earner in the household and the presence of a mortgage.

These results though raise an important puzzle. Why is there so little difference in the length of unemployment spells between those with low and high liquid wealth? If those with low net wealth are induced to take jobs earlier than they would otherwise, why is there no relationship between liquid wealth and the duration of unemployment? Looking at Table 1 in Chetty (2008), we see that those with above median net liquid wealth have shorter unemployment durations than those with below median wealth.

Chetty (2008) argues that there are likely unobserved differences between those with high and low assets. Examples of these differences might be heterogeneity in tastes for savings, discount rates, or anticipated expenses (such as college tuition payments). While this argument seems plausible, it has the implication that it would invalidate the comparisons across wealth quartiles that are the heart of the paper. If those with high assets have different preferences, then how they respond to differences in benefits should be different as well. Chetty argues that the variation in UI benefits is exogenous, but an exogenous variable interacted with an endogenous variable is still endogenous. Another reason that one might be concerned about the interpretation of differences by wealth is that individuals with low liquid wealth may have the ability to get jobs more easily (i.e., their labor market may be closer to a spot market or they can work odd jobs, and "under the table" work is more available). In that case, a larger response to benefits may not be due to liquidity constraints but alternative reasons that might mean greater responsiveness to UI.

Chetty (2008) points out that one could infer the size of the liquidity effect from the results by asset quartile if one assumes the substitution effect is the same magnitude across asset quartiles. Rather than making this assumption, Chetty turns to evidence from severance pay where the idea is that only an income effect will be present. He combines data from two sources: a 25 state Mathematica Policy Research dataset and data from the Pennsylvania Reemployment Project. He finds that those with severance pay have longer durations of unemployment. However, severance pay is not randomly assigned so one might wonder whether the result is a case of correlation rather than causation. One might expect that such pay is most often received by those with large expected losses from unemployment (such as those with more firm specific skills) who will have difficulty finding a job. To resolve this issue, Chetty examines if there is a larger effect of severance pay for those individuals with low predicted liquid wealth. Wealth is predicted using data on age, wage, education, and marital status from the severance pay data using the parameters of a prediction equation from SIPP data. We will employ this approach below to predict wealth. He also finds that those who likely have larger payments (those with above median tenure) have larger changes in durations.

This line of research examines some situations where there is an income effect of payments to the unemployed but no substitution effect. It is worth considering cases where the reverse is true. An example may be a series of randomized experiments among UI recipients who were offered substantial payments (but ones they would not receive for several weeks or months) if they took a job more quickly (Meyer, 1995). These experiments seem to show fairly substantial effects on job finding. Future work might try to estimate substitution effects from this evidence, though the difficulty of assessing the magnitude of the transaction costs of receiving the payments may complicate the analysis.

A second important recent line of research examines the effects of UI using a series of age discontinuities in benefit duration in Germany. Schmieder, von Wachter, and Bender (2012, 2013) examine several related issues about the effects of the length of unemployment benefits. What is the effect of longer benefits? Does the effect of benefits differ in good and bad times? And do longer duration benefits enable the unemployed to find better jobs? While this work relies on evidence from Germany rather than the United States, it provides compelling empirical evidence on these questions. The authors rely on sharp differences in the potential duration of UI by exact age allowing the use of a regression discontinuity design. The evidence is especially convincing because of several features of the study: there are discontinuities at different ages, these ages change over time, there do not appear to be discontinuities in recipient characteristics or in claim filing at these ages. The discontinuities in potential duration then lead to obvious discontinuities in unemployment and non-employment duration that require little modeling to observe.

These studies find that unemployment benefits that last one month longer lead to about 0.15 months more non-employment and about 0.30 more months of UI receipt. In the case of extensions of UI potential duration, the effects on UI receipt will be larger than effects on non-employment because extensions will cover periods of unemployment that were previously uncovered even without a behavioral unemployment response.[7] A striking result of this work is that there is little difference in the non-employment effect of UI extensions when the unemployment rate is higher. An important caveat is that we know little about general equilibrium effects (e.g., that longer durations of unemployment for one group might make durations shorter for another). The authors also surprisingly find that a longer time out of work induced by the longer benefits leads to a decline in weekly earnings of about 0.6 to 0.8 percent for each additional month out of work.

The main difficulty with this research from an American perspective is whether it is applicable to the United States. There are several important differences between the German program and labor market and the U.S. situation. German UI benefits are typically more generous, replacing about two-thirds of previous earnings rather than the typical 40 to 50 percent in the United States. While this difference might suggest larger effects of UI in Germany, a second feature works in the opposite direction. While the expiration of benefits in the United States leaves workers with little support except possibly food stamps, the expiration of benefits in Germany generally leads workers to receive unemployment assistance (UA). UA is less generous than UI but is still substantial. The authors report that the program replaces about 35 percent of past earnings on average for men and 10 percent for women (because the benefit amount depends on family earnings). Thus, the drop in compensation at benefit expiration is probably similar or a bit smaller in Germany than the United States, despite the initially more generous benefits. It is this drop in compensation that should be relevant for the incentive effects of UI.

Other issues in interpreting the results are that the discontinuities occur for those in their forties and affect those who expect they might have long spells, as UI lasts at least 12 months for the groups they consider. We usually expect labor supply to be less responsive for prime-age workers. The length of time benefits are received is long, but not without precedent in the United States where benefits lasted up to 55 weeks in the early 1980s and 99 weeks more recently.

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7 We show below that the reverse is true when UI weekly payment amounts are increased — the effect on non-employment will be larger than the effect on UI receipt (but the elasticities will not necessarily have the same relationship).

If we are willing to take these results as applicable to the United States, they have important implications. It is commonplace for economists to assert that UI will have less of an effect in a recession. The German evidence, which is the best evidence on this question to date, implies that this assertion should be called into question.[8]

Second, it is also commonplace for economists to suggest that longer durations are not a social loss because of productive search. The Schmieder, von Wachter, and Bender (2013) results raise the question about whether search is productive given that skills may decay in the meantime. Their paper estimates the causal effect of longer durations using the discontinuities in duration by age. Thus, the study design circumvents the problem that those with longer durations may truly be less productive. This evidence is also consistent with results from Card, Chetty, and Weber (2007) from Austria, and the earlier evidence from the U.S. unemployment experiments that found that incentives that induced shorter durations did not lead to lower earnings (Meyer, 1995). The result that UI induced additional time out of work does not produce higher reemployment wages might not be surprising given the evidence that on average the unemployed spent little time searching — 30 minutes a day as reported by Krueger and Mueller (2010). However, potential employers may still believe those out of work are less productive and be reluctant to hire them (or pay them well), so that the estimated effect may still be partial equilibrium and overstate the effect of increasing potential durations for all workers.

III. THE NEW YORK STATE UI LAW AND THE DATA

> Breaking a longstanding deadlock, legislative leaders and Gov. Mario M. Cuomo agreed today to increase New York State's maximum unemployment benefit by 36 percent, the first raise in five years. Under the plan, which was quickly approved by both houses of the Legislature, the maximum weekly benefit of $180 will immediately rise to $245.
>
> *The New York Times*, April 12, 1989, p. B1

This section describes the main characteristics of New York State's UI law and the data used in our study. As the above quotation indicates, the benefit increase we examine was unexpected and unrelated to economic conditions. To qualify for UI, an individual had to have worked at least 20 weeks out of the preceding 52 and have earned an average of at least $80 during those weeks worked.[9] The weekly benefit paid after a one-week waiting period was 50 percent of average weekly earnings, so that the minimum weekly

[8] That UI can have large effects in a recession is suggested by earlier work by Moffitt (1985) and Meyer (1990) who found large effects relying on data mostly from the high unemployment years of the late 1970s and early 1980s.

[9] A provision which applied to less than 1 percent of claims allowed eligibility for those who worked 15 weeks over the last 52 weeks with a weekly wage of at least $80 and who worked at least 40 weeks in the last 104 weeks with at least $3,200 in total earnings during those 40 weeks.

benefit was $40. The maximum weekly benefit was originally $180 and increased to $245 on April 17, 1989. The maximum weekly benefit rose again on April 16, 1990 to $260. The potential duration of benefits was a uniform 26 weeks during the period examined.

The individual claim data used in the study come from separate data files for 1988 and 1989 which include all UI recipients who began claims in those years. The number of days of benefits received is recorded, as well as age, sex, race, education, the 4-digit SIC industry code of the previous employer, the week the claim was filed, previous earnings and weeks worked, and the 5-digit zip code of the claimant. Close to one-half million claims are available for each year.

We asked the New York State Department of Labor to delete some classes of observations from the files on which we perform most analyses. Claims from firms with mass layoffs during the year are dropped, as are claims from firms with extended strikes. These deletions were made because strikes might unduly influence the results and individual observations from mass layoffs or strikes cannot be taken as independent in either incidence or duration calculations. In New York, workers on strike are eligible for UI benefits after eight weeks. An examination of *Current Wage Developments* (U.S. Department of Labor, 1990a) reveals only three work stoppages involving 1,000 or more workers in New York during the sample period. Observations from these firms are deleted including 32,000 NYNEX Corporation workers who were on strike from August 6 to December 4 of 1989. Observations from firms with mass layoffs according to the BLS definition are also excluded. The BLS defines a mass layoff to be a layoff of at least 31 days duration, involving 50 or more individuals who filed initial claims for UI during a consecutive three-week period.[10] The exclusion of mass layoff data is based partly on the value of the dependent variable, so it likely induces a small amount of bias in duration estimates.[11] In all, the strike and mass layoff exclusions reduce the 1988 sample from 476,173 to 454,169 and the 1989 sample from 581,881 to 519,846.

IV. ECONOMIC CONDITIONS IN NEW YORK STATE

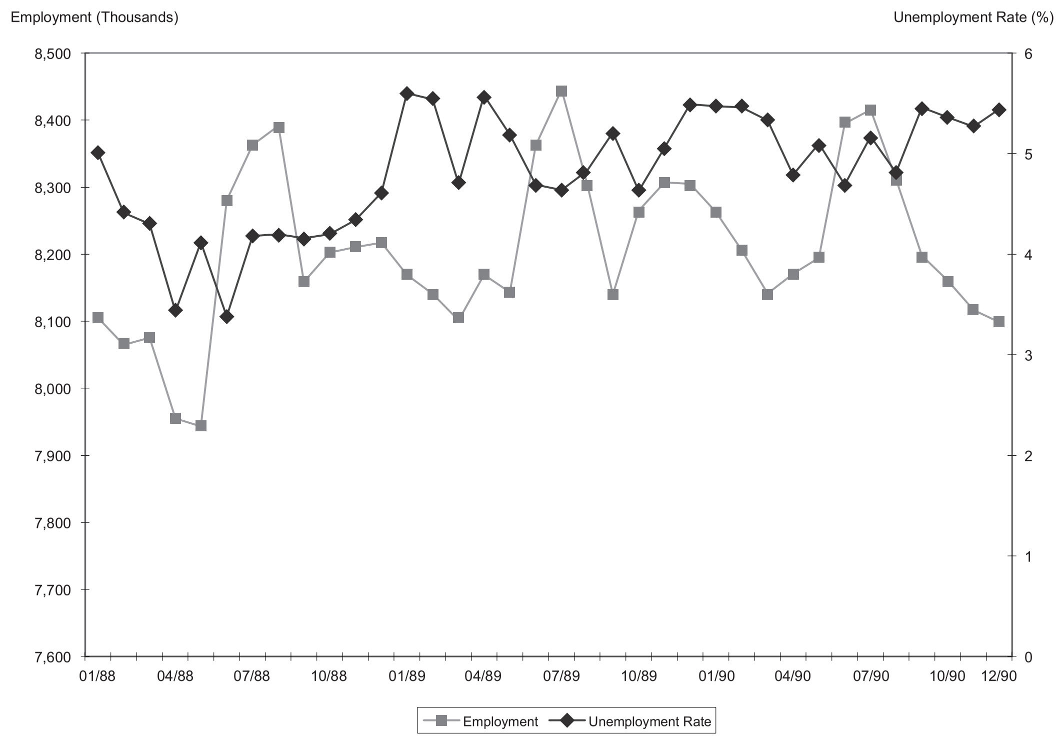
This section provides some background statistics on the New York State labor market around the time of the benefit increase. As this paper examines data from 1988 and 1989, it is important to describe economic conditions at the time of the increase in UI benefits. Figure 2 reports monthly data on employment and unemployment for the 1988–1990 period. Throughout this period, the unemployment rate is quite low, averaging 4.2, 5.1, and 5.2 percent, respectively, in the three years. Employment rises by about 100,000 in each of 1988 and 1989 and then falls back to its earlier level by the end of 1990. During the period on which we primarily focus, the first two quarters

[10] The U.S. Department of Labor (1989, 1990b) provide tabulations of mass layoffs by industry and time period.

[11] The changes in duration and incidence are very similar in the first two quarters if these exclusions are not made. For the last two quarters, the main change is that 32,000 striking workers from NYNEX Corporation are deleted.

Figure 2

Employment Level and Unemployment Rate in New York State 1988-1990



Source: *Employment and Earnings*, various issues of 1988 and 1989, unemployment is almost 1 percentage point higher in the second year, while employment is over 1 percent higher.

Employment patterns by industry are a bit more complex. Appendix Table A1 reports employment by broad industry group. None of the industries have pronounced secular increases or decreases in employment. Employment in Durable and Nondurable Manufacturing does decline somewhat, while the other industries tend to show increases. The bottom several lines of the table report measures of volatility or dispersion of industry level quarterly employment. We report the coefficient of variation of industry level employment measured in levels and logarithms. We also report the variance of the residuals of log employment after regressing it on a constant and a time trend. In these statistics construction is clearly much more variable over time than any industry. In fact, the variance of the de-trended residuals for construction is more than fifteen times that of the closest other industry. This volatility of the construction industry motivates our focus through much of the rest of the paper on a non-construction sample.

V. DESCRIPTIVE STATISTICS ON INCIDENCE AND DURATION

In analyzing the benefit increase in New York, we first examine the number of claims and their mean duration by quarter and earnings group. These numbers allow us to estimate simple difference-in-difference estimates of duration as well as effects on incidence. While these results are suggestive, we cannot rule out macroeconomic changes that differ by earnings group as an alternative explanation for the results. Thus, we will end up stressing the duration models of the next section that focus on changes in benefits using the exact timing and amount of the increase, while controlling for time specific effects for each of the earnings groups.

The descriptive statistics of most interest are the first quarter duration means, and the second through fourth quarter incidence counts. The first quarter duration numbers could not have been affected by changes in the pool of recipients, as UI claimants did not know about the increase at the time they filed for benefits. Increases in duration for these claimants cannot be attributed to changes in the claimant pool, and may be attributable to the benefit increase. Almost all of the second, and all of the third and fourth quarters of 1989 took place after the benefit increase, so these quarters should be examined for changes in claim filing after the increase.

Table 1 reports the incidence and duration of UI claims by quarter for 1988 and 1989.[12] We report separate estimates for the three earnings groups defined in Section I and Figure 1. The three groups are the High Earnings group, whose members experienced the full effect of the benefit increase after April 17, 1989, the Medium Earnings group which

[12] These tabulations exclude observations with no previous earnings, with weeks worked less than 20, or real weekly earnings less than $80. We also exclude the 0.3 percent of observations with pension benefit reductions to avoid the complications they would add. These exclusions eliminate 2.8 percent of observations in 1988 and 2.0 percent in 1989. We also exclude those who worked in the construction industry, which is a further 16.2 percent of observations in 1988 and 16.8 percent of observations in 1989.

Table 1

Incidence and Duration of UI Claims by Quarter
Year Before (1988) and Year of Benefit Increase (1989)

	First Quarter	Second Quarter	Third Quarter	Fourth Quarter
Number of Claims (Incidence)				
High Earnings Group				
1988	25,384	18,961	21,926	19,604
1989	23,722	22,718	30,842	27,456
1989/1988	0.9345	1.1981	1.4066	1.4005
	(0.0084)	(0.0118)	(0.0124)	(0.0131)
Medium Earnings Group				
1988	16,604	11,980	12,767	14,833
1989	15,802	13,746	15,862	17,430
1989/1988	0.9517	1.1474	1.2424	1.1751
	(0.0106)	(0.0143)	(0.0148)	(0.0131)
Low Earnings Group				
1988	63,450	47,374	52,193	62,727
1989	64,066	57,089	58,981	74,468
1989/1988	1.0097	1.2051	1.1301	1.1872
	(0.0057)	(0.0075)	(0.0068)	(0.0064)
High – Low	–0.0752	–0.0069	0.2766	0.2134
	(0.0102)	(0.0140)	(0.0142)	(0.0146)
Medium – Low	–0.0580	–0.0577	0.1124	–0.0121
	(0.0120)	(0.0162)	(0.0163)	(0.0146)
Average Duration of Claims (Weeks)				
High Earnings Group				
1988	14.831	16.368	13.893	16.136
1989	16.106	17.071	13.665	16.324
1989–1988	1.2759	0.7027	–0.2275	0.1884
	(0.0852)	(0.0932)	(0.0884)	(0.0891)
Medium Earnings Group				
1988	15.555	16.712	15.917	16.370
1989	16.273	17.298	16.365	17.243
1989–1988	0.7181	0.5852	0.4473	0.8723
	(0.1058)	(0.1187)	(0.1163)	(0.1060)
Low Earnings Group				
1988	14.888	15.605	14.268	16.027
1989	15.501	15.797	15.075	16.481
1989–1988	0.6128	0.1920	0.8069	0.4538
	(0.0535)	(0.0586)	(0.0571)	(0.0500)
High – Low	0.6631	0.5107	–1.0344	–0.2654
	(0.1007)	(0.1100)	(0.1052)	(0.1022)
Medium – Low	0.1053	0.3932	–0.3596	0.4185
	(0.1186)	(0.1324)	(0.1295)	(0.1172)

Notes: Standard errors are in parentheses. The standard errors for ratios of number of claims are calculated using the delta method applied to (sample size in 1989)/(sample size in 1989 + sample size in 1988) treated as a binomial. The sample excludes those who worked in the construction industry prior to employment and includes some claimants who did not receive unemployment benefits.

received on average less than half of the increase of the High Earnings group, and the Low Earnings group whose benefits were unchanged by the UI reform. The brackets for these earnings groups are indexed using average weekly earnings in New York.[13]

For each quarter, we report the ratio of the number of claims in the two years and the difference in the average number of weeks of benefits received in the two years. At the bottom of the upper panel, we also report the change in incidence for the High and Medium Earnings groups relative to the Low Earnings group. At the bottom of the lower panel, we report the differences-in-differences for duration, comparing the changes for the High and Medium Earnings groups to those for the Low Earnings group in each quarter. We should emphasize that we construct standard errors in the conventional way; however, because of the possibility of common shocks to a given earnings group and quarter, the standard errors may be understated (Anderson and Meyer, 2000; Conley and Taber, 2009).

Several patterns are evident in the data. First, there is a pronounced seasonality to both the incidence and duration of claims. Incidence is lowest in the second quarter for all earnings groups and both years. Duration is longest in the second and fourth quarters for all earnings groups and both years. The pronounced seasonality is the reason for comparing the different calendar quarters of 1989 to the same quarter in the previous year.

There are only moderate changes in incidence for all of the earnings groups in the first quarter, but large changes in later quarters. In the first quarter, the High and Medium Earnings groups experience a 5 to 7 percent fall in the number of claims, while Low Earnings incidence rises by 1 percent. The roughly stable pattern of incidence for the first quarter of 1989 relative to 1988 is another reason we focus on this quarter in subsequent duration analyses. There are large changes in incidence during the other quarters, particularly quarters three and four. In those quarters, High Earnings claims rise 40 percent while Medium and Low claims rise about 20 and 15 percent, respectively. These changes are highly statistically significant as the standard errors are always less than 1.5 percent and often smaller. These numbers are consistent with large effects of the higher benefits on the relative incidence of claims. The implied incidence elasticities for the 3rd quarter are 0.95 and 0.86 for the High and Medium Earnings groups. For the 4th quarter they are 0.73 and –0.09, respectively.[14] There is a possibility that these numbers could be due to macroeconomic shocks to industries or regions that are disproportionate employers of High and Medium Earnings workers. We have examined whether such shocks are the explanation and found that the above patterns hold within sub-state region and industry. Thus, shocks to particular regions or industries can be ruled out, but not broader shocks that disproportionately affect certain earnings groups.

[13] Earnings E_1, E_2 and E_3 in Figure 1 have been indexed using the annual change in average weekly earnings of employees covered by the New York State UI law over 1987–1989, which was 5.3 percent (supplied by the New York State Department of Labor). Precisely, E_1, E_2 and E_3 are taken to be 80, 360, and 490/1.053 in 1988, respectively, and 80*1.053, 360*1.053, and 490 in 1989.

[14] The percentage increases in benefits for the High, Medium, and Low Earnings groups are 12.6, 4.2, and 1.6 percent, respectively, in the first quarter. They are approximately 29.1, 13.2, and 0 percent in the other three quarters.

The duration numbers are also consistent with UI benefit effects. There is a large increase in mean duration of UI receipt in the first quarter for all earnings groups. The changes between 1988 and 1989 are larger for the High and Medium Earnings groups. If one subtracts the change for Low Earnings individuals, the High and Medium Earnings changes are 0.66 and 0.11 weeks, with standard errors of 0.10 and 0.12, respectively. Thus the increase in benefits appears to be associated with an increase in weeks of UI receipt. One can scale these increases to arrive at elasticities after making several assumptions. High Earnings individuals in the first quarter are affected more by the benefit increase if their spell began closer to when benefits rose. We calculate an average benefit for someone receiving 20 weeks of continuous benefits following the claim week. We use 20 weeks because the mean duration is about 16 weeks, but many individuals' period of receipt is likely interrupted by periods when they do not receive UI.[15] Using these assumptions, we calculate elasticities of mean duration with respect to the average benefit of 0.41 for High Earnings individuals and 0.26 for Medium Earnings individuals.

The second quarter has patterns similar to those of the first, while a very different view of duration effects would be obtained from looking at the third and fourth quarters. In these last two quarters the duration of High Earnings claims falls relative to those of Low Earnings individuals. One should note, though, that the increased relative incidence of High Earnings claims may be associated with changes in composition of the pool of claimants. The data from the third and fourth quarter may provide a good example of the biases that can arise in duration estimates when the effects of benefits on incidence are ignored. This possibility of bias in duration estimates when incidence is ignored was one of the key implications of the model in Meyer and Mok (2007) summarized in the introduction. We should note that macroeconomic shocks that disproportionately affect different earnings groups are an alternative explanation for these patterns.

VI. HAZARD MODEL ESTIMATES OF DURATION

To more convincingly identify the effects of UI, hazard models allow us to focus on in-progress spells that could not be contaminated by entry affects and allow us to control for job finding rates that may differ by earnings group and time due to macroeconomic changes. Hazard models also provide a sensible way to account for two key features of the data. First, durations are both left censored at zero and right censored at 26 weeks as discussed above. Second, the level of the weekly UI benefit amount varies over the course of a spell for those who filed shortly before the April 1989 benefit increase (and are in the Medium or High Earnings groups). Hazard models easily incorporate these two features of the data.

We estimate a series of specifications for the hazard of leaving UI as a function of measures of time, the UI benefit, and individual characteristics. Formally, let T_i be the

[15] The total weeks of benefits received in the benefit year may come from several spells. Often benefits are received over a longer calendar period than the number of weeks of UI receipt.

length of individual i's period of UI receipt. Then the hazard for individual i at time t, $\lambda_i(t)$, is defined by

$$\lambda_i(t) \equiv \lim_{h \to 0^+} \frac{\Pr[t+h > T_i \geq t \mid T_i \geq t]}{h}.$$

Before estimating models with controls for individual characteristics, time and various interactions, we plot the hazard rate of those in the High Earnings group and that of the Low Earnings group for comparison using the bi-weekly hazard derived from daily data. The top panel of Figure 3 shows this pattern for 1988, while the bottom panel displays the 1989 pattern. In 1988 the High and Low Earnings groups have very similar hazards. In 1989, after the increase in benefits in the 16th week for those in the High Earnings group, there appears to be a fall in the hazard of those in the High Earnings group relative to that for the Low Earnings group. This pattern accords with the expected decline in the departure rate from the UI rolls after benefits have increased.

To account for individual characteristics and economic conditions, we specify the hazard using a proportional hazards form, i.e., $\lambda_i(t) = \lambda_0(t)\exp(z_i(t)'\beta)$. The funcion $\lambda_0(t)$ is called the baseline hazard and captures how exit rates change as a spell progresses. The time varying explanatory variables $z_i(t)$ include measures of benefit generosity, indicators for the current calendar week, and interactions of time and earnings group. Thus, we can account for the changing benefit and potentially changing conditions in the labor market in a sensible way.

Given our specification and weekly data, spell continuation probabilities can be written as

$$(1) \qquad P[T_i \geq t+1 \mid T_i \geq t] = \exp\left[-\int_t^{t+1} \lambda_i(u)\,du\right] = \exp\left[-\exp\left(z_i(t)'\beta\right)\int_t^{t+1} \lambda_0(u)\,du\right]$$

if $z_i(t)$ is constant between t and $t+1$. Equation (1) can be rewritten as

$$(2) \qquad P[T_i \geq t+1 \mid T_i \geq t] = \exp\left[-\exp\left(z_i(t)'\beta\right) + \gamma(t)\right],$$

where $\gamma(t) = \ln\left[\int_t^{t+1} \lambda_0(u)\,du\right]$.

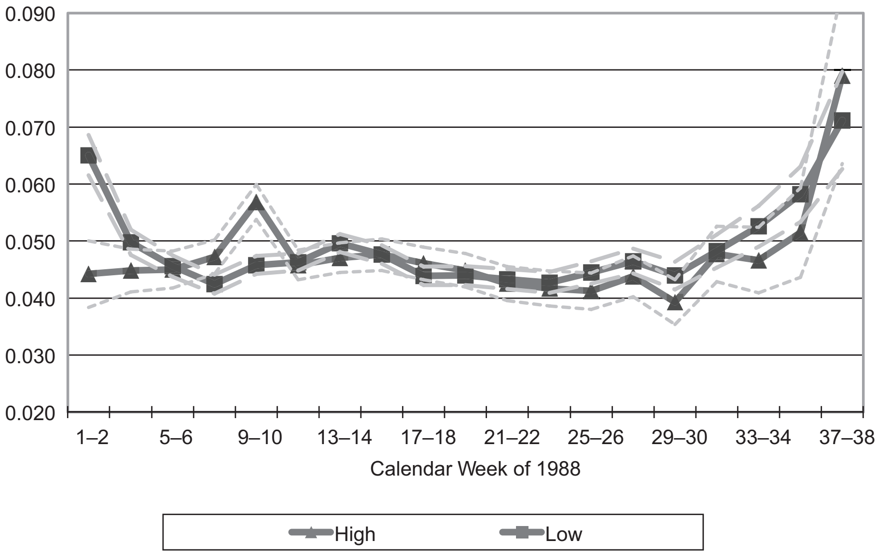
A likelihood function can then be constructed from terms like (2) and one minus the probability in (2) as in Meyer (1990).

The first set of hazard model estimates is reported in Table 2.[16] Appendix Table A2 provides the means by period and earnings group for the covariates that are included in

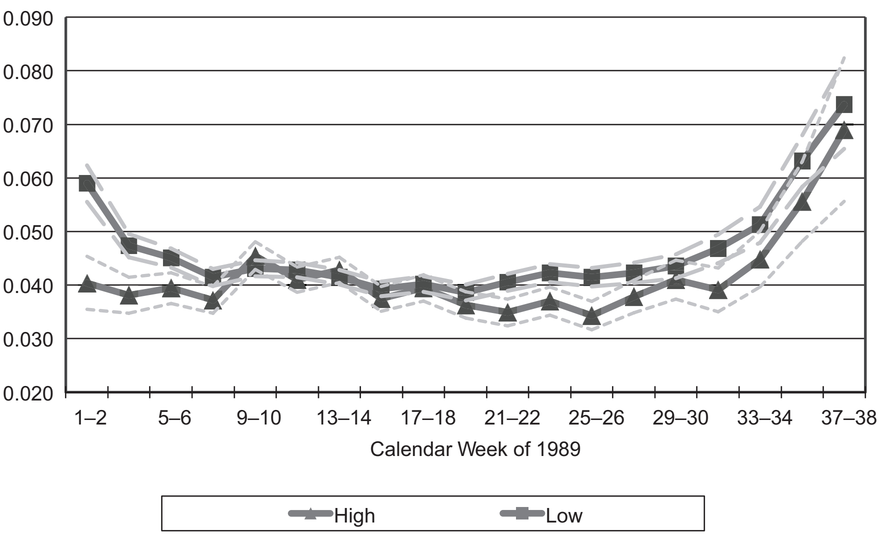
[16] The hazard models estimates drop observations with missing age, sex, race, education, earnings or industry. They also drop out of state claims which by itself excludes 7.2 percent of the observations. Including the exclusions of Section V, 20.8 percent of the 1988 observations and 12.7 percent of the 1989 observations are dropped (mostly due to missing education).

Figure 3

Empirical Hazard of 1988 First Quarter Claimants,
High Earnings Group and Low Earnings Group



Empirical Hazard of 1989 First Quarter Claimants,
High Earnings Group and Low Earnings Group



Notes: These graphs show the Kaplan-Meier hazards for the High and Low Earnings Groups. Only first quarter data are included, and those who worked in the construction industry are deleted; 95% confidence bands are also displayed.

Table 2
First Quarter Duration Model Estimates,
Using Time-Period Interactions to Identify the Effect of the Benefit Increase

	Specification					
Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)
After 16th week of 1989* High Earnings Group	–0.0587 (0.0205)	–0.0584 (0.0205)	–0.0582 (0.0205)	–0.0578 (0.0205)	–0.0608 (0.0205)	–0.0701 (0.0282)
After 16th week of 1989* Medium Earnings Group			–0.0418 (0.0237)	–0.0419 (0.0237)	–0.0437 (0.0237)	0.0032 (0.0324)
High Earnings Group	0.2297 (0.0149)	0.2380 (0.0149)	0.2252 (0.0148)	0.2333 (0.0148)	0.2266 (0.0149)	0.1680 (0.0203)
Medium Earnings Group			0.0267 (0.0133)	0.0312 (0.0133)	0.0267 (0.0134)	–0.0097 (0.0182)
1989*High Earnings	–0.0691 (0.0178)	–0.0795 (0.0179)	–0.0684 (0.0178)	–0.0785 (0.0178)	–0.0636 (0.0182)	–0.0202 (0.0232)
1989*Medium Earnings			0.0027 (0.0207)	–0.0036 (0.0207)	0.0056 (0.0208)	–0.0301 (0.0265)
Week Spell Began Indicators		Yes		Yes	Yes	Yes
Industry and Region* 1989 Interactions					Yes	Yes
Medium Earnings Group Included in Sample			Yes	Yes	Yes	Yes
Only Spells Beginning in Weeks 1-6						Yes
Number of Spells	147,428	147,428	173,927	173,927	173,927	93,292

Notes: Standard errors are in parentheses. Controls for previous earnings, previous weeks worked, age, gender, education, race, industry and region are included. In addition, indicator variables for each calendar week are included. Only Q1 observations are included, those in construction, with missing demographics, no previous earnings, with weeks worked less than 20, real weekly earnings less than $80 ($84.24 in 1989), out of state observations and those with pension reductions are deleted.

the models (variable definitions are provided in the Data Appendix). These specifications include a dummy variable for the week in question being after the benefit increase (After 16th week of 1989) interacted with the High Earnings or Medium Earnings group. Controls for previous earnings, previous weeks worked, age, gender, education, race, industry, region, as well as indicators for the calendar week and the current spell length are included. All of these specifications control for being in the High Earnings group after the benefit increase. Benefit effects are identified through the exact timing of when the benefit increase took place.

Specifications (1) and (2) do not include the Medium Earnings group. Specification (2) and (4) through (6) include indicator variables for the week the spell began. Specifications (5) and (6) include indicators for industry and region interacted with being in the year of the benefit increase.

These specifications indicate that the hazard of ending a UI claim falls by about 6 percent after the weekly benefit rises for the High Earnings group. The coefficients do not differ much across specifications. As expected, there is a smaller coefficient for the Medium Earnings group, but the coefficient is only marginally significantly different from zero. The inclusion of controls for the week a spell began or the interaction of industry and region with the second year of data has little effect on the coefficients. While these specifications control for being in the High Earnings group after the increase it is a concern that the coefficient on *After*High Earnings* is significantly different from zero even when we control for the exact timing of the benefit increase. These coefficients suggest that there is an independent effect of being in the *After*High Earnings* group or that the *After 16th week* variable does not fully capture the benefits change. Specification (6) only includes the spells beginning in the first six weeks of the year. This sample does not seem to suffer from this problem as the coefficient on *After*High Earnings* is now small and not significant. This sample is also of interest for a second reason. This sample will disproportionately include those whose benefits change later in their spell and will thus emphasize changes in benefits near the end of the benefit entitlement period. Previous work has emphasized that the effect of UI on the hazard should fall with duration (Arulampalam and Stewart, 1995). This effect is a general prediction of search models, but does not necessarily hold in labor supply models of unemployment. We find little support for this prediction here as the coefficient estimate is slightly higher in specification (6) than in the other specifications, the opposite of what some models predict.

In our second set of duration estimates, reported in Table 3, we use the amount of the benefit increase for each individual rather than indicator variables as well as the exact timing of the benefit increase within spells to identify the UI effect. Specifically, we include two UI variables in these specifications, the logarithm of the weekly benefit amount in the current week minus the benefit under the old UI law as well as a variable for the weekly benefit under the old law. The first variable captures the effect of the change in the schedule due to the benefit increase, while the second variable identifies the effect of benefits through the bend in the schedule. The other control variables are the same as those in the specifications of Table 2. In addition specification (7) is added that also includes a spline in *ln(Earnings)* to illustrate that in a cross section identification of benefit effects comes from the bend in the schedule.

These specifications suggest that a 10 percent increase in the benefit lowers the hazard of ending a UI spell by about 3 percent. The estimates are not appreciably affected by adding controls for the week a spell began or interactions of industry and region with the year of the increase. Specifications (5) and (6) in this table also control for being in the High and Medium Earnings groups after the benefit increase as in Table 2, and are identified through the exact timing of when benefits increase and by the amount that

Table 3

First Quarter Duration Model Estimates,
Using Benefit Level Variables to Capture the Effect of the Benefit Increase

	Specification						
Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Ln(WBA in Current Week) -Ln(WBA Under Old Law)	–0.3308 (0.0600)	–0.3468 (0.0599)	–0.3119 (0.0560)	–0.2934 (0.0563)	–0.1837 (0.0613)	–0.1776 (0.0848)	–0.3168 (0.0567)
Ln(WBA Under Old Law)	–0.1825 (0.0243)	–0.1820 (0.0243)	–0.2022 (0.0237)	–0.2056 (0.0238)	–0.2049 (0.0238)	–0.1047 (0.0323)	–1.9532 (0.7808)
High Earnings Group	0.1314 (0.0160)	0.1355 (0.0160)	0.1228 (0.0157)	0.1209 (0.0157)	0.1539 (0.0172)	0.1310 (0.0233)	–0.0025 (0.0431)
Medium Earnings Group			0.0259 (0.0104)	0.0252 (0.0104)	0.0288 (0.0134)	–0.0088 (0.0182)	0.0474 (0.0363)
1989*High Earnings					–0.0760 (0.0163)	–0.0368 (0.0212)	
1989*Medium Earnings					–0.0104 (0.0171)	0.0345 (0.0229)	
Week Spell Began Indicators		Yes	Yes	Yes	Yes	Yes	Yes
Industry and Region* 1989 Interactions				Yes	Yes	Yes	Yes
Ln(Earnings)Spline							Yes
Medium Earnings Group Included in Sample			Yes	Yes	Yes	Yes	Yes
Only Spells Beginning in Weeks 1–6						Yes	
Compensated Duration Elasticity	0.1629	0.1686	0.1500	0.1413	0.0886	0.0885	0.1525
Total Duration Elasticity	0.1915	0.1983	0.1743	0.1638	0.1027	0.1090	0.1525
Number of Spells	147,428	147,428	173,927	173,927	173,927	93,292	173,927

Notes: Standard errors are in parentheses. Controls for previous earnings, previous weeks worked, age, gender, education, race, industry and region are included. In addition, indicator variables for each calendar week, and the current spell length are included. Only Q1 observations are included, those in construction, with missing demographics, no previous earnings, with weeks worked less than 20, real weekly earnings less than $80 ($84.24 in 1989), out of state observations and those with pension reductions are deleted. The compensated and total duration elasticities are computed based on a simulated 10% benefit increase.

they increase. The coefficient on the change in the weekly benefit amount falls somewhat, but is still significantly different from zero. In specification (6) we again restrict the sample to the first half of the quarter with the coefficient on *After*High Earnings* again much smaller and not significantly different from zero, while the change in benefit coefficient is little altered.

In most of the specifications, the estimated effect of the benefit under the old UI law is slightly smaller than the coefficient on the change in the weekly benefit, but still strongly significant. In some cases we can reject the restriction that the two weekly benefit amount coefficients are equal; the evidence indicates that the benefit effect identified by the schedule nonlinearity is statistically different from that identified by the benefit increase in specifications (1), (2) and (7). We should emphasize that this former coefficient cannot be identified if one includes a completely flexible set of controls for prior earnings. The last specification (7) makes this clear, as the inclusion of a spline in past earnings drives the standard error on this coefficient sharply upward and the point estimate becomes implausible.[17]

To aid in interpreting the estimates, we also convert the coefficient on the change in the UI benefit to elasticities. For the specifications in Table 3, using the first quarter of 1988 sample, the resulting elasticities are reported in the last two rows. The compensated duration elasticities range from 0.09–0.17, while the total duration elasticities range from 0.10–0.20. Overall, the benefit elasticity estimates are smaller than many that have been found in the literature, such as those in Moffit (1985), Meyer (1989, 1990), Classen (1979), and Solon (1985).

VII. Estimates for Subsamples and an Investigation of Liquidity Constraints

While the elasticities that we find for the overall sample are not large, there may be subsamples of the population whose durations respond strongly to UI. Splitting the sample also allows us to investigate whether there is support in these data for the effect of UI being larger for those who are more likely to be liquidity constrained. In Table 4 we report coefficient estimates on the logarithm of the change in benefits for four partitions of the sample: by gender, age, education, and predicted net wealth. Net wealth is predicted using the SIPP data used by Chetty (2008) with the only difference being that we do not have one variable he used (marital status) so that the adjusted R-square of the regression falls slightly (from a fairly low 0.062 to 0.058). The specifications that we estimate are just those reported in the first six columns of Table 3, but estimated on these subsamples. The estimates differ sharply across the subsamples. The total duration elasticities for males range from 0.06–0.22, while for females they are much larger, ranging from 0.30–0.39. Labor supply elasticities are generally found to be larger for women, and this finding may be just another example of this regularity. The total duration elasticities for those under age 40, are close to zero, while those for individuals

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[17] For specification (7) the compensated and total elasticities are the same due to the implausibly large coefficient on *Ln(WBA Under Old Law)* that is no longer well-identified.

Table 4
First Quarter Duration Model Estimates, Table 3 Specifications, Compensated and Total Duration Elasticities for Subsamples

	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
Male						
Ln(WBA in Current Week)	–0.3177	–0.3212	–0.2565	–0.2330	–0.1077	–0.3307
-Ln(WBA Under Old Law)	(0.0740)	(0.0740)	(0.0700)	(0.0704)	(0.0769)	(0.1048)
Compensated Elasticity	0.1594	0.1594	0.1258	0.1144	0.0529	0.1649
Total Duration Elasticity	0.1955	0.1947	0.1518	0.1376	0.0636	0.2225
Number of Spells	76,502	76,502	93,258	93,258	93,258	50,752
Female						
Ln(WBA in Current Week)	–0.5520	–0.5843	–0.6828	–0.6661	–0.7338	–0.6623
-Ln(WBA Under Old Law)	(0.1168)	(0.1167)	(0.1043)	(0.1051)	(0.1128)	(0.1609)
Compensated Elasticity	0.2631	0.2745	0.3164	0.3095	0.3405	0.3229
Total Duration Elasticity	0.3028	0.3170	0.3619	0.3530	0.3882	0.3621
Number of Spells	70,926	70,926	80,669	80,669	80,669	42,540
Age less than 40						
Ln(WBA in Current Week)	–0.0583	–0.0691	–0.0713	–0.0466	0.0178	0.0155
-Ln(WBA Under Old Law)	(0.0823)	(0.0823)	(0.0759)	(0.0764)	(0.0822)	(0.1144)
Compensated Elasticity	0.0309	0.0362	0.0369	0.0242	–0.0092	–0.0083
Total Duration Elasticity	0.0327	0.0384	0.0388	0.0255	-0.0097	–0.0089
Number of Spells	84,755	84,755	100,558	100,558	100,558	53,658
Age 40 and above						
Ln(WBA in Current Week)	–0.7067	–0.7414	–0.6775	–0.6671	–0.5517	–0.4703
-Ln(WBA Under Old Law	(0.0884)	(0.0884)	(0.0835)	(0.0841)	(0.0930)	(0.1275)
Compensated Elasticity	0.3166	0.3269	0.2962	0.2916	0.2415	0.2133
Total Duration Elasticity	0.4297	0.4454	0.3986	0.3874	0.3204	0.3071
Number of Spells	62,673	62,673	73,369	73,369	73,369	39,634
Less than High School						
Ln(WBA in Current Week)	–0.2997	–0.3301	–0.2447	–0.2336	–0.0007	0.3721
-Ln(WBA Under Old Law	(0.1395)	(0.1395)	(0.1250)	(0.1256)	(0.1391)	(0.1847)
Compensated Elasticity	0.1409	0.1529	0.1129	0.1079	0.0003	–0.1797
Total Duration Elasticity	0.1585	0.1715	0.1239	0.1182	0.0004	–0.2172
Number of Spells	44,416	44,416	50,420	50,420	50,420	28,079

Table 4 (Continued)

First Quarter Duration Model Estimates, Table 3 Specifications, Compensated and Total Duration Elasticities for Subsamples

	Specification					
	(1)	(2)	(3)	(4)	(5)	(6)
High School and Above						
Ln(WBA in Current Week)	–0.2325	–0.2403	–0.2280	–0.1980	–0.0800	–0.0870
-Ln(WBA Under Old Law	(0.0677)	(0.0677)	(0.0637)	(0.0641)	(0.0695)	(0.0974)
Compensated Elasticity	0.1165	0.1190	0.1113	0.0969	0.0392	0.0439
Total Duration Elasticity	0.1346	0.1377	0.1276	0.1108	0.0448	0.0525
Number of Spells	103,012	103,012	123,507	123,507	123,507	65,213
Less than Median Net Assets						
Ln(WBA in Current Week)	0.0175	0.0053	–0.0895	–0.0684	0.0193	0.3059
-Ln(WBA Under Old Law)	(0.1257)	(0.1257)	(0.1056)	(0.1060)	(0.1137)	(0.1539)
Compensated Elasticity	–0.0091	–0.0027	0.0457	0.0351	–0.0099	–0.1625
Total Duration Elasticity	–0.0094	–0.0028	0.0468	0.0359	–0.0102	–0.1724
Number of Spells	75,128	75,128	86,963	86,963	86,963	47,223
More than Median Net Assets						
Ln(WBA in Current Week)	–0.5977	–0.6221	–0.5824	–0.5656	–0.4788	–0.4331
-Ln(WBA Under Old Law)	(0.0753)	(0.0753)	(0.0720)	(0.0725)	(0.0798)	(0.1105)
Compensated Elasticity	0.2742	0.2815	0.2614	0.2538	0.2150	0.2002
Total Duration Elasticity	0.3158	0.3247	0.3004	0.2904	0.2459	0.2362
Number of Spells	72,300	72,300	86,964	86,964	86,964	46,069

Notes: Standard errors for the Ln(WBA in Current Week)-Ln(WBA Under Old Law) coefficients are in parentheses. The specifications for these models are the same as those in columns (1) to (6) of Table 3. See Table 3 for details on specification. The compensated and total duration elasticities are simulated as described in the Appendix.

age 40 and older are quite large, ranging from 0.31–0.45. Since younger workers are generally thought to be more likely to be liquidity constrained, this finding does not support the notion that liquidity constraints drive the duration response to UI (Chetty, 2008), though other factors could be behind the difference between younger and older workers. The differences by education are less sharp, with similar elasticities for those without a high school degree and those with at least a high school degree. Finally, we examine differences by predicted wealth. Contrary to the results of Chetty (2008), we find that there is a sharply higher elasticity for those with above median predicted net assets. The estimated effects for the low wealth individuals are never significantly different from zero. These results suggest that the evidence of larger effects of UI on unemployment durations for liquidity constrained individuals may not be particularly robust and should be investigated further in other datasets.

VIII. DISCUSSION AND CONCLUSIONS

In this paper we first discuss two recent lines of research on the effects of unemployment insurance that challenge conventional wisdom. The first line of research emphasizes that if UI recipients are liquidity constrained, part of the duration response to benefits is a non-distortionary income effect. While we believe there are reasons to question the magnitude of the estimates of the income effect in some of this work, the basic argument seems correct and important. A second line of research examines a series of convincing discontinuities in UI potential duration by age in Germany, finding substantial effects of longer potential durations on non-employment that do not fall in high unemployment times. Longer durations induced by the longer potential durations lead to lower rather than higher wages at reemployment. While the applicability of the results to the United States is difficult to resolve, the evidence is more convincing than the U.S. studies on the same questions because of clear identification, multiple similar estimates, and good precision.

Second, we examine the effect of a 36 percent increase in the maximum UI benefit in New York State. The benefit increase in New York had the unusual feature that it applied to only high and medium earnings claimants and to in-progress spells. The results suggest that this increase in UI benefits led to a large increase in the number of unemployment insurance claims. However, we cannot exclude the possibility that shocks that disproportionately affected high-wage workers resulted in the increases in claims among high-earning workers that received the higher benefits. We are able to rule out that shocks to particular industries or regions were responsible for this result.

There is strong evidence of an effect of the benefit increase on the duration of claims. We examine differences in means but emphasize hazard model estimates that use the exact timing and amount of the benefit increase and control for each calendar week as well as the interaction of the earnings groups and time. The estimates of the elasticity of unemployment with respect to the benefit range from 0.10–0.20. This range is lower that found by Classen (1979), Solon (1985), and Meyer (1989) who also examined data around changes in benefit generosity, and a bit lower than the median of other previous estimates. We note that the identification of the weekly benefit effect through the bend in the benefit schedule alone yields roughly similar, though somewhat lower, estimates. While similar, we can reject equality in about half the cases, so we take the evidence to be mixed. We also emphasize that endogenous UI take-up can bias estimates of the effect of the level of benefits on the mean duration of UI receipt and note the theoretical ambiguity of the direction in the bias.

While the overall elasticities that we find tend to be low, for some large subsamples, in particular those age 40 and older and women, we find substantial elasticities. The subsample analysis also does not indicate larger effects for those individuals who are more likely to be liquidity constrained, contrary to the evidence in Chetty (2008). There are several factors to consider when comparing the estimates in this paper to other estimates. First, it may be that the estimates are biased because of macroeconomic shocks that disproportionately affect High Earnings claims. Our duration model estimates rely on the exact timing and amount of the benefit increase and control for earnings interacted with time to reduce the likelihood of this possibility. Second, it may be that the effect of a given benefit increase is smaller when the level of after-tax benefits is low relative to previous earnings. Most previous work has examined UI in a period when benefits were not taxable and thus after-tax replacement rates were high. We are also primarily examining changes in benefits for the group with the lowest replacement rates, the High Earnings group. Even after the increase in benefits, the average replacement rate for this group is only 0.37, as reported in Appendix Table A2. Third, we capture a slightly different partial derivative than usual since the benefit increase was a surprise. Changes in savings and other responses could have not taken place. This short-run elasticity may be lower than the long-run one.[18] Fourth, it may be that UI benefits have a different impact during periods of very low unemployment such as New York in the late 1980's. Fifth, our first quarter duration estimates will mostly capture the effects of UI towards the end of the eligibility period. One might expect a given UI benefit increase to then have a smaller impact closer to benefit exhaustion as suggested in Mortensen (1977) and emphasized by Arulampalam and Stewart (1995). We have tested for this hypothesis above (specification (6) in Tables 2 and 3) by examining the sample of spells starting in weeks 1 to 6 of the first quarter. Since the coefficient estimates from these alternative specifications are very similar to the full sample estimates, they do not support this hypothesis.

ACKNOWLEDGEMENTS

We would like to thank Joyce Burnette and Uri Kogan for outstanding research assistance, John Comiskey of the New York State Department of Labor for supplying the data, and Joseph Altonji, Raj Chetty, John Ham, Robert Fairlie, Johannes Schmieder, Till von Wachter, Paula Worthington, and seminar participants at the Institute for Research on Poverty, Northwestern University, the NBER Summer Institute, the University of Chicago, and the Federal Reserve Bank of Chicago for their comments. We are grateful to the National Science Foundation, the Searle Fund, and the Stigler Center for their support.

[18] The effect of a surprise increase in benefits on unemployment could be either bigger or smaller than the effect of an expected change. If workers expect a benefit increase, they might change the type of job they take, or change the care they take to avoid being laid off (much of this would appear in incidence but might affect duration through changes in the composition of the unemployed population). These types of changes would likely make the response to an expected increase greater than that to a surprise increase. On the other hand, if one considers savings responses to UI, an increase in benefits that is expected will lead people to save less. Thus, the difference in unemployment durations between the high and low benefit regimes would be reduced because assets would be lower in the high benefit regime, implying that high benefit durations would be shorter than they would be if assets did not change.

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APPENDIX: SIMULATING DURATION ELASTICITIES

This appendix describes how we simulate duration elasticities. Following (2), we can write the estimated survivor function for observation i as

$$\text{(A.1)} \quad \hat{S}_i(t) \equiv P[T_i \geq t] = \exp\left[\sum_{\tau=0}^{t-1} - \exp[z_i(\tau)'\hat{\beta} + \hat{\gamma}(\tau)]\right] = \exp\left[\sum_{\tau=0}^{t-1} - \hat{h}_i(\tau)\right], \text{ for } t \geq 1,$$

where $\hat{h}_i(\tau) = \exp[z_i(\tau)'\hat{\beta} + \hat{\gamma}(\tau)]$. The average estimated survivor function then is

$$\text{(A.2)} \quad \bar{S}(t) \equiv \frac{1}{N}\sum_{i=1}^{N} \hat{S}_i(t), \text{ with } \bar{S}(0) \equiv 1.$$

The predicted mean duration of UI receipt (compensated duration) and full duration are then $\sum_{\tau=1}^{26} \bar{S}(\tau)$ and $\sum_{\tau=1}^{26} \bar{S}(\tau)$, respectively. To calculate full duration we need to assume a value for $\hat{h}_i(\tau)$ for $\tau > 25$. We use the predicted average over 0 to 25 if there were no UI benefits. Letting $\hat{\beta}_1$ be the estimated coefficient on l*n(WBA in Current Week)-ln(WBA Under Old Law)* and $\hat{\beta}_2$ the estimated coefficient on *ln(WBA Under Old Law)* we just approximate this value by setting $\hat{\beta}_1 = \hat{\beta}_2 = 0$. To simulate the effect of raising the level of UI benefits by 10 percent we can just multiply $\hat{h}_i(\tau)$ in (A.1) by $\exp(0.1\hat{\beta}_1)$ for $\tau < 26$, where $\hat{\beta}_1$ is taken from Table 3 or 4. We estimate elasticities by dividing the proportional change in estimated duration from this exercise by 0.1.

Appendix Table A1

Quarterly Employment by Major Industry, its Ratio to the 1987 Average, and Measures of Variability
New York State, 1987–1989 (Thousands)

	Construction	Durable Manufacturing	Nondurable Manufacturing	Transport	Trade	FIRE	Services	Government	Non-Agriculture
1987:1	286.4 0.8709	672.7 1.0117	546.3 0.987	284.3 0.9812	1650.7 0.975	774.2 0.9757	2,158.0 0.9752	1,398.0 0.9971	7,770.6 0.9788
1987:2	329.2 1.0011	662.9 0.997	553.7 1.0003	289.8 1.0004	1,688.4 0.9973	789.6 0.9951	2,214.3 1.0007	1,416.5 1.0102	7,944.5 1.0008
1987:3	354.4 1.0778	659.9 0.9925	556.4 1.0052	288.9 0.9971	1,698.3 1.0031	807.4 1.0175	2,221.6 1.0039	1,367.2 0.9751	7,954.2 1.002
1987:4	345.4 1.0502	664.0 0.9987	557.7 1.0075	295.9 1.0213	1,734.5 1.0245	802.8 1.0117	2,257.6 1.0202	1,426.8 1.0176	8,084.7 1.0184
1988:1	300.7 0.9145	657.6 0.989	542.4 0.9799	290.2 1.0016	1,679.4 0.992	796.8 1.0041	2,214.3 1.0006	1,433.1 1.0221	7,914.5 0.997
1988:2	342 1.04	665.9 1.0015	547.1 0.9883	296.0 1.0218	1,711.3 1.0108	797.9 1.0056	2,266.4 1.0242	1,450.5 1.0345	8,077.1 1.0175
1988:3	360.8 1.0972	666.7 1.0027	549.2 0.9921	297.8 1.0278	1720 1.016	799.3 1.0073	2,281.8 1.0311	1,396.9 0.9963	8,072.4 1.0169
1988:4	347.7 1.0574	669.3 1.0066	551.8 0.9969	304.1 1.0498	1,758.2 1.0385	795.2 1.0021	2,312.5 1.045	1,445.5 1.0310	8,184.4 1.031
1989:1	304.2 0.9251	656 0.9867	537.6 0.9711	299.7 1.0344	1699.8 1.0041	788.30 0.9934	2,,287.1 1.0335	1,450.1 1.0342	8,022.8 1.0106
1989:2	339.1 1.0313	652.5 0.9813	543.7 0.9821	308.1 1.0634	1,726.1 1.0195	794.3 1.0009	2,345.7 1.0600	1,465.5 1.0452	8,174.9 1.0298

Appendix Table A1 (Continued)

Quarterly Employment by Major Industry, its Ratio to the 1987 Average, and Measures of Variability
New York State, 1987–1989 (Thousands)

	Construction	Durable Manufacturing	Nondurable Manufacturing	Transport	Trade	FIRE	Services	Government	Non-Agriculture
1989:3	360	646.7	543.8	308.0	1,725.5	794.0	2,352.8	1,404.6	8,135.2
	1.0946	0.9726	0.9823	1.063	1.0192	1.0006	1.0632	1.0018	1.0248
1989:4	342.9	639.1	536.9	318.5	1,744.2	786.4	2,375.6	1,470	8,213.5
	1.0428	0.9612	0.9699	1.0994	1.0302	0.991	1.0735	1.0484	1.0346
Coefficient of Variation									
Levels	7.315	1.474	1.242	3.3	1.747	1.082	2.876	2.189	1.622
Logs	1.302	0.228	0.197	0.575	0.236	0.163	0.373	0.303	0.181
Variance of Detrended Log Employment Residuals (*10^3)									
	4.856	0.099	0.089	0.113	0.162	0.118	0.071	0.314	0.082

Source: Unpublished tabulations supplied by the New York State Department of Labor

Appendix Table A2

Means and Standard Deviations of Various Characteristics, First Quarter Observations in 1988–1989

	High Earnings Group		Medium Earnings Group		Low Earnings Group	
	1988	1989	1988	1989	1988	1989
Age 25–34	0.312	0.300	0.363	0.358	0.298	0.301
Age 35–44	0.280	0.283	0.233	0.241	0.196	0.204
Age 45–54	0.195	0.207	0.157	0.164	0.148	0.145
Age 55–64	0.148	0.147	0.115	0.116	0.120	0.117
Age 65	0.021	0.023	0.022	0.023	0.041	0.044
Male	0.741	0.737	0.637	0.628	0.438	0.452
Black	0.097	0.097	0.139	0.154	0.145	0.168
Hispanic	0.054	0.055	0.109	0.113	0.159	0.145
Other Race	0.018	0.016	0.020	0.020	0.038	0.036
9–11 Years of Education	0.106	0.095	0.140	0.129	0.201	0.197
12 Years of Education	0.353	0.375	0.412	0.432	0.420	0.442
13–15 Years of Education	0.204	0.188	0.208	0.194	0.160	0.145
16 Years of Education	0.142	0.138	0.088	0.087	0.038	0.039
17 Years of Education	0.144	0.153	0.054	0.070	0.021	0.031
Previous Industry						
Agriculture	0.023	0.016	0.015	0.014	0.010	0.008
Durable and Nondurable Manufacturing	0.300	0.292	0.316	0.282	0.345	0.316
Transport	0.071	0.074	0.067	0.069	0.046	0.047
Finance, Insurance, and Real Estate	0.129	0.101	0.105	0.095	0.050	0.047
Services and Trade	0.447	0.486	0.468	0.511	0.520	0.552
Government	0.019	0.017	0.024	0.023	0.025	0.027
Communication	0.012	0.014	0.005	0.006	0.003	0.003

Appendix Table A2 (Continued)

Means and Standard Deviations of Various Characteristics, First Quarter Observations in 1988–1989

	High Earnings Group		Medium Earnings Group		Low Earnings Group	
	1988	1989	1988	1989	1988	1989
<u>Sub-state Region</u>						
New York PMSA	0.201	0.203	0.172	0.180	0.151	0.149
Nassau-Suffolk PMSA	0.403	0.404	0.418	0.422	0.355	0.362
Albany, Schenectady, and Troy MSA	0.031	0.041	0.035	0.037	0.050	0.050
Poughkeepsie, Orange County, Binghamton, and Utica-Rome MSA	0.050	0.054	0.060	0.073	0.082	0.085
Buffalo MSA	0.081	0.081	0.084	0.079	0.114	0.116
Rochester MSA	0.052	0.049	0.051	0.044	0.063	0.058
Syracuse MSA	0.048	0.035	0.053	0.045	0.050	0.046
Initial Replacement Rate	0.284	0.269	0.444	0.422	0.500	0.499
	(0.072)	(0.068)	(0.033)	(0.031)	0.000	(0.003)
Replacement Rate		0.366		0.500		0.500
After Benefit Increase		(0.093)		(0.000)		(0.000)
Real Weekly Earnings	696.47	695.37	407.26	407.04	228.20	229.19
	(267.162)	(263.011)	(30.088)	(30.054)	(70.034)	(70.699)
Weeks Worked in Base Year	44.203	44.390	42.639	43.078	38.857	39.479
	(9.241)	(9.230)	(9.929)	(9.805)	(10.676)	(10.705)
N	17,878	19,173	12,884	13,615	52,699	57,678

Notes: Standard deviations are in parentheses. Only Q1 observations are included, those with missing demographics, no previous earnings, with weeks worked less than 20, real weekly earnings less than $80 ($84.24 in 1989), out of state observations and those in construction, with pension reductions are deleted. Initial Replacement Rate is defined as the ratio of the benefit amount in the first week of claim to the person's average weekly earnings.

DATA APPENDIX

Explanatory Variables Used in Hazard Models

Previous Weeks Worked: Number of weeks worked in the base year.

Average Weekly Earnings: Base year earnings divided by weeks worked in the base year. Base year earnings are the earnings in the last 52 weeks prior to the week of filing.

Week Spell Began: 13 indicator variables for the first 13 calendar weeks. Equals one if the individual's claim began in the particular week.

Age: Indicator variables for age 25–34, 35–44, 45–54, 55–64, and 65+.

Race: Indicator variables for black, Hispanic, and other non-white groups.

Education: Indicator variables for years of education 9–11, 12, 13–15, 16, and 17 or more.

Gender: Indicator variable for male.

Industry: Indicator variables for agriculture, durable manufacturing, nondurable manufacturing, transport, FIRE (finance, insurance and real estate), services, government, trade and communication. The reference industry is trade.

Sub-state Region: Indicator variables for New York City, Bronx, Suffern, Westchester, Long Island, Riverhead, AST (Albany, Schenectady and Troy), Kingston, Poughkeepsie, Monticello, Glens Falls, Plattsburg, Syracuse, Utica, Watertown, Binghamton, Buffalo, Rochester, Jamestown, and Elmira. The reference region is Jamestown.

High Earnings Group: Indicator variable for those whose real average weekly earnings (in 1988 dollars) are above $465.34.

Medium Earnings Group: Indicator variable for those whose real average weekly earnings (in 1988 dollars) are between $360 and $465.34.

Low Earnings Group: Indicator variable for those whose real average weekly earnings (in 1988 dollars) were between $80 and $360.

1989: Indicator variable for those who filed a claim in 1989 (the year of benefit increase).

WBA under Old Law: Amount of weekly benefits under the law prior to the increase. It is 50% of nominal average weekly earnings for those with nominal average weekly earnings between $80 and $360. For those with average weekly earnings over $360, it is $180.

WBA under New Law: Average amount of weekly benefits under the law after the increase assuming a 20 week spell beginning with the file date. Weekly benefit amount from the date of increase is 50% of nominal average weekly earnings for those with nominal average weekly earnings between $80 and $490. For those with average weekly earnings over $490, it is $245.

Calendar Week: Indicator variables for each calendar week the person is at risk in the person-week format of the data (38 × 2 variables).

Ln(Earnings) Spline: The positive part of the difference between Ln(Real Average Weekly Earnings) and the logarithm of each decile of real average weekly earnings in the sample (9 variables in addition to Ln(Earnings)).