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By

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ABSTRACT

THREE ESSAYS IN LABOR ECONOMICS

By

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The first of the three essays examines immigrant wage gaps from 1960 to 2000. Previous research has suggested that the U.S.-specific labor market skills of successive immigrant cohorts from declined from 1970 to 1990; that is, compared with earlier cohorts, recent cohorts started off with lower wages relative to natives and assimilated at slower rates. We argue that the decline in immigrant skills within country-of-origin groups is not supported by the long-term evidence. Analysis of US Census data from 1970 to 2000 suggests that the unexplained wage gap after ten years contradicts the hypothesis of declining skills for cohorts after 1965. More broadly, the unexplained wage gap should not be treated solely as an indicator of a change in immigrant cohort skills.

The second essay examines how benefit levels for unemployment insurance (UI) affect the duration of unemployment. Most research on the effects of UI on unemployment duration has been limited by the use of a censored measure of unemployment spells. This essay reexamines the impact of UI benefit levels on unemployment duration using a dataset that allows examination of actual unemployment spells. We find that while censoring concerns are legitimate, the main problem in estimating the impact of UI benefit generosity on the duration of UI benefit receipt and jobless duration is finding exogenous variation in UI weekly benefit amounts. Using a quasi-experimental difference-in-difference approach, we find that

the effect of benefit generosity on unemployment duration may be smaller than previously estimated.

The third essay considers how an increase in the potential duration of unemployment benefits affects the duration of unemployment. We examine the extent to which increasing the potential duration of unemployment benefits increases the length of unemployment spells using a national sample of workers who were laid off and claimed unemployment insurance (UI) benefits during the recession of the early 1990s. The research design takes advantage of changes in the potential duration of benefits that occurred due to the Emergency Unemployment Compensation Act of 1991. Our attempts to reconcile the disparate findings of existing research suggest that different econometric estimators can produce substantially different inferences about the effects of increased potential benefit duration. We also find that estimates of the effect of potential benefit duration on weeks of benefit receipt often bear little relation to the estimates of the effect of potential duration on weeks of joblessness.

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CHAPTER 1

EXAMINING IMMIGRANT WAGE GAPS, 1960-2000

Introduction

The ongoing immigration debate in the United States has highlighted again the importance of the skills and capabilities that immigrants bring with them. Chiswick's pioneering work on the earnings of immigrants to the United States indicated a low entry wage relative to natives, but rapid 'assimilation' as immigrant earnings grew quickly and eventually exceeded natives' earnings (Chiswick 1978). Borjas, by contrast, accounted for cohort effects, and revealed that Chiswick's cross-sectional estimates overestimated the rate at which immigrants reach parity with their native counterparts (Borjas 1985). Borjas has also suggested that immigrant cohorts from 1970 to 1990 have been decreasing in 'quality'; that is, they started off with lower wages relative to natives than previous cohorts, and have assimilated at slower rates than previous cohorts (Borjas 1985, 1995). These conclusions were drawn by examining the unexplained wage gap between cohorts. That is, any differences that were not explained by differences in age, education, or length of time in the United States were attributed to differences in the 'quality' or US-specific skills of recent immigrant cohorts.[1]

---

1 Chiswick and Lalonde and Topel have suggested that the decline stems from both declining U.S. specific-job-market skills within groups and from a shift in country of origin, with more immigration coming from less-skilled countries (Chiswick 1986, Lalonde and Topel 1991). Yuengert (1994) agreed with the latter, but found that the skills of Mexican immigrants actually increased after 1964. Funkhouser and Trejo (1995) and Barrett (1996) both accept the idea of declining skill since 1965, but find that the decline ended in the 1980s.

If immigrant quality or skill has not been decreasing since the immigration reforms of 1985, we might well rethink our opinions about immigration policy. I argue that the unexplained earnings gap does not support the idea of declining quality or skill. Using Borjas's methodology and U.S. Census data from 1970 to 2000, I find no evidence of an increasing unexplained earnings gap for more recent cohorts. This is consistent with other recent work suggesting that the low wages of recent immigrants are not due to declining skill.[2] I also find that the evidence of an increasing unexplained earnings gap from 1960 to 1970 is contradicted by looking at the same immigrant cohorts later in their immigration experience.

Theory

In a cross-section, it is typical to see recent immigrants earning lower wages than immigrants who arrived earlier. Some of this wage gap can be explained by differences in age, experience, and educational levels, but part is also explained by differences in the length of time each cohort has spent in the United States. Any wage difference not explained by differences in observable socio-economic characteristics or by length of time in the United States is the unexplained wage gap.

Figure 1 illustrates the wage gaps to be estimated below. In panel A, the lower line shows the average predicted earnings of the 1980 cohort at various times since immigration, holding socio-economic characteristics constant. The upper line does the

---

2 Butcher and DiNardo (2002) find that changes in the wage structure are responsible for lower relative wages of more recent immigrants, not a decline in skills. Smith (2006) finds that the lower relative wages of more recent immigrants are explained entirely by changes in the wage distribution and the returns to skill, rather than a decrease in immigrant skill by cohort.

same for the 1970 cohort. The cross-sectional gap between the 1970 and 1980 cohorts observed in the year 2000 is the difference in predicted earnings for the two cohorts after controlling for socio-economic characteristics ($\hat{y}_{2000,1970} - \hat{y}_{2000,1980}$, where the first subscript denotes the year in which earnings are observed, and the second subscript denotes the immigrant cohort). This cross-sectional gap can be broken into two parts. The first is the growth of earnings for the 1970 cohort in during the 1990s ($\hat{y}_{2000,1970} - \hat{y}_{1990,1970}$). In 2000, the 1970 cohort had been in the United States ten years longer than the 1980 cohort, and the earnings growth they experienced in those extra ten years explains part of the cross-sectional gap. The unexplained wage gap is the difference between the predicted earnings of the cohorts at the same point in time since immigration ($\hat{y}_{1990,1970} - \hat{y}_{2000,1980}$). Specifically, the unexplained wage gap shown in panel A is the difference in average predicted earnings of the 1970 cohort after 20 years in the United States (observed in 1990) and the average predicted earnings of the 1980 cohort at after 20 years in the United States (observed in 2000).

The predicted wage lines in panel A indicate a positive unexplained earnings gap between the 1970 and 1980 cohorts, indicating that something unobserved allowed the 1970 cohort to have higher predicted earnings than the 1980 cohort. What does this mean? Borjas (1985, 1995) found a positive unexplained earnings gap between the 1970 and 1980 cohorts, and attributed this to the higher relative quality or skill of the 1970 cohort. An alternative explanation could be that changes in the wage structure are responsible for lower relative wages of more recent immigrants, (Butcher and DiNardo (2002), Smith (2006)). A positive gap could also be due to factors on the demand side of the US labor market. For example, if demand for recent immigrant labor were slack or if US employers were less willing to hire recent immigrants in 1980 than in 1970, the unexplained earnings gap would again be positive.

Panel B shows a negative unexplained earnings gap, which would indicate that some unobserved factor led the 1970 cohort to have lower earnings than the 1980 cohort. A negative gap could be explained by an increase in quality or skills, by an increasing willingness of US employers to hire new immigrants. It would also be possible that recent immigrants are benefiting from having and easier path to assimilation, thanks to the efforts of and help from previous cohorts[3].

Model

We now look at how to estimate the unexplained wage gap for various immigrant cohorts following Borjas (1985). We start by estimating a cross-sectional earnings function for each census year. Using a sample of immigrants observed in 1990, we have:[4]

$$\begin{aligned}\ln(earnings_{1990}) = X_{1990}\gamma_{1990} + \alpha_{85}D_{85} \\ + \alpha_{80}D_{80} + \alpha_{75}D_{75} + \alpha_{70}D_{70} \\ + \alpha_{65}D_{65} + \alpha_{60}D_{60} + \alpha_{50}D_{50} \\ + \alpha_{40}D_{40} + \varepsilon_{1990}\end{aligned} \tag{1}$$

3 See Chiswick and Miller (forthcoming).

4 For simplicity, I confine the explanation in this section to the use of 1990 and 2000 data, but the idea is the same for any two census years.

and using a sample of immigrants in 2000, we have:

$$\begin{aligned}\ln(earnings_{2000}) = {} & X_{2000}\gamma_{2000} + \beta_{95}D_{95} \\ & + \beta_{90}D_{90} + \beta_{85}D_{85} + \beta_{80}D_{80} \\ & + \beta_{75}D_{75} + \beta_{70}D_{70} + \beta_{65}D_{65} \\ & + \beta_{60}D_{60} + \beta_{50}D_{50} + \beta_{40}D_{40} \\ & + \varepsilon_{1990}\end{aligned} \tag{2}$$

where $\ln(earnings)$ denotes the natural logarithm of the yearly earnings in tens of dollars, X denotes the socio-economic characteristics of the individual (no constant term included), and the D_k are dummy variables denoting the immigrant cohort; that is, the time period during which immigration occurred.[5]

In order to decompose the cross-sectional earnings gap, we need to construct three sets of predicted earnings. The first two are the predicted earnings of an average immigrant for each of the cohorts in each of the two census years.

$$\hat{y}_{1990,k} = \bar{X}_{2000,k}\hat{\gamma}_{1990} + \hat{\alpha}_k \tag{3}$$

$$\hat{y}_{2000,k} = \bar{X}_{2000,k}\hat{\gamma}_{2000} + \hat{\beta}_k \tag{4}$$

---

5 Each D_k indicates immigration in a particular time period: D_{95} indicates 1995-99, D_{90} indicates 1990-94, D_{85} indicates 1985-89, D_{80} indicates 1980-84, D_{75} indicates 1975-79, D_{70} indicates 1970-74, D_{65} indicates 1965-69, D_{60} indicates 1960-64, D_{50} indicates 1950-59, and D_{40} indicates the individual immigrated before 1950. Notice that the intervals are not all of equal length. These differences arise due to the manner in which year of immigration information is coded in the Census, not from any theoretical concerns.

where k indexes the cohort (or five-year period during which immigration occurred), and $\bar{X}_{2000,k}$ denotes the average values of X for immigrant cohort k in 2000.[6] Hence, $\hat{y}_{1990,k}$ and $\hat{y}_{2000,k}$ give the predicted 1990 and 2000 earnings for the average immigrant in cohort k.

The third equation needed for the decomposition is the predicted earnings in 2000 for the average immigrant from cohort k if they had immigrated ten years later:

$$\hat{y}_{2000,k+10} = \bar{X}_{2000,k}\hat{\gamma}_{2000} + \hat{\beta}_{k+10} \tag{5}$$

The year 2000 cross-sectional earnings gap between cohort k immigrants and cohort k+10 immigrants is simply the difference in earnings for an individual who immigrated in period k and an observationally similar individual who immigrated ten years later. This gap is illustrated in Figure 1 for the 1970 (k) and 1980 (k+10) cohorts.

This cross-sectional gap can be broken into two parts:

$$\begin{aligned} \hat{y}_{2000,k} - \hat{y}_{2000,k+10} &\\ &= (\hat{y}_{2000,k} - \hat{y}_{1990,k}) \\ &+ (\hat{y}_{1990,k} - \hat{y}_{2000,k+10}) \end{aligned} \tag{6}$$

The first term on the right is cohort k's earnings growth during the 1990s, or the 'within-cohort growth.' The second term is the unexplained earnings gap, or the

6 Using the average values of X from the latter census year (2000 in this case) is an arbitrary decision. The average values of 1990 would be no less (or more) appropriate. All that is required for our purposes is that the predicted earnings in equations (3) and (4) be based on the same values of the vector X.

difference in earnings between immigrants from different cohorts but with the same length of time in the US (observed in two different census years). A positive unexplained earnings gap suggests higher wages for the earlier cohort.

The decomposition in equation (6) does not take into account changes in economic conditions that may have occurred over the decade. For example, if wages generally increased during the decade, the estimated within-cohort growth will overstate the wage growth that would otherwise have occurred, and the unexplained earnings gap will understate the differences in human capital between the two immigrant cohorts. Borjas (1985) suggests handling this problem by examining the earnings of immigrants in relation to those of natives – that is, by normalizing with respect to native earnings. Again, we begin with two cross-sectional regressions. The first uses a sample of natives in 1990:

$$\ln(earnings_{1990,n}) = X_{1990,n}\delta_{1990} + \alpha_n + \eta_{1990} \tag{7}$$

and a second using a sample of natives in 2000:

$$\ln(earnings_{2000,n}) = X_{2000,n}\delta_{2000} + \beta_n + \eta_{2000} \tag{8}$$

where n indicates a native of the United States, and η is the error term. We then substitute the average characteristics of immigrant cohort k into these estimated native earnings structures:

$$\hat{y}_{1990,k,n} = \bar{X}_{2000,k}\hat{\delta}_{1990} + \hat{\alpha}_n \tag{9}$$

$$\hat{y}_{1990,k,n} = \bar{X}_{2000,k}\hat{\delta}_{2000} + \hat{\beta}_n \tag{10}$$

Equations (9) and (10) simulate the 1990 and 2000 earnings of a native who had the average characteristics of immigrants in cohort k. We can now decompose the cross-section growth (5) in a new way:

$$\begin{aligned}\hat{y}_{2000,k} - \hat{y}_{2000,k+10} \\ &= [(\hat{y}_{2000,k} - \hat{y}_{2000,k,n}) \\ &- (\hat{y}_{1990,k} - \hat{y}_{1990,k,n})] \\ &+ [(\hat{y}_{1990,k} - \hat{y}_{1990,k,n}) \\ &- (\hat{y}_{2000,k+10} - \hat{y}_{2000,k,n})]\end{aligned} \tag{11}$$

The first term on the right is the growth of earnings *relative to natives* for immigrant cohort k over the 1990s. The second term is the unexplained earnings gap, or the predicted difference in immigrant earnings, *relative to natives*, in 1990 and 2000, holding constant years since immigration.

Data

The data come from the U.S. Census of Population, Public Use Micro Sample 1970 (F1 Metro), 1980 (5% State), 1990 (5% State sample), and 2000 (1% sample). In each set of comparisons, the earlier data contain only males in the age range 18-54, and the later data contain only males aged 28-64. So there will be within/across-cohort effects for 1970 to 1980, 1980 to 1990, and 1990 to 2000. (The estimates for 1970 to

1980 have already been published in Borjas 1985.)[7]

Individuals living in group quarters, part of the US armed forces, or self-employed at the time of the Census are excluded from the analysis. As in Borjas (1985), the data are divided into six mutually exclusive groups: Cuban, Mexican, Other Hispanic, Asian, with the remainder classified as either White or Black. (In particular, the Census asks a question about 'Hispanic origin' and a separate question about 'race'. If a respondent indicates Other Hispanic to the first question and White to the second, they are designated as Other Hispanic in this study.) Each group is subdivided into two categories: immigrant or native. Table 1 contains the number of observations for each of these groups in each of the four census years. A few of the groups are quite small, particularly for the pre-1950 cohort. The number of Cuban immigrants in the 1980-85 cohort is very large relative to other Cuban cohorts, corresponding to the Mariel Boatlift when Castro allowed Cubans to freely leave during the summer of 1980.

The socio-economic variables are defined as follows:

Education (*edu*) = Years of completed schooling.

Experience (*exper*) = Age minus years of education minus 6.

Marital Status (*marr*) = 1 if married with spouse present, 0 otherwise.

SMSA (*SMSA*) = 1 if resides in a metropolitan area, 0 otherwise.

Work disabilities (*hlwth*) = 1 if disability limits work, 0 otherwise.

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7. Going back further is not possible, as earlier censuses do not ask for year of immigration. However, by combining census questions on nativity and migration within the last five years, it is possible to repeat this analysis for recent, or 'short-time' immigrants for 1940, 1960, 1970, 1980, 1990 and 2000. The data for these additional years would come from the 1940 General, and 1960 General P.U.M.S..

Findings

Table 2 presents the estimated coefficients of the years-since-migration variables obtained from the 2000 cross-section for each of the six groups of immigrants (the comparison group is the 1995-99 cohort). At the 5% level, all coefficients are significant. Each coefficient in the table shows the log difference in earnings between an immigrant cohort and the most recent cohort in the same group. For example, White immigrants who came in the early 1990s have 46.93% percent higher earnings than White immigrants who came in the late 1990s. This differential increases up to 90.5% for White immigrants who came in the early 1970s.

Table 3 shows estimates of the within-cohort growth and the unexplained earnings gap from the decomposition in equation 11. The figures in the cross-section column are the difference in regression-adjusted year 2000 earnings between each immigrant cohort and the same-ethnicity immigrant cohort arriving ten years later ($\hat{y}_{2000,k} - \hat{y}_{2000,k+10}$, where k is the cohort listed in the first column). The within-cohort column shows the estimated earnings growth of cohort k over the 1990s, again relative to the same-ethnicity immigrant cohort that arrived ten years later -- $(\hat{y}_{2000,k} - \hat{y}_{2000,k,n})-(\hat{y}_{1990,k} - \hat{y}_{1990,k,n})$. The unexplained earnings gap column shows the estimated difference in relative earnings between each cohort and the same-ethnicity cohort arriving ten years later, holding constant years since immigration ($[(\hat{y}_{1990,k} - \hat{y}_{1990,k,n}) - (\hat{y}_{2000,k+10} - \hat{y}_{2000,k,n})]$). These unexplained earnings gaps are generally negative, indicating higher predicted earnings for more recent cohorts.

For example, the Asian 1975-79 cohort has a cross-sectional earnings gap of 0.277, meaning that earnings in 2000 for the 1975-79 cohort were 27.7% higher than the earnings of the Asian cohort arriving 10 years later. This gap has two sources: First, the 64.3% increase in earnings for the 1975-79 cohort over the 1990s, (as seen in the within-cohort estimate), and second, the unexplained earnings gap of -0.367, meaning the 1975-79 cohort of Asian immigrants did 36.7% worse than the cohort arriving ten years later, holding years since immigration constant. If we interpret the unexplained earnings gap as reflecting differences in skill, the skills of Asian immigrants increased between 1975-1979 and 1985-1989.

In general, the findings reported in Table 3 suggest that the skills of immigrants have increased since the 1970s. While the positive unexplained earnings gaps for the earliest cohorts (1950-59 as well as 1960-64 for some groups) are consistent with falling skills during the 1960s and early 1970s, the large negative unexplained earnings gaps for all other cohorts contradict such an interpretation.[8]

Tables 4 and 5 show the results for the 1980-to-1990 and 1970-to-1980 comparisons. Note that in the 1970-to-1980 comparison (Table 5), the unexplained earnings gaps are positive or close to zero. This is consistent with Borjas' findings for the same census years, and could be interpreted as an indication of lower skill for cohorts

8 The positive unexplained earnings gap for the 1970-74 Cuban cohort appears to be an artifact of the aforementioned Mariel boatlift and its effect on the 1980-84 Cuban cohort.

since the late 1960s.[9] The compositions in Tables 3 and 4, however, tend to overturn this finding. In fact, the unexplained earnings gap for most of the cohorts is not constant as years since immigration increase. Consider the unexplained earnings gap for the Black 1965-69 cohort. In Table 5 their unexplained earnings gap is .289, suggesting they earned 28.9% more in their first 5 years in the U.S. than Black immigrants who arrived ten years later. In Table 4, however, the Black 1965-69 cohort earned 36.0% *less* than the Black cohort that arrived ten years later did 10-to-15 years into theirs. Finally, in Table 3, the Black 1965-69 cohort earned 21.4% less in years 20-25 than the cohort that arrived ten years later, but is not significant at the 5% level. The changing sign and magnitude of the unexplained earnings gap contradicts the idea that the unexplained wage gap is measuring changes in cohort quality or skill. In each case, we are comparing the 1965-69 cohort with the 1975-79 cohort, yet the unexplained wage gap is changing. To maintain that these gaps all indicate differences in the innate quality or U.S. specific skills of these cohorts seems nonsensical.

Clearly, the unexplained earnings gap is changing over the immigration experience. Comparing two cohorts 5 and 15 years after immigration may yield different results than a comparison of the same cohorts 25 and 35 years after immigration. This can be seen in Figures 2-7, which show the earnings (relative to

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[9] Table 7 offers a direct comparison with Borjas' 1970-to-1980 results. Of the 18 sets of estimates being compared, 4 are positive and significant in both studies, 3 are positive in my findings but insignificant in Borjas', 3 are positive in Borjas' findings, but insignificant in mine, and 8 are insignificant in both studies, The magnitudes of estimates are not entirely similar, though the story of declining immigrant skill is supported by both. Unfortunately, the programming code from the original study by Borjas is no longer available.

natives) of the various cohorts of immigrants at the same points in their immigration experience. Consider, for example, Figure 2, which shows the relative earnings for White cohorts. Each curve represents the trend of immigrant cohort earnings implied by the unexplained earnings gaps estimated above (Tables 3-5). For example, the "0-to-5" curve shows the earnings of various cohorts during their first five years in the United States. The unexplained earnings gap from the 1970-1980 comparison (Table 5) suggests the White cohort that arrived in 1975-79 earned 60% less in their first five years than the 1965-69 cohort did in their first five years. Thus, earnings in the first five years since immigration decreases from 1 (normalized) for the 1965-69 cohort to a nadir of 0.40 for the 1975-79 cohort. Continuing in the same fashion, the unexplained earnings gap for the 1975-79 cohort in the 1980-1990 comparison (Table 4) suggests 33% higher earnings for the 1985-89 cohort, or 0.53. Likewise, the unexplained earnings gap for the 1985-89 cohort in the 1990-2000 comparison (Table 3) shows 75% higher earnings for the 1995-99 cohort, or 0.93. Each of the curves is constructed in a similar manner, and allows us to see how different cohorts compare to each other holding years since immigration constant.

In comparing the cohorts in their first five years after immigration, we see evidence consistent with a skill decline for the late 1970s cohort. And while their earnings relative to natives is increasing, cohorts in the late 1980s and 1990s still have lower relative earnings than the late 1960s cohort in their first five years. Looking at the same cohorts 10-to-20 years after immigration, however, shows no evidence of a post 1960s skill decline.

Figure 2 also shows where the information available to Borjas ended. For earnings in the first five years after immigration, the last information reported in Borjas (1985) pertains to the 1975-79 cohort, indicated by the letter B by that profile. Similarly, Borjas' data about earnings 5-to-10 after immigration ended with the 1970-74 cohort, and data about earnings after 10-to-20 years ended with the 1960-64 cohort.[10] Looking only at the portions of the curves available to Borjas (1985), it is easy to see evidence of a skill decline since the late 1960s, with significant declines in relative earnings both 0- 5 and 5-10 years after immigration. When looking at data through the year 2000, however, the story changes. The declines for the 1970s cohorts during their first ten years are erased after that first decade. Indeed, looking at relative earnings after ten years would be consistent with *higher* immigrant skills. Additionally, subsequent cohorts see increases over the 1970s cohorts in all years since immigration.

The same findings hold for the other immigrant groups, with the exception of the Cuban 1965-69 cohort, which appears to do better than the 1975-79 cohort at all points in their experience (Figure 6). I suspect this is the effect of the Mariel boatlift, with a large decrease in relative earnings for the group arriving just before the 1980 census. The estimates for these Cuban cohorts actually result in a negative relative earnings; this cannot be taken literally, of course, but it does indicate a substantial decrease in relative earnings for this cohort.

10 Borjas (1985) contains no information on earnings 20 or more years after immigration.

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Conclusions

The unexplained wage gap between two cohorts generally varies over time. In particular, more recent cohorts have lower relative wages in their first 5-10 years, but higher relative wages afterward. The higher relative wages after 10 years contradict the idea of a skill decline for cohorts after 1965. The broader point, however, is that the changes in unexplained wage gaps indicate that these gaps should not be considered purely as indications of cohort skill.

Using the method developed by Borjas (1985) and Census data from 1970 to 2000, I reconfirm that the relative earnings of new immigrants (that is, those within five years of arriving in the United States) were lower than those of earlier immigrants in the 1970s and 1980s. . The relative earnings after five years, however, are roughly equal to earlier cohorts, and even higher for some groups. After ten years, immigrant who arrived during the 1990s show higher relative earnings than earlier immigrant cohorts at the same point in their immigration experience. Immigrants arriving in the 1990s had higher relative earnings than any other immigrant cohort in the sample, even 0-to-5 years into their immigration experience.

If we are to interpret the unexplained earnings gap as an indicator of skill, the long-term evidence shows that after ten years in the country, each cohort has *higher* U.S.-specific job-market skill than the one that preceded it. Overall, the evidence does not support the conclusion that the skills of successive immigrant cohorts have been declining. Rather, although the 1970s cohorts do seem to have had lower skills compared to earlier cohorts, but this alleged skill gap was eliminated in less than ten years.

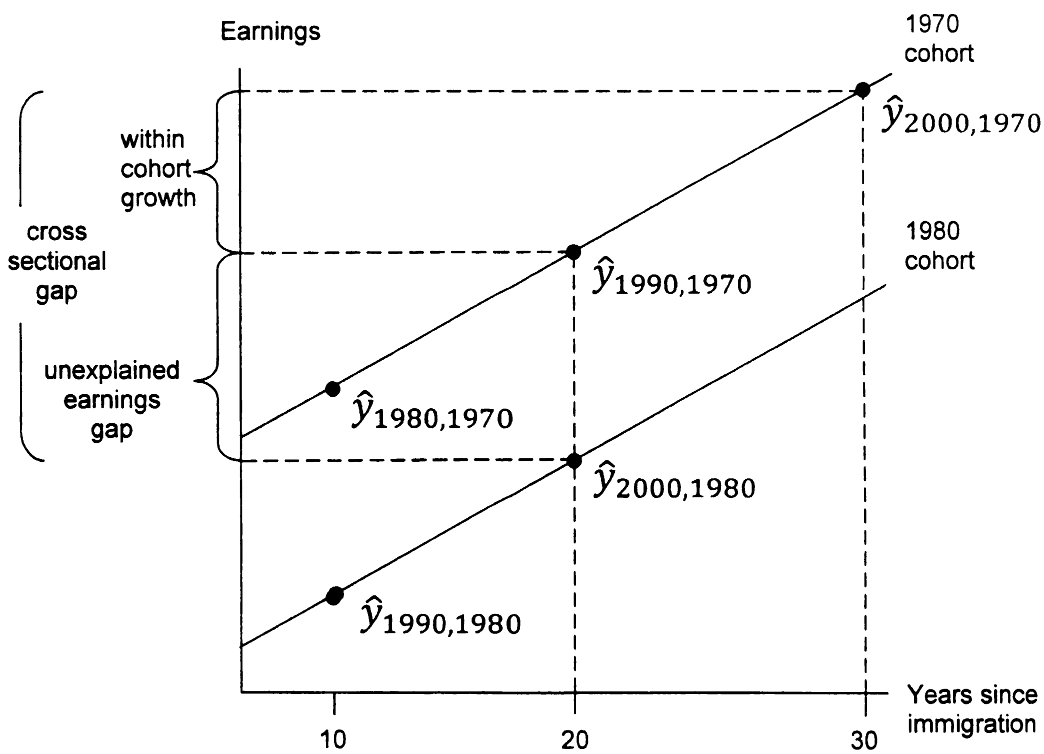
The results could be affected by patterns of return migration that differed by skill for different cohorts. That is, if higher skilled immigrants from earlier cohorts left the United States more frequently than higher skilled immigrants from more recent cohorts, earlier cohorts would appear less skilled than they were at entry, especially late into their immigration experience. Without data on return migration rates by skill level, it is not possible to determine if there is such variation by skill.

More broadly, the unexplained earnings gap should not be considered synonymous with differences U.S.-specific job-market skills. It is theoretically possible that demand for immigrant labor has increased over the decade, increasing the size of the unexplained wage gaps of more recent immigrants. This would require an increase in the demand specifically for immigrant labor that exceeded any increase in demand for native labor. Other explanations include changes in the acceptance of recent immigrants by employers, unmeasured differences in educational quality, assimilation assistance by earlier cohorts, or a combination of these factors.

In fact, even after seeing the estimates of negative unexplained wage gaps, it is still possible that immigrant skills actually are in decline, but these other factors have offset the skill decline. It simply is not possible to determine the true trend in immigrant skills using the unexplained wage gap alone.

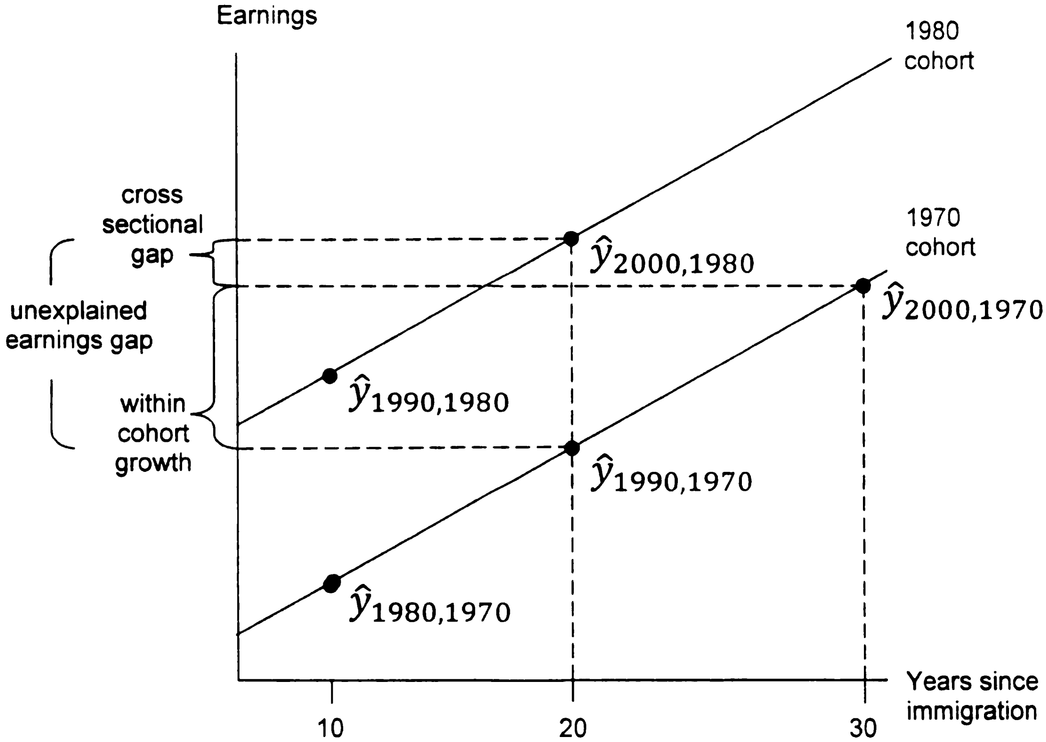
Figure 1. Decomposition of the earnings gap between two immigrant cohorts

A. A positive unexplained earnings gap



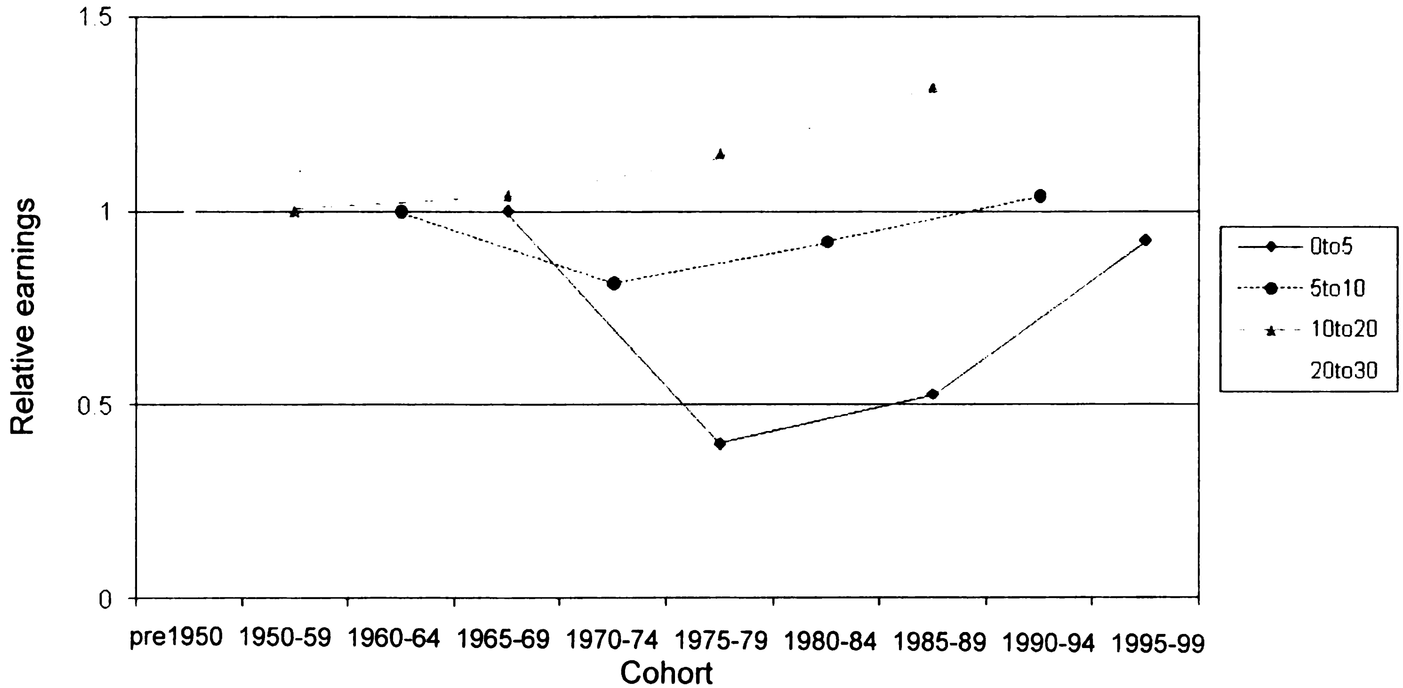
Notes: Illustration of the decomposition in equation (6). Each $\hat{y}_{t,k}$ is the earnings in year t for cohort k. For example, $\hat{y}_{1990,1970}$ is the 1990 earnings of the 1970 immigrant cohort.

B. A negative unexplained earnings gap.



Notes: Illustration of the decomposition in equation (6). Each $\hat{y}_{t,k}$ is the earnings in year t for cohort k. For example, $\hat{y}_{1990,1970}$ is the 1990 earnings of the 1970 immigrant cohort.

Figure 2. Relative earnings: White Cohorts



Notes to Figures 2-7: Each curve represents the trend of immigrant cohort skill measured at the various points in their immigration experience. Each curve has been normalized to begin with a relative earnings of 1. The unexplained wage gaps from Tables 3-5 are then used to calculate the relative earnings for later cohorts. The B represents the last data point observable using only the 1970 and 1980 Census data for any given series. (It is in bold for the "0 to 5" curve and in light gray for the "10 to 15" curve.)

Figure 3. Relative earnings: Black Cohorts

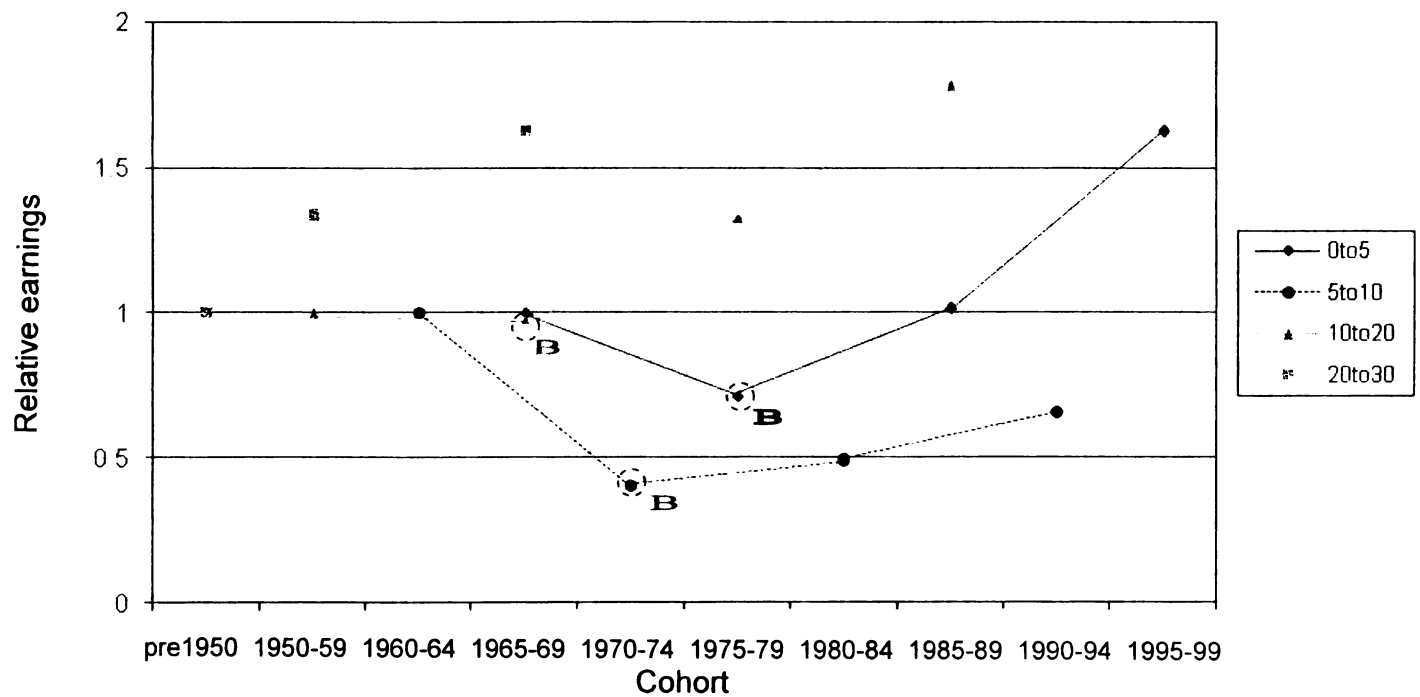


Figure 4. Relative earnings: Asian Cohorts

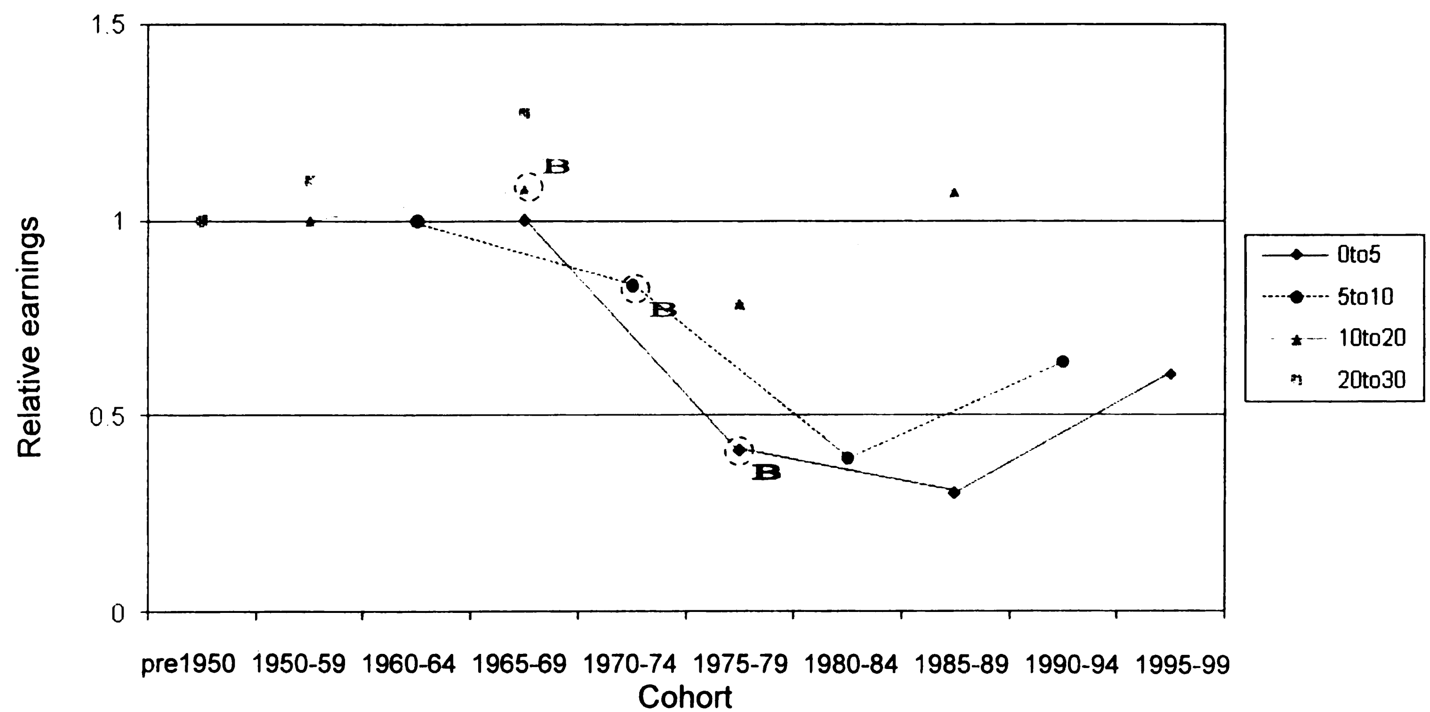


Figure 5. Relative earnings: Mexican Cohorts

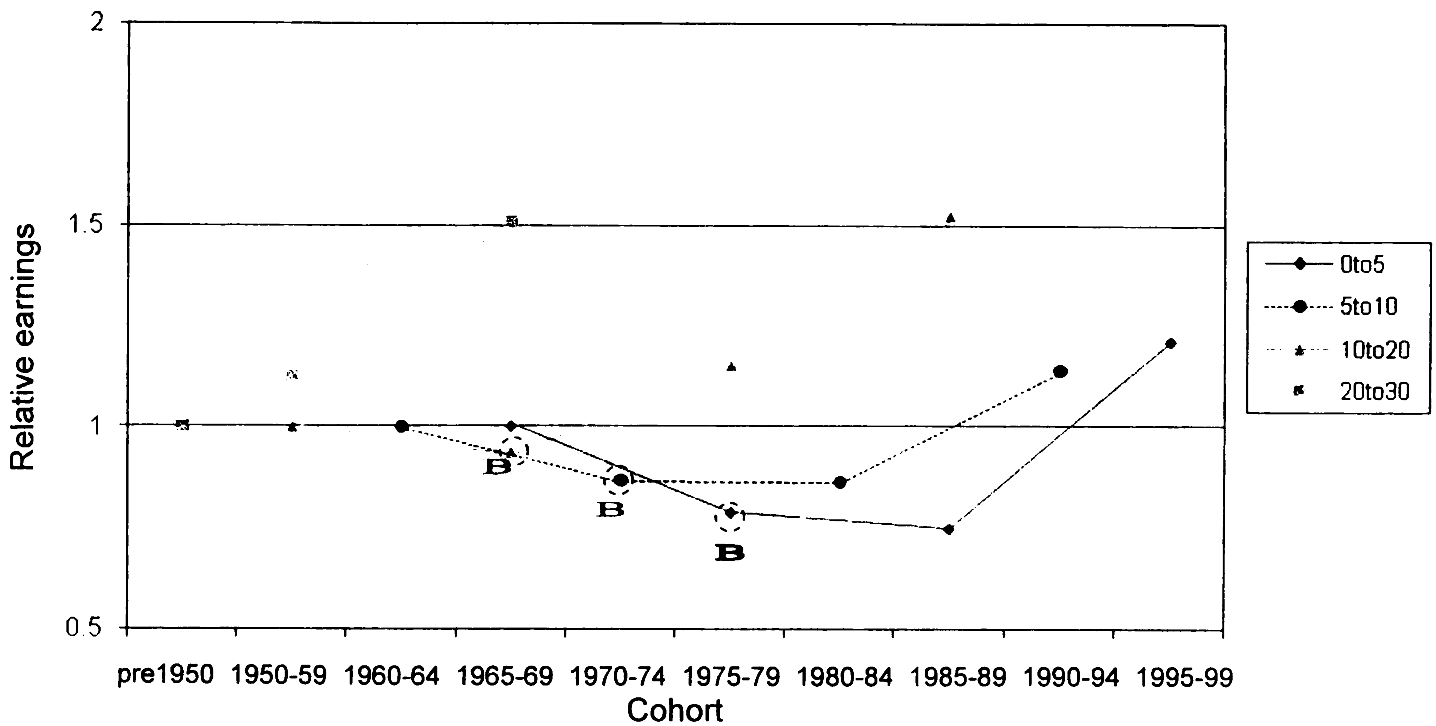


Figure 6. Relative earnings: Cuban Cohorts

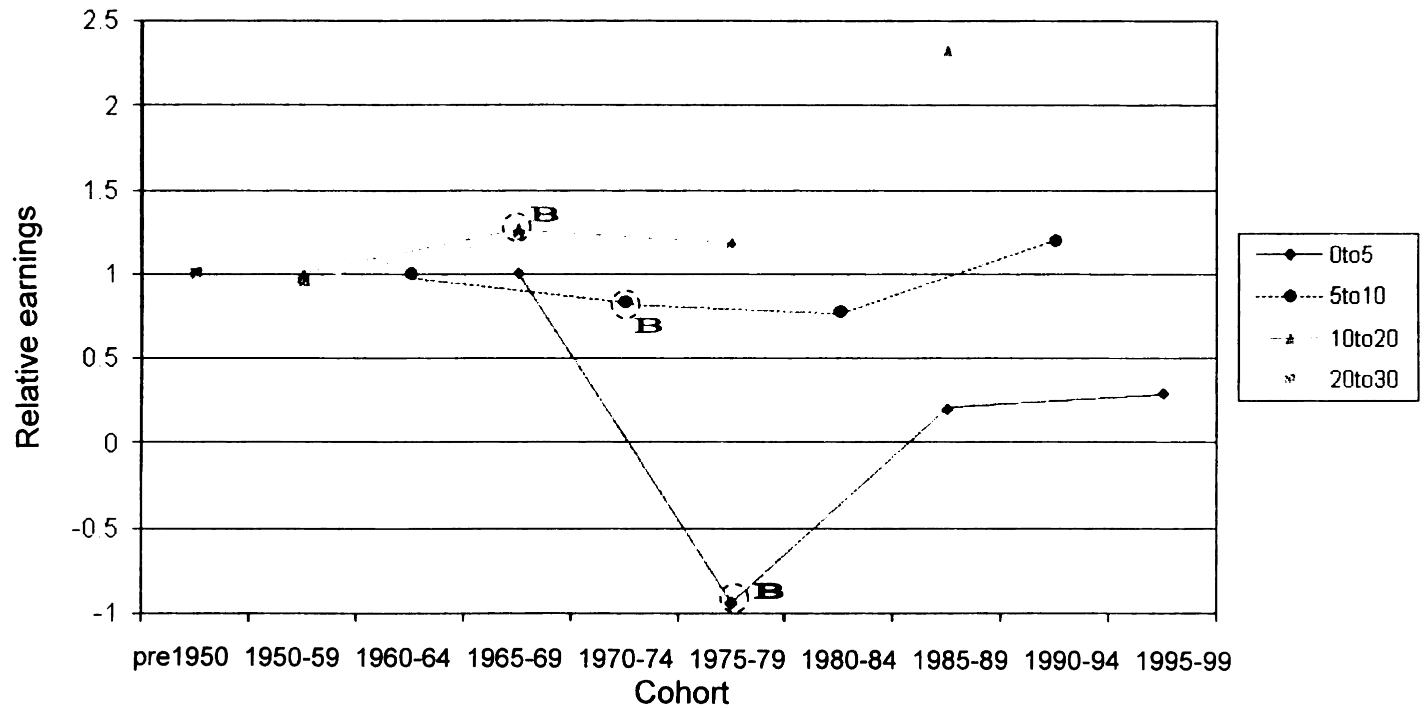


Figure 7. Relative earnings: Other Hispanic Cohorts

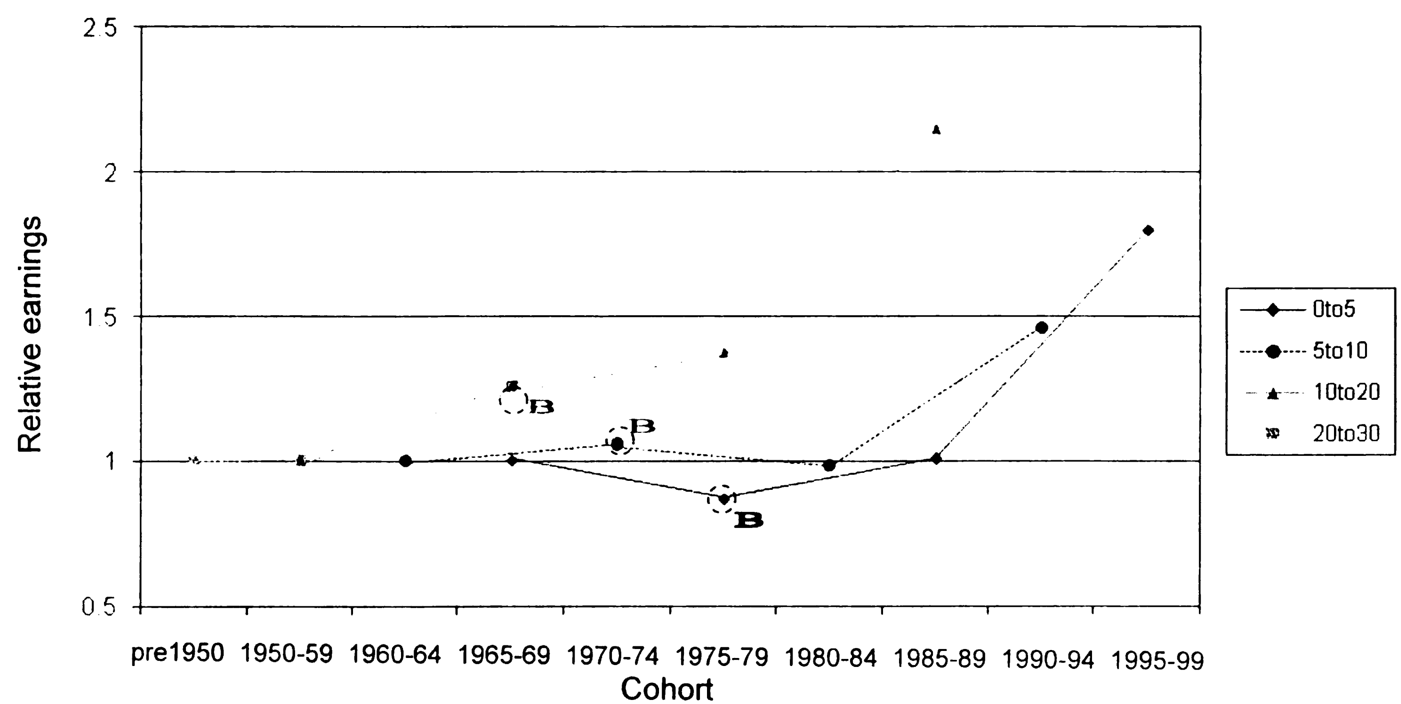


Table 1
Number of Observations by Country-of-Origin Groups and Census Year

		Census year			
		2000	1990	1980	1970
White:	native	393,444	1,810,908	1,843,576	303,926
	immigrant	17,262	71,104	60,248	10,753
Black:	native	59,462	213,315	262,768	38,121
	immigrant	7,746	14,455	11,913	730
Asian:	native	3,015	12,247	12,830	1,777
	immigrant	16,772	49,913	27,338	1,205
Mexican:	native	13,144	55,934	62,136	6,508
	immigrant	23,372	54,514	37,117	2,158
Cuban:	native	423	882	1,041	75
	immigrant	2,133	9,415	7,943	1,141
Other Hispanic:	native	7,645	17,815	37,557	6,496
	immigrant	14,277	41,021	18,730	1,463
Total:	native	477,133	2,111,101	2,219,908	356,903
	immigrant	81,562	240,422	163,289	17,450

Source: U.S. Census of Population, Public Use Micro Sample 1970 (F1 Metro), 1980 (5% State), 1990 (5% State sample), and 2000 (1% sample).

Table 2
Coefficient Estimates of Years-since-migration Variables - 2000 Cross Section

	Country of Origin Group					
	White	Black	Asian	Mexican	Cuban	Other Hispanic
D40	0.578	1.144	0.654	-0.152	0.724	-0.270
D50	0.800	0.942	0.741	-0.061	0.620	0.359
D60	0.814	0.853	0.963	0.439	0.985	0.231
D65	0.849	0.935	0.986	0.479	0.765	0.196
D70	0.905	1.119	0.997	0.664	1.001	0.583
D75	0.880	1.155	0.969	0.630	0.810	0.574
D80	0.813	0.898	0.816	0.588	0.324	0.646
D85	0.638	0.976	0.692	0.474	0.889	0.620
D90	0.469	0.812	0.632	0.330	0.517	0.504

Notes:

1. Estimates of the β_K from estimation of equation (2) for immigrants from the 2000 Census of Population. Coefficients represent the difference in predicted ln(earnings) associated with being in a particular cohort, relative to the 2000 cohort of the same country-of-origin group. All coefficients are significant at the 5% level. Additional controls included in the estimated equations are education, experience, marital status, SMSA, and work-limiting disability.

Table 3

Decomposition of Relative Earnings, 1990 to 2000 comparison

	Cross-Section		Within-Cohort		Unexplained Gap	
	coef.	t	coef.	t	coef.	t
White						
1950-59	-0.014	-0.17	**-0.304**	-5.17	**0.290**	4.15
1960-64	-0.091	-0.92	**-0.262**	-3.75	**0.171**	2.14
1965-69	-0.032	-0.39	-0.066	-1.12	0.034	0.50
1970-74	0.092	0.92	**0.429**	5.48	**-0.337**	-4.74
1975-79	**0.243**	2.79	**0.390**	5.78	**-0.148**	-2.26
1980-84	**0.344**	4.04	**0.472**	6.68	**-0.129**	-2.16
1985-89	**0.638**	8.20	**1.389**	21.76	**-0.751**	-13.83
Black						
1950-59	0.089	0.31	0.082	0.32	0.007	0.03
1960-64	-0.266	-1.24	-0.253	-1.21	-0.013	-0.08
1965-69	-0.220	-1.59	-0.006	-0.05	-0.214	-1.83
1970-74	0.221	1.64	**0.538**	4.27	**-0.317**	-3.35
1975-79	0.179	1.50	**0.515**	4.81	**-0.337**	-3.51
1980-84	0.086	0.79	**0.419**	4.72	**-0.333**	-3.59
1985-89	**0.976**	9.12	**1.577**	17.37	**-0.601**	-6.60
Asian						
1950-59	-0.222	-0.90	-0.411	-1.87	0.189	1.04
1960-64	-0.034	-0.20	-0.038	-0.22	0.004	0.03
1965-69	0.017	0.17	0.172	1.61	-0.155	-1.73
1970-74	**0.180**	1.99	**0.417**	4.16	**-0.237**	-3.00
1975-79	**0.277**	3.79	**0.644**	7.88	**-0.367**	-4.69
1980-84	**0.184**	2.82	**0.815**	10.93	**-0.630**	-8.33
1985-89	**0.692**	10.12	**1.700**	22.31	**-1.008**	-13.21

Table 3 (continued)

	Cross-Section		Within-Cohort		Unexplained Gap	
	coef.	t	coef.	t	coef.	t
Mexican						
1950-59	**-0.500**	-3.29	**-0.627**	-4.82	0.127	1.13
1960-64	**-0.225**	-2.00	-0.069	-0.63	**-0.156**	-2.03
1965-69	**-0.151**	-2.07	**0.193**	2.73	**-0.344**	-5.54
1970-74	0.076	1.10	**0.514**	7.82	**-0.438**	-8.12
1975-79	**0.156**	2.77	**0.479**	8.73	**-0.323**	-6.49
1980-84	**0.258**	4.34	**0.577**	11.29	**-0.319**	-5.66
1985-89	**0.474**	8.69	**1.099**	23.30	**-0.625**	-11.48
Cuban						
1950-59	-0.365	-1.09	-0.636	-1.81	0.270	1.11
1960-64	-0.016	-0.06	-0.174	-0.79	0.158	0.55
1965-69	-0.045	-0.12	0.238	1.14	-0.283	-0.74
1970-74	**0.678**	2.51	0.445	1.58	0.233	1.08
1975-79	-0.079	-0.19	**0.875**	2.22	**-0.954**	-3.06
1980-84	-0.193	-0.85	0.357	1.71	**-0.550**	-2.28
1985-89	**0.889**	3.12	**1.336**	4.45	**-0.447**	-1.98
Other Hispanic						
1950-59	0.128	0.81	-0.099	-0.52	0.227	1.17
1960-64	**-0.351**	-2.37	-0.150	-0.78	-0.201	-1.10
1965-69	**-0.378**	-3.63	-0.120	-0.71	-0.258	-1.51
1970-74	-0.064	-0.58	**0.361**	2.02	**-0.425**	-2.61
1975-79	-0.046	-0.51	**0.517**	3.06	**-0.563**	-3.49
1980-84	0.143	1.75	**0.628**	3.91	**-0.485**	-2.99
1985-89	**0.620**	7.51	**1.414**	8.90	**-0.794**	-4.84

Notes:

1. Decompositions are based on equation (11). Bold indicates significance at the 5% level. As in equation 11, a positive unexplained earnings gap indicates higher relative earnings than the cohort arriving ten years later, holding years since immigration constant. Positive within-cohort terms simply indicate higher earnings for the cohort over the decade.

Table 4
Decomposition of Relative Earnings, 1980 to 1990 comparison

		Cross-Section		Within-Cohort		Unexplained Gap	
		coef.	t	coef.	t	coef.	t
White							
	1950-59	-0.021	-0.74	**0.085**	3.30	**-0.106**	-3.49
	1960-64	0.058	1.57	**0.271**	8.26	**-0.213**	-5.76
	1965-69	**0.268**	7.23	**0.370**	10.34	**-0.102**	-2.83
	1970-74	**0.278**	7.05	**0.410**	10.75	**-0.132**	-3.54
	1975-79	**1.176**	33.44	**1.510**	45.82	**-0.334**	-10.72
Black							
	1950-59	-0.123	-0.96	0.216	1.56	**-0.339**	-2.66
	1960-64	-0.029	-0.30	**0.500**	4.15	**-0.529**	-5.24
	1965-69	0.082	1.06	**0.443**	5.03	**-0.360**	-4.40
	1970-74	**0.293**	4.63	**0.512**	7.13	**-0.219**	-3.29
	1975-79	**1.128**	16.91	**1.556**	22.74	**-0.428**	-6.34
Asian							
	1950-59	0.037	0.47	0.140	1.50	-0.102	-1.14
	1960-64	**0.146**	2.39	0.010	0.12	0.137	1.83
	1965-69	**0.261**	5.78	-0.014	-0.21	**0.274**	4.83
	1970-74	**0.636**	16.97	**0.105**	1.94	**0.531**	10.68
	1975-79	**1.339**	42.05	**1.073**	23.24	**0.266**	5.91
Mexican							
	1950-59	**-0.096**	-1.97	0.026	0.47	**-0.122**	-2.20
	1960-64	0.057	1.40	**0.322**	6.11	**-0.265**	-5.81
	1965-69	**0.096**	2.69	**0.327**	7.16	**-0.231**	-5.68
	1970-74	**0.263**	8.49	**0.259**	7.36	0.004	0.11
	1975-79	**0.857**	27.98	**0.806**	24.63	0.051	1.41
Cuban							
	1950-59	-0.037	-0.41	-0.079	-0.48	0.043	0.28
	1960-64	**0.183**	2.45	0.015	0.11	0.169	1.16
	1965-69	0.100	0.78	0.034	0.24	0.066	0.37
	1970-74	**0.370**	4.82	**0.302**	2.00	0.068	0.46
	1975-79	**0.766**	5.15	**1.975**	10.16	**-1.209**	-6.80
Other Hispanic							
	1950-59	-0.041	-0.80	-0.039	-0.48	-0.002	-0.03
	1960-64	0.023	0.44	-0.028	-0.40	0.051	0.76
	1965-69	0.079	1.63	**0.172**	2.86	-0.093	-1.57
	1970-74	**0.106**	2.43	0.037	0.66	0.069	1.31
	1975-79	**0.844**	19.75	**1.001**	18.65	**-0.156**	-3.09

Notes:
See Table 3.

Table 5
Decomposition of Relative Earnings, 1970 to 1980 comparison

		Cross-Section		Within-Cohort		Unexplained Gap	
		coef.	t	coef.	t	coef.	t
White							
	1950-59	**0.063**	2.22	**0.105**	3.05	-0.042	-1.08
	1960-64	**0.239**	6.70	0.054	1.12	**0.185**	3.73
	1965-69	**1.070**	32.16	**0.466**	10.78	**0.604**	13.94
Black							
	1950-59	-0.057	-0.47	-0.079	-0.36	0.021	0.10
	1960-64	0.196	1.96	-0.400	-1.77	**0.596**	2.78
	1965-69	**1.050**	13.93	**0.760**	5.20	**0.289**	1.98
Asian							
	1950-59	-0.109	-1.38	-0.026	-0.16	-0.083	-0.51
	1960-64	**0.306**	4.78	0.141	0.89	0.166	1.10
	1965-69	**1.279**	30.32	**0.689**	5.85	**0.590**	5.13
Mexican							
	1950-59	**0.134**	2.79	0.070	0.76	0.064	0.69
	1960-64	**0.092**	2.03	-0.041	-0.41	0.133	1.36
	1965-69	**0.677**	15.58	**0.464**	4.97	**0.213**	2.29
Cuban							
	1950-59	-0.092	-1.24	0.180	0.49	-0.272	-0.74
	1960-64	**0.304**	4.37	0.135	0.38	0.169	0.47
	1965-69	**2.296**	22.92	0.351	0.97	**1.945**	5.27
Other Hispanic							
	1950-59	0.034	0.46	**0.294**	2.01	-0.260	-1.83
	1960-64	**0.152**	2.52	0.210	1.72	-0.057	-0.48
	1965-69	**0.929**	17.20	**0.798**	7.63	0.131	1.25

Notes:
See Table 3.

Table 6
Comparison of Unexplained Earnings Gap Estimates

		Allgrunn		Borjas	
		coef.	t	coef.	t
White					
	1950-59	-0.042	-1.08	-0.022	-0.37
	1960-64	**0.185**	3.73	**0.097**	2.21
	1965-69	**0.604**	13.94	-0.012	-0.84
Black					
	1950-59	0.021	0.10	0.183	1.87
	1960-64	**0.596**	2.78	0.184	1.83
	1965-69	**0.289**	1.98	**0.297**	3.71
Asian					
	1950-59	-0.083	-0.51	0.003	0.22
	1960-64	0.166	1.10	0.049	0.91
	1965-69	**0.59**	5.13	0.07	1.55
Mexican					
	1950-59	0.064	0.69	0.003	0.24
	1960-64	0.133	1.36	**0.093**	2.00
	1965-69	**0.213**	2.29	**0.122**	2.58
Cuban					
	1950-59	-0.272	-0.74	0.17	0.85
	1960-64	0.169	0.47	0.196	1.12
	1965-69	**1.945**	5.27	**0.61**	2.75
Other Hispanic					
	1950-59	-0.26	-1.83	0.036	0.71
	1960-64	-0.057	-0.48	**0.1**	2.10
	1965-69	0.131	1.25	**0.147**	3.31

Notes:
1. Decompostition of the unexplained earnings gap from equation (11). A positive unexplained earnings gap indicates higher relative earnings than the cohort arriving ten years later, holding years since immigration constant.
2. Borjas coefficients come from Borjas 1985, Table 5, in the column labeled "Across-Cohort Growth". Bold indicates significance at the 5% level. For the sample sizes used in compiling these estimates, see Table 1, or Borjas 1985, Table 1.

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CHAPTER 2

UNEMPLOYMENT INSURANCE AND UNEMPLOYMENT DURATION: A REEXAMINATION

Introduction

The effects of unemployment insurance (UI) on unemployment duration and earnings after reemployment have generated a large empirical literature and the continued interest of economists and policy makers. Curiously, though, labor economists and policy analysts have never reached a consensus on the quantitative effects of UI on key outcomes. These effects are of more than academic interest: In the late 1990s, the Clinton Administration introduced UI profiling (Black, Smith, Berger, and Noel 2003), and more recently the Bush Administration has proposed private reemployment accounts. Such reforms have the goal of speeding reemployment and improving the quality of the subsequent job match; however, whether they do so in fact depends on underlying worker behavior and market responses that are not wholly understood.

There are two main difficulties in estimating the effect of unemployment benefits on the duration of unemployment spells. The first is that researchers rarely have data that includes accurate information on both unemployment benefits and the duration of unemployment spells. UI administrative data is the most widely used in this literature, and with good reason; it has the actual data used to determine a worker's eligibility, benefit amounts, and potential duration, as well as the actual amount of benefits dispersed. It also contains data on the length of *insured* unemployment (the duration of benefit receipt), rather than the actual variable of interest, the total length of unemployment.[11] For example, a worker whose maximum duration of benefits is 16 weeks cannot be observed as unemployed for longer than 16 weeks in the administrative data. Many recent efforts in this literature have treated insured duration data as a censored measure of weeks jobless, and attempt to account for censoring using various econometric techniques.

The second problem is that each worker's weekly benefits and potential duration are correlated with his or her past earnings history through a formula that varies from state to state. Specifically, UI benefit generosity (both weekly benefits and potential duration) is greater for workers with stable earnings histories and higher earnings. This creates an endogeneity problem that has been neglected in most of the methods that have been used to date.

In this paper, we discuss how the use of UI administrative data has masked the severity of the endogeneity problem and use a difference-in-difference-in difference approach that can generate consistent estimates of the effect of benefit increases on unemployment duration.

In the next section, we discuss the theoretical effect of a benefit increase on unemployment and why treating insured unemployment as a censored measure of actual unemployment is misleading. In the data section we describe the dataset and why it is particularly useful for this application. In the methodology section we describe the endogeneity problem in traditional estimation techniques, provide illustrations of the problem using our data, and introduce the difference-in-difference-in-difference approach which allows us to avoid the same pitfall. Finally, we discuss the results and their robustness to changes in specification.

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[11] The effect of benefit generosity on the weeks of benefit collection itself may also be an interesting consideration, and is discussed as part of the empirical findings section of this paper.

Theory

Job-search models are the most common theoretical framework for considering the implications of unemployment insurance. Unemployed individuals receive wage offers from a known wage distribution with variable search intensity, and accept wage offers above a reservation wage. Job search models predict that higher weekly benefit amounts decrease the probability of becoming reemployed during the period of benefit receipt, but have no effect after benefit exhaustion.[12] If this theoretical prediction were correct, the censoring problems from using insured unemployment durations instead of actual unemployment durations would be a non-issue, as all of the effect of benefit increases would occur during the insured spell.

A simple labor-leisure model, however, provides a rationale for why a benefit increase may increase the uninsured portion of unemployment spells. The model presented here is similar to Moffitt and Nicholson (1982), and begins with the assumption that individuals faced with job loss make decisions about how to allocate their work and leisure over a relatively long horizon. The individual may take a job right away or choose to remain unemployed for some period. Time unemployed may be seen as utility-enhancing because it allows a period of leisure, or because it allows more time to be spent on job search. The budget constraint facing such an individual in a world with no unemployment benefits is represented as the straight line AB in Figure 8. The kinked line ACD represents the budget constraint when unemployment benefits are available for up to U_{max} weeks. The slope of segment CD represents the weekly wage rate of the new job, while the slope of segment AC represents the difference between the wage and the weekly benefit amount.

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[12] See Mortensen (1977), Moffitt (1985), Katz and Meyer (1990) for some relatively straightforward examples.

The effect of an increase in the weekly benefit amount is also shown in Figure 8, where the kinked line AEF is the new budget constraint. For individuals who would not have exhausted benefits under the lower benefit level (line segment AC), the increase in benefits has both an income effect and a substitution effect. Both effects lead the individual to choose more leisure (less work). For individuals who would have chosen to exhaust under the lower benefit level (line segment CD), there is also an income effect, with individuals choosing more leisure. Thus, higher benefits will have a positive effect on both the insured portion of an unemployment spell and the uninsured portion.

The labor-leisure model also provides insight as to whether insured durations should be thought of as a censored measure of total weeks jobless. For those who exhaust benefits, the end of benefits is not simply a censoring point; it marks the transition to a separate portion of the budget constraint where the effect of higher benefits is determined in a different fashion. Duration models that deal with censored data are designed to deal with censoring that occurs due to data collection techniques, typically the censoring of durations that have not ended at the time of data collection. The end of benefits, however, is not an artifact of data collection techniques. It is a policy-imposed limit that has real implications in the labor-leisure framework. The effect of higher benefits on the insured spell of unemployment is not the same as the effect on the uninsured spell, and attempts to estimate the effect of higher benefits on total weeks jobless using solely insured duration miss this difference.

Data

In what follows, we examine a nationally representative sample of 3,907 workers who made initial claims for UI during calendar year 1998. The data were collected by Mathematica Policy Research, Inc., for a study of UI exhaustees contracted by the US Department of Labor (Needels, Corson, and Nicholson 2002).

Mathematica used a two-stage clustered sampling design in which roughly 27,500 were randomly drawn from 25 states chosen from geographic strata. Each of the 25 states provided selected UI administrative data on the 27,500 claimants. These data include standard administrative variables such as the benefit year beginning date, first and last payments dates, base period earnings, weekly benefit amount, and the balance of UI benefits remaining for each worker at the end of his or her benefit year. (Notably, the administrative records do not include base period earnings broken down by quarter.)

Mathematica then selected random subsamples of UI exhaustees and nonexhaustees from the 27,500 claimants and administered a follow-up survey between mid-July 2000 and mid-February 2001. Mathematica's goal was to complete 2,000 surveys of exhaustees and another 2,000 surveys of nonexhaustees. They completed interviews with 1,864 exhaustees and 2,043 nonexhaustees. Given the timing of the survey, the interview covered labor market and other outcomes of the workers over a 2.2-year period on average, starting with the job loss that led to the UI claim.[13]

From our standpoint, the most important data obtained from the survey is information on whether and when each worker became reemployed following the UI claim. This allows us to observe the length of the spell during which each worker was actually jobless. In contrast, UI administrative records track only the number of weeks of UI benefits a worker receives (up to 26 weeks of benefits in most states); whether and when a worker becomes reemployed are rarely recorded. As a result, administrative data yield information only about the duration of *insured unemployment*, which is a censored estimate of the duration of the jobless spell. The follow-up survey also includes information on up to five jobs held after the UI spell,

[13] For details of the sample and survey designs, see Needels, Corson, and Nicholson (2002).

earnings in each of those jobs, and the industry and occupational classification of each of those five jobs. Finally, the follow-up survey includes information on characteristics of workers not typically included in administrative records, such as age, race, education, and marital status.

Table 7 displays some descriptive statistics of the sample. The table lists the 25 states included in the survey and the number of observations drawn from each. It also shows the maximum weekly benefit amount in each state both before and after July 1, 1998.[14] In 13 of the 25 states, the maximum weekly benefit amount increased at midyear, a fact we use below to obtain difference-in-difference-in-difference estimates of the impact of UI benefits on insured and jobless durations. Finally, Table 7 shows the median weekly benefits amounts, median number of weeks of benefits received, and median weeks jobless for workers in the sample. Comparing median weeks of benefits (or insured duration) with median weeks jobless makes it clear that weeks of benefits is a poor proxy for the actual time a worker spends without a job. The median worker in the sample received fewer than 17 weeks of UI benefits, but spent more than 30 weeks without a job.

Figures 10-12 provide additional information about our duration variables. Figure 10 shows the distribution of benefit duration for our sample. There is a spike in the density at 26 weeks, the most common exhaustion point. Figure 11 shows the distribution of jobless duration for our sample, including workers who did not obtain reemployment before the survey date. Here we see a general decline in density until about 75 weeks. The bump in density from 75-150 weeks comes from the censored durations of individuals who did not obtain reemployment before the survey date.

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[14] Several states gives workers with dependents an additional "dependency allowance." For states where a range is shown for the maximum weekly benefit amount, the lower figure is the maximum for a worker with no dependents, whereas the higher figure is the maximum for a worker with dependents.

Figure 12 omits these censored observations, showing the distribution of jobless duration for the portion of our sample that found reemployment.

Methodology

To motivate the primary methodology used in this paper, we begin with a demonstration of the problems with traditional techniques. The pioneering studies of how UI affects unemployment duration (Ehrenberg and Oaxaca 1976, Classen 1977, Holen 1977) used UI administrative data on UI recipients to estimate OLS models of the form:

$$t = \alpha_0 + \alpha_1 uiben + \alpha_2 potdur + X\beta + u \qquad (1)$$

where t denotes either the number of weeks of UI benefits received by the worker (*durui*) or the number of weeks the worker went jobless (*jobless*), and *uiben* denotes a measure of UI benefits for which the worker was eligible — either the weekly benefit amount (*wba*) or the UI replacement rate (*rr*, defined as the weekly benefit amount divided by usual weekly earnings on the pre-UI job). Models in the literature vary in their specification, but they usually include a set of controls for the earnings history and characteristics of the worker (X), and often include the worker's maximum potential duration of UI (*potdur*).

Table 8 displays results of estimating equation (1) with the Mathematica data described above; the dependent variable is the log of *durui* in columns 1 and 2, and the log of *jobless* in columns 3 and 4. Consistent with the existing literature, the coefficient on *wba* in column 1 suggests that a higher weekly benefit amount has a small, positive effect on the duration of unemployment; the point estimate suggests that a \$100 increase in the weekly benefit amount would increase insured duration by about 10%. The inclusion of potential duration (column 2) increases this estimated effect of *wba* to about 12%. It is tempting to think that these results give us some measure of the duration effect of benefit generosity.

The problem with these estimates is twofold. First, these estimates, and other OLS estimates in this literature, are using a measure of insured duration rather than total duration: a worker whose potential duration of benefits is 16 weeks cannot be observed as unemployed for longer than 16 weeks in the administrative data. Because workers with longer potential duration can be observed longer in UI administrative records, the finding that higher benefits are associated with longer unemployment duration is hardly surprising.

The second problem is that each worker's weekly benefit amount and potential duration are correlated with his or her past earnings history through a formula that varies from state to state. Specifically, UI benefit generosity (both weekly benefits and potential duration) is greater for workers who have shown stronger labor force attachment. Although the estimates in Table 8 control for several correlates of labor force attachment — education, pre-UI job tenure, base period earnings — it is likely that *wba* and *potdur* are correlated with *u*. If workers with strong labor force attachment are more likely to have shorter spells of unemployment, OLS will impart a negative bias to estimates of the effect of *wba* and *potdur*. Notice that this second problem, endogeneity bias, might be masked by the first problem, the positive bias from the use of insured durations.

Using data on total weeks jobless allows us to focus solely on the endogeneity issue. The estimates in columns 3 and 4 of Table 8 show to what degree the use of insured weeks masked the endogeneity problem. The coefficients on *wba* (columns 3 and 4) now suggest that a higher weekly benefit amount has no significant effect on the duration of unemployment. The hypothesized positive effect of *wba* appears to be swamped by the fact that workers with stronger labor force attachment (as proxied by a higher wba) have shorter spells of unemployment.

One possible way around the first problem is to treat insured duration as a censored version of weeks jobless. Censored regression, parametric hazard models, and semi-parametric hazard models are possible econometric remedies for dealing with censored data. Censored regression deals with censoring by recognizing that censored observations have a different density than uncensored observations and adjusting the log-likelihood for each observation accordingly.[15] The hazard model is a tool from duration (or survival) analysis that accounts for non-normality of the error distribution.[16] We estimate equation (1) using censored regression, and again using a Weibull hazard model. For simplicity, we report the accelerated failure-time analog of the Weibull hazard model, which allows the results to be interpreted, as in our other models, as the percentage effect of an increase on spell duration. (We have also estimated semiparametric Cox proportional hazard models and compared these with the Weibull hazard model. These latter two models give essentially similar results, suggesting that the distributional assumption imposed in the Weibull models is reasonable and that little is gained, in this case, by relaxing the assumption and moving to a semiparametric model.)

Censored and Weibull regression estimates of equation (1) are shown in Tables 9 and 10. Columns 1 and 2 show that $100 increase in weekly benefit amount is associated with a statistically significant increase in unemployment duration of 22-27% using the censored regression estimates, and a 14-17% increase using the Weibull estimates. If weeks of benefits were simply a censored version of weeks jobless, we

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[15] For a basic treatment of censored regression, see Wooldridge 2003, Chapter 17. For a more advanced discussion, see Wooldridge 2002, Chapter 16.

[16] Lancaster (1979), Moffitt (1985a,b), and Solon (1985) were among the first to use parametric and semiparametric hazard models to estimate the impact of UI benefits on unemployment duration. For a detailed discussion of survival analysis, see Cleves, Gould, and Gutierrez,2004, Allison, 1984, Cameron and Trevedi , 2005, Chapter 17, or Wooldridge 2002, Chapter 20.

might expect to see similar results using actual weeks jobless as the dependent variable. Columns 3 and 4 of Tables 9 and 10 show that this is not the case, with small, statistically insignificant coefficients for *wba*. Neither censored nor Weibull regression attempt to address endogeneity bias, so these estimates likely understate the effect of higher benefits on jobless duration.

Given these findings it seems clear that econometric techniques that account for censoring in the dependent variable are unconvincing approaches to estimating the effect of unemployment benefits on jobless duration. Rather, the primary consideration in estimating the effect on duration is finding exogenous variation in UI weekly benefit amounts. While three social experiments have been devoted to determining whether a reemployment bonus would shorten unemployment spells of UI recipients (Robins and Spiegelman 2001), the reemployment bonus experiments did not examine whether changes in weekly benefit amounts would affect unemployment duration. Indeed, it is difficult to imagine an experiment in which workers were assigned randomly to different weekly benefit amounts or potential durations of benefits. Accordingly, we need to find a quasi-experiment — an event that gives rise to (arguably) exogenous changes in UI benefits.

Meyer (1989) did just this by taking advantage of 15 increases in the maximum weekly benefit amount that occurred in 5 states during 1979 through 1983. Using a difference-in-differences approach, he examined the effects of these benefit increases on the duration of unemployment using data from the Continuous Wage Benefit History, which comes from UI administrative records and contains information on the length of insured unemployment spells. Meyer's difference-in-differences approach is attractive because it avoids the problems caused by the correlation of past earnings histories and weekly benefit amounts.[17]

Here we follow a similar approach, using a difference-in-difference-in-difference model and data on both insured duration (from the administrative records component of the Mathematica data) and jobless duration (from the survey component). Thirteen of the twenty-five states in our data raised the maximum weekly benefit amount on July 1, 1998 (see Table 7). These increases were regularly scheduled increases and were not in response to changing labor market conditions. Treating these increases as a quasi-experiment, we first divide workers in the sample into two groups based on whether they began their benefit year before or after July 1, 1998. We then divide workers into high-earnings and low-earnings groups. The high-earnings group consists of workers whose earnings histories would (or did) put them at the maximum weekly benefit amount before July 1, 1998 — these are the workers who would be affected by an increase in the maximum weekly benefit amount. The low-earnings group consists of workers whose earnings histories place them below the pre-July maximum weekly benefit amount, so they would not be affected by an increase in the maximum. Figure 9 illustrates these groups and the effect of an increase in the UI benefit schedule. Finally, we divide workers by whether or not they were in one of the 13 states that increased the maximum weekly benefit amount on July 1, 1998.

The basic difference-in-difference-in-differences (DDD) regression model is:

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[17] Meyer, Viscusi, and Durbin (1995) also use this approach to examine the effects of a similarly structured increase in Workers' Compensation benefits.

$$\begin{aligned}\ln t = \lambda_0 &+ \lambda_1 after \cdot high \cdot increase \\ &+ \lambda_2 after \cdot high + \lambda_3 after \\ &\cdot increase + \lambda_4 high \cdot increase \\ &+ \lambda_5 after + \lambda_6 high \\ &+ \lambda_7 increase + \sum_{i=1}^{13} \varphi_i state_i \\ &+ X\beta + \varepsilon\end{aligned} \tag{2}$$

where t denotes either duration of insured unemployment (*durui*) or duration of joblessness (*jobless*), *after* is an indicator equal to 1 for a UI claim starting after July 1, 1998, *high* is an indicator equal to 1 for a claimant whose earnings history places him or her at the maximum weekly benefit amount, and *increase* is an indicator equal to 1 for claimants residing in states that increased the maximum weekly benefit amount on July 1, 1998. The basic specification also includes state indicators for states where an increase took place. Augmented specifications add various additional controls (X), as will be seen in Table 11. The main coefficient of interest is λ_1, the percentage increase in duration resulting from an increase in the weekly benefit amount.

Empirical Findings

The estimates obtained using the DDD method explained above can be seen in Table 11, with results from using weeks of insured unemployment seen in the six specifications in Panel A. The point estimates suggest that an increase in the maximum benefit amount increases weeks of benefits collected by about 4 to 9 percent; however, none of these estimates is statistically significant. In the data we are examining, the average increase in the maximum benefit amount was about 5 percent.

Accordingly, the point estimates suggest that a 5-percent increase in the weekly benefit amount increases the duration of insured unemployment by 4 to 9 percent — an increase of roughly 0.7 to 1.5 weeks of insured unemployment for those at the median weeks of benefits (16.5).

Meyer's estimates suggest that an increase in the maximum benefit amount increases insured duration by between 3 and 8 percent. Notably, four of Meyer's six estimates are statistically significant at the 5% level, perhaps because his samples are in the range of 12,500 to 16,000, or because the increases in the maximum weekly benefit amount for that time period are somewhat larger— an average increase of about 9 percent. Meyer's estimates correspond to an increase of 0.6 to 1.8 weeks. Meyer interprets his estimates as the effect of the increases on actual weeks jobless, but it is important to recall that they are based on insured unemployment spells.[18] For reasons discussed above, it seems more reasonable to interpret the estimates as effects of a benefit increase on insured duration.

Estimates displayed in Panel B use actual jobless duration. The point estimates suggest that an increase in the maximum weekly benefit amount increases the duration of jobless spells by 6 to 12 percent. At the median weeks jobless in our sample, 30.6, this corresponds to an increase of 1.8 to 3.7 roughly double that of the effect on insured weeks alone. While the effects are relatively large, and much larger than any of the estimates obtained using equation (1), none are statistically significant. The estimates using weeks jobless are generally higher than those using weeks of benefits received, offering some support for the idea that insured duration and total duration respond differently to changes in the weekly benefit amount.

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[18] Meyer includes a brief discussion of why data on weeks insured can be used to estimate weeks jobless (p. 21-22), but appears to have assumed that benefits only affect the insured portion of the unemployment spell.

It should be noted that the effects estimated by our approach would not necessarily mirror the effect we would see in the entire population. Since the changes involved are with the maximum benefit amount, and not the underlying benefit formula, the effect is coming entirely from high-wage earners. It is possible that the treatment effect for this group differs from the effect we would see from increasing benefits for lower wage workers.[19]

One possible concern with the approach used in Table 11 is that UI claimants may have known about the upcoming benefit increase, and timed their claim to come just after the increase rather than just before.[20] (For example, a claimant in Tennessee who would be eligible for the maximum weekly benefit of $255 if she filed on July 1 would only get $220 if she filed on June 30.) If individuals planning on longer unemployment spells are more likely to wait until after the change to file for benefits, the estimated effect would be biased upward. Table 12 shows the results of the same difference-in-difference approach, but excluding from the sample all claims within 30 days of the July 1 change. This exclusion shrinks all estimates of the effect of the weekly benefit amount, which may indicate that individuals expecting longer unemployment spells may have been more likely to time their claims to take advantage of the increase. As in Table 11, estimates using weeks jobless are generally higher than those using weeks of benefits received.

Given the relative difficulty of obtaining data on the full duration of jobless spells, it is worth considering whether it is possible to obtain estimates of the effect on actual weeks jobless using only data on weeks of insured unemployment. Table 13

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[19] There are periodic changes in the *minimum* weekly benefit amount in some states, so it would theoretically be possible to measure the effect of benefits for low-wage workers. The minimum levels, however, are low enough that very few individuals are affected by such increases.

[20] Rogers (1998) finds that individuals have "significant foresight" about changes in UI provisions.

shows the results of using a difference-in-difference technique with censored regression (Panel A) or a representative hazard model technique (Panel B) on insured weeks. As in Table 10, we report estimates from an accelerated failure-time Weibull model; this allows the results to be interpreted, as in our other models, as the percentage effect of a weekly benefit increase on spell duration.[21] The point estimates from both techniques are generally larger than the estimates found in the other estimations of equation 2. This is consistent with the idea that treating insured weeks as a censored measure of total weeks leads to overestimates of the effect of higher benefits, but the estimates are still statistically insignificant.

Finally, we expand our data to the full sample of administrative data. In table 14, we report estimates of equation (2) for the full sample (comparable to the estimates in Table 11, Panel A), the full sample omitting those filing within 30 days of the increase (comparable to the estimates in Table 12, Panel A), and using censored regression (comparable to the estimates in Table 13, Panel A). Since the administrative data does not include all the variables necessary for specifications 2-6, we only estimate the first specification. In each case, the point estimate is smaller than the corresponding estimates using the survey sample, and each is still statistically insignificant.

Conclusions

The endogeneity of weekly benefit amounts is a problem that cannot be ignored. Both the potential duration and level of weekly benefits depend intentionally on variables associated with past employment patterns. It stands to reason that

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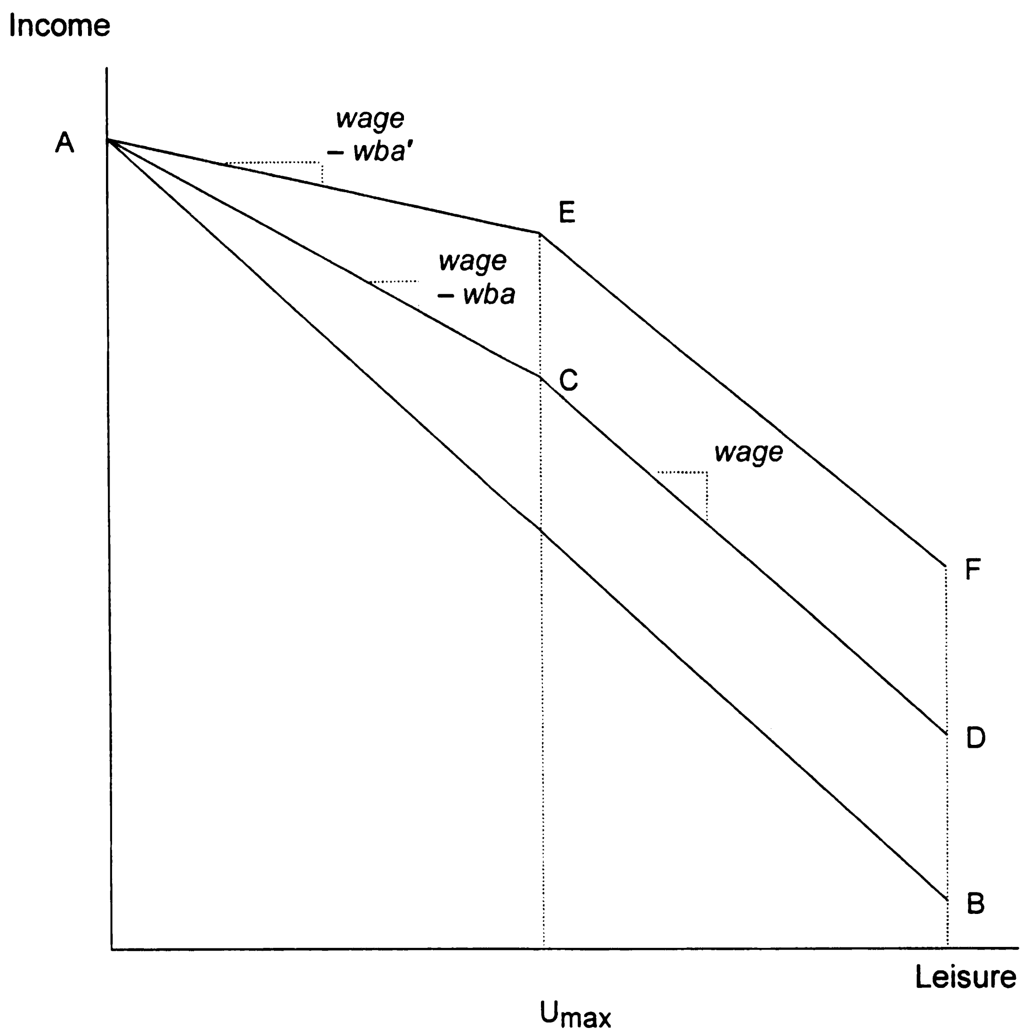
[21] Again, we have also estimated Weibull hazard models [the hazard model analog to the accelerated failure-time model reported here] and semiparametric Cox proportional hazard models. These latter two models again give similar results, suggesting that little is gained by relaxing the distributional assumption imposed in the Weibull.

unobservables that affect unemployment duration are correlated with both the weekly benefit amount and potential duration of benefits. Estimates of the effect of benefits based on insured duration tend to mask this problem due to the disproportionate right-censoring of the spells of those who have short benefit durations. Using data on actual unemployment duration reveals the limitations of methods that do not account for the relationship between earnings history and unemployment duration. Our quasi-experimental approach finds that increased benefits do not have a statistically significant effect on either insured duration or weeks jobless.

Figure 8
Budget constraint with unemployment benefits and an increase in benefits



Notes:
1. AB is the budget constraint absent unemployment benefits. ACD is the budget constraint with unemployment benefits. AEF is the budget constraint with an increase in the weekly benefit amount from *wga* to *wga'*.

Figure 9
Illustrative UI benefit schedule with an increase in maximum benefit amount

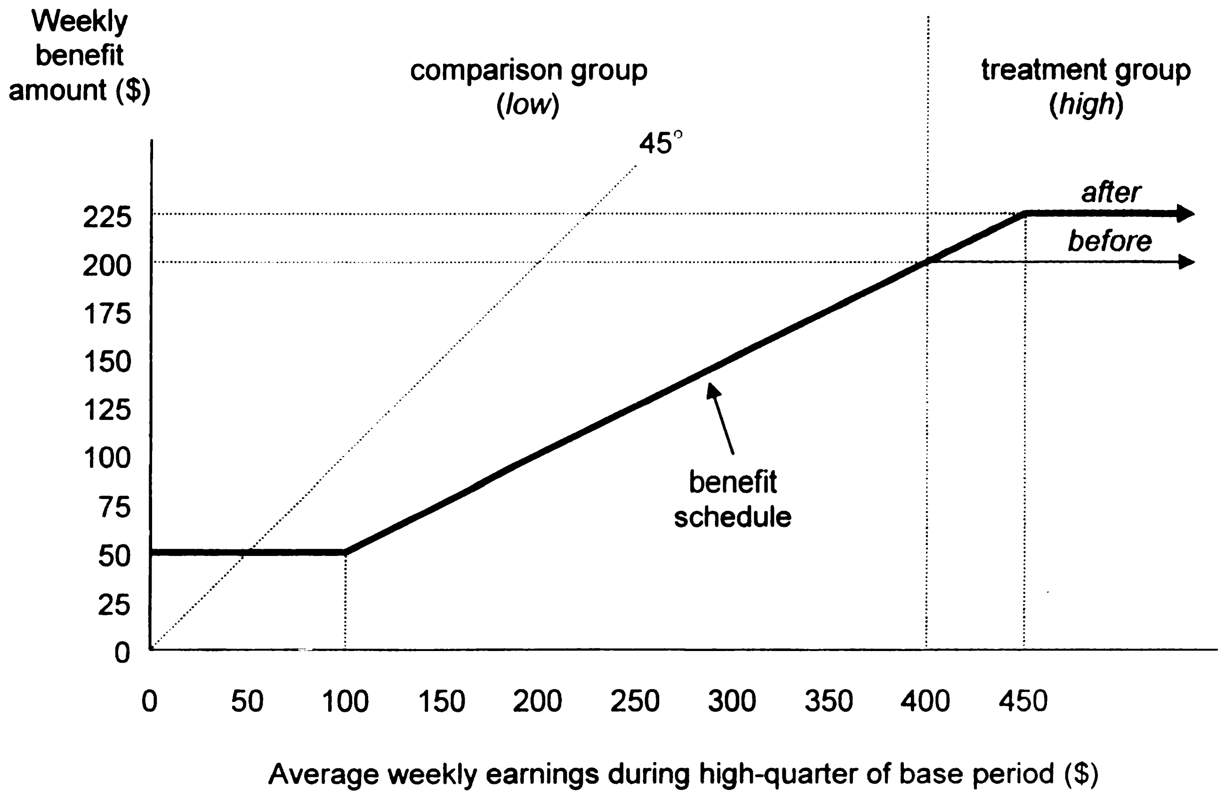
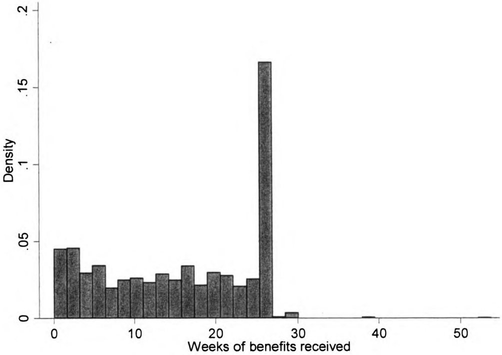
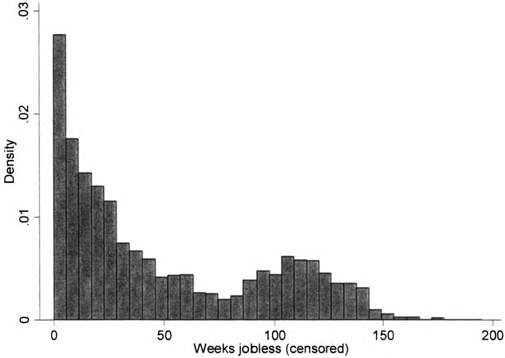


Figure 10
Distribution of Benefit Duration



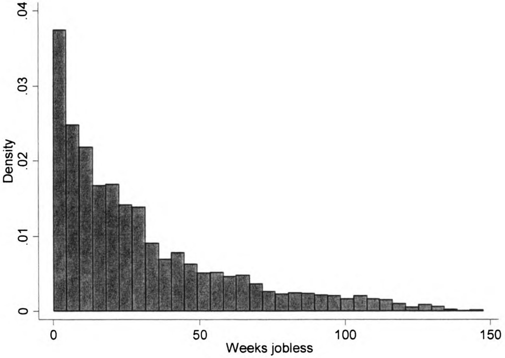
Notes:
1. Spike occurs at the most common exhaustion point of 26 weeks.
2. Sample size = 3,158

Figure 11
Distribution of Jobless Duration (including censored observations)



Notes:
1. The increase in density after 75 weeks is almost entirely due to censored observations. (Workers who fail to find reemployment before the survey date.) See Figure 12 for the distribution without censored observations.
2. Sample size = 3,158

Figure 12
Distribution of Jobless Duration (excluding censored observations)



Notes:
1. Only includes workers who found reemployment before the survey date.
2. Sample size = 3,158

Table 7
Sample descriptive statistics and changes in maximum weekly benefit amounts

State	N	maximum weekly benefit amount ($) before	after	median weekly benefits ($)	median weeks of benefits	median weeks jobless
CA	429	230	same	150	21.0	40.6
FL	127	275	same	229	16.9	39.0
GA	71	224	244	200	14.3	33.3
HI	103	356	same	330	24.0	40.7
ID	94	265	273	198	14.0	23.6
IL	110	269-355	same	259	19.1	35.1
IA	111	239-293	251-307	248	12.3	16.6
KY	100	256	268	238	8.8	22.6
ME	101	216-324	227-340	202	18.0	31.3
MI	156	300	same	287	15.0	19.0
MN	116	331	same	306	15.0	19.8
MS	99	180	190	162	15.9	31.7
MT	93	237	246	197	14.7	19.7
NJ	132	390	same	255	20.1	41.8
NY	169	300	same	268	26.0	53.3
NC	94	322	same	221	12.1	45.6
OH	102	267-358	same	255	7	17.9
OK	114	255	262	254	14.0	24.6
PA	210	375-383	same	262	19.9	37.9
RI	89	347-433	364-455	255	13.9	25.7
TN	102	220	255	197	16.1	36.6
TX	167	280	same	235	17.1	41.3
VA	89	226	228	220	11.0	26.0
WA	76	384	410	221	19.2	24.2
WI	104	282	290	257	9.2	24.1
Total	3158	na	na	224	16.5	30.6

Sources: Authors' tabulations of data from Needels, Corson, and Nicholson (2002) and US Department of Labor (various years).

Table 8
OLS estimates of insured unemployment duration and jobless duration

Independent variables	(1)	(2)	(3)	(4)
	Dependent variables			
	ln (Weeks of benefits)		ln(Weeks jobless)	
weekly benefit amount	0.1023	0.1213	0.0714	0.0661
($100s)	(0.0358) **	(0.0366) **	(0.0467)	(0.0477)
potential duration	--	0.0122	--	-0.0034
of UI benefits		(0.0049) *		(0.0064)
age	0.0118	0.0118	0.0166	0.0166
	(0.0017) **	(0.0017) **	(0.0022) **	(0.0022) **
years of education	0.0267	0.0272	-0.0179	-0.0180
	(0.0060) **	(0.0060) **	(0.0078) *	(0.0079) *
pre-UI job tenure	-0.0115	-0.0116	-0.0009	-0.0009
(years)	(0.0027) **	(0.0027) **	(0.0035)	(0.0035)
white	-0.1322	-0.1332	-0.0953	-0.0950
	(0.0410) **	(0.0410) **	(0.0534)	(0.0534)
female	0.0275	0.0191	0.0884	0.0907
	(0.0375)	(0.0376)	(0.0488)	(0.0490)
married	-0.0639	-0.0623	-0.0499	-0.0503
	(0.0376)	(0.0376)	(0.0491)	(0.0491)
dependents < 18	0.0366	0.0367	0.0308	0.0308
	(0.0157) *	(0.0157) *	(0.0205)	(0.0205)
log of base period	-0.1662	-0.2206	-0.2665	-0.2514
earnings	(0.0408) **	(0.0463) **	(0.0532) **	(0.0604) **
state unemployment rate	0.1477	0.1410	0.1410	0.1429
	(0.0190) **	(0.0192) **	(0.0248) **	(0.0250) **
constant	2.4744	2.6958	4.6234	4.5620
	(0.3483) **	(0.3593) **	(0.4538) **	(0.4686) **
Observations	3158	3158	3158	3158
Adjusted R-squared	0.0537	0.0553	0.0526	0.0523

Notes:
1. OLS estimates of equation (1). Coefficients are the percentage change in weeks of benefit duration or jobless duration with respect to a 1 unit increase in each independent variable.
2. ** significantly different from 0 at the 1% level; * significantly different from 0 at the 5% level. Standard errors in parentheses.
3. Each specification includes a set of industry indicators.

Table 9
Censored regression estimates of insured unemployment duration and jobless duration

	(1)	(2)	(3)	(4)
	Dependent variables			
Independent variables	ln (Weeks of benefits)		ln(Weeks jobless)	
weekly benefit amount	0.2694	0.2229	0.0816	0.0812
($100s)	(0.0647) **	(0.0660) **	(0.0585)	(0.0597)
potential duration	--	-0.0362	--	-0.0003
of UI benefits		(0.0093) **		(0.0081)
age	0.0218	0.0216	0.0238	0.0238
	(0.0031) **	(0.0031) **	(0.0028) **	(0.0028) **
years of education	0.0467	0.0455	-0.0396	-0.0396
	(0.0109) **	(0.0109) **	(0.0102) **	(0.0102) **
pre-UI job tenure	-0.0154	-0.0153	0.0027	0.0027
(years)	(0.0047) **	(0.0047) **	(0.0044)	(0.0044)
white	-0.2634	-0.2593	-0.1195	-0.1195
	(0.0742) **	(0.0744) **	(0.0671)	(0.0671)
female	0.0440	0.0639	0.1043	0.1045
	(0.0675)	(0.0680)	(0.0613)	(0.0615)
married	-0.0960	-0.0990	-0.0463	-0.0464
	(0.0676)	(0.0678)	(0.0616)	(0.0616)
dependents < 18	0.0649	0.0645	0.0388	0.0388
	(0.0283) *	(0.0283) *	(0.0257)	(0.0257)
log of base period	-0.5494	-0.4096	-0.3330	-0.3318
earnings	(0.0758) **	(0.0840) **	(0.0669) **	(0.0758) **
state unemployment rate	0.2326	0.2505	0.1636	0.1637
	(0.0339) **	(0.0343) **	(0.0310) **	(0.0313) **
constant	5.5249	5.0642	5.2965	5.2915
	(0.6461) **	(0.6603) **	(0.5698) **	(0.5875) **
Observations	3158	3158	3158	3158
censored	1508	1508	1508	1508
LR chi-squared	215	230.31	213.48	213.48
Pseudo R-squared	0.0252	0.027	0.0198	0.0198

Notes:
1. Censored regression estimates of equation (1). Coefficients are the percentage change in weeks of benefit duration or jobless duration with respect to a 1 unit increase in each independent variable.
2. ** significantly different from 0 at the 1% level; * significantly different from 0 at the 5% level. Standard errors in parentheses.
3. Each specification includes a set of industry indicators.

Table 10
Weibull (AFT) regression estimates of insured unemployment duration and jobless duration

	(1)	(2)	(3)	(4)
	Dependent variables			
Independent variables	Weeks of benefits		Weeks jobless	
weekly benefit amount ($100s)	0.1713 (0.0544) **	0.1443 (0.0557) *	0.0380 (0.0523)	0.0472 (0.0534)
potential duration of UI benefits	--	-0.0187 (0.0074) *	--	0.0062 (0.0071)
age	0.0181 (0.0028) **	0.0179 (0.0028) **	0.0219 (0.0025) **	0.0220 (0.0025) **
years of education	0.0358 (0.0094) **	0.0358 (0.0095) **	-0.0523 (0.0089) **	-0.0519 (0.0089) **
pre-UI job tenure (years)	-0.0084 (0.0041) *	-0.0078 (0.0041)	0.0143 (0.0041) **	0.0142 (0.0041) **
white	-0.2305 (0.0673) **	-0.2297 (0.0678) **	-0.1208 (0.0601) *	-0.1211 (0.0601) *
female	0.1146 (0.0594)	0.1252 (0.0598) *	0.0921 (0.0546)	0.0882 (0.0547)
married	-0.0914 (0.0598)	-0.0911 (0.0601)	-0.0249 (0.0549)	-0.0248 (0.0549)
dependents < 18	0.0520 (0.0251)	0.0524 (0.0252) *	0.0495 (0.0228) *	0.0503 (0.0228) *
log of base period earnings	-0.3711 (0.0640) **	-0.3031 (0.0706) **	-0.2529 (0.0595) **	-0.2797 (0.0670) **
state unemployment rate	0.2122 (0.0294) **	0.2187 (0.0296) **	0.1096 (0.0279) **	0.1061 (0.0282) **
constant	4.7260 (0.5367) **	4.5469 (0.5547) **	5.6318 (0.5011) **	5.7350 (0.5130) **
shape parameter (p)	0.884566	0.879708	0.780283	0.780388
Observations	3158	3158	3158	3158
LR chi-squared	180.71	187.07	262.89	263.63

Notes:

1. Weibull (AFT) regression estimates of equation (1). Coefficients are the percentage change in weeks of benefit duration or jobless duration with respect to a 1 unit increase in each independent variable.
2. ** significantly different from 0 at the 1% level; * significantly different from 0 at the 5% level. Standard errors in parentheses.
3. Each specification includes a set of industry indicators.

Table 11
DDD estimates of UI benefit duration and jobless duration

Panel A

Weeks of benefits received	Specifications (1)	(2)	(3)	(4)	(5)	(6)
after•high•increase	0.0704	0.0697	0.0525	0.0392	0.0883	0.0681
	(0.1545)	(0.1533)	(0.1530)	(0.1510)	(0.1434)	(0.1423)
log of base period earnings	--	--	-0.1709	-0.1313	-0.1301	-0.1522
			(0.0379) **	(0.0382) **	(0.0363) **	(0.0366) **
potential duration of UI benefits	--	--	0.0127	0.0106	0.0087	0.0107
			(0.0052) *	(0.0051) *	(0.0049) *	(0.0049) *
age, race, gender, education	--	yes	yes	yes	yes	yes
industry indicators	--	--	--	yes	yes	yes
recall indicators	--	--	--	--	yes	yes
occupation, married, dependents	--	--	--	--	--	yes
Observations	3158	3158	3158	3158	3158	3158
Adjusted R-squared	0.0295	0.0441	0.0497	0.0756	0.1665	0.1825

Table 11 (continued)

Panel B						
Weeks jobless	Specifications					
	(1)	(2)	(3)	(4)	(5)	(6)
after•high•increase	0.0667	0.0764	0.0628	0.0611	0.1197	0.1064
	(0.2017)	(0.1991)	(0.1984)	(0.1977)	(0.1915)	(0.1914)
log of base period earnings	--	--	-0.2127	-0.2037	-0.2114	-0.2300
			(0.0492) **	(0.0500) **	(0.0485) **	(0.0492) **
potential duration of UI benefits	--	--	-0.0028	-0.0050	-0.0070	-0.0055
			(0.0067)	(0.0067)	(0.0065)	(0.0065)
age, race, gender, education	--	yes	yes	yes	yes	yes
industry indicators	--	--	--	yes	yes	yes
recall indicators	--	--	--	--	yes	yes
occupation, married, dependents	--	--	--	--	--	yes
Observations	3158	3158	3158	3158	3158	3158
Adjusted R-squared	0.0246	0.0495	0.0573	0.0649	0.1234	0.1270

Notes:

1. Estimates of equation (2). Coefficients give the percentage change in weeks of benefit duration (Panel A) or jobless duration (Panel B) with respect to a 1 unit increase in each independent variable. For *after•high•increase*, the coefficients give the percentage change in weeks of benefits received or weeks jobless for those affected by the July 1 increase in benefits.
2. ** significantly different from 0 at the 1% level; * significantly different from 0 at the 5% level. Standard errors in parentheses.

Table 12
DDD estimates of UI benefit duration and jobless duration, omitting claims within 30 days of the increases

Panel A

Weeks of benefits received	Specifications (1)	(2)	(3)	(4)	(5)	(6)
after•high•increase	0.0468	0.0470	0.0354	0.0215	0.0513	0.0107
	(0.1697)	(0.1683)	(0.1680)	(0.1667)	(0.1592)	(0.1577)
log of base period earnings	--	--	-0.1316	-0.1164	-0.1253	-0.1455
			(0.0413) **	(0.0417) **	(0.0400) **	(0.0402) **
potential duration of UI benefits	--	--	0.0109	0.0098	0.0093	0.0104
			(0.0057) *	(0.0057)	(0.0054)	(0.0054) *
age, race, gender, education	--	yes	yes	yes	yes	yes
industry indicators	--	--	--	yes	yes	yes
recall indicators	--	--	--	--	yes	yes
occupation, married, dependents	--	--	--	--	--	yes
Observations	2601	2601	2601	2601	2601	2601
Adjusted R-squared	0.0260	0.0426	0.0456	0.0626	0.1455	0.1643

Table 12 (continued)

Panel B

Weeks jobless	Specifications					
	(1)	(2)	(3)	(4)	(5)	(6)
after•high•increase	0.0535	0.0474	0.0344	0.0275	0.0665	0.0325
	(0.2155)	(0.2125)	(0.2120)	(0.2117)	(0.2056)	(0.2052)
log of base period earnings	--	--	-0.1529	-0.1658	-0.1848	-0.2039
			(0.0521) **	(0.0530) **	(0.0516) **	(0.0523) **
potential duration of UI benefits	--	--	-0.0058	-0.0068	-0.0073	-0.0062
			(0.0072)	(0.0072)	(0.0070)	(0.0070)
age, race, gender, education	--	yes	yes	yes	yes	yes
industry indicators	--	--	--	yes	yes	yes
recall indicators	--	--	--	--	yes	yes
occupation, married, dependents	--	--	--	--	--	yes
Observations	2601	2601	2601	2601	2601	2601
Adjusted R-squared	0.0257	0.0526	0.0577	0.0630	0.1162	0.1226

Notes:

1. Estimates of equation (2). The sample is limited to those filing 30 days before or after the July 1 increases. Coefficients give the percentage change in weeks of benefit duration (Panel A) or jobless duration (Panel B) with respect to a 1 unit increase in each independent variable. For *after•high•increase*, the coefficients give the percentage change in weeks of benefits received or weeks jobless for those affected by the July 1 increase in benefits.
2. ** significantly different from 0 at the 1% level; * significantly different from 0 at the 5% level. Standard errors in parentheses.

Table 13
DDD estimates of UI benefit duration and jobless duration using weeks of benefits received

Panel A

Censored regression	Specifications (1)	(2)	(3)	(4)	(5)	(6)
after•high•increase	0.1494	0.1390	0.1451	0.1125	0.2248	0.2079
	(0.2735)	(0.2711)	(0.2709)	(0.2667)	(0.2516)	(0.2486)
log of base period earnings	--	--	-0.2789	-0.2204	-0.2224	-0.2738
			(0.0688) **	(0.0691) **	(0.0655) **	(0.0659) **
potential duration of UI benefits	--	--	-0.0432	-0.0461	-0.0459	-0.0418
			(0.0099) **	(0.0098) **	(0.0092) **	(0.0091) **
age, race, gender, education	--	yes	yes	yes	yes	yes
industry indicators	--	--	--	yes	yes	yes
recall indicators	--	--	--	--	yes	yes
occupation, married, dependents	--	--	--	--	--	yes
Observations	3158	3158	3158	3158	3158	3158
censored	1508	1508	1508	1508	1508	1508
LR chi-squared	110.53	167.74	235.24	313.01	631.56	712.42
Pseudo R-squared	0.013	0.0197	0.0276	0.0367	0.0741	0.0836

Table 13 (continued)

Panel B

Weibull (AFT) regression	(1)	(2)	(3)	(4)	(5)	(6)
			Specifications			
after•high•increase	0.2026	0.1966	0.1675	0.1464	0.1227	0.0359
	(0.2327)	(0.2307)	(0.2332)	(0.2322)	(0.2178)	(0.2159)
log of base period earnings	--	--	-0.2030	-0.1844	-0.2330	-0.0349
			(0.0609) **	(0.0616) **	(0.0583) **	(0.0076) **
potential duration of UI benefits	--	--	-0.0278	-0.0289	-0.0385	-0.0349
			(0.0082) **	(0.0079) **	(0.0075) **	(0.0076) **
age, race, gender, education	--	yes	yes	yes	yes	yes
industry indicators	--	--	--	yes	yes	yes
recall indicators	--	--	--	--	yes	yes
occupation, married, dependents	--	--	--	--	--	yes
Shape parameter (p)	0.8824	0.8902	0.8814	0.8866	0.9471	0.9571
Observations	3158	3158	3158	3158	3158	3158
LR chi-squared	103.06	167.94	208.29	267.37	677.73	755.95

Notes:

1. Estimates of equation (2). Coefficients give the percentage change in weeks jobless with respect to a 1 unit increase in each independent variable. For *after•high•increase*, the coefficients give the percentage change in weeks jobless for those affected by the July 1 increase in benefits.
2. ** significantly different from 0 at the 1% level; * significantly different from 0 at the 5% level. Standard errors in parentheses.

Table 14
DDD estimates of UI benefit duration and jobless duration using only administrative data

	DDD	DDD (Omit)	Censored regression
after•high•increase	-0.0059	-0.0186	-0.0002
	(0.0563)	(0.0603)	(0.0765)
log of base period earnings	--	--	--
potential duration of UI benefits	--	--	--
age, race, gender, education	--	--	--
industry indicators	--	--	--
recall indicators	--	--	--
occupation, married, dependents	--	--	--
Observations	25,572	21,112	25,572
Adjusted R-squared	0.0094	0.0077	--
Pseudo R-squared	--	--	0.0043
LR chi-squared	--	--	306.62

Notes:
1. Estimates of equation (2). Coefficients give the percentage change in weeks of benefit duration (Columns 1 and 2) or jobless duration (Column 3) for those affected by the July 1 increase in benefits.
2. ** significantly different from 0 at the 1% level; * significantly different from 0 at the 5% level. Standard errors in parentheses.

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CHAPTER 3

EMERGENCY UNEMPLOYMENT COMPENSATION AND THE DURATION OF UNEMPLOYMENT

Introduction

In each economic downturn since 1958, the U.S. Congress has extended by fiat the potential duration of unemployment benefits available to laid-off workers. Roughly a dozen empirical studies have examined the extent to which extended unemployment benefits lead to longer spells of unemployment in the United States. The studies have used a variety of data sources covering different time periods, different states, and different extended benefit programs. Also, they have used a variety of the econometric methods to model duration and the timing of reemployment. It is perhaps not surprising that the estimated effect of an additional week of unemployment insurance (UI) on the duration of unemployment varies dramatically over these studies — from 0 to 0.9 week. By implication, a typical 13-week emergency extension of benefits could add anywhere from 0 to nearly 12 weeks to a typical worker's spell of unemployment.

This paper seeks to understand the extent to which differing econometric models and assumptions can explain the wide range of estimates of the impact of extended benefits. To do so, we use a single detailed dataset collected specifically to analyze the Emergency Unemployment Compensation Act of 1991 (EUC), which extended benefits between November 1991 and February 1994. The data come from the administrative records of 16 states, supplemented by an extensive follow-up survey. They include information on 2,304 workers who began collecting UI benefits between January 1991 and September 1993.

Our empirical work addresses two main questions. First, when Congress increases the potential duration of UI by one week, by how much does the average number of weeks of UI paid to claimants increase? This first question is important because policymakers need to know the financial implications of extending UI benefits. Second, when the potential duration of UI increases by one week, by how much does the average jobless spell increase? This is the question usually addressed in empirical research on extended benefits because search and matching models imply that increased availability of benefits will lead workers to reduce their job search effort and extend their spells of joblessness.

To address these questions, we systematically apply a range of estimators and assumptions to the data with the goal of sorting out how these differences affect statistical inferences. Our results suggest that much of the variation in existing estimates can be attributed to variation in econometric methods and assumptions. We describe the relationship between methods and inferences and offer some speculation on which methods (and hence which inferences) are most convincing.

The next section gives background on unemployment benefit duration and extended benefits, with a focus on EUC. We then briefly review previous studies of the effect of UI benefit duration on the duration of unemployment. After describing the data we use, we present estimates and discuss the findings from different econometric specifications.

Policy Background

The UI system in the United States can be thought of as having three "tiers."[22] The first tier consists of regular state UI benefits, which have a maximum potential duration of 26 weeks in all but two states. (The exceptions are Massachusetts and Washington, where the maximum potential duration is 30 weeks.) Regular state benefits vary in amount and duration from state to state, are funded by experience-rated payroll taxes collected from employers, and have the stated goal of offering a limited number of weeks of benefits during a temporary spell of unemployment. In 8 states, the potential duration of benefits is 26 weeks for all claimants who qualify for any benefits.[23] In all other states, potential duration varies with a claimant's work experience in the base period — roughly the year preceding the claim for benefits — according to formulas that vary from state to state.

The second tier of the UI system is the "standby" Extended Benefits program (EB), a federal program enacted by Congress in 1970 that is intended to activate automatically and increase the potential duration of UI benefits in an economic downturn. EB extends benefits to claimants who exhaust their regular state benefits by one–half of regular benefit duration, up to 13 weeks. It is financed equally by federal and state revenues and provides the same weekly benefit amount to a claimant as the regular state program.

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[22] For detailed descriptions of UI benefit duration in the United States, see Woodbury and Rubin (1995).

[23] Connecticut, Hawaii, Illinois, Maryland, New Hampshire New York, Vermont, and West Virginia.

Vroman and Woodbury (2004, chapter 3) and Wenger and Walters (2006) show that during its first 13 years (1971–1983) EB activated frequently, but since then it has activated rarely and is no longer an important program for the unemployed. This is at least partly by design: In 1981 Congress revised EB to make it more difficult for EB to activate. Also, the indicator that activates EB — the insured unemployment rate in a state — has trended downward since the early 1980s, making it difficult for EB to activate. Finally, state governors have frequently suspended EB when federal emergency benefits are available because EB is funded half by state UI trust funds, whereas emergency benefits are wholly federally funded.

The third tier of the UI system consists of federal "emergency" benefit extensions. Unlike the EB program, emergency extensions do not activate automatically. Rather, Congress has enacted them by fiat in every recession starting with the recession of 1958 — a total of seven to date. Table 15 gives a brief history of the three most recent emergency extensions, which have varied significantly in the number of additional weeks they provide, sources of funding, and eligibility criteria.

In this paper, we examine the Emergency Unemployment Compensation program (EUC), the sixth emergency benefit extension, which was enacted in November 1991 in response to the downturn of the early 1990s. EUC was the most complicated emergency benefit extension to date: It went through five "phases" as Congress amended it, provided different potential benefit durations among different states at a given time (depending on whether a state was classified as "high unemployment" or low unemployment"), and changed potential durations within a state over time (both by Congressional fiat and when a state changed between the "high" and "low" unemployment classification) — see again Table 15.

As an example, Table 16 shows how the potential duration of benefits varied under EUC in Pennsylvania. Between November 1991, when EUC became effective, and February 1994, when Phase V of EUC ended, the potential duration of benefits in Pennsylvania changed nine times. Six of these changes resulted from enactment of EUC or legislative changes during the life of EUC, and three resulted because Pennsylvania was reclassified as high- or low-unemployment.

The empirical work below takes advantage of the variation over time and among states in the potential duration of unemployment; however, we do not claim that EUC represents a quasi-experiment because it was implemented during a recession and provided longer extensions in states designated as high-unemployment than in those designated as low-unemployment. Because longer spells of unemployment are expected in bad labor market conditions, and EUC coincided with bad labor market conditions, potential benefit duration under EUC must be considered endogenous. We try to cope with this endogeneity by including state-level indicators and indicators of the labor market conditions facing unemployed workers in an attempt to control for workers' reemployment opportunities. This amounts to a "plug-in" approach to controlling for otherwise omitted variables. Also, the link between labor market conditions in a state and potential benefit duration under EUC and was quite loose. Specifically, although the recession of the early 1990s was concentrated in the northeastern states and on the west coast, all states were covered by EUC and had benefit extensions, including many (like Illinois and North Carolina) that experienced only mild downturns.

Previous Research

Efforts to estimate the impact of extended UI benefits date to the mid 1970s, not long after the standby EB program came into being. Holen (1976) used administrative records on UI claim spells and regressed the number of weeks of UI benefits received by a claimant on appropriate explanatory variables, including measures of the UI replacement rate and potential duration of benefits. Her estimates suggested that a one-week increase in the potential duration of benefits led to an additional 0.8 weeks of benefits received. (Table 17 summarizes Holen's and others' findings.) Solon (1979) found that a one-week increase in potential duration led to an additional 0.16 weeks of unemployment for a sample of New York workers. Classen (1979) and Newton and Rosen (1979) both examined UI claim spells but estimated Tobit models to handle truncation of the dependent variable (weeks of UI claimed). Classen found that a one-week increase in potential duration led to at most an additional 0.12 week of benefits received, whereas Newton and Rosen's estimates suggested an additional 0.6 week.

Katz and Ochs (1980), Moffitt (1985b), and Solon (1985) estimated parametric models of unemployment duration, along the lines of those described by Lancaster (1979). Katz and Ochs estimated that an additional week of potential duration increased unemployment duration by 0.17–0.23 weeks. Moffitt (1985b) obtained a range of estimates between 0.17 and 0.45 weeks when he examined a 15-state sample and data from Georgia data. Solon (1985) examined Georgia data and estimated that an additional week of potential benefit duration leads to 0.36 additional weeks of unemployment.

Moffitt (1985a, 1985b), Ham and Rea (1987), Grossman (1989), and Katz and Meyer (1990) estimated semi-parametric hazard models along the lines of those developed by Cox (1972). Moffitt (1985a, 1985b) and Katz and Meyer (1990) suggest an additional week of potential duration leads to an increase in unemployment duration of 0.15 to 0.2 week. Ham and Rea's (1987) estimate of 0.26-0.35 week is somewhat higher and comes from data on Canadian men. Grossman's (1989) estimate of 0.9 week, from Phase IV of the Federal Supplemental Compensation program (the emergency extension program of the early 1980s), is the highest of all; however, her estimate comes from a sample of claimants who had exhausted their regular state UI benefits, so it is an estimate of the impact of an additional week of UI benefits on the expected duration of unemployment conditional on exhausting regular UI benefits. Jurajda and Tannery(2003) use a flexible hazard model and find that potential duration decreases both the new job and recall hazard in individuals with more than 1 weeks of remaining benefit eligibility.

Data

We examine a nationally representative sample of 2,304 workers from 16 states who made initial claims for UI between January 1992 and September 1993 and/or received EUC between July 1992 and May 1994. The data were collected by Mathematica Policy Research, Inc., under contract to the US Department of Labor.

They are unusual because they include two matched components — UI administrative records and survey data from interviews conducted between April 1996 and April 1997 (roughly three and a half years after the workers' first benefit payments). The following brief description is based on Cederbaum (1997) and Corson, Needels, and Nicholson (1999), who fully describe the sample design and public use data.

Information available in UI administrative data vary from state to state, but variables central to administering UI are always included — base period earnings, weekly benefit amount, the initial claim date, benefit year beginning date, benefit payment dates, and the balance of UI benefits remaining for each worker at the end of his or her benefit year. Many analyses of potential benefit duration have relied solely on UI administrative, which are extremely rich: For example, they often include basic demographic data on claimants, in addition to information on the amount and timing of benefits received.

UI administrative records are rich in that they offer relatively complete information about UI benefits paid to workers; however, they generally exclude information on reemployment and claimants' subsequent earnings. Hence, although they indicate the duration of UI claim spells, they do not provide data on spells of unemployment or joblessness. The follow-up survey conducted by Mathematica was designed to remedy this problem by collecting information on whether the claimant became reemployed, the timing of reemployment, and post-reemployment earnings. The follow-up survey also collected a consistent set of data on various worker characteristics that are not always available in UI administrative data. The combination of UI administrative records with the follow-up survey results in a dataset in which each component complements the other. Specifically, the follow-up survey includes information on reemployment, jobless duration, marital status, educational attainment, race, previous earnings, and recall variables, while the administrative data includes information about benefit duration, potential benefit duration, benefit exhaustion, weekly benefit amount, gender, age, the state in which the UI claim was filed, and whether the claimant's previous job was in manufacturing.

After dropping observations with missing values, we have a sample of 1,821 unique individuals. Table 18 defines the variables and displays summary statistics of this sample. The average duration of insured unemployment is 25.09 weeks, while the average potential duration is 41.58 weeks. 40% of workers exhausted their UI and/or EUC benefits. Jobless duration shows that the average time to a stable job (one lasting more than 3 weeks) is 153.23 weeks, though this is censored for 63.7% of our sample (that is, the interview date occurs before the individual finds stable employment). 34% of the sample expected recall, but only 9% had a definite recall date. 83% of workers are reemployed at some point before the survey date, but only 36% find stable jobs in that time.

The Continuous Wage and Benefit History data used in Moffit (1985b) had an average potential duration of 33 weeks with an average duration of only 13 weeks. Of the papers with measures of actual reemployment, Jurajda and Tannery have 86% of their sample becoming reemployed by their censoring date, while Katz and Meyer (1990) have 76%. Neither provide figures for stable reemployment.

Figures 13-15 provide additional information about our duration variables. Figure 13 shows the distribution of benefit duration for our sample. There is a spike in the density at the beginning (workers who only take a few weeks of benefits), and then a general decline with spikes at various exhaustion points: 25-26 weeks, 33-34 weeks, 45-46 weeks, and 51-52 weeks. These spikes are consistent with the exhaustion of benefits (26 weeks of UI and either 0, 8, 20, or 26 weeks of EUC).

Figure 14 shows the distribution of jobless duration for our sample, including workers who did not obtain stable reemployment before the interview date. Here we see a general decline in density until about 26 weeks, higher densities from 26 to 60 weeks, then a steady decline until 120 weeks. The large second bump (from 120 to 280) consists almost entirely of censored observations (individuals who did not obtain stable reemployment before the interview date). Figure 15 omits these censored observations, showing the distribution of jobless duration for the portion of our sample that found stable reemployment.

Econometric Models

Parametric and semi-parametric models

We model the effect of potential duration of benefits on both benefit duration and on jobless duration. These duration models can be written as:

$$\begin{aligned} duration_i = \beta_0 + \beta_1 potdur_i + \beta_2 x_{2i} + \cdots \\ + \beta_K x_{Ki} + u_i \end{aligned} \tag{1}$$

where *duration*$_i$ denotes either the number of weeks of benefits received by worker *i*, or the number of weeks worker *i* was jobless, *potdur*$_i$ denotes the potential duration of

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UI and EUC benefits (combined) for which the worker was eligible at the time of the initial benefit claim, and x_{2i} through x_{Ki} denote controls.

In the case of benefit duration, each observation is a spell of benefit receipt. In the case of jobless duration, each observation is a spell of joblessness. In both cases, no individual contributes more than one spell to the data. Table 18 describes the control variables included in the specification. We include indicators for female, marital status (married; divorced, separated, widowed; with never married as the reference group), highest level of educational attainment (high school dropout; associate's degree or vocational degree; college or graduate degree; some other form of education; with high school only as the reference group) , state in which the UI claim was filed, race (African-American; Asian; Mexican; other non-White; with white as the reference group), whether the claimant's previous job was in manufacturing, whether the claimant expected recall, and whether the claimant had a definite recall date. (Claimants who expected recall may or may not have had a definite recall date). We also include interactions of female with the two indicators for marital status listed above. To control for the labor market conditions facing the worker, we include the monthly state unemployment rate at the time the initial claim for benefits was made. Finally, we include the claimant's average weekly earnings during the year before he or she claimed benefits, the weekly benefit amount for which the claimant was eligible, and age at first claim.

We estimate four versions of equation (1) for both benefit and jobless duration. In the first we ignore censoring issues, simply estimating the model by OLS. In the second we recognize that benefit exhaustion (in the case of benefit duration) and the and of

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end of the survey (in the case of jobless duration) right-censor our dependent variable, and estimate using censored normal regression. For the third version we use the Weibull hazard model, which recognizes the censoring issue, but assumes that the duration distribution is distributed Weibull and that the baseline hazard is monotonically increasing, monotonically decreasing, or stable. The Weibull model can be parameterized either as an accelerated failure time model, providing duration estimates that can easily be compared with estimates from OLS and Censored regression, or as a proportional hazard model, which provides hazard estimates that can be compared with the results from other hazard models. In any proportional hazard model, covariates are assumed to have a proportional effect on the hazard rate. For example, the effect of an extra week of potential benefits is assumed to be the same whether it is an increase from 12 to 13 weeks or an increase from 51 to 52 weeks. While Figures 1-3 suggest that the duration distributions are not distributed Weibull, we include the Weibull model as a bridge between the duration models and the hazard models.

Finally, we employ the Cox proportional hazard model (Cox 1972). This model is semi-parametric in that it does not assume a particular duration nor a corresponding baseline hazard, but it does assume that the conditional hazard is proportional to the baseline hazard (Cameron and Trivedi 2005), meaning that the covariates are assumed to have a proportional effect on the hazard rate just as in the Weibull hazard model. The Cox PH model imposes fewer assumptions than parametric models, and is generally preferred, a priori, to the other models (Box-Steffenmeier & Jones 2004).

Non-parametric hazard models

To estimate the effect of EUC on the probability of reemployment, we specify a flexible discrete-time hazard model with time–varying explanatory variables. This involves transforming the data from spell-level data (in which the unit of observation is the spell of unemployment experienced by a person) to person-week data (in which each observation represents a week a person spends unemployed). Each spell of unemployment contributes T_i observations to the person-week dataset, where T_i is the number of weeks of unemployment experienced by person i.[24] We construct two dependent variables:

$benefitEnd_{it}$, equal to 1 if person i stopped taking unemployment benefits in week t, 0 otherwise.

$reemployment_{it}$, equal to 1 if person i became reemployed in week t, 0 otherwise.

While it is possible that an individual may stop taking benefits for a time before resuming within the same benefit year, $benefitEnd_{it}$ treats all spells as continuous (or compressed). As before, only stable jobs (those lasting more than three weeks) are counted as reemployment events ($reemployment_{it}$). The empirical model for either dependent variable can be written:

[24] See Allison (1984) and Han and Hausman (1990).

$$\Pr(event_{it} = 1|X) = F[\beta_0 + \beta_1 x_{1i} + \beta_2 x_{2i} + \cdots + \beta_K x_{Ki} + \alpha_1 z_{1i} + \alpha_2 z_{2i} + \cdots + \alpha_K z_{Ki} + u_i \tag{2}$$

where $event_{it}$ is either $benefitEnd_{it}$ or $reemployment_{it}$, x_{1i} through x_{Ki} denote K characteristics of person i that do not vary over time, and z_{1it} through z_{Nit} denote N time-varying influences on reemployment, including measures of the potential duration of UI benefits (see below). Setting $F = 1$ implies a linear model in which the coefficients are interpreted as effects of a unit change in an independent variable on the probability of reemployment. A positive estimated coefficient suggests a higher probability of reemployment and hence a shorter spell of unemployment. Coefficient estimates from equation (2) can also be used to infer the effect of each independent variable on jobless duration following the method described in Davidson and Woodbury (1991).

The time-invariant person characteristics we include in equation (2) include indicators for female, marital status (married; divorced, separated, widowed; with never married as the reference group), highest level of educational attainment (high school dropout; associate's degree or vocational degree; college or graduate degree; some other form of education; with high school only as the reference group) , state in which the UI claim was filed, race (African-American; Asian; Mexican; other non-White, with white as the reference group), whether the claimant's previous job was in manufacturing, whether the claimant expected recall, and whether the claimant had a definite recall date. (Claimants who expected recall may or may not have had a definite recall date). We also include interactions of female with the two indicators for marital status listed above. We also include the claimant's age at first claim, base period earnings (earnings during approximately the year before he or she claimed UI benefits), and UI weekly benefit amount.

The main time-varying influence we include in equation (2) is the potential duration of benefits. We model this in two alternative ways. First, we follow Grossman (1989) and Katz and Meyer (1990) and construct a variable indicating the expected potential duration of benefits for which a claimant believes herself eligible in the current week, based on the status of her regular state benefits and EUC in the current week — *maxexp*. This variable is constant during a spell of unemployment unless the potential duration of EUC changes.

Specifically, for weeks when a claimant is drawing regular state benefits, we define *maxexp* as the potential duration of regular benefits for which she was eligible at the initial claim *plus* the potential duration of EUC for which she would be eligible if regular benefits were exhausted in the current week. (EUC entitlements were set at the time a claimant first claimed EUC, typically when she exhausted regular state benefits. Once set, the entitlement could increase but not decrease.) Because the EUC entitlement was not set until a worker exhausted her regular benefits, and EUC durations both rose and fell while workers were drawing regular benefits, *maxexp* can either rise or fall in weeks when a worker is drawing regular benefits.

For weeks when a claimant is drawing EUC, we define *maxexp* as the potential duration of regular benefits for which she was eligible at the initial claim *plus* the potential duration of EUC for which she is eligible in the current week. In this latter case, *maxexp* can change if (and only if) a claimant's EUC entitlement increased because a state changed from low- to high-unemployment status or Congress increased the duration of EUC benefits. We also include a quadratic term to allow variation in the change in the benefit exit hazard (or reemployment hazard) associated with a one-week increase in the maximum potential duration of benefits.

The second approach to specifying the potential duration of benefits is to include two time-varying independent variables in equation (2): the number of weeks until the claimant can expect to exhaust benefits [*wksleft*(t)] and the number of elapsed weeks since the beginning of the spell of unemployment [*elapsed*(t)]. Specifically, for weeks when a claimant is drawing regular state benefits, we define *wksleft*(t) as the number of weeks of regular benefits remaining *plus* the number of weeks of EUC benefits that would be provided if the worker exhausted regular benefits in the current week. For weeks when a claimant is drawing EUC, we define *wksleft*(t) as the number of weeks of EUC benefits currently remaining in week t. Note that *wksleft*(t) typically falls each week as the claimant moves closer to exhausting benefits; however, *wksleft*(t) may also change from one week to the next if the expected potential duration of benefits (*potdur*) changes. As with *maxexp*, we include a quadratic term to allow the effect on benefit exit (or reemployment) to vary over time. Ham and Rea (1987) also used a quadratic specification. We also specify *elapsed*(t) as a quadratic.

To control for the labor market conditions facing the worker, we include the monthly state unemployment rate corresponding to the week being observed in all specifications.

Empirical Findings

The top panel of Table 19 reports the results of applying three alternative estimators to the duration model we have written as equation (1). The left column shows estimated effects of a one-week increase in potential benefit duration—*potdur*—on the expected duration of UI benefit receipt. The right column shows estimated effects of a one-week increase in *potdur* on the duration of joblessness. (We report estimates of the full models from which all estimates in Tables 19 and 20 are derived in Appendix A, Tables A1, A2, A3, and A4.)

The OLS estimates suggest that a one-week increase in potential duration increases the duration of benefit receipt by 0.41 weeks and increases the duration of joblessness by 1.64 weeks, other things equal. Both results are statistically significant at the 5% level. The former estimate is comparable to those of Holen (1977) and Moffitt (1985b), both of whom used OLS to estimate the effect on weeks of benefit receipt. Holen (1977) estimated the effect to be between 0.77 and 0.81 week, whereas Moffitt (1985b) reported estimates of 0.11 weeks for males and 0.19 for females.

The OLS estimator does not account for censoring of the dependent variable; accordingly it is a biased and inconsistent estimator of the effect of potential duration. This is because OLS treats censored durations the same as durations that actually end in a true event (in this case, exit from benefit receipt or joblessness) in the same time period. As will become clear, the bias in this case will be upward—an overstatement of the effect of potential duration on both benefit receipt and on joblessness. For joblessness, the upward bias may be due to the changing size of EUC benefit extensions. The largest increase in potential duration occurred in the early phases of EUC, with maximum extensions as large as 33 weeks early in the program, and as small as 7 weeks by the end of the program. Individuals filing claims near the end of EUC are closer to the interview date, which is the censoring point for our measure of joblessness. Such individuals will have both shorter potential duration and be censored earlier in their jobless experience (that is, they would appear to have shorter jobless spells due to the censoring), thus causing an upward bias in the OLS estimate. This would also explain why our estimate of the effect of a one-week increase in potential duration on jobless duration (an increase of 1.64 weeks) is substantially larger than that estimated by Solon (1979)—0.16 week for a sample of workers in New York. In Solon's case, the increase in benefits was a single increase in benefit duration of 13 weeks, which occurred only for individuals filing later in the sample period. This would make individuals with longer potential duration appear to have shorter spells of joblessness, implying a downward bias to his estimate.

For benefit receipt, censoring occurs at the time of benefit exhaustion. The OLS estimator would be upward-biased if workers were more likely to exhaust benefits during longer EUC extensions. This would be consistent with the idea that EUC benefit extensions increased in response to poor labor market conditions.

Table 19, Panel A also shows the censored regression estimates of the effect of potential duration on benefit receipt and joblessness. Censored regression accounts for censoring of the dependent variable (weeks of benefit or weeks of joblessness) and assumes that the underlying distribution of durations is normal. A one-week increase in potential duration is associated with a 0.17 week increase in benefit receipt (column

1) and a 1.44 week increase in joblessness (column 2). Both results are statistically significant at the 5% level. That both estimates are smaller than the corresponding OLS estimates is consistent with the above reasoning about the bias of the OLS estimator.

Using censored regression estimators, Classen (1979) found that a one-week increase in potential duration leads to at most an additional 0.12 week of benefit receipt; Newton and Rosen (1979) obtained an estimate of 0.6 week; and Moffitt (1985b) obtained estimates of 0.085 week for males, and 0.116 for females. Our estimate for weeks of benefit receipt is similar to Classen's and Moffitt's. Again, the censored regression estimate we obtain is smaller than our OLS estimate (.41 weeks), a pattern also seen in Moffitt's results. Newton and Rosen speculate that their relatively large estimate may have to do with Georgians having higher sensitivity to changes in potential duration, but ultimately conclude that it is "difficult to ascertain" the source of the differences between their results and other estimates.

The censored regression estimates we obtain for both UI duration and weeks of joblessness are lower than their OLS counterparts. This is consistent with the upward bias in the OLS estimators as suggested above. The decline is more dramatic in the case of benefit exit, perhaps because the censoring occurs much sooner than in the case of joblessness.

The bottom row of Panel A reports estimates from a Weibull accelerated failure time estimator, which takes into account the censoring issue and assumes that the underlying distribution of durations can be characterized by the Weibull distribution. (This is equivalent to assuming that the baseline hazard is monotonically increasing, monotonically decreasing, or constant.) The estimate of 0.001 (in Column 1) suggests that an additional week of potential benefit duration has no significant effect on the duration of benefit receipt. Similarly, the estimate on 0.014 (in Column 2) suggests that an additional week of potential benefit duration also has a negligible (and statistically insignificant) effect on weeks jobless. Both estimates are far smaller than those obtained using the OLS and censored regression estimators.

Perhaps the most informative comparison is with Moffitt's (1985b) estimates, which assume the distribution of insured spells is exponential (a special case of the Weibull). He finds no significant effect on benefit receipt for either males or females. It is worth noting that Moffitt's results show the same pattern as ours—OLS estimates are larger than censored regression estimates (which assume a normal distribution), and Weibull (exponential) estimates are the smallest and statistically insignificant.[25]

If we assume that the underlying distribution of spells is closer to a Weibull than to a normal distribution, then the Weibull estimates are preferable to the censored regression estimates given earlier. Nevertheless, the distributions shown in Figures 1, 2, and 3 do not appear consistent with either assumption, so the estimators are still likely to be inconsistent.

Panel B of Table 19 again shows results of applying the Weibull estimator, but this time in hazard form. These estimates differ from the first three we report in that we are now looking at the effect of potential duration on the probability of benefit exit rather than on the duration of benefit receipt. (Similarly, we are looking at the effect

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[25] Katz and Ochs (1990) also use an estimator that assumes an exponential distribution and find that an additional week of potential duration increased jobless duration by 0.17–0.23 weeks.

of potential duration on the probability of becoming reemployed rather than the duration of joblessness.) A negative estimate implies a decrease in the probability of exit from benefit receipt (or joblessness) and an increase in the duration of benefit receipt (or joblessness). The Weibull hazard estimate of -0.001 (column 1) suggests that an additional week of potential benefit duration decreases in the likelihood of benefit exit, though the estimate is not statistically significant. The estimate of -0.009 (column 2) suggests that a one-week increase in potential duration is associated with a (marginally statistically significant) 0.009 decrease in the hazard of moving to reemployment.

Panel B also reports estimates from the Cox proportional hazard model. In this case, an added week of potential benefits implies a 0.003 decrease in the hazard of benefit exit, and a 0.006 decrease in the hazard of reemployment, although neither estimate is statistically significant. Katz and Meyer (1990) estimate a comparable model and find a hazard coefficient of -0.025 for benefit exit—an order of magnitude larger than our estimate of -0.003.

As mentioned above, the Cox model is less restrictive than the Weibull hazard model; however, it does impose the proportional hazards assumption. When we test this assumption using a Schoenfeld residuals test, we soundly reject the null hypothesis that the proportional hazards assumption holds in either the benefit exit and reemployment regressions.[26] This implies that the estimated standard errors are inconsistent and that the coefficient on potential duration overstates the effect on the

[26] The chi-square test statistic is 124.57 for the benefit exit regression, and 64.97 for the reemployment regression, each with 37 degrees of freedom. Both reject the null of proportional hazards at the 99% level.

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hazard in both regressions (Box-Steffensmeier and Zorn 2001). Accordingly, it makes sense to move to a fully non-parametric with these data.

Table 20 reports the results of estimating four alternative specifications of equation (2). In Panel A, potential benefit duration is specified as the maximum expected duration of all UI benefits (both regular state and EUC) the claimant could expect to receive in the observed week—*maxexp*—and its square. The left column reports the estimated coefficients from models in which benefit exit is the dependent variable. The estimated coefficient on *maxexp* (-0.002) suggests that an added week of potential benefits decreases the hazard of benefit exit by 0.2%, although the positive estimate on the quadratic term also suggests that this effect may diminish as *maxexp* increases. (The main effect is statistically significant, and the quadratic term is marginally so.) For an individual with 26 weeks of potential benefits, the marginal effect of an added week of potential benefits on benefit exit is -0.0018—a 0.18% decrease in the conditional probability of benefit exit. As shown in Appendix B (Table B1),[27] this marginal effect corresponds to a .08-week increase in benefits received. This estimate is at the low end of estimates obtained by others by others who have used hazard models (Moffitt 1985a, 1985b; Ham and Rea 1987; and Katz and Meyer 1990), whose estimates range from .10 to .35 weeks. Although the sizes of the estimated effects vary, all suggest that increased potential duration may reduce the probability of ending a spell of UI and lengthen the duration of benefit receipt. Because the estimates reported in Table 20 make the fewest restrictive assumptions

[27] The conversion from hazard rates to expected duration follows the method outlined in Davidson and Woodbury (1991). See Tables B1 and B2 for the hazard functions from which the durations are calculated.

about the distribution of spells, their findings should be preferred to the Weibull or Cox estimates.

The right column of Panel A shows the estimated effect of *maxexp* on the conditional probability of reemployment. The coefficient of -0.00008 suggests that an added week of potential benefits decreases the hazard of reemployment by only .008%, and the estimate is statistically insignificant. The quadratic term again indicates that this effect may diminish at higher levels of potential duration. (Taken together, *maxexp* and $maxexp^2$ are jointly significant at the 95% level.) As shown in Appendix Table B2, these estimates suggest that the effect of an added week of UI benefits for an individual with 26 weeks of benefits, is a negligible .01-week increase in weeks jobless. The existing literature offers few opportunities for comparison, as past studies using hazard models have modeled the probability of exiting UI, not the probability of exiting a jobless spell. Juarajda and Tannery (2003) do have data on jobless spells and find that the new job hazard is depressed by increased potential duration, but they do not report a marginal change that would allow a clear comparison with our estimates.

Panel B of Table 20 displays estimates from models in which potential benefit duration is specified as the number of weeks of all UI benefits remaining for the claimant in the observed week—*wksleft*—and its square. This specification also includes a control for the number of weeks in the current spell (that is, weeks since the start of benefits). Using this specification, the estimated effects of potential benefit duration on the conditional probabilities of UI exit and on reemployment are larger (and also statistically significant) than in those from the *maxexp* specifications. In particular, the estimated coefficients on the main effects (*wksleft*) are -0.008 (for UI exit) and -0.0002 (for reemployment). The marginal effect of an added week of benefits on the UI exit hazard at 26 weeks of benefits is -0.0047, a larger decrease than in the *maxexp* specification, and statistically significant. As shown in Appendix Table B1, these hazard estimates correspond to a .28-week increase in benefits received in response to one additional week of benefit entitlement. This is close to the upper end of the range of effects estimated by other authors using hazard models (Moffitt 1985a,b; Ham and Rea 1987; Katz and Meyer 1990), which range from .10 to .35 weeks.

The right column of Panel A shows the estimated effect of *wksleft* on the conditional probability of reemployment. The estimated marginal effect of *wksleft* on the conditional probability of reemployment is again larger than the estimated effect of *maxexp*, and corresponds to a .06-week increase joblessness for workers at 26 weeks of benefits. Although somewhat larger than the effect of potential duration on jobless duration estimated using the *maxexp* specification, it is still quite small.

Estimates from the *wksleft* specifications are substantially larger than those from the *maxexp* specifications. Is there a reasonable way of choosing between the two? The *wksleft* specifications have a better fit than their *maxexp* counterparts, as measured by the adjusted-R^2. Although it would be useful to explore differences between to the two specifications further, the better fit of the *wksleft* specifications may imply that workers respond more to their weeks of remaining benefits than to the maximum potential duration of benefits. If so, using maximum potential duration of benefits as the key right-hand-side variable in parametric models (like those reported in Table 19) may be yet another specification error to which those models are vulnerable.

The nonparametric estimates in Table 20 suggest that an addition week of potential benefit duration increases a typical UI spell by about .08-.28 weeks (and these estimates are statistically significant at conventional levels). The estimated effect of an addition week of potential benefit duration on a typical jobless spell is substantially less—only .01-.06 weeks (and only the higher end of this range is statistically significant at the 5-percent level). Why are the estimated effects of increased UI benefit duration on UI spells larger than the estimated effects on the length of jobless spells? Consider a one-week reduction in the potential benefit duration of a worker who exhausts benefits. By definition, such a worker would receive one less week of UI benefits, but his or her jobless duration may not change at all. It makes sense, then, that estimates of the effect of potential benefit duration on the length of UI spells should be larger than estimated effects on the length of jobless spells.

Summary and Conclusions

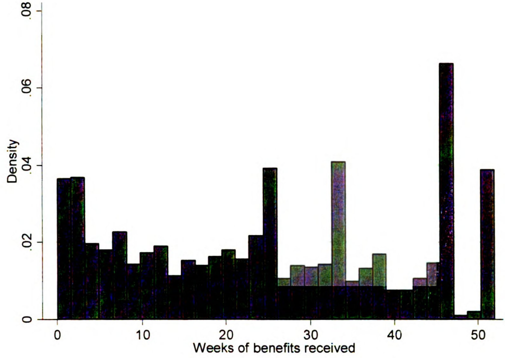
When we apply a range of econometric estimators to a high-quality dataset that includes information on both the duration of UI spells and the duration of jobless spells, we obtain a wide range in the estimated effect of extended benefits on the duration of UI benefit receipt and the duration of joblessness. The estimated effect of an additional week of potential duration on benefit receipt ranges from .00-.41 weeks (Tables 19, 20, and Appendix Table B1). The most convincing estimates—that is, the estimates that come from models that impose the fewest restrictions—come from non-parametric hazard models (Table 19 and Appendix Table B1) and suggest a narrower range of.08-.28 weeks.

The estimated effects of an additional week of potential duration on weeks jobless cover a wider range—from .01-1.64 weeks (Tables 19, 20, and Appendix Table B2). Estimates at the high end of this range come from the parametric and semi-parametric estimators. For example, OLS and censored regression suggest that an additional week of potential benefit duration increases jobless duration by about 1.5 weeks. In contrast, nonparametric estimates, which impose fewer restrictions, suggest far more modest effects in the range of .01-.06 weeks.

The estimates we find most convincing are the nonparametric estimates reported in Table 20 and Appendix Tables B1 and B2. These estimates suggest that a 13-week increase in potential benefits (as in phase 1 of EUC) would increase a typical UI spell by about 1.0-3.6 weeks, and would increase the typical jobless spell by .1-.8 week.

Because we have used a single dataset, we can safely attribute the variation in our estimates to differences in model specification and the estimators we apply to the data. The findings suggest that such differences substantially affect the inferences we derive from the data. To the extent possible, we have traced the connections between these differences in specification and estimators (on one hand) and the disparate results found in existing research (on the other). For the most part, the inferences we derive are consistent with the inferences past researchers have obtained using various datasets and a range of estimators. Perhaps the most consistent finding we have is that estimates of the effect of potential benefit duration on weeks of benefit receipt often bear little relation to the estimates of the effect of potential duration on weeks of joblessness.

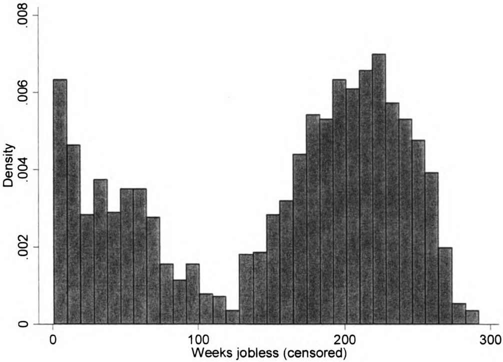
Figure 13
Distribution of Benefit Duration



Notes:

1. Spikes occur at exhaustion points: 26, 34, 46, and 52 weeks. (26 weeks of UI and either 0, 8, 20, or 26 weeks of EUC).
2. Sample size = 1,821

Figure 14
Distribution of Jobless Duration (including censored observations)



Notes:
1. The large increase in density after 120 weeks is almost entirely due to censored observations. (Workers who fail to find stable reemployment before the interview date.) See Figure 3 for the distribution without censored observations.
2. Sample size = 1,821

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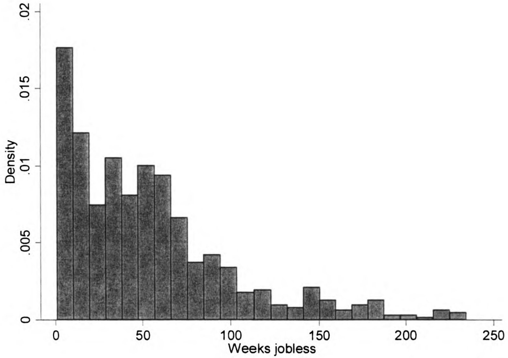
Figure 15
Distribution of Jobless Duration (excluding censored observations)



Notes:
1. Only includes workers who found stable reemployment (> 3 weeks) before the interview date.
2. Sample size = 656

Table 15
Summary of Extended Unemployment Benefit Programs, 1958 to present

Table 15
Summary of Extended Unemployment Benefit Programs, 1958 to present

Program and enabling legislation	Effective dates and extensions	Potential duration of extended benefits provided	Financing	Notes
Temporary Unemployment Compensation Act, P.L. 85-441	6/58 - 7/59	50% of regular state duration, up to 13 weeks	Interest-free loans to 17 participating states	State participation voluntary
Temporary Extended Unemployment Compensation Act (TEUC), P.L. 87-6	4/61 - 6/62	50% of regular state duration, up to 13 weeks	Temporary increases in Federal Unemployment Tax (.4% in 1962, .25% in 1963)	
Extended Unemployment Compensation Act of 1970 (EB), P.L. 91-373, with major amendments in P.L. 96-364, P.L. 96-499, P.L. 97-35, P.L. 102-318	8/70 to present	50% of regular state duration, up to 13 weeks	One-half from Federal Unemployment Tax revenues paid to Extended Unemployment Compensation Account; one-half from state UI reserves	EB activated in a state by an insured unemployment rate (IUR) trigger, 8/70 to present; EB could activate in all states by a national IUR trigger, 8/70-8/81. Effective 1981, EB denied to claimants refusing to seek or accept suitable work, and to claimants who had quit or been discharged. State triggers made more restrictive, 8/81. Eligibility for EB more restrictive, effective 9/82. States permitted to adopt a total unemployment rate (TUR) trigger, 6/92.

Table 15 (continued)

Table 15 (continued)

Program and enabling legislation	Effective dates and extensions	Potential duration of extended benefits provided	Financing	Notes
Emergency Unemployment Compensation Act, P.L. 92-224, P.L. 92-329	1/72 - 9/72, extended to 3/73	50% of regular state duration, up to 13 weeks	Extended Unemployment Compensation Account (EUCA)	State-level triggers (different from EB triggers) used to activate program
Federal Supplemental Benefits (FSB), P.L. 93-572, P.L. 94-12, P.L. 94-45, P.L. 95-19	1/75 - 12/76, extended to 1/78	50% of regular state duration, up to 13 weeks (1/75-2/75 and 5/77-1/78); additional 50% of regular state duration, up to 13 weeks provided 3/75-4/77 (that is, up to 26 weeks of FSB total)	Repayable advances to EUCA from general revenues; general revenues after 3/77	EB program activated in all states, so total potential benefit duration was 65 weeks for those exhausting EB between 3/75 and 4/77. State-level triggers applied starting 1/76. Uniform federal eligibility and disqualification standards implemented 4/77 (P.L. 95-19)

Table 15 (continued)

Table 15 (continued)

Program and enabling legislation	Effective dates and extensions	Potential duration of extended benefits	Financing	Notes
Federal Supplemental Compensation (FSC), P.L. 97-458, P.L. 94-424, P.L. 98-21, P.L. 98-135	9/82 – 3/83, extended to 9/83 and 3/85	FSC-I (9/82-1/83): 50% of regular state duration, up to 6 or 10 weeks. FSC-II (1/83-3/83): 65% of regular state duration, up to 8 or 16 weeks. FSC-III (4/83-9/83): 55% of regular state duration, up to 8 or 14 weeks. FSC-IV (10/83-3/85): Same as FSC-III, but entitlement did not vary once established.	General revenues	Potential duration varied with state's EB status and separate FSC triggers. Except in FSC-IV, potential duration would vary when state's EB or FSC status changed. FSC-I and FSC-II exhaustees could collect FSC-III benefits, but not FSC-IV benefits. EB eligibility criteria applied to all phases of FSC. Available regular state benefits and EB (if activated) had to be exhausted to receive FSC.

Table 15 (continued)

Table 15 (continued)

Program and enabling legislation	Effective dates and extensions	Potential duration of extended benefits provided	Financing	Notes
Emergency Unemployment Compensation Act of 1991 (EUC), P.L. 102-164, P.L. 102-182, P.L. 102-244, P.L. 102-318, P.L. 103-6, P.L. 103-152	11/91 – 6/92, extended to 7/92, 3/93, 10/93, and 2/94	EUC-I (11/91-2/92): lesser of 100% of regular benefits, or 13 or 20 weeks. EUC-II (2/92-7/92): lesser of 130% of regular benefits, or 26 or 33 weeks. EUC-III (7/92-3/93): lesser of 100% of regular benefits, or 20 or 26 weeks. EUC-IV (3/93-10/93): lesser of 60% of regular benefits, or 10 or 15 weeks. EUC-V (10/93-2/94): lesser of 50% of regular benefits, or 7 or 13 weeks	EUC-I, EUC-II, and EUC-V from Extended Unemployment Compensation Account; EUC-III and EUC-IV from general revenues	Potential duration determined at time of filing; depended on state's classification as high- or low-unemployment. EUC entitlement could increase if state moved from low to high status or if program became more generous; EUC entitlement could not decrease. Claimants exhausting benefits between 3/91 and 11/91 could receive benefits under "reach-back" provisions (but no retroactive benefits paid). Under EUC-III and EUC-IV, claimants had option of drawing EUC benefits at the start of a new benefit year (see text). EB eligibility criteria applied to all phases of EUC. Once EUC exhausted, a claimant needed to regain regular UI eligibility to receive additional EUC.

Table 15 (continued)

Table 15 (continued)

Program and enabling legislation	Effective dates and extensions	Potential duration of extended benefits provided	Financing	Notes
Temporary Extended Unemployment Compensation Act of 2002 (TEUC), P.L. 107-147	3/02-12/02, extended to 8/03	50% of regular benefit duration, up to 13 weeks; additional 50% of regular benefits, up to 13 weeks, in high unemployment states.	Extended Unemployment Compensation Account	Additional extended benefits available in states based on state insured unemployment rate.

Table 16
Potential Durations of Extended UI Benefits in Pennsylvania Under Emergency Unemployment Compensation

EUC phase	Dates	Unemployment level	Potential duration (weeks)	Notes
I	11/17/91 – 1/25/92	L	13	EUC I began 11/17/91 P.L. 102-164 & 102-182
I	1/26/92 – 2/08/92	H	20	
II	2/09/92 – 6/13/92	H	33	EUC II approved 2/07/92 P.L. 102-244, 33 or 26 weeks for claims on or before 6/13/92
II	6/14/92 – 7/04/92	H	26	Per P.L. 102-244, 13 (L) and 20 (H). This was changed retroactively. See note 2.
III	7/05/92 – 8/15/92	H	26	EUC III approved 7/03/92 P.L. 102-318
III	8/16/92 – 3/20/93	L	20	
IV	3/21/93 – 6/19/93	H	26	EUC IV approved 3/04/93 P.L. 103-6. Limits are the same as EUC III.
IV	6/20/93 – 9/11/93	L	20	
IV	9/12/93 – 10/02/93	L	10	
V	10/03/93 – 2/05/94	L	7	EUC V approved 11/24/93, See note 3.

Notes:

1. Per P.L. 102-318, the reduction in the maximum EUC entitlement when the NTUR fell below 7% affected only new EUC claimants, not those already collecting benefits.
2. As noted above, according to P.L. 102-244, maximums of 20 or 13 weeks would apply to new claims made after 6/13/92. However, in P.L. 102-318, this was changed retroactively to 20 or 26 weeks for new claims made after 6/13/92.
3. EUC legislation lapsed from 10/03/93 to 11/27/93. When EUC V was approved on 11/24/93, the new EUC entitlements were made retroactive to 10/03/96. (Per conversations with Mike Miller at DOL/ETA/UIS.)
4. EUC trigger reports for 7/11/93 - 7/31/93 indicate a potential duration under EUC of 10 weeks. According to Mike Miller, the maximum of 20 weeks was maintained throughout the period from 6/20/93 to 9/11/93. Due to confusion over interpretation of NTUR, the 20 week maximum was made retroactive to cover the weeks in July that were reported as 10 weeks.

Table 17
Estimated Effects of Increased Potential Duration of UI Benefits for the United States and Canada

Study	Data	Change in weeks of unemployment from 1 added week of potential UI	Remarks
Holen (1977)	UI claimants in San Francisco, Boston, Phoenix, Seattle, Minneapolis, 1969-70	0.77–0.81	OLS linear duration estimates
Solon (1979)	UI exhaustees in New York, 1972-1973 (pre- and post-EB, surveyed after exhaustion)	0.16	OLS linear duration estimates
Classen (1979)	UI claimants in Arizona and Pennsylvania, 1967-69	0–0.12	Tobit duration estimtates
Newton and Rosen (1979)	UI recipients in Georgia, 1974-76	0.6	Tobit duration estimates
Katz and Ochs (1980)	Current Population survey, individuals in 26 states, 1968-70 and 1973-77	0.17–0.23	Maximum likelihood duration estimates
Moffitt and Nicholson (1982)	Recipients of EB and FSC, 15 states, 1975-77	0.1	Labor supply model, maximum likelihood
Moffitt (1985a)	Continuous Wage and Benefit History, 1978-83	0.15	UI exit rate estimates
Moffitt (1985b)	Continuous Wage and Benefit History, 1978-83:		UI exit rate estimates
	White men	0.17	
	White women	0.10	
	FSB and EB recipients in 15 states, 1975-78:		Maximum likelihood duration estimates
	Men	0.45	
	Women	0.28	
	UI recipients in Georgia, 1974-76:		Maximum likelihood duration estimates
	Men	0.17	
	Women	0.37	

Table 17 (continued)

Study	Data	Change in weeks of unemployment from 1 added week of potential UI	Remarks
Solon (1985)	UI claimants in Georgia, 1978-79	0.36	Maximum likelihood duration estimates
Ham and Rea (1987)	Canadian men, 1975-80	0.26–0.35	UI exit rate estimates
Grossman (1989)	Continuous Wage and Benefit History, individuals in 3 states, 1981-1984	0.9	UI exit rate estimates of FSC impacts
Katz and Meyer (1990)	Continuous Wage and Benefit History, individuals in 3 states, 1981-84	0.16–0.20	UI exit rate estimates
Jurajda and Tannery (2003)	Continuous wage and Benefit History, Pittsburg and Philadelphia, 1981-84	not reported	New job rate estimates - hazard of new job is decreased by increased potential duration

Table 18
Brief Variable Definitions and Sample Summary Statistics

Variable	Description	Mean	Std. dev.
benefit duration	Weeks of benefits received	25.09	16.08
jobless duration	Weeks from initial claim until taking a job lasting at least 3 weeks (censored)	153.23	84.65
reemployed	1 if became reemployed by the survey date, 0 otherwise	0.83	0.37
stable reemployed	1 if became reemployed in a job lasting at least 3 weeks by the survey date, 0	0.36	0.48
potential benefit duration	Sum of weeks of UI and EUC the individual is eligible to receive	41.58	8.79
female	1 if female, 0 otherwise	0.41	0.49
age	Age in years	39.06	11.89
married	1 if currently married	0.63	0.48
not married	1 if divorced, separated, or widowed; 0 otherwise	0.16	0.37
female, not married	1 if female and married, 0 otherwise	0.09	0.28
female, married	1 if female and not married, 0 otherwise	0.25	0.43
dropout	1 if less than high school, 0 otherwise	0.16	0.36
post secondary	1 if associate's or vocational degree, 0 otherwise	0.17	0.37
college	1 if college or graduate degree, 0 otherwise	0.14	0.34
other education	1 if some other form of education, 0 otherwise	0.02	0.13
african-american	1 if African-American, 0 otherwise	0.11	0.32
asian	1 if Asian, 0 otherwise	0.01	0.12
mexican	1 if Mexican, 0 otherwise	0.07	0.26
other non-white	1 if Non-White, 0 otherwise	0.05	0.21

Table 18 (continued)

Variable	Description	Mean	Std. dev.
manufacturing	1 if previous job in manufacturing, 0 otherwise	0.33	0.47
state unemployment rate	Unemployment rate in the state at the time of claiming benefits (%)	7.37	1.86
previous weekly earnings	Average weekly earnings ($10) in the year preceeding benefit collection	45.95	30.33
expect recall	1 if worker expects recall, 0 otherwise	0.34	0.47
definite recall date	1 if worker has definite recall date, 0 otherwise	0.09	0.29
weekly benefit amount	UI/EUC weekly benefit amount	183.79	68.37
exhausted benefits	1 if exited UI/EUC by exhausting benefits, 0 otherwise	0.40	0.49

sample size = 1,821

Source: Author's tabulations of variables constructed from the Emergency Unemployment Compensation Public Use File compiled by Mathematica Policy Research, Inc. All calculations are computed using spell data.

Notes:

1. The variable jobless duration denotes the time from the initial claim to the first stable reemployment (a job lasting at least 3 weeks). For workers interviewed before finding stable reemployment, jobless duration is time from the initial claim to the interview date.

2. Demographic variables (those involving gender, marital status, race, or educational attainment) were collected at the time of the survey, not at the time of the initial claim.

Table 19
Estimated Effects of Increased Potential Benefit Duration from Parametric and Semi-Parametric Models

Estimation procedure		
Panel A: Duration estimates	Change in weeks of benefit duration from 1 added week of potential benefits	Change in weeks jobless from 1 added week of potential benefits
OLS	0.413	1.639
	(0.046)	(0.252)
adjusted R-squared	0.1777	0.0990
Censored regression	0.172	1.441
	(0.074)	(0.292)
log likelihood	-5385.7	-5009.5
Weibull accelerated	0.001	0.014
failure time	(0.004)	(0.008)
log likelihood	-2442.2	-2161.9
Panel B: Hazard estimates	Change in benefit exit hazard from 1 added week of potential benefits	Change in reemployment hazard from 1 added week of potential benefits
Weibull hazard	-0.001	-0.009
	(0.004)	(0.005)
log likelihood	-2442.2	-2161.9
Cox proportional	-0.003	-0.006
hazard	(0.004)	(0.005)
log likelihood	-7404.1	-4748.1
sample size	1,821	1,821

Notes:

1. Estimates of equation (1). Panel A contains estimates in which the dependent variable is either weeks of benefits received during the benefit year (column 1) or weeks jobless (column 2). See Appendix A, Tables A1 and A2 for estimates of the complete model. OLS and Censored regression estimates are the change in weeks of benefit duration or jobless duration with respect to a 1 week increase in potential benefits. Weibull accelerated failure time estimates give the proportional change in benefit duration or jobless duration with respect to a 1 week increase in potential benefits. Panel B gives estimates in which the dependent variable is either benefit exit or reemployment. See Appendix A, Table A3 for estimates of the complete

Table 19 (continued)

model. Estimated coefficients give the proportional change in the hazard of benefit exit or jobless duration with respect to a 1 week increase in potential benefits. Standard errors in parentheses.

2. Included control variables: previous weekly earnings, weekly benefit amount, age, monthly state unemployment rate, and indicators for gender, marital status, interactions of gender and marital status indicators, educational attainment, state, race, manufacturing, expected recall, and recall with a definite date.

Table 20
Estimated Effects of Increased Potential Benefit Duration from Nonparametric Models

Panel A	**Change in benefit exit hazard from 1 added week of potential benefits**	**Change in reemployment hazard from 1 added week of potential benefits**
maxexp	-0.002076	-0.000079
	(0.000592)	(0.000047)
$maxexp^2$	0.000012	0.000002
	(0.000008)	(0.000001)
adjusted R-squared	0.0050	0.0011
sample size	33,048	254,191
Panel B		
wksleft	-0.007675	-0.000203
	(0.000446)	(0.000030)
$wksleft^2$	0.000118	0.000003
	(0.000008)	(0.000001)
adjusted R-squared	0.0443	0.0025
sample size	33,048	254,191

Notes:

1. Estimates of equation (2). See text for further discussion. See Appendix A, Table A4 for estimates of the complete model. Estimated coefficients give the change in the probability of benefit exit or reemployment with respect to a 1 week increase in potential benefits. Standard errors in parentheses.

2. Included control variables: previous weekly earnings, weekly benefit amount, age, monthly state unemployment rate, and indicators for gender, marital status, interactions of gender and marital status indicators, educational attainment, state, race, manufacturing, expected recall, and recall with a definite date.

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APPENDICES

APPENDIX A

Table A1
Benefit Duration Estimates

	OLS	Censored regression	Weibull (AFT)
potential benefit duration	0.4129	0.1719	0.0007
	(0.0460)	(0.0742)	(0.0044)
female	-1.3360	-2.1761	-0.0203
	(1.5941)	(2.5015)	(0.1454)
age	0.2702	0.3748	0.0169
	(0.1754)	(0.2837)	(0.0175)
age squared	-0.0001	-0.0001	-0.0000
	(0.0002)	(0.0003)	(0.0000)
married	-3.7177	-5.7919	-0.2702
	(1.2227)	(1.9103)	(0.1086)
not married	-1.6670	-1.3791	-0.0320
	(1.6459)	(2.6293)	(0.1584)
female, not married	1.8769	3.2371	0.0762
	(2.3419)	(3.7427)	(0.2252)
female, married	3.7301	6.9092	0.3007
	(1.8360)	(2.8837)	(0.1681)
dropout	-0.3215	0.1131	0.0336
	(1.0589)	(1.6544)	(0.0953)
post secondary	2.8040	4.4008	0.3114
	(0.9765)	(1.5495)	(0.0936)
college	1.5697	3.1390	0.2163
	(1.1089)	(1.7731)	(0.1082)
other education	5.4173	5.8423	0.0916
	(2.7455)	(4.4528)	(0.2578)
african-american	3.5408	6.2799	0.3940
	(1.1923)	(1.9455)	(0.1238)
asian	-4.4658	-6.2346	-0.3339
	(3.0331)	(4.6214)	(0.2652)
mexican	2.2149	3.4075	0.2354
	(1.5630)	(2.4798)	(0.1430)
other non-white	1.3566	1.8871	0.1381
	(1.7050)	(2.7550)	(0.1694)

Table A1 (continued)

	OLS	Censored Regression	Weibull (AFT)
manufacturing	-1.0527	-0.9298	-0.1482
	(0.7633)	(1.1947)	(0.0683)
state unemployment rate	0.7988	1.0521	0.0483
	(0.5078)	(0.7855)	(0.0464)
previous weekly earnings	0.0101	0.0044	0.0002
	(0.0149)	(0.0232)	(0.0013)
expect recall	-4.0104	-6.6714	-0.3789
	(0.8454)	(1.3060)	(0.0753)
definite recall date	-9.0943	-12.6815	-0.8681
	(1.3362)	(1.9871)	(0.1039)
weekly benefit amount	0.0085	0.0196	0.0013
	(0.0071)	(0.0112)	(0.0007)
constant	-8.1138	-0.7546	2.2411
	(5.0617)	(8.0015)	(0.4753)
sample size	1,821	1,821	1,821
adjusted R-squared	0.1777	--	--
Log likelihood	--	-5385.7	-2442.2

Notes:

1. Estimates of equation (1) in which weeks of benefits received during the benefit year is the dependent variable. OLS and censored regression estimates independent variable. Weibull AFT estimates give the proportional change in benefit duration with respect to a unit change in each independent variable. Standard errors in parentheses.
2. All specifications also include state indicators.

Table A2
Jobless Duration Estimates

	OLS	Censored regression	Weibull (AFT)
potential benefit duration	1.6435 (0.2535)	1.4408 (0.6604)	0.0139 (0.0082)
female	-5.0420 (8.7863)	-17.0974 (21.6193)	-0.1900 (0.2389)
age	3.1319 (0.9670)	7.8451 (2.5860)	0.0978 (0.0327)
age squared	-0.0024 (0.0011)	-0.0055 (0.0030)	-0.0001 (0.0000)
married	13.3215 (6.7394)	29.9840 (17.1283)	0.3821 (0.2026)
not married	20.7402 (9.0719)	44.4206 (23.7364)	0.5187 (0.2928)
female, not married	-20.1865 (12.9082)	-48.5688 (33.0753)	-0.5571 (0.3931)
female, married	0.9796 (10.1199)	13.8123 (25.4291)	0.1900 (0.2945)
dropout	11.5686 (5.8365)	22.9998 (15.8824)	0.2555 (0.2058)
post secondary	-6.0042 (5.3825)	-18.9077 (13.8622)	-0.2453 (0.1652)
college	-11.1150 (6.1122)	-28.9345 (15.7627)	-0.3545 (0.1887)
other education	-4.8369 (15.1330)	-14.1281 (39.2833)	-0.1562 (0.4641)
african-american	8.0855 (6.5717)	12.0489 (17.1702)	0.0498 (0.2026)
asian	12.5858 (16.7182)	46.3573 (46.6493)	0.5960 (0.6051)
mexican	25.3250 (8.6148)	47.8970 (22.8936)	0.5166 (0.2919)
other non-white	2.7780 (9.3977)	8.3174 (24.8922)	0.1724 (0.3136)
manufacturing	-1.1818 (4.2073)	-2.9079 (11.0689)	-0.0185 (0.1370)
state unemployment rate	12.9625 (2.7990)	18.4753 (7.3581)	0.1985 (0.0894)
previous weekly earnings	-0.0267 (0.0819)	-0.1705 (0.2158)	-0.0028 (0.0026)

Table A2 (continued)

	OLS	Censored regression	Weibull (AFT)
expect recall	5.9793	15.3085	0.2241
	(4.6594)	(12.2895)	(0.1532)
definite recall date	3.5007	19.7948	0.1949
	(7.3652)	(20.1784)	(0.2622)
weekly benefit	-0.0733	-0.1081	-0.0009
amount	(0.0391)	(0.1039)	(0.0012)
constant	-88.5374	-143.9320	1.8464
	(27.8994)	(73.2411)	(0.9013)
sample size	1,821	1,821	1,821
adjusted R-squared	0.0990	–	–
Log likelihood	–	-5009.5	-2161.9

Notes:

1. Estimates of equation (1) in which weeks jobless is the dependent variable. OLS and censored regression estimates give the change in jobless duration with respect to a unit change in each independent variable. Weibull AFT estimates give the proportional change in jobless duration with respect to a unit change in each independent variable. Standard errors in parentheses.
2. All specifications also include state indicators.

Table A3
Benefit Exit and Reemployment Hazard Estimates - Parametric and Semi-Paremetric

	Weibull (hazard)		Cox PH	
	Benefit exit	Reemployment	Benefit exit	Reemployment
potential benefit	-0.0007	-0.0090	-0.0028	-0.0055
duration	(0.0043)	(0.0053)	(0.0044)	(0.0055)
female	0.0197	0.1226	0.0099	0.1194
	(0.1409)	(0.1541)	(0.1389)	(0.1538)
age	-0.0163	-0.0631	-0.0153	-0.0635
	(0.0169)	(0.0210)	(0.0166)	(0.0208)
age squared	0.0000	0.0000	0.0000	0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
married	0.2620	-0.2466	0.2558	-0.2286
	(0.1052)	(0.1306)	(0.0984)	(0.1334)
not married	0.0310	-0.3347	0.0182	-0.3140
	(0.1535)	(0.1888)	(0.1528)	(0.1873)
female, not married	-0.0738	0.3595	-0.0487	0.3397
	(0.2183)	(0.2535)	(0.2160)	(0.2520)
female, married	-0.2915	-0.1226	-0.2839	-0.1303
	(0.1628)	(0.1900)	(0.1617)	(0.1913)
dropout	-0.0326	-0.1649	-0.0335	-0.1526
	(0.0924)	(0.1327)	(0.0905)	(0.1331)
post secondary	-0.3019	0.1583	-0.3026	0.1448
	(0.0907)	(0.1065)	(0.0921)	(0.1064)
college	-0.2097	0.2287	-0.2123	0.2253
	(0.1048)	(0.1216)	(0.1055)	(0.1234)
other education	-0.0888	0.1008	-0.0868	0.0910
	(0.2499)	(0.2995)	(0.1881)	(0.3016)
african-american	-0.3820	-0.0321	-0.3846	-0.0433
	(0.1199)	(0.1307)	(0.1254)	(0.1259)
asian	0.3237	-0.3846	0.3293	-0.3714
	(0.2571)	(0.3903)	(0.2476)	(0.4069)
mexican	-0.2282	-0.3334	-0.2138	-0.3214
	(0.1386)	(0.1882)	(0.1321)	(0.1914)
other non-white	-0.1338	-0.1113	-0.1268	-0.1200
	(0.1642)	(0.2023)	(0.1559)	(0.2037)
manufacturing	0.1436	0.0119	0.1369	0.0172
	(0.0662)	(0.0884)	(0.0654)	(0.0909)

Table A3 (continued)

	Weibull (hazard)		Cox PH	
	Benefit exit	Reemployment	Benefit exit	Reemployment
state unemployment	-0.0469	-0.1281	-0.0481	-0.1084
rate	(0.0449)	(0.0576)	(0.0465)	(0.0580)
previous weekly	-0.0002	0.0018	-0.0002	0.0018
earnings	(0.0013)	(0.0017)	(0.0013)	(0.0016)
expect recall	0.3673	-0.1446	0.3766	-0.1354
	(0.0728)	(0.0988)	(0.0714)	(0.1000)
definite recall date	0.8416	-0.1258	0.8270	-0.1158
	(0.1006)	(0.1692)	(0.1028)	(0.1773)
weekly benefit	-0.0012	0.0006	-0.0013	0.0005
amount	(0.0006)	(0.0008)	(0.0006)	(0.0008)
constant	-2.1726	-1.1915	--	--
	(0.4631)	(0.5861)		
sample size	1,821	1,821	1,821	1,821
Log likelihood	-2442.2	-2161.9	-7404.1	-4748.1

Notes:

1. Estimates of equation (1) with distributional assumption and dependent variable as noted. Estimated coefficients give the proportional change in the hazard of benefit exit or jobless duration with respect to a unit change in each independent variable. Standard errors in parentheses.
2. All specifications also include state indicators

Table A4
Benefit Exit and Reemployment Hazard Estimates - Non-Parametric

	maxexp		*wksleft*	
	Benefit exit	Reemployment	Benefit exit	Reemployment
maxexp	-0.00208	-0.00008	--	--
	(0.0006)	(0.0000)		
$maxexp^2$	0.00001	0.00000	--	--
	(0.0000)	(0.0000)		
wksleft	--	--	-0.00767	-0.00020
			(0.0004)	(0.0000)
$wksleft^2$	--	--	0.00012	0.00000
			(0.0000)	(0.0000)
elapsed	--	--	0.00254	-0.00013
			(0.0004)	(0.0000)
$elapsed^2$	--	--	0.00002	0.00000
			(0.0000)	(0.0000)
weekly benefit	-0.00004	0.00002	-0.00005	-0.00000
amount	(0.0000)	(0.0000)	(0.0000)	(0.0000)
base period	0.00000	-0.00000	0.00000	0.00000
earnings	(0.0000)	(0.0000)	(0.0000)	(0.0000)
female	0.00109	0.00032	-0.00143	0.00032
	(0.0058)	(0.0005)	(0.0056)	(0.0005)
age	-0.00053	-0.00020	-0.00110	-0.00021
	(0.0006)	(0.0000)	(0.0006)	(0.0000)
age squared	0.00000	0.00000	0.00000	0.00000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
married	0.00747	-0.00113	0.01040	-0.00092
	(0.0043)	(0.0004)	(0.0042)	(0.0004)
not married	0.00201	-0.00129	0.00320	-0.00104
	(0.0057)	(0.0005)	(0.0055)	(0.0005)
female, not married	-0.00262	0.00067	-0.00362	0.00055
	(0.0082)	(0.0007)	(0.0080)	(0.0007)
female, married	-0.00677	-0.00016	-0.00807	-0.00026
	(0.0066)	(0.0006)	(0.0065)	(0.0006)
dropout	0.00023	-0.00035	-0.00076	-0.00031
	(0.0038)	(0.0003)	(0.0038)	(0.0003)
post secondary	-0.00514	0.00021	-0.00800	0.00022
	(0.0034)	(0.0003)	(0.0033)	(0.0003)
college	-0.00263	0.00057	0.00045	0.00058
	(0.0039)	(0.0003)	(0.0039)	(0.0003)

Table A4 (continued)

	maxexp		*wksleft*	
	Benefit exit	Reemployment	Benefit exit	Reemployment
other education	-0.00402	-0.00012	-0.00794	-0.00033
	(0.0093)	(0.0008)	(0.0091)	(0.0008)
african-american	-0.00698	-0.00033	-0.01052	-0.00027
	(0.0042)	(0.0003)	(0.0041)	(0.0003)
asian	0.00869	-0.00087	0.00863	-0.00110
	(0.0131)	(0.0010)	(0.0128)	(0.0010)
mexican	-0.01056	-0.00056	-0.01355	-0.00043
	(0.0058)	(0.0005)	(0.0057)	(0.0005)
other non-white	-0.00249	-0.00010	-0.00348	-0.00016
	(0.0060)	(0.0005)	(0.0058)	(0.0005)
manufacturing	0.00669	-0.00008	0.00803	0.00000
	(0.0027)	(0.0002)	(0.0027)	(0.0002)
previous weekly	-0.00004	0.00000	-0.00001	0.00000
earnings	(0.0001)	(0.0000)	(0.0001)	(0.0000)
expect recall	0.00461	-0.00033	0.00705	-0.00032
	(0.0030)	(0.0002)	(0.0030)	(0.0002)
definite recall date	0.03490	0.00005	0.05066	-0.00001
	(0.0057)	(0.0004)	(0.0056)	(0.0004)
state	0.05127	-0.08086	-0.11672	-0.07999
unemployment rate	(0.7103)	(0.0582)	(0.6962)	(0.0581)
constant	0.12579	0.01000	0.14869	0.01763
	(0.0198)	(0.0016)	(0.0179)	(0.0014)
sample size	33,048	254,191	33,048	254,191
adjusted R squared	0.0050	0.0011	0.0443	0.0025

Notes:

1. Estimates of equation (2) with dependent variable as noted. See text for further discussion. Estimated coefficients give the change in the probability of benefit exit or reemployment with respect to a unit change in each independent variable. Standard errors in parentheses.
2. All specifications also include state indicators

APPENDIX B

Table B1
Conditional Benefit Exit Probabilities (Discrete Hazards)

Weeks since initial claim	Risk set	Unadjusted	Cox	maxexp	potdur
			Hazards		
0-1	1821	0.0992	0.0964	0.0974	0.0927
2-3	1652	0.0612	0.0585	0.0595	0.0548
4-5	1558	0.0493	0.0465	0.0475	0.0428
6-7	1487	0.0474	0.0447	0.0457	0.0410
8-9	1422	0.0483	0.0455	0.0465	0.0418
10-11	1359	0.0564	0.0536	0.0546	0.0499
12-13	1289	0.0631	0.0604	0.0614	0.0567
14-15	1215	0.0638	0.0610	0.0620	0.0573
16-17	1145	0.0710	0.0682	0.0692	0.0646
18-19	1072	0.0953	0.0925	0.0935	0.0889
20-21	981	0.0891	0.0863	0.0874	0.0827
22-23	904	0.1029	0.1002	0.1012	0.0965
24-25	823	0.8385	0.8358	0.8368	0.8321
26-27	231	0.0702	0.0674	0.0684	0.0637
28-29	223	0.0660	0.0633	0.0643	0.0596
30-31	216	0.1111	0.1083	0.1093	0.1047
32-33	205	0.2841	0.2813	0.2823	0.2777
34-35	180	0.0159	0.0131	0.0141	0.0094
36-37	179	0.0968	0.0940	0.0950	0.0903
38-39	173	0.1607	0.1579	0.1589	0.1543
40-41	164	0.0851	0.0823	0.0833	0.0787
42-43	160	0.1373	0.1345	0.1355	0.1308
44-45	153	0.1364	0.1336	0.1346	0.1299
46+	147	0.7895	0.7867	0.7877	0.7830
Implied expected benefit duration (in weeks)		19.43	19.55	19.50	19.70
Change in weeks of benefit duration from 1 added week of potential benefits		–	0.12	0.08	0.28

Notes:

1. The risk set is the number of workers in the sample who are still on benefits at the start of each two-week period. The unadjusted hazard is the proportion of workers in the risk set who exit benefits during the period. The final three columns are the adjusted hazards for each of the three hazard specifications.

Table B1 (continued)

2. For each model, the hazard of benefit exit in each period is adjusted according to the change in the benefit exit hazard rate due to a one week increase in potential benefits.

3. Implied expected durations, d calculated as:

$$d = 2\sum_{t=1}^{\infty} f_t t$$

where

$$f_t = (1-m_1)(1-m_2)\ldots(1-m_{t-1})(m_t)$$

and where m_t is the conditional probability of benefit exit in time period t.

Table B2
Conditional Reemployment Probabilities (Discrete Hazards)

			Hazards		
Weeks since initial claim	Risk set	Unadjusted	Cox	maxexp	potdur
0-1	1821	0.0927	0.0872	0.0926	0.0925
2-3	1764	0.0466	0.0411	0.0466	0.0464
4-5	1738	0.0432	0.0377	0.0432	0.0431
6-7	1715	0.0452	0.0397	0.0451	0.0450
8-9	1692	0.0432	0.0377	0.0432	0.0430
10-11	1671	0.0344	0.0289	0.0344	0.0342
12-13	1655	0.0490	0.0435	0.0490	0.0488
14-15	1633	0.0351	0.0296	0.0351	0.0350
16-17	1618	0.0291	0.0236	0.0291	0.0290
18-19	1606	0.0550	0.0495	0.0550	0.0548
20-21	1584	0.0185	0.0130	0.0185	0.0184
22-23	1577	0.0270	0.0215	0.0269	0.0268
24-25	1567	0.0305	0.0250	0.0304	0.0303
26-27	1556	0.0257	0.0202	0.0257	0.0255
28-29	1547	0.0469	0.0414	0.0469	0.0468
30-31	1531	0.0400	0.0345	0.0400	0.0398
32-33	1518	0.0417	0.0362	0.0416	0.0415
34-35	1505	0.0201	0.0146	0.0200	0.0199
36-37	1499	0.0341	0.0286	0.0341	0.0340
38-39	1489	0.0247	0.0193	0.0247	0.0246
40-41	1482	0.0326	0.0271	0.0326	0.0324
42-43	1473	0.0637	0.0582	0.0636	0.0635
44-45	1456	0.0320	0.0265	0.0320	0.0318
46-47	1448	0.0413	0.0358	0.0413	0.0412
48-49	1438	0.0259	0.0204	0.0258	0.0257
50-51	1432	0.0442	0.0388	0.0442	0.0441
52-53	1422	0.0370	0.0316	0.0370	0.0369
54-55	1414	0.0817	0.0762	0.0817	0.0816
56-57	1397	0.0628	0.0573	0.0628	0.0627
58-59	1385	0.0503	0.0448	0.0502	0.0501
60-61	1376	0.0941	0.0886	0.0941	0.0940
62-63	1360	0.0260	0.0205	0.0259	0.0258
64-65	1356	0.0667	0.0612	0.0666	0.0665
66-67	1346	0.0429	0.0374	0.0428	0.0427
68-69	1340	0.0149	0.0094	0.0149	0.0148
70-71	1338	0.0303	0.0248	0.0303	0.0301
72-73	1334	0.0469	0.0414	0.0468	0.0467
74-75	1328	0.0410	0.0355	0.0409	0.0408
76-77	1323	0.0342	0.0287	0.0341	0.0340
78-79	1319	0.0354	0.0299	0.0354	0.0352

Table B2 (continued)

Weeks since initial claim	Risk set	Unadjusted	Hazards Cox	maxexp	potdur
80-81	1315	0.0275	0.0220	0.0275	0.0274
82-83	1312	0.0377	0.0323	0.0377	0.0376
84-85	1308	0.0196	0.0141	0.0196	0.0194
86-87	1306	0.0500	0.0445	0.0500	0.0498
88-89	1301	0.0632	0.0577	0.0631	0.0630
90-91	1295	0.0787	0.0732	0.0786	0.0785
92-93	1288	0.0610	0.0555	0.0609	0.0608
94-95	1283	0.0649	0.0595	0.0649	0.0648
96-97	1278	0.0139	0.0084	0.0139	0.0137
98-99	1277	0.0282	0.0227	0.0281	0.0280
100-101	1275	0.0435	0.0380	0.0434	0.0433
102-103	1272	0.0455	0.0400	0.0454	0.0453
104-105	1269	0.0317	0.0263	0.0317	0.0316
106-107	1267	0.0328	0.0273	0.0327	0.0326
108-109	1265	0.0169	0.0115	0.0169	0.0168
110+	1264	0.5043	0.4989	0.5043	0.5042
Implied expected joblessness duration (in weeks)		66.05	67.58	66.06	66.11
Change in weeks jobless from 1 added week of potential benefits		–	1.53	0.01	0.06

Notes:

1. The risk set is the number of workers in the sample who have not yet found a a stable job at the start of each two-week period. The unadjusted hazard is the proportion of workers in the risk set who become reemployed during the period. The final three columns are the adjusted hazards for each of the three hazard specifications.
2. For each model, the hazard of benefit exit in each period is adjusted according to the change in the reemployment hazard rate due to a one week increase in potential benefits.
3. Implied expected durations, d calculated as:

$$d = 2\sum_{t=1}^{\infty} f_t t$$

where

$$f_t = (1-m_1)(1-m_2)\ldots(1-m_{t-1})(m_t)$$

and where m_t is the conditional probability of benefit exit in time period t.