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Unemployment Insurance and Job Search in the Great Recession

ABSTRACT More than 2 years after the official end of the Great Recession, the labor market remains historically weak. One candidate explanation is supply-side effects driven by dramatic expansions of unemployment insurance (UI) benefit durations, to as long as 99 weeks. This paper investigates the effect of these extensions on job search and reemployment. I use the longitudinal structure of the Current Population Survey to construct unemployment exit hazards that vary across states, over time, and between individuals with differing unemployment durations. I then use these hazards to explore a variety of comparisons intended to distinguish the effects of UI extensions from other determinants of employment outcomes. The various specifications yield quite similar results. UI extensions had significant but small negative effects on the probability that the eligible unemployed would exit unemployment. These effects are concentrated among the long-term unemployed. The estimates imply that UI extensions raised the unemployment rate in early 2011 by only about 0.1 to 0.5 percentage point, much less than implied by previous analyses, with at least half of this effect attributable to reduced labor force exit among the unemployed rather than to the changes in reemployment rates that are of greater policy concern.

Although the so-called Great Recession officially ended in June 2009, the labor market remains stagnant. In November 2011 the unemployment rate was 8.6 percent, only the third time in 2.5 years that it was below 9 percent. Nearly 45 percent of the unemployed had been out of work for more than 6 months.

An important part of the policy response to the Great Recession has been a dramatic expansion of unemployment insurance (UI) benefits. Preexisting law provided for up to 26 weeks of benefits, plus up to 20 additional weeks under the Extended Benefits (EB) program in states experiencing high unemployment rates. But in past recessions Congress has frequently authorized additional weeks on an ad hoc basis, and in June 2008 it enacted the Emergency Unemployment Compensation (EUC) program, which, in a series of extensions, has brought statutory benefit durations to as long as 99 weeks.

Unemployment benefits subsidize continued unemployment. Thus, it seems likely that the unprecedented UI extensions have contributed to some degree to the elevated unemployment rate. However, the magnitude and interpretation of this effect are not clear. Several recent analyses have found that the extensions contributed around 1.0 percentage point to the unemployment rate in 2010 and early 2011 (see, for example, Mazumder 2011, Valletta and Kuang 2010, Fujita 2011), and some observers have claimed that the effects were several times that size.[1]

There are two channels by which UI can raise unemployment, with very different policy implications (Solon 1979). On the one hand, UI benefits can lead recipients to reduce their search effort and raise their reservation wage, slowing the transition into employment. On the other hand, these benefits, which are available only to those engaged in active job search, provide an incentive for continued search for those who might otherwise exit the labor force. This second channel raises measured unemployment but does not reduce the reemployment of displaced workers. Partly on the basis of this observation, David Howell and Bert Azizoglu (2011) find "no support" for the view that the recent UI extensions reduced employment. Unfortunately, most studies of the effect of UI on the duration of unemployment have been unable to distinguish the two channels.

Determining the portion of any rise in unemployment attributable to UI extensions on labor market outcomes is difficult because these extensions are endogenous by design. UI benefits are extended in severe recessions precisely because it is seen as unreasonable to demand that workers find jobs quickly when the labor market is weak. Thus, obtaining a credible estimate of the effect of the recent UI extensions requires a strategy for distinguishing this effect from the confounding influence of historically weak labor demand.

This paper uses the haphazard rollout of the EUC and EB programs during the Great Recession and its aftermath to identify the partial equilibrium effects of the recent UI extensions on the labor market outcomes of workers who have lost their jobs and are actively seeking new employment. I use the longitudinal structure of the Current Population Survey (CPS) to

1. Grubb (2011); Robert Barro, "The Folly of Subsidizing Unemployment," *Wall Street Journal,* August 30, 2010.

construct hazard rates for unemployment exit, reemployment, and labor force exit that vary across states, over time, and between individuals entering unemployment at different dates.

I explore a variety of strategies for isolating the effects of UI extensions. One strategy exploits the gradual rollout and repeated expiration of EUC benefits through successive federal legislation to generate variation in benefit durations across labor markets facing plausibly similar demand conditions. Second, following a recent study by Rob Valletta and Katherine Kuang (2010), I use UI-ineligible job seekers as a control group for eligible unemployed workers in the same state and month. A third strategy exploits decisions by individual states to take up or decline optional EB provisions that alter the availability of benefits; this strategy uses a "control function" to distinguish the effects of the economic conditions that define eligibility. Finally, I exploit differences in remaining benefit eligibility among UI-eligible workers displaced at different times, but searching for work in the same labor markets, to identify the effect of approaching benefit exhaustion.

All of the strategies point to broadly similar conclusions. The availability of extended UI benefits (under both EB and EUC) caused small reductions in the probability that an unemployed worker exited unemployment, reducing the monthly hazard in the fourth quarter of 2010, when the average unemployed worker anticipated a total benefit duration of 65 weeks, by between 1 and 3 percentage points on a base of 22.4 percent. Not more than half of this unemployment exit effect comes from effects on reemployment: my preferred specification indicates that UI extensions reduced the average monthly reemployment hazard of unemployed job losers in 2010Q4 by 0.5 percentage point (on a base of 13.4 percent) and reduced the monthly labor force exit hazard by 1.0 percentage point (on a base of 9.0 percent).

The labor force exit effect raises the possibility that UI extensions actually raise the reemployment rate of those who lose their jobs in bad economic times, by extending the time until they abandon their search.[2] However, estimating this effect requires strong assumptions, along with ad hoc corrections for shortcomings in the data. Using such assumptions and corrections, I simulate the effect of the 2008–10 UI extensions on aggregate unemployment and on the fraction of unemployed workers out of

2. In addition, UI may reduce hysteresis by increasing labor force attachment and thereby slowing the deterioration of job skills. If so, UI extensions could make displaced workers more employable when demand recovers. A related possibility is that UI extensions deter displaced workers from claiming disability payments (Duggan and Imberman 2009, Joint Economic Committee 2010).

work 27 weeks or more (the long-term unemployment share). All of the estimates are of partial equilibrium effects, as I ignore any effects of reduced job search by one worker on others' search behavior or job finding rates. This almost certainly leads me to overstate the effect of UI extensions.

Nevertheless, I find quite small effects. My preferred specification indicates that in the absence of UI extensions, the unemployment rate in December 2010 would have been about 0.2 percentage point lower, and the long-term unemployment share would have been about 1.6 percentage points lower. Even the specification yielding the largest effects indicates that UI extensions contributed only 0.5 percentage point to the unemployment rate. Moreover, simulations that include only the labor force participation effects yield estimates at least half as large as do simulations with both participation and reemployment effects, suggesting that reduced job search due to UI extensions raised the unemployment rate by only 0.1 to 0.2 percentage point.

The remainder of the paper is organized as follows. Section I reviews recent labor market trends and discusses the UI extensions that have been an important part of the policy response. It also presents a simple model of the effects of UI benefit durations and reviews existing estimates of the effect of the recent extensions. Section II discusses the longitudinally linked CPS data that I use to study the effects of the UI extensions. Section III presents my empirical strategies for isolating these effects. Section IV reports estimates of the effect of UI benefit durations on the unemployment exit hazard. Section V develops a simulation methodology that I use to extrapolate these estimates to obtain effects on labor market aggregates, and presents results. Section VI concludes.

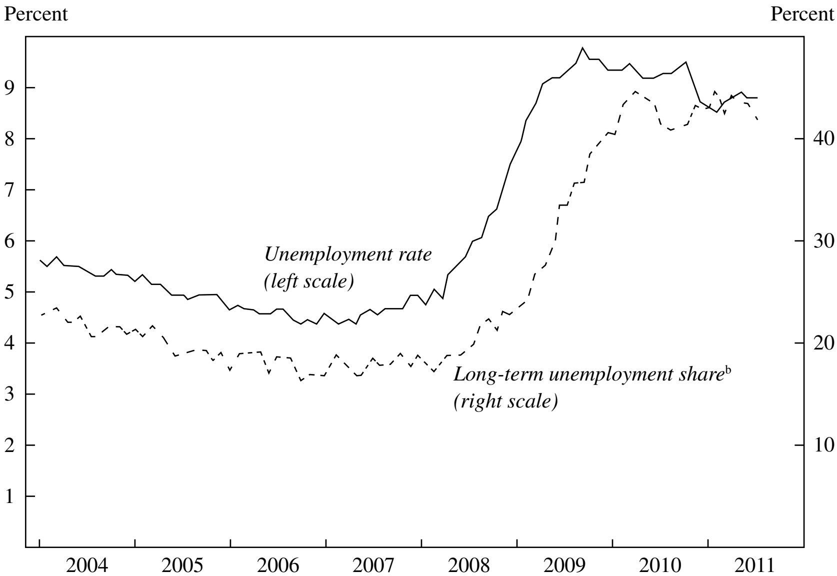
I. The Labor Market and Unemployment Insurance in the Great Recession

The Great Recession officially began in December 2007, but the downturn was slow at first: seasonally adjusted real GDP fell at an annual rate of only 1.8 percent in the first quarter of 2008, then grew at a 1.3 percent rate in the second quarter. Conditions then worsened sharply, and GDP contracted at an annual rate of 8.9 percent in the fourth quarter of 2008.

I.A. Labor Market Trends

The labor market downturn also began slowly. Figure 1 shows that the unemployment rate began trending up in 2007, but it remained only 5.8 percent as of July 2008. Over the next year, however, it rose 3.7 per-

Figure 1. Unemployment and Long-Term Unemployment, 2004–11[a]



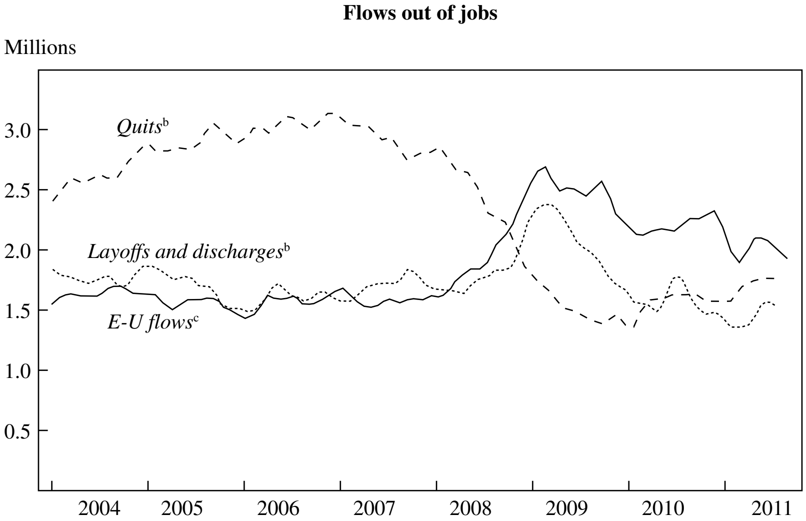
Source: Bureau of Labor Statistics data.
a. Both series are seasonally adjusted monthly data.
b. Fraction of the unemployed who have been out of work 27 weeks or longer.

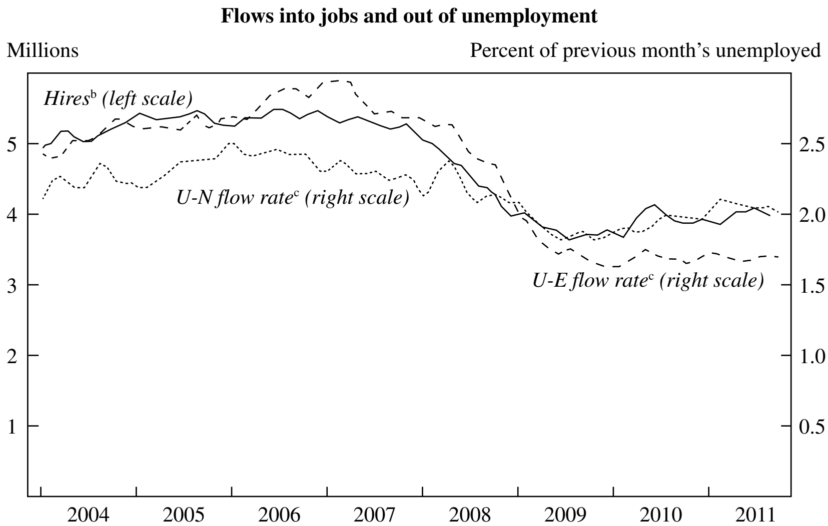
centage points, to 9.5 percent, and it has fallen below 9 percent in only three months since. Employment data show similar trends: nonfarm payroll employment rose through most of 2007, fell by 738,000 in the first half of 2008, and then fell by nearly 6.8 million over the next 12 months. Job losses continued at slower rates in the second half of 2009, followed by modest and inconsistent growth in 2010. As of August 2011, employment remained 6.9 million below its prerecession peak.

Figure 1 also shows the long-term unemployment share. This measure has lagged the overall unemployment rate by about 6 months or perhaps a bit more: it began to increase slowly in early 2008 and much more quickly in late 2008, reaching a peak of around 45 percent in early 2010—nearly 20 percentage points higher than the previous record of 26.0 percent, recorded in June 1983—and remaining mostly stable since then.

Figure 2 illustrates gross labor market flows during and after the recession. These are obtained from two sources: the Job Openings and Labor Turnover Survey (JOLTS), which derives from employer reports, and the

Figure 2. Gross Labor Market Flows, 2004–11[a]





Source: Bureau of Labor Statistics data.

a. All series are seasonally adjusted monthly data, smoothed with a 3-month symmetric triangular moving average, $y_t^{sm} = (y_{t-1} + 2y_t + y_{t+1})/4$.

b. From the Job Openings and Labor Turnover Survey data, which derive from employer surveys.

c. From the research series on labor force status flows constructed by the BLS from longitudinally linked monthly CPS files.

gross flows research series computed by the Bureau of Labor Statistics (BLS) from matched monthly CPS household data, discussed at length below. The top panel shows flows out of work: quits and layoffs from the JOLTS ("other separations," including retirements, are not shown), and gross flows from employment to unemployment (E-U) from the CPS. The bottom panel shows flows into work: hires from the JOLTS and unemployment-to-employment (U-E) flows from the CPS. It also shows unemployment-to-nonparticipation (U-N) flows; both the U-E and the U-N flows are expressed as shares of the previous month's unemployed population.

The two panels of figure 2 shed a good deal of light on the dynamics of the rise and stagnation of the unemployment rate.[3] The top panel shows that layoffs spiked and quits collapsed in late 2008, indicating an extreme weakening of labor demand; interestingly, the decline in quits seems to have preceded the increase in layoffs by several months. Not surprisingly, the number of monthly E-U transitions increased by about one-third over the course of 2008. Layoffs returned to (or even below) normal levels in late 2009, but quits remained just over half of their prerecession level and E-U flows remained high, suggesting that weak demand continued to dissuade workers from leaving their jobs and to impede the usual quick transition of laid-off workers into new jobs.

The bottom panel of figure 2 shows that the collapse in new hires was more gradual than the spike in layoffs and began much earlier, in late 2007. The rate at which unemployed workers transitioned into employment also began to decline at this time, then fell much more sharply in late 2008. Recall that the rapid run-up in long-term unemployment was in mid-2009, roughly 6 months later, again suggesting that the usual process by which job losers are recycled into new jobs was substantially disrupted around the time of the financial crisis. U-E flows remain very low at this writing. Finally, the U-N flow rate fell rather than rose during the recession, despite weak labor demand that might plausibly have led unemployed workers to become discouraged. This is plausibly a consequence of UI benefit extensions, which created incentives for ongoing search even if the prospect of finding a job was remote.

I.B. The Policy Response

Congress responded quickly to the deteriorating labor market, authorizing the EUC program in June 2008, but proceeded in fits and starts

3. See Elsby, Hobijn, and Şahin (2010) for a more detailed examination of these and other aggregate data.

Table 1. Changes in the Emergency Unemployment Compensation Program over 2008–10

	Weeks of benefits available under EUC tier				
Date[a]	*I*	*II*	*III*[b]	*IV*[c]	*Scheduled EUC expiration*
Jun. 30, 2008	13				Mar. 28, 2009
Nov. 21, 2008	20	13[b]			Mar. 28, 2009
Feb. 17, 2009	20	13[b]			Dec. 26, 2009
Nov. 6, 2009	20	14	13	6	Dec. 26, 2009
Dec. 19, 2009	20	14	13	6	Feb. 28, 2010
Feb. 28, 2010	0	0	0	0	NA
Mar. 2, 2010	20	14	13	6	Apr. 5, 2010
Apr. 5, 2010	0	0	0	0	NA
Apr. 15, 2010	20	14	13	6	Jun. 2, 2010
Jun. 2, 2010	0	0	0	0	NA
Jul. 22, 2010	20	14	13	6	Nov. 30, 2010
Nov. 30, 2010	0	0	0	0	NA
Dec. 17, 2010	20	14	13	6	Jan. 3, 2012
Dec. 23, 2011	20	14	13	6	Mar. 6, 2012[d]

Source: Fujita (2010) and Department of Labor bulletins.

a. Dates on which legislation creating, changing, or reauthorizing the program was enacted or the program expired. After each expiration, the eventual reauthorization was retroactive. NA = not applicable.

b. Benefits available only in states with unemployment rates above 6 percent.

c. Benefits available only in states with unemployment rates above 8.5 percent.

d. As this volume goes to press.

thereafter.[4] The June 2008 legislation made 13 weeks of EUC benefits available to anyone who exhausted regular benefits before March 28, 2009. The program was subsequently expanded in November 2008. That expansion extended the original EUC (now called EUC tier I) benefits to 20 weeks and added a second tier of 13 weeks of benefits in states with unemployment rates above 6 percent. A second expansion in November 2009 changed tier II benefits to 14 weeks and added tier III, 13 weeks of benefits in states with unemployment rates above 6 percent, and tier IV, an additional 6 weeks in states with unemployment rates above 8.5 percent. Individuals in states qualifying for all four tiers were thus eligible for 53 weeks of EUC benefits. The first four columns of table 1 show the number of tiers and number of weeks available over time.

The EUC program was originally set to expire on March 28, 2009. However, the program was reauthorized several times to delay the scheduled

4. This discussion draws heavily on Fujita (2010). I neglect a number of details of the UI program rules. In particular, claimants whose tenure in their previous job was short are not eligible for the full 26 weeks of regular benefits.

expiration. The last column of table 1 shows the scheduled expiration date as it changed over time. For much of the program's history, the expiration date was quite close. Indeed, on three occasions, in April, June, and November 2010, Congress allowed the program to expire. Each time, Congress eventually reauthorized it retroactive to the previous expiration date, but following the June expiration this took 7 weeks.

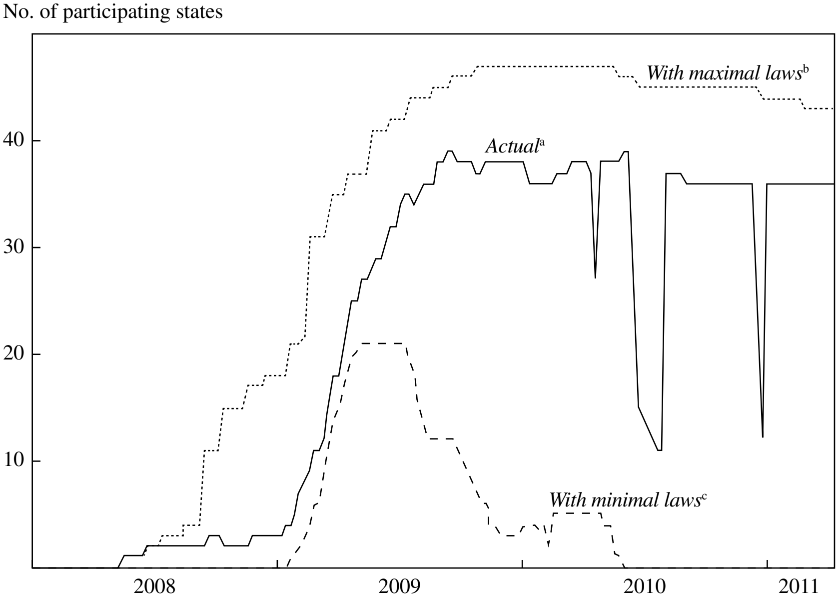
The EUC program complemented a preexisting program, the EB program, which allowed for 13 or 20 weeks of extra benefits in states with elevated unemployment rates. EB is an optional program: participating states can choose among several options regarding the specific triggers that will activate benefits. As costs are traditionally split evenly between the state and the federal government, many states have opted not to participate or have chosen relatively stringent triggers. However, the American Recovery and Reinvestment Act of 2009, enacted in February of that year, provided for full federal funding of benefits under EB. This induced a number of states to begin participating in the program and to adopt more generous triggers.[5]

Figure 3 shows the number of states in which benefits under the EB program have been available over time, along with simulated counts of the number of states where benefits would have been available had every state adopted minimal or maximal triggers. At the beginning of 2009, only three states offered benefits under this program, but by July of that year benefits were available in 35 states. Figure 3 shows that this change reflected a combination of increased EB participation, which brought the actual series well above the minimal series, and deteriorating economic conditions, which would have expanded EB participation even if states had not changed their trigger choices.[6] The figure also shows that participation plummeted each time the EUC program was allowed to expire: a number of states wrote

5. The Recovery Act also provided for tax deductibility of a portion of UI benefits, for somewhat expanded eligibility, and for more generous weekly benefit amounts.

6. During the period covered by my sample, the minimal triggers provided benefits only when the 13-week moving average of the insured unemployment rate (IUR) was at least 5 percent and above 120 percent of the maximum of its values 1 year and 2 years earlier. It is this lookback period that accounts for the decline in the minimal series in late 2009. The maximal triggers also provided benefits in states with 13-week IURs above 6 percent (regardless of their lagged values) or with a 3-month moving average total unemployment rate (the traditional measure) above 6.5 percent and above 110 percent of the value either 1 or 2 years earlier. Each simulated benefits series allows a state's status to change no more than once in 13 weeks, following program rules; the maximal series also assumes that the optional 3-year lookback was adopted when it became available in 2011. See National Employment Law Project (2011) and the Federal-State Extended Unemployment Compensation Act of 1970 (workforcesecurity.doleta.gov/unemploy/EB_law_for_web.pdf, accessed June 28, 2011).

Figure 3. Extended Benefits Availability and the Role of Optional Triggers, 2008–11



Source: Author's calculations using data from the BLS and the Employment and Training Administration.

a. Computed from the Employment and Training Administration's weekly EB trigger notices.

b. Simulated for a state that has adopted all three of the following: the alternative insured unemployment rate (IUR) trigger, which provides EB if the IUR is above 6 percent, regardless of its lagged values; the optional total unemployment rate (TUR) trigger, which provides EB if the TUR exceeds 6.5 percent and is above the lowest of the 1-year, 2-year, or (optionally) 3-year lagged TURs; and the 3-year lookback enacted in December 2010 (assumed to have gone into effect on January 1, 2011).

c. Series is simulated for a state that participates in the EB program but does not adopt the optional 3-year lookback period or any of the optional triggers available under the EB legislation. In such a state, eligibility for EB depends on having an IUR that exceeds 5 percent and is above 120 percent of the higher of the 1-year-lagged or the 2-year-lagged IUR.

their EB implementing legislation to provide for state participation only as long as the federal government paid 100 percent of the cost, and this provision expired and was reauthorized each time along with the EUC program. Other than these spikes, participation has been relatively stable over time.

A final feature of figure 3 is the wide disparity between the simulated minimal and maximal series: relatively few states, and none after mid-2010, qualified for benefits under the least generous triggers, but nearly all states did so under the most generous options. Thus, Alabama and Mississippi, each with January 2010 total unemployment rates of 10.4 percent but insured unemployment rates below 4 percent, both qualified under the maximal triggers but not under the minimal triggers; because Alabama had adopted the most generous optional triggers but Mississippi had not, unemployed individuals in Alabama were eligible for 20 weeks of EB but those in Mississippi were ineligible.

When regular (26 weeks), EUC (as many as 53 weeks), and EB program benefits (as many as 20 weeks) are combined, statutory benefit durations have reached as long as 99 weeks in many states. However, this overstates the number of weeks that any individual claimant could expect. According to EUC program rules, after the program expires, participants can draw out the remaining benefits from any tier already started but cannot transition to the next tier. Throughout 2010, the expiration date of the program was never more than a few months away. Thus, no individual exhausting regular benefits in 2010 could have anticipated being able to draw benefits from EUC tiers III or IV absent further congressional action.

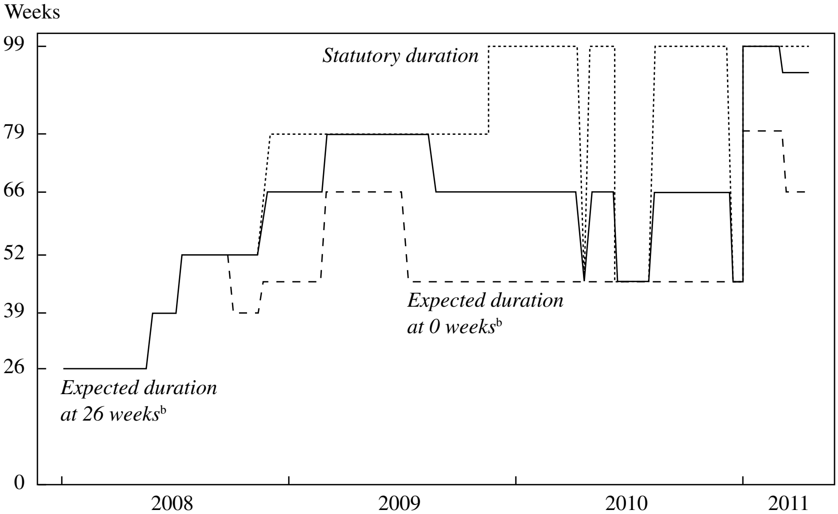
It is not clear how to model UI recipients' expectations in the weeks leading up to a scheduled EUC expiration. Recipients might reasonably have expected an extension, if only to smooth the "cliff" in benefits that would otherwise be created. However, each extension has been highly controversial, facing determined opposition and filibusters in the Senate. It would have been quite a leap of faith in mid-2010, in the midst of a Republican resurgence, for an unemployed worker to assume that the program would be extended beyond its November 30 expiration. Moreover, even a worker who foresaw an eventual extension might (correctly) have expected a gap in benefits between the program's expiration and its eventual reauthorization. For a UI recipient facing binding credit constraints, benefits paid retroactively are much less valuable than those paid on time.

Figure 4 provides two ways of looking at the changes in UI benefit durations over time. The top panel shows estimates for the state with the longest benefit durations at any point in time. After late 2008, this is a state qualifying for 20 weeks of EB program benefits and all extant EUC tiers. The bottom panel shows the (unweighted) average across states. Each panel shows the maximum number of weeks available by statute over time, as well as the expectations of a worker just entering unemployment and of a worker who has just exhausted regular benefits, under the assumption that workers do not anticipate future EUC extensions or trigger events.

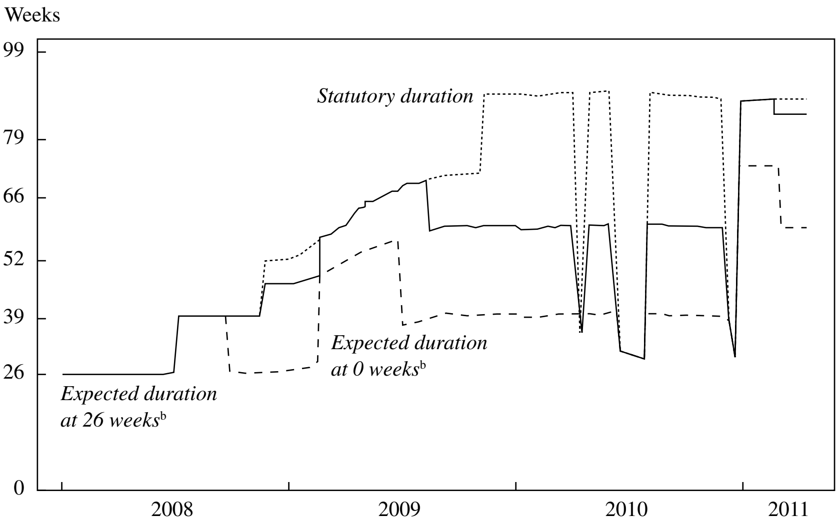
The statutory series shows a rapid run-up, due primarily to EUC expansions and secondarily to EB triggers, in 2008 and throughout 2009, followed by repeated collapses in 2010 when the EUC program temporarily expired. However, the other two series, adopting the perspectives of individuals early in their allowed benefits, show much more gradual changes.

Figure 4. UI Benefit Durations, Statutory and as Perceived by Recipients, 2008–11

State with maximum benefit duration[a]



Average state[c]



Source: Author's calculations using data from the Employment and Training Administration.

a. State with the highest statutory benefit duration in a given week.

b. Expected durations are those of a UI benefit recipient at the start of ("0 weeks") or at exhaustion of ("26 weeks") regular UI benefits who does not anticipate further federal legislative changes, changes in the state's EB program participation (including those determined by already-legislated triggers), or state trigger events.

c. Unweighted mean across states.

Newly unemployed workers who did not expect further legislative action would have seen the EUC program as largely irrelevant for most of its existence, because only on three occasions (roughly, the third quarter of 2008, the second quarter of 2009, and the period since December 2010) was the program's expiration further away than the 26 weeks it would take for such a worker to exhaust regular benefits. Workers just exhausting their regular benefits, by contrast, would have anticipated at least tier I benefits at all times except during the temporary sunsets. Even these workers, however, could not have looked forward to tier II, III, or IV benefits for most of the history of the program. Only in December 2010 and at the very beginning of 2011 could any such worker have anticipated eligibility for tier IV benefits. A final feature to notice is that the average state was quite close to the maximum from 2009 on, as most states had adopted at least one of the EB options, and most had hit their triggers.

I.C. A Model of Job Search and UI Durations

To fix ideas, I develop a simple discrete time model of job search with exogenous wages and time-limited UI. The model yields two main results. First, search intensity rises as UI benefit expiration approaches, and it is higher for UI exhaustees than for those still receiving benefits. Thus, an extension of UI benefits reduces the reemployment chances of searching individuals, both those who have exhausted their regular benefits and those who are still drawing regular benefits and thus not directly affected by the extension. Second, when UI benefit receipt is conditioned on continuing job search, benefit extensions can raise the probability of search continuation. Both results imply positive effects of benefit extensions on measured unemployment. However, because the second channel can increase search, the net effect on the reemployment of displaced workers is ambiguous.

I assume that individuals cannot borrow or save.[7] The income and therefore the consumption of an unemployed individual is y_0 if she does not receive UI benefits and $y_0 + b$ if she does. Her per-period flow utility is $u(c) - s,$ where c is her consumption and s is the amount of effort she devotes to search. If she finds a job, it will be permanent and will offer an exogenous wage $w > y_0 + b$ and flow utility $u(w)$. The probability that she finds a job in a given period is an increasing function of search effort, $p(s)$, with $p'(s) > 0, p''(s) < 0, p(0) = 0, p'(0) = \infty$, and $p(s) < 1$ for all s. Although

7. Chetty (2008) finds that much of the search effect of UI is concentrated among those who are credit constrained, and that lump-sum severance pay has an effect similar to that of UI benefit extensions (see also Card, Chetty, and Weber 2007a).

$p(s)$ might naturally be modeled as a function of changing labor market conditions, to avoid excessive complexity from dynamic anticipation effects I assume that job seekers treat it as fixed. I assume that unemployment benefits are available for up to D periods of unemployment. Initially, I model these benefits as conditional only on continued unemployment; later, I condition also on a minimum level of search effort.

These assumptions lead to a dynamic decision problem with state variable d corresponding to the number of weeks of benefits remaining. Let $V_U(d)$ represent the value function of an unemployed individual with $d > 0$ weeks of benefits remaining. The Bellman equation is

$$(1)\quad V_U(d) = \max_{s_d} u(y_0 + b) - s_d + \delta\left[p(s_d)V_E + (1 - p(s_d))V_U(d-1)\right],$$

where s_d represents the chosen search effort, V_E is the value function of an employed worker, and $1 - \delta$ is the per-week discount rate.[8]

The first-order condition then implies that the choice of search effort satisfies

$$p'(s_d) = \frac{1}{\delta(V_E - V_U(d-1))}$$

for $d \geq 1$. The following results are proved in the appendix.

Proposition 1. The value function $V_U(d)$ is increasing in d: $V_U(d+1) > V_U(d)$ for all $d \geq 0$.

Proposition 2. Search effort increases as benefit exhaustion approaches, reaching its final level in the penultimate period of benefit receipt: $s_{d+1} < s_d < s_1 = s_0$ for all $d \geq 2$.

Proposition 2 implies that UI extensions will reduce job finding rates at all unemployment durations below the new maximum benefit duration D and will shift the time-until-reemployment distribution rightward. The relative magnitude of the effect at different unemployment durations depends on the shape of the $p()$ function, but under plausible parameterizations, $(s_{d-1} - s_d)$ declines with d, so benefit extensions will have the largest effects on the search effort of those who would otherwise be at or near benefit exhaustion.[9]

8. Once benefits are exhausted ($d = 0$), the problem becomes stationary: $V_U(0) = \max_{s0} u(y_0) - s_0 + \delta[p(s_0)V_E + (1 - p(s_0))V_U(0)]$.

9. For example, this holds under the parameters considered by Chetty (2008, p. 8), which in my notation correspond to constant relative risk aversion (CRRA) utility $u(c) = \frac{c^1 - \gamma}{1 - \gamma}$, with $\gamma = 1.75$, $y_0 = 0.25w$, $b = 0.5w$, $p(s) = 0.25s^{0.9}$, $\delta = 1$, and $V_E = 500u(w)$.

These results neglect the impact of UI job search requirements. To incorporate them, I assume that an individual is considered a part of the labor force and therefore eligible to receive UI benefits only if his search effort is at least $\theta > 0$. Those who choose lower search effort receive no benefit payments but preserve their benefit entitlements (that is, d is not decremented). The Bellman equation for an individual with $d > 0$ weeks remaining is now

$$(2)\quad \tilde{V}_U(d) = \max_{s_d} \begin{cases} u(y_0 + b) - s_d + \delta\left[p(s_d)V_E + (1 - p(s_d))\tilde{V}_U(d-1)\right] \\ \quad \text{if } s_d \geq \theta \\ u(y_0) - s_d + \delta\left[p(s_d)V_E + (1 - p(s_d))\tilde{V}_U(d)\right] \\ \quad \text{if } s_d < \theta. \end{cases}$$

Unemployment benefits may deter an unemployed individual from exiting the labor force if search productivity is low—that is, if $p'\theta < \frac{1}{\delta\left[V_E - V_U(d-1)\right]}$—and if benefit levels are high relative to θ. It can be shown that:

Proposition 3. Any individual who chooses search effort $s \geq \theta$ with d weeks of benefits remaining would also choose $s \geq \theta$ with d' weeks remaining, for all $d, d' > 0$.

Intuitively, an individual who chooses $s < \theta$ when her UI entitlement has not yet been exhausted does not use any of her remaining entitlement, so the state variable, and therefore the optimization problem, is the same the following week. She will thus never choose $s > \theta$ again. This then implies that the value of the state variable was irrelevant the previous week, as remaining benefit eligibility has no effect on someone who will never again draw benefits. The only temporally consistent strategies are to exit the labor force immediately after a job loss or to remain in the labor force at least until benefits are exhausted.

UI benefit extensions thus reduce nonparticipation by delaying the exit of those who plan to exit when d reaches zero. This implies that the net effect of UI extensions is ambiguous when job search requirements are enforced: those who would have searched intensively will reduce their search effort, while some of those who would have dropped out of the labor force will increase their effort. The relative strength of these two effects is likely to vary over the business cycle: when labor demand is strong and search productivity therefore high, the former is likely to dominate, but when search productivity is low, the latter may be more important.

Finally, two important factors not captured by this model are worth mentioning. First, $p(s)$ may vary over the business cycle. If $p(s)$ is temporarily low but expected to recover later, UI extensions might keep individuals searching through the low-demand period. If search productivity is increasing in past search effort, as implied by many discussions of hysteresis, this could lead to higher employment when the economy recovers. Even without state dependence in $p(s)$, UI extensions may bring discouraged workers back into the labor force earlier in the business cycle upswing. Second, I do not model search externalities. In reality, reduced search effort by one person likely increases the productivity of search for all others: if a UI recipient does not take an available job, this merely makes the job available to someone else. This consideration is particularly important if the labor market is demand constrained, but it arises whenever labor demand is downward sloping. In the presence of search externalities, partial equilibrium estimates of the effect of UI extensions on recipients' reemployment probabilities will overstate the aggregate effects.

I.D. Earlier Estimates of the Effect of UI Extensions in the Great Recession

A number of studies have estimated the effect of the recent UI extensions on labor market outcomes. Nearly all involve extrapolations from prerecession estimates of the effect of UI benefit durations or from prerecession unemployment exit rates.

Bhashkar Mazumder (2011) uses estimates of the effect of UI durations from Lawrence Katz and Bruce Meyer (1990a) and David Card and Philip Levine (2000) to conclude that UI extensions contributed 0.8 to 1.2 percentage points to the unemployment rate in February 2011.[10] But UI durations in the Great Recession and its aftermath have been longer and labor market conditions have been different in a variety of ways than in the periods examined by the earlier studies. The effect of UI durations in the earlier estimates largely reflects a spike in the unemployment exit hazard in the weeks immediately before benefit exhaustion. Katz and Meyer (1990b) find that much of this spike is attributable to laid-off workers being recalled to their previous job; these recalls are thought to have become much less common in recent years. Card, Raj Chetty, and Andrea Weber (2007a, 2007b)

10. Aaronson, Mazumder, and Schechter (2010), Fujita (2010), and Elsby and others (2010) use similar strategies and obtain similar results.

suggest that much of the remaining spike is attributable to labor force exit rather than reemployment, highlighting the importance of distinguishing these two channels.[11]

Shigeru Fujita (2011) extrapolates from reemployment and labor force exit hazards observed in 2004–07 to infer counterfactual hazards in 2009–10 had UI benefits not been extended. To absorb confounding effects from changes in labor demand, he controls linearly for the job vacancy rate. He finds larger effects of UI extensions on unemployment than does Mazumder (2011), primarily attributable to reduced reemployment rather than reduced labor force exit. However, these conclusions are based on the extrapolated effects of a reduction in the job vacancy rate that is roughly twice as large as the range observed in the earlier period.

Mary Daly, Bart Hobijn, and Valletta (2011), drawing on Valletta and Kuang (2010), contrast changes in the unemployment durations of those laid off from their previous jobs (whom I refer to as "job losers" below), many of whom are eligible for UI benefits, and of other unemployed individuals (many of whom quit their previous jobs), who are not, over the course of the recession and after. They conclude that UI extensions raised the unemployment rate by 0.8 percentage point in 2009 and early 2010. This comparison identifies the UI effect in the presence of arbitrary changes in demand conditions, so long as the two groups are otherwise similar. However, the collapse in the quit rate seen in figure 2 above suggests that UI extensions may not be the only source of changes in the relative outcomes of job losers and job leavers. If the remaining job leavers come largely from sectors where job openings are plentiful, while the job losers come from sectors hit hard by the recession (such as construction), the comparison between them will overstate any negative effect of UI extensions.

A larger estimate comes from Robert Barro, in the op-ed cited in the introduction, who assumes that the long-term unemployment share in 2009 would have been the same as in 1983 if not for the UI extensions. Barro concludes that extensions raised the unemployment rate by 2.7 percentage points. David Grubb's (2011) literature review comes to a quite similar conclusion. In contrast, Howell and Azizoglu (2011) conclude that any

11. Another potential explanation for large spikes in at least some of the earlier studies is so-called heaping in reported unemployment durations: improbably large numbers of observations occur at certain durations. Katz (1986) and Sider (1985) suggest that in retrospective reports, much of the observed heaping—which is especially prominent at 26 weeks (6 months), the maximum duration of regular UI benefits—reflects recall error or other factors (Card and Levine 2000) rather than UI effects.

effect is much smaller and primarily attributable to reduced labor force exit induced by the UI job search requirement.

A final relevant paper is by Henry Farber and Valletta (2011). That paper was written simultaneously with and independently of this one but pursues a similar strategy of using recent data and competing-risks models to identify the effect of UI extensions on reemployment and labor force exit hazards. Unsurprisingly, Farber and Valletta obtain results very similar to those presented below. The analysis here differs from theirs in three respects: it explores several alternative specifications that isolate different components of the variation in UI benefits; it examines the sensitivity of the results to unavoidable ad hoc assumptions made about expected benefit availability; and it addresses an important discrepancy in the CPS data, discussed below, that leads survival analyses to drastically understate the long-term unemployment share and that has the potential to substantially obscure effects of UI extensions on unemployment durations.

II. Data

I use the Current Population Survey rotating panel to measure the labor market outcomes of a large sample of unemployed workers in the very recent past. Three-quarters of each month's CPS sample are targeted for another interview the following month, and it is possible to match over 70 percent of monthly respondents (94 percent of the attempted reinterviews) to employment status in the following month. (The most important source of mismatches is individuals who move, who are not followed.) This permits me to measure 1-month-later employment outcomes for roughly 4,000 unemployed workers each month during and since the Great Recession, and thereby to construct monthly reemployment and labor force exit hazards that vary by state, date of unemployment, and unemployment duration.

The CPS data have advantages and disadvantages relative to other data that have been used to study UI extension effects. Advantages include larger and more current samples, the ability to track outcomes for individuals who have exhausted their UI benefits or who are not eligible, and the ability to distinguish reemployment from labor force exit.

These are offset by important limitations. First, the monthly CPS does not contain measures of UI eligibility or receipt. Only job losers, those who were laid off from their previous job rather than having quit or having newly entered the labor force, are eligible for UI benefits. Past research has found that fewer than half of the eligible unemployed actually receive UI benefits (Anderson and Meyer 1997). This fraction appears to have risen somewhat since the onset of the Great Recession: I estimate that over half of job losers unemployed more than 3 months in early 2010 received UI benefits.[12] Although the UI participation rate is far less than 100 percent, I simulate remaining benefit durations for all job losers, assuming that each is eligible for full benefits. As I estimate relatively sparse specifications without extensive individual controls, the estimates can be seen as the "reduced form" average effect of available durations on the labor market outcomes of all job losers, pooling recipients and nonrecipients. To implement the simulation, I match the CPS data to detailed information about the availability of EUC and EB program benefits at a state-week level and compute eligibility for benefits in each week between the beginning of the unemployment spell and the initial CPS interview (including those paid retroactively because of delayed reauthorizations). I assume that 1 week of eligibility has been used for each week of covered unemployment (including retroactive coverage due to delayed reauthorizations).

In modeling expectations for benefits subsequent to the CPS interview, I assume in my main specifications that the individual anticipates no further legislative action or triggering of benefits on or off after that date, as in figure 4. Insofar as unemployed individuals are able to forecast future legislation, I may understate the duration of expected benefits and overstate the amount of variation across unemployment entry cohorts within the same state. It is unclear in which direction this nonclassical measurement error biases my results; I explore specifications aimed at reducing this bias below.

A second limitation of the CPS data is that employment status and unemployment durations are self-reported, and respondents may not fully understand the official definitions. Officially, only someone who is out of work, is available to start work, and has actively looked for work at least once in the last 4 weeks should be classified as unemployed, with a duration of unemployment reaching back to the last time he or she did not meet these conditions. Someone who has not actively searched or is unavailable to start a job is out of the labor force. But the line between unemployment and nonparticipation can be blurry, particularly when there are few suitable job openings or when job search is intermittent. The data

12. Observations in February, March, and April can be matched to data from the Annual Demographic Survey, which includes questions about UI income in the previous calendar year. In early 2010, 56 percent of job leavers whose unemployment spells appear to have started before December 1, 2009, reported nonzero UI income, up from 39 percent in early 2005.

suggest that reported unemployment durations often stretch across periods of nonparticipation or short-term employment back to the perceived "true" beginning of the unemployment spell. Reinterviews with CPS respondents in the 1980s indicate important misclassification of labor force status, particularly for unemployed individuals, who are often misclassified as out of the labor force. This leads to substantial overstatement of unemployment exit probabilities (Poterba and Summers 1984, 1995, Abowd and Zellner 1985).[13] Relatedly, examination of the unemployment duration distributions indicates substantial heaping at monthly, semiannual, and annual frequencies, suggesting that many respondents round their reported unemployment durations.

To minimize the misclassification problem, my primary estimates count someone who is observed to exit unemployment in one month but return the following month—that is, someone whose 3-month trajectory is unemployed-nonparticipating-unemployed (U-N-U) or unemployed-employed-unemployed (U-E-U)—as a nonexit.[14] This means that I can measure unemployment exits only for observations with at least two subsequent interviews. I also estimate alternative specifications that count all measured exits or that exclude many of the heaped observations, with similar results.[15] I discuss these issues at greater length in section V.

Finally, as mentioned, the CPS does not attempt to track respondents who change residences between interviews. Mobility and nonresponse lead to the attrition of roughly 8 percent of the sample and 10 percent of the unemployed respondents each month. If UI eligibility affects the propensity to move (Frey 2009, Kaplan and Schulhofer-Wohl 2011), this could bias my estimates in unknown ways. However, when I estimate my main specifications using mobility as the dependent variable, I find no evidence that it is (conditionally) correlated with my UI duration measures.

Table 2 presents summary statistics for my full CPS sample, which pools data for interviews between May 2004 and January 2011, matched

13. CPS procedures were altered in 1994, in part to reduce classification error. There are no public-use reinterview samples from the post-1994 period. However, my analysis of data supplied by Census Bureau staff suggests that the misclassification of unemployment remains an important issue even after the redesign.

14. Fujita (2011) also recodes some U-N-U trajectories as U-U-U. I am grateful to Hank Farber for helpful conversations about this issue.

15. I am unable to address a related potential problem: although the CPS data collection is independent of that used to enforce job search requirements, these requirements may lead some true nonparticipants to misreport themselves as active searchers. This may cause my estimates of the effect of UI extensions on reported labor force participation to overstate the effect on actual job search.

Table 2. Summary Statistics[a]
Percent except where stated otherwise

Statistic	All unemployed workers[b]: Job losers	All unemployed workers[b]: Job leavers, entrants, and reentrants	Subsample with two or more follow-up interviews[c]: Job losers	Subsample with two or more follow-up interviews[c]: Job leavers, entrants, and reentrants
N	95,485	77,913	77,813	61,105
Share matched to 1 follow-up interview	91	91	100	100
Share matched to 2 follow-up interviews	85	83	100	100
Unemployment duration (spells in progress)				
Average (weeks)	22.7	21.8	23.1	22.2
Share 0–13 weeks	54	59	54	59
Share 14–26 weeks	17	15	17	15
Share 27–98 weeks	23	20	24	20
Share 99 weeks or more	5	6	5	6
Share exiting unemployment by next month				
Counting all exits (1 or more follow-ups)				
Total	39	52	38	51
To employment	23	20	23	20
Out of labor force	15	32	15	31
Not counting U-N-U or U-E-U transitions (2 or more follow-ups)				
Total	30	42	29	41
To employment	20	18	20	18
Out of labor force	10	24	10	24
Anticipated duration of unemployment benefits (weeks)				
Total	43.9	NA	44.2	NA
Remaining	24.1	NA	24.0	NA
Total (anticipating EUC reauthorization)	56.7	NA	57.0	NA
State unemployment rate	7.7	6.9	7.7	6.9

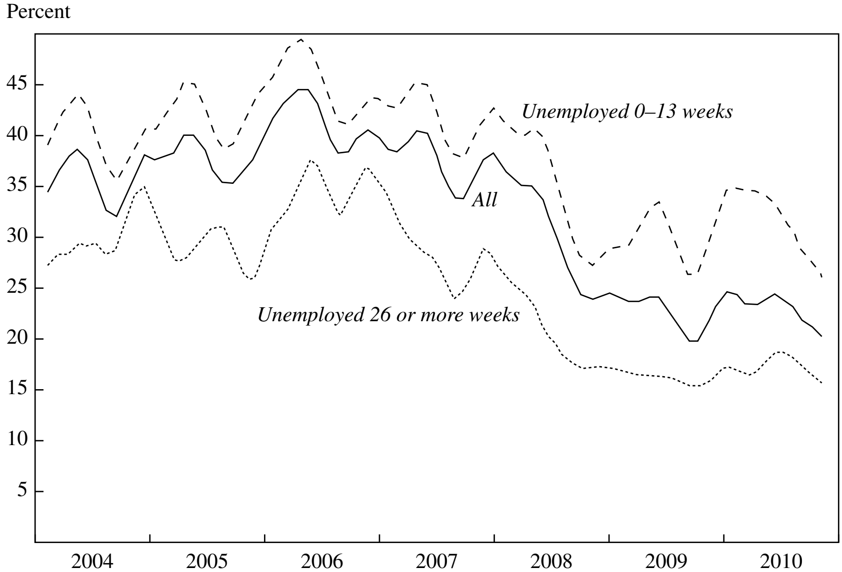
Source: Author's analysis.

a. All statistics use CPS weights. Shares may not sum to totals because of rounding. NA = not applicable.

b. All observations of unemployed workers from the May 2004–January 2011 CPS samples with month-in-sample 1, 2, 5, or 6.

c. Excludes observations with missing or allocated labor force status in the base survey or in either of the two following interviews, or with allocated unemployment duration in the base survey.

Figure 5. Monthly Unemployment Exit Hazards for Displaced Workers, by Duration Group, 2004–10[a]



Source: Author's calculations using data from the Current Population Survey.

a. Displaced workers are defined as unemployed individuals who report having lost their last job. Hazards represent the probability of being employed or out of the labor force 1 month hence and not unemployed the following month. Series are not seasonally adjusted and are smoothed using a 5-month symmetric triangle moving average.

to interviews in each of the next 2 months. (Rotation groups that would not have been targeted for two follow-up interviews are excluded.) Figure 5 presents average monthly exit probabilities for unemployed workers who report having been laid off from their previous job (as distinct from new entrants to the labor force, reentrants, and voluntary job leavers) over the sample period. The overall exit hazard fell from about 40 percent in mid-2007 to about 25 percent throughout 2009 and 2010.[16] The figure also reports exit hazards for those unemployed zero to 13 weeks and 26 weeks or more. The hazard is higher for the short-term than for the long-term unemployed. However, both series fell at rates similar to the overall average in 2007 and 2008, indicating that only a small portion of the overall

16. This is a lower exit rate than is apparent in the BLS gross flows data, which also derive from matched CPS samples but do not incorporate my adjustment for U-N-U and U-E-U trajectories.

exit hazard decline can be attributed to composition effects arising from the increased share of long-term unemployed.

III. Empirical Strategy

The matched CPS data allow me to measure whether an unemployed individual exits unemployment over the next month, but they do not allow me to follow those who do not exit to the end of their spells. I thus focus on modeling the exit hazard directly. I assume the monthly hazard follows a logistic function. To distinguish between the different forms of unemployment exit, I turn to a multinomial logit model that takes reemployment, labor force exit, and continued unemployment as possible outcomes.

Let n_{ist} be the number of weeks that unemployed person i in state s in month t has been unemployed (censored at 99); let D_{ist} be the total number of weeks of benefits available to her, including the n_{ist} weeks already used as well as weeks she expects to be able to draw in the future; and let Z_{st} be a measure of economic conditions. Using a sample of job losers, I estimate specifications of the form

$$\text{(3)} \qquad \ln\left(\frac{\lambda_{ist}}{1-\lambda_{ist}}\right) = D_{ist}\beta + P_n\left(n_{ist};\gamma\right) + P_Z\left(Z_{st};\delta\right) + \alpha_s + \eta_t .$$

Here λ_{ist} is the probability that the individual exits unemployment by month $t + 1$; α_s and η_t are fixed effects for states and months, respectively; and P_n and P_Z are flexible polynomials. This logit specification can be seen as a maximum likelihood estimator of a censored survival model with stock-based sampling and a logistic exit hazard, with each individual observed for only two periods.[17] However, as I discuss below, modeling survival functions in the CPS data is challenging because of inconsistencies between stock-based and flow-based measures of survival. In section V, I develop a simulation approach to recovering survival curves from the estimated exit hazards that are consistent with the observed duration profile. For now I focus on modeling the hazards themselves.

17. In principle, individuals can be followed for three periods in the CPS data. (Although the CPS is a four-period rotating sample, I cannot measure exit between period 3 and period 4 because, as discussed above, I require a follow-up observation to identify temporary exits.) Accounting for this would give rise to a somewhat more complex likelihood function. I treat an individual observed for three periods as two distinct observations, one on exit from period 1 to period 2 and another on exit from period 2 to period 3 (if she survives in unemployment in period 2), allowing for dependence of the error term across the observations.

After some experimentation, I settled on the following parameterization of P_n:

$$(4) \qquad P_n\left(n_{ist};\gamma\right) = n_{ist}\gamma_1 + n_{ist}^2\gamma_2 + n_{ist}^{-1}\gamma_3 + 1\left(n_{ist} \leq 1\right)\gamma_4 .$$

This appears flexible enough to capture most of the duration pattern. I have also estimated versions of equation 3 using fully nonparametric specifications of $P_n(n_{ist}; \gamma)$, with little effect on the results.

As discussed above, the main challenge in identifying the effect of D_{ist} is that it covaries importantly with labor demand conditions. Absent true random assignment of D_{ist}, I explore several alternative strategies, aimed at isolating different components of the variation in D_{ist} that are plausibly exogenous to unobserved determinants of unemployment exit.

My first strategy attempts to absorb labor demand conditions through the P_Z function. In my preferred specification, P_Z is a cubic polynomial in the state unemployment rate. I also explore richer specifications that control as well for cubics in the insured unemployment rate (an alternative measure of unemployment based only on UI-eligible workers) and in the number of new UI claims in the CPS week, expressed as a share of the employed eligible population. The remaining variation in D_{ist} comes primarily from the haphazard rollout of EUC, which creates variation over time in the relationship between Z_{st} and the number of weeks of available UI benefits. Additional variation derives from the repeated expiration and renewal of the EUC program and from states' decisions about whether to participate in the optional EB program. Note that labor demand is likely to be negatively correlated with the availability of benefits, so specifications of P_Z that do not adequately capture demand conditions will likely lead me to overstate the negative effect of UI benefits on job finding.

A second strategy uses job seekers who are not eligible for UI, either because they are new entrants to the labor market or because they left their former jobs voluntarily, to control nonparametrically for state labor market conditions (Valletta and Kuang 2010, Farber and Valletta 2011). Using a sample that pools all of the unemployed, I estimate

$$(5) \quad \ln\left(\frac{\lambda_{ist}}{1-\lambda_{ist}}\right) = D_{ist}\omega + e_{ist}D_{ist}\beta + P_n\left(n_{ist}, e_{ist};\gamma\right) + e_{ist}P_Z\left(Z_{st};\delta\right) + \alpha_{st} ,$$

where α_{st} is a full set of state $\times$ month indicators and e_{ist} is an indicator for whether individual i is a job loser (and therefore presumptively

UI-eligible). $P_n(n_{ist}, e_{ist}; \gamma) = P_n(n_{ist}; \gamma_0) + e_{ist} P_n(n_{ist}; \gamma_1) + e_{ist} \gamma_2$ represents the full interaction of the unemployment duration controls in equation 4 with the eligibility indicator, and $e_{ist} P_Z(Z_{st}; \delta)$ indicates that the relative labor market outcomes of job losers and other unemployed are allowed to vary parametrically with observed labor market conditions. The D_{ist} measure of the number of weeks available is calculated for everyone, eligible and ineligible alike, and is entered both as a main effect, to absorb any correlation between cohort employability and benefits, and interacted with the eligibility indicator e_{ist}. The effect attributable to UI duration, β, is identified from covariance between UI extensions and changes in the relative unemployment exit rates of job losers and other unemployed workers who entered unemployment at the same time, over and above that which can be explained by the Z_{st} controls.

This specification has the advantage that it does not rely on parametric controls to measure the absolute effect of economic conditions on job finding rates. However, recall that figure 2 indicated that the quit rate has been low throughout the recession and since. If the ineligible unemployed during the period when benefits were extended are disproportionately composed of people who have relatively good employment prospects, the evolving prospects of the population of ineligibles may not be a good guide to those of eligibles, leading the specification in equation 5 to overstate the effect of UI extensions. I attempt to minimize this by adding controls for several individual covariates—age, education, sex, marital status, and former occupation and industry—to equation 5.

My third strategy returns to the eligibles-only sample but narrows in on the variation in UI durations coming from state decisions about which EB triggers to adopt, using a control function to absorb all other variation in D_{ist}. I augment equation 3 with a direct control for the number of EUC weeks available. This leaves variation only in EB program benefits (and, incidentally, eliminates my reliance on assumptions about job seekers' expectations of future EUC reauthorization, as the EB program is not set to expire). I also add controls for the availability of EB program benefits in the state × month cell under maximal and minimal state participation in the EB program (as graphed in figure 3), along with indicators for whether the state has exceeded each of the four EB thresholds.[18] With these controls,

18. Three of the triggers are described in note 6. The fourth is activated when the 3-month moving-average total unemployment rate exceeds 8 percent and is above 110 percent of the lesser of its 1-year and 2-year lagged values. States adopting optional trigger 3 are required to also adopt trigger 4, which when activated provides an additional 7 weeks of benefits on top of the normal 13.

the only variation in D_{ist} should come from differences among states in similar economic circumstances in take-up of the optional EB triggers.

My final strategy turns to an entirely different source of variation, focusing on the interaction between the number of available weeks in the state and the number of weeks that the individual has used to date. Equations 3 and 5 model the effect of UI extensions as a constant shift in the log odds of unemployment exit, reemployment, or labor force exit; in some specifications I allow separate effects on those unemployed more or less than 26 weeks. But this is a crude way of capturing the effects, which the model in section I.C suggests are likely to be stronger for those facing imminent exhaustion than for those for whom an extension only adds to the end of what is already a long stream of anticipated future benefits.

To focus better on this, I turn to a specification that parameterizes the UI effect in terms of the time to exhaustion:

$$\ln\left(\frac{\lambda_{ist}}{1-\lambda_{ist}}\right) = f\left(d_{ist};\beta\right) + \sum_{\upsilon=0}^{99} 1\left(n_{ist} = \upsilon\right)\gamma_{\upsilon} + \alpha_{st}. \tag{6}$$

Here $d_{ist} = \max\{0, D_{ist} - n_{ist}\}$ represents the number of weeks of benefits remaining, with $f(\cdot;\beta)$ a flexible function; I impose only the normalization that $f(0;\beta) = 0$, implying that UI durations have no effect on job searchers who have already exhausted all UI benefits. The second term on the right-hand side of equation 6 is a full set of indicators for unemployment duration, and the third is a full set of state × month indicators. There are two sources of variation that allow separate identification of the effects of d and n, within state × month cells, without parametric restrictions. The first is the nonlinearity of the mapping from D_{ist} and n_{ist} to d_{ist}: across–state × month variation in benefit availability has one-for-one effects on d_{ist} for those who have not yet exhausted benefits but not for those who have. Second, the EUC expiration rules mean that the addition of new EUC tiers extends d for those who will transition onto the new tiers before the EUC program expires but not for those with lower n_{ist}, who will expect the program to have expired before they reach the new tiers.

IV. Estimates

The top panel of table 3 presents logit estimates of equation 3, with standard errors clustered at the state level. The table shows the unemployment duration coefficient and its standard error. Below these, it also shows the estimated effect of the UI extensions on the average exit hazard in the

Table 3. Logit Regressions Estimating Effects of UI Extensions on Unemployment Exit Hazards[a]

	Sample: job losers (N = 77,813)[b]					*Sample: all unemployed workers (N = 138,883)*[c]	
Independent variables and calculated effects of UI extensions	*3-1*	*3-2*	*3-3*	*3-4*	*3-5*	*3-6*	*3-7*
Assuming constant effect of UI across all durations							
Weeks of UI benefits/100	–0.33	–0.27	–0.31	–0.34	–0.37	–0.15	–0.19
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Effect of UI extensions on average exit hazard, 2010Q4 (percentage points)[d]	–2.1	–1.7	–1.9	–2.1	–2.3	–0.9	–1.2
Controls							
State unemployment rate	No	Linear	Cubic	Cubic	Cubic	No	No
State insured unemployment rate[e]	No	No	No	Cubic	Cubic	No	No
State new UI claims rate[f]	No	No	No	Cubic	Cubic	No	No
State employment growth rate	No	No	No	Cubic	Cubic	No	No
Individual covariates[g]	No	No	No	No	Yes	No	Yes

(continued)

Table 3. Logit Regressions Estimating Effects of UI Extensions on Unemployment Exit Hazards[a] *(Continued)*

	Sample: job losers (N = 77,813)[b]					*Sample: all unemployed workers (N = 138,883)*[c]	
Independent variables and calculated effects of UI extensions	*3-1*	*3-2*	*3-3*	*3-4*	*3-5*	*3-6*	*3-7*
Allowing effect to vary by individual unemployment duration[h]							
Weeks of UI benefits/100 × unemployed less than 26 weeks	0.08	0.20	0.13	0.10	0.10	–0.11	–0.13
	(0.15)	(0.15)	(0.15)	(0.14)	(0.14)	(0.19)	(0.19)
Weeks of UI benefits/100 × unemployed 26 or more weeks	–0.37	–0.30	–0.34	–0.36	–0.40	–0.19	–0.23
	(0.09)	(0.10)	(0.09)	(0.09)	(0.09)	(0.10)	(0.11)
Effect of UI extensions on average exit hazard, 2010Q4 (percentage points)[d]	–1.5	–1.0	–1.3	–1.4	–1.6	–1.0	–1.3

Source: Author's analysis.

a. Standard errors clustered at the state level are in parentheses.

b. Average monthly exit hazard in the full sample is 29.4 percent; that in the 2010Q4 subsample is 22.4 percent. All specifications using this sample use the CPS sample weights and include state fixed effects, month fixed effects, and unemployment duration controls (weeks of unemployment as reported in the beginning-of-month survey, its square, its inverse, and an indicator variable for being unemployed 1 week or less).

c. Specifications include unemployment duration controls (see note b), state × month fixed effects, an indicator variable for whether the individual is a job loser, interactions of the job loser indicator with the unemployment duration controls and with a cubic in the state unemployment rate, and the number of weeks of benefits the individual would receive if eligible. Estimation is by conditional logit and uses the average CPS weight in the state × month cell.

d. Difference between the average fitted exit probability and the fitted probability implied by the model if benefit durations had been held fixed at 26 weeks.

e. UI claimants as a share of all insured workers.

f. New UI claims as a share of all insured workers.

g. Sex and marital status indicators, a female-married interaction, and age, education, and preunemployment industry indicators (6, 4, and 15 categories, respectively).

h. Specifications are the same as in the top panel but also include an indicator for whether the individual has been unemployed 26 weeks or more.

fourth quarter of 2010, computed as the difference between the average fitted exit probability and the average fitted probability implied by the model with benefit durations set to 26 weeks for the entire sample.[19] The regression reported in column 3-1 is estimated using only job losers, who are presumed to be eligible for UI benefits, and includes state and month fixed effects and the n_{ist} controls indicated by equation 4, but no controls for economic conditions in the state or for individual characteristics. It indicates a significant negative effect of UI benefit durations on the probability of unemployment exit, with a net effect of the UI extensions on the 2010Q4 exit rate of −2.1 percentage points (on a base of 22.4 percent). Columns 3-2 through 3-5 add additional controls: column 3-2 adds a control for the state unemployment rate, column 3-3 uses a cubic in that rate, column 3-4 adds cubics in three other measures of slackness (the number of UI claimants and the number of new UI claims, each expressed as a share of insured employment, and the state employment growth rate), and finally column 3-5 adds a vector of individual-level covariates, including indicators for education, age, sex, marital status, and industry of previous employment. The estimated effects of UI durations move around a bit as the covariate vector is expanded, but within a fairly narrow range: the implied effects on the exit hazard in 2010Q4 range from −1.7 to −2.3 percentage points.

Columns 3-6 and 3-7 turn to my second strategy, adding to the sample over 60,000 unemployed individuals who left their jobs voluntarily or are new entrants to the labor force and are therefore not eligible for UI benefits. As indicated by equation 5, this allows me to add state × month fixed effects.[20] I also include an indicator for (simulated) UI eligibility and its interaction with the duration and unemployment rate controls, as well as a "simulated UI duration" control that is common to both the job losers and the job leavers and designed to capture any unobserved cohort effects that are common to both groups but correlated with my UI measure. Column 3-7 also adds the full vector of individual covariates, as a guard against the possibility of important differences in employability between the job losers and the UI-ineligible comparison group. With or without these covariates, the estimates indicate notably smaller effects than in the first five columns.

19. Strictly speaking, I use observations from the September through November surveys. December observations are excluded because the EUC program had expired and not yet been renewed at the time of the December survey; see section I.B.

20. For computational reasons, I estimate the specification by conditional logit, then back out consistent but inefficient estimates of the α_{st} fixed effects for use in predicted exit probabilities.

Table 4. Specifications Examining the Sensitivity of Results to the Recipient Expectations Model[a]

Independent variables and calculated effects of UI extensions	*4-1*[b]	*4-2*	*4-3*	*4-4*	*4-5*
Weeks of UI benefits/100 × unemployed less than 26 weeks	0.13 (0.15)	–0.08 (0.17)	0.07 (0.20)	0.02 (0.26)	–0.12 (0.22)
Weeks of UI benefits/100 × unemployed 26 or more weeks	–0.34 (0.09)	–0.44 (0.17)	–0.43 (0.19)	–0.48 (0.34)	–0.62 (0.27)
Weeks of UI benefits/100 × unemployed less than 26 weeks × expectations range[c]			–0.20 (0.62)		
Weeks of UI benefits/100 × unemployed 26 or more weeks × expectations range			–0.62 (0.39)		
Estimated effect of UI extensions on average exit hazard, 2010Q4 (percentage points)	–1.3	–3.0	–1.8	–2.1	–3.1
Controls					
Forecast EUC reauthorization?[d]	No	Yes	No	No	No
EUC weeks available	No	No	No	Yes	Yes
EB trigger status	No	No	No	No	Yes
EB availability under alternative rules	No	No	No	No	Yes

Source: Author's analysis.

a. All specifications include state and month fixed effects, unemployment duration controls, and a cubic in the state unemployment rate. See the text for description of additional covariates.

b. Specification from table 3, bottom panel, column 3-3.

c. Absolute value of the difference in expected durations between the two forecasting models.

d. All recipients are assumed to expect the EUC program to be extended seamlessly and indefinitely.

There is no particular reason to think that benefit extensions have the same effects on those near benefit exhaustion as on those just beginning their unemployment spells. As a first step toward loosening this assumption, in the bottom panel of the table I allow D_{ist} to have different effects on those unemployed less than 26 weeks and those unemployed 26 weeks or longer. The negative effect of D on unemployment exit is found to be entirely concentrated among the latter, with estimated effects on the shorter-term unemployed that are close to zero, never statistically significant, and in many cases positive. The coefficients for the long-term unemployed are somewhat larger than in the top panel, though the differences are small. The implied effects of UI extensions on exit hazards are smaller than those in the top panel in the first five columns, but larger in the last two, narrowing the gap between the two sets of specifications.

Table 4 presents several specifications aimed at gauging the sensitivity of the estimates to the measurement of expected future benefits. Column 4-1 repeats the results for the baseline specification from column 3-3 in the bottom panel of table 3. Column 4-2 replaces the anticipated

UI duration measure with an alternative calculated under the assumption that all recipients expect the EUC program to be extended seamlessly and indefinitely (as in Farber and Valletta 2011). This leads to larger estimated UI extension effects, more than doubling the effect on the monthly exit rate.

Measurement error in the two benefit duration proxies is likely concentrated in the months shortly preceding expiration of the EUC program, when the two expectations models yield quite different durations; the simulated benefit durations should match recipient expectations much more closely in subsamples where the two expectations models are in closer agreement. Column 4-3 presents a specification that builds on this intuition. Here I measure the absolute difference between the *D*s calculated under the two expectations models and interact this difference with the simulated benefit duration (returning to the "myopic" expectations model used in column 4-1). I interpret the *D* main effect in this specification—the effect of durations when the two expectations models are in agreement—as indicating the effect of *D* actually attributable to UI benefit duration, and I interpret the interaction as a measure of the bias due to mismeasurement of *D* when EUC expiration approaches. Point estimates for the main effects are intermediate between those in columns 4-1 and 4-2; the interaction coefficients are negative for both the short- and the long-term unemployed but are imprecisely estimated.

Column 4-4 takes a different approach to the difficulty of forecasting EUC extensions: I simply control directly for the (simulated) number of EUC weeks available. With this control, the only remaining variation in *D* comes from benefits received under the EB program, which are not directly dependent upon EUC reauthorization. The estimated UI extension effects are somewhat larger than in my baseline specification but in the same general range.

Finally, column 4-5 turns to my third strategy for identifying the UI extension effect, using a control function to isolate variation in EB program benefits coming from state decisions about which version of the EB triggers to use.[21] I add to the specification in column 4-4 controls for the

21. Identification in this specification comes from variation in state take-up of a program that, for much of the period under study, was entirely funded by the federal government. Insofar as states that turned down this free money (an important determinant of which seems to be the presence of a governor who vocally opposed federal economic stimulus in 2009) experienced sharper labor market downturns (conditional on my controls), this strategy may lead me to overstate the effect of UI. Of course, an association in the opposite direction would lead me to understate this effect.

Table 5. Multinomial Logit Regressions Estimating Effects of UI Extensions on Reemployment and Labor Force Exit Hazards[a]

Independent variables and calculated effects of UI extensions	*5-1*	*5-2*	*5-3*	*5-4*	*5-5*
Specification and sample (column from previous table)	3-1	3-3	3-5	4-3	4-5
Effects on reemployment					
Weeks of UI benefits/100 × unemployed less than 26 weeks	0.19	0.24	0.18	0.48	0.01
	(0.19)	(0.19)	(0.19)	(0.24)	(0.33)
Weeks of UI benefits/100 × unemployed 26 or more weeks	–0.44	–0.42	–0.47	–0.29	–0.64
	(0.13)	(0.14)	(0.14)	(0.21)	(0.37)
Effects on labor force exit					
Weeks of UI benefits/100 × unemployed less than 26 weeks	–0.19	–0.12	–0.11	–0.41	–0.32
	(0.21)	(0.21)	(0.21)	(0.45)	(0.26)
Weeks of UI benefits/100 × unemployed 26 or more weeks	–0.38	–0.34	–0.42	–0.55	–0.58
	(0.13)	(0.13)	(0.15)	(0.37)	(0.34)
Effect of UI extensions on average hazard, 2010Q4 (percentage points)					
Reemployment	–0.6	–0.5	–0.7	0.2	–1.2
Labor force exit	–1.2	–1.0	–1.2	–2.0	–1.8

Source: Author's analysis.

a. Estimation is by multinomial logit for a trichotomous outcome (unemployment, employment, or not in labor force) instead of for a dichotomous outcome (unemployment or nonunemployment) as in tables 3 and 4. Average monthly hazards in the full sample are 19.9 percent for reemployment and 9.6 percent for labor force exit; in the 2010Q4 subsample they are 13.4 percent and 9.0 percent, respectively.

status of each of the four EB triggers and for simulated EB eligibility under the most and least generous versions of the triggers. This inflates the coefficients, which now indicate that UI extensions reduced the monthly exit rate by 3.1 percentage points.

Next, I explore the distinction between reemployment and labor force exit. Table 5 reports multinomial logit estimates of several of the specifications from tables 3 and 4, using three outcomes: continued unemployment (the base case), exit to employment, and exit to nonparticipation in the labor force. For the long-term unemployed, the results indicate that UI benefit durations have significant, negative effects of roughly similar magnitude on the logit indexes for both types of unemployment exit. For the short-term unemployed, the estimates indicate positive effects on reemployment and negative effects on labor force exit, both insignificant in most specifications. The bottom rows show the effects of UI extensions on average exit hazards in 2010Q4. Benefit extensions appear to lead to larger reductions in the probability of labor force exit than in the probability of reemployment, reflecting in part the positive point estimates for reemployment of the short-term unemployed. Given the imprecision in those estimates, however, effects of comparable magnitude on the two margins are clearly within the confidence intervals.

The multinomial logit model requires the "independence of irrelevant alternatives" (IIA) assumption, which corresponds to independent risks of reemployment and labor force exit. This assumption may be incorrect here, particularly if (as in the model in section I.C) search effort is continuous and labor force participation simply corresponds to an arbitrary effort threshold. However, note that the labor force exit and reemployment effects indicated in the bottom rows of table 5 sum to a net effect on unemployment exit that is, in each column, quite similar to the effect implied by the corresponding binomial logit model. This is at least suggestive that violations of IIA are not dramatically biasing the results.

Two additional considerations support the same general conclusion. The most likely source of IIA violations is unobserved heterogeneity: individuals with low job finding probabilities may be most likely (and those with high job finding probabilities least likely) to exit the labor force. Recall from table 3, however, that controlling for unobservables has little impact on the estimated UI extension effects. The same is true in the multinomial specifications (compare column 5-3 of table 5, which includes the individual covariates, with column 5-2, which does not). This is at least suggestive that neglected individual heterogeneity is not driving the results. Second, insofar as heterogeneity is producing IIA violations, it likely leads me to overstate the negative effect of UI extensions on reemployment: if extensions dissuade individuals with low job finding probability from exiting the labor force, this will reduce average job finding rates among the unemployed through a pure composition effect, on top of any effect operating through UI's disincentive for intensive search. My estimates of the reemployment effect will thus be biased downward. As even the estimated effects in table 5 are quite small, it seems safe to conclude that UI extensions have not had large effects on the job finding probabilities of the unemployed.

Table 6 presents a number of alternative specifications of the multinomial logit regression, focusing on the implied effects of UI extensions on the 2010Q4 exit hazards. The first row repeats the results from column 5-2 in table 5. The second row allows the UI effect to differ for those with initial durations under 26 weeks, exactly 26 weeks, and over 26 weeks, as there is substantial heaping at 26 weeks in the raw data (presumably

Table 6. Multinomial Logit Regressions: Alternative Specifications and Subsamples

	Reemployment		Labor force exit	
Specification and sample	*Average hazard, 2010Q4 (percent)*	*Effect of UI extensions (percentage points)*	*Average hazard, 2010Q4 (percent)*	*Effect of UI extensions (percentage points)*
Baseline (N = 77,813)[a]	13.4	–0.5	9.0	–1.0
Alternative specifications and samples				
Separate effect at exactly 26 weeks[b]	13.4	–0.5	9.0	–1.0
Drop round-number and inconsistent durations (N = 61,854)[c]	12.8	–0.5	7.9	–1.5
Drop durations under 8 weeks (N = 49,852)	14.2	0.1	9.6	–1.1[f]
Count all U-N and U-E transitions as exits from unemployment (N = 127,526)[d]	16.5	–0.6	13.7	–1.3
Subsamples[e]				
Ages 25–54 (N = 53,104)	14.4	–1.0	7.5	–1.8
Ages 55 and over (N = 13, 990)	11.6	1.4	9.7	0.5[f]
Men (N = 47,782)	13.7	–0.2[f]	7.3	–1.2
Women (N = 30,031)	13.0	–1.0	11.7	–0.8[f]
High school or less (N = 43,628)	13.3	–0.4	10.0	–1.8
Some college or more (N = 34,185)	13.7	–0.5	7.8	–0.1[f]
Construction and manufacturing workers (N = 25,584)	14.2	0.4[f]	7.4	–2.1[f]
All other industries (N = 52,229)	13.1	–0.9	9.7	–0.4[f]

Source: Author's analysis.

a. From table 5, column 5-2.

b. Adds an indicator variable for unemployment duration of exactly 26 weeks and an interaction of that variable with the number of weeks of UI benefits available.

c. Drops observations where the unemployment duration at the beginning of the spell or at the first CPS interview was 26, 52, or 78 weeks, and those in month-in-sample 2 that are inconsistent with the duration in month 1.

d. Counts all transitions from unemployment to nonparticipation or employment as exits from unemployment, even if the individual returns to unemployment the following month (that is, U-N-U and U-E-U transitions).

e. Baseline specification is used.

f. UI effects are jointly insignificant at the 5 percent level.

due to rounding of durations reported in months). Although the point estimates (not reported) show that the effects are largest for those unemployed exactly 26 weeks, this group is not large enough to change the overall average exit hazards.

The third row of table 6 offers another approach to investigating the impact of duration heaping: I exclude from my sample all individuals who reported durations of exactly 26, 52, or 78 weeks when first asked about their unemployment spells (in their first months in the CPS sample), as well as all who reported inconsistent durations from one month to the next.[22] This leads to larger effects of UI extensions on labor force exit but does not change the substantive story. The fourth row excludes individuals who were unemployed for less than 8 weeks at the first survey. This reduces the precision of the estimates, and a test of the hypothesis that the effects of UI durations on labor force exit of the short- and long-term unemployed are both zero now is only marginally significant ($p = 0.06$). However, the basic pattern is again similar to that seen earlier.

The fifth row explores the sensitivity of the results to the definition of unemployment "exit." My main specifications count only exits that do not backslide into unemployment the following month, in order to exclude those most likely to be spurious consequences of measurement error in employment status. This specification instead counts all exits, which allows me to expand the sample by over 50 percent, as I require only one follow-up interview to measure exit. This raises the baseline hazards substantially, particularly for labor force exit, but has little impact on the estimated effect of UI extensions.

The remaining rows of table 6 show estimates for different subsamples. The sixth and seventh rows show that the negative effects of UI extensions on exit hazards are concentrated among prime-age workers; for workers 55 and over, extensions appear to raise the unemployment exit probability, but only the effect on reemployment is statistically significant. The next two rows show effects by sex; there is no clear pattern here. The following two rows show that the labor force exit effect is concentrated among non-college-educated workers, while the reemployment effects are similar for more and less educated workers. The last two rows show that labor force exit effects are concentrated among workers in the construction and manufacturing sectors, where employment was especially hard hit in the recession, whereas reemployment effects derive from workers who lost jobs in other sectors.

Next, I turn to my fourth strategy, that described in equation 6, which allows the effects of UI durations to operate through the time until benefit exhaustion. As in the baseline specifications, I include state and month indicators and a cubic in the state unemployment rate. I also include an

22. For example, an unemployment duration of 9 weeks in interview 2 would be considered inconsistent unless the individual reported in interview 1 being unemployed for between 3 and 6 weeks.

extremely flexible parameterization of the unemployment duration.[23] As discussed in section III, the time-until-exhaustion effects are identified from variation across state × month cells in the number of weeks available, D_{st}—with one-for-one effects on d_{ist} only for those whose durations do not exceed the higher D value—and from variation in D_{ist} across unemployment cohorts within cells due to the projected expiration of EUC benefits at fixed calendar dates, which means that earlier unemployment cohorts expect to be able to start more EUC tiers than do later cohorts.

I begin with a multinomial logit specification that allows for unrestricted d_{ist} effects. The line labeled "nonparametric" in figure 6 plots the d coefficients from this specification.[24] The reemployment results, in the top panel, show a clear pattern of negative coefficients that are perhaps trending downward as d_{ist} falls toward about 10, then rising toward zero as d_{ist} falls further. This is consistent with the general pattern one would expect from reasonably parameterized search models (see section I.C), with depressed search effort from those with many weeks left and increasing effort as benefit exhaustion approaches, reaching a maximum value at the time of exhaustion, with constant search effort thereafter.[25] The labor force exit coefficients, in the bottom panel, show a roughly similar pattern: negative and fairly stable coefficients for large d_{ist} values, rising as d_{ist} falls from 10 toward zero. This time, however, the coefficients are generally positive for the lowest d_{ist} values, indicating that those very near benefit exhaustion are more likely to exit the labor force than are those who have already exhausted their benefits. This, too, is consistent with the search model presented earlier, which indicated that benefit exhaustion would trigger labor force exit among at least a subset of UI claimants.[26]

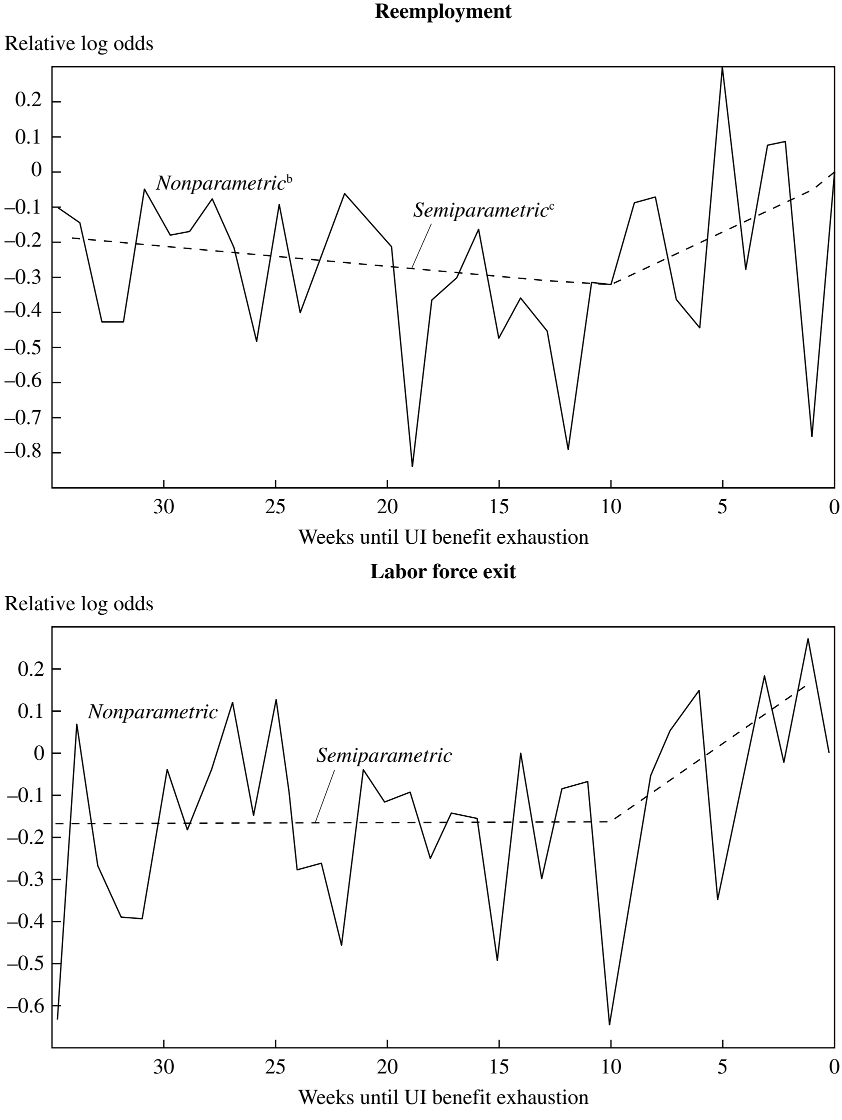
23. The duration density gets thin beyond 1 year, and most respondents seem to round their durations to the nearest month. I thus include weekly duration indicators for durations up to 26 weeks, plus separate linear weekly duration controls within each of eight bins (26–30, 31–40, 41–50, 51–60, 61–70, 71–80, 81–90, and 91–99 weeks).

24. The maximum value of d_{ist} in my sample is 83 weeks, but the frequency of individual values above 35 weeks is often quite low, so I show coefficients only for the lower portion of the distribution.

25. The increase in the exit rate as d approaches zero is consistent with the presence of a spike in the exit rate at or near the exhaustion of benefits (that is, at $d = 0$ or $d = 1$; see, for example, Katz and Meyer 1990a). The CPS data are not well suited to the identification of sharp spikes, however, as the monthly frequency smooths out week-to-week changes.

26. In the model, exits occur either immediately upon job loss or upon benefit exhaustion. Thus, the model does not perfectly fit the data, which show positive rates of labor force exit even for nonexhaustees. The gradual rise in labor force exit rates as the date of exhaustion approaches is also inconsistent with the model but may be explained by an imperfect correspondence between my simulated exhaustion date and the true one.

Figure 6. Parametric and Nonparametric Specifications of the Time-to-Exhaustion Effect[a]



Source: Author's calculations.

a. Each series is obtained from a multinomial logit regression with state and time indicators, a cubic in the unemployment rate, and the unemployment duration controls described in the notes to table 7.

b. Specification includes a full set of weeks-to-benefit exhaustion dummies (zero is the excluded category). The estimation sample includes values as high as 99 weeks, but only coefficient estimates for weeks below 35 weeks are shown here.

c. Specification replaces the weeks-to-exhaustion (d) dummies with a dummy for $d > 0$, a linear control for d, and a control for $\max\{0, d - 10\}$. Coefficients are reported in table 7, row 7-3.

Table 7. Logit Regressions Estimating Effects of Time until UI Benefit Exhaustion[a]

Regression		Time-to-exhaustion variable[b]: Any weeks left	Time-to-exhaustion variable[b]: No. of weeks left/10	Time-to-exhaustion variable[b]: max {0, no. of weeks – 10}/10	Effect of UI extensions, 2010Q4 (percentage points)
7-1	Logit for unemployment exit with state, month, and unemployment rate controls[c]	0.12 (0.08)	–0.36[e] (0.10)	0.39[e] (0.11)	–0.7
7-2	Logit for unemployment exit with state × month controls[d]	0.10 (0.08)	–0.33[e] (0.11)	0.37[e] (0.12)	–0.5
7-3	Multinomial logit with state, month, and unemployment rate controls[c]				
	For reemployment	–0.03 (0.11)	–0.29[e] (0.13)	0.35[e] (0.14)	–0.0
	For labor force exit	0.20[e] (0.10)	–0.36[e] (0.12)	0.35[e] (0.13)	–0.6

Source: Author's analysis.

a. Each numbered row reports a separate regression specification. All regressions include indicator variables for the duration of the unemployment spell, by week up to 26 weeks, plus a linear spline with kinks at 30, 40, 50, 60, 70, 80, and 90 weeks.

b. Calculation of weeks until UI benefit exhaustion is based on the expectations model described in the text, applied to the date of the baseline survey.

c. Includes state and month indicators and a cubic in the state unemployment rate.

d. Includes state × month indicators.

e. Significant at the 5 percent level.

Given the pattern of coefficients in figure 6, I next turn to a semiparametric specification that allows for three duration terms: a linear term in d_{ist}, a second linear term in $\max\{0, d_{ist} - 10\}$ that allows for a change in the slope when d_{ist} exceeds 10, and an intercept that applies to all individuals with remaining benefits (that is, with $d_{ist} > 0$). Estimates from a logit specification are shown in the first row of table 7. As in figure 6, exit rates are lower for those with many weeks of remaining benefits than for those whose benefits have been exhausted, roughly constant across $d > 10$ (the main d term and the additional term for $d > 10$ cancel out), and sharply increasing as d falls from 10 toward zero. There is no significant difference in exit rates between those in their last weeks of benefits and those who have already exhausted them, holding constant the length of the spell. The rightmost column of table 7 shows that the implied effect of UI extensions on the UI exit rate is somewhat smaller than those implied by the earlier estimates.

The second specification reported in table 7 includes a full set of state × month indicators. This yields results very similar to those in the less restrictive specification. The third specification returns to the control variables from the first but uses a multinomial logit that distinguishes alternative types of exit from unemployment. (Coefficients from this specification are plotted as the series labeled "semiparametric" in figure 6.) As before, UI extensions have substantial effects on both margins, but the impact on unemployment exit hazards is smaller than in the earlier analyses.

V. Simulations of the Effect of Unemployment Insurance Extensions

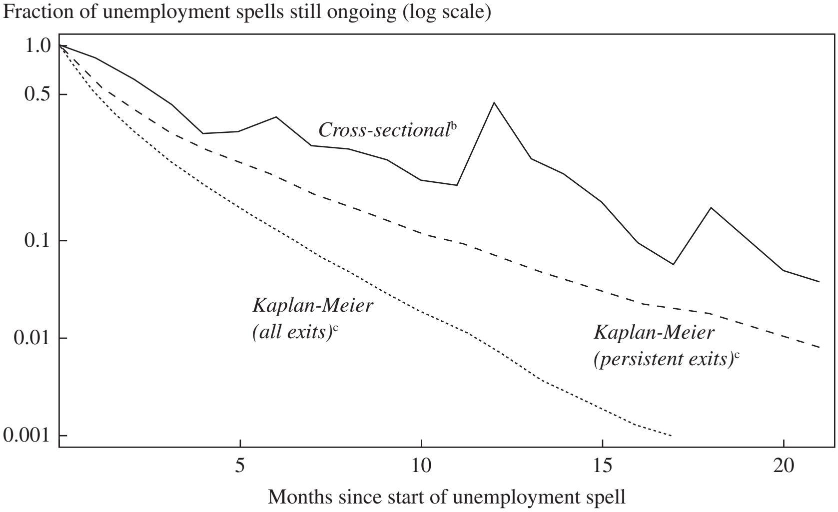
The results in tables 3 through 7 indicate that the UI benefit extensions enacted in 2008–10 reduced both the probability that a UI recipient found a job and the probability that the recipient exited the labor force, with somewhat larger estimated impacts on the latter probability than on the former. Moreover, the results are quite stable across a variety of specifications that exploit different components of the variation in UI benefits. However, the magnitudes are difficult to interpret. This section presents simulations of the net effect of the extensions on labor market aggregates, obtained by comparing actual unemployment exit hazards with counterfactual hazards that would have been observed in the absence of UI extensions.

V.A. Stocks and Flows in the CPS

Extrapolation of the estimated hazards to the aggregate level requires confronting an important limitation of the longitudinally linked CPS data: the exit hazards seen in the data are inconsistent with the cross-sectional duration profile. Figure 7 illustrates this by plotting survival curves computed in two different ways. The uppermost line uses the CPS as repeated cross sections, without attempting to link observations between months. The estimated survival rate to duration n of the cohort entering unemployment in month m is simply the ratio of the number of unemployed workers observed in month $m + n$ with duration n to the number observed in m with duration 0.[27] To smooth the estimated rate, I pool both numerator and denominator across all entrance months in calendar 2008.

27. In practice, the unemployment duration measure is in weeks, whereas the CPS sample is monthly. For figure 7, I compute the duration in months as floor($n/4.3$), where n is the duration in weeks and 4.3 is the average number of weeks in a month. Note that this construction does not constrain the survival curve to be downward sloping, and indeed the data show upward slopes at 6, 12, and 18 months, presumably a reflection of rounding in reported durations.

Figure 7. Alternative Unemployment Survival Curves from Cross-Sectional and Longitudinally Linked Data[a]



Source: Author's calculations from Current Population Survey data.

a. All series refer to unemployment spells beginning in 2008.

b. Number unemployed for d months in month $m + d$ divided by the number unemployed 0 months in month m (with each aggregated over months m in 2008).

c. The Kaplan-Meier survival curves are the product from $t = 0$ to $t = d - 1$ of the share of those unemployed in month $m + t$ with duration t who remain unemployed in month $m + t + 1$, computed from longitudinally linked data. The "persistent exits" series counts someone as remaining unemployed in $m + t + 1$ if he or she is unemployed in $m + t + 2$, regardless of the individual's measured status in $m + t + 1$.

Note: Figure has been corrected from the print version.

The figure also shows Kaplan-Meier survival curves based on unemployment exit hazards estimated from the linked CPS sample described in section II. The survival rate to duration n is computed as $\prod_{t=0}^{n-1} p(m + t, t)$, where $p(x, t)$ represents the share of unemployed individuals in month x at duration t who remain unemployed in month $x + 1$. The line labeled "Kaplan-Meier (all exits)" uses 2-month panels to estimate p, counting as survivors only those who report being unemployed in the second month (that is, only U-U transitions). The line labeled "Kaplan-Meier (persistent exits)" uses my preferred survival measure, using a 3-month panel to measure persistence of exits and counting exits between month 1 and month 2 only when the person does not return to unemployment in month 3 (that is, U-E-E, U-N-N, U-N-E, and U-E-N transitions count as exits between months 1 and 2, but U-E-U and U-N-U cycles are treated as survival in unemployment into month 2). As with the cross-sectional curve, both of the Kaplan-Meier curves are computed by pooling all unemployment entry cohorts from calendar 2008.

Both Kaplan-Meier survival curves are substantially below the curve computed from repeated cross-sectional data. The most important contributor to this discrepancy is the phenomenon highlighted in section II: it is not uncommon for an unemployed individual in month t to report being out of the labor force or employed in $t + 1$ and then unemployed again (often with a long unemployment duration) in $t + 2$. Although some of these transitions are real, a large share appear to be artifacts of measurement error in the $t + 1$ labor force status (Abowd and Zellner 1985, Poterba and Summers 1984, 1986). The alternative Kaplan-Meier survival curve based on the 3-month panel substantially reduces the discrepancy with the repeated cross-sectional data.

Extensive exploration of the CPS data points to two other factors contributing to the remaining discrepancy. The first is what has been called rotation group bias: the measured unemployment rate is higher in the first month of the CPS panel than in later months, even though each rotation group should be a random sample from the population (see, for example, Bailar 1975, Solon 1986, Shockey 1988). Second, individuals starting a new unemployment spell often report long durations. This phenomenon is particularly common when the employment spell that precedes the entry into unemployment is short, suggesting that respondents may be conflating what appear to be distinct spells into a longer superspell. However, this "late entry" phenomenon does not seem to be a complete explanation. In 2006 and 2007, for example, nearly 2,400 respondents are observed to be employed for 3 consecutive months and then unemployed in the fourth month; 10 percent of these report unemployment durations in the fourth month of longer than 6 weeks.

V.B. Reconstructing Survival Curves Consistent with the Observed Stocks

A full econometric model of measurement error in CPS labor force status and unemployment durations is beyond the scope of this paper. Instead, I use ad hoc procedures similar in spirit to the "raking" algorithm that the BLS uses in constructing the gross flows data (Frazis and others 2005) to force consistency between the Kaplan-Meier survival curve and the cross-sectional duration profile. I take the view that the cross-sectional profile is correct and that differences between this profile and my (adjusted)

Kaplan-Meier survival curve are due to "late entries" into unemployment.[28] I use two different adjustment methods; I argue below that one of these is likely to lead me to somewhat overstate the effect of UI extensions whereas the other is likely to understate it.

Let $u(m, n, s)$ be the count of individuals observed in month m in state s with duration n (in months) obtained from cross-sectional data; let $p(m, n, s)$ represent the probability that an individual in month m in state s with duration n persists in unemployment into month $m + 1$; and let $p^c(m, n, s)$ be the counterfactual persistence probability that would be observed in the absence of UI extensions. Both p and p^c are obtained from fitted values from the exit regressions presented in section IV.

The unemployed at duration n are the survivors from among the unemployed 1 month earlier, at $n - 1$. This creates a link between the $u()$ and $p()$ functions:

$$u(m,n,s) = u(m-1,n-1,s)\,p(m-1,n-1,s) + e(m,n,s). \tag{7}$$

In population data without measurement error, the residual $e(m, n, s)$ would be identically zero. The actual residual in equation 7 has two components. The first is mean-zero sampling error, which may cause the number of unemployed in newly entering rotation groups to differ from the number rotating out. The second is the late entry phenomenon discussed above, which leads to $E[e(m, n, s)] > 0$ for most n.

I wish to compare $u(m, n, s)$ with the counterfactual unemployment $u^c(m, n, s)$ that would be observed had the persistence probabilities been p^c rather than p. To do this, I assume that entry into unemployment at duration 0 is not affected by UI extensions: $u(m, 0, s) = u^c(m, 0, s)$ for all m and s. My two methods differ in their assumptions about the counterfactual values of $e(m, n, s)$.

My first method begins with an expression for $u(m, n, s)$ obtained by recursively substituting into the right-hand side of equation 7:

$$u(m,n,s) = u(m-n,0,s)\prod_{t=0}^{n-1} p(m-n+t,t,s) + E(m,n,s), \tag{8}$$

where $E(m,n,s) \equiv \sum_{r=1}^{n}\left[e(m-n+r,r,s)\prod_{t=r}^{n} p(m-n+t,t,s)\right]$. (Hereafter, I suppress the month and state subscripts, understanding that incre-

28. The UI system tabulates the number of individuals who exhaust their (regular program) benefits each month, providing an independent measure of survival. The implied exhaustion rates are much more nearly consistent with the cross-sectional survival curve than with the Kaplan-Meier curve.

ments to duration require corresponding increments to the month of observation in order to maintain a focus on the same entry cohort.) In this method I assume that the cumulative count of surviving late entries $E(n)$ is unaffected by UI extensions. I estimate $\hat{E}(n) \equiv u(n) - u(0)\prod_{t=0}^{n-1} p(t)$. This is simply the vertical distance between the top and middle lines in figure 7, evaluated at duration n. I use equation 8 to construct a counterfactual unemployment count:

$$(9) \qquad \hat{u}^{c1}(n) \equiv u(0)\prod_{t=0}^{n-1} p^{c}(t) + \hat{E}(n).$$

For my second method, I assume instead that the per-period late entries $e(n)$ are unaffected by UI extensions but that the subsequent persistence of these late entrants is affected. Following equation 7, I estimate $\hat{e}(d) = u(n) - u(n-1)p(n-1)$ and then define the counterfactual count iteratively as

$$(10) \qquad \hat{u}^{c2}(n) = \hat{u}^{c2}(n-1)p^{c}(n-1) + \hat{e}(n).$$

This can be rewritten to yield an intuitive expression for $\hat{u}^{c2}(n)$ in terms of actual counts $u(n)$ and two adjustments:

$$(11) \qquad \hat{u}^{c2}(n) \equiv u(n) + u(n-1)\left[p^{c}(n-1) - p(n-1)\right] + \left[\hat{u}^{c2}(n-1) - u(n-1)\right]p^{c}(n-1).$$

The first adjustment (the second term on the right-hand side of equation 11) reflects differences between the actual and the counterfactual scenarios in unemployment persistence at duration $n - 1$. The second adjustment (the last term in equation 11) captures differences in exit at durations $t < n - 1$, multiplied by the probability of surviving from $n - 1$ to n.

Neither assumption about the late entries is particularly plausible. First, there is no reason to expect that the job search behavior of late entrants to unemployment will be unaffected by UI extensions, particularly if these late-entrant observations are in part an artifact of measurement error in the preunemployment labor force status. If the late entrants are in fact affected, $E^{c}(n) < E(n)$ and $\hat{u}^{c1}(n) > u^{c}(n)$. This implies that the UI effect inferred from the comparison of $u(n)$ with $\hat{u}^{c1}(n)$ will understate the effect of UI extensions.

On the other hand, insofar as the late entries reflect people cycling from unemployment to nonparticipation and back, UI extensions that reduce the flow from unemployment into nonparticipation would also likely reduce the number of subsequent late entries. This would imply $e^{c}(n) > e(n)$ and

$\hat{u}^{c2}(n) < u^{c}(n)$, so a UI effect inferred from the comparison of $u(n)$ with $\hat{u}^{c2}(n)$ will likely overstate the effect of UI extensions on employment. Thus, there is reason to think that the two counterfactuals should bracket the true effect of UI extensions (assuming, of course, that the estimated effects of UI extensions on exit hazards obtained from the specifications in section IV are accurate).[29]

V.C. Results

Figure 8 presents the two counterfactual simulations of the number of unemployed, using the model from table 5, column 5-2, to construct p and p^{c} and aggregating across all durations at each point in time. The simulated results are plotted together with the actual, non–seasonally adjusted counts from the monthly CPS. The simulation using counterfactual method 1 indicates essentially no effect of the UI extensions: its line is hard to distinguish from the "actual" series. Counterfactual method 2 offers only a slightly different conclusion, suggesting that the UI extensions increased unemployment in 2010 and early 2011 by about 2.6 percent.

The top panel of table 8 presents more results from the simulations, using each of my four main strategies to generate predicted exit hazards and then simulating aggregate unemployment and the long-term unemployment share in January 2011.[30] The first specification is the one graphed in figure 8, using a cubic in the state unemployment rate to absorb endogeneity in the availability of extended UI benefits. The second specification uses the comparison of job losers with job leavers reported in table 3, column 3-6, to generate the exit hazards. The third uses the control function specification from table 5, column 5-5, identified from state decisions about whether and how to participate in the EB program. The fourth uses the time-to-exhaustion model from the third regression in table 7.

The estimates indicate that UI extensions raised the number of unemployed in January 2011 by between 5,000 and 759,000, the unemployment

29. State × month–level estimates of $E(n)$ and $e(n)$ are extremely noisy. However, national-level monthly estimates can be obtained by aggregating across states. The time-series relationship between $\hat{E}(n)$ and UI benefit durations is robustly negative, consistent with the view that method 1 understates the effect of UI extensions. The estimated relationship between $\hat{e}(n)$ and benefit durations is weaker and generally not statistically significant.

30. I count anyone unemployed 6 months or more as long-term unemployed. This means that I generally include people who report being unemployed for exactly 26 weeks on the survey date, whereas the BLS long-term unemployment definition uses durations of 27 weeks or more. This accounts for the discrepancy between the baseline long-term unemployment rate in table 8 and the published rate of 42.2 percent.

Figure 8. Actual Unemployment and Counterfactual Simulations without UI Extensions, 2007–10[a]

Source: Author's calculations.

a. Counterfactual simulations are based on the specification in column 5-2 of table 5. See the text for details.

b. Actual, non–seasonally adjusted counts from the monthly CPS.

rate by 0.1 to 0.5 percentage point, and the long-term unemployment share by between 0.3 and 2.8 percentage points. In each case the largest estimates come from counterfactual method 2 and the control function specification (strategy 3); when these are omitted, the upper ends of the ranges are 370,000 unemployed, 0.2 percentage point on the unemployment rate, and 1.6 percentage points of long-term unemployment. These are much smaller effects than are indicated by the extrapolations discussed in section I.D.

The bottom panel of table 8 presents an alternative and more speculative set of counterfactual simulations. An important question regarding the effects in the top panel is whether the effect of UI extensions on unemployment reflects reduced job search behavior or simply reduced labor force exit. As a first effort to assess this, I rerun the simulations, turning off the effects of UI extensions on the propensity to become reemployed and retaining only the effects on the labor force exit propensity. Specifically, let X_{ist} be the observed values of the explanatory variables, and let ψ_e and ψ_n

Table 8. Effect of UI Extensions on Labor Market Aggregates in January 2011[a]

	Specification (column or row in previous table)	Increase in unemployment				Increase in long-term unemployment share (percentage points)	
		Thousands of workers		Rate (percentage points)			
		Method 1	*Method 2*	*Method 1*	*Method 2*	*Method 1*	*Method 2*
Actual, January 2011		14,937		9.0 percent		45.5 percent	
Full effect of UI extension							
Strategy 1	5-2	87	370	0.1	0.2	0.5	1.6
Strategy 2	3-6	131	297	0.1	0.2	0.3	0.9
Strategy 3	5-5	283	759	0.2	0.5	0.9	2.8
Strategy 4	7-3	5	226	0.0	0.1	0.6	1.5
Effect operating through labor force participation[b]							
Strategy 1	5-2	98	264	0.1	0.2	0.3	0.9
Strategy 3	5-5	183	476	0.1	0.3	0.5	1.6
Strategy 4	7-3	92	208	0.1	0.1	0.3	0.8

Source: Author's calculations.

a. Effects are differences between the actual level or rate of unemployment or the long-term unemployment share and a simulation that holds benefit durations fixed at 26 weeks throughout 2004–11, using estimated coefficients from the indicated specifications. "Method 1" and "Method 2" refer to alternative treatments in the counterfactual of residuals obtained from simulating the actual data; see the text for details.

b. It is assumed that in the counterfactual scenario the multinomial logit index for the labor force exit outcome would change but that the index for the reemployment outcome would be unaffected.

be the full vectors of covariates from the employment and nonparticipation equations, respectively, of the multinomial logit model.

The one-period survival probability is then $p_{ist} = [1 + \exp(X_{ist}\psi_e) + \exp(X_{ist}\psi_n)]^{-1}$, and the counterfactual survival probability used for the simulations in the top panel of table 8 is $p^c_{ist} = [1 + \exp(X^c_{ist}\psi_e) + \exp(X^c_{ist}\psi_n)]^{-1}$, where X^c_{ist} represents the explanatory variables in the counterfactual scenario where benefits are fixed at 26 weeks. In the bottom panel I use instead $p^c_{ist} = [1 + \exp(X_{ist}\psi_e) + \exp(X^c_{ist}\psi_n)]^{-1}$. Comparisons of simulations based on p_{ist} and p^c_{ist} reveal how much of the overall effect revealed by the $p_{ist} - p^c_{ist}$ comparison is due to labor force exit. The results in this panel indicate that just turning off the effect of UI extensions on labor force exit reduces unemployment by more than half as much as did turning off both UI effects in the top panel.[31] In other words, the majority of the effect of UI extensions on overall unemployment and on long-term unemployment operates through the labor force exit channel, by keeping people in the labor force who would otherwise have exited, rather than through reduced reemployment rates.

These last results must be interpreted with some caution, as they rest importantly on the assumption of independent risks. With this assumption, an individual who is dissuaded from exiting the labor force in one month has approximately a 13 percent chance of becoming reemployed the next month, the same as would an individual who never considered abandoning job search. This is probably not realistic; one might expect that the unemployed with the worst employment prospects are the most likely to exit the labor force. Thus, the results in the bottom panel of table 8 might overstate the share of the UI effects attributable to labor force exit decisions. Even so, it is clear from the top panel alone that any negative reemployment effect must be small.

VI. Discussion

The design of unemployment insurance policy trades off generosity to workers who have experienced negative shocks against the disincentive to return quickly to work created by the availability of generous nonwork benefits. In bad economic times, displacement from a job is a much larger shock, as it can take much longer to find new work. Moreover, insofar as weak labor markets reflect a shortage of labor demand, the negative consequences of reduced search effort among the unemployed may be relatively

31. I do not report estimates for strategy 2 in this panel, as the multinomial logit version of this specification is computationally intractable.

small.[32] It thus stands to reason that one might want to extend UI benefit durations during bad times (Landais, Michaillat, and Saez 2010, Kroft and Notowidigdo 2011, Schmieder, von Wachter, and Bender forthcoming). Such extensions can have macroeconomic benefits as well, as the unemployed likely have a high marginal propensity to consume, and UI payments thus have relatively large multipliers (Congressional Budget Office 2010).

However, the advisability of long UI extensions depends importantly on the view that the reduced job search induced by these extensions will not overly slow the labor market matching process. Many commentators have argued that the 99 weeks of benefits available through the EUC and EB programs in 2010 and 2011 have gone too far, and some have pointed to the apparent outward shift of the Beveridge curve in 2010 (Elsby and others 2010) as evidence that UI extensions have reduced labor supply sufficiently to noticeably slow the recovery of the labor market.

It is ultimately an empirical question whether UI extensions lead to large reductions in job finding. But the effect is hard to identify, because extensions are usually implemented in response to poor labor market conditions. Fortunately for the researcher (if not for the UI recipients themselves), the haphazard way in which UI benefits were extended generates a great deal of variation in benefit availability that is plausibly exogenous to the demand conditions that otherwise confound efforts to estimate the benefit duration effect.

Using a variety of comparisons that isolate different components of the variation in benefit availability, I find that extended UI benefits do reduce the rate at which unemployed workers reenter employment. But the reductions are small, in most specifications smaller than the effects on labor force exit and always much smaller than what one would have expected based on older estimates in the literature. The two effects both lead to increases in measured unemployment, but combined they have raised the unemployment rate by only about 0.2 percentage point, implying that the vast majority of the 2007–09 increase in the unemployment rate was due to demand shocks rather than to UI-induced supply reductions. Moreover, less than half of the small UI effect comes from reduced reemployment rather than from reduced nonparticipation (that is, from increased labor supply).

Any negative effects of the recent UI extensions on job search are clearly quite small, too small to outweigh the consumption-smoothing and

32. See, for example, Kroft and Notowidigdo (2011). Schmieder, von Wachter, and Bender (forthcoming) find evidence in Germany, however, that the reemployment effect of UI durations is relatively constant across the business cycle.

equity-promoting benefits of UI (Gruber 1997). The latter are likely to be particularly large when the marginal recipient has been out of work for over a year in conditions where job finding prospects are bleak. Moreover, the estimates herein should be seen as reflecting the partial equilibrium effects of UI, as they do not account for search externalities: when jobs are scarce, a job claimed by one searcher reduces the probability that other searchers will find employment.[33] Incorporating these spillovers would make extensions more attractive, as reduced job search among a subset of the unemployed would not translate one for one into reduced employment but rather would simply shift jobs from the UI recipients to other job seekers (Landais and others 2010). The evidence here thus supports the view that optimal UI program design would tie benefit durations to labor market conditions, to give those who have lost their jobs realistic chances of finding new employment before their benefits expire.

APPENDIX

Proofs of Propositions

All proofs are by induction.

Proof of Proposition 1. An individual's decision problem in state $d > 0$, holding search effort for all lower d fixed, is to choose s to maximize

$$V_U(s, d) = u(y_0 + b) - s + \delta\left[p(s)V_E + (1 - p(s))V_U(d - 1)\right].$$

The optimal s is labeled s_d and by definition satisfies $V_U(s_d, d) = V_U(d)$.

Note that the maximization problem is identical whether $d = 1$ or $d = 0$. (Compare equation 1, evaluated at $d = 1$, with the problem in note 8—they differ only by an additive term $u(y_0 + b) - u(y_0) > 0$ that is invariant to search effort.) Thus, $s_1 = s_0$ and $V_U(1) - V_U(0) > 0$. Second, assume $V_U(x) > V_U(x - 1)$ for some $x > 0$. Then

$$\begin{aligned} V_U(x+1) - V_U(x) &= V_U(s_{x+1}, x+1) - V_U(s_x, x) \\ &\geq V_U(s_x, x+1) - V_U(s_x, x) \\ &= \delta\left(V_U(x) - V_U(x-1)\right)\left(1 - p(s_x)\right) > 0. \end{aligned} \tag{A.1}$$

Thus, $V_U(d + 1) > V_U(d)$ for all d. □

33. In principle, estimates identified from across–state × month comparisons should capture these externalities. However, because my samples for these estimates exclude large fractions of job seekers, only a portion is captured.

Proof of Proposition 2. See above for $s_1 = s_0$. For $d \geq 1$, s_d satisfies the first-order condition $p'(s_d) = \dfrac{1}{\delta[V_E - V_U(d-1)]}$. Proposition 1 thus implies that $p'(s_{d+1}) > p'(s_d)$, so $p''(s) < 0$ implies $s_{d+1} < s_d$. □

Proof of Proposition 3. Let $\tilde{s}_d = \arg\max_s \tilde{V}_U(s, d)$, where

$$\tilde{V}_U(s,d) = \begin{cases} u(y_0 + b) - s + \delta[p(s)V_E + (1-p(s))V_U(d-1)] & \text{if } s \geq \theta \\ u(y_0) - s + \delta[p(s)V_E + (1-p(s))V_U(d)] & \text{if } s < \theta, \end{cases}$$

and let $\eta_d = 1(\tilde{s}_d \geq \theta)$. I show that $\eta_{d+1} \neq \eta_d$ for any $d > 0$ yields a contradiction. Without loss of generality, suppose that $\eta_d = \eta_{d-1} = \ldots = \eta_0$; this merely means that we have chosen the smallest d such that $\eta_{d+1} \neq \eta_d$.

Begin by considering the case where $\eta_d = 1$, so $\tilde{s}_x \geq \theta$ for all $x \leq d$. Then an argument identical to that above implies that the search requirement is never binding: $\tilde{s}_1 = \tilde{s}_0$, and for all $x > 0$, $\tilde{V}_U(x+1) - \tilde{V}(x) > 0$ and $\tilde{s}_{x+1} > \tilde{s}_x$. In particular, $\tilde{s}_{d+1} > \tilde{s}_d$, so $\eta_{d+1} = 1$.

Next, suppose that $\eta_d = 0$ but $\eta_{d+1} = 1$. The former implies that

$$\text{(A.2)} \qquad \begin{aligned} \tilde{V}_U(x) &= \max_{s<\theta} u(0) - s + \delta[p(s)V_E + (1-p(s))\tilde{V}_U(x)] \\ &= \max_{s<\theta} \frac{u(0) - s + \delta p(s)V_E}{1 - \delta(1-p(s))} \end{aligned}$$

for all $0 \leq x \leq d$. Note that the right-hand side of equation A.2 does not vary with x, so the left-hand side does not either. In particular, $\tilde{V}_U(d) = \tilde{V}_U(d-1)$. Moreover, because labor force exit with $s = \tilde{s}_d < \theta$ is a feasible option with $d + 1$ weeks of benefits available, it must be the case that $\tilde{V}_U(d+1) > \tilde{V}_U(d)$. Next, note that

$$\text{(A.3)} \qquad \begin{aligned} \tilde{V}_U(d) &< \tilde{V}_U(d+1) \\ &= \tilde{V}_U(\tilde{s}_{d+1}, d+1) \\ &= u(b) - \tilde{s}_{d+1} + \delta[p(\tilde{s}_{d+1})V_E + (1-p(\tilde{s}_{d+1}))\tilde{V}_U(d)] \\ &= \tilde{V}_U(\tilde{s}_{d+1}, d) + \delta(1-p(\tilde{s}_{d+1}))(\tilde{V}_U(d) - \tilde{V}_U(d-1)) \\ &< \tilde{V}_U(d) + \delta(1-p(\tilde{s}_{d+1}))(\tilde{V}_U(d) - \tilde{V}_U(d-1)), \end{aligned}$$

where the final inequality follows from a revealed preference argument for benefit duration d. This implies that $\tilde{V}_U(d) > \tilde{V}_U(d-1)$, a contradiction.

There are thus only three possible values for the η_d sequence: $\eta_d = 1$ for all $d \geq 0$; $\eta_d = 0$ for all $d \geq 0$; or $\eta_d = \begin{cases} 0 & \text{if } d = 0 \\ 1 & \text{if } d > 0 \end{cases}$. Unemployment-to-nonparticipation transitions thus occur only when benefits are exhausted; benefit extensions will delay these transitions for those who would otherwise have exhausted their benefits. □

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Comments and Discussion

COMMENT BY

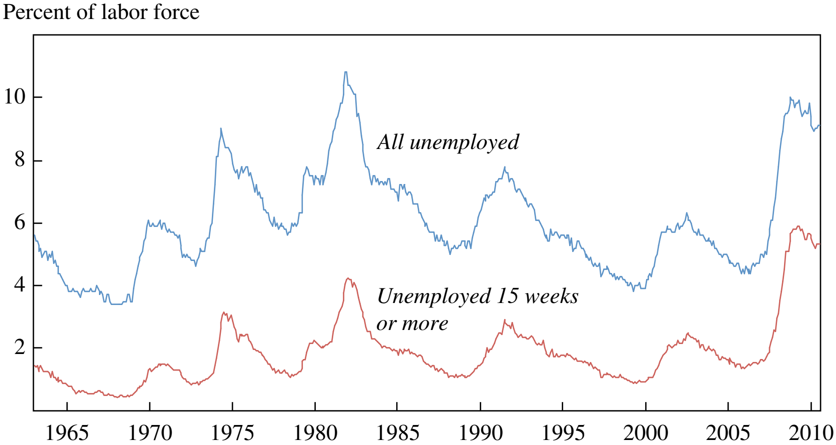
STEPHANIE AARONSON[1] This paper by Jesse Rothstein examines the extent to which the significant recent expansion of unemployment insurance (UI) benefit durations, first enacted in June 2008 and gradually extended to allow up to 99 weeks of benefits, has contributed to the persistently high level of unemployment during the 2007–09 recession and its aftermath. Let me state up front that Rothstein's paper really appealed to me. It addresses a question that has important implications for macroeconomic policy and takes advantage of all the abundant variation that one finds in microdata to answer it.

Before I focus on Rothstein's empirical strategy, it is worth laying out the macro question in a bit of detail. At issue is whether the current high unemployment is due to a shortfall in aggregate demand or to an increase in structural unemployment. The answer has important implications for both fiscal and monetary policy. To the extent that the cause is a shortfall in aggregate demand, there is room for monetary and fiscal policy to bring about an improvement. If, on the other hand, the cause is a rise in structural unemployment—for instance, because the UI program has made people less likely to move from unemployment into employment—then there is less scope for policies that stimulate aggregate demand, although there could be room for policies that improve the functioning of the labor market.

Properly measuring the costs and benefits of UI benefits is important. Families with a member who has been unemployed for a long time are

1. This review represents the views of the author and does not necessarily represent the views of the U.S. Department of the Treasury, the Board of Governors of the Federal Reserve System, the Federal Reserve System, or their staffs.

Figure 1. Unemployment and Long-Term Unemployment, 1964–2011[a]

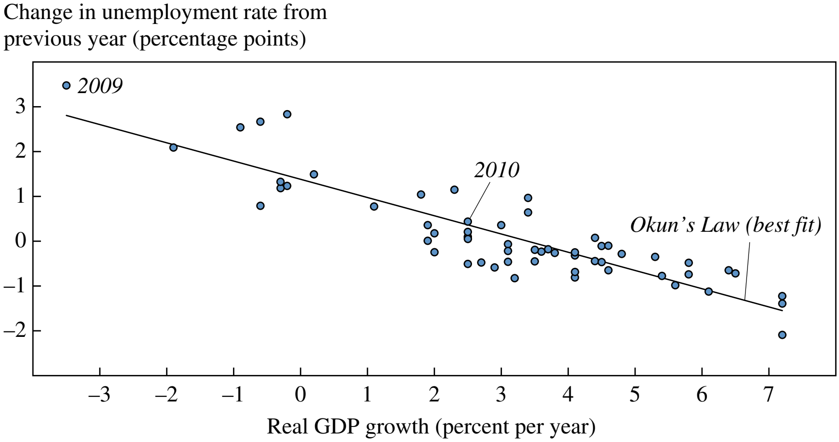


Source: Bureau of Labor Statistics.
a. Monthly data.

likely to be struggling financially, and evidence indicates a high marginal propensity to consume out of UI benefits, close to 1. At the same time, however, the UI extensions are not costless. If recipients are people who would otherwise be working, then the program expansion could in theory be making the problem worse. The individual optimization problem should take into consideration not only the immediate labor-leisure trade-off in the presence of the benefits, but also the impact of these workers' current unemployment on their future job opportunities. If this latter effect is not adequately accounted for, there could be an unintended individual cost. From the perspective of society, the costs include not only the direct expenditure on benefits, but also any shortfall in output due to lower employment and any externality from the high unemployment, for instance in terms of future productivity.

Before I turn to the econometric approach Rothstein takes, it is worth examining the work disincentive effects of UI benefits more closely. A considerable economic literature has shown that extended UI benefits do reduce exit from unemployment. The question is whether the effect is large enough to explain a significant portion of the increase in unemployment seen since the start of the recession. My figure 1 is similar to Rothstein's figure 1 but shows a longer time series. As can be seen, both the unemployment rate and the share of the labor force that has been unemployed at least 15 weeks have increased dramatically, even when compared with the

Figure 2. Okun's Law Relationship, 1951–2010



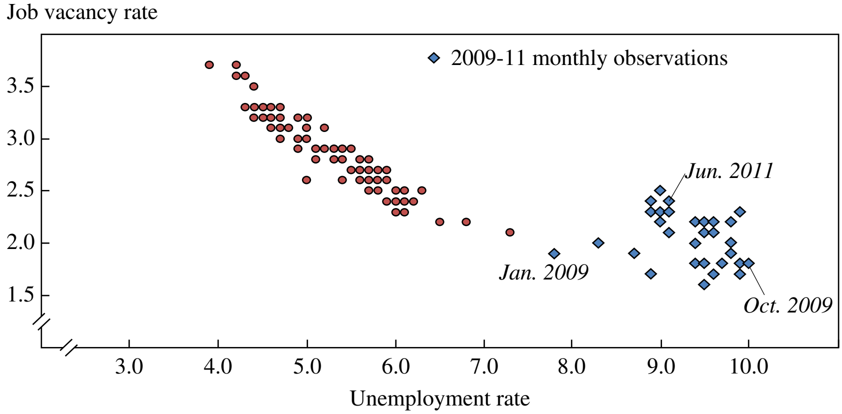
Sources: Bureau of Labor Statistics and Bureau of Economic Analysis.

previous severe recession, that of the early 1980s, and remain at high levels now.[2] Although the unemployment rate was higher then, so was the natural rate of unemployment. Of course, the recent recession was the deepest in the postwar period, so the run-up in unemployment and long-term unemployment is not entirely surprising. Another way to think about whether the unemployment rate is unusually high is to compare the increase in the unemployment rate with the change in GDP—the Okun's Law relationship. As my figure 2 shows, in 2009 the unemployment rate rose more than would be expected given the decline in output. However, in 2010 the unemployment rate moved about in line with growth in GDP, and evidence suggests that in 2011 the unemployment rate has fallen more than would be anticipated by the relatively modest rise in output. Thus, the unemployment rate as of this writing does not seem particularly high by this measure.

Another way to think about whether the unemployment rate is unusually high is in terms of the Beveridge curve. As my figure 3 shows, compared with the relationship before the recession, recent readings on the unemployment rate are elevated relative to job vacancies. However, in the normal cyclical pattern, a rising unemployment rate moves the Beveridge

2. It is worth noting that the survey from which these data are drawn, the Current Population Survey, was redesigned in 1994. The redesign was partly aimed at better identifying an individual's labor market status. Although the redesign had only a marginal impact on the reported aggregate unemployment rate, it substantially increased reported average unemployment durations (Polivka and Miller 1998).

Figure 3. Beveridge Curve, December 2000–October 2011



Sources: Job Openings and Labor Turnover Survey and Current Population Survey.

curve counterclockwise during a recession, and so the rise in structural unemployment suggested by the current Beveridge curve is perhaps on the order of 1 percentage point.

Of course, this discussion of the Beveridge curve raises precisely the problem faced by Rothstein and others who have examined the relationship between UI benefits and unemployment. Because Congress extends UI benefits at times when aggregate demand is weak, it is difficult to distinguish the rise in the unemployment rate induced by the extension from the rise due to the lack of demand for labor.

How does Rothstein attack the problem? His basic approach is to carefully model the institutional details of the UI program and to adopt a variety of identification strategies to estimate its effects, which allows him to test the robustness of the results. He also decomposes the total impact of extended UI benefits on unemployment into a part due to the impact on labor force participation and a part due to the impact on employment. This decomposition is helpful because it gets at the question of whether economic activity is being hampered by the program. It is also worth noting one thing that Rothstein does not do, which is to use information from past episodes in which benefits were extended. This is important because, relative to previous episodes, the contraction that precipitated the recent extension was unusually prolonged and the recovery has been relatively anemic. This suggests that the behavioral response could be different than in the past. In addition, the 1994 redesign of the Current Population Survey (see my note 1) may limit the usefulness of previous episodes.

Of the four identification strategies Rothstein uses, I will focus on three. The first uses variation in the availability of UI benefits due to stops and starts in program implementation, controlling for labor demand. The expansion of the UI program responded not only to economic conditions, but also to the exigencies of the political process. Congress regularly let the program expire, and renewals were always uncertain. Finally, states could choose whether to participate in the extended benefits (EB) portion of the program. These idiosyncrasies in implementation break the link between economic conditions and the duration of available benefits. Although Rothstein models all these institutional details carefully, his use of state unemployment rates to control for economic conditions raises a problem: even with these controls (and controls for state fixed effects), there could be an omitted variable relating to the political economy of a state—for example, the availability of other benefits—that both contributes to the availability of EB and affects the probability that people remain unemployed.

What Rothstein denotes as his third identification strategy is very similar to the one just described but adds controls for the estimated duration of the federal component of the UI expansion, called emergency unemployment compensation (EUC). He also adds control functions that model the individual state EB triggers. This leaves variation in the state adoption of EB as the only source of identification. It is worth noting that this specification does increase the estimated impact of the UI extensions on labor force decisions. In particular, the impact on the average exit hazard in 2010Q4 rises from about 2 percentage points in the various specifications that follow the first identification strategy to about 3 percentage points in this one.

A third identification strategy (the second in the order in which the paper presents them) is to use voluntary job leavers as a control group. The problem here is that people who leave their job but remain in the labor force must have information that the researcher does not about their job opportunities or their willingness to drop out of the labor force; these individuals likely have better opportunities or are more willing to exit the labor force than a similarly situated person who has been laid off. Rothstein is well aware of the problems with this approach and takes a number of steps to control for the differences between the two groups that one can observe: his regressions include as controls an estimated duration of UI benefits for the job leavers and a variety of individual covariates. In addition, the inclusion of job leavers allows Rothstein to include state-month fixed effects. This actually enables him to control for the omitted variable that, as I proposed above, could be correlated with both an individual's decision to receive UI and the state's decision to offer EB. Interestingly, the estimated effect of the UI extensions is smaller in the specifications identified using this third strategy (the average reduction in the 2010Q4 exit hazard falls to about 1 percentage point), although, perhaps unsurprisingly given the amount of variation soaked up by the state-month fixed effects, the coefficients are less precisely estimated than in other specifications. Despite Rothstein's considerable work on this specification, I have mixed feelings about it. On the one hand, it seems that despite the individual controls, there must still be unobserved differences between job losers and job leavers that affect the probability of exit from unemployment. On the other hand, the inclusion of the state-month fixed effects seems desirable, even if their usefulness is limited somewhat by the reduction in power.

Having identified the impact of UI extensions on unemployment exit hazards, Rothstein turns to decomposing this effect into changes due to reemployment probabilities and changes due to labor force exit. For this he uses multinomial logits. However, as he himself notes, the IIA (independence of irrelevant alternatives) assumption implicit in a multinomial logit is likely to be violated. The problem is that the choice between being unemployed and exiting the labor force is not completely clear for the marginal displaced worker—it is likely to be a matter of search effort. But UI extensions probably have a particularly large impact on people who would otherwise have exited because their reemployment probabilities are low. As a result, the extensions could appear to depress reemployment probabilities simply by increasing the share of the unemployment pool that is less employable. Rothstein attempts to ameliorate this problem by including a standard set of controls for personal characteristics. Although these may help, the differences in search effort are likely driven by unobserved heterogeneity. For this reason an alternative (albeit computationally more costly) estimation strategy is probably worth pursuing. In particular, Rothstein could have used multinomial probits, which do not require the IIA assumption, or multinomial logits with random effects, which would absorb some of the unobserved heterogeneity. In the absence of results from one of these alternative techniques, I hesitate to put too much weight on these results (or the analogous results dividing the unemployment rate effect of UI extensions into parts due to reemployment and labor force exit), although I find them suggestive.

The penultimate section of the paper maps the estimated effect of UI extensions on exit hazards onto effects on the stocks of the unemployed. To accomplish this, Rothstein statistically forces the survival rates derived from the transitions observed in the matched monthly CPS to equal the survival rates reported by respondents. This raises the question of whether one should trust the reported durations more than the transition-derived durations.

There are a number of obvious problems with the reported durations. First, they are subject to substantial recall bias. Rothstein provides evidence from the matched CPS data that people who report new spells of unemployment often report durations longer than is consistent with their observed history. Moreover, a quick look at figure 7 of Rothstein's paper shows substantial heaping of responses at certain durations, which suggests that recall bias is important. In addition, there is the problem of dependent coding of the monthly CPS: if a person who is unemployed in one month is determined to be unemployed in the next, that person's duration is automatically increased by 4 weeks, regardless of whether he or she was unemployed the whole time.

In contrast, some of the criticisms leveled against the transition-based survival hazards are not particularly relevant. For instance, with regard to the dependent coding, it has been argued that even if individuals actually experience a short spell of employment or nonparticipation between surveys, this is not a meaningful exit from unemployment, and therefore it is not a problem for them to have been counted as unemployed for the entire period. However, even if a person is actually out of work for the reported duration, he or she might not have been unemployed by the CPS definition, which is what one is trying to match. Rothstein also presents evidence from validation studies done in the 1980s showing significant numbers of spurious transitions. However, one goal of the 1994 CPS redesign was to improve the identification of labor market status, and there is evidence in papers by Bureau of Labor Statistics (BLS) staff at the time that it did improve their ability to consistently identify unemployment. Therefore, validation studies from the 1980s criticizing the transitions are not so relevant. Here I should point out that Rothstein did talk to staff at the BLS to obtain updated information on the validity of the transitions reported in the monthly CPS, but the BLS would not release the data. Finally, it should be noted that even the monthly flows understate transitions. Christopher Nekarda (2009), using weekly data from the Survey of Income and Program Participation, finds that gross flows are understated by 15 to 24 percent in monthly data. This does not suggest that one's prior should be that U-N-U and U-E-U transitions are spurious.

Unfortunately, Rothstein does not test the robustness of his results to forcing the survival curve from the flow data to look like that from the reported durations, although he does present two different methods of reconciling the curves. From private correspondence, I think he believes he would find even smaller results using the transition-based durations. Nonetheless, I would have liked to see a robustness check of this assumption.[3]

All that said, I found the results that UI extensions have had a small impact on the unemployment rate in the recent recession and recovery compelling. Rothstein uses a variety of identification strategies to estimate his results and subjects them to numerous specification tests. Moreover, the simple fact that Rothstein estimates, rather than extrapolates, the results, and the care with which he performs the analysis, make this an important contribution to the literature. Although his results are on the low side of other estimates, they are not orders of magnitude different from those of other carefully performed analyses, even those that do extrapolate. Whether the UI program has raised the unemployment rate by 0.2 percentage point or 1 percentage point (or somewhere in between, as I suspect), the fact is that extended UI benefits can explain only a small portion of the rise in the unemployment rate since the recession.

By itself the paper cannot answer the question I raised at the outset: whether the current increase in the unemployment rate is due to a shortfall in aggregate demand or to a rise in structural factors. However, the finding does eliminate one potential cause of higher structural unemployment. Moreover, the fact that the impact of the UI extensions program is small argues in favor of extending UI benefits as part of a fiscal stimulus package, since it helps families in need and has a high multiplier, with only a small downside.

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3. Rothstein does test whether the decision to exclude individuals with U-E-U and U-N-U transitions biases the results of his hazard rate models. The estimated effects of UI on the hazard rates are a bit larger, but of the same order of magnitude.

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COMMENT BY

LISA B. KAHN In this paper Jesse Rothstein asks whether the recent extensions to unemployment insurance (UI) benefits have led to an increase in the unemployment rate. Basic agency theory suggests that subsidizing unemployment creates a disincentive for workers to search for jobs. However, it is unclear whether, in a period of severely depressed labor demand, this moral hazard imposes so large a cost as to outweigh the many benefits associated with UI extensions. Rothstein finds that the recent incarnations of the Emergency Unemployment Compensation (EUC) and Extended Benefits (EB) programs have had only small impacts on the overall unemployment rate, raising it by 0.1 to 0.5 percentage point. He shows that the bulk of the effect is in dissuading unemployed workers from exiting the labor force, with a smaller share being driven by reduced reemployment rates. His findings are remarkably robust to four different identification strategies.

The impact of UI on a worker's labor supply is one of the most important questions facing policymakers today. The EUC and EB programs, which currently extend UI coverage from 26 weeks to potentially 99 weeks, are frequently up for renewal and thus continuously in need of justification. However, previous research serves as only a rough guide to the cost-benefit analysis of these programs.

An older literature on the impacts of UI on job search produced a wide range of estimates that are quite out of date. The gold standard approaches were those of Lawrence Katz and Bruce Meyer (1990) and Robert Moffitt (1985), who exploited the observation of a large spike in the probability of exiting unemployment in the last week of coverage, and the natural experiment approach of David Card and Philip Levine (2000). Katz (2010) himself points out that labor market institutions such as temporary layoffs and recalls have changed substantially since the period most of these papers study. Further, the methodology exploited in most of the previous literature did not allow for separate estimates of the impacts of UI on reemployment and labor force exit. Especially for policy, the distinction is important.

In response to renewed policy interest in this question, a new iteration of papers has emerged. One strand extrapolates from the previous literature, and for the reasons stated above, its results can be quite misleading. Meanwhile many of the regional Federal Reserve banks have quickly filled policymakers' need for up-to-date research with clever back-of-the-envelope calculations. Their estimated impacts of UI extensions on the unemployment rate range from about 0.4 to 2.0 percentage points (Aaronson, Mazumder, and Schechter 2010, Fujita 2010, Valletta and Kuang 2010). These papers helpfully provided reasonable, relatively consistent estimates in a hurry.

However, the literature still cried out for systematic econometric studies performed on data contemporaneous to the crisis. Rothstein provides this.[1] His approach is able to estimate the separate impacts of UI on reemployment and labor force exit—a distinction that matters for policymakers, and one that many of the previous papers were not able to make. He makes extremely careful use of longitudinally linked Current Population Survey data. He very carefully considers sources of variation in UI benefit durations, a point that I discuss in more detail below. Finally, his results pass the smell test: he finds some evidence for moral hazard, which aligns with economic theory, but he also finds that the distortions are small, as the current distressed state of labor demand might suggest.

The difficulty in studying this problem is that benefits are extended at precisely the moment when job finding rates fall. Rothstein's approach to dealing with this problem is a veritable kitchen sink, exploiting four different sources of variation. He mainly exploits discrete changes in maximum benefit length based on triggers that vary across states and over time, allowing him to control for changes in local labor market conditions. The downside is that no single approach is perfect; each requires somewhat unpalatable assumptions. For example, in some specifications he must parametrically control for economic conditions, whereas in others he exploits cross-sectional variation in UI eligibility and can control for state-month fixed effects. The latter strategy alleviates reliance on functional form but requires the assumption that those ineligible for UI are a good control group for the eligible. The assumptions thus differ across approaches, yet each approach yields estimates that are remarkably consistent with the others. This suggests that no one assumption can be driving the results, so that one feels better about the overall package.

The regressions reported in the paper take the form of a job finding hazard on the left-hand side and expected benefit duration on the right,

1. So do Farber and Valletta (2011) in a contemporaneously written paper.

along with several different controls for current labor market conditions. The main difficulty, as I see it, is in measuring the length of time unemployed workers *expect* to receive benefits. Rothstein equates expectations with current law, assuming that workers expect no further action by Congress to extend benefits. Henry Farber and Robert Valletta (2011) obtain similar results with the opposite approach, one that assumes that current law will be extended throughout the worker's UI duration. Both assumptions are reasonable, but both will suffer from measurement error since it is impossible to capture true expectations.

This means that the main coefficient suffers from attenuation bias. Moreover, the problem is worse than that since, for a given worker, the accuracy of this proxy for expectations will change over time. In particular, as unemployment duration increases, current law probably becomes a more accurate predictor of a worker's expectations.

For example, consider a worker who became unemployed in January 2010 and lived in a state with 99 weeks of UI. At that point Rothstein's measure of expected duration for that worker, D_{its}, would have been 46 weeks (26 weeks of regular UI coverage and 20 weeks of EB), since the EUC program was scheduled to sunset later that winter.[2] By late July 2010, if the worker were still unemployed, D_{its} would have included one tier of EUC, since the program was reauthorized through November 2010. In December, when all four EUC tiers were reauthorized until January 2012, D_{its} for this worker would have been the full 99 weeks. Ex post, we know that this worker would be eligible for 99 weeks of UI, but Rothstein's measure would have only incorporated this about a year into the UI spell. It is unclear what the worker's expectations would have been throughout the spell. However, both Rothstein's measure and the worker's expectations would have been most accurate toward the end of the 99 weeks.

The attenuation bias generated by this measurement error is a bigger problem for workers with low durations of unemployment than for those with high durations. This could be why Rothstein finds effects that are almost always larger in magnitude for those with more than 26 weeks of unemployment. He addresses this problem as best he can, with some robustness checks. But it is worth thinking about whether this measurement error problem causes him to slightly understate the impact of UI extensions on the unemployment rate.

2. In fact, the worker might have expected only 26 weeks, since many states also had automatic triggers that would end their EB programs when 100 percent federal funding expired along with the EUC program.

Despite the problem of measurement error, I believe this paper establishes quite well that in the current economy, UI extensions pose minimal consequences to job search behavior. Given that conclusion, it is worth thinking next about the benefits that UI provides, and in particular the benefits of extending UI in an economic slump. Rothstein, rightly, does not expound on these issues in his paper; he sets out a specific, important question and answers it well. In the rest of this comment, I will touch on some of the issues beyond the scope of his paper, including the value of the extra search time that UI provides unemployed workers and the value of UI as economic stimulus.

It has been commonly suggested that UI allows workers more time to search for the right job, thus helping them find better matches. Recent evidence suggests that the stakes to finding the right job are particularly high in recessions. In their paper in this volume, Steven Davis and Till von Wachter summarize and provide new evidence on the long-term costs of job displacement, finding that the effects are particularly large and damaging when displacement comes in a recession. Further, a growing body of work finds that workers who graduate from school in a downturn receive on average, lower wages, which persist long into their careers, even though they spend little time in unemployment (Kahn 2010, Oreopoulos, von Wachter, and Heisz 2012, Oyer 2008).

These findings suggest that having to search for work during an economic slump is particularly damaging. Indeed, job matches in recessions are typically of lower quality and in worse firms (Bowlus 1995, Davis, Haltiwanger, and Schuh 1996). Furthermore, in recent work (Kahn 2011) I have shown that despite ending up in worse jobs, workers who take jobs in a downturn actually stay in those jobs longer than do other workers at the same firm. It is unclear what mechanism drives these results, but they do suggest that job placement in a downturn is crucial to future success. Extensions to UI allow workers some flexibility toward putting themselves in the best job possible.

A report by the Council of Economic Advisers (2010) estimates that as of 2010Q3, 40 million people had benefited from EUC or EB either as recipients themselves or through receipt by household members. In 42 percent of these families, UI was essentially the only source of income. In addition to the private benefits associated with UI, the extensions benefit the economy as a whole. EUC and EB are a particularly well-targeted form of economic stimulus, since they go to people who are very likely to spend the money (Elmendorf 2010). They may also help keep workers off of disability insurance, a typically irreversible transition (Autor and Duggan 2006), and may help avoid mortgage foreclosures (Foote and others 2009). These benefits are important to keep in mind when weighing the costs and benefits of extending UI.

As of this writing (October 2011), labor demand is still severely depressed. There are almost 14 million unemployed, including almost 6 million long-term unemployed, in addition to the 1 million discouraged workers who have exited the labor force but will, one hopes, reenter at some point. Posted vacancy rates hover at low levels that imply more than four job seekers per job opening. Unemployed workers thus need more time to find jobs than they would during normal times; indeed, job finding rates are about half what they were in good times and have not recovered any ground. UI gives these workers much-needed support. Rothstein's paper contributes to a growing body of evidence that distortions to job search behavior caused by UI are much lower in recessions (see also Kroft and Notowidigdo 2011 and Schmieder, von Wachter, and Bender forthcoming). This evidence should weigh heavily in the policy debate on whether the UI extensions should be renewed in the course of 2012 and beyond.

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GENERAL DISCUSSION Jeffrey Kling recalled that previous work by Daron Acemoglu and Robert Shimer had suggested that unemployment insurance can facilitate mobility to new occupations by providing a safety net if job transitions do not work out—which may also lead to better matches between workers and jobs. He also reiterated Lisa Kahn's concern about worker mobility as a potential source of error in the analysis, since the CPS tracks households and not individuals, who may move between households. Jesse Rothstein replied that he had not examined data on wages upon reemployment that would provide evidence about the quality of matches; he was fairly confident that the mobility issue was not corrupting his results but acknowledged he needed to add more statistics to support that.

Robert Hall questioned Rothstein's use of reemployment hazard rates in his econometric framework, given the structure of errors in employment data in the CPS. Errors arise when respondents do not classify themselves in accordance with the technical definitions of "employed," "unemployed," and "not in the labor force." He noted a feature of the CPS that may exacerbate reporting error: not every person in the CPS is interviewed directly; rather, a single respondent is designated to respond for all members of the household. Hall suggested that instead of calculating hazard rates, which amounts to taking first differences of the data, Rothstein come up with an indirect inference approach more clearly based on the theoretical model of unemployment he presented in the paper, even though such an approach would represent a significant departure from previous literature.

Hall pointed out further that a large recent drop in job matching efficiency remained unexplained. He saw Rothstein's results as compelling evidence that unemployment insurance does not explain this drop, which leaves open the question of what does. Hall also asked whether Steven Davis could comment on the possible sources of recent deviations from the historical Beveridge curve, which is meant to measure changes in labor market efficiency.

Responding to Hall, Steven Davis cited two pieces of evidence about the sources of recent deviations from the historical Beveridge curve and the recent breakdown in the empirical relationships implied by a standard matching function. First, in work with John Haltiwanger and Jason Faberman, Davis found that the intensity of recruitment to fill vacant positions had declined sharply during the recent recession and remains low. Second, a recent Brookings Paper by Alan Krueger and Andreas Mueller found that job search intensity declines with the duration of an unemployment spell. In his comment on that paper, Davis showed that their evidence, combined with the recent increase in average spell durations at the aggregate level, implies a sizable drop in search intensity per unemployed person.

Rothstein suggested that the decline in matching efficiency was driven by the very low level of job openings. Matching theory posits a frictional rate of job vacancies, and Rothstein thought that for a period during the recession, the vacancy rate had fallen below the frictional level. For a while, therefore, the matching rate might have fallen even though people were searching harder for jobs, because there were so few job openings. This interpretation led Rothstein to doubt the need for new models to describe a breakdown in the job matching process. Such an exercise, he thought, would require too much extrapolation from previous circumstances to provide new insights.

Davis also hoped that someone would apply Rothstein's methodology to previous extensions of UI, to see whether UI had different effects in different macroeconomic environments. He thought that both the policy debate and academic research dwelled too much on the question of whether UI extensions are good or bad; lawmakers and researchers should instead spend more time evaluating the benefits and costs of UI extensions against alternative policy options, especially since UI extensions are expensive. Davis expressed disappointment at the federal government's willingness to legislate additional expenditure on UI without encouraging states to conduct randomized controlled experiments on alternative ways to help the unemployed, especially the long-term unemployed, get back to work. In his view, setting aside the macroeconomic effects, the main welfare benefit of UI comes from its income and consumption smoothing effects. Two alternative policies might achieve the same effects at lower cost: workers could make some type of prepayment, building up funds to be drawn on during spells of unemployment, or the government could offer low-cost loans to unemployed workers, to be repaid when they are once again employed.

Rothstein disagreed, on the grounds that a prepayment or loan plan would be no less expensive than UI but would simply be accounted for differently. He argued that, aside from whatever moral hazard they create, both UI and these alternative policies amount to transfer programs. People can differ on the merits of the transfer, but economic analysis can only lend insight into the costs of moral hazard and any other incentive distortions created by the policies.

George Akerlof seconded a point that Kahn had made about interpreting the welfare impact of UI. If one considers the labor market to be a rationed market, then a subsidy in the market has a positive impact on welfare. By enabling unemployed workers to search longer rather than take the first job offered, UI enables them to find a better match. He reminded the Panel that another benefit of UI in recessions is that it provides economic stimulus by putting money in the hands of people who will spend it. Rothstein replied that he had not spent much time investigating the impact of UI extensions on job match quality because earlier research had not found a relationship between the two.

Till von Wachter suggested that past empirical research on UI extensions had been unclear about whether it was measuring pure partial equilibrium effects or general equilibrium effects. Like these earlier papers, Rothstein's estimates actually represented a hybrid of partial and general equilibrium effects, because they exploited both state variation and time variation in UI availability over a period during which economic conditions were also changing. Von Wachter also revisited Rothstein's point that back-of-the-envelope extrapolations from prerecession estimates far overestimated the impact of UI extensions on unemployment. He noted that straightforward adjustments to these extrapolations, such as allowing for congestion in the labor market, imperfect take-up of UI benefits, or low job arrival rates, could produce estimates much closer to Rothstein's results.

Finally, von Wachter sought to clarify a point on the implications of a paper he had written with Johannes Schmieder and Stefan Bender on the varying effects of extended UI over the business cycle. Using 25 years of unemployment data from Germany and a clean identification strategy, they had found that rates of exhaustion of UI rose sharply during recessions, but that the effect of UI on the probability of regaining employment stayed constant over the business cycle. The upshot of these two facts, he argued, is that the moral hazard effect of UI falls during recessions, providing a clear rationale for extending UI during economic slumps.

Ricardo Reis pointed out what seemed to him a contradiction between Rothstein's results and previous research. An extensive literature from the 1990s had shown, using cross-country comparisons, that high levels and durations of UI benefits helped explain the high levels of unemployment in some European countries. Reis argued that, in theory, a rule promising extended UI benefits during a recession should have the same effect on unemployment as a higher level of UI benefits throughout the business cycle, but Rothstein's results suggested this was not the case.

John Quiggin thought it bizarre that the political debate focused so much on the moral hazard effects of UI benefit extensions. He saw the potential for much costlier moral hazard among people who had exhausted their UI benefits and sought disability insurance, since those who successfully apply for DI seldom return to the labor force and instead receive benefits for the rest of their lives. Rothstein said he planned to look more into the relationship between UI and DI using data on DI income from the CPS.

Betsey Stevenson noted that President Obama's proposed American Jobs Act called for spending $5 billion on experiments aimed at getting long-term unemployed workers back to work. She also reminded the Panel of how few people are actually eligible for UI: currently about a quarter of the unemployed received state-based UI, and another quarter received UI through the federal extensions, leaving half of the unemployed without benefits at all. She inferred that if UI reduces job search intensity among recipients, it should skew job-finding rates in favor of those who are ineligible.