

Unemployment Insurance and Job Search in the Great Recession

Jesse Rothstein*

University of California, Berkeley and NBER

October 16, 2011

Abstract

Nearly two 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 many as 99 weeks. This paper investigates the effect of these UI 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, concentrated among the long-term unemployed. The estimates imply that UI benefit extensions raised the unemployment rate in early 2011 by only about 0.1–0.5 percentage points, much less than is 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.

1 Introduction

While the so-called "Great Recession" officially ended in June 2009, the labor market remains stagnant. In September 2011, the unemployment rate remained above nine percent — it

*E-mail: rothstein@berkeley.edu. I thank Stephanie Aaronson, David Card, Hank Farber, Lisa Kahn, Anne Polivka, John Quigley, Gene Smolensky, Rob Valletta, Till von Wachter, the editors, participants at the Brookings Panel conference, and seminar participants at Berkeley, NBER, Santa Barbara, and Wharton for many helpful comments and suggestions. I gratefully acknowledge research support from the Institute for Research on Labor and Employment and the Center for Equitable Growth, both at UC Berkeley. Ana Rocca provided excellent research assistance.

I served in the Obama administration in 2009-10 and participated in internal discussions of the Unemployment Insurance extensions studied here, but all opinions expressed herein are my own.

has fallen below that threshold for only 2 of the last 29 months — and nearly 45% of the unemployed had been out of work for more than six 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 of "Extended Benefits" (EB) in states experiencing high unemployment rates. But Congress has frequently authorized additional weeks on an ad hoc basis in past recessions, and starting in June 2008 it enacted a series of UI extensions that 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 is not clear. Several recent analyses have found that extensions of UI benefits contributed around 1.0 percentage points to the unemployment rate in 2010 and early 2011 (see, e.g., Mazumder, 2011; Valetta and Kuang, 2010; Fujita, 2011), while some observers have claimed that the effects were several times that size (Grubb, 2011; Barro, 2010).

There are two channels by which UI can raise unemployment, with very different policy implications (Solon, 1979). On the one hand, UI extensions can lead recipients to reduce their search effort and raise their reservation wages, slowing the transition into employment. On the other hand, UI benefits — which are available only to those engaged in active job search — also provide an incentive for continued search for those who might otherwise have exited the labor force. The latter raises measured unemployment but has no effect — or possibly even a positive effect — on the reemployment of displaced workers. Based in part on this observation, Howell and Azizoglu (2011) find “no support” for the view that UI extensions have reduced employment. Unfortunately, most studies of the effect of UI on the duration of unemployment have been unable to distinguish the two channels.

Uncovering the causal effect of UI extensions on labor market outcomes is difficult because these extensions are badly 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 confound-

ing influence of historically weak labor demand.

This paper uses the haphazard roll-out of the EUC and EB programs during the Great Recession to identify the partial equilibrium effects of the recent UI extensions on the labor market outcomes of workers who have been displaced from their previous jobs and are actively seeking new ones. I use the longitudinal structure of the Current Population Survey to construct hazard rates for unemployment exit, reemployment, and labor force exit that vary across states, over time, and between individuals displaced at different dates.

I explore a variety of strategies for isolating the causal 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, as in Valetta and Kuang (2010), I use UI-ineligible job seekers as a control group for eligible unemployed workers in the same state-month labor markets. A third strategy exploits state decisions to take up or decline optional EB provisions that alter the availability of EB benefits, using 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 caused small reductions in the probability that unemployed workers 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 one and three percentage points on a base of 22.4%. Not more than half of the unemployment exit effect comes from effects on reemployment: My preferred specification indicates that UI extensions reduced the average monthly reemployment hazard of unemployed displaced workers in 2010:Q4 by 0.5 percentage points (on a base of 13.4%), and reduced the monthly labor force exit hazard by 1.0 percentage points (on a base of 9.0%).

The labor force exit effect raises the possibility that UI extensions might actually raise the employment rate of formerly displaced workers in bad economic times, by extending

the time until they abandon their search.¹ 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 2008-2010 UI extensions on aggregate unemployment and on the long-term unemployment share. All of the estimates are partial equilibrium, as I assume that reduced job search from one worker has no effect on the search behavior or job-finding rate of any other worker. 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 unemployment insurance extensions, the unemployment rate in December 2010 would have been about 0.2 percentage points lower and the long-term share of the unemployed 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 points 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 points.

The remainder of the paper is organized as follows. Section 2 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 discusses existing estimates of the effect of the recent extensions. Section 3 discusses the longitudinally-linked CPS data that I use to study the effects of UI. Section 4 presents my empirical strategies for isolating the UI effect. Section 5 presents estimates of the effect of UI benefit durations on the unemployment exit hazard. Section 6 develops a simulation methodology that I use to extrapolate these estimates to obtain effects on labor market aggregates, and presents results. Section 7 concludes.

¹In 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 may deter displaced workers from claiming disability payments (Duggan and Imberman, 2009; Joint Economic Committee, 2010).

2 The Labor Market and Unemployment Insurance in the Great Recession

2.1 Labor market trends

The recession officially began in December 2007, but the downturn was slow at first: Seasonally adjusted U.S. real GDP fell at an annual rate of only 0.7 percent in the first quarter of 2008. Conditions worsened sharply in late 2008 and GDP contracted at an annual rate of 6.8 percent in the fourth quarter.

The labor market downturn also began slowly. Figure 1 shows that the unemployment rate began trending up in 2007, but remained only 5.8% in July 2008. Over the next year, however, it rose 3.7 percentage points, to 9.5 percent, and has fallen below 9 percent in only two months since. Employment data show similar trends: Non-farm 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 year. 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 pre-recession peak.

Figure 1 also shows the long-term unemployment rate, defined as the share of the unemployed who have been out of work for six months or more. It generally lags the overall unemployment rate by about six months or perhaps a bit more: It began to increase slowly in early 2008 and much more quickly in late 2008, reaching a peak around 45% in early 2010 — nearly twenty percentage points higher than the previous record of 25.7%, recorded in June 1983 — and remaining mostly stable since then.

Figures 2A and 2B illustrate gross labor market flows over the course of the recession. These are obtained from two sources: The Job Openings and Labor Turnover Survey (JOLTS), which derives from employer reports, and the gross flows research series computed by the Bureau of Labor Statistics from matched monthly Current Population Survey (CPS) household data discussed at length below. Figure 2A shows flows out of work: Quits and layoffs from the JOLTS (“other separations,” including retirements, are not shown), and gross employment to unemployment (E-U) flows from the CPS. Figure 2B shows flows into

work: Hires from the JOLTS and unemployment to employment (U-E) flows from the CPS. It also shows unemployment to non-participation (U-N) flows, with both the U-E and U-N flows expressed as shares of the previous month's unemployed population.

Together, Figures 2A and 2B shed a good deal of light on the dynamics of the rise and stagnation of the unemployment rate.² Figure 2A 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 employment-to-unemployment 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 pre-recession 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 displaced workers into new jobs.

Turning to Figure 2B, we see 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 six months later, again suggesting that the usual process by which displaced workers are recycled into new jobs was substantially disrupted around the time of the financial crisis. U-E flows remain very low through the present day. Finally, the U-N flow rate fell rather than rose during the recession, despite weak labor demand which might plausibly have led unemployed workers to become discouraged. This is plausibly a consequence of Unemployment Insurance benefit extensions, which created incentives for ongoing search even if the prospect of finding a job was remote.

2.2 The policy response

Congress responded quickly to the deteriorating labor market, authorizing Emergency Unemployment Compensation (EUC) benefits in June 2008, but proceeded in fits and starts thereafter.³ The June 2008 legislation made 13 weeks of EUC benefits available to anyone

²See Elsbey et al. (2010) for a more detailed examination of these and other aggregate data.

³This discussion draws heavily on Fujita (2010). I neglect a number of details of the UI program rules. In particular, claimants whose previous jobs were short are not eligible for the full 26 weeks of regular benefits

who exhausted his or her regular benefits before March 28, 2009. The EUC program was subsequently expanded in November 2008. That expansion brought basic EUC benefits to 20 weeks, and also added a second “tier” of 13 weeks of benefits in states with unemployment rates above 6%. A second expansion in November 2009 changed Tier II benefits to 14 weeks and added Tiers III, 13 weeks of benefits when the unemployment rate was above 6%, and Tier IV, an additional 6 weeks when the unemployment rate was above 8%. Adding the four tiers together, individuals in high-unemployment states were eligible for 53 weeks of EUC benefits. Columns 1-4 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 expiration. Column 5 of Table 1 shows the scheduled expiration date of EUC benefits over time. For much of the program’s history, the expiration date was quite close. Indeed, on three occasions, in April, June, and November of 2010, Congress allowed the program to expire. Each time, Congress eventually reauthorized it retroactive to the expiration date, but in June this took seven weeks.

The EUC program complemented a preexisting program, Extended Benefits (EB), that allowed for 13 or 20 weeks of extra benefits in states with elevated unemployment rates. EB is an optional program, and participating states can choose among several options regarding the specific triggers that will activate EB 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 (February) 2009 provided for full Federal funding of EB benefits. This induced a number of states to begin participating in the program and to adopt more generous triggers.⁴

Figure 3 shows the number of states in which EB benefits have been available over time, along with simulated counts of the number of weeks that would have been available had every state adopted minimal or maximal triggers. At the beginning of 2009, only three states offered EB benefits, but by July of that year benefits were available in 35 states.

Figure 3 shows that this reflected a combination of increased EB participation — which

or for the indicated number of weeks of EUC benefits.

⁴The Recovery Act also provided for tax deductibility of a portion of UI benefits, for somewhat expanded eligibility, and for more generous weekly benefits.

brought the “actual” series well above the “minimal” series — and deteriorating economic conditions that would have expanded EB participation even with fixed triggers.⁵ The Figure also shows that participation plummeted each time the EUC program was allowed to expire: A number of states wrote their EB implementing legislation to provide for state participation only as long as the federal government paid 100% of the cost, and this provision expired and was reauthorized each time along with EUC. Other than these spikes, participation has been relatively stable over time.

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

Combining regular benefits (26 weeks), EUC (as many as 53 weeks) and EB (as many as 20 weeks), 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 her 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 workers’ expectations in the weeks prior to a scheduled

⁵During the period covered by my sample, “minimal” triggers provided EB benefits only when the 13-week moving average of the insured unemployment rate (IUR) was at least 5% and above 120% of the maximum of its values one year and two years prior. It is this lookback period that accounts for the decline in the minimal series in late 2009. “Maximal” triggers also provided benefits in states with 13-week IURs above 6% (regardless of their lagged values) or with three-month moving average total unemployment rates (TUR; the traditional measure) above 6.5% and 110% of the value either one or two years prior. 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 Federal-State Extended Unemployment Compensation Act of 1970 (Undated).

EUC expiration. They 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 evolution of UI durations. The left 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 benefits and all extant EUC tiers. The right panel shows the (unweighted) average across states. In each panel, the short dashes show the maximum number of weeks available by statute over time, while the long dashes and the solid line show the expectations of a worker just entering unemployment and of a worker who has just exhausted her regular benefits, respectively, 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 show much more gradual changes from the perspective of individuals early in their allowed benefits. Newly displaced workers who did not expect further legislative action would have seen the EUC program as largely irrelevant for most of its existence, as only on three occasions (roughly, the 3rd quarter of 2008, the 2nd quarter of 2009, and the period since December 2010) was the expiration of the EUC program farther away than the 26 weeks it would take for a newly displaced worker to exhaust his regular benefits. Workers already 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 look forward to Tier II, III, or IV benefits for most of the history of the program. It is only in December 2010 and the very beginning of 2011 that any such worker could anticipate 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.

2.3 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 unemployment insurance. The model yields two main results: First, search intensity rises as UI benefit expiration approaches, and 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.⁶ 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 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 $p(s)$ 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 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 correspond-

⁶Chetty (2008) finds that much of the search effect of unemployment insurance is concentrated among those who are credit constrained, and also that lump-sum severance pay has a similar effect to UI benefit extensions (see also Card et al., 2007a).

ing to the number of weeks of benefits remaining. Letting $V_U(d)$ represent the value function of an unemployed individual with $d > 0$ weeks of benefits remaining, the Bellman equation is

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

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.⁷

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

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

for $d \geq 1$. The following results are proved in an 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 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 unemployment insurance 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 $p(\cdot)$ 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 exhaustion.⁸

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 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:

⁷Once benefits are exhausted ($d = 0$), the problem becomes stationary: $V_U(0) = \max_{s_0} u(y_0) - s_0 + \delta [p(s_0) V_E + (1 - p(s_0)) V_U(0)]$.

⁸For example, this holds under the parameters considered by Chetty (2008, p. 8), which in my notation correspond to 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 $V_E = 500u(w)$.

$$\tilde{V}_U(d) = \max_{s_d} \begin{cases} u(b) - s + \delta [p(s) V_E + (1 - p(s)) \tilde{V}_U(d - 1)] & \text{if } s \geq \theta \\ u(0) - s + \delta [p(s) V_E + (1 - p(s)) \tilde{V}_U(d)] & \text{if } s < \theta. \end{cases} \quad (2)$$

Unemployment benefits may deter an unemployed individual from exiting the labor force if search productivity is low (i.e., if $p'(\theta) < \frac{1}{\delta(V_E - \tilde{V}_U(d-1))}$) 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 re-enter the labor force. 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 search again. The only temporally consistent policies 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 non-participation by delaying the exit of those who plan to exit when d reaches 0. 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 negative effect is likely to dominate, but when search productivity is low the former may be more important.

Finally, it is worth mentioning two important factors that are not captured by this model. First, $p(s)$ may evolve 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 is 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. Reduced search effort from 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 is particularly important if the labor market is demand constrained, but arises anytime 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.

2.4 Prior estimates of the effect of UI extensions in the Great Recession

There have been a number of estimates of the effect of the recent UI extensions on labor market outcomes. Nearly all involve extrapolations from pre-recession estimates of the effect of UI durations or from pre-recession unemployment exit rates.

Mazumder (2011) uses estimates of the effect of UI durations from Katz and Meyer (1990a) and Card and Levine (2000) to conclude that UI extensions contributed 0.8 to 1.2 percentage points to the unemployment rate in February 2011.⁹ But UI durations in the current recession are longer and labor market conditions are different in a variety of ways than in the periods used for 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 prior to benefit exhaustion. Katz and Meyer (1990b) find that much of this spike is attributable to laid off workers recalled to their previous job; these recalls are thought to have become much less common in recent years. Card et al. (2007a,b) suggest that much of the remaining spike is attributable to labor force exit rather than reemployment, highlighting the importance of distinguishing these two channels.¹⁰

Fujita (2011) extrapolates from reemployment and labor force exit hazards observed in 2004-2007 to infer counterfactual hazards in 2009-2010 had UI benefits not been extended.

To absorb confounding effects from changes in labor demand, he controls linearly for the job

⁹Aaronson et al. (2010), Fujita (2010), and Elsbey et al. (2010) use similar strategies and obtain similar results.

¹⁰Another potential explanation for large spikes in at least some of the earlier studies is heaping in reported unemployment durations. Katz (1986) and Sider (1985) suggest that in retrospective reports much of the observed heaping — especially prominent at 26 weeks (or 6 months), the duration of regular UI benefits — reflects recall error or other factors (Card and Levine, 2000) rather than UI effects.

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.

Daly et al. (2011), drawing on Valetta and Kuang (2010), contrast changes in the unemployment durations of job-losers, many of whom are eligible for UI benefits, and job-leavers, who are not, over the course of the recession. They conclude that UI extensions raised the unemployment rate by 0.8 percentage points 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 2A 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 job losers come from those hit hard by the recession (e.g., construction), the comparison between them will overstate any negative effect of UI extensions.

A larger estimate comes from Barro (2010), who assumes that the long-term unemployment rate in 2009 would have been the same as in 1983 if not for the UI extension. Barro concludes that extensions raised the unemployment rate by 2.7 percentage points. Grubb's (2011) literature review comes to quite similar conclusion, while Howell and Azizoglu (2011) conclude that any 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 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 on reemployment and labor force exit hazards. Unsurprisingly, Farber and Valletta obtain very similar results to those presented below. Relative to Farber and Valletta, I (a) explore several alternative specifications that isolate different components of the variation in UI benefits; (b) explore the sensitivity of the results to unavoidable ad hoc assumptions made about expected benefit availability; and (c) address an important discrepancy in the CPS data, discussed below, that leads to drastic understatement of the long-term unemployment rate and that has the

potential to substantially obscure effects of UI extensions on unemployment durations.

3 Data

I use the Current Population Survey (CPS) 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 is targeted for another interview the following month, and it is possible to match over 70% of monthly respondents (94% of the attempted reinterviews) to employment statuses in the following month. (The most important source of mismatches is individuals who move, who are not followed.) This permits me to measure one-month-later employment outcomes for roughly 4,000 unemployed workers each month during 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 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 displaced workers — those who were laid off from their previous jobs rather than having quit or being new entrants to the labor force — are eligible for UI benefits. Past research has found that less than half of the eligible unemployed actually receive UI benefits (Anderson and Meyer, 1997). This appears to have risen somewhat in the current recession; I estimate that over half of displaced workers unemployed more than three months in early 2010 received UI benefits.¹¹ Although the participation rate is far less than 100%, I simulate remaining benefit durations for all displaced workers, 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 displaced workers,

¹¹Observations 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% of job-leavers whose unemployment spells appear to have started before December 1, 2009 reported non-zero UI income, up from 39% in early 2005.

pooling recipients and non-recipients. To implement the simulation, I match the CPS data to detailed information about the availability of EUC and EB benefits at a state-week level and compute eligibility for benefits in each week between the time of displacement and the initial CPS interview (including those paid retroactively due to delayed reauthorizations). I assume that one 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 we would expect this nonclassical measurement error to bias 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, someone who is out of work, is available to start work, and has actively looked for work at least once in the last four weeks should be classified as “unemployed,” with a duration of unemployment reaching back to the last time he/she was not in this state. 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 non-participation can be blurry, particularly when there are few suitable job openings to which to apply or when job search is intermittent. The data suggest that reported unemployment durations often stretch across periods of non-participation 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).¹² Relatedly, examination of the unemployment duration distributions indicates substantial heaping at

¹²CPS 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.

monthly, semi-annual, and annual frequencies, suggesting that many respondents round their 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 three-month trajectory is U-N-U or U-E-U — as a non-exit.¹³ This means that I can only measure unemployment exits for observations with at least two subsequent interviews. I have also estimated alternative specifications that count all measured exits or that exclude many of the “heaped” observations, with similar results.¹⁴ I discuss these issues at greater length in Section 6.

Finally, the CPS does not attempt to track respondents who change residences between interviews. Mobility and nonresponse lead to the attrition of roughly 8% of the sample — and 10% 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 sign 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 to subsequent interviews in each of the next two 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 displaced from their previous jobs (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% in mid 2007 to about 25% throughout 2009 and 2010.¹⁵ The Figure also reports exit hazards for those unemployed 0-13 weeks and 26 weeks or more. The hazard is higher for the short-term than for the long-term unemployed.

¹³Fujita (2011) also recodes some U-N-U trajectories as U-U-U. I am grateful to Hank Farber for helpful conversations about this issue.

¹⁴I 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 non-participants to misreport themselves as active searchers. This may lead my estimates of the effect of UI on reported labor force participation to overstate the effect on actual job search.

¹⁵This 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 trajectories.

However, both series fell similarly to the overall average in 2007 and 2008, indicating that only a small portion of the overall exit hazard decline can be attributable to composition effects arising from the increased share of long-term unemployed with low exit rates.

4 Empirical Strategy

The matched CPS data allow me to measure whether an unemployed individual exits unemployment over the next month, but 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 displaced workers, I estimate specifications of the form:

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

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; 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.¹⁶ However, as I discuss below, modeling survival functions in the CPS data is challenging due to inconsistencies between stock-based and flow-based measures of survival. In Section 6, I develop a simulation approach to recovering survival curves from

¹⁶In principle, individuals can be followed for three periods in the CPS data. (Although the CPS is a 4-period rotating sample, I cannot measure exit between period 3 and period 4 because I require a follow-up observation to measure 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.

the estimated exit hazards that are consistent with the observed duration profile. For now, I focus on modeling the hazards themselves.

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

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

This appears flexible enough to capture most of the duration pattern. I have also estimated versions of (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 a source of 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 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 roll-out 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 state decisions about whether to participate in the optional EB program. Note that labor demand is likely 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 non-parametrically for state labor market conditions (Valetta and Kuang, 2010; Farber and

Valletta, 2011). Using a sample that pools all of the unemployed, I estimate:

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

where α_{st} is a full set of state-month indicators and e_{ist} is an indicator for whether individual i is a job loser (and therefore presumptively UI-eligible). $P_n(n_{ist}, e_{ist}; \gamma)$ represents the full interaction of the unemployment duration controls (4) with the eligibility indicator, while $e_{ist}P_Z(Z_{st}; \delta)$ 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 that will absorb any correlation between cohort employability and benefits and interacted with the eligibility indicator e_{ist} . The causal effect of UI duration is θ , and identified from covariance between UI extensions and changes in the *relative* unemployment exit rates of job losers and other unemployed who entered unemployment at the same time, over and above that which can be explained via 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 2A indicated that the quit rate has been low throughout the recession. 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 specification (5) to overstate the causal effect of UI benefits. I attempt to minimize this by adding controls for individual covariates — age, education, gender, marital status, and former occupation and industry — to (5).

My third strategy returns to the eligible-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 (3) with a direct control for the number of EUC weeks available. This leaves variation only in EB 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 benefits in the s - t cell under maximal and minimal state participation in EB (as graphed in Figure 3), along with indicators for the status of each of the four EB triggers.¹⁷ With these controls, 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 2.3 suggests are likely to be strongest for those facing imminent exhaustion that 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:

$$\ln\left(\frac{\lambda_{ist}}{1-\lambda_{ist}}\right) = f(d_{ist}; \theta) + \sum_{v=0}^{99} 1(n_{ist} = v) \gamma_v + \alpha_{st}. \quad (6)$$

Here, $d_{ist} = \max\{0, D_{ist} - n_{ist}\}$ represents the number of weeks of benefits remaining, with $f(\cdot; \theta)$ a flexible function; I impose only the normalization that $f(0; \theta) = 0$, implying that UI extensions have no effect on job searchers who have already exhausted even their extended benefits. The second term in (6) is a full set of indicators for unemployment duration, and the third is a full set of state-by-month indicators. There are two sources of variation that allow separate identification of the effects of d and n , within state-by-month cells, without parametric restrictions. The first is the nonlinearity of the mapping from D_{ist} and n_{ist} to d_{ist} : across- (s, t) 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

¹⁷Three of the triggers are described in note 5. The fourth is activated when the 3-month moving average TUR exceeds 8% and is above 110% of the minimum of its one-year and two-year lagged values. States adopting optional trigger 3 are required to also adopt 4, which when activated provides an additional 7 weeks of EB benefits on top of the normal 13.

transition onto the new tiers before the EUC program expires but not for those with lower n_{ist} who expect the program to have expired before they reach the new tiers.

5 Estimates

Panel A 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 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.¹⁸ Column 1 is estimated using only displaced workers who are presumed to be eligible for UI benefits, and includes state and month fixed effects and the n_{ist} controls indicated by (4), but no controls for economic conditions in the state. 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 2010:Q4 exit rate of -2.3 percentage points (on a base of 22.4%). Columns 2–5 add additional controls: First a control for the state unemployment rate, then a cubic in that rate, then 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 then finally a vector of individual-level covariates, including education, age, marital status, and indicators for previous industry. The estimated UI effects move around a bit as the covariate vector is expanded, but within a fairly narrow range: The implied effects of UI expansions on the exit hazard in 2010:Q4 range from -1.7 to -2.3 percentage points.

Columns 6 and 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-by-month fixed effects.¹⁹ I also include an indicator for (simulated) UI eligibility and

¹⁸Strictly, I use observations from the September–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 2.2.

¹⁹For computational reasons, I estimate the specification by conditional logit, then back out consistent

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 7 also adds the full vector of individual covariates, as a guard against the possibility that there are important differences in employability between the job-losers and the job-leavers comparison group. With or without these covariates, the estimates indicate notably smaller effects than in columns 1-5.

There is no particular reason to think that benefit extensions have the same effects on those near exhaustion as on those just beginning their spells. As a first step toward loosening this assumption, in Panel B I allow D_{ist} to have distinct effects on those unemployed more and less than 26 weeks. The negative effect of D on unemployment exit is found to be entirely concentrated among those unemployed 26 weeks or more, 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 Panel A, though the differences are small. The implied effects of UI extensions on exit hazards are smaller than those in Panel A in columns 1–5, but larger in columns 6 and 7, 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 1 repeats the baseline specification from Table 3, Panel B, Column 3. Column 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.²⁰ This leads to larger estimated UI 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 3 presents a specification that builds on this intuition. I measure

but inefficient estimates of the α_{st} fixed effects for use in predicted exit probabilities.

²⁰This is the measure used by Farber and Valletta (2011).

the absolute difference between the D s calculated under the two expectations models, and interact this difference with the simulated benefit duration (using my “myopic” expectations model). I interpret the D main effect in this specification — the effect of durations when the two expectation models are in agreement — as indicating the causal effect of D , 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 1 and 2; the interaction coefficients are negative for both the short- and long-term unemployed, but are imprecisely estimated.

Column 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 EB benefits, which are not directly dependent upon EUC reauthorization. The estimated UI effects are somewhat larger than in my baseline specification but in the same general range.

Finally, Column 5 turns to my third strategy for identifying the UI effect, using a control function to isolate variation in EB benefits coming from state decisions about which version of the EB triggers to use.²¹ I add to the Column 4 specification controls for the status of each of the four EB triggers and for simulated EB benefits under the most and least generous versions of the triggers. This inflates the coefficients, which 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 non-participation in the labor force. For the long-term unemployed, the results indicate that benefit durations have negative, significant effects of roughly similar magnitude on the logit indexes for both types of unemployment exit. For the short-term unemployed, estimates indicate positive effects on reemployment and negative effects on labor force exit,

²¹Identification in this specification comes from variation in state take-up of a program that was for much of the period under study entirely funded by the federal government. Insofar states that turned down this free money — an important consideration seems to be the presence of a governor who was vocally opposed to federal economic stimulus in 2009 — experienced sharper downturns in labor market conditions (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.

both insignificant in most specifications. The bottom rows show the effects of UI extensions on average exit hazards in 2010:Q4. 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 may be incorrect here, particularly if (as in the model in Section 2.3) 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 last 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 to exit the labor force (and vice versa). Recall from Table 3, however, that controlling for unobservables has little effect on the estimated UI effects. The same is true in the multinomial specifications (compare Column 3 of Table 5, which includes the individual covariates, with Column 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 low-job-finding-probability individuals from labor force exit, 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 2010:Q4 exit hazards. The first

row repeats the results from Table 5, column 2. Row 2 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 in the raw data (due, presumably, to rounding of durations reported in months). Although point estimates (not shown) show that effects are largest for those with exactly 26 weeks, this group is not large enough to change the overall average exit hazards.

Row 3 offers another approach to investigating the impact of duration heaping: I exclude from my sample anyone who reported a duration of exactly 26, 52, or 78 weeks when first asked about his unemployment spell (in his first month in the CPS sample), as well as anyone who reports an inconsistent duration 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. Row 4 excludes individuals who have been 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.

Row 5 explores the sensitivity of the result to the definition of unemployment “exit.” Where my main specifications count only exits that don’t 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 counts all exits. This allows me to expand the sample by over 50%, as I only require one follow-up interview to measure exit. It 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 on different subsamples. Rows 6 and 7 show that the effect of UI extensions is concentrated among prime-age workers; for workers over 55, extensions appear to *raise* the unemployment exit probability (though only the effect on reemployment is statistically significant). Rows 8 and 9 show effects by gender; there is no clear pattern here. Rows 10 and 11 show that the labor force exit effect is con-

²²That is, 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.

concentrated among non-college workers, though reemployment effects are similar for more- and less-educated workers. Finally, rows 12 and 13 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, while reemployment effects derive from workers displaced from other sectors.

Next, I turn to my fourth strategy, as described in equation (6), allowing the effects of UI durations to operate through the time to exhaustion. As in the baseline specifications earlier, I control for state and month indicators and a cubic in the state unemployment rate. I also include an extremely flexible parameterization of the unemployment duration²³. As discussed in Section 4, the time-until-exhaustion effects are identified due to 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 to 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 d coefficients from this specification are illustrated as the solid lines in Figure 6.²⁴ The reemployment coefficients, in the left panel, show a clear pattern of negative coefficients that are perhaps falling as d_{ist} falls toward about 10, then rise toward zero as d_{ist} falls further. This is consistent with the general pattern one would expect from reasonably parameterized search models (see Section 2.3), with depressed search effort from those with many weeks left and increasing effort as benefit exhaustion approaches that reaches a maximum value at the time of exhaustion, with constant search effort thereafter.²⁵ The labor force exit coefficients, in the right panel, show a roughly similar pattern: Negative and fairly stable for

²³The duration density gets thin at above one 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 and monthly indicators thereafter, plus separate linear weekly duration controls within each of 8 bins (26-30 weeks, 31-40, 41-50, 51-60, 61-70, 71-80, 81-90, and 91-99).

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

²⁵The 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 (i.e., at $d = 0$ or $d = 1$; see, e.g., 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.

large d_{ist} values, rising as d_{ist} falls from 10 toward 0. This time, however, the coefficients are generally positive for the lowest d_{ist} values, indicating that those very near 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 exits among at least a subset of UI claimants.²⁶

Based on the pattern of coefficients in Figure 6, I next turn to a semi-parametric 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 (i.e. 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 greater than 10 — the main d term and the additional term for $d > 10$ cancel out — and sharply increasing as d falls from 10 toward 0. There is no significant difference in exit rates between those in their last weeks of benefits and those who have already exhausted, holding constant the length of the spell. The rightmost column of Table 7 shows that the implied effect of UI benefits on the UI exit rate is somewhat smaller than those implied by the earlier estimates.

The second row of Table 7 shows a specification that includes a full set of state-by-month indicators. This yields very similar results to those in the less restrictive specification. In row 3, I return to the control variables from row 1, but use a multinomial logit that distinguishes alternative types of exit from unemployment. (Coefficients from this specification are plotted as dashed lines in Figure 6.) As before, we see substantial effects of UI benefits on both margins, but the impact on unemployment exit hazards is smaller than in the earlier analyses.

²⁶In the model, exits occur either immediately upon job loss or upon exhaustion. Thus, the model does not perfectly fit the data, which show positive rates of labor force exit even for non-exhaustees. 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.

6 Simulations of the Effect of Unemployment Insurance Extensions

The results in Tables 3 – 7 indicate that the UI benefit extensions enacted in 2008-2010 reduced both the probability that a UI recipient found a job and the probability that he exited the labor force, with somewhat larger estimated impacts on the latter than 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 benefit extensions.

6.1 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 solid 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 observations in month $m + n$ with duration n to the number of unemployed observations in m with duration 0.²⁷ To smooth the estimated rate, I pool both numerator and denominator across all entrance months in calendar year 2008.

The dotted and dashed lines are Kaplan-Meier survival curves based on unemployment exit hazards estimated from the linked CPS sample described in Section 3. 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 dotted line uses two-month panels to estimate p , counting as survivors only those who

²⁷In practice, the unemployment duration measure is in weeks, where the CPS sample is monthly. For Figure 7, I compute the duration in months as $\text{floor}(n/4.3)$, where n is the duration in weeks and 4.3 is the average number of weeks in a month. Note also 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.

report being unemployed in the second month (that is, only U-U transitions). The dashed line uses my preferred survival measure, using a three-month panel to measure persistence of exits and only counting exits between month 1 and month 2 where 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 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 year 2008.

Both of the Kaplan-Meier survival curves are substantially below the curve computed from repeated cross-section data. The most important contributor to this discrepancy is the phenomenon highlighted in Section 3: 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 $t+2$. While 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, 1986, 1984). The alternative Kaplan-Meier survival curve based on the three-month panel substantially reduces the discrepancy with the repeated cross section data.

Extensive exploration of the CPS data points to two other factors contributing to the remaining discrepancy. The first is so-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, e.g., Bailer, 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 super-spell. However, this does not seem to be a complete explanation. In 2006 and 2007, for example, there are nearly 2,400 respondents observed to be employed for three consecutive months and then unemployed in the fourth month; 10% of these report unemployment durations in the fourth month of longer than 6 weeks.

6.2 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 Bureau of Labor Statistics uses in constructing the gross flows data (Frazis et al., 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 adjustments; I argue below that one approach is likely to lead me to somewhat overstate the effect of UI extensions while 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 by month $m + 1$; and let $p^c(m, n, s)$ be the counterfactual persistence probability that would be observed in the absence of unemployment insurance extensions. Both p and p^c are obtained from fitted values from the exit regressions presented in Section 5.

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

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

In population data without measurement error, the residual $e(m, n, s)$ would be identically zero. The actual residual in (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 .

We wish to compare $u(m, n, s)$ to 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,

²⁸The 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.

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 approaches differ in their assumptions about the counterfactual values of $e(m, n, s)$.

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

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

where $E(m, n, s) \equiv \sum_{r=1}^n [e(m - n + r, r, s) \prod_{t=r}^n p(m - n + t, t, s)]$. (Hereafter, I suppress the month and state subscripts, understanding that increments to duration require corresponding increments to the month of observation in order to maintain a focus on the same entry cohort.) In this approach, 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 solid and long-dashed lines in Figure 7, evaluated at duration n . I use (8) to construct a counterfactual unemployment count

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

My second approach assumes 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 (7), I estimate $\hat{e}(d) = u(n) - u(n-1)p(n-1)$, then define the counterfactual count iteratively as:

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

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

$$\begin{aligned} \hat{u}^{c2}(n) \equiv & u(n) + u(n-1)[p^c(n-1) - p(n-1)] \\ & + [\hat{u}^{c2}(n-1) - u(n-1)]p^c(n-1) \end{aligned} \quad (11)$$

The first adjustment — the second term on the right side of (11) — reflects differences between the actual and counterfactual scenarios in unemployment persistence at duration $n - 1$, while the second adjustment — the third term in (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 entrants are in part an artifact of measurement error in the pre-unemployment 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 magnitude of the effect of UI extensions.

On the other hand, insofar as the late entries reflect people cycling from unemployment to non-participation and back, UI extensions that reduce the flow from unemployment into non-participation 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 magnitude of 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 effects of UI extensions on exit hazards obtained from the specifications in Section 5 are accurate).²⁹

6.3 Results

Figure 8 presents the two counterfactual simulations of the number of unemployed, using the model from Table 5, Column 2 to construct p and p^c and aggregating across all durations at each point in time. The solid line shows the actual, non-seasonally-adjusted counts from the monthly CPS. The two counterfactual simulations \hat{u}^{c1} and \hat{u}^{c2} are plotted as short and long dashes, respectively. Counterfactual approach 1 indicates essentially no effect of the UI extensions, making the short-dashed line hard to distinguish from the solid “actual” series.

²⁹State-by-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.

Counterfactual approach 2 offers only a slightly different conclusion, suggesting that the UI extensions increased unemployment in 2010 and early 2011 by about 2.6%.

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 to job-leavers reported in Table 3, Column 6 to generate the exit hazards. Third, I use the control function specification from Table 5, Column 5 — identified from state decisions about whether and how to participate in the EB program. Finally, I use the time-to-exhaustion model from Table 7, Row 3.

The estimates indicate that UI extensions raised the number of unemployed in January 2011 by between 5,000 and 759,000, the unemployment rate by 0.1 to 0.5 percentage points, 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; leaving these out, the upper end of the ranges are 370,000 unemployed, 0.2 percentage points 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 2.4.

The lower panel of Table 8 presents an alternative and more speculative set of counterfactual simulations. An important question regarding the effects in Panel B of Table 8 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 re-run the simulations, turning off the effects of UI 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 β_e and β_n be the full vectors of covariates from the employment and non-participation equations, respectively, of the multinomial logit model.

³⁰I 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, where 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%.

The one-period survival probability is then $p_{ist} = [1 + \exp(X_{ist}\beta_e) + \exp(X_{ist}\beta_n)]^{-1}$ and the counterfactual survival probability used for the simulations in Panel B of Table 8 is $p_{ist}^c = [1 + \exp(X_{ist}^c\beta_e) + \exp(X_{ist}^c\beta_n)]^{-1}$, where X_{ist}^c represents the explanatory variables in the counterfactual scenario where benefits are fixed at 26 weeks. In Panel C, I use instead $p_{ist}^{c'} = [1 + \exp(X_{ist}\beta_e) + \exp(X_{ist}^c\beta_n)]^{-1}$. Comparisons of simulations based on p_{ist} and $p_{ist}^{c'}$ reveal how much of the overall effect revealed by the p_{ist} - p_{ist}^c comparison is due to labor force exit. The results in Panel C 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 Panel B.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% chance of becoming reemployed the next month, the same as would an individual who never considered abandoning his 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 Table 8, Panel C might overstate the share of the UI effects that is attributable to labor force exit decisions. Even so, it is clear from Panel B alone that any negative reemployment effect must be small.

7 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 non-work 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

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

reduced search effort among the unemployed may be relatively small.³² It thus stands to reason that one might want to extend unemployment insurance benefit durations during bad times (Landais et al., 2010; Kroft and Notowidigdo, 2011; Schmieder et al., 2011). 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, some pointing to the apparent outward shift of the Beveridge Curve in 2010 (Elsby et al., 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 of extensions on job finding rates is hard to identify, because extensions are usually implemented in response to poor labor market conditions. Fortunately for the researcher (if not for UI recipients themselves), the haphazard way that the EUC program was gradually expanded and then repeatedly renewed 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 benefits do reduce the rate at which unemployed workers reenter employment. But the reductions are small, in most specifications smaller than effects of extended benefits 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 points, implying that the vast majority of the 2007–2009 increase in the unemployment rate was due to demand shocks rather than to UI-induced supply reductions.

³²See, e.g., Kroft and Notowidigdo (2011). Schmieder et al. (2011) find evidence in Germany, however, that the reemployment effect of UI durations is relatively constant across the business cycle.

Moreover, less than half of the small UI effect comes from reduced reemployment rather than from reduced non-participation (i.e., from *increased* labor supply).

Any negative effects of the recent unemployment insurance extensions on job search are clearly quite small, too small to outweigh the consumption-smoothing and equity-promoting benefits of UI (Gruber, 1997). The latter are likely to be particularly large when the marginal recipients have 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 would rather simply shift jobs from the UI recipients to other job seekers (Landais et al., 2010). The evidence here thus supports the view that optimal UI program design would tie benefit durations to labor market conditions, to give displaced workers realistic chances of finding new employment before their benefits expire.

References

- Aaronson, Daniel, Bhashkar Mazumder, and Shani Schechter**, “What is behind the rise in long-term unemployment?,” *Economic Perspectives (Federal Reserve Bank of Chicago)*, 2nd Quarter 2010, 34 (2).
- Abowd, John M. and Arnold Zellner**, “Estimating gross labor-force flows,” *Journal of Business & Economic Statistics*, 1985, 3 (3), 254–283.
- Anderson, Patricia M. and Bruce D. Meyer**, “Unemployment Insurance takeup rates and the after-tax value of benefits,” *Quarterly Journal of Economics*, 1997, 112 (3), 913–937.
- Bailar, Barbara A.**, “The effects of rotation group bias on estimates from panel surveys,” *Journal of the American Statistical Association*, 1975, 70 (349), 23–30.
- Barro, Robert**, “The folly of subsidizing unemployment,” *Wall Street Journal*, August 30 2010, August 30.

³³In 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 of the externality is captured.

- Card, David and Philip B. Levine**, “Extended benefits and the duration of UI spells: Evidence from the New Jersey extended benefit program,” *Journal of Public Economics*, 2000, 78 (1-2), 107–138.
- , **Raj Chetty, and Andrea Weber**, “Cash-on-hand and competing models of intertemporal behavior: New evidence from the labor market,” *Quarterly Journal of Economics*, 2007, 122 (4), 1511–1560.
- , —, and —, “The spike at benefit exhaustion: Leaving the unemployment system or starting a new job?,” *The American Economic Review*, 2007, 97 (2), 113–118.
- Chetty, Raj**, “Moral hazard vs. liquidity and optimal unemployment insurance,” *Journal of Political Economy*, 2008, 116 (2), 173–234.
- Congressional Budget Office**, “Policies for increasing economic growth and employment in 2010 and 2011,” Publication 4077 January 2010.
- Daly, Mary, Bart Hobijn, and Rob Valetta**, “The Recent Evolution of the Natural Rate of Unemployment,” Working paper 2011-05, Federal Reserve Bank of San Francisco January 2011.
- Duggan, Mark and Scott Imberman**, “Why are the DI rolls skyrocketing? The contribution of population characteristics, program changes, and economic conditions,” in David Cutler and David Wise, eds., *Health at Older Ages*, Chicago, IL: University of Chicago Press, 2009.
- Elsby, Michael W, Bart Hobijn, and Aysegul Sahin**, “The labor market in the Great Recession,” *Brookings Papers on Economic Activity*, 2010, *Spring*.
- Farber, Henry S. and Robert Valletta**, “Extended unemployment insurance and unemployment duration in the Great Recession: The U.S. experience,” manuscript June 24 2011.
- Federal-State Extended Unemployment Compensation Act of 1970**, http://workforcesecurity.doleta.gov/unemploy/EB_law_for_web.pdf. Accessed June 28, 2011. Undated.
- Frazis, Harley J., Edwin L. Robison, Thomas D. Evans, and Martha A. Duff**, “Estimating gross flows consistent with stocks in the CPS,” *Monthly Labor Review*, September 2005, 128 (9), 3–9.
- Frey, William H.**, “The great American migration slowdown: Regional and metropolitan dimensions,” Technical Report, The Brookings Institution December 2009.
- Fujita, Shigeru**, “Economic effects of the unemployment insurance benefit,” *Business Review (Federal Reserve Bank of Philadelphia)*, Fourth Quarter 2010.
- , “Effects of extended unemployment insurance benefits: Evidence from the monthly CPS,” Working paper 10-35/R, Federal Reserve Bank of Philadelphia January 2011.
- Grubb, David**, “Assessing the impact of recent unemployment insurance extensions in the United States,” Working paper, OECD May 25 2011.

- Gruber, Jonathan**, “The consumption smoothing benefits of unemployment insurance,” *American Economic Review*, March 1997, 87 (1), 192–205.
- Howell, David R. and Bert M. Azizoglu**, “Unemployment benefits and work incentives: The U.S. labor market in the Great Recession,” Working Paper 257, Political Economy Research Institute, University of Massachusetts Amherst May 2011.
- Joint Economic Committee**, “Extending unemployment insurance benefits: The cost of inaction for disabled workers,” Report May 2010.
- Kaplan, Greg and Sam Schulhofer-Wohl**, “Interstate migration has fallen less than you think: Consequences of hot deck imputation in the Current Population Survey,” Staff Report 458, Federal Reserve Bank of Minneapolis June 2011.
- Katz, Lawrence**, “Layoffs, recall, and the duration of unemployment,” Working paper 1825, National Bureau of Economic Research January 1986.
- Katz, Lawrence F. and Bruce D. Meyer**, “The impact of the potential duration of unemployment benefits on the duration of unemployment,” *Journal of Public Economics*, 1990, 41 (1), 45–72.
- and —, “Unemployment Insurance, recall expectations, and unemployment outcomes,” *The Quarterly Journal of Economics*, November 1990, 105 (4), 973–1002.
- Kroft, Kory and Matthew J. Notowidigdo**, “Should unemployment insurance vary with the unemployment rate? Theory and evidence,” working paper 17173, National Bureau of Economic Research June 2011.
- Landais, Camille, Pascal Michailat, and Emmanuel Saez**, “Optimal unemployment insurance over the business cycle,” working paper 16526, National Bureau of Economic Research November 2010.
- Mazumder, Bhashkar**, “How did unemployment insurance extensions affect the unemployment rate in 2008-10?,” *Chicago Fed Letter*, April 2011, 285.
- National Employment Law Project**, “Q&A: The basics of the Extended Benefits program,” Fact Sheet February 3 2011.
- Poterba, James M. and Lawrence H. Summers**, “Response variation in the CPS: Caveats for the unemployment analyst,” *Monthly Labor Review*, 1984, 107 (3), 37–43.
- and —, “Reporting errors and labor market dynamics,” *Econometrica*, November 1986, 54 (6), 1319–1338.
- and —, “Unemployment benefits and labor market transitions: A multinomial logit model with errors in classification,” *The Review of Economics and Statistics*, May 1995, 77 (2), 207–216.
- Schmieder, Johannes F., Till von Wachter, and Stefan Bender**, “The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over twenty years,” Working paper March 2011.
- Shockey, James W.**, “Adjusting for response error in panel surveys,” *Sociological Methods & Research*, 1988, 17 (1), 65.

Sider, Hal, “Unemployment duration and incidence: 1968-82,” *The American Economic Review*, June 1985, 75 (3), 461–472.

Solon, Gary, “Labor Supply Effects of Extended Unemployment Benefits,” *The Journal of Human Resources*, Spring 1979, 14 (2), 247–255.

—, “Effets of rotation group bias on estimation of unemployment,” *Journal of Business and Economic Statistics*, January 1986, 4, 105–109.

Valetta, Rob and Katherine Kuang, “Extended unemployment and UI benefits,” *FRBSF Economic Letter*, April 2010, 2010 (12).

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

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

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 when $d = 1$ as when $d = 0$. (Compare equation (1), evaluated at $d = 1$, with the problem in note 7 — 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

$$\begin{aligned} V_U(x + 1) - V_U(x) &= V_U(s_{x+1}, x + 1) - V_U(s_x, x) \\ &\geq V_U(s_x, x + 1) - V_U(s_x, x) \\ &= \delta (V_U(x) - V_U(x - 1)) (1 - p(s_x)) > 0. \end{aligned} \quad (12)$$

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

Proof of Proposition 2. See above for $s_1 = s_0$. For $d \geq 1$, s_d satisfies the first order condition $p'(s_d) = \frac{1}{\delta(V_E - V_U(d-1))}$. 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 $\eta_d = \eta_{d-1} = \dots = \eta_0$; 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 $x > 0$, $\tilde{V}_U(x + 1) - \tilde{V}_U(x) > 0$ 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

$$\begin{aligned}\tilde{V}_U(x) &= \max_{s < \theta} u(0) - s + \delta \left[p(s) V_E + (1 - p(s)) \tilde{V}_U(x) \right] \\ &= \max_{s < \theta} \frac{u(0) - s + \delta p(s) V_E}{1 - \delta(1 - p(s))}\end{aligned}\tag{13}$$

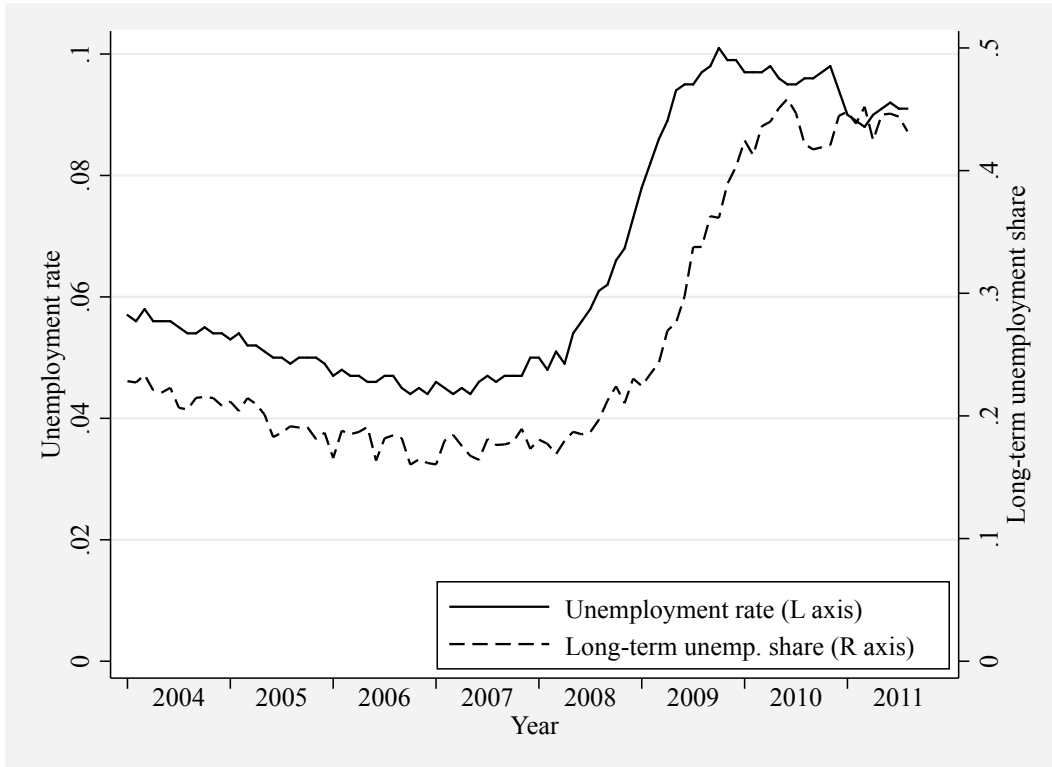
for all $0 \leq x \leq d$. Note that the right-hand side of (13) does not vary with x , so the left side does not either. In particular, $\tilde{V}_U(d) = \tilde{V}_U(d-1)$. Moreover, because labor force exit with $s = \tilde{s}_d < \theta$ is a feasible option with $d+1$ weeks of benefits available, it must be the case that $\tilde{V}_U(d+1) > \tilde{V}_U(d)$. Next, note that

$$\begin{aligned}\tilde{V}_U(d) &< \tilde{V}_U(d+1) \\ &= \tilde{V}_U(\tilde{s}_{d+1}, d+1) \\ &= u(b) - \tilde{s}_{d+1} + \delta \left[p(\tilde{s}_{d+1}) V_E + (1 - p(\tilde{s}_{d+1})) \tilde{V}_U(d) \right] \\ &= \tilde{V}_U(\tilde{s}_{d+1}, d) + \delta(1 - p(\tilde{s}_{d+1})) \left(\tilde{V}_U(d) - \tilde{V}_U(d-1) \right) \\ &< \tilde{V}_U(d) + \delta(1 - p(\tilde{s}_{d+1})) \left(\tilde{V}_U(d) - \tilde{V}_U(d-1) \right),\end{aligned}\tag{14}$$

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.

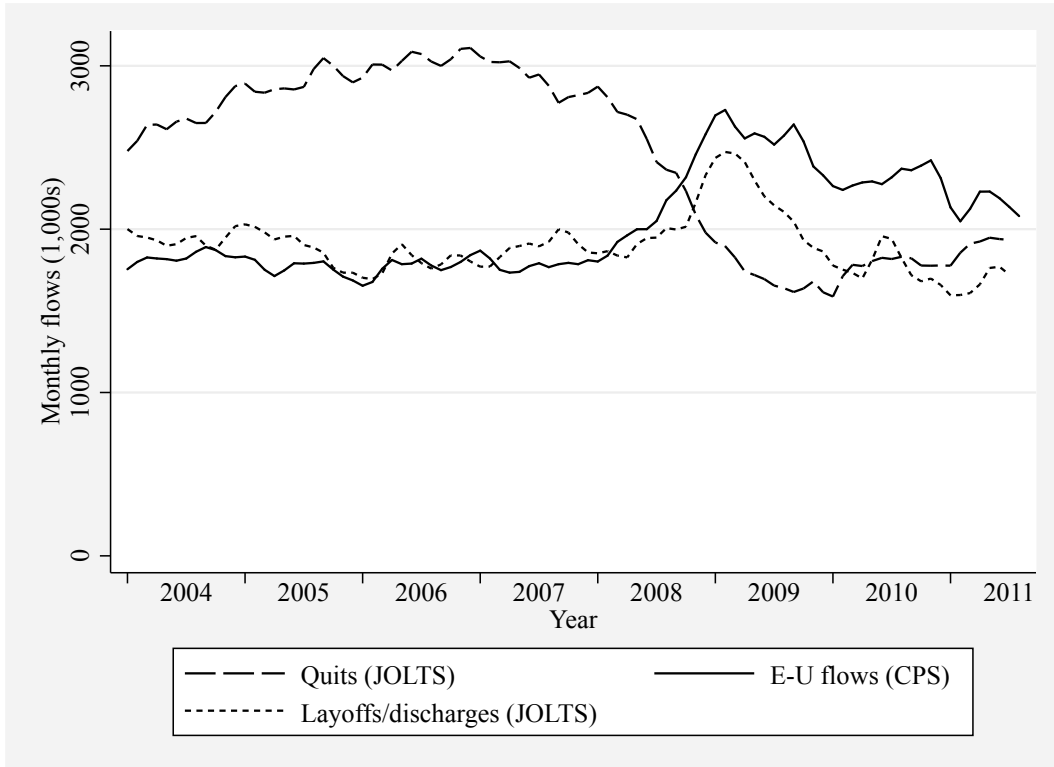
There are thus only three possible values for the η_d sequence: $\eta_d = 1$ for all $d \geq 0$; $\eta_d = 0$ for all $d \geq 0$; or $\eta_d = \begin{cases} 0 & \text{if } d = 0 \\ 1 & \text{if } d > 0 \end{cases}$. Unemployment to non-participation transitions thus occur only when benefits are exhausted;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

Figure 1. Unemployment and long-term unemployment



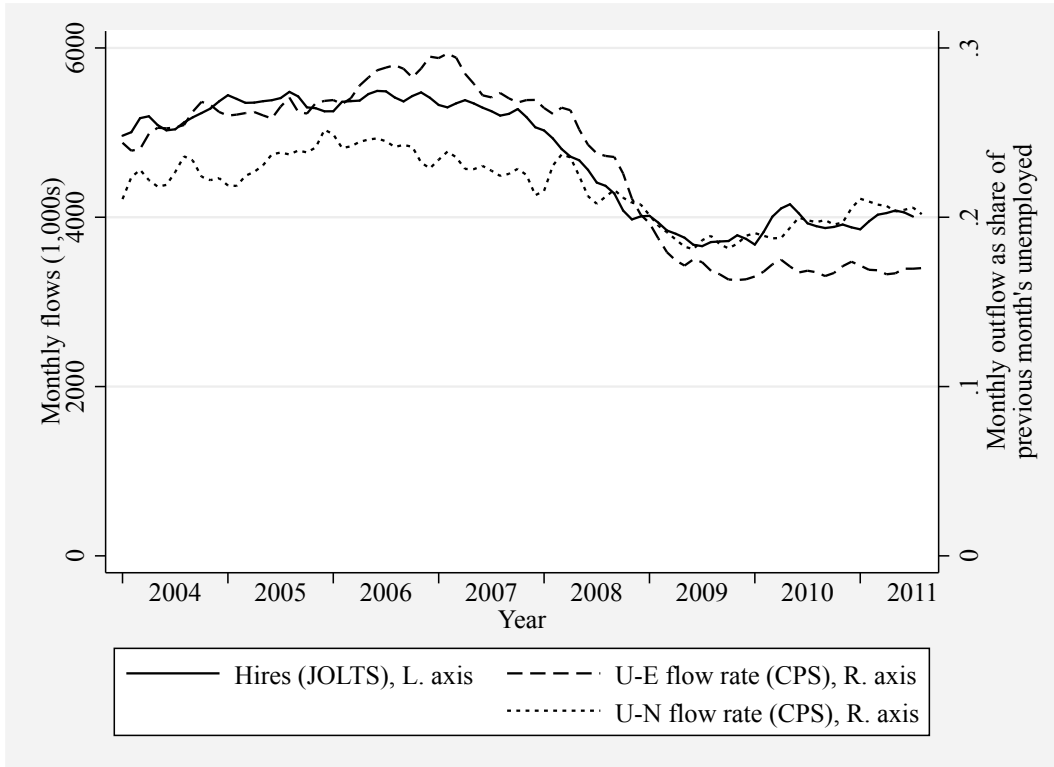
Notes: The long-term unemployment share is the fraction of the unemployed who have been unemployed for 27 weeks or more. Both series are seasonally adjusted.

Figure 2A. Monthly flows out of jobs



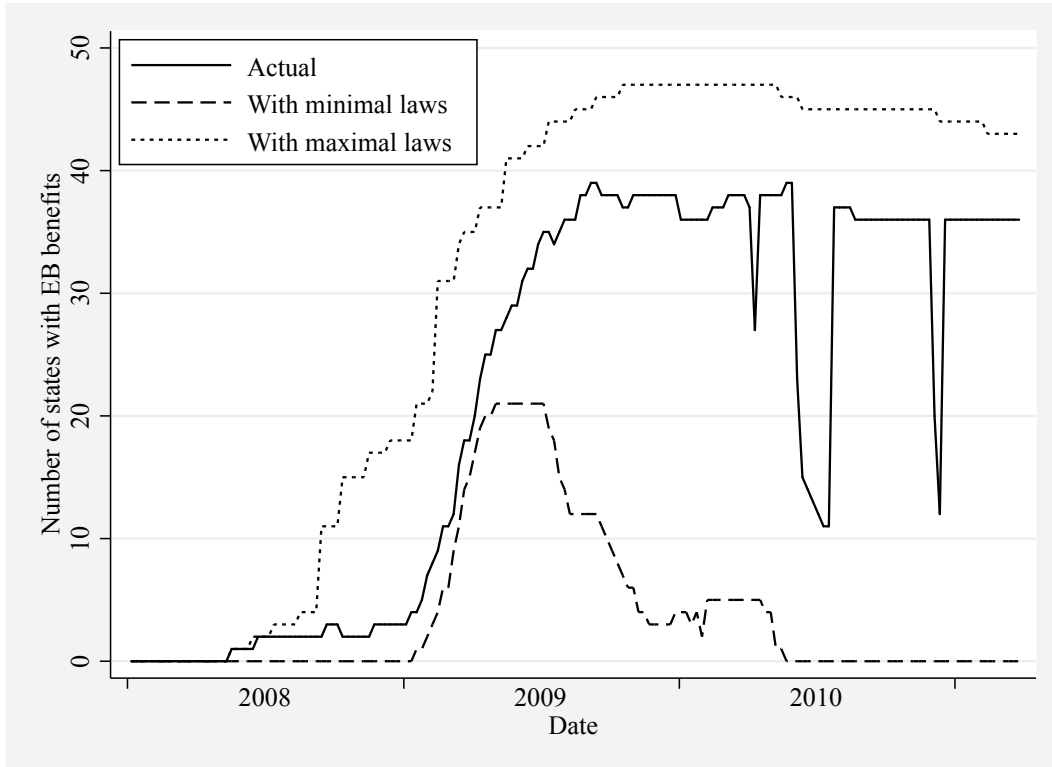
Notes: Quits and layoffs/discharges come from the JOLTS data, which derive from employer surveys. Employment-to-unemployment (E-U) flows are taken from the research series on labor force status flows constructed by the Bureau of Labor Statistics from longitudinally linked monthly CPS files. All series are seasonally adjusted and smoothed with a 3-month symmetric triangular moving average, $y_t^{sm} = (y_{t-1} + 2y_t + y_{t+1})/4$.

Figure 2B. Monthly flows into jobs / out of unemployment



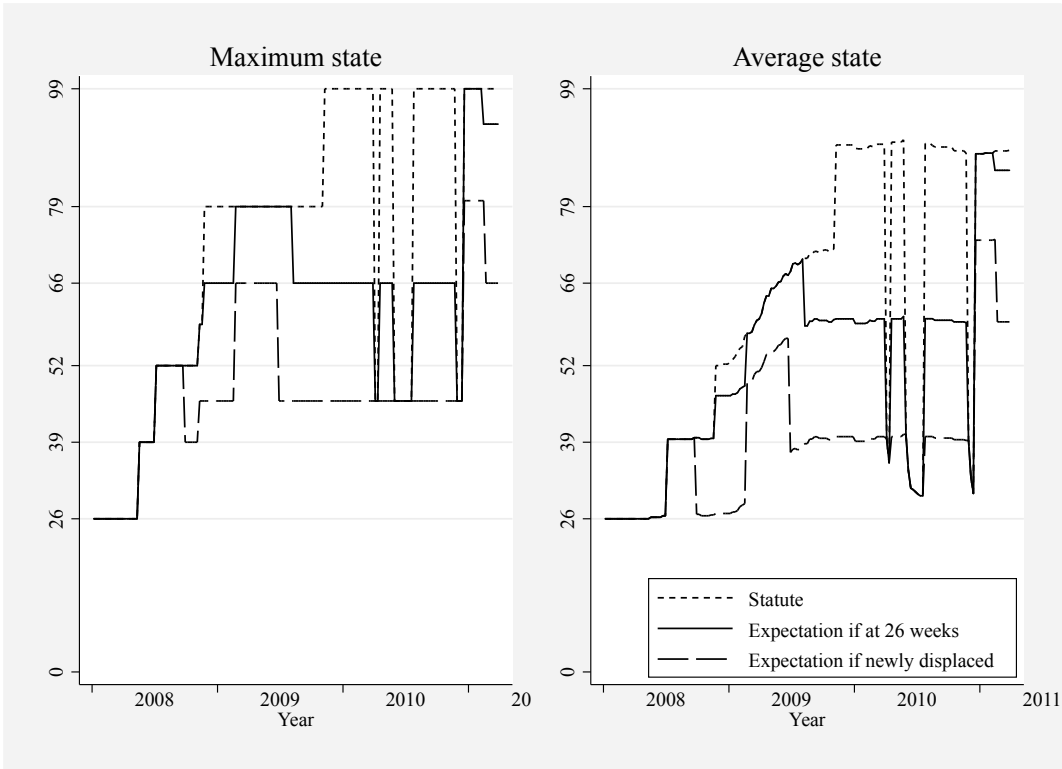
Notes: Hires come from the JOLTS data, which derive from employer surveys. Unemployment-to-employment (U-E) and unemployment-to-nonparticipation (U-N) flows are taken from the research series on labor force status flows constructed by BLS from longitudinally linked monthly CPS files and are expressed as shares of the previous month's unemployed population. All series are seasonally adjusted and smoothed with a 3-month symmetric triangular moving average.

Figure 3. Extended Benefits (EB) availability and the role of optional triggers



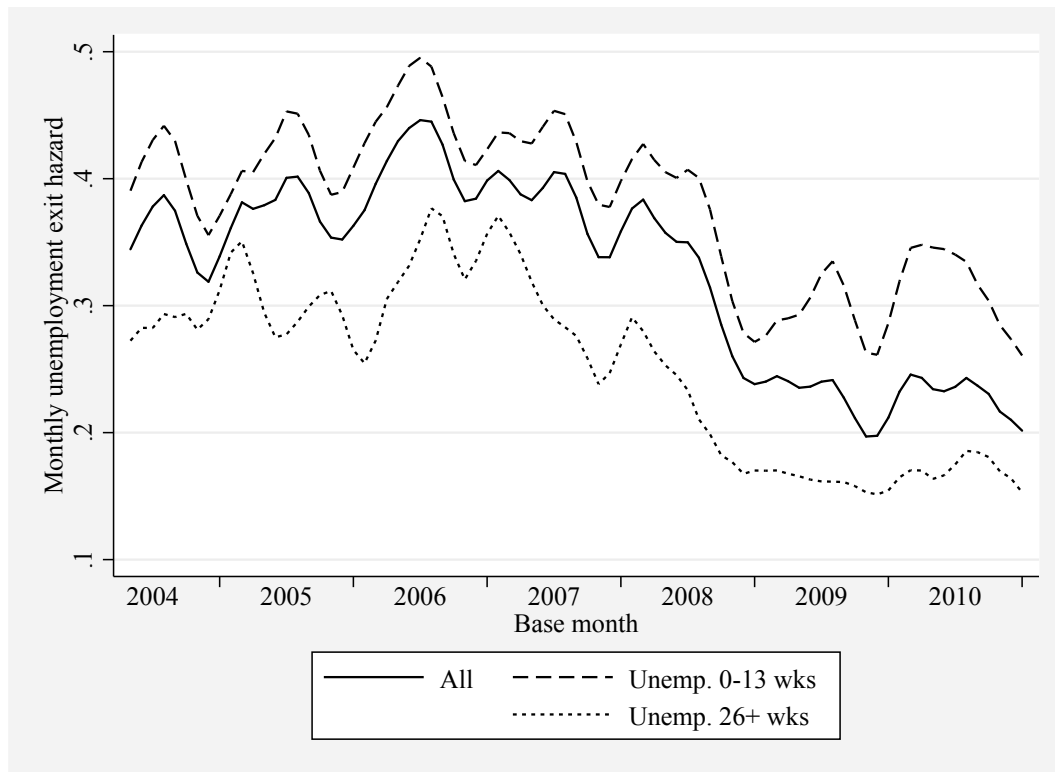
Notes: “Actual” series is computed from the Employment and Training Administration’s weekly EB trigger notices. “Minimal laws” series is simulated for states that participate in the EB program but do not adopt the optional 3-year lookback period enacted in December 2010 or any of the optional triggers available in the EB law. In these states, eligibility for EB depends on having an insured unemployment rate that exceeds 5% and is above 120% of the maximum of the one-year-lagged and two-year-lagged IURs. “Maximal laws” series is simulated for a state that has adopted (a) the alternative IUR trigger, which provides EB if the IUR is above 6%, regardless of its lagged values; (b) the optional total unemployment rate (TUR) trigger, which provides EB if the TUR exceeds 6.5% and is above the minimum of the one-year, two-year, and (optionally) three-year lagged TURs; and (c) the 3-year lookback (assumed to go into effect on January 1, 2011).

Figure 4. UI benefit durations, statutory and as perceived by recipients



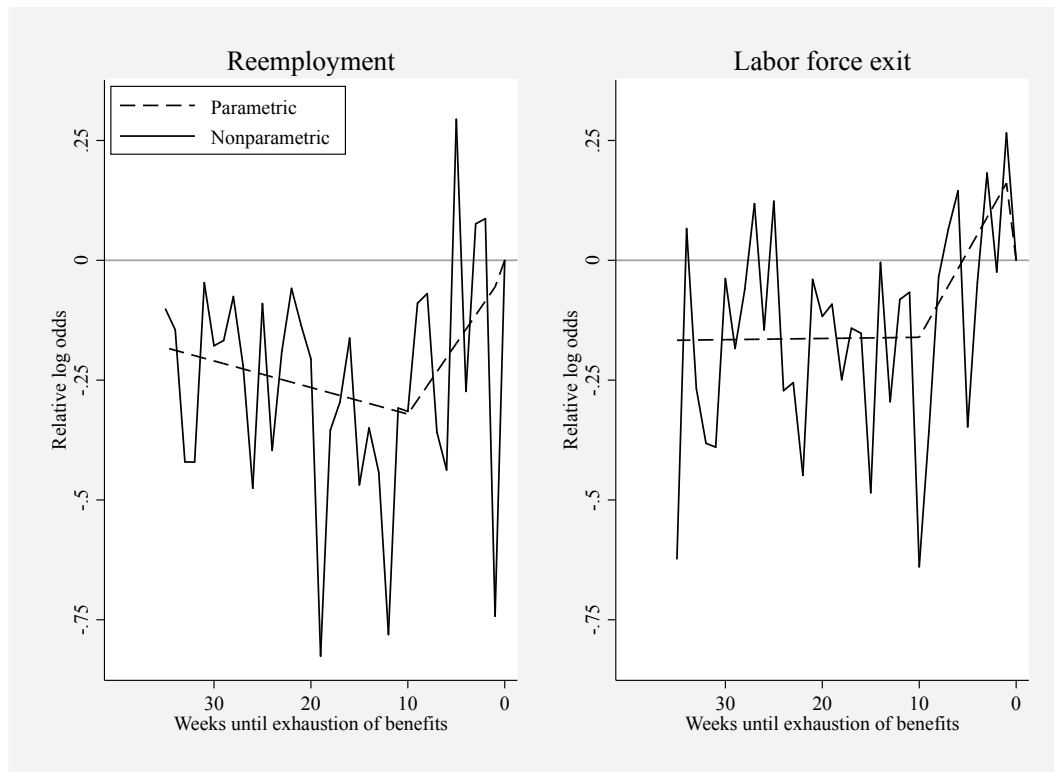
Notes: Expectations are those of recipients who do not anticipate further federal legislation, changes in a state’s EB participation (including those determined by already-legislated triggers), or state trigger events. “Maximum state” is the state with the highest value in a given month. “Average state” is the unweighted mean across states.

Figure 5. Monthly unemployment exit hazards for displaced workers, by duration group



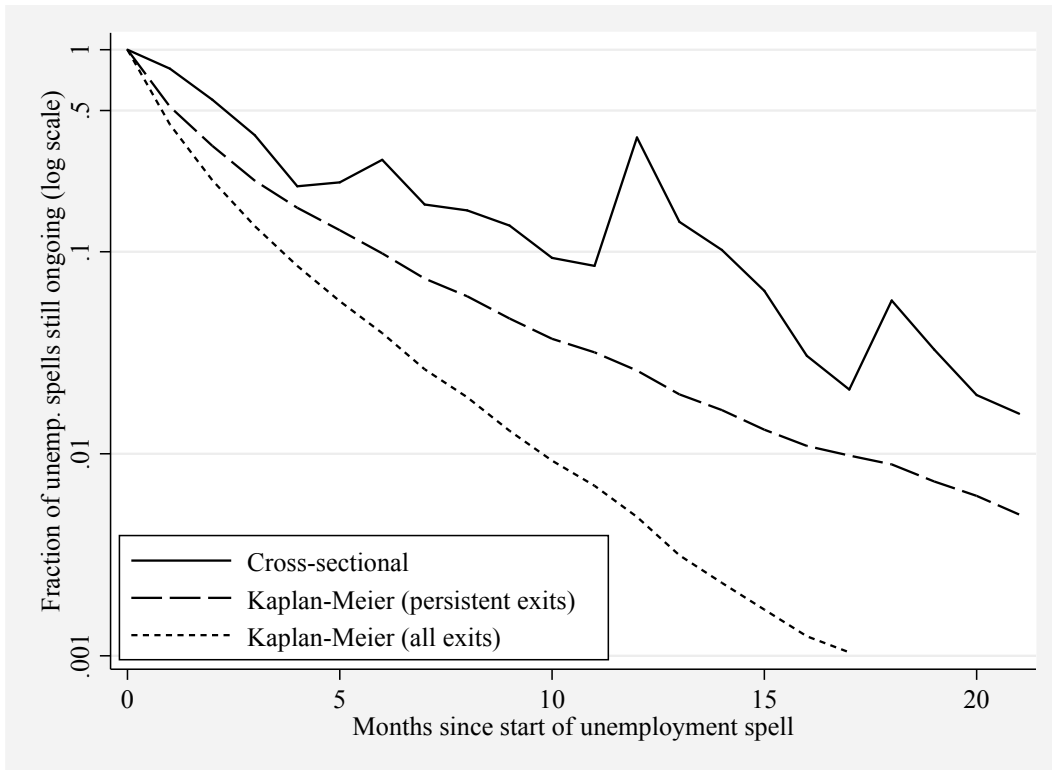
Notes: Displaced workers are unemployed individuals who report having lost their last job. Hazards represent the probability of being employed or out of the labor force one month later and not unemployed the following month. Series are not seasonally adjusted and are smoothed using a 5-month symmetric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

Figure 6: Parametric vs. nonparametric specifications of the time-to-exhaustion effect



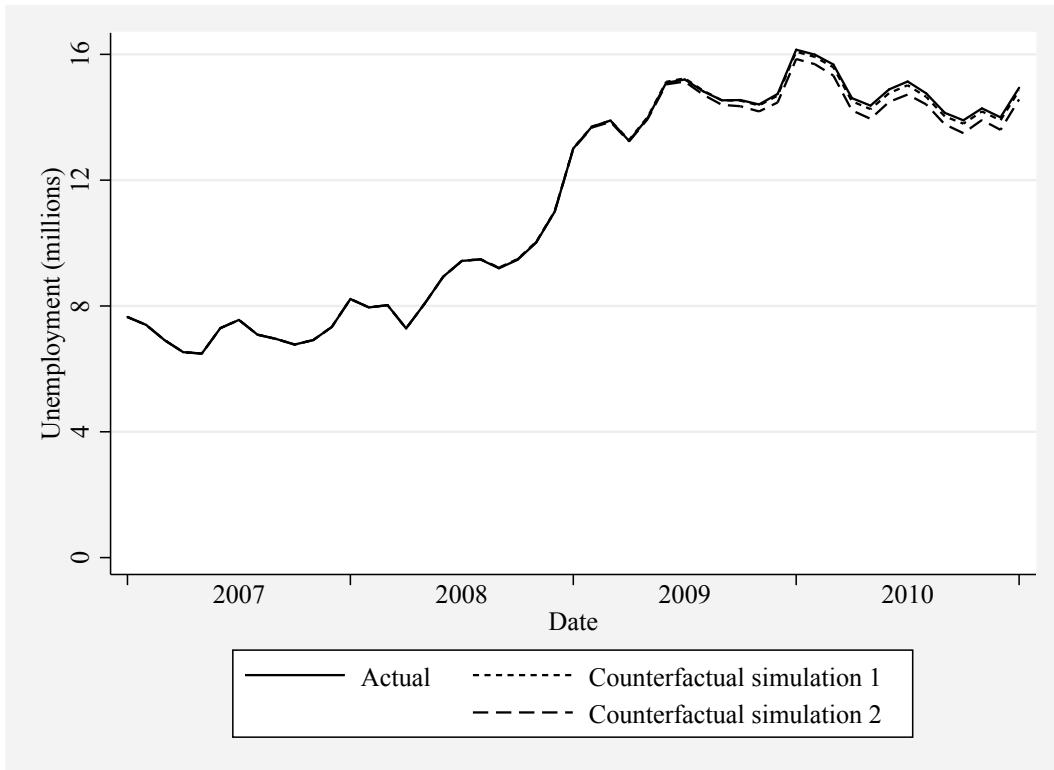
Notes: Each series is obtained from a multinomial logit regression with state and time indicators, a cubic in the unemployment rate, and the unemployment duration controls described in the notes to Table 7. “Nonparametric” specification has a full set of time-to-exhaustion indicators (0 is the excluded category). Estimation sample includes values as high as 99, but only those below 35 are shown here. “Parametric” series replaces time-to-exhaustion (d) dummies with an indicator for $d > 0$, a linear control for d , and a control for $\max(0, d-10)$. Coefficients from this specification are reported in Table 7, row 3.

Figure 7: Alternative survival curves from cross-sectional and longitudinally-linked data.



Notes: All series refer to unemployment spells beginning in 2008. The cross-sectional survival curve is computed as the number unemployed d months in month $m+d$ divided by the number unemployed 0 months in month m (with each aggregated over months m in 2008). Kaplan-Meier survival curves are the product from $t=0$ to $d-1$ 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 she is unemployed in $m+t+2$, regardless of her measured status in $m+t+1$.

Figure 8. Actual unemployment and counterfactual simulations without UI extensions



Notes: Counterfactual simulations are based on the specification in Table 5, Column 2. See text for details.

Table 1. The evolution of Emergency Unemployment Compensation (EUC)

Date	Weeks available under EUC Tier				Scheduled EUC expiration
	I (1)	II (2)	III (3)	IV (4)	
Jun. 30, 2008	13				Mar. 28, 2009
Nov. 21, 2008	20	13 C			Mar. 28, 2009
Feb. 17, 2009	20	13 C			Dec. 26, 2009
Nov. 6, 2009	20	14	13 C	6 H	Dec. 26, 2009
Dec. 19, 2009	20	14	13 C	6 H	Feb. 28, 2010
Feb. 28, 2010	0	0	0	0	n/a
Mar. 2, 2010	20	14	13 C	6 H	Apr. 5, 2010
Apr. 5, 2010	0	0	0	0	n/a
Apr. 15, 2010	20	14	13 C	6 H	Jun. 2, 2010
Jun. 2, 2010	0	0	0	0	n/a
Jul. 22, 2010	20	14	13 C	6 H	Nov. 30, 2010
Nov. 30, 2010	0	0	0	0	n/a
Dec. 17, 2010	20	14	13 C	6 H	Jan. 3, 2012

Notes: Dates listed are those on which legislation creating, changing, or reauthorizing EUC was enacted and those on which the EUC program expired. After expiration, the eventual reauthorization was in each case retroactive. "C" indicates that benefits under the indicated tier were available only in states with unemployment rates above 6%; "H" indicates availability only with unemployment rates above 8.5%.

Table 2. Summary statistics

	All unemployed		Subsample with 2+ follow-up interviews	
	Job leavers / entrants /		Job leavers / entrants /	
	losers	reentrants	losers	reentrants
	(1)	(2)	(3)	(4)
N	95,485	77,913	77,813	61,105
Share matched to one follow-up interview	91%	91%	100%	100%
Share matched to two 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	5%	6%	5%	6%
Share exiting unemployment by next month				
Counting all exits (1+ 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+ follow-ups)				
Total	30%	42%	29%	41%
To employment	20%	18%	20%	18%
Out of labor force	10%	24%	10%	24%
Anticipated weeks of unemployment benefits				
Total	43.9	--	44.2	--
Remaining	24.1	--	24.0	--
Total (anticipating EUC reauthorization)	56.7	--	57.0	--
State unemployment rate	7.7%	6.9%	7.7%	6.9%

Notes: "All unemployed" are unemployed observations from the May 2004 – January 2011 CPS samples with month-in-sample 1, 2, 5, or 6. All statistics use CPS weights. Subsample in columns 3-4 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.

Table 3. Logit models for monthly unemployment exit

	Sample is job-losers					Sample is all unemployed	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Constant effect of UI across all durations							
# of weeks of UI benefits (/100)	-0.33 (0.10)	-0.27 (0.10)	-0.31 (0.10)	-0.34 (0.10)	-0.37 (0.10)	-0.15 (0.10)	-0.19 (0.10)
Effect of UI extensions on avg. exit hazard in 2010:Q4	-2.1 p.p.	-1.7 p.p.	-1.9 p.p.	-2.1 p.p.	-2.3 p.p.	-0.9 p.p.	-1.2 p.p.
Controls							
State FEs	Y	Y	Y	Y	Y		
Month FEs	Y	Y	Y	Y	Y		
State-by-month FEs						Y	Y
Unemp duration controls	Y	Y	Y	Y	Y	Y	Y
State unemployment rate		linear	cubic	cubic	cubic		
State insured unemp rate				cubic	cubic		
State new UI claims rate				cubic	cubic		
State employment growth rate				cubic	cubic		
Individual covariates					Y		Y
Job loser indicator						Y	Y
Unemployment duration X job loser						Y	Y
Unemployment rate X job loser						Y	Y
# of weeks of benefits if elig.						Y	Y
Panel B: Allowing effect to vary by individual unemployment duration							
Weeks of benefits (/100) X unemployed < 26 weeks	0.08 (0.15)	0.20 (0.15)	0.13 (0.15)	0.10 (0.14)	0.10 (0.14)	-0.11 (0.19)	-0.13 (0.19)
Weeks of benefits (/100) X unemployed 26+ weeks	-0.37 (0.09)	-0.30 (0.10)	-0.34 (0.09)	-0.36 (0.09)	-0.40 (0.09)	-0.19 (0.10)	-0.23 (0.11)
average exit hazard in 2010:Q4	-1.5 p.p.	-1.0 p.p.	-1.3 p.p.	-1.4 p.p.	-1.6 p.p.	-1.0 p.p.	-1.3 p.p.

Notes: N = 77,813 in columns 1-5 and 138,883 in columns 6-7. Average monthly exit hazard in the full sample of job-losers is 29.4%; in the 2010:Q4 subsample it is 22.4%. Unemployment duration controls are the number of weeks of unemployment (as reported in the beginning-of-month survey), its square, its inverse, and an indicator for being newly unemployed (≤ 1 week). Specifications in Panel B also include an indicator for being unemployed 26 weeks or more. Individual covariates in columns 5 and 7 are gender; marital status; a female-married interaction; and age, education, and pre-unemployment industry dummies (6, 4, and 15 categories, respectively). See text for description of the covariates in columns 6 and 7. Specifications in columns 1-5 use the CPS sample weight, while columns 6 and 7, which are estimated by conditional logit, use the average CPS weight in the state-month cell. All standard errors are clustered at the state level.

Table 4. Sensitivity of the results to recipient expectations model

	(1)	(2)	(3)	(4)	(5)
Panel A: Constant effect of UI across all durations					
Weeks of benefits (/100) X unemployed < 26 weeks	0.13 (0.15)	-0.08 (0.17)	0.07 (0.20)	0.02 (0.26)	-0.12 (0.22)
Weeks of benefits (/100) X unemployed 26+ weeks	-0.34 (0.09)	-0.44 (0.17)	-0.43 (0.19)	-0.48 (0.34)	-0.62 (0.27)
Weeks of benefits (/100) X UE<26 weeks X abs(expectations range)			-0.20 (0.62)		
Weeks of benefits (/100) X UE<26 weeks X abs(expectations range)			-0.62 (0.39)		
Effect of UI extensions on average exit hazard in 2010:Q4	-1.3 p.p.	-3.0 p.p.	-1.8 p.p.	-2.1 p.p.	-3.1 p.p.
Controls					
State FEs	Y	Y	Y	Y	Y
Month FEs	Y	Y	Y	Y	Y
Unemp duration controls	Y	Y	Y	Y	Y
State unemployment rate	cubic	cubic	cubic	cubic	cubic
Forecast EUC reauthorization?	N	Y	N	N	N
EUC weeks available				Y	Y
EB trigger status					Y
EB availability under alternative rules					Y

Notes: Specification in Column 1 is that in Column 3 of Table 3, Panel B. Sample is identical in subsequent columns; see text for description of additional covariates.

Table 5. Multinomial logit models for reemployment and labor force exit versus continued unemployment

	Specification & sample				
	T3, C1	T3, C3	T3, C5	T4, C3	T4, C5
	(1)	(2)	(3)	(4)	(5)
Reemployment					
Weeks of benefits (*100) X unemployed < 26 weeks	0.19 (0.19)	0.24 (0.19)	0.18 (0.19)	0.48 (0.24)	0.01 (0.33)
Weeks of benefits (*100) X unemployed 26+ weeks	-0.44 (0.13)	-0.42 (0.14)	-0.47 (0.14)	-0.29 (0.21)	-0.64 (0.37)
Labor force exit					
Weeks of benefits (*100) X unemployed < 26 weeks	-0.19 (0.21)	-0.12 (0.21)	-0.11 (0.21)	-0.41 (0.45)	-0.32 (0.26)
Weeks of benefits (*100) X unemployed 26+ weeks	-0.38 (0.13)	-0.34 (0.13)	-0.42 (0.15)	-0.55 (0.37)	-0.58 (0.34)
Effect of extensions on average hazards in 2010:Q4					
Reemployment	-0.6 p.p.	-0.5 p.p.	-0.7 p.p.	0.2 p.p.	-1.2 p.p.
Labor force exit	-1.2 p.p.	-1.0 p.p.	-1.2 p.p.	-2.0 p.p.	-1.8 p.p.

Notes: Samples and specifications are as in the indicated columns of Tables 3 and 4, using a multinomial logit for a trichotomous outcome (unemployment, employment, or not in labor force) in place of the logits for dichotomous outcomes (unemployment versus non-unemployment) used in those tables. Average monthly hazards in the full sample are 19.9% for reemployment and 9.6% for labor force exit; in the 2010:Q4 subsample they are 13.4% and 9.0%, respectively.

Table 6. Alternative specifications & subsamples

		Reemployment		Labor force exit	
		Avg. hazard in 2010:Q4	Effect of UI extensions	Avg. hazard in 2010:Q4	Effect of UI extensions
		(1)	(2)	(3)	(4)
Alternative specifications & samples					
(1)	Baseline	13.4%	-0.5 p.p.	9.0%	-1.0 p.p.
(2)	Separate effect on 26 wks Drop round number & inconsistent	13.4%	-0.5 p.p.	9.0%	-1.0 p.p.
(3)	durations	12.8%	-0.5 p.p.	7.9%	-1.5 p.p.
(4)	Drop very short durations	14.2%	+0.1 p.p.	9.6%	-1.1 p.p.
(5)	Count all UE exits	16.5%	-0.6 p.p.	13.7%	-1.3 p.p.
Subsamples					
Age					
(6)	Age 25-54 (N=53,104)	14.4%	-1.0 p.p.	7.5%	-1.8 p.p.
(7)	Age 55+ (N=13,990)	11.6%	+1.4 p.p.	9.7%	+0.5 p.p.
Gender					
(8)	Men (N=47,782)	13.7%	-0.2 p.p.	7.3%	-1.2 p.p.
(9)	Women (N=30,031)	13.0%	-1.0 p.p.	11.7%	-0.8 p.p.
Education					
(10)	HS or less (N=43,628)	13.3%	-0.4 p.p.	10.0%	-1.8 p.p.
(11)	Some college or more (N=34,185)	13.7%	-0.5 p.p.	7.8%	-0.1 p.p.
Industry					
(12)	Const./manuf. (N=25,584)	14.2%	+0.4 p.p.	7.4%	-2.1 p.p.
(13)	All other industries (N=52,229)	13.1%	-0.9 p.p.	9.7%	-0.4 p.p.

Notes: Baseline specification in Row 1 is that from Column 2 of Table 5; N=77,813. Row 2 adds an indicator for durations of exactly 26 weeks and its interaction with number of weeks of UI benefits available. Row 3 drops observations whose unemployment duration at the beginning of the spell or the base CPS interview was 26, 52, or 78 weeks, as well as those in month-in-sample 2 that are inconsistent with the duration in month 1; N=61,854. Row 4 drops all observations with durations under 8 weeks; N=49,852. Row 5 counts all U-N and U-E transitions as exits from unemployment, even those (the U-N-U and U-E-U transitions) that return to unemployment the following month. Rows 6-13 use the baseline specification on different subsamples, with sample sizes as indicated in the table. Italicized estimates in columns 2 and 4 indicate specifications where the UI effects were jointly insignificant at the 5% level.

Table 7. Effects of time until UI exhaustion

	Any weeks left	# of weeks left (/10)	max(0, # of weeks - 10) (/10)	Effect of UI extensions in 2010:Q4 (p.p.)
	(1)	(2)	(3)	(4)
Logit for unemployment exit				
(1) Logit for unemployment exit with state, month, UR controls	0.12 (0.08)	-0.36 (0.10)	0.39 (0.11)	-0.7 p.p.
(2) Logit for unemployment exit with state-by-month controls	0.10 (0.08)	-0.33 (0.11)	0.37 (0.12)	-0.5 p.p.
(3) Multinomial logit with state, month, UR controls				
Reemployment	-0.03 (0.11)	-0.29 (0.13)	0.35 (0.14)	-0.0 p.p.
Labor force exit	0.20 (0.10)	-0.36 (0.12)	0.35 (0.13)	-0.6 p.p.

Notes: Each numbered row represents a separate specification. All include indicators for the duration of the unemployment spell, in weeks (up to 26), plus a linear spline with kinks at 30, 40, 50, 60, 70, 80, and 90 weeks. Rows 1 and 3 include state and month indicators plus a cubic in the state unemployment rate; row 2 replaces these with state-by-month indicators. Calculation of weeks until UI exhaustion is based on the expectation model described in the text, applied to the date of the baseline survey.

Table 8. Effect of UI extensions on labor market aggregates in January 2011

	Unemployment				Long-term unemp. share	
	Level (1,000s)		Rate			
	Method 1	Method 2	Method 1	Method 2	Method 1	Method 2
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline						
Actual in January 2011	14,937		9.0%		45.5%	
Panel B: Full effect of UI extension						
Strategy 1 (Table 5, Col. 2)	+87	+370	+0.1 p.p.	+0.2 p.p.	+0.5 p.p.	+1.6 p.p.
Strategy 2 (Table 3, Col. 6)	+131	+297	+0.1 p.p.	+0.2 p.p.	+0.3 p.p.	+0.9 p.p.
Strategy 3 (Table 5, Col. 5)	+283	+759	+0.2 p.p.	+0.5 p.p.	+0.9 p.p.	+2.8 p.p.
Strategy 4 (Table 7, Row 3)	+5	+226	+0.0 p.p.	+0.1 p.p.	+0.6 p.p.	+1.5 p.p.
Panel C: Effect operating through labor force participation						
Strategy 1 (Table 5, Col. 2)	+98	+264	+0.1 p.p.	+0.2 p.p.	+0.3 p.p.	+0.9 p.p.
Strategy 3 (Table 5, Col. 5)	+183	+476	+0.1 p.p.	+0.3 p.p.	+0.5 p.p.	+1.6 p.p.
Strategy 4 (Table 7, Row 3)	+92	+208	+0.1 p.p.	+0.1 p.p.	+0.3 p.p.	+0.8 p.p.

Notes: Indicated effects are the difference between the actual level (or rate) and a simulated level (or rate) obtained with benefit durations held fixed at 26 weeks throughout the 2004-2011 period, using coefficients from the indicated specifications. “Method 1” and “Method 2” refer to alternative treatment in the counterfactual of residuals obtained from simulating the actual data; see text for details. Simulations in Panel C assume that in the counterfactual scenario the multinomial logit index for the labor force exit outcome would change but the index for the reemployment outcome would be unaffected.