

Unemployment Insurance and Job Search in the Great Recession

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Abstract

Nearly two years after the official end of the "Great Recession," the labor market remains historically weak. Many commentators have attributed the ongoing weakness in part to 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 by only about 0.2–0.6 percentage points, much less than is implied by previous analyses. Half or more of this effect is due to reduced labor force exit among the unemployed rather than to the changes in reemployment rates that are of greater policy concern; some analyses even suggest that UI extensions, by keeping displaced workers in the labor market, may have increased the share who were later reemployed.

1 Introduction

While the so-called “Great Recession” officially ended in June 2009, word has not yet reached the labor market. In August 2011, the unemployment rate remained above nine percent —

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it has fallen below that threshold for only 2 of the last 28 months — and over 40% 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 in 2008 and 2009 have contributed to some degree to the elevated unemployment rate. However, the magnitude and interpretation of this effect is not clear. In an op-ed, Barro (2010) presents a “rough” estimate that extensions of UI benefits contributed 2.7 percentage points to the unemployment rate in June 2010, and Grubb’s (2011) comprehensive review of the literature leads him to a similar conclusion (p. 34). Several direct analyses of recent labor force survey data (see, e.g., Mazumder, 2011; Valetta and Kuang, 2010; Fujita, 2011) find smaller but still substantial effects.

There are two channels by which UI can raise unemployment, however, 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 expect a displaced worker in a weak labor market to find a job by the expiration of regular benefits. Thus, obtaining a credible

estimate of the effect of the recent UI extensions requires a strategy for distinguishing this effect from the confounding influence of historically weak labor demand.

This paper uses the haphazard 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 displaced workers. 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 effect 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. A second 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. Third, 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. Finally, I exploit differences in expected eligibility for EUC benefits by date of unemployment, driven by the uneven way that the EUC program phases out when it expires, to generate variation in UI benefit durations among UI-eligible workers within state-month cells.

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 two percentage points (on a base of 23.0%). 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 0.6 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. Adopting such assumptions, 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 0.3 percentage points lower and the long-term share of the unemployed would have been about 1.6 percentage points lower, with over half of each effect coming from reduced labor force exit. Even the specification yielding the largest effects indicates that UI extensions contributed only 0.6 percentage points to the unemployment rate. A simulation of the outcomes of workers displaced in the first quarter of 2009 indicates that UI extensions *raised* the share who became reemployed by January 2011 by about 1.3 percentage points (on a base of 68%) by reducing the share who exited the labor force. As this simulation requires an independent risks assumption that is not easily defensible, it would be premature to conclude that the EUC and EB programs have had net positive effects on the reemployment of displaced workers. However, it is clear that any negative effects are quite small.

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

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).

aggregates, and presents results. Section 7 concludes.

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: Nonfarm payroll employment rose through most of 2007, fell by 738,000 in the first half of 2008, and then fell by nearly 6.8 million over the next 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 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 data 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 em-

ployment 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

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

thereafter.³ The June 2008 legislation made 13 weeks of EUC benefits available to anyone who exhausted his regular benefits before March 28, 2009. The EUC program was subsequently extended and expanded several times by Congressional action:

- In November 2008, an additional seven weeks were added to what was thereafter referred to as “Tier I” of EUC benefits. On top of this, 13 weeks of Tier II benefits were made available in states with unemployment rates above 6 percent, permitting as many as 33 weeks of EUC benefits in those states.
- In February 2009, the expiration of the EUC program was extended to December 26, 2009.
- In November 2009, Tier II benefits were extended by one week and made unconditional. 13 weeks of Tier III benefits were added in states with unemployment rates above 6 percent, and six further weeks of Tier IV benefits were provided for states with unemployment rates above 8.5 percent. In such states, the four tiers together provided as many as 53 weeks of benefits. However, the program expiration date was unchanged from December 26, 2009.
- On December 19, 2009, one week before the scheduled expiration, the expiration date was pushed back to February 28, 2010.
- On March 2, 2010, the expiration date was extended to April 5 of that year, retroactive to the February 28 expiration.
- On April 15, 2010, the expiration date was again retroactively extended to June 2.
- On July 22, 2010, seven weeks after the June 2 expiration, the EUC program was once again retroactively extended to November 30.
- On December 17, 2010, the expiration date was extended, again retroactively, to January 3, 2012.

³This discussion draws heavily on Fujita (2010). I neglect a number of complexities of the UI program. In particular, claimants whose previous jobs were short are not eligible for the full 26 weeks of regular benefits or for the indicated number of weeks of EUC benefits. There are also important complexities having to do with unemployment spells interrupted by periods of employment or inactivity.

On top of these EUC program expansions and extensions, the American Recovery and Reinvestment Act of (February) 2009 made several other changes to the UI program: It provided for \$25 in extra weekly benefits to each recipient, for full Federal funding of the EB program (formerly split equally between state and federal budgets), for tax deductibility of a portion of UI benefits, and for somewhat expanded eligibility for benefits. The EB funding change induced a number of states to begin participating in the program and to adopt its optional, more generous triggers, further adding to the number of weeks of benefits available to unemployed workers.

Combining 26 weeks of regular benefits, up to 53 weeks of EUC, and as many as 20 weeks of EB, statutory benefit durations have reached as long as 99 weeks. 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, although as many 99 weeks of EUC benefits were available in statute starting in November 2009, 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, keeping maximum anticipated benefit durations below 70 for anyone who was not already out of work for a year or more.

It is not clear how to model workers' expectations in the weeks leading up to a scheduled 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 (reasonably) 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. I thus assume throughout that workers assume at all times that the EUC program will expire as scheduled according to then-current law

and that neither state nor federal legislation will change the terms of the program.⁴ I also assume that workers forecast that their states will neither trigger on to EUC tiers or EB benefits that they are not yet on nor trigger off of those that they are currently on.

Figure 3 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 newly displaced worker and of a worker who has just exhausted her regular benefits, respectively.

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 sunsetted. 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 for a brief period in early 2009 and then after 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 13 weeks of EB benefits and most had hit their triggers.

⁴Farber and Valletta (2011), in an analysis otherwise similar to this one, assume instead that workers act as if they anticipate seamless extensions. They obtain similar results to those here.

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.⁵ The income — and therefore the consumption — of an unemployed individual is b if she receives UI benefits and is 0 otherwise. 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 $w > b$ 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.

With these assumptions, the value function of someone who has a job at the beginning of a period is $V_E = \sum_{t=0}^{\infty} \delta^t u(w)$, where $1 - \delta$ is the per-period discount rate. The value function of an unemployed individual depends on her search effort and on the number of

⁵Chetty (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). Both results suggest that the income effects of UI benefits may be more important than the substitution effects.

weeks of benefits remaining, d :

$$V_U(s, d) = \begin{cases} u(b) - s + \delta [p(s) V_E + (1 - p(s)) V_U(d - 1)] & \text{if } d > 0 \\ u(0) - s + \delta [p(s) V_E + (1 - p(s)) V_U(0)] & \text{if } d = 0 \end{cases} \quad (1)$$

where $V_U(d) \equiv \max_s V_U(s, d)$. Search effort is chosen to maximize $V_U(s, d)$. The optimal choice will satisfy

$$p'(s_d^*) = \frac{1}{\delta(V_E - V_U(0))}$$

for $d \in \{0, 1\}$ and

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

for $d > 1$. 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 $p()$ 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.

But 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 $\theta > 0$; otherwise, he receives no benefit but preserves his remaining benefit entitlement.⁶ The value function is now:

⁶It is mathematically convenient but not substantively important that the range of s for which benefits are paid be closed on the left. Thus, I assume θ is strictly positive, although it can be arbitrarily close to zero.

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

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 faces identical optimization problems in both the preceding and the following weeks. Thus, labor force exit occurs either immediately after a job loss or upon exhaustion of UI benefits; UI benefit extensions reduce non-participation among those who would otherwise have exhausted their benefits. This implies that the net effect of UI extensions when job search requirements are enforced is ambiguous: 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 general equilibrium effects, or “crowding out.” 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 kind of search externality 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 reemployment probabilities will overstate the general equilibrium 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. All involve extrapolations from pre-recession estimates of the effect of UI durations or from pre-recession unemployment exit rates. Barro (2010) assumes that in the absence of the extensions the long-term unemployment rate would have held to the 24.5% level seen in 1983.⁷ This leads him to conclude that the unemployment rate would have been 2.7 percentage points lower in June 2010 than it actually was.

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 2011.⁸ 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.⁹

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.

⁷This ignores the long-run secular increase in the long-term unemployment rate, which at the business cycle peak in December 2007 was more than double its level at the January 1980 peak (18.9% vs. 8.3%).

⁸Aaronson et al. (2010), Fujita (2010), and Elsby et al. (2010) use similar strategies and obtain similar results.

⁹Another 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.

To absorb confounding effects from changes in labor demand, he controls linearly for the job vacancy rate. He finds larger effects of UI extensions on unemployment than does Mazumder (2011), primarily attributable to reduced reemployment rather than reduced labor force exit. However, these conclusions are based on the extrapolated effects of a reduction in the job vacancy rate that is roughly twice as large as the range observed in the earlier period.

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, in principle identifying the UI effect in the presence of arbitrary changes in demand conditions. They conclude that UI extensions raised the unemployment rate by 0.8 percentage points in mid-2010. 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.

Finally, Grubb’s (2011) and Howell and Azizoglu’s (2011) literature reviews come to very different conclusions about the likely effect of the current extensions. Grubb concludes that UI extensions are responsible for much of the increase in unemployment over the recession, while Howell and Azizoglu conclude that any effect is much smaller and primarily attributable to reduced labor force exit induced by the UI job search requirement.

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. Past research has found that only about half of the 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.¹⁰ 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, 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 that the individual anticipates no further legislative action or “triggering” of benefits on or off after that date, as in Figure 3. 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.

¹⁰Observations 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.

However, in a paper written simultaneously with this one Farber and Valletta (2011) use similar identification strategies along with an assumption that individuals perfectly forecast reauthorization of the EUC program, and obtain very similar results to those here.

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).¹¹ Relatedly, examination of the unemployment duration distributions indicates substantial heaping at 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.¹² 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.¹³ I discuss these issues

¹¹CPS procedures were altered in 1994, in part to reduce classification error. However, the Census Bureau has not released data from reinterview surveys since the redesign, so it is unclear how successful this was.

¹²Fujita (2011) also recodes some U-N-U trajectories as U-U-U.

¹³I 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 me to overstate the true impact of UI durations on labor force participation decisions.

at greater length in Section 6.

Finally, the CPS does not attempt to track respondents who change residences between interviews. If UI eligibility affects the propensity to move, this could bias my estimates in unknown ways. Although the mobility rate may have fallen over the course of the recession (Frey, 2009; Kaplan and Schulhofer-Wohl, 2011), my ability to match a CPS respondent to a follow-up outcome does not appear to be correlated with my UI duration measures, conditional on the covariates discussed below.

Table 1 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 4 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 2010.¹⁴ 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. However, both series fell similarly to the overall average in 2007 and 2008, suggesting that only a small portion of the overall exit hazard decline can be due 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

¹⁴This 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.

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 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.¹⁵ 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 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) that include a full set of dummy variables for all 100 possible values of n_{ist} , 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. My first empirical strategy exploits the haphazard roll-out of EUC, the discontinuous triggers in EUC and EB, and the repeated expiration and renewal of the federal authorizing legislation to generate variation in the duration of

¹⁵In principle, individuals can be followed for more than two periods in the CPS data. 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.

UI benefits among labor markets experiencing plausibly similar economic conditions. This requires absorbing 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). 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.

My second strategy narrows in on the variation 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 controls for the availability of EB benefits under maximal and minimal state participation in EB, along with indicators for the status of each of the four EB triggers and for the actual number of EUC weeks available.¹⁶ 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.

Both of these strategies rely on parametric controls to ensure that D_{ist} is conditionally uncorrelated with labor demand. A third 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 (Valette and Kuang, 2010; Farber and Valletta, 2011). Using a sample that pools all of the unemployed, I estimate:

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

¹⁶During the period covered by my sample, trigger 1 is “on” when the 13-week moving average of the insured unemployment rate (IUR) is at least 5% and above 120% of the maximum of its values one year and two years prior. Trigger 2 is on when the IUR is at least 6%, without the lookback provision. Trigger 3 is on when the three-month moving average of the total unemployment rate (TUR; the traditional measure) exceeds 6.5% and is above 110% of the minimum of its values one year and two years prior. Trigger 4 is on when the TUR exceeds 8%, with a similar lookback. Trigger 1 applies to all states; states can opt to use trigger 2 or trigger 3 as well, but if they use trigger 3 they must also provide 20 weeks (in place of the usual 13) of EB benefits if trigger 4 is on. My minimal and maximal simulated EB eligibility measures are an indicator for trigger 1 being on and an indicator for one of triggers 1, 2, and 3 being on; these simulated measures, following program rules, can change status no more than once in 13 weeks. See National Employment Law Project (2011) and Federal-State Extended Unemployment Compensation Act of 1970 (Undated).

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 UI duration coefficient θ is identified from covariance between UI extensions and changes in the *relative* unemployment exit rates of job losers and other unemployed, over and above that which can be explained via a cubic in the unemployment rate. 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 ineligible may not be a good guide to those of eligibles, leading specification (5) to overstate the causal effect of UI benefits.

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. But it seems more likely that these extensions would have larger effects on the job search behavior of those who directly benefit from them than on those who anticipate being eligible for extended benefits many months in the future. I explore this in two ways. First, I allow the θ coefficient to vary with n_{ist} , the length of the unemployment spell, allowing for different effects on those unemployed for more than or less than 26 weeks. Second, following the result in Section 2.3 that the intensity of search effort increases as benefit exhaustion approaches, I turn to a fourth estimation strategy that specifies 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 $f(0; \theta) = 0$, corresponding to an assumption 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. The effect of d_{ist} is identified from comparisons among individuals of different unemployment durations in the same state-month labor market. There are two sources of variation that allow separate identification of the effects of d and n without parametric restrictions, using a sample solely consisting of displaced workers. First, across- st variation in benefit availability has one-for-one effects on d_{ist} for those who have not yet exhausted benefits but not for those who have. Second, the EUC expiration rules mean that the addition of new EUC tiers extends d for those who will transition onto the new tiers before the EUC program expires but not for those with lower n_{ist} who expect the program to have expired before they reach the new tiers.

5 Estimates

Panel A of Table 2 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. I also show 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 fitted probability implied by the model if benefit durations had been held fixed at 26 weeks.¹⁷ Column 1 is estimated using only displaced workers who are presumed to be eligible for UI benefits, and includes state and month fixed effects, the n_{ist} controls indicated by (4), and a linear control for the state unemployment rate. It indicates a significant, albeit small 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 -1.7 percentage points (on a base of 22.4%). Columns 2, 3, and 4 add additional controls: First a cubic in the state unemployment rate in column 3, then cubics in two other measures of slackness — the number of UI claimants and the number of new UI claims, each expressed as a share of insured employment — in column 3, and finally a cubic in the state employment growth

¹⁷Strictly, 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.

rate, in column 4. These specifications indicate modestly larger UI duration effects.

Column 5 turns to my second strategy, using a “control function” to isolate variation in benefit durations coming from state decisions about which version of the EB triggers to use. I augment the specification from Column 2 with controls for the number of weeks of EUC benefits, 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 leaves little variation in the D variable and produces large standard errors, but the point estimate is similar to those in columns 1–4.

Finally, column 6 turns to my third strategy, returning to my preferred outcome variable and 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 specification includes state-by-month fixed effects.¹⁸ plus controls for separate duration and unemployment rate effects for job losers relative to the other unemployed. The UI effect is notably larger in this column than in the earlier specifications, perhaps indicating that the UI-ineligible unemployed are not an ideal control group for the eligible unemployed during the recession. However, even in this specification the estimates indicate that the UI extensions reduced the monthly exit hazard by only 3.7 percentage points. By comparison Figure 4 indicates that the exit hazard fell by about 20 percentage points between 2006 and 2009.

Panel B of Table 2 loosens the specifications from Panel A, allowing 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 heavily concentrated among those unemployed 26 weeks or more, with only one of the six estimates of the effect on shorter-term unemployed significantly different from zero. The implied effects of UI extensions on exit hazards are smaller than those in Panel A in columns 1–4, but larger in columns 5 and 6. Now column 5 shows a UI effect on the 2010:Q4 exit rate nearly twice as large as in column 4, but the standard errors indicate that the difference is not statistically significant.

Table 3 repeats the specifications from columns 1–5 of Table 2, Panel B, this time using

¹⁸For computational reasons, I estimate the within- st coefficients by conditional logit, then back out consistent but inefficient estimates of the α_{st} fixed effects for use in predicted exit probabilities.

multinomial logit models that distinguish between exits to employment and exits to non-participation in the labor force.¹⁹ The multinomial logit specification imposes the “independence of irrelevant alternatives” (IIA) assumption, which corresponds to an independent risks assumption for competing risk models. This is likely 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. Nevertheless, given the important differences in the interpretation of UI effects on reemployment and on labor force exit, distinguishing them even imperfectly can be useful to our understanding.

The results in Table 3 are quite stable across specifications. For the long-term unemployed, benefit durations have negative, significant effects of similar magnitude on the logit indexes for both types of unemployment exit. For the short-term unemployed, estimates indicate positive and moderately large but statistically insignificant effects on reemployment and roughly zero effect on labor force exit. 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 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.

It is worth considering the impact of violations of the IIA assumption on the multinomial logit estimates. The most likely source of violation is unobserved heterogeneity: Individuals with low job-finding probabilities may be most likely to exit the labor force. If so, UI extensions that dissuade these individuals from labor force exit will reduce the average job-finding probability among the unemployed through a pure composition effect, on top of any effect operating through UI’s disincentive for intensive search. Thus, one might expect the estimated effects on job-finding that result from the IIA assumption to somewhat overstate the true constant-composition effects. As even the estimated effects in Table 3 are quite small, it seems safe to conclude that UI extensions have not had large effects on the job-finding probabilities of the unemployed. I discuss the magnitudes of the estimated effects

¹⁹The multinomial logit version of column 6 from Table 2 is computationally quite intensive and is not included here.

at greater length in Section 6.

Table 4 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 3, column 2. Row 2 adds a number of individual-level control variables, including education, age, and previous industry indicators. This has essentially no effect on the estimates, suggesting that heterogeneity in exit rates is not creating important bias in the baseline results. Row 3 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. 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 4 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). This leads to larger effects of UI extensions on labor force exit, but does not change the substantive story. 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 a much smaller impact on the estimated effect of UI extensions.

Rows 6-8 of Table 4 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 the associated coefficients are mostly insignificant). Rows 8 and 9 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 an alternative specification (6) that allows the effects of UI durations to operate through the time to exhaustion. The basic specification is analogous to Column 2 of

Tables 2 and 3, with state and month indicators and a cubic in the state unemployment rate. I also include a full set of unemployment duration indicators²⁰ and controls for the time until exhaustion. 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 specification that allows for unrestricted d_{ist} effects. The d coefficients from this specification are illustrated as the solid line in Figure 5.²¹ They show a clear pattern of negative coefficients that are roughly constant across d_{ist} for $d_{ist} > 10$, then shrinking toward zero as d_{ist} falls toward zero. 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. Thus, 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 $d_{ist} > 0$). Note that the model in Section 2.3 implies that labor force participation may be higher when $d_{ist} = 1$ than when $d_{ist} = 0$ but conditional on participation job-finding rates should be the same in the two periods. This implies that in a multinomial logit specification the $d_{ist} > 0$ coefficient should be zero in the reemployment equation and positive in the labor force exit equation

The estimates from a logit version of the resulting specification are shown as a dashed line in Figure 5, and in the first row of Table 5. As in the semi parametric specification, 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, and sharply increasing as

²⁰The 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).

²¹The maximum value of d_{ist} in my sample is 83, but the number of observations is quite small for values above 46 so I show coefficients only for the lower portion of the distribution.

d falls from 10 toward 0.²² 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 5 shows that the implied effect of UI benefits on the UI exit rate is quite similar to those in Panel B of Table 2.

The second row of Table 5 shows a specification that includes a full set of state-by-month indicators. This shows 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. As before, we see substantial effects of UI benefits on both margins, though the net effects are somewhat smaller than in Table 3. Note also the large positive coefficient for the ($d > 0$) indicator in the labor force exit equation. This indicates that people in their last week of benefits are more likely to exit the labor force than are those who have already exhausted, consistent with the idea that UI benefits are keeping people in the labor force who would otherwise have abandoned their searches. However, this coefficient is only marginally significant.

6 Simulations of the Effect of Unemployment Insurance Extensions

The results in Tables 2 – 5 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. But 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.

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

²²The 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.

data are inconsistent with the cross-sectional duration profile. Figure 6 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 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 0.²³ 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 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).

The Figure indicates that the Kaplan-Meier survival curves are both 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

²³In practice, the unemployment duration measure is in weeks, where the CPS sample is monthly. For Figure 6, 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.

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.

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.²⁴ 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 $n - 1$

²⁴Of course, it is possible that the Kaplan-Meier curve (perhaps even the unadjusted one) is correct and that the cross-sectional curve overstates survival due to misreporting of durations. However, external evidence suggests that this is unlikely. For example, the UI system tabulates the number of individuals who exhaust their (regular program) benefits each month; the implied exhaustion rates are much more nearly consistent with the cross-sectional survival curve than with the Kaplan-Meier curve.

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, 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 alternative 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 $\hat{E}(n) \equiv u(n) - u(0) \prod_{t=0}^{n-1} p(t)$, then use (8) to construct a counterfactual unemployment count

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

Note that $\hat{E}(n)$ is simply the vertical distance between the solid and long-dashed lines in

Figure 6, evaluated at duration n .

The 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 entrants are composed of people completing a cycle 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).²⁵

Figure 7 presents the two counterfactual simulations of the number of unemployed, using the model from Table 3, 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. Counterfactual approach 2 offers only a slightly different conclusion, suggesting that the UI extensions increased unemployment in 2010 and early 2011 by about 2.5%.

Table 6 presents more results from the simulations, using three specifications of the exit hazard regression to examine aggregate unemployment and the long-term unemployment share in January 2011.²⁶ The first specification is the one graphed in Figure 7, using a cubic in the state unemployment rate to absorb endogeneity in the availability of extended UI benefits. The second specification is the multinomial logit model from Table 5, which models the effect of UI as operating through the number of weeks until exhaustion. Finally, I use the state-by-month fixed effect specification from column 6 of Table 2, identified from comparisons between job-losers and job-leavers, which showed the largest estimated effects of UI durations of any of the specifications examined in Section 5.

The estimates indicate that UI extensions raised the number of unemployed in January 2011 by between 87,000 and 978,000 and the long-term unemployment share by between 0.5 and 3.0 percentage points. In each case the largest estimates come from counterfactual method 2 and the state-by-month fixed effects specification; leaving these out, the upper end

²⁵State-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.

²⁶My simulations focus on unemployment durations in months, and count anyone unemployed 6 months or more as long-term unemployed. This is a somewhat more generous definition than that used by BLS, as I generally include people who report being unemployed for exactly 26 weeks on the survey date where BLS does not. This accounts for the discrepancy between the baseline long-term unemployment rate in Table 5 and the published rate of 42.2%.

of the ranges are 442,000 unemployed and 2.1 percentage points of long-term unemployment. The high-end estimate corresponds to an effect of the UI extensions on the January 2011 unemployment rate of 0.6 percentage points, while the rest are in the 0.2–0.3 percentage point range with counterfactual method 2 and under 0.1 percentage points with method 1. These are much smaller effects than are indicated by the extrapolations discussed in Section 2.4.

The lower panel of Table 6 presents an alternative and more speculative set of counterfactual simulations. An important question regarding the effects in Panel B of Table 6 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. 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 6 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. 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.

Table 7 takes another approach to this question. Returning to the original counterfactuals, I simulate not just survival in unemployment but also exit into each of the alternative states. Importantly, I treat both reemployment and non-participation as absorbing states. In practice, this means that I assume that anyone who leaves unemployment for non-

participation (and remains out of the labor force for two months running) does not thereafter return and find employment within two years of the beginning of the initial unemployment spell. I also maintain the assumption of the multinomial logit model that reemployment and labor force exit are independent risks. The table shows the implied distribution across the two absorbing states in January 2011 for the cohort that entered unemployment in the first quarter of 2009. (In both the actual data and the counterfactuals, less than 1% of this cohort remains unemployed at this point.) Both sets of parameter estimates indicate that the UI extensions *raised* the share of unemployment entrants who ultimately found their way to employment, by 1.2–1.3 percentage points, and reduced the share who exited the labor force.

These 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, one might not want to take the positive effect of UI extensions on reemployment at face value. Even so, it is clear that any negative 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 reduced search effort among the unemployed may be relatively small.²⁷ 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

²⁷See, 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.

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 0.3 percentage points, implying that the vast majority of the increase in the unemployment rate was due to demand shocks rather than to UI-induced supply reductions. Moreover, less than half of the small UI effect comes from reduced reemployment rather than from reduced non-participation (i.e., from *increased* labor supply). Some simulations even suggest that the availability of extended benefits might have raised reemployment rates of displaced workers, by keeping them from abandoning their searches prematurely.

Any negative effects of the recent unemployment insurance extensions on job search are

clearly quite small, too small to outweigh the benefits of transfers to people who have been out of work for over a year in conditions where job-finding prospects are bleak (Gruber, 1997). Moreover, all of the estimates herein are partial equilibrium; I do not account for congestion on the supply side of the labor market. 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, particularly at times when jobs are rationed (Landais et al., 2010). The evidence here thus supports the view that optimal UI program design would provide for generous extensions of benefit durations in deep recessions that last until the labor market is strong enough to give displaced workers a realistic chance of finding new employment before their benefits expire.

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Appendix: Proofs of propositions

All proofs are by induction.

Proof of Proposition 1. First, note that $\frac{\partial V_U(s,0)}{\partial s} = \frac{\partial V_U(s,1)}{\partial s}$ for all s . Thus, $s_1^* = s_0^*$ and $V_U(1) - V_U(0) = u(b) - u(0) > 0$. Second, assume $V_U(x) > V_U(x-1)$ for some x . Then

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

Note that the inequality here follows from the definition of $s_{x+1}^* \equiv \arg \max V_U(s, x+1)$. \square

Proof of Proposition 2. See above for $s_1^* = s_0^*$. For $d \geq 1$, s_d^* is defined by $p'(s_d^*) = \frac{1}{\delta(V_E - V_U(d-1))}$. Proposition 1 thus implies that $p'(s_{d+1}^*) < p'(s_d^*)$, so $p''(s) < 0$ implies $s_{d+1}^* > s_d^*$. \square

Proof of Proposition 3. Let $s_d^{**} = \arg \max \tilde{V}_U(s, d)$ and let $\eta_d = 1(s_d^{**} \geq \theta)$. I show that $\eta_{d+1} = \eta_d$ for all $d > 0$.

Begin by considering the case where $\eta_0 = 1$ and $s_0^{**} > \theta$. Then an argument identical to that above implies that the search requirement is never binding: $s_1^{**} = s_0^{**}$ and for all $x > 0$, $\tilde{V}_U(x+1) - \tilde{V}_U(x) > 0$, $s_{x+1}^{**} > s_x^{**}$ and $\eta_x = 1$.

Next, suppose that $\eta_d = 0$ for some $d \geq 1$, but $\eta_{d+1} = 1$. Without loss of generality, assume that $\eta_x = 0$ for all $0 \leq x \leq d$. (This merely means that we have chosen the smallest d such that $\eta_{d+1} = 1$.) This implies that

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

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 $s_{d+1} = s_d^* < \theta$ 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

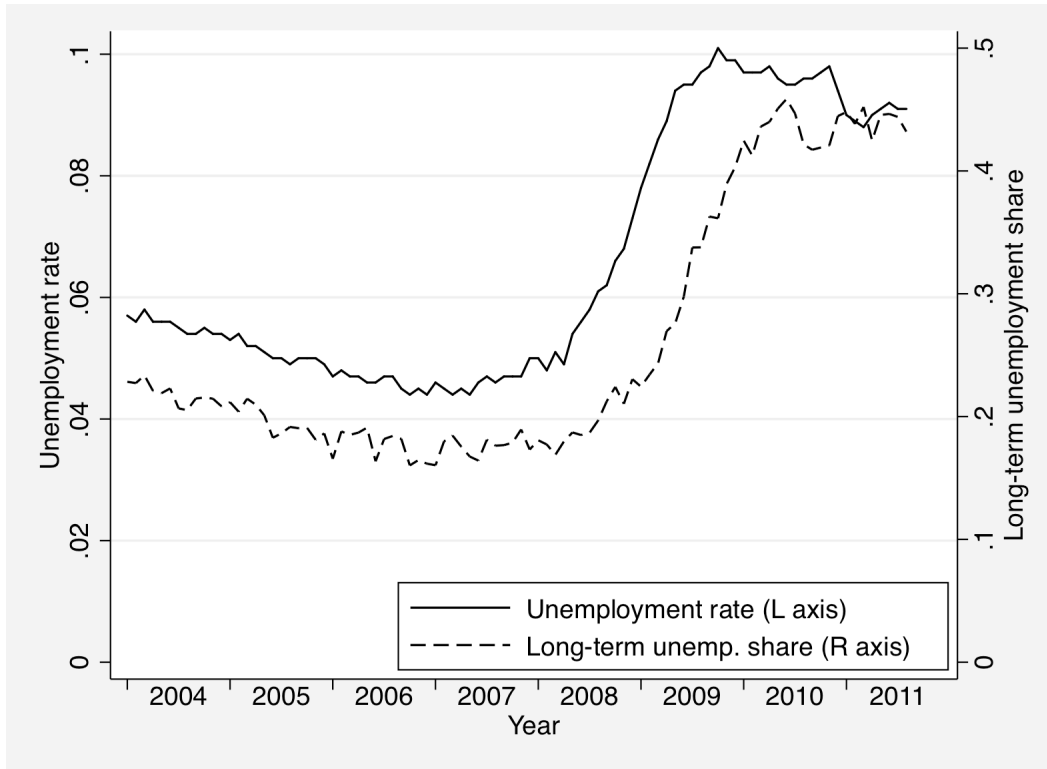
$$\begin{aligned} \tilde{V}_U(d) &< \tilde{V}_U(d+1) \\ &= \tilde{V}_U(s_{d+1}^{**}, d+1) \\ &= u(b) - s_{d+1}^{**} + \delta \left[p(s_{d+1}^{**}) V_E + (1 - p(s_{d+1}^{**})) \tilde{V}_U(d) \right] \\ &= \tilde{V}_U(s_{d+1}^{**}, d) + \delta(1 - p(s_{d+1}^{**})) (\tilde{V}_U(d) - \tilde{V}_U(d-1)) \\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

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

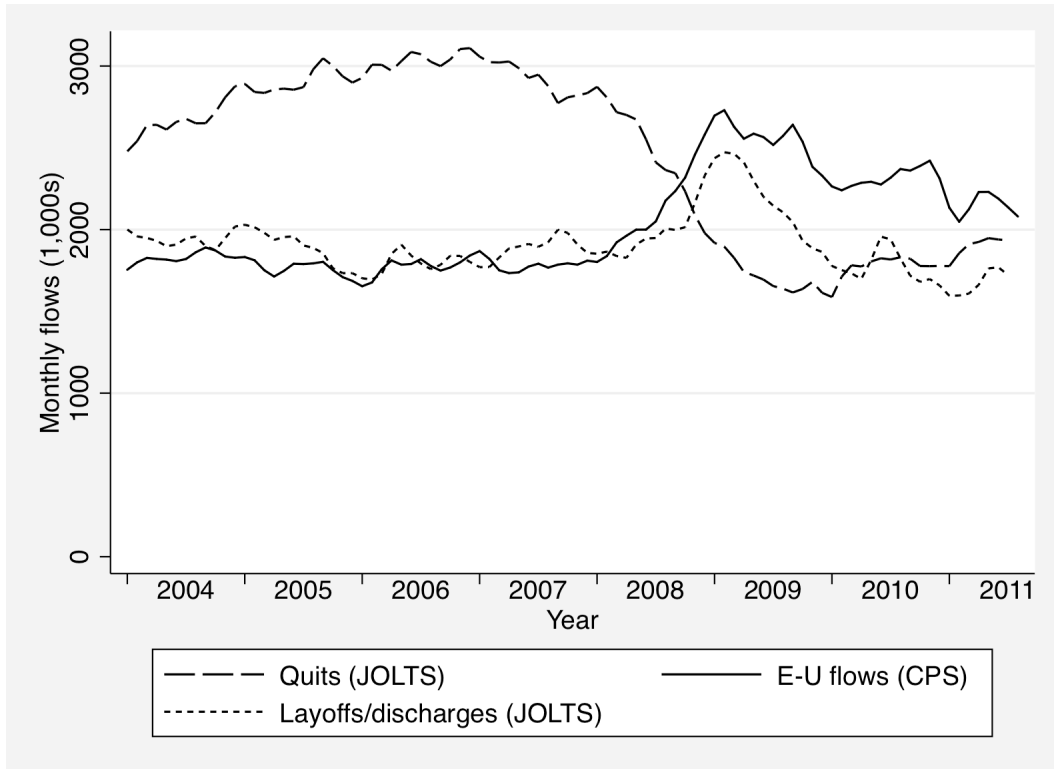
occur only when benefits are exhausted; benefit extensions will delay these transitions for those who would otherwise have exhausted their benefits. \square

Figure 1. Unemployment and long-term unemployment



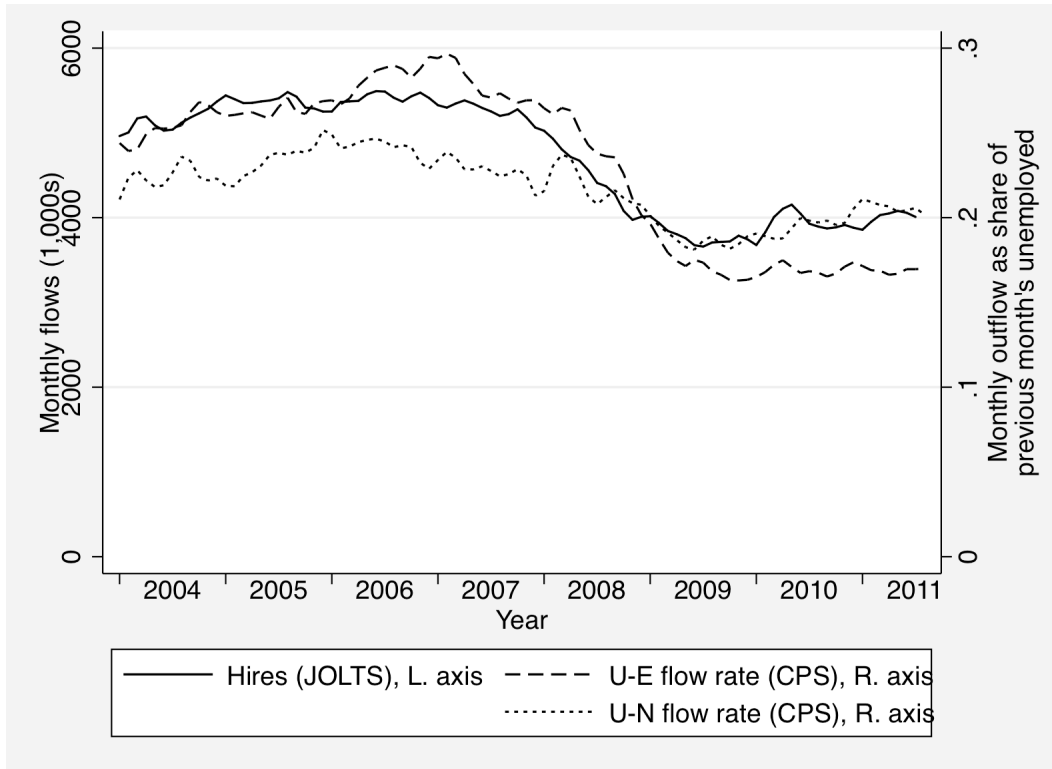
Notes: The long-term unemployment share is the fraction of the unemployed who have been unemployed for 6 months or more. Both series are seasonally adjusted.

Figure 2A. Monthly flows out of jobs



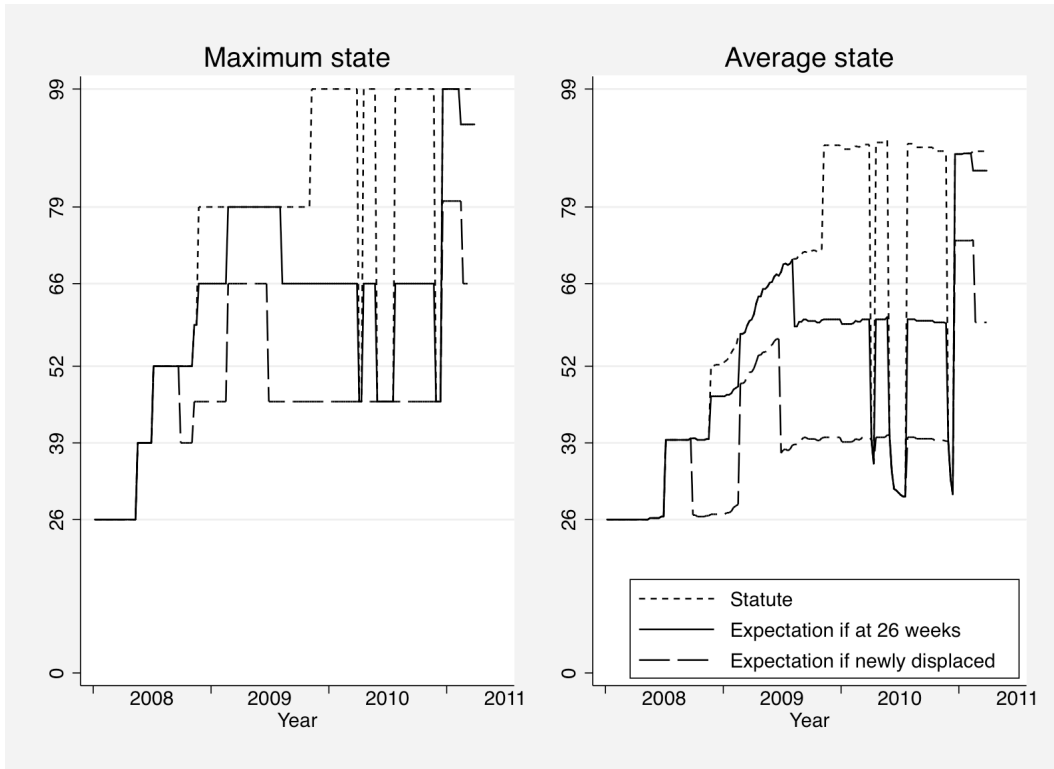
Notes: Quits and layoffs/discharges come from the JOLTS data, which derive from employer surveys. Employment-to-unemployment (E-U) flows are taken from the research series on labor force status flows constructed by the Bureau of Labor Statistics from longitudinally linked monthly CPS files. All series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

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



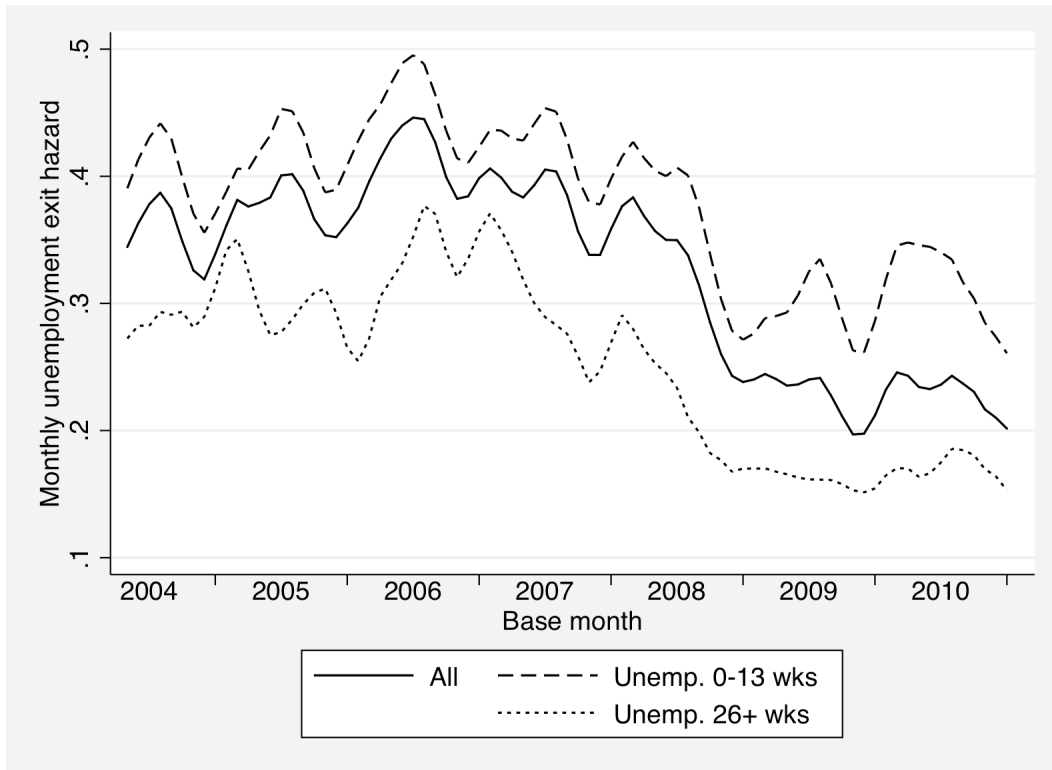
Notes: Hires come from the JOLTS data, which derive from employer surveys. Unemployment-to-employment (U-E) and unemployment-to-nonparticipation (U-N) flows are taken from the research series on labor force status flows constructed by BLS from longitudinally linked monthly CPS files and are expressed 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. UI benefit durations, statutory and as perceived by recipients



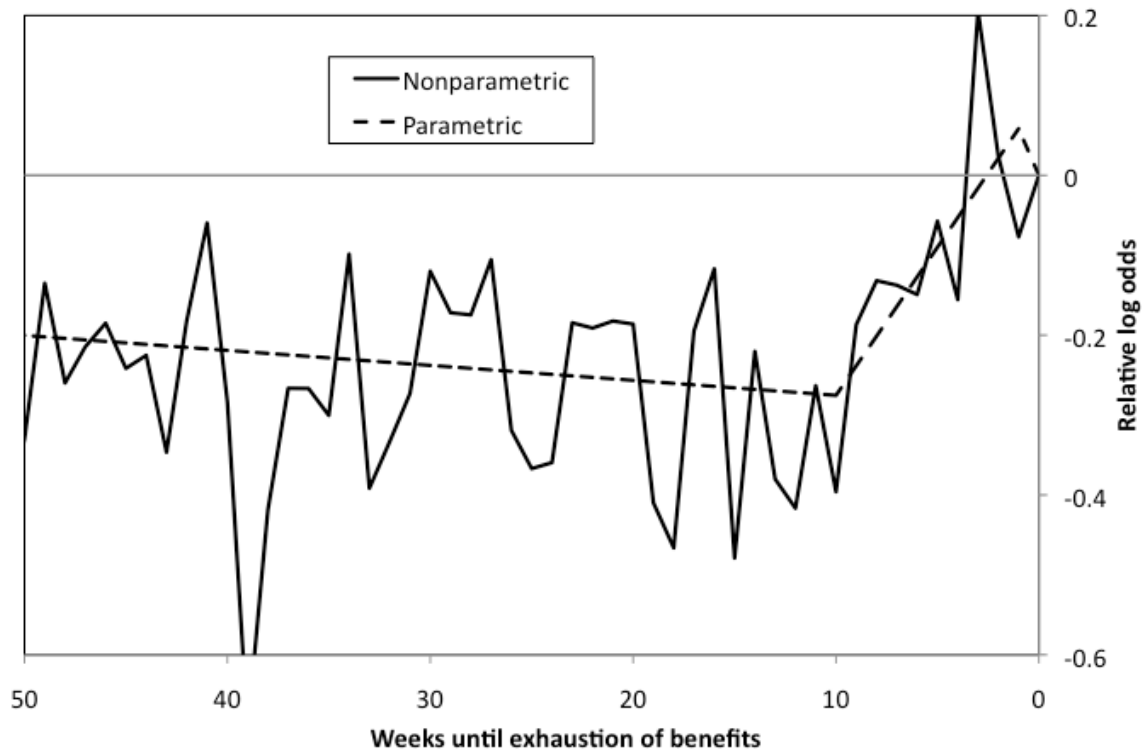
Notes: Expectations are those of recipients who do not anticipate further legislative changes (including extensions of the EUC program) or state trigger events. "Maximum state" is the state with the highest value in a given month. "Average state" is the unweighted mean across states.

Figure 4. Monthly unemployment exit hazards for displaced workers, by duration group



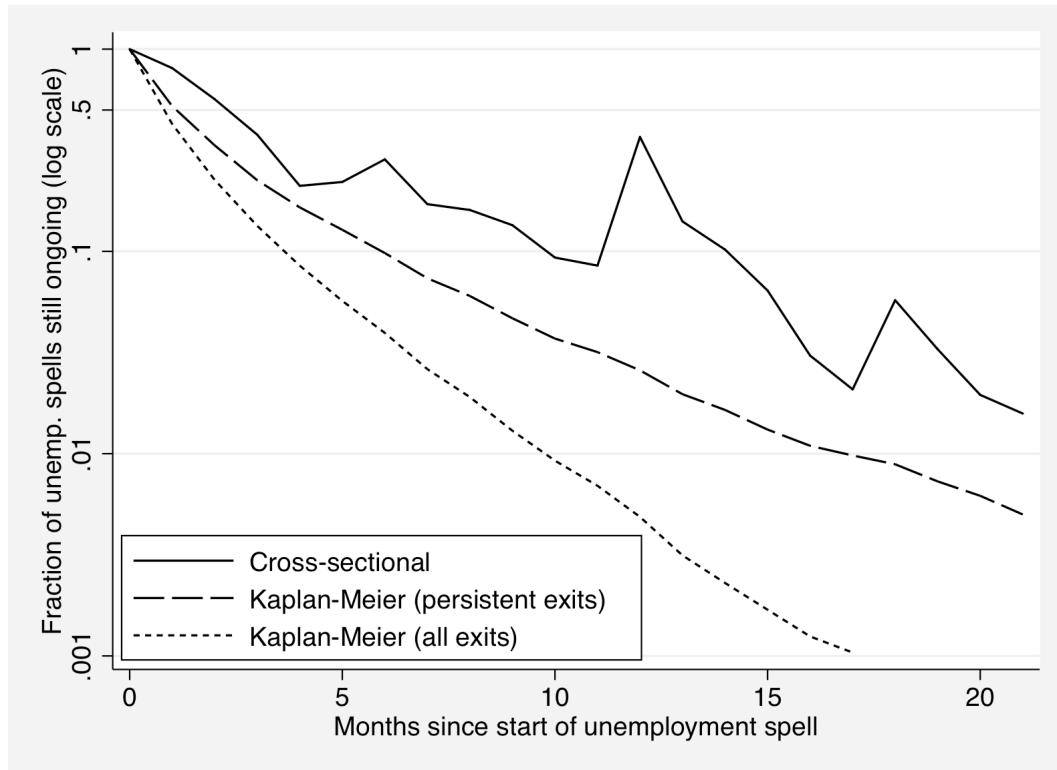
Notes: Hazards represent the probability of being employed or out of the labor force one month later and not unemployed the following month. Series are not seasonally adjusted and are smoothed using a 5-month symmetric triangle moving average.

Figure 5: Parametric vs. nonparametric specifications of the time-to-exhaustion effect



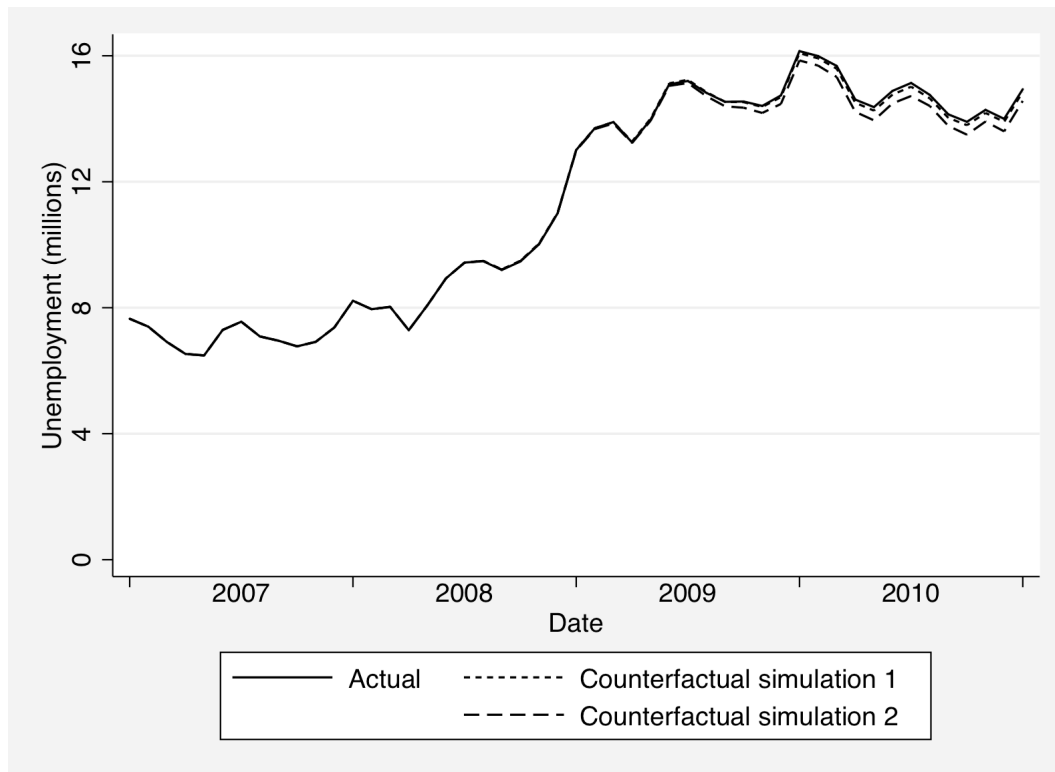
Notes: “Parametric” series graphs time-to-exhaustion effect as indicated by coefficients of Table 5, row 1. “Nonparametric” series replaces the three time-to-exhaustion measures from that specification with a full set of time-to-exhaustion indicators.

Figure 6: Alternative survival curves from cross-sectional and longitudinally-linked data.



Notes: Figure refers to unemployment spells beginning in 2008. 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 . 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 7. Actual unemployment and counterfactual simulations without UI extensions



Notes: Counterfactual simulations are based on the specification in Table 3, Column 2. See text for details.

Table 1. Summary statistics

	All unemployed		Subsample with 2+ follow-up interviews	
	Job leavers / entrants /		Job leavers / entrants /	
	Job losers	reentrants	Job losers	reentrants
	(1)	(2)	(3)	(4)
N	95,485	77,913	77,815	61,118
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%
Number of weeks of unemployment benefits				
Statutory	53.6	--	54.0	--
Expected (total)	43.9	--	44.2	--
Expected (remaining)	24.1	--	24.0	--
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 either of the two following interviews, or with allocated unemployment duration in the base month.

Table 2. Logit models for monthly unemployment exit

	Sample is job-losers					Sample is all unemp.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Constant effect of UI across all durations						
# of weeks of UI benefits (/100)	-0.27 (0.10)	-0.31 (0.10)	-0.34 (0.10)	-0.34 (0.10)	-0.24 (0.21)	-0.57 (0.07)
Effect of UI extensions on average exit hazard in 2010:Q4	-1.7 p.p.	-1.9 p.p.	-2.2 p.p.	-2.1 p.p.	-1.5 p.p.	-3.7 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	Y
State unemployment rate	linear	cubic	cubic	cubic	cubic	
State insured unemp rate			cubic	cubic		
State new UI claims rate			cubic	cubic		
State employment growth rate				cubic		
EUC weeks available					Y	
EB trigger controls					Y	
State-by-month FEs						Y
Job loser indicator						Y
Unemployment duration X job loser						Y
Unemployment rate X job loser						Y
Panel B: Allowing effect to vary by individual unemployment duration						
Weeks of benefits (/100) X unemployed < 26 weeks	0.20 (0.15)	0.13 (0.15)	0.09 (0.14)	0.10 (0.14)	-0.06 (0.20)	-0.45 (0.12)
Weeks of benefits (/100) X unemployed 26+ weeks	-0.30 (0.10)	-0.34 (0.09)	-0.36 (0.09)	-0.36 (0.09)	-0.56 (0.25)	-0.64 (0.09)
Effect of UI extensions on average exit hazard in 2010:Q4	-1.0 p.p.	-1.3 p.p.	-1.5 p.p.	-1.4 p.p.	-2.7 p.p.	-3.9 p.p.

Notes: N = 77,815 in columns 1-5 and 138,898 in column 6. 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. See text for description of the EB trigger controls (column 5). Specifications in columns 1-5 use the CPS sample weight, while column 6, which is estimated by conditional logit, uses the average CPS weight in the state-month cell. All standard errors are clustered at the state level.

Table 3. Multinomial logit models for reemployment and labor force exit versus continued unemployment

	Sample is job-losers				
	(1)	(2)	(3)	(4)	(5)
Reemployment					
Weeks of benefits (*100) X unemployed < 26 weeks	0.32 (0.18)	0.24 (0.19)	0.16 (0.19)	0.17 (0.19)	0.06 (0.31)
Weeks of benefits (*100) X unemployed 26+ weeks	-0.37 (0.13)	-0.42 (0.14)	-0.46 (0.14)	-0.46 (0.14)	-0.59 (0.36)
Labor force exit					
Weeks of benefits (*100) X unemployed < 26 weeks	-0.08 (0.21)	-0.12 (0.21)	-0.12 (0.22)	-0.12 (0.22)	-0.26 (0.29)
Weeks of benefits (*100) X unemployed 26+ weeks	-0.31 (0.13)	-0.34 (0.13)	-0.35 (0.14)	-0.35 (0.14)	-0.52 (0.35)
Effect of extensions on average hazards in 2010:Q4					
Reemployment	-0.3 p.p.	-0.5 p.p.	-0.6 p.p.	-0.6 p.p.	-1.0 p.p.
Labor force exit	-0.9 p.p.	-1.0 p.p.	-1.0 p.p.	-1.0 p.p.	-1.6 p.p.

Notes: Samples and specifications are as in the corresponding columns of Table 2, Panel B, using a multinomial logit for a trichotomous outcome (unemployment, employment, or not in labor force) in place of Table 2's logits for dichotomous outcomes (unemployment versus non-unemployment). 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 4. Alternative specifications

	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)
(1) Baseline	13.4%	-0.5 p.p.	9.0%	-1.0 p.p.
(2) Add indiv. covariates	13.4%	-0.5 p.p.	9.0%	-1.2 p.p.
(3) Separate effect on 26 wks	13.4%	-0.5 p.p.	9.0%	-1.0 p.p.
(4) Drop round number durations	12.8%	-0.5 p.p.	7.9%	-1.5 p.p.
(5) Count all UE exits	16.5%	-0.6 p.p.	13.7%	-1.3 p.p.
Subsamples				
(6) Age 25-54	14.1%	-1.1 p.p.	7.5%	-1.8 p.p.
(7) Age 55+	9.9%	1.2 p.p.	9.8%	0.6 p.p.
(8) Const./manuf.	13.7%	0.4 p.p.	7.5%	-2.2 p.p.
(9) All other industries	13.3%	-0.9 p.p.	9.6%	-0.4 p.p.

Notes: Baseline specification in row 1 is that from column 2 of Table 3. Row 2 adds controls for gender, marital status, gender*married, five age groups, three education categories, and 12 pre-displacement industries. Row 3 adds an indicator for durations of exactly 26 weeks and its interaction with number of weeks of UI benefits available. Row 4 drops observations whose unemployment duration at the beginning of the spell or the first CPS interview was 26, 52, or 78 weeks. 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 4-7 use the baseline specification on different subsamples; the hazards correspond to average hazards for the subsamples.

Table 5. 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.) (4)
	(1)	(2)	(3)	
Logit for unemployment exit				
(1) Logit for unemployment exit with state, month, UR controls	0.10 (0.07)	-0.37 (0.10)	0.39 (0.10)	-1.4 p.p.
(2) Logit for unemployment exit with state-by-month controls	0.07 (0.08)	-0.34 (0.11)	0.36 (0.12)	-1.5 p.p.
(3) Multinomial logit with state, month, UR controls				
Reemployment	-0.06 (0.11)	-0.31 (0.13)	0.35 (0.14)	-0.6 p.p.
Labor force exit	0.19 (0.10)	-0.37 (0.12)	0.36 (0.13)	-0.7 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 and in months thereafter, 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 laws and triggers in effect at the time of the baseline survey.

Table 6. Simulated aggregates in January 2011 without UI extensions

	Unemployment			Long-term unemp. share		
	Level	Effect of UI		Level	Effect of UI	
	(1,000s)	extensions		(%)	extensions	
		Level	%		p.p.	%
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Baseline						
Actual in January 2011	14,937			45.5%		
Panel B: Counterfactuals, allowing for UI effects on reemployment and labor force exit						
Table 3, column 2						
Counterfactual method 1	14,850	+87	+0.6%	45.0%	+0.5	+1.2%
Counterfactual method 2	14,567	+370	+2.5%	43.9%	+1.6	+3.7%
Table 5, row 3						
Counterfactual method 1	14,798	+140	+0.9%	44.6%	+0.9	+2.0%
Counterfactual method 2	14,496	+442	+3.0%	43.4%	+2.1	+4.8%
Table 2, column 6, panel B						
Counterfactual method 1	14,514	+424	+2.9%	44.6%	+0.9	+2.0%
Counterfactual method 2	13,959	+978	+7.0%	42.5%	+3.0	+7.1%
Panel C: Counterfactuals, allowing for UI effects only on labor force exit						
Table 3, column 2						
Counterfactual method 1	14,840	+98	+0.7%	45.2%	+0.3	+0.7%
Counterfactual method 2	14,673	+264	+1.8%	44.6%	+0.9	+2.1%
Table 5, row 3						
Counterfactual method 1	14,830	+107	+0.7%	45.1%	+0.4	+0.8%
Counterfactual method 2	14,693	+244	+1.7%	44.6%	+0.9	+2.0%

Notes: Counterfactuals show simulated outcomes if benefit durations were fixed at 26 weeks throughout the 2004-2011 period, constructed from coefficients from the indicated specifications. Counterfactual methods differ in their treatment 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.

Table 7. Simulated counterfactual outcomes in January 2011 for cohort entering unemployment in 2009:Q1

	Share reemployed			Share out of labor force		
	Level (%)	Effect of UI extensions		Level (%)	Effect of UI extensions	
		p.p.	%		p.p.	%
	(1)	(2)	(3)	(4)	(5)	(6)
Table 3, column 2						
Baseline	67.8%			31.1%		
Counterfactual method 1	66.4%	+1.3	+2.0%	32.9%	-1.8	-5.3%
Table 5, row 3						
Baseline	68.4%			30.4%		
Counterfactual method 1	67.2%	+1.2	+1.7%	32.0%	-1.6	-5.1%

Notes: Columns 1 and 4 do not add to 100%, as a small share of spells beginning in 2009:Q1 continued through January 2011.