**Essays on the Macroeconomics of Labor Markets**

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Essays on the Macroeconomics of Labor Markets

A dissertation presented

by

John Coglianese

to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Political Economy and Government

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Essays on the Macroeconomics of Labor Markets

Abstract

Chapter 1 documents that much of the decline in labor force participation of U.S. prime age men comes from “in-and-outs”—who I define as men who temporarily leave the labor force. Individuals moving in and out of the labor force have been an understudied margin of labor supply but account for at least one fifth of the decline in participation between 1977 and 2015. Most in-and-outs are in the labor force for the large majority of their career, but take an occasional short break in between jobs. In-and-outs are distinct from unemployed individuals, experiencing no loss of future income as a result of their time out of the labor force, and represent a distinct margin of labor supply from long-term labor force dropouts. Examining explanations for the rise of in-and-outs, I find little evidence to suggest that changes in labor demand are responsible.

Chapter 2 investigates whether changes in household structure—specifically the type of household and composition of household labor supply—can explain part of the decline in labor force participation among prime age men in the U.S. Focusing on the decline in participation due to in-and-outs documented in Chapter 1, I find that half of the decline has come from married or cohabiting men, and I show that this portion of the decline can be explained by a wealth effect from their partners’ growing earnings, using variation in the growth of female wages across demographic groups. Additionally, I find that changes in household structure, particularly from young men increasingly living with their parents, account for much of the rest of the decline in participation. To examine both effects within a unified framework, I construct and estimate a dynamic model of labor supply and household formation. The model estimates imply that labor supply factors are responsible for nearly the entire decline in participation due to in-and-outs, while changes in labor demand have contributed little.

Chapter 3 is based on joint work with Gabriel Chodorow-Reich and Loukas Karabarbounis. We ask by how much does an extension of unemployment benefits affect macroeconomic outcomes such as unemployment? Answering this question is challenging because U.S. law extends benefits for
states experiencing high unemployment. We use data revisions to decompose the variation in the
duration of benefits into the part coming from actual differences in economic conditions and the part
coming from measurement error in the real-time data used to determine benefit extensions. Using
only the variation coming from measurement error, we find that benefit extensions have a limited
influence on state-level macroeconomic outcomes. We apply our estimates to the increase in the
duration of benefits during the Great Recession and find that they increased the unemployment rate
by at most 0.3 percentage point.
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Chapter 1

The Rise of In-and-Outs: Declining Labor Force Participation of Prime Age Men

1.1 Introduction

In 1960, more than 97% of American men between the ages of 25 and 54 were either employed or actively looking for work. By 2015, this rate had fallen to 88%, representing nearly 5.5 million fewer prime age workers in the labor force at any point in time. This declining trend has motivated a growing body of work seeking to understand why men are leaving the labor force.¹

In this paper, I document that participation has changed along an understudied margin of labor supply. I find that “in-and-outs”—men who temporarily leave the labor force—represent a growing fraction of prime age men across multiple data sources and are responsible for roughly one third of the decline in the participation rate since 1977. In-and-outs take short, infrequent breaks out of the labor force in between jobs, but they are otherwise continuously attached to the labor force. Leading explanations for the growing share of permanent labor force dropouts, such as disability, do not apply to in-and-outs. Instead, evidence indicates that the rise of in-and-outs reflects a shift in labor supply, largely due to a wealth effect as evidenced by rising consumption at constant wages.

¹Examples include Juhn (1992; 2003), Moffitt (2012), CEA (2016), and Eberstadt (2016).
Together, these facts paint a different picture of declining labor supply among prime age men than documented previously.

The rise of in-and-outs is not apparent from measures of the participation rate taken at a single point in time, resulting in prior work overlooking this margin of labor supply. If the participation rate falls by one percentage point, it could have been driven by one percent of men leaving the labor force permanently, or two percent spending half of their time out of the labor force, or twelve percent deciding to take one month of the year to spend out of the labor force. It is important to distinguish between these scenarios because they each may have different implications for inequality, human capital accumulation, and policy responses.

I show that the rise of in-and-outs is a robust phenomenon observed since the 1970s across a number of different longitudinal data sources. In the Survey of Income and Program Participation (SIPP), the share of men who are out of the labor force in any given month rose 5 p.p., with 1 p.p. of this increase coming from uncensored nonparticipation spells lasting less than two years. Similar increases in temporary nonparticipation are evident in other longitudinal datasets. However, these represent lower bounds on the true rise of in-and-outs, since many short spells of nonparticipation are either missed due to time aggregation or have censored duration. Additionally, retrospective data sources inaccurately estimate the number of in-and-outs, as measurement error in retrospective collection is growing over time, possibly due to recall problems or increased rounding by respondents.

To estimate the total change in labor supply resulting from the rise of in-and-outs, I use a nonparametric Bayesian model applied to participation dynamics in the spirit of Heckman and Willis (1977). Treating the labor force histories of individuals as arising from a mixture of continuous-time markov chains, I estimate the model separately for prime age men born in 1945–49 and 1965–69 combining data from the SIPP, CPS, and PSID. The model estimates imply that the rise of in-and-outs has resulted in an increase in the share of men who are in the labor force for the large majority of their careers, but spend between one and four years out of the labor force during ages 25–54.

In-and-outs are different from unemployed individuals in a number of ways. First, in-and-outs are not actively searching for work while out of a job. In-and-outs’ consumption dynamics are similar to retirees’, and do not experience as large of a decline after a job separation as that of unemployed individuals. Additionally, in-and-outs experience no decline in future income relative to their peers as a result of their time out of the labor force, in contrast to the large long-run costs faced by unemployed individuals.
The rise of in-and-outs appears to be a distinct margin of labor supply from changes in permanent dropouts. The two groups cite different reasons for leaving the labor force, report different qualities of life, and show up in different regions. While some in-and-outs could turn into dropouts, the distinctions between these two groups make it more natural to treat them as separate components of the participation decline, with separate explanations, rather than intertwined aspects of the same phenomenon. Additionally, some of the most common explanations that have been put forward for the growth of permanent dropouts, such as disability insurance or incarceration, cannot explain the rise of in-and-outs.

In-and-outs do not represent men switching between market-sector work and home production. While out of the labor force, in-and-outs replace time spent working with leisure activities, primarily watching television. In-and-outs do not spend much more time on child care, care for adults, educational activities, or job search during these breaks.

Next, I turn to understanding the forces responsible for the rise of in-and-outs more formally. In particular, I differentiate between explanations related to changes in labor demand, reflected in diminished market opportunities, from changes in labor supply, representing lower participation holding market opportunities constant.

I find that changes in men’s market opportunities can explain little of the rise of in-and-outs. Since in-and-outs are in the labor force at least some of the time, I can directly measure the market opportunities available to them and examine how they have changed. While factors such as automation and offshoring have reduced market opportunities for some prime age men, average real wages have actually risen slightly since 1977, casting doubt on this explanation. Furthermore, I show that even among groups that have experienced wage declines, such as less-educated men, these declines are not nearly large enough to explain the rise of in-and-outs using conventional labor supply elasticities from the literature. In-and-outs have risen across all industries and occupations, suggesting this phenomenon is not limited to particular types of jobs. Additionally, even though changes in labor demand could directly affect employment in the short run (as is the case if wages are rigid), these effects appear to be temporary and therefore cannot explain the long-term downward trend of participation.

This paper relates to several literatures. First, the pattern of changing participation among prime

---

2 This provides a large advantage for studying the growth of in-and-outs as opposed to dropouts, since potential wages are unobserved for the latter group. As such, it is unknown whether dropouts’ potential wages have increased or decreased appreciably over the last several decades.
age men documented in this paper contrasts with several prior examinations of the change in prime age male participation. Juhn (1992; 2003), Juhn et al. (1991; 2002), and Moffitt (2012) conclude that the decline in participation was driven almost entirely by an increase in dropouts, based on evidence from the March CPS, and they attribute this to changing market opportunities. Elsby and Shapiro (2012), Autor, Dorn and Hanson (2013), Autor and Wasserman (2013), and CEA (2016) also point to reduced market opportunities for prime age men explaining some of the decline in participation. The availability of disability insurance has been highlighted by Autor and Duggan (2003), Eberstadt (2016), and Winship (2017) as a potential explanation for the decline. Schmitt and Warner (2011) and Eberstadt (2016) also point to the rising population of men with a criminal record as a factor in lowering the share of men working. Aguiar, Bils, Charles and Hurst (2017) focus on young men and show that improvements in video game technology may account for much of the decline in participation among this group. Krueger (2017) shows that the decline in participation among prime age men may be related to the opioid crisis, as participation has fallen more in areas with higher rates of opioid prescriptions.

Documenting the rise of in-and-outs also provides evidence for a key margin of adjustment for aggregate labor supply. In-and-outs rising in response to a decrease in desired labor supply is a key prediction of the time-averaging aggregation theory of Mulligan (2001) and Ljungqvist and Sargent (2007), who introduce this approach to reconcile large aggregate labor supply elasticities with small micro elasticities. In contrast, prior theories of labor supply aggregation point more towards labor supply adjusting along the dropout margin (Hansen, 1985; Rogerson, 1988). Additionally, the rise of in-and-outs documented in this paper contributes to the discussion of homogeneity versus heterogeneity in participation rates dating back to Heckman and Willis (1977). While Goldin (1989) shows that the rapid growth of female labor supply during the 20th Century reflects heterogeneity in participation rates, the analysis presented in this paper shows that the decline of male labor supply in substantial part reflects homogeneity (i.e. in-and-outs).

The rise of in-and-outs also stands in contrast to a literature on declining dynamism in US labor markets. Previous studies have noted declines in job flows (Davis and Haltiwanger, 2014), geographic mobility (Molloy, Smith and Wozniak, 2011), short-term jobs (Hyatt and Spletzer, 2017), and startup rates (Haltiwanger, Jarmin and Miranda, 2013), among other measures. However, the evidence in this paper shows that an understudied dimension of fluidity—cycling in and out of the labor force—has been growing over the same time period.
The remainder of the paper proceeds as follows. Section 2 describes how I measure the rise of in-and-outs. Section 3 compares in-and-outs to several other types of nonemployment—unemployed individuals, permanent dropouts, and workers engaged in home production. In Section 4, I describe how labor demand and supply forces affect in-and-outs. Section 5 examines explanations for the rise of in-and-outs. Section 6 concludes.

1.2 Measuring the Rise of In-and-Outs

This section documents the rise of in-and-outs across several different data sources. I begin by showing that temporary nonparticipation has risen in longitudinal datasets, but not in retrospective data sources. The latter appear to be suffering from increasing bias over time, resulting in these divergent measures. However, the growth of temporary nonparticipation provides only a lower bound for the rise of in-and-outs. To estimate the total rise of in-and-outs, I use a nonparametric model consisting of a mixture of continuous-time markov chains estimated from labor force histories across several datasets. The model estimates imply that in-and-outs have resulted in marginal declines in lifetime labor supply, as most in-and-outs are in the labor force for the majority of their careers with only occasional breaks out of the labor force.

1.2.1 Growth of Temporary Nonparticipation

I define in-and-outs as individuals who are short-term or temporary nonparticipants. This definition involves two components.

First, the individual must be out of the labor force, meaning neither working nor actively searching for work. Accordingly, in-and-outs are a distinct group from the unemployed, who are actively searching for work, although both groups are jobless. This distinction may seem trivial since neither group is employed, but I show in Section 1.3.1 that in-and-outs behave differently from the unemployed across a number of dimensions while out of work.

Second, in-and-outs are out of the labor force only temporarily, which I define as less than two years at a time. This definition separates in-and-outs from individuals who are persistently out of the labor force for several years or more, whom I term dropouts. Dropouts have also grown over the last several decades, but appear to be qualitatively distinct from in-and-outs on many dimensions (see Section 1.3.2 for more). I choose two years out of the labor force as the dividing line between these
Temporary nonparticipation has risen dramatically over the last several decades, as shown in Figure 1.1. While only 1.5% of the prime age male population was out of the labor force temporarily
in 1984, this share had risen to 2.9% by 2010. Given that there were nearly 62 million prime age men in the US in 2010, the increase in this share represents about 1 million additional men out of work at any point in time due to this margin alone. Since many of these spells are very short, with the average spell lasting four months or less, many men must be taking such breaks to produce such a large number of men out of work at any point in time.

However, the share of temporary nonparticipation in this dataset represents a lower bound on the total rise of in-and-outs. Some nonparticipation spells begin before the sample period starts or end after sampling has ended, such that the true duration of these spells is unobserved in the dataset. To be conservative, I classify all of these spells as “Censored” and count them neither as in-and-outs nor dropouts. Many may be in-and-out spells, since these spells are more numerous at any single point in time, but the exact magnitude cannot be determined without additional parametric restrictions, which I defer to Section 1.2.2.

**Robustness**

This section repeats the exercise above with three additional panel datasets: the Current Population Survey (CPS), the Panel Study of Income Dynamics (PSID), and the Social Security Administration (SSA) Earnings Public-Use File. Each of these datasets uses a slightly different measurement frequency and panel length, meaning that it is not straightforward to compare the magnitudes of temporary nonparticipation across datasets. However, it is possible to compare whether temporary nonparticipation is increasing or decreasing across datasets. The details of data construction for each of these datasets are described in Appendix A.2.1.

The growth of temporary nonparticipation is a robust phenomenon evident across all of these datasets. Table 1.1 shows the fraction of the prime age male population that was out of the labor force temporarily for each of the four longitudinal datasets I consider in columns 1-4. All of these datasets show an increase in temporary nonparticipation, although the magnitudes are not directly comparable since they measure labor force status at different frequencies and for different lengths of time. Similar to the increase in the SIPP, temporary nonparticipation approximately doubled in the CPS from 0.9% in 1980 to 1.8% in 2015. The PSID experienced an increase from 1.6% in 1975 to 2.9% in 1995, and a subsequent measurement using biennial data suggests this had risen to 4.5% by 2005.

In the SSA dataset, temporary nonemployment rose from 5.3% in 1960 to 6.6% in 2005. Since this dataset is constructed from earnings records alone, there is no distinction between unemployment
Table 1.1: Temporary Nonparticipation Across Datasets

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<th>Year</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
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<td>PSID</td>
<td>SSA</td>
<td>SSA (Adjusted)</td>
<td>March CPS</td>
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<td>1.8</td>
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<td></td>
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Notes: Levels of temporary nonparticipation are not directly comparable across datasets due to differences in frequency and duration of labor force status measurement. Details of each dataset’s construction are reported in Appendix A.2.1. Column 5 subtracts from column 4 the change in the natural rate of unemployment from 1960 using estimates published by CBO (2018). Data sources in columns 1-5 are all longitudinal, column 6 uses retrospective data from the March CPS.

* The observation for 2005 in the PSID comes from biennial data, unlike prior year PSID observations which use annual data.

and nonparticipation, complicating comparisons over time. As documented by Hall (1970b), Juhn et al. (1991), and Shimer (1999), the natural rate of unemployment rose from the 1960s to the 1980s, before falling afterwards. To account for this, I subtract an estimate of the natural rate of unemployment and report the resulting series in column 5. This adjusted series shows a steady increase of about the same magnitude as the unadjusted series, consistent with the rise of temporary nonparticipation being responsible for this trend. Since the SSA dataset is constructed from administrative sources, this indicates that the rise of in-and-outs is not an artifact of measurement error problems affecting household surveys.

Bias of Retrospective Sources

All of the datasets examined previously are panels that measure labor force status more or less contemporaneously, but some other data sources rely on retrospective collection of this information. In particular, the March CPS Supplement has been used previously to measure temporary nonparticipation by looking at the number of weeks an individual reported being in the labor force over the previous calendar year (see Juhn et al. 1991, 2002; Moffitt 2012). However, retrospective collection of labor force status may introduce some bias due to imperfect recall or rounding of recalled spell duration (Akerlof and Yellen, 1985).

Looking at the March CPS, one would conclude that temporary nonparticipation has decreased
over the last several decades. Table 1.1 shows that only 2.5% of prime age men reported participating in the labor force between one and fifty-one weeks in 2015, down from 3.6% in 1965. This trend was noted by Juhn et al. (1991) and Juhn et al. (2002), who concluded that permanent nonparticipation was responsible for the entire increase of nonparticipation. The trend in the March CPS stands in contrast to the other datasets, though, which suggests that either it is biased or all of the other datasets are biased.

I directly test for the presence of bias in the March CPS trend by examining the accuracy of its respondents’ answers. Since the March CPS sample is a follow-on survey from the monthly CPS, I can examine whether respondents’ retrospective reports match their contemporaneous reports during the previous year. Specifically, I match respondents to the March CPS who are in their second rotation in the monthly CPS to their four monthly responses in their first rotation, which fall in the previous year.

Two types of misreporting can be unambiguously measured in the data: a) respondents who
reported being in the labor force for all fifty-two weeks of the prior year in the March CPS, but in the monthly CPS reported participating only three out of four months or fewer, and b) respondents who reported being out of the labor force for fifty-two weeks in the March CPS, but in the monthly CPS reported participating at least one month out of four. Figure 1.2 displays the prevalence of these two types of errors from 1989-2015, showing that both types of errors have become more common. The increases in these unambiguous errors are large enough to explain most of the divergence between the March CPS and other datasets.

1.2.2 Estimating the Quantity of In-and-Outs

How many in-and-outs are there? The previous section indicates that in-and-outs are rising, but two challenges make it difficult to infer the exact magnitude from these measures. First, nonparticipation spells in some datasets may be censored, so without further assumptions it is not possible to determine whether these are temporary spells or not. Second, all of the above datasets measure labor force participation at discrete points in time and may miss short spells that begin and end in between measurements. Ideally, a dataset containing continuous labor force histories covering ages 25–54 for all individuals would address both of these problems, but unfortunately no such data source exists. Instead, I use the available data to construct an approximation to this ideal data source.

Total Participation Distribution

I begin by estimating how the distribution of individual labor force participation over ages 25–54 has changed from cohort to cohort. I combine information from datasets covering different frequencies and timespans by treating labor force participation histories in these different datasets as arising from a mixture of continuous-time markov chains. Estimating this statistical model for men in different birth cohorts allows me to infer how many more men in recent cohorts are taking short breaks out of the labor force.

Consider an individual \( i \) for whom we observe a sequence of labor force participation outcomes \( \{y_{i0}, \ldots, y_{iT}\} \). I assume that labor force participation for this individual follows a continuous-time markov chain, such that the likelihood of observing this sequence given parameters can be written as:

\[
\Pr(y_{i1}, \ldots, y_{iT}; \mu_i) = \prod_{t=1}^{T} \Pr(y_{it} | y_{i(t-1)}; \mu_i)
\]
where \( \mu_i = \begin{pmatrix} \mu_{1i} \\ \mu_{2i} \end{pmatrix} \) represents the vector of flow rates out of and into the labor force, respectively. Since this is a continuous-time markov chain, the likelihood term \( \Pr(y_{it}|y_{it-1}; \mu_i) \) is non-degenerate over any possible frequency of measurement, which is convenient when combining data of different frequencies.

Individuals may have different labor force histories either because they have different \( \mu_i \) or because they experience a different realization of the markov chain holding \( \mu_i \). For simplicity of computation, I assume that the former aspect of heterogeneity takes the form of a mixture distribution over a finite set of possibilities \( M = \{\mu_0, ..., \mu_K\} \). Specifically, let the probability of an individual \( i \) having flow rates \( \mu_k \) be given by

\[
\Pr(\mu_i = \mu_k) = \begin{cases} \theta_k & \mu_k \in M \\ 0 & \text{otherwise} \end{cases}
\]

The full likelihood of this mixture model sums the probabilities of generating each observation over each possible vector of flow rates:

\[
\Pr(y_{i1}, ..., y_{iT}) = \prod_{t=1}^{T} \sum_{k=1}^{K} \theta_k \Pr(y_{it}|y_{it-1}; \mu_k) 
\]

This mixture model imposes two strong assumptions on the data generating process: that \( \mu_i \) only has support at a finite number of discrete points, and that conditional on \( \mu_i \) labor force participation follows a markov process. The first assumption differs from the approach employed by Heckman and Willis (1977), who specify that individual participation rates \( l_i = \frac{\mu_2}{\mu_1+\mu_2} \) follow a continuous beta distribution. I use a set of discrete points since the distribution of male participation rates is substantially more skewed than the female participation rates Heckman and Willis (1977) modeled and thus not well approximated by a beta distribution. The second assumption is more restrictive, as it imposes that the individuals’ hazard rates for leaving or entering the labor force are constant over time and rules out duration dependence. While the observed pattern of declining reemployment hazard rates over the duration of nonemployment spells could be with consistent duration dependence, the same pattern can be generated by heterogeneity across individuals with no duration dependence (Ahn and Hamilton, 2017).

I estimate the model using data on individuals in different birth cohorts across several datasets. Pooling observations across the SIPP, CPS, and PSID, I create a sample of labor force histories for
men born between 1945-1949 or 1965-69 and estimate the model separately for each cohort. In choosing the number of support points for \( \mu_i \), I examined a range of options and settled on a baseline specification of seven support points; adding additional support points beyond seven does not appreciably change the results, though. I placed weakly informative priors on the parameter values in order to prevent explosive estimates (Gelman, Jakulin, Pittau, Su et al., 2008). Maximum likelihood estimation was conducted using Stan (Carpenter, Gelman, Hoffman, Lee, Goodrich, Betancourt, Brubaker, Guo, Li and Riddell, 2017).

With estimates of the mixture parameters, I construct the implied distribution of total years of labor force participation between ages 25 and 54 across individuals. For a two-state, continuous-time Markov chain, the density of this distribution has a closed form solution as derived by Pedler (1971). I compute this density for each mixture component and form an aggregate distribution using the mixture weights.

The estimated cumulative distributions for both birth cohorts are shown in Figure 1.3. For the 1945-49 cohort, close to half of the population spent no time out of the labor force between ages 25
and 54, while for the 1965-69 cohort only about 40% had no time out of the labor force. Additionally, the later cohort’s distribution shows both an increase in the share of individuals with between one and four years out of the labor force (from 32% to 38%), as well as an increase in the share with ten to twenty years out of the labor force (from 5% to 10%). As a result, the average number of years out of the labor force increased from 2.1 years for the earlier cohort to 3.1 years for the later cohort, with the average excluding those who are always in the labor force increasing from 4.3 to 5.2 years.

The growth of men out of the labor force only for a few years over their career implies that most in-and-outs are not repeatedly taking breaks out of the labor force. Instead, the rise of in-and-outs reflects growth of men taking short breaks, who are otherwise attached to the labor force for the vast majority of their careers.

**Approximation**

In this section, I describe a useful approximation to the distribution in the previous section that can be formed from CPS panel data. The share of men who are in the labor force between one and seven months out of the eight total interviews roughly lines up with the share of in-and-outs as inferred by the parametric model. This approximation will be useful when examining the heterogeneity in the rise of in-and-outs across groups and in examining which factors can explain the rise of in-and-outs.

I start with the observation that the share of men who are sometimes, but not always, in the labor force in the CPS has risen along with the rise in temporary nonparticipation documented previously. Respondents to the CPS report their labor force status for eight months over a sixteen-month period. The share of sometimes participators, i.e. men who are in the labor force between one and seven months out of eight, rose from about 6% in 1980 to more than 10% by 2015.

As this measure has grown with the rise of in-and-outs, it is worth investigating the extent to which these two measures are linked. If the share of sometimes participators corresponds to the rise of in-and-outs as measured by changes in the density of nonparticipation, then changes in the stochastic process of participation that affect one measure should similarly affect the other.

Formally, I define the strength of the relationship between the density function and the share of sometimes participators with a vector projection. For each metric, I take the gradient of the metric with respect to the underlying parameters in the mixture model to get an estimate of how this metric varies as the underlying stochastic process is perturbed. I conduct this separately for each point \( x \) on the density function (denoted \( d_x \)) and once for the sometimes participator share (denoted \( s \)).
Notes: Graph shows the similarity between points on the density function estimated in Section 1.2.2 and the sometimes participator share measured in the CPS. Similarity is computed by projecting the gradient of the former onto the gradient of the latter, as defined in equation 1.2.

The extent to which the sometimes participator share is informative about different points along the density can be measured by projecting the gradient vector of each point on the density function onto the gradient of the sometimes participator share:

$$\rho_x = \nabla d_x \cdot \frac{\nabla s}{|\nabla s|}$$  \hspace{1cm} (1.2)

Plotting the similarity measures $\rho_x$ indicates that the sometimes participator share is highly informative of the rise of in-and-outs. As shown in Figure 1.4, the sometimes participator share is most similar to the density of individuals who spend about one to four years out of the labor force in total during their prime ages. It provides little information on the density of individuals who are out of the labor force for more than ten years and is strongly negatively related to the share who never leave the labor force at all.

The sometimes participant share is an imperfect measure of the rise of in-and-outs, but it can be easily computed for any group in the CPS, including small subgroups. This aspect makes it a convenient approximation to the parametric model outlined in Section 1.2.2 that can be used to
examine the extent of the rise of in-and-outs across many dimensions of heterogeneity. For simplicity, when using this approximation in the sections that follow I refer to it as the “in-and-out share” instead of “sometimes participator share” since it nearly entirely reflects variation in in-and-outs.

1.3 Comparison to Other Forms of Nonemployment

This section examines how in-and-outs compare to other types of nonemployed workers. Although in-and-outs have been relatively understudied, other forms of nonemployment have been more extensively studied in the literature. I start by showing that in-and-outs appear to be different from unemployed workers, experiencing different dynamics of consumption and income. Additionally, in-and-outs have little in common with permanent labor force dropouts, suggesting that these are distinct margins of labor supply. Lastly, I show that in-and-outs are not switching between formal work and home production, and instead are substituting between labor and leisure.

1.3.1 Unemployment

This section examines whether the in-and-out phenomenon is simply a new form of unemployment. Although in-and-outs are not actively engaged in job search, they may be waiting for a job offer to arrive and thus are in similar economic circumstances as those who are actively searching. However, the consumption and income dynamics of in-and-outs and unemployed individuals show very different patterns. While consumption of unemployed individuals falls substantially after a job separation, in-and-outs’ consumption dynamics are more similar to that of retirees. Additionally, in-and-outs do not appear to suffer permanently lower income as a result of their time out of the labor force. Taken together, these suggest that in-and-outs are not simply unemployed individuals with a different name.

Consumption Dynamics

In-and-outs may also differ from the unemployed in terms of their consumption behavior. Household expenditures of individuals who become unemployed typically fall upon separation and do not rebound for several years later. This evidence is consistent with unemployment being a unanticipated or uninsured negative shock to permanent income for these individuals.
Figure 1.5: Consumption of In-and-outs

Sample: Men in the PSID from 1968–1997. In-and-outs are prime age men employed in year 0, nonparticipating in year 1, and participating again by at least year 2 or 3. Unemployed are prime age men employed in year 0 and unemployed job losers in year 1. Retirees are men ages 62-68 employed in year 0 and nonparticipants after. For each group, I plot the coefficients from estimating equation 1.3 over \( k = 2, \ldots, 3 \). Since I control for year fixed effects and a cubic in age, the change in log food expenditures is relative to continuously employed individuals of the same age in the same year. Standard errors are clustered at the individual level. Food expenditure includes food purchased for consumption at home as well as food away from home. I exclude observations with an annual change in log food consumption in excess of 3 to avoid problematic measurement error.

To compare the evolution of consumption for in-and-outs and the unemployed, I use longitudinal data from the PSID. I proxy for consumption with total household food expenditures (including both food at home and food away from home). The evolution of expenditures for in-and-outs and the unemployed are captured by regressing the \( k \)-period change in log household food expenditures on an indicator for each type of separation \( s \), repeating this over different horizons \( k \):

\[
\Delta y_{i,t,t+k} = \beta_s^{(k)} \delta_s + a X_{i,t} + \Delta \epsilon_{i,t,t+k}
\]  

(1.3)

where \( \delta_s \) is an indicator for separations of type \( s \) and \( X_{i,t} \) is a vector of individual controls containing a cubic in age as well as time period fixed effects. I first conduct these regressions with \( s = "In-and-Out" \) and exclude all other types of separations from the sample, such that \( \beta_{\text{In-and-Out}}^{(k)} \) measures the evolution of in-and-outs’ expenditures relative to continuously employed individuals, and then repeat this procedure replacing in-and-outs with unemployed individuals.
In contrast to the unemployed, in-and-outs’ expenditures fall only slightly when leaving the labor force and rebound within two years of the separation. Figure 1.5 shows that in-and-outs’ expenditures are flat before separation, decrease by 4.8% after separating, before recovering two years later to end up only 1.5% below the pre-separation baseline. In contrast, unemployed individuals’ expenditures drop by 8.8% in the year of job loss and remain more than 6% below the baseline for at least three years following the separation. The differences in the evolution of expenditures between these two groups suggest that in-and-outs may not experience shocks to permanent income when separating from a job, as unemployed individuals do, or alternatively may be insured from the effects of such shocks.

The evolution of in-and-outs’ expenditures more closely resembles that of retirees. Figure 1.5 also plots the evolution of food expenditures for retirees as a comparison representing a voluntary separation from employment. Retirees cut food expenditures by 4.1% in the first year of retirement, the same as observed for in-and-outs. Even though retirement is voluntary, expenditures may decline if they partially reflect work-related expenses. Aguiar and Hurst (2005) show that retirees decrease spending on food away from home, but compensate by spending more time cooking, and as a result total consumption is unchanged. In-and-outs may be decreasing work-related expenses as well when leaving a job, which would imply that their consumption is the same while in and out of the labor force.

Income Dynamics

A common consequence of becoming unemployed is a persistent decline in earnings, continuing even after reemployment (Davis, Von Wachter et al., 2011; Jacobson, LaLonde and Sullivan, 1993). However, it is not clear that in-and-outs necessarily suffer the same consequences from losing a job. If in-and-outs have job opportunities available, but choose to delay these opportunities while taking a break in between jobs, they may not experience any wage cut.

To determine whether in-and-outs experience similar consequences from joblessness as the unemployed, I examine how in-and-outs’ income evolves before and after they leave the labor force. I use the same set of regressions as in equation 1.3 above to measure the evolution of personal income for in-and-outs and the unemployed at a monthly frequency in the SIPP. I vary the horizon \( k \) from -6 to 24 in order to cover a two-and-a-half-year window around job separation for both groups. As with consumption, I control for a cubic in age as well as time-period fixed effects to estimate the
Figure 1.6: Personal Income of In-and-outs around Labor Force Transitions

![Graph showing the change in log personal income over time for in-and-outs and unemployed job losers.](image)

Source: SIPP 1984–2008, men ages 25–54. In-and-outs are employed in month 0, non-participants in month 1, and are employed again by at least month 12. Unemployed job losers are employed in month 0 and fired or laid off but looking for work in month 1. For each group, I plot the coefficients from estimating equation 1.3 over $k = -6, \ldots, 24$, where $k$ refers to a monthly frequency. Since I control for year fixed effects and a cubic in age, the change in log personal income is relative to continuously employed individuals of the same age in the same year. Standard errors are clustered at the individual level. Personal income includes labor income, business income, transfers, and other income attributed to the respondent, but not income of other family members.

The evolution of income for each group relative to their continuously employed peers.

In-and-outs do not appear to suffer the same loss of earnings ability as the unemployed, as shown in Figure 1.6. In fact, not only are in-and-outs’ earnings two years after separation above unemployed workers’ earnings, they actually catch back up to the earnings of their continuously employed peers. Nearly all of this recovery happens within the first 10 months after job separation, which is when about 80% of in-and-outs have returned to the labor force. In comparison, the earnings of individuals who become unemployed fail to fully recover even two years later and end up about 30-40% lower relative to their continuously employed peers.

This event study shows that in-and-outs do not face large costs of nonemployment on average, but it may not mean that nonparticipation more broadly has no long-term costs. In-and-outs by definition have returned to the labor force within two years of a separation, which introduces the potential for selection to explain why in-and-outs experience don’t experience the same costs as
### Table 1.2: Reasons for Non-Participation

<table>
<thead>
<tr>
<th>Self-Reported Reason</th>
<th>Dropouts</th>
<th>In-and-Outs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Disability</td>
<td>74.6</td>
<td>24.3</td>
</tr>
<tr>
<td>Sickness</td>
<td>0.6</td>
<td>3.3</td>
</tr>
<tr>
<td>Retired</td>
<td>10.4</td>
<td>5.6</td>
</tr>
<tr>
<td>Taking Care of House/Family</td>
<td>6.6</td>
<td>21.8</td>
</tr>
<tr>
<td>In School</td>
<td>4.9</td>
<td>13.9</td>
</tr>
<tr>
<td>Other</td>
<td>2.9</td>
<td>30.9</td>
</tr>
</tbody>
</table>

Source: IPUMS CPS matched longitudinally, 1993–2015. For both dropouts and in-and-outs, I compute the share of individuals within that category who report each reason for non-participation (i.e. shares add to 100% for both dropouts and in-and-outs). Shares are pooled over the whole time period. Non-participating individuals are first asked if they are disabled, retired, or other, and, if the latter category, they are asked what their main activity during the reference week was. The reason for non-participation is only available for in-and-outs during months in which they are out of the labor force, overweighting in-and-outs that are out more often, so I reweight these observations to count all in-and-outs equally. All statistics are computed using survey weights.

The unemployed. For instance, individuals might experience earnings shocks while out of the labor force, and only the ones with positive shocks return to employment. A necessary condition for selection of this kind to be explaining the result is that reemployment should be unpredictable before the initial job separation, since it will depend on the shocks received while out of the labor force. To test this condition, I use a machine learning algorithm to examine the extent to which reemployment is predictable based only on pre-separation characteristics. I find that reemployment is largely predictable, indicating that selection explains little of the above results. Full details of this analysis and the results are contained in Appendix A.4.

### 1.3.2 Dropouts

This section compares in-and-outs and dropouts across a range of different characteristics. The two groups report different reasons for leaving the labor force, show large disparities in terms of health outcomes, and appear in different regions of the country. Taken together, this suggests that these two groups are distinct and represent separate margins of labor supply.

**Reasons for Non-Participation** In-and-outs and dropouts report being out of the labor force for very different reasons. In the CPS, non-participants are asked about their main activity and their reasons for non-participation. I combine these into six mutually exclusive categories and compute the shares of in-and-outs and dropouts who cite each reason, as shown in Table 1.2. Relatively few in-and-outs report being retired, disabled, or ill, and these shares have not changed much in the last few decades. Instead, the rise of in-and-outs has all come from non-participants who say they are
either in school, taking care of their house or family, or out of the labor force for some other reason.\footnote{Despite the growth of in-and-outs citing school as the reason for non-participation, the share of in-and-outs who are actively attending school is very small and has not risen substantially. I measure attendance in any type of schooling from the CPS October Education Supplement.}

In contrast, the majority of dropouts report that they are disabled and this category accounts for most of the growth of dropouts over the last few decades.

**Quality of Life** In-and-outs report having a higher quality of life than dropouts. Appendix Table A.1 shows several measures of quality of life from the American Time Use Survey (ATUS), which has been matched to the monthly CPS to separate in-and-outs from other groups. In-and-outs report a higher level of life satisfaction on a 10-point scale compared to dropouts, averaging 6.3/10 and 5.9/10 respectively.\footnote{The 10-point Cantril ladder measure used by the ATUS is described in more detail by Krueger (2017).} Dropouts also appear to have higher levels of pain and disabilities. A majority of dropouts (61\%) report taking some form of pain medication yesterday, compared to 29\% for in-and-outs and only 19\% for always participators. Dropouts are also much more likely than in-and-outs or always participators to report experiencing physical or cognitive difficulties.

**Geographic Distribution** In-and-outs and dropouts are regionally distinct. Appendix Figure A.1 shows the change in participation along each of these margins for all 50 states and DC over the period 1980-2013. The rise of in-and-outs has not correlated with growth of dropouts across states. Dropouts have grown most in states like Kentucky and West Virginia as well as many states in the Rust Belt. Meanwhile, in-and-outs have grown across many different regions of the US, with the largest increases of in-and-outs coming in New Mexico, Alabama, Delaware, and New York. Dropouts appear to be rising most in states that have seen declining opportunities relative to other states over this time period, but no such clear pattern emerges for in-and-outs.

**1.3.3 Home Production**

When individuals leave formal employment, one possibility is that they substitute into home production. If in-and-outs are switching between formal work and home production, they may be expending a similar amount of effort when in and out of the labor force.

I construct measures of time spent on different activities from the American Time Use Survey (ATUS) through IPUMS (Hofferth, Flood and Sobek, 2017). Since the ATUS is a follow-up survey...
Table 1.3: Time Use of In-and-Outs While In and Out of the Labor Force

<table>
<thead>
<tr>
<th>Activity</th>
<th>Hours per Weekday In LF</th>
<th>Hours per Weekday Out of LF</th>
<th>Activity</th>
<th>Hours per Weekday In LF</th>
<th>Hours per Weekday Out of LF</th>
</tr>
</thead>
<tbody>
<tr>
<td>Child Care</td>
<td>0.3</td>
<td>0.5</td>
<td>Leisure</td>
<td>4.1</td>
<td>7.2</td>
</tr>
<tr>
<td>Care for Adults</td>
<td>0.1</td>
<td>0.1</td>
<td>Watching TV</td>
<td>2.7</td>
<td>4.5</td>
</tr>
<tr>
<td>Education</td>
<td>0.3</td>
<td>0.5</td>
<td>Computer Use</td>
<td>0.2</td>
<td>0.4</td>
</tr>
<tr>
<td>Household Activities</td>
<td>1.1</td>
<td>2.1</td>
<td>Video Games</td>
<td>0.1</td>
<td>0.2</td>
</tr>
<tr>
<td>Personal Care</td>
<td>8.7</td>
<td>10.0</td>
<td>Socializing</td>
<td>0.5</td>
<td>0.7</td>
</tr>
<tr>
<td>Health-related care</td>
<td>0.0</td>
<td>0.3</td>
<td>Job Search</td>
<td>0.1</td>
<td>0.1</td>
</tr>
<tr>
<td>Sleeping</td>
<td>8.0</td>
<td>9.3</td>
<td>Working</td>
<td>6.6</td>
<td>0.1</td>
</tr>
</tbody>
</table>

Source: IPUMS American Time Use Survey, 2003–2015, matched to basic monthly IPUMS CPS.
Note: Subcategories listed in grey italics. Categories are not exhaustive, so time use may not add to 24 hours. Unemployed in-and-outs have been excluded for comparability. Unemployed in-and-outs have time use similar to men out of the labor force. Each category includes travel time associated with that activity. All statistics are computed using survey weights.

from the CPS, I can match ATUS responses to individuals’ labor force participation during their 8 months in the CPS. This allows me to examine the time use of in-and-outs who are subsequently in the labor force versus out of the labor force at the time of the ATUS interview.

In-and-outs replace time spent on work mostly with additional leisure activities. When working, in-and-outs work an average of 6.6 hours per weekday. Upon leaving the labor force, this time is mainly split between leisure activities (increase of 3.1 hours), sleeping (1.3 hours), and household activities (1.0 hour). In-and-outs do not substantially increase their time spent on child care, care for adults, education, or health-related care. Out of the three-hour increase in leisure, more than two thirds is spent watching TV, bringing total TV watching up to nearly 5 hours per day on average. In-and-outs only slightly increase their time spent on video games when leaving the labor force.5

1.4 Determinants of In-and-Outs

This section presents a framework describing how different forces affect labor supply along the in-and-out margin. The rise of in-and-outs represents a different type of change in labor supply than in classical labor supply models, which focus on the choice of hours per week at the margin. In contrast, in-and-outs appear to adjust lifetime labor supply at the margin through taking short breaks out of the labor force for several months at a time. However, this margin responds to changes

5This finding is in contrast to recent results by Aguiar et al. (2017), which suggest that declining participation can accompany increasing time spent playing video games. In Appendix A.3, I decompose the contributions of different restrictions to the divergence between these results, finding that the main reasons for the difference in results are that I focus on in-and-outs as opposed to all non-employed individuals and that I examine a sample of men ages 25–54 instead of men ages 21–30.
in wages and unearned income as the hours margin does in a classical labor supply model.

Consider an individual choosing whether to work or not at every point in time over some period. The individual also chooses instantaneous consumption subject to an intertemporal budget constraint equating total consumption with total income. For the moment, I assume that the individual does not discount the future and that the individual can save or borrow across periods with no interest. A utility maximizing agent faces the problem:

$$\max_{c(t), e(t)} \int v_t(c(t), e(t)) dt \quad s.t. \quad \int c(t) dt = \int we(t) dt + Y$$

where $v_t(c(t), e(t))$ is the utility function over consumption and labor supply respectively, $w$ is the wage the individual faces, and $Y$ is the total amount of unearned income. The labor supply choice at each point in time is whether to work or not, i.e. $e(t) \in \{0, 1\}$.

For the moment, I additionally make the assumption that utility is additively separable between consumption and labor supply. This implies that the utility function can be written as:

$$v_t(c(t), e(t)) = u_t(c(t)) - \gamma_t e(t)$$

where $\gamma_t$ is the time-varying disutility of labor supply. For simplicity, I assume that the utility of consumption is constant over time, i.e. $u_t(\cdot) = u(\cdot)$, which results in a constant consumption path $c(t) = \bar{c}$ for the usual reasons.

It is convenient to recast the problem in terms of the individual choosing the fraction of the period to work in, which I denote by $n \in [0, 1]$. This provides a clear analogue to how I measure in-and-outs above, as in-and-outs will have $n$ close to, but strictly less than, 1. For a given value of $n$, optimizing individuals will choose to work in the fraction $n$ of the time period with the lowest disutility of labor $\gamma_t$. With this framing, the problem above becomes:

$$\max_{c, n} u(c) - v(n) \quad s.t. \quad c w n + Y$$

where $v(n) = \int_0^n \gamma dF(\gamma)$ is the cumulative disutility of labor over the points in time in which the individual works (with $F(\gamma)$ as the cumulative distribution of $\gamma_t$ ordered from smallest to largest).

The first order condition of this model returns a standard result from classical models of labor supply, where individuals equate the marginal rate of substitution with the marginal rate of transformation:

$$-\frac{u'(n^*)}{u'(c^*)} = w$$
This condition can be log-linearized around the optimum to examine how changes in wages and consumption affect labor supply \( n \):

\[
\tilde{n} = \theta [\tilde{w} - \phi \tilde{c}]
\]

where \( \tilde{x} \) is the percentage deviation of \( x \) from its steady-state value. The coefficient \( \theta \) represents the Frisch, or consumption-constant, elasticity of labor supply and the coefficient \( \phi \) is the individual’s relative risk aversion.

Changes in labor supply may be driven by changes in the returns to work, although this may be attenuated by corresponding changes in consumption. If wages increase holding consumption constant, then labor supply should increase by \( \theta \tilde{w} \); however, consumption may increase as well in response to the wage change. As an extreme example, in a case with balanced-growth preferences \( (\phi = 1) \) and no unearned income \( (Y = 0) \), consumption will increase in perfect proportion with wages (i.e. \( \tilde{c} = \tilde{w} \)) resulting in no net effect on labor supply. This is the well-known case where income and substitution effects cancel.

Changes in unearned income may also affect labor supply, but only through effects on consumption. In response to an increase in \( Y \), consumption rises \( \tilde{c} \) and labor supply falls by \( \theta \phi \tilde{c} \).

In examining the explanations for the rise of in-and-outs, it is important to distinguish between these two channels. The first channel involves changes in the available wage that stem from changes in labor demand. The second channel, rising unearned income, affects labor supply through altering the marginal rate of substitution.

This framework omits several key factors that may affect real world labor supply. The return to work in reality is affected both by the pre-tax wage as well as the marginal rate of income tax. Additionally, search frictions may further attenuate the returns to work if individuals anticipate that some fraction of their work effort will go to job search instead of formal employment. Individuals may also choose the amount of hours to work at each point in time in addition to the choice of whether to work. I abstract from these elements to focus on the two explanations highlighted above, since none of these factors appear to have a first-order effect.

1.5 Explanations

This section examines explanations for the rise of in-and-outs, distinguishing between explanations related to declining labor demand and those related to a rising marginal rate of substitution. I
start by showing that observed changes in wages cannot explain the rise of in-and-outs, nor can differential trends across industries or occupations.

1.5.1 The Role of Labor Demand

The decline of prime age male labor force participation is commonly attributed to diminished labor demand or lower work incentives from taxes and transfers, but this section shows that neither of these forces can explain the rise of in-and-outs. I start by examining the wages available to in-and-outs, since lower wages could induce men to work less, but the rise of in-and-outs appears to have occurred at constant wages. Based on conventional estimates of labor supply elasticities, these changes in wages cannot explain a decrease in participation of the magnitude observed. The rise of in-and-outs has occurred across all industries and occupations, suggesting that this phenomenon is not related to particular types of jobs. Additionally, while wage rigidities can lead labor demand shocks to result in a temporary rise of in-and-outs, these effects disappear in the long-run.

Wages

Declining wages could be an important driver of diminished participation among men. Over the last few decades, several forces have emerged that could reduce the available wages for men, including skill-biased technological change, declining manufacturing, and decreased union coverage. Falling wages could induce men to spend less time working and take more time out of the labor force. Additionally, if wages fall to just above men’s reservation wage, small temporary shocks to productivity could lead men to leave the labor force for a short time before returning to work.

Fortunately, I can directly observe the wages of in-and-outs and examine how they have changed over time. I compute real hourly wages from the CPS Outgoing Rotation Groups for all prime age men, adjusting for top coding.\(^6\) Wages for some in-and-outs are missing if they happened to be out of the labor force during the month of the ORG interview, but since CPS cohorts are randomly selected, this meets the missing-at-random criterion so dropping these observations will not confound the estimates (Rubin, 1976).\(^7\) The full details describing how I construct wage data are contained in Appendix A.2.\(^\)
Across the skill distribution, participation has fallen holding market opportunities constant. In Figure 1.7, I divide men into wage deciles based on their rank in the annual wage distribution and plot average participation (excluding dropouts) against average wages within each decile, repeating this separately for several eras. In-and-outs have become more common at every wage level, resulting in this curve shifting to the left over time. This shift is more pronounced at the bottom of the skill distribution, where real wages have slightly declined, but higher skill levels have seen rising in-and-outs as well, even as wages have increased. Although there are some groups that have experienced declining wages, the leftward shift of this curve at every wage level makes it unlikely that changing wages are the main factor responsible for the rise of in-and-outs.

It is worth asking how much of the rise of in-and-outs can be attributed to the observed changes in wages of prime age men. The response of labor supply to changes in wages over the long-run is summarized by the uncompensated, or Marshallian, labor supply elasticity. Given the observed attachment, but the results are nearly identical under several different imputation strategies.
Table 1.4: Contributions of Changing Wages to Participation Decline, 1977–2015

(a) Constant Elasticity

<table>
<thead>
<tr>
<th>Elasticity Value</th>
<th>ΔLFP</th>
</tr>
</thead>
<tbody>
<tr>
<td>0.04</td>
<td>0.63</td>
</tr>
<tr>
<td>-0.05</td>
<td>-0.78</td>
</tr>
<tr>
<td>0.0</td>
<td>0.00</td>
</tr>
<tr>
<td>0.1</td>
<td>1.56</td>
</tr>
<tr>
<td>0.2</td>
<td>3.13</td>
</tr>
</tbody>
</table>

(b) Heterogeneous Elasticities

<table>
<thead>
<tr>
<th>Elasticities Source</th>
<th>ΔLFP</th>
</tr>
</thead>
<tbody>
<tr>
<td>Juhn, Murphy, Topel (1991)</td>
<td>1.06</td>
</tr>
<tr>
<td>Pencavel (2002)</td>
<td>-3.05</td>
</tr>
</tbody>
</table>

Source: CPS men ages 25–54, 1977–2015. Predicted percent change in LFP is computed by multiplying the observed change in average wages by the given elasticity, which is then converted into the predicted percentage point change in LFP, which is reported above. For the heterogeneous elasticity calculations, this procedure is conducted separately for each decile of the wage distribution and the resulting predictions are aggregated to form an estimate of the overall predicted change. Wage changes are computed using survey weights.

Change in wages, the portion of the decline in participation attributable to labor demand can be expressed as

\[
\Delta \log \text{LFPR} = \epsilon_{\log w} \cdot \Delta \log \frac{w}{p}
\]

To measure the uncompensated elasticity, I average estimates of this elasticity from a long literature on male labor supply. The average of these estimates is 0.04, although the estimates range from a minimum of -0.02 to 0.14 at maximum. The elasticities used here measure the response of total labor supply to wages, including both in-and-outs and dropouts. Therefore, these can be thought of as an upper bound for the relevant labor supply elasticity, which would measure the response along the in-and-out margin alone.

Changes in wages explain very little of the rise of in-and-outs. Table 1.4(a) shows the predicted change in participation implied by the total change in wages and several possible values of the uncompensated labor supply elasticity. Given the 16.6% increase in real wages between 1977 and 2015, the central elasticity estimate from the literature of 0.04 predicts that changes in wages alone would lead to a 0.7% increase in labor supply, or a 0.6 p.p. increase in the participation rate. For

---

8Specifically, I take the elasticity estimates from Hall (1970a); Hausman (1981); Pencavel (1986); Macurdy, Green and Paarsch (1990); Triest (1990); Juhn et al. (1991); Ziliak and Kniesner (1999); Juhn et al. (2002); Pencavel (2002); Eissa and Hoyynes (2004); and Moffitt (2012).

9This range is lower on average than the range of labor supply elasticities highlighted in the meta-analyses of Chetty, Guren, Manoli and Weber (2013) and Chetty (2012), primarily due to the fact that I am focusing on prime age men in the United States, which excludes some of the higher elasticity estimates included in those meta-analyses.

10The average wage for employed prime age men rose 16.6% between 1977 and 2015, but this may overstate the rise in potential wages if lower wage individuals are increasingly not employed. When wages of the non-employed are imputed using the procedures outlined in Juhn (1992) and Blau and Kahn (2007), the increase in overall average real wages falls to
higher values of the uncompensated elasticity an even larger increase in participation would be predicted. Only under a negative elasticity, indicating that income effects are substantially larger than substitution effects, would the observed changes in wages predict a decline in labor supply.

Table 1.4(b) relaxes the assumption of an identical labor supply elasticity for every individual, allowing elasticities to vary across the skill distribution. I take estimates of the elasticities at different skill levels from Juhn et al. (1991) and Pencavel (2002). I assign individuals to skill deciles based on their rank in the annual wage distribution and compute the change in average wages within each skill decile. Multiplying this change in wages by the elasticity for the decile and aggregating yields an estimate of the overall predicted change in participation. Using the elasticity estimates of Juhn et al. (1991), I estimate that the participation rate would have risen by 1.06 p.p. between 1977 and 2015, due to a small decline in participation for the lower half of the skill distribution being more than offset by an increase in participation among the top half. In contrast, the elasticities estimated by Pencavel (2002) imply a net reduction in participation of 3.05 p.p. over this time period, mainly driven by large income effects among the top two deciles. Neither of these approaches matches the observed pattern of rising in-and-outs across deciles, with in-and-outs rising among every wage decile and larger increases at the bottom end of the distribution.

**Industries and Occupations**

The increase of men cycling in and out of the labor force has happened in every industry, despite large differences in how industries have evolved over this time period. I take in-and-outs in the CPS and categorize them based on the industry (or industries) they work in while they are employed. Appendix Table A.2 shows the change in the share of employment made up by in-and-outs within each industry from 1980 to 2015. All industries have seen at least some increase in the share of in-and-outs and across most industries the shares of in-and-outs have risen between 3 and 7 p.p. over this time period. Industries with very different types of jobs have nonetheless seen similar increases
Table 1.5: Rise of In-and-Outs Across Occupations

<table>
<thead>
<tr>
<th>Occupation Group</th>
<th>Δln-and-Outs, 1980–2015 (p.p.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>High Abstract Tasks</td>
<td>2.2</td>
</tr>
<tr>
<td>High Routine Tasks</td>
<td>3.7</td>
</tr>
<tr>
<td>High Manual Tasks</td>
<td>3.7</td>
</tr>
</tbody>
</table>

Source: IPUMS CPS, 1980–2015. In-and-outs are defined as men ages 25–54 with between 1 and 7 months in the labor force out of 8 total. Individuals are categorized to occupations based on the occupation of their job when they are employed. Individuals who are not employed in any of their 8 CPS responses are excluded. For task content groups, I take occupations in the highest third of task content for each task type, where task content is measured as in Autor, Levy, & Murnane (2003). For each occupational category, I report the change in the share of individuals within that category who are in-and-outs between 1980 and 2015. All statistics are computed using survey weights.

in in-and-outs, as well as industries on different trajectories. The common growth of in-and-outs across a wide array of industries with different work patterns and different prospects for the future suggest that the rise of in-and-outs may not be strongly related to characteristics of jobs.

In-and-outs have grown as a share of many different types of occupations. Table 1.5 shows the change in the in-and-out share of employment among occupations involving different types of tasks. Among occupations with high abstract task content, the in-and-out share has risen by 2.2 p.p., only slightly less than the 3.7 p.p. seen in high routine and manual task occupations. Appendix Table A.3 further shows that in-and-outs have risen across six major groups of occupations, providing more evidence against the notion that the rise of in-and-outs is related to particular types of jobs. Since the rise of in-and-outs has occurred similarly across many different types of jobs, this suggests that changes in labor demand are unlikely to explain most of the rise of in-and-outs.

Shocks to Employment Opportunities

Changing labor demand could alternatively result in lower opportunities for employment without changing wages. For example, if wages are rigid, a fall in productivity may induce employers to ration jobs, which would result in fewer men being employed without a drop in wages. Blanchard and Katz (1992) provide evidence that regional labor demand shocks produce responses in employment and wages consistent with this pattern, although changes in participation in response to these shocks are found to be largely temporary.

I test the contribution of this force using regional shocks to labor demand. Shocks may have long-term effects on participation, which are captured by using an event study design to examine responses to shocks over a long horizon. I regress the change in the in-and-out share over different
horizons on shocks to employment and controls:

$$\Delta \text{ln-and-Outs}_{s,t-k} = \beta^{(k)} \Delta E_{s,t-1} + X_{s,t} + \epsilon_{s,t} \quad (1.4)$$

I repeat this regression for values of $k$ between -4 and 5 to examine the response of in-and-outs to employment shocks in the five years before and after the shock. State-level in-and-out shares are computed from the CPS, and state-level employment growth is taken from the Quarterly Census of Employment and Wages (QCEW).\(^\text{12}\) In the baseline, I control for time fixed effects alone, but since I conduct the regression in differences, this implicitly controls for state fixed effects as well. Standard errors are clustered at the state level.

To avoid potential endogeneity of employment changes, I instrument for the actual employment growth with predicted employment growth based on industry composition.\(^\text{13}\) I construct the predicted change in state $s$ and year $t$ by combining national growth rates of industries $i$ with lagged local industry shares:

$$\Delta \hat{E}_{s,i,t-1} = \sum_i (\log E_{s,i,t} - \log E_{s,i,t-1}) \cdot \frac{E_{s,i,t-3}}{E_{s,t-3}} \quad (1.5)$$

By using national growth rates excluding state $s$ to predict the change in employment in state $s$, this avoids mechanical correlation between predicted and actual employment growth. I take data on employment by state and industry from the QCEW.

Employment shocks appear to have little effect on temporary non-participation in the form of in-and-outs. Figure 1.8 plots the $\beta^{(k)}$ coefficients from the regression above to show the dynamic response of in-and-outs around a positive 1% shock to employment growth. Immediately after the shock, the in-and-out share appears to drops slightly by 0.08 p.p., although a response of zero cannot be ruled out. The immediate response does not seem to be very persistent, though, as the in-and-out share appears unchanged five years after the shock. The confidence intervals rule out an increase or decrease of more than 0.2 p.p. in the five years after a 1% employment shock. This indicates that shocks to labor demand holding wages constant explain little to none of the rise of in-and-outs.

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\(^{12}\)The QCEW employment count includes jobs worked by non-prime-age-men. One concern could be that prime age men are not affected by general shocks to labor demand, making this a poor measure of labor demand for prime age men. However, employment of prime age men as measured by the CPS responds one-for-one to shocks to QCEW employment, allaying this concern. Another data source, the Quarterly Workforce Indicators (QWI), allows one to break out prime age male employment by state and industry separately, but unfortunately many of these data points are censored for confidentiality purposes and so this data source produces very noisy estimates. For this reason, I present estimates based on the QCEW data series.

\(^{13}\)I use the approach of Bartik (1991) and Blanchard and Katz (1992) to compute predicted employment growth, but this is similar to using local industry shares directly as instruments (Goldsmith-Pinkham, Sorkin and Swift, 2017).
Figure 1.8: Response of In-and-Out Share to Labor Demand Shock

Source: State-level in-and-out share computed from IPUMS CPS using survey weights. Employment and Bartik instrument computed from QCEW. Using equation 1.4, the change in the state-level in-and-out share over a horizon $k$ is regressed on the growth rate of employment instrumented with a Bartik measure of predicted employment growth. By plotting the coefficients $\hat{b}(k)$, this traces out the impact of an exogenous 1% increase in state-level employment on the in-and-out share. The Bartik measure is computed as described in equation 1.5 using a leave-one-out procedure. The first stage F statistic is above 10 for every horizon.

1.6 Conclusion

Many different margins can contribute to a decline in labor supply. This paper has identified an understudied margin responsible for one-third of the participation decline among prime age men since the mid-1970s. The rise of in-and-outs forms a different picture of declining labor supply than the common view of men permanently withdrawn from the labor force. In-and-outs are highly attached to the labor force, work typical jobs, and are only notable in that they take brief breaks out of the labor force. I have presented evidence that the increasing prevalence of these breaks reflects a change in the desired amount of lifetime labor supply, due to several different labor supply factors, with little role for explanations related to labor demand.

Since in-and-outs’ breaks are typically short and infrequent, as well as incurring no permanent costs, the rise of in-and-outs likely has had a minimal effect on the inequality of lifetime incomes among men. In contrast, the growth of permanent dropouts over the last several decades, responsible
for the other two-thirds of the decline in participation since 1977, likely raised lifetime income inequality substantially. Dropouts not only forgo years of income while out of the labor force but may also experience lower wages if they return to the labor force, compounding the increase in inequality. The differences in consequences between changes along these two margins illustrates the importance of separating in-and-outs from dropouts when studying changes in labor supply.

To the extent that the rise of in-and-outs over the last several decades continues through the next several decades, this phenomenon could have important consequences for many aspects of the economy. For example, reduced labor force growth can slow the overall rate of economic growth, as occurred over the decade following the Great Recession (Fernald, Hall, Stock and Watson, 2017). Additionally, declining labor force participation may make jobless recoveries more common, as employment takes longer to reach its previous peak after a recession. The rise of in-and-outs may have social consequences too, particularly for families, as men more frequently take short breaks out of the labor force. This phenomenon merits continued study to understand the full consequences of rising in-and-outs.
Chapter 2

Prime Age Male Labor Supply and Household Wealth Effects

2.1 Introduction

Over the last half century, the share of men between the ages of 25 and 54 who are either working or looking for work—the prime age male labor force participation rate—has fallen substantially. At the same time, the types of households that these men are likely to live in have changed as well. A growing fraction of men, particularly young men, are unmarried and living with their parents, and those men that are married are more likely to have an employed spouse.

This paper asks whether changes in household structure can explain part of the decline in labor force participation of prime age men. I show descriptively that much of the decline is accounted for by two components: changes within married households, and growth of men living with their parents. Focusing on the first element, I use reduced-form evidence to test whether the rising earnings of married women have created a wealth effect within married households, finding evidence in favor of this explanation. To understand how this wealth effect interacts with the growth of unmarried men living with their parents, I build and estimate a structural model of labor supply and household formation. The model estimates imply that labor supply factors are responsible for both components, with labor demand factors contributing little on net.

I focus in the paper on the portion of the decline in participation coming from rising in-and-
outs—men who are temporarily out of the labor force. In a companion paper, I document how this phenomenon is responsible for about one third of the decline in participation since 1970 (Coglianese, 2018). These individuals appear to be adjusting lifetime labor supply at the margin, which indicates that this change can be well-modeled by a classical labor supply framework. While future work could extend the results in this paper to the case of permanent dropouts, I do not focus on them for the moment because their change in labor supply appears to be inframarginal.

I begin by outlining several descriptive facts about prime age male labor supply, household structure, and family income. Over the last half century, earnings of prime age man have fallen in relative terms compared to both the earnings of prime age women and the earnings of parents of prime age men. This has corresponded to declines in marriage rates and growth of unmarried men living with their parents from cohort to cohort. Finally, I show that two components account for nearly all of the decline in participation: a change within married households and growth of men living with their parents.

Focusing on married households first, I test whether rising female earnings have created a wealth effect in these households. Using variation across demographic groups over time, I show that groups which experienced larger increases in married women’s wages also experienced larger declines in male labor supply, providing support for the wealth effect channel. To remove any potential reverse causality, I instrument for the rise in married women’s wages with the rise of single women’s wages in the same demographic group, with similar results. The magnitude of this wealth effect is nearly identical to the prediction from a toy model of household labor supply calculated using estimated elasticities from the literature. Extrapolating this coefficient suggests that this wealth effect can explain all of the decline in participation among married men.

To account simultaneously for the wealth effect and other changes in household structure, I construct a structural dynamic model of labor supply and household formation. Fitting this model to observed data on participation rates, household structure, and income allows me to estimate how much of the decline in participation comes from changes in the wages available to men and women, as well as changes in factors affecting household formation. I find that the growth of female wages is responsible for half of the decline in participation, while changes in men’s wages predict a slight increase in participation. These estimates are close in magnitude to the reduced-form results, indicating that the direct effects of wage changes on participation dominate and indirect effects are small. Most of the rest of the decline in participation is attributed to a combination of increased
transfers from parents and preferences over household formation, as these forces combined account for the growth of men living alone or with their parents. In total, these findings imply that nearly all of the decline in participation reflects a decline in the desired amount of labor supply on the part of prime age men.

This paper highlights the role of household channels in explaining changes in labor supply. This approach was pioneered by Becker (1965), who focuses on the importance of home production in affecting labor supply of family members. While this framework has been used extensively to study female labor supply, relatively little work has applied this approach to studying male labor supply. Jones, Manuelli and McGrattan (2015) show that a calibrated unitary model of households implies that the large reduction in the gender wage gap over the last half century should have reduced male labor supply by much more than actually observed through a large wealth effect. Knowles (2012) offers a potential explanation for this puzzle through a collective model of households, as in Chiappori (1992), and shows that the data are consistent with rising female wages increasing women’s bargaining power within households, resulting in no reduction in male labor supply from a wealth effect. This paper offers a complementary solution to this puzzle by showing that male labor supply has indeed fallen along certain margins due to wealth effects from rising female wages, albeit by a smaller magnitude than predicted by Jones et al. (2015).

The remainder of the paper proceeds as follows. Section 2 details several descriptive facts about how labor supply, household structure, and income have changed over time. Section 3 examines the labor supply of men with partners using a toy model and reduced-form evidence to test for a wealth effect. Section 4 uses a dynamic model of labor supply and household formation to understand the interactions between changes in household structure and other labor supply factors in explaining the decline in participation. Section 5 concludes.

2.2 Descriptive Facts

This section provides three sets of descriptive facts about how household structure, income, and labor supply have changed for prime age men over the last half-century. I begin by showing that earnings of both prime age women and the parents of prime age men have increased relative to prime age men’s earnings. I then show that these trends have corresponded to changes in labor supply and

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1Greenwood, Guner and Vandenbroucke (2017) provide a survey of the large literature that has followed this approach.
household structure across cohorts of prime age men. Finally, I use a shift-share decomposition to show that the decline in prime age male labor supply is primarily driven by a within-group change among partnered households, and to a lesser extent by reallocation across household types.

### 2.2.1 Earnings of Other Groups

This section provides evidence that income for groups besides prime age men have risen dramatically. This trend is especially pronounced for prime age women, but is also exhibited among parents of prime age men. At the same time, income for prime age men has risen only slightly. As a result, income of prime age men has declined relative to these other groups.

Individual income of prime age women has grown rapidly over the last half century. Figure 2.1 shows the evolution of median income among prime age women and men from 1947–2015. While men and women both experienced rapid growth in income before 1970, the two groups subsequently diverged. Women’s income has grown about 50% since 1970, while men’s income is little changed.

The past half century has also seen children’s income fall relative to their parents. As documented by Chetty et al. (2016), the level of absolute mobility in the US has fallen steadily since 1940. Figure 2.2 shows the average probability that a son earns more than his father by birth cohort over this time period. While less than 10% of men born in 1940 earned less than their parents, more than half of all men born after 1970 have earned less than their parents.
2.2.2 Lifecycle Trends

This section highlights how changes in the earnings of other groups have accompanied several substantial changes across cohorts of prime age men. I start by documenting the change in labor force participation due to in-and-outs and then turn to changes in household structure and unearned income. All of this analysis uses CPS data aggregated into five-year birth cohorts. I also exclude dropouts from all graphs, including only men who are in the labor force at least some of the time.

Labor force participation has fallen steadily from cohort to cohort. Men born in 1945–49 participated at least 97% of the time at every age, while men born in 1975–79 have participated less than this threshold at every age, as shown in Figure 2.3. Declines in participation are evident at every age over the lifecycle, but are especially pronounced at younger ages in the late 20s and early 30s. The decline in participation reflects mostly a fall in the participation rate across cohorts, rather than changes within cohorts.

As income for other groups has risen, this has led to an increase in other family income for men who are living in households with these groups. Average spousal income for married men has increased more than 60% comparing men born in 1950–54 and 1970–74 at similar ages, as shown in Figure 2.4. This steady rise from cohort to cohort appears to be a direct consequence of the increase in women’s earnings documented above, with a very similar magnitude over the same time period. While the composition of married men may have changed, particularly as cohabitation of unmarried
couples has become more common, this is unlikely to explain much of the change across cohorts as the same fact emerges using broader groups of men meant to include cohabiting couples (e.g., men who are households heads or spouses in households with multiple earners). A smaller rise in the income of parents is also evident from looking at the change in other family income among men living with parents. Figure 2.5 shows that parents’ income rose about 20–30% from the 1950–54 to 1970–74 birth cohorts.

At the same time, household structure has changed substantially for men over the lifecycle, likely an additional consequence of the increased income of other groups. Recent cohorts tend to get married later and are less likely to ever get married than earlier generations, as shown in Figure 2.6. Some of this decline represents a shift towards cohabiting couples, but even broader measures that include cohabiting couples have declined (albeit less drastically). At the same time, even within married couples, men are less likely to be the primary breadwinner. About 40% of married men in the 1975–79 birth cohort reported being a spouse rather than the household head when asked about their relationship to their family members in the CPS, compared to less than 10% among men born in 1950–54 at similar ages.

Men are also more likely to live with their parents during their late 20s and early 30s, compared to prior generations. Figure 2.7 shows that nearly a third of men born in 1985–89 lived with their parents during their late 20s, more than triple the amount from the 1950–54 birth cohort. However, living with parents appears to be mostly transitory, as recent cohorts show less elevated rates of
living with parents through their 30s and 40s.

2.2.3 The Role of Household Structure

This section uses a descriptive decomposition to demonstrate that household structure is important for explaining the rise of in-and-outs. I conduct a standard shift-share decomposition across household types to capture both changes in participation within household types as well as changes accounted for by reallocation across household types.

I start by dividing men by household structure. I create four mutually exclusive categories: 1) men with a partner within the household, 2) heads of un-partnered households, 3) men living with their parents, and 4) all other men.\(^2\) In 1990, the first group comprised 76% of prime age men, the second comprised 13%, the third comprised 8%, and the last comprised less than 3%.

The change in the participation rate due to in-and-outs can be decomposed into changes within household types and reallocation across household types. Letting \(g\) index household types and \(t\) index time periods with \(s_{gt}\) as the share of men in household type \(g\) in year \(t\), and \(LFP_{gt}\) as the participation rate within group \(g\) in year \(t\) excluding dropouts (so that changes in this rate reflect

\(^2\)I have explored further divisions, including separate categories for heads and spouses, splitting on marital status, or using the presence of children in the household, but these divisions are not necessary to capture the key first-order trends. Therefore, for simplicity, I present results here based on the split into the four categories described above.
only changes in in-and-outs), the change between any two years can be written as

\[
\Delta LFP_{t+j} = \sum_s s_{gt+j} LFP_{gt+j} - \sum_s s_{gt} LFP_{gt} \\
= \sum_s (s_{gt+j} - s_{gt}) LFP_{gt+j} + \sum_s s_{gt} (LFP_{gt+j} - LFP_{gt}) \\
= \sum_s (s_{gt+j} - s_{gt}) (LFP_{gt+j} - LFP_{t+j}) + \sum_s s_{gt} (LFP_{gt+j} - LFP_{gt})
\]

(2.1)

where the second line adds and subtracts by \(\sum_s s_{gt} LFP_{gt+j}\) and rearranges. The third line adds in \(\sum_s (s_{gt+j} - s_{gt}) LFP_{t+j}\), using the fact that \(\sum_s (s_{gt+j} - s_{gt}) = 0\) and that the aggregate participation rate \(LFP_{t+j}\) is the same for all groups. Each group contributes to the change in the participation rate through two components. The term labeled “Reallocation Component” in equation 2.1 represents the change in participation due to men moving into or out of groups with participation rates different than the average participation rate. The “Within Component” represents the change due to participation rates changing within a particular household type, holding the population share of that household type constant.

Both within-group changes and reallocation across household types account for the rise of in-and-outs. Table 2.1 shows the contribution of each component by household type to the change in participation due to in-and-outs over 1977–2015. The within-group change among men with partners accounts for about half (1.2 p.p.) of the total decline in participation due to in-and-outs.
Figure 2.6: Marriage Rates Across Cohorts

Figure 2.7: Fraction of Men Living with Parents Across Cohorts

Table 2.1: Decomposition of Participation by Household Types, 1977–2015

<table>
<thead>
<tr>
<th>Household Type</th>
<th>Reallocation</th>
<th>Within</th>
</tr>
</thead>
<tbody>
<tr>
<td>Heads with Partners</td>
<td>0.0</td>
<td>-1.2</td>
</tr>
<tr>
<td>Heads without Partners</td>
<td>-0.2</td>
<td>-0.1</td>
</tr>
<tr>
<td>Living with Parents</td>
<td>-0.5</td>
<td>-0.1</td>
</tr>
<tr>
<td>Other</td>
<td>-0.0</td>
<td>-0.0</td>
</tr>
<tr>
<td>Total</td>
<td>-0.7</td>
<td>-1.4</td>
</tr>
</tbody>
</table>

Source: IPUMS CPS, 1977–2015. Individuals are assigned to household type based on the modal household type reported over their 8 months in the sample. The reallocation and within-group components are computed as described in equation 2.1. All statistics are computed using survey weights.
The reallocation of men across different household types accounts for an additional third of the rise of in-and-outs. Of the total 2.2 p.p. decline in participation, 0.7 p.p. is attributed to the reallocation component, with most of this (0.5 p.p.) coming from the increase of men living with their parents. In total, the within-group change among men with partners and the reallocation of men across household types put together account for nearly all of the rise of in-and-outs.

2.3 Labor Supply of Men with Partners

This section focuses on the labor supply of men with partners to isolate the potential wealth effect of rising female wages. I start by outlining a labor supply framework for a household with two earners to outline the forces that affect labor supply of each individual. I then test the predicted wealth effect of this model using variation across demographic groups. I find that in groups that experienced larger than average increases in married women’s wages there are corresponding decreases in male labor supply.

2.3.1 Labor Supply Framework

This section outlines a model of household labor supply which shows how the income of other earners can affect individuals’ labor supply. Under a few simplifying assumptions, the response of male labor supply to a change in female wages is governed by a few elasticities that can be estimated empirically. I use estimates of these elasticities from the literature to predict the size of the wealth from rising female wages.

The household’s preferences over consumption $c$ and labor provided by two adults $n_1$ and $n_2$ are given by $u(c, n_1, n_2)$ and the household is subject to a budget constraint, so that the household’s problem can be expressed as

$$\max_{c, n_1, n_2} u(c, n_1, n_2) \quad s.t. \quad c = w_1 n_1 + w_2 n_2 + Y$$

where $w_1$ and $w_2$ are post-tax wages for the two adults and $Y$ includes all unearned income. For simplicity, I assume that changes in labor supply come from the in-and-out margin alone, although this is not a very restrictive assumption since in an augmented model with additional margins of labor supply the in-and-out margin is likely to account for all changes in labor supply anyway (Prescott, Rogerson and Wallenius, 2009; Rogerson and Wallenius, 2013). The first order conditions
from optimization equate the marginal rate of substitution with the returns to work for each adult:

\[ \frac{-u_{n_1}}{u_c} = w_1; \quad \frac{-u_{n_2}}{u_c} = w_2 \]

For simplicity, I make an assumption that the disutility of labor is separable, i.e. \( \frac{\partial^2 u}{\partial n_1 \partial n_2} = 0 \).

Log-linearizing these conditions and re-arranging gives

\[ \tilde{n}_1 = q_1 \tilde{w}_1 + f_1 a_2 (\tilde{w}_2 + \tilde{n}_2) + f_1 a_Y \tilde{Y} \]  \hspace{1cm} (2.2)

\[ \tilde{n}_2 = q_2 \tilde{w}_2 + f_2 a_1 (\tilde{w}_1 + \tilde{n}_1) + f_2 a_Y \tilde{Y} \]  \hspace{1cm} (2.3)

where \( \tilde{x} \) represents the percentage change in the variable \( x \), \( \theta_i \) is the uncompensated labor supply elasticity for adult \( i \), \( \phi_i \) is the income elasticity of labor supply, and \( a_i \) is the budget share of income earned by adult \( i \). Importantly, changes in labor supply \( \tilde{n}_1 \) and \( \tilde{n}_2 \) are jointly determined in response to changes in wages.

Consider a change in the wages available to the second adult. In response, labor supply of both adults will adjust to a new equilibrium level. The equilibrium response of each adults’ labor supply can be decomposed into several components, including a wealth effect on the labor supply of the first adult. Letting \( \tilde{n}_1^* \) and \( \tilde{n}_2^* \) denote the equilibrium response of each adults’ labor supply in response to a change in wages \( \tilde{w}_2 \), holding all else constant, equations 2.2 and 2.3 above can be rewritten as:

\[ \tilde{n}_1^* = \phi_1 a_2 (1 + \theta_2) \tilde{w}_2 + \phi_1 a_1 \tilde{n}_1 \]  \hspace{1cm} \text{Wealth Effect} \hspace{1cm} (2.4)

\[ \tilde{n}_2^* = q_2 \tilde{w}_2 + \phi_2 a_1 \tilde{n}_1 \]  \hspace{1cm} \text{Direct Effect} \hspace{1cm} (2.5)

The change in the first adults’ labor supply in response to an increase in the wages of the second adult is mostly governed by the wealth effect, since the remaining terms are proportional to the product of income elasticities and therefore are likely to be small.

**Proposition.** If the product of income elasticities and budget shares is small, i.e. \( \phi_1 \phi_2 a_1 a_2 \ll 1 \), then the equilibrium response of the first adult’s labor supply \( \tilde{n}_1^* \) to a change in wages \( \tilde{w}_2 \) will be approximately equal to \( \phi_1 a_2 (1 + \theta_2) \tilde{w}_2 \).

The magnitude of the wealth effect can be approximated using estimates of these parameters.

---

3I focus here on the case where both adults are at an interior solution for labor supply. For men, this is a trivial assumption since my empirical analysis examines labor supply among households containing men who are in the labor force at least some of the time. For women, this is a more binding assumption, since some women in these households may be permanently out of the labor force and thus the relevant first order condition for these individuals would be an inequality rather than equality.
from the literature. I take the average income elasticity for men across six studies (Eissa and Hoynes, 2004; MaCurdy et al., 1990; Moffitt, 2012; Pencavel, 1986; Triest, 1990; Ziliak and Kniesner, 1999), yielding $\phi_m = -0.06$. The female uncompensated labor supply elasticity is estimated by Eissa and Hoynes (2004) to be $\theta_f = 0.27$. For the budget share of female labor income, I use a sample of March CPS households containing prime age men with a spouse or unmarried partner and compute the median share to be $a_f = 0.3$. Putting these together yields an estimated wealth effect of -0.02.

### 2.3.2 Wealth Effects of Rising Partner Income

This section tests whether wealth effects have contributed to the decline in participation of prime age men. As shown earlier, wages earned by prime age women have grown substantially faster than the wages earned by prime age men. As women’s wages rose and women joined the workforce in greater numbers, household income in dual-earner households rose accordingly. Among these dual-earner households, one possible effect of this rising wealth could be that it induced men to work less and take time off from the labor force periodically.

I test the role of wealth effects using variation across demographic groups. Using a sample of households containing a prime age man with either a spouse or unmarried female partner from the CPS, I divide households into groups based on state of residence and the age, race, and education level of the man.\(^4\) Within each group $g$ and for each year $t$, I compute the average male participation rate (excluding dropouts), $n_{m,g,t}$, and the average female post-tax hourly real wage, $w_{f,g,t}$.\(^5\) I then regress male labor supply on female wages controlling for group-level fixed effects:

$$n_{m,g,t} = \beta w_{f,g,t} + \gamma_g + \epsilon_{g,t}$$

With group-level fixed effects, a negative coefficient $\beta$ indicates that participation fell by more in groups where women’s wages rose by more.

To identify the equilibrium response, I need to account for the potential endogeneity of women’s wages in the presence of shocks to men’s market opportunities. If men’s labor supply receives a

\(^4\)Three categories for age are used (25-34, 35-44, and 45-54), four categories for race (non-Hispanic white, black, non-black Hispanic, and all others), four categories for education (less than high school, high school graduate, some college, and college graduates), and 51 for states (including DC). This gives a total of 2,448 possible categories, although in any given year some of these cells are empty.

\(^5\)The participation rate is calculated excluding dropouts, so that variation only comes from changes in in-and-outs. Post-tax wages are computed from multiplying the average CPS ORG wage within a group by the average net-of-tax rate in the group computed from March CPS data and the NBER TAXSIM calculator.
negative shock, then one possible response could be women choosing to work in higher paying but less enjoyable jobs, which could produce a negative estimate of \( \beta \) even in the absence of any wealth effects. This is an important concern since I am using a large number of relatively fine groups. To obviate this concern, I instrument for the wages of married women using the average post-tax hourly real wages of single women in the same group, \( w_{f,g,t}^{\text{single}} \). The validity of this instrument depends on the exclusion restriction that the wages of single women are not directly correlated with the labor supply of married men, which holds if the variation in the growth of single women’s wages comes from reductions in discriminatory forces across different demographic groups as in Hsieh, Hurst, Jones and Klenow (2013).\(^7\) The first stage equation is given by:

\[
\hat{w}_{f,g,t} = \alpha w_{f,g,t}^{\text{single}} + \delta_g + v_{g,t}
\]

By using the wages of single women to instrument for the wages of married/cohabiting women, the coefficient \( \beta \) can be interpreted as measuring the strength of the wealth effect on male labor supply.

Across households, a clear pattern emerges of in-and-outs rising in households where women’s wages increased more. Figure 2.8 shows a binned scatterplot of the second stage of the regression, where the observations are divided into 30 equal size bins and within each bin \( n_{m,g,t} \) is plotted against the predicted values from the first stage \( \hat{w}_{f,g,t} \). The relationship is summarized by the solid orange best-fit line, which indicates that the equilibrium response is downward-sloping. Households with larger predicted increases in female wages also experienced a larger rise of in-and-outs. The slope indicates that a 10% predicted increase in female wages reduced the male participation rate by 0.2 p.p. through the in-and-out margin. The magnitude of this effect is identical to the predicted wealth effect from the previous section.

Furthermore, the estimated wealth effect accounts for the entire rise of in-and-outs among men with partners, which accounts for half of the overall decline due to in-and-outs. Using the estimated strength of the wealth effect from above, I compute the predicted rise of in-and-outs among men

\(^6\)For a married woman in a demographic group \( g' \), I compute the average post-tax real hourly wages of single women in the same demographic group, \( w_{f,g,t}^{\text{single}} \). I then aggregate these wages to the demographic group of the husband, \( g \), by averaging across all married women married to husbands in demographic group \( g \), yielding \( w_{f,g,t}^{\text{single}} \), which represents the average wages of single women with the same demographics as women married to men in group \( g \).

\(^7\)One potential concern is that the variation in the growth of single women’s wages could in part reflect broader labor demand shocks, which would be correlated with the wages of married men and thus violate the exclusion restriction. I can examine this concern directly by repeating this regression with married men’s wages as the outcome. I find a weakly positive effect on married men’s wages, suggesting that in-and-outs should have, if anything, decreased slightly among this group absent a wealth effect.
Figure 2.8: Declining Participation due to In-and-Outs vs. Change in Partner’s Wage

Source: IPUMS CPS, 1989-2015. Prime age men with partners are divided into 2,448 groups based on education, race, age, and state. Dropouts are excluded from this analysis. Blue dots show a scatterplot of the data broken into 30 equal size bins, plotting the mean of each variable within each bin. Female partner wages are predicted from wages of single women in the same group, controlling for group fixed effects. Data are residualized on group fixed effects before binning and plotting. Estimated coefficient equal to $-0.0228 (0.0019)$. All statistics are computed using survey weights.

with partners using the observed increase in the average wage of their partners. Figure 2.9 shows this predicted decline in participation due to in-and-outs with the orange dashed line, which captures nearly all of the decline in participation due to in-and-outs from 1977 to 2015. This result suggests that the wealth effect from the rising wages of men’s partners can explain the entire rise of in-and-outs among men with partners.

This evidence of wealth effects contrasts sharply with the conclusions of prior work. In particular, the analysis of Juhn et al. (1991) suggests that participation fell more in households where women’s earnings rose the least. There are two novel elements of the analysis presented here that lead to the opposite conclusions from prior work. First, I separate changes in participation due to in-and-outs from changes due to dropouts. This is important because dropouts are both less likely to have a partner at all compared to in-and-outs and are more concentrated in the lowest skill categories, where women’s earnings have not risen as much. The second novel aspect of the analysis presented here is the use of post-tax wages as the measure of returns to work. While pre-tax wages and earnings
Sample: 25-54 year old men in the CPS with a spouse or unmarried partner, excluding dropouts. The predicted change in participation is computed from the change in spousal post-tax real hourly wages times -0.0228. Both series are smoothed with an Epanechnikov kernel with bandwidth equal to 1. Participation and wage series are computed using survey weights.

have risen the most for the highest-skilled women, tax rates for higher-skilled households have changed little while households lower in the income distribution have experienced substantial tax cuts, resulting in middle-skill households experiencing the largest rise in post-tax female earnings. By focusing on in-and-outs and using post-tax wages to measure the returns to work, I find evidence in support of a wealth effect that prior work had not uncovered.

2.4 Structural Model

This section combines the channels identified in previous sections within a unified framework. Reduced-form evidence suggests that changes both within and across households account for the rise of in-and-outs, so I begin by laying out a dynamic model of labor supply and household formation to capture these two channels. This model allows for changes in participation within households coming from both supply and demand forces, such as those analyzed previously, as well as changes induced by reallocation across household types. The model is fit to data on participation and
household types over the last several decades to estimate unobserved parameters. I estimate the model from 1978 to 2015 and measure the contribution of changes in each parameter to the rise of in-and-outs. The model estimates imply that labor supply factors are responsible for nearly the entire rise of in-and-outs, with little contribution from changes in labor demand.

2.4.1 Setup

This section outlines the setup of a dynamic model of labor supply and household formation and its solution. As in Chiappori (1992), I use a collective model of households in which individuals’ outside options influence the allocation within households. Individuals face the choice of whether or not to move in with their parents, similar to the model introduced by Kaplan (2012), as well as whether to get married, as in Knowles (2012).\footnote{Relative to this literature, the model used in this paper stands out for allowing individuals to choose over more than two household types as well as allowing for a rich set of shocks to preferences over household type. The model of Knowles (2012) involves individuals choosing between marriage and living alone with a single shock to preferences, while the model of Kaplan (2012) involves individuals and parents playing a repeated game in which the child can choose to move home and the parents choose an amount of resources to share with their child.}

The model consists of a continuum of men and women facing the same problem. Each period, individuals choose an amount of consumption $c$, labor supply $n \in [0, 1]$, and a household type $h \in \{\text{Alone, Living with Parents, Married}\}$. I assume that individuals have common preferences over consumption and labor supply given by the utility function $u(c, n)$.

Labor supply is assumed to represent changes in labor force participation along the in-and-out margin only. Instead of modeling labor supply as binary within many small periods, I allow $n$ to be continuous between zero and one, representing the fraction of time across some longer period in which an individual is in the labor force. I treat the periods in the model as years, meaning that $n$ is equal to the fraction of the year that an individual participates and $1 - n$ is the fraction of the year that the individual is out of the labor force as an in-and-out. Accordingly, changes in $n$ are assumed to come solely from the in-and-out margin.\footnote{When taking the model to the data, I exclude dropouts in calculating the empirical moments to be consistent with this assumption.}

Each period individuals choose their household type out of the available options. Since consumption and labor supply may vary across household types, individuals’ potential utility will vary across their possible choices, although this variation is the same for all individuals. To introduce a role for idiosyncratic differences in utility across potential household types, I allow for an
additively-separable, individual-specific utility component $\omega_i^h$, which represents the benefit or cost to individual $i$ of living in household type $h$. Combining these components, individual preferences are given by:

$$U_i^h = u(c, n) + \omega_i^h$$

Individuals receive a new i.i.d. draw of $\Omega_i \equiv \begin{pmatrix} \omega_i^A \\ \omega_i^{IWP} \\ \omega_i^M \end{pmatrix}$ each period.\(^{10}\)

Income comes from two sources. First, individuals receive labor income for working at a gender-specific wage rate $w_g$. This assumption focuses the model on capturing the effects of differences in average wages between genders on labor supply, while omitting a role for heterogeneity in wages within each gender. This is a somewhat restrictive assumption, since it ignores changes in moments of the distribution of wages beyond the mean. Nevertheless, the analysis of changing wages by decile in Coglianese (2018) reveals that capturing changes across the wage distribution does not help explain the rise of in-and-outs, so to a first-order this dimension is not important for the model to include.\(^{11}\) Additionally, accounting for wage heterogeneity within genders would require that the household formation choice to be conditional on the wages of potential partners and incorporating strategic matching of this type is outside the scope of the present paper.

The second form of income individuals receive is unearned income $Y_i$, representing asset income or other forms of non-labor income, which I allow to vary across household types. For individuals living with their parents, this unearned income may represent a transfer of income or a public good like housing provided within the household.\(^{12}\)

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\(^{10}\)The assumption that the random utility component is i.i.d. is necessary for tractability, as otherwise the household formation choice by individuals would need to be conditional on the past distribution of random utility components across the population of potential matches.

\(^{11}\)With the assumption of a common wage rate for each gender, the model also cannot speak to whether individuals change household type in response to negative labor market shocks. This channel is important in the model of Kaplan (2012), as some individuals may use the option of moving back in with their parents as a form of insurance against negative shocks. However, while the wages of prime age men differ across household types, these differences are not large enough to explain the observed differences in labor force participation using conventional labor supply estimates from the literature, so this channel is unlikely to explain much of the rise of in-and-outs.

\(^{12}\)I do not formally model the parents’ decision, but the quantity of this transfer could be taken as the outcome of a dynamic game between parents and their children where parents have some altruistic preferences, as in Kaplan (2012).
Consumption and Labor Supply. For individuals living alone or with their parents, the problem takes a straightforward form. Individual $i$ maximizes utility subject to a budget constraint:

$$\max_{c_i,n_i} U_i^h = u(c_i,n_i) + \omega_i^h$$
$$s.t. \quad c_i = w_{g(i)} n_i + Y_h$$  \hspace{1cm} (2.6)

Married households follow a collective model, as in Chiappori (1992). I assume that the allocation within a married household maximizes a weighted sum of individual utilities given a Pareto weight for each individual. Formally, for a household with individuals $i$ and $j$, consumption and labor supply are determined by:

$$\max_{c_i,n_i,c_j,n_j} \mu_{ij} U_i^M + (1 - \mu_{ij}) U_j^M$$
$$s.t. \quad c_i + c_j = w_{g(i)} n_i + w_{g(j)} n_j + Y_M$$  \hspace{1cm} (2.7)

where $U_i^M = u(c_i,n_i) + \omega_i^M$ and $U_j^M$ is defined analogously for $j$.

The additive structure of utility within a married household disallows any complementarities in leisure between married individuals.

Household Formation. Each period, individuals must choose which type of household to live in. Single individuals are matched randomly with a partner of the opposite gender and must decide whether to marry or not. If they choose not to marry, then they choose whether to live alone or with their parents. Married individuals must choose whether to remain married or get a divorce, and if the latter they then each choose whether to live alone or with their parents. Divorce incurs a fixed cost $k$ for each individual.

This setup gives rise to threshold rules governing the choice of housing type. For example, an individual will choose to live with his or her parents if the random utility component of preferences $\omega_i^{LWP}$ is above a threshold $\tilde{\omega}^{LWP}(\mu, \Omega; h)$, where $\mu$ is the potential Pareto weight he or she would face if married.\textsuperscript{13} The choice of household type can be summarized with a household decision rule for each gender $\Lambda^h_g(\mu, \Omega)$.

\textsuperscript{13}The dependence of this threshold on household type $h$ is only due to the fixed cost of divorce. As a result of this, the threshold is the same for individuals living alone or with their parents, but is different for married individuals.
Value Functions  Given a value for the Pareto weight $\mu$, the value function for an individual of gender $g$ living in household type $h$ with random utility draw $\Omega$ can be written as:

$$V^h_g(\mu, \Omega) = U^h_g(\mu, \Omega) + \beta \left[ \sum_{h'} \Pr(h' = \Lambda^h_g(\mu, \Omega)) E_{\mu', \Omega'} \left( V^{h'}_g(\mu', \Omega') - k \cdot 1(\text{Divorce}) \right) \right]$$

(2.8)

Note that the dependence of $V$ on $\mu$ is only relevant for married households.

Bargaining Solution  I now turn to the process by which the Pareto weights $\mu$ are determined. Since individuals can choose household type each period, I allow the Pareto weight to adjust in response to changes in the outside option of each individual, consistent with the evidence on labor supply responses presented by Chiappori, Fortin and Lacroix (2002). Let the gains from marriage be given by:

$$S_i(\mu, \Omega) = V^M_M(\mu, \Omega) - \max\{V^S_{g(i)}(\Omega), V^{LWP}_{g(i)}(\Omega)\} - k$$

Given these gains for each individual, I assume that $\mu_{ij}$ is determined through Nash bargaining with equal bargaining weights. This is equivalent to:

$$\mu(\Omega_i, \Omega_j) = \arg \max_{\mu} S_i(\mu, \Omega_i) \cdot S_j(1 - \mu, \Omega_j)$$

(2.9)

As a result of this setup, marriage and divorce are always mutually agreed on. If there exists a Pareto weight $\mu$ such that both individuals prefer the allocation with this weight to their outside options, then the individuals will agree to be married with this bargaining weight rather than divorce. Equivalently, married individuals will only divorce if there exists no weight $\mu$ and associated allocation that makes both individuals prefer marriage to their outside option.

**Definition 1.** A stationary recursive equilibrium of this model consists of value functions for each household type $V^h_g(\mu, \Omega)$, a bargaining solution $\mu(\Omega_i, \Omega_j)$, household decision rules $\Lambda^h_g(\mu, \Omega)$, and consumption and labor supply policies $c^h(\mu)$ and $n^h(\mu)$ such that:

1. The value functions $V^h_g(\mu, \Omega)$ solve equation 2.8;

2. The bargaining solution $\mu(\Omega_i, \Omega_j)$ is equal to the Nash bargaining solution of equation 2.9;

3. The household decision rule maximizes individuals’ value, i.e.

$$\Lambda^h_g(\mu, \Omega) = \arg \max_{\mu} V^{h'}_g(\mu, \Omega)$$
4. Consumption and labor supply policies of single individuals solve equation 2.6 and those of married individuals solve equation 2.7.

2.4.2 Estimation

I now turn to estimating the parameters of this model over time. Within each time period, some of the parameters of this model can be estimated directly from the data while others need to be estimated indirectly by matching the model to empirical moments. I conduct indirect inference on the latter set of parameters using the Simulated Method of Moments (SMM) separately for each year as described below.

Functional Forms  I make several assumptions regarding the functional forms of the model. First, I assume that the common utility function over consumption and labor supply takes on a standard balanced-growth form,

\[ u(c, n) = \log(c) - \psi \frac{n^{1+\frac{1}{\theta}}}{1+\frac{1}{\theta}} \]

where \( \theta \) is the Frisch, or consumption-constant, elasticity of labor supply and \( \psi \) captures forces that shift the marginal disutility of work relative to the marginal utility of consumption. This choice is somewhat conservative since these preferences balance substitution and income effects and allow for a stable path of labor supply with economic growth, while some other possible preferences feature income effects that dominate substitution effects and imply that labor supply must decrease with economic growth Boppart and Krusell (2016).

I assume the random utility component \( \Omega \) follows a multivariate normal distribution. Without loss of generality, I rewrite the random utility component \( \Omega_i \) with three components in terms of the differences between shocks, yielding a transformed vector \( \tilde{\Omega}_i \) with only two components:

\[ \tilde{\Omega}_i \equiv \left( \begin{array}{c} \omega_i^{LWP} - \omega_i^S \\ \omega_i^M - \omega_i^S \end{array} \right) \]

Each component represents the net utility benefit or cost of a household type relative to living alone. Since the shocks are generated by a multivariate normal distribution, \( \tilde{\Omega}_i \) also follows a multivariate normal distribution:

\[ \tilde{\Omega}_i \sim N \left( \begin{array}{c} \alpha_i^{LWP} \\ \alpha_i^M \end{array} \right), \begin{pmatrix} (\sigma_i^{LWP})^2 & \rho \sigma_i^{LWP} \sigma_i^M \\ \rho \sigma_i^{LWP} \sigma_i^M & (\sigma_i^M)^2 \end{pmatrix} \]
Calibrated Parameters  The calibrated values of several parameters are reported in Table 2.2. I estimate the wages for each gender, \( w_g \), directly from the data, normalizing all quantities relative to the wages of men in 1978-79, which I set to be equal to 1. Additionally, I estimate the median levels of unearned income \( Y_h \) for individuals living alone and married individuals directly from the data, normalizing these values relative to annual income of a full-time full-year male worker in 1978-79. This measure includes all income from transfers, assets, and businesses within the household.

Estimated Parameters  The remaining parameters are all estimated via Simulated Method of Moments (SMM). These include the preference parameters \( \psi \) and \( \theta \), the mean and variance parameters of the random utility component, the divorce cost \( k \), and the amount of unearned income for individuals living with parents \( Y_{LWP} \).

For each time period, the model is matched to three sets of moments. First, the model is required to match the share of men living in each household type. Second, the simulated labor supply within each household type is matched to its empirical counterpart. Lastly, I set the model to match the annual transition rates between household types, e.g. the probability of presently living in a married household conditional on having lived with parents one year prior. All moments are computed from the CPS excluding dropouts, to be consistent with the assumption that labor supply in the model reflects in-and-outs only. I compute these moments pooling all CPS respondents that began their interviews in years 1978-79 and again for 2014-2015.

---

14 While unearned income for other household types is observed, the amount of parents’ income that is shared with their children must be estimated indirectly.

15 Annual transition rates are measured in the CPS by comparing longitudinal matches one year apart, e.g. the first and fifth interviews or the second and sixth interviews, etc.
Table 2.3: Estimated Parameters

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<thead>
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<tbody>
<tr>
<td>( \psi )</td>
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<tr>
<td>( \sigma^\text{LWP} )</td>
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<tr>
<td>( \sigma^\text{LWP} )</td>
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Notes: Parameters are estimated separately for each time period using SMM. The model is simulated for 100,000 individuals for 100 time periods, after a 10 period burn-in. The moments are computed pooling across all individuals and time periods. Empirical moments are computed from the monthly IPUMS CPS matched longitudinally for each time period. Empirical shares of household types do not add to 100%, due to “Other” category, which is missing from the model, but this difference is negligible. Participation rates within each household type are computed excluding dropouts, so variation in participation comes from in-and-outs only. Hazard rates for transitions between household types are computed at an annual frequency using individuals’ household type during the last four months of the CPS rotation compared to household type from the first four months of their rotation. Empirical moments are computed using survey weights.

2.4.3 Results

I start by examining the fit of the model to the empirical moments, finding that it matches the distribution of household types and the participation rates quite well, while somewhat overestimating the flows between household types. Next, I use the estimated values of the parameters to measure the contribution of different supply and demand forces to the rise of in-and-outs. I find that growing female wages are responsible for about half of the rise of in-and-outs, consistent with the reduced-form evidence presented above, while the remainder is largely due to a combination of other labor supply forces, including transfers from parents.

Model Fit Table 2.3 shows the estimated parameters for 1978-79 and 2014-15 along with the values of the moments used in SMM. For the usual reasons, there is not a direct mapping between each parameter and corresponding moments, but Table 2.3 matches up parameters and the moments used to identify them approximately.

The model is capable of matching the shares of each household type and the participation rates within each household type quite well. The model estimates large average utility benefits of marriage, while individuals get a substantial utility cost from living with their parents on average. As the
observed share of married men has declined over time, the model reflects this with a slight decline in the random utility component for marriage.

The pattern of simulated participation rates across household types is very similar to the observed pattern. Within each time period, participation is highest for married men and only slightly lower for men living alone, while men living with their parents participate at substantially lower levels. The model accounts for this by estimating that men living with their parents receive substantial transfers, between about one third to one half of average annual wage income. In both time periods, the estimated Frisch elasticity is about 0.4, which is only slightly higher than the meta-analysis estimate of Chetty et al. (2013) and within the bounds estimated by Chetty (2012).

While most of the moments are matched quite well, the model somewhat overestimates the flows between household types, particularly for flows into marriage from the other two household types. This is a consequence of the assumption that the random utility component is i.i.d. rather than being somewhat persistent. However, a persistent shock would introduce strategic matching elements into the household formation decision, which I have elected to avoid in the interest of parsimony. While the simulated flows consistently overestimate the empirical flows, the overall pattern is very similar, with the largest hazard rates for transitions into marriage and lower hazard rates for the other types of transitions. Since the focus of the model is on participation rates, which are fit quite well, the overestimation of transition rates should not be a central concern.

Contributions  The estimated model can be used to quantify the contributions of different forces to the decline in participation due to in-and-outs. In particular, I focus on the contributions of several different labor supply factors, including wages of men’s partners, unearned income, and preferences, as well as the contribution of labor demand through changing male wages. For each of these factors, the model is able to estimate the total contribution to the participation, capturing both direct effects on participation within households as well as indirect effects through changing the distribution of household types.

I estimate the contributions of several sets of parameters by examining the simulated change in participation holding all other parameters constant. Specifically, I begin by dividing the total set of parameters into Q non-overlapping sets \( S^1, \ldots, S^Q \). I denote the set of parameters \( S^q \) evaluated at their estimated period \( t \) values as \( S^q_t \). The participation rate in period \( t \) simulated by the model can be written as a function of the parameters in that period, i.e. \( n(S^1_t, \ldots, S^Q_t) \). Using this setup, the
Table 2.4: Estimated Contributions

<table>
<thead>
<tr>
<th>Parameters</th>
<th>Contribution</th>
</tr>
</thead>
<tbody>
<tr>
<td>$w_m$</td>
<td>0.41</td>
</tr>
<tr>
<td>$w_f$</td>
<td>-0.92</td>
</tr>
<tr>
<td>$Y_A$, $Y_{LWP}$, $Y_M$</td>
<td>-0.43</td>
</tr>
<tr>
<td>$\sigma_{LWP}$, $\sigma_M$, $\sigma_P$, $\rho$</td>
<td>-0.36</td>
</tr>
<tr>
<td>$\psi$</td>
<td>-0.4</td>
</tr>
<tr>
<td>$\theta$</td>
<td>-0.11</td>
</tr>
<tr>
<td>$k$</td>
<td>-0.054</td>
</tr>
<tr>
<td>Residual</td>
<td>-0.23</td>
</tr>
<tr>
<td>Total Decline, 1978-2015</td>
<td>-2.1</td>
</tr>
</tbody>
</table>

Notes: Decline refers to the change in participation due to in-and-outs between 1978 and 2015. Contributions are measured by allowing one set of parameters to change over time in line with the estimated values from each time period, holding all other parameters constant. Residual contribution measures the difference between the simulated 1978-2015 decline in the model and the actual decline measured in the longitudinal monthly CPS.

The total change in the simulated participation rate between period 0 and $t$ can be decomposed into the contributions of changes in each parameter as follows:

$$\Delta n(S^1_1, \ldots, S^Q_t) = n(S^1_1, \ldots, S^Q_t) - n(S^1_0, \ldots, S^Q_0)$$

$$= \sum_{q} n(\ldots, S^q_{t-1}, S^q_t, S^q_0, \ldots) - n(\ldots, S^q_{t-1}, S^q_0, \ldots)$$

In this way, I examine the contributions of each set by feeding their changing values into the model, one set at a time, holding all other parameters constant. I add to this a residual component equal to the difference between the simulated decline in participation and the actual decline, since the model is not able to match the participation rate in each period exactly. This yields a complete decomposition of the decline in participation due to in-and-outs into the contributions of different components, including supply and demand forces.

Table 2.4 reports the contribution of each set of factors to the decline in participation due to in-and-outs. Consistent with the reduced-form evidence presented earlier, increased male wages are estimated to have offset the decline somewhat (contributing 0.4 p.p.), while increased female wages are responsible for slightly less than half of the decline (-0.9 p.p.). One fifth of the decline is attributed to changing unearned income (-0.4 p.p.) and an additional fifth is attributed to changes in preferences for household type (-0.4 p.p.). Most of the remainder is attributed to a shift in the disutility of labor represented by $\psi$ (-0.4 p.p.).

The estimated contributions are not sensitive to the order in which I examine them, varying only slightly across different orderings.
The fact that the estimated contributions of changing wages match the reduced-form evidence indicates that the indirect effects of wages on household structure are minor. This is consistent with findings by Shenhav (2016) and Autor, Dorn and Hanson (2017) that changes in relative wages can explain less than one third of the decline in marriage. Instead of relative wages, the model mostly attributes the observed changes in household structure to the combination of changes in unearned income and preferences for household type. These forces could reflect underlying explanations related to the growth of men living with their parents, such as the effects of rising housing costs, which indirectly lower labor supply through the wealth effect from intergenerational transfers.

Additionally, the shift in the disutility of labor reflected by $\psi$ suggests a small role for a labor supply shift that affects all households equally. This could include a change in the value of leisure, such as the increasing quality of video game technology proposed by Aguiar et al. (2017), or alternatively an increase in the disutility of work. Nevertheless, the model presented here suggests that such an explanation can only explain about one fifth of the total rise of in-and-outs.

2.5 Conclusion

This paper investigates whether changes in household structure have led to a decline in participation among prime age men, finding evidence in the affirmative. Using reduced-form evidence, I show directly that rising female wages led to a wealth effect in married households, leading men to cut back labor supply in response. Additionally, the growth of men living with their parents accounts for much of the remainder of the decline in participation, which structural model estimates attribute to rising intergenerational transfers. These two channels combined mean that prime age men are increasingly living in households where they are not the sole earner and are accordingly spending less of their lifetime working.

The phenomena outlined in this paper point to an understudied aspect of household labor supply—the effect of secondary earners on primary earners. While canonical models sometimes treat the choice of labor supply by the primary earner as independent from the choice of the secondary earner (see Hausman (1981) for an example), this approach overlooks the channels identified in this paper. Admittedly, the magnitude of the primary earner’s labor supply response to changes in the secondary earner’s wage are typically much smaller than the reverse, so for small changes ignoring this channel may be sufficient for a first-order approximation. However, when looking at long-run
changes over time, given the large increase in secondary-earner income relative to that of primary earners, this effect is no longer negligible.

This paper has focused on the portion of the decline in participation attributable to in-and-outs, but future work should investigate whether these channels affect other margins of nonemployment similarly. A concern is that the wealth effect from a change in female earnings may be inframarginal for permanent dropouts, but given the large increase in female wages over the last several decades, this effect may be large enough to affect the labor supply of these individuals.
Chapter 3

The Limited Macroeconomic Effects of Unemployment Benefit Extensions

3.1 Introduction

Responding to the increase in unemployment during the Great Recession, the potential duration of unemployment insurance (UI) benefits in the United States increased from 26 weeks to up to 99 weeks. Recent studies have found mixed effects of these benefit extensions on individual outcomes (Farber and Valletta, 2015; Johnston and Mas, Forthcoming; Rothstein, 2011). The effect on macroeconomic outcomes has been even more controversial. According to one view, by making unemployment relatively more attractive to the jobless, the extension of benefits contributed substantially to the slow recovery of the labor market (Barro, 2010; Hagedorn, Karahan, Manovskii and Mitman, 2015a). Others have emphasized the potential stimulus effects of increasing transfers to unemployed individuals (Congressional Budget Office, 2012; Summers, 2010). Distinguishing between these possibilities has important implications for the design of UI policy and for economists’ understanding of labor markets.

Quantifying the effects of UI benefit extensions on macroeconomic outcomes is challenging. Federal law links actual benefit extensions in a state directly to state-level macroeconomic conditions. This policy rule mechanically generates a positive correlation between unemployment and benefit

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1Co-authored with Gabriel Chodorow-Reich and Loukas Karabarbounis.
extensions, complicating the identification of any direct effect that benefit extensions may have on macroeconomic outcomes.

To shed light on this policy debate, we propose a novel empirical design that exploits state variation in benefit extensions caused by measurement error. Our results are inconsistent with large effects of benefit extensions on state-level macroeconomic aggregates including unemployment, employment, vacancies, and worker earnings. Instead, we find that the extension of benefits has only a limited influence on macroeconomic outcomes.

Our empirical approach starts from the observation that, at the state level, the duration of UI benefits depends on the unemployment rate as estimated in real time. However, real-time data provide a noisy signal of the true economic fundamentals. It follows that two states differ in the duration of their UI benefits either because of differences in fundamentals or because of measurement error. We use subsequent revisions of the unemployment rate to separate the fundamentals from the measurement error. We then use the measurement error component of UI benefit extensions to identify the effects of benefit extensions on state-level macroeconomic aggregates. Effectively, our strategy exploits the randomness in the duration of benefits with respect to economic fundamentals caused by measurement error in the fundamentals.

Table 3.1 uses the example of Louisiana and Wisconsin in April 2013 to illustrate our approach. Under the 2008 emergency compensation program, the duration of benefits in a state increased by 14 additional weeks if a moving average of the state’s unemployment rate exceeded 6 percent. The

<table>
<thead>
<tr>
<th></th>
<th>Louisiana</th>
<th>Wisconsin</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Real-Time Data</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unemployment Rate (Moving Average)</td>
<td>5.9%</td>
<td>6.9%</td>
</tr>
<tr>
<td>Duration of Benefit Extensions</td>
<td>14 Weeks</td>
<td>28 Weeks</td>
</tr>
<tr>
<td><strong>Revised Data</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Unemployment Rate (Moving Average)</td>
<td>6.9%</td>
<td>6.9%</td>
</tr>
<tr>
<td>Duration of Benefit Extensions</td>
<td>28 Weeks</td>
<td>28 Weeks</td>
</tr>
<tr>
<td>UI Error</td>
<td>-14 Weeks</td>
<td>0 Weeks</td>
</tr>
</tbody>
</table>

Table 3.1: April 2013 Example
unemployment rate measured in real time in Louisiana was 5.9 percent while that in Wisconsin was 6.9 percent, resulting in an additional 14 weeks of potential benefits in Wisconsin relative to Louisiana. However, data revised as of 2015 show that both states actually had the same unemployment rate of 6.9 percent. According to the revised data, both states should have qualified for the additional 14 weeks. We refer to the 14 weeks that Louisiana did not receive as a “UI error.” This error reflects mismeasurement of the economic fundamentals rather than differences in fundamentals between the two states and, therefore, provides variation to identify the effects of UI benefit extensions on state aggregates. The actual unemployment rate (from the revised data as of 2015) evolved very similarly following the UI error, declining by roughly 0.2 percentage point between April and June 2013 in both states. Our empirical exercise amounts to asking whether this apparent limited influence of extending benefits on unemployment generalizes to a larger sample.

We begin our analysis by discussing relevant institutional details of the UI system, the measurement of real-time and revised state unemployment rates, and the UI errors that arise because of differences between real-time and revised data. The Bureau of Labor Statistics (BLS) constructs state unemployment rates by combining a number of state-level data sources using a state space model. Revisions to state unemployment rates occur due to revisions to the input data, the use of the full time series of available data in the state space estimation at the time of the revision, and the introduction of technical improvements in the statistical model itself. Of these, the technical improvements account for the largest share of the variation in the measurement error in the unemployment rate. The unemployment rate measurement error gives rise to more than 600 state-month cases between 1996 and 2015 in which, as in the example of Louisiana and Wisconsin in April 2013, the duration of benefits using the revised data differs from the actual duration of benefits. Almost all of these UI errors occur during the Great Recession. This concentration reflects both the additional tiers of benefits duration created by the 2008 emergency compensation program and the fact that most states experienced unemployment rates high enough for measurement errors to affect their eligibility for extended benefits. Once a UI error occurs, it takes on average nearly 4 months to revert to zero.

We estimate impulse responses of state-level labor market variables to an unexpected innovation in the UI error. Our identifying assumptions are that innovations in the UI error occur randomly with respect to the true economic fundamentals and that the revised unemployment rate measures the true economic fundamentals in the state. Our main result is that innovations in the UI error have negligible effects on state-level unemployment, employment, vacancies, and worker earnings. In the
baseline specification, a one-month increase in the maximum potential duration of benefits generates at most a 0.02 percentage point increase in the state unemployment rate. Crucially, a positive UI error innovation raises the fraction of the unemployed who receive UI benefits by a statistically significant and an economically reasonable magnitude, with the additional recipients being in the tiers affected by the error. Therefore, our results do not reflect a failure of UI errors to lead to a larger fraction of unemployed receiving benefits. They simply reflect the small macroeconomic effects of an increase in UI eligibility and receipt.

These impulse responses answer the question of what would happen if a state increased the duration of unemployment benefits around the neighborhood of a typical UI error, or by about 3 months after a state has already extended benefits by nearly one year. To assess the informativeness of these estimates for other types of policies, we examine the heterogeneity of responses with respect to the initial level of benefit duration and the persistence of the UI error. The responses of labor market variables such as unemployment and vacancies do not vary along either dimension. Therefore, a linear extrapolation of our estimates provides a reasonable guide to the macroeconomic effects of longer extensions. Taking the upper bound of our preferred specification, we find that extending benefits from 26 to 99 weeks increases the unemployment rate by at most 0.3 percentage point.

We show the robustness of our results to the inclusion of a number of controls into the baseline specification and to alternative specifications. Most important, a concern with using revisions in the unemployment rate to construct UI errors is that the incorporation of the full time series of data in the revision process makes the unemployment rate revision in month $t$ partly dependent on realizations of variables after month $t$. To make sure this aspect of the revision process does not affect our results, we add to our regression controls for linear and non-linear functions of the unemployment rate measurement error. The responses of labor market variables remain similar to our baseline estimates, reflecting the fact that this aspect of the revision process contributes very little to the variation in the unemployment rate measurement error. Further, we develop an alternative series of UI errors using sampling error in the Current Population Survey (CPS). We infer the sampling error from the difference between a measure of the population eligible for regular benefits in the CPS and administrative data on UI receipt. Constructing UI errors based only on this more restrictive source of measurement error implies that UI errors do not depend on realizations of variables in future dates. We continue to find a limited effect of unemployment benefit extensions on labor market outcomes using this approach.
Finally, we derive a bound for the consistency of our estimator when the revised data still contain measurement error. Intuitively, the bound depends on the measurement error in the revised data relative to that in the real-time data. We show that the macroeconomic effects of benefit extensions are small so long as the revised data measure true economic conditions at least as well as the real-time data and provide empirical support for this condition from horse-race regressions in which measures of consumer spending and survey attitudes and beliefs load on the revised but not on the real-time unemployment rate.

In the last part of the paper we complement our empirical results by analyzing a DMP model (Diamond, 1982; Mortensen and Pissarides, 1994) augmented with a UI policy. The model provides an alternative approach to considering larger extensions in the duration of benefits and allows anticipation effects by workers and firms to differ according to whether a benefit extension is caused by a transitory UI error or by a persistent increase in unemployment that triggers the extension. As well known in the literature, the effect of UI policy on macroeconomic outcomes depends crucially on the level of the opportunity cost of employment. We show that with a low level of the opportunity cost of employment, such as the one estimated in Chodorow-Reich and Karabarbounis (2016), a one-month UI error innovation leads to a less than 0.02 percentage point increase in the unemployment rate, a magnitude similar to our empirical estimates. To mimic the U.S. experience in the aftermath of the Great Recession, we subject the model to a sequence of large negative shocks that increase unemployment from below 6 percent to roughly 10 percent and increase the duration of benefits from 6 months to 20 months. Removing benefit extensions leads to a decline in the unemployment rate by at most 0.3 percentage point in the model. Thus, both a linear extrapolation of the empirical results and the model exercise suggest a small response of unemployment to the extension of benefits around the Great Recession.

The economic literature on the effects of benefit extensions has followed two related lines of inquiry. Motivated in part by a partial equilibrium optimal taxation result linking the optimal provision of UI to individual search behavior (Baily, 1978; Chetty, 2006), a microeconomic literature has studied how various aspects of UI policy affects individual labor supply (for a survey see Krueger and Meyer, 2002). Studies which find a small effect of benefit extensions following the Great Recession on individual job finding rates and unemployment duration include Rothstein (2011) and Farber and Valletta (2015), while Johnston and Mas (Forthcoming) find somewhat larger effects in a
study of a single benefit cut in Missouri in 2011.\textsuperscript{2}

The macroeconomic effects of UI benefits concern their effect on aggregate unemployment.\textsuperscript{3} Economic theory does not provide a one-to-one mapping between the magnitude of the microeconomic and macroeconomic effects. For example, in a standard DMP model with exogenous job search effort and Nash bargaining, an increase in UI benefits raises workers’ outside options, putting an upward pressure on wages and depressing firm vacancy creation. Exogenous search effort implies a zero microeconomic effect, but the decline in total vacancies generates a rise in total unemployment, i.e. a non-zero macroeconomic effect (Hagedorn \textit{et al.}, 2015a). Alternatively, in models with job rationing, large microeconomic effects could be consistent with small macroeconomic effects if the job finding rate of UI recipients falls but that of non recipients rises (Lalive, Landais and Zweimüller, 2015; Landais \textit{et al.}, 2015; Levine, 1993). Crepon, Duflo, Gurgand, Rathelot and Zamora (2013) provide experimental evidence that such displacement effects occur in the related setting of job placement assistance programs.

A number of papers starting with Hagedorn \textit{et al.} (2015a, HKMM) and Hagedorn, Manovskii and Mitman (2015b, HMM) use a county border discontinuity design to estimate the macroeconomic effects of UI benefit extensions. Different from our results, HKMM and HMM find a large positive effect of benefit extensions on unemployment. However, the subsequent literature has challenged these findings. Hall (2013) first pointed out problems that arise from the imputation of the unemployment rate at the county level and raised conceptual questions about the identification strategy in HKMM. Amaral and Ice (2014) argue the results in HKMM are sensitive to changes in the data sources and the specification, points developed further in Boone, Dube, Goodman and Kaplan (2016) and Dieterle, Bartalotti and Brummet (2016). Boone \textit{et al.} (2016) find near zero effects of UI extensions on employment using a county border design and a more flexible empirical model. They further show that using newer vintages of the unemployment data substantially reduces or eliminates the positive effect of benefit extensions on unemployment found in HKMM and HMM.\textsuperscript{4} Dieterle \textit{et al.} (2016)

\textsuperscript{2}Schmieder, von Wachter and Bender (2012) and Kroft and Notowidigdo (2015) show that the effect of UI benefit extensions on unemployment duration becomes smaller during recessions.

\textsuperscript{3}Our estimates of the macroeconomic effects are particularly informative for general equilibrium models with UI policy. See Hansen and Imrohoroglu (1992), Krusell, Mukoyama and Sahin (2010), and Nakajima (2012) for earlier general equilibrium analyses of unemployment insurance policy. Landais, Michaillat and Saez (2015) and Kekre (2016) extend the Baily-Chetty partial equilibrium optimal UI formula to a general equilibrium setting and show how it depends on the macroeconomic effects of benefit extensions.

\textsuperscript{4}For example, Boone \textit{et al.} (2016) find that HMM’s estimated effect falls by three-quarters and becomes statistically indistinguishable from zero using the newer data. They also forcefully question the assumptions underlying the quasi-forward
point out that shocks triggering UI extensions in one state may not affect neighboring countries similarly because population does not concentrate at the border. They refine the border-county-pair strategy by controlling for polynomials in the distance to the border and find a small response of unemployment to benefit extensions. Finally, both Dieterle et al. (2016) and Marinescu (2017) cite job search spillovers across counties to question the appropriateness of a border design to study UI extensions.

Other papers using cross-state variation find mixed macroeconomic effects. Johnston and Mas (Forthcoming) use a sudden change in benefits in Missouri to estimate both the microeconomic and macroeconomic effects. They estimate macroeconomic effects of similar magnitude to the microeconomic effects, but their estimate of the macroeconomic effect depends on a difference-in-difference research design with Missouri the only treated observation. Marinescu (2017) uses data from a large job board and documents an insignificant effect of benefit duration on vacancies. Relative to this literature, ours is the first paper to use quasi-experimental cross-state variation to estimate the macroeconomic effect of UI extensions on unemployment.

3.2 Unemployment Insurance in the United States

The maximum number of weeks of UI benefits available in the United States varies across states and over time. Regular benefits in most states provide 26 weeks of compensation, with a range between 13 and 30 weeks. The existence of regular UI benefits does not depend on economic conditions in the state. Extended benefits (EB) and emergency compensation provide additional weeks of benefits during periods of high unemployment in a state. The EB program has operated since 1970 and is 50 percent federally funded except for the period 2009-2013 when it became fully federally funded. Emergency compensation programs are authorized and financed on an ad hoc basis by the federal government. In our sample (1996-2015), the Temporary Emergency Unemployment Compensation (TEUC) program operated between March 2002 and December 2003 and the Emergency Unemployment Compensation (EUC) program operated between July 2008 and December 2013. We refer to the combination of EB and emergency compensation as UI benefit extensions.

differencing procedure used in HKMM. As they point out, if the true effect of UI extensions were to cause unemployment to slightly decrease, applying the quasi-differencing procedure would nonetheless cause a researcher to conclude benefit extensions increase unemployment.
Whether a state qualifies for benefit extensions typically depends on the unemployment rate exceeding some threshold. Two measures of unemployment arise in the laws governing these extensions. The insured unemployment rate (IUR) is the ratio of recipients of regular benefits to employees covered by the UI system. The total unemployment rate (TUR) is the ratio of the total number of individuals satisfying the official definition of not working and on layoff or actively searching for work to the total labor force. To avoid high frequency movements in the available benefit extensions, both the IUR and the TUR enter as three-month moving averages into the trigger formulas determining extensions. A trigger may also contain a lookback provision which requires that the indicator exceed its value during the same set of months in prior years. Appendix C.1.1 lists the full set of benefit extension programs, tiers, and triggers in operation during our sample.

Not every unemployed individual qualifies for regular benefits, with eligibility determined by reason for separation from previous employer, earnings over the previous year, and search effort. An individual becomes eligible to receive benefits under EB or an emergency program only after qualifying for and exhausting entitlement under regular benefits. Any individuals who have exhausted eligibility under all previous tiers become immediately eligible to receive benefits when their state triggers onto a new tier. Conversely, as soon as a state triggers off a tier all individuals lose eligibility immediately regardless of whether they had begun to collect benefits on that tier.

3.3 Empirical Design

We organize the discussion of our empirical methodology around a linear relationship between a labor market variable $y_{s,t+h}$ observed in state $s$ at date $t+h$, the maximum duration of unemployment benefit receipt in the state at date $t$, which we denote as $T_{s,t}^*$, and all other influences of the labor market variable $e_{s,t+h}$:

$$y_{s,t+h} = \beta(h)T_{s,t}^* + e_{s,t+h}. \quad (3.1)$$

Two main challenges arise in using Equation (3.1) to estimate the causal effect $\beta(h)$ of extending benefits on state-level labor market outcomes. First, the extension of benefits depends on labor market outcomes such as the state unemployment rate which induces a correlation between $T_{s,t}^*$ and $e_{s,t+h}$. In Section 3.3.1 we separate the benefit duration $T_{s,t}^*$ into the part which depends on true economic fundamentals in the state and the part which depends on measurement error in the
fundamentals and address this identification challenge by using only the latter part in equation (3.1). Second, labor market variables such as the unemployment rate and vacancies may depend not only on contemporaneous but also on past and future values of the duration of benefit extensions. To the extent that the duration of benefits is autocorrelated over time, this again induces a correlation between \( T_{s,t}^* \) and \( e_{s,t+h} \). Section 3.3.2 explains how we address this issue of serial correlation in the duration of benefits.

### 3.3.1 Endogeneity of Benefit Duration

The key idea underlying our approach is to use the variation in the duration of benefits caused only by measurement error in state-level labor market outcomes. To implement this idea, we decompose the benefit duration \( T_{s,t}^* \) into the part which depends on true economic fundamentals in the state, \( T_{s,t} \), and the part which depends on measurement error in the fundamentals, \( \hat{T}_{s,t} \). Let \( f_{s,t}(\cdot) \) be the UI law which maps a history of unemployment rates in a state \( s \) into the maximum duration of UI benefit extensions in the state. The time subscript \( t \) on the function indicates that the mapping can change due to temporary legislation such as an emergency compensation program. As described in Section 3.2, whether a state extends its duration of benefits or not depends on the most recently reported or “real-time” estimate of the state-level unemployment rate:

\[
T_{s,t}^* = f_{s,t}(u_{s,t-1}^*), \tag{3.2}
\]

where \( u_{s,t-1}^* \) denotes the real-time unemployment rate reported in month \( t \) for the latest available month, \( t - 1 \).

The reported unemployment rate in real time, \( u_{s,t}^e \), may deviate from the true unemployment rate, \( u_{s,t} \), because of measurement error, denoted by \( \hat{u}_{s,t} = u_{s,t}^e - u_{s,t} \). Our empirical strategy exploits variation in this measurement error to extract the component of benefit extensions which is

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5For expositional reasons, we simplify a few details in writing monthly UI duration as a function of the previous month’s unemployment rate. The actual determination of UI benefit extensions eligibility occurs weekly and is based on unemployment rate data available at the start of the week. The BLS typically releases the real-time state total unemployment rate data for month \( t - 1 \) around the 20th day of month \( t \). Therefore, for the first weeks of month \( t \) the most recent real-time unemployment rate which enters into the eligibility determination is for month \( t - 2 \) while for the last weeks the most recent unemployment rate affecting eligibility is for month \( t - 1 \). We aggregate in the text to a monthly frequency and capture the reporting lag for the real-time data by writing UI benefits in month \( t \) as a function of the unemployment rate in month \( t - 1 \). Next, as we discuss in Appendix C.1.1, benefit duration typically depends on a three month moving average of unemployment rates and may also depend on a “lookback” to the unemployment rate 12 and 24 months before, so that further lags of the unemployment rates also enter into the eligibility determination. Third, duration also depends on the insured unemployment rate, although this trigger binds very rarely in our sample. While we appropriately take into account all of these details in our implementation, they do not affect the general econometric approach so we omit them in the main text for clarity.
uncorrelated with state economic conditions. More formally, we first define a hypothetical duration of benefit extensions, \( T_{s,t} \), based on the true unemployment rate \( u_{s,t} \) and the same function \( f_{s,t}(\cdot) \) that appears in equation (3.2):

\[
T_{s,t} = f_{s,t}(u_{s,t-1}).
\]  

(3.3)

We then define the UI error \( \hat{T}_{s,t} \) from the relationship:

\[
T_{s,t}^* = T_{s,t} + \hat{T}_{s,t}.
\]  

(3.4)

Equation (3.4) shows that variation in the actual duration of benefit extensions \( T_{s,t}^* \) comes from the component \( T_{s,t} \) which depends on the true economic fundamentals and from the component \( \hat{T}_{s,t} \) which reflects measurement error in the state unemployment rate. Our approach is to use the part of the variation in \( T_{s,t}^* \) in regression (3.1) induced only from the UI error \( \hat{T}_{s,t} \) to identify the effects of benefit extensions on state-level outcomes. The remainder of this section discusses how we construct the measurement error component \( \hat{T}_{s,t} \).

### Measurement of State Unemployment Rates

A key step in our methodology is to use the revised unemployment rate to proxy for the true unemployment rate \( u_{s,t} \) used in Equation (3.3) to construct \( T_{s,t} \).\(^6\) We now discuss the measurement of the real-time and revised unemployment rates which underlie our construction of \( \hat{T}_{s,t} \). The Local Area Unemployment Statistics (LAUS) program at the Bureau of Labor Statistics (BLS) produces estimates of state-level unemployment rates. Unlike the national unemployment rate, which derives directly from counts from the Current Population Survey (CPS) of households, state unemployment rates incorporate auxiliary information to overcome the problem of small sample sizes at the state level (roughly 1,000 labor force participants for the median state). Better source data and improved statistical methodology imply substantial revisions in the estimated unemployment rate over time.

**Real-time unemployment rate** \( u_{s,t}^* \). The real-time unemployment rate is calculated as the ratio of real-time unemployment to real-time unemployment plus employment. The BLS uses a state space filter to estimate separately real-time counts of unemployed and employed persons (see Section C.1.2

\(^6\)We later show that our main conclusions remain unchanged if the revised unemployment rate also contains measurement error and we provide additional results using an alternative proxy of the true unemployment rate. While the IUR also enters into the determination of \( T_{s,t}^* \), the real-time IUR uses as inputs administrative data on UI payments and covered employment and contains minimal measurement error, with a standard deviation of the real-time error in the IUR of 0.02 percentage point. Since revisions in the IUR do not meaningfully affect \( \hat{T}_{s,t} \) we do not discuss them further.
for additional details). For unemployment the observed variables are the CPS count of unemployed individuals in the state and the number of insured unemployed. For employment the observed variables are the CPS count of employed individuals and the level of payroll employment in the state from the Current Employment Statistics (CES) program. From 2005 to 2014, the procedure also included a real-time benchmarking constraint that allocated pro rata the residual between the sum of filter-based levels across states and the total at the Census division or national level. Finally, in 2010 the BLS began applying a one-sided moving average filter to the state space filtered and benchmarked data.

Revised unemployment rate $u_{s,t}$. The BLS publishes revisions of its estimates of the state unemployment rates. Revisions occur for three reasons. First, the auxiliary data used in the estimation – insured unemployment and payroll employment – are updated with comprehensive administrative data not available in real time.\textsuperscript{7} Second, the BLS incorporates the entire time series available at the time of the revision into its model, replacing the state space filter with a state space smoother and the one-sided moving-average filter with a symmetric filter. Third, the BLS periodically updates its estimation procedure to reflect methodological improvements. Most recently, in 2015 the BLS replaced the external real-time benchmarking constraint with a benchmarking constraint internal to the state space model, improved the treatment of state-specific outliers in the CPS, and improved the seasonal adjustment procedure. Bureau of Labor Statistics (2015) describes these changes as resulting in “more accurate and reliable estimates.” We investigate the importance of different components of the revision process in Section C.1.2 by regressing the unemployment rate measurement error $\hat{u}_{s,t}$ on the components. We find that the 2015 methodological update and the treatment of outliers account for the largest amount of the variation in $\hat{u}_{s,t}$. Importantly, the incorporation of the full time series at the time of revision accounts for very little of the variation in $\hat{u}_{s,t}$.

Implementation

To separate $T_{s,t}^*$ into the component $T_{s,t}$ based on the revised unemployment rate data and the UI error $\hat{T}_{s,t}$, we use the weekly trigger notices produced by the Department of Labor (DOL). The DOL produces each week a trigger notice that contains for each state the most recent available moving

\textsuperscript{7}The revisions to the insured unemployment data reflect corrections of the administrative records, explaining why they are quite small. The annual revision of the CES state employment data replaces state-level real-time monthly employment based on a survey of approximately 400,000 establishments with administrative data derived from tax records covering a virtual universe of private sector employment.
Table 3.2: Accuracy of Our Algorithm for Calculating UI Benefit Extensions

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Original Trigger Notices</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Same as our algorithm</td>
<td>3982</td>
<td>14291</td>
<td>25541</td>
<td>63729</td>
</tr>
<tr>
<td>Different from our algorithm</td>
<td>18</td>
<td>9</td>
<td>9</td>
<td>35</td>
</tr>
<tr>
<td><strong>Corrected Trigger Notices</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Same as our algorithm</td>
<td>3999</td>
<td>14300</td>
<td>25548</td>
<td>63793</td>
</tr>
<tr>
<td>Different from our algorithm</td>
<td>1</td>
<td>0</td>
<td>2</td>
<td>4</td>
</tr>
</tbody>
</table>

Notes: The table reports the number of state-weeks where applying our algorithm to real-time unemployment rate data gives the same UI benefit tier eligibility as the published DOL trigger notices. The top panel compares our algorithm to the raw trigger notices. In the bottom panel, we have corrected the information in the raw trigger notices when we find conflicting accounts in either contemporary media sources or in the text of state legislation.

averages of IUR and TUR, the ratios of IUR and TUR relative to previous years, and information on whether a state has any weeks of EB available and whether it has adopted optional triggers for EB status. During periods with emergency compensation programs, the DOL also produces separate trigger notices with the relevant input data and status determination for the emergency programs. We scraped data for EB notices from 2003-2015 and for the EUC 2008 programs from the DOL’s online repository. The TEUC notices are not available online but were provided to us by the DOL. Finally, the DOL library in Washington, D.C. contains print copies of trigger notices before 2003, which we scanned and digitized. We augment these data with monthly real-time unemployment rates by digitizing archived releases of the monthly state and local unemployment reports from the BLS.

We use the revised unemployment data as of 2015 as inputs into the trigger formulas described in Appendix Table C.1 to calculate \( T_{s,t} \). The UI error then equals \( \hat{T}_{s,t} = T_{s,t}^* - T_{s,t} \). To verify the accuracy.

8The URL for the online data is http://www.oui.doleta.gov/unemploy/claims_arch.asp. The library could not locate notices for part of 1998. We also digitized notices for the EUC program in operation between 1991 and 1994. However, we found only few non-zero UI errors. We, therefore, exclude this period from our analysis and start in 1996, which is the year in which the BLS began using state space models to construct real-time unemployment for all 50 states.

9States have the option of whether or not to adopt two of the triggers for EB status. We follow the actual state laws in determining whether to apply the optional triggers. A complication arises with a temporary change in the law between December 17, 2010 and December 31, 2013. The EB total unemployment rate trigger requires the (three-month) moving average of the unemployment rate in a state to exceed 110% of its level in the same period in either of the two previous years. With unemployment in many states still high at the end of 2010 but no longer rising, Congress temporarily allowed states to pass laws extending the lookback period by an additional year. Many states passed such laws in the week in which the
of our algorithm for constructing $T_{s,t}$, we apply the same algorithm to the real-time unemployment rate data and compare the duration of extensions $T_{s,t}$ implied by our algorithm to the actual duration reported in the trigger notices. Our algorithm does extremely well, as shown in Table 3.2. Of 63,800 possible state-weeks, our algorithm agrees exactly with the trigger notices in all but 7 cases.\footnote{Our algorithm does better than the trigger notices, in the sense that it identifies more than 50 instances where the trigger notices report an incorrect duration or aspect of UI law which we subsequently correct using contemporary local media sources, by comparing to the real-time unemployment rate data reported in LAUS press releases, or by referencing state legislation. We suspect but cannot confirm that the remaining discrepancies also reflect mistakes in the trigger notices. A number of previous papers have relied on information contained in the trigger notices (Hagedorn et al., 2015a,1; Marinescu, 2017; Rothstein, 2011). Our investigation reveals that, while small in number, uncorrected mistakes in the trigger notices could induce some attenuation bias.}

We use the EB program in the state of Vermont to illustrate the two components. Figure 3.1 plots four lines. The blue solid step function shows the additional weeks of benefits available to eligible unemployed in Vermont in each calendar week, $T_{VT,t}$. This series depends on the most recently reported three month moving-average real-time unemployment rate, plotted by the dashed blue line. The red dashed step function shows $T_{VT,t}$, the additional weeks of benefits that would have been two-year lookback period would have implied an expiration of EB. When we use the revised unemployment rate to construct the duration of benefits under the EB program, we find that five states would have lost eligibility for EB earlier than in reality. Therefore, in constructing $T_{s,t}$, we assume that states would have adopted the three-year lookback option earlier had the duration of benefits under the EB program followed the revised rather than the real-time unemployment rate. Specifically, we set to zero the UI error from the EB program in any week in which a state had not adopted the three-year lookback trigger, the state did eventually adopt the three-year lookback trigger, and the UI error would have been zero had the state adopted the three-year lookback trigger in that week. This change affects a negligible fraction of observations in our sample (a total of 20 state-week observations).

Figure 3.1: Extended Benefits and Unemployment in Vermont

Notes: The figure plots the actual duration of benefits $T_{s,t}$, and the duration based on the revised data $T_{s,t}$ (left axis) together with the real-time $u_{s,t}$ and revised unemployment rates $u_{s,t}$ (right axis).
available in Vermont using the revised unemployment rate series plotted by the dashed red line.

Vermont extended its benefits by an additional 13 weeks in the beginning of 2009. Because the real-time and the revised unemployment rates move closely together in this period, Vermont would have triggered an EB extension using either the real-time or the revised data as an input in the trigger formula. The unemployment rate peaks at the end of 2009. As the unemployment rate starts to decline, a UI error occurs. In the beginning of 2010, the real-time unemployment rate temporarily increases by a small amount whereas the revised rate continues to decline steadily. Under the revised data, EB should have been discontinued at the beginning of 2010. However, under the real-time data, EB remained in place until roughly the middle of 2010. The UI error $\hat{T}_{VT,t}$, which is the difference between the blue and red step functions, takes the value of 13 weeks during the first part of 2010. In Section C.1.2 we show that Vermont’s UI error is entirely accounted for by the 2015 methodological improvement in the LAUS statistical model.

### Relationship to Instrumental Variables

The next step in our methodology is to replace actual benefit duration $T_{s,t}^*$ in Equation (3.1) with the UI error $\hat{T}_{s,t}$. This step illustrates the close connection between the UI error approach and an instrumental variables design. Formally, if variation in the measurement error $\hat{u}_{s,t}$ is random to the underlying state-level economic conditions, then the UI error $\hat{T}_{s,t}$ can be used as an instrument for the endogenous variable $T_{s,t}^*$ in regression (3.1). Running the implied first-stage regression $T_{s,t}^* = \pi_0 + \pi_1 \hat{T}_{s,t} + \pi_2 \mathbb{I}\{t \in \text{TEUC02, EUC08}\} + \epsilon_{s,t}$, where $\mathbb{I}$ is an indicator that controls for the common increase in duration of benefits during the two emergency compensation periods, we estimate $\pi_1 = 1.01$ with a standard error of 0.17. Thus, $\hat{T}_{s,t}$ and $T_{s,t}^*$ move one-for-one. A first stage coefficient of 1 makes the second stage of a two-stage least squares regression equal to the reduced form regression which replaces $T_{s,t}^*$ in Equation (3.1) with $\hat{T}_{s,t}$. We impose this first stage coefficient of 1 and work with the reduced form version because it facilitates our treatment of the dynamics in the duration of benefits, as we explain next.

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$^{11}$This statement ignores a subtlety caused by the non-linear mapping which transforms $\hat{u}_{s,t-1}$ into $\hat{T}_{s,t}$. We defer discussion of this subtlety and why controlling for lags of $\hat{u}_{s,t}$ addresses it until we present our estimating equation. An approach conceptually similar to ours is to regress $T_{s,t}^*$ on $T_{s,t}$ and define $\hat{T}_{s,t}$ to be the residuals from this regression. These residuals display a correlation of 0.99 with our measure of $\hat{T}_{s,t}$ obtained as the difference between $T_{s,t}^*$ and $T_{s,t}$.  

71
3.3.2 Serial Correlation of Benefit Duration

The UI error $\hat{T}_{st}$ is a serially correlated process because the underlying measurement error in unemployment $\hat{u}_{st}$ is serially correlated as shown for example in Figure 3.1 for Vermont. When an outcome variable $y_{st+h}$ depends on leads or lags of a persistent right-hand side variable $\{\hat{T}_{st+j}\}_{j=1}^{h}$, a regression of $y_{st+h}$ only on the contemporaneous $\hat{T}_{st}$ confounds the response to $\hat{T}_{st}$ with the effect of lags and leads of the UI error. In economic terms, labor market outcomes in some period may depend on both previous UI errors and expected future UI errors.

We follow the macroeconomic approach of plotting impulse responses with respect to structural innovations to overcome this difficulty. Define the current period unexpected component of the UI error:

$$e_{st} = \hat{T}_{st} - \mathbb{E}_{t-1} \hat{T}_{st},$$

where $\mathbb{E}_{t-1} \hat{T}_{st}$ denotes the expectation of $\hat{T}_{st}$ using information available until period $t-1$. We will estimate the specification:

$$y_{st+h} = \beta(h)e_{st} + \Gamma(h)X_{st} + v_{st+h},$$

where $y_{st+h}$ is an outcome variable in state $s$ and period $t+h$, $e_{st}$ is the UI error innovation in state $s$ and period $t$, and $X_{st}$ is a vector of covariates. Specification (3.6) deals with the correlation between $\hat{T}_{st}$ and past UI errors by replacing it with the serially-uncorrelated innovation $e_{st}$. Thus, the coefficients $\beta(h) = \mathbb{E}[y_{st+h}\vert \hat{T}_{st} = 1, \hat{T}_{st-1}, \hat{T}_{st-2}, \ldots, X_{st}] - \mathbb{E}[y_{st+h}\vert \hat{T}_{st} = 0, \hat{T}_{st-1}, \hat{T}_{st-2}, \ldots, X_{st}]$ for $h = 0, 1, 2, \ldots$ trace out the impulse response function of $y$ with respect to an unexpected one-month increase in the UI error. For labor market variables, this response reflects both the direct contemporaneous effect of increased eligibility due to the UI error and the change in agents’ expectations about future benefit duration. Section 3.5.1 reports the persistence of the increase in actual potential benefit duration following a UI error innovation by setting $y_{st+h} = \hat{T}_{st+h}$ in equation (3.6). We discuss the effect on expectations in Section 3.5.2.

We implement three methods to identify the unexpected component in the UI error $e_{st}$. Our

---

12The persistence in the UI error reflects both the UI law (once triggered onto a tier a state must remain on for at least 13 weeks) and serial correlation in $\hat{u}_{st}$. To give a sense of the latter, the first 8 autocorrelation coefficients of $\hat{u}_{st}$ are 0.78, 0.63, 0.52, 0.45, 0.40, 0.35, 0.32, 0.29.

13Ramey (2016) extensively surveys the use of this approach and Stock and Watson (2017) provide a detailed econometric treatment. Our implementation follows Romer and Romer (1989) and Jordà (2005) in directly estimating the horizon $h$ response to a shock.
preferred approach allows the UI error $\hat{T}_{s,t}$ to follow a first-order discrete Markov chain with probability $\pi_T (\hat{T}_{s,t+1} = x_j \mid \hat{T}_{s,t} = x_i; u_{s,t}, t)$ that $\hat{T}$ transitions from a value $x_i$ to a value $x_j$. A Markov chain is more general than an autoregressive process. Indeed, inspection of the time series of the UI errors in Figure 3.1 reveals a stochastic process better described by occasional discrete jumps than by a smoothly evolving diffusion. The transition probabilities may depend on the unemployment rate and calendar time because the mapping from a measurement error in the unemployment rate to a UI error depends on whether the measurement error occurs in a region of the unemployment rate space sufficiently close to a trigger threshold. In practice, we aggregate $\hat{T}_{s,t}$ up to a monthly frequency and estimate each probability $\pi_T (\hat{T}_{s,t+1} = x_j \mid \hat{T}_{s,t} = x_i; u_{s,t}, t)$ as the fraction of transitions of the UI error from $x_i$ to $x_j$ for observations in the same unemployment rate and calendar time bin. We form a vector of discrete possible values of $x$ from one-half standard deviation wide bins of $\hat{T}_{s,t}$. Finally, once we have estimated the transition probabilities of the Markov process, we calculate the expectation $E_{t-1} \hat{T}_{s,t}$ and form the UI error innovation $\epsilon_{s,t}$ using equation (3.5).  

In sensitivity exercises we show that our results are robust to two alternative processes for $\hat{T}_{s,t}$ which impose additional structure. First, we obtain the innovations by first-differencing the UI error, $\epsilon_{s,t} = \hat{T}_{s,t} - \hat{T}_{s,t-1}$. This transformation is simpler than a first-order discrete Markov chain but comes at the cost of imposing a martingale structure on the UI error. Second, we obtain the innovations as the residual from a regression of $\hat{T}_{s,t}$ on lags of itself (and $X_{s,t}$). This approach imposes smooth autoregressive dynamics on the process for $\hat{T}_{s,t}$ and is equivalent to estimating impulse responses with respect to $\hat{T}_{s,t}$ directly while controlling for lags of the UI error. \footnote{For example, measurement error in the mid-2000s does not cause a UI error for Vermont in Figure 3.1 because the unemployment rate is far below the threshold for triggering an extension of benefits. Conditioning on calendar time reflects the time variation in UI laws and triggers due to enactment of an emergency compensation program.}

\footnote{A trade-off exists between finer partitioning of the state space and retaining sufficient observations to make the exercise non-trivial. We estimate separate transition matrices for each of the following sequential groupings, motivated by the divisions shown in Table C.1: December 2008 – May 2012 and $5.5 \leq u_{s,t} < 7$; December 2008 – May 2012 and $7 \leq u_{s,t} < 8.5$; December 2008 – May 2012 and $u_{s,t} \geq 8.5$; June 2012 – December 2013 and $5.5 \leq u_{s,t} < 7$; June 2012 – December 2013 and $7 \leq u_{s,t} < 9$; June 2012 – December 2013 and $u_{s,t} \geq 9$; January 2002 – December 2003 and $u_{s,t} \geq 5.5$; $u_{s,t} \geq 5.5$; $u_{s,t} < 5.5$. We have experimented with coarser groupings and larger bins of $\hat{T}_{s,t}$ with little effect on our results.}

\footnote{This last approach also ensures orthogonality of the innovation to lagged values of $\hat{T}_{s,t}$ in a finite sample, which the general Markov approach does not. We have verified the innovations under the Markov approach have approximately zero correlation with lagged values of $\hat{T}_{s,t}$ in our sample.}
3.4 Data and Summary Statistics

We draw on a number of sources to obtain data for state-level outcome variables. From the BLS, along with the revised unemployment rate, we use monthly payroll employment from the Current Employment Statistics (CES) program and monthly labor force participation from the LAUS program. The CES data have the advantage of deriving (after revisions) directly from administrative tax records. We obtain data on the number of UI payments across all programs by state and month from the DOL ETA 539 and ETA 5159 activity reports and from special tabulations for the July 2008 to December 2013 period. We obtain monthly data on vacancies from the Conference Board Help Wanted Print Advertising Index and the Conference Board Help Wanted Online Index. We use the first for the years 1996-2003 and aggregate local areas up to the state level. We use the online index for 2007-2015. The print index continues until June 2008 and the online index begins in 2005. However, the two indexes exhibit conflicting trends between 2004 and 2006 as vacancy posting gradually transitioned from print to online and we exclude this period from our analysis of vacancies. Our measure of worker wages, available at quarterly frequency, is the earnings of all and of new workers from the Census Bureau Quarterly Workforce Indicators.

Table 3.3 reports summary statistics. Our sample covers the period between 1996 and 2015 for the 50 U.S. states. The average error in the real-time state total unemployment rate, \( \hat{u}_{s,t} \), is close to zero, with a standard deviation of 0.37 percentage point. Measurement error in the unemployment rate is spread across states and months as its standard deviation changes little after controlling for state and month fixed effects.

A potential concern is that there are too few or too small UI errors to identify significant effects of benefit extensions on macroeconomic outcomes. Table 3.3 shows that this is not true. There are 618 cases in which a state would have had a different duration of extensions using the revised data. Conditional on a UI error occurring, that is \( \hat{T}_{s,t} \neq 0 \), the standard deviation of the UI error is

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17 These are found at http://www.ows.doleta.gov/unemploy/DataDownloads.asp and http://workforcesecurity.doleta.gov/unemploy/euc.asp respectively, last accessed February 10, 2016. The data report the total number of UI payments each month. To express as a share of the total unemployed, we divide by the number of unemployed in the (revised) LAUS data and multiply by the ratio \( 7/\text{[days in month]} \) because the number of unemployed are a stock measure as of the CPS survey reference week.

18 The loss of these years has little effect for our results because these years contain very few UI errors. See Sahin, Song, Topa and Violante (2014) for a description of the vacancy data and a comparison to JOLTS.

19 We exclude months in which a benefit extension program had temporarily lapsed for at least half the month (June, July, and December 2010) and the months immediately following (August 2010 and January 2011).
### Table 3.3: Summary Statistics of Selected Variables

<table>
<thead>
<tr>
<th>Variable</th>
<th>Symbol</th>
<th>Mean</th>
<th>S.D.</th>
<th>Within S.D.</th>
<th>P(25)</th>
<th>P(75)</th>
<th>Obs</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unemployment Rate Error</td>
<td>$\hat{u}_{s,t}$</td>
<td>-0.08</td>
<td>0.37</td>
<td>0.32</td>
<td>-0.29</td>
<td>0.13</td>
<td>11700</td>
</tr>
<tr>
<td>Actual Duration of Benefit Extensions</td>
<td>$T^*_s,t$</td>
<td>3.07</td>
<td>5.04</td>
<td>1.33</td>
<td>0.00</td>
<td>3.23</td>
<td>11700</td>
</tr>
<tr>
<td>UI Error</td>
<td>$\tilde{T}_{s,t}$</td>
<td>0.02</td>
<td>0.49</td>
<td>0.47</td>
<td>0.00</td>
<td>0.00</td>
<td>11700</td>
</tr>
<tr>
<td>UI Error Innovation</td>
<td>$\epsilon_{s,t}$</td>
<td>-0.00</td>
<td>0.31</td>
<td>0.30</td>
<td>-0.00</td>
<td>0.00</td>
<td>11550</td>
</tr>
<tr>
<td>Unemployment Rate (Revised 2015)</td>
<td>$u_{s,t}$</td>
<td>5.55</td>
<td>1.93</td>
<td>0.82</td>
<td>4.20</td>
<td>6.60</td>
<td>11700</td>
</tr>
<tr>
<td>Fraction Unemployed Receiving UI</td>
<td>$\phi_{s,t}$</td>
<td>36.47</td>
<td>16.59</td>
<td>6.71</td>
<td>23.86</td>
<td>45.94</td>
<td>11500</td>
</tr>
<tr>
<td>Log Vacancies (Detrended)</td>
<td>log $v_{s,t}$</td>
<td>0.04</td>
<td>0.27</td>
<td>0.16</td>
<td>-0.15</td>
<td>0.24</td>
<td>7656</td>
</tr>
<tr>
<td>Log CES Payroll Employment (Detrended)</td>
<td>log $E_{s,t}$</td>
<td>0.00</td>
<td>0.03</td>
<td>0.02</td>
<td>-0.02</td>
<td>0.02</td>
<td>11700</td>
</tr>
<tr>
<td>Log Earnings of All Workers (Detrended)</td>
<td>log $w_{s,t}$</td>
<td>0.00</td>
<td>0.03</td>
<td>0.03</td>
<td>-0.02</td>
<td>0.02</td>
<td>3204</td>
</tr>
<tr>
<td>Log Earnings of New Hires (Detrended)</td>
<td>log $\bar{w}_{s,t}$</td>
<td>0.00</td>
<td>0.05</td>
<td>0.04</td>
<td>-0.03</td>
<td>0.03</td>
<td>3066</td>
</tr>
</tbody>
</table>

**Memo:**
- Duration of Benefit Extensions ($\tilde{T}_{s,t} \neq 0$) $T^*_{s,t}$
- UI Error ($\tilde{\epsilon}_{s,t} \neq 0$) $\tilde{\epsilon}_{s,t}$
- UI Error Innovation ($\Delta \tilde{T}_{s,t} \neq 0$) $\epsilon_{s,t}$
- Length of Episode $3.86$

Notes: All variables except for Log Earnings are measured at monthly frequency. Denoted variables have been detrended with a state-specific linear time trend. Within S.D. is the standard deviation of the variable’s residual from a regression of the variable on state and month fixed effects.

The interquartile range is roughly 3.5 months. The fact that there is enough variation in the UI error relative to outcome variables such as the unemployment rate explains the small standard errors of our estimates below.

The average episode of a non-zero UI error lasts nearly 4 months and occurs when benefit extensions already provide an additional 11 months of UI eligibility. Most of these episodes occur during the Great Recession. As already discussed in Section 3.3.2, measurement error in the unemployment rate $\hat{u}$ translates into a UI error $\hat{T}$ only if the state’s unemployment rate is sufficiently near a trigger threshold. This fact explains why we construct $\hat{T}$ rather than using $\hat{u}$ directly and why the UI errors occur mostly in the Great Recession, a period when both the EUC program created additional trigger thresholds and most states had unemployment rates high enough for measurement error in the unemployment rate to translate into a UI error.

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20Throughout the paper, when referring to months of benefit extensions we use the convention that one month equals 4.33 weeks.
3.5 Labor Market Effects of Benefit Extensions

In this section we present impulse responses of labor market outcomes to UI benefit extensions triggered purely by measurement error. To review, our basic approach for overcoming the endogeneity of UI benefit extensions to macroeconomic conditions is to isolate the component of benefit extensions arising from mismeasurement of state unemployment rates in real time, which we denote by $\hat{T}_{s,t}$, and to construct the unexpected and serially uncorrelated component of the UI error, $\epsilon_{s,t}$. Motivated by our investigation of the sources of unemployment rate revisions, we begin our analysis in Section 3.5.1 under the assumption that the measurement error in the unemployment rate underlying $\epsilon_{s,t}$ is random. We discuss the interpretation of the impulse responses in Section 3.5.2. Section 3.5.3 relaxes the assumption that $\epsilon_{s,t}$ is random. In Section 3.5.4 we discuss the possibility of measurement error in the revised unemployment rate and provide auxiliary evidence that the revised unemployment rate better measures true economic conditions than the real-time unemployment rate. Finally, Section 3.5.5 presents additional sensitivity analysis.

3.5.1 Baseline Results

We measure the responses of labor market variables to a one-month UI error innovation $\epsilon_{s,t}$ using the specification:

$$y_{s,t+h} = \beta(h)\epsilon_{s,t} + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + u_{s,t+h}, \tag{3.7}$$

where $y_{s,t+h}$ is an outcome variable in state $s$ and period $t+h$, $\epsilon_{s,t}$ is the UI error innovation in state $s$ and period $t$, and $d_s(h)$ and $d_t(h)$ are state and month fixed effects. Including lags of the unemployment rate as controls approximates the experiment of comparing two states on similar unemployment paths until one receives an unexpected UI error. These covariates also directly address the fact that, even when $\hat{u}_{s,t}$ is strictly exogenous, the non-linear mapping from $\hat{u}_{s,t}$ to $\hat{T}_{s,t}$ depends on $u_{s,t-1}$.\footnote{The mapping is easiest to see in a hypothetical example in which a single extension threshold $\bar{u}$ determines the extension of benefits. In this case, a positive UI error, $T_{s,t} = T_{s,t} - T_{s,t} > 0$, is associated with a low revised unemployment rate, $u_{s,t-1} > \bar{u} > u_{s,t-1}$, and the opposite for a negative UI error. Controlling for the lagged unemployment rate directly addresses any such correlation. Even without controlling for the lagged unemployment rate, this correlation would have a minor affect on our estimates. In results available as supplemental material, we use actual unemployment rate revisions but artificial trigger thresholds to construct artificial UI error series during the late 1990s, a period with very few actual UI extensions. We then estimate regressions of $u_{s,t}$ on $\epsilon_{s,t}$ without controlling for lags of the unemployment rate. These placebo regressions produce coefficients clustered around zero. Moreover, with multiple thresholds the sign of any correlation is ambiguous. The twelve lags of the unemployment rate also directly control for the small increment to the variation in the measurement error $\epsilon$.} We include state and month fixed effects because they increase precision by
absorbing substantial variation in our main outcome variables.

The coefficients $\beta(h)$ for $h = 0, 1, 2, \ldots$ trace out the impulse response function of $y$ with respect to a one-month unexpected change in the UI error. The identifying assumption that $\varepsilon_{s,t}$ is orthogonal to $\nu_{s,t+h}$, $\mathbb{E}[\varepsilon_{s,t} \times \nu_{s,t+h} | \text{controls}] = 0$, is valid if the underlying measurement error in the unemployment rate $\hat{u}_{s,t}$ that gives rise to $\varepsilon_{s,t}$ is random.

Figure 3.2 shows impulse responses of the innovation $\varepsilon$ and the UI error $\hat{T}$ to a one-month innovation $\varepsilon$. In all figures, dashed lines report the 90 percent confidence interval based on standard errors two-way clustered by state and by month. The innovation exhibits essentially no serial correlation. The lack of serial correlation provides support for our choice of modeling $\hat{T}$ as a first-order Markov process.\footnote{Time aggregation from weekly to monthly frequency could explain the small correlation between months $t$ and $t+1$, as an increase in $\hat{T}$ in week 3 or 4 of month $t$ would produce a positive innovation in both $t$ and $t+1$.}

The UI error $\hat{T}$ rises one-for-one with $\varepsilon$ on impact and then decays over the next few months with a half-life of roughly 2 months.

Figure 3.3 illustrates the main result of the paper. The left panel shows the responses from regression (3.7) when the left-hand side variable is the (revised) unemployment rate. The unemployment accounted for by lags of the unemployment rate shown in Table C.2. When we plot impulse responses of $u_{s,t+h}$, we continue to include both the fixed effect and the lagged values of $u_{s,t}$ in an OLS framework since the large time series (more than 200 monthly observations) exceeds the cross-sectional component (Alvarez and Arellano, 2003).

\footnote{Time aggregation from weekly to monthly frequency could explain the small correlation between months $t$ and $t+1$, as an increase in $\hat{T}$ in week 3 or 4 of month $t$ would produce a positive innovation in both $t$ and $t+1.$}
Figure 3.3: Impulse Response of Unemployment Rate and Log Vacancies

Notes: The figure plots the coefficients on $e_{s,t}$ from the regression $y_{s,t+h} = \beta(h) e_{s,t} + \sum_{j=1}^{12} \gamma_j(h) u_{s,t-j} + d_s(h) + d_t(h) + u_{s,t+h}$, where $y_{s,t+h} = u_{s,t+h}$ is the unemployment rate (left panel) or $y_{s,t+h} = \log v_{s,t+h}$ is log vacancies (right panel). The dashed lines denote the 90 percent confidence interval based on two-way clustered standard errors.

The right panel of Figure 3.3 reports the response of vacancy creation. The macroeconomic effect of benefit extensions on unemployment may exceed the microeconomic effect because of a general equilibrium mechanism intermediated by vacancies. The mechanism posits that, following

To give a sense of the small magnitude of the responses, in the same figure we plot a dashed line at roughly 0.14 percentage point. This is the response generated by a version of the standard DMP model (Diamond, 1982; Mortensen and Pissarides, 1994) discussed in Section 3.6 and parameterized in a way that rationalizes a persistent increase of 3.1 percentage points in unemployment caused by the extension of benefits from 6 to 20 months in the Great Recession. Our baseline point estimate is more than 6 standard errors below this level.

The small standard errors reflect the substantial variation in the right-hand side variable $e$ relative to the outcome variable $u$ shown in Table 3.3. To get a back-of-the-envelope estimate of the standard error without controlling for lags of $u$, consider a bivariate regression with a zero coefficient and no clustering. The standard error of the coefficient would be $\frac{1}{\sqrt{N}} \approx 0.023$. This standard error is close to the standard errors we estimate when we two-way cluster by month and state but do not control for lags of $u$. Two-way clustering at the quarter and state level instead of the month and state level to allow for correlation in the residuals across states and over time has almost no effect on the standard errors shown in Figure 3.3. For example, the standard error of the unemployment rate response at the one month horizon would increase from 0.009 to 0.010 and the standard error at the four month horizon is identical up to three decimal places.
the extension of benefits, firms bargain with unemployed who have higher opportunity cost of working. The result is higher wages and lower firm profits from hiring, discouraging vacancy creation (Hagedorn et al., 2015a). However, Figure 3.3 shows that vacancies are unresponsive to a UI error innovation. The dashed line plotted at $-0.045$ denotes the response of log vacancies in the version of the DMP model in Section 3.6 parameterized such that the extension of benefits from 6 to 20 months caused unemployment in the Great Recession to remain persistently high.

Figure 3.4 demonstrates that the absence of a response of unemployment and vacancies occurs despite a higher fraction of the unemployed receiving UI benefits following a UI error innovation. The left panel shows that upon impact, the fraction of unemployed receiving UI benefits increases by 0.5 percentage point. The fraction remains high for the next two months and then declines to zero. This response is reasonable. The innovations in the UI error take place when benefits have, on average, already been extended for roughly 11 months. Using CPS data we estimate that between 0.5 and 1 percent of unemployed would be affected by such an extension, implying a take-up rate in the range of estimates documented by Blank and Card (1991). The right panel of Figure 3.4 splits the increase in UI receipt into recipients on tiers without a UI error (dashed green line with triangles) and recipients on tiers affected by the UI error (solid red line with crosses). All of the additional take-up of UI benefits occurs among individuals on tiers directly affected by the UI error.

Finally, Table 3.4 summarizes the responses of a number of labor market variables. The left panel of the table reports the point estimates and standard errors at horizons 1 and 4 for the variables already plotted along with employment, labor force participation, and worker earnings. The right panel displays results for a slight modification of our baseline regression (3.7) in which we replace the dependent variable with its difference relative to the period before the UI error innovation occurs. If UI error innovations are uncorrelated with lagged outcome variables, then including the dependent variable in either levels or differences will yield a similar coefficient. Across all variables, we find

---

24 We do not have UI receipt by tier for the EB or TEUC02 programs. Therefore, the sample in the right panel of Figure 3.4 starts in 2008 and the sum of the two lines in the right panel does not equal the impulse response in the left panel which is based on the full sample.

25 For $u_{t-1}$, the lags of the unemployment rate included in the baseline regression (3.7) make the differencing with respect to $u_{t-1}$ redundant, but for the other variables we have not imposed a zero effect in $t-1$ in the levels specification of the left panel Table 3.4. We prefer the levels specification in the left panel because of a time-aggregation issue. An increase in $\hat{T}$ in week 4 of month $t-1$ that persists through month $t$ would be associated with an increase in $e_{t}$ and may also be correlated with variables in $t-1$. Indeed, we have already noted the small serial correlation of $e_{t}$ due to this time aggregation issue. The attenuation from differencing with respect to $t-1$ is likely quite small for variables based on the CPS (the unemployment rate and labor force participation rate) or the CES (payroll employment) which use as a reference period the week or pay period containing the 12th day of the month. Likewise, the reference period for the vacancy measure for month $t$ is from mid month in $t-1$ to mid month in month $t$. However, the problem is larger for the fraction of unemployed who receive UI,
3.5.2 Interpretation of Responses

Our results provide direct evidence of the limited macroeconomic effects of increasing the duration of unemployment benefits around the neighborhood of a typical UI error, or by about 3 months after a state has already extended benefits by nearly one year. In this section we discuss the informativeness of this evidence for changes in labor market outcomes in response to other UI policies such as increasing benefits all the way from 26 to 99 weeks as observed in some states after the Great Recession.

We start by performing a linear extrapolation and then discuss the merits of this procedure. Extrapolating linearly the upper bound of a 0.02 percentage point increase in the unemployment rate with respect to a one-month UI error innovation, we conclude that increasing benefits from 26 to 99 would increase the unemployment rate by roughly $0.02 \times 17 \approx 0.3$ percentage point. Similarly,
Table 3.4: Response of Variables to UI Error Innovation

<table>
<thead>
<tr>
<th>Horizon:</th>
<th>Levels</th>
<th>Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>4</td>
</tr>
<tr>
<td>1. Unemployment rate</td>
<td>0.003</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.015)</td>
</tr>
<tr>
<td>2. Log Vacancies</td>
<td>0.003</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.004)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>3. Fraction Receiving UI</td>
<td>0.751**</td>
<td>-0.039</td>
</tr>
<tr>
<td></td>
<td>(0.253)</td>
<td>(0.263)</td>
</tr>
<tr>
<td>4. Log CES Payroll Employment</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td></td>
<td>(0.000)</td>
<td>(0.000)</td>
</tr>
<tr>
<td>5. Labor Force Participation Rate</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.022)</td>
</tr>
<tr>
<td>6. Log Earnings (All Workers)</td>
<td>0.001</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.002)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>7. Log Earnings (New Hires)</td>
<td>-0.000</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.003)</td>
<td>(0.004)</td>
</tr>
</tbody>
</table>

Notes: Each cell reports the result from a separate regression of the dependent variable indicated in the left column on the innovation in the UI error $e_{it}$, controlling for state and period fixed effects and 12 monthly or 4 quarterly lags of $u_{it}$. In the panel headlined “Levels” the dependent variable enters in levels. In the panel headlined “Differences” the dependent variable enters with a difference relative to its value in $t-1$ (rows 1, 3, 4, 5) or $t-2$ (rows 2, 6, 7). Standard errors clustered by state and time period are shown in parentheses. ** denotes significance at the 1% level.

linearly extrapolating a lower bound of $-0.03$ percentage point yields a maximum decrease in the unemployment rate of $0.5$ percentage point for an extension of benefits from 26 to 99 weeks.\textsuperscript{26}

These calculations neglect two potentially important differences between the variation underlying our estimated impulse responses and a typical extension of benefits in the aftermath of the Great Recession. First, the response of labor market outcomes to an extension from a baseline level of 26 weeks may differ from the response to an extension from a baseline level of 70 weeks. Second, the UI errors have lower persistence relative to a policy that increases maximum benefits to 99 weeks as in the Great Recession. We discuss each difference in turn and argue neither appears especially important in practice.

\textsuperscript{26} The lower bound encompasses the estimates of Di Maggio and Kermani (2015) who find a UI output multiplier of 1.9. To compare to Di Maggio and Kermani (2015), note that total EB and EUC payments between 2009 and 2013 were $50.5$ billion, $79.2$ billion, $58.7$ billion, $39.7$ billion, and $22.0$ billion. Applying a multiplier of 1.9 to the peak amount of $79.2$ billion in 2010 gives an increase in output in 2010 of $1.0\%$ of GDP. An application of Okun’s law yields a $0.3$-$0.5$ percentage point decline in the unemployment rate in that year.
Table 3.5: Baseline Duration and Length of Episode

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Unemployment rate</th>
<th>Log vacancies</th>
<th>Fraction receiving</th>
</tr>
</thead>
<tbody>
<tr>
<td>Horizon:</td>
<td>1</td>
<td>4</td>
<td>1</td>
</tr>
<tr>
<td>Panel A:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\epsilon_{s,t}$</td>
<td>0.004</td>
<td>-0.003</td>
<td>0.002</td>
</tr>
<tr>
<td>(0.010)</td>
<td>(0.018)</td>
<td></td>
<td>(0.008)</td>
</tr>
<tr>
<td>$\epsilon_{s,t} \times [T_{s,t} &gt; 10.5]$</td>
<td>-0.002</td>
<td>0.000</td>
<td>0.003</td>
</tr>
<tr>
<td>(0.008)</td>
<td>(0.019)</td>
<td></td>
<td>(0.011)</td>
</tr>
<tr>
<td>Panel B:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\epsilon_{s,t}$</td>
<td>0.008</td>
<td>0.008</td>
<td>0.002</td>
</tr>
<tr>
<td>(0.011)</td>
<td>(0.019)</td>
<td></td>
<td>(0.005)</td>
</tr>
<tr>
<td>$\epsilon_{s,t} \times [\text{Length}_{s,t} &gt; 6]$</td>
<td>-0.020</td>
<td>-0.042</td>
<td>0.003</td>
</tr>
<tr>
<td>(0.012)</td>
<td>(0.027)</td>
<td></td>
<td>(0.012)</td>
</tr>
<tr>
<td>Observations</td>
<td>10,850</td>
<td>10,700</td>
<td>7,084</td>
</tr>
</tbody>
</table>

Notes: Each column of each panel reports the coefficients from a separate regression. All regressions control for state and month fixed effects and 12 lags of $u_{s,t}$. In panel A, the UI error innovation $\epsilon_{s,t}$ is interacted with whether benefit duration without the error exceeds 10.5 months. In panel B, the UI error innovation $\epsilon_{s,t}$ is interacted with whether the length of the episode during which the UI error remains non-zero exceeds 6 months. Standard errors two-way-clustered by state and month and are in parentheses. **, * denote significance at the 1% and 5% levels.

Baseline Level of Benefit Duration

The typical UI error in our sample causes an increase in the maximum potential duration of benefits starting from a baseline level of roughly 16.5 months.27 A concern for the linear extrapolation that we performed may be that labor market variables respond more to benefit extensions occurring around a lower baseline level of duration, as these extensions directly affect the eligibility of a larger fraction of unemployed.

Panel A of Table 3.5 assesses this possibility by allowing the effect of a UI innovation $\epsilon_{s,t}$ in regression (3.7) to vary depending on the baseline level of duration of benefits. Specifically, the table reports the effects on unemployment, vacancies, and claimants of a UI error innovation interacted with whether the extension of benefits occurs when the duration of extended benefits is above 10.5 months (so the duration of total benefits is above the median of 16.5 months). The first four

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27 The variation in the duration of benefits around a baseline level well beyond the 6 months of regular benefits is typical of studies based on cross-state variation. The reason is that cross-state variation in benefit duration concentrates in recessions when the first tier of emergency compensation uniformly increases benefit duration across all states and many states qualify for multiple additional tiers. For example, Hagedorn et al. (2015a) study county border pairs where the potential duration of benefits differs across the two counties. We calculate that the median maximum duration is roughly 16.5 months for the border county with the lower duration in the pair, exactly the same as in our study.
columns show that the effect of a UI error innovation on the unemployment rate and vacancies does
not vary significantly with the baseline duration of extensions. Column (5) shows a larger point
estimate for the fraction of unemployed claiming UI in response to a UI error innovation when the
baseline duration is lower, consistent with an extension from a lower baseline level directly affecting
a larger fraction of unemployed persons (however, this interaction is not statistically significant). The
small response of unemployment and vacancies to a UI error even at low baseline levels of duration
supports the plausibility of a linear extrapolation.

Persistence

The typical extension of benefits is more persistent than a typical UI error in our sample. Let us
start with a discussion of why this difference might not matter. The fraction of the unemployed
who become immediately eligible for benefits does not depend on the persistence of the extension.
Therefore, whether a benefit extension arises due to a UI error or not only affects the immediate
response of unemployment and vacancies insofar as workers and firms have different expectations
of future benefit eligibility depending on the source of the extension. While we do not have direct
evidence on this point, it seems unlikely that agents could distinguish in real-time between an
increase due to the UI error component $\hat{T}_{s,t}$ and an increase due to the component $T_{s,t}$, because
doing so would require agents to know in real-time the unemployment rate error made by the BLS.
If agents do not distinguish the source of a change in benefit extension duration, then the impact
response of labor market variables to a UI error equals the response to a typical extension of benefits
even though realized subsequent extensions may differ. In Section 3.6 we use the structure of the
DMP model to show that the responses we estimated with respect to a one-month UI innovation
imply limited macroeconomic effects of benefit extensions even when agents perceive a benefit
extension caused by a UI error to be more transitory than a benefit extension caused by the increase

---

28 The duration of a typical benefit extension in our data has a half-life of 12.5 months as opposed a half-life of roughly 2.5
months for a typical UI error. While the extensions above 26 weeks around the Great Recession lasted for 5 years, no state
experienced a benefit extension to the maximum of 99 weeks for the whole of the EUC program. Rather, adjustments to the
EUC law frequently changed the maximum potential duration across states and changes in unemployment caused states to
trigger off and on tiers. Moreover, the temporary nature of the authorization for the EUC program meant that during the
Great Recession the average time remaining until the program’s expiration was roughly 5 months.

29 Related to this point, the UI literature contains conflicting evidence on how forward-looking are potential unemployment
a small decline in exit hazard for regular benefit recipients when a benefit extension occurs. Johnston and Mas (Forthcoming)
find evidence of a decline in exit hazard for recipients far from the benefit extension but no effect on the behavior of recipients
within 30 weeks of the extension. Ganong and Noel (2017) find a large decline in consumption when exhaustion occurs,
suggesting agents do not anticipate the exhaustion.
in unemployment.

We next demonstrate that the magnitude of the responses of unemployment and vacancies does not depend significantly on the length of the UI error episode. While this type of evidence does not allow us to directly infer agents’ expectations about the persistence of the UI errors, the stability of responses across different realized lengths of UI error spells is reassuring for the plausibility of a linear extrapolation. Panel B of Table 3.5 reports coefficients from interacting the UI innovation $\varepsilon_{s,t}$ in our baseline regression (3.7) with an episode length of greater than 6 months, where an episode means the length of time a UI error remains non-zero. The median episode of longer than 6 months lasts a total of 11 months. The first two sets of columns show that unemployment and vacancies do not respond differentially during episodes of length greater than 6 months. The small response of unemployment and vacancies to UI errors of greater lengths again enhance the plausibility of a linear extrapolation. The third set of columns shows that the fraction of unemployed who are receiving benefits does increase in the length of the episode and especially at longer horizons. This difference is expected because, by construction, an episode of longer than 6 months has a direct effect on eligibility at the 4 month horizon whereas an episode shorter than 6 months might not.

### 3.5.3 Robustness to Process for $\hat{u}_{s,t}$

In Section 3.3.1, we distinguished among three sources of revisions to the state unemployment rate. One of these, the use of a state space smoother in the revision process, makes the revised unemployment rate in each month dependent on the full available time series of the input variables at the point of revision. This dependence raises a concern that the unemployment rate revision in month $t$ partly depends on realizations of variables after month $t$. Importantly for our empirical design, we found that this source of revisions contributes little to the variation in $\hat{u}_{s,t}$ and hence $\varepsilon_{s,t}$. Nonetheless, we now implement two alternative strategies which remain valid even if the BLS revisions process induces a correlation between $\hat{u}_{s,t}$ and the future path of variables.

**Controlling for $\hat{u}_{s,t}$**

We augment our baseline specification to:

$$y_{s,t+h} = \beta(h)e_{s,t} + g\left(\{\hat{u}_{s,t}\}\right) + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + n_s,t + \epsilon_{s,t+h},$$

(3.8)
where the flexible function \( g(.) \) may allow for leads, lags, and non-linear transformations of the measurement error in the unemployment rate \( \hat{u}_{s,t} \). Specification (3.8) controls directly for any correlation between functions of \( \hat{u}_{s,t} \) and the future path of \( y_{s,t+h} \) which may arise from the revision process.

To build intuition for specification (3.8), it helps to start with the case where \( y_{s,t+h} = u_{s,t+h} \) and \( g(.) = \rho(h)\hat{u}_{s,t-1} \). Recalling that \( \epsilon_{s,t} \) depends on data in period \( t-1 \) due to reporting lags, \( \hat{u}_{s,t-1} \) controls for the measurement error in the unemployment rate during the same month as the data which determine \( \epsilon_{s,t} \). The term \( \rho(h)\hat{u}_{s,t-1} \), therefore, partials out any “normal” covariation between \( \hat{u}_{s,t-1} \) and \( u_{s,t+h} \) which might result from the revision process. The identification exploits the fact that the mapping between \( \hat{u}_{s,t-1} \) and \( \hat{T}_{s,t} \) is not strictly monotonic; there are many instances of measurement error in the unemployment rate which do not give rise to a UI error, as illustrated in Figure 3.1 in the case of Vermont. Formally, the identification assumption becomes \( \mathbb{E} \left[ \epsilon_{s,t} \times v_{s,t+h} \mid X_{s,t}, \hat{u}_{s,t-1} \right] = 0 \). A sufficient condition for this to hold is that any correlation between the unemployment rate measurement error \( \hat{u}_{s,t-1} \) and the future path of unemployment does not change if \( \hat{u}_{s,t-1} \) causes a UI error, except through the direct response of future variables to the UI error. That is,

\[
\mathbb{E} \left[ u_{s,t+h} \mid X_{s,t}, \hat{u}_{s,t-1}, \epsilon_{s,t} = \epsilon \right] = \mathbb{E} \left[ u_{s,t+h} \mid X_{s,t}, \hat{u}_{s,t-1}, \epsilon_{s,t} = 0 \right] + \beta(h)\epsilon = \Gamma(h)X_{s,t} + \rho(h)\hat{u}_{s,t-1} + \beta(h)\epsilon.
\]

Including leads, lags, or non-linear transformations of \( \hat{u}_{s,t-1} \) in the function \( g(.) \) allows for the baseline correlation of \( \hat{u}_{s,t-1} \) and \( u_{s,t+h} \) to vary with the level or path of \( \hat{u}_{s,t} \).

We begin in Figure 3.5 by reporting the impulse response functions for the unemployment rate \( u_{s,t} \) and the total fraction receiving benefits \( \phi_{s,t} \) based on specification (3.8) with only \( \hat{u}_{s,t-1} \) added to the regression. Both impulse response functions appear nearly identical to those without the measurement error in the unemployment rate control. Specifically, the response of unemployment to a positive one-month UI error innovation is essentially zero while the fraction of unemployed receiving UI increases by roughly 0.5 percentage point.

We next allow for more flexible functions of the measurement error in the unemployment rate to enter into the specification. Table 3.6 reports the one and four month responses of the unemployment rate, log vacancies, and the fraction of unemployed receiving UI. Each cell of the table reports the coefficient or standard error on the UI error innovation \( \epsilon_{s,t} \) from a separate regression of the dependent variable in the column header on the UI error innovation, the baseline controls of twelve lags of the unemployment rate and state and month fixed effects, and the additional controls for the measurement error in the unemployment rate shown in the rows. Row 1 reports coefficients when
Figure 3.5: Impulse Responses Controlling for Measurement Error $\hat{u}_{s,t}$

Notes: The figure plots the coefficients on $e_{s,t}$ from the regression $y_{s,t+h} = \beta(h) e_{s,t} + p(h) \hat{u}_{s,t-1} + \Sigma_{j=1}^{12} \gamma_j(h) u_{s,t-j} + d_s(h) + d_t(h) + \epsilon_{s,t+h}$, where $y_{s,t+h} = u_{s,t+h}$ is the unemployment rate (left panel) or $y_{s,t+h} = \phi_{s,t+h}$ is the fraction of unemployed receiving UI on all tiers (right panel). The dashed lines denote the 90 percent confidence interval based on two-way clustered standard errors.

Table 3.6: Sensitivity of Impulse Responses to Controlling for Measurement Error $\hat{u}_{s,t}$

<table>
<thead>
<tr>
<th>Dependent Variable:</th>
<th>Unemployment Rate</th>
<th>Log Vacancies</th>
<th>Fraction Receiving</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>4</td>
<td>1</td>
</tr>
<tr>
<td><strong>Additional controls</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. $\hat{u}_{s,t-1}$</td>
<td>0.011</td>
<td>0.011</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.015)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>2. ${\hat{u}<em>{s,t-1}^{12}</em>{j=12}}$</td>
<td>0.013</td>
<td>0.014</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.015)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>3. $\hat{u}<em>{s,t-1}, \hat{u}</em>{s,t-1}^{2}<em>{s,t-1}, \hat{u}</em>{s,t-1}^{3}_{s,t-1}$</td>
<td>0.010</td>
<td>0.010</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.015)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>4. $\hat{u}<em>{s,t-1} \times I{\hat{u}</em>{s,t-1} \geq 0}$</td>
<td>0.011</td>
<td>0.011</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.015)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>5. $\hat{u}_{s,t-1} \times I{t \in \text{year}}$</td>
<td>0.008</td>
<td>0.005</td>
<td>0.005</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.015)</td>
<td>(0.004)</td>
</tr>
</tbody>
</table>

Notes: Each cell reports the coefficient from a separate regression of the dependent variable indicated in the table header on the UI error innovation $e_{s,t}$, controlling for the baseline control variables, $\Sigma_{j=1}^{12} \gamma_j(h) u_{s,t-j}$, and $d_s(h)$, and the additional control variables indicated in the leftmost column of the row. Standard errors are clustered by state and time period and are reported in parentheses. ** denotes significance at the 1% level.
controlling only for $\hat{u}_{s,t-1}$. Row 2 adds 12 leads and lags of $\hat{u}_{s,t}$. Row 3 incorporates a cubic in $\hat{u}_{s,t-1}$ as a control. Row 4 allows the coefficient on $\hat{u}_{s,t-1}$ to depend on the sign of the unemployment rate measurement error. Row 5 allows the coefficient on $\hat{u}_{s,t-1}$ to vary by year so that $\rho(h)$ could change with the introduction of real-time benchmarking in 2005 or the higher average unemployment during the Great Recession.

Our results do not change significantly in any of these specifications. In particular, the responses of unemployment and vacancies to a UI error innovation are always close to zero and never statistically significantly different from zero, whereas we always detect an increase in the fraction of the unemployed receiving benefits. The stability of the point estimates across specifications and the close similarity to the baseline results shown above reinforces the baseline identifying assumption in Section 3.5.1.

**Alternative Series for $\hat{T}_{s,t}$**

Our second approach is to construct an alternative series for $\hat{T}_{s,t}$ which does not depend at all on the BLS unemployment rate revision process. Instead, we exploit CPS sampling error and generate an alternative unemployment rate series to proxy for the true unemployment rate $u_{s,t}$. Specifically, sampling error in the CPS contributes to a wedge between the insured unemployment rate in administrative records and the insured unemployment rate calculated from the CPS based on the number of CPS respondents reporting unemployment duration of less than 26 weeks and job loss as the reason for unemployment. Thus, we can infer CPS sampling error from the gap between these two measures and remove this sampling error to obtain an alternative proxy for $u_{s,t}$. In practice, we regress the BLS real-time unemployment rate $u_{s,t}^*$ on the contemporaneous and 12 lags of the administrative insured unemployment rate and the labor force share of unemployed ineligible for regular UI taken directly from the CPS (i.e. duration greater than 26 weeks or not job-loser), as well as state and month fixed effects. The $R^2$ of this regression is 0.96. We use the fitted value of this regression instead of the revised unemployment rate to proxy for $u_{s,t}$. We then construct the UI error $\hat{T}_{s,t} = T_{s,t}^* - T_{s,t}$, where in $T_{s,t}$ we use the new $u_{s,t}$ as the input into the mapping $f_{s,t}(\cdot)$, and extract the innovations $e_{s,t}$ from the newly constructed UI error.

We estimate impulse responses using the specification (3.7) but with our alternative series for the UI error innovation $e_{s,t}$. Relative to the baseline results, the implementation here more tightly restricts the variation in UI duration to coming from a particular source of error in the real-time
unemployment rate. By construction, the UI error and the underlying measurement error in the unemployment rate now do not depend on the subsequent path of variables. Figure 3.6 reports the impulse response functions for the unemployment rate and the total fraction receiving benefits based on the alternative series for the UI errors. Once again, both impulse response functions are quite similar to those reported above. The response of unemployment to a positive one-month UI error innovation is statistically indistinguishable from zero while the fraction of unemployed receiving UI increases by roughly 0.8 percentage point.

### 3.5.4 Information Content of Revisions

Our baseline analysis assumes that the revised unemployment rate coincides exactly with the true unemployment rate. Yet, if revisions to the unemployment rate contained little new economic information, then the error component of the benefit duration would be relatively uninformative for estimating the effects of benefit extensions on labor market outcomes. Additionally, even if the revised data better reflect the economy’s fundamentals, whether firms and workers respond to these fundamentals or to the data published in real time matters for the interpretation of our results.

In Appendix C.2 we consider formally the case where the revised unemployment rate also
contains measurement error with respect to the true unemployment rate. We obtain three results. First, the response of a variable to an innovation in \( \hat{T}_{s,t} \) is attenuated toward zero if \( T_{s,t} \), which is based on the revised unemployment rate, differs from the duration one would calculate based on the true unemployment rate. Intuitively, the true UI error is (roughly) a function of the difference between the real-time rate and the true unemployment rate, so if the revised rate equals the true unemployment rate plus random noise then the UI error will inherit that noise. Second, the size of the attenuation bias is decreasing in the variance share of the innovation \( \varepsilon_{s,t} \) generated by true UI errors rather than measurement error in the revised unemployment rate. Third, if the revised unemployment rate is at least as good a measure of the true unemployment rate as the real-time rate, then the bias is bounded above by a factor of 2.

The remainder of this section substantiates the informativeness of the revisions and argues that the revised unemployment rate better measures the true economic fundamental than the real-time rate. We have already presented two types of evidence consistent with the data revisions containing new information. First, Section 3.3.1 and Section C.1.2 described the new source data and methodological improvements incorporated in the revisions process. Second, we would not have obtained the economically significant response of the fraction of unemployed receiving benefits if the revised data added only noise to the real-time estimates.

We now show that the revised unemployment rate better correlates with actual consumer spending. We estimate a horse-race specification:

\[
y_{s,t} = \beta_{\text{revised}} u_{s,t-2}^{\text{revised}} + \beta_{\text{real-time}} u_{s,t-2}^{\text{real-time}} + v_{s,t},
\]

where \( y_{s,t} \) denotes either new auto registrations (from R.L. Polk) or new building permits (from the Census Bureau). Both series reflect spending done by a state’s residents, derive from actual registration data, and have no mechanical correlation with either the real-time or the revised unemployment rate. We interpret the coefficients \( \beta_{\text{revised}} \) and \( \beta_{\text{real-time}} \) as the weights one should assign to the revised and real-time unemployment rates as statistical predictors of spending behavior. The unemployment rates enter the regression with a two-month lag to reflect the timing of the release of the LAUS state unemployment data, which usually occurs for month \( t - 1 \) around the 20th day of month \( t \). Therefore, agents at the beginning of month \( t \) have access to the real-time unemployment rate for month \( t - 2 \) but not for month \( t - 1 \) or \( t \). Agents do not know the revised unemployment rate for \( t - 2 \) at the start of month \( t \), but may respond to the economy’s true fundamentals. Under the
Table 3.7: Spending Decisions and Unemployment Data

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Dependent Variable</strong></td>
<td>Auto Sales</td>
<td>Building Permits</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Revised UR&lt;sub&gt;(s,t-2)\right.</td>
<td>-0.42** (0.11)</td>
<td>-0.52** (0.13)</td>
<td>-0.09** (0.02)</td>
<td>-0.10** (0.02)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Real-time UR&lt;sub&gt;(s,t-2)\right.</td>
<td>-0.34** (0.10)</td>
<td>0.09+ (0.05)</td>
<td>-0.07** (0.02)</td>
<td>0.01 (0.02)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Dep. var. mean</td>
<td>5.4</td>
<td>5.4</td>
<td>5.4</td>
<td>0.5</td>
<td>0.5</td>
<td>0.5</td>
</tr>
<tr>
<td>Dep. var. sd</td>
<td>2.0</td>
<td>2.0</td>
<td>2.0</td>
<td>0.4</td>
<td>0.4</td>
<td>0.4</td>
</tr>
<tr>
<td>(R^2)</td>
<td>0.61</td>
<td>0.61</td>
<td>0.61</td>
<td>0.73</td>
<td>0.76</td>
<td>0.77</td>
</tr>
<tr>
<td>Observations</td>
<td>10,096</td>
<td>9,847</td>
<td>9,847</td>
<td>15,800</td>
<td>12,147</td>
<td>12,147</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is indicated in the table header. The auto sales data come from R.L. Polk and correspond to the state of residency of the purchaser. The permits data are for new private housing units and come from the Census Bureau. Standard errors are clustered by state and month and denoted in parentheses. ***, * denote significance at the 1%, 5%, and 10% level.

Maintained assumption that higher unemployment is associated with lower spending, a finding of \(\beta_{\text{revised}} < 0\) and \(\beta_{\text{real-time}} = 0\) provides support for the joint hypothesis that revised data improve the quality of measurement of economic fundamentals and that agents in real time base their decisions on these fundamentals and ignore the measurement error.

Table 3.7 reports the results. Columns (1), (2), (4), and (5) show that both the revised and the real-time unemployment rates are negatively correlated with spending. The key results are shown in columns (3) and (6) in which we introduce jointly both variables in regression (3.9). For both auto sales and building permits, we estimate \(\beta_{\text{revised}} < 0\) and \(\beta_{\text{real-time}} \approx 0\). The estimates of \(\beta_{\text{revised}}\) are close in magnitude to the estimates in columns (1) and (4) which exclude the real-time rate. Thus, the revised unemployment rate contains all the information about spending patterns and, given knowledge of both series, one should put essentially no weight on the real-time data to predict actual spending.

Survey responses from the Michigan Survey of Consumers (MSC) provide further evidence that the revised unemployment data contains significant new information. The MSC asks 500 respondents each month a series of questions covering their own financial situation and their views on the economy. For survey months in or after the year 2000, the Michigan Survey Research Center allowed
Table 3.8: Beliefs and Unemployment Data

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>AVG</th>
<th>PJOB</th>
<th>PEXP</th>
<th>PINC2</th>
<th>INEX</th>
<th>DUR</th>
<th>CAR</th>
<th>BUS12</th>
<th>BUS5</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
<td>(8)</td>
<td>(9)</td>
<td></td>
</tr>
<tr>
<td>Revised UR_{s,t}</td>
<td>0.028⁺</td>
<td>0.663⁺</td>
<td>0.012</td>
<td>-1.086⁺</td>
<td>-0.186</td>
<td>0.043⁺</td>
<td>0.025</td>
<td>0.007</td>
<td>0.004</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.310)</td>
<td>(0.016)</td>
<td>(0.476)</td>
<td>(0.224)</td>
<td>(0.021)</td>
<td>(0.018)</td>
<td>(0.034)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Real-time UR_{s,t}</td>
<td>-0.015</td>
<td>-0.472</td>
<td>-0.006</td>
<td>0.477</td>
<td>0.042</td>
<td>-0.025</td>
<td>-0.016</td>
<td>0.005</td>
<td>-0.007</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.310)</td>
<td>(0.012)</td>
<td>(0.403)</td>
<td>(0.197)</td>
<td>(0.018)</td>
<td>(0.015)</td>
<td>(0.027)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>State FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Time FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Weighted</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Individual controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Dep. var. mean</td>
<td>-0.01</td>
<td>18.82</td>
<td>2.61</td>
<td>46.02</td>
<td>3.31</td>
<td>2.08</td>
<td>2.22</td>
<td>3.18</td>
<td>3.14</td>
</tr>
<tr>
<td>Dep. var. sd</td>
<td>1.00</td>
<td>25.16</td>
<td>1.31</td>
<td>36.95</td>
<td>16.50</td>
<td>1.73</td>
<td>1.81</td>
<td>1.92</td>
<td>1.79</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.16</td>
<td>0.47</td>
<td>0.83</td>
<td>0.71</td>
<td>0.14</td>
<td>0.64</td>
<td>0.64</td>
<td>0.78</td>
<td>0.79</td>
</tr>
<tr>
<td>Observations</td>
<td>82,291</td>
<td>81,719</td>
<td>80,529</td>
<td>70,036</td>
<td>79,425</td>
<td>78,631</td>
<td>78,626</td>
<td>75,571</td>
<td>79,123</td>
</tr>
</tbody>
</table>

Notes: The dependent variable is indicated in the table header. AVG: simple mean of normalized variables with higher values denoting worse subjective expectations. PJOB: chance will lose job in 5 years. PEXP: personal finances b/w next year (1: Will be better off. 3: Same. 5: Will be worse off). PINC2: percent chance of income increase. INEX: family income expectations 1 year recoded. DUR: durables buying attitudes (1: Good. 3: Pro-con. 5: Bad). CAR: vehicle buying attitudes (1: Good. 3: Pro-con. 5: Bad). BUS12: economy good/bad next year (1: Good times. 2: Good with qualifications. 3: Pro-con. 4: Bad with qualifications. 5: Bad times). BUS5: economy good/bad next 5 years (1: Good times. 2: Good with qualifications. 3: Pro-con. 4: Bad with qualifications. 5: Bad times). Individual controls: sex, marital status, age, age^2, age^3, four educational attainment categories, and log income, each interacted with month. Regressions are weighted using survey weights. Standard errors are clustered by state and month and denoted in parentheses. ⁺,⁺ denote significance at the 5% and 10% level.

us to merge external state-level data to anonymized responses. Because sample sizes are too small to aggregate to the state-month level, we instead run our horse-race regression at the individual level and cluster standard errors by state and by month:

$$y_{i,s,t} = \beta^{\text{revised}} u^{\text{revised}}_{s,t} + \beta^{\text{real-time}} u^{\text{real-time}}_{s,t} + \Gamma X_{i,s,t} + v_{i,s,t}. \quad (3.10)$$

Table 3.8 reports results for a subset of questions in the survey that we expect to correlate with the local unemployment rate. For brevity, we report only specifications with both unemployment rates. Averaging across the eight outcomes we consider, the first column shows that a higher revised unemployment rate is associated with worse subjective perceptions of economic conditions. It also shows that, conditional on the revised unemployment rate, the real-time unemployment rate appears to add no information. This result repeats in various individual outcomes as shown in columns (2) to (9).

To summarize, the results in Tables 3.7 and 3.8 provide direct additional evidence that the
Table 3.9: Sensitivity of Impulse Responses to Alternative Specifications

<table>
<thead>
<tr>
<th>Regressor Controls</th>
<th>Unemployment Rate</th>
<th>Log Vacancies</th>
<th>Fraction Receiving</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>4</td>
<td>1</td>
</tr>
<tr>
<td>horizon: 1</td>
<td>0.003</td>
<td>-0.003</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.015)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>horizon: 1, 4</td>
<td>0.003</td>
<td>-0.003</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.009)</td>
<td>(0.015)</td>
<td>(0.004)</td>
</tr>
<tr>
<td>horizon: 0.014</td>
<td>-0.014</td>
<td>-0.023</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.026)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>horizon: 0.003</td>
<td>-0.003</td>
<td>-0.029</td>
<td>0.015</td>
</tr>
<tr>
<td></td>
<td>(0.078)</td>
<td>(0.077)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>horizon: 0.004</td>
<td>0.004</td>
<td>0.004</td>
<td>-0.000</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.012)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>horizon: 0.002</td>
<td>0.002</td>
<td>-0.003</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.014)</td>
<td>(0.005)</td>
</tr>
<tr>
<td>horizon: 0.006</td>
<td>0.006</td>
<td>0.011</td>
<td>-0.003</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.012)</td>
<td>(0.003)</td>
</tr>
</tbody>
</table>

Notes: Each cell reports the coefficient from a separate regression of the dependent variable indicated in the table header on the right-hand side variable indicated in the leftmost column of the row, controlling for the variables indicated in the second column of the row. Standard errors are clustered by state and time period and are reported in parentheses. **, * denote significance at the 1% and 10% level.

revised data better align with true economic fundamentals than the real-time data. Therefore, the conservative upper bound for the possible attenuation bias derived in Section C.2 holds. Applying this upper bound to the confidence interval upper bound of a 0.02 percentage point increase in the unemployment rate in response to a one-month UI error yields a maximum response of 0.04 percentage point.

3.5.5 Further Robustness

In this section we investigate the robustness of our main findings along various other dimensions. Table 3.9 compares the one and four month responses of the unemployment rate, log vacancies, and the fraction of unemployed receiving UI in the baseline specification to the responses in alternative specifications. The first row of the table repeats the baseline results from Section 3.5.1.

Rows 2 and 3 assess the practical importance of controlling for lags of the unemployment rate. In
row 2, we additionally control non-parametrically for the lagged unemployment rate by partitioning the lagged unemployment rate into 0.5 percentage point wide bins and adding indicator variables for whether the lagged rate lies in each bin. Row 3 removes the lags of the unemployment rate from the baseline specification. In all cases, we obtain very similar results to the baseline. Row 4 removes the lags of the unemployment rate and the state $d_s$ and month $d_t$ fixed effects from the specification (so there are no controls). We again find similar point estimates. However, the standard errors more than double in row 3 with no controls because fixed effects absorb a large fraction of the variation in outcome variables unrelated to the UI error innovation.

Next, we assess the robustness of our results to the assumed process for the UI errors used in extracting the innovations. To account for the sparsity and non-linearity of the UI error process, our preferred approach imposes a first-order Markov process that generalizes the autoregressive persistence usually imposed on macroeconomic data. In row 5, we instead simply first difference the UI error and replace $\epsilon_s, t$ in equation (3.7) with $\Delta \hat{T}_s, t = \hat{T}_s, t - \hat{T}_s, t-1$. In row 6, we report the coefficient on the level of $\hat{T}_s, t$ but controlling for twelve lags of $\hat{T}_s, t$. This specification is conceptually similar to defining the UI error innovation as the structural residual from a vector autoregression in $\hat{T}_s, t$ and $u_s, t$ with twelve lags and $\hat{T}_s, t$ first in a Cholesky ordering. The limited response of unemployment and vacancies and the significantly positive response of the fraction receiving UI remain robust to these alternative specifications.

Finally, the last row of Table 3.9 shows a control function specification in which we regress outcomes $y_s, t+h$ on the change in the observed UI duration $\Delta T^*_s, t$ controlling for the change in the UI duration $\Delta T_s, t$ based on the revised data, the lags of unemployment, state dummies, and monthly dummies. Because we control for changes in UI duration due to fundamentals with $\Delta T_{s,t}$, the remaining variation in $\Delta T^*_{s,t}$ reflects changes in UI benefit duration that arises from measurement error only. As the last row shows, the effects of unemployment and vacancies remain small and statistically insignificant whereas the response of the fraction of unemployed receiving UI increases slightly.

---

30Formally, after demeaning with respect to the state and month fixed effects, the specification is a Jordà (2005) local projection based on a bivariate system in $u_s, t$ and $\hat{T}_s, t$ with twelve lags and $\hat{T}_s, t$ being first in the Cholesky ordering. The Cholesky identification assumption is that the forecast error in $\hat{T}_s, t$ does not respond to the contemporaneous structural innovation to the unemployment rate. As a justification for the ordering, recall that due to reporting lags UI benefits in month $t$ are only a function of unemployment rates for month $t-1$ and earlier.

31We thank an anonymous referee for suggesting the similarity of our approach to a control function approach.
3.6 DMP Model with Benefit Extensions

Our empirical estimates suggest a small macroeconomic effect of extending benefits. In this section we interpret these results through the lens of a standard DMP model (Diamond, 1982; Mortensen and Pissarides, 1994). The model illustrates the basic logic of why benefit extensions might lead to higher unemployment. We use it to assess the sensitivity of our conclusions when workers and firms perceive a UI error to be more transitory than a benefit extension caused by a persistent increase in the unemployment rate and to extensions which persist for longer than a year as in the aftermath of the Great Recession. Additionally, we show that in the model the extension of benefits from 26 to 99 weeks does not introduce a significant degree of non-linearity, corroborating the results in Section 3.5.2 for the stability of the responses of labor market variables to UI errors at different baseline levels of duration.

We augment a standard DMP model with a UI policy. The model shares many features with the models used by Hagedorn et al. (2015a) and Mitman and Rabinovich (2014) to argue that benefit extensions cause unemployment to remain persistently high following a negative shock. We reach a different conclusion because our empirical estimates imply a lower level of the opportunity cost of employment in the model than assumed by these papers.\(^{32}\) We describe here only the elements of the model essential to our argument and provide additional detail in Appendix C.3.

Each period a measure \(u_t\) of unemployed search for jobs and a measure \(1 - u_t\) of employed produce output. Unemployed individuals find jobs at a rate \(f_t\) which is determined in equilibrium. Employed individuals separate from their jobs at an exogenous rate \(d_t\). Employed individuals who lose their jobs become eligible for UI benefits with probability \(g_t\). Unemployed who are eligible for UI and do not find jobs lose their eligibility with probability \(e_t\). The key policy variable in our model is the (expected) duration of benefits \(T_t^*\) which equals the inverse of the expiration probability, \(T_t^* = 1/e_t\). Ineligible unemployed who do not find jobs remain ineligible for UI benefits.

Risk-neutral individuals discount the future with a factor \(\beta\). Employed individuals consume their wage earnings \(w_t\). The value of an individual who begins period \(t\) as employed is given by \(W_t\). Ineligible unemployed derive a flow value from non-market work equal to \(\zeta\). The value of an individual who begins period \(t\) as ineligible is \(U_t^I\). Eligible unemployed additionally receive a UI benefit \(B\). The value of an individual who begins period \(t\) as eligible is \(U_t^E\). We define the value of the

\(^{32}\)This result echoes Costain and Reiter (2008), who point out that models with a high level of opportunity cost generate stronger effects of policies on labor market outcomes than the effects found in cross-country comparisons.
average unemployed individual as \( U_t = \omega_t U_t^E + (1 - \omega_t) U_t^I \), where \( \omega_t \) is the fraction of unemployed who are eligible for and receive UI.

The surplus of employment for the average unemployed is given by the difference between the value of working and the value of unemployment, \( S_t = W_t - U_t = w_t - z_t + \beta(1 - \delta_t - f_t) E_t S_{t+1} \), where \( z_t \) denotes the flow opportunity cost of employment for the average unemployed:

\[
z_t = \xi + \omega_t B - (\delta_t(\gamma - \omega_t) + (1 - f_t) \omega_t e_t) \beta \left( E_t U_t^{E} - E_t U_t^{I} \right),
\]

(3.11)

In equation (3.11), \( \xi \) denotes the flow value of non-market work and \( b_t \) denotes the benefit component of the opportunity cost of employment. This expression nests the corresponding expression for the opportunity cost in the standard DMP model (for instance, Shimer, 2005) where \( b_t = B \) if \( e_t = 0 \) and \( \gamma = \omega_t = 1 \), that is when all unemployed receive benefits. More generally, \( b_t \) is lower than the benefit \( B \). The difference occurs because some unemployed are not eligible for benefits and, even for those unemployed who are eligible, benefits eventually expire.\(^3\)

Extending benefits, which here means a decline in the expiration probability \( e_t \), increases the fraction of unemployed who are eligible \( \omega_t \) and raises \( b_t \) and \( z_t \).

The value of a firm which has matched with a worker is given by \( J_t = p_t - w_t + \beta(1 - \delta_t) E_t J_{t+1} \), where \( p_t \) denotes aggregate labor productivity. Free entry drives the expected value of creating a vacancy to zero, giving \( \xi = \beta E_t J_{t+1} \), where \( \xi \) denotes the upfront cost that an entrant pays to create a vacancy and \( q_t \) denotes the rate at which vacancies are filled. A constant returns to scale matching technology \( m_t = m_t(u_t, v_t) \) converts job seekers and vacancies into new matches. Denoting market tightness by \( \theta_t = v_t / u_t \), an unemployed matches with a firm at rate \( f_t(\theta_t) = m_t / u_t \) and firms fill vacancies at rate \( q_t(\theta_t) = m_t / v_t = f_t(\theta_t) / \theta_t \).

Firms and workers split the surplus from an additional match according to the generalized Nash bargaining solution. We denote by \( \mu \) the bargaining power of workers. The wage is chosen to maximize the product \( S_t^{\mu} J_t^{1-\mu} \). This leads to a standard wage equation:

\[
w_t = \mu p_t + (1 - \mu) z_t + \mu \kappa \theta_t.
\]

(3.12)

The duration of UI benefits is given by \( T_t^* = T_t + \hat{T}_t \), where \( T_t \) denotes the duration of UI benefits in the absence of any measurement error and \( \hat{T}_t \) is the UI error. Consistent with the results in

\(^3\)The first effect is captured by the first term of \( b_t \) which is lower than \( B \) when \( \omega_t < 1 \). The second effect is captured by the second term which is positive because \( \gamma > \omega_t \) and \( E_t U_t^{E} > E_t U_t^{I} \).
Section 3.5.4 that agents respond only to the revised unemployment rate, we assume that firms and workers know the underlying fundamentals (for instance, $u_t$, $p_t$, $w_t$ etc.) at the beginning of each period. The statistical agency makes errors in the measurement of the true unemployment rate which result in UI errors $\hat{T}_t$. Thus, agents distinguish in real time between extensions caused by UI errors and extensions caused by true fundamentals.

We now discuss the effects of UI policy in this model. An increase in the current duration of benefits affects equilibrium outcomes to the extent that firms and workers expect it to persist in future periods. Combining the definition of firm’s value $J_t$ with the free entry condition, the decision to create a vacancy in the current period depends on the expectation of the present discounted value of firm profits:

$$\frac{\kappa}{q_t(\theta_t)} = \mathbb{E}_t \left[ \sum_{j=1}^{\infty} \beta^j \left( \prod_{i=1}^{l} \frac{(1 - \delta_{i+j-1})}{(1 - \delta_i)} \right) (p_{t+j} - w_{t+j}) \right],$$

(3.13)

where $q_t(\theta_t)$ is a decreasing function of current market tightness $\theta_t = v_t / u_t$. By raising the fraction of unemployed who are eligible for UI, an extension of benefits increases future opportunity costs and wages as shown in Equation (3.12). Higher wages lower the expected present value of firm profits and decrease firms’ willingness to create vacancies. Fewer vacancies make it more difficult for the unemployed to find jobs, increasing the unemployment rate.\(^{34}\)

We parameterize two versions of the model (see Appendix C.3 for more details). In the “low $b$” model we pick $b = 0.06$ and $z = \zeta + b = 0.87$ in the steady state of the model. The value of $b = 0.06$ accords with the finding in Chodorow-Reich and Karabarbounis (2016) that benefits comprise a small fraction of the average opportunity cost. In the “high $b$” model we pick $b = 0.15$ and $z = \zeta + b = 0.96$. The value of $z = 0.96$ was chosen by Hagedorn and Manovskii (2008) to target the sensitivity of wages with respect to productivity in the aggregate data.

Figure 3.7 plots the impulse of the unemployment rate with respect to a one-month UI error innovation using model-simulated data. As described above, an extension of UI benefits reduces firm profits from filling a vacancy. In the high $b$ model, firm profits are very small on average because average match surplus – the difference between the marginal product and the opportunity cost of employment – is small. Therefore, the extension of benefits lowers firms’ willingness to create vacancies substantially. As the left panel of Figure 3.7 shows, the maximal response of the

\(^{34}\)In our model all workers have the same job-finding rate irrespective of UI eligibility, a point we return to in the conclusion. Allowing UI policy to affect worker search intensity in this model would further discourage vacancy creation by reducing the job-filling rate as well as directly increase unemployment by lowering the match rate for a given number of vacancies and unemployed.
unemployment rate is close to 0.14 percentage point in the high $b$ model. In the low $b$ model depicted in the right panel, the unemployment rate increases by less than 0.02 percentage point. With a low $b$, firm profits are on average higher and the extension of benefits leads to smaller movements in equilibrium vacancies and unemployment.

We next examine the effects of a benefit extension caused by a recession rather than by measurement error. For this experiment, we shut down all UI errors and set $\hat{T}_t = 0$ for all periods. We start each of the low $b$ and high $b$ economies in a stochastic steady state in which no shock occurs for a large number of periods. Beginning in month 10, we introduce a sequence of productivity and separation shocks chosen so that unemployment reaches roughly 10 percent with benefit extensions turned on. Appendix C.3 reports the paths of these shocks.

The left panel of Figure 3.8 plots the paths of unemployment in the high $b$ model with and without a benefit extension policy. The upper line shows the path when benefit extensions follow a policy rule similar to that in place during the Great Recession, so that the duration of benefits rises from 6 to eventually 20 months. Unemployment peaks at roughly 10 percent and remains persistently high. The lower line shows the path of unemployment in an alternative UI policy regime where the duration of benefits always equals $T_t^* = T_t = 6$ months. Consistent with the conclusions of Mitman and Rabinovich (2014) and Hagedorn et al. (2015a), the difference between the two lines
shows the large effect that benefit extensions have on the path of the unemployment rate in the DMP model with a high $b$.

By contrast, the right panel of Figure 3.8 shows a much smaller effect of benefit extensions on unemployment dynamics. As in the high $b$ model, the duration of benefits increases to 20 months as soon as the unemployment rate exceeds 9 percent. However, the level of the opportunity cost is small on average and, therefore, this extension does not affect significantly the path of the unemployment rate. The average distance between the two unemployment paths is less than 0.3 percentage point, close to the linear extrapolation in Section 3.5.2. Because only the low $b$ model matches the response of unemployment to a one-month UI error innovation, the results in Figure 3.8 validate the limited influence of UI extensions in a DMP model with large and persistent benefit extensions.

### 3.7 Conclusion

Identifying the effect of UI benefit extensions on macroeconomic outcomes is challenging because benefits are extended in times of elevated unemployment. This simultaneity happens both because U.S. law makes benefit extensions a function of state economic conditions and because policymakers enact emergency compensation in recessions. We show how to use data revisions to decompose
variation in the duration of benefits over time and across states into the part coming from actual differences in economic fundamentals and the part coming from measurement error in the real-time data used to determine benefit extensions. This methodology is potentially applicable to other policy variables which depend on measured economic conditions, other outcome variables, or in different countries.\footnote{For example, states with high unemployment rates can receive waivers for the cap on the number of months an able-bodied adult without benefits can receive SNAP benefits (food stamps) in the United States and many countries extend UI benefits based on regional unemployment rates.}

Using only the measurement error component for identification, we find an economically reasonable increase in the number of individuals receiving UI, but only a limited influence of benefit extensions on key state-level macroeconomic outcomes including unemployment, employment, vacancies, and wages. Our results imply that the unprecedented increase in benefits during the Great Recession contributed at most 0.3 percentage point to the increase in the unemployment rate.

A standard DMP model can rationalize this small response if the opportunity cost of giving up benefits is low for the average unemployed. Other economic channels which may also explain the limited influence of benefit extensions we measure in the data include an offsetting stimulus effect from transferring resources to unemployed individuals with high marginal propensity to consume, labor market spillovers as lower search effort by UI recipients raises job finding rates for non-recipients, and wage bargaining protocols that do not depend on the opportunity cost of employment. Quantifying each of these channels separately would be a valuable step for future research. On the other hand, we know of no labor market theory in which UI extensions substantially raise unemployment without requiring a high opportunity cost of giving up benefits and a much larger response of unemployment to a UI error than we measure in the data.

In this paper we do not estimate how individual-level outcomes respond to benefit extensions. Recent studies have found mixed effects at the individual level (Farber and Valletta, 2015; Johnston and Mas, Forthcoming; Rothstein, 2011). Can one reconcile the small macroeconomic effects we find with those studies which find larger microeconomic effects such as Johnston and Mas (Forthcoming)? Models with job rationing provide one such avenue. In these models, the job finding rate of UI recipients declines, giving large microeconomic effect, but the job finding rate of non-recipients increases due to the declining competition in the job market. Such displacement effects are consistent with the findings of Crepon \textit{et al.} (2013) and Lalive \textit{et al.} (2015) among others.

Finally, the microeconomic function of UI is to provide income replacement for individuals who
have lost their jobs. The value of this insurance mechanism may increase in the duration of an
unemployment spell as individuals draw down on their assets and other sources of income. The
results in this paper do not speak to this income support function nor to the microeconomic rationale
for increasing insurance during recessions when the typical duration of unemployment spells rises.
Our results simply say that UI extensions do not have large negative macroeconomic effects.
References


Appendix A

Appendix to Chapter 1

A.1 Additional Results

Quality of Life

Table A.1: Differences in Quality of Life

<table>
<thead>
<tr>
<th></th>
<th>Dropouts</th>
<th>In-and-outs</th>
<th>Always Participators</th>
</tr>
</thead>
<tbody>
<tr>
<td>Life Satisfaction (1-10)</td>
<td>5.9</td>
<td>6.3</td>
<td>7.1</td>
</tr>
<tr>
<td>% Took Pain Medication Yesterday</td>
<td>61</td>
<td>29</td>
<td>19</td>
</tr>
<tr>
<td>% With Any Physical or Cognitive Difficulty</td>
<td>58</td>
<td>24</td>
<td>8</td>
</tr>
<tr>
<td>% With Physical Difficulty</td>
<td>40</td>
<td>8</td>
<td>1</td>
</tr>
<tr>
<td>% With Mobility Difficulty</td>
<td>26</td>
<td>4</td>
<td>0</td>
</tr>
<tr>
<td>% With Difficulty Remembering</td>
<td>21</td>
<td>5</td>
<td>0</td>
</tr>
<tr>
<td>% Very Well Rested</td>
<td>27</td>
<td>42</td>
<td>38</td>
</tr>
</tbody>
</table>

Geographic Distribution

Figure A.1: Changes in Participation Across States

Source: IPUMS CPS matched longitudinally, 1977–2015. Slope is reported with heteroskedasticity-robust standard error in parentheses. All statistics are computed using survey weights.
## Industries and Occupations

### Table A.2: Rise of In-and-Outs Across Industries

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Agriculture</td>
<td>6.5</td>
<td>Real Estate</td>
<td>0.2</td>
</tr>
<tr>
<td>Mining</td>
<td>1.6</td>
<td>Professional Services</td>
<td>3.6</td>
</tr>
<tr>
<td>Utilities</td>
<td>7.1</td>
<td>Administrative Services</td>
<td>0.3</td>
</tr>
<tr>
<td>Construction</td>
<td>3.0</td>
<td>Education</td>
<td>8.0</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>3.6</td>
<td>Health</td>
<td>2.8</td>
</tr>
<tr>
<td>Wholesale Trade</td>
<td>3.3</td>
<td>Entertainment</td>
<td>1.1</td>
</tr>
<tr>
<td>Retail Trade</td>
<td>6.3</td>
<td>Food and Hospitality</td>
<td>1.0</td>
</tr>
<tr>
<td>Transportation</td>
<td>4.9</td>
<td>Other Services</td>
<td>4.5</td>
</tr>
<tr>
<td>Information</td>
<td>7.0</td>
<td>Public Administration</td>
<td>1.2</td>
</tr>
<tr>
<td>Finance and Insurance</td>
<td>1.6</td>
<td>Multiple or Unknown</td>
<td>4.5</td>
</tr>
</tbody>
</table>

Source: IPUMS CPS, 1977–2015. In-and-outs are defined as men ages 25–54 with between 1 and 7 months in the labor force out of 8 total. Individuals are categorized to industries based on the industry (or industries) they work in when employed. Individuals who are not employed in any of their 8 CPS responses are excluded. For each industry, I report the increase in the share of individuals within that industry who are in-and-outs between 1977 and 2015. All statistics are computed using survey weights.
<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Management, Professional, Technical</td>
<td>2.3</td>
<td>Production and Crafts</td>
<td>4.8</td>
</tr>
<tr>
<td>Admin. Support &amp; Retail Sales</td>
<td>3.5</td>
<td>Machine Operators</td>
<td>3.7</td>
</tr>
<tr>
<td>Low-Skill Services</td>
<td>3.6</td>
<td>Construction, Mining, Agriculture</td>
<td>3.7</td>
</tr>
</tbody>
</table>

Source: IPUMS CPS, 1980–2015. In-and-outs are defined as men ages 25–54 with between 1 and 7 months in the labor force out of 8 total. Individuals are categorized to occupations based on the occupation of their job when they are employed. Individuals who are not employed in any of their 8 CPS responses are excluded. For each occupational category, I report the increase in the share of individuals within that category who are in-and-outs between 1980 and 2015. All statistics are computed using survey weights.
Tax Rates

Figure A.2: Income Tax Rates of In-and-Outs

Source: IPUMS monthly CPS matched to IPUMS March CPS, 1989–2015. Tax rates are computed for all individuals in the sample using NBER’s TAXSIM calculator (details in Appendix A.2). These individual-level tax rates are then averaged across all in-and-outs. All statistics are computed using survey weights.

A.2 Datasets

This section describes several aspects of the datasets I construct for this analysis. First, I describe how I construct each of the samples I use to measure the growth of temporary nonparticipation. Then, I discuss the methods used to link CPS responses both longitudinally over time as well as between the monthly CPS and March CPS supplement. Next, I describe how I construct wage series along with several alternative imputation procedures. Finally, I outline how I estimate tax rates for individuals.

A.2.1 Construction

Survey of Income and Program Participation (SIPP) The SIPP is a nationwide longitudinal survey of individuals, organized into panels lasting typically two to five years. I use monthly
interviews from each panel of the SIPP conducted between 1984 and 2008, excluding the short 1989 panel. Monthly labor force participation is defined analogously to the CPS definition.\textsuperscript{1} I label an individual as being temporarily out of the labor force for all nonparticipation spells lasting less than twenty four consecutive months.

**Current Population Survey (CPS)** Using the method developed by Drew, Flood and Warren (2014) implemented in IPUMS CPS (Flood, King, Ruggles and Warren, 2017), I match individuals’ responses across interviews to construct a panel dataset. I restrict the sample to men ages 25–54 who can be matched successfully across all eight interviews, which is about 60\% of all prime age men in the sample. These individuals are slightly more attached to the labor force on average compared to all prime age men, but they have experienced nearly the same decline in participation as the overall population of prime age men. To capture temporary nonparticipation spells of less than twenty four months, I report the share of individuals who are currently out of the labor force, but previously and subsequently are in the labor force for at least one month in the CPS. Since the CPS interviews cover a sixteen-month span, this is a conservative measure of the total amount of temporary nonparticipation.

**Panel Study of Income Dynamics (PSID)** The PSID is a longitudinal survey of families that began in 1968 and has continued to interview the same families, as well as their descendants and new family members, over several decades. I use all responses to the PSID through 2013, which includes annual responses from 1968–1997 and biennial responses afterwards. The sample includes all men ages 25–54 in the original sample as well as those who join original PSID families for the years in which they are interviewed. Labor force participation is measured at the time of the interview and is defined analogously to the CPS definition.\textsuperscript{2} For observations before 1997, I label an individual as an in-and-out if he responded as being in the labor force for one or two of the last three years. For observations after 1997, I label an individual as an in-and-out if he responded as being in the labor force for exactly one of the last two interviews (which cover a three year period). In computing the growth of in-and-outs, I compute growth separately before and after 1997 to allow for a structural

\textsuperscript{1}I denote an individual as employed in a week if he had a job or business, including if he was absent from work. I denote an individual as being unemployed if he is actively looking for work or on temporary layoff in a week. Individuals are counted as being in the labor force in a month if they are employed or unemployed for at least one week out of the month.

\textsuperscript{2}Individuals are counted as being in the labor force if they are either employed or unemployed at the time of the interview. Unemployed individuals include those on temporary layoff.
break when this definition changes.

Social Security Administration (SSA) Earnings Public-Use File  The SSA Earnings Public-Use File contains annual earnings records for a 1% sample of all individuals issued Social Security numbers prior to 2007. I create measures of annual participation from SSA records of men ages 25–54 covering the 1960–2006 period.\(^3\) I classify an individual as being in the labor force in a given year if his annual earnings exceed half the minimum wage times 40 hours per week times 13 weeks per year. I label an individual as an in-and-out if he participated for one or two of the last three years.

March CPS Supplement  Starting in 1976, the March CPS asked respondents to report the number of weeks (out of 52) that an individual was employed or looking for work in the prior calendar year. I label an individual as an being temporarily out of the labor force if that individual reports being in the labor force for between one and fifty-one weeks during the prior calendar year. I also use a more restricted sample limited to individuals whose March CPS responses can be matched to eight basic monthly CPS responses, which is limited to responses from 1989–2015 (matching details are reported in the next section).

A.2.2 Linking Responses

I link CPS data in two ways. First, I link responses to the monthly CPS longitudinally over time. Second, I link responses from the monthly CPS in March to the March CPS supplement.

Longitudinal Linking  The CPS has a 4-8-4 rotation group structure whereby individuals are interviewed for 4 consecutive months, rotate out for 8 months, and then are interviewed for another 4 months. As a result, individuals’ responses can be matched to form a panel of 8 months covering a 16 month period. However, not all individuals can be successfully matched, since respondents are not followed if they move addresses or stop responding to the survey.

I use the method of Drew et al. (2014) to match individuals’ responses across CPS interviews, as implemented in the IPUMS CPS Flood et al. (2017). This approach relies on mechanical matches between interviews based on longitudinal links provided in the CPS, as opposed to matching based

\(^3\)While the dataset contains earnings records starting in 1951, these records are unusually volatile in the mid-1950s and produce a sharp break in the share of in-and-outs between 1958 and 1959, possibly due to changes in measurement. To avoid this break affecting the estimated growth of in-and-outs, I drop records from before 1960.
on characteristics. In most years, about 60% of prime age men can be matched for all 8 responses, similar to the rates reported for the overall population by Drew et al. (2014).

Appendix Figure A.3 shows the evolution of the participation rate for all respondents as well as for those who can be matched to all 8 months. The fully-matched group is clearly a selected sample, as their participation rate is consistently higher than the average. However, the decline in participation is nearly identical for the two groups, so that although the fully-matched sample is selected, the degree of this selection has not changed over time.

**Linking Monthly CPS to March CPS** All respondents to the March monthly CPS are also administered a supplemental survey. However, since the data files for this supplement are released separately the responses must be matched to the monthly CPS to compare responses. Additionally, in some years the March CPS files are released with a different set of identifiers than the monthly CPS, making it impossible to match these responses.

To match responses between the two surveys, I utilize the method of Flood and Pacas (2016). This method focuses on mechanical matches using the identifiers released in the two surveys. I match
responses during the 1989-2015 period, in which all monthly CPS responses can be matched to the corresponding March CPS response. However, not every March CPS response can be matched to a monthly CPS response, since the March CPS includes some individuals who do not appear in the monthly CPS.

A.2.3 Wages

I use several measures of wages in this analysis. In this section, I start by outlining the baseline measure of wages, which does not include those who report missing wages. Then I turn to different imputation procedures to estimate the wages available to those reporting missing wages.

Baseline Wage Measure To measure the wages of employed men, I use information from the CPS Outgoing Rotation Group (ORG) interviews. The ORG interviews take place for all individuals in their 4th and 8th month in the CPS sample and consistent of several questions about work, including nominal wages and hours worked.

From the ORG interviews, I construct a measure of real hourly wages. I start by dropping all observations with missing wages. Many individuals report wages at a weekly frequency instead of hourly. These weekly earnings are subject to a topcode for privacy reasons. I replace topcoded observations with the mean of weekly earnings above the topcode, assuming that weekly earnings follow a lognormal distribution (this method is outlined in Schmitt (2003)). After this adjustment, I convert weekly wages into hourly wages by dividing by hours worked per week. I convert nominal hourly wages to real wages using the PCE price index. Finally, I drop outlier observations, defined as hourly wages below $1/hour or above $300/hour in 2016$.

Imputations I use three different imputation procedures to estimate the wages available to those with missing wage information.

First, wages can be imputed non-parametrically using the method of Juhn (1992). In this approach, I estimate the distribution of wages within each year, specifically the average wage within each decile of the wage distribution. Each individual with missing wages is assigned probabilities of appearing in each decile based on the observed distribution of individuals with the same level of labor force attachment, where attachment is measured by the number of months in the labor force out of 8 in the CPS. In this way, each individual with missing wages is imputed the full distribution of wages.
based on similarly attached individuals.

I also use the regression imputation method of Blau and Kahn (2007). For each year, I regressed wages on a quadratic in age, 4 education dummies (less than high school, high school graduate, some college, and college graduate), 4 racial group dummies (white non-Hispanic, black, non-black Hispanic, and other), 3 marital status dummies (married, widowed/divorced, and never married), 9 census division dummies, a dummy for living in an urban area, and the total number of months the individual spent unemployed in the CPS (out of 8). I take the predicted values from this regression and use them as imputed wages for those with missing wage information.

Lastly, I impute wages using the predicted values from a regression with a selection correction as in Heckman (1979). I use the same covariates as in the regression imputation above, and use weekly spousal wages as the excluded variable to identify the selection equation. For any observation with missing wage information, I impute wages with the predicted value from the selection-corrected regression.

A.2.4 Tax Rates

I use the NBER TAXSIM program to estimate state and federal taxes for the sample of households I can match from the basic monthly CPS to the March supplement. To calculate taxes, I make the following assumptions: 1) I code all married couples as joint tax filing units and all other individuals as single filers; 2) I ignore mortgage interest, rent paid, child care expenses, and short-term capital gains (unavailable in March CPS).

I compute the marginal tax rate for an individual as the sum of the marginal federal income tax rate, the marginal state income tax rate, and the marginal payroll tax rate (including both employee and employer payroll taxes). The average tax rate is computed by taking total tax payments and dividing by total taxable income for each tax unit. To avoid problems with outliers, I winsorize both marginal and average tax rates at the 1% and 99% levels.

A.3 Time Use on Video Games

This section reconciles the results on time use presented in Table 1.3 with previous results from the literature. Aguiar et al. (2017) point out that non-employed young men spend a growing fraction of their time playing video games. They interpret this fact through a model of time use as implying
that the quality of video games has improved, which they then show can account for some of the decline in labor supply among this population since 2000. However, I find that in-and-outs spend relatively little time on video games and experience little increase in time spent on video games when leaving the labor force.

There are many differences between these two analyses that could account for the apparent discrepancy. While I focus on in-and-outs, Aguiar et al. (2017) look at non-employment overall. Additionally, they focus on young men between the ages of 21 and 30, while I examine men between the ages of 25 and 54. There are also some differences in time period and sample restrictions. To examine the influence of these differences, I recompute the time spent on video games, changing one aspect at a time to fully crosswalk the two sets of estimates.

Appendix Table A.4 shows how the estimated hours per week spent on video games changes with each adjustment. The 0.2 hours per day reported in Table 1.3 is equivalent to 1.6 hours per week. Breaking out separate time periods, as is done by Aguiar et al. (2017), doesn’t substantially alter the result. However, including dropouts and other non-employed individuals raises the average time spent on video games substantially, approximately double the average of just in-and-outs within each time period. This implies that these other groups spend significantly more time on video games than in-and-outs do. Expanding time use to include weekend days makes little difference. Changing the sample to look at 21-30 year olds increases the estimated time spent on video games in both time periods, but especially in 2012-2015. Dropping full-time students has little effect. The second-to-last
row gives estimates using the same concept as Aguiar et al. (2017), but although the results are close they are not identical for unknown reasons.

The two deviations between the analyses that make the most difference are: 1) the choice of in-and-outs versus all non-employed, and 2) the focus on 25–54 year olds instead of 21–30 year olds. Both of these choices appear to contribute about equally to the difference in estimates.

A.4 Evolution of In-and-Outs’ Income

One potential problem with the event study analysis of section 1.3.1 is that the sample of in-and-outs is defined using ex-post information, specifically whether individuals returned to the labor force after a short period of time. In this way, the analysis may exclude individuals who anticipated taking a short break out of the labor force, but ended up experiencing large declines in available wages and remained out of the labor force. This would suggest that while in-and-outs do not suffer permanent income loss, this is merely because they were lucky rather than because a short break out of the labor force cannot incur a permanent cost.

The extent of this problem can be gauged by using a sample selected based on ex-ante characteristics only, since this avoids using ex-post information. Using the SIPP sample, I examine all employment to non-participation separations and use pre-separation characteristics to predict the duration of the non-participation spell. This relies on the notion that individuals with similar characteristics are likely to spend similar amounts of time out of the labor force, or put another way that in-and-outs can be separated from dropouts by only examining their pre-separation characteristics. This is an empirically testable statement. I use all individuals who transition from $E \rightarrow N$ at least two years before the end of the sample and use as covariates their demographic, household, job, and income characteristics from 4 months before the separation.\footnote{The restriction that the transition must occur at least two years before the end of the sample avoids including transitions where the non-participation spell duration is right-censored under two years. It is unclear whether these excluded observations are in-and-outs or dropouts due to this censoring. I use covariates from four months before the separation so that all characteristics are measured in a previous SIPP wave, since characteristics may be mechanically correlated within a SIPP wave.}

To predict the duration of non-participation based on pre-separation characteristics, I use a machine learning algorithm known as Gradient Boosted Trees (GBT).\footnote{GBT is described in detail in Hastie, Tibshirani and Friedman (2009). I use the LightGBM package to implement this algorithm.} GBT fits the outcome by iteratively applying a series of small decision trees. The first tree is fit to the raw data, splitting...
branches of the tree to maximize the share of variation explained by the tree. The next tree is fit to the residuals from the first tree and the process repeats, with each tree in the series fitting the residuals of the previous one. However, each tree is weighted according to the marginal improvement in fit, resulting in later trees receiving less weight as the algorithm reaches a point of diminishing returns in improving the fit of the model. Each tree is constrained to be very shallow. By repeatedly applying shallow trees to the residuals of past iterations, this method can approximate very flexible functional forms, with later trees correcting for misspecifications introduced by earlier trees. I use a total of 50 trees with a learning rate of 0.05, as selected by cross-validation.

Each characteristic’s contribution to the prediction can be quantified. I compute the total “gain” provided by a variable by summing up the reduction in the sum of squared residuals from every time that variable is used to split a branch across all of the 50 trees. Appendix Figure A.4 shows this measure for each variable, expressed as a share of the highest gain. Disability is the most important characteristic for separating in-and-outs from dropouts, accounting for more than double the importance of any other variable. Age, income, and education are also judged to be important predictors of time spent out of the labor force.

Next, I examine whether this algorithm is able to adequately separate in-and-outs from dropouts. I order individuals by their predicted duration and categorize all individuals with predicted duration

Figure A.4: Importance of SIPP Characteristics in Predicting Non-Participation Duration

![Variable Importance](image_url)
below some threshold to be predicted in-and-outs. For a given threshold, this results in some true positive rate, i.e. the share of actual in-and-outs who are categorized as such, and a false positive rate, i.e. the share of dropouts who are incorrectly categorized as in-and-outs. I vary this threshold to trace out a curve trading off these two different rates, known in the machine learning literature as a Receiver-Operating Characteristic (ROC) curve, which is plotted in Appendix Figure A.5. The ROC curve can be compared to the 45° degree line to measure the predictability of in-and-outs. If in-and-outs are completely random, the ROC curve will coincide with the 45° line, while if in-and-outs are perfectly predictable, the ROC curve will be equal to 1 everywhere. In this case, the ROC curve is quite a bit above the 45° line, indicating that in-and-outs can be fairly easily separated from dropouts. The area under the ROC curve measures the deviation from randomness and in this case it is equal to 0.83, indicating a strong degree of prediction, although not quite complete prediction.

Using these predictions, I construct a sample of predicted in-and-outs and repeat the event study from above. I select a sample of predicted in-and-outs that captures about 60% of actual in-and-outs while including less than 20% of actual dropouts (marked with a dot in Appendix Figure A.5). As above, I estimate the following equation for an outcome $Y,$

$$\Delta Y_{i,t+k} = \beta^{(k)} 1 (\text{In-and-Out}_{i,t}) + \delta X_{i,t} + \Delta \epsilon_{i,t+k}$$

(A.1)

varying $k$ between -6 and 24 to cover a two-and-a-half year window, controlling for a cubic in age as well as time fixed effects.
First, I show that the predicted in-and-outs behave like actual in-and-outs using this event study. I estimate equation A.1 using labor force participation as the outcome and plot the resulting coefficients in Appendix Figure A.6. Predicted in-and-outs return to the labor force quite quickly, with the majority returning within the first six months. By twelve months after the separation, predicted in-and-outs have similar participation rates as actual in-and-outs. Predicted dropouts, on the other hand, remain disconnected from the labor force even after two years, as do actual dropouts. The event study shows how the choice of trading off true positive rates and false positive rates affects the groups, as I have chosen a sample of predicted in-and-outs that contain very few actual dropouts, but exclude some actual in-and-outs, resulting in a gap between predicted dropouts and actual dropouts. However, I examine the evolution of income for the predicted in-and-outs only and not for predicted dropouts, so this tradeoff is sensible.

Next, I turn to the evolution of income for the group of predicted dropouts. Appendix Figure A.7 shows the estimated evolution of income for predicted in-and-outs, actual in-and-outs, and unemployed job losers, where all three sets of estimates are computed using equation A.1. While predicted in-and-outs experience an even larger decline in personal income than actual in-and-outs at the time of separation, this gap narrows as both groups return to the labor force and the income
Sample: 25–54 year old men in the SIPP. In-and-Outs are employed in month 0, nonparticipants in month 1, and are employed again by at least month 12. Unemployed job losers are employed in month 0 and fired or laid off but looking for work in month 1. Re-employment time is predicted from covariates four months before the separation using gradient boosted trees and individuals are categorized based on whether the predicted time is less than a threshold delivering a false positive rate below 20%.

of the two groups is statistically indistinguishable by one year after the separation. Both groups experience no significant change in income between the month before the separation and two years after the separation, while unemployed job losers experience significantly lower income.
Appendix B

Appendix to Chapter 2

B.0.1 Wages

To measure the wages of employed individuals, I use information from the CPS Outgoing Rotation Group (ORG) interviews. The ORG interviews take place for all individuals in their 4th and 8th month in the CPS sample and consist of several questions about work, including nominal wages and hours worked.

From the ORG interviews, I construct a measure of real hourly wages. I start by dropping all observations with missing wages. Many individuals report wages at a weekly frequency instead of hourly. These weekly earnings are subject to a topcode for privacy reasons. I replace topcoded observations with the mean of weekly earnings above the topcode, assuming that weekly earnings follow a lognormal distribution (this method is outlined in Schmitt (2003)). After this adjustment, I convert weekly wages into hourly wages by dividing by hours worked per week. I convert nominal hourly wages to real wages using the PCE price index. Finally, I drop outlier observations, defined as hourly wages below $1/hour or above $300/hour in 2016$.

B.0.2 Tax Rates

I use the NBER TAXSIM program to estimate state and federal taxes for the sample of households I can match from the basic monthly CPS to the March supplement. To calculate taxes, I make the following assumptions: 1) I code all married couples as joint tax filing units and all other individuals as single filers; 2) I ignore mortgage interest, rent paid, child care expenses, and short-term capital
gains (unavailable in March CPS).

I compute the marginal tax rate for an individual as the sum of the marginal federal income tax rate, the marginal state income tax rate, and the marginal payroll tax rate (including both employee and employer payroll taxes). The average tax rate is computed by taking total tax payments and dividing by total taxable income for each tax unit. To avoid problems with outliers, I winsorize both marginal and average tax rates at the 1% and 99% levels.
Appendix C

Appendix to Chapter 3

C.1 Data Appendix

In this data appendix we describe the extended benefits programs and the BLS methodology to estimate the state unemployment rates.

C.1.1 Extended Benefits and Emergency Compensation Programs

In Table C.1 we list the full set of benefit extension programs, tiers, and triggers in operation during our sample.

C.1.2 State Unemployment Rate Estimation Methodology


The first step of the real-time estimation involves estimating the state space models separately for total unemployment and employment. The unemployment rate is constructed from these two estimates. Let $y_{s,t} + o_{s,t}$ denote the direct count of a variable such as state employment or unemployment from the CPS, where $o_{s,t}$ denotes any outlier component identified using intervention
Table C.1: Extended Benefit and Emergency Compensation Programs, 1996-2015

<table>
<thead>
<tr>
<th>Program</th>
<th>Tier</th>
<th>Weeks</th>
<th>Period</th>
<th>Triggers</th>
</tr>
</thead>
<tbody>
<tr>
<td>EB 1</td>
<td>13</td>
<td>09/25/1982-</td>
<td></td>
<td>IUR≥5, IUR≥1.2][(IUR(year ago) + IUR(2 years ago))/2] OR IUR≥6 OR TUR≥6.5 and TUR≥1.1 * max{TUR(year ago), TUR(2 years ago)}</td>
</tr>
<tr>
<td>EB 2</td>
<td>7</td>
<td>03/07/1993-</td>
<td></td>
<td>TUR≥8 and TUR≥1.1 * max{TUR(year ago), TUR(2 years ago)}</td>
</tr>
<tr>
<td>EB 1,2</td>
<td>n.a.</td>
<td>12/17/2010-12/31/2013</td>
<td>Replace with max{TUR(year ago), TUR(2 years ago), TUR(3 years ago)} in EB Tier 1 Trigger 3 and EB Tier 2 Trigger 1</td>
<td></td>
</tr>
<tr>
<td>TEUC</td>
<td>1</td>
<td>13</td>
<td>03/09/2002-12/31/2003</td>
<td>Available in all states</td>
</tr>
<tr>
<td>TEUC</td>
<td>2</td>
<td>13</td>
<td>03/09/2002-12/31/2003</td>
<td>Eligible for EB or would be eligible if EB Tier 1 Trigger 1 cutoff for IUR were 4 instead of 5</td>
</tr>
<tr>
<td>EUC 1</td>
<td>13</td>
<td>06/30/2008-11/20/2008</td>
<td>Available in all states</td>
<td></td>
</tr>
<tr>
<td>EUC 1</td>
<td>20</td>
<td>11/21/2008-09/01/2012</td>
<td>Available in all states</td>
<td></td>
</tr>
<tr>
<td>EUC 1</td>
<td>14</td>
<td>09/02/2012-12/31/2013</td>
<td>Available in all states</td>
<td></td>
</tr>
<tr>
<td>EUC 2</td>
<td>13</td>
<td>11/21/2008-11/05/2009</td>
<td>Eligible for EB OR IUR≥4 OR TUR≥6</td>
<td></td>
</tr>
<tr>
<td>EUC 2</td>
<td>14</td>
<td>11/06/2009-05/31/2012</td>
<td>Available in all states</td>
<td></td>
</tr>
<tr>
<td>EUC 2</td>
<td>14</td>
<td>06/01/2012-12/31/2013</td>
<td>TUR≥6</td>
<td></td>
</tr>
<tr>
<td>EUC 3</td>
<td>13</td>
<td>11/06/2009-05/31/2012</td>
<td>IUR≥4 OR TUR≥6</td>
<td></td>
</tr>
<tr>
<td>EUC 3</td>
<td>13</td>
<td>06/01/2012-09/01/2012</td>
<td>IUR≥4 OR TUR≥7</td>
<td></td>
</tr>
<tr>
<td>EUC 3</td>
<td>9</td>
<td>09/02/2012-12/31/2013</td>
<td>IUR≥4 OR TUR≥7</td>
<td></td>
</tr>
<tr>
<td>EUC 4</td>
<td>6</td>
<td>11/06/2009-05/31/2012</td>
<td>IUR≥6 OR TUR≥8.5</td>
<td></td>
</tr>
<tr>
<td>EUC 4</td>
<td>6</td>
<td>06/01/2012-09/01/2012</td>
<td>IUR≥6 OR TUR≥9</td>
<td></td>
</tr>
<tr>
<td>EUC 4</td>
<td>10</td>
<td>09/02/2012-12/31/2013</td>
<td>IUR≥6 OR TUR≥9</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Triggers written in italics are optional. IUR is the average of the insured unemployment rate in the thirteen weeks ending two weeks before the week of the trigger notice. TUR is the average of the total unemployment rate in the three months ending with the last month of data reported as of the third Friday before the Sunday starting the week of the trigger notice. All programs and tiers obey a thirteen week rule whereby once triggered on a tier a state remains on that tier for at least thirteen weeks (barring any changes in law), and once triggered off a tier the state remains off for at least thirteen weeks. Time periods reported exclude phase-outs. EB Tier 1 Trigger 3 became operational on 03/07/1993. Authorization of the TEUC programs lapsed temporarily between 01/01/2003 and 01/07/2003. Authorization of the EUC programs lapsed temporarily between 02/22/2012 and 05/31/2012. The main source for this table is Department of Labor (2015).
model methods. For each state, the observation equation is:

\[ y_{s,t} = \alpha_{s,t} x_{s,t} + L_{s,t} + S_{s,t} + \varepsilon_{s,t}, \]  

where \( x_{s,t} \) is an external regressor (insured unemployment for unemployment and CES payroll employment for employment), \( L_{s,t} \) is a trend level, \( S_{s,t} \) is a seasonal component, and \( \varepsilon_{s,t} \) is the observation error. The state space model employment or unemployment is

\[ Y_{s,t} = \alpha_{s,t} x_{s,t} + L_{s,t} + S_{s,t} = y_{s,t} - \varepsilon_{s,t}. \]

The model state equations are:

\[ \alpha_{s,t} = \alpha_{s,t-1} + \eta_{\alpha,s,t}, \]  

\[ L_{s,t} = L_{s,t-1} + R_{s,t} + \eta_{L,s,t}, \]  

\[ R_{s,t} = R_{s,t-1} + \eta_{R,s,t}, \]  

\[ S_{s,t} = \sum_{j=1}^{6} S_{j,s,t}, \]

where \( \varepsilon_{s,t}, \eta_{\alpha,s,t}, \eta_{L,s,t}, \) and \( \eta_{R,s,t} \) are independent normal random variables, and \( S_{j,s,t} \) are seasonal frequency functions. A generalized Kalman filter estimates the system.\(^1\)

BLS introduced a major update in 2005 with the incorporation of real-time benchmarking to Census Division and national totals. Each month, after estimation of the state space system, BLS would allocate the residual between the sum of model estimates of not seasonally adjusted series for Census Divisions \((L_t + I_t)\) and the national CPS total pro rata to each division, and then repeat the process for states within a division.\(^2\) In that way, the real-time sum of state employment and unemployment would always equal the national total. However, the pro rata allocation meant that state-specific residuals would “spillover” to neighboring states. In 2010, BLS began applying a one-sided moving average Henderson filter to the benchmarked series.

The most recent major update to the real-time model occurred in 2015 and involved three main changes. First, the benchmarking constraint now enters directly into the state space filter. The observation vector is augmented to include the difference between the sum of not seasonally adjusted model state unemployment and employment levels and their Census Division direct

\(^1\)Because of the rotating panel structure of the CPS sample, the observation equation errors may be serially correlated. The generalized Kalman filter uses GLS instead of OLS to find the conditional mean of the state vector given the updated observation vector.

\(^2\)At the Census Division level the state space estimation excludes the external regressors insured unemployment or payroll employment. In terms of Equations (C.1) to (C.5), \( a_{sdj} = 0 \) and \( \text{var}(\eta_{cdj}) = 0. \)
estimate (excluding identified outliers), and the estimation constrains the variance of the innovation in this component to be zero. Incorporating benchmarking within the state space filter more efficiently allocates the benchmark residual across states. Second, outlier components \( o_{s,t} \) identified by intervention model methods are added back to the states from which they originated after the state space estimation. Both of these changes reduce spillovers of unusual residuals across states within a division. Third, the 2015 redesign incorporated an improved seasonal adjustment procedure.

Table C.2 provides an overview of the importance of different components of the revision process using as a metric the \( R^2 \) from a regression of \( \hat{u}_{s,t} \) on the components.\(^3\) The first row shows that the revisions to the CES employment data explain a small part of the unemployment rate revision. While the CES revisions themselves can be large, they enter into the unemployment rate only through the denominator and therefore have a smaller effect on the unemployment rate revision. The second row adds elements related to the 2015 LAUS redesign and the treatment of state-specific outliers in the CPS. Specifically, we add to the regression the difference between the vintage 2014 and vintage 2015 LAUS seasonally adjusted unemployment rates, the difference between the unemployment rate constructed directly from the CPS monthly files and the real-time LAUS seasonally unadjusted unemployment rate, the difference between the unemployment rate constructed directly from the CPS files and seasonally adjusted using an X-11 moving average and the average of the same variable for three months before and three months after the observation, and the labor force weighted average of the previous variable for other states in the same Census Division. These variables increase the explained part of \( \hat{u}_{s,t} \) to 49%. In row 3, adding the component due to updated seasonal factors in the revised data further increases the explained part of \( \hat{u}_{s,t} \) to 59%. Rows 4 and 5 next add lags and leads of \( u_{s,t} \) to explore whether the path of the unemployment rate affects the revision through the state space smoother and symmetric filter. In row 4, adding 12 lags of the unemployment rate raises the \( R^2 \) by 0.02, while in row 5 adding the contemporaneous and 12 leads of the unemployment rate raises it by an additional 0.01.\(^4\) Overall, these components explain 62% of the variation in the unemployment rate revision. Because the LAUS process uses a nonlinear state space model, we would not expect a linear projection on the major sources of revisions to generate an \( R^2 \) of 1.

Figure C.1 illustrates that in our example of Vermont the 2015 LAUS technical improvements

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\(^3\)Because the procedure for the real-time data changed in 2005 and most of the UI errors in our sample occur during the Great Recession, we limit the sample in this table to 2005 to 2013.

\(^4\)The incremental \( R^2 \) is not invariant to the ordering of variables. Including just the 12 lags of the unemployment rate produces an \( R^2 \) of 0.10. Adding the contemporaneous and 12 leads raises the \( R^2 \) to 0.15.
Table C.2: Determinants of Unemployment Rate Errors

<table>
<thead>
<tr>
<th>Determinants</th>
<th>$R^2$</th>
</tr>
</thead>
<tbody>
<tr>
<td>CES revisions</td>
<td>0.03</td>
</tr>
<tr>
<td>+ 2015 LAUS redesign and identification of outliers</td>
<td>0.49</td>
</tr>
<tr>
<td>+ Updated seasonal factors</td>
<td>0.59</td>
</tr>
<tr>
<td>+ 12 lags of unemployment rate</td>
<td>0.61</td>
</tr>
<tr>
<td>+ Contemporaneous and 12 leads of unemployment rate</td>
<td>0.62</td>
</tr>
</tbody>
</table>

Notes: The table reports the $R^2$ from a regression of the measurement error in the unemployment rate $\hat{u}_{s,t}$ on the regressors indicated in the left column. The sample is January 2005 to December 2013. In the first row, CES revisions are the log difference between the real-time and revised nonfarm seasonally unadjusted employment level from the CES. The second row adds the difference between the vintage 2014 and vintage 2015 LAUS seasonally adjusted unemployment rates, the difference between the unemployment rate constructed directly from the CPS monthly files and the real-time LAUS seasonally unadjusted unemployment rate, the difference between the unemployment rate constructed directly from the CPS files and seasonally adjusted using an X-11 moving average and the average of the same variable for three months before and three months after the observation, and the labor force weighted average of the previous variable for other states in the same Census Division. The third row adds the difference between the revised LAUS seasonally adjusted unemployment rate and the real-time seasonally unadjusted unemployment rate after rescaling the numerator and denominator by the revised seasonal factors for LAUS unemployment and employment. The fourth row adds 12 lags of the revised unemployment rate. The fifth row adds the contemporaneous and 12 leads of the revised unemployment rate.

![Figure C.1: Extended Benefits and Unemployment in Vermont](image)

Notes: The figure plots the actual duration of benefits $T^s_{x,t}$ and the duration based on the revised data $T^s_{x,t}$ (left axis) together with the real-time $u^r_{s,t}$ and revised unemployment rates $u_{s,t}$ (right axis). The dashed green line shows the unemployment rate using the 2014 vintage of data.

account for all of the unemployment rate error during the period of the UI error in the beginning of 2010.
C.2 Measurement Error in the Revised Data

In this appendix we examine the case in which the revised data measure the fundamentals with some error. Measurement error in the revised data introduces an attenuation bias in our estimated impulse responses. We derive an upper bound of this bias under the plausible assumption that the revised data measure fundamentals with less error than the real-time data. Even under this upper bound, we can reject the hypothesis that our estimated responses are consistent with large effects of UI benefit extensions on unemployment.

Our discussion applies to observations at the state-month level, but we drop state-month subscripts to ease the notation. Let the observed duration of benefits, $T^*$, be equal to the sum of two orthogonal components:

$$T^* = T^F + T^E,$$  \(\text{(C.6)}\)

where $T^F$ denotes the duration of benefits using the true unemployment rate and $T^E$ denotes the duration of benefits due to measurement error of the true unemployment rate. The true unemployment rate and $T^F$ are unknown to the econometrician. We allow $T$ to be based on an imperfect measure of the fundamentals:

$$T = T^F + T^X,$$  \(\text{(C.7)}\)

where $T^X$ is a component due to measurement error in the revised data.

The UI error that we defined in the main text, $\hat{T}$, can be written as:

$$\hat{T} = T^* - T = T^E - T^X.$$  \(\text{(C.8)}\)

In the presence of measurement error in the revised data, the UI error $\hat{T}$ is the difference between the measurement error in the true unemployment rate, $T^E$, and the measurement error in the revised data, $T^X$.

The three primitive objects of our analysis are $T^F$, $T^E$, and $T^X$. We write each variable $j = \{F, E, X\}$ as the sum of its expected value plus an innovation, $T^j = \mathbb{E}T^j + e^j$. All innovations $e^j$'s are serially uncorrelated and uncorrelated with each other. The innovations in the measurement error components, $e^E$ and $e^X$, are uncorrelated with the fundamentals $F$. By contrast, the innovation $e^F$ is potentially correlated with the fundamentals $F$.

Taking expectations in equation (C.6) and using the definition of the innovations, we write the
innovation in the real-time duration of benefits as:

\[ e^T = e^F + e^E. \]  \hfill (C.9)

Similarly, using equations (C.7) and (C.8), we write the innovation in the duration of UI benefits under the revised data and the innovation in the UI error (which we called \( e \) in the main text) as:

\[ e^T = e^F + e^X, \]  \hfill (C.10)

\[ e^\hat{T} = e^E - e^X. \]  \hfill (C.11)

Suppose the relationship between some outcome variable \( y \) (that could be measured in a future period) and the innovation in the duration of benefits under the real-time data is:

\[ y = \beta e^T + \gamma F, \]  \hfill (C.12)

where \( F \) collects all other factors that affect \( y \). The fundamentals in \( F \) are potentially correlated with \( e^T \) through \( e^F \) but are uncorrelated with the measurement error component \( e^E \). Using equations (C.9) and (C.11) we can write:

\[ y = \beta e^F + \beta e^X + \beta e^\hat{T} + \gamma F. \]  \hfill (C.13)

The OLS coefficient in a bivariate regression of \( y \) on \( e^\hat{T} \) is given by:

\[ \beta_{OLS} = \frac{\text{Cov} \left( y, e^\hat{T} \right)}{\text{Var} \left( e^\hat{T} \right)} = \frac{\text{Cov} \left( \beta e^X + \beta e^\hat{T}, e^\hat{T} \right)}{\text{Var} \left( e^\hat{T} \right)} = \beta \left( 1 - \frac{\text{Var} \left( e^X \right)}{\text{Var} \left( e^\hat{T} \right)} \right), \]  \hfill (C.14)

where the second equality uses equation (C.13) and the fact that \( \text{Cov} \left( F, e^\hat{T} \right) = \text{Cov} \left( e^F, e^\hat{T} \right) = 0 \), and the third equality uses the fact that \( \text{Cov} \left( e^X, e^\hat{T} \right) = \text{Cov} \left( e^X, e^E - e^X \right) = -\text{Var} \left( e^X \right) \). If the revised data measure the true fundamentals without any error up to a constant, \( \text{Var} \left( e^X \right) = 0 \), then the OLS estimator is unbiased \( \beta_{OLS} = \beta \). The attenuation bias is increasing in the variance of the measurement error in the revised data relative to the variance of the UI error, \( \text{Var} \left( e^X \right) / \text{Var} \left( e^\hat{T} \right) \).

We now show that attenuation bias in our estimates is too small to affect our main conclusions under the plausible assumption that revised data do not deteriorate the quality of measurement of true fundamentals. We say that the revised data are a (weakly) better measure of the true fundamentals than the real-time data if the measurement error in the revised data has a (weakly) lower variance:

\[ \text{Var} \left( e^X \right) \leq \text{Var} \left( e^E \right). \]  \hfill (C.15)
The assumption that the revised data contain less measurement error than the real-time data places an upper bound on the attenuation bias. From equation (C.11), we see that $\text{Var}(e^\uparrow) = \text{Var}(e^X) + \text{Var}(e^E)$ and, therefore, under assumption (C.15) less than 50 percent of the variance of $e^\uparrow$ is attributed to $e^X$:

$$\frac{\text{Var}(e^X)}{\text{Var}(e^\uparrow)} \leq 0.5.$$  \hspace{1cm} \text{(C.16)}

We estimate in the data an upper bound of $\beta^{\text{OLS}} = 0.02$. Using the upper bound of the bias $\text{Var}(e^X) / \text{Var}(e^\uparrow) = 0.50$, the true coefficient could be as large as $\beta = 0.04$. Using a standard error of 0.02, this $\beta$ is still 4.5 standard errors below the 0.14 level that would rationalize a large effect of extended benefits on unemployment during the Great Recession.

This calculation is very conservative because it assumes that revisions do not improve measurement and uses the upper bound of our estimates of $\beta$. In Section 3.5.4 we provided evidence that revisions are informative about actual spending patterns and beliefs. This implies that $\text{Var}(e^X) / \text{Var}(e^\uparrow)$ is likely to be smaller than 0.5. Indeed, we find in the data that there is smaller variance of outcomes in the revised data and, consistent with our assumption that $\text{Var}(e^X) \leq \text{Var}(e^E)$, that $\text{Var}(e^\uparrow) < \text{Var}(e^{\uparrow'})$. If we apply, for example, $\text{Var}(e^X) / \text{Var}(e^\uparrow) = 0.25$ to our maximum estimate of $\beta^{\text{OLS}} = 0.02$, we obtain that the true coefficient is $\beta < 0.03$. In general, the more informative is the revised data for the true fundamentals, the lower is $\text{Var}(e^X) / \text{Var}(e^\uparrow)$ and the smaller is the attenuation bias.

C.3 Model Appendix

This appendix contains a self-contained description of our model validation exercise.

C.3.1 Model Description

Labor Market and Eligibility Flows. Each period a measure $u_t$ of unemployed search for jobs and a measure $1 - u_t$ of employed produce output. Unemployed individuals find jobs at a rate $f_t$ which is determined in equilibrium. Employed individuals separate from their jobs at an exogenous rate $\delta_t$. The law of motion for unemployment is:

$$u_{t+1} = (1 - f_t)u_t + \delta_t(1 - u_t).$$  \hspace{1cm} \text{(C.17)}
Employed individuals who lose their jobs become eligible for UI benefits with probability $\gamma$. There are $u_t^E$ unemployed who are eligible for and receive UI benefits. Eligible unemployed who do not find jobs lose their eligibility with probability $e_t$. The key policy variable in our model is the (expected) duration of benefits $T_t^*$ which equals the inverse of the expiration probability, $T_t^* = 1/e_t$.\footnote{For expository reasons, in the model $T_t^*$ denotes the total duration of benefits (including the regular benefits), whereas in the data we defined $T_t^*$ as the extension of benefits beyond their regular duration.}

Finally, there are $u_t - u_t^E$ ineligible unemployed. Ineligible unemployed who do not find jobs remain ineligible for UI benefits.

We denote by $\omega_t = u_t^E / u_t$ the fraction of unemployed who are eligible for and receive UI. This fraction evolves according to the law of motion:\footnote{In the data we have a measure of the fraction of unemployed who receive UI benefits (what we called $f$ in the empirical analysis) based on administrative data on UI payments. Constructing a high quality panel of take-up rates at the state-month level is not feasible with currently available data. A difference relative to the model of Chodorow-Reich and Karabarbounis (2016) is that, because of this data unavailability, here we do not consider the take-up decision of an unemployed who is eligible for benefits. Therefore, we use interchangeably the terms eligibility for UI benefits and receipt of UI benefits.}

$$\omega_{t+1} = \frac{\delta_t (1 - u_t)}{u_{t+1}} + \left( \frac{u_t (1 - f_t)(1 - e_t)}{u_{t+1}} \right) \omega_t. \quad (C.18)$$

**Household Values.** All individuals are risk-neutral and discount the future with a factor $\beta$. Employed individuals consume their wage earnings $w_t$. The value of an individual who begins period $t$ as employed is given by:

$$W_t = w_t + \beta (1 - \delta_t) \mathbb{E}_t W_{t+1} + \beta \delta_t \left( \gamma \mathbb{E}_t U_t^E + (1 - \gamma) \mathbb{E}_t U_t^I \right), \quad (C.19)$$

where $U_t^E$ denotes the value of an eligible unemployed and $U_t^I$ denotes the value of an ineligible unemployed. These values are given by:

$$U_t^E = \xi + B + \beta f_t \mathbb{E}_t W_{t+1} + \beta (1 - f_t) \left( e_t \mathbb{E}_t U_{t+1}^I + (1 - e_t) \mathbb{E}_t U_{t+1}^E \right), \quad (C.20)$$

$$U_t^I = \xi + \beta f_t \mathbb{E}_t W_{t+1} + \beta (1 - f_t) \mathbb{E}_t U_{t+1}^I, \quad (C.21)$$

where $\xi$ is the value of non-market work and $B$ is the UI benefit per eligible unemployed.\footnote{Benefit extensions were federally funded between 2009 and 2013. We think of our model as applying to an individual state during this period and, therefore, we do not impose UI taxes on firms.} We assume that both $\xi$ and $B$ are constant over time. This allows us to focus entirely on the role of benefit extensions for fluctuations in the opportunity cost of employment.\footnote{In previous work (Chodorow-Reich and Karabarbounis, 2016), we found that the $\xi$ component of the opportunity cost is procyclical. Benefit extensions typically occur when unemployment is high and $\xi$ is low. However, our empirical exercise...}
Surplus and Opportunity Cost of Employment. Firms bargaining with workers over wages cannot discriminate with respect to workers’ eligibility status. Therefore, there is a common wage for all unemployed. This implies that we need to keep track of values and flows for the average unemployed. We define the value of the average unemployed individual as:

\[ U_t = \omega_t U_t^E + (1 - \omega_t)U_t^I. \]  

(C.22)

The surplus of employment for the average unemployed is given by the difference between the value of working and the value of unemployment. We take:

\[ S_t = W_t - U_t = w_t - z_t + \beta(1 - \delta_t - f_t)E_tS_{t+1}, \]  

(C.23)

where \( z_t \) denotes the (flow) opportunity cost of employment for the average unemployed.

The opportunity cost of employment is defined as the flow utility that an unemployed forgoes upon moving to employment. It is given by:

\[ z_t = \xi + \omega_tB - (\delta_t(\gamma - \omega_t) + (1 - f_t)\omega_t e_t)\beta \left( E_tU_{t+1}^E - E_tU_{t+1}^I \right), \]  

(C.24)

where \( b_t \) denotes the benefit component of the opportunity cost of employment. The expression nests the standard model (for instance, Shimer, 2005) that has \( b_t = B \) if \( e_t = 0 \), that is when benefits do not expire, and \( \gamma = \omega_t = 1 \), that is when all unemployed are eligible for benefits. More generally, the flow utility loss \( b_t \) of moving an average unemployed to employment is lower than the benefit \( B \). The difference occurs because some unemployed are not eligible for benefits and, even for those unemployed who are eligible, benefits will eventually expire.\(^9\) Additionally, \( b_t \) is in general time varying. Extending benefits, which here means a decline in the expiration probability \( e_t \), increases the fraction of unemployed who are eligible \( \omega_t \) and raises \( b_t \) and the opportunity cost of employment \( z_t \).

Firm Value, Matching, and Bargaining. The value of a firm from matching with a worker is given compares two states with different duration of benefits that have the same economic fundamentals and, therefore, it is appropriate to not control for \( \xi \) in our regressions. The constancy of \( \xi \) in the model is conservative for our conclusions in this section. Allowing \( \xi \) to respond endogenously would lead to an even smaller effect of benefit extensions on unemployment because the decline in \( \xi \) would tend to offset the increase in the value of benefits (denoted \( b \) below) in the opportunity cost \( z = \xi + b \).

\(^9\)The first effect is captured by the first term of \( b_t \) which is lower than \( B \) when \( \omega_t < 1 \). The second effect is captured by the second term which is positive because \( \gamma > \omega_t \) and \( E_tU_{t+1}^E > E_tU_{t+1}^I \).
by:
\[ J_t = p_t - w_t + \beta(1 - \delta_t)E_t J_{t+1}, \]  
(C.25)
where \( p_t \) denotes aggregate labor productivity. There is free entry and, therefore, the expected value of creating a vacancy equals zero:
\[ \frac{\kappa}{q_t} = \beta E_t J_{t+1}, \]  
(C.26)
where \( \kappa \) denotes the upfront cost that an entrant pays to create a vacancy and \( q_t \) denotes the rate at which vacancies are filled.

Trade in the labor market is facilitated by a constant returns to scale matching technology that converts searching by the unemployed and vacancies by firms into new matches,
\[ m_t = M v_t^\eta u_t^{1-\eta}. \]
We denote by \( \eta \) the elasticity of the matching function with respect to vacancies. We define market tightness as \( q_t = v_t / u_t \). An unemployed matches with a firm at a rate \( f_t(\theta_t) = m_t / u_t \) and firms fill vacancies at a rate \( q_t(\theta_t) = m_t / v_t = f_t(\theta_t) / \theta_t \).

Firms and workers split the surplus from an additional match according to the generalized Nash bargaining solution. We denote by \( \mu \) the bargaining power of workers. The wage is chosen to maximize the product \( S_t^\mu f_t^{1-\mu} \), where \( J_t \) in equation (C.25) is a firm’s surplus of employing a worker and \( S_t \) in equation (C.23) is the surplus that the average unemployed derives from becoming employed. This leads to a standard wage equation:
\[ w_t = \mu p_t + (1 - \mu)z_t + \mu \kappa \theta_t. \]  
(C.27)
The wage is an increasing function of labor productivity, the opportunity cost, and market tightness.

**UI Policy.** The duration of UI benefits is given by \( T_t = T_t + \hat{T}_t \), where \( T_t \) denotes the duration of UI benefits in the absence of any measurement error and \( \hat{T}_t \) is the UI error. Consistent with the results in Section 3.5.4 that agents respond only to the revised unemployment rate, we assume that firms and workers know the underlying fundamentals (for instance, \( u_t, p_t, w_t \) etc.) at the beginning of each period. The statistical agency makes errors in the measurement of the true unemployment rate which result in UI errors \( \hat{T}_t \).
The process for $T_t$ is:
\[
T_t = \begin{cases} 
T^1, & \text{if } 0 \leq u_t < \hat{u}^1, \\
T^2, & \text{if } \hat{u}^1 \leq u_t < \hat{u}^2, \\
& \vdots \\
T^J, & \text{if } \hat{u}^{J-1} \leq u_t < \hat{u}^J = 1.
\end{cases}
\] (C.28)

The UI error follows a first-order Markov process $p_T(\hat{T}_t \mid \hat{T}_{t-1}; u_t)$. As in the data, the unemployment rate enters into the Markov process to capture the fact that UI errors occur only in particular regions of the state space.\(^{10}\)

**Equilibrium.** The state vector of the economy is given by $x_t = [u_t, \omega_t, p_t, \delta_t, \hat{T}_t]$. Given exogenous and known processes for $p_t$, $\delta_t$, and $\hat{T}_t$, an equilibrium of this model consists of functions of the state vector:
\[
\left\{ u_{t+1}(x_t), \omega_{t+1}(x_t), \theta_t(x_t), W_t(x_t), U^E_t(x_t), U^I_t(x_t), w_t(x_t), J_t(x_t), b_t(x_t), T_t(x_t) \right\},
\]
such that: (i) The law of motion for unemployment (C.17) and the law of motion for eligibility (C.18) are satisfied. (ii) Worker values in equations (C.19), (C.20), and (C.21) are satisfied. (iii) The firm value is given by equation (C.25) and the free-entry condition (C.26) holds. (iv) Wages are determined by equation (C.27), where the opportunity cost of employment is given by equation (C.24). (v) The duration of UI benefits in the absence of measurement error is given by the schedule (C.28). Starting from each state vector $x_t$, we have 10 equations to solve for the 10 unknowns.

**Effects of UI Policy in the Model.** An increase in the current duration of benefits ($T^*_t = 1/e_t$) affects equilibrium outcomes to the extent that firms and workers expect it to persist in future periods. Combining equations (C.25) and (C.26), the decision to create a vacancy in the current period depends on the expectation of the present discounted value of firm profits:
\[
\frac{\kappa}{q_t(\theta_t)} = E_t \sum_{j=1}^{\infty} \beta^j \left( \prod_{i=1}^{j} \frac{1 - \delta_t + 1 - \delta_{t+i-1}}{1 - \delta_t} \right) (p_{t+j} - w_{t+j}),
\] (C.29)

where $q_t(\theta_t)$ is a decreasing function of current market tightness $\theta_t = v_t/u_t$. By raising the fraction

\(^{10}\) The timing convention in our model follows the convention in the DMP literature in which the unemployment rate $u_t$ is a state variable and has been determined in period $t - 1$. For this reason UI policy in the model depends on $u_t$. We remind the reader that in the data the unemployment rate in period $t - 1$ determines the extension of benefits in period $t$.\hspace{1cm}
of unemployed who are eligible for UI, an extension of benefits increases future opportunity costs and wages. The increase in wages lowers the expected present value of firm profits and decreases firms’ willingness to create vacancies in the current period. The decline in vacancies makes it more difficult for the unemployed to find jobs, which increases the unemployment rate.

### C.3.2 Parameterization

A model period corresponds to a month. The schedule for the $T_t$ component of UI benefit duration is:

$$T_t = \begin{cases} 
6, & \text{if } u_t < 0.065, \\
9, & \text{if } 0.065 \leq u_t < 0.08, \\
12, & \text{if } 0.08 \leq u_t < 0.09, \\
20, & \text{if } 0.09 \leq u_t.
\end{cases} \quad (C.30)$$

For the UI error component, $\hat{T}_t$, we estimate the probabilities $\pi_T (\hat{T}_t | \hat{T}_{t-1}; u_t)$ in the data separately for each region $u_t < 0.06, 0.06 \leq u_t < 0.065,$ and $u_t \geq 0.065$.

Table C.3 lists values for other parameters of the model. The discount factor equals $\beta = 0.997$.

Log productivity follows an AR(1) process $\log p_{t+1} = \beta \log p_t + \sigma v_t^p$, with $v_t^p \sim N(0, 1)$, where from the data we estimate that at monthly frequency $\beta = 0.91$ and $\sigma = 0.008$. The mean separation rate is $\delta = 0.035$. We set the elasticity of the matching function with respect to vacancies to $\eta = 0.60$, worker’s bargaining power to $\mu = 0.40$, and the value of non-market work to $\xi = 0.81$. We then calibrate four parameters, $M$, $\gamma$, $B$, and $\kappa$, to hit four targets in the steady state of the model with no benefit extensions (so $T^* = 6$ months).\(^{11}\)

\(^{11}\) We target $\theta^T = 1$, $\omega^T = 0.055$, $\alpha^T = 0.65$, and $b^T = \{0.06, 0.15\}$. Because we do not consider the take-up decision of the unemployed, $B$ should be understood as the after-tax value of benefits for the average eligible unemployed. This differs from the replacement rate per recipient because of taxes, utility costs of taking up benefits, and a take-up rate below one.
We parameterize two versions of the model. In the “low b” model we pick $B$ such that $b = 0.06$ in the steady state and so $z = \xi + b = 0.87$. The value of $b = 0.06$ accords with the finding in Chodorow-Reich and Karabarbounis (2016) that benefits comprise a small fraction of the average opportunity cost. In the “high b” model we pick $B$ such that $b = 0.15$ and $z = \xi + b = 0.96$. The value of $z = 0.96$ was found by Hagedorn and Manovskii (2008) to match the rigidity of wages with respect to productivity.

C.3.3 Computation

We solve the model globally by iterating on the equilibrium conditions. We begin by guessing functions $\theta^0(u_t, \omega_t, p_t, \delta_t, \hat{T}_t)$ and $b^0(u_t, \omega_t, p_t, \delta_t, \hat{T}_t)$ defined over grids of state variables. Given these guesses, we obtain $f(.), T(.), u'(.)$ and $\omega'(.)$, where primes denote next period values, and use equation (C.27) to obtain the wage function $w(.)$. Next, we iterate on equation (C.25) to solve for firm value $J(\cdot)$. Finally, we use the free-entry condition (C.26) and the definition of the opportunity cost in equation (C.24) to obtain the implied $\theta^1(.)$ and $b^1(.)$ functions. We update the guesses and repeat until convergence. To evaluate value functions at points $u'$ and $\omega'$ we use linear interpolation. When solving for the equilibrium policy functions, we impose that the probabilities $f(.)$ and $q(.)$ lie between zero and one. These restrictions also guarantee that $v$ and $\theta$ are always positive.

C.3.4 Additional Results

In Figures C.2, C.3, and C.4, we present the impulses of the fraction of unemployed receiving UI, the log opportunity cost, and log vacancies to a one-month increase in the UI error innovation. In Figures C.5 and C.6 we depict the path of productivity and separations shocks underlying the experiment depicted in Figure 3.8 in the main text. In each figure, the left panel corresponds to the high $b$ model and the right panel corresponds to the low $b$ model.

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12Our calibration is conservative in the sense that reducing the level of $\xi$ would produce even smaller effects of UI policy on aggregate outcomes. Chodorow-Reich and Karabarbounis (2016) show that, with standard preferences, $z$ is between 0.47 and 0.75. Hornstein, Krusell and Violante (2011) argue that $z$ has to be even smaller in order for models to generate large frictional wage dispersion. Hall and Mueller (2015) also arrive at a small value of $z$ given the large observed dispersion in the value of a job. Costain and Reiter (2008) first pointed out that models with a high level of $z$ generate stronger effects of policies on labor market outcomes than the effects found in cross-country comparisons.
Figure C.2: Impulse Response of Fraction Receiving UI in the Model

Notes: The figure plots the coefficients on $e_t$ from the regression $\omega_{t+h} = \beta(h)e_t + \sum_{j=0}^{11} \gamma_j(h)u_{t-j} + v_{t+h}$ using data generated from model simulations.

Figure C.3: Impulse Response of Log Opportunity Cost in the Model

Notes: The figure plots the coefficients on $e_t$ from the regression $\log b_{t+h} = \beta(h)e_t + \sum_{j=0}^{11} \gamma_j(h)u_{t-j} + v_{t+h}$ using data generated from model simulations.
Figure C.4: Impulse Response of Log Vacancies in the Model

Notes: The figure plots the coefficients on $e_t$ from the regression $\log v_{t+h} = \beta(h)e_t + \sum_{j=0}^{11} \gamma_j(h)u_{t-j} + v_{t+h}$ using data generated from model simulations.

Figure C.5: Productivity Path in the Model

Notes: The figure plots the path of productivity used to generate the simulation in Figure 3.8.
Figure C.6: Separations in the Model

Notes: The figure plots the path of the separation rate used to generate the simulation in Figure 3.8.