

The Limited Macroeconomic Effects of Unemployment Benefit Extensions*

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Abstract

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 use our estimates to quantify the effects of 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.

JEL-Codes: E24, E62, J64, J65.

Keywords: Unemployment Insurance, Measurement Error, Unemployment.

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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 (Rothstein, 2011; Farber and Valletta, 2015; Johnston and Mas, Forthcoming). 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, 2015). Others have emphasized the potential stimulus effects of increasing transfers to unemployed individuals (Summers, 2010; Congressional Budget Office, 2012). 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 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 research design that explores the variation of 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 because of differences in fundamentals or because of measurement error. We use subsequent revisions of the unemployment rate to separate the

Table 1: April 2013 Example

		Louisiana	Wisconsin
Real-Time Data	Unemployment Rate (Moving Average)	5.9%	6.9%
	Duration of Benefit Extensions	14 Weeks	28 Weeks
Revised Data (2015)	Unemployment Rate (Moving Average)	6.9%	6.9%
	Duration of Benefit Extensions	28 Weeks	28 Weeks
	UI Error	-14 Weeks	0 Weeks

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 1 uses the example of Louisiana and Wisconsin in April 2013 to illustrate our strategy. 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 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. Innovations in the UI error have negligible effects on state-level unemployment, employment, vacancies, and worker earnings. In our 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 UI errors failing to lead to more unemployed

receiving benefits. They simply reflect the small macroeconomic effects of an increase in UI eligibility and receipt.

Our empirical exercise provides a direct answer to 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. The policy debate following the Great Recession has focused on the different, but related, question of the macroeconomic effects of extending benefits all the way from 26 to as many as 99 weeks. Extrapolating linearly the upper bound of our estimates, we find that extending benefits from 26 to 99 weeks would increase the unemployment rate by at most 0.3 percentage point. We explore the informativeness of a linear extrapolation in the data by estimating responses separately for high and low initial levels of benefit duration and more and less persistent UI errors. The response of labor market variables such as unemployment does not vary along either dimension, suggesting the plausibility of a linear extrapolation.

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.

We show the robustness of our results to the inclusion of a number of controls into the baseline specification, to alternative specifications, and to alternative ways of extracting innovations in the UI error. The UI error approach also depends on the information content of the unemployment rate revisions. We derive a bound for the consistency of our estimator so long as the revised data measure true economic conditions at least as well as the real-time data. Beyond describing the information contained in the revision process, we 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.

Finally, we validate our results through the lens of a standard DMP model (Diamond, 1982; Mortensen and Pissarides, 1994) augmented with a UI policy. Relative to the linear extrapolation of the responses of labor market variables to UI errors, the model allows for anticipation effects by workers and firms that differ according to whether a benefit extension is caused by a transitory UI error or by a persistent increase in unemployment. The effect of UI policy on macroeconomic outcomes depends crucially on the level of the opportunity cost of employment. We show that, under the low level of the opportunity cost of employment 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 2011.¹

The macroeconomic effects of UI benefits concern their effect on aggregate unemployment.² 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, Karahan, Manovskii, and Mitman, 2015). 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 (Levine, 1993; Landais, Michaillat, and Saez, 2015; Lalive, Landais, and Zweimüller, 2015). 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, Karahan, Manovskii, and Mitman (2015, HKMM) and Hagedorn, Manovskii, and Mitman (2015, HMM) use a county border discontinuity design to directly 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

¹Schmieder, 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.

²Our 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.

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 by both Boone, Dube, Goodman, and Kaplan (2016) and Dieterle, Bartalotti, and Brummet (2016). Boone, Dube, Goodman, and Kaplan (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.³ Dieterle, Bartalotti, and Brummet (2016) 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, Bartalotti, and Brummet (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.

³For example, Boone, Dube, Goodman, and Kaplan (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 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.

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.

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 A.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 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 $t + h$ and the maximum duration of unemployment benefit receipt in the state at date t , which we denote as $T_{s,t}^*$:

$$y_{s,t+h} = \beta T_{s,t}^* + e_{s,t+h}. \quad (1)$$

Two main challenges arise in using equation (1) to estimate the causal effect β 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 the error term $e_{s,t+h}$. In Section 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 (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 the error term $e_{s,t+h}$. Section 3.2 explains how we address this issue of serial correlation in the duration of benefits.

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 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:

$$T_{s,t}^* = f_{s,t}(u_{s,t-1}^*), \quad (2)$$

where $u_{s,t-1}^*$ denotes the real-time unemployment rate reported in month t for the latest available month, $t - 1$.⁴

The reported unemployment rate in real time, $u_{s,t}^*$, may deviate from the true unemployment rate, $u_{s,t}$, because of measurement error, denoted by $\hat{u}_{s,t} = u_{s,t}^* - u_{s,t}$. Our empirical strategy exploits variation in this measurement error to extract the component of benefit extensions which is 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 (2):

$$T_{s,t} = f_{s,t}(u_{s,t-1}). \quad (3)$$

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

$$T_{s,t}^* = T_{s,t} + \hat{T}_{s,t}. \quad (4)$$

⁴For 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 A.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.

Equation (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 (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 $\hat{T}_{s,t}$.

3.1.1 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) to construct $T_{s,t}$.⁵ 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 Appendix A.2 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

⁵We 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.

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.⁶ 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 Appendix A.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 $\hat{u}_{s,t}$.

3.1.2 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

⁶The 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 2: Accuracy of Our Algorithm for Calculating UI Benefit Extensions

	EB	TEUC02	EUC08	Total
Original Trigger Notices				
Same as our algorithm	45456	3982	14291	63729
Different from our algorithm	44	18	9	71
Corrected Trigger Notices				
Same as our algorithm	45494	3999	14300	63793
Different from our algorithm	6	1	0	7

Notes: The table reports the number of state-weeks where our algorithm gives the same UI benefit tier eligibility as the published DOL trigger notices. The period is 1996-2015. 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.

(DOL). The DOL produces each week a trigger notice that contains for each state the most recent available moving 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.⁷ 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 A.1 to calculate $T_{s,t}$. The UI error then equals $\hat{T}_{s,t} = T_{s,t}^* - T_{s,t}$.⁸ To

⁷The 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.

⁸States have the option of whether or not to adopt two of the triggers for EB status. We follow the actual state

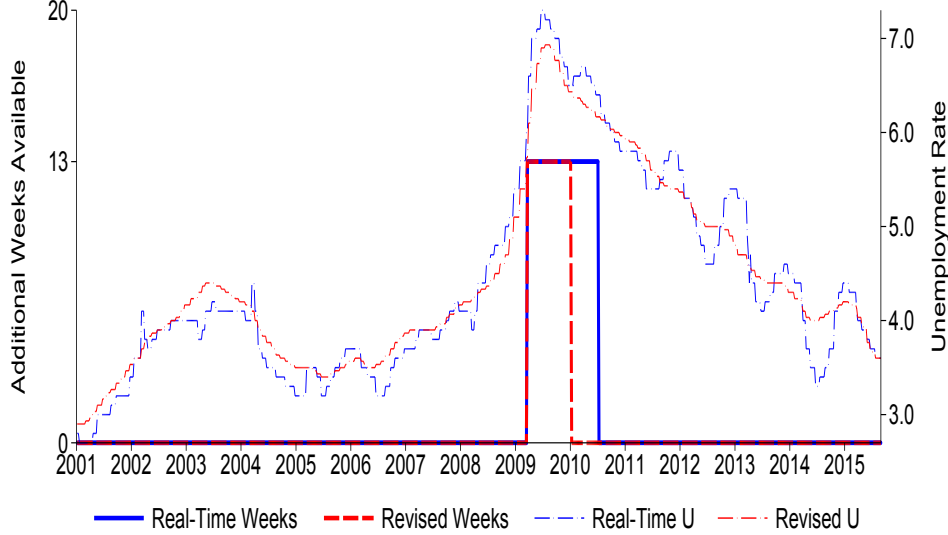


Figure 1: Extended Benefits and Unemployment in Vermont

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).

verify the accuracy of our algorithm for constructing $T_{s,t}$, we apply the same algorithm using 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 2. Of 63,800 possible state-weeks, our algorithm agrees exactly with the trigger notices in all but 7 cases.⁹

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 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).

⁹Our 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 or by referencing the actual text of 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 (Rothstein, 2011; Hagedorn, Karahan, Manovskii, and Mitman, 2015; Hagedorn, Manovskii, and Mitman, 2015; Marinescu, 2017). 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 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 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 Appendix A.2 we show that Vermont’s UI error is entirely accounted for by the 2015 methodological improvement in the LAUS statistical model.

3.1.3 Relationship to Instrumental Variables

The next step in our methodology is to replace actual benefit duration $T_{s,t}^*$ in equation (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 (1).¹⁰ Running the implied first-stage

¹⁰Alternatively, the unemployment rate errors themselves could serve as instruments for $T_{s,t}^*$. However, using the unemployment rate errors instead of the UI errors disregards our knowledge of the exact nonlinear mapping from $\hat{u}_{s,t}$ to the UI errors $\hat{T}_{s,t}$. The UI error $\hat{T}_{s,t}$ makes full use of this information and produces much tighter confidence intervals in what follows.

regression $T_{s,t}^* = \pi_0 + \pi_1 \hat{T}_{s,t} + \pi_2 \mathbb{I}\{t \in \text{TEUC02, EUC08}\} + e_{s,t}$, 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 simply replaces $T_{s,t}^*$ in equation (1) with $\hat{T}_{s,t}$. We work with the reduced form version because it facilitates our treatment of the dynamics in the duration of benefits, as we explain next.

3.2 Serial Correlation of Benefit Duration

The UI error $\hat{T}_{s,t}$ is a serially correlated process because the underlying measurement error in unemployment $\hat{u}_{s,t}$ is serially correlated as shown for example in Figure 1 for Vermont.¹¹ When an outcome variable $y_{s,t+h}$ depends on leads or lags of a persistent right-hand side variable $\{\hat{T}_{s,t+j}\}_{j=J_l}^{J_h}$, a regression of $y_{s,t+h}$ only on the contemporaneous $\hat{T}_{s,t}$ confounds the response to $\hat{T}_{s,t}$ 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.¹² Define the current period unexpected component of the UI error:

$$\epsilon_{s,t} = \hat{T}_{s,t} - \mathbb{E}_{t-1} \hat{T}_{s,t}, \quad (5)$$

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

$$y_{s,t+h} = \beta(h)\epsilon_{s,t} + \Gamma(h)\mathbf{X}_{s,t} + \nu_{s,t+h}, \quad (6)$$

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 $\mathbf{X}_{s,t}$ is a vector of covariates. Specification (6) deals with

¹¹The 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 $\hat{u}_{s,t}$. To give a sense of the latter, the first 8 autocorrelation coefficients of $\hat{u}_{s,t}$ are 0.78, 0.63, 0.52, 0.45, 0.40, 0.35, 0.32, 0.29.

¹²Ramey (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.

the correlation between $\hat{T}_{s,t}$ and past UI errors by replacing it with the serially-uncorrelated innovation $\epsilon_{s,t}$. Thus, the coefficients $\beta(h) = \mathbb{E}[y_{s,t+h} | \hat{T}_{s,t} = 1, \hat{T}_{s,t-1}, \hat{T}_{s,t-2}, \dots] - \mathbb{E}[y_{s,t+h} | \hat{T}_s = 0, \hat{T}_{s,t-1}, \hat{T}_{s,t-2}, \dots]$ for $h = 0, 1, 2, \dots$ 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 5.1 reports the persistence of the increase in actual potential benefit duration following a UI error innovation by setting $y_{s,t+h} = \hat{T}_{s,t+h}$ in equation (6). We discuss the effect on expectations in Section 5.2.2.

We implement three methods to identify the unexpected component in the UI error $\epsilon_{s,t}$. Our preferred approach allows the UI error $\hat{T}_{s,t}$ to follow a first-order discrete Markov chain with probability $\pi_T \left(\hat{T}_{s,t+1} = x_j \mid \hat{T}_{s,t} = x_i; u_{s,t}, t \right)$ 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 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.¹³ In practice, we aggregate $\hat{T}_{s,t}$ up to a monthly frequency and estimate each probability $\pi_T \left(\hat{T}_{s,t+1} = x_j \mid \hat{T}_{s,t} = x_i; u_{s,t}, t \right)$ 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 $\mathbb{E}_{t-1} \hat{T}_{s,t}$ and form the UI error innovation $\epsilon_{s,t}$ using equation (5).¹⁴

¹³For example, measurement error in the mid-2000s does not cause a UI error for Vermont in Figure 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.

¹⁴A 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 A.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

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 $\mathbf{X}_{s,t}$). 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.¹⁵

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 employment growth from the Current Employment Statistics program and monthly labor force participation from the LAUS program. 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.¹⁶ 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

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.

¹⁵This 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.

¹⁶These 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/[days in month] because the number of unemployed are a stock measure as of the CPS survey reference week.

Table 3: Summary Statistics of Selected Variables

Variable	Symbol	Mean	S.D.	Within S.D.	P(25)	P(75)	Obs.
Unemployment Rate Error	$\hat{u}_{s,t}$	-0.08	0.37	0.32	-0.29	0.13	11700
Actual Duration of Benefit Extensions	$T_{s,t}^*$	3.07	5.04	1.33	0.00	3.23	11700
UI Error	$\hat{T}_{s,t}$	0.02	0.49	0.47	0.00	0.00	11700
UI Error Innovation	$\epsilon_{s,t}$	-0.00	0.31	0.30	-0.00	0.00	11550
Unemployment Rate (Revised 2015)	$u_{s,t}$	5.55	1.93	0.82	4.20	6.60	11700
Fraction Unemployed Receiving UI	$\phi_{s,t}$	36.47	16.59	6.71	23.86	45.94	11500
Log Vacancies (Detrended)	$\log v_{s,t}$	0.04	0.27	0.16	-0.15	0.24	7656
Log Payroll Employment (Detrended)	$\log E_{s,t}$	0.00	0.03	0.02	-0.02	0.02	11700
Log Earnings of All Workers (Detrended)	$\log w_{s,t}$	0.00	0.03	0.03	-0.02	0.02	3204
Log Earnings of New Hires (Detrended)	$\log w_{s,t}$	0.00	0.05	0.04	-0.03	0.03	3066
Memo:							
Duration of Benefit Extensions ($\hat{T}_{s,t} \neq 0$)	$T_{s,t}^*$	10.94	4.18		8.03	15.07	618
UI Error ($\hat{T}_{s,t} \neq 0$)	$\hat{T}_{s,t}$	0.43	2.09		-1.39	2.08	618
UI Error Innovation ($\Delta \hat{T}_{s,t} \neq 0$)	$\epsilon_{s,t}$	0.02	1.33		-0.95	0.96	573
Length of Episode		3.86	3.13		2.00	4.00	161

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.

our analysis of vacancies.¹⁷ 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 reports summary statistics. Our sample covers the period between 1996 and 2015 for the 50 U.S. states.¹⁸ 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 with our empirical approach is that there are too few or too small errors to identify significant effects of benefit extensions on macroeconomic outcomes. Table 3 shows that this is not true. There are 618 cases in which a state would have had a different duration

¹⁷The 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.

¹⁸We 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).

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 larger than 2 months.¹⁹ 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 3.2, 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.

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 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 5.2. Section 5.3 relaxes the assumption that $\epsilon_{s,t}$ is purely random and Section 5.4 presents additional sensitivity analyses of our baseline specifications. Finally, in Section 5.5 we discuss the possibility of measurement error in the revised unemployment rate and provide auxiliary evidence that the revised unemployment rate better measures true

¹⁹Throughout the paper, when referring to months of benefit extensions we use the convention that one month equals 4.33 weeks.

economic conditions than the real-time unemployment rate.

5.1 Baseline Results

In this section we measure the response of labor market variables to a one-month UI error innovation $\epsilon_{s,t}$ using the specification:

$$y_{s,t+h} = \beta(h)\epsilon_{s,t} + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + \nu_{s,t+h}, \quad (7)$$

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. The coefficients $\beta(h)$ for $h = 0, 1, 2, \dots$ trace out the impulse response function of y with respect to a one-month unexpected change in the UI error. We include state and month fixed effects because, as seen in Table 3, they absorb substantial variation in our main outcome variables and, therefore, increase precision. The lags of the (revised) unemployment rate approximate the experiment of comparing two states on similar unemployment paths until one receives an unexpected UI error. The identifying assumption that $\epsilon_{s,t}$ is orthogonal to $\nu_{s,t+h}$, $\mathbb{E}[\epsilon_{s,t} \times \nu_{s,t+h} | \text{controls}] = 0$, is valid if the underlying measurement error in the unemployment rate $\hat{u}_{s,t}$ that gives rise to $\epsilon_{s,t}$ is random.²⁰ In all figures, dashed lines report the 90 percent confidence interval based on standard errors two-way clustered by state and by month.

Figure 2 shows impulse responses of the innovation ϵ and the UI error \hat{T} to a one-month innovation ϵ . 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.²¹ The UI error \hat{T} rises one-for-one with ϵ on impact and then decays over the next few months with a half-life of roughly 2 months.

²⁰This identification argument applies without controls and we report the robustness of our results from specifications without controls in Section 5.4. The twelve lags of the unemployment rate directly control for the small increment to the variation in the measurement error \hat{u} accounted for by lags of the unemployment rate shown in Table A.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).

²¹Time 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 $t+1$.

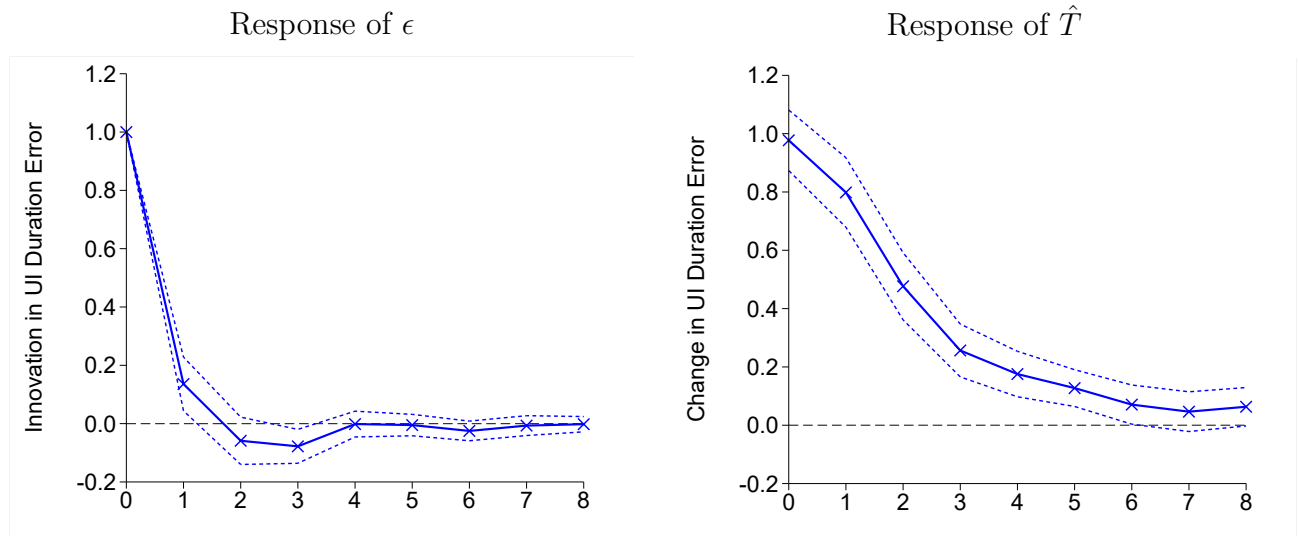


Figure 2: Serial Correlation

Notes: The figure plots the coefficients on $\epsilon_{s,t}$ from the regression $y_{s,t+h} = \beta(h)\epsilon_{s,t} + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + \nu_{s,t+h}$, where $y_{s,t+h} = \epsilon_{s,t+h}$ is the UI error innovation (left panel) or $y_{s,t+h} = \hat{T}_{s,t+h}$ is the UI error (right panel). The dashed lines denote the 90 percent confidence interval based on two-way clustered standard errors.

Figure 3 illustrates the main result of the paper. The left panel shows the responses from regression (7) when the left-hand side variable is the (revised) unemployment rate. The unemployment rate barely responds to the increase in the duration of benefits. The point estimate for the response is essentially zero. The upper bound is roughly 0.02 percentage point. The data do not reject a zero response of the unemployment rate at any horizon.²²

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 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 right panel of Figure 3 reports the response of vacancy creation. The macroeconomic effect of benefit extensions on unemployment may exceed the microeconomic effect because

²²The small standard errors reflect the substantial variation in the right-hand side variable ϵ relative to the outcome variable u shown in Table 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 $\frac{1}{\sqrt{N}} \frac{\sigma_u}{\sigma_\epsilon} = \frac{1}{\sqrt{11550}} \frac{0.82}{0.33} \approx 0.023$. The two-way clustered standard error differs only slightly from this back-of-the-envelope estimate.

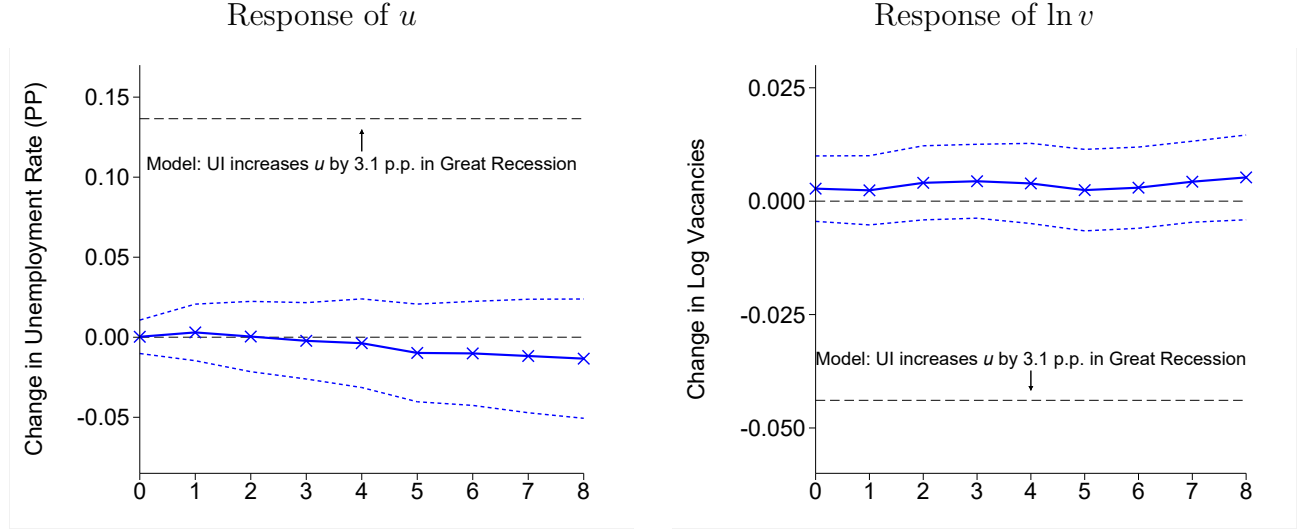


Figure 3: Impulse Response of Unemployment Rate and Log Vacancies

Notes: The figure plots the coefficients on $\epsilon_{s,t}$ from the regression $y_{s,t+h} = \beta(h)\epsilon_{s,t} + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + \nu_{s,t+h}$, 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.

of a general equilibrium mechanism intermediated by vacancies. The mechanism posits that, following 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, Karahan, Manovskii, and Mitman, 2015). However, Figure 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 6 parameterized such that the extension of benefits from 6 to 20 months caused unemployment in the Great Recession to remain persistently high.

Figure 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

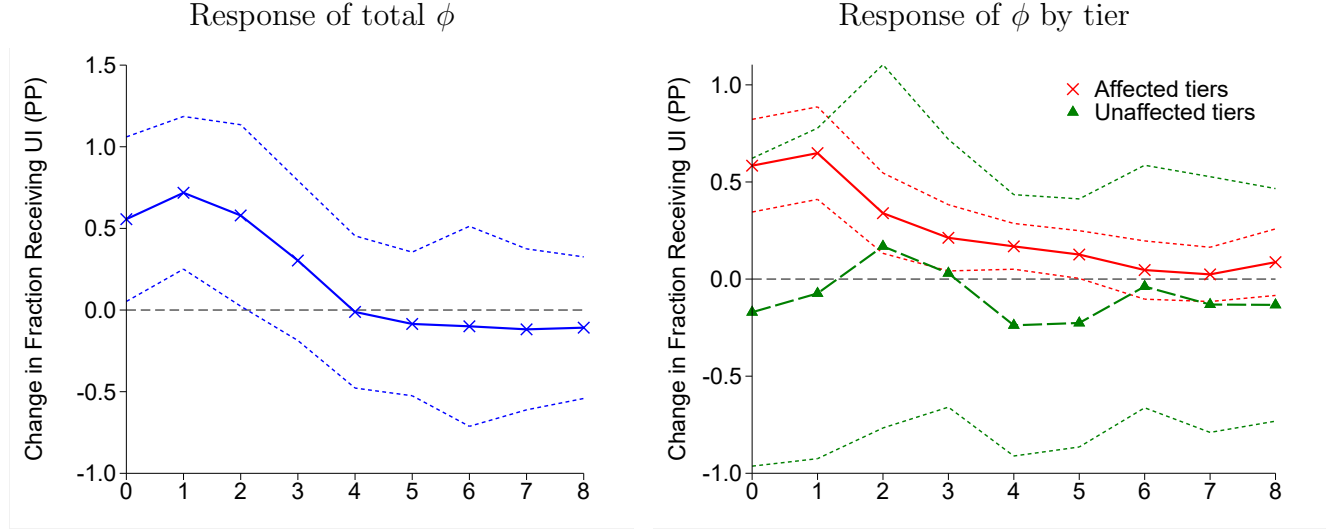


Figure 4: Impulse Response of Fraction Receiving UI

Notes: The figure plots the coefficients on $\epsilon_{s,t}$ from the regression $\phi_{s,t+h} = \beta(h)\epsilon_{s,t} + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + \nu_{s,t+h}$. In the left panel, $\phi_{s,t+h}$ includes UI recipients in all tiers. The right panel plots separate impulse response functions for UI recipients in tiers with a UI error (solid red line) and in tiers without a UI error (dashed green line). In the right panel the sample starts in 2008. The dashed lines denote the 90 percent confidence interval based on two-way clustered standard errors.

a take-up rate in the range of estimates documented by Blank and Card (1991). The right panel of Figure 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).²³ All of the additional take-up of UI benefits occurs among individuals on tiers directly affected by the UI error.

Finally, Table 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 (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.²⁴

²³We do not have UI receipt by tier for the EB or TEUC02 programs. Therefore, the sample in the right panel of Figure 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.

²⁴For $u_{s,t}$, the lags of the unemployment rate included in the baseline regression (7) make the differencing with respect to $u_{s,t-1}$ 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 4. We prefer the levels specification in the left panel because of a time-

Table 4: Response of Variables to UI Error Innovation

Horizon:	Levels		Differences	
	1	4	1	4
1. Unemployment rate	0.003 (0.009)	−0.003 (0.015)	0.003 (0.009)	−0.003 (0.015)
2. Log Vacancies	0.003 (0.004)	0.004 (0.005)	0.001 (0.002)	0.002 (0.003)
3. Fraction Receiving UI	0.751** (0.253)	−0.039 (0.263)	0.914** (0.291)	0.129 (0.285)
4. Log Payroll Employment	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	−0.000 (0.000)
5. Labor Force Participation Rate	0.001 (0.018)	0.001 (0.022)	0.012+ (0.007)	0.014 (0.014)
6. Log Earnings (All Workers)	0.001 (0.002)	−0.001 (0.002)	0.004 (0.004)	0.002 (0.003)
7. Log Earnings (New Hires)	−0.000 (0.003)	0.004 (0.004)	−0.000 (0.004)	0.004 (0.007)

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 $\epsilon_{s,t}$, controlling for state and period fixed effects and 12 monthly or 4 quarterly lags of $u_{s,t}$. 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.

Across all variables, we find economically negligible responses to a positive one-month innovation in the UI error. The standard errors rule out effects much larger in magnitude.

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

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 $\epsilon_{s,t}$ and may also be correlated with variables in $t - 1$. Indeed, we have already noted the small serial correlation of $\epsilon_{s,t}$ 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, which counts all UI payments during the month, and for the wage measures which include total earnings over the month. We account for this issue in Table 4 by taking a difference of these variables with respect to their $t - 2$ value.

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, 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.²⁵

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.

5.2.1 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.²⁶ A concern for the linear extrapolation that we performed may be that labor market variables respond more to benefit

²⁵The 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.

²⁶The 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, Karahan, Manovskii, and Mitman (2015) 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.

Table 5: Baseline Duration and Length of Episode

Dependent variable:	Unemployment rate		Log vacancies		Fraction receiving	
Horizon:	1	4	1	4	1	4
Panel A:						
$\epsilon_{s,t}$	0.004 (0.010)	-0.003 (0.018)	0.002 (0.008)	0.007 (0.010)	0.925** (0.249)	-0.051 (0.280)
$\epsilon_{s,t} \times [T_{s,t} > 10.5]$	-0.002 (0.008)	0.000 (0.019)	0.003 (0.011)	-0.006 (0.012)	-0.425 (0.395)	0.028 (0.297)
Panel B:						
$\epsilon_{s,t}$	0.008 (0.011)	0.008 (0.019)	0.002 (0.005)	0.004 (0.006)	0.596* (0.257)	-0.345 (0.226)
$\epsilon_{s,t} \times [\text{Length}_{s,t} > 6]$	-0.020 (0.012)	-0.042 (0.027)	0.003 (0.012)	-0.001 (0.013)	0.584 (0.472)	1.149* (0.569)
Observations	10,850	10,700	7,084	6,932	10,750	10,600

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 twoway-clustered by state and month and are in parentheses. **, * denote significance at the 1% and 5% levels.

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 5 assesses this possibility by allowing the effect of a UI innovation $\epsilon_{s,t}$ in regression (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 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.

5.2.2 Persistence

The typical extension of benefits is more persistent than a typical UI error in our sample.²⁷ 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.²⁸ In Section 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 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

²⁷The 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 implied that states triggered 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.

²⁸Related to this point, the UI literature contains conflicting evidence on how forward-looking are potential unemployment benefit recipients with respect to future benefit eligibility. Card and Levine (2000) and Card, Chetty, and Weber (2007) estimate 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.

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 5 reports coefficients from interacting the UI innovation $\epsilon_{s,t}$ in our baseline regression (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 set 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.

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

In Section 3.1.1, 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 $\epsilon_{s,t}$. 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.

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

We augment our baseline specification to:

$$y_{s,t+h} = \beta(h)\epsilon_{s,t} + g(\{\hat{u}_{s,t}\}) + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + \nu_{s,t+h}, \quad (8)$$

where the flexible function $g(\cdot)$ may allow for leads, lags, and non-linear transformations of the measurement error in the unemployment rate $\hat{u}_{s,t}$. Specification (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 (8), it helps to start with the case where $y_{s,t+h} = u_{s,t+h}$ and $g(\cdot) = \rho(h)\hat{u}_{s,t-1}$. 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 determines $\epsilon_{s,t}$. 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 1 in the case of Vermont. Formally, the identification assumption becomes $\mathbb{E}[\epsilon_{s,t} \times \nu_{s,t+h} | \mathbf{X}_{s,t}, \hat{u}_{s,t-1}] = 0$. 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, $\mathbb{E}[u_{s,t+h} | \mathbf{X}_{s,t}, \hat{u}_{s,t-1}, \epsilon_{s,t} = \epsilon] = \mathbb{E}[u_{s,t+h} | \mathbf{X}_{s,t}, \hat{u}_{s,t-1}, \epsilon_{s,t} = 0] + \beta(h)\epsilon = \Gamma(h)\mathbf{X}_{s,t} + \rho(h)\hat{u}_{s,t-1} + \beta(h)\epsilon$. Including leads, lags, or non-linear transformations of $\hat{u}_{s,t-1}$ in the function $g(\cdot)$ 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 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 (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

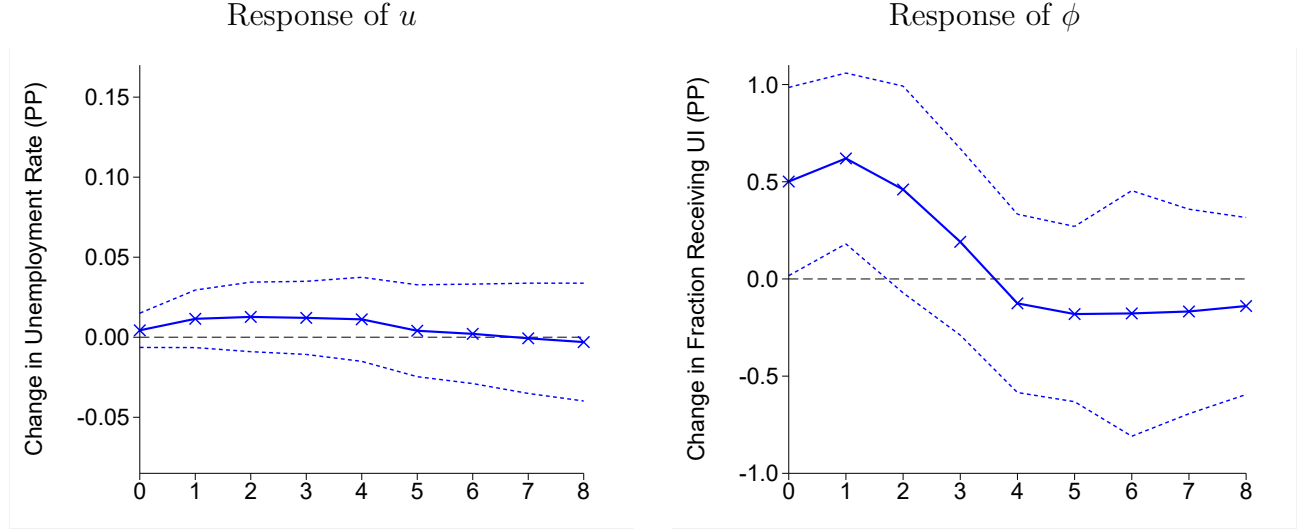


Figure 5: Impulse Responses Controlling for Measurement Error $\hat{u}_{s,t}$

Notes: The figure plots the coefficients on $\epsilon_{s,t}$ from the regression $y_{s,t+h} = \beta(h)\epsilon_{s,t} + \rho(h)\hat{u}_{s,t-1} + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + \nu_{s,t+h}$, 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.

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 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 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

Table 6: Sensitivity of Impulse Responses to Controlling for Measurement Error $\hat{u}_{s,t}$

Dependent Variable:	Unemployment Rate		Log Vacancies		Fraction Receiving	
Horizon:	1	4	1	4	1	4
Additional controls						
1. $\hat{u}_{s,t-1}$	0.011 (0.009)	0.011 (0.015)	0.004 (0.004)	0.004 (0.005)	0.645** (0.230)	-0.146 (0.244)
2. $\{\hat{u}_{s,t+j}\}_{j=-12}^{12}$	0.013 (0.010)	0.014 (0.015)	0.004 (0.005)	0.005 (0.005)	0.633** (0.230)	-0.166 (0.234)
3. $\hat{u}_{s,t-1}, \hat{u}_{s,t-1}^2, \hat{u}_{s,t-1}^3$	0.010 (0.009)	0.010 (0.015)	0.005 (0.004)	0.005 (0.005)	0.654** (0.242)	-0.148 (0.247)
4. $\hat{u}_{s,t-1}, \hat{u}_{s,t-1} * \mathbb{I}\{\hat{u}_{s,t-1} \geq 0\}$	0.011 (0.009)	0.011 (0.015)	0.004 (0.004)	0.005 (0.005)	0.650** (0.235)	-0.135 (0.241)
5. $\hat{u}_{s,t-1} \times \mathbb{I}\{t \in \text{year}\}$	0.008 (0.010)	0.005 (0.015)	0.005 (0.004)	0.005 (0.005)	0.600** (0.199)	-0.211 (0.240)

Notes: Each cell reports the coefficient from a separate regression of the dependent variable indicated in the table header on the UI error innovation $\epsilon_{s,t}$, controlling for the baseline control variables, $\sum_{j=1}^{12} \gamma_j u_{s,t-j}$, d_s , and d_t , 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.

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 5.1.

5.3.2 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 loser” as the reason for unemployment. Thus, we can infer CPS

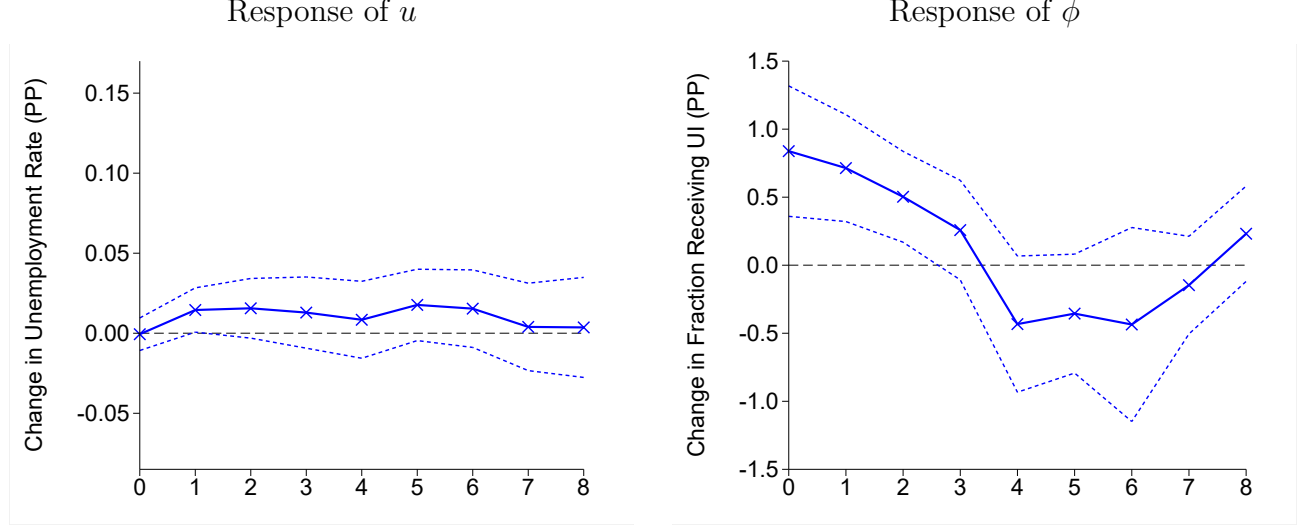


Figure 6: Impulse Responses Restricting Variation in Measurement Error

Notes: The figure plots the coefficients on $\epsilon_{s,t}$ from the regression $y_{s,t+h} = \beta(h)\epsilon_{s,t} + \sum_{j=1}^{12} \gamma_j(h)u_{s,t-j} + d_s(h) + d_t(h) + \nu_{s,t+h}$, 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.

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 $\epsilon_{s,t}$ from the newly constructed UI error.

We estimate impulse responses using the specification (7) but with our alternative series for the UI error innovation $\epsilon_{s,t}$. 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 6 reports the impulse response functions for the unemployment rate and the total fraction receiving

Table 7: Sensitivity of Impulse Responses to Alternative Specifications

Dependent Variable:		Unemployment Rate		Log Vacancies		Fraction Receiving	
Horizon:		1	4	1	4	1	4
Regressor	Controls						
1. $\epsilon_{s,t}$	$\{u_{s,t-j}\}_{j=1}^{12}, d_s, d_t$	0.003 (0.009)	-0.003 (0.015)	0.003 (0.004)	0.004 (0.005)	0.751** (0.253)	-0.039 (0.263)
2. $\epsilon_{s,t}$	d_s, d_t	-0.014 (0.025)	-0.023 (0.026)	0.006 (0.005)	0.007 (0.006)	0.684** (0.235)	-0.137 (0.235)
3. $\epsilon_{s,t}$	None	-0.003 (0.078)	-0.029 (0.077)	0.015 (0.009)	0.019 ⁺ (0.011)	0.548 (0.664)	-0.478 (0.702)
4. $\Delta \hat{T}_{s,t}$	$\{u_{s,t-j}\}_{j=1}^{12}, d_s, d_t$	0.004 (0.008)	0.004 (0.012)	-0.000 (0.003)	-0.001 (0.003)	0.556** (0.133)	0.081 (0.187)
5. $\hat{T}_{s,t}$	$\{\hat{T}_{s,t-j}, u_{s,t-j}\}_{j=1}^{12}, d_s, d_t$	0.002 (0.008)	-0.003 (0.014)	-0.001 (0.005)	-0.001 (0.005)	0.911** (0.266)	0.012 (0.242)

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.

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.

5.4 Further Robustness

In this section we investigate the robustness of our main findings along various other dimensions. Table 7 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 5.1.

Row 2 removes the lags of the unemployment rate from the specification. We obtain very similar results to the baseline. Row 3 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 4, we instead simply first difference the UI error and replace $\epsilon_{s,t}$ in equation (7) with $\Delta\hat{T}_{s,t} = \hat{T}_{s,t} - \hat{T}_{s,t-1}$. In row 5, 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.²⁹ The limited response of unemployment and vacancies and the significantly positive response of the fraction receiving UI remain robust to these alternative specifications.

5.5 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 B we consider formally the case where the revised unemployment rate also contains measurement error with respect to the true unemployment rate. We obtain three

²⁹Formally, 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.

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 $\epsilon_{s,t}$ 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 3.1.1 and Appendix A.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:

$$y_{s,t} = \beta^{\text{revised}} u_{s,t-2}^{\text{revised}} + \beta^{\text{real-time}} u_{s,t-2}^{\text{real-time}} + \nu_{s,t}, \quad (9)$$

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 β^{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

Table 8: Spending Decisions and Unemployment Data

	Dependent Variable					
	Auto Sales			Building Permits		
	(1)	(2)	(3)	(4)	(5)	(6)
Revised $UR_{s,t-2}$	-0.42** (0.11)		-0.52** (0.13)	-0.09** (0.02)		-0.10** (0.02)
Real-time $UR_{s,t-2}$		-0.34** (0.10)	0.09+ (0.05)		-0.07** (0.02)	0.01 (0.02)
State FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean	5.4	5.4	5.4	0.5	0.5	0.5
Dep. var. sd	2.0	2.0	2.0	0.4	0.4	0.4
R^2	0.61	0.61	0.61	0.73	0.76	0.77
Observations	10,096	9,847	9,847	15,800	12,147	12,147

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.

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 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 8 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 (9). For both auto sales and building permits, we estimate $\beta^{\text{revised}} < 0$ and $\beta^{\text{real-time}} \approx 0$. The estimates of β^{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 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:

$$y_{i,s,t} = \beta^{\text{revised}} u_{s,t}^{\text{revised}} + \beta^{\text{real-time}} u_{s,t}^{\text{real-time}} + \Gamma \mathbf{X}_{i,s,t} + \nu_{i,s,t}. \quad (10)$$

Table 9 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 8 and 9 provide direct additional evidence that the 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 Appendix B 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.

Table 9: Beliefs and Unemployment Data

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

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, age², age³, 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.

6 Model Validation Exercise

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 5.2.1 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, Karahan, Manovskii, and Mitman (2015) 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.³⁰ We describe here only the elements of the model essential to our argument and provide additional detail in Appendix C.

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 δ_t . Employed individuals who lose their jobs become eligible for UI benefits with probability γ . 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 β . 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 ξ . 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 average unemployed individual as $U_t = \omega_t U_t^E + (1 - \omega_t) U_t^I$, where ω_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, $S_t = W_t - U_t = w_t - z_t + \beta(1 - \delta_t - f_t)\mathbb{E}_t S_{t+1}$,

³⁰This 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.

where z_t denotes the flow opportunity cost of employment for the average unemployed:

$$z_t = \xi + \underbrace{\omega_t B - (\delta_t(\gamma - \omega_t) + (1 - f_t)\omega_t e_t)}_{b_t} \beta (\mathbb{E}_t U_{t+1}^E - \mathbb{E}_t U_{t+1}^I), \quad (11)$$

In equation (11), ξ denotes the flow value of non-market work and b_t denotes the benefit component of the opportunity cost of employment. The expression nests the standard 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.³¹ Extending benefits, which here means a decline in the expiration probability e_t , increases the fraction of unemployed who are eligible ω_t and raises b_t and z_t .

The value of a firm which has matched with a worker is given by $J_t = p_t - w_t + \beta(1 - \delta_t)\mathbb{E}_t J_{t+1}$, where p_t denotes aggregate labor productivity. Free entry drives the expected value of creating a vacancy to zero, giving $\frac{\kappa}{q_t} = \beta\mathbb{E}_t J_{t+1}$, where κ 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 μ 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:

$$w_t = \mu p_t + (1 - \mu)z_t + \mu\kappa\theta_t. \quad (12)$$

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 Section 5.5 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

³¹The 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 $\mathbb{E}_t U_{t+1}^E > \mathbb{E}_t U_{t+1}^I$.

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:

$$\frac{\kappa}{q_t(\theta_t)} = \mathbb{E}_t \sum_{j=1}^{\infty} \beta^j \left(\prod_{i=1}^j \frac{(1 - \delta_{t+i-1})}{(1 - \delta_t)} \right) (p_{t+j} - w_{t+j}), \quad (13)$$

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 (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.³²

We parameterize two versions of the model (see Appendix C for more details). In the "low b " model we pick $b = 0.06$ and $z = \xi + b = 0.87$ 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 $z = \xi + b = 0.96$. 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 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

³²In 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.

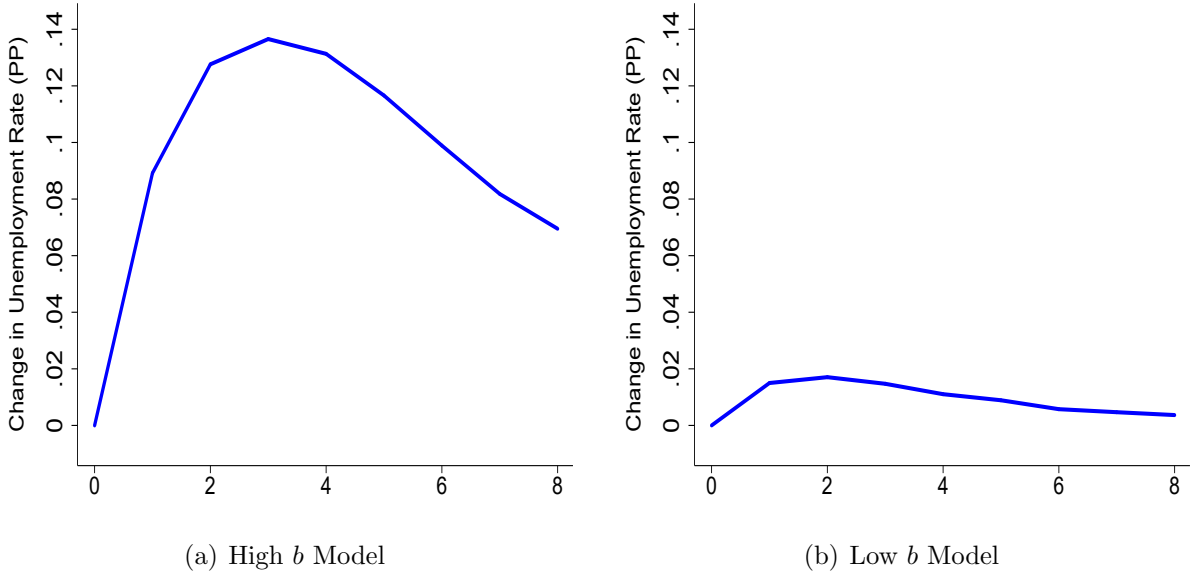


Figure 7: Impulse Response of Unemployment Rate in the Model

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

opportunity cost of employment – is small. Therefore, the extension of benefits lowers firms’ willingness to create vacancies substantially. As the left panel of Figure 7 shows, the maximal response of the 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 reports the paths of these shocks.

The left panel of Figure 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

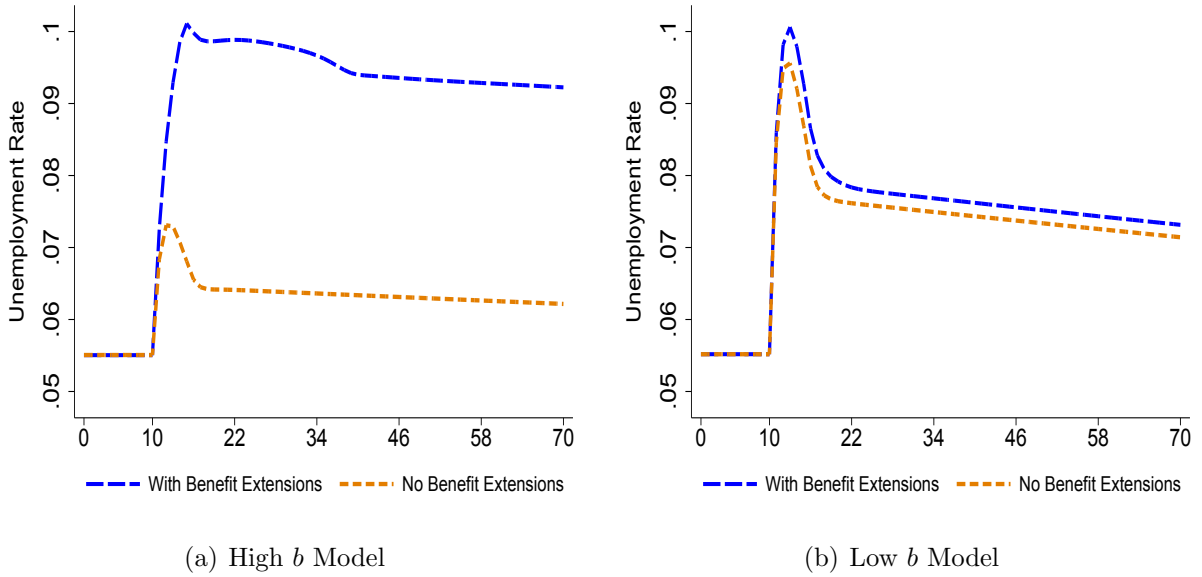


Figure 8: Unemployment Path in the Model

Notes: The figure plots the path of unemployment in response to a sequence of negative shocks with and without benefit extensions in the high b and the low b model.

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, Karahan, Manovskii, and Mitman (2015), 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 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 5.2. Because only the low b model matches the response of unemployment to a one-month UI error innovation, the results in Figure 8 validate the limited influence of UI extensions in a DMP model with large and persistent benefit extensions.

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.³³

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

³³For 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.

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 (Rothstein, 2011; Farber and Valletta, 2015; Johnston and Mas, Forthcoming). 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, Duflo, Gurgand, Rathelot, and Zamora (2013) and Lalive, Landais, and Zweimüller (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.

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The Limited Macroeconomic Effects of Unemployment Benefit Extensions

Online Appendix

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A Data Appendix

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

A.1 Extended Benefits and Emergency Compensation Programs

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

A.2 State Unemployment Rate Estimation Methodology

We outline the BLS methodology for estimating the state unemployment rates. The BLS first introduced state space models in 1989 and began to apply these models to all states in 1996. Bureau of Labor Statistics (2014) provides an in-depth but non-technical overview of what follows and Tiller (1992) and Pfeiffermann and Tiller (1996) provide a more technical treatment.

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 model methods. For each state, the observation equation is:

$$y_{s,t} = \alpha_{s,t}x_{s,t} + L_{s,t} + S_{s,t} + e_{s,t}, \tag{A.1}$$

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 $e_{s,t}$ is the

Table A.1: Extended Benefit and Emergency Compensation Programs, 1996-2015

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

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. The 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 04/04/2010 and 04/14/2010, between 05/30/2010 and 07/21/2010, and between 11/28/2010 and 12/16/2010. Between 02/22/2012 and 05/31/2012 individuals could receive up to 16 weeks of EUC Tier 4 benefits if their state was not in an EB period. The main source for this table is Department of Labor (2015).

observation error. The state space model employment or unemployment is $Y_{s,t} = \alpha_{s,t}x_{s,t} + L_{s,t} + S_{s,t} = y_{s,t} - e_{s,t}$.

The model state equations are:

$$\alpha_{s,t} = \alpha_{s,t-1} + \eta_{\alpha,s,t}, \quad (\text{A.2})$$

$$L_{s,t} = L_{s,t-1} + R_{s,t} + \eta_{L,s,t}, \quad (\text{A.3})$$

$$R_{s,t} = R_{s,t-1} + \eta_{R,s,t}, \quad (\text{A.4})$$

$$S_{s,t} = \sum_{j=1}^6 S_{j,s,t}, \quad (\text{A.5})$$

where $e_{s,t}$, $\eta_{\alpha,s,t}$, $\eta_{L,s,t}$, 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 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

¹Because 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.

²At the Census Division level the state space estimation excludes the external regressors insured unemployment or payroll employment. In terms of equations (A.1) to (A.5), $\alpha_{cd,t} = 0$ and $var(\eta_{cd,t}) = 0$.

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 A.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

³Because 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.

⁴The 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 A.2: Determinants of Unemployment Rate Errors

Determinants	R^2
CES revisions	0.03
+ 2015 LAUS redesign and identification of outliers	0.49
+ Updated seasonal factors	0.59
+ 12 lags of unemployment rate	0.61
+ Contemporaneous and 12 leads of unemployment rate	0.62

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.

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 A.1 illustrates that in our example of Vermont the 2015 LAUS technical improvements account for all of the unemployment rate error during the period of the UI error in the beginning of 2010.

B 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.

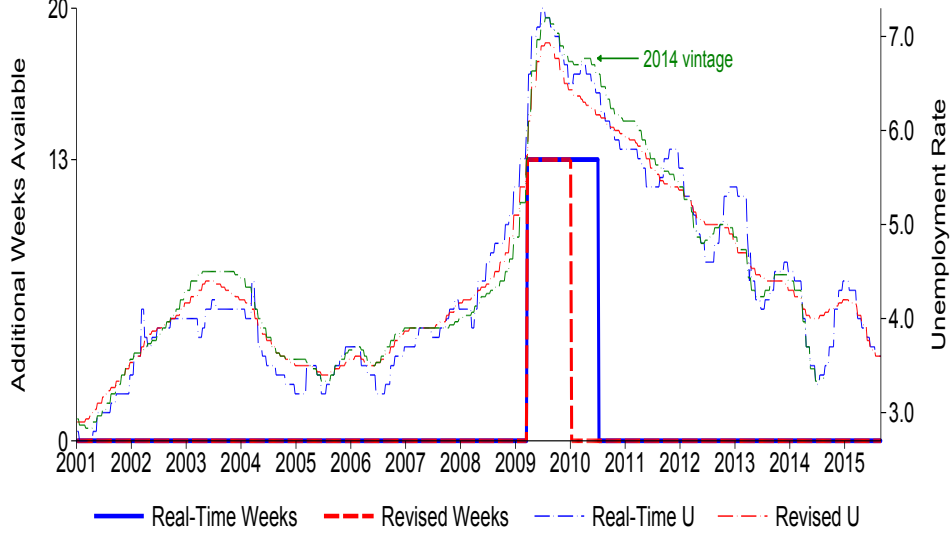


Figure A.1: Extended Benefits and Unemployment in Vermont

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). The dashed green line shows the unemployment rate using the 2014 vintage of data.

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:

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

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:

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

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:

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

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, $T^j = \mathbb{E}T^j + \epsilon^j$. All innovations ϵ^j 's are serially uncorrelated and uncorrelated with each other. The innovations in the measurement error components, ϵ^E and ϵ^X , are uncorrelated with the fundamentals F . By contrast, the innovation ϵ^F is potentially correlated with the fundamentals F .

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

$$\epsilon^{T^*} = \epsilon^F + \epsilon^E. \quad (\text{A.9})$$

Similarly, using equations (A.7) and (A.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 ϵ in the main text) as:

$$\epsilon^T = \epsilon^F + \epsilon^X, \quad (\text{A.10})$$

$$\epsilon^{\hat{T}} = \epsilon^E - \epsilon^X. \quad (\text{A.11})$$

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:

$$y = \beta\epsilon^{T^*} + \gamma F, \quad (\text{A.12})$$

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

$$y = \beta\epsilon^F + \beta\epsilon^X + \beta\epsilon^{\hat{T}} + \gamma F. \quad (\text{A.13})$$

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

$$\beta^{\text{OLS}} = \frac{\text{Cov}(y, \epsilon^{\hat{T}})}{\text{Var}(\epsilon^{\hat{T}})} = \frac{\text{Cov}(\beta\epsilon^X + \beta\epsilon^{\hat{T}}, \epsilon^{\hat{T}})}{\text{Var}(\epsilon^{\hat{T}})} = \beta \left(1 - \frac{\text{Var}(\epsilon^X)}{\text{Var}(\epsilon^{\hat{T}})} \right), \quad (\text{A.14})$$

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

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:

$$\text{Var}(\epsilon^X) \leq \text{Var}(\epsilon^E). \quad (\text{A.15})$$

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 (A.11), we see that $\text{Var}(\epsilon^{\hat{T}}) = \text{Var}(\epsilon^X) + \text{Var}(\epsilon^E)$ and, therefore, under assumption (A.15) less than 50 percent of the variance of $\epsilon^{\hat{T}}$ is attributed to ϵ^X :

$$\frac{\text{Var}(\epsilon^X)}{\text{Var}(\epsilon^{\hat{T}})} \leq 0.5. \quad (\text{A.16})$$

We estimate in the data an upper bound of $\beta^{\text{OLS}} = 0.02$. Using the upper bound of the bias $\text{Var}(\epsilon^X) / \text{Var}(\epsilon^T) = 0.50$, the true coefficient could be as large as $\beta = 0.04$. Using a standard error of 0.02, this β 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 β . In Section 5.5 we provided evidence that revisions are informative about actual spending patterns and beliefs. This implies that $\text{Var}(\epsilon^X) / \text{Var}(\epsilon^{\hat{T}})$ 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 $\text{Var}(\epsilon^X) \leq \text{Var}(\epsilon^E)$, that $\text{Var}(\epsilon^T) < \text{Var}(\epsilon^{T*})$. If we apply, for example,

$\text{Var}(\epsilon^X) / \text{Var}(\epsilon^{\hat{T}}) = 0.25$ 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 $\text{Var}(\epsilon^X) / \text{Var}(\epsilon^{\hat{T}})$ and the smaller is the attenuation bias.

C Model Appendix

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

C.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 δ_t . The law of motion for unemployment is:

$$u_{t+1} = (1 - f_t)u_t + \delta_t(1 - u_t). \quad (\text{A.17})$$

Employed individuals who lose their jobs become eligible for UI benefits with probability γ . 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, $T_t^* = 1/e_t$.⁵ 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:⁶

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

⁵For 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.

⁶In the data we have a measure of the fraction of unemployed who receive UI benefits (what we called ϕ 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.

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

$$W_t = w_t + \beta(1 - \delta_t)\mathbb{E}_t W_{t+1} + \beta\delta_t (\gamma\mathbb{E}_t U_{t+1}^E + (1 - \gamma)\mathbb{E}_t U_{t+1}^I), \quad (\text{A.19})$$

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:

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

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

where ξ is the value of non-market work and B is the UI benefit per eligible unemployed.⁷ We assume that both ξ 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.⁸

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:

$$U_t = \omega_t U_t^E + (1 - \omega_t) U_t^I. \quad (\text{A.22})$$

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:

$$S_t = W_t - U_t = w_t - z_t + \beta(1 - \delta_t - f_t) \mathbb{E}_t S_{t+1}, \quad (\text{A.23})$$

⁷Benefit 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.

⁸In previous work (Chodorow-Reich and Karabarbounis, 2016), we found that the ξ component of the opportunity cost is procyclical. Benefit extensions typically occur when unemployment is high and ξ is low. However, our empirical exercise compares two states with different duration of benefits that have the same economic fundamentals and, therefore, it is appropriate to not control for ξ in our regressions. The constancy of ξ in the model is conservative for our conclusions in this section. Allowing ξ to respond endogenously would lead to an even smaller effect of benefit extensions on unemployment because the decline in ξ would tend to offset the increase in the value of benefits (denoted b below) in the opportunity cost $z = \xi + b$.

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:

$$z_t = \xi + \underbrace{\omega_t B - (\delta_t(\gamma - \omega_t) + (1 - f_t)\omega_t e_t)}_{b_t} \beta (\mathbb{E}_t U_{t+1}^E - \mathbb{E}_t U_{t+1}^I), \quad (\text{A.24})$$

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 ω_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 by:

$$J_t = p_t - w_t + \beta(1 - \delta_t)\mathbb{E}_t J_{t+1}, \quad (\text{A.25})$$

where p_t denotes aggregate labor productivity. There is free entry and, therefore, the expected value of creating a vacancy equals zero:

$$\frac{\kappa}{q_t} = \beta \mathbb{E}_t J_{t+1}, \quad (\text{A.26})$$

where κ 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, $m_t =$

⁹The 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 $\mathbb{E}_t U_{t+1}^E > \mathbb{E}_t U_{t+1}^I$.

$Mv_t^\eta u_t^{1-\eta}$. We denote by η the elasticity of the matching function with respect to vacancies. We define market tightness as $\theta_t = v_t/u_t$. 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 μ the bargaining power of workers. The wage is chosen to maximize the product $S_t^\mu J_t^{1-\mu}$, where J_t in equation (A.25) is a firm's surplus of employing a worker and S_t in equation (A.23) is the surplus that the average unemployed derives from becoming employed. This leads to a standard wage equation:

$$w_t = \mu p_t + (1 - \mu)z_t + \mu\kappa\theta_t. \quad (\text{A.27})$$

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 5.5 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:

$$T_t = \begin{cases} T^1, & \text{if } 0 \leq u_t < \bar{u}^1, \\ T^2, & \text{if } \bar{u}^1 \leq u_t < \bar{u}^2, \\ \dots & \\ T^J, & \text{if } \bar{u}^{J-1} \leq u_t < \bar{u}^J = 1. \end{cases} \quad (\text{A.28})$$

The UI error follows a first-order Markov process $\pi_T(\hat{T}_t | \hat{T}_{t-1}; u_t)$. 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

¹⁰The timing convention in our model follows the convention in the DMP literature in which the unemployment

Equilibrium. The state vector of the economy is given by $\mathbf{x}_t = [u_t, \omega_t, p_t, \delta_t, \hat{T}_t]$. Given exogenous and known processes for p_t , δ_t , and \hat{T}_t , an equilibrium of this model consists of functions of the state vector:

$$\{u_{t+1}(\mathbf{x}_t), \omega_{t+1}(\mathbf{x}_t), \theta_t(\mathbf{x}_t), W_t(\mathbf{x}_t), U_t^E(\mathbf{x}_t), U_t^I(\mathbf{x}_t), w_t(\mathbf{x}_t), J_t(\mathbf{x}_t), b_t(\mathbf{x}_t), T_t(\mathbf{x}_t)\},$$

such that: (i) The law of motion for unemployment (A.17) and the law of motion for eligibility (A.18) are satisfied. (ii) Worker values in equations (A.19), (A.20), and (A.21) are satisfied. (iii) The firm value is given by equation (A.25) and the free-entry condition (A.26) holds. (iv) Wages are determined by equation (A.27), where the opportunity cost of employment is given by equation (A.24). (v) The duration of UI benefits in the absence of measurement error is given by the schedule (A.28). Starting from each state vector \mathbf{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 (A.25) and (A.26), the decision to create a vacancy in the current period depends on the expectation of the present discounted value of firm profits:

$$\frac{\kappa}{q_t(\theta_t)} = \mathbb{E}_t \sum_{j=1}^{\infty} \beta^j \left(\prod_{i=1}^j \frac{(1 - \delta_{t+i-1})}{(1 - \delta_t)} \right) (p_{t+j} - w_{t+j}), \quad (\text{A.29})$$

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. 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.

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 .

Table A.3: Parameter Values

β	ρ	σ	η	μ	δ	ξ	M	γ	B	κ
0.997	0.91	0.008	0.60	0.40	0.035	0.81	0.60	0.72	$\{0.26, 0.10\}$	$\{0.05, 0.17\}$

C.2 Parameterization

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

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

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 A.3 lists values for other parameters of the model. The discount factor equals $\beta = 0.997$. Log productivity follows an AR(1) process $\log p_{t+1} = \rho \log p_t + \sigma \nu_t^p$, with $\nu_t^p \sim N(0, 1)$, where from the data we estimate that at monthly frequency $\rho = 0.91$ 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 , γ , B , and κ , to hit four targets in the steady state of the model with no benefit extensions (so $T^* = 6$ months).¹¹

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

¹¹We target $\theta^T = 1$, $u^T = 0.055$, $\omega^T = 0.65$, 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.

finding in Chodorow-Reich and Karabarbounis (2016) that benefits comprise a small fraction of the average opportunity cost.¹² 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.

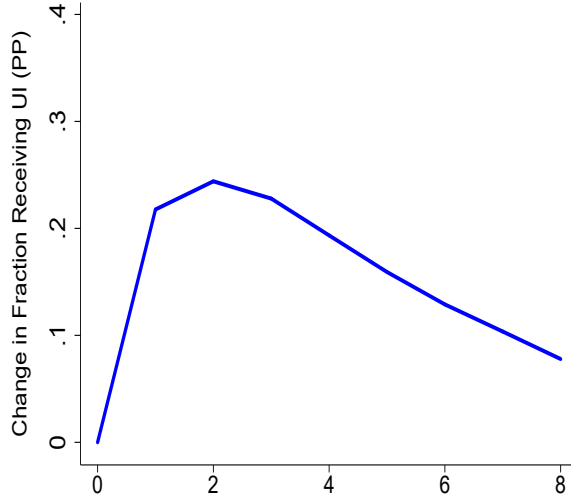
C.3 Computation

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 $f(\cdot)$, $T(\cdot)$, $u'(\cdot)$ and $\omega'(\cdot)$, where primes denote next period values, and use equation (A.27) to obtain the wage function $w(\cdot)$. Next, we iterate on equation (A.25) to solve for firm value $J(\cdot)$. Finally, we use the free-entry condition (A.26) and the definition of the opportunity cost in equation (A.24) to obtain the implied $\theta^1(\cdot)$ and $b^1(\cdot)$ functions. We update the guesses and repeat until convergence. To evaluate value functions at points u' and ω' we use linear interpolation. When solving for the equilibrium policy functions, we impose that the probabilities $f(\cdot)$ and $q(\cdot)$ lie between zero and one. These restrictions also guarantee that v and θ are always positive.

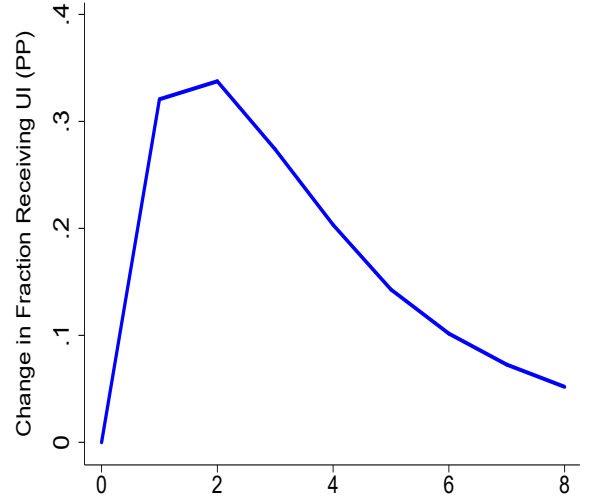
C.4 Additional Results

In Figures A.2, A.3, and A.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 A.5 and A.6 we depict the path of productivity and separations shocks underlying the experiment depicted in Figure 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.

¹²Our calibration is conservative in the sense that reducing the level of ξ 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.



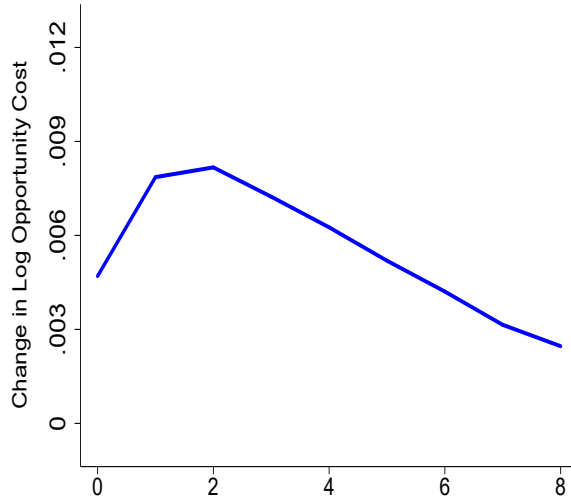
(a) High b Model



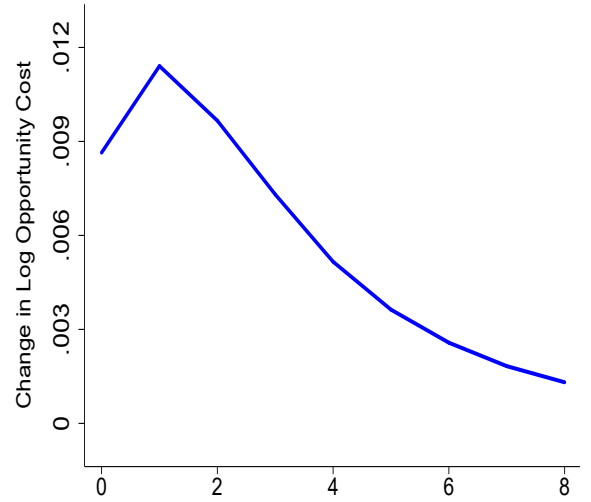
(b) Low b Model

Figure A.2: Impulse Response of Fraction Receiving UI in the Model

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



(a) High b Model



(b) Low b Model

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

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

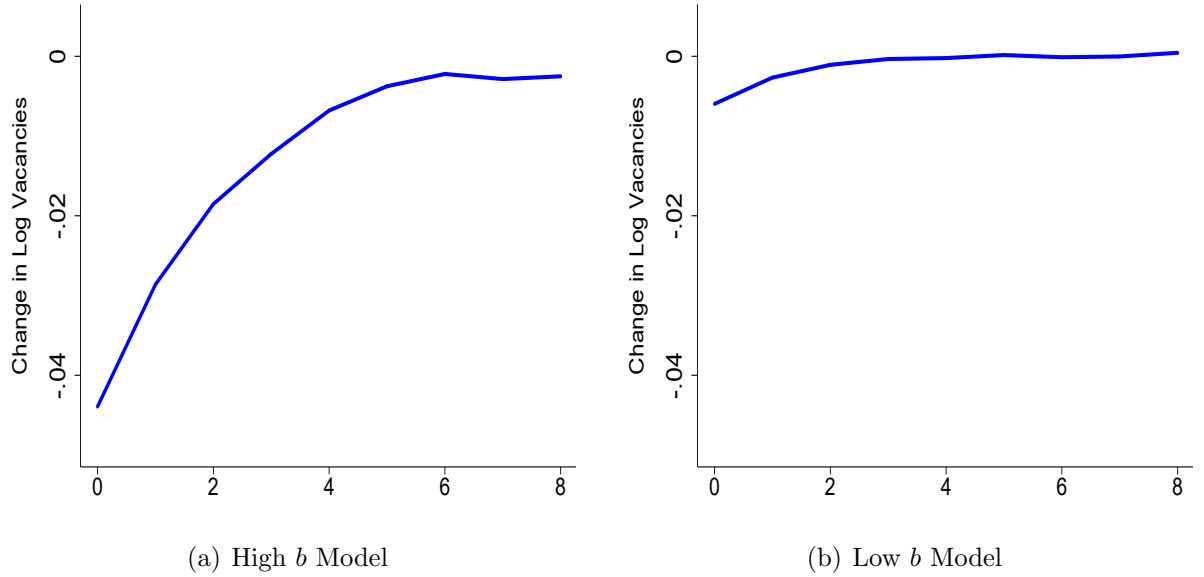


Figure A.4: Impulse Response of Log Vacancies in the Model

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

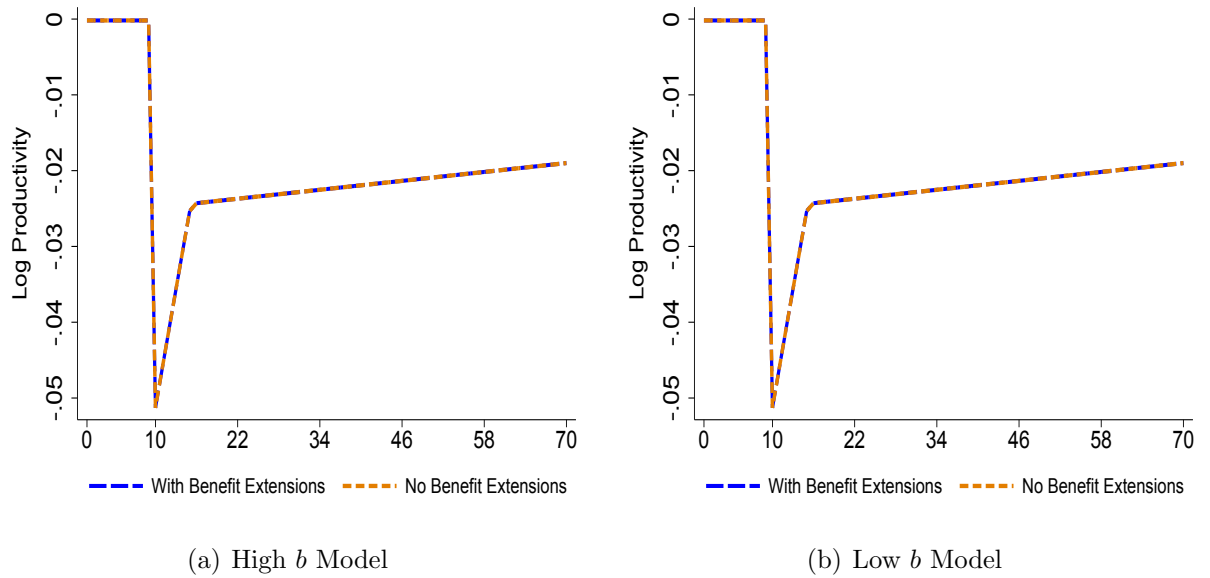
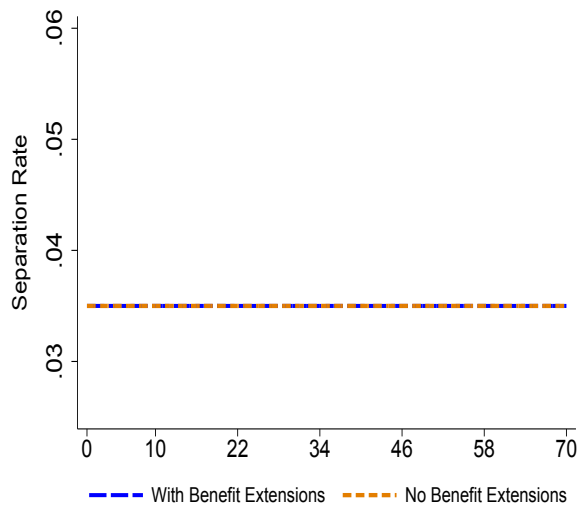
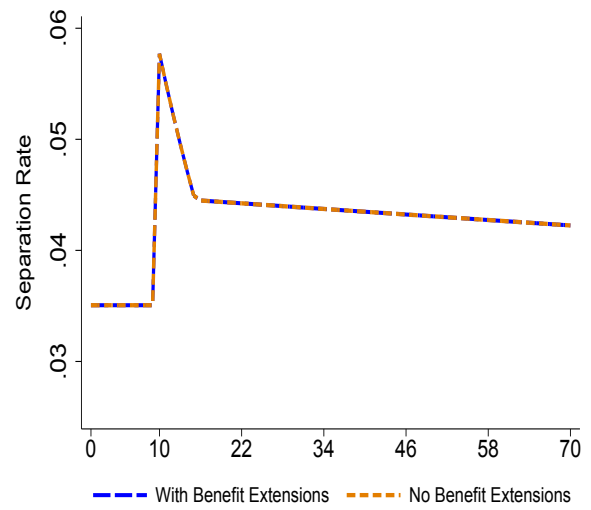


Figure A.5: Productivity Path in the Model

Notes: The figure plots the path of productivity used to generate the simulation in Figure 8.



(a) High b Model



(b) Low b Model

Figure A.6: Separations in the Model

Notes: The figure plots the path of the separation rate used to generate the simulation in Figure 8.