Macroeconomic Effects of UI Extensions at Short and Long Durations

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Abstract

We study the macroeconomic effects of unemployment insurance (UI) benefit extensions in the United States at short and long durations. To do this, we develop a new state level dataset on trigger variables for UI extensions and a "UI Benefits Calculator" based on detailed legislative and administrative sources spanning five decades. Our identification approach exploits variation across states in the options governing the Extended Benefits program. We find that UI extensions during time periods when UI benefit durations are already long—such as in the Great Recession—have minimal effects. However, UI extensions at shorter initial durations have substantial effects on the unemployment rate and the number of people receiving UI, with larger estimates during Covid. We relate our estimates to microeconomic estimates of the effects of UI extensions through the lens of partial and general equilibrium models.

JEL Classification: E2, J6

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1 Introduction

When workers lose their jobs in the United States, many turn to unemployment insurance (UI) to maintain part of their incomes. Regular UI benefits last a maximum of 26 weeks in most states. The Extended Benefits program, introduced in 1970, provides additional weeks of benefits to workers who have exhausted regular UI in recessions. In addition to the original Extended Benefits program, every recession since 1958 has seen the creation of temporary federal UI extension programs.[1] UI extensions "trigger on" during recessions through complex, time-varying rules that take as inputs various measures of unemployment in the recent and more distant past.

Debates rage about whether UI extensions, though intended to help workers, have the potential to prolong periods of high unemployment. The partial equilibrium effect of UI extensions on unemployment has been estimated in a voluminous empirical literature surveyed by Schmieder and von Wachter (2016). A corrected version of their analysis implies that the elasticity of unemployment duration to the potential benefit duration among UI recipients ranges from 0.33 to 0.49 in U.S. studies.[2] These facts have been influential in the large literature on optimal unemployment insurance which emphasizes how UI disincentivizes worker search (Baily, 1978; Chetty, 2006).

General equilibrium forces could potentially weaken or even overturn the partial equilibrium disincentive effects of UI extensions. UI extensions transfer resources to households with a high marginal propensity to consume, and might constitute an important form of fiscal stimulus during recessions (McKay and Reis, 2016; Kekre, 2021). On the other hand, general equilibrium forces could also amplify the contractionary partial equilibrium effects if longer UI reduces the incentives of firms to post vacancies since workers have better outside options and can bargain for higher wages (Hagedorn et al., 2019).[3]

Given that even the sign of the macroeconomic effect of UI extensions on unemployment is ambiguous on theoretical grounds, empirical evidence is particularly vital. Estimating the macroeconomic effects of UI extensions is, however, very challenging. In the U.S., UI extensions are often

[1] These include Federal Supplemental Benefits (FSB, 1975-1978), Federal Supplemental Compensation (FSC, 1982-1985), Extended Unemployment Compensation (EUC, 1991-1994), Temporary Extended Unemployment Compensation (TEUC, 2002-2004), Extended Unemployment Compensation (2008 EUC, 2008-2013), and several Covid era programs (2020-2021) including PEUC (additional weeks of benefits), FPUC (larger payments for all UI recipients), PUA and MEUC (programs for non-traditional workers).

[2] Schmieder and von Wachter (2016) report a lower minimum value of this range of 0.1 based on estimates from Card and Levine (2000). In private correspondence with Johannes Schmieder, we have confirmed an error in the values they report for this paper. Appendix C describes our corrected calculations in detail.

[3] Other general equilibrium effects could also be important. Generous UI policies may encourage firms to institute temporary layoff policies (Feldstein, 1976; Gertler, Huckfeldt, and Trigari, 2022), worsening the partial equilibrium disincentive effects. On the other hand, UI extensions may improve labor market outcomes for those not eligible for UI by reducing labor market congestion (Lalive, Landais, and Zweimüller, 2015).

triggered automatically when unemployment is high and rising. In addition, new UI programs are introduced during recessions. For these reasons, there is a very severe reverse causality problem that must be overcome to estimate the macroeconomic effects of UI extensions on unemployment.

We propose a new approach to estimating the macroeconomic effects of UI benefit extensions based on detailed features of the Extended Benefits program. All states are subject to mandatory "trigger rules" for UI extensions. States may also adopt additional optional trigger rules that lower the threshold to qualify for a UI extension. We compare states that qualify for the same trigger rules but have adopted different trigger rules. Under the assumption that historical option adoption is orthogonal to current economic conditions, this isolates the variation in UI extensions that is *not* due to variation in economic conditions.

Intuitively, two factors play into whether a state receives a UI extension in a given period: 1) the state's economic situation and 2) whether they have adopted "options" that make the UI extension rules more lenient. We isolate the variation due to the latter of these two reasons by controlling for a partition of the state space into "risk sets" based on which options a state satisfies the trigger rules for. This empirical strategy has been used in the education literature to estimate the effect of attending particular schools on student outcomes in cases where multiple lotteries determine assignment of students to schools (Abdulkadiroğlu et al., 2011; Angrist et al., 2022). Our approach also builds on the approach taken by Rothstein (2011) in the UI extension setting.[4]

We find that when baseline potential benefit duration is less than 60 weeks, a standard 13-week extension raises unemployment by 0.28 percentage points.[5] In contrast, when baseline potential duration is already very long, the same extension has virtually no effect on unemployment (our point estimate is -0.03). The 60 week breakpoint for short versus long duration extensions is not important; other breakpoints yield similar results. Our baseline results exclude Covid, but including Covid leads to somewhat larger (but very noisy) point estimates with similar qualitative results. The idea that UI extensions are less consequential at long durations is a direct implication of theories of the behavioral effects of UI: most UI spells are short. So, few individuals remain (or expect to remain) on UI at long durations.

How does this relate to the existing literature? As we note above, a large microeconometric

[4]Rothstein's "third" identification strategy (Table 4 Spec 4-5) isolates EB variation by controlling for EUC weeks, and controls for simulated EB eligibility under maximally and minimally generous options. He also controls for the status of each of the four EB triggers. We also interact the eligibility controls with time dummies to allow for differential effects by time period, e.g. due to aggregate shocks. Rothstein (2011) uses this and several other identification approaches to analyze the effect of UI extensions during the Great Recession.

[5]"Baseline potential benefit duration" is the potential benefit duration that a state would have paid if it had no optional trigger rules in place.

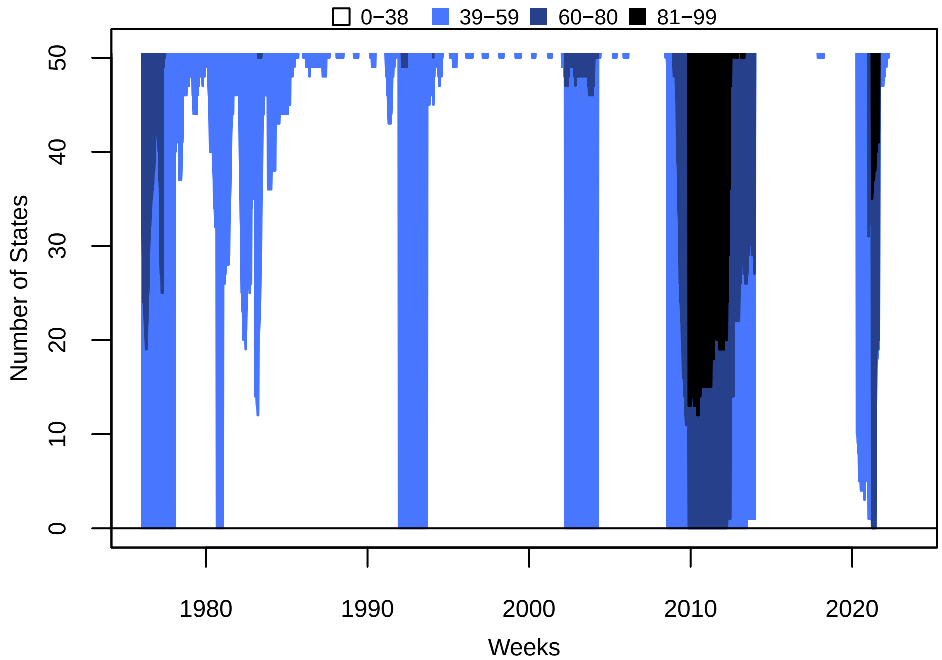


Figure 1: Distribution of Actual Potential Benefit Duration over States and Time

NOTE. This figure plots potential benefit duration for all states over time. States are grouped into bins for potential benefit duration of 0-38 months, 39-59 months, 60-80 months and 81-99 months.

literature surveyed by Schmieder and von Wachter (2016) finds that UI extensions substantially lengthen unemployment spells. In contrast, a number of recent estimates of the macro effects of UI extensions focusing on the Great Recession period cluster around zero (with some important exceptions). In Appendix B.1, we survey the results from these and other studies and convert their estimates into the same units as our results.[6] In section 6, we develop a user-friendly back-of-the-envelope formula to convert between the duration elasticities reported in the microeconomics literature and the unemployment elasticities reported in our paper.[7] Plugging the (corrected) range of micro estimates reported by Schmieder and von Wachter (2016) into this formula leads to macro effects that are roughly consistent with our short-duration estimates.

Our long-duration estimates are consistent with earlier macro estimates from the Great Recession period. While multiple factors are likely at play, we argue that a key factor to consider in interpreting the Great Recession results is the exceptionally long benefits that were already in

[6]Important papers in this literature include Rothstein (2011), Amaral and Ice (2014), Farber and Valletta (2015), Marinescu (2017), Dieterle, Bartalotti, and Brummet (2020), Boone, Dube, Goodman, and Kaplan (2021) and Chodorow-Reich, Coglianese, and Karabarbounis (2019) (who consider the period 1996-2015). In contrast, Johnston and Mas (2018) estimate a much larger effect for a 16 week benefit cut in Missouri in 2011 as do Hagedorn, Karahan, Manovskii, and Mitman (2019).

[7]This formula yields similar results to the numerical simulations of Card and Levine (2000) and Johnston and Mas (2018) in a more transparent manner.

place when the natural experiments leveraged in the literature on the Great Recession took place. The Great Recession stands out as a period when the "base" duration of benefits was particularly long. Figure 1 reports the distribution of potential benefit duration across states over time. Potential benefit duration peaked between 81 and 99 weeks in many states during the Great Recession.[8]

Aside from providing more evidence on extensions at shorter durations, we also offer a new identification approach. This is important since none of the existing empirical designs used to estimate the macroeconomic effects of UI extensions offers a perfect experiment to address the policy question at hand. Aside from the external validity of extensions at long durations, an important limitation of the "border discontinuity" identification approach is sensitivity to labor market spillovers: if workers can travel across borders (the discontinuity) this will confound the results (Dieterle, Bartalotti, and Brummet, 2020).[9] A critique of the revisions shock approach of Chodorow-Reich et al. (2019) is that the shocks are highly transitory, and potentially endogenous. The smoothing and seasonal adjustment algorithms used to construct revised labor market data imply that revisions are partly a function of *future* labor market outcomes. (For example, if time $t + s$ is rosy, then time t is likely to be revised in a favorable direction.) In turn, falling unemployment in the future tends to cause downward revisions in the past (and therefore positive UI shocks due to revisions). This potentially leads the effects of UI extensions to be underestimated. A drawback of both approaches is that they cannot be implemented on pre-1990 data since county-level employment data is only available from the LAUS starting in 1990 and pre-1993 UI triggers depended on variables that are not revised substantially. Recent work has analyzed the consequences of UI extensions to non-traditional workers during the Covid recession and also UI supplements, but has not estimated the macroeconomic effects of more conventional UI extensions.[10]

Our analysis also relies on important assumptions, namely that the timing of option adoption is not endogenous to turning points in local unemployment during our sample. We analyze whether states exhibit unusual trends in local unemployment prior to option adoption and find no evidence of this. Most changes in UI option adoption during our sample period arose from the creation of new options and from nationwide changes in federal government funding, i.e., states

[8]Beyond the exceptionally long duration of UI benefits during the Great Recession, Kroft and Notowidigdo (2016) show that the severity of the recession itself also played a role in lessening the effects of generous UI benefits on unemployment. Relatedly, Huckfeldt (2023) emphasizes the potential for crowding out of active search during recessions.

[9]In addition, Boone et al. (2021) and Amaral and Ice (2014) show that the large estimates of Hagedorn et al. (2019) are sensitive to the specification, data source, and time period used.

[10]See Holzer, Hubbard, and Strain (2021); Coombs, Dube, Jahnke, Kluender, Naidu, and Stepner (2022); Ganong, Greig, Noel, Sullivan, and Vavra (2023); Bartik, Bertrand, Lin, Rothstein, and Unrath (2020); Marinescu, Skandalis, and Zhao (2021); Finamor and Scott (2021); Petrosky-Nadeau and Valletta (2023).

adopting options when federal funding for the Extended Benefits program was increased to 100% ("free" UI from the perspective of state budgets). We explore these issues in detail in the paper.

To interpret our results, we present (in addition to the back-of-the-envelope calculation discussed above) a general equilibrium search-and-matching model. While the theoretical ingredients are standard, we are careful to include a number of detailed features that are important in thinking seriously about the relationship between the micro and macro effects of UI extensions. The model incorporates endogenous search effort and a limited duration of UI benefits. It also includes UI takeup costs, which explain why many people who are eligible for UI benefits never take them up. In line with our back-of-the-envelope calculation, we find that the calibration that best matches our empirical findings implies small general equilibrium effects of UI extensions.

Implementing our empirical approach required a substantial data construction effort, that we hope will pay dividends for future researchers. There is no existing historical source for when and why states trigger for UI extensions. Constructing this data required several steps. First, we coded up state-level UI extension rules. We then developed a "UI Benefits Calculator" that accurately predicts, for all states going back to 1976, whether a state would receive any federal UI extension (e.g., Extended Benefits) as a function of state-level trigger variables and the state's option status. The UI Benefits Calculator is our codification of state UI legislation, which we recovered in narrative form from primary sources (legislative records).

Beyond the trigger rules, it is also essential to know the *inputs* into these rules. These are not available in standard datasets. While it is straightforward to download data on, for example, state-level unemployment, the same datasets do not provide the real-time data actually used to determine UI extensions. Since data revisions are large, it is essential to obtain real-time data on the full-set of "trigger variables" that enter into the UI extension rules. We collected and digitized information on state-level trigger variables, trigger status, and option status going back to 1976. This dataset draws on the archives and library of the Department of Labor, the Library of Congress, university libraries, online archives of the Federal Register, and news reports.

The paper proceeds as follows. Section 2 describes the rules that govern the duration of UI benefits, and what causes them to change. Section 3 describes the data construction we undertook for this project. Section 4 describes our empirical methodology. Section 5 presents our main results. Section 6 interprets our results through the lens of a theoretical model and puts them in the context of the existing literature on the micro and macro effects of UI extensions. Section 7 concludes.

2 Trigger Rules for UI Extensions

The rules governing potential benefit duration (i.e., the maximum available duration) of unemployment insurance (UI) in the United States are complex and have changed frequently over the past 50 years. Furthermore, some of these rules differ from state to state. Our identification strategy leverages these differences across states. A crucial aspect of our identification strategy is our ability to calculate potential benefit duration in one state using the rules in place in another state—i.e., counterfactual potential benefit duration. Here we start by giving an overview of these complex rules. We focus on the rules that differ from state to state.

2.1 The Extended Benefits Program

Most states offer 26 weeks of regular UI benefits. The Extended Benefits (EB) program is a federal program that has since 1970 provided additional weeks of UI when certain conditions are met. Some of these conditions (typically referred to as "trigger rules") of the EB program are mandatory—i.e., all states must adopt these conditions—while other conditions are optional. It is these optional rules of the EB program that yield the variation in potential benefit duration across states that we will exploit in our analysis. In addition to the EB program, Congress has passed laws that have temporarily extended UI during each recession since 1958 (U.S. Department of Labor, 2018). All of these recession-specific federal programs have been mandatory. They therefore do not yield cross-state variation of the type we exploit in our analysis. They do, however, create variation in the "base" potential benefit duration over time. This variation plays an important role in our heterogeneity analysis. Table A.1 in the appendix lists the recession-specific programs and all of their trigger rules.

Table 1 lists all of the trigger rules for UI extensions under the EB program. At any given point in time, there has been one mandatory trigger rule in place. Since 1982, this mandatory rule has been a 13 week extension if two conditions hold: 1) a 13-week moving average of the insured unemployment rate (IUR) in the state is above 5%; and 2) the current 13-week moving average of the IUR is larger than 120% of the average of the 13-week moving average of the IUR one and two years prior.[11] This second condition is known as a lookback provision.

[11] This rule is triggered if $\text{IUR}_{\text{MA}(t)}/[(\text{IUR}_{\text{MA}(t-52)} + \text{IUR}_{\text{MA}(t-104)})/2] > 1.2$ where $\text{IUR}_{\text{MA}(t)}$ is the 13-week moving average of the IUR in week t. With a few exceptions, the trigger rules for other programs typically rely on the same trigger variables used for the EB program (but with different thresholds). One such exception is that the early 1990s EUC program used an "adjusted IUR," which added recent UI exhaustees to the numerator of the IUR, to account for the fact that the IUR becomes mechanically lower as people exhaust benefits.

Table 1: Trigger Rules for the Extended Benefits Program

Rule Type	Rule Description	Effective Years
	13 Weeks	
Mandatory	IUR MA $\geq$ 4% and IUR Lookback $\geq$ 120%	1970–1971, 1981–1982
Mandatory	(IUR MA $\geq$ 4% and IUR Lookback $\geq$ 120%) or National IUR $\geq$ 4.5%	1972–1981
Optional	IUR MA $\geq$ 5%	1976–1982
Mandatory	IUR MA $\geq$ 5% and IUR Lookback $\geq$ 120%	1982–
Optional	IUR MA $\geq$ 6%	1982–
Optional	IUR MA $\geq$ 5% and 3-year IUR Lookback $\geq$ 120%	2011-2013
Optional	TUR MA $\geq$ 6.5% and 1- or 2–year TUR Lookback $\geq$ 110%	1993–
Optional	TUR MA $\geq$ 6.5% and 1-, 2-, or 3-year TUR Lookback $\geq$ 110%	2011-2013
	7 Additional Weeks	
Optional	TUR MA $\geq$ 8.0% and 1- or 2–year TUR Lookback $\geq$ 110%	1993–
Optional	TUR MA $\geq$ 8.0% and 1-, 2-, or 3-year TUR Lookback $\geq$ 110%	2011-2013
	Interactions with other Programs	
Optional*	FSB only triggered if EB triggered	1975–1978
Optional*	States that recently triggered EB also triggered FSC early on	1982–1983
Optional*	Triggering EB also triggered TEUC benefits	2002–2004
Optional*	EUC Tier IV offered more weeks when EB not triggered	2012

NOTE. This table summarizes all trigger rules for the Extended Benefits (EB) program. All states must adopt the mandatory rules. States are free to choose whether they adopt the optional rules. IUR is the insured unemployment rate. TUR is the total unemployment rate. MA denotes a 13-week or 3-month moving-average for the IUR and TUR, respectively. The rows with "optional*" rule types refer to other federal programs that had trigger rules tied to a state's EB trigger status (this generates cross-state variation because of the optional EB trigger rules). See Table A.1 in the appendix for a full description of the rules for federal programs.

The logic of these two conditions is that the IUR should be high and rising for the EB program to trigger on. Importantly, as a general matter, once an EB condition triggers on, the resulting UI extension remains active for at least 13 weeks. If a trigger rule continues to be satisfied for longer than 13 weeks, the UI extension remains on until all triggers lapse. Symmetrically, once all EB conditions lapse, UI extensions must remain off for at least 13 weeks (and stay off until an EB condition triggers on again). We refer to the 13-week minima discussed in this paragraph as the "13-week rule."[12]

In addition to the mandatory EB rule, states can adopt several optional trigger rules. Three such rules are in effect as of this writing. The first of these triggers a 13-week extension to UI if the 13-week moving average of the state IUR is above 6%. This trigger rule is known as the "IUR option." The second optional rule triggers a 13-week extension to UI if the 3-month moving average of the state total unemployment rate (TUR) is above 6.5% and this rate is higher than 110% of the 3-month moving average of the state TUR 1- or 2-years prior. This trigger rule is known as the "TUR option." Importantly, the extensions from these rules (the mandatory rule, the IUR option, and the TUR option) do not cumulate. So, the effect of adopting the IUR and TUR

[12] In practice, we find that variation in potential benefit duration arising from the 13-week rule contributes negligibly to our results, despite its plausibility as an exogenous source of UI extensions.

options is to make a 13-week extension more likely during a downturn (not to get an additional 13-weeks). The TUR option has a second tier that triggers 7 additional weeks (on top of the 13 weeks already triggered) if the TUR is above 8% and this rate is higher than 110% of the 3-month moving average of the state TUR 1- or 2-years prior. As Table 1 details, other optional trigger rules have been in place at various points in time, usually during downturns.[13]

The IUR is a rather limited metric of the extent of pain in the labor market. It only includes those collecting UI. And since 1982, it excludes those collecting UI through the EB program or other federal extensions. This is one reason why the TUR option was added to the EB program in 1993. Finally, it is important to clarify that potential benefit duration measures the maximum number of weeks for which an individual may be eligible to receive UI benefits. The exact number of weeks for which an unemployed individual is eligible depends on the distribution of their earnings over several quarters, and can be less than this maximum amount.

2.2 What Causes Options to Change?

Arguably the most important threat to identification in our analysis is that states may choose to implement optional (i.e., more lenient) trigger thresholds for the EB program because they have received some bad news about future local labor market outcomes. Perhaps states select into adopting options in a way that might lead to endogeneity bias. Perhaps states experiencing worse economic conditions are more likely to opt in. We evaluate this concern in section 4.5. We find no evidence that states experiencing worse economic conditions (in terms of unemployment) are more likely to adopt options that make UI more generous.

In this section, we help provide intuition for this finding by analyzing what causes options to change. Figure 2 plots the number of states with each option in place at a given point in time. We see that the IUR option has had a fairly stable take-up of between 36 and 39 states since its creation in November, 1976. Take-up of the TUR option, created in 1993, was much lower initially: 7 states at the end of the year it was created, rising to 11 states in 2007. Take-up of the TUR option increased dramatically in 2009, decreased dramatically in early 2014, increased again during the Covid-19 crisis, and then fell back after Covid. These swings coincided with times when the EB program became fully federally funded.

We have manually examined each option switch over the period 1981-2022 and categorized

[13] We ignore the 3-year IUR lookback (which added a third year to the averaging period for the typical IUR lookback discussed above) in our analysis because, in practice, it is completely irrelevant for our identifying variation. During its existence, EB statuses for all states are invariant to adding or removing the option.

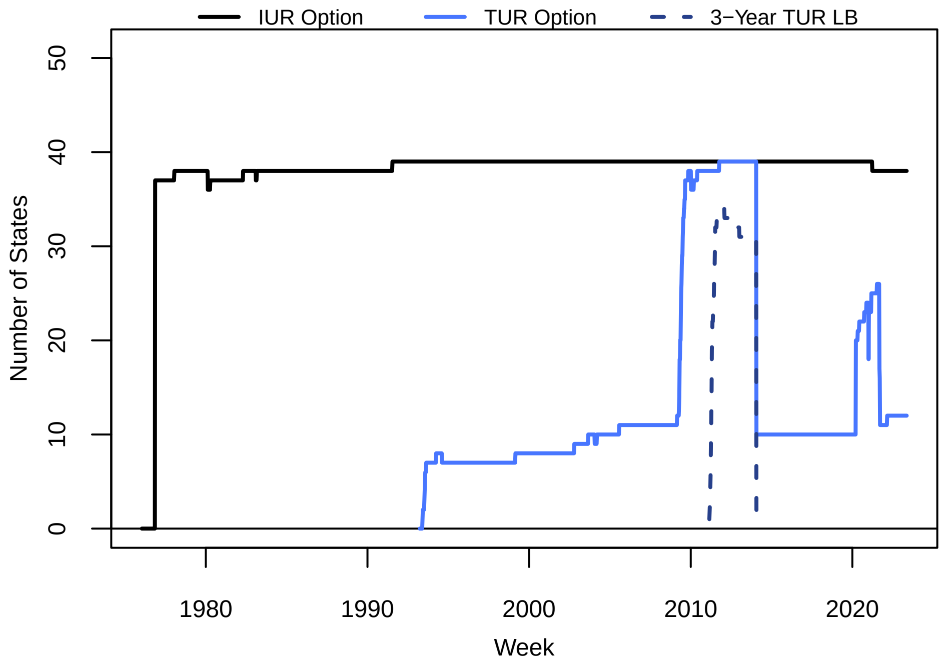


Figure 2: Changes in Option Status

NOTE. This figure shows the number of states in each week that have adopted each of the three options listed. We impute the values of these options during the 3 lapses in federal funding for the EUC and EB programs in 2010 and 2012 to their pre-lapse values. We use these imputed option values to compute potential benefit duration during those lapses.

each one according to what we understand to be the dominant policy motive behind it. Table A.2 in the appendix presents the results of this analysis. States in our sample implemented changes in trigger rules 202 times in total. The vast majority of these changes relate to the TUR option and the "TUR 3-year lookback" which was available between 2011 and 2013 only.

Most option switches (85%) occur as a response to changes in the availability of federal funding. In every recession since 2000, Congress has—directly or indirectly—increased the share of EB program costs borne by the federal government. For example, during the Great Recession and the Covid-19 recession, federal funding for the EB program was increased to 100%—making it "costless" to state budgets to adopt the IUR and TUR options (the EB program is typically 50% federally funded). This led a large number of states to temporarily adopt the TUR option ("free" UI) while the temporary federal provisions were in place. Some states introduced legislative clauses that mechanically tied adoption of the TUR option to 100% federal funding.[14]

[14]Some states tied adoption to the presence of full federal funding under the American Reinvestment and Recovery Act or Families First Act specifically, while other states tied adoption to full federal funding, regardless of the specific legislation. This explains the asymmetric rise in the TUR option between the Great Recession and Covid recession. Similar clauses were adopted when the 3-year lookback option was adopted in 2011–2013. In principle, we could associate these cases with "option creation," since the option was created in 2011. However, the motive of these states in adopting the option was the availability of federal funding. Additionally, in the aftermath of the 2001 recession, the

There are two other federal legislative sources of options switches. The first relates to the "Reagan reforms" in the early 1980s. President Reagan's 1981 Omnibus Budget Reconciliation Act made several changes to federal UI that effectively made it more difficult for states to qualify for the EB program.[15] Between the passage of the Act in August 1981 and its full implementation in October 1982, seven states reacted to the change in federal legislation by either dropping or adopting the IUR option. We classify one of these cases (West Virginia) as discretionary because the specific timing may have been motivated by a state-level economic downturn.[16] Table A.2, thus, lists 6 options switches due to the Reagan Reforms, which accounted for all but two of the IUR trigger switches that have occurred in the 40 years since 1982. A final legislative source of options switches is the creation of the TUR option in 1993. Seven states adopted this option shortly after it was introduced. We list these under "Option Creation" in Table A.2.

We label all the remaining options changes—which are not obviously a response to federal legislative changes—as "discretionary." Some of these changes occurred for ideological reasons.[17] We also label as discretionary cases where option changes were plausibly linked to federal legislation, but the timing was far enough removed to leave some doubt. We present a robustness exercise later in the paper where we drop 2 years before and after the discretionary changes and find this makes little difference for our results.

Why do some states adopt the IUR and TUR options when they reflect "free money" from the federal government while others do not? As we describe above, we show in section 4.5 that this does not seem to be related to local economic conditions. If one thought that states would adopt options when they anticipated a bad recession, then one would expect the estimated effects to be smaller dropping switches. In fact, we find the opposite—see appendix Table A.5, panel (r).

The most natural interpretation is that state governments have differing views about the costs and benefits of UI extensions: some states believe that UI extensions hurt the economy, while others believe that extensions not only improve the welfare of the unemployed but also provide

TEUC program added two tiers of 13 week UI extensions between 2002 and 2004, which were 100% federally funded. The first of these tiers had no qualification threshold. States had to, however, qualify for the second tier. One way to qualify for this second tier was to qualify for the EB program. This provided states with an incentive to adopt the TUR option in the EB program to increase their chances of qualifying the the second tier of TEUC.

[15]The Act changed the definition of the IUR to exclude EB recipients in the numerator, raised the IUR moving-average thresholds for EB by a percentage point, and terminated the national EB trigger.

[16]Of the 7 states that adopted the IUR option when it was introduced, only West Virginia did not make this change in October 1982. West Virginia's adoption of this option in April 1982 allowed it to immediately start paying EB.

[17]For example, Washington, switched its IUR option a few times in the early 1980s, in concert with shifts in the political party in control of the state legislature. In 1994, Maine passed a law that temporarily adopted the TUR option for only a six week period explicitly to qualify for EB benefits, since benefits under the federal Extended Unemployment Compensation legislation were about to expire.

economic stimulus. The decision not to adopt is correlated with political attitudes. Using data from the MIT Election Data and Science Lab (2017), we find that 10 of the 12 states that did not adopt the TUR option in 2012 had below-median Obama vote shares in the 2008 election. In ascending order of Obama vote share, the states that did not adopt the TUR option in 2012 were Wyoming, Oklahoma, Utah, Arkansas, Louisiana, Nebraska, Mississippi, North Dakota, South Dakota, Montana, Iowa, and Hawaii.

3 Data

Our empirical methodology builds on two major data collection efforts: 1) the development of a "UI Benefits Calculator" that accurately predicts whether a state triggers onto federal UI extensions as a function of state trigger variables and the EB program options it has in place, for all US states and weeks back to 1976; and 2) the development of a dataset on real-time values of the "trigger variables" that enter into the trigger rules of these federal programs.

3.1 UI Benefits Calculator

Our UI Benefits Calculator consists of code that predicts whether a state triggers onto federal UI extensions given data on the underlying state-level trigger variables and the EB program options the state has adopted. Importantly, the UI Benefits Calculator is able to calculate counterfactual potential benefit durations for each state had the state made a different choice regarding EB program options. The construction of the Calculator involved a detailed case-by-case analysis of all federal and state UI legislation since 1976. The starting point of this analysis was the *Chronology of Federal Unemployment Compensation Laws*, published by the U.S. Department of Labor (2018). We also made use of the text of the relevant federal and state legislation, news reports, notices in the Federal Register, memoranda written by the Department of Labor to state UI agencies, and we corresponded with members of the Division of Fiscal & Actuarial Services at the Department of Labor (the division that is responsible for publishing Trigger Notices). We provide a detailed description of the rules in Appendix A.1.

Table 2 demonstrates the accuracy of the UI Benefits Calculator. Given real-time measures of trigger variables and option adoptions, the calculator returns an accurate trigger status for well over 99% of state-weeks since 1976. To achieve this level of accuracy, we manually examined every discrepancy between the predicted and actual potential benefit durations in earlier versions

Table 2: UI Benefits Calculator Performance

Program	Applicable State-Weeks	Incorrect	% Correct
EB (1976–2022)	125,970	52	99.96
FSB (1976–1978)	5,457	112	97.95
FSC (1982–1985)	6,732	14	99.79
EUC (1991–1994)	5,814	0	100.00
TEUC (2002–2004)	5,508	0	100.00
EUC (2008–2013)	14,535	40	99.72
Total	164,016	218	99.87

NOTE. This table shows the performance of our UI benefits calculator, broken down by federal program (first column). The column "applicable state-weeks" displays the number of state-weeks for which the program was in place. The column "incorrect" shows the number of state weeks for which, given trigger variables, the calculator returns the incorrect benefits status. The final column shows the latter as a percent of the former.

of our calculator. In many cases, we found that the Trigger Notices were mistaken—e.g., the state in question did pay EB while the Trigger Notice indicated it did not—and we corrected our data accordingly.[18] In the remaining cases, we believe that states indeed paid "incorrect" benefits—typically because they started paying benefits slightly too early or too late. The duration of these "mistakes" is very short (with a median of 2 weeks)—perhaps their existence is not surprising given the complexity of the underlying rules. We do not correct these in our data, but drop them from our analysis.

3.2 Trigger Notices

The second major data collection effort we undertook as a part of this project consisted of gathering real-time data on the trigger variables and option statuses that the UI Benefits Calculator takes as inputs. Since there was no existing machine-readable source for this data for much of our sample period, we collected and digitized real-time information on state-level trigger variables and option statuses going back to 1976. These data include trigger variables for *all* potential determinants for all federal UI extension programs at a given point in time (not just the EB program). We obtain this information from the Department of Labor's (DOL's) weekly Trigger Notices. The DOL

[18] In most cases, we can determine the correct trigger status for a state in a given week by referencing the trigger sheet from a later week, since trigger sheets report when the most-recent trigger episode began or ended. In a small number of cases, this procedure cannot be used to determine the correct trigger status for a state in a given week—for example, when there are multiple tiers of EB, the trigger sheet lists the date when the current tier first became active, but this does not provide information about when all previous tiers triggered/expired. In these cases, we have supplemented the information from the trigger notices with other sources such as the Federal Register (the daily journal of the U.S. government, available from federalregister.gov). Chodorow-Reich et al. (2019) also report finding mistakes in UI trigger sheets using their benefit calculator over their 1996–2015 sample.

publishes Trigger Notices for the EB program and each supplemental federal extension program that has trigger rules.[19] It also indicates whether each state has triggered on each program—this information underlies the performance test of our calculator in Table 2. In fact, this is the only source of data on state-level trigger status and, thus, potential benefit duration.

We were able to scrape Trigger Notices for the period since October 2002 from the website of the Department of Labor (DOL).[20] For the period 1976-2002, we hand-collected Trigger Notices from archives of the DOL's *UI Weekly Claims Report* and digitized these into machine-readable form. The bulk of our archives are from the Wirtz Labor Library at the DOL, though we supplemented this material—which had many missing weeks—with archives from other sources: the archives of the Division of Fiscal & Actuarial Services at DOL, the Library of Congress, several university libraries through the Hathi Trust Digital Library, and the University of Texas Libraries.[21]

There is no Trigger Notice analog for regular state UI: these benefits (and corresponding trigger rules/variables) are not systematically reported by any source. Between 2000 and 2017, we have monthly data on regular state UI from Farber and Valletta (2015), generously provided to us by Rob Valletta. For the rest of the 1976–2022 period, we hand-gathered and digitized the maximum UI benefit duration for regular state UI from the DOL's publication *Archived Significant Provisions of State UI Laws*, which is updated every six months.[22] For months between publication we linearly interpolate the maximum UI benefit duration.[23]

3.3 LAUS, CPS, CES, and Administrative UI Data

We merge the new data sources described above with a variety of existing data sources on employment and unemployment outcomes.[24] We use the Local Area Unemployment Statistics (LAUS) from the Bureau of Labor Statistics (BLS) to measure unemployment, the labor force, and

[19]The federal UI extension programs enacted during the Covid recession provided extra weeks of benefits that were not tied to state labor market conditions. These programs thus did not have trigger variables and Trigger Notices.

[20]The Trigger Notices are available at https://oui.doleta.gov/unemploy/claims_arch.asp.

[21]We were unable to find data for 34 weeks over the period 1976–2002: 12 of these were never printed because of government shutdowns (4) or program lapses (8); 13 come from September–December 1981; and the final nine are from other sporadic weeks. For all but the 1981 episode we are able to impute missing values using real-time data that we gather from various sources. Until early 1980, Trigger Notices did not report the status of optional UI legislation, so we inferred these using trigger status and the values of trigger variables (and supplemented these where possible with Federal Register notices and news reports).

[22]Available from https://oui.doleta.gov/unemploy/statelaws.asp.

[23]Some states have PBDs that depend on state labor market outcomes. The *Archived Significant Provisions* do not consistently provide a description of these rules—instead, they list a range (in weeks) for maximum UI benefit duration (we take the maximum value).

[24]Potential benefit duration, and all trigger sheet variables, are reported weekly, while most of our outcome variables are measured monthly. We merge outcome data from month m with all weeks whose end (Saturday) falls in month m.

population. We also constructed a new, real-time version of this dataset by scraping the BLS's online news-release archive (back to 1994) and by digitizing the corresponding print publication,"Employment and Earnings" (back to 1976). We construct and analyze an alternative measure of unemployment from Current Population Survey (CPS) data. We use employment data from the BLS' Current Employment Statistics (CES) program—i.e., the BLS' establishment survey.

We also merge our new data with UI administrative records on payouts and the number of people on UI. We obtain data on UI payments, the number of initial claims, and the number of UI recipients by state from DOL administrative data (report ETA 5159).[25] Appendix A.3 describes the exact construction of the variables used in our analysis. We deflate the dollar amount of UI benefits using the national PCE deflator from the Bureau of Economic Analysis.

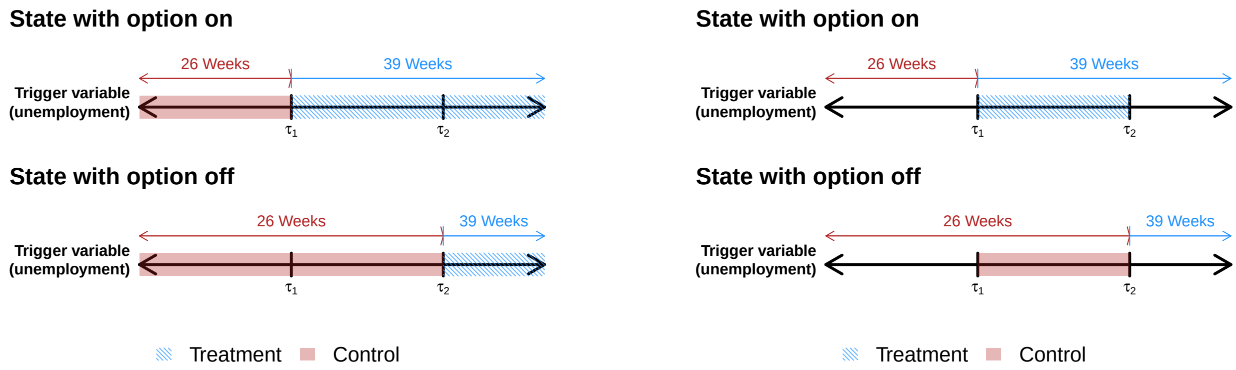
4 Empirical Methodology

Our objective is to identify the effects of UI extensions on labor market outcomes, such as the unemployment rate. The primary challenge when doing this is the fact that it is changes in the unemployment rate that cause a state to extend UI benefits. This introduces a severe reverse causality problem. Our approach to overcoming this reverse causality problem is most easily explained through a simple example.

Figure 3 depicts a scenario where the trigger rules for UI extensions are functions only of the unemployment rate in a prior month. There are two trigger thresholds: τ_1 and τ_2. The higher threshold, τ_2, is a mandatory threshold: whenever a state's unemployment rate surpasses this level, the state must offer 13 weeks of additional benefits (on top of the 26 weeks of regular benefits). The lower threshold, τ_1, is an optional threshold: some states have adopted this trigger and offer 13 weeks of additional benefits when their unemployment rate is above τ_1, while other states have not adopted this trigger. In the figure, the black horizontal lines denote the unemployment rate. Above these lines, we indicate the number of weeks of benefits available as a function of the unemployment rate in a state with the optional threshold in place (top), and in a state without the optional threshold (bottom).

Panel (a) highlights the reverse causality problem. Consider the naive approach of simply averaging the unemployment rate among states offering 39 weeks of benefits (shaded in broken-

[25]These data are available for regular state UI programs, the EB program, and all special federally-financed programs beginning with the 1991–1994 EUC program. Data for the 1975–1979 (FSB) and 1982–1985 (FSC) programs are unavailable from this source.



(a) No Controls

(b) With Controls

Figure 3: Illustration of Identification

NOTE. This figure illustrates our approach to identifying the effects of UI extensions. In the figure, a state extends benefits from 26 weeks to 39 weeks if its unemployment rate is above τ_2 (the mandatory threshold), or if its unemployment rate is above τ_1 (the optional threshold) and it has elected to have that optional threshold in place ("on").

blue) and among states offering 26 weeks of benefits (shaded in solid red). Even if the true effect of UI extentions on unemployment is zero, the "effect" estimated by taking the difference between these two averages would be positive. The problem is that a higher unemployment rate causes states to have higher levels of UI benefits.

Our solution to this is to focus on variation in UI benefit levels that arises because states have made different choices regarding the optional trigger τ_1. Panel (b) of Figure 3 illustrates this approach. Notice that the two triggers, τ_1 and τ_2, partition the unemployment rate into three intervals: $[0, \tau_1]$, $[\tau_1, \tau_2]$, and $[\tau_2, \infty]$. The key idea is to include separate fixed effects in our regression specification for each of these three intervals.

When these fixed effects are included, the coefficient on weeks of benefits is identified off of variation within these intervals. Importantly, there is no variation in weeks of benefits across states in the intervals $[0, \tau_1]$ and $[\tau_2, \infty]$. The coefficient on weeks of benefits is therefore identified only off of variation in the middle interval $[\tau_1, \tau_2]$. The variation in weeks of benefits within this interval is not due to differences in the unemployment rate. It is *only* due to the choice of whether the state adopted the optional trigger. Any differences in labor market outcomes can thus be attributed to the difference in the level of UI benefits, rather than the difference in underlying labor-market conditions.[26]

The example depicted in Figure 3 makes clear why our empirical approach depends on us having data on real-time trigger variables and the UI Benefits Calculator. The fixed effects needed

[26]We discuss identification in this simple illustrative example in more detail in Appendix A.6.

to implement our approach require that we know the values of the trigger variables in each state at each point in time. In the example, the only trigger variable is the unemployment rate. In reality, there are more trigger variables and they are more complex as we discussed in section 2. Our full approach includes fixed effects for a full partition of the state space by qualifying status for each option (i.e., the IUR option and each flavor of the TUR option) interacted with time fixed effects. To include these fixed effects, we need to have real-time data on all trigger variables and the UI Benefits Calculator to determine qualifying status.

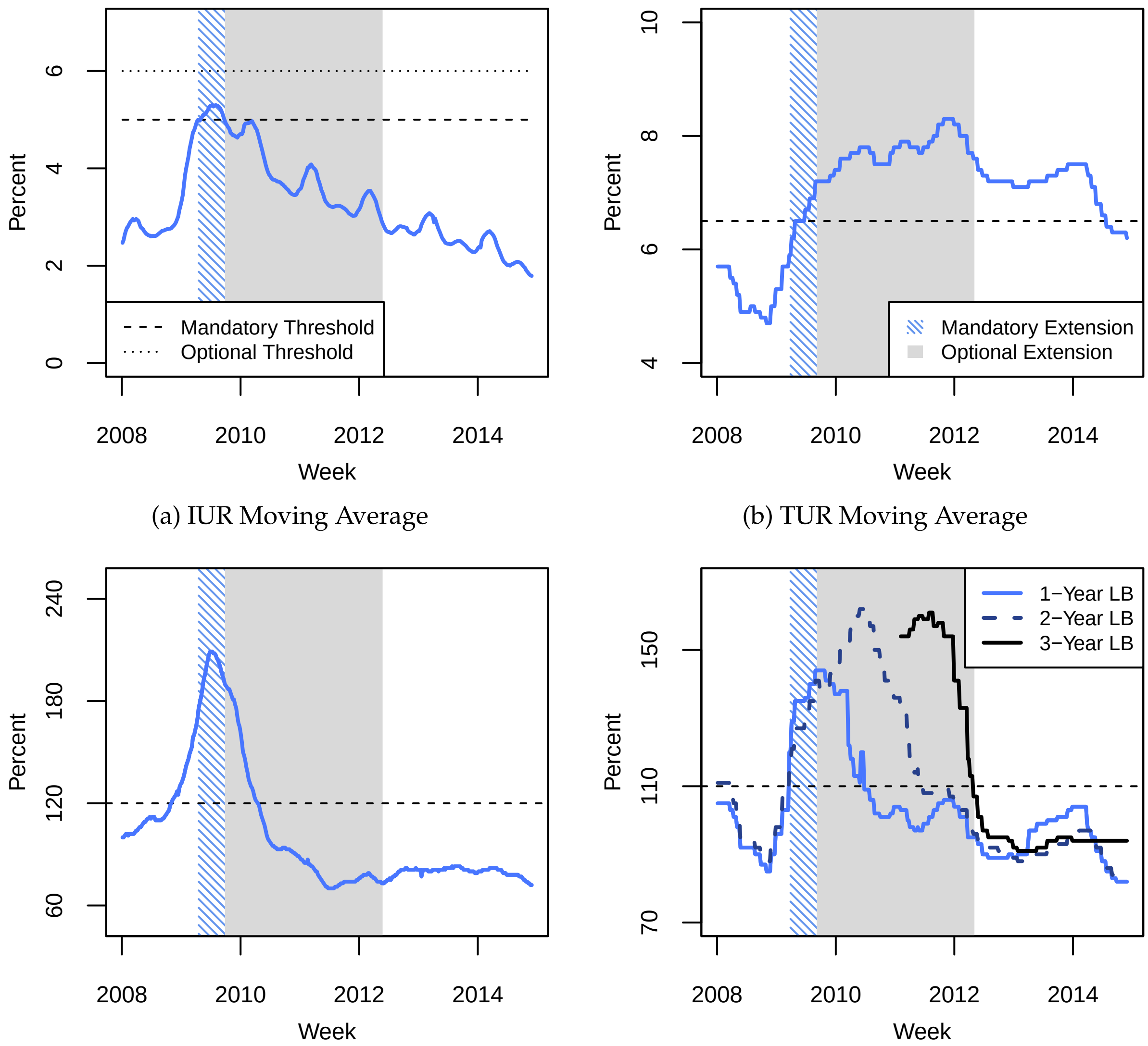
We are implicitly making the identifying assumption that option adoption is exogenous to current labor market conditions. Given this assumption, the reverse causality in the example can be fully controlled for because it arises *only* from different states qualifying for UI extensions through different known options. By including the fixed effects discussed above, we focus exclusively on the part of the variation in UI extensions that is plausibly exogenous, i.e., due to option adoption. The main threat to identification is endogeneity of option adoption. We discussed in section 2 what drives option adoption, and demonstrate that the changes are typically motivated by federal policy. In section section 4.5, we provide direct empirical evidence that states do not have unusual unemployment trends leading up to option adoption.

4.1 An Example: Arkansas

Let us now consider a real-world example of the identification approach described above: the case of Arkansas. Figure 4 plots time series for the relevant trigger variables for the EB program in Arkansas during the Great Recession and the recovery that followed. These variables and Arkansas' decisions regarding which optional trigger rules to adopt fully determine the potential benefit duration available to the unemployed in Arkansas under the EB program.

Panel (a) plots the 13-week moving average for the state IUR, while panel (c) plots the state IUR lookback ratio. When both of these trigger variables are above the broken horizontal line in these panels, Arkansas satisfies the mandatory trigger rule. This occurred between April and September 2009. We have shaded this time period in broken-blue in the figure. Had the state IUR in Arkansas risen above 6%, it would have also satisfied the IUR option. This never happens during this period. The IUR option is therefore irrelevant in this example.

Panel (b) plots the 3-month moving average for the state TUR, while panel (d) plots three different state TUR lookback ratios. During this period, states that had adopted both TUR options available at that time triggered if any one of these three lookback ratios was above the broken hor-



(a) IUR Moving Average

(b) TUR Moving Average

(c) IUR Lookback

(d) TUR Lookback

Figure 4: Trigger Variables in Arkansas during and after the Great Recession

NOTE. This figure shows the trigger variables for the EB program in Arkansas (solid lines), along with their thresholds (dashed horizontal lines). Panel (a) shows the 13-week moving-average of the IUR ($\bar{i}_w$ in week w), along with its mandatory 5% threshold. Panel (c) show the IUR "lookback" variable, defined as $100 \times \bar{i}_w / \left(\frac{\bar{i}_{w-52}+\bar{i}_{w-104}}{2}\right)$, along with its mandatory 120% threshold. Panel (b) shows the 3-month moving-average of the unemployment rate ($\bar{u}_m$ in month m), along with its optional 6.5% threshold. Finally, panel (d) shows the TUR lookback variables, given by $100 \times u_m/u_{m-12}$, $100 \times u_m/u_{m-24}$, or $100 \times u_m/u_{m-36}$. The third lookback was only a trigger variable over the period 2011–2013. The broken-blue-shaded regions show the period during which EB was paid in Arkansas. The solid-gray-shaded region shows the period when Arkansas surpassed the optional TUR thresholds and, thus, would have paid EB had it implemented the TUR option.

izontal line in panel (d) and the 3-month moving average for state TUR was above the horzontal line in panel (b). The additional time period—over and above the broken-blue shaded period—for which Arkansas would have triggered on because of the TUR options is shaded solid-gray in the figure. Notice that the state TUR in Arkansas rose above 8% in 2011. At that point, the second tier of the TUR options could have triggered. Had Arkansas adopted these options, the potential benefit duration in Arkansas would have increased by a total of 20 weeks from EB program extensions at that point.

In reality, Arkansas had none of the optional trigger rules in place over this period. For this reason, EB extensions were available only for the short *broken-blue* period from April through September 2009. Had Arkansas implemented the optional TUR trigger rules, however, UI recipients would have been eligible for EB extensions for the much longer *solid-gray* period from April 2009 through May 2012.

Intuitively speaking, Arkansas serves as a control in our analysis. It had no optional trigger rules in place. It therefore had shorter potential benefit duration than other states that were otherwise identical except that they had adopted some or all of the optional trigger rules. In Appendix B.2, we present a similar analysis for a state that adopted the optional TUR trigger rules and is thus a "treated" state, Washington over the 2002–2004 period.

4.2 Regressions Conditioning on Risk Sets

Whether a state receives a UI extension in a given period is determined by two factors: 1) which options the state satisfies the trigger rules for at that point in time, and 2) which options the state has adopted. The basic idea behind our identification strategy is to consider the second of these factors to be exogenous to current labor market conditions conditional on controls. Since the trigger rules are known and we have real-time data on the trigger variables, we can partition the state space into "risk sets" based on which options a state satisfies the trigger rules for and include dummy variables for each such risk set interacted with time fixed effects as controls in our empirical specification. By doing this, we soak up all variation associated with which options a state satisfies the trigger rules for and, thus, focus exclusively on variation in UI extensions that arise only from which options a state has adopted.

This identification strategy has been applied in the education literature to estimate the effect of attending particular schools on student outcomes in cases were multiple lotteries determine assignment of students to schools (Abdulkadiroğlu et al., 2011; Angrist et al., 2022). In that application, students that enter different lotteries—e.g., sibling vs. non-sibling lotteries or lotteries for different schools—have different assignment risks and may also be different in other ways. Controlling for risk sets then allows researchers to focus exclusively on the random assignment associated with the lottery number each student gets. Our method is also conceptually related to the simulated instruments methodology pioneered by Currie and Gruber (1996a,b), though it is technically different and allows us to use more of the quasi-random variation in our data.[27]

The considerations discussed above motivate using the following empirical specification:

$$y_{s,t+h} = \beta_h \widehat{W}_{s,t} + \text{qual. controls}_{h,s,t} + \delta_{h,s} + \Gamma x_{h,s,t} + \epsilon_{h,s,t}. \tag{1}$$

where s denotes states, t is time measured in weeks, $y_{s,t+h}$ is an outcome of interest (e.g., the unemployment rate) in week $t+h$, $\widehat{W}_{s,t}$ is our treatment variable of interest (described below), qual. controls$_{h,s,t}$ are the "qualifying controls" discussed above (more detail below), $\delta_{h,s}$ is a set of state fixed effects, $x_{h,s,t}$ is a set of additional controls, and $\epsilon_{h,s,t}$ represents other unmodelled determinants of the outcome variable. The coefficient of interest in this specification is β_h. Notice that since we are interested in estimating dynamic effects of UI extensions, we consider specifications with outcome variables at different horizons h.[28]

Our treatment variable of interest $\widehat{W}_{s,t}$ is defined as the difference between actual potential benefit duration in state s at time t and the counterfactual potential benefit duration of state s at time t if the state had adopted no options.[29] The coefficient on this variable β_h measures the effect of a UI extension on the outcome variable at horizon h. Why use $\widehat{W}_{s,t}$ as opposed to simply the level of actual potential benefit duration in state s at time t as the treatment variable? The reason

[27]Borusyak and Hull (2023a) develop a general framework and alternative estimator for settings such as the one we study. They refer to their estimator as a "recentered IV" estimator. Their approach regresses the outcome on the treatment recentered by the mean of the treatment within each risk group as opposed to controlling directly for risk-group fixed effects as we do. We could alternatively adopt this approach. To achieve similar precision, however, we would also need to condition on our "qualifying controls" (see Borusyak and Hull's "controlling" approach).

[28]Our approach assumes constant effects of extended benefits across groups and over time. Recent work has made progress in allowing for non-parametric heterogeneity in treatment effects, while still assuming a parametric form for the evolution of the untreated units (Sun and Abraham, 2021; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; de Chaisemartin and D'Haultfœuille, 2020; Borusyak, Jaravel, and Spiess, 2023). Borusyak and Hull (2023b) note that the concerns that have been emphasized in this literature regarding non-convex averages of heterogeneous treatment effects do not apply if one interprets the analysis from a design-based perspective with a randomly assigned treatment. This is a natural interpretation of our analysis since we argue option adoption is as-good-as-randomly assigned conditional on controls. At a practical level, the alternative estimators that have been proposed to address this issue cannot be easily applied to our setting because it is not clear whether one should consider the treated units as those that receive or fail to receive extended benefits. It is worth noting that we present estimates for short and long-duration samples below as well as pooled estimates. The pooled estimates are roughly in-between the separate estimates for those sub-groups. This suggests that the issues that have been emphasized in the two-way fixed effects literature are not severe in our setting.

[29]For the purpose of constructing $\widehat{W}_{s,t}$, we assume that states always pay 26 weeks of regular state benefits. This removes variation in weeks provided by UI extensions that arises from a small number of observations in our data where states pay less than 26 weeks of regular state benefits. For most programs, the weeks provided by UI extensions are proportional to regular state benefits up to a maximum, which is binding for most states and weeks in our data. We do this to address the potential concern that states' regular UI benefit duration may be correlated with our treatment variable. In practice, however, this does not materially affect our estimates of the effects of UI extensions, as the results are nearly unchanged when taking this variation into account.

Table 3: Determinants of Risk Sets

	IUR Option	TUR Option	3-Year TUR Lookback Option
13-Week EB Tier	✓	✓	✓
20-Week EB Tier		✓	✓
FSC "Reachback" Tier	✓		
FSC Tier II	✓		
TEUC "X" Tier		✓	
EUC Tier IV	✓	✓	✓

NOTE. This table illustrates the determinants of the risk sets sets that arise from optional trigger rules. Each row corresponds to a tier of UI extensions. Each column corresponds to an optional trigger rule. A check-mark in a given cell indicates that this option can trigger this tier at some point during our sample period. The risk sets at a given point in time are all the different combinations of cells with check-marks at that point in time (including the empty set).

for this is that $\widehat{W}_{s,t}$ isolates the component of the variation in potential benefit duration associated with the EB program—the only program with optional trigger rules. Differences in the duration of regular UI benefits across states and in the incidence of other (mandatory) UI extension programs across states do not contribute to variation in $\widehat{W}_{s,t}$. Focusing on $\widehat{W}_{s,t}$, thus, allows us to focus exclusively on the potential reverse causality problems arising from the EB program, as opposed to also considering endogeneity in the other trigger rules.[30] As seen in Figure A.1 in the appendix, variation in $\widehat{W}_{s,t}$ has been present throughout most of our sample period.

The heart of our identification strategy is the "qualifying controls" term. These controls take the following form:

$$\text{qual. controls}_{h,s,t} = \sum_{z,t} \alpha_{z,h,t} I_s(z,t), \tag{2}$$

where $I_s(z,t)$ is a dummy variable that is equal to one if two conditions are satisfied: 1) the time period is t, and 2) state s is in risk set z. The risk sets z are a partition of the state space based on which options and benefit levels a state satisfies the trigger rules for. The set of options and associated benefit levels available varied over time. The full set of options and relevant benefit levels are described in Table 3. The risk sets for a given point in time are all the possible subsets of the set of options and associated benefit levels that were available at that time (including the empty set, i.e., no options). In Table 3, this is all subsets of cells that have a check-mark at that point in time. Intuitively, we are interacting time fixed effects with dummies for the risk sets. Within

[30] Most EB extensions are 13 weeks (or 20 weeks including the second TUR tier), i.e., the most-frequent non-zero values of $\widehat{W}_{s,t}$ are 13 and 20. There is, however, some "non-standard" variation. During parts of the FSC, TEUC, and 2008 EUC programs, the benefits available from these programs depended on a state's EB status. We discuss these cases in more detail in Appendix A.4. Panel (b) of Table A.5 presents results for a specification that removes this non-standard variation in $\widehat{W}_{s,t}$ using a binary alternative. This yields results similar to our baseline results.

these risk sets, the variation in $\widehat{W}_{s,t}$ arises from a state's option adoption.[31] We verify this property of our estimator via a placebo exercise in Appendix B.3. There, we re-compute $\widehat{W}_{s,t}$ (many times) using the options adopted by another state s' at time t and find no effect of $\widehat{W}_{s,t}$.

In addition to the qualifying controls, we also include state fixed effects and a vector of additional controls $x_{s,t}$. The additional controls are lags of potential benefit duration and $y_{s,t}$ in the distant past (quarterly lags of these variables from two years prior to four years prior (t-104 to t-207)). Section 4.5 shows that we find no evidence that states adopting options have unusual trends in unemployment preceding the option adoption.

Table A.5 contains results with additional controls to account for other potential identification concerns. Among these, we estimate a version that includes lagged controls of several labor market variables (panel (c)), a version that includes industry-employment shares as controls (panel (d)), and a version with the state-level Covid stringency index of Hale et al. (2021) as a control (panel (e)). We assume that option adoption is exogenous to local labor market conditions conditional on these controls.

4.3 The Timing of the Effects of UI Extensions

There are several factors that complicate the identification of the *timing* of the effects of UI extensions. First, UI extensions are determined every week, while most of our outcome variables are monthly. Second, there is a three-week delay between the week in which a state surpasses a threshold and the week in which it actually extends benefits. For many extensions, this implies that the extension is announced three weeks before it starts.[32] The thirteen-week rule can also cause changes in benefits to be known as far as twelve weeks in advance.[33] Third, the smoothing procedure used to address sampling error in the state-level LAUS data may also generate timing error in our main outcome variable. Fourth, many optional trigger rules since the Great Recession have been based on the presence of federal funding for EB. This federal funding was implemented by temporary legislation, the beginning and ending of which were sometimes known in advance. Finally, it may be possible to foresee when a state will trigger off substantially in

[31] In Appendix A.5, we present an alternative specification in which we only use observations for which option status is pivotal in determining a state's benefit level. This yields similar estimates—in some cases somewhat larger, though less precisely estimated.

[32] Indeed, trigger sheets may explicitly say that a state "will" trigger in a few weeks. For example, see `https://oui.doleta.gov/unemploy/trigger/2002/trig_102702.html`. In that example, Alaska is going to start paying EB13 on 11/10, and North Carolina is stopping on 11/16.

[33] One example of this phenomenon relates to the 13-week rule. For example, in 2020, Utah's IUR surpassed its threshold for a single seven-week period, effectively giving recipients six weeks of notice of when benefits would end (since the state had no options in place).

advance, given that unemployment falls persistently during recoveries, and remaining "triggered on" for Extended Benefits often requires unemployment to rise due to the lookback rule. These complications motivate the following two alternative specifications.

First, variation in any of the timing factors described in the previous paragraph can introduce noise into the estimation of the causal effect of UI extensions for any particular horizon. To address this, we construct a summary measure of the contemporaneous quarterly effect of UI extensions on various outcomes by estimating a "time-averaged" version of equation (1). This measure is the estimate of β in:

$$\mathbf{y}_{s,t} = \beta\,\mathbf{W}_{s,t} + \text{qual. controls}_{s,t} + \delta_s + \Gamma x_{s,t} + \epsilon_{s,t}. \tag{3}$$

where $\mathbf{y}_{s,t} \equiv \sum_{\ell=-6}^{6} y_{s,t+\ell}$ is the quarterly sum of $y_{s,t}$ centered on week t, and $\mathbf{W}_{s,t}$ is the analogous quarterly sum of potential benefit duration. Crucially, we instrument for $\mathbf{W}_{s,t}$ with $\widehat{W}_{s,t}$. The contemporaneous quarterly effect β thus measures the ratio of the quarterly increase in the outcome variable in question to the quarterly increase in potential benefit duration resulting from the variation in UI extensions that we identify. This formulation reduces noise in our estimates arising from the timing issues described in the previous paragraph.[34]

Second, we have designed a specification with the objective of identifying the timing of the effects, taking into account the complicating factors above. Specifically, we estimate a version of equation (1) in which we control for the previous month's treatment $\widehat{W}_{s,t-4}$.[35] We also control for quarterly lags of the left-hand side variable starting one quarter before t, similar to the standard practice in the local projection literature. Typically, local projections control for the left-hand side variable starting the period before the shock, but we control for the lags starting one quarter in the past in order to allow for the possibility of anticipation effects. This specification takes the form

$$y_{s,t+h} = \beta_h \widehat{W}_{s,t} + \Gamma_h \left[\widehat{W}_{s,t-4} \times \text{qual. controls}'_{h,s,t}\right] + \text{qual. controls}'_{h,s,t} + \delta_{h,s} + \Xi_h x'_{h,s,t} + \epsilon_{h,s,t}. \tag{4}$$

In contrast to equation (1), here we must include qualifying controls for both period t and $t-4$, and interact these qualifying controls with $\widehat{W}_{s,t}$ and lags of the left-hand side variable.[36] This controls for differences in dynamics across different values in our qualifying controls. We have verified in

[34]As robustness, Table A.6 presents OLS estimates of equation (1) for $h = 0$. Panels (g) and (h) in Table A.5 in the appendix show alternatives to the *quarterly* window, yielding similar results.

[35]We use a four week lag because our outcome variables are monthly. We carry out the analysis at a weekly frequency because the trigger rules are weekly objects, and must be aligned with risk sets at a weekly frequency.

[36]That is, qual. controls$'_{h,s,t} = \sum_{z,t} \left(\alpha_{z,h,t} I_s(z,t) + \beta_{z,h,t} I_s(z,t-4) + \gamma_{z,h,t} I_s(z,t) \times I_s(z,t-4)\right)$. The term $x'_{h,s,t}$ in equation (4) contains quarterly lags of potential benefit duration and $y_{s,t}$ from $t-13$ to $t-116$ (two years), all interacted with qual. controls$'_{h,s,t}$.

our Monte Carlo simulations that these interactions must be included to accurately estimate the treatment effect. Because we are particularly interested in the timing of the effects in this version of the estimating equation, we merge the monthly outcome data to weeks of treatment data from the previous month, which ensures that the outcome data always postdates the treatment week we are plotting.

We estimate equations (1) and (4) by OLS, and equation (3) by two-stage least squares (2SLS). In all specifications, we cluster standard errors by state and week. For ease of interpretation, we multiply the left-hand side variables by 13. This implies that all the empirical estimates we report can be interpreted as the response to a typical (13 week) extension. We drop Alaska in our baseline specification. Alaska has the peculiar feature that it triggers onto EB nearly every year, due to its highly seasonal industry structure. This is not representative of the behavior of other states in the sample and introduces substantial noise into our estimates. We also drop the small number of state-weeks for which our UI Benefits Calculator predicts UI benefits incorrectly.

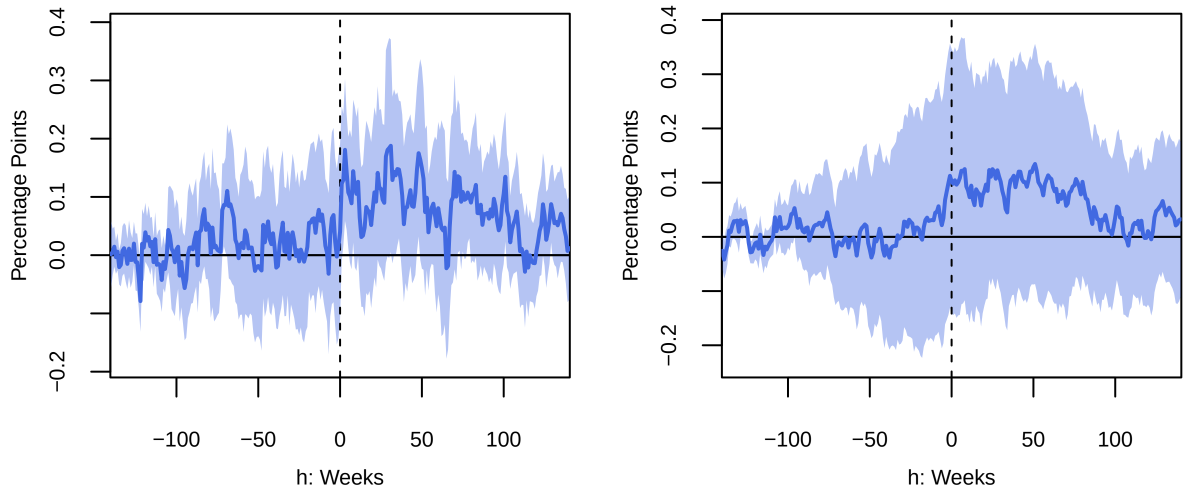
4.4 Monte Carlo Analysis of Empirical Methodology

Appendix A.6.2 reports a Monte Carlo simulation of our empirical methodology, using simulated data. We confirm that our preferred methodology is able to recover the magnitude of the treatment effect. We also confirm that the Monte Carlo simulation matches the shape of the dynamics of our estimates in the data from the various regression specifications discussed above. Given the potential for anticipation effects, as well as measurement error in the timing of when potential UI recipients learn about benefit extensions, it is important to allow for the possibility that the effects of changes in $\widehat{W}_{s,t}$ may not be contemporaneous. We show that estimating equation (1) is less sensitive to timing and measurement error than equation (4), for reasons analogous to those emphasized by Griliches and Hausman (1986).

4.5 Endogeneity of Option Changes

Perhaps the most important threat to identification in our analysis is that states may choose to implement optional (i.e., more lenient) trigger thresholds for the EB program because they have received some bad news about future local labor market outcomes.

Figure 5 presents evidence on the dynamics of unemployment in advance of option switches. We estimate a version of equation (1) in which we replace $\widehat{W}_{s,t}$ with an indicator for whether a state has adopted any option in week t. The left-hand side variables are the insured unemployment rate



(a) Insured Unemployment

(b) Unemployment Rate

Figure 5: Labor Market Trends Around Options Switches

NOTE. This figure shows estimates of equation (1), in which we replace $\widehat{W}_{s,t}$ with an indicator for whether a state adopted any optional trigger rule in each week. We present the relationship between option adoption and either the insured unemployment rate, or the unemployment rate. Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in -150, \ldots, 150$. The sample runs from the beginning of 1980 through 2019, and excludes Alaska. Standard errors are two-way clustered by state and week.

(panel (a)) and the unemployment rate (panel (b)). For the unemployment rate, the relationship is statistically insignificant at all horizons considered and the point estimate fluctuates around zero. For the insured unemployment rate, we see no statistically significant relationship before the option is adopted, and a small positive (albeit noisy) relationship after adoption, reflecting the causal effect of the resulting extensions.[37]

5 Results

We present estimates of the effect of UI extensions on the labor market for several different sample periods. Our sample starts in 1980, but we use lagged controls back to 1976. Our baseline analysis focuses on the pre-Covid period. We discuss results including Covid at the end of the section. We estimate the effect of UI extensions separately for periods with "short" and "long" baseline potential benefit duration (<60 weeks vs. ≥60 weeks). The average baseline potential benefit durations are 34 and 74 weeks respectively in the two samples.

In our baseline analysis, we estimate separate state fixed effects and coefficients of interest β_h

[37]We also present results for option *terminations* in Appendix A.13, though this evidence is harder to interpret because, in our data, most states that terminate options had adopted them between 2 and 5 years prior (and, thus, were more likely to be treated).

Table 4: Effects of a 13-Week Benefit Extension

	Insured Unemployment		Unemployment		\$ Spent per Capita	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.60	0.93	0.28	0.44	7.21	10.13
	(0.08)	(0.19)	(0.13)	(0.16)	(1.10)	(2.05)
$\mathbf{W}_{s,t}$, Long Dur.	0.25	0.30	-0.03	0.05	2.93	3.31
	(0.13)	(0.14)	(0.11)	(0.10)	(1.36)	(1.48)
Observations	103494	111298	103494	111298	103494	111298

NOTE. This table shows 2SLS estimates of β in equation (3), with the variables in the column headers as left-hand side variables, all multiplied by 13. The sample runs from the beginning of 1980 through 2019 and excludes Alaska. The columns labeled "Short Dur." and "Long Dur." correspond to the estimated effect when baseline potential benefit duration is below and at least 60 weeks, respectively. Standard errors are two-way clustered by state and week and shown in parentheses.

for the short-duration and long-duration subsamples, but pool the remaining coefficients.[38] We present results for a case where the cutoff is set to 40 weeks rather than 60 weeks in panel (l) of Table A.5. Results are similar to our baseline results.

5.1 Effects of UI Extensions

In Table 4, we present estimates of the contemporaneous quarterly effect of UI extensions from equation (3). The first row focuses on UI extensions at short baseline potential benefit durations ($<$ 60 weeks)—"short durations" for short—while the second row presents estimates for long baseline potential benefit durations ($\geq$ 60 weeks)—"long durations" for short. We present estimates for the insured unemployment rate, the unemployment rate, and dollars spent per capita. The last outcome is an accounting measure of dollars spent on UI, calculated as total UI payments in each month divided by the labor force, and converted to December 2007 dollars using the national PCE price index. We focus in this section on discussing the pre-Covid (1980-2019) results.

At short durations, a standard 13-week UI extension leads to a 0.6 percentage point increase in the insured unemployment rate (which we define here as including Extended Benefits and other federal programs).[39] The increase in the overall unemployment rate is roughly half as large: 0.28 percentage points.[40] The difference presumably arises because UI extensions likely add some peo-

[38]Panels (i), (j), and (k) of Table A.5 present results for alternative assumptions about pooling the auxiliary coefficients across the short and long-duration samples, which are similar.

[39]The IUR measure used in UI trigger rules excludes Extended Benefits and other federal programs, and therefore underestimates the fraction of people on UI.

[40]These results use the LAUS unemployment rate. In panel (n) of Table A.5, we show that using a measure constructed directly from the CPS yields similar results.

ple to the UI rolls who would have been unemployed regardless of any behavioral or equilibrium response to UI.

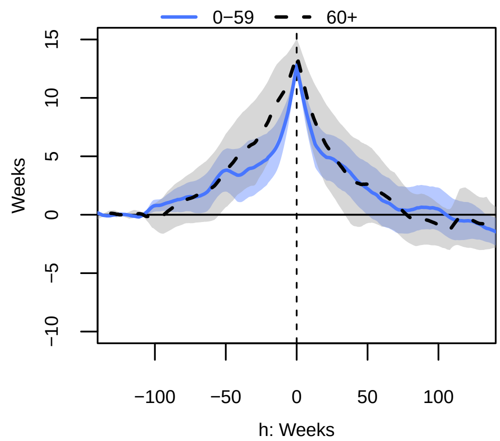
In contrast, the estimated effects on both measures of unemployment at long durations are small and statistically insignificant. For this subsample, we estimate an effect on the insured unemployment rate of 0.3 percentage points, and an effect on the overall unemployment rate of -0.03 percentage points. These small estimates at long durations are consistent with the findings of other empirical studies of the macro effects of UI extensions for samples dominated by the Great Recession period. Appendix B.1 reviews these estimates and converts them into the units we use—i.e., the effect of a 13-week UI extension on the unemployment rate. A leading example from this literature is Chodorow-Reich, Coglianese, and Karabarbounis (2019). Their estimate implies that a 13-week extension raises the unemployment rate by 0.01 percentage points.

The third column shows that UI extensions raise dollars spent on UI, but the effect is quite small. The contemporaneous quarterly effect is only 7 dollars per month per capita at short durations, and 3 dollars at long durations. It is important to remember, however, that this—like all of the effects we estimate—is a local average treatment effect. It measures the effect at the margin when a UI extension occurs. Even in our short duration (<60 weeks) sample, the baseline potential benefit duration is often larger than 40 weeks when an extension occurs. Only those that are unemployed for more than this amount of time benefit directly from the extension. The number of dollars spent (and potential for fiscal stimulus) on the first weeks of UI benefits is much larger than for extensions that occur later on since many more people are directly affected.

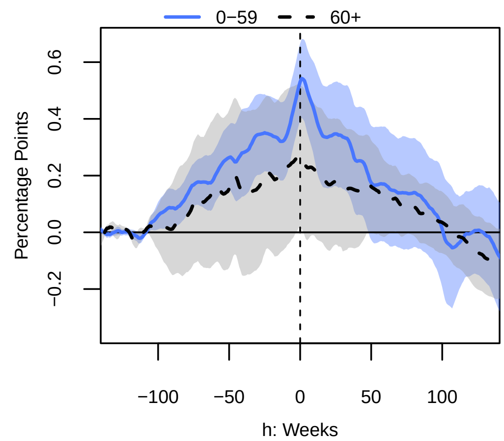
Figure 6 presents estimates from equation (1). The top left-hand panel presents results for potential benefit duration, which is akin to the "first-stage" in our analysis. Each point in the graph reflects a different time horizon h, positive or negative. The maximum effect is at $h = 0$ and effects follow a roughly symmetrical tent-shape for both positive and negative horizons. This reflects the persistence of the treatment variable $\widehat{W}_{s,t}$, for which a high value at $h = 0$ also signals a high value in surrounding time periods. If a state was on EB in a particular week, it is likely the state was also on EB in surrounding weeks.[41]

The remaining panels of Figure 6 present estimates for the five outcome variables we consider: the unemployment rate, the insured unemployment rate, the employment rate (based on the CES),

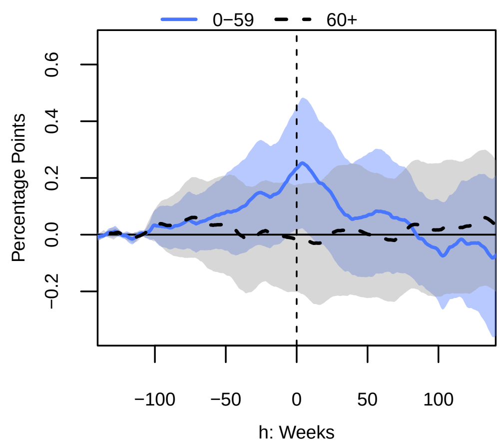
[41]We view the persistence of the effects we estimate on potential benefit duration as a strength of our empirical design. It contrasts with the much more transitory shocks to UI extensions considered, for example, by Chodorow-Reich, Coglianese, and Karabarbounis (2019), which lead to elevated potential benefit duration for only roughly 4 months.



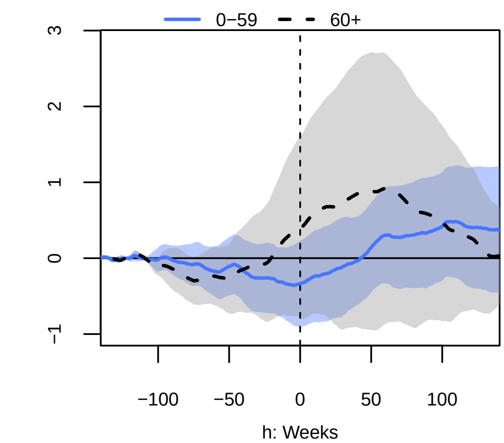
(a) Potential Benefit Duration



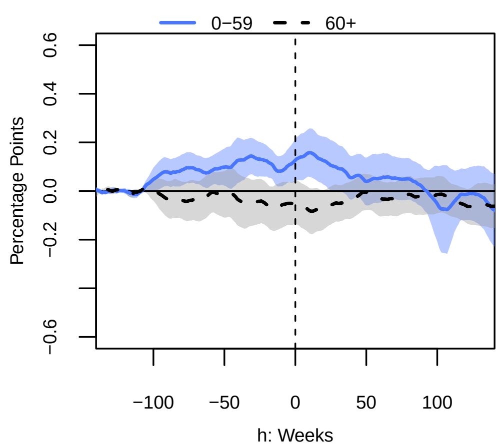
(b) Insured Unemployment



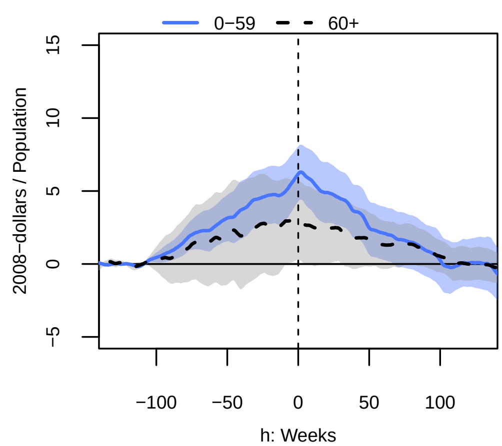
(c) Unemployment Rate



(d) Employment Rate, CES



(e) Insured Unemployment
Non-Mechanical



(f) Dollars Spent per Capita

Figure 6: Macro Effects of UI Extensions: Long vs. Short Baseline Potential Benefit Durations

NOTE. Each panel shows OLS estimates of β_h for short baseline potential benefit durations (the blue solid lines) and for long baseline potential benefit durations (the black dashed lines) from equation (1), with the variable in the panel titles as left-hand side variable. Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in -150, \ldots, 150$. The sample runs from the beginning of 1980 through 2019, and excludes Alaska. Standard errors are two-way clustered by state and week.

the non-mechanical component of the insured unemployment rate, and dollars spent per capita.[42] These can be viewed as the "reduced form" of our analysis. The "non-mechanical component of the insured unemployment rate" counts only regular state UI. This excludes Extended Benefits and other federal programs (the "mechanical" effect of our treatment) but is otherwise defined analogously to our modified insured unemployment rate.[43] The solid blue lines are estimates for short durations, while the black-dashed lines are estimates for long durations.

The treatment variable—potential benefit duration—exhibits a similar dynamic pattern at long durations and at short durations. In other words, the "experiment" is similar for these two subsamples. However, the remaining outcomes all have larger magnitudes for the short than long duration samples. The pattern of effects for the employment rate is roughly inverted in the short duration sample relative to the patters of effects for unemployment: employment exhibits a decline of 0.4 percentage points. However, these results are substantially noisier (and the confidence intervals intersect zero).[44] At long durations, the effect we estimate for the employment rate has the "wrong" sign, but is estimated with little precision.

The non-mechanical component of the insured unemployment people rises significantly in the short duration sample, but not at all in the long-duration sample. This measure excludes the part of the insured unemployment rate associated directly with the receipt of Extended Benefits and other federal programs. It must therefore reflect a behavioral or general equilibrium response. Again, the effect is much larger at short durations (0.15 percentage points) than the zero effect at long durations.

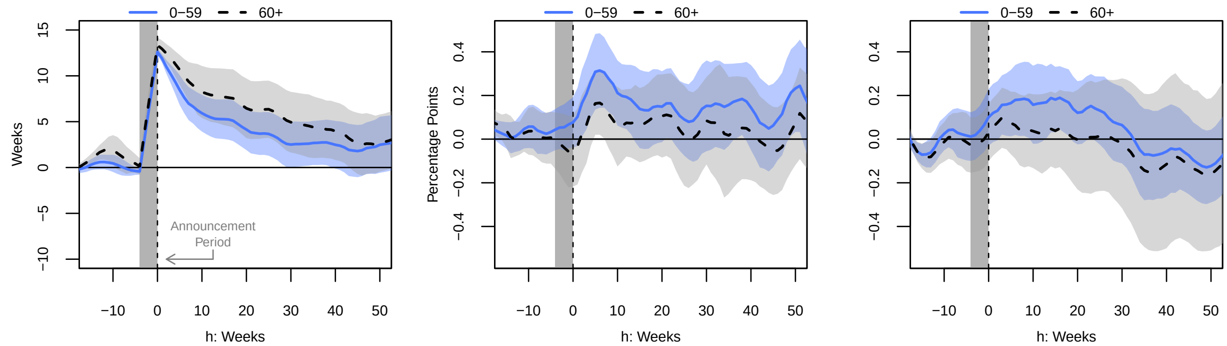
Figure B.3 in the appendix presents analogous estimates where we do not split the sample by baseline potential benefit duration. These "full sample" results generally lie between the short-duration and long-duration results. Panel (i) of Table A.5 presents the corresponding contemporaneous quarterly effects.

We have emphasized the importance of qualifying controls in accounting for reverse causation

[42]After extensive discussions with BLS, we decided to make use of the CES rather than the QCEW due to its greater suitability for longitudinal analysis. QCEW was first developed in the 1980s and has been subject to various administrative changes: coverage was quite incomplete early in the sample period; individual state laws have changed; and there have been changes to the national UI system. The CES attempts to account for these issues to create a consistent time series whereas QCEW does not. As a consequence various states have unusual labor market trends in the QCEW, particularly early in the time series.

[43]This measure is quite similar to the IUR definition used in UI trigger rules, which also excludes Extended Benefits and other federal programs.

[44]The relative imprecision of the employment results is driven in part by larger time variation in trends in the employment rate. The employment rate is sensitive to secular shifts in multiple job holding, commuting behavior, agricultural employment, and self-employment. Panel (n) in Table A.5 presents results for labor force participation, and log employment.



(a) Potential Benefit Duration (b) Insured Unemployment (c) Real-time Unemployment

Figure 7: Timing of Effects: Regressions on $\widehat{W}_{s,t}$ Controlling for Lags

NOTE. This figure shows estimates of β_h—the effects of a UI extension when baseline potential benefit duration is short—in equation (4) at different horizons, with the variable in the panel titles as the left-hand side variable. The shaded "announcement" region denotes the period during which the beginning and end of EB payments are typically announced. Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in -15, \ldots, 50$. The sample runs from the beginning of 1980 through 2019, and excludes Alaska. Standard errors are two-way clustered by state and week.

in our regression specification. Figure A.5 in the appendix demonstrates this point quantitatively. When we drop the qualifying controls, UI extensions are estimated to have a substantially larger effect on the unemployment rate—including at long durations. The estimated effects are particularly large if we also drop the time fixed effects.

5.2 Timing of Effects

Figure 7 presents estimates from equation (4). This specification substantially reduces the persistence of the shock by controlling for the one-month lag of $\widehat{W}_{s,t}$. This is meant to help identify the timing of the effects of UI extensions. The gray shaded area in Figure 7 highlights the three week period from when EB extensions are typically announced and when the benefits start being paid out (which occurs at $h = 0$ in the figure). The left-hand side graph reports results for the treatment variable (potential benefit duration). The middle graph reports the results for the insured unemployment rate, and the right-hand side graph reports the results for the real-time unemployment rate. We focus on the real-time unemployment rate results here since it removes the smoothing inherent in the revised LAUS unemployment rates.[45]

The estimates in Figure 7 show that the insured unemployment rate rises rapidly around the time that benefits are extended. The unemployment rate also increases, though somewhat less rapidly, consistent with the notion that the unemployment response reflects slower-moving be-

[45]Results for the revised unemployment rate are presented in Figure A.4.

havioral and equilibrium forces. Generally, these results are qualitatively similar, though somewhat smaller, than the estimates reported for equation (1) above. This is consistent with our Monte Carlo results in Appendix A.6.2 demonstrating that estimating equation (1) is less sensitive to timing and measurement error than equation (4), for reasons analogous to those emphasized by Griliches and Hausman (1986).

5.3 Why Do UI Extensions Have Different Effects at Short versus Long Durations?

Most theories of the effects of UI extensions imply that UI extensions matter less at longer baseline durations. One reason for this is simple: the number of workers impacted by a UI extension falls over time as workers find jobs. Over the period 1994–2021, the median duration of unemployment spells was only 11 weeks. Even in 2012, during the depths of the Great Recession, the median spell lasted 19 weeks.[46]

This will affect the responsiveness to UI extensions through both direct and indirect mechanisms. First, there are relatively few long-term unemployed. Hence, a UI extension at long baseline potential benefit duration prevents relatively few cases of benefits expiring. Second, workers may anticipate that they are unlikely to use a UI extension far in the future. This will imply that their job search behavior is not much affected by a long-duration UI extension. Finally, those most at risk of long-term unemployment may also be less employable and thus respond less to UI extensions.[47]

UI extensions during the recoveries after deep recessions sometimes involve substantial political uncertainty. This occurred both after the Great Recession—when the federal EUC program and EB in many states lapsed three times due to a lack of federal funding—and also after Covid. The uncertainty around these programs may have also contributed to the lack of response from unemployed job seekers to the extensions.

5.4 Effects Including the Covid Recession

We next discuss results based on the extended sample period up to 2022, including the pandemic recession and ensuing recovery. This period saw UI extensions at both short and long baseline potential benefit durations. Table 4 presents contemporaneous quarterly effects for the insured

[46]Appendix A.9 shows the cumulative distribution function (CDF) of unemployment durations over two periods: 1994-2021 and 2012. Since most unemployment spells are short, the CDF rises rapidly at low values.

[47]Mueller and Spinnewijn (2023) show that job finding probabilities indeed are highly heterogeneous across job seekers and thus job seekers differ strongly in their probability of becoming long-term unemployed.

unemployment rate, the unemployment rate, and dollars spent per capita. For the short duration sample, the point estimates are roughly 50% larger including the Covid recession (though the standard errors also become larger). The response of the insured unemployment rate is 0.9 percentage points (vs. 0.6 pre-Covid). The response of the unemployment rate is 0.44 percentage points (vs. 0.28 pre-Covid). Dollars spent per capita increase by $10 (vs. $7 pre-Covid).

Figure 8 presents results that are analogous to our baseline results from Figure 6, but with a sample that ends in 2022.[48] The treatment variable, potential benefit duration, behaves similarly excluding and including the Covid period. The remaining variables respond considerably more at short durations when the Covid period is included. The results at long durations are, for the most part, still small and statistically insignificant. One new *qualitative* result is that initial UI claims responded significantly to UI extensions at short durations during the pandemic recession, but not at all in the pre-Covid sample (a contemporaneous quarterly effect of 0.15 versus 0.03—see panel (a) of Table A.5 for more details).

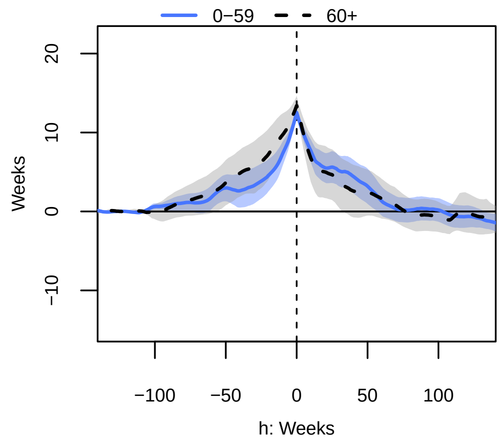
What might explain the larger effects of UI extensions during the Covid recession? One factor is that UI replacement rates were exceptionally high during the Covid recession. Ganong et al. (2020) estimate that the median replacement rate was 145% in mid-2020, in contrast to a typical replacement rate of around 50%. In addition, the Covid period saw much wider UI coverage for non-traditional workers (e.g., gig workers and contractors).[49]

The UI extensions during the Covid period were also highly publicized, and likely particularly salient.[50] In fact, during the pandemic, the number of UI recipients well exceeded the official count of the unemployed (Appendix A.12). It may also have played a role that the Covid recession featured many temporary layoffs because the downturn was expected to be short. Katz and Meyer (1990) highlight that a substantial portion of individuals receiving unemployment insurance expect to be recalled to their original employer. They find that these workers tend to search with less intensity. This can lead to larger unemployment effects if these workers are not eventually

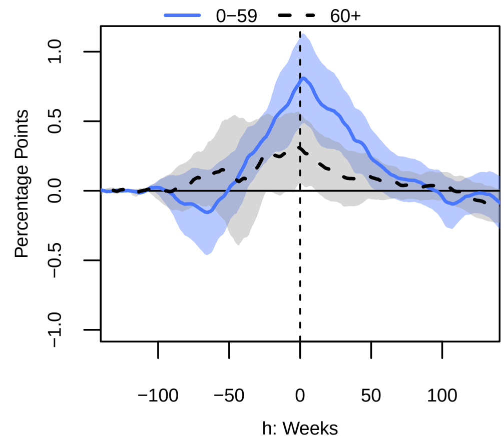
[48]Panel (o) of Table A.5 presents results estimated using only observations from 2020–2022. Unsurprisingly, these estimates are quite noisy.

[49]The Covid era programs include PEUC, which provided unconditional additional weeks of benefits for workers covered by traditional UI (first 13 weeks, then 24, then 53); PUA, which was the same as PEUC for workers not traditionally covered by UI such as gig workers (first 39, then 50, then 79 weeks); FPUC, which provided an additional $600, then $300 per week for all UI recipients; and MEUC, which provided an additional $100 per week for other non-covered unemployed workers.

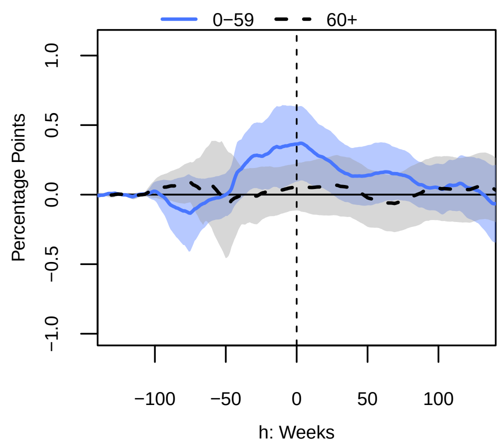
[50]Appendix A.10 shows that while Google searches for "unemployment insurance" closely mirrored the national insured unemployment rate, searches for "how to file for unemployment insurance" were much more prevalent during the Covid recession than the Great Recession. This suggests that there were likely more first-time UI recipients during the Covid period.



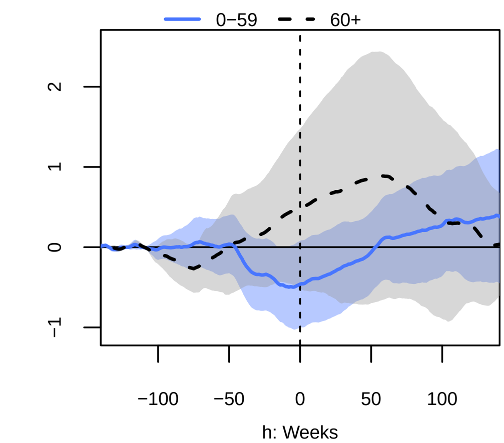
(a) Potential Benefit Duration



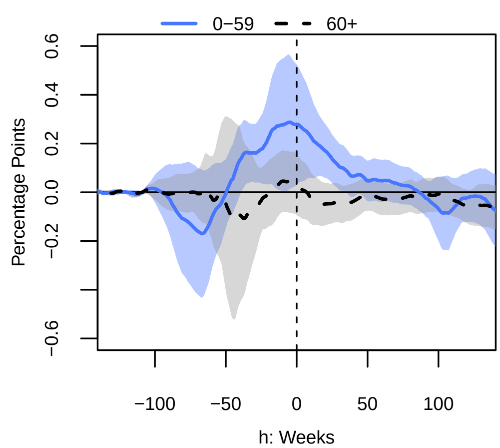
(b) Insured Unemployment



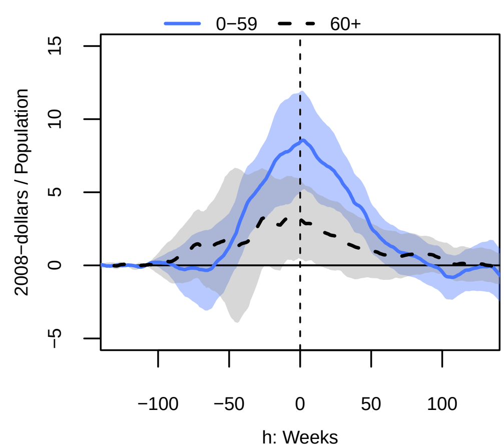
(c) Unemployment Rate



(d) Employment Rate, CES



(e) Insured Unemployment Non-Mechanical



(f) Dollars Spent per Capita

Figure 8: Effects Including Covid Recession

NOTE. This figure shows estimates analogous to those in Figure 6, but with an extended sample period that runs through the end of 2022. The estimates in these panels for $h > 1$ contain progressively fewer observations as fewer leads become available. Standard errors are two-way clustered by state and week.

recalled.[51] The prevalence of temporary layoffs during the Covid period may also explain why we find effects on initial UI claims in this period but not before, as discussed above.

5.5 Additional Robustness

Panel (r) of Table A.5 presents results in which we drop 2-years before and after any change in option status. Doing this raises our estimate for the short-duration sample although the difference is not statistically significant. For the long-duration sample, this case is very imprecisely estimated. This is because the vast majority of observations for the long-duration sample occurred during the Great Recession, and most occur within two-years of an option switch. In order to maintain precise estimates and still address the endogeneity concern, panel (s) of Table A.5 presents results for a case where we drop 2-years before and after any option switches that we label as "discretionary." The results for this case are very similar to our baseline results.

Whether a state has adopted EB options might affect its sensitivity to shocks. To address this, in panel (f) of Table A.5, we present results in which we control for interactions of option status with changes in national macroeconomic variables. The additional controls have little impact on our estimates.

6 Are Our Estimates Large or Small?

We next discuss in more detail how our estimates compare to earlier empirical work on UI extensions and also how they compare to the implications of a general equilibrium search-and-matching model. An extensive and credible literature has provided estimates of the elasticity of unemployment duration to UI extensions in partial equilibrium. We start by deriving a simple formula to evaluate whether these estimates are consistent with our findings. We do this using a plain-vanilla search model that abstracts from general equilibrium effects. We then consider a general equilibrium search-and-matching model that incorporates a number of detailed features including the response of hiring behavior to UI extensions. Finally, we relate our analysis to work emphasizing the role of UI as fiscal stimulus.

[51] Ganong et al. (2023) discuss the role of temporary layoffs during the Covid recession.[52] Our results are not directly comparable to theirs, since they consider the effect of UI benefit *amounts*, and we consider the effect of UI benefit *duration*.

6.1 Relationship to Micro Estimates

Consider a simple search and matching model with a job finding rate f_t and a constant separation rate s. Assume, for simplicity, that the labor force participation rate is constant. In this case, the law of motion for unemployment will be $U_t = U_{t-1} - f_t U_{t-1} + sE_{t-1}$ where U_t denotes the stock of unemployed individuals at time t and E_t is the stock of employed individuals at time t. This equation forms the basis for much of the existing work that has sought to convert microeconomic estimates of duration elasticities of UI extensions into macroeconomic effects on unemployment.

In this case, the steady state unemployment rate is $u^\star = s/(s+f)$ where f is the steady state job finding rate. Totally differentiating this equation assuming that the job finding rate can change across steady states but the separation rate remains unchanged yields $d\log(u^\star) = -(1-u^\star)d\log(f)$, where we use the fact that $1 - u^\star = f/(s+f)$. Using the fact that unemployment duration is the reciprocal of the job finding rate $D = 1/f$ and dividing by the log change in potential benefit duration $d\log(\Pi)$ we get that

$$\frac{d\log(D)}{d\log(\Pi)} = \frac{1}{1-u^\star}\frac{d\log(u^\star)}{d\log(\Pi)}. \tag{5}$$

This equation provides a simple formula relating the duration elasticity typically calculated in the microeconomics literature and the effect of a UI extention on the unemployment rate. Appendix C derives an analogous relationship for a more detailed (and realistic) case.

Schmieder and von Wachter (2016) survey the literature that estimates the effect of UI extensions on unemployment duration, $d\log(D)/d\log(\Pi)$ (and related concepts). Appendix C corrects an error in their analysis of Card and Levine (2000). With this correction, the evidence surveyed in Schmieder and von Wachter (2016) suggests that $d\log(D)/d\log(\Pi)$ is between 0.33 and 0.49 in the United States. Importantly, the micro evidence for the US comes entirely from "short duration" UI benefit extensions. Hence, we will compare the results from this analysis to the empirical results from our short-duration sample.[53]

The cited studies typically estimate the effect of UI extensions on UI recipients. Our estimates consider the effects on all the unemployed. Therefore, to make the 0.33–0.49 range comparable to our estimates, we need to adjust for the fact that not all of the unemployed receive UI. A simple way to perform this adjustment is to multiply the range by the average UI recipiency rate, which we define as the ratio of UI recipients to all of the unemployed. In our short-duration sample, the average UI recipiency rate is 0.36. Multiplying the range by this number leads to a range of

[53]We thank Cohen and Ganong for tabulating information on initial benefit durations and sharing this information with us.

0.12–0.18. We consider a more complex case in Appendix C that takes into account the fact that the search effort of the non-UI recipients may not be affected by the UI extension (among other things). This case leads to a range of 0.15–0.22.

How does this range compare to our estimates? To see this requires us to convert our baseline estimates—for the effect of a 13-week UI extension on the level of unemployment—into elasticity form (to plug into the right-hand side of equation (5)). Our estimate of the marginal effect of a 13-week UI extension on the unemployment rate in the short duration sample is 0.28 percentage points. This implies an elasticity of unemployment to potential benefit duration of 0.11.[54] Dividing this number by $1-u^{\star}$ yields 0.12. This lies within the 0.12–0.18 range for the simple case discussed above and slightly below the range for the more complex case. Evidently, the gap between our estimates and the previous micro literature is small.

The simple method described above is the steady-state counterpart of the dynamic simulations carried out numerically by, for example, Johnston and Mas (2018) and Rothstein (2011). We have also performed such numerical simulations and found their quantitative implications to be similar to the steady state analysis we present above. This is a standard result in the search literature. The job finding rate in the U.S. is high enough that the stock-flow dynamics that the steady state approach abstracts from are short lived. With a weekly job finding rate of 5%, the half-life of these dynamics is only 13.5 weeks.

6.2 Macro Model of UI Benefit Extensions

We next present a general equilibrium search-and-matching model to elucidate the relationship between the micro and macro effects of UI extensions and the heterogeneity in these effects at short versus long durations. Our model builds on the standard Diamond-Mortenson-Pissarides (DMP) framework and adds the following features: (i) limited duration of UI benefits; (ii) endogenous search effort; (iii) incomplete take up of UI benefits; and (iv) transitions in and out of the labor force.[55] A limited duration of UI benefits is clearly essential to analyze UI extensions. Endogenous search effort is important in matching the micro-evidence on duration elasticities. The third and fourth features allow us to match the relatively low levels of UI take-up among unemployed workers.

[54]The average unemployment rate is 6.79%. The percentage change in the unemployment rate is therefore 0.28 /6.79. The average baseline potential benefit duration is 34 weeks. The percentage change in potential benefit duration is therefore 13/34. The ratio of these two is $\frac{0.28}{6.79} \times \frac{34}{13} = 0.11$.

[55]Our model extensions (i) and (ii) are inspired by the partial-equilibrium model of Mortensen (1977).

Model setup. Time is discrete and the discount factor is β. Firms post vacancies, v, to hire workers. Workers are either employed (e), unemployed (u) or inactive (n). Firm-worker matches produce output p. Matching between firms and workers is random and governed by a constant returns to scale matching function $M(S, v)$, where S is the effective number of searchers and $\theta = \frac{v}{S}$ is the labor market tightness.

Unemployed workers qualify for T periods of UI benefits, b_{UI}, and receive a flow value of leisure/home production, b_L. As a result, the flow value of unemployment is $b(\tau) = \mathbb{1}[\tau > 0]b_{UI} + b_L$, where τ is the number of periods of UI benefits the unemployed worker has left. Unemployed workers exert search effort s at cost $c(s)$, where $c(0) = 0$, $c'(s) > 0$ and $c''(s) > 0$. They are matched to firms at rate $s\lambda(\theta)$. Optimal search effort depends on the number of periods of UI benefits an unemployed worker has left, which we denote as $s(\tau)$. Aggregate search effort is the unemployment-weighted matching efficiency $S = \sum_{\tau=0}^{T} u(\tau)s(\tau)$, where $u(\tau)$ is the mass of unemployed with τ periods of UI benefits left.[56]

Employed workers are laid off from their jobs with probability δ. At the beginning of an unemployment spell, unemployed workers draw an i.i.d. take-up cost ξ from the distribution $G(\xi)$. Workers who draw a high enough cost will find it optimal not to take up UI benefits. Each period workers also draw a home production shock with probability ι, which leads them to leave the labor force. They re-enter the labor force through unemployment with probability ρ. We assume that workers who exit the labor force lose eligibility to UI benefits. Workers who have lost eligibility for UI benefits requalify for a full spell of UI benefits with probability h once they find a job.

As in the standard DMP model, wages are determined by Nash Bargaining with the worker bargaining share α. Vacancies are determined endogenously by the condition that the flow cost, c, is equal to the expected discounted profit of opening a vacancy. Appendix D describes the model in more detail, shows the value functions for workers and firms, and defines the stationary equilibrium.

Calibration. We calibrate a number of parameters of our model to standard values from the literature, but others to match statistics we estimate for our short duration sample. Table D.1 in the appendix summarizes all the calibrated parameter values, while Table D.2 shows the targeted moments from our sample and corresponding moments from the calibrated model. We calibrate the

[56]Note that $u(0)$ includes the following categories of workers: 1) those who have exhausted UI benefits, 2) those who are not eligible for UI benefits and 3) those who decide not to claim UI benefits.

model at the monthly frequency with a discount factor of $\beta = 0.996$. In line with Shimer (2005), we assume a Cobb-Douglas matching function of the form $M = S^{0.72}v^{0.28}$ and set the worker's bargaining share to $\alpha = 0.72$. Following Hall and Milgrom (2008), we calibrate the average flow value of unemployment as $E(b_{UI} + b_L)/p = 0.71$. We calibrate $b_{UI} = \$1027$ (in December 2007 dollars) to match the average in our sample period. We set $p = \frac{b_{UI}}{0.35}$ to match a 35% UI replacement rate.[57] The UI re-qualification probability is set to $h = 1/6$, in line with the 6 months it typically take to requalify for UI benefits in the United States. We normalize the flow cost of posting the vacancy, c, to match $\theta = 1$. We choose the separation rate δ to match the E-to-U transition rate of 1.63% in our short duration sample, and the home production shock, ι, to match the unemployment rate of 6.8% in our sample. We choose ρ to match the labor force participation rate (LFPR) of 66.0% in our sample. We assume that UI take-up costs follow the uniform distribution, but are censored at 0. The mean is chosen to match the insured unemployment rate of 2.5% in our sample and the range is chosen to match the estimated macro response of the UI recipiency rate to a 3-month extension in our data. The search cost function is assumed to take the following shape, $c(s) = \kappa s^{1+\frac{1}{\gamma}}$, where we choose κ to match the average job-finding rate of 26.7% in our sample. Finally, we choose $\gamma = 0.43$ to target prior empirical estimates of the micro-elasticity of unemployment duration to potential benefit duration (see Appendix D for details).

Results. Table 5 presents the effects of UI extensions on the steady-state unemployment rate and insured unemployment rate. We report these effects for two different values of baseline potential benefit duration, $T = 8$ months and $T = 17$ months, i.e., the average baseline potential benefit duration in our sample for the short and long duration samples respectively. For these cases, we present both the "micro" and "macro" effects of UI extensions. The micro effects hold labor market tightness constant. The macro effect is the full steady-state response including the general equilibrium effects that operate through labor market tightness.

The model is calibrated to match prior estimates of the micro-elasticity of unemployment duration. It is therefore no surprise that the model yields a substantial micro effect on the unemployment rate. The effect in the model at short durations is 0.30, which is close to our estimate in the data (0.28). The model captures the fact that the insured unemployment rate responds more to UI extensions than the total unemployment rate. The micro response for the insured unemployment

[57] This corresponds to the after-tax UI replacement rate as estimated by Anderson and Meyer (1997). Note also that wages are close to productivity and thus the replacement rate in terms of wages is very close the replacement rate in terms of productivity.

Table 5: Responses to 3-Month UI Extension, By Months of Initial UI Duration (T)

	Data		Model (Micro)		Model (Macro)	
Variable	$T = 8$	$T = 17$	$T = 8$	$T = 17$	$T = 8$	$T = 17$
Unemployment	0.28	-0.03	0.30	0.22	0.43	0.32
Insured unemployment	0.60	0.25	0.47	0.27	0.51	0.31

NOTE. The table shows the responses in percentage points to a 3-month increase in the potential duration of UI benefits. Columns 1 and 2 show our empirical estimates for the short and long duration sample. Columns 3 and 4 show the microeconomic responses in our model at initial UI duration of 8 and 17 months. The microeconomic effect is defined as the change in the variable in the model when holding labor market tightness constant. Columns 5 and 6 show the full—i.e., macroeconomic—effects in the model by initial UI duration.

rate is 0.47 in the model.

The model yields a smaller micro effect on unemployment at long durations than at short durations: 0.22 at long durations compared to 0.30 at short durations. The model-implied difference between the effect on the insured unemployment rate at long and short durations is larger (0.27 versus 0.47) and matches the difference seen in the data reasonably well. For the overall unemployment rate, however, the model does not match the full magnitude the difference between the short duration effect and the long duration effect seen in the data (0.28 versus -0.03).

In the model, most unemployed job seekers do not expect to stay unemployed long enough to make use of these UI extensions at long baseline durations. This implies that for newly unemployed job seekers, the response of job search and thus job finding to a 3-month extension is smaller at long versus short baseline duration (see Figure D.1 in the appendix). This force, however, is not strong enough to explain the difference we see in the data between the effects on unemployment at short and long durations.

The partial equilibrium results in the middle two columns of Table 5 leave little room for general equilibrium effects operating through reduced vacancy creation to improve the model fit. The partial equilibrium effects are already large enough to explain our estimates of the effects of UI extensions on the unemployment rate. Adding negative vacancy creation effects makes the unemployment effects larger and brings the effects on the total and insured unemployment rates closer together. This worsens the fit of the model to the data.[58]

Most of the observations in the pre-Covid long-duration sample come from the Great Recession. Special features of the Great Recession may have been important in explaining the small estimated effects of UI extensions in this period, beyond the exceptionally long duration of benefits.

[58]Table D.4 in the appendix provides results for an alternative calibration that implies smaller general equilibrium effects, more in line with the data.

Rothstein (2011) emphasizes considerable uncertainty surrounding the expiration of extensions during the Great Recession period (benefits actually did lapse during several periods in 2010). Several authors have emphasized that job search may be less effective during recessions and this may be particularly important during deep recessions like the Great Recession. Finally, though the standard errors are too large to draw any strong conclusions, our estimates suggest that labor force participation responded negatively to extensions at long baseline durations (see appendix Table A.5). This might also help explain the disconnect between the effects on the insured and total unemployment rates for the long-duration sample.

6.3 Aggregate Demand Effects

Debates about UI often emphasize its role in providing fiscal stimulus and acting as an "automatic stabilizer" during recessions. UI provides transfers to the unemployed, who are likely to have particularly high marginal propensities to consume. Our results in section 4 show that the magnitude of the transfers associated with UI extensions at the margin are 7 dollars per capita per month at short durations, and 3 dollars per capita for extensions that occur at long durations. A back-of-the-envelope calculation of short-run aggregate demand effects would multiply these dollar amounts by the marginal propensity to consume and an estimate of the fiscal multiplier. This implies numbers close to zero for UI extensions at long durations, and modest effects at short durations.[59]

The fiscal stimulus effects of the first weeks of UI benefits are potentially much larger than for UI extensions, for the simple reason that the short-term unemployed are much more numerous than those that are unemployed for 40 weeks, let alone 80 weeks.[60] The same is true of increases in UI benefit payments of the type that occurred with some Covid-era programs studied by Ganong et al. (2023). In other words, our estimates are local average treatment effects, as are others in the empirical literature. This is particularly important for studies using variation from the Great

[59]Similarly, Kekre (2021) finds that the unemployment rate would have been 0.4 percentage points higher without the 73 weeks of UI extensions during the Great Recession. In Kekre's model (which assumes a smaller micro disincentive effect than our calibration), UI payments stimulate aggregate demand through increased spending (as in the back-of-the-envelope calculation above), as well as general equilibrium effects associated with wage inflation at the zero lower bound. See also the general equilibrium models of Christiano et al. (2016) and Gorn and Trigari (2023) for estimates of the effect of UI on unemployment, although it is more difficult to compare their results to ours because they model changes in UI generosity as changes in the UI benefit level and the UI coverage rate respectively.

[60]In a model with hand-to-mouth consumers one would expect that the fiscal stimulus effects of UI scale with the increase in UI dollars spent per capita, which is strongly decreasing in the baseline duration ($7 vs. $2 per capita increase in the short vs. the long duration sample). The micro disincentive effects of UI should scale to a lesser degree with the baseline duration due to anticipatory effects of UI extensions on job search (i.e., unemployed workers' search effort should respond to an extension even if they never end up claiming the extended UI benefits).

Recession, since the relevant quasi-experimental variation in these studies often occurs at long horizons.

7 Conclusion

This paper studies the macroeconomic effects of UI extensions in the United States over the last five decades. While there is a large and established literature that credibly identifies the "micro" effects of UI generosity on individual-level job finding, relatively few studies estimate the effects of UI extensions on aggregate labor market outcomes using quasi-experimental methods. Understanding the macroeconomic effects of UI extensions is, however, crucial in evaluating policy debates about whether to extend UI during recessions.

Our paper contributes to this debate. We develop and implement a novel identification strategy that addresses the endogeneity of extensions to local labor market conditions by exploiting variation in the adoption of optional UI trigger rules across states. To implement this strategy, we develop a "UI Benefits Calculator" from legislative sources on state-level UI trigger rules, and combine it with a real-time dataset on labor market variables for the period 1976 to 2022. We find that the macroeconomic effects of UI extensions on the unemployment rate are minimal at times when UI durations are already long, such as in the Great Recession. However, these effects are substantial when initial UI benefit durations are short. A simple intuition for this divergence is that extensions at long horizons affect relatively few people.

We compare our findings to the predictions of a calibrated general equilibrium search-and-matching model. The model can generate the effects of UI extensions on the unemployment rate we observe in the data through microeconomic disincentive effects alone, without any amplifying general equilibrium effects that operate through vacancy creation. The model can also explain smaller effects of UI extensions at long durations. However, it cannot explain *how low* we estimate the effects of UI extensions to be in periods such as the Great Recession. In principle, aggregate demand effects might explain the differences, but UI extensions have a very small effect on dollars of UI per capita at long durations, suggesting small aggregate demand effects even for large multipliers. Other potential culprits are reduced search efficiency during the Great Recession, the considerable political uncertainty about the continuation of the UI extensions during this period, and the effect of UI extensions on labor force participation. We leave these questions for future research.

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Online Appendix

A.1 Trigger Rules

The rules governing whether a state can extend UI duration under federal programs are enormously complex. Table A.1 shows the trigger rules for all programs since the beginning of our sample in 1976. These rules represent the thresholds that states' labor market indicators must surpass in order to "trigger on" to federal UI benefit programs. There is a subtle distinction between "triggering on" to a program and actually paying out benefits under that program. Technically, a state's "payable status" begins three weeks after a state's trigger variable surpasses its threshold. For the TUR (a monthly variable) this is three weeks after state unemployment data is released; for the IUR this is three weeks after the state's IUR surpasses a threshold.[61] A state must then pay these additional benefits for at least thirteen weeks (unless a state becomes eligible to pay even more weeks of benefits). A state stops paying benefits when either this thirteen-week period ends, or four weeks after the state drops below the threshold—whichever comes second. The analysis in the main text is all based on payable status, i.e., when benefits start being paid. By averaging over a quarter, the contemporaneous quarterly effects that we report should be relatively insensitive to the three week delay between "triggering" and "paying."

Table A.1: Trigger Rules for Federal UI Extension Programs

Rule Type	Rule Description	Effective Years
	Extended Benefits (1970–)	
	13 Weeks	
Mandatory	IUR MA $\geq$ 4% and IUR Lookback $\geq$ 120%	1970–1971, 1981–1982
Mandatory	(IUR MA $\geq$ 4% and IUR Lookback $\geq$ 120%) or National IUR $\geq$ 4.5%	1972–1981
Optional	IUR MA $\geq$ 5%	1976–1982
Mandatory	IUR MA $\geq$ 5% and IUR Lookback $\geq$ 120%	1982–
Optional	IUR MA $\geq$ 6%	1982–
Optional	IUR MA $\geq$ 5% and 3-year IUR Lookback $\geq$ 120%	2011-2013
Optional	TUR MA $\geq$ 6.5% and 1- or 2–year TUR Lookback $\geq$ 110%	1993–
Optional	TUR MA $\geq$ 6.5% and 1-, 2-, or 3-year TUR Lookback $\geq$ 110%	2011-2013
	7 Additional Weeks	
Optional	TUR MA $\geq$ 8.0% and 1- or 2–year TUR Lookback $\geq$ 110%	1993–
Optional	TUR MA $\geq$ 8.0% and 1-, 2-, or 3-year TUR Lookback $\geq$ 110%	2011-2013

[61] Since IUR data is published with a three-week lag, knowledge of triggering coincides with the start of payments. For the TUR, payments start three weeks after knowledge of triggering.

Table A.1: Trigger Rules for Federal UI Extension Programs — Continued

Rule Type	Rule Description	Effective Years
Pandemic Emergency Unemployement Compensation and Pandemic Unemployment Assistance (2020–2021)		
PEUC: Available to those typically eligible for UI Weeks: 13 (3/20–12/20) 24 (12/20–3/21) 53 (3/21–9/21)		
Mandatory	Unconditional	3/20–9/21
PEUC: Expanded eligibility Weeks: 39 (3/20–12/20) 50 (12/20–3/21) 79 (3/21–9/21)		
Mandatory	Unconditional	3/20–9/21
Extended Unemployment Compensation (2008–2013)		
Tier I Weeks: 13 (2008) 20 (2008/2012) 14 (2012/2013)		
Mandatory	Unconditional	2008–2013
Tier II Additional weeks: 13 (2008/2009) 14 (2009/2013)		
Mandatory	IUR MA $\geq$ 4%	2008–2009
Mandatory	TUR MA $\geq$ 6%	2008–2009
Mandatory	Paying EB	2008–2009
Mandatory	Unconditional	2009–2012
Mandatory	TUR MA $\geq$ 6%	2012–2013
Tier III Additional weeks: 13 (2009/2012) 9 (2012/2013)		
Mandatory	IUR MA $\geq$ 4%	2009–2013
Mandatory	TUR MA $\geq$ 6%	2009–2012
Mandatory	TUR MA $\geq$ 7%	2012–2013
Tier IV Additional weeks: 6 (2009/2012) 10 (2012/2013) 16 (Mar.–Jun. 2012, not paying EB)		
Mandatory	IUR MA $\geq$ 6%	2009–2013
Mandatory	TUR MA $\geq$ 8.5%	2009–2012
Mandatory	TUR MA $\geq$ 9%	2012–2013
Temporary Extended Unemployment Compensation (2002–2004)		
TEUC Weeks: 13		
Mandatory	Unconditional	2002–2004
TEUC-X Additional weeks: 13		
Mandatory	Paying EB	2002–2004
Mandatory	IUR MA $\geq$ 4% and IUR Lookback $\geq$ 120%	2002–2004
Extended Unemployment Compensation (1991–1994) *Note: states could opt out of EB and pay EUC instead*		
Tier I Weeks: 13 (11/91–02/92) 20 (02/92–06/92) 13 (06/92–07/92) 7 (07/92–01/94) 3 additional if nat. TUR $\geq$ 6.8, 10 additional if nat. TUR $\geq$ 7 (07/92–10/93)		
Mandatory	Unconditional	1991–1994
Tier II Additional Weeks: 7 (11/91–02/92) 13 (02/92–06/92) 7 (06/92–07/92) 6 (07/92–01/94) 2 additional if nat. TUR$\geq$ 6.8, 11 additional if nat. TUR$\geq$ 7 (07/92–10/93)		
Mandatory	AIUR MA $\geq$ 5% or MTUR MA $\geq$ 9	1991–1994

Table A.1: Trigger Rules for Federal UI Extension Programs — Continued

Rule Type	Rule Description	Effective Years
Federal Supplemental Compensation (1982–1985)		
Reachback Tier		
Weeks: 10 (9/82–1/83) 14 (1/83–4/83)		
Mandatory	EB at some point since 6/82	1982–1983
Tier I		
Weeks: 6 (9/82–1/83) 8 (1/83–3/85)		
Mandatory	Not eligible for reachback	1982–1983
Mandatory	Unconditional	1983–1985
Tier II		
Additional weeks: 2		
Mandatory	IUR MA $\geq$ 3.5%; not eligible for reachback	1982–1983
Mandatory	IUR MA $\geq$ 4%	1983
Mandatory	IUR MA $\geq$ 4% or LIUR $\geq$ 4%	1983–1985
Tier III		
Additional weeks: 2 (1/83–3/85)		
Mandatory	IUR MA $\geq$ 4.5%; not eligible for reachback	1983
Mandatory	IUR MA $\geq$ 5%	1983
Mandatory	IUR MA $\geq$ 5% or LIUR $\geq$ 4.5%	1983–1985
Tier IV		
Additional weeks: 4 (1/83–4/83) 2 (4/83–3/85)		
Mandatory	IUR MA $\geq$ 6%; not eligible for reachback	1983
Mandatory	IUR MA $\geq$ 6%	1983
Mandatory	IUR MA $\geq$ 6% or LIUR $\geq$ 5.5	1983–1985
Federal Supplemental Benefits (1975–1978)		
Tier I		
Weeks: 13		
Mandatory	Paying EB	1975
Mandatory	Paying EB and IUR MA $\geq$ 5%	1975–1978
Tier II		
Additional weeks: 13 (1975–1978)		
Mandatory	Paying EB and IUR MA $\geq$ 6%	1975–1977

NOTE. This table summarizes all triggers for UI duration extension programs. It thus excludes the pandemic programs that provided increased payments for UI recipients (e.g., the $600 weekly payments made under the Federal Pandemic Unemployment Compensation program). Within each tier of each program (e.g., 13 week EB, Tier II of EUC 2008), a state can trigger based on *any* of the mandatory triggers in place, and any optional trigger it has in place. That is, a state can trigger on for surpassing an optional threshold but not a mandatory one. IUR MA stands for the thirteen-week moving-average of the state's insured unemployment rate, and TUR MA is the three-month moving-average of the state's total unemployment rate. The IUR lookback is the current IUR MA divided by the average IUR MA in the same week in the previous two years, and the n-year TUR lookback is the ratio of the current TUR MA to it's value over the same months n years ago. The adjusted IUR (AIUR) was a thirteen-week moving average of a variable that is similar to the IUR (continued claims/labor force), except that the number of people who have recently exhausted benefits is added to the numerator. The mean TUR (MTUR) was the 6-month moving average of a state's unemployment rate. Different states used different averaging periods: "direct use" states used unemployment rates taken directly from the CPS, while "non-direct use" used unemployment rates that had been statistically adjusted by the BLS. Effectively this meant that direct-use states used more up-to-date data. The long-term IUR (LIUR) was the average of weekly IURs starting after January 1, 1983 and running through "the last week of the second calendar quarter ending before" the current week. The "national TUR" used in the determination of the 1990s EUC program was, from 7/92–7/93, the two-month moving average of the national TUR. From 7/93–10/93, it was the maximum of the two most-recent months of the national TUR.

A.2 Sources of Changes in Trigger Rules

Table A.2: Sources of Changes in Trigger Rules

Description	Optional Rules				
	TUR	IUR	TUR 3-Y LB	Total	Percent
Federal Funding	104	0	68	172	85
ARRA (2009)	*51*	*0*	*68*	*119*	*59*
General (2009+)	*22*	*0*	*0*	*22*	*11*
Fam. First (2020)	*29*	*0*	*0*	*29*	*14*
TEUC (2003)	*2*	*0*	*0*	*2*	*1*
Reagan Reforms	0	6	0	6	3
Option Creation	7	0	0	7	3
Discretionary	8	9	0	17	8
Total	119	15	68	202	100

NOTE. This table shows our breakdown of the dominant policy motive behind each change in trigger rules since 1981. The rows labeled "federal funding" denote changes that were made as a response to increased federal funding for the EB program. That row is further split by the exact program that provided funding: "ARRA" represents states that tied their option to full federal funding provided by the American Recovery and Reinvestment Act; "General" represents states that tied their option to federal funding, without reference to ARRA; "Fam. First" represents states that tied their option to federal funding under the Families First Coronavirus Response Act; and TEUC represents states that adopted an option during the period of the 2002–2004 TEUC program. The row "Reagan reforms" represents changes in October 1982, and "option creation" represents adoptions following the 1993 creation of the TUR option. The row "discretionary" contains switches that do not neatly fall into another category. The columns represent the different options (TUR option, IUR option, and the 3-year TUR lookback option) and summary statistics of the counts.

A.3 Data Sources

Potential Benefit Duration is the maximum total number of weeks of benefits available in a state in a particular week. The sources for this variable are discussed in section 3.2.

The **Insured Unemployment Rate** is constructed as the number of UI recipients divided by the labor force. To count the number UI recipients, we take the "All weeks compensated: Number" variable from the tables for regular state UI (ar5159; column c38), EB (ae5159; c29), the 1991–1994 EUC program (ac5159; c32), the 2000–2002 TEUC program (at5159; c29), the 2008-2013 EUC program (au5159; c29+c52+c68+c84), and the pandemic programs: PEUC (ap5159; c29), MEUC (902m; c5), and PUA (902p; c5).[62] Data for the 1982–1985 FSC program are unavailable. This means that we undercount federal recipients before 1985. Together, these data give the number of weeks of benefits claimed in a month. We divide this by 4.33 to arrive at (a floor of) the number

[62]These are available for download at https://oui.doleta.gov/unemploy/DataDownloads.asp. They are subject to minor revisions and corrections. We last downloaded these data on May 8, 2023.

of UI recipients in a month.[63] Denote this variable by $\text{UI}_{s,t}$. The *insured unemployment rate* is $\text{UI}_{s,t}/\text{LF}^n_{s,t}$, where $\text{LF}^n_{s,t}$ is the non-seasonally-adjusted number of people in the labor force, taken from the LAUS. The *non-mechanical component* of the insured unemployment rate uses only state UI recipients (ar5159) in the numerator. We last pulled the LAUS on May 9, 2023.

The **Unemployment Rate** is seasonally adjusted, and taken from the LAUS.

The **UI Recipiency Rate** is the ratio of $\text{UI}_{s,t}$ to the unemployment rate.

We constructed the **Real-time Unemployment Rate** series from two sources. For data since 1994, we scraped the table "Civilian labor force and unemployment by state, not seasonally adjusted" from the online archive of LAUS news releases, available at https://www.bls.gov/bls/news-release/laus.htm. Prior to 1994, we digitized the tables titled "Labor force and unemployment by State and selected areas" in the BLS' monthly publication "Employment and Earnings." We downloaded the bulk of these files from FRASER (https://fraser.stlouisfed.org/title/60), and filled in any missing or poorly-digitized pages from other online archives available through HathiTrust.

We construct the **Employment Rate** using the number of employed people ("all employees") from the BLS' Current Employment Statistics establishment survey, which we downloaded May 12, 2023. The denominator is the seasonally-adjusted labor force from the LAUS.

To construct **Dollars Spent per Capita** we take the "All weeks compensated: amount" variable from the same forms we used to count the number of UI recipients. (See discussion of "insured unemployment rate" above.) The specific variable for regular state UI is c45; c35 for EB; c35, c57, c73, and c89 for EUC 2008; c35 for TEUC; c35 for PEUC; c39 for EUC 91–94; c6 for PUA; and c7 for MEUC. We divide the sum of all these variables by the non-seasonally-adjusted population variable from the LAUS. We then convert the resulting variable into December 2007 dollars using the PCE deflator (`PCEPI` from FRED, last downloaded May 9, 2023).

New UI Claims is the ratio of initial claims (c51 of report ar5159, mentioned above) to $\text{LF}^n_{s,t}$.

We construct controls for state-level **Industry Employment Shares** following the construction of Guren et al. (2021). Specifically, we calculate the share of employment in each state-month in the following sectors: real estate (SIC H65, NAICS 53), construction (SIC C, NAICS 23), manufacturing (SIC D, NAICS 31–33), and retail trade (SIC G, NAICS 44-45). A small fraction of observations are missing or set to zero in the raw industry-level and aggregate level—we linearly interpolate between missing values. In our regressions, we allow the coefficients on these shares to change

[63] If some UI recipients in a month do not claim during all weeks of the month, the number of UI recipients will be higher than this floor.

every half decade (i.e., in 1985, 1990, etc.). We downloaded this data on June 2, 2023.

To measure **Unemployment Spell Durations** we take the variable `DURUNEMP`, weighted by `WTFINL`, from the monthly survey of the Current Population Survey (CPS), downloaded from IPUMS (Flood et al., 2021) on April 20, 2023. That is also the source of our data for the **CPS State-Level Unemployment and Job-Finding Rates**, where the latter are defined as the fraction of unemployed workers who were employed in the following interview (among all individuals who were matched across two consecutive monthly interviews). We downloaded this May 4, 2023.

We downloaded the **national unemployment rate** and **national real GDP** from FRED (mnemonics `UNRATE` and `GDPC1`, respectively) on October 30, 2023.

A.4 Sources of Variation in $\widehat{W}_{s,t}$

In Table A.3, we tabulate all of the values that $\widehat{W}_{s,t}$ takes. Of the 7,349 observations in our sample with non-zero values, the vast marjority have the "standard" values of either 13 (state qualified for the first tier of EB because of an optional rule), 20 (the state qualified for both tiers because of an optional rule), or 7 (the state qualified for the first tier under a mandatory rule, and the second because of an optional rule). Another 203 observations have non-standard values because of the fact that weeks provided under EB are a function of a state's regular level of UI benefits. 276 observations have non-standard values because of interactions of EB with other federal programs. Eight observations have negative values, owing to the 13-week rule.

While we have no reason to suspect that these non-standard values of $\widehat{W}_{s,t}$ induce endogenous variation, panel (b) in Table A.5 presents estimates that eliminate this non-standard variation. The estimates from this specification are similar to those from our baseline specification.

In figure A.1, we plot the number of states in each week that have $\widehat{W}_{s,t} \neq 0$. Much of our identifying variation in the short-duration sample comes from the early 1980s and, to a lesser extent, the early 1990s and 2000s. Indeed, in pabel (q) of Table A.5, we show that our results are similar in the short-duration sample when using pre-2007 data.

Table A.3: Tabulating all Variation in $\widehat{W}_{s,t}$

Category	# Obs.	Value of $\widehat{W}_{s,t}$	Explanation
Total	7329	–	–
"Normal" values	6896	7, 13, or 20	Extension can make state eligible for 13 weeks or 20 weeks (and 7 = 20 – 13). Recall: EB20 was created with the TUR option in 1993.
EUC Tier IV	149	10	Between March and May 2012, Tier IV of the EUC program provided 6 weeks if the state was paying EB, and 16 if it was not. Thus, states with options on that triggered on 20-week EB and Tier IV of EUC paid 26 weeks combined, while states that qualified for 20-week EB (but did not have options on) and triggered on Tier IV of the EUC program paid 16 ($= 26 - 10$) weeks.
TEUC	170	26 or 33	One of the TEUC-X (2002-4) triggers was that a state was paying EB. So, when a state triggered on to EB (13 or 20 weeks) it also got an additional 13 weeks of TEUC
FSC "Reachback"	106	2, 4 or 15	From 9/82—1/83, states were eligible for 10 weeks of FSC benefits if they had paid EB at some point since 6/82 (even after the EB period ended). If they had not paid EB over that period but surpassed the (mandatory) FSC IUR threshold, they were eligible for 8 weeks. If they had not paid EB over that period and did not surpass the (mandatory) FSC IUR threshold, they were eligible for 6 weeks. Thus, for a state that was actively paying EB only because of the option, and surpassed the mandatory threshold, 15 weeks are attributable to option status ($2 = 10-8$ from the FSC program, 13 from EB). Once that state's EB status ended, it would still remain eligible for the extra 2 FSC weeks. For a state that had paid EB previously and was not above the mandatory threshold, 4 weeks were available.
13-week rule	8	-13	In 1982, Oregon had a high IUR which briefly dipped below the optional IUR threshold (but above the mandatory one). It was then required to not pay EB benefits for 13 weeks. During that period, however, its IUR lookback surpassed its threshold, so the state qualified for EB under the mandatory rule. In this case, potential benefit duration (without options) was thus higher than actual PBD, and lead to a negative value of $\widehat{W}_{s,t}$. This accounts for 7 observations. The final observation comes from Washington in 1994, which had the TUR option in place, but not the IUR option. At the beginning of 1994, Washington was paying EB because its TUR was above the optional threshold. It stopped paying EB in February, when its TUR fellow below the threshold. Twelve weeks later, its IUR went above the mandatory threshold, but Washington could not start paying EB until one week later—thirteen weeks after it had stopped paying. During that week, then, it would have paid 13 weeks of EB without the IUR option, but paid 0 weeks with the option, leading to a negative value of $\widehat{W}_{s,t}$.

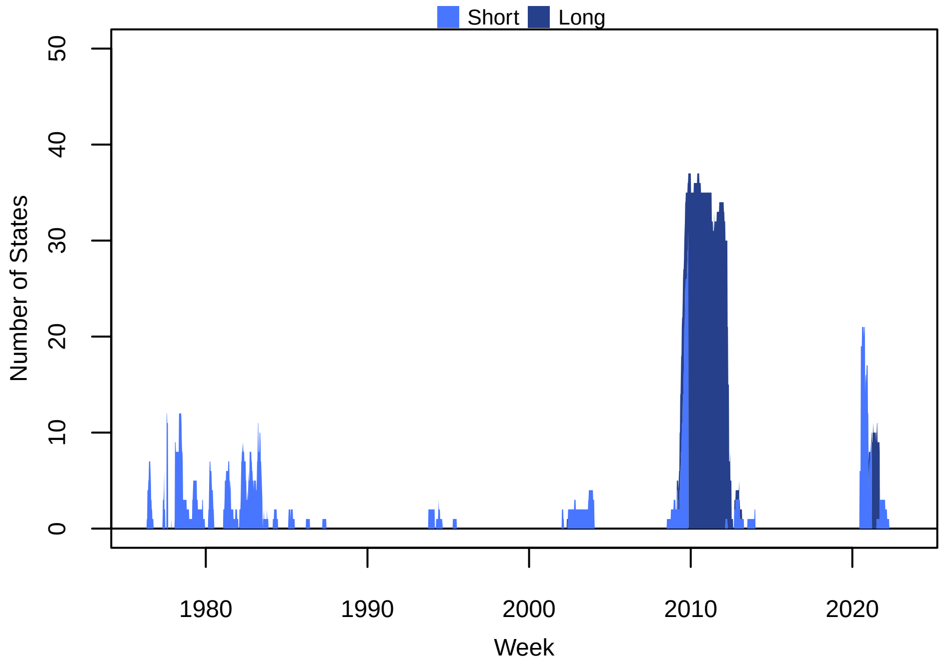


Figure A.1: Number of States with $\widehat{W}_{s,t} \neq 0$

NOTE. This figure reports the number of states in each week that have $\widehat{W}_{s,t} \neq 0$. The light- and dark-shaded regions split this count by whether the state is in the short- or long-duration sample, respectively

A.5 Tight Sample

Table A.4 presents results for a case where we restrict the sample to only include state-time observations for which the choice of options was pivotal in determining whether a state offered extended benefits. In other words, we drop observations for which $\widehat{W}_{s,t}$ is invariant to option status: they would *always* trigger or *never* trigger regardless of option status. This excludes both economic expansions—when states were far from qualifying under any trigger rule (the majority of the sample)—and periods of bad economic downturns—when states would have qualified regardless of their options. We refer to this sample as the "tight sample." An additional step we take to make the tight sample more homogeneous is that we restrict attention to cases where a treated state-week (i.e., a state-week with $\widehat{W}_{s,t} > 0$) within the tight sample has an untreated state-week (i.e., a state-week with $\widehat{W}_{s,t} = 0$) within the tight sample within 2 years (and vice versa).

For Arkansas, the "tight sample" is the gray area in Figure 4. In all other time periods—when Arkansas is sufficiently far from recession—the state's choice of options are irrelevant to whether it receives extended benefits, since it is far from qualifying. The same is true in the worst part of the recession (the blue area in Figure 4), when Arkansas qualifies under the relatively strict mandatory trigger rules.

Table A.4: Tight Sample Results

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.59	1.50	0.63	0.65
	(0.13)	(0.36)	(0.15)	(0.21)
$\mathbf{W}_{s,t}$, Long Dur.	0.13	0.31	-0.03	0.05
	(0.17)	(0.25)	(0.14)	(0.22)
Observations	8796	10658	8796	10658

Table A.4 shows that the results for the tight sample turn out to be broadly similar to our baseline results, although less precisely estimated. The main potential advantage of the tight sample specification is that it allows for additional heterogeneity. It refrains from pooling parameters (e.g., state fixed effects) over highly heterogeneous periods such as expansions and recessions. In other words, the sample is selected as one in which economic conditions are more homogeneous. A downside, however, is that it requires the "auxiliary" parameters such as state fixed effects to be estimated off of a much shorter time series.

A.6 Identification in a Simple Example

Consider a simple setting with a single optional threshold for extended benefits: a state pays extended benefits if it has opted in, and if its unemployment rate is above a threshold τ. Define the "fundamental" unemployment rate of a particular state in a particular week to be $\tilde{u}$. The fundamental unemployment rate is the unemployment rate that prevails when the state does not have extended benefits. We denote the cumulative distribution function of $\tilde{u}$ by F. Suppose for simplicity that a state's option status $\mathcal{O}$ is drawn from a Bernoulli distribution:

$$\mathcal{O} = \begin{cases} \text{opt in} & \text{with probability } \psi \\ \text{opt out} & \text{with probability } 1-\psi. \end{cases}$$

We then define an indicator for whether a state receives an extension: $w = \mathbf{1}\{\tilde{u} \geq \tau\}\mathbf{1}\{\mathcal{O} = \text{opt in}\}$.

Suppose the effect of extended benefits on unemployment is δ. For simplicity, suppose that this effect has no dynamics: it comes into effect immediately when benefits are extended and dissipates immediately when extended benefits lapse. In this case, the unemployment rate is

$$u = \tilde{u} + \delta w.$$

We denote the average fundamental unemployment rate among states above the threshold as $\bar{u} \equiv \mathbb{E}[\tilde{u} \mid \tilde{u} \geq \tau]$ and the average unemployment rate among states below the threshold as $\underline{u} \equiv \mathbb{E}[\tilde{u} \mid \tilde{u} < \tau]$. Note that in this single-threshold setting, the indicator for benefit extension, w, coincides with the more-complicated regressor we use in our empirical analysis, $\hat{W}_{s,t}$.

The bias resulting from reverse causality in our setting can be seen by considering a regression of the unemployment rate on the extension indicator with no controls. In that case, the regression coefficient is given by

$$\begin{aligned}
\beta_{\mathbb{E}} &= \mathbb{E}[u \mid w = 1] - \mathbb{E}[u \mid w = 0] \\
&= \delta + \left(\frac{F(\tau)}{1 - \psi(1 - F(\tau))} \right) (\bar{u} - \underline{u}) \\
&= \delta + \left(\frac{\text{prob. below threshold}}{\text{prob. not treated}} \right) (\bar{u} - \underline{u})
\end{aligned}$$

The bias is positive. It arises because treated states—that need to have an unemployment rate above the threshold to be treated—are compared to *all* untreated states—including those with a fundamental unemployment rate below the threshold—rather than comparable untreated states—i.e., those with a fundamental unemployment rate above the threshold.

Our approach to removing this bias is to control for whether the fundamental unemployment rate of a state is above and below the threshold. We do this by including as a control a dummy variable for whether the state would have been treated had it opted into the program. The ideal such "qualifying control" is $q \equiv \mathbf{1}\{\tilde{u} \geq \tau\}$. However, a complication arises since $\tilde{u}$ is not observed. In practice, we must use the qualifying control $q \equiv \mathbf{1}\{u \geq \tau\}$. This potentially introduces a downward bias in our estimate of δ in a dynamic setting. We discuss this problem in more detail below. But for ease of exposition, let's start by assuming we can control for $q \equiv \mathbf{1}\{\tilde{u} \geq \tau\}$.

The "qualifying controls" approach consists of estimating

$$u = \alpha + \beta w + \gamma q + e. \tag{7}$$

The Frisch-Waugh-Lovell theorem implies that β from equation (7) is identical to β from

$$\mathbf{u} = \beta \mathbf{w} + \epsilon \tag{8}$$

where $\mathbf{u}$ and $\mathbf{w}$ are the residuals in the following regressions

$$u = a_u + b_u q + \mathbf{u} \qquad\qquad w = a_w + b_w q + \mathbf{w}.$$

The values of these can be calculated analytically. They are simply the values of u and w less their average in each group:

$$\mathbf{u} = \begin{cases} u - (\bar{u} + \psi\delta) & \text{if } \tilde{u} \geq \tau \\ u - \underline{u} & \text{if } \tilde{u} < \tau \end{cases} \qquad\qquad \mathbf{w} = \begin{cases} (w - \psi) & \text{if } \tilde{u} \geq \tau \\ 0 & \text{if } \tilde{u} < \tau \end{cases}$$

With these expressions and a bit of algebra, one can show that

$$\beta = \frac{\mathbb{E}[\mathbf{uw}]}{\mathbb{V}(\mathbf{w})} = \delta. \tag{9}$$

The bias is removed by subtracting it off via the inclusion of the q control.[64]

A.6.1 Dynamic Selection

In the discussion above, we made the simplifying assumption that we could observe $\tilde{u}$ and, thus, construct a qualifying control based on this variable. In reality, $\tilde{u}$ is unobserved and we must base our qualifying controls on u rather than $\tilde{u}$. The use of qualifying controls based on u can, however, introduce another form of bias in a dynamic setting: the effect of a UI extension can affect whether a state qualifies for extensions in the future. If UI extensions raise the unemployment rate, they make a state more likely to qualify in the future, e.g., they allow states to qualify for longer than they otherwise would. This can lead to a downward bias in estimates of the effect of UI extensions.

Consider Figure A.2. Here, we plot the evolution of the unemployment rate for two states that are initially on identical trajectories. One of these state has the option on (dashed red line), while

[64] In the example at the beginning of section 4, with two thresholds, the qualifying controls are $\mathbf{1}\{u \leq \tau_1\}$, $\mathbf{1}\{\tau_1 \leq u \leq \tau_2\}$, and $\mathbf{1}\{u \geq \tau_2\}$. In this case, the identifying variation continues to come only from differences in options. To see this, we need to define the variable that is our main treatment variable in the empirical analysis, $\hat{w}$. This variable is equal to actual potential benefit duration less the potential benefit duration a state would have had with no options in place. This variable is always 0 for state with $u \leq \tau_1$ and $u \geq \tau_2$. It is also always 0 for the state with the option off. For the state with the option on, this equals 13 when u is between τ_1 and τ_2. Now, suppose that no state adopts the option. In this case, $\hat{w}$ equals zero, so there is no variation in the right-hand side variable. If everyone adopts the option, then all variation in $\hat{w}$ is absorbed by the three qualifying controls. In the intermediate case where some states adopt the option and others do not, we can apply the Frisch-Waugh-Lowell theorem to obtain β by running an OLS regression on residualized u and $\hat{w}$. What is being taken out by the qualifying controls is the difference in u between states that did and did not receive the UI extension that arises from ex ante heterogeneity, not the treatment itself—i.e., reverse causality.

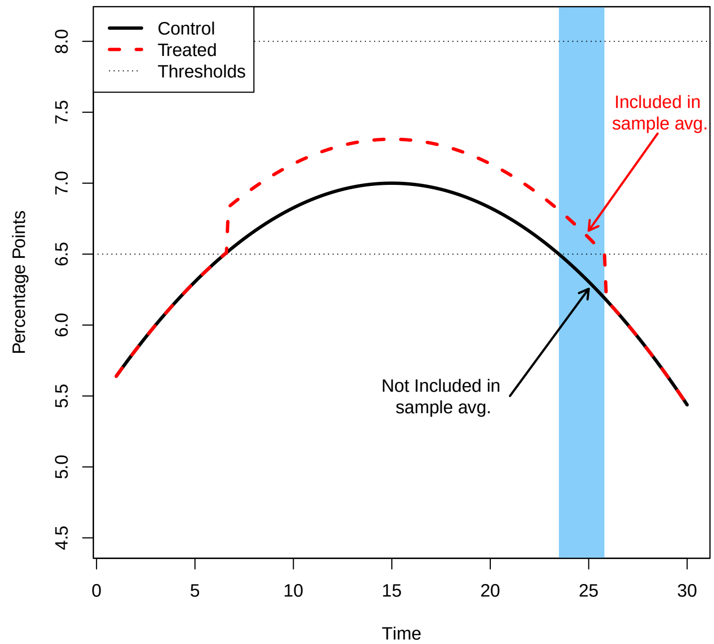


Figure A.2: Selection Effect

the other has the option off (solid black line). When the unemployment rate in these states crosses the threshold (6.5% in the figure), the state with the option on triggers on to extended benefits, while the other state does not. This leads the unemployment rate to rise by δ in the "treated" state relative to the "control" state. The fundamental unemployment rate (equal to the black line for both states) then continues to evolve, eventually falling enough that the treated state triggers off.

The dynamic selection bias arises from the time period that is shaded blue in the figure. At this time, the fundamental unemployment rate has fallen below the threshold. This implies that the unemployment rate in the control state is below the threshold. The unemployment rate in the treated state is, however, still above the threshold due to the treatment effect δ. If the qualifying controls are based on the unemployment rate, rather than the fundamental unemployment rate, the two states will be in different risk sets during the time period that is shaded blue: the treated state will be in the "above τ" risk set, while the control state will be in the "below τ" risk set.

An unbiased estimator of the treatment effect would compare the unemployment rate of the treated and control states while the *treated state* is above the threshold (i.e., same period for both treated and control states). If the qualifying control is based on the observed unemployment rate rather than the fundamental unemployment rate, a regression that controls for risk sets will, in contrast, compare the unemployment rate in the treated state while it is above the threshold (which includes the shaded blue time period) to the unemployment rate in the control state while *it* is above the threshold (which does not include the shaded blue time period). This results in a downward bias of the treatment effect since the low unemployment rate in the control state during the shaded blue time period is excluded from the comparison raising the average unemployment rate in the control state over the comparison period.

A simple way to construct bias-corrected estimates is to subtract the estimated treatment effect to the unemployment rate of untreated states and re-calculate the qualifying controls, with the inclusion of indicators for whether the observation *changes* qualifying status once the treatment effect has been subtracted. We have also confirmed that this adjustment yields an unbiased estimate in the more-realistic Monte Carlo exercise in Appendix A.6.2. We have also done this for our main empirical specification. Specifically, we estimate our baseline specification (equation (1)) for $h = 0$ for the variables used in the construction of the trigger variables (unemployment and insured unemployment). We then subtract the estimated treatment effect to these variables for untreated observations and re-calculate the qualifying controls and lagged controls. We then re-estimate the baseline specification with these modified controls. In practice, this makes little difference for our estimates. The short-duration contemporaneous quarterly effect on the unemployment rate increases from 0.28 to 0.31 (s.e. 0.14).

A.6.2 Monte Carlo

To complete the discussion, we now turn to the Monte Carlo exercises that allow us to assess whether the arguments from earlier in this appendix carry through to a more empirically realistic specification. We model each state's "fundamental" (pre-treatment) unemployment rate as

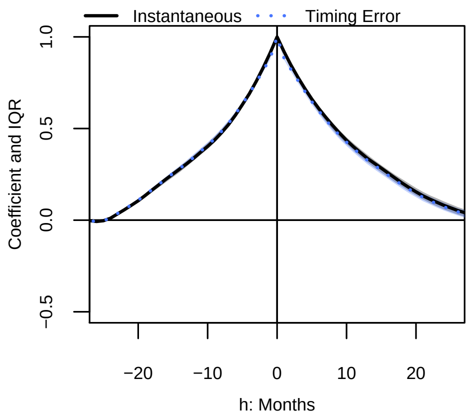
$$\tilde{u}_{s,t} = \alpha_s + \beta_s U_t + \varepsilon_{s,t} \tag{10a}$$

$$\varepsilon_{s,t} = \rho_s \varepsilon_{s,t-1} + \nu_{s,t} \tag{10b}$$

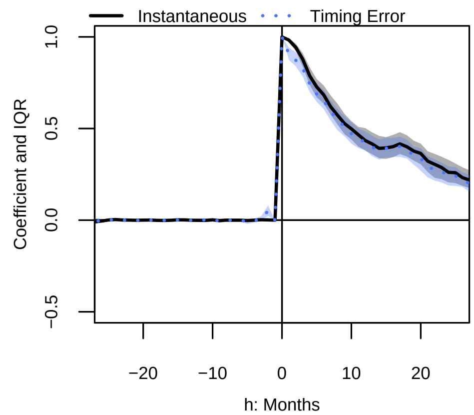
$$\nu_{s,t} \sim \mathcal{N}(0, \sigma_s^2) \tag{10c}$$

where U_t is the national unemployment rate, and we estimate the tuple $\theta_s \equiv (\alpha_s, \beta_s, \rho_s, \sigma_s)$ for each state by estimating equations (10a) and (10b) in our data. We hold U_t and θ_s constant and simulate 200 monthly panels of $\tilde{u}_{s,t}$ between 1976 and 2019 for all states by drawing values of $\nu_{s,t}$.

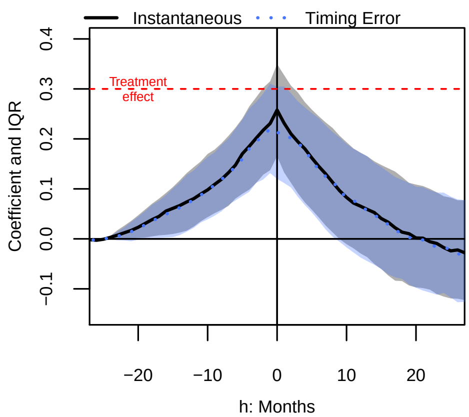
Each state's unemployment rate, $u_{s,t}$, equals $\tilde{u}_{s,t}$ plus a treatment effect, δ, if a UI extension has triggered in the state. There are two thresholds that determine trigger status: an optional



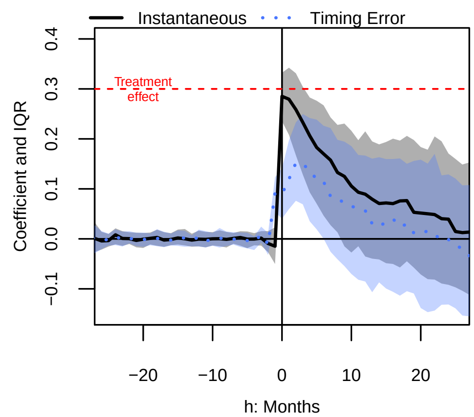
(a) Extension, Levels Spec.



(b) Extension, Timing Spec.



(c) Unemployment, Levels Spec.



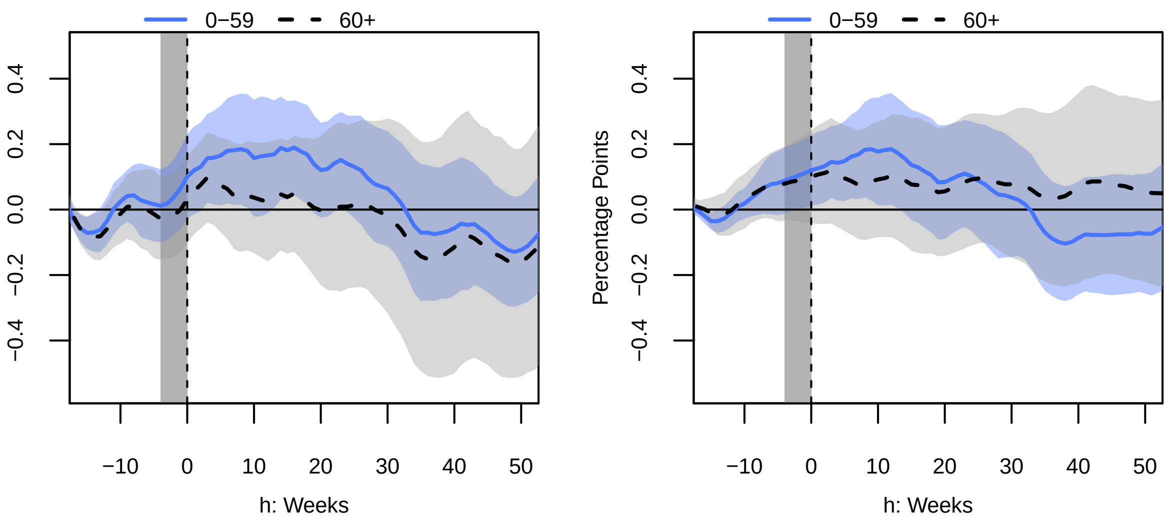
(d) Unemployment, Timing Spec.

Figure A.3: Monte Carlo Estimates

NOTE. Each figure panel shows the median and interquartile range across 200 simulations of β_h from either equation (1) (panels (a) and (c), labeled "Levels Spec.") or (4) (panels (b) and (d), labeled "Timing Spec."). The solid-black lines and shading represents the baseline calibration of our simulations, and the blue-dashed lines and shading represent the specification with timing error, where we have set $\ell = 3$. The red-dashed lines represent the true treatment effect.

threshold, τ_o, and a mandatory threshold, $\tau_m > \tau_o$, calibrated so that states surpass them with about the same frequency as they trigger on 13- and 20-week EB in our data, respectively. In each simulation, half of the states are randomly assigned the presence of the optional threshold. We say that a state is triggered on at time t, denoted by the indicator $w_{s,t}$ if either (1) $u_{s,t-\ell} \geq \tau_m$ or (2) the state has the optional threshold in place and $u_{s,t-\ell} \geq \tau_o$. In our baseline, $\ell = 1$. We also present results for a "timing error" calibration in which we draw ℓ randomly and uniformly from -3 to 3. Figure A.3 shows the results when we estimate the levels and shocks specifications in our simulations.

A.7 Real-Time vs. Revised Unemployment



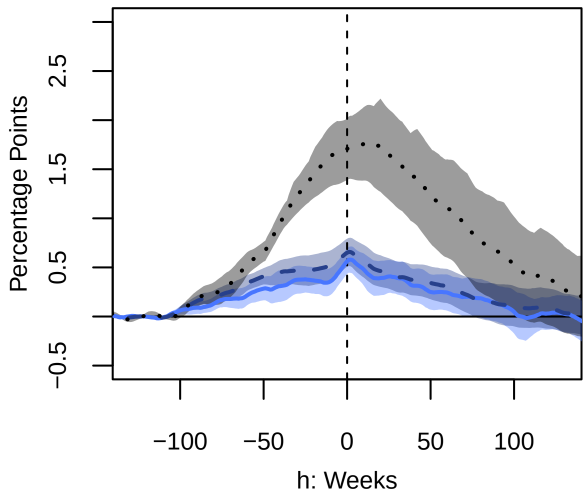
(a) Real-time

(b) Revised

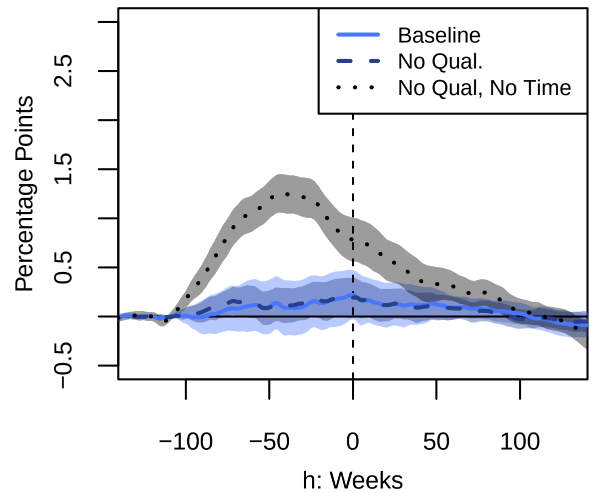
Figure A.4: Effects for Real-Time vs. Revised Unemployment Rates

NOTE. This figure comapares the estimated effects on the real-time and revised unemployment rates based on equation (4).

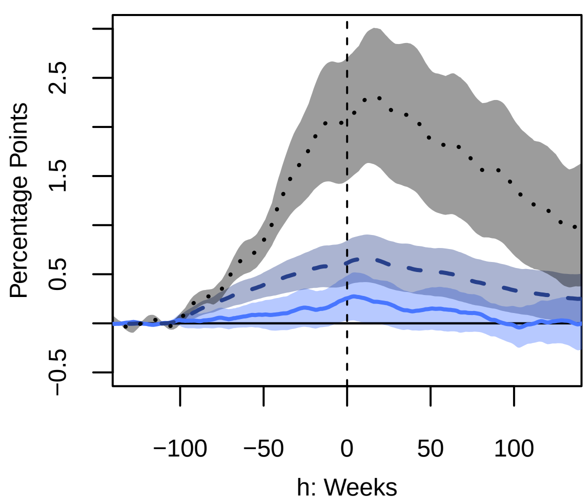
A.8 Role of Qualifying Controls



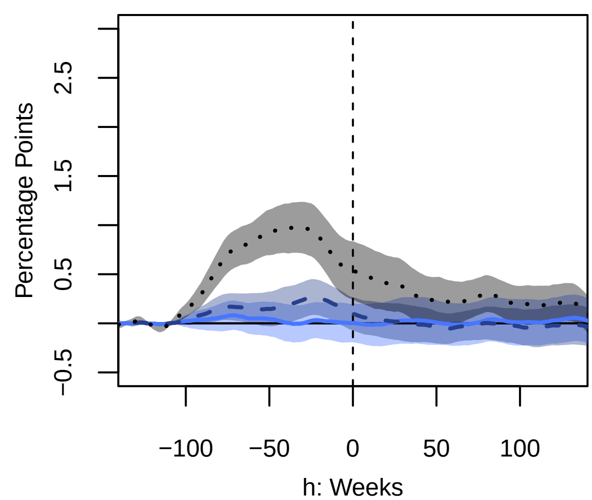
(a) Insured Unemp.: Short Duration



(b) Insured Unemp.: Long Duration



(c) Unemployment Rate: Short Duration



(d) Unemployment Rate: Long Duration

Figure A.5: Importance of Qualifying Controls and Time Fixed Effects

NOTE. Each panel shows OLS estimates of different versions of equation (1). Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in -150, \ldots, 150$. The sample runs from the beginnning of 1980 through 2019, and excludes Alaska. Standard errors are two-way clustered by state and week. The left panels show results for short (<60 weeks) baseline potential benefit duration, while the right panels show results for long (>60 weeks) baseline potential benefit duration. The lines labeled "baseline" are our baseline estimates. The lines labeled "No Qual." are based on a specification that does not have the qualifying controls but does have time fixed effects. The lines labeled "No Qual, No Time" are based on a specification that also removes time fixed effects.

A.9 Unemployment Duration

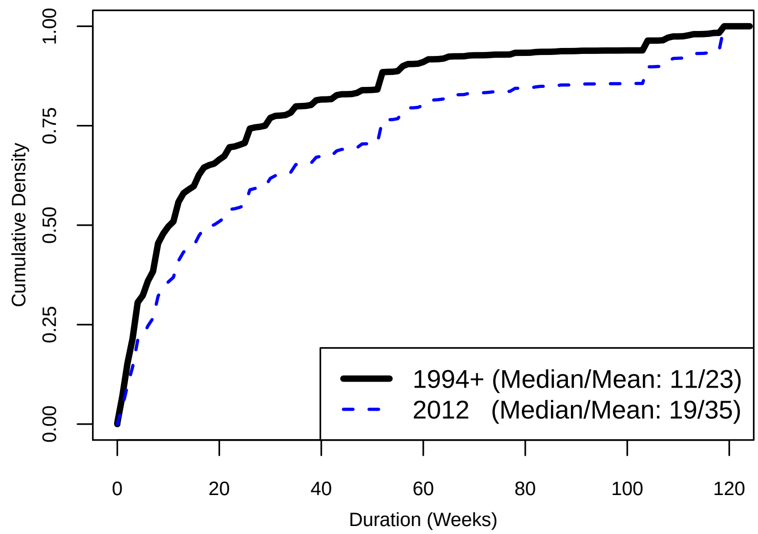


Figure A.6: CDF of Unemployment Spell Durations

NOTE. This figure shows the empirical cumulative distribution function of unemployment spell durations from the CPS, in weeks. The CPS duration mnemonic is `DURUNEMP`, which we weight by `WTFINL`. We retain unemployed individuals who are at least 16 years old. Our mean estimate is below the officailly-reported mean because the public-use microdata is top coded. This binds in 2012, when the mean was closer to 40 weeks. These are self-reported lengths of unemployment from the CPS. There are jumps in the distribution at 1 and 2 years (52 and 104 weeks), likely due to rounding. The sample covers 1994–2021.

A.10 Salience of UI

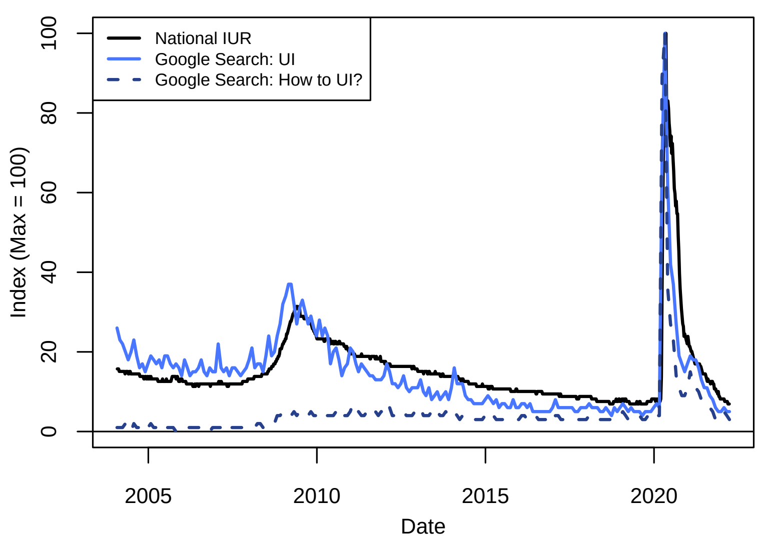


Figure A.7: Salience of UI

NOTE. This figure shows the national insured unemployment rate (`IURSA` from FRED), Google searches for "unemployment insurance," and Google searches for "how to file for unemployment." All series are normalized to have a maximum of 100 over the period shown. The Google data are from Google Trends. The query was run on May 9, 2023.

A.11 Baseline Potential Benefit Duration

Figure A.8 plots the distribution of baseline potential benefit duration across states.

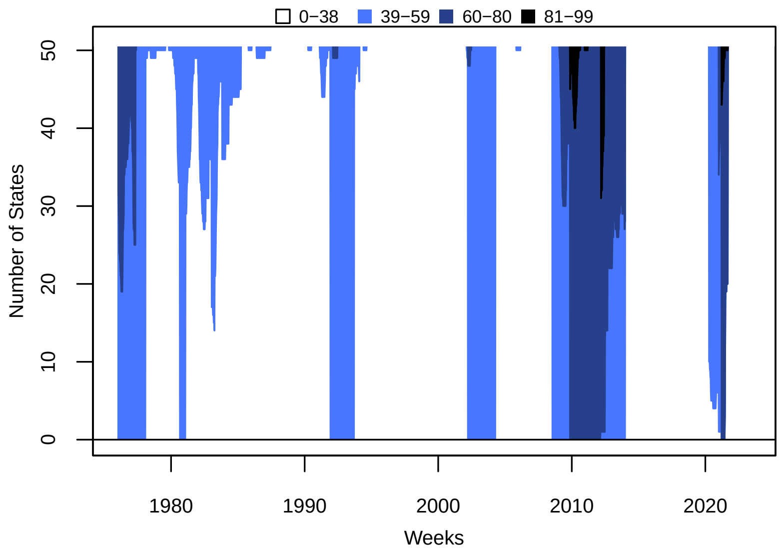


Figure A.8: Distribution of Baseline Potential Benefit Duration over States and Time

NOTE. This figure shows the number of states in each week in each bin of "baseline potential benefit duration," which we define as the potential benefit duration that a state would have paid if, all else equal, it had no optional trigger rules in place. This will differ from the potential benefit duration actually available in a state (shown in Figure 1) when UI in a state is extended only because it has an optional trigger rule in place.

A.12 UI Recipiency

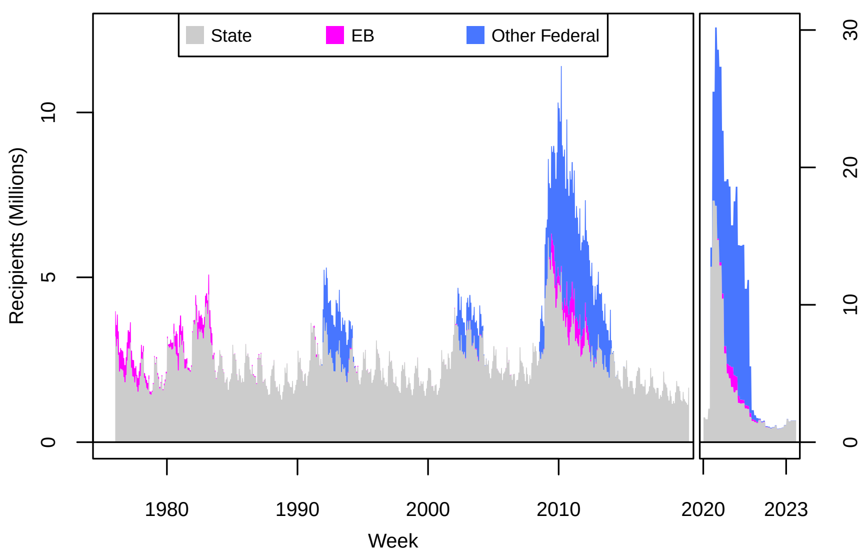
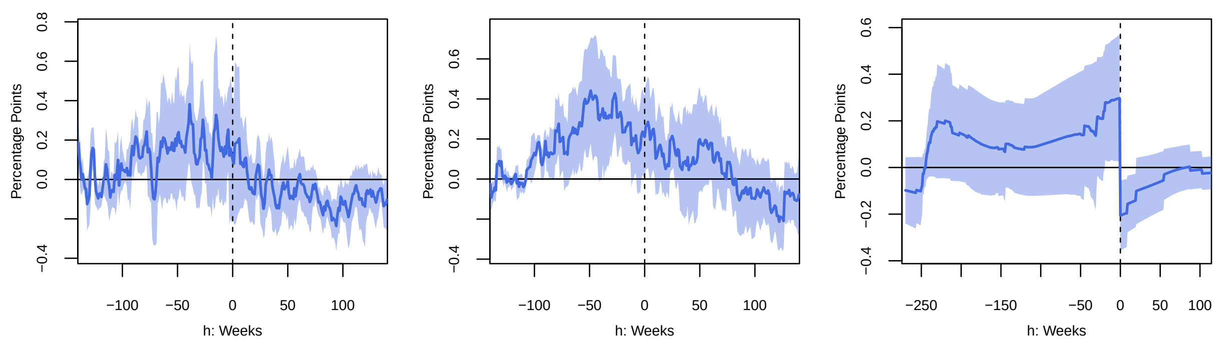


Figure A.9: UI Recipiency Rates

NOTE. This figure shows the ratio of UI recipients receiving either regular state UI—labelled "State"—or federal benefits (including EB)—labelled "Federal"—to the number of people counted as officially unemployed in the country.

A.13 Option Switches

Figure 5 focuses on option adoption. Here, we expand this exercise to include option termination. Panels (a) and (b) of Figure A.10 present results analogous to Figure 5 but where the main right-hand side variable is an indicator for whether a state *terminated* any option at time t. The results for the insured unemployment rate are a mirror image of the option-adoption results in Figure 5. The story is slightly different for the unemployment rate. In contrast to the case of option adoption, we would expect differences between the treated and control groups when looking at terminations since the treatment groups are going to be affected by the treatment effect of being treated. In order to terminate, the state must have adopted at an earlier date. So, states that terminate will have had longer UI in the period prior to termination. The positive coefficients in panel (b) may reflect these treatment effects in the pre-period.



(a) LHS: Insured Unemployment (b) LHS: Unemployment (c) LHS: TUR Option

Figure A.10: Option Terminations

NOTE. Panels (a) and (b) show the relationship between option termination and the insured unemployment rate and the unemployment rate, respectively, conditional on the same controls as we discuss for equation (1). In panel (c), the dependent variable, instead, is an indicator for TUR option status.

Correborating the argument above, panel (c) of Figure A.10 shows evidence that states that terminate options at t implemented said options in the past. We focus on the TUR option in the pre-Covid sample as our dependent variable, but similar results can be obtained for other options and time periods. The graph shows that option termination at t is correlated with TUR option status for roughly the past 5 years.

A.14 Contemporaneous Effects: Robustness

Table A.5 presents estimates β from equation (3) for a number of alternative cases relative to our baseline case for which results are presented in Table 4. These are the effects of a 13-week UI extension. Panel (a) includes estimates for our other outcome variables of interest, i.e., those shown in Figure 6.

Binary Treatment We start with the definition of our baseline treatment variable, $\widehat{W}_{s,t}$. That variable has "intensive margin" variation arising from the two tiers of the TUR program as well as a few other sources discussed in Appendix A.4. Panel (b) presents results for a case where we eliminate this intensive margin variation. We consider a case in which we include only a binary indicator for whether a state is paying EB and would not have in the absence of optional triggers. Specifically, we replace $\widehat{W}_{s,t}$ with the following indicator variable:

$$\mathcal{I}_{s,t} \equiv \mathbf{1}\left\{\left(\text{EB}^{13}_{s,t} = 1 \text{ and } \text{EB}^{13,\text{no options}}_{s,t} = 0\right) \text{ or } \left(\text{EB}^{20}_{s,t} = 1 \text{ and } \text{EB}^{20,\text{no options}}_{s,t} = 0\right)\right\},$$

where $\text{EB}^{13}_{s,t}$ is an indicator for whether state s has triggered onto the 13-week tranche of the EB program, $\text{EB}^{13,\text{no options}}_{s,t}$ is an indicator for whether s would have triggered onto the 13-week tranche of the EB program with no options on, and $\text{EB}^{20}_{s,t}$ and $\text{EB}^{20,\text{no options}}_{s,t}$ are defined analogously for the 20-week EB tranche. In this case, we do not multiply the left-hand side variables by 13.

Additional Controls We next consider a variety of additional controls. We estimate a version that includes lagged controls of several labor market variables in panel (c). Panel (d) presents results controlling for the impact of industry shocks by including state-level industry-employment shares interacted with dummies for consecutive 5-year periods. These controls are similar to controls used by Guren et al. (2021). More specifically, we construct (from the QCEW) the share of employment in real estate, manufacturing, construction, and retail trade in each state and quarter. We interact these shares with 5-year time fixed effects, for the time periods 1980–1984, 1985–1989, and so on. We aggregate the time fixed effects into 5-year increments for computational reasons.

Panel (e) presents results in which we control for the state-level Covid stringency index of Hale et al. (2021). This variable is a composite measure based on nine response indicators including school closures, workplace closures, and travel bans.

Panel (f) presents results in which include an interaction of a state fixed effect with the most-recent value of the national unemployment rate. Such interaction terms might be important if having the option in place affects the response of a state's unemployment rate to *all* shocks. The coefficients are little-changed. This suggests our results are not driven by states having differential sensitivities to macroeconomic shocks depending on option status. Rather, our empirical identification derives from differential timing of eligibility for UI extensions across states. The timing of UI extensions *caused* by option status differs from the timing of the incidence of aggregate shocks (for example, it occurs more frequently at the start and end of recessions, as opposed to during the worst phases of a downturn, and depends on a state's previous UI benefits history).

Alternative "Contemporaneous Effect" Specifications In panels (g) and (h), we present estimates of contemporaneous effects for alternative lengths of the window over which $\mathbf{y}_{s,t}$ is constructed. Specifically, we vary ω in $\mathbf{y}_{s,t} = \sum_{\ell=-\omega}^{\omega} y_{s,t+\ell}$, and analogously vary ω when constructing $\mathbf{W}_{s,t}$. Our baseline results in Table 4 present results with $\omega = 6$. In panels (g) and (h), we present results with $\omega = 0, 3,$ and 12.

In Table A.6, we abandon the IV specification and instead present OLS estimates of β_h from equation (1) for $h = 0$. These are equivalent to the "first-stage" and "reduced form" of the IV specification based on equation (3) for $\omega = 0$.

Alternative Pooling of Coefficients and Controls In our baseline specification, we allow the coefficient on $\widehat{W}_{s,t}$ and the state fixed effects to vary based on whether baseline potential benefit duration is above or below 60 weeks, but do not allow other controls to vary in this way. Table A.5 presents results for several modifications of this choice. In panel (i), we do not allow any coefficients to vary by baseline potential benefit duration (including the coefficient on $\widehat{W}_{s,t}$). In panel (j), we present results for a case where we only allow $\widehat{W}_{s,t}$ to vary by short and long baseline potential benefit duration but do not allow any controls to vary in this way. In panel (k), we present results for a case where in which we allow $\widehat{W}_{s,t}$ and all controls to vary by short and long baseline potential benefit duration. In all cases, we find similar parameter estimates to our baseline. This is reassuring, since a recent literature has suggested that this behavior is not guaranteed with two-way fixed effects (de Chaisemartin and D'Haultfœuille, 2020).

The cases discussed above all maintain our baseline breakpoint between "short" and "long" baseline potential benefit duration of 60 weeks. To assess whether our results are sensitive to this choice, we consider a specification where we split the sample at baseline potential benefit duration of 40 weeks. Panel (l) presents results for this case.

Effects by Program Panel (m) presents results for insured unemployment by program. Our baseline measure of insured unemployment includes, in the numerator, the number of insured unemployed receiving benefits under state UI, the EB program, and other federal programs. In panel (m), we show results when only EB recipients are included (first two columns) or excluded (second two columns), and when only state UI recipients are included (final two columns).

Additional Outcome Variables Panel (n) presents results for additional labor market outcomes. Specifically, we present estimates for the unemployment rate calculated directly from the CPS. This sidesteps the Kalman filtering embedded in LAUS estimates. Estimates for this variable are similar to those for the unemployment rate for LAUS. We also present results for log employment from the CES and the labor force participation rate from LAUS. These are insignificant.

Alternative Sample Periods Panel (o) presents results for a sample restricted to 2020-2022 (the Covid pandemic period). For this period, the difference in the effects of UI extensions at short versus long horizons appear even more stark than in our baseline, though they are much less precisely estimated. Panel (p) presents results for a sample period restricted to 1986-2019. This helps assess the sensitivity of our findings to excluding the very start of the sample when temporary layoffs were common. Our results are quite robust to this alternative sample period. Panel (q) presents estimates for samples split before and after 2007. This allows us to see whether our short duration results are coming from the early Great Recession period alone. We see effects of UI at short durations in both sample periods that are similar to the full-sample estimates.

Dropping Periods around Option Switches Finally, panels (r) and (s) present results for cases where we drop 2-years before and after a change in option status for all changes and "discretionary" changes, respectively.

Table A.5: Contemporaneous Quarterly Effects

(a) Baseline: Additional Outcomes

	Employment Rate (CES)		Insured Unemployment Non Mechanical	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	-0.39	-0.55	0.15	0.33
	(0.33)	(0.32)	(0.05)	(0.15)
$\mathbf{W}_{s,t}$, Long Dur.	0.49	0.60	-0.07	0.02
	(0.67)	(0.58)	(0.05)	(0.07)
Observations	103494	111298	103494	111298

	UI Recipiency Rate		Real-time Unemployment		Initial Claims/LF	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	4.94	6.18	0.40	0.49	0.03	0.15
	(0.96)	(2.20)	(0.14)	(0.17)	(0.05)	(0.09)
$\mathbf{W}_{s,t}$, Long Dur.	1.92	1.53	-0.21	-0.17	-0.05	-0.03
	(1.39)	(3.13)	(0.10)	(0.10)	(0.03)	(0.06)
Observations	103494	111298	103494	111298	103494	111298

(b) Binary Treatment

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.59	1.21	0.29	0.57
	(0.08)	(0.39)	(0.13)	(0.26)
$\mathbf{W}_{s,t}$, Long Dur.	0.22	0.26	-0.03	0.07
	(0.13)	(0.16)	(0.12)	(0.12)
Observations	103494	111298	103494	111298

(c) Saturated Controls

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.60	0.97	0.29	0.47
	(0.08)	(0.20)	(0.15)	(0.18)
$\mathbf{W}_{s,t}$, Long Dur.	0.25	0.30	-0.01	0.07
	(0.12)	(0.14)	(0.11)	(0.10)
Observations	98954	106758	98954	106758

(d) Industry Employment Share Controls

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.60	0.95	0.28	0.45
	(0.07)	(0.18)	(0.14)	(0.16)
$\mathbf{W}_{s,t}$, Long Dur.	0.27	0.32	-0.02	0.05
	(0.12)	(0.14)	(0.10)	(0.10)
Observations	102597	110401	102597	110401

Table A.5: Contemporaneous Quarterly Effects — Continued

(e) Adding Covid Stringency Index Control

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.60	0.89	0.28	0.42
	(0.08)	(0.17)	(0.13)	(0.16)
$\mathbf{W}_{s,t}$, Long Dur.	0.25	0.36	-0.03	0.07
	(0.13)	(0.13)	(0.11)	(0.11)
Observations	103494	111298	103494	111298

(f) Interacting State Fixed-Effect with National Unemployment Rate

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.58	0.87	0.40	0.56
	(0.07)	(0.16)	(0.15)	(0.18)
$\mathbf{W}_{s,t}$, Long Dur.	0.23	0.27	-0.03	0.03
	(0.12)	(0.14)	(0.11)	(0.11)
Observations	103494	111298	103494	111298

(g) Varying the "Contemporaneous Effect" Window (ω): Unemployment Rate

	$\omega = 0$		$\omega = 3$		$\omega = 12$	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.23	0.37	0.26	0.41	0.30	0.49
	(0.11)	(0.14)	(0.12)	(0.15)	(0.15)	(0.19)
$\mathbf{W}_{s,t}$, Long Dur.	-0.02	0.04	-0.03	0.05	-0.03	0.04
	(0.10)	(0.09)	(0.10)	(0.10)	(0.12)	(0.12)
Observations	103494	111298	103494	111298	103494	111298

(h) Varying the "Contemporaneous Effect" Window (ω): Insured Unemployment

	$\omega = 0$		$\omega = 3$		$\omega = 12$	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.54	0.81	0.58	0.88	0.63	1.01
	(0.07)	(0.17)	(0.07)	(0.18)	(0.08)	(0.22)
$\mathbf{W}_{s,t}$, Long Dur.	0.24	0.28	0.25	0.29	0.26	0.32
	(0.12)	(0.13)	(0.12)	(0.14)	(0.14)	(0.15)
Observations	103494	111298	103494	111298	103494	111298

(i) Pooling All Coefficients across Short and Long Duration Samples

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$	0.45	0.69	0.14	0.26
	(0.09)	(0.13)	(0.10)	(0.12)
Observations	103494	111298	103494	111298

Table A.5: Contemporaneous Quarterly Effects — Continued

(j) Pooling All *Controls* across Short and Long Duration Samples

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.56	0.86	0.23	0.38
	(0.08)	(0.18)	(0.12)	(0.15)
$\mathbf{W}_{s,t}$, Long Dur.	0.35	0.50	0.06	0.14
	(0.12)	(0.12)	(0.12)	(0.13)
Observations	103494	111298	103494	111298

(k) Interacting All Coefficients with Short-Duration Indicator

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.59	0.93	0.28	0.44
	(0.08)	(0.20)	(0.13)	(0.17)
$\mathbf{W}_{s,t}$, Long Dur.	0.19	0.30	0.08	0.22
	(0.08)	(0.16)	(0.07)	(0.11)
Observations	103456	111255	103456	111255

(l) Split Sample at Baseline Potential Benefit Duration of 40 Weeks

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.70	1.01	0.39	0.54
	(0.09)	(0.22)	(0.19)	(0.22)
$\mathbf{W}_{s,t}$, Long Dur.	0.24	0.50	-0.08	0.01
	(0.10)	(0.15)	(0.12)	(0.13)
Observations	103494	111298	103494	111298

(m) Insured Unemployment by UI Program

	EB Only		Federal + State (No EB)		State Only	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.27	0.35	0.34	0.63	0.15	0.33
	(0.03)	(0.04)	(0.07)	(0.27)	(0.05)	(0.15)
$\mathbf{W}_{s,t}$, Long Dur.	0.23	0.22	0.00	0.14	-0.07	0.02
	(0.02)	(0.03)	(0.11)	(0.20)	(0.05)	(0.07)
Observations	103494	111298	103494	111298	103494	111298

(n) Other Unemployment/Employment Indicators

	U. Rate, CPS		Log Emp., CES		LFPR, LAUS	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.35	0.48	-0.26	-0.22	0.12	0.08
	(0.16)	(0.19)	(0.34)	(0.40)	(0.13)	(0.12)
$\mathbf{W}_{s,t}$, Long Dur.	0.06	0.09	0.11	0.09	-0.55	-0.54
	(0.11)	(0.10)	(0.36)	(0.38)	(0.34)	(0.29)
Observations	102533	110337	103494	111298	103494	111298

Table A.5: Contemporaneous Quarterly Effects — Continued

(o) Pre- and Post-Covid Samples

	Insured Unemployment		Unemployment	
	1980–2019	2020–2022	1980–2019	2020–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.60	2.43	0.28	0.72
	(0.08)	(0.62)	(0.13)	(0.36)
$\mathbf{W}_{s,t}$, Long Dur.	0.25	0.05	-0.03	0.48
	(0.13)	(0.43)	(0.11)	(0.25)
Observations	103494	7804	103494	7804

(p) Alternative Sample: 1986+

	Insured Unemployment		Unemployment	
	1986–2019	1986–2022	1986–2019	1986–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.57	0.94	0.25	0.42
	(0.08)	(0.21)	(0.14)	(0.17)
$\mathbf{W}_{s,t}$, Long Dur.	0.25	0.30	-0.04	0.04
	(0.13)	(0.14)	(0.10)	(0.10)
Observations	88034	95838	88034	95838

(q) Pre- and Post-2007

	Insured Unemployment		Unemployment	
	1980–2006	2007–2019	1980–2006	2007–2019
$\mathbf{W}_{s,t}$, Short Dur.	0.66	0.40	0.25	0.33
	(0.07)	(0.13)	(0.12)	(0.19)
$\mathbf{W}_{s,t}$, Long Dur.		0.33		0.16
		(0.11)		(0.07)
Observations	69842	33652	69842	33652

(r) Drop Around Switches

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.76	0.86	0.47	0.60
	(0.10)	(0.20)	(0.19)	(0.23)
$\mathbf{W}_{s,t}$, Long Dur.	0.16	0.06	-0.31	-0.26
	(0.19)	(0.22)	(0.24)	(0.20)
Observations	81989	87119	81989	87119

(s) Drop Discretionary Switches

	Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022
$\mathbf{W}_{s,t}$, Short Dur.	0.63	0.98	0.29	0.46
	(0.07)	(0.20)	(0.13)	(0.18)
$\mathbf{W}_{s,t}$, Long Dur.	0.21	0.29	-0.02	0.06
	(0.14)	(0.15)	(0.12)	(0.11)
Observations	99965	107455	99965	107455

Table A.6: $h = 0$ Effects

	Potential Benefit Duration		Insured Unemployment		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\widehat{W}_{s,t}$, Short Dur.	12.8	12.5	0.53	0.79	0.23	0.36
	(0.2)	(0.3)	(0.07)	(0.16)	(0.11)	(0.14)
$\widehat{W}_{s,t}$, Long Dur.	13.6	13.4	0.27	0.30	-0.01	0.06
	(0.8)	(0.6)	(0.13)	(0.13)	(0.10)	(0.09)
Observations	103494	111298	103494	111298	103494	111298

NOTE. This table shows OLS estimates of β_0 in equation (1), with the variables in the column headers as left-hand side variables, all multiplied by 13. The sample runs from the beginning of 1980 through 2019, and excludes Alaska. Standard errors are two-way clustered by state and week and shown in parentheses.

B Comparison to Literature and Additional Empirical Results

B.1 Other Estimates of "Macro Effects" of UI Extensions

Table B.1 contains a list of prior estimates of the macro effects of UI extensions. Where applicable, we have adjusted the numbers reported in these papers in order to match our "experiment"—a 13-week increase in potential benefit duration that dies out gradually over 80 weeks. Below, we provide more detail for each of these adjustments.

Table B.1: Response to 13-Week Extension: Headline Numbers

Authors	Estimate (p.p.)
Rothstein (2011)	0.02–0.10
Boone et al. (2021)	-0.02
Farber and Valletta (2015)	0.08
Chodorow-Reich et al. (2019)	0.01
Dieterle et al. (2020)	0.09
Amaral and Ice (2014)	0.16
Hagedorn et al. (2019)	0.72
Johnston and Mas (2018)	0.57–0.81
Jessen et al. (2023)	0.21

NOTE. This table shows the estimated responses of the unemployment rate (in percentage points) to a 13-week increase in potential benefit duration, taken from the papers in the first column. The text of Appendix B.1 contains more details on how these estimates are constructed.

Rothstein (2011) Rothstein implements several approaches to identifying the effect of UI extensions during the Great Recession. He reports—in the abstract and in Table 8—effects of these extensions on the unemployment rate of between 0.1 and 0.5 percentage points. The average extension—i.e., average potential benefit duration including the extension less potential benefit duration with only regular state UI—between January 2010 and January 2011 was 63 weeks. Dividing Rothstein's range by 63 and multiplying it by 13—to convert it into the effect of a 13-week extension—yields a range of 0.02 to 0.10.

Boone et al. (2021) Boone et al. estimate the effect of a 73-week UI extension on the employment-to-population ratio (EPOP). The point estimate using their preferred specification is 0.180p.p (page 73). Scaled to a 13-week extension, this becomes 0.032. To convert this to an effect on the unemployment rate, we regress the aggregate EPOP (EMRATIO, in FRED) on the aggregate unemployment rate (UNRATE, in FRED) using data from January 1976 to December 2019. We then multiply the resulting coefficient (-0.558) by the rescaled effect estimated by Boone et al.. This yields -0.02.

Farber and Valletta (2015) Farber and Valletta report an effect of UI extensions in 2010 on the unemployment rate of 0.4 percentage points. As discussed above, the average extension in 2010 was 63 weeks. Converting their estimate into the effect to a 13 week extension yields an estimate of 0.08 percentage points.

Chodorow-Reich et al. (2019) Chodorow-Reich et al. report an effect of a one-month increase in potential benefit duration of 0.003 percentage points (Table IV). Scaling this to a 13-week extension yields an effect of 0.01 percentage points.

Dieterle et al. (2020) Dieterle et al. find that an increase in benefits from 26 to 99 weeks increases the unemployment rate by 0.5 percentage points (Table 1, column (2)). Scaling this to a 13-week extension yields an effect of 0.09 percentage points.

Hagedorn et al. (2019) Hagedorn et al. (HKMM) show that the percent change in the unemployment rate u from an increase in potential benefit duration from ω_1 to ω_2 is given by

$$\Delta \log(u)) = \alpha \times \frac{1-(\beta(1-s))^n}{1-\beta(1-s)} \times (\log(\omega_2) - \log(\omega_1)) \tag{11}$$

where $\alpha = 0.053$ is their estimate of the elasticity of the quasi-differenced unemployment rate to potential benefit duration, $\beta = 0.99$ is the discount factor, and $s = 0.1$ is the job separation rate. (This is equation (13) in their paper.) In the last paragraph before section 4.1.1 in their paper, HKMM report a counterfactual experiment in which they increase potential benefit duration from $\omega_1 = 26$ to $\omega_2 = 99$ weeks permanently ($n = \infty$), with an initial unemployment rate of 5%. From equation (11), this leads to a 65% increase in the unemployment rate, or an increase of 4.6 percentage points ($100\exp(0.65+\log(0.05))-5$). Table B.1 reports the change in the unemployment rate implied by equation (11) when benefits are increased by 13 weeks from $\omega_1 = 26$ to $\omega_2 = 39$ weeks for $n = 10$ quarters. This yields a percentage point increase of 0.72.

Amaral and Ice (2014) Amaral and Ice pursue an approach similar to that of HKMM, using a different sample period. The authors do not report an estimate of α, but they do present (in Figure 6 of their paper) a comparison of their results with those of HKMM. They find an increase in the unemployment rate in 2008Q2 of 0.85 percentage points, while the comparable increase for

HKMM is 3.8 percentage points (a number we read off of the lines plotted in Figure 6). We thus scale the HKMM effect by the ratio $0.223 = 0.85/3.8$ to arrive at 0.16.

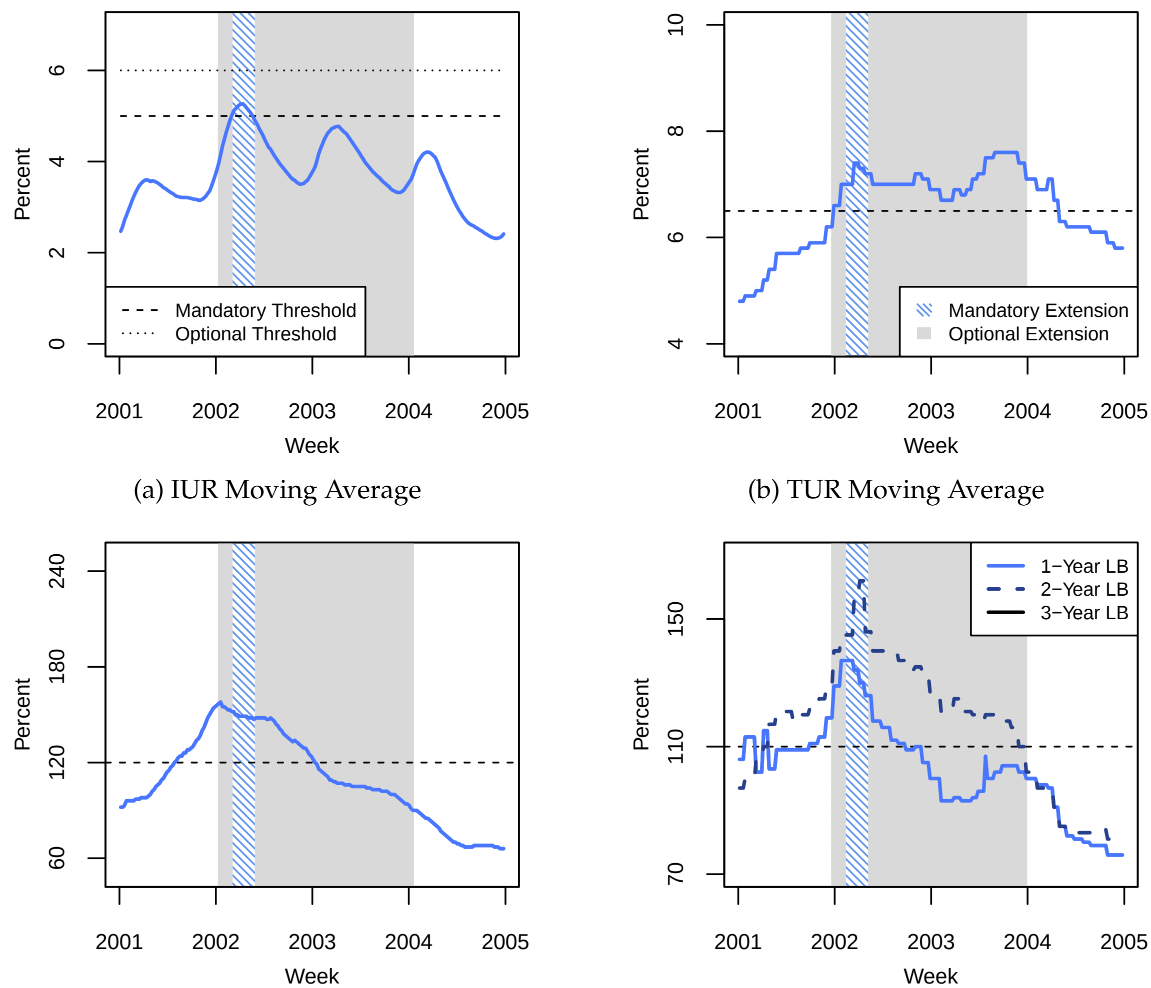
Johnston and Mas (2018) Johnston and Mas present several counterfactual "macro effect" simulations. Looking across the unemployment rate effects in Table 5 of this paper, we see that the 16 week cut in benefits led to somewhere between a 0.7 and 1 percentage point decrease in the unemployment rate. Scaling this range by $13/16$, we arrive at a range of $0.57 - 0.81$. Johnston and Mas (2018) studied a cut to regular state UI benefits which removed weeks of UI benefits from the *middle* of workers' UI spells. Some recipients needed to switch from regular state UI to federal UI earlier than they would have otherwise, and a higher-than-expected number of these workers may have failed to make this switch. This may have made their estimates somewhat higher than they would otherwise have been.

Amaral and Ice (2014), Dieterle et al. (2020) and Boone et al. (2021) build on the empirical strategy of Hagedorn et al. (2019) but find the original results are sensitive to the specific data and estimation strategy used in the original paper.

Jessen et al. (2023) Jessen et al. study the macroeconomic effects of UI extensions in Poland using a regression discontinuity design and find that an extension from 6 to 12 months of potential benefit duration increases unemployment by 3 percent. At an average unemployment rate of 14% in their sample, this corresponds to an increase in the unemployment rate of 0.42 percentage points. Dividing by two to get to the effect of a 13-week extension, this yields a 0.21 percentage point increase in unemployment. One has to be somewhat careful with direct comparisons to U.S. studies, because of the much higher unemployment rate during the sample period (14%) as well as the relatively low fraction of the UI eligible unemployed (21%).

B.2 Second Example of Trigger Rule Adoption Affecting UI: Washington

In Figure B.1, we present data from Washington state for the period 2001-2005 that is analogous to Figure 4 in the main text. Over this period, Washington has the optional TUR trigger rule in place (in contrast to Arkansas). For this reason, Washington paid extended benefits over the entire shaded period (both the solid gray, and dashed blue), since its TUR moving average was above the 6.5% threshold, and the maximum of the TUR lookbacks was above 110% over this period. During the much shorter blue dashed period, Washington would have qualified for EB regardless of its option status, since its IUR moving average and IUR lookback were above their respective thresholds. This implies that $\widehat{W}_{s,t} = 0$ during the blue dashed period.



(a) IUR Moving Average (b) TUR Moving Average

(c) IUR Lookback (d) TUR Lookback

Figure B.1: Trigger Variables in Washington during 2002–2004

NOTE. This figure is analogous to Figure 4 in the text, but instead data for Washington for the period 2001-2005 is shown. Washington has the optional TUR trigger rule in place over the entire period shown.

B.3 Placebo Test

To verify that the identifying variation in our estimator arises from differences in option status across states, we present a placebo exercise that reshuffles the option status of different states. Specifically, we carry out the following procedure:

1. For each state s, we randomly draw a state s' without replacement and give state s all option statuses of state s' (the entire time series).

2. We re-compute $\widehat{W}_{s,t}$ using these reshuffled option statuses.

3. We estimate our baseline specification (equation (1)) using the re-computed $\widehat{W}_{s,t}$, but leaving everything else unchanged.

We repeat this procedure 100 times. Figure B.2 presents the mean of the distribution of estimates across the 100 draws as well as the 2.5% and 97.5% quantiles.

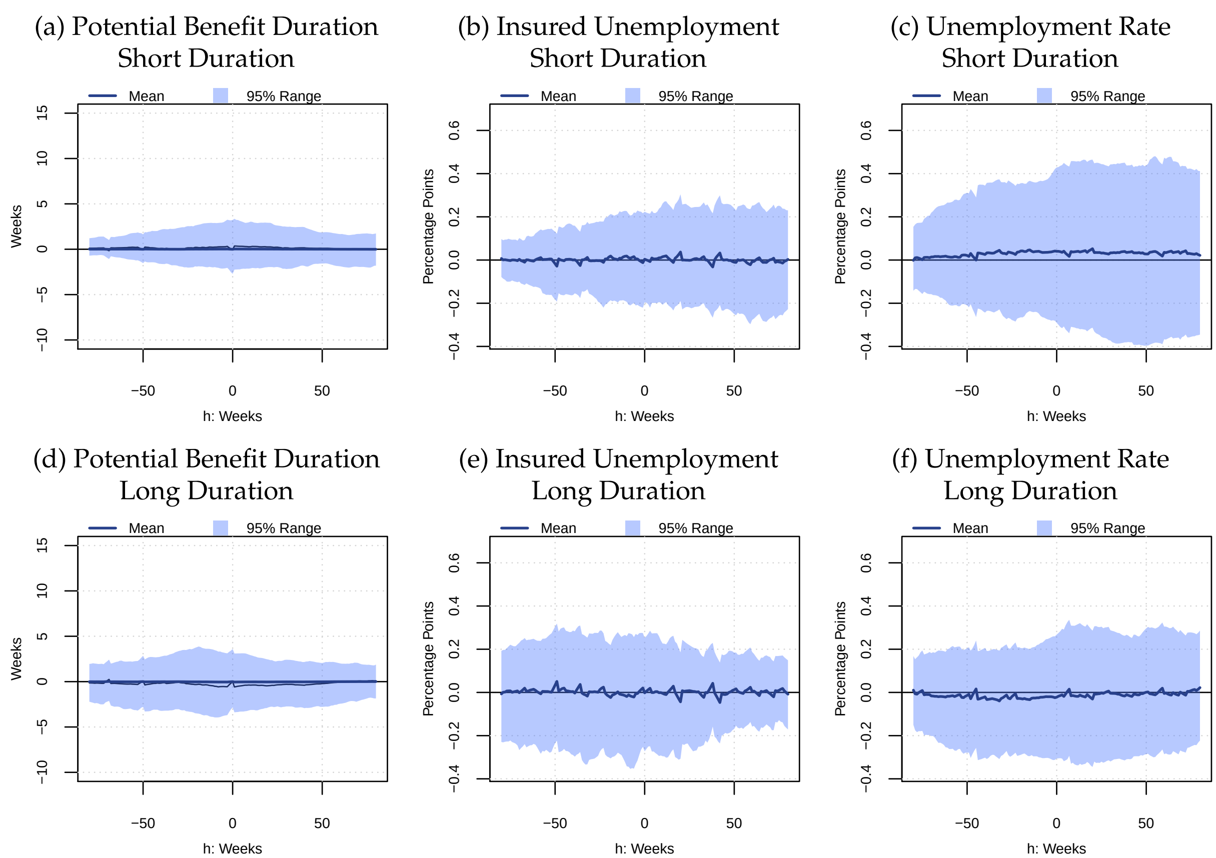
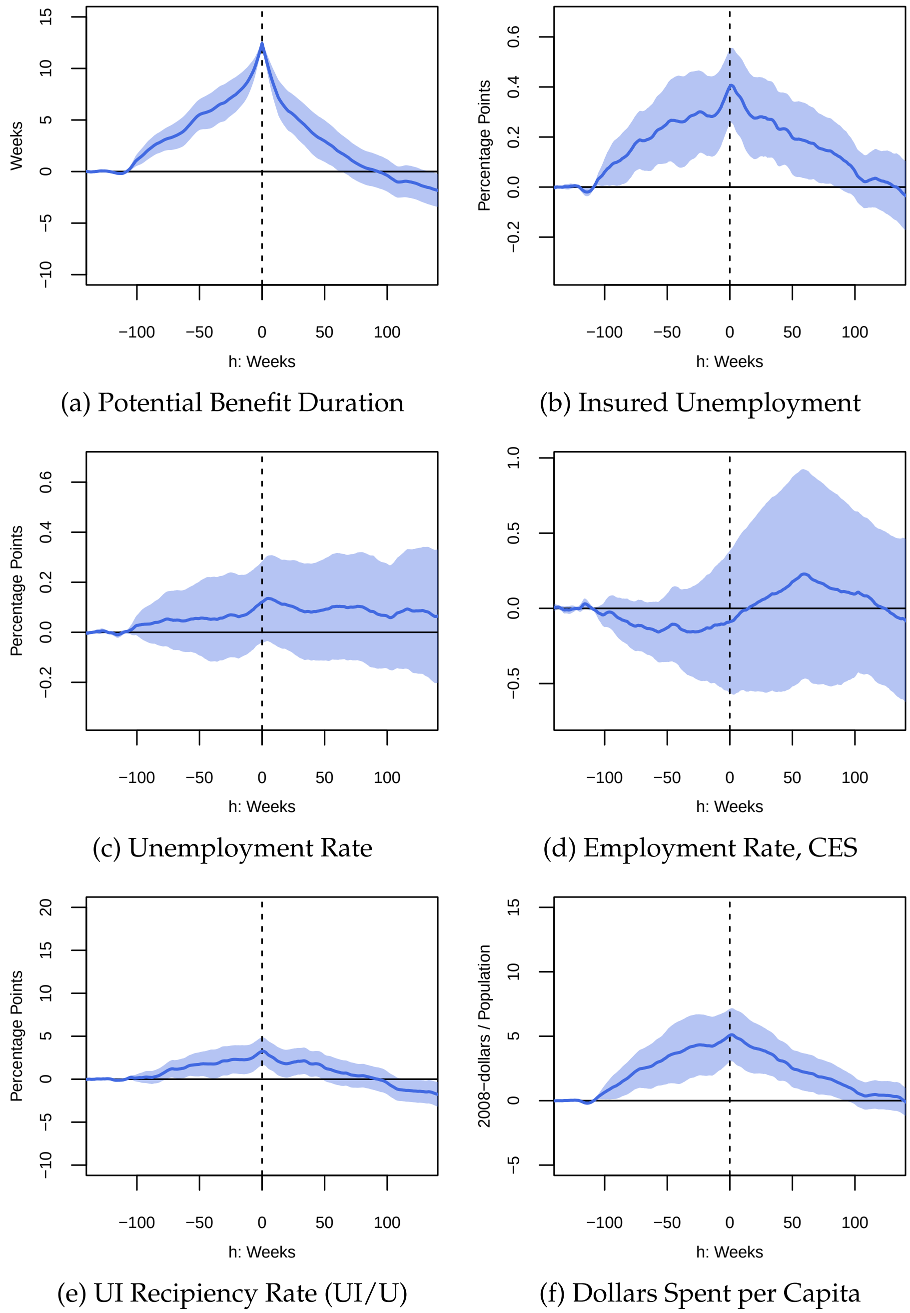


Figure B.2: Placebo Test Estimates

B.4 Full Sample Estimation



(a) Potential Benefit Duration

(b) Insured Unemployment

(c) Unemployment Rate

(d) Employment Rate, CES

(e) UI Recipiency Rate (UI/U)

(f) Dollars Spent per Capita

Figure B.3: Macro Effects of UI Extensions: Full Sample

NOTE. Each panel shows OLS estimates of equation (1), with the variable in the panel titles as left-hand side variables, $y_{s,t+h}$. Here, we do not interact any variables on the right-hand side of equation (1) with the initial PBD—these results thus average over the short- and long-duration samples. Each point and surrounding 95% shaded confidence interval is from a separate regression—one each for $h \in -150, \ldots, 150$. The sample runs from the beginning of 1980 through 2019, and excludes Alaska. Standard errors are two-way clustered by state and week.

C PE Framework for Comparing Micro and Macro Estimates

The simple partial equilibrium calculation of the effects of UI extensions on unemployment in section 6.1 abstracts from a number of realistic features. Here, we provide a more detailed partial equilibrium mapping. The approach we develop here is also used by Schmieder and von Wachter (2016).

C.1 Setup

Consider a continuous time setting where the fraction of job losers who become UI recipients is ϕ. These people receive UI benefits until their benefits expire or they find a new job (whichever comes first). The remaining fraction of job losers, $1-\phi$, never receive UI. We assume that UI recipients have a job finding rate of f_c, while the job finding rate of the unemployed that are not receiving UI is f_x. We focus on comparing steady states with different values of the model parameters.[65] We can then write that the fraction ϕ of job losers that receive UI have the following re-employment hazard function:

$$\lambda(t) = \begin{cases} f_c & \text{if } t \leq \Pi \\ f_x & \text{if } t > \Pi, \end{cases}$$

where Π denotes potential benefit duration. This yields a re-employment survival function

$$S(t) = \exp\left(-\int_0^t \lambda(x)dx\right) = \begin{cases} \exp\left(-f_c t\right) & \text{if } t \leq \Pi \\ \exp\left(-\left(\Pi f_c + (t-\Pi) f_x\right)\right) & \text{if } t > \Pi. \end{cases} \tag{12}$$

The fraction $1-\phi$ of the unemployed that do not receive UI have a survival function given by $S_n(t) = \exp\left(-f_x t\right)$. Taking a weighted average of these two survival functions yields the average *unemployment duration*

$$\begin{aligned} D(f_x, f_c, \phi, \Pi) &= \phi\left[\int_0^\infty S(t)dt\right] + (1-\phi)\left[\int_0^\infty S_n(t)dt\right] \\ &= \phi\left[\frac{1-\exp(-f_c\Pi)}{f_c} + \frac{\exp(-f_c\Pi)}{f_x}\right] + (1-\phi)\left[\frac{1}{f_x}\right]. \end{aligned} \tag{13}$$

[65] We have also carried out dynamic simulations. These yield similar results regarding the quantities we are interested in.

We can also calculate the average *unemployment insurance duration*. This is

$$B = \int_0^{\Pi} S(t)dt = \frac{1 - \exp(-f_c \Pi)}{f_c}. \tag{14}$$

Finally, we can calculate the average *regular UI duration*. This is

$$B_r = \int_0^{26} S(t)dt = \frac{1 - \exp(-f_c 26)}{f_c}. \tag{15}$$

Here, we are assuming that the the potential benefit duration for regular UI is 26 weeks, which it is in most states. Average regular UI duration is the outcome reported by Card and Levine (2000) in their simulation.

Below, we use equations (13)–(15) to scale different micro-elasticies so that they are comparable. Before performing these comparisons, it is useful to note a few features of these equations. First, even with no effect on search behavior (i.e., no change in f_c and f_x), an increase in potential benefit duration (Π) will increase average unemployment insurance duration mechanically (equation (14)). This is not true for regular UI duration (equation (15)), a statistic that also does not require taking a stand on UI take-up. Second, equation (13) highlights several factors that make the effect of potential benefit duration on average unemployment duration a fairly detailed calculation. The calculation is straightforward if UI recipients and the rest of the unemployed find jobs at the same rate, and are affected equally by UI extensions ($f_x = f_c$). In that case, equation (13) collapses to f_c^{-1}. Most studies, however, examine the effect of potential benefit duration on the job search behavior of UI recipients, making such studies more informative about f_c than f_x. In turn, any effect on f_c will only affect average unemployment duration through non-exhaustees, which depends on ϕ and Π.

C.2 Converting the Schmieder and von Wachter (2016) Elasticities

Schmieder and von Wachter (2016) survey the literature on estimates of the elasticity of unemployment duration of UI recipients with respect to potential benefit duration. Appendix C.3 corrects an error in their analysis of Card and Levine (2000). With this correction, the evidence they survey suggests that this elasticity is between 0.33 and 0.49 in the United States. In terms of the notation used in section C.1, the elasticity they report is

$$\xi \equiv \frac{\Delta f_c^{-1}}{\Delta \Pi} \frac{\Pi}{f_c^{-1}}. \tag{16}$$

Table C.1: Summary Statistics

Variable	Short-Dur. Sample	Long-Dur. Sample	Full Sample
Baseline Potential Benefit Dur.	34	74	43
Unemployment Rate	6.8	8.4	7.1
Insured Unemployment Rate	2.5	4.6	2.9
Labor Force Part. Rate	66	65	66
UI Recipiency Rate	36	53	40
EU Transition Rate (Monthly)	1.6	1.5	1.6
UE Transition Rate (Monthly)	27	19	25
UE Transition Rate (Weekly)	6.9	4.7	6.4
Monthly UI Payment	1027	1188	1061

NOTE. This table presents summary statistics of the main variables used for calibrating the models described in Section 6, this appendix, and Appendix D. As in our baseline sample, we retain all state-months between January 1980 and December 2019 for which our UI benefits calculator correctly predicts UI benefits, and exclude Alaska. Here, we also exclude months in which no state is paying UI benefits under the EB program. Among the remaining observations, the left column shows averages in the short-duration sample (baseline potential benefit duration < 60 weeks); the middle shows averages in the long-duration sample (≥ 60 weeks); and the right includes all such observations. Baseline potential benefit duration is reported in weeks, the monthly UI payment is reported in December 2007 dollars, and the remaining variables are reported in percent.

Equation (13) shows that this is not equivalent to the elasticity of average unemployment duration, D, with respect to potential benefit duration in the case were $f_c \neq f_x$. In this case, the elasticity of average unemployment duration with respect to potential benefit duration also depends on f_x, Π, and ϕ. In order to convert an estimate of ξ into $d\log(D)/d\log(\Pi)$, we must estimate—before and after the extension—a job finding rate for non-UI recipients, f_x; a job finding rate for UI recipients, f_c; a UI recipiency rate, ϕ; and potential benefit duration, Π. The estimates for ξ from Schmieder and von Wachter (2016) will pin down one of these (f_c after the extension). But we need other data to pin down the others.

The data we use for this calibration is from our short-duration sample and is presented in Table C.1. Let a superscript 0 index pre-intervention values and a superscript 1 index post-intervention values. We set $f_x^0 = f_c^0 = 0.069$, the average weekly UI transition rate in our data. We set $\phi^0 = 0.40$ to target the UI recipiency rate of 0.36. We set $\Pi^0 = 34$, the average baseline potential benefit duration. We set f_x^1 and ϕ^1 to their pre-extension values, and set $\Pi^1 = \Pi^0 + 13$. This implies that we are estimating the effects of a 13-week extension. Finally we set $f_c^1 = \frac{f_c^0}{1+\xi\left(\frac{\Pi^1-\Pi^0}{\Pi^0}\right)}$, which solves equation (16) for the new job-finding rate consistent with an estimate for ξ and the percent change in benefit duration, $\frac{\Pi^1-\Pi^0}{\Pi^0}$.

We then calculate the average pre- and post-extension duration for all unemployed: $D_0 =$

$D\left(f_x^0, f_c^0, \phi^0, \Pi^0\right)$ and $D_1 = D\left(f_x^1, f_c^1, \phi^1, \Pi^1\right)$, respectively, and report the implied macro elasticity as $\frac{\frac{D_1 - D_0}{D_0}}{\frac{\Pi^1 - \Pi^0}{\Pi^0}}$. Schmieder and von Wachter's range of 0.33–0.49 for ξ then yields a range of 0.15–0.22 for the elasticity of average unemployment duration with respect to potential benefit duration.

C.3 The Card and Levine (2000) Citation by Schmieder and von Wachter

Through correspondence with Schmieder and von Wachter, we found that their citation of the *unemployment duration* elasticity of Card and Levine (2000) was instead the *regular UI duration* elasticity. The conclusion of Card and Levine states (emphasis and footnote added)

> Starting with the sample of 1997 UI claimants as a reference population, we calculated claim survivor functions assuming that the weekly hazard rates were 16.6% lower than the observed rates. The results of the simulation suggest that the 'long run' effect of a 13-week extended benefit program would be a 7 percentage point increase in the **regular UI exhaustion rate**, and a roughly 1 week increase in the average number of weeks of **regular UI** collected by claimants.[66]

While people spent one more week on regular UI, they likely spent even more time on UI including extended benefits (B), and unemployed (D). Using summary statistics from the paper and other reported details about the simulation, we calculated an implied percent change in average unemployment duration among UI recipients of 0.2.[67] Dividing this by the percent change in potential benefit duration (0.5=13/26) gives an unemployment duration elasticity of 0.4.[68] Similarly, Schmieder and von Wachter report an elasticity of 0.34 from Moffitt (1985), but our reading of Moffitt suggests an elasticity of 0.49.

[66]We verify the outcome of the Card and Levine simulation as follows. From their Table 4, we get that the average UI exhaustion rate (with a potential benefit duration of 26 weeks) is 39.3 percent. We can use equation (12) to back out a job-finding rate by solving $0.394 = \exp(-f_c \times 26)$ for f_c, which yields $f_c = -\log(0.394)/26 = 0.036$. Card and Levine report decreasing this job-finding rate by 16.6% in their simulation. Calculating average UI duration in equation (15) for $f_c = 0.036 \times (1 - 0.166)$ and $f_c = 0.036$ and taking the difference yields an increase of 1.16, consistent with the "roughly 1 week" the authors report.

[67]Among UI recipients, the implied average unemployment duration is simply $1/f_c$. Thus, going from a pre-extension job-finding rate of f_0 to a post-extension rate of βf_0 yields a percent change in average duration (among UI recipients) of $\frac{\frac{1}{\beta f_0} - \frac{1}{f_0}}{\frac{1}{f_0}} = \frac{1}{\beta} - 1$. Card and Levine report $\beta = 1 - 0.166$, yielding a percent-change of 0.2.

[68]The 16.6% change in hazard rate is based on a log approximation to Card and Levine's logit specification. Under a logit specification, the coefficient δ on potential benefit duration is *approximately equal to* the percent change in hazard rate. However, under the logit specification, δ is *exactly equal to* the change in the log-odds ratio implied by a unit increase in potential benefit duration, i.e., $\delta = \log\left(\frac{\lambda'}{1-\lambda'}\right) - \log\left(\frac{\lambda}{1-\lambda}\right)$, where λ is the initial hazard rate and λ' is the new rate. With $\delta = 0.1662$ and $\lambda = 0.036$, we can solve this equation to find that $\lambda' = 0.042$. This implies a percent change of λ of $\frac{0.042-0.036}{0.036} = 17.3\%$. Setting $\beta = 1 - 0.173$ in $1/\beta - 1$ from footnote 67 yields a percent change in average duration among UI recipients of 0.21, or an elasticity with respect to potential benefit duration of 0.418 (instead of 0.4 using the log approximation).

D Macro Model of UI Benefit Extensions

In what follows we describe in detail the setup, timing, value functions, law of motions, stationary equilibrium, the calibration, and the main results of the model in section 6.2 (with some repetition from the main part of the paper). In the results section, we also describe some additional results not included in the main part of the paper.

Model setup. Time is discrete and the discount factor is β. Firms post vacancies, v, to hire workers. Workers are either employed (e), unemployed (u) or inactive (n). Firm-worker matches produce output p. Matching between firms and workers is random and governed by a constant returns to scale matching function $M(S, v)$, where S is the effective number of searchers and $\theta = \frac{v}{S}$ is the labor market tightness.

Unemployed workers qualify for T periods of UI benefits, b_{UI}, and receive a flow value of leisure/home production, b_L. As a result, the flow value of unemployment is $b(\tau) = \mathbb{1}(\tau > 0)b_{UI} + b_L$, where τ is the number of periods of UI benefits an unemployed worker has left. Unemployed workers exert search effort s at cost $c(s)$, where $c(0) = 0$, $c'(s) > 0$ and $c''(s) > 0$. They are matched to firms at rate $s\lambda(\theta)$. Optimal search effort depends on the number of periods of UI benefits left, which we denote as $s(\tau)$. Aggregate search effort is the unemployment-weighted matching efficiency $S = \sum_{\tau=0}^{T} u(\tau)s(\tau)$, where $u(\tau)$ is the mass of unemployed with τ periods of UI benefits left.[69]

Employed workers are laid off from their jobs with probability δ. At the beginning of an unemployment spell, unemployed workers draw an i.i.d. take-up cost ξ from the distribution $G(\xi)$. Workers who draw a high enough cost will find it optimal not to take up UI benefits. Each period workers also draw a home production shock with probability ι, which leads them to leave the labor force. They re-enter the labor force through unemployment with probability ρ. We assume that workers who exit the labor force loose eligibility to UI benefits. Workers who have lost eligibility to UI benefits, requalify for a full spell of UI benefits with probability h once they find a job.

As in the standard DMP model, wages are determined by Nash Bargaining with the worker bargaining share α. Vacancies are determined endogenously by the condition that the flow cost, c, is equal to the expected discounted profit of opening a vacancy.

[69] Note that $u(0)$ includes those who exhausted UI benefits as well as those who are not eligible for UI benefits as well as those who decide not to claim UI benefits.

Timing. We assume the following timing of events within the period:

1. Newly unemployed workers draw the UI benefit take-up cost ξ and decide whether to claim UI benefits or not.

2. Workers and firms match based on the stocks of vacancies and unemployed and their optimal search efforts.

3. Employed and unemployed workers draw a home production shock with probability ι and move to inactivity next period. Inactive workers draw a home production shock at rate ρ and move to unemployment next period.

4. Those employed workers who were employed at the beginning of the period and did not draw a home production shock, draw a separation shock at rate δ and move to unemployment next period. Unemployed workers who found a job within the period do not draw a separation shock.

5. Those employed workers who were employed at the beginning of the period and did not draw a home production shock nor a separation shock, draw a UI re-qualification shock at rate h. Unemployed workers who found a job within the period do not draw a re-qualification shock.

Value functions. The value of the unemployed worker with τ periods of UI left is:

$$U(\tau) \quad = \quad \max_s \left\{ b(\tau) - c(s) + \beta(1-\iota)[(1-s\lambda(\theta))U(\tau-1) + s\lambda(\theta)W(\tau-1)] + \beta\iota N \right\}, \tag{17}$$

where $b(\tau) = \mathbb{1}[\tau > 0]b_{UI} + b_L$ is the flow value of unemployment with τ periods of UI left, $W(\tau)$ is the value of employment with τ periods of UI left in the event of a new spell of unemployment, and N is the value of the inactive labor market state. The first-order condition for search effort is

$$c'(s(\tau)) = \beta(1-\iota)\lambda(\theta)(W(\tau-1) - U(\tau-1)), \tag{18}$$

which states that the marginal cost of search effort is equal to the marginal increase in the probability of finding a job, $\lambda(\theta)$, times the present discounted gain of finding a job.

Note that for an unemployed worker with $\tau = 0$, the environment is stationary because the flow value after UI exhaustion remains constant. For this reason, the value function at $\tau = 0$ can be written as

$$U(0) = \max_s \{b_L - c(s) + \beta(1-\iota)[(1-s\lambda(\theta))U(0) + f(s)\lambda(\theta)W(0)] + \beta\iota N\}.$$

The value function above is for an unemployed worker who decided to claim UI benefits. Unemployed workers decide whether to claim benefits based on a take-up cost ξ they draw at the beginning of their unemployment spell. If they decide not to claim benefits, the value of unemployment is the same as for an unemployed worker who exhausted all UI benefits and gets flow value b_L. The value of an unemployed worker at the beginning of an unemployment spell (before the realization of the UI take up cost) thus is

$$\tilde{U}(\tau) = \omega(\tau)(U(\tau) - E(\xi|\xi < \xi(\tau)) + (1-\omega(\tau))U(0), \tag{19}$$

where $\omega(\tau) = G(U(\tau) - U(0))$ is the probability that the take-up cost is low enough so that it is optimal to take up UI benefits and where $\xi(\tau) = U(\tau) - U(0)$.

The value of inactivity N is

$$N = b_N + \beta((1-\rho)N + \rho U(0)), \tag{20}$$

where b_N is the flow value during inactivity and ρ is the probability of moving back to the labor force.

The value of employment for the worker who was hired out of unemployment with τ periods of UI remaining is

$$W(\tau) = w(\tau) + \beta(1-\iota)[(1-\delta)(hW(T) + (1-h)W(\tau)) + \delta\tilde{U}(\tau)] + \beta\iota N. \tag{21}$$

The value function depends on UI benefits at the time of hire, as the worker has only τ periods of UI benefits left in case of job loss. Workers qualify for the full length of benefits T with probability h.

On the employer side, the value of an unfilled vacancy is

$$V = -c + \beta\left[(1 - q(\theta)(1-\iota))V + q(\theta)(1-\iota)\left(\sum_{\tau=0}^{T}\pi(\tau)J(\tau)\right)\right], \tag{22}$$

where $\pi(\tau) = \frac{u(\tau)s(\tau)}{S}$ is the probability of being matched with an unemployed worker with τ

periods of UI left. The value of a filled vacancy is

$$J(\tau) \quad = \quad p - w(\tau) + \beta[(1-\delta)(1-\iota)(hJ(T) + (1-h)J(\tau)) + (1-(1-\delta)(1-\iota))V], \tag{23}$$

which depends on τ because the re-employment wage will depend on the UI benefits workers have remaining at the time of hire.

Bargaining. We assume that wages are negotiated in each period according to the Nash-Bargaining solution

$$\arg\max_{w} = (W(\tau) - \tilde{U}(\tau))^{\alpha}(J(\tau) - V)^{1-\alpha}, \tag{24}$$

where α is the worker's bargaining share. The solution to the Nash-Bargaining problem will depend on the periods of UI the worker has remaining at the time of hire because the unemployed workers will not re-qualify for the full length of UI benefits right away at the time of hire. We denote the Nash-Bargained wage for a worker eligible for τ periods of UI benefits as $w(\tau)$.

Law of motion. The law of motion of employment (e), unemployment (u) and inactivity (n) for a given labor market tightness θ, optimal search effort $s(\tau)$ and optimal take-up decision $\omega(\tau)$ are

$$\begin{aligned} e(t+1) &= e(t) + F(t)(1-\iota)u(t) - (\iota + (1-\iota)\delta)e(t) && (25)\\ u(t+1) &= u(t) + \delta(1-\iota)e(t) - F(t)(1-\iota)u(t) - \iota u(t) + \rho n(t) && (26)\\ n(t+1) &= n(t) + \iota(1-n(t)) - \rho n(t), && (27) \end{aligned}$$

where $F(t) = \frac{\lambda(\theta)}{u(t)}\left(\sum_{\tau=0}^{T} s(\tau)u(\tau,t)\right)$ is the aggregate job-finding rate and $u(\tau,t)$ is the mass of unemployed workers with τ periods of UI benefits left. Note that $u(0,t)$ includes those who have exhausted UI benefits as well as those who joined the labor force from inactivity and those who chose not to claim UI benefits. The law of motion for the distributions of unemployed and employed with $\tau = T$ are as follows

$$\begin{aligned} u(T,t+1) &= \delta(1-\iota)\omega(T)e(T,t) && (28)\\ e(T,t+1) &= (1-(\iota+(1-\iota)\delta))e(T,t) + (1-\iota)(1-\delta)h(e(t) - e(T,t)). && (29) \end{aligned}$$

Equation (28) states that the the mass of newly unemployed who qualify for a full spell of UI benefits at $t+1$ is equal to the mass of employed who qualify for full spell of periods this period times the probability that they are laid off and choose to claim benefits. Equation (29) states that the mass of employed who qualify for a full spell of UI benefits next period is equal to the mass this period times the probability of not separating into unemployment or inactivity plus the mass of employed who re-qualify this period. The law of motion for the distributions of unemployed and employed with $0 < \tau < T$ are

$$u(\tau, t+1) = (1-\iota)(1-s(\tau+1)\lambda(\theta))u(\tau+1, t) + \delta(1-\iota)\omega(\tau)e(\tau, t) \tag{30}$$

$$e(\tau, t+1) = (1-(\iota+(1-\iota)(\delta+(1-\delta)h)))e(\tau, t) + (1-\iota)s(\tau+1)\lambda(\theta)u(\tau+1, t). \tag{31}$$

Equation (30) states that the mass of unemployed with τ periods of UI eligibility left next period is equal to the mass of unemployed with $\tau+1$ periods of UI eligibility left this period times the probability of still being unemployed, plus the mass of employed who qualified only for τ periods of UI times the probability of being laid off and claiming UI. Equation (31) states that the mass of employed with τ periods of UI eligibility left next period is equal to the mass this period times the probability of still being employed and not re-qualifying plus the mass of unemployed this period with $\tau+1$ periods of UI eligibility left times the probability of finding a job. For $\tau = 0$, the law of motion for the distributions of unemployed and employed are

$$\begin{aligned} u(0, t+1) &= (1-s(0)\lambda(\theta))u(0, t) + (1-\iota)(1-s(1)\lambda(\theta))u(1, t) + \delta(1-\iota)e(0, t) \\ &\quad +\delta(1-\iota)\sum_{\tau=1}^{T}(1-\omega(\tau))e(\tau, t) + \rho n(t) \end{aligned} \tag{32}$$

$$\begin{aligned} e(0, t+1) &= (1-(\iota+(1-\iota)(\delta+(1-\delta)h)))e(0, t) + (1-\iota)s(1)\lambda(\theta)u(1, t) \\ &\quad +(1-\iota)s(0, t)\lambda(\theta)u(0, t). \end{aligned} \tag{33}$$

Equation (32) states that the mass of unemployed not receiving UI benefits next period is equal to the mass of unemployed not receiving UI benefits this period and not finding a job, plus the mass of unemployed exhausting UI this period, the mass of employed not eligible for UI being laid off this period, the mass of employed being laid off but not claiming UI this period and the mass of inactive rejoining the labor force this period. Equation (33) states that the mass of employed next period who are not eligible for UI is equal to the mass of employed this period who are not laid off and not re-qualifying, plus the mass of unemployed with 0 or 1 period of UI left and finding a job.

Steady state. Imposing $u(t+1) = u(t) = u$, $e(t+1) = e(t) = e$, $u(\tau, t+1) = u(\tau, t) = u(\tau)$ and $e(\tau, t+1) = e(\tau, t) = e(\tau)$, we can derive the following steady-state relationships

$$u = \frac{\iota + (1-\iota)\delta}{\iota + (1-\iota)(\delta + F)}(1-n) \tag{34}$$
$$n = \frac{\iota}{\iota + \rho} \tag{35}$$
$$e = 1 - u - n. \tag{36}$$

The steady-state unemployment and employment distributions with $\tau = T$ satisfy the following equations

$$u(T) = \delta(1-\iota)\omega(T)e(T) \tag{37}$$
$$(\iota + (1-\iota)\delta)e(T) = (1-\iota)(1-\delta)h(e - e(T)). \tag{38}$$

For $0 < \tau < T$, the steady-state unemployment and employment distributions satisfy the following equations

$$u(\tau) = (1-\iota)(1 - s(\tau+1)\lambda(\theta))u(\tau+1) + \delta(1-\iota)\omega(\tau)e(\tau) \tag{39}$$
$$(\iota + (1-\iota)(\delta + (1-\delta)h))e(\tau) = (1-\iota)s(\tau+1)\lambda(\theta)u(\tau+1). \tag{40}$$

For $\tau = 0$, the steady-state unemployment and employment distributions satisfy the following equations

$$(\iota + (1-\iota)s(0)\lambda(\theta))u(0) = (1-\iota)(1 - s(1)\lambda(\theta))u(1) + \delta(1-\iota)e(0) \tag{41}$$
$$+\delta(1-\iota)\sum_{\tau=1}^{T}(1-\omega(\tau))e(\tau) + \rho n \tag{42}$$
$$(\iota + (1-\iota)(\delta + (1-\delta)h))e(0) = (1-\iota)s(1)\lambda(\theta)u(1) + (1-\iota)s(0)\lambda(\theta)u(0). \tag{43}$$

These equations are steady-state conditions, which state that the inflows (on right hand side) are equal to the outflows (on left hand side). The logic for these equations follows from the law of motion defined and described in detail further above.

Stationary equilibrium. A stationary equilibrium is defined as the labor market tightness θ, the search efforts $s(\tau)$, the wages $w(\tau)$, the UI take-up decisions $\omega(\tau)$, the mass of unemployed u, the mass of employed e, the mass of inactive n, the distributions $u(\tau)$ and $e(\tau)$, and the values $U(\tau)$, $\tilde{U}(\tau)$, $W(\tau)$, $J(\tau)$, N and V that satisfy the equations (17)-(24), (34)-(43) and the zero profit condition $V = 0$.

Calibration. We calibrate a number of parameters of our model to standard values from the literature, but others to match statistics we estimate for our short duration sample. Table D.1 summarizes all the calibrated parameter values, while Table D.2 shows the targeted moments from our short duration sample and corresponding moments from the calibrated model. We calibrate the model at the monthly frequency with a discount factor of $\beta = 0.996$. We assume a Cobb-Douglas matching function of the form $M = S^{0.72}v^{0.28}$ following Shimer (2005). We also follow Shimer (2005) in setting the workers' bargaining share to $\alpha = 0.72$. Following Hall and Milgrom (2008), we calibrate the average flow value of unemployment as $E(b_{UI} + b_L)/p = 0.71$, where b_{UI} is the unemployment benefit and b_L the value of leisure. We calibrate $b_{UI} = \$1,027$ (in December 2007 dollars) in line with our data (see Table C.1) and $p = \frac{b_{UI}}{0.35}$ in line with a 35% UI replacement rate.[70] The potential duration of UI benefits, T, is set to 8 months, which corresponds to the average level of UI benefits prior to the UI extensions we consider in our analysis for the short duration sample. The UI re-qualification probability is set to $h = 1/6$, in line with the 6 months it typically takes to requalify for UI benefits in the United States. The search cost function is assumed to take the following shape: $c(s) = \kappa s^{1+\frac{1}{\gamma}}$, where we choose κ to match the average job-finding rate of 26.7% in our sample. As a baseline, we choose $\gamma = 0.43$. This value yields a micro-elasticity of unemployment duration to potential benefit duration of 0.33, which is at the lower end the (corrected) range of 0.33-0.49 reported for this elasticity by Schmieder and von Wachter (2016). We choose the flow cost of posting the vacancy, c, to yield $\theta = 1$. This is a normalization. We choose the separation rate δ to match the E-to-U transition rate of 1.63% in our sample, the home production shock, ι, to match the unemployment rate of 6.8% in our sample, and ρ to match the labor force participation rate (LFPR) of 66.0% in our sample. Finally, we assume that UI take-up costs follow a censored uniform distribution, where the mean is chosen to match the fraction of the labor force on UI of 2.5% in our sample and the range is chosen to match the macro response

[70] This corresponds to the after-tax UI replacement rate as estimated by Anderson and Meyer (1997). Note also that in our model wages are close to productivity and thus the replacement rate in terms of wages is very close the replacement rate in terms of productivity.

Table D.1: Calibrated Parameter Values in the Model

Symbol	Parameter Description	Value	Source/Target
β	Discount factor	0.996	Annual interest rate of 5%
c	Vacancy posting cost	517.4	$\theta = 1$
μ	Matching efficiency	1	Normalization
η	Elasticity of matching function	0.72	Shimer (2005)
α	Worker's bargaining share	0.72	Hosios condition
T	Potential UI benefit duration	8	Average PBD prior to EB extensions
b_{UI}	UI benefit	1,027	Average UI benefit in 2008 dollars
p	Aggregate productivity	2,934	$\frac{b_{UI}}{p} = 0.35$; Anderson and Meyer (1997)
b_L	Flow value of leisure	1,713	$\frac{E(b_{UI}+b_L)}{p} = 0.71$; Hall and Milgrom (2008)
b_N	Flow value of home production	2,934	$b_N = p$
h	Requalification probability	0.167	Average requalification period of 6 months
δ	Separation rate	0.0164	EU transition rate
ι	Home production shock	0.0031	Unemployment rate
ρ	Home production shock	0.0061	Labor force participation rate (LFPR)
μ_G	Mean of take-up costs	6,415	Insured unemployment rate
σ_G	Std. of take-up costs	12,005	Response of UI recipiency rate to PBD
κ	Search cost scaling parameter	26,796	UE transition rate
γ	Search cost elasticity	0.43	Micro-elasticity of duration to PBD

of the UI recipiency rate to a 3-month UI extension of 4.9 percentage points that we estimate in our sample. The uniform distribution is censored at 0. That is, we assume there to be a mass point of take-up cost at 0, which is equal to the G(0) of the uncensored uniform distribution.

Solution algorithm. To solve the model, we proceed according to the following algorithm:

1. Guess value functions $U(\tau)$, $\tilde{U}(\tau)$, $W(\tau)$, and $J(\tau)$, the labor market tightness θ, and wages $w(\tau)$.

2. Solve for optimal search efforts $s(\tau)$ and UI take-up decisions $\omega(\tau)$, update value functions, and iterate until convergence of the value functions.

3. Update wages $w(\tau)$, and repeat steps 2. and 3. until convergence of wages.

4. Solve for the distributions $u(\tau)$ and $e(\tau)$, update labor market tightness θ, and repeat steps 2., 3. and 4. until $V = 0$ is satisfied.

Table D.2: Targeted Moments

Variable	Target	Model
A. Steady-state averages:		
Unemployment rate (%)	6.79	6.79
Insured unemployment rate (%)	2.45	2.45
Job-finding rate (%)	26.7	26.7
Job-separation rate (%)	1.63	1.63
Labor force participation rate (%)	66.0	66.0
B. Steady-state responses to 3-month extension of PBD:		
Micro-elasticity of duration of UI recipients	0.33	0.33
Macro-response of UI recipiency rate (p.p.)	4.94	4.94

NOTE. Panel A of the table shows steady-state averages of a number of statistics for the baseline calibration of our model with initial potential benefit duration of $T = 8$ and the corresponding data targets in the short duration sample. Panel B shows the responses to a 3-month extension of potential benefit duration in the calibration with initial potential benefit duration of $T = 8$. The corresponding data targets are the lower bound of the range of micro-elasticities reported by Schmieder and von Wachter (2016) and our estimated macro-response of the UI recipiency rate to a 3-month extension in the short duration sample.

Results. Table D.3 reports steady-state values of a number of key labor market variables in our model as well as their steady-state response to a 3-month UI extension. Panel A reports these statistics for our baseline calibration where the initial duration of UI benefits is set to 8 months. Panel B reports these statistics for the case where the initial duration of UI benefits is set to 17 months, which matches our long duration sample. A subset of the results in panels A and B are also reported in Table 5 of the main paper. For simplicity, the calibration used in panel B is the same as that used in panel A except for the duration of UI benefits.

Our model matches well the average values of the unemployment rate, the fraction of the labor force on UI, the job-finding rate, the job-separation rate, and the labor force participation rate, all of which are targeted moments in our calibration. This is best seen in Table D.2. The first column of Table D.3 reports steady-state values for additional variables. Our model predicts a UI take-up rate of 44 percent. This value is in the middle of the range reported in Anderson and Meyer (1997) and in line with the recent evidence in Lachowska, Sorkin, and Woodbury (2022).[71]

As we explained in the calibration section, we chose γ to target a micro-elasticity of unemployment duration to potential benefit duration of 0.33 for our short duration sample (panel A,

[71] We define the UI take-up rate in the model as the fraction of the UI eligible unemployed who take up UI benefits at the beginning of the unemployment spell.

row labeled "Duration, UI recipients (months)", third data column). This value is at the lower end of the (corrected) range of 0.33-0.49 reported by Schmieder and von Wachter (2016). Given this elasticity, our model predicts a micro effect of a 3-month extension on the unemployment rate of 0.30 percentage points (panel A, row labeled "Unemployment Rate (%)", second data column). This value is quite close to matching our estimated macro effect of a 3-month UI extension on the unemployment rate in the short duration sample of 0.28 percentage points (see Table 4). The macro effect in the model is somewhat larger at 0.43 percentage points due to the additional effect on labor market tightness and the response of search effort to market tightness. Overall, these results suggest that matching the evidence in the micro literature on the elasticity of unemployment duration to potential benefit duration leaves little or no room for general equilibrium effects that operate through vacancy creation.

Our model also matches the 4.9 percentage point increase in the UI recipiency rate to the extension in the short duration sample (see Table D.2). This is driven both by the longer potential benefit duration as well as increased UI take up due to the longer benefit duration. In addition to these main outcomes, Table D.3 also reports the effects on duration for non-recipients, which is zero at the micro level but positive at the macro level due to the reduced tightness and the response of search effort to it. The model also implies a small positive effect on the wage at the micro level in response to the UI extension. At the macro level, however, the effect on the wage is slightly negative due the reduced tightness and thus a lower value of the workers' outside option.

Figure D.1 provides further insight by reporting the job-finding rate and the re-employment wage by duration of unemployment and the effect of UI extensions on these variables. The figure reports these statistics separately for the case when UI duration is relatively short (T=8)—the top two panels—and in the case when UI duration is relatively long (T=17)—bottom two panels. Let's focus first on the black solid lines showing the job-finding rate and the re-employment wage prior to the UI extensions. Panels (a) and (c) show that the job-finding rate increases with duration of unemployment due to increased search effort as workers progressively exhaust their UI. After exhaustion, search effort is constant and so is the job finding rate. Panels (b) and (d) show that the re-employment wage falls as workers progressively exhaust their UI since their outside option in terms of the value of staying unemployed deteriorates.

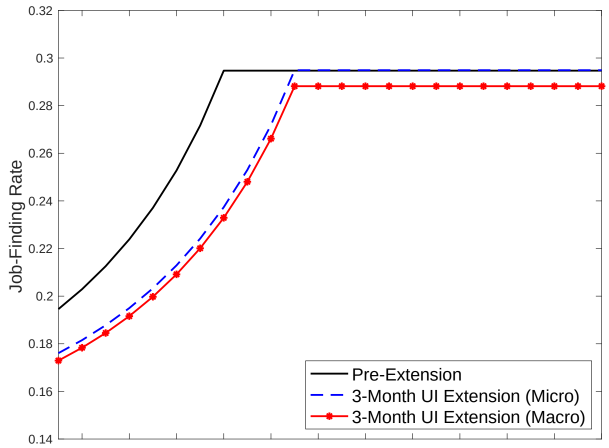
Turning to the effects of a 3-month UI extension—i.e., comparing the black solid line in each panel with the other two lines—we see that the effect of the extension is substantial. The UI extension reduces the job finding rate and it raises the re-employment wage (at least early on). However,

Table D.3: Steady-State Responses and Elasticities — More Detailed Results

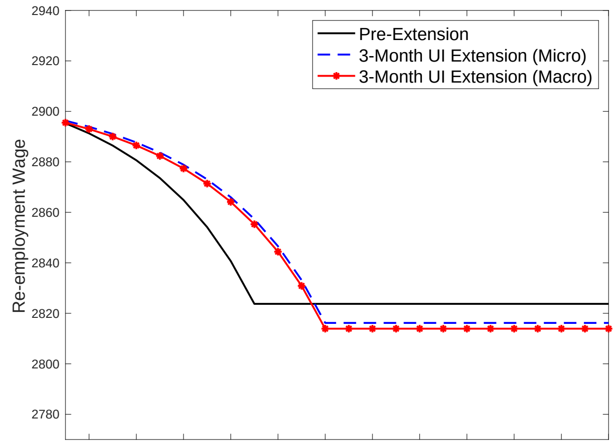
A. Short UI Duration ($T = 8$)		Micro Effects		Macro Effects	
	x	dx	$\frac{dx}{dT}\frac{T}{x}$	dx	$\frac{dx}{dT}\frac{T}{x}$
Unemployment rate (%)	6.79	0.30	0.13	0.43	0.19
Insured unemployment rate (%)	2.45	0.47	0.55	0.51	0.60
Wage ($)	2,889	0.65	0.00	-0.28	0.00
Job-finding rate (%)	26.7	-1.20	-0.14	-1.71	-0.21
Duration, all (months)	3.70	0.17	0.14	0.25	0.21
Duration, UI recipients (months)	4.38	0.49	0.33	0.56	0.38
Duration, non-recipients (months)	3.37	0.00	0.00	0.08	0.07
UI take-up rate (%)	43.9	1.02	0.07	1.19	0.08
UI recipiency rate (%)	36.1	5.12	0.42	4.94	0.40
UI per capita ($)	16.6	3.19	0.55	3.48	0.60
B. Long UI Duration ($T = 17$)		Micro Effects		Macro Effects	
	x	dx	$\frac{dx}{dT}\frac{T}{x}$	dx	$\frac{dx}{dT}\frac{T}{x}$
Unemployment rate (%)	7.98	0.22	0.17	0.32	0.24
Insured unemployment rate (%)	3.76	0.27	0.42	0.31	0.49
Wage ($)	2,888	0.28	0.00	-0.29	0.00
Job-finding rate (%)	22.4	-0.65	-0.18	-0.93	-0.26
Duration, all (months)	4.40	0.13	0.18	0.19	0.26
Duration, UI recipients (months)	5.92	0.34	0.35	0.41	0.41
Duration, non-recipients (months)	3.57	0.00	0.00	0.05	0.09
UI take-up rate (%)	46.5	0.30	0.04	0.45	0.06
UI recipiency rate (%)	47.1	1.99	0.25	1.99	0.25
UI per capita ($)	25.5	1.80	0.42	2.13	0.49

NOTE. The table shows the steady-state averages, x, as well as the steady-state responses, dx, and steady-state elasticities, $\frac{dx}{dT}\frac{T}{x}$, to an increase in the potential duration of UI benefits (T) of 3 months. Panel A shows the results for the calibration with $T = 8$ and panel B for the calibration with $T = 17$. The micro effect is defined as the effect on the job-finding rate or the re-employment wage but holding the labor market tightness constant.

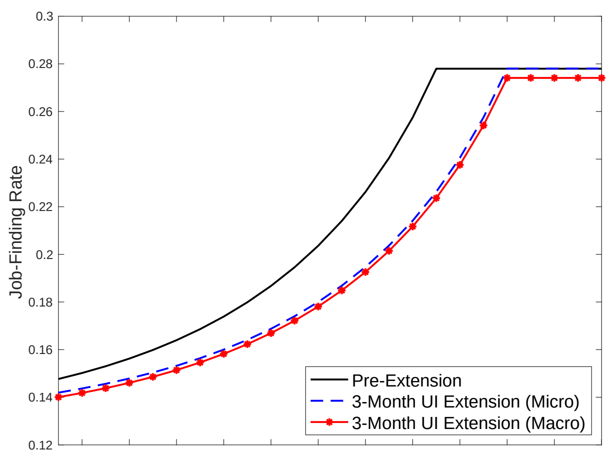
Figure D.1: The Effect of a 3-Month UI Extension on the Job-Finding Rate and Re-Employment Wage, by Duration of Unemployment



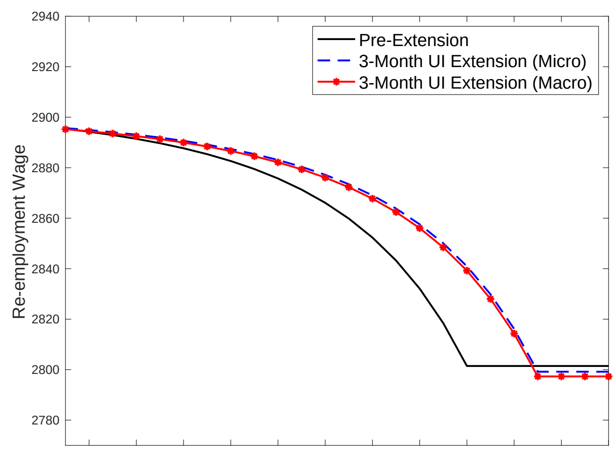
(a) Short UI Duration: Job-Finding Rate



(b) Short UI Duration: Re-Employment Wage



(c) Long UI Duration: Job-Finding Rate



(d) Long UI Duration: Re-Employment Wage

NOTE. The dashed blue line shows the micro effect, which is the effect on the job-finding rate but holding the labor market tightness constant.

the difference between the micro and macro effect is rather small. A second important observation is that effects are larger for unemployed workers who are closer to the UI exhaustion point. In fact, the re-employment wage for unemployed workers at the beginning of a UI spell hardly responds to the UI exhaustion. The reason is that they perceive the likelihood of exhausting UI as being relatively small even before the UI extension. Because most of the mass of unemployed is concentrated at the short durations, the average re-employment wage changes little in response to the UI extension, as shown in Table D.3. Finally, it is noteworthy that the re-employment wage at very high durations of unemployment actually falls in response to the UI extension. This is the entitlement effect discussed first in Mortensen (1977): in response to UI extensions unemployed workers perceive future spells of unemployment as more valuable and thus set a lower reservation wage when they exhaust UI benefits in the current spell of unemployment. As a result, the bargained wage is also lower after UI exhaustion in the economy with a higher duration of UI benefits.

Sensitivity Analysis. Our model produces a macro effect on unemployment that is too large relative to the data. As explained above this is due to equilibrium responses of tightness and the response of search effort to tightness. Table D.4 reports additional results for calibrations that aim at generating smaller general equilibrium effects such that macro effects are more in line with the data, at least for the short duration sample.

Panel A of Table D.4 shows the results presented in the Table 5 in the main paper. Panel B shows results for a calibration with a higher value of the elasticity of the matching function with respect to unemployment ($\eta = 0.85$). Note that we re-calibrate all the relevant parameters of the model to match the moments in Table C.1. This alternative calibration reduces the macro response in the model from 0.43 to 0.36, making it much closer to the data (0.28). The reason is that with a higher η, the job seekers' job finding rate $f = s\lambda(\theta) = s\theta^{1-\eta}$ becomes less responsive to changes in tightness. This occurs for two reasons: 1) tightness has less of a direct effect on job finding, but also 2) the job seekers' marginal benefit of search effort (and thus search effort), $\frac{\partial f}{\partial s} = \lambda(\theta) = \theta^{1-\eta}$, is less responsive to tightness. A value of $\eta = 0.85$ is higher than the values typically used in the literature, but still within the range of available estimates (Petrongolo and Pissarides, 2001).

Panel C reports results where we re-calibrate the flow value of leisure, b_L, to a lower value such that the flow of unemployment is $E(b_{UI} + b_L)/p = 0.5$ instead of 0.71. Surprisingly, this model calibration does not produce lower macro effects as one would expect. The reason is that we re-

Table D.4: Responses to 3-Month UI Extension for Alternative Calibrations of the Model

	Data		Model (Micro)		Model (Macro)	
Variable	$T=8$	$T=17$	$T=8$	$T=17$	$T=8$	$T=17$
A. Baseline						
Unemployment	0.28	-0.03	0.30	0.22	0.43	0.32
Insured unemployment	0.60	0.25	0.47	0.27	0.51	0.31
UI recipiency rate	4.94	1.92	5.12	1.99	4.94	1.99
B. $\eta = 0.85$ **(instead of** $\eta = 0.72$**)**						
Unemployment	0.28	-0.03	0.29	0.20	0.36	0.25
Insured unemployment	0.60	0.25	0.46	0.24	0.48	0.26
UI recipiency rate	4.94	1.92	5.01	1.87	4.92	1.86
C. $E(b_{UI} + b_L)/p = 0.5$ **(instead of** 0.71**)**						
Unemployment	0.28	-0.03	0.30	0.18	0.45	0.27
Insured unemployment	0.60	0.25	0.47	0.23	0.52	0.28
UI recipiency rate	4.94	1.92	5.14	1.77	4.95	1.80

NOTE. The table shows the responses in percentage points to a 3-month increase in the potential duration of UI benefits. Columns 1 and 2 show our empirical estimates for the short and long duration sample. Columns 3 and 4 show the microeconomic responses in our model at initial UI duration of 8 and 17 months. The microeconomic effect is defined as the change in the variable in the model when holding labor market tightness constant. Columns 5 and 6 show the full—i.e., macroeconomic—effects in the model by initial UI duration.

calibrated the model to match the moments in Table C.1, including the micro-elasticity of duration of UI recipients. A lower flow value of unemployment reduces the micro-elasticity of duration of UI recipients and thus we could match this moment only by substantially increasing the elasticity of the search cost function to a value of $\gamma = 1.7$. As a result, search effort is much more responsive to changes in equilibrium tightness. In sum, even if a lower flow value of unemployment reduces the response of equilibrium tightness to UI extensions (as one would expect), it also increases the response of search effort to changes in equilibrium tightness, leaving the overall macro responses nearly unchanged (0.45 vs. 0.43 in baseline calibration).

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