Macroeconomic Effects of UI Extensions at Short and Long Durations

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August 23, 2023

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 benefit 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 when initial durations are shorter have substantial effects on the unemployment rate and the number of people receiving UI. Through the lens of a search-and-matching model, we show that our estimates are consistent with microeconomic estimates of the duration elasticity to UI, implying small general equilibrium effects of UI extensions.

JEL Classification: E2, J6

*We thank Massimiliano Cologgi, Robert Hovakimyan, Victoria de Quadros, Anders Yding, and David Yu for excellent research assistance. We thank Isaiah Andrews, Jonathan Cohen, Peter Ganong, Alex Mas, Miikka Rokkanen, Jesse Rothstein, Rob Valletta, Christopher Walters, Danny Yagan and participants at seminars and workshops at Brookings, the Federal Reserve Board, LSE, Stanford University, University of British Columbia, UC Berkeley and UT Austin for comments and discussions. We thank Rob Valletta for sharing data on regular state UI benefits. We thank Columbia University, the Washington Center for Equitable Growth, and the Smith Richardson Foundation for research support for this project. The views expressed here are solely the responsibility of the authors and should not be interpreted as reflecting the view of the Board of Governors of the Federal Reserve System or of any other person associated with the Federal Reserve System.

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 addition to the original Extended Benefits program, every recession since 1973 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 0.41 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 negative 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 B 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, states can pay additional weeks of UI through the Extended Benefits program for a combination of two reasons: because their economy is doing worse which has led them to qualify for more UI and because they have adopted more lenient UI extension rules. 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]

Most theories of the effects of UI extensions imply that UI extensions are less consequential when potential benefit duration—the maximum amount of time that individuals can receive UI—is already elevated, since fewer individuals are (and expect to be) affected. As a consequence, we estimate the effect of UI extensions separately for periods with "short" and "long" baseline potential benefit duration.[5] When baseline potential benefit duration is below 60 weeks, we find that a standard 13-week extension raises unemployment by 0.29 percentage points. In sharp contrast, when baseline potential benefit duration is more than 60 weeks, we find that a 13-week extension raises unemployment by only 0.04 percentage points, which is not statistically significant.

Much of the prior literature on the macroeconomic effects of UI extensions has focused on

[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]We define "baseline potential benefit duration" as the potential benefit duration that a state would have paid if it had no optional trigger rules in place.

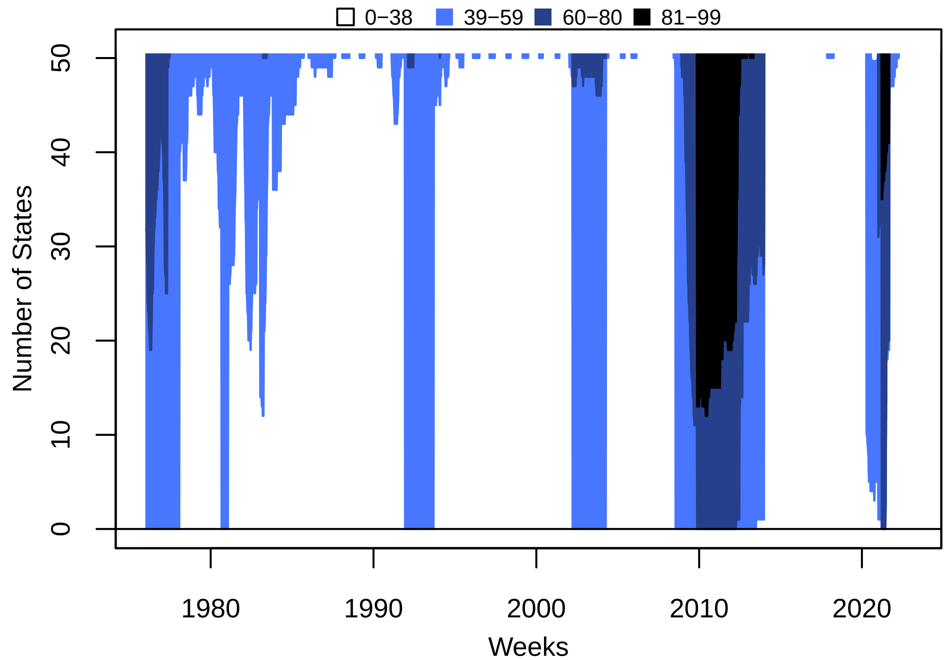


Figure 1: Distribution of Actual Potential Benefit Duration over States and Time

NOTE. This figure plots potential benefit duration for different 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.

the Great Recession and its aftermath. This period stands out as one in which potential benefit duration 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! Our long duration sample is mostly the Great Recession. Our low estimates of the effects of UI extensions at long durations are therefore consistent with earlier work that has found similarly small macro effects of UI extensions for the Great Recession (e.g., Chodorow-Reich, Coglianese, and Karabarbounis, 2019; Boone et al., 2021).

The effects we estimate at short durations are much larger. How do they compare to existing partial equilibrium estimates from the microeconomics literature? Making such a comparison involves converting both sets of estimates into the same units. A "back-of-the-envelope" calculation—similar to calculations employed by Card and Levine (2000) and Johnston and Mas (2018)—implies that our macro estimate at short durations is at the low end of the (corrected) range of micro estimates reported by Schmieder and von Wachter (2016). In other words, the gap between our "macro" estimates and the implications of earlier "micro" estimates is quite small.

An important limitation of the back-of-the-envelope calculation discussed above is that it abstracts from general equilibrium effects on the job finding rate. We also present an equilibrium search-and-matching model with endogenous search effort to formally evaluate of the macroeconomic implications of existing partial equilibrium estimates. 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.

We carry out the bulk of our analysis excluding the Covid period. Our point estimates are somewhat larger when Covid is included but the standard errors are also increased when the Covid period is included. The larger effects of the UI extensions during Covid could be driven by the higher-than-usual UI replacement rates, as well as the salience of UI and the prevalence of temporary layoffs during this period.

As we discuss above, our analysis relies on the assumption that option adoption is not endogenous to local economic fundamentals during our sample. 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 adopting options when federal funding for the Extended Benefits program was increased to 100% ("free" UI from the perspective of state budgets). There are relatively few idiosyncratic state-level changes in option adoption, and our findings are robust to dropping these cases (and the years immediately surrounding all option switches). One might still worry that the selection of states into option adoption is affected by local economic conditions, but there is no evidence that states adopt options due to bad economic shocks. Moreover, our results for the short baseline duration sample exclude variation from the Great Recession when the largest federal funding related changes in option status occurred. We explore these issues in detail below.

Implementing our empirical approach required a substantial data construction effort. We hope this effort will be useful for future researchers. It is surprisingly complex to determine when and why states trigger for UI extensions. The first step is to code up state-level UI extension rules. There is no existing source for this information going back in time. For this reason, we 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.

We focus on baseline potential benefit duration as the organizing framework for heterogeneity in the effects of UI extensions. There may, however, be other reasons why the response to UI extensions was particularly low during the Great Recession. Kroft and Notowidigdo (2016) find that more generous unemployment benefits have smaller effects on unemployment durations during severe recessions.[6] Huckfedlt (2023) emphasizes the potential for crowding out of active search during recessions. Those most at risk of long-term unemployment are less employable and thus may respond less to UI extensions. Katz (2010) and Gertler et al. (2022) emphasize that temporary layoffs have been declining over time, potentially affecting the impact of UI extensions.

Our work is most directly related to recent work that has sought to estimate the macroeconomic effects of UI extensions using quasi-experimental methods. As we have noted, much of this research has focused on the Great Recession and its aftermath. Important papers in this literature include Rothstein (2011), Amaral and Ice (2014), Farber and Valletta (2015), Marinescu (2017), Dieterle, Bartalotti, and Brummet (2020), Boone et al. (2021) and Chodorow-Reich, Coglianese, and Karabarbounis (2019) (which considers the period 1996-2015). These studies find that 13 weeks of additional UI benefits increase unemployment by only about 0.05 percentage points. There are some exceptions: Johnston and Mas (2018) find larger effects, as do Hagedorn et al. (2019).[7] Ap-

[6]Note, however, that given our identification strategy, the state-level unemployment rate prior to the extensions is similar in our short and long duration sample.

[7]Most of the papers cited above study changes to federal UI extensions that add or subtract potential weeks of benefits from the end of workers' UI spells. Johnston and Mas (2018) instead study a cut to regular state UI benefits which removed weeks of UI benefits from the middle of workers' UI spells. This caused some recipients to need to switch from regular state UI to federal UI earlier than they would have otherwise. The results of Johnston and Mas (2018) are consistent with the view that a higher-than-expected number of these workers failed to make this switch following the benefit cut. According to this interpretation, the benefit cut shares some features with benefit cuts made at relatively short durations. This may explain the large effects that the authors find. We also note that both 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. These papers consider longer time periods, different empirical specifications, and alternative data sources (since the Local Area Unemployment Statistics data employed by Hagedorn et al. uses statewide data to impute county-level data, which plays an important role in border discontinuity designs). These alternative specifications yield much smaller estimates of the effect of UI extensions on unemployment.

pendix A.1 surveys the results from these studies, and converts estimates from these studies into the same units as our results. Recent work has analyzed the consequences of UI extensions to non-traditional workers during the Covid recession (Holzer, Hubbard, and Strain, 2021; Coombs et al., 2022) and increased UI supplements (Ganong, Noel, and Vavra, 2020).

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 the early 1980s. 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

Table 1: Trigger Rules for the Extended Benefits Program

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

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). FSB stands for the "Federal Supplemental Benefits" program (1975–1978), FSC stands for the "Federal Supplemental Compensation" program (1982–1985), and TEUC stands for the "Temporary Extended Unemployment Compensation" program (2002–2004). See Table A.2 in the appendix for a full description of the rules for federal programs.

analysis. Table A.2 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.[8] This second condition is known as a lookback provision.

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

[8]This rule is triggered if $IUR_{MA(t)}$=[($IUR_{MA(t\ \ 52)}$ + $IUR_{MA(t\ \ 104)}$)=2] > 1:2 where $IUR_{MA(t)}$ 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.

condition triggers on again). We refer to the 13-week minima discussed in this paragraph as the "13-week rule."[9]

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 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.[10]

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?

The EB program is 50% federally funded and has at times been made 100% federally funded. The federal government, thus, heavily subsidizes state adoption of the optional components of this program. Nevertheless, some states have not adopted the IUR or TUR options. 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.

[9] 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.

[10] 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.

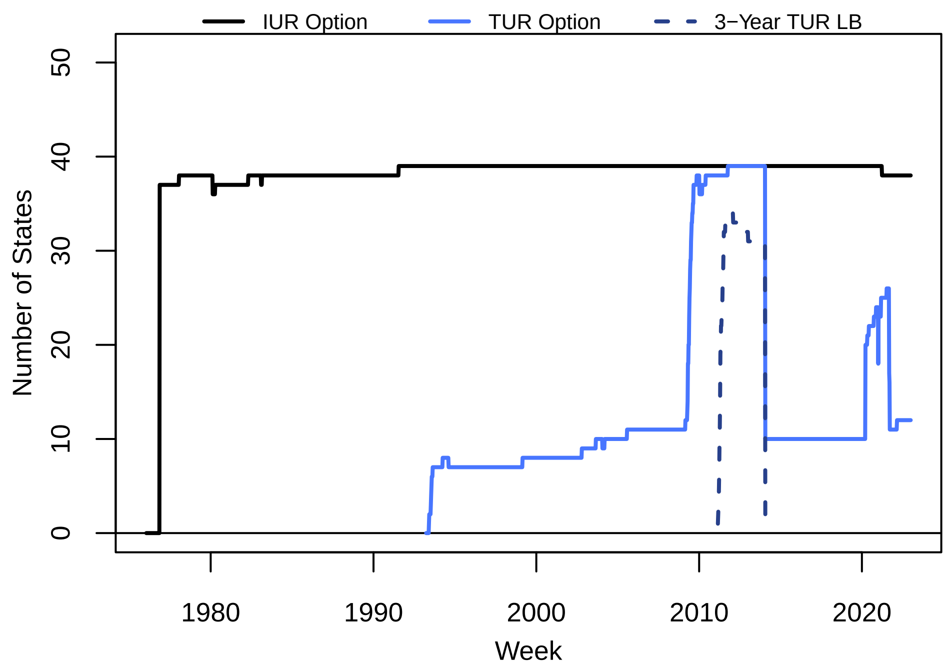


Figure 2: Changes in Option Status

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.

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 2012, 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.

A potential threat to our identification strategy is that these option switches may be endogenous to the state of the business cycle in the switching states (conditional on time and state fixed effects). We have manually examined each option switch over the period 1981-2022 and categorized each one according to what we understand to be the dominant policy motive behind it. Table 2 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

Table 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.

"costless" to state budgets to adopt the IUR and TUR options. This led a large number of states to temporarily adopt the TUR option ("free" UI) while the temporary federal provisions were in place. Some state introduced legislative clauses that mechanically tied adoption of the TUR option to 100% federal funding.[11]

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.[12] Between the passage of the Act in August 1981 and its full implementation

[11]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. In the aftermath of the 2001 recession, the 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.

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

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.[13] Table 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 2.

The timing of the options changes we describe above was a consequence of national policy, rather than state-level labor market performance. 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.[14] 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 to 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? 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 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.

This section establishes that most options changes are driven by changes in federal legislation and funding (and our results are invariant to dropping time periods around the remaining ones). However, we must also consider the possibility that states select into adopting options (in response to federal legislation) 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 5.6. We

[13]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.

[14]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.

find no evidence that states experiencing worse economic conditions (in terms of unemployment) are more likely to adopt options that make UI more generous.

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 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 Department of Labor's *Chronology of Federal Unemployment Compensation Laws*.[15] 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.2.

Table 3 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 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. In the remaining cases, we believe that states indeed paid "incorrect" benefits—

[15] Available at https://oui.doleta.gov/unemploy/pdf/chronfedlaws.pdf.

Table 3: UI Benefits Calculator Performance

Program	Applicable State-Weeks	Incorrect	% Correct
EB (1976–2022)	125,001	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	163,047	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.

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 publishes Trigger Notices for the EB program and each supplemental federal extension program that has trigger rules.[16] It also indicates whether each state has triggered on each program—this information underlies the performance test of our calculator in Table 3.

We were able to scrape Trigger Notices for the period since October 2002 from the website of the Department of Labor (DOL).[17] 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

[16]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.

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

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.[18]

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.[19] For months between publication we linearly interpolate the maximum UI benefit duration.[20]

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. We use the Local Area Unemployment Statistics (LAUS) from the Bureau of Labor Statistics (BLS) to measure unemployment, the labor force, and population.[21] 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 merged our new data with UI administrative records on payouts and the number of people on UI. We obtained data on UI payments, the number of initial claims, and the number of UI recipients by state from DOL administrative data (report ETA 5159).[22] 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.

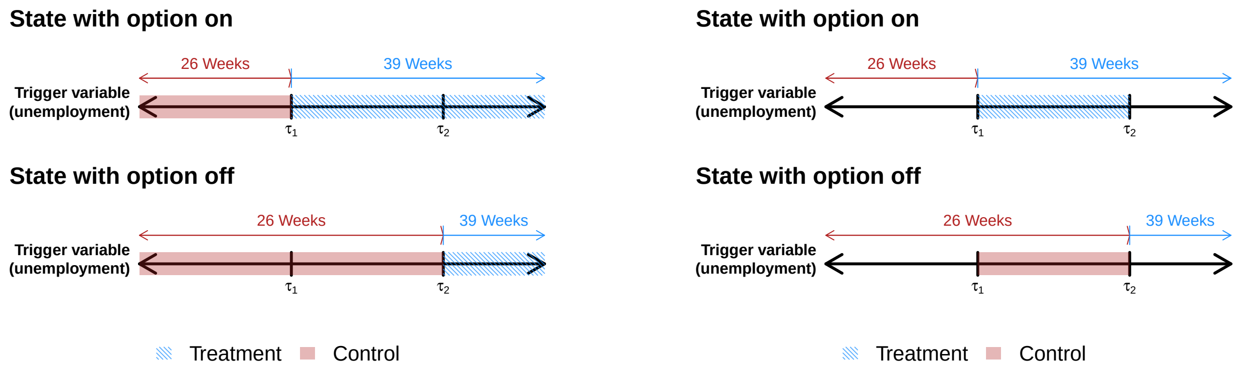
[18] We were unable to find data for 34 weeks over the period 1976–2002: 12 of these were never printed because of government shutdowns (4) or program lapses (8); 13 come from September–December 1981; and the final nine are from other sporadic weeks. For all but the 1981 episode we are able to impute 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).

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

[20] 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).

[21] We use seasonally-adjusted labor force and unemployment figures.

[22] 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 $_2$ (the mandatory threshold), or if its unemployment rate is above $_1$ (the optional threshold) and it has elected to have that optional threshold in place ("on").

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: $_1$ and $_2$. The higher threshold, $_2$, 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, $_1$, is an optional threshold: some states have adopted this trigger and offer 13 weeks of additional benefits when their unemployment rate is above $_1$, 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-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 ${}_1$. Panel (b) of Figure 3 illustrates this approach. Notice that the two triggers, ${}_1$ and ${}_2$, partition the unemployment rate into three intervals: $[0; {}_1]$, $[{}_1; {}_2]$, and $[{}_2; 1]$. 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 $[0; {}_1]$ and $[{}_2; 1]$. The coefficient on weeks of benefits is therefore identified only off of variation in the middle interval $[{}_1; {}_2]$. 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.[23]

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 why we think this threat is not a serious one in our setting, and revisit the question empirically in section 5.6.

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 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

[23] We discuss identification in this simple illustrative example in more detail in Appendix A.8.

Benefits Calculator to determine option status.

4.1 An Example: Arkansas

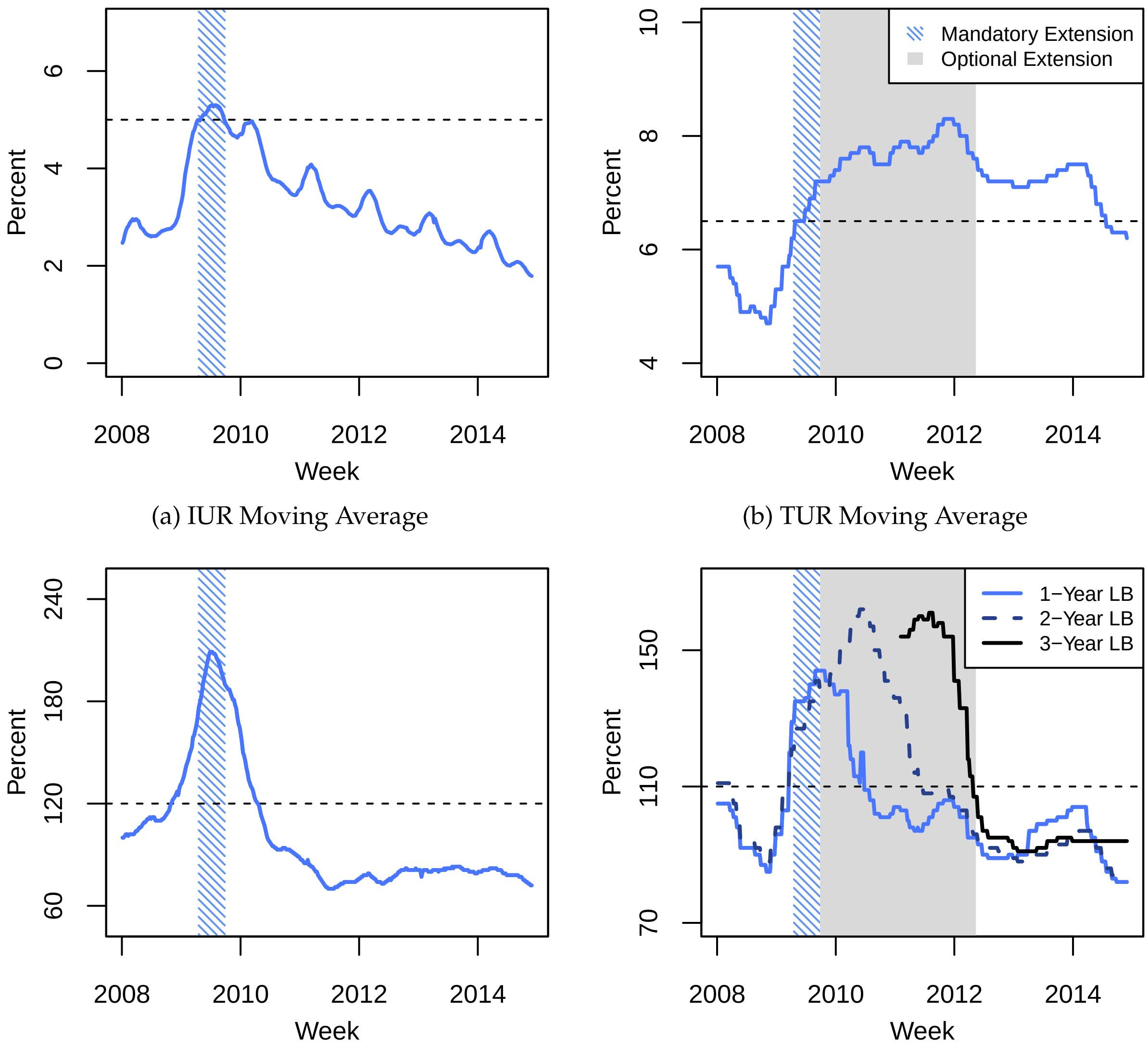
Let us now consider a real-world example of the identification approach described above: the case of Arkansas. Figure 4 plots time series for the relevant trigger variables for the EB program in Arkansas during the Great Recession and the recovery that followed. These variables and Arkansas' decisions 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 horizontal 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 option 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 option 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 and not for the much longer *solid-gray* period. Had Arkansas implemented the optional TUR trigger rules, however, UI recipients would have been eligible for EB extensions 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



(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 (i_w in week w), along with its mandatory 5% threshold. Panel (c) show the IUR "lookback" variable, defined as $100 \quad i_w = \frac{i_{w \ 52} + i_{w \ 104}}{2}$, along with its mandatory 120% threshold. Panel (b) shows the 3-month moving-average of the unemployment rate (u_m in month m), along with its optional 6.5% threshold. Finally, panel (d) shows the TUR lookback variables, given by $100 \quad u_m = u_{m \ 12}$, $100 \quad u_m = u_{m \ 24}$, or $100 \quad u_m = u_{m \ 36}$. The third lookback was only a trigger variable over the period 2011–2014. 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.

A.4, we present a similar analysis for a state that adopted the optional TUR trigger rule 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, while the first of these factors is clearly endogenous to current labor market conditions. 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.[24]

The considerations discussed above motivate using the following empirical specification:

$$y_{s,t+h} = {}_h\widehat{W}_{s,t} + \text{qual. controls}_{h,s,t} + {}_{h,s} + {}^{0}x_{h,s,t} + {}_{h,s,t}. \tag{1}$$

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

[24] Borusyak and Hull (2020) develop a general framework for analyzing this type of setting, and propose a simulation-based estimator for situations in which the approach we use is infeasible.

different horizons h.[25]

Our treatment variable of interest $\widetilde{W}_{s;t}$ 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. The coefficient on this variable $\;_h$ measures the effect of a UI extension on the outcome variable at horizon h. Why use $\widetilde{W}_{s;t}$ as opposed to simply the level of actual potential benefit duration in state s at time t as the treatment variable? The reason for this is that $\widetilde{W}_{s;t}$ 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 $\widetilde{W}_{s;t}$. Focusing on $\widetilde{W}_{s;t}$, 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.[26]

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

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

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 a state satisfies the trigger rules for. Which options were available varied over time. The full set is the 1) IUR option, 2) TUR option with 1- or 2-year lookback, 3) TUR option with 1-, 2-, and 3-year lookback, 4) second tier of TUR option with 1- or 2-year lookback, and 5) second tier of TUR option with 1-, 2-, and 3-year lookback. The risk sets for a give point in time are all the possible subsets of the set of options that were available at that time (including the empty set,

[25]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'Haultfæuille, 2020; Borusyak, Jaravel, and Spiess, 2023). These estimators cannot be easily applied to our setting because it is ambiguous whether the treated units are those who receive or who 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.

[26]Most EB extensions are 13 weeks (or 20 weeks including the second TUR tier), i.e., the most-frequent non-zero values of $\widetilde{W}_{s;t}$ are 13 and 20. There is, however, some variation. In the early stages of the FSC and 2008 EUC programs, the benefits available from these programs depended on a state's EB status. In addition, the EB program rules (for the "13-week" tier) specify that EB offers the minimum of 13 weeks and 50% of the weeks provided under regular state UI. In cases where the state offers less than 26 weeks, EB extensions may be less than 13 weeks. This occurs about 10% of the time in our data. We discuss these cases in more detail in Appendix A.5. Panel (b) of Table A.5 presents results for a specification that removes this "non-standard" variation in $\widetilde{W}_{s;t}$ using a binary alternative. This yields results similar to our baseline results.

i.e., no options). Intuitively, we are interacting time fixed effects with dummies for the risk sets. Within these risk sets, the variation in $\widetilde{W}_{s,t}$ arises from a state's option status.[27]

In addition to the qualifying controls, we also include state fixed effects and a vector of additional controls $x_{s,t}$ to account for differences across states that may be correlated with option adoption. The additional controls are lags of potential benefit duration and $y_{s,t}$ in the distant past (quarterly lags for t-24 to t-48). These help equalize the level of the outcome variables in the distant past. As stated above, we assume that option adoption is exogenous to local labor market conditions conditional on these controls. Table A.5 contains results with additional controls.[28]

We also estimate a "differences" version of equation (1) with $\ \widetilde{W}_{s,t}$ as the main independent variable of interest:

$$y_{s,t+h} = \ _h \ \widetilde{W}_{s,t} + \text{qual. controls}_{h,s,t} + \ _{h,s} + \ ^{0}x_{h,s,t} + \ _{h,s,t}. \tag{3}$$

The only other difference versus equation (1) is that we must include qualifying controls for both period t and $t \ 1$ to account for reverse causality.[29] This differences specification allows us to gauge the timing of the effects of changes in $\widetilde{W}_{s,t}$ more clearly than the levels specification. However, $\widetilde{W}_{s,t}$ may be an imperfect measure of workers' and firms' *perceptions* about the EB program's effects on the duration of UI. Given the complexity of the rules, this seems likely. In particular, the timing of changes in people's perceptions may not line up perfectly with $\ \widetilde{W}_{s,t}$. In this case, the differences specification is likely to amplify attenuation bias due to such perception errors (Griliches and Hausman, 1986). For this reason, we view the levels specification—equation (1)—as the main specification in the paper and we present a larger set of results for that specification.[30]

We estimate equations (1) and (3) by OLS. We cluster standard errors by state and month. 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

[27]In Appendix A.6, 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—though less precisely estimated—results.

[28]Briefly, we estimate a version that includes lagged controls of several labor market variables (panel (c)), a version that includes industry-employment shares (panel (d)), and a version with the state-level Covid stringency index of Hale et al. (2021) as a control (panel (e)).

[29]That is, qual. $\text{controls}_{h,s,t} = \sum_{z,t}(\ _{z,h,t} I_s(z,t) + \ _{z,h,t \ 1} I_s(z,t \ 1) + \ _{z,h,t} I_s(z,t) \ \ _{z,h,t \ 1} I_s(z,t \ 1))$.

[30]In addition, the LAUS unemployment rate is based on a Kalman smoother. This implies that we have a better measure of the level of the unemployment rate than of high-frequency changes in unemployment.

drop the small number of state-months for which our UI Benefits Calculator predicts UI benefits incorrectly.

A potential concern with our methodology is that it can underestimate the magnitude of the effect of UI extensions due to a dynamic selection effect. If UI extensions raise the unemployment rate, they affect when states that receive EB extensions satisfy trigger rules—e.g., they may lead a state to satisfy a trigger rule for longer than it otherwise would. Since this is not the case for states with options off, states that are identical except for their option adoption can end up in different risk sets leading to a bias. This bias is towards zero and its size shrinks to zero as the true treatment effect goes to zero. Appendix A.8 provides a more detailed explanation of this bias and discusses a bias correction method. In practice, we find that this bias is small.

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 also extend our sample to include the period since the outbreak of the Covid pandemic. Given that extensions at long baseline durations affect many fewer people than those at short durations, we estimate the effect of UI extensions separately for periods with "short" and "long" baseline potential benefit duration (<60 weeks vs. 60 weeks). The long duration sample comes mostly from the Great Recession and its aftermath (Figure A.2). This period has also been the focus of most of the prior quasi-expemental literature on the macro effects of UI extensions.

We estimate these short and long duration effects using a pooled specification where we allow for different state fixed effects and different $_h$ coefficients across the short-duration and long-duration subsamples, but pool the remaining coefficients.[31] In our short-duration sample and in months in which at least one state is paying EB, the average baseline potential benefit duration is 35 weeks. This average is 74 weeks in our long-duration sample. We present results for a case where the cutoff is set to 40 weeks rather than 60 weeks in panel (i) of Table A.5. Results are similar to our baseline results.

[31] Panels (f), (g), and (h) of Table A.5 present results for alternative assumptions about pooling the auxiliary coefficients across the short and long-duration samples. These results are similar to those for our baseline specification. 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).

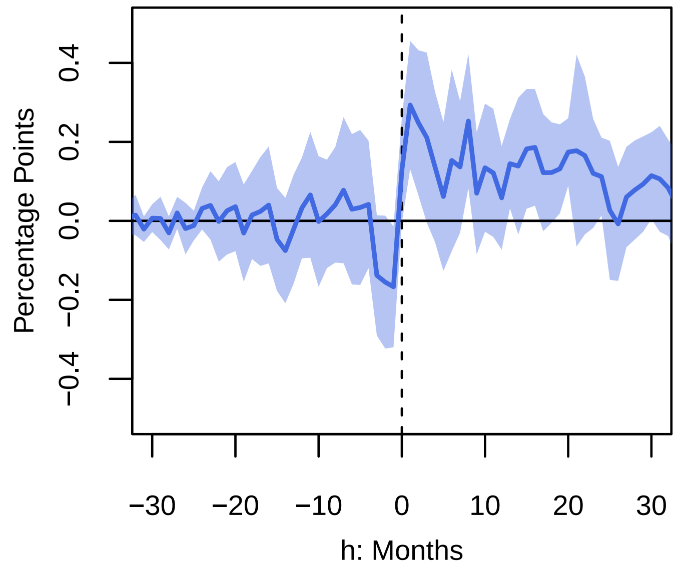


Figure 5: Effect of $\widehat{W}_{s,t}$ on Percent of Labor Force Receiving UI

NOTE. This figure shows estimates of β_h—the effects of a UI extension when baseline potential benefit duration is short—in equation equation (3), with the percent of the labor force receiving UI as $y_{s,t+h}$. Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in -35, \ldots, 35$. The sample runs from 1980m1–2019m12, and excludes Alaska. Standard errors are clustered by state and month.

5.1 The Short Baseline Potential Benefit Duration Sample (<60 Weeks)

Figure 5 presents results from our "differences" specification—equation (3). This specification is useful for establishing the timing of effects. Hence, we present results for the faction of the labor force on UI, since this outcome variable is particularly good for determining high frequency movements—it is estimated using administrative data on counts of people receiving UI. We see that the fraction of the labor force on UI increases abruptly exactly when a state triggers onto EB ($h = 0$). There is no pre-trend (as might arise from reverse causality). In fact, there is a dip before $h = 0$ reflecting the fact that states cannot trigger onto EB unless they have been off EB for 13 weeks (and conversely, cannot trigger off if they haven't been on for 13 weeks). Appendix A.9 presents estimates of equation (3) for other outcome variables.

Table 4 presents estimates from our "levels" specification—equation (1)—which for reasons discussed in section 4 is better suited to assess the quantitative magnitude of the effects of UI extensions. Table 4 presents estimates of β_0 for potential benefit duration, the fraction of the labor force on UI, and the unemployment rate.[32] The table shows that a standard 13-week extension of potential benefit duration leads to approximately a 0.6 percentage point increase in the fraction

[32]We use the unemployment rate from the LAUS, which measures the unemployment rate using a smoother. In panel (j) of Table A.5, we show that using a measure constructed directly from the CPS yields similar results.

Table 4: Effects of a 13-Week Benefit Extension

	PBD	Frac. LF on UI	Unemployment
$\tilde{W}_{s,t}$, Short Dur.	12.9	0.60	0.29
	(0.2)	(0.07)	(0.11)
Observations	23795	23795	23795

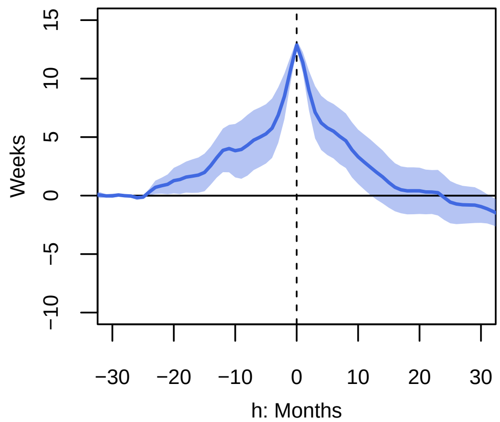
NOTE. This table shows estimates of $_0$ in equation (1) for extensions that occur when baseline potential benefit duration is short, with the variables in the column headers as left-hand side variables, all multiplied by 13. PBD refers to potential benefit duration. The sample runs from 1980m1–2019m12, excludes Alaska, and only uses state-months for which the baseline potential benefit duration is below 60 weeks. Standard errors are clustered by state and month and shown in parentheses.

of the labor force on UI and a 0.29 percentage point increase in the unemployment rate. The effect on both of these variables is highly statistically significant. Interestingly, the effect on the unemployment rate is substantially larger than most existing estimates—see the discussion in Section 1 and Appendix A.1. We also see that the effect on the fraction of the labor force on UI is about twice as large as the effect on the unemployment rate. This is intuitive since the extension leads some people who would otherwise exhaust benefits to collect UI for longer even if they are not unemployed for longer.

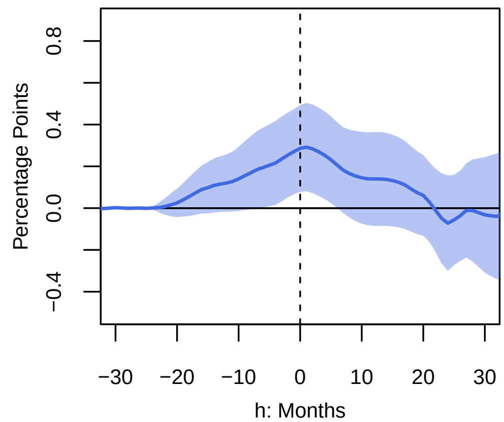
Figure 6 presents estimates of equation (1) for different values of h into the past and future. The top left-hand panel presents results for potential benefit duration. This is the treatment variable in our natural experiment. The estimates in this panel are therefore akin to "first-stage" estimates. The effects we estimate on potential benefit duration display a tent-shaped pattern: the maximum effect is at $h = 0$ and effects fall in magnitude roughly symmetrically for positive and negative horizons h. The point estimates of the effects on potential benefit duration remain positive for roughly 20 months into the past and future.

The reason we see effects on potential benefit duration both in the past and future is simple: the treatment variable $\tilde{W}_{s,t}$ is a persistent variable. This means that a high value of $\tilde{W}_{s,t}$ in a particular period signals that $\tilde{W}_{s,t}$ is also high in surrounding periods. Another way to say this is: if a state was on EB in a particular month, it is likely the state was also on EB in surrounding months. We view the persistence of the effects we estimate on potential benefit duration to be 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.

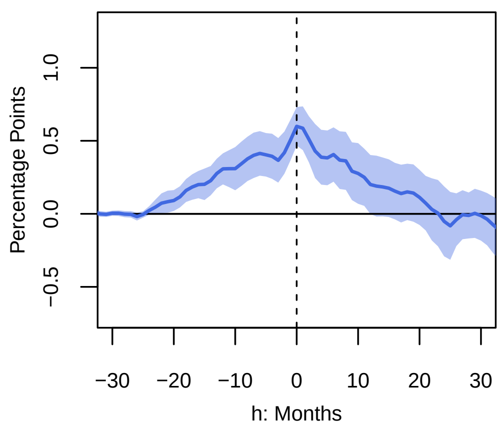
The remaining panels of Figure 6 present estimates for the five outcome variables we consider: the fraction of the labor force on UI, the unemployment rate, the CES employment rate, the UI



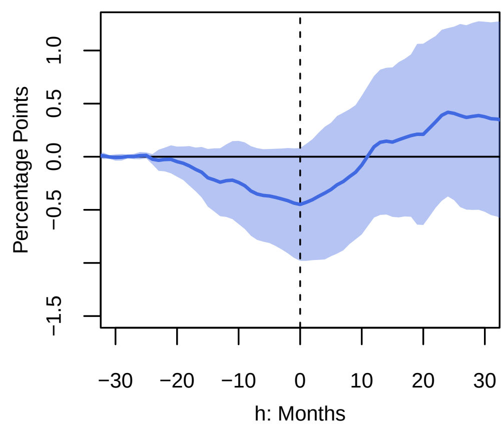
(a) Potential Benefit Duration



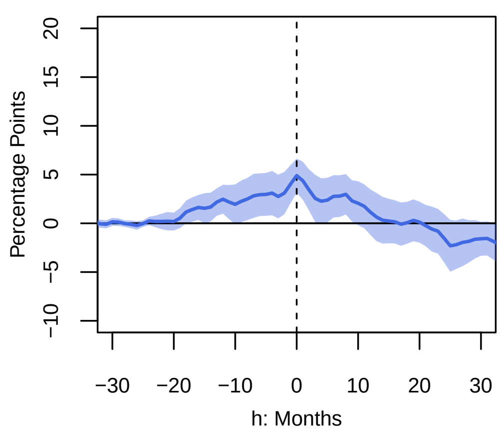
(b) Unemployment Rate



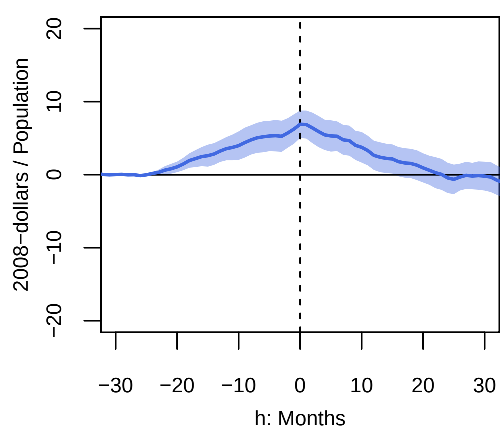
(c) Fraction of LF on UI



(d) Employment Rate, CES



(e) UI recipiency (UI/U)



(f) Dollars Spent per Capita

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

NOTE. Each panel shows OLS estimates of β_h from equation (1) for extensions that occur when baseline potential benefit duration is short, with the variable in the panel titles as left-hand side variables. Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in \{-35, \ldots, 35\}$. The sample runs from 1981m1–2019m12, and excludes Alaska. Standard errors are clustered by state and month.

recipiency rate, and dollars spent per capita. These results can be viewed as "reduced form" results of our analysis. A striking feature of the results for all five of these outcome variables is that they have the same tent-shaped profile as do the effects on potential benefit duration. In all cases, the results are roughly symmetric around $h = 0$ and non-zero roughly between $t - 20$ and $t + 20$ months.

The simplest interpretation of these facts is that the tents reflect the persistence of the treatment variable as opposed to any true dynamic effects. Were there substantial true dynamics effects, we would likely see some difference in the dynamics of the estimates for the outcome variables relative to the estimates for potential benefit duration. We therefore interpret our results as supporting the notion that UI extensions mostly have contemporaneous effects on other labor market outcomes.

In the middle right-hand panel, we see that UI extensions cause employment rates measured from the BLS's establishment survey to decline just as they cause unemployment rates to rise. The effect on the employment rate is similar in magnitude to the effect on the unemployment rate: a decline of 0.4 percentage points at $h = 0$. However, our estimates of the effects on employment are much noisier, with confidence intervals that intersect zero. A likely explanation for this relative imprecision is the larger time variation in trends in the employment rate. The employment rate is sensitive to secular shifts in multiple job holding, commuting behavior, female employment, agricultural employment, and self-employment. An advantage of reporting results for the employment rate is that they come from an entirely different data source (CES) than the unemployment rate (LAUS).

The lower left-hand panel of Figure 6 presents results for the UI recipiency rate—the fraction of the unemployed on UI benefits, which reflects both the UI take-up rate and the fraction of unemployed eligible for UI.[33] We see that the UI recipiency rate rises by 5 percentage points at $h = 0$ in response to UI extensions. This is intuitive since UI extensions lower the fraction of UI recipients who exhaust their benefits and raise the benefits of applying for UI.

The last outcome we present results for in Figure 6 is an accounting measure of dollars spent on UI. We calculate this as total UI payments in each month divided by the labor force. We convert this measure to December, 2007 dollars using the national PCE price index. UI extensions raise dollars spent on UI by 7 dollars per month per capita. Notably, this effect is quite small. It is

[33]We define the UI recipiency rate as the ratio of people receiving regular state UI, EB, or other federal UI benefits to the number of people counted as officially unemployed. Two highly cited estimates of the UI takeup rate at the low and high end of estimates in the literature are 39% (Anderson and Meyer, 1997) and 65% (Blank and Card, 1991).

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 since many more people are directly affected.

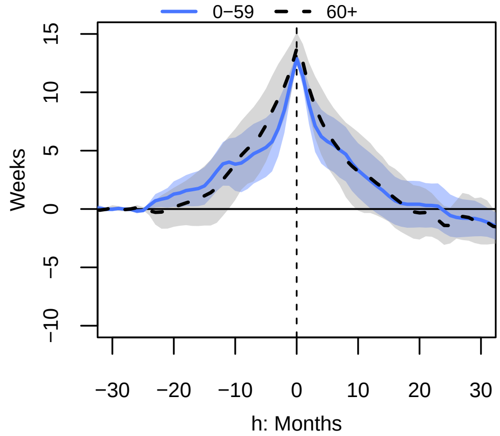
We have emphasized the importance of qualifying controls in accounting for reverse causation in our regression specification. Figure A.5 in the appendix demonstrates this point quantitatively. When we drop the qualifying controls, UI extensions appear to have a substantially larger effect on the unemployment rate (including long-run effects). The effect is particularly large if we also drop time fixed effects.

5.2 The Long Baseline Potential Benefit Duration Sample (60 weeks)

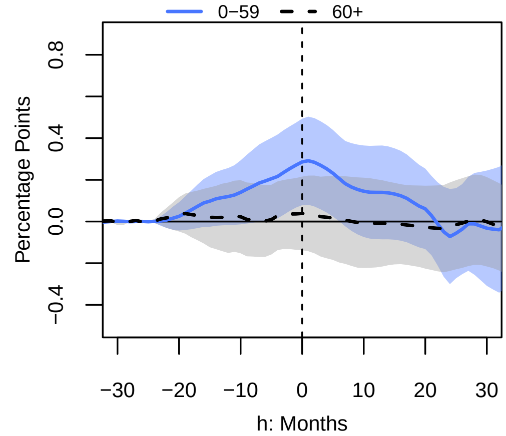
Figure 7 presents results for UI extensions that occur at longer baseline potential benefit durations, and contrasts these with the results we have already presented for shorter baseline potential benefit durations. The solid blue lines are estimates for short baseline potential benefit durations (< 60 weeks)—"short durations" for short—while the black-dashed lines are estimates for long baseline potential benefit durations (60 weeks)—"long durations" for short. The effects on the treatment variable—potential benefit duration—in the top left-hand panel of Figure 7 are similar at long durations and at short durations. In particular, they have a similar tent-shaped profile.

The results for the remaining variables, however, differ sharply at short vs. long durations. The estimated effects at long durations are much smaller than those at short durations, and they are generally statistically insignificant. Focusing on effects at $h = 0$, the effect we estimate for the unemployment rate is only 0.04 percentage points at long durations (versus 0.29 at short durations); for the fraction of the labor force on UI, the effect we estimate is 0.2 percentage points at long durations (versus 0.6 at short durations); for the UI recipiency rate, the effect is 1 percentage point at long durations (versus 5 at short durations); for dollars spent on UI per capita, the effect is 2 dollars at long horizons (versus 7 dollars at short horizons). The effect we estimate for the employment rate has the "wrong" sign at long durations. But this response is estimated with little precision.

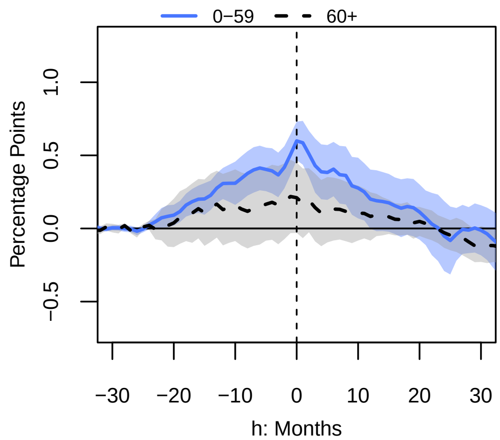
These small estimates at long durations are consistent with the findings of other empirical



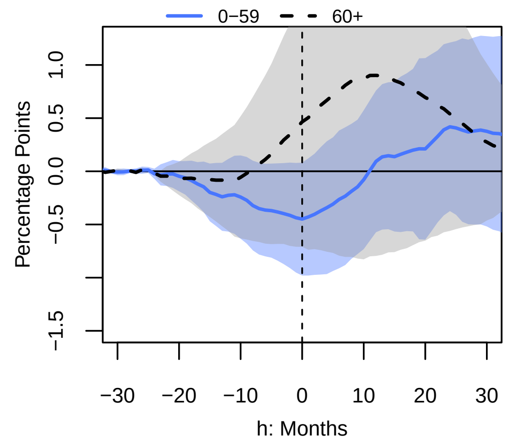
(a) Potential Benefit Duration



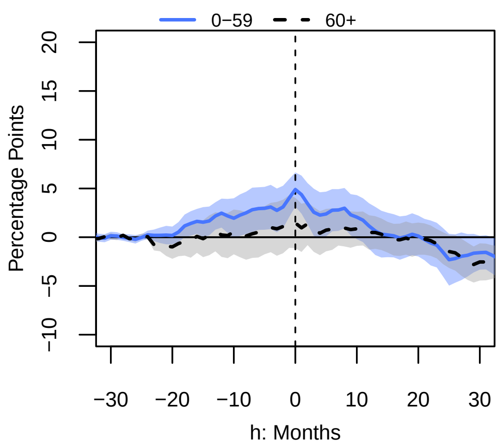
(b) Unemployment Rate



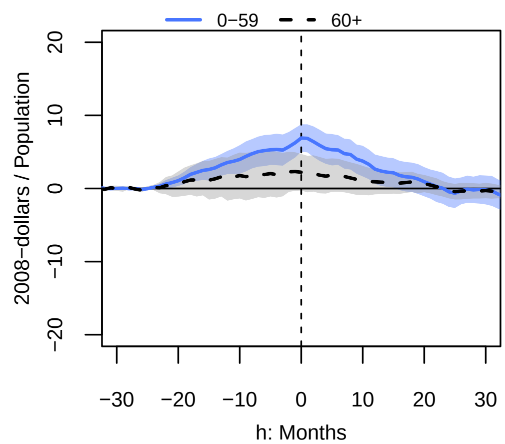
(c) Fraction of LF on UI



(d) Employment Rate, CES



(e) UI Recipiency Rate (UI/U)



(f) Dollars Spent per Capita

Figure 7: Macro Effects of UI Extensions: Long vs. Short Baseline Potential Benefit Durations

NOTE. Each panel shows OLS estimates of $_h$ 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 2 35; : : : ; 35. The sample runs from 1981m1–2019m12, and excludes Alaska. Standard errors are clustered by state and month.

studies of the macro effects of UI extensions for samples dominated by the Great Recession period. Appendix A.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. Figure A.7 in the appendix presents estimates for a case 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.

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. Figure 8 plots the cumulative distribution function (CDF) of unemployment durations over two periods: 1994-2021 and 2012.[34] Since most unemployment spells are short, the CDF rises rapidly at low values. 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.

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.[35]

UI extensions during the recoveries after deep recessions sometimes involve substantial political uncertainty. This occurred both after the Great Recession—when the federal EUC and EB programs 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.

[34]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.

[35]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.

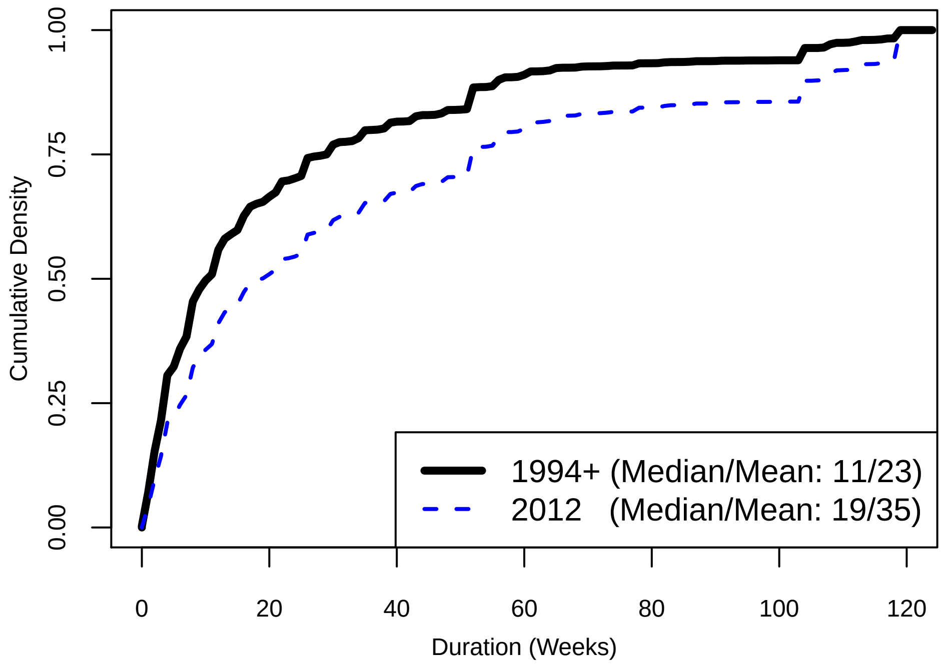


Figure 8: CDF of Unemployment Spell Durations

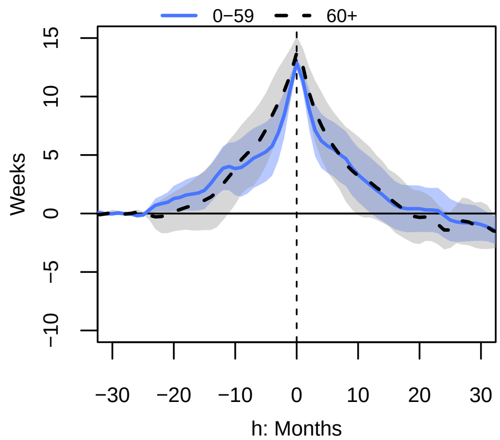
NOTE. This figure shows the empirical cumulative distribution function of unemployment spell durations from the CPS, in weeks. The CPS duration mnemonic is `DURUNEMP`, which we weight by `WTFINL`. We retain unemployed individuals who are at least 16 years old. Our mean estimate is below the officailly-reported mean because the public-use microdata is top coded. This binds in 2012, when the mean was closer to 40 weeks. The data run from 1994 through 2021.

5.4 Effects Including the Covid Recession

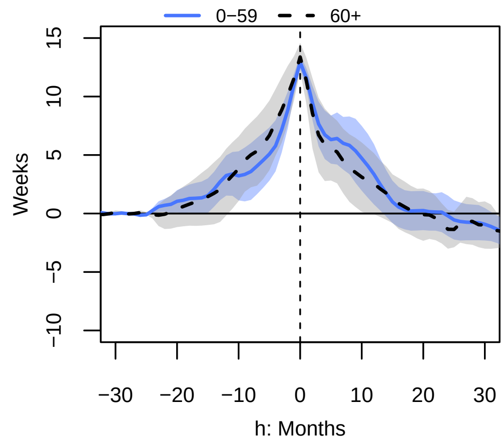
We next extend our sample period to 2022, to include the pandemic recession and ensuing recovery. This period saw UI extensions at both short and long baseline potential benefit durations. The left-hand side panels of Figure 9 repeat our results for the baseline sample that ends in 2019 (i.e., the results from Figure 7), while the right-hand side panels show analogous results for the sample that ends in 2022.[36]

For the short duration sample, the point estimates are roughly 50% larger including the Covid recession (though the standard errors also become larger). Focusing on the effects at $h = 0$, the response of the unemployment rate is 0.44 percentage points (vs. 0.29 pre-Covid). The fraction of the labor force on UI rises by 0.9 percentage points (vs. 0.6 pre-Covid). UI extensions raise UI recipiency by 6 percentage points (vs. 5 pre-Covid) and dollars spent per capita by $9 including Covid (vs. $7 pre-Covid). Panel (l) shows that initial UI claims responded strongly to UI extensions at short durations during the pandemic recession in contrast to the pre-Covid sample. This

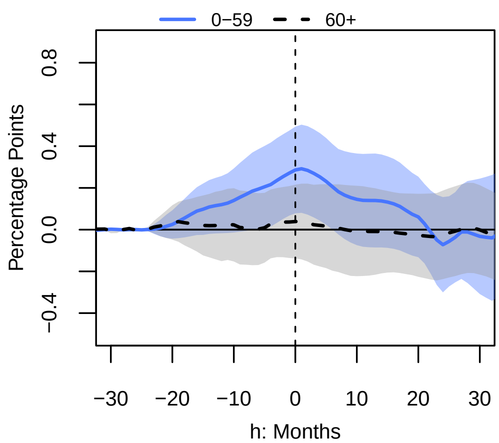
[36]Panel (k) of Table A.5 presents results estimated using only observations from 2020–2022. Unsurprisingly, these estimates are quite noisy.



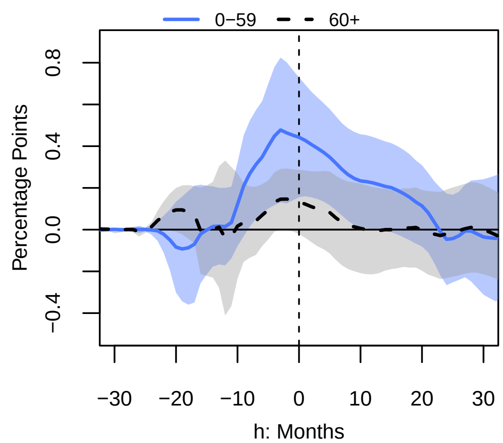
(a) Potential Benefit Duration, 1980–2019



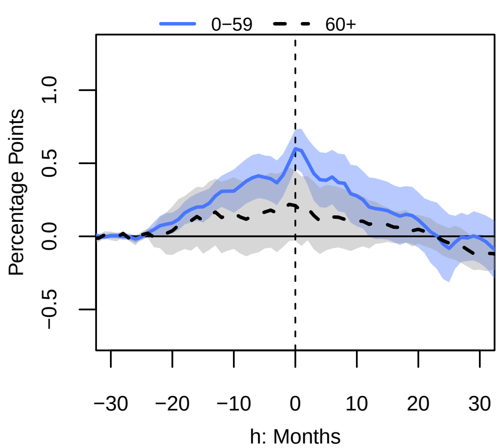
(b) Potential Benefit Duration, 1980–2022



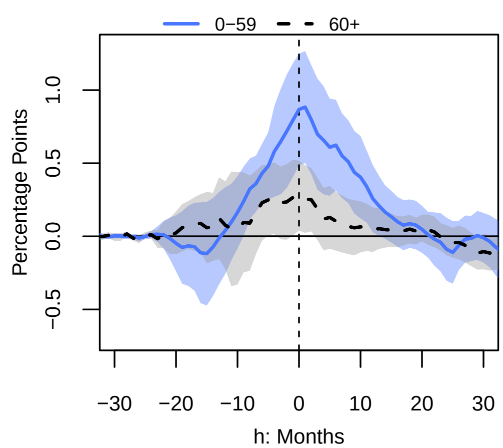
(c) Unemployment Rate, 1980–2019



(d) Unemployment Rate, 1980–2022

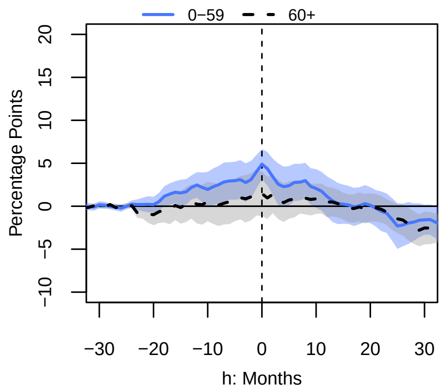


(e) Fraction of LF on UI, 1980–2019

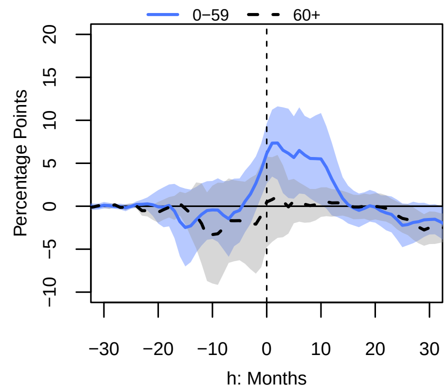


(f) Fraction of LF on UI, 1980–2022

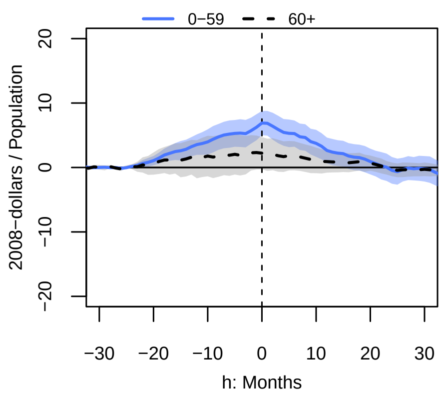
Figure 9: Effects Including Covid Recession



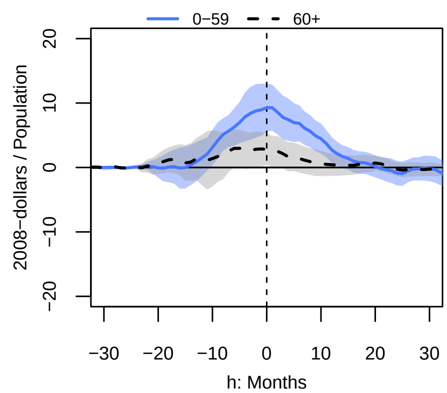
(g) UI Recip. Rate (UI/U), 1980–2019



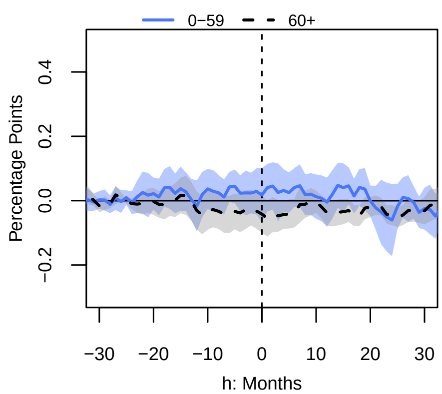
(h) UI Recip. Rate (UI/U), 1980–2022



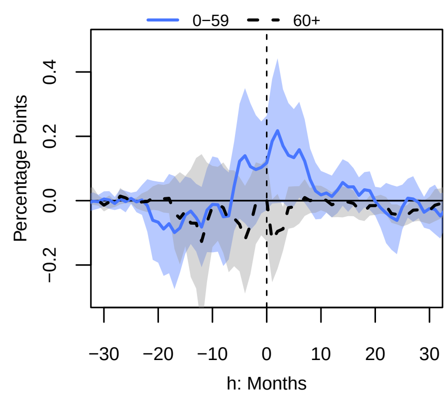
(i) Dollars Spent per Capita, 1980–2019



(j) Dollars Spent per Capita, 1980–2022



(k) Initial Claims/LF 1981–2019



(l) Initial Claims/LF 1981–2022

Figure 9: Effects Including Covid Recession (Continued)

NOTE. Panels (a), (c), (e), (g), and (i) are identical to the corresponding panels in Figure 7. Panel (k) shares the same specification as those estimates, but has new UI claims divided by the labor force as the outcome variable. The remaining panels extend the sample period from that baseline, which ends in 2019, through 2022. The estimates in these panels for $h > 1$ contain progressively fewer observations as fewer leads become available. Standard errors are clustered by state and month.

is the case despite the fact that the treatment variable, potential benefit duration, behaves similarly excluding and including the Covid period. The results at long durations are, for the most part, still small and statistically insignificant.

5.5 Why Was Covid Different?

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%. This made filing for UI more attractive to the typical worker during the Covid recession than in normal times. In addition, the Covid period saw much wider UI coverage for non-traditional workers (e.g., gig workers and contractors).[37]

The UI extensions during the Covid period were also highly publicized, and likely particularly salient. Figure 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. In Appendix A.11, we show that during the pandemic, the number of UI recipients well exceeded the official count of the unemployed. As noted above, the effect of UI extensions on UI recipiency was much larger during the pandemic recession.

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 recalled.[38] We note, however, that temporary layoffs do not seem to be a major factor in determining our baseline (pre-Covid) estimates: panel (l) of Table A.5 shows that our results remain largely unchanged when excluding the period 1980–1985 when temporary layoffs were most common (see also Katz, 2010; Gertler, Huckfeldt,

[37]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.

[38]Ganong et al. (2020) discuss the role of temporary layoffs during the Covid recession. 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*.

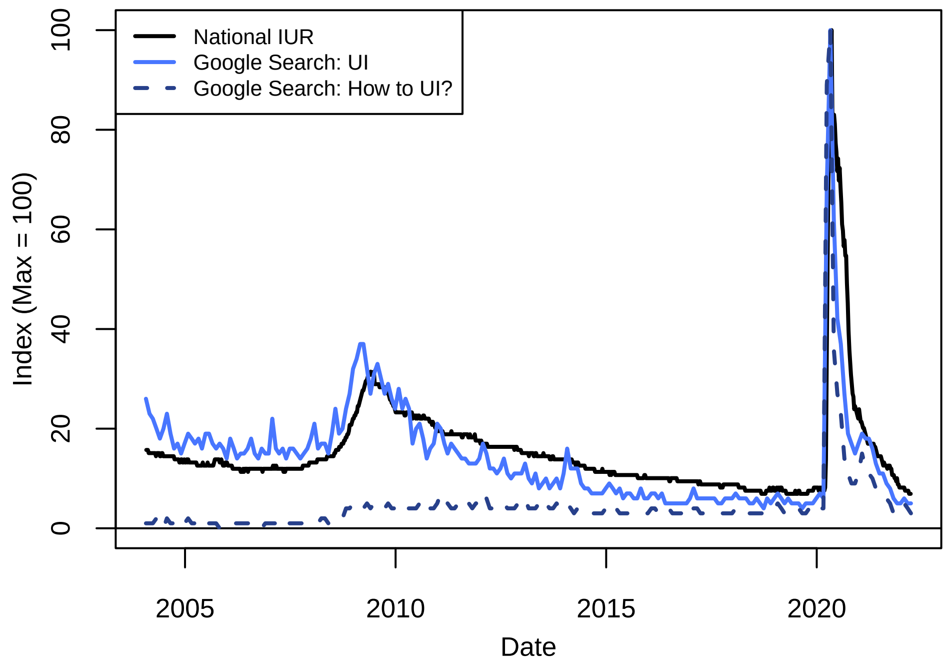


Figure 10: Salience of UI

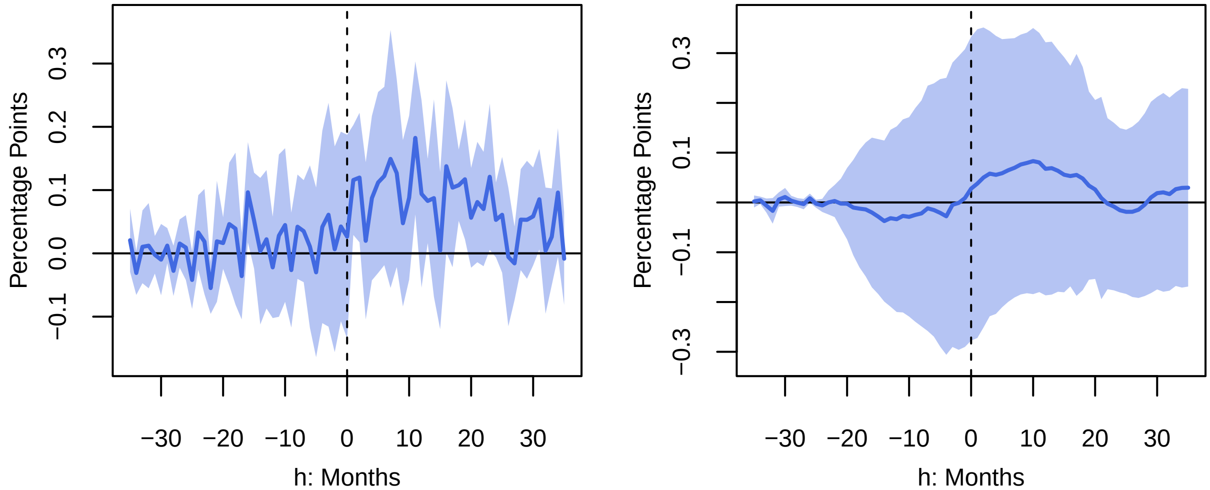
NOTE. This figure shows the national insured unemployment rate (IURSA from FRED), Google searches for "unemployment insurance," and Google searches for "how to file for unemployment." All series are normalized to have a maximum of 100 over the period graphed. The Google data are from Google Trends. The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

and Trigari, 2022).

5.6 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. We address this issue in several ways. First, panel (m) 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 (n) 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.

Figure 11 presents evidence on the dynamics of unemployment in advance of option switches.



(a) Fraction of Labor Force on UI

(b) Unemployment Rate

Figure 11: What Predicts Option Switches?

NOTE. This figure shows estimates of equation (1), in which we replace $\mathcal{W}_{s,t}$ with an indicator for whether a state adopted any optional trigger rule in each month. We present the relationship between option adoption and either the fraction of the labor force on UI, or the unemployment rate. Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in -35, \ldots, 35$. The sample runs from 1981m1–2019m12, and excludes Alaska. Standard errors are clustered by state and month.

We estimate a version of equation (1) in which we replace $\mathcal{W}_{s,t}$ with an indicator for whether a state has adopted any option in month t. The left-hand side variables are the percent of the labor force on UI (panel (a)) and the state's 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 fraction of the labor force on UI, 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.[39]

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

[39] 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).

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.

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

$$U_t = U_{t-1} - f_t U_{t-1} + sE_{t-1} \tag{4}$$

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

$$u^\star = \frac{s}{s+f} \tag{5}$$

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

$$d\log(u^\star) = -(1-u^\star)d\log(f); \tag{6}$$

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 $d\log(\;\,)$ we get that

$$\frac{d\log(D)}{d\log(\;\,)} = \frac{1}{1-u^\star}\frac{d\log(u^\star)}{d\log(\;\,)}. \tag{7}$$

This equation provides a simple formula relating the duration elasticity typically calculated in the microeconomics literature and the effect on the unemployment rate. Appendix B 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, $d\log(D)/d\log(\ )$ (and related concepts). Appendix B 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 $d\log(D)/d\log(\ )$ is between 0.33 and 0.41 in the United States.

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 0.33–0.41 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 in our short-duration sample of 0.36. Doing so leads to a range of 0.12–0.15. We consider a more complex case in Appendix B that takes into account 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 0.15–0.18.

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 right-hand side of equation (7)). Our estimate of the marginal effect of a 13-week UI extension on the unemployment rate in the short duration sample is 0.29 percentage points. This implies an elasticity of unemployment to potential benefit duration of 0.11.[40] Dividing this number by $1 \quad u^?$ yields 0.12. This lies within the 0.12–0.15 range for the simple case and slightly below the range for the 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 in papers such as 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.

[40]The average unemployment rate is 6.75%. The percentage change in the unemployment rate is therefore 0.29 /6.75. 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 $\frac{0.29}{0.07} \quad \frac{34}{13} = 0.11$.

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.[41] 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.

Model setup. Time is discrete and the discount factor is . 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 $= \frac{v}{S}$ 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 $b(\ ) = 1[\ > 0]b_{UI} + b_L$, where 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 $s\ (\ )$. Optimal search effort depends on the number of periods of UI benefits an unemployed has left, which we denote as $s(\ )$. Aggregate search effort is the unemployment-weighted matching efficiency $S = \sum_{=0}^{T} u(\ )s(\ )$, where $u(\ )$ is the mass of unemployed with periods of UI benefits left.[42]

Employed workers are laid off from their jobs with probability . At the beginning of an unemployment spell, unemployed workers draw an i.i.d. take-up cost from the distribution $G(\ )$. 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 , which leads them to leave the labor force. They re-enter the labor force through unemployment with probability . We assume that workers who exit the labor force lose eligibility to UI benefits. Workers who have lost

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

[42] 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.

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 (NB) with the worker bargaining share . Vacancies are determined endogenously by the condition that the flow cost, c, is equal to the expected discounted profit of opening a vacancy. Appendix C 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 sample. Table C.7 summarizes all the calibrated parameter values, while Table C.8 shows the targeted moments from our sample and corresponding moments from the calibrated model. We calibrate the model at the monthly frequency with a discount factor of $= 0.996$. 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 $= 0.72$. Following Hall and Milgrom (2008), we calibrate the average flow value of unemployment as $E(b_{UI} + b_L)/p = 0.71$. We calibrate $b_{UI} = \$1060$ (in December 2007 dollars) to match the average in our sample period (see Table B.6). We set $p = \frac{b_{UI}}{0.35}$ to match a 35% UI replacement rate.[43] 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 $= 1$. We choose the separation rate to match the E-to-U transition rate of 1.62% in our sample, and the home production shock, , to match the unemployment rate of 7.1% in our sample. We choose to match the labor force participation rate (LFPR) of 65.8% 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 the fraction of the labor force on UI of 2.9% 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 (see Table 5). The search cost function is assumed to take the following shape, $c(s) = \; s^{1+\frac{1}{\;}}$, where we choose to match the average job-finding rate of 25.0% in our sample. Finally, we choose $= 0.62$ to target prior empirical estimates of the micro-elasticity of unemployment duration to potential benefit duration (see Appendix C for details).

[43] 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 rate	0.29	0.04	0.33	0.25	0.48	0.35
Fraction of LF on UI	0.60	0.21	0.49	0.30	0.53	0.34
UI recipiency rate	4.88	1.47	5.20	2.11	4.92	2.04

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.

Results. Table 5 present results on the steady-state responses of the unemployment rate, the fraction of the labor force on UI, and UI recipiency rate (i.e., the fraction of the unemployed on UI). We do this for two different values of baseline PBD, T= 8 months and T= 17 months, i.e., the average baseline PBDs 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.

Since the model is calibrated to match match prior estimates of the micro-elasticity, it is no surprise that it yields a micro effect on the unemployment rate of 0.33 percentage points at short durations. The macro effect in the model is somewhat larger at 0.48 percentage points due to the additional effect on labor market tightness–larger than in the data. The macro elasticities in the model are 0.53 for the fraction of the labor force on UI (vs. 0.60 in the data) and 4.92 for the UI recipiency rate (vs. 4.88 in the data). This leaves little room for general equilibrium effects operating through vacancy creation that might raise the macro effects.

The model predicts that the effects of a UI extension will be smaller at longer baseline UI durations. The macro effect on unemployment at long baseline PBD duration is 0.35 percentage points, about 25 percent smaller than at short baseline duration. The reason why extensions yield smaller effects at long durations in the model is that they have less impact on search effort and re-employment wages for the bulk of the unemployed, who have been unemployed for only a short time period. This arises because unemployed job seekers are unlikely to stay unemployed long enough to make use of these long-duration UI extensions. Figure C.1 in the appendix shows the effects of UI extensions on job finding and re-employment wages by duration of unemployment, confirming this intuition.

However, our model does not fully explain the gap between the effects of short and long duration extensions that we identify in the data. 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. Rothstein (2011) emphasizes considerable uncertainty surrounding the expiration of extensions during the Great Recession period. (Benefits actually did lapse during several periods in 2010.) Also, several authors have emphasized that job search may be less effective during recessions and this may be particularly the case during deep recessions like the Great Recession.

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 may have particularly high marginal propensities to consume. However, our results in section 4 show that the magnitude of the transfers associated with UI extensions *at the margin* are less than 10 dollars per capita per month, and closer to 2 dollars per capita for extensions that occur at long baseline potential benefit durations. The stimulatory effect of such modest transfers is clearly going to be modest. This important feature of our local average treatment effect is common to many empirical studies of the effects of UI extensions, especially those using variation from the Great Recession (since the relevant quasi-experimental variation in these studies often occurs at long horizons).[44]

The fiscal stimulus effects of the first weeks of UI benefits are potentially much larger than for UI extensions. This is the case for the simple reason that the short-term unemployed are much more numerous that those that are unemployed for 40 weeks, let alone 80 weeks. 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. (2020).

[44]This conclusion may seem at odds with Kekre (2021), whose quantitative model suggests that UI played a stimulative role during the Great Recession. Absent the 73 weeks of UI extensions during the Great Recession, Kekre finds that the unemployment rate would have been 0.4 percentage points higher. For a 13-week extension, this is equivalent to a 0.07 percentage point *decrease* in unemployment. In contrast, our estimates imply that UI extensions results in a modest *increase* in unemployment—0.04 percentage points—in the long-duration sample. Kekre simulates the effects of UI extensions when interest rates are at their zero lower bound (as they were in the Great Recession). In Kekre's model, higher wage inflation leads to lower real interest rates which, in turn, spurs on aggregate demand. This is an important force driving the effects of UI extensions during the Great Recession in his paper, and is opposite to the dampening effect of wages on the labor market in neoclassical search models. While our empirical estimates incorporate local general equilibrium effects, they do not incorporate global general equilibrium effects associated with the zero lower bound.

7 Conclusion

This paper studies the macroeconomic effects of UI extensions in the United States over the last four decades. From a theoretical perspective, the macroeconomic and microeconomic effects may differ due to general equilibrium effects operating through firms' hiring and firing decisions, due to crowding out effects between workers' search efforts, and due to Keynesian stimulus effects of UI payments. While there is a large and established literature that credibly identifies the effects of UI generosity on individual-level job finding, there are only a few empirical studies of the effects of UI extensions on aggregate labor market outcomes using quasi-experimental methods. Understanding the macroeconomic effects of UI extensions is, however, of the utmost importance given the perennial policy debates about whether to extend UI during recessions.

Our paper contributes to this debate along several dimensions. 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 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 initially short. This is intuitive given 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 explain the macroeconomic effects of UI that we estimate via microeconomic estimates of the duration elasticity in the existing literature. Large general equilibrium effects are thus not needed to account for the estimated macroeconomic effects. Another important observation is that—while the model does generate lower unemployment elasticities at longer initial durations—it cannot explain *how low* the effects of UI extensions were in periods such as the Great Recession. Potential culprits are reduced search efficiency during the Great Recession as well as the considerable political uncertainty about the continuation of the UI extensions during this period. We leave these issues to future research.

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Appendix

A Empirical Appendix

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

Table A.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 20 months. Below, we provide more detail for each of these adjustments.

Table A.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

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 A.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

$$\Delta\log(u)) = \frac{1-(\beta(1-s))^n}{1-\beta(1-s)}\,\epsilon\,(\log(\omega_2)-\log(\omega_1)) \tag{8}$$

where $\epsilon = 0.053$ 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 (8), 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 A.1 reports the change in the unemployment rate implied by equation (8) 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 ϵ, 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 $0.57 - 0.81$.

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.

A.2 Trigger Rules

The rules governing whether a state can extend UI duration under federal programs are enormously complex. Table A.2 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.[45] 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. As we have aggregated weekly data to the monthly frequency, the three week delay between "triggering" and "paying" should have little effect on our estimates outside of a very narrow window around $h = 0$.

[45] 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.2: Trigger Rules for Federal UI Extension Programs

Rule Type	**Rule Description**	**Effective Years**
	Extended Benefits (1970–)	
	13 Weeks	
Mandatory	IUR MA 4% and IUR Lookback 120%	1970–1971, 1981–1982
Mandatory	(IUR MA 4% and IUR Lookback 120%) or National IUR 4.5%	1972–1981
Optional	IUR MA 5%	1976–1982
Mandatory	IUR MA 5% and IUR Lookback 120%	1982–
Optional	IUR MA 6%	1982–
Optional	IUR MA 5% and 3-year IUR Lookback 120%	2011-2013
Optional	TUR MA 6.5% and 1- or 2–year TUR Lookback 110%	1993–
Optional	TUR MA 6.5% and 1-, 2-, or 3-year TUR Lookback 110%	2011-2013
	7 Additional Weeks	
Optional	TUR MA 8.0% and 1- or 2–year TUR Lookback 110%	1993–
Optional	TUR MA 8.0% and 1-, 2-, or 3-year TUR Lookback 110%	2011-2013
	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 4%	2008–2009
Mandatory	TUR MA 6%	2008–2009
Mandatory	Paying EB	2008–2009
Mandatory	Unconditional	2009–2012
Mandatory	TUR MA 6%	2012–2013
	Tier III Additional weeks: 13 (2009/2012) 9 (2012/2013)	
Mandatory	IUR MA 4%	2009–2013
Mandatory	TUR MA 6%	2009–2012
Mandatory	TUR MA 7%	2012–2013
	Tier IV Additional weeks: 6 (2009/2012) 10 (2012/2013)	
Mandatory	IUR MA 6%	2009–2013
Mandatory	TUR MA 8.5%	2009–2012
Mandatory	TUR MA 9%	2012–2013

Table A.2: Trigger Rules for Federal UI Extension Programs — Continued

Rule Type	**Rule Description**	**Effective Years**
	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 4% and IUR Lookback 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 6:8, 10 additional if nat. TUR 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 6:8, 11 additional if nat. TUR 7 (07/92–10/93)	
Mandatory	AIUR MA 5% or MTUR MA 9	1991–1994
	Federal Supplemental Compensation (1982–1985)	
	Reachback Tier	
	Weeks: 10 (9/82–1/93) 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 3.5%; not eligible for reachback	1982–1983
Mandatory	IUR MA 4%	1983
Mandatory	IUR MA 4% or LIUR 4%	1983–1985
	Tier III	
	Additional weeks: 2 (1/83–3/85)	
Mandatory	IUR MA 4.5%; not eligible for reachback	1983
Mandatory	IUR MA 5%	1983
Mandatory	IUR MA 5% or LIUR 4.5%	1983–1985
	Tier IV	
	Additional weeks: 4 (1/83–4/83) 2 (4/83–3/85)	
Mandatory	IUR MA 6%; not eligible for reachback	1983
Mandatory	IUR MA 6%	1983
Mandatory	IUR MA 6% or LIUR 5.5	1983–1985

Table A.2: Trigger Rules for Federal UI Extension Programs — Continued

Rule Type	Rule Description		Effective Years
Federal Supplemental Benefits (1975–1978)			
Tier I			
Weeks: 13			
Mandatory	Paying EB		1975
Mandatory	Paying EB and IUR MA	5%	1975–1978
Tier II			
Additional weeks: 13 (1975–1978)			
Mandatory	Paying EB and IUR MA	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.3 Labor Market Variable Definitions

This appendix describes the exact variables used in our analysis.

Potential Benefit Duration is the maximum total number of weeks of benefits available in a state in a particular month. This is a weekly variable. We also construct a monthly value for this variable by taking an average value over the month. The sources for this variable are discussed in section 3.2.

The **Fraction of the Labor Force Receiving UI** 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).[46] 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 of UI recipients in a month.[47] Denote this variable by $\mathrm{UI}_{s;t}$. The *fraction of the labor force on UI* is $\mathrm{UI}_{s;t}$=$\mathrm{LF}^{n}_{s;t}$, where $\mathrm{LF}^{n}_{s;t}$ is the non-seasonally-adjusted number of people in the labor force, taken from the LAUS. 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 $\mathrm{UI}_{s;t}$ (described above under "fraction of the labor force receiving UI") to the unemployment rate.

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 "fraction of the labor force receiving UI" 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

[46] 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.

[47] 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.

dollars using the PCE deflator (`PCEPI` from FRED, last downloaded May 9, 2023).

The **New UI Claims** variable is the ratio of initial claims (c51 of report ar5159, mentioned above) to $\mathrm{LF}^{n}_{s;t}$.

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. We then seasonally adjust each series, as well as total QCEW employment, using X-13 before constructing employment shares. In our regressions, we allow the coefficients on these shares to change 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 those individuals who were matched across two consecutive monthly interviews). We downloaded these data from IPUMS on May 4, 2023.

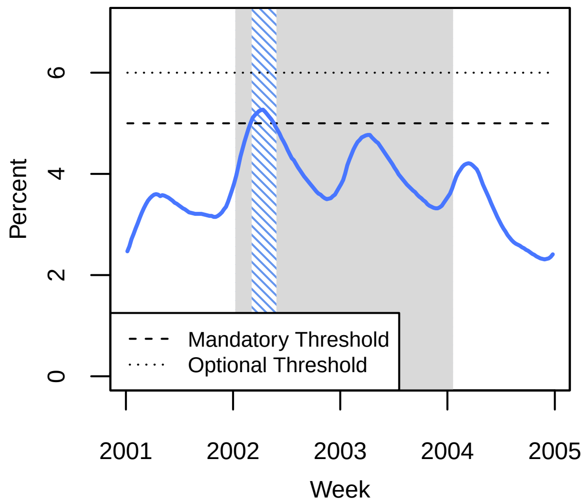
A.4 Second Example of Trigger Rule Adoption Affecting UI: Washington

In Figure A.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 $W_{s;t} = 0$ during the blue dashed period.

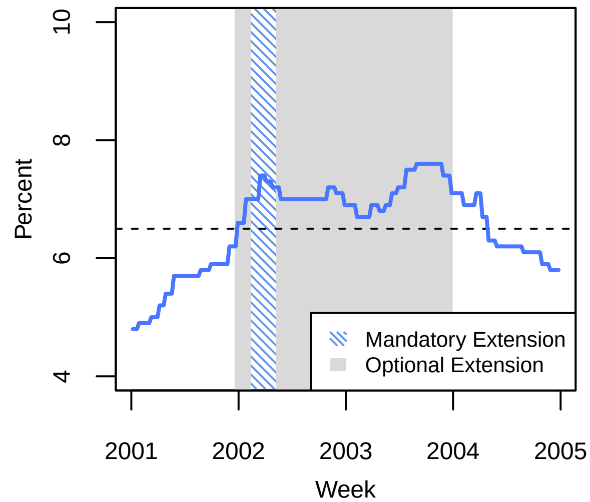
A.5 Sources of Variation in $W_{s;t}$

In Table A.3, we tabulate all of the values of $W_{s;t}$. Of the 1,798 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 323 observations have non-integer values that arises from the fact that potential benefit duration is determined every month, but we aggregate our data from a weekly to a monthly frequency. Next, 36 observations have non-standard values because of the fact that weeks provided under EB are technically a function of a state's regular level of UI benefits, as described in the table. 52 observations come from interactions of EB with other federal programs. A single observation has a negative value, owing to the 13-week rule—the table provides details.

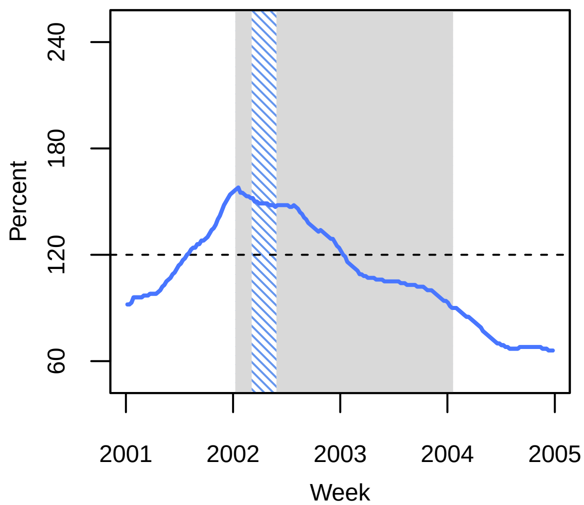
While we have no reason to suspect that these non-standard values of $W_{s;t}$ induce endogenous variation, panel (b) in Table A.5 presents estimates that eliminate this non-standard variation. The estimates from this specification are similar to those from our baseline specification.



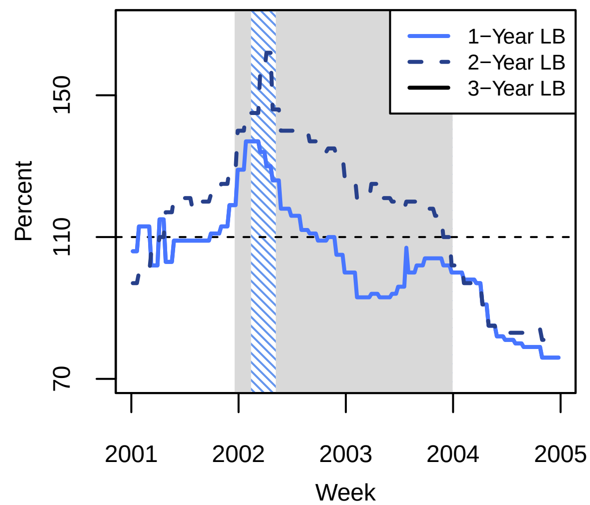
(a) IUR Moving Average



(b) TUR Moving Average



(c) IUR Lookback



(d) TUR Lookback

Figure A.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.

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

Category	# Obs.	Value of $\widehat{W}_{s,t}$	Explanation
Total	1798	–	–
"Normal" values	1386	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.
Time aggregation	323	Mostly non-integers	We calculate the average of "potential duration" (a weekly variable) within a month
State weeks < 26	36	Mostly non-integers	The two tiers of EB (EB13 and EB20) can pay less than 13 and 20 weeks if a state's regular weeks of benefits are less than 26 weeks. • "EB13" $= \min(13$ wk$, 0.5$ regular state UI) • "EB20" $= \min(20$ wk$, 0.8$ regular state UI)
TEUC	34	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"	18	2 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. 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.
13-week rule	1	-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 $\widehat{W}_{s,t}$.

A.6 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 state-time observations that would have a particular value of $\mathcal{W}_{s;t}$ regardless of option status. I.e., 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 particularly bad economic downturns—when states would have qualified regardless of their options. We refer to this sample as the "tight sample."

Table A.4: Tight Sample Results

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.9	13.0	0.69	1.43	0.62	0.66
	(0.2)	(0.4)	(0.10)	(0.34)	(0.14)	(0.17)
$\mathcal{W}_{s;t}$, Long Dur.	13.0	12.8	0.13	0.21	0.09	0.16
	(0.8)	(0.7)	(0.11)	(0.16)	(0.10)	(0.13)
Observations	2097	2557	2097	2557	2097	2557

The identifying variation in the tight sample comes solely from variation in the generosity of options in a given state. As an additional procedure to make the tight sample more homogeneous, we restrict attention to cases where a treated state-month (i.e., a state-month with $\mathcal{W}_{s;t} > 0$) within the tight sample has an untreated state-month (i.e., a state-month with $\mathcal{W}_{s;t} = 0$) within the tight sample within 12 months (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 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 periods when a states are in recessions versus expansions. 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 in our analysis such as state fixed effects to be estimated off of a much shorter time series.

A.7 Baseline Potential Benefit Duration

Figure A.2 plots the distribution of baseline potential benefit duration across states.

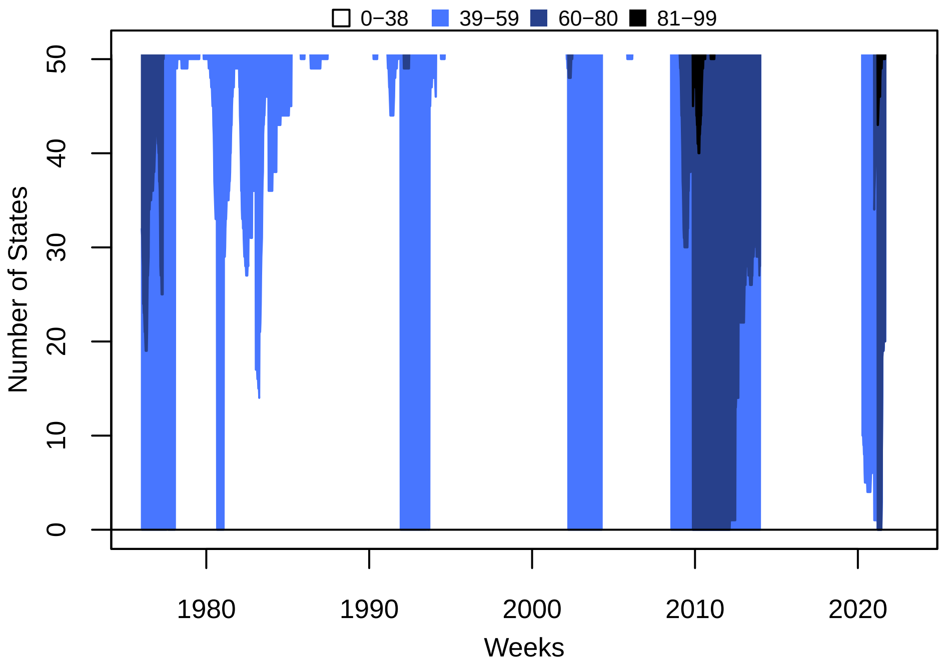


Figure A.2: Distribution of Baseline Potential Benefit Duration over States and Time

NOTE. This figure shows the number of states in each month 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.8 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 θ. Define the "fundamental" unemployment rate of a particular state in a particular month 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 O is drawn from a Bernoulli distribution:

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

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

Suppose the effect of extended benefits on unemployment is β. 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

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

We denote the average fundamental unemployment rate among states above the threshold as $\bar{u} \equiv \mathrm{E}[\tilde{u} \mid \tilde{u} \geq \theta]$ and the average unemployment rate among states below the threshold as $\underline{u} \equiv \mathrm{E}[\tilde{u} \mid \tilde{u} < \theta]$. 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 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

$$\begin{aligned} \beta_E &= \mathrm{E}[u \mid w = 1] - \mathrm{E}[u \mid w = 0] \\ &= \beta + \frac{F(\theta)}{1 - (1 - F(\theta))\pi}(\bar{u} - \underline{u}) \\ &= \beta + \frac{\text{prob. below threshold}}{\text{prob. not treated}}(\bar{u} - \underline{u}) \end{aligned}$$

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 differences in the fundamental unemployment rate of states 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 $q \quad \mathbf{1}\{\bar{u} \quad \}$. However, a complication arises since $\bar{u}$ is not observed. In practice, we must use the qualifying control $q \quad \mathbf{1}\{u \quad \}$. This potentially introduces a downward bias 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 $q \quad \mathbf{1}\{\bar{u} \quad \}$.[48]

The "qualifying controls" approach consists of estimating

$$u = \quad + \quad w + \quad q + e. \tag{10}$$

The Frisch-Waugh-Lovell theorem implies that from equation (10) is identical to from

$$\mathbf{u} = \quad \mathbf{w} + \tag{11}$$

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:

$$\mathbf{u} = \begin{cases} u \quad (u + \quad) & \text{if } \bar{u} \\ u \quad \underline{u} & \text{if } \bar{u} < \end{cases} \qquad\qquad \mathbf{w} = \begin{cases} (w \quad) & \text{if } \bar{u} \\ 0 & \text{if } \bar{u} < \end{cases}$$

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

$$= \frac{\mathrm{E}[\mathbf{uw}]}{\mathrm{V}(\mathbf{w})} = \quad . \tag{12}$$

[48] In Appendix A.6, we also discuss a "tight sample" approach. In the simple example discussed here, that approach amounts to limiting the sample to states with $\bar{u} >$. With that restriction, the bias term disappears since there are no observations below the threshold.

The bias is removed by subtracting it off via the inclusion of the q control.[49]

A.8.1 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.3. 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 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 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 . 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 " risk set, while the control state will be in the "below " risk set.

An unbiased estimator of the treatment effect would compare the unemployment rate of the

[49] In the example at the beginning of section 4, with two thresholds, the qualifying controls are $\mathbf{1}\{u \quad _1\}$, $\mathbf{1}\{ \ _1 \ u \quad _2\}$, and $\mathbf{1}\{u \quad _2\}$. 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 $u \quad _1$ and $u \quad _2$. 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 $_1$ and $_2$. 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 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.

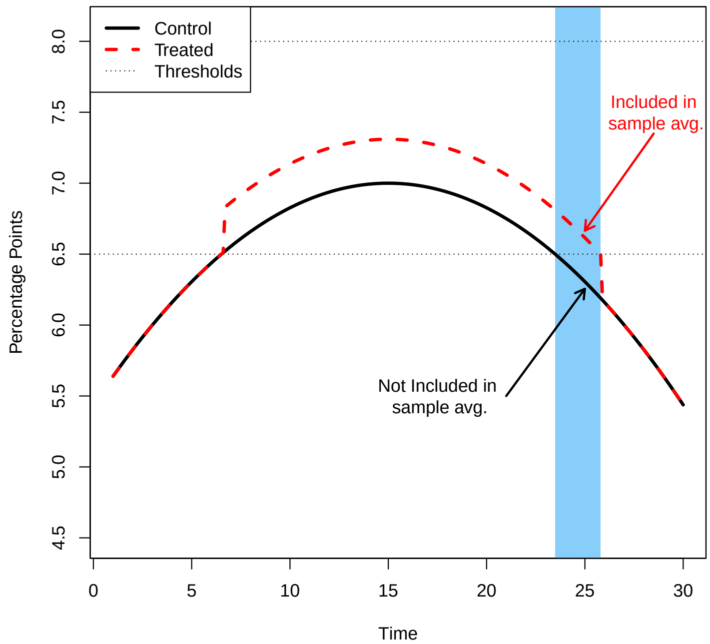


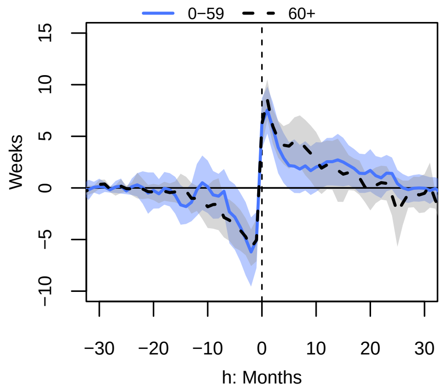
Figure A.3: Selection Effect

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.

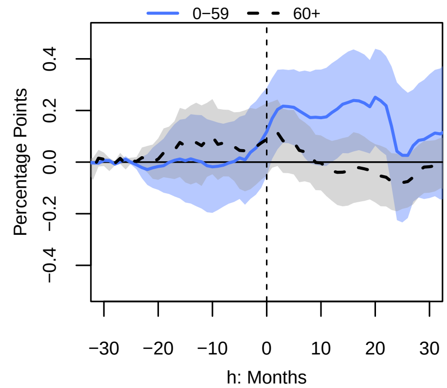
A simple way to construct bias-corrected estimates is to add the estimated treatment effect to the unemployment rate of untreated states and re-calculate the qualifying controls. We have done this for our main empirical specification. Specifically, we estimate our baseline specification for $h = 0$ for various outcome variables. We then add the estimated treatment effect to the outcome variables for untreated observations. We then re-estimate the baseline specification with these new data. In practice, this makes little difference for our estimates. The short-duration effect on the unemployment rate increases from 0.29 to 0.34 (s.e. 0.10).

We have run a Monte Carlo simulation to verify that this bias-correction approach yields an unbiased estimate. The Monte Carlo simulation we ran is for a case similar to the example presented in this appendix, but made more quantitatively realistic in two ways. First, we simulate panels of data in which states have fundamental unemployment rates that are correlated (across states) and autocorrelated. Second, we have both a mandatory and optional trigger threshold. Both the unemployment process and thresholds are calibrated to be consistent with our data.

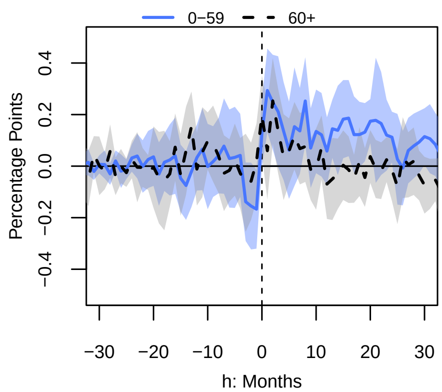
A.9 Differences Specification



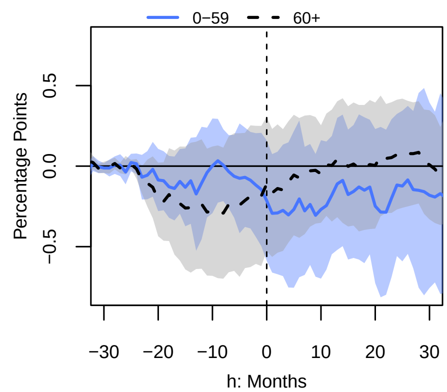
(a) Potential Benefit Duration



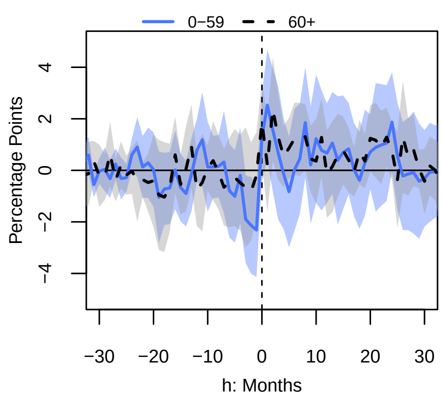
(b) Unemployment Rate



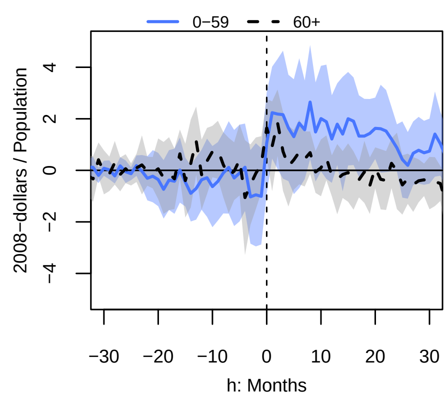
(c) Fraction of LF on UI



(d) Employment Rate, CES



(e) UI Recipiency Rate (UI/U)

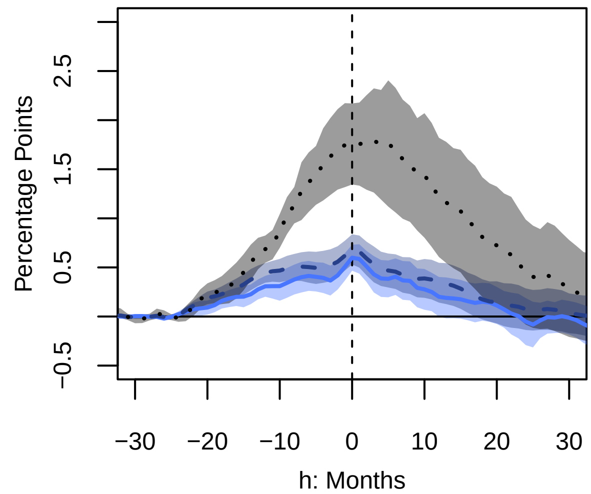


(f) Dollars Spent per Capita

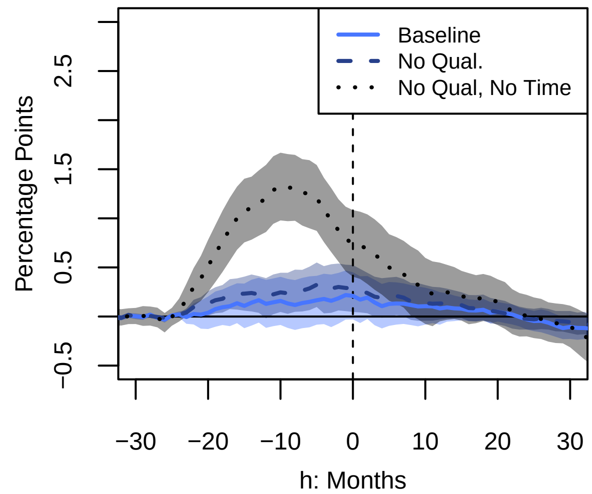
Figure A.4: Differences Specification

NOTE. Each panel shows OLS estimates of equation (3) for short baseline potential benefit durations (the blue solid lines) and for long baseline potential benefit durations (the black dashed lines), with the variable in the panel titles as left-hand side variables, $y_{s,t+h}$. Each point and surrounding shaded 95% confidence interval is from a separate regression—one each for $h \in -35, \ldots, 35$. The sample runs from 1981m1–2019m12, and excludes Alaska. Standard errors are clustered by state and month.

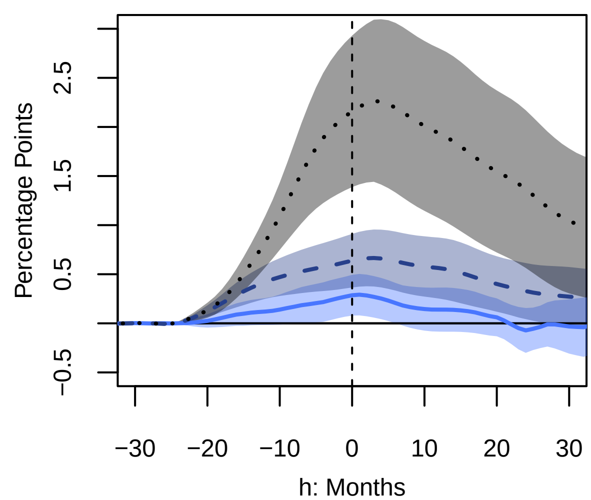
A.10 Role of Qualifying Controls



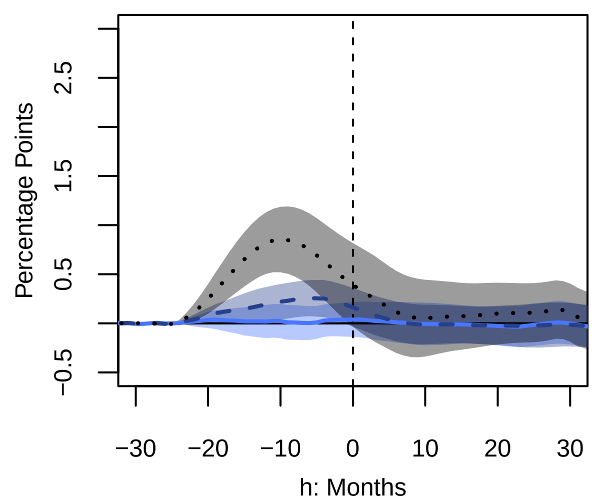
(a) % of LF on UI: Short Duration



(b) % of LF on UI: 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 $h \in -35, \ldots, 35$. The sample runs from 1980m1–2019m12, and excludes Alaska. Standard errors are clustered by state and month. 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.11 UI Recipiency

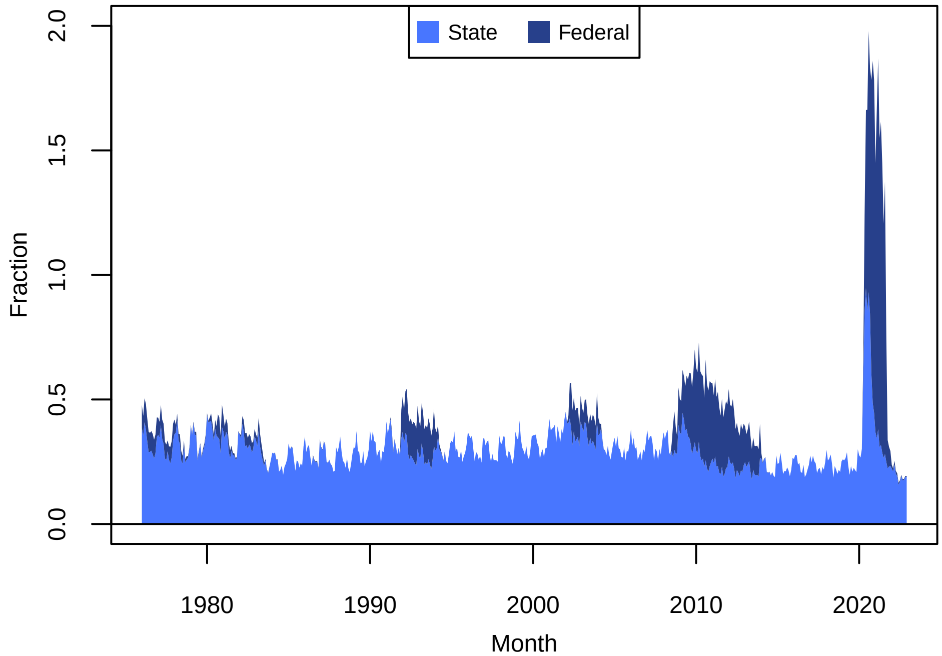
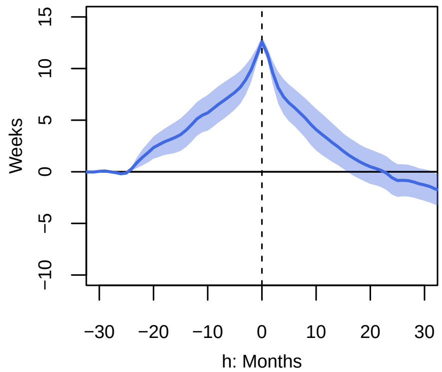


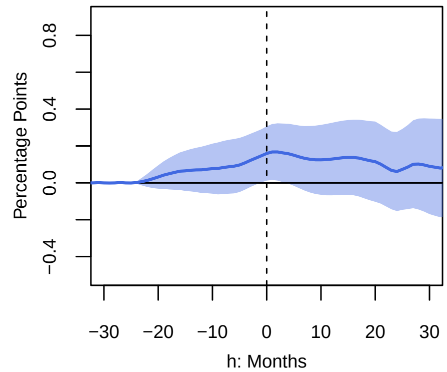
Figure A.6: UI Recipiency Rates

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.

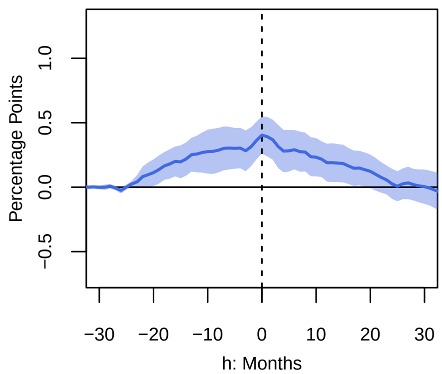
A.12 Full Sample Estimation



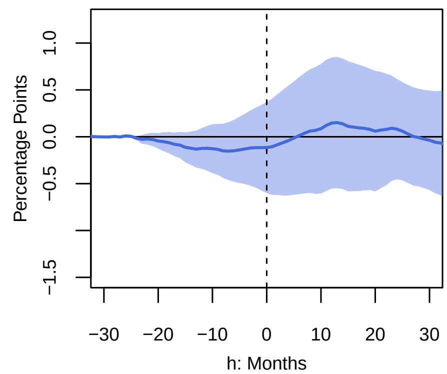
(a) Potential Benefit Duration



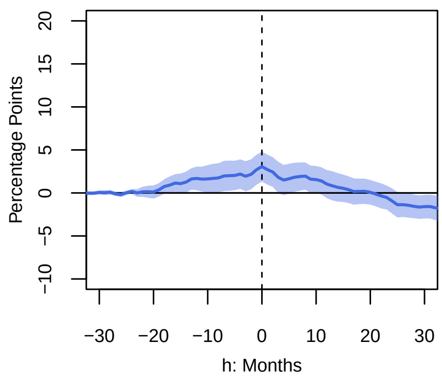
(b) Unemployment Rate



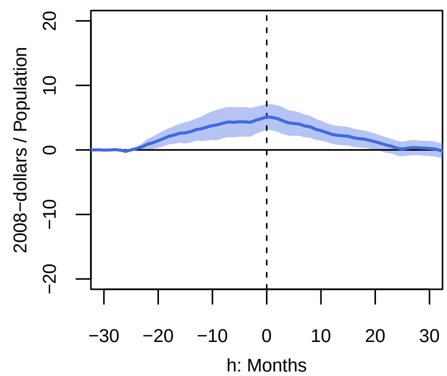
(c) Fraction of LF on UI



(d) Employment Rate, CES



(e) UI Recipiency Rate (UI/U)



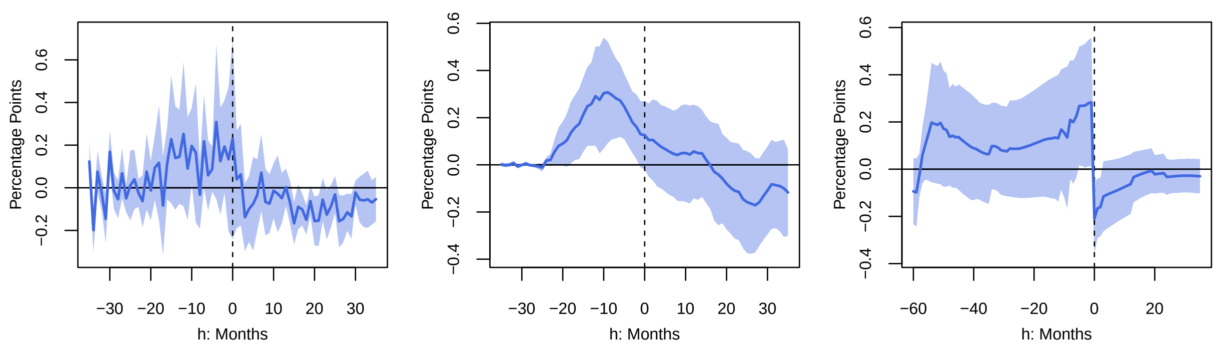
(f) Dollars Spent per Capita

Figure A.7: 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 $h \in -35, \ldots, 35$. The sample runs from 1981m1–2019m12, and excludes Alaska. Standard errors are clustered by state and month.

A.13 Option Switches

Figure 11 focuses on option adoption. Here, we expand this exercise to include option termination. Panels (a) and (b) of Figure A.8 present results analogous to Figure 11 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 fraction of the labor force receiving UI are a mirror image of the option-adoption results in Figure 11. 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: Frac. LF on UI (b) LHS: Unemployment (c) LHS: TUR Option

Figure A.8: Option Terminations

NOTE. Panels (a) and (b) show the relationship between option termination and the fraction of the labor force receiving UI 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.8 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 Robustness Results

Table A.5 presents estimates of β_h from equation (1) for horizon $h = 0$ for a number of alternative cases relative to our baseline analysis. In most cases, we report the response of potential benefit duration (PBD), the fraction of the labor force receiving UI, and the unemployment rate. For each variable, we present results for our baseline sample of 1980-2019 as well as the extended sample that includes the period since the onset of the Covid pandemic (1980-2022). Panel (a) replicates our analysis in Figure 7 for $h = 0$.

Binary Treatment We start with the definition of our baseline treatment variable, $\widetilde{W}_{s,t}$. That variable can have some "intensive margin" variation arising from time aggregation, the interaction of EB with other programs, and from variation in the duration of regular state UI across states. 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 $\widetilde{W}_{s,t}$ with the following indicator variable:

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

where $\mathrm{EB}^{13}_{s,t}$ is an indicator for whether state s has triggered onto the 13-week tranche of the EB program, $\mathrm{EB}^{13,\text{no options}}_{s,t}$ is an indicator for whether s would have triggered onto the 13-week tranche of the EB program with no options on, and $\mathrm{EB}^{20}_{s,t}$ and $\mathrm{EB}^{20,\text{no options}}_{s,t}$ 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. First, panel (c) presents a "saturated controls" specification. In this case, we control for quarterly lags (from t-24 to 48) of five variables for all outcome variables: potential benefit duration (PBD), the fraction of the labor force on UI, the labor force participation rate, the unemployment-to-employment transition rate, and the unemployment rate.

Panel (d) presents results controlling for the impact of industry shocks through industry-employment shares interacted with indicators for consecutive 5-year periods. Here, we follow Guren et al. (2021) in the construction of this control. 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.

Finally, 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.

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

These robustness checks have maintained 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 (i) presents results for this case.

Additional Outcome Variables Panel (j) 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 (k) 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 (l) 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.

Dropping around Option Switches Finally, panels (m) and (n) 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 ($h = 0$) Effects

(a) Baseline

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.9	12.9	0.60	0.87	0.29	0.44
	(0.2)	(0.3)	(0.07)	(0.20)	(0.11)	(0.14)
$\mathcal{W}_{s;t}$, Long Dur.	13.7	13.4	0.21	0.28	0.04	0.12
	(0.7)	(0.6)	(0.12)	(0.12)	(0.09)	(0.08)
Observations	23795	25584	23795	25584	23795	25584

	Emp. Rate (CES)		UI Recipiency Rate		$ Spent per Capita	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	-0.45	-0.57	4.88	6.07	6.90	9.30
	(0.27)	(0.28)	(0.89)	(1.90)	(0.96)	(1.91)
$\mathcal{W}_{s;t}$, Long Dur.	0.47	0.42	1.47	0.96	2.22	2.86
	(0.60)	(0.51)	(1.32)	(2.66)	(1.28)	(1.27)
Observations	23795	25584	23795	25584	23795	25584

(b) Binary Treatment

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$I_{s;t}$, Short Dur.	15.5	12.1	0.68	1.07	0.26	0.48
	(1.1)	(1.1)	(0.11)	(0.34)	(0.13)	(0.22)
$I_{s;t}$, Long Dur.	14.9	14.4	0.29	0.38	-0.02	0.11
	(1.7)	(1.4)	(0.15)	(0.17)	(0.13)	(0.12)
Observations	23795	25584	23795	25584	23795	25584

Table A.5: Contemporaneous ($h = 0$) Effects — Continued

(c) Saturated Controls

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.8	12.9	0.60	0.88	0.30	0.46
	(0.2)	(0.3)	(0.08)	(0.20)	(0.14)	(0.15)
$\mathcal{W}_{s;t}$, Long Dur.	13.5	13.2	0.22	0.27	0.05	0.11
	(0.7)	(0.6)	(0.12)	(0.13)	(0.10)	(0.08)
Observations	23795	25584	23795	25584	23795	25584

(d) Industry Employment Share Controls

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.9	13.0	0.62	0.90	0.29	0.47
	(0.2)	(0.3)	(0.07)	(0.18)	(0.11)	(0.14)
$\mathcal{W}_{s;t}$, Long Dur.	13.8	13.5	0.23	0.30	0.02	0.11
	(0.7)	(0.6)	(0.12)	(0.12)	(0.09)	(0.08)
Observations	23795	25584	23795	25584	23795	25584

(e) Adding Covid Stringency Index Control

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.9	12.9	0.60	0.85	0.29	0.43
	(0.2)	(0.3)	(0.07)	(0.17)	(0.11)	(0.13)
$\mathcal{W}_{s;t}$, Long Dur.	13.7	13.4	0.21	0.33	0.04	0.14
	(0.7)	(0.6)	(0.12)	(0.12)	(0.09)	(0.08)
Observations	23795	25584	23795	25584	23795	25584

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

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$	12.6	12.5	0.40	0.60	0.16	0.27
	(0.2)	(0.3)	(0.07)	(0.12)	(0.08)	(0.10)
Observations	23795	25584	23795	25584	23795	25584

Table A.5: Contemporaneous ($h = 0$) Effects — Continued

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

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.6	12.3	0.57	0.81	0.26	0.40
	(0.3)	(0.5)	(0.07)	(0.19)	(0.10)	(0.13)
$\mathcal{W}_{s;t}$, Long Dur.	12.6	12.6	0.27	0.42	0.08	0.16
	(0.5)	(0.5)	(0.10)	(0.11)	(0.11)	(0.11)
Observations	23795	25584	23795	25584	23795	25584

(h) Interacting All Coefficients with Short-Duration Indicator

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.8	13.0	0.59	0.89	0.28	0.44
	(0.2)	(0.2)	(0.07)	(0.21)	(0.11)	(0.15)
$\mathcal{W}_{s;t}$, Long Dur.	13.4	13.8	0.17	0.27	0.17	0.26
	(0.7)	(0.6)	(0.07)	(0.14)	(0.06)	(0.09)
Observations	23787	25572	23787	25572	23787	25572

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

	PBD		Frac. LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.6	12.2	0.66	0.67	0.25	0.31
	(0.2)	(0.4)	(0.07)	(0.07)	(0.11)	(0.11)
$\mathcal{W}_{s;t}$, Long Dur.	12.8	12.3	0.22	0.41	-0.02	0.05
	(0.6)	(0.5)	(0.09)	(0.13)	(0.09)	(0.10)
Observations	23795	25584	23795	25584	23795	25584

(j) Other Unemployment/Employment Indicators

	U. Rate, CPS		Log Emp., CES		LFPR, LAUS	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	0.39	0.51	-0.29	-0.27	0.10	0.08
	(0.14)	(0.16)	(0.27)	(0.34)	(0.12)	(0.12)
$\mathcal{W}_{s;t}$, Long Dur.	0.12	0.15	0.16	0.09	-0.48	-0.43
	(0.11)	(0.09)	(0.31)	(0.29)	(0.32)	(0.27)
Observations	23533	25322	23795	25584	23795	25584

Table A.5: Contemporaneous ($h = 0$) Effects — Continued

(k) Pre- and Post-Covid Samples

	PBD		% LF on UI		Unemployment	
	1980–2019	2020–2022	1980–2019	2020–2022	1980–2019	2020–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.9	13.4	0.60	2.16	0.29	0.67
	(0.2)	(1.2)	(0.07)	(0.64)	(0.11)	(0.34)
$\mathcal{W}_{s;t}$, Long Dur.	13.7	13.6	0.21	-0.34	0.04	0.24
	(0.7)	(2.8)	(0.12)	(0.33)	(0.09)	(0.19)
Observations	23795	1789	23795	1789	23795	1789

(l) Alternative Sample: 1986+

	PBD		% LF on UI		Unemployment	
	1986–2019	1986–2022	1986–2019	1986–2022	1986–2019	1986–2022
$\mathcal{W}_{s;t}$, Short Dur.	13.3	13.3	0.60	0.90	0.24	0.42
	(0.2)	(0.3)	(0.08)	(0.22)	(0.12)	(0.15)
$\mathcal{W}_{s;t}$, Long Dur.	13.7	13.4	0.20	0.28	0.03	0.12
	(0.7)	(0.6)	(0.12)	(0.12)	(0.08)	(0.07)
Observations	20237	22026	20237	22026	20237	22026

(m) Drop Around Switches

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.8	13.3	0.70	0.80	0.47	0.62
	(0.4)	(0.4)	(0.09)	(0.17)	(0.17)	(0.20)
$\mathcal{W}_{s;t}$, Long Dur.	19.9	15.1	0.27	0.07	-0.33	-0.25
	(1.7)	(2.1)	(0.29)	(0.26)	(0.33)	(0.22)
Observations	18765	19946	18765	19946	18765	19946

(n) Drop Discretionary Switches

	PBD		% LF on UI		Unemployment	
	1980–2019	1980–2022	1980–2019	1980–2022	1980–2019	1980–2022
$\mathcal{W}_{s;t}$, Short Dur.	12.9	13.1	0.61	0.92	0.31	0.48
	(0.2)	(0.3)	(0.07)	(0.21)	(0.11)	(0.15)
$\mathcal{W}_{s;t}$, Long Dur.	13.6	13.4	0.17	0.25	0.05	0.13
	(0.8)	(0.7)	(0.13)	(0.13)	(0.09)	(0.08)
Observations	22976	24693	22976	24693	22976	24693

B 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).

B.1 Setup

Consider a continuous time setting where the fraction of job losers who become UI recipients is α. These people receive UI benefits until their benefits expire or they find a new job (whichever comes first). The remaining fraction of job losers, $1-\alpha$, 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.[50] We can then write that the fraction α of job losers that receive UI have the following re-employment hazard function:

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

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

$$S(t) = \exp\left(-\int_0^t \lambda(x)dx\right) = \begin{cases} \exp(-f_c t) & \text{if } t \leq P \\ \exp(-(f_c P + (t-P) f_x)) & \text{if } t > P. \end{cases} \tag{13}$$

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

$$\begin{aligned} D(f_x, f_c, P, \alpha) &= \alpha \int_0^\infty S(t)dt + (1-\alpha)\int_0^\infty S_n(t)dt \\ &= \alpha\left(\frac{1-\exp(-f_c P)}{f_c} + \frac{\exp(-f_c P)}{f_x}\right) + (1-\alpha)\frac{1}{f_x}. \end{aligned} \tag{14}$$

[50] 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

$$B = \int_0 S(t)dt = \frac{1 \quad \exp(\quad f_c \quad)}{f_c}. \tag{15}$$

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

$$B_r = \int_0^{26} S(t)dt = \frac{1 \quad \exp(\quad f_c 26)}{f_c}. \tag{16}$$

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 (14)–(16) 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 ( ) will increase average unemployment insurance duration mechanically (equation (15)). This is not true for regular UI duration (equation (16)), a statistic that also does not require taking a stand on UI take-up. Second, equation (14) 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 (14) 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 and .

B.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 B.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 0.41 in the United States. In terms of the notation used in section B.1, the elasticity they report is

$$\frac{f_c^{\ 1}}{} \frac{}{f_c^{\ 1}}. \tag{17}$$

Table B.6: Summary Statistics

Variable	Short-Dur. Sample	Long-Dur. Sample	Full Sample
Baseline Potential Benefit Dur.	34	74	42
Unemployment Rate	6.7	8.4	7.1
Fraction of LF on UI	2.4	4.6	2.9
Labor Force Part. Rate	66	65	66
UI Recipiency Rate	36	53	39
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	1025	1189	1060

NOTE. This table presents summary statistics of the main variables used for calibrating the models described in Section 6, this appendix, and Appendix C. 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 (14) 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, , and . In order to convert an estimate of   into $d\log(D)/d\log(\ )$, 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, ; and potential benefit duration, . The estimates for   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 B.6. 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 $^0 =$ 0.40 to target the UI recipiency rate of 0.36. We set $^0 = 34$, the average baseline potential benefit duration. We set f_x^1 and ^{1} to their pre-extension values, and set $^1 = \ ^0 + 13$. This implies that we are estimating the effects of a 13-week extension. Finally we set $f_c^1 = \frac{f_c^0}{1+\left(\frac{\ ^1\ \ ^0}{\ ^0}\right)}$, which solves equation (17) for the new job-finding rate consistent with an estimate for   and the percent change in benefit duration, $\frac{\ ^1\ \ ^0}{\ ^0}$.

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

$D \;\; f_x^0; f_c^0; \;\; ^0; \;\; ^0$ and $D_1 = D \;\; f_x^1; f_c^1; \;\; ^1; \;\; ^1$, respectively, and report the implied macro elasticity as $\frac{\frac{D_1 \;\; D_0}{D_0}}{\frac{1 \;\; 0}{0}}$. Using Schmieder and von Wachter's range of 0.33–0.41 for then yields a range of 0.15–0.18 for the elasticity of average unemployment duration with respect to potential benefit duration.

B.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.[51]

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.[52] Dividing this by the percent change in potential benefit duration (0.5=13/26) gives an unemployment duration elasticity of 0.4.[53]

[51]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 (13) to back out a job-finding rate by solving $0{:}394 = \exp(\;\; f_c \;\; 26)$ 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 (16) for $f_c = 0{:}036 \;\; (1 \;\; 0{:}166)$ and $f_c = 0{:}36$ and taking the difference yields an increase of 1.16, consistent with the "roughly 1 week" the authors report.

[52]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 $\;\; f_0$ yields a percent change in average duration (among UI recipients) of $\frac{\frac{1}{f_0} \;\; \frac{1}{f_0}}{\frac{1}{f_0}} = \frac{1}{\;} \;\; 1$. Card and Levine report $\;\; = 1 \;\; 0{:}166$, yielding a percent-change of 0.2.

[53]We note that 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 on potential benefit duration is *approximately equal to* the percent change in hazard rate. However, under the logit specification, is *exactly equal to* the change in the log-odds ratio implied by a unit increase in potential benefit duration, i.e., $\;\; = \log \frac{^0}{1 \;\; ^0} \;\; \log \frac{\;}{1 \;\;}$, where is the initial hazard rate and ^{0} is the new rate. With $\;\; = 0{:}1662$ and $\;\; = 0{:}036$, we can solve this equation to find that $^0 = 0{:}042$. This implies a percent change of of $\frac{0{:}042 \;\; 0{:}036}{0{:}036} = 17{:}3\%$. Setting $\;\; = 1 \;\; 0{:}173$ in $1{=} \;\; 1$ from footnote 52 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).

C 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 . 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 $= \frac{v}{S}$ 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 $b( ) = 1(> 0)b_{UI} + b_L$, where 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 $s\ ( )$. Optimal search effort depends on the number of periods of UI benefits left, which we denote as $s( )$. Aggregate search effort is the unemployment-weighted matching efficiency $S = \sum_{=0}^{T} u( )s( )$, where $u( )$ is the mass of unemployed with periods of UI benefits left.[54]

Employed workers are laid off from their jobs with probability . At the beginning of an unemployment spell, unemployed workers draw an i.i.d. take-up cost from the distribution $G( )$. 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 , which leads them to leave the labor force. They re-enter the labor force through unemployment with probability . 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 (NB) with the worker bargaining share . Vacancies are determined endogenously by the condition that the flow cost, c, is equal to the expected discounted profit of opening a vacancy.

[54]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 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 and move to inactivity next period. Inactive workers draw a home production shock at rate 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 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 periods of UI left is:

$$U(\ ) \quad = \quad \max_{s} fb(\ ) \quad c(s) + \quad (1 \quad)[(1 \quad s\ (\ ))U(\quad 1) + s\ (\ )W(\quad 1)] + \quad Ng; \tag{18}$$

where $b(\ ) = 1[\ > 0]b_{UI} + b_L$ is the flow value of unemployment with periods of UI left, $W(\ )$ is the value of employment with 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

$$c^{0}(s(\ )) = \quad (1 \quad)\ (\ )(W(\quad 1) \quad U(\quad 1)); \tag{19}$$

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

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

$$U(0) = \max_{s} \{ b_L - c(s) + (1 -)[(1 - f(s) ( ))U(0) + f(s) ( )W(0)] + N \}.$$

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 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

$$\tilde{U}( ) = \omega( )(U( ) - E(| < ( )) + (1 - \omega( ))U(0), \tag{20}$$

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

The value of inactivity N is

$$N = b_N + ((1 -)N + U(0)), \tag{21}$$

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

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

$$W( ) = w( ) + (1 -)[(1 -)(hW(T) + (1 - h)W( )) + \tilde{U}( )] + N. \tag{22}$$

The value function depends on UI benefits at the time of hire, as the worker has only 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

$$V = -c + \left[(1 - q( )(1 -))V + q( )(1 -) \sum_{=0} ( )J( ) \right], \tag{23}$$

where $( ) = \frac{u( )s( )}{S}$ is the probability of being matched with an unemployed worker with periods of UI left. The value of a filled vacancy is

$$J(\ ) = p \quad w(\ ) + \quad [(1 \quad)(1 \quad)(hJ(T) + (1 \quad h)J(\ )) + (1 \quad (1 \quad)(1 \quad))V]; \tag{24}$$

which depends on 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

$$\arg\max_{w} = (W(\ ) \quad U(\ )) \ (J(\ ) \quad V)^{1} \quad ; \tag{25}$$

where 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 periods of UI benefits as $w(\ )$.

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

$$e(t+1) = e(t) + F(t)(1 \quad)u(t) \quad (\ + (1 \quad) \ )e(t) \tag{26}$$

$$u(t+1) = u(t) + \ (1 \quad)e(t) \quad F(t)(1 \quad)u(t) \quad u(t) + \ n(t) \tag{27}$$

$$n(t+1) = n(t) + \ (1 \quad n(t)) \quad n(t); \tag{28}$$

where $F(t) = \frac{(\ )}{u(t)} \sum_{=0}^{T} s(\ )u(\ ; t)$ is the aggregate job-finding rate and $u(\ ; t)$ is the mass of unemployed workers with 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 $= T$ are as follows

$$u(T; t+1) = \quad (1 \quad)!(T)e(T; t) \tag{29}$$

$$e(T; t+1) = (1 \quad (\ + (1 \quad) \ ))e(T; t) + (1 \quad)(1 \quad)h(e(t) \quad e(T; t)): \tag{30}$$

Equation (29) 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 (30) 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 $0 < \quad < T$ are

$$
\begin{aligned}
u(\ ; t+1) &= (1 \quad)(1 \quad s(\ +1) \ (\ ))u(\ +1; t) + \ (1 \quad) ! (\ )e(\ ; t) && (31)\\
e(\ ; t+1) &= (1 \quad (\ +(1 \quad)(\ +(1 \quad)h)))e(\ ; t) + (1 \quad)s(\ +1) \ (\ )u(\ +1; t). && (32)
\end{aligned}
$$

Equation (31) states that the mass of unemployed with periods of UI eligibility left next period is equal to the mass of unemployed with $+ 1$ periods of UI eligibility left this period times the probability of still being unemployed, plus the mass of employed who qualified only for periods of UI times the probability of being laid off and claiming UI. Equation (32) states that the mass of employed with 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 $+ 1$ periods of UI eligibility left times the probability of finding a job. For $= 0$, the law of motion for the distributions of unemployed and employed are

$$
\begin{aligned}
u(0; t+1) &= (1 \quad s(0) \ (\ ))u(0; t) + (1 \quad)(1 \quad s(1) \ (\ ))u(1; t) + \ (1 \quad)e(0; t)\\
&\quad + \ (1 \quad) \sum_{=1} (1 \quad ! (\ ))e(\ ; t) + \ n(t) && (33)\\
e(0; t+1) &= (1 \quad (\ +(1 \quad)(\ +(1 \quad)h)))e(0; t) + (1 \quad)s(1) \ (\ )u(1; t)\\
&\quad +(1 \quad)s(0; t) \ (\ )u(0; t). && (34)
\end{aligned}
$$

Equation (33) 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 (34) 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 $u(t+1) = u(t) = u$, $e(t+1) = e(t) = e$, $u(\; ; t+1) = u(\; ; t) = u(\;)$ and $e(\; ; t+1) = e(\; ; t) = e(\;)$, we can derive the following steady-state relationships

$$u = \frac{ + (1 \quad)}{ + (1 \quad)(\; + F)}(1 \quad n) \tag{35}$$

$$n = \frac{\quad}{ \; + \; } \tag{36}$$

$$e = 1 \quad u \quad n. \tag{37}$$

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

$$u(T) = (1 \quad)\,!\,(T)e(T) \tag{38}$$

$$(\; + (1 \quad) \;)e(T) = (1 \quad)(1 \quad)h(e \quad e(T)). \tag{39}$$

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

$$u(\;) = (1 \quad)(1 \quad s(\; + 1) \; (\;))u(\; + 1) + (1 \quad)\,!\,(\;)e(\;) \tag{40}$$

$$(\; + (1 \quad)(\; + (1 \quad)h))e(\;) = (1 \quad)s(\; + 1) \; (\;)u(\; + 1). \tag{41}$$

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

$$s(0) \; (\;)u(0) = (1 \quad)(1 \quad s(1) \; (\;))u(1) + (1 \quad)e(0) \tag{42}$$

$$+ \; (1 \quad) \sum_{=1}^{\bar{}} (1 \quad !\,(\;))e(\;) + \; n \tag{43}$$

$$(\; + (1 \quad)(\; + (1 \quad)h))e(0) = (1 \quad)s(1) \; (\;)u(1) + (1 \quad)s(0) \; (\;)u(0). \tag{44}$$

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 $\ $, the search efforts $s(\ )$, the wages $w(\ )$, the UI take-up decisions $\omega(\ )$, the mass of unemployed u, the mass of employed e, the mass of inactive n, the distributions $u(\ )$ and $e(\ )$, and the values $U(\ )$, $\breve{U}(\ )$, $W(\ )$, $J(\ )$ and V that satisfy the equations (18)-(25), (35)-(44) 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 sample. Table C.7 summarizes all the calibrated parameter values, while Table C.8 shows the targeted moments from our sample and corresponding moments from the calibrated model. We calibrate the model at the monthly frequency with a discount factor of $\ = 0.996$. 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 $\ = 0.72$. 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 $b_{UI} = \$1,060$ (in December 2007 dollars) in line with our data (see Table B.6) and $p = \frac{b_{UI}}{0.35}$ in line with a 35% UI replacement rate.[55] The potential duration of UI benefits, T, is set to 10 months, which corresponds to the average level of UI benefits prior to the UI extensions we consider in our analysis. 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: $c(s) = \ s^{1+\frac{1}{\ }}$, where we choose $\ $ to match the average job-finding rate of 25.0% in our sample. As a baseline, we choose $\ = 0.62$. 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 0.33-0.41 reported for this elasticity by Schmieder and von Wachter (2016). We choose the flow cost of posting the vacancy, c, to yield $\ = 1$. This is a normalization. We choose the separation rate $\ $ to match the E-to-U transition rate of 1.62% in our sample, the home production shock, $\ $, to match the unemployment rate of 7.1% in our sample, and $\ $ to match the labor force participation rate (LFPR) of 65.8% 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 2.9% in our sample and the range is chosen to match the macro response of the UI recipiency rate to a 3-month UI extension of 4.9

[55]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 C.7: Calibrated Parameter Values in the Model

Symbol	Parameter Description	Value	Source/Target
	Discount factor	0:996	Annual interest rate of 5%
c	Vacancy posting cost	586:1	$= 1$
	Matching efficiency	1:00	Normalization
	Elasticity of matching function	0:72	Shimer (2005)
	Worker's bargaining share	0:72	Hosios condition
T	Potential UI benefit duration	10	Average PBD prior to EB extensions
b_{UI}	UI benefit	1; 060	Average UI benefit in 2008 dollars
p	Aggregate productivity	3; 029	$\frac{b_{UI}}{p} = 0:35$; Anderson and Meyer (1997)
b_L	Flow value of leisure	1; 717	$\frac{E[b_{UI}+b_L]}{p} = 0:71$; Hall and Milgrom (2008)
b_N	Flow value of home production	3; 029	$b_N = p$
h	Requalification probability	1=6	Average requalification period of 6 months
	Separation rate	0:0162	EU transition rate
	Home production shock	0:0029	Unemployment rate
	Home production shock	0:0056	Labor force participation rate (LFPR)
$_G$	Mean of take-up costs	8; 447	Fraction of LF on UI
$_G$	Std. of take-up costs	21; 488	Response of UI recipiency rate to PBD
	Search cost scaling parameter	17; 719	UE transition rate
	Search cost elasticity	0:62	Micro-elasticity of duration to PBD

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 $U( )$, $\breve{U}( )$, $W( )$, and $J( )$, the labor market tightness , and wages $w( )$.

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

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

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

Table C.8: Targeted Moments

Variable	Target	Model
A. Steady-state averages:		
Unemployment rate (%)	7.10	7.09
Fraction of LF on UI (%)	2.90	2.89
Job-finding rate (%)	25.0	25.1
Job-separation rate (%)	1.62	1.62
Labor force participation rate (%)	65.8	65.8
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.)	4.89	4.92

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 $T = 10$ and the corresponding data targets in the full 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 C.9 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 10 months. Panels B and C report these statistics for cases where the initial duration of UI benefits is set to 8 months (which matches our short duration sample) and 17 months (which matches our long duration sample). A subset of the result in panels B and C are also reported in Table 5 of the main paper. For simplicity, the calibration used in panels B and C 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 C.8. The first column of Table C.9 reports steady-state values for additional variables. Our model predicts a UI take-up rate of 45 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).[56]

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

[56]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 0.33-0.41 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 0.33 percentage points (panel B, 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 0.29 percentage points (see Table 4). The macro effect in the model is somewhat larger at 0.48 percentage points due to the additional effect on labor 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 C.8). 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 C.9 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. 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 C.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, the difference between the micro and macro effect is rather small. A second important observa-

Table C.9: Steady-State Responses and Elasticities — More Detailed Results

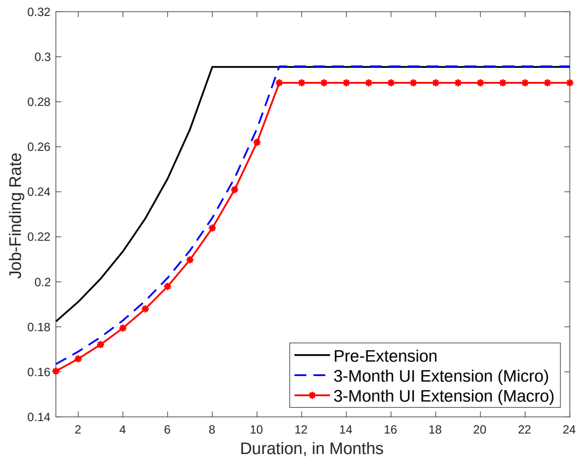
A. Baseline (T = 10)		Micro Effects		Macro Effects	
	x	dx	$\frac{dx}{dT}\frac{T}{x}$	dx	$\frac{dx}{dT}\frac{T}{x}$
Unemployment rate (%)	7.09	0.32	0.15	0.45	0.21
Fraction of LF on UI (%)	2.89	0.44	0.50	0.48	0.55
Wage ($)	2,975	0.59	0.00	-0.50	0.00
Job-finding rate (%)	25.10	-1.16	-0.15	-1.62	-0.22
Duration, all (months)	3.95	0.19	0.16	0.27	0.23
Duration, UI recipients (months)	4.98	0.53	0.36	0.61	0.41
Duration, non-recipients (months)	3.42	0.00	0.00	0.08	0.07
UI take-up rate (%)	44.67	0.49	0.04	0.60	0.04
UI recipiency rate (%)	40.81	4.12	0.34	3.90	0.32
UI per capita ($)	20.19	3.04	0.50	3.34	0.55

B. 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 (%)	6.77	0.33	0.13	0.48	0.19
Fraction of LF on UI (%)	2.53	0.49	0.52	0.53	0.56
Wage ($)	2,975	0.70	0.00	-0.49	0.00
Job-finding rate (%)	26.38	-1.33	-0.13	-1.86	-0.19
Duration, all (months)	3.76	0.20	0.14	0.28	0.20
Duration, UI recipients (months)	4.54	0.56	0.33	0.64	0.38
Duration, non-recipients (months)	3.36	0.00	0.00	0.08	0.06
UI take-up rate (%)	44.14	0.65	0.04	0.75	0.05
UI recipiency rate (%)	37.33	5.20	0.37	4.92	0.35
UI per capita ($)	17.63	3.45	0.52	3.73	0.56

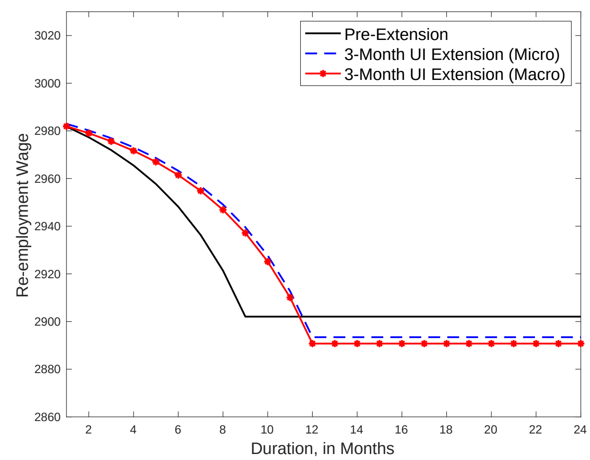
C. Long UI Duration (T = 17)		Micro Effects		Macro Effects	
	x	dx	$\frac{dx}{dT}\frac{T}{x}$	dx	$\frac{dx}{dT}\frac{T}{x}$
Unemployment rate (%)	8.09	0.25	0.17	0.35	0.25
Fraction of LF on UI (%)	3.91	0.30	0.43	0.34	0.50
Wage ($)	2,974	0.32	0.00	-0.42	0.00
Job-finding rate (%)	21.78	-0.70	-0.18	-0.99	-0.26
Duration, all (months)	4.54	0.15	0.19	0.21	0.27
Duration, UI recipients (months)	6.32	0.41	0.37	0.48	0.43
Duration, non-recipients (months)	3.58	0.00	0.00	0.05	0.09
UI take-up rate (%)	45.83	0.21	0.03	0.31	0.04
UI recipiency rate (%)	48.39	2.11	0.25	2.04	0.24
UI per capita ($)	27.29	2.06	0.43	2.40	0.50

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 $T = 10$, panel B for the calibration with $T = 8$ and panel C 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.

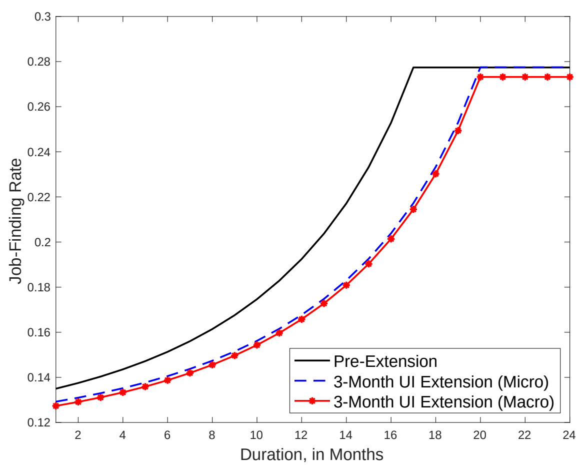
Figure C.1: The Effect of a 3-Month UI Extension on the Job-Finding Rate and Re-Employment Wage, by Duration of Unemployment



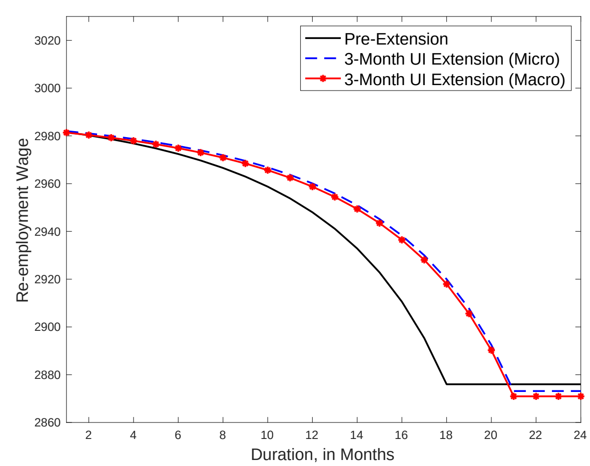
(a) Short UI Duration: Job-Finding Rate



(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.

tion 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 C.9. 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.