Lagged and Gender Heterogeneous Effects of Unemployment Compensation Extensions in the Great Recession

Student name: Kamalika Patra†
Student number: 422978

Supervisor: dr. K.F.J. Spiritus
Second assessor: dr. L.C.G. Pozzi

MSc International Economics
Erasmus School of Economics
Erasmus University Rotterdam

April 22, 2020

Abstract

During the Great Recession, the US implemented the Emergency Unemployment Compensation 2008 (EUC08), an unemployment insurance (UI) extension intending to support the unemployed. Exploiting the program's state and month differential application, this study conducts a dynamic panel difference-in-difference to analyse the gender differential effect of EUC08 on unemployment. It also examines whether program unemployment effects occur instantaneously or with delay. Results suggest a lag period of two to four months, wherefore it may be beneficial for speedy economic recovery to trigger UI extensions based on forecasted versus historical unemployment rates. Findings also show EUC08 reduced the gender unemployment gap by 0% to 0.8%, thereby reducing the disproportionate effect of the Great Recession on male unemployment. Consequently, this research suggests UI extensions are modestly effective unemployment stabilisers.

---

†The views stated in this thesis are those of the author and not necessarily those of Erasmus School of Economics or Erasmus University Rotterdam.

Contents

1 Introduction **4**

2 Structure of US Unemployment Insurance **6**
2.1 Emergency Unemployment Compensation 2008 . . . 6
2.2 Federal-State Extended Benefits . . . 8
2.3 Temporary Extended Unemployment Compensation . . . 8
2.4 Additional Benefits . . . 8

3 Theory **9**
3.1 Search Theory . . . 9
3.2 Matching Theory . . . 12

4 Literature Review **15**
4.1 Microeconomic Studies . . . 15
4.2 Macro Studies . . . 16
4.3 Gender Differences and the Great Recession . . . 17

5 Empirical Strategy **18**

6 Data and Methodology **23**
6.1 Treatment Indicators . . . 24
6.2 Additional Control Variables . . . 24
6.3 Seasonality Corrections . . . 25
6.4 Pre-Estimation Tests . . . 26
6.5 Gender Differential Effects . . . 27
6.6 Descriptive Statistics . . . 27

7 Threats to Identification **30**
7.1 Common Trends . . . 30
7.2 Mobility . . . 31

8 Results **32**

9 Robustness Checks **37**
9.1 Additional Control Variables . . . 37
9.2 TEUC Trends . . . 41
9.3 Excluding Additional Benefits States . . . 44

10 Conclusion **47**

11 Discussion **48**

12 Appendices **49**
12.1 Appendix 1: Wage-Labour Market Tightness Relation . . . 49
12.2 Appendix 2: Short-Run ARDL Error Correction Model . . . 50
12.3 Appendix 3: Tables Pre-Estimation Tests . . . 52

12.4 Appendix 4: Graphs Mean Unemployment over Treatment Period 54
12.5 Appendix 5: Graphs Common Trends 55
12.6 Appendix 6: Anticipatory Effects 56

1 Introduction

In the aftermath of the 2007 financial crisis, global economies spiralled into the Great Recession, persisting from January 2008 until June 2009 (Federal Reserve Bank of St.Louis, 2019b). Although GDP began to recover after the recessionary period, nationwide unemployment in the US continued on an upward path, reaching its zenith at 10 percent in October 2009. In response, president Obama signed several emergency unemployment insurance (UI) duration extensions, which came to be known collectively as the Emergency Unemployment Compensation of 2008 (EUC08) (United States Department of Labor, 2019). It temporarily extended potential benefits durations (PBD) from 26 weeks to 99 weeks in four tiers applicable differentially between states based on monthly state unemployment rates. EUC08, thus, provides ideal circumstances for a quasi-experimental difference-in-difference (DID) study. EUC08, however, was not the first US emergency UI extension: the Temporary Extended Unemployment Compensation (TEUC) was implemented in response to the more temperate 2001 recession. Prior to 2002, the Emergency Unemployment Compensation of 1990, was active from November 1991 until May 1997.

EUC08 intended to provide "support for those who are suffering due to economic conditions, help them return to work sooner and increase economic growth and job creation" (Executive Office of the President, 2011). Nonetheless, research dictates UI extensions can increase unemployment (Card & Levine, 2000; Lalive, 2008; Novo & Silva, 2017). UI extensions improve mental, social and financial well-being of the unemployed during recessions (Theodossiou, 1998) and act as (automatic) stabilisers, reducing GDP volatility by decreasing the sensitivity of aggregate demand to adverse shocks, thereby smoothing consumption and income (Faccini & Rendahl, 2016; Schmieder, Wachter, & Bender, 2012). On the other hand, UI extensions disincentivise job search in the short-run by increasing jobseekers' reservation wage and reducing search intensity, thereby increasing unemployment (Mortensen, 1977; Van den Berg, 1990). This conflict is termed the *insurance-incentive trade-off*, and begs the question if gains from UI extensions outweigh costs. The trade-off emphasises why it is important to understand the position of UI extensions on balance: so policymakers better know how and when to start and stop UI extensions to maximise program benefits at the smallest cost.

The existing research is wanting in the analysis of subgroup-heterogeneous UI extension effects on unemployment rates (Caliendo, Tatsiramos, & Uhlendorff, 2013). As such, the first of this paper's research objectives is to explore if EUC08 has differential effects on unemployment growth for males and females. The literature shows the Great Recession had vastly different effects on males compared to females. Industries with predominantly male workers were impacted much more severely by the recession than female-dominant industries (e.g. Grown & Tas, 2011; Rafferty, 2014). It follows that males suffered the brunt of the negative employment consequences of the recession. Naturally, it is then of interest to explore whether the EUC08, a remedial program attempting to re-stabilise the economy and relieve unemployment stress on individuals, managed to compensate the relatively imbalanced toll of the recession on men versus women. Additionally, looking for heterogeneous effects allows us to observe differences in the behaviour of each gender subgroup in response to UI policy, as well as contemplate which mechanisms might be at play in inducing such differences, if present.[1]

[1]We are aware gender is a social, and sex a biological concept. However, because it is difficult to

This paper's second contribution to the literature, is detecting the distribution of EUC08 effects on unemployment rates over time. Delayed UI extension effects on unemployment remains unexplored in the UI literature. However, if mechanisms through which UI stimuli act take time to register in unemployment figures, delayed effects may be more substantial than instantaneous effects. This is especially true for labour markets, which due to the presence of abundant rigidities are slow to react (Federal Reserve Bank of Cleveland, 2019). Notably, this holds true less in the US than in many European countries with more employment protection (see OECD, 2019). Furthermore, lags capture the staggered effects of treatment, in our case emergency UI extensions, on our outcome variable of interest, unemployment. They permit us to observe if stimulus effects grow or fade over time, how long they persist, and with what delay. Such information is important to policymakers, as it enables better planning and timing of benefits extensions to execute exact control over these labour market stimuli. In combination with subgroup segmentation, lags also give insight into differential response delay between subgroups.

We proceed by outlining EUC08's structure, then relay contemporary theories and research on the effect of UI extensions on unemployment in sections 3 and 4. The econometric model is described in section 5, followed by descriptions of the data and methodology. Before presenting results and robustness checks, we include a section covering threats to DID identification. Finally, this paper ends with conclusions and a discussion addressing limitations and points of reference for upcoming research.

know which was intended by Current Population Survey respondents, and since both may play a role in determining outcome differences, we liberally assume respondents mean both. Consequently, we use these terms interchangeably. Note, economics as a social science, attributes male and female outcome differences more to *gendered*, rather than sex, differences.

2 Structure of US Unemployment Insurance

US Basic Federal-State UI lasts up to 26 weeks. It is applicable to individuals who have become unemployed "due to no fault of their own", are actively searching for, willing and able to work, and meet specific eligibility criteria administered by individuals' state of residence, under Federal guidelines (United States Department of Labor, 2018). These often include a minimum number of hours worked and minimum earnings in a set pre-unemployment period (e.g. for New York, see New York State, 2019). In addition, the US has several labour market institutions providing extended UI conditional on state-specific criteria. These include permanent extension programs, such as Extended Benefits (EB) and Additional Benefits (AB), and temporary programs, such as the TEUC and EUC08. These are elaborated on below. We discuss EB and AB, as they may pose as possible confounders for this study (see section 5).

2.1 Emergency Unemployment Compensation 2008

EUC08 was a temporary UI extension instituted as part of the Supplemental Appropriations Act of 2008. Temporary UI extensions are instituted during periods of extreme economic depression like during the Great Recession (Fujita, 2010). Due to several prolongations EUC08 lasted 42 months, starting July 2008 and ending December 2013. Although the US has seen seven prior temporary UI extensions (Whittaker & Isaacs, 2012), EUC08 stands out due to the staggering 53 weeks of maximum extension granted at its peak, so that the unemployed could receive up to 99 weeks of UI including EB and basic UI.

EUC08 was administered differentially between states to the long-term unemployed, on the basis of two state-level unemployment criteria, or "unemployment rate thresholds" (Whittaker & Isaacs, 2012). The first is the previous 13-week average Insured Unemployment Rate (IUR), which is the ratio of UI claimants to the number of unemployment-insured workers. The second is the average Total Unemployment Rate (TUR), computed as the total number of unemployed over the total labour force in the past 13 weeks (Whittaker & Isaacs, 2014). The US labour force consists of all US residents age 16 and over willing and able to work, excluding those on active duty in the US army and institutional inmates (Bureau of Labor Statistics, 2019a). The IUR and TUR are both calculated from seasonally adjusted unemployment measures. The duration (in weeks) of extension provided was determined by where each state's IUR and TUR fell on a four-tier scale. Initially, EUC08 consisted only of one tier providing a 13-week extension. Tiers two to four were appended as EUC08 was prolonged. In addition to adding tiers, the extension durations and eligibility criteria for the tiers were changed several times. General requirements stated individuals should have no rights to basic Federal-State UI, and "have exhausted all rights to regular compensation with respect to a benefit year that ended on or after May 1, 2007" (United States Department of Labor, 2018).

Table 1 summarises durations, trigger criteria, and start and end dates per program tier. Suffixes a-d in EUC08 tier names are appended to clarify adjustments in eligibility criteria for the respective tier number, so that it effectively replaces the previous version of that tier, and for our purposes, may be considered a separate treatment (see section 5). For instance, initially tier 2 was implemented state-selectively in November 2008. Tier 2 eligibility criteria were officially changed later, and provided 14 versus 13 weeks extension. We label the altered, successive tier 2 as tier 2b, and the prior 2a. Once 2b is activated, 2a lapses. Note 3a and 4a can be and are active concurrently with 2b, because they belong to different official tiers (i.e. tiers 3 and 4 versus 2). Basic 26-week national UI is included in table 1 for reference.

From September 2012, Congress passed duration and unemployment threshold adjustments to EUC08, beginning gradual EUC08 sequestration (Whittaker & Isaacs, 2014). These are displayed under the "EUC08 Sequestration" subheading in table 1.

Table 1: US UI and Extended UI Programs

Tier	Duration (Weeks)	Start	End	State-Level Criteria
		Regular UI		
-	26	-	-	-
		EUC08		
1a	13	06/07/08	23/11/08	None
1b	20	24/11/08	01/09/12	None
2a	13	24/11/08	07/11/09	IUR≥4% or TUR≥6%
2b	14	08/11/09	26/05/12	None
3a	13	08/11/09	26/05/12	IUR≥4% or TUR≥6%
3b	13	27/05/12	01/09/12	IUR≥4% or TUR≥7%
4a	6	08/11/09	18/02/12	IUR≥6% or TUR≥8.5%
4b	16	19/02/12	26/05/12	IUR≥6% or TUR≥8.5% and no active EB
4c	6	27/05/12	01/09/12	IUR≥6% or TUR≥9%
		Emergency Unemployment Compensation 2008 (EUC08) Sequestration		
1c	14	02/09/12	28/12/13	None
2c	14	27/05/12	28/12/13	TUR≥6%
3c	9	02/09/12	28/12/13	IUR≥4% or TUR≥7%
4d	10	02/09/12	28/12/13	IUR≥6% or TUR≥9%
		Federal-State Extended Benefits (EB)		
EB1	13	-	-	IUR≥5% and ≥120% of past IUR[2]
EB2	7	-	-	IUR≥6% or TUR≥6.5% and ≥110% of past TUR[3]
		Temporary Extended Unemployment Compensation (TEUC)		
TEUC1	13	01/03/2002	31/01/2004	None
TEUC2	13	01/03/2002	31/01/2004	IUR≥4% and ≥120% of past IUR

Reading week counts: for any given date select active EUC08 tiers by looking at the 'start' and 'end' columns, then add tiers' respective durations. Subsequently, sum the number of weeks for EB 1 and EB2 if the state has a voluntary EB component. For instance, for November 2009, those in high unemployment states obtained 20 + 14 + 13 + 6 from EUC08 tiers 1b, 2b, 3a, 4a; add 13 and 7 weeks if EB1 and EB2 are applicable, for a total of 99 weeks extension granted.

[2] Corresponding week average IUR (13 week insured unemployment rate) in each of previous 2 years

[3] Corresponding week TUR (13-week average total unemployment rate) in either/both of previous 2 years

2.2 Federal-State Extended Benefits

EB are permanent UI extensions applicable to states satisfying the state-level unemployment thresholds. These criteria are also based on the state's IUR and TUR, as in table 1. Over the EUC08 duration, EB were granted according to two sets of state-differential criteria, wherefore EB1 and EB2 are separately denoted in the table. EB1 is a mandatory EB component, while EB2 is a state-voluntary additional extension. If EB2 is also provided, recipients enjoy up to 20 extra weeks of UI from the EB program atop the basic 26 weeks and EUC08. Individuals may only receive EB after exhausting basic UI and EUC08 extensions (Bureau of Labor Statistics, 2019b).

Notably, EB criteria are more stringent than EUC08's. Beyond current IUR and TUR thresholds, states must also satisfy conditions comparing unemployment statistics to historical counterparts. For EB1 to trigger, the state's IUR must additionally be at least 120% of corresponding week average 13-week IUR in each of the previous two years. For EB2, TUR also needs to be at least 110% of corresponding week average 13-week TUR in either or both of prior two years (Whittaker & Isaacs, 2014).

2.3 Temporary Extended Unemployment Compensation

The TEUC program is included in table 1 for information on a robustness check conducted in section 9. TEUC was a temporary act with two tiers. The first is indiscriminate between states, and provides an additional 13 weeks compensation on top of the 26 weeks. The second provided a supplementary 13 weeks, triggered by state-specific unemployment criteria requiring a 4% IUR and a 120% corresponding week average 13-week IUR in each of the previous two years. Termination was initially planned for 31 May 2003, but was delayed to January 2004 (Lake, 2003).

2.4 Additional Benefits

AB are state-specific programs to help unemployed reintegrate in the labour market. They trigger by state-specific criteria such that activation varies across states over time, as EB and EUC08. Nonetheless, AB are structurally more complicated, as they vary in type: some states provide higher benefits, others extend them, provide vocational training or other reintegration support, or several of these. In most states AB are permanent, but in states like Hawaii and Minnesota they are temporary, and must be approved by state officials (United States Department of Labor, 2014). Additional complicators are the variable extension durations and state funding availability.

In total 22 of 51 states maintain simple and composite AB programs.[4] Three states (California, Connecticut, District of Columbia) have AB trigger components based on state-unemployment indicators similar to EUC08 and EB (United States Department of Labor, 2014).[5] The majority of the 22 states, however, have permanent AB programs that constrain eligibility on individuals' (un)employment characteristics. For instance, individuals must engage in certified vocational training to be eligible in Wisconsin.

[4]Henceforth, the District of Columbia is referred to as a 'state'.

[5]In total six states' AB are triggered by state-level criteria, but three of these are not confounding for our study, because their UI extension is only triggered when federal (EUC08 or EB) extensions are not, which is never the case in our sample.

As individual-level microdata is inaccessible, this paper's analysis is restricted to aggregated macrodata, which precludes controlling for AB. Nevertheless, average aggregate AB payouts as a share of average EUC08 payouts are dwarfed (0.002%) over EUC08's lifetime. AB could be controlled for in the three AB states if trigger notices were available, but no such information is published. To ensure AB do not confound benchmark results, a robustness check excluding the three AB states is conducted in section 9.

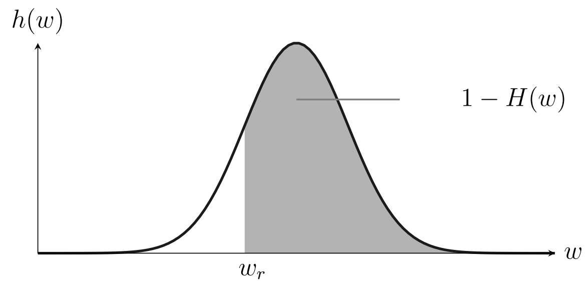
3 Theory

The main reason UI are collectively instituted and publicly provided is that private UI is unfeasible from an insurer's perspective (Tatsiramos & Van Ours, 2014). The problem initially arises from asymmetric information, dictating that the labourer has more information regarding their unemployment probability than the insurer. This ensures they have interest in downplaying their unemployment risk such that they may be charged a lower premium. The insurer's solution is to charge premiums based on the average unemployment risk. However, this makes UI unappealing for low unemployment risk workers, leading to adverse selection: the pool of UI candidates applying for UI consists primarily (if not solely) of high risk workers. Typically, the answer is for insurers to select applicants after screening procedures, or to engage in price discrimination, charging premiums specific to individuals' risk profiles. Neither solution is attractive from an ethical standpoint. The only solution remaining is for UI to be publicly instituted, whereby premiums are unrepresentative of risk profiles, as risk is pooled across individuals in an economy (Tatsiramos & Van Ours, 2014) and are funded by payroll taxes (Boeri & Van Ours, 2013). The alternative is for UI to not exist at all, which has dire consequences for the individual and by extension for society and the economy. Nevertheless, publicly provided benefits are by nature morally hazardous, inducing workers to reduce job search efforts, subsequently raising unemployment. This is known as the insurance-incentive trade-off that rests at the core of the optimal UI debate. Discussion is centred around UI generosity typically being a multidimensional labour market institution, such that it is, for instance, a function of benefit levels, potential benefits durations (PBDs) and eligibility.

The key theories providing insights into the effect of benefits generosity on labour market conditions are *search theory*, and *matching theory*. Both mathematical frameworks make substantial predictions on the effect of benefits generosity on job search and job separation (micro effects) and vacancy creation (macro effect). These theories are covered next, focusing on the predicted effects of PBD on unemployment, which is the UI dimension of interest in our study of the EUC08.

3.1 Search Theory

Search theory focuses on the microeconomic, partial equilibrium market outcomes as determined by jobseekers' search behaviour. The theory was pioneered in a labour market context by Lippman and McCall (1979), Mortensen (1977) and Van den Berg (1990). In search theory the unemployed's search behaviour is characterised by the opportunity cost of job search in terms of forgone leisure. Active jobseekers evaluate job vacancies arriving at the rate λ ($0 \leq \lambda < \infty$) by sequentially sampling respective wage offers conditional on a known wage distribution $H(w) = \int_0^w h(w)\ dw$, and deciding to accept and become employed or reject and continue search until they chance upon a job meeting expectations. This search pattern is termed *random search* (Mortensen, 1977). The static random search model assumes static job offer arrival rates, benefits duration is as long as the unemployment spell, UI levels are constant, no on-the-job search and distribution drawn wages are invariant over employment, so there are no promotions once a job is secured (Boeri & Van Ours, 2013; Van den Berg, 1990). In these circumstances optimal job search is typified by a stopping rule: the jobseeker accepts the first offer larger than a threshold wage, called the *dynamic* reservation wage w_r, which makes the seeker indifferent between job acceptance and continued search (Boeri & Van Ours, 2013; Lancaster & Chesher, 1983; Yoon, 1983). Hence, in the diagram below $1 - H(w)$ marks the cumulative probability of job offer acceptance.



Job and wage expectations are contingent on w_r, wherefore it is the sole mechanism through which jobseekers can influence job finding rates and unemployment duration. If w_r increases, the probability of acceptance declines such that unemployment duration rises. Mathematically, the dynamic reservation wage is (Van den Berg, 1990):

$$w_r = b + \frac{\lambda}{\rho + \delta} \int_{w_r}^{\infty} [w - w_r] \, dH(w).$$

Where b ($0 \leq b < \infty$) to encompasses all benefits generosity dimensions, including UI extensions, ρ ($0 < \rho < \infty$) is the discount factor and δ the exogenous job separation rate, or the rate at which jobs are terminated. Thus, the seeker determines their optimal reservation wage by equating the marginal cost to the marginal benefit of job search, given by UI generosity (b) received whilst searching, plus the present value of future earnings in excess of w_r, received from current search efforts. The reservation wage, w_r, is an increasing function of b, because b decreases the opportunity cost of unemployment by closing the earnings or utility gap between employment and unemployment. In other words, the difference in the utility of being employed and the utility of being unemployed decreases. Therefore, a rise in b shrinks the shaded portion in the diagram. Similarly, w_r is increasing in λ, as higher job offer arrival rates imply the seeker has more choice, ceteris paribus, and so can be more reserved in accepting. Contrarily, w_r is decreasing in δ, because a higher separation rate means the individual attaches less weight to excess future earnings. Consequently, the marginal benefit of search falls.

The static model can be expanded to include search effort as the second endogenous factor allowing jobseekers to influence job finding and unemployment duration. In this framework, job offer arrival rates are a function of search intensity s: $\lambda = \alpha\lambda(s)$, with $\lambda'(s) > 0$ $\lambda''(s) < 0$. Here α is an exogenous parameter representing labour market conditions, including changes in aggregate unemployment rates due to business cycle fluctuations (Van den Berg, 1990). Optimal search effort is then determined by equating marginal search costs, $c'(s) > 0$, to marginal search benefit (Boeri & Van Ours, 2013):

$$c'(s) = \frac{\alpha \lambda'(s)}{\rho + \delta} \int_{w_r}^{\infty} [w - w_r] \, dH(w).$$

When b increases, w_r increases. Consequently, the marginal benefit of search declines. To balance this, marginal search costs must fall, which is achieved by the jobseeker revising search efforts, s, downwards.

Thus, increases in b influence job finding and unemployment duration through two channels. Firstly, as b rises w_r falls by decreasing the opportunity cost of unemployment and closing the earnings gap between employment and unemployment. Secondly, a rise in b leads jobseekers to reduce search intensity, keeping marginal search costs and benefits equal. The lower search intensity and higher reservation wages contribute to lower job finding rates and higher unemployment (durations). The effect of these channels on unemployment is collectively termed the *search effect* (Boeri & Van Ours, 2013).

In partial equilibrium there is also the *entitlement effect* influencing unemployment rates (Tatsiramos & Van Ours, 2014). In countries like the US, UI is proportional to pre-unemployment earnings, such that UI eligibility is conditional on prior employment. Hence, changes in b affect the unemployed (entitled) differently than the inactive and long-term unemployed (nonentitled). The effect on the entitled is the *search effect*. The effect on the nonentitled is the *entitlement effect*. To explain, it is important to introduce the *static* reservation wage, w_r^n, which is the threshold minimum wage required for an inactive individual to exit inactivity and enter the active labour force.[6] The nonentitled's static reservation wages expressed in terms of entitled workers' dynamic reservation wages (w_r) are (Boeri & Van Ours, 2013):

$$\rho w_r^n = (\rho + \delta) b^n - \delta w_r + \lambda \int_{w_r^n}^{\infty} [w - w_r^n] \, dH(w).$$

As above, δ is the job separation rate. Specifically, δ is the probability of becoming unemployed in the future for the eligible *and* the ineligible once they have been employed. The nonentitled's income sources, other than work and UI, constitute b^n.

A negative relationship between generosity b and w_r^n exists through the second term $-\delta w_r$. Known from the search effect, as b increases the entitled's w_r rises. Consequently, w_r^n declines so the relative value of inactivity versus activity declines. This is because the value of benefits entitlement during a *future* unemployment spell, once employed, increases. This also implies the rise in b motivates intensive job search by *ineligible* active labour market participants, because employment grants future benefits entitlement (again). The *entitlement effect*, thus, decreases unemployment rates by incentivising the nonentitled to accept jobs in the hope of receiving UI in the future (Tatsiramos & Van Ours, 2014). Note, however, it may encompass an initial increase in unemployment as the inactive transition to unemployment.

[6] Note the nonentitled's static reservation wage equals their dynamic reservation wage, i.e. the wage to exit inactivity and become a *nonentitled* unemployed (because they have no prior work history), equals the minimum wage required for them to accept a job offer and go from being unemployed to employed.

Higher b also affects unemployment by affecting the employed. Because it reduces the cost of unemployment, it reduces the employed's work effort, leading to shirking. Subsequently, workers are more likely to be laid-off which increases the involuntary separation rate (Karni, 1999). Moreover, because the value of unemployment over employment rises, higher b increase voluntary separation. Both of these are minor effects, but cause an increase in unemployment rates. We call these the *separation effect*.

It follows from the *search effect* that a ceteris paribus increase in benefits generosity, like an UI extension, increases the unemployed's dynamic reservation wage and disincentivises job search, resulting in higher unemployment. The *separation effect* increases unemployment by increasing (in)voluntary job separation. Conversely, the *entitlement effect* motivates the nonentitled to accept job offers, which reduces unemployment rates. Overall, these counteracting forces result in higher short-run partial equilibrium aggregate unemployment rates, because the entitlement effect is typically smaller than the sum of the search and separation effects.

3.2 Matching Theory

The search-matching model studies general equilibrium in the labour market. Search-matching theory built on Diamond (1984) by Mortensen and Pissarides (1994), takes search theory further. It looks at how seeker-employer interactions shape labour market outcomes such that demand side forces also influence macroeconomic labour market equilibria through the *wage effect*. The search-matching process described next generates the wage effect. It is adapted from Mortensen and Pissarides (1994), with the job separation rate, δ, being exogenous instead of endogenous, and on-the-job search excluded for simplicity. Workers are either job-seeking unemployed or employed and occupied in production. Product markets are homogeneous, characterised by free market entry and exit, while labour markets face costless job creation and destruction.

The search-matching process models demand and supply-side interactions with the constant-returns-to-scale matching function $M(x, u)$. Here M is the flow of matches (number of jobs created) as a function of the *vacancy rate*, x, determined by normalising the number of vacancies over the total labour force. The unemployment rate is given by u. The unknowns of the model are x and u. On the supply-side, jobseekers search for jobs befitting expectations. On the demand-side, firms engage in *vacancy creation* by extending wage offers to the market for new openings. The *vacancy filling* or *transition rate*, is given by $m(\theta) = M(x, u)/x$, where $\theta = x/u$ denotes labour market tightness. The transition rate is decreasing in θ ($m(\theta)' < 0$), as in tight labour markets employment is close to the full employment level, wherefore $u \to 0$ whilst x is high.[7] Hence, excess labour demand makes filling vacancies difficult. Similarly, when labour markets are loose, u is high but $x \to 0$. Thus, with excess labour supply, the probability of filling existing vacancies is high. Finally, jobseekers' *job finding rate* is given by $\theta m(\theta)$, the product of the transition rate and labour market tightness.

This search-matching process yields two outcomes that create flows between the employment and unemployment pool. The first is *job creation*, which occurs when a jobseeker meets an attractive vacancy and production begins (Mortensen & Pissarides, 1994). It is the desired and mutually beneficial outcome of the bilaterally costly search-matching

[7] Empirically, we see $0 < \theta < 1$ in the US (Federal Reserve Bank of St.Louis, 2019a)

process. Firms want to fill their positions to begin production and turn profits, whilst jobseekers want to find employment to earn wages. The other matching process outcome is *job destruction*, which occurs when a filled job exits the market through separation.

The firm's key choice is to fill a post or keep it vacant. In this setup, the per unit time recruitment cost incurred by the firm for a posted vacancy is κ. The discounted value of (or profit from) a vacancy to the firm is then:

$$\rho\Pi_v = -\kappa + m(\theta)[\Pi_e - \Pi_v]. \tag{i}$$

Here, ρ is a generic discount factor. The expected value of a filled position to the firm is Π_e. Profits from a vacancy, thus, equal the producer's gain from leaving the position vacant over the alternative of filling the position ($m(\theta)[\Pi_e - \Pi_v]$), less the per-unit time recruitment cost. Given free entry in the product market, the average cost of a posted vacancy must equal the profits from such a vacancy, such that $\Pi_v = 0$.

Employers' discounted profits from a filled job are:

$$\rho\Pi_e = q - w + \delta[\Pi_v - \Pi_e]. \tag{ii}$$

Thus, expected profits are the output produced by the filled position (q) less the wage paid to the employee (w), plus the gain from not having left the position vacant $[\Pi_v - \Pi_e]$ multiplied by the probability of match termination (δ). From (i) and (ii), it is possible to derive the following expression (Boeri & Van Ours, 2013):

$$\frac{\kappa}{m(\theta)} = \frac{q-w}{\rho+\delta}. \tag{iii}$$

It indicates there exists a negative relation between wages w, and labour market tightness, θ. Appendix 12.1 gives a formal derivation.

The key decision of the worker is to be employed or remain unemployed. The value of unemployment to the entitled jobseeker (V_u) is:

$$\rho V_u = b + \theta m(\theta)[V_e - V_u], \tag{iv}$$

with b broadly denoting benefits generosity (or the value of leisure) as before, and V_e being the value or utility of employment to an employed worker. The unemployment value is, therefore, equal to the generosity of benefits plus the value of forgone leisure if the worker had been employed, $\theta m(\theta)[V_e - V_u]$.

Wages are the endogenous outcome of bilateral bargaining between a rational, self-interested employer and jobseeker. The bargaining involves splitting the total surplus from a 'match' or filled position (S) between the worker and firm. The worker and firm split according to their fixed surplus share, or bargaining power, β and $1-\beta$, respectively. The total surplus is the sum of employer and employee rents:

$$\begin{aligned} S &= [\Pi_e - \Pi_v] + [V_e - V_u] \\ &= \Pi_e + [V_e - V_u]. \end{aligned}$$

Where Π_e and V_e are the values obtained by the employers and workers' from matching. Because $\Pi_v = 0$ due to free product market entry, new jobs are created until all rents are depleted (Mortensen & Pissarides, 1994). The wage resulting from this bargaining process is (Boeri & Van Ours, 2013):

$$w = \rho V_u + \beta[q - \rho V_u]$$

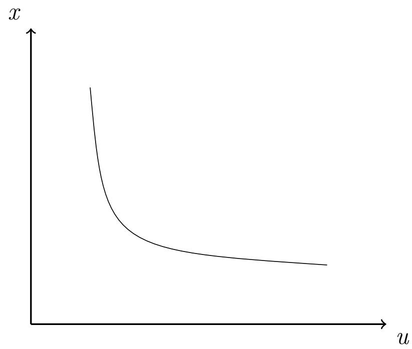
Hence, the negotiated wage is the value of unemployment to the worker, plus the worker's share of value added from production. This expression can be rewritten to:

$$w = b + \Lambda(\theta)[q - b]. \tag{v}$$

Here $\Lambda(\theta)$ is workers' total bargaining weight, with $\Lambda(\theta) = \frac{\beta[\rho+\delta+\theta m(\theta)]}{\rho+\delta+\beta\theta m(\theta)}$. Hence, workers' total bargaining weight, and thus the negotiated wage, is a function of β, labour market tightness, the discount factor, and the job separation rate. Furthermore, in the steady-state, where the unemployment pool's inflow and outflow are equal, bargaining results in the equilibrium unemployment rate:

$$u = \frac{\delta}{\delta + \theta m(\theta)}, \tag{vi}$$

expressed as the job separation rate (δ) in proportion to itself plus the job finding rate, $\theta m(\theta)$. Graphically, this relation is known as the Beveridge Curve:



Thus, from (iv) any increase in benefits generosity (b), like a UI extension, increases workers' utility from unemployment, subsequently increasing w in (v). As result, firms' expected profits from a match, on the right hand side of (iii), fall. As such, firms must decrease the total cost incurred from all vacancies, given by the left hand side of (iii), by reducing vacancy creation, which reduces labour market tightness, θ. This leads to a reduction in the seeker's job finding rate, $\theta m(\theta)$, "the rate at which seekers meet vacancies" (Mortensen & Pissarides, 1994), increasing equilibrium unemployment, u, in (vi). The channel through which b affects unemployment is also referred to as the *wage effect*.

In sum, the purpose of the two-sided matching model is to determine the unknown vacancy and unemployment rates by acknowledging the roles of both the jobseeker and employer. The unemployment rate is eventually given by the Beveridge relationship, (vi). How benefits generosity affects unemployment through the Beveridge relationship is also known as the *wage effect*. It predicts UI extensions raise equilibrium wages, such that employers' future profits diminish. Subsequently, they contract vacancy creation, thereby reducing labour market tightness. This reduces the unemployed's job finding rate, which reduces equilibrium unemployment.

4 Literature Review

The empirical literature on UI extensions can be partitioned into two: the microeconomic side and the macroeconomic side. The former relates to the effects of UI extensions on jobseekers, observing outcome variables such as individuals' re-employment outcomes and search intensities. It is a direct empirical reflection of search theory. Conversely, the latter comprises the optimal UI policy debate, analysing economy-wide general-equilibrium effects of UI extensions using simulations and macro-empirics. Accordingly, the macroeconomic side tests matching theory empirically. We discuss both strains as this study unifies the two facets of the research to some degree. We contribute to the latter as we use macro data, using unemployment (growth) rates as the outcome variable of interest. We build on the former by pioneering the analysis of gender heterogeneous UI extensions effects, departing from the fully aggregated approach characteristic to macroeconomics. Finally, we relay some of the findings on the differential effects of the Great Recession on unemployment across the sexes to gain insight into the channels behind possible gender heterogeneous EUC08 effects.

4.1 Microeconomic Studies

There is a large literature analysing microeconomic effects of UI on search behaviour. This literature provides critical perspectives from the supply-side of the labour market, observing how job-seekers revise search efforts in response to changes in benefits generosity. Older studies on the effect of benefits on UI duration, like Ham and Rea Jr (1987); Hunt (1995); Moffitt (1985); Moffitt and Nicholson (1982), agree that unemployment spells increase with benefits, consistent with search theory. Although it is unexplained why, Røed and Zhang (2003) also find men's benefits elasticity to be higher than women's, whilst the end-of-benefits-spike in unemployment exits is higher for women than men.

This significant positive relation between benefits and unemployment spells can also be extended to other dimensions of benefits generosity. Papers exploring UI extensions and maximum benefits durations also find higher PBDs increase unemployment spells (older works include Katz & Meyer, 1990; Winter-Ebmer, 1998). An influential work by Card and Levine (2000) capitalises on the politically motivated 1996 New Jersey Extended Benefits program, which provided a 13-week extension for six months. Using a DID with the neighbouring state of Pennsylvania as control, the authors find New Jersey saw a 1-3% increase in basic benefits exhaustion by claimants. This provides support for higher PBDs decreasing search efforts. Novo and Silva's (2017) findings are similar. The authors use the 1999 Portuguese UI reform, giving differential increases in maximum PBD across age groups, to conduct a DID study. They find the PBD results in a rise in unemployment rates and unemployment duration, in addition to a reduction in labour force participation. Thus, the empirical work tends to the consensus that jobseekers behave according to search theory, and decrease search efforts with the provision of UI extensions.

Recent literature expands UI extension research to alternative outcomes, and deepens it by segregating PBD effects by sex (e.g. Card, Chetty, & Weber, 2007; Lalive, Van Ours, & Zweimüller, 2006). For instance, Lalive (2007) exploit a discontinuity in PBD for individuals becoming unemployed at age 50 and older in Austria: they received up to 170 weeks more benefits than ineligible individuals. Lalive deduces small UI extensions (13 weeks) do not increase unemployment duration for men, but do for women, whereas longer increases significantly raise unemployment spells for both. Neither long nor short extensions affect post-unemployment earnings. Lalive (2008) further finds that the extension increased search duration by 0.09 weeks for men per extra week of benefits and by 0.32 weeks for women. They explain this incongruence by the lower early retirement age for women, such that the longer UI provides a pathway into early retirement. Furthermore, Caliendo, Tatsiramos, and Uhlendorff (2013) analyse the effect of UI extensions on individuals' unemployment duration, post-unemployment job quality, wages and stability in Germany. They utilise a sharp discontinuity in benefits duration, with individuals age 45 and above experiencing a rise from 12 to 18 weeks. They conclude individuals receiving extensions see significant improvements in all outcomes. This is because of non-stationary search intensity and reservation wages: search intensity spikes near benefits exhaustion as reservation wages plummet. Consequently, individuals eligible for benefits extensions are more selective in accepting jobs, leading to better outcomes. They find this effect to be significant for men, but not women, suggesting women might generally be less job selective across the board, or have more stationary search patterns and reservation wages.

The microeconomic literature, thus, concludes empirics follow search theory closely: increased benefits generosity results in longer unemployment spells. The effect on post-unemployment outcomes like wages and job quality is not so clear-cut. Some find insignificant (Card et al., 2007; Lalive, 2007; Van Ours & Vodopivec, 2008) changes attributable to changes in PBD, while others (Caliendo et al., 2013) demonstrate the contrary. Overall, no consistent pattern in UI extension effects on males versus females can be found across the literature. In some countries, like Austria, UI extensions disproportionately increase unemployment duration for females (Lalive, 2008), whilst in others, like Germany, it solely affects males (Caliendo et al., 2013). Notably, the gender heterogeneous UI research is sparse, and non-existent for the US. We aim to supplement this deficit.

4.2 Macro Studies

Compared to the microeconomic isolation of supply-side ramifications of UI policy, the macroeconomic research draws a more wholesome picture. It additionally examines how firms respond to UI and UI-related policy measures by adjusting vacancy creation. As such, Hagedorn, Karahan, Manovskii, and Mitman (2013) stress job finding and equilibrium unemployment rates are determined by the interaction of job creation *and* search efforts, as posited by matching theory. Simply investigating search intensities provides only half the picture. The authors utilise a regression discontinuity design (RDD) on the discontinuity of EUC08 across bordering counties in neighbouring states to analyse the effect of EUC08 on unemployment. They conclude UI extensions raise unemployment rates also by shrinking job creation. This result is conform prior research showing that UI changes affecting wages, like extensions, have large effects on firm profits. Consequently they significantly affect vacancy creation and equilibrium unemployment, as demonstrated by the Beveridge Curve (Costain & Reiter, 2008; Hagedorn & Manovskii, 2008).

Another branch of the macro literature focuses on the insurance-incentive trade-off and how UI extensions affect moral hazard over the business cycle. Schmieder et al. (2012) make a substantial contribution to this subject. They employ a RDD over age, which determines PBD in Germany, and correlate it with indicators of economic health, using

20 years of individuals' unemployment histories. They conclude unemployment durations in recessions insignificantly decrease due to extended benefits, versus in expansions. The implication is that the moral hazard posed by the search disincentive of UI extensions is significantly less during downturns than booms. Contrarily, UI claims rise considerably in recessions, producing vast positive effects on the economy as an automatic stabiliser. They infer the relative benefit of UI extensions far exceed costs. Di Maggio and Kermani (2016) obtain similar results. They find UI extensions negatively impact unemployment, but also decrease volatility in economic growth, consumption, employment, and income. Furthermore, they find extended UI boosts aggregate demand, buffering adverse economic shocks. Further research by Faccini and Rendahl (2016), introduces precautionary savings as result of idiosyncratic uncertainty and liquidity constraints into the matching model, to mimic US unemployment volatility. Their simulations and cross-country fixed-effects regressions suggest UI generosity significantly increases long-run unemployment rates and decreases GDP. In recessions, however, generous UI decrease unemployment growth and mediate the negative impact of recessions on income and consumption by stimulating spending. Their findings reinforce generous UI are effective automatic stabilisers.

In sum, macro-empirics highlight the importance of the demand-side wage channel through which UI extensions affect equilibrium unemployment. They confirm employers' downward vacancy revisions as result of extended UI increase unemployment rates. Furthermore, the literature agrees that although UI extensions increase unemployment rates, they significantly moderate the negative impact of recessions on unemployment. Research concentrating on the the insurance-incentive trade-off unanimously advances a pro-UI extensions position, particularly during economic downturns.

4.3 Gender Differences and the Great Recession

As evident from the micro-empirics subsection there is sparse research on gendered UI generosity effects. Nonetheless, there is ample work on gender heterogeneous unemployment effects of the Great Recession. This we outline here as background to our research.

Globally, the literature supports the Great Recession impacted men more acutely than women. The reasons are multifold. Firstly, males are the predominant workers in industries with pro-cyclical labour flows, such as construction and manufacturing, while women constitute the main public and service sector workforce (De la Rica & Rebollo-Sanz, 2017; Sahin, Song, & Hobijn, 2010). Consequently, men are more afflicted by recessions. Yet, Bachmann, Bechara, Kramer, and Rzepka (2015) show male unemployment was disproportionately stricken in the 2007 recession due to the Great Recession's toll on men's temporary employment. This relates to Grown and Tas's (2011) findings that women were over-represented in leaving unemployment for part-time work, supporting the *substitution hypothesis*. It dictates women are a cheap alternative source of labour during recessions Grown and Tas (2011). Moreover, Sahin et al. (2010) find an influx of male non-participants (e.g. students, discouraged male workers) reentering the labour force from non-employment with the recession's progression: a potential manifestation of the *entitlement effect* induced by the EUC08. Addabbo, Rodríguez-Modroño, and Gálvez-Muñoz (2013) also note the *discouraged worker effect* impacted women more heavily, so that more women exited unemployment into inactivity than men. Particularly true for women with young children, the effect reflects a higher opportunity cost to employment for women due to traditional gender roles. Addabbo et al. (2013); Starr (2014) also demonstrate gendered differences are due to the *added worker effect*: women increase labour supply in recessions to counteract household income loss from their partners' reduced earnings.

Subtleties in the impact of the Great Recession also exist within male and female populations. Factors like ethnicity, educational background and social class play a role (Rafferty, 2014). For example, Afro-Americans and Asians faced the brunt of the recessionary blow within sex-groups, with their average unemployment durations rising to 42 weeks. Households headed by single mothers also saw particularly dire employment circumstances compared to worst-off male subgroups (Grown & Tas, 2011).

Ultimately, the 2007-2009 recession impacted men's employment more severely than women. This is in part because male dominated industries move in closer tandem with the business cycle, amongst other reasons particular to the Great Recession, like more non-active males reentering activity. Arising from these findings is the question whether the EUC08 extension satisfied its purpose as (automatic) stabiliser by moderating the negative effect of the recession on unemployment for the most distressed groups. It follows, males might enjoy the benefits of EUC08 comparatively more *because* they were more afflicted. Although we can only investigate this question of interest per aggregate gender group, we explore it in the remainder of this thesis.

5 Empirical Strategy

The state-differential trigger timing of EUC08 generates a natural experiment allowing analysis of UI extensions on monthly state unemployment rates. This paper adopts a quasi-experimental DID approach, capitalising on two sources of variation in the data. The first is the time variation of treatment, EUC08, which lasted exclusively from 6 July 2008 until 28 December 2013. The second derives from locational differences in EUC08 eligibility, per the IUR and TUR explained in section 2.1. This allows for a control group of states that did not qualify for the UI extensions, and a treatment group that did, *in any given month*.

Ideally, the true causal effect of EUC08 would be found by comparing the difference in unemployment of the treated states (those receiving UI extensions) in any given month after EUC08 is triggered, to the counterfactual outcome of those same states at that same point in time in the hypothetical state-of-affairs where EUC08 is not triggered. This counterfactual is, per definition unobservable, such that the true causal effect cannot be identified by such methods. Instead, classic DID compares the difference in unemployment of the treated states before and after EUC08 is triggered, to the difference in unemployment of the untreated control states before and after treatment. Hence, the name 'difference-in-difference'.

Our case, however, is slightly more complex as there are no obvious treatment and control 'groups' in the classic DID sense. Instead our sample treatment and control groups consist of *state-month observations* that are and are not treated, respectively. To explain in simple terms, suppose Alaska is treated in month 60 of our sample, whilst California and Hawaii are not. The EUC08 phase consists of months 52 until 113. Then, our DID first takes the difference between the unemployment rate of the treated Alaska in month 60 and the average of the unemployment rates in the untreated California and Hawaii in month 60. In the second step, it deducts the mean of Californian and Hawaiian unemployment rates from Alaska's average unemployment rate in months where both the 'treatment' (Alaska) *and* 'control' (California and Hawaii) states are *untreated* (in the pre-treatment and post-treatment phases) to obtain the treatment effect in month 60. The complication arises because in month 61, it might be California and Hawaii which are treated while Alaska is not, in which case the treated and untreated states are reversed for month 61. In our sample, all states are treated at some point, so no states are exclusively treatment or control states, hence why treatment and control *groups* consist of *state-month* observations. It is only possible to identify the treatment effect because the states are never treated all at once during EUC08 implementation, which, if it were untrue, would preclude identifying the first step and thus the treatment effect.

The causal effect of interest is effectively identified on the assumption that treatment and control states in any given month are on similar paths for the outcome variable prior (posterior) to treatment (Angrist & Pischke, 2008). This is known as the common trends assumption, and is the key identifying assumption of DID. This assumption implies the difference in control states' outcomes pre-treatment (and post-treatment) are similar to the treated states' counterfactual difference in outcomes had they not received treatment. The assumption cannot obviously be tested, as treated states' counterfactual outcomes are unobserved. Nonetheless, a visual inspection of states' graphed historical unemployment rates provide a rough guideline (see section 7.1). A formal test looks for leading effects. These are effects that treatment has on the dependent variable prior (posterior) to treatment implementation. A one-month lead, for instance, assumes a value of one if in the next month treatment is 'on'. It equals zero otherwise. Leading effects test the common trends assumption by looking for pre-treatment trends that differ between control and treatment states. In a DID model, treatment variables' leading effects should be null for the common trends assumption to hold, so the true causal effect of treatment can be identified. Moreover, Hagedorn et al. (2013) note if firms are aware of upcoming UI extensions prior to policy implementation they may preemptively adjust vacancy creation, such that the wage effect and consequent policy effects on unemployment occur prior to the policy itself. In that case the treatment may wrongly appear to have no (contemporary) effect. Anticipatory effects, therefore, also capture the possibility of anticipation. Hence, to test the common trends assumption and observe potential anticipation we include anticipatory (leading) effects in our reference model, specified as a dynamic fixed effects (FE) model for total unemployment:

$$\begin{aligned} U_{st} = {} & \gamma_t + \beta_s + \beta_s \mathbf{X}_t + \sum_{k=1}^{x} \varrho_k U_{st-k} \\ & + \sum_{l=-5}^{5} \delta_l TIER2a_{st+l} + \sum_{m=-5}^{5} \vartheta_m TIER2c_{st+m} + \sum_{n=-5}^{5} \tau_n TIER3a_{st+n} \\ & + \sum_{p=-5}^{5} \psi_p TIER3b_{st+p} + \sum_{q=-5}^{5} \varphi_q TIER3c_{st+q} + \sum_{r=-5}^{5} \eta_r TIER4a_{st+r} \\ & + \sum_{u=-5}^{5} \omega_u TIER4b_{st+u} + \sum_{v=-5}^{5} \xi_v TIER4c_{st+v} + \sum_{w=-5}^{5} \lambda_w TIER4d_{st+w} \\ & + \phi EB_{st} + \varepsilon_{st}. \end{aligned} \tag{1}$$

Here, U_{st} is the total unemployment rate in state s in month t. The first month in our sample is August 2004 and the last (160^{th}) is November 2017. The full set of month FEs are given by γ_t. These capture state-invariant time FEs in the t^{th} month, like international diplomatic relations and concurrent national (policy) events, such as the Temporary Payroll Tax Cut Continuation Act of 2011, which may be correlated with EUC08 benefits extensions and state unemployment growth. Similarly, β_s denote the full set of state FEs. These parameters capture time-invariant state-specific FEs for the s^{th} state, including factors like geoclimatic conditions and (institutional) history. To allow certain time FEs to have varying effects across states over and above what is common to all states, we include an interaction between the state FEs and a vector $\mathbf{X}_t$ of these otherwise state-invariant, time-variant variables. The vector includes tiers 1a-1c and 2b of EUC08, and the national unemployment rate, to capture any state differential effects US economic conditions may have, for instance, due to different sector specialisations and intensities. To prevent collinearity between the time FEs and state-specific time FEs, we omit interacting one reference state (Alabama) in the state-specific time FEs. Notably, (1) is the long-run autoregressive distributed lag (ARDL) model for total unemployment. The unemployment rate in levels is nonstationary, but since the model variables are cointegrated (see section 6.4) we estimate it in levels. The corresponding short-run error correction model (ECM) ARDL is derived in appendix 12.2. We also estimate (1) in first differences to get a clearer picture of EUC08's effect on cyclical unemployment, and observe whether UI during recessions slows unemployment growth during recessions as purported by Faccini and Rendahl (2016). The idiosyncratic error is given by ε_{st}.

The $TIER_{st}$ variables are treatment indicators equal to one in months tiers are activated (triggers are 'on') in s, otherwise equalling zero. Tiers 1 and 2b are excluded, as these are captured by γ_t and $\beta_s\mathbf{X}_t$. The tiers are defined according to table 1. As clarified in section 2.1, the *a-d* suffixes on the tiers are not officially separate tiers, but considered as such for this paper because the trigger criteria and extension durations differ between them. Because trigger notices are published every week, we consider a state-month combination 'treated' ($TIER_{st} = 1$), if the tier has been triggered in that month's first full week. As EB programs trigger differentially between states at different times their omission might bias EUC08 estimators. Hence, EB programs are controlled for using EB_{st}, a dummy equal to one in months EB are activated in s.

We opt to model EUC08 treatment by its multi-tier scheme instead of with a single continuous treatment variable capturing the average effect of an incremental week of benefits over the whole EUC08 duration. The average effect of an incremental week of UI extensions has been widely researched (see section 4), and has been explored for EUC08 by Hagedorn et al. (2013). Instead, we exploit the unique multi-tiered quality of EUC08 to examine in detail the exact pattern of effects EUC08 has over its entire duration. We can, for instance, see whether unemployment rates consistently increase throughout the program, in accordance with search and matching theory, or whether these fluctuate, and if so by how much. Moreover, using leads it permits detecting the exact timing (i.e. for which tiers and thus also for how many months ahead of policy implementation) of preemptive vacancy creation by firms, in case it is present. This is further aided by the higher data frequency we use compared to Hagedorn et al. (2013), who use quarterly unemployment rates.

Furthermore, as EUC08 continues paying out to claimants until their extension duration is exhausted, it is likely that there are also effects in post-trigger months, irrespective of whether the treatment was triggered in that post-trigger month or not. For example, $TIER4a_{5,60} = 1$, can have effects in $t = 61$, irrespective of $TIER4a_{5,61} = 1$ or 0. Moreover, they allow us to see whether treatment effects fade or grow with time, and how the delayed effects compare to the instantaneous treatment effects. Accordingly, this thesis aims to test for delayed, or lagged treatment effects, following Autor (2003). A one-month lagged effect assumes a value of one if in the prior month treatment was triggered. Hence, we abstract from the literature by focusing on the anticipation and intermediate impact of UI policy by capitalising on EUC08's multi-tier scheme, and by looking at delayed UI extension effects, aside from analysing gender heterogeneity, discussed below.

Coefficients δ_l, ϑ_m, τ_n, ψ_p, ϖ_q, η_r, ω_u, ξ_v, λ_w, ϕ_x, ζ_y, φ_z catch delayed treatment effects when *l-z* are less than zero, anticipatory effects when *l-z* are larger than zero and the immediate effects when *l-z* equal zero. As there is no consensus on the number of leads and lags to include in the literature, we exploit the richness of the time series and set them to five, contrary to the three lags and two leads in Autor (2003). The number of leads and lags are constrained to be equal for all treatment levels across subgroups.

Up to x lags of unemployment are included in (1) for two reasons. Firstly, EUC08 is *not* an exogenous treatment: EUC08 is triggered by thresholds on prior 4-month (or, precisely, 13-week) averaged unemployment rates, by the IUR and TUR trigger criteria. As such, our study could suffer from serious policy endogeneity. Policy endogeneity describes the phenomenon where a policy treatment effect is overestimated or underestimated because the regression captures an evolutionary pattern within the regressand (Chang, 2018). Since unemployment rates are already high in pre-treatment periods, particularly for treated states, not controlling for the lagged dependent variable can result in observing (larger) positive EUC08 effects on unemployment growth than truly is the case, because of the endogenous timing and activation of EUC08 in treated states. The DID would wrongly attribute higher unemployment growth rates in treated states to the policy. In general terms we might refer to this endogeneity as reverse causality. To simply demonstrate the intuition behind the problem and solution, suppose a stylised version of model (1):

$$U_{st} = \gamma_t + \beta_s + \beta_s \mathbf{X}_t + \delta TIER_{st} + \varepsilon_{st}.$$

The FEs model is valid conditional on *strict exogeneity*: $\mathbb{E}[\varepsilon_{st}|\gamma_t, \beta_s, \beta_s\mathbf{X}_t, TIER_{st}] = 0$. In words, the random shocks to unemployment are orthogonal to present, past and future values of $\delta TIER_{st}$ and the FEs. In our case the reverse causality correlates current values of treatment $TIER_{st}$ with ε_{st}. This is as $\delta TIER_{st}$ depends on $\sum_{k=1}^{4} U_{st-k}$, and the dynamic or autocorrelated nature of unemployment implies U_{st} depends on $\sum_{k=1}^{x} U_{st-k}$. The lagged unemployment terms fall into the residual if omitted as regressors, thereby violating the strict exogeneity assumption. Following the method proposed by Leszczensky and Wolbring (2018) for eliminating reverse causality, we include four lags of the dependent variable in model (1), as treatment is determined by prior four months' unemployment. Note Hagedorn et al.'s (2013) RDD method resolves policy endogeneity more efficiently than our dynamic panel model. Yet, we do not employ it because it uses county-level data, which, due to discontinued reporting, has too limited a time-span to analyse delayed EUC08 effects. Proceeding, our amended stylised model is:

$$U_{st} = \gamma_t + \beta_s + \beta_s \mathbf{X}_t + \delta TIER_{st} + \textstyle\sum_{k=1}^{4} \varrho U_{st-k} + \varepsilon_{st}.$$

Therefore, the dynamic nature of the new stylised model and so (1), clears reverse causality by accounting for the interplay between unemployment growth and EUC08 activation over time. However, dependent variable lags introduce endogeneity in the model by correlating the random error with the lagged dependent variables. This constitutes a violation of the *sequential exogeneity* assumption behind linear dynamic panel models:

$$\mathbb{E}[\varepsilon_{st}|\gamma_t,\ \beta_s,\ \beta_s\mathbf{X}_t,\ TIER_{st}, ..., TIER_{st_0},\ U_{st}, ...,\ U_{st_0}] = 0,$$

$\forall\ \gamma_t,\ \beta_s,\ \beta_s\mathbf{X}_t,\ TIER_{st}, ..., TIER_{st_0},\ U_{st}, ...,\ U_{st_0}$, and $\forall\ s,\ t \geq t_0$, where t_0 is the first period in our sample. In words, EUC08 and lagged unemployment growth must be uncorrelated with present and future values of the random shocks to current unemployment ε_{st} (Acemoglu, Naidu, Restrepo, & Robinson, 2019; Leszczensky & Wolbring, 2018). This implies there must be no serial correlation in the residual. Hence, we come to our second reason for including dependent variable lags. We include x lags of unemployment growth, instead of four, in (1), to remove serial correlation from the residuals. In accordance with Hagan (1995), we assume unemployment growth is autoregressive of order one, AR(1):

$$\varepsilon_{st} = \rho\varepsilon_{st-1} + \epsilon_t,$$

with random error ϵ_{st}.[8] This allows us to obtain valid estimators using the classic within estimator, which for the stylised model is given by:

$$\begin{aligned} U_{st} - \frac{1}{T_s}\sum_y U_{sy} = \gamma_t + \delta(TIER_{st} - \frac{1}{T_s}\sum_y TIER_{sy}) \\ + \sum_{k=1}^{x} \varrho(U_{st-k} - \frac{1}{T_s}\sum_y U_{sy-k}) + (\varepsilon_{st} - \frac{1}{T_s}\sum_y \varepsilon_{sy}). \end{aligned}$$

Here $t = y$ is the final month in our sample and T is the total number of time points in the sample (160 in this study). The within estimators δ and ϱ still suffer from the order $1/T$ asymptotic Nickell bias (Nickell, 1981), caused by the correlation between U_{st-k} and $\frac{1}{T_s}\sum_y \varepsilon_{sy}$ when errors are autoregressive (both depend on ε_{st-k}). This violates the sequential exogeneity assumption. However, with a $T > 50$ Nickell bias is negligible, as $\text{plim}(\frac{1}{T_s}\sum_y \varepsilon_{sy}) = \mathbb{E}[\varepsilon_s] = 0$. This permits the dynamic FEs models to produce approximately consistent estimators (Acemoglu et al., 2019).

The equation (1) equivalent for males, and females if unemployment superscripts M are replaced by F, is given by (2). It includes both four lags of the *total* unemployment

[8] Literature also supports AR(2) unemployment processes (e.g. Fuller, 2009).

rate, to eliminate policy endogeneity, and z lags of the regressor to oust serial correlation:

$$\begin{aligned} U^M_{st} &= \gamma_t + \beta_s + \beta_s \mathbf{X}_t + \sum_{k=1}^{4} \varrho_k U_{st-k} + \sum_{d=1}^{z} \varsigma_d U^M_{st-d} \\ &+ \sum_{l=-5}^{5} \delta_l TIER2a_{st+l} + \sum_{m=-5}^{5} \vartheta_m TIER2c_{st+m} + \sum_{n=-5}^{5} \tau_n TIER3a_{st+n} \\ &+ \sum_{p=-5}^{5} \psi_p TIER3b_{st+p} + \sum_{q=-5}^{5} \varphi_q TIER3c_{st+q} + \sum_{r=-5}^{5} \eta_r TIER4a_{st+r} \\ &+ \sum_{u=-5}^{5} \omega_u TIER4b_{st+u} + \sum_{v=-5}^{5} \xi_v TIER4c_{st+v} + \sum_{w=-5}^{5} \lambda_w TIER4d_{st+w} \\ &+ \phi EB_{st} + \varepsilon_{st}. \end{aligned} \tag{2}$$

In our study of gender heterogeneous effects, the dependent variable (lag) is substituted with $U^M_{st} - U^F_{st}$, denoting the difference in unemployment between sexes.

In addition to the baseline regressions, several robustness checks are conducted. These include adding additional control variables to the regressions that may covary with explanatory variables of interest and the outcome variable. These may be incorporated into (1) and (2) with a vector $\Omega \mathbf{Y}_{st}$ including state-level productivity (GDP per worker), tax revenues, and the US Department of Agriculture's (USDA) simultaneously timed Supplemental Nutrition Assistance Program (SNAP) extension, also called the food-stamps program. The second check relaxes the common trends assumption by including a state-specific linear time trend controlling for potential long-term (longer than five months) lagged effects the 2002-2004 TEUC program may have on unemployment rates. This involves including a term $\mu(\beta_s \times TEUC_s \times t)$ in (1) and (2), where $TEUC_s$ is a time-invariant component giving the average number of TEUC weeks compensated over 2002-2004, and t is the linear trend equal to the sample month count. The last sensitivity analysis excludes the three states with state-month variant AB program components.

6 Data and Methodology

This thesis utilises a panel of repeated monthly cross-sectional data on state-level unemployment rates over 13 years. The raw macrodata required to build this panel consists of state-level unemployment numbers retrieved from the US Census Bureau (USCB) from each month's Current Population Survey (CPS).[9] Subsequently, the numbers are converted to rates, spliced into time series for each state, adjusted for seasonality, and combined into a panel.

The time series run from August 2004 up until and including November 2017, for a total of 160 months and 8160 state-month observations over 50 states and the District of Columbia. This time interval is selected to omit the confounding effects of the TEUC

[9]Note all data is macrodata, as microdata is only accessible to researchers.

program, which paid out until July 2004 (Bureau of Labor Statistics, 2019b). The observations last until 2017 to ensure an equal spread of time points before and after EUC08, which increases statistical power (Zhang, Wagner, & Ross-Degnan, 2011) and allows the use of a dynamic panel that makes testing EUC08 leads and lags possible. Since the leads and lags (also of the dependent variable) generate missing values, the regressed observation count falls from the total 8160 (see results tables for model and subgroup-specific counts). All regressions are estimated with standard errors (SEs) clustered by region. These adjust OLS SEs for any serial correlation remaining after adding dependent variable lags, and for heteroskedasticity defined by the cross-sectional co-dependence between states within each of the nine US Bureau of Economic Analysis regions. Given either serial correlation or intra-cluster correlation is underestimated with fewer than 42 clusters (Angrist & Pischke, 2008) clustered SEs are corrected for bias using the bias reduced linearisation process described by Pustejovsky and Tipton (2018).

6.1 Treatment Indicators

For general information regarding special benefits programs, official reports by the Congressional Research Service are consulted (see Whittaker & Isaacs, 2012, 2014). For program and tier activation data, the US Department of Labor's official trigger notices are used.[10] Note that we are unable to create the treatment dummies by programming an 'if' condition into `R` (the statistical program used), as the IUR is a program-based statistic (Whittaker & Isaacs, 2014), which requires data on the number of workers covered by UI, unavailable to us. In trigger notices, state-month treatment combinations labelled 'will begin' are marked 'on', as they satisfy the trigger criteria. The opposite is true for those marked 'will end'. When the end of one and beginning of the next *successive* tier overlap in one month, the value of the ending and beginning tier for that month is determined by whether that tier is active for at least ($TIER_{st} = 1$), or less than ($TIER_{st} = 0$) half of the weeks in that month. For instance, tier 2a, *ends* 7 November; tiers 3a and 4a *start* 8 November. Thus, 2a is considered inactive ('off') throughout November, whilst 3a and 4a are considered active ('on'). A similar decision rule is used for the nationally active tiers, 1a - 1c and 2b, used for the state-specific time FEs. These are, however, programmed with an 'if' condition.

6.2 Additional Control Variables

State-level productivity in thousands of dollars per worker, is computed by dividing state GDP by total employment.[11] The state-level seasonally adjusted quarterly GDP growth data is retrieved from the Bureau of Economic Analysis, part of the US Department of Commerce.[12] Monthly state-level employment is obtained from the US Bureau of Labor Statistics (BLS).[13] Monthly data on the expansion of food-stamps programs is from the US Department of Agriculture's Economic Research Service.[14] Quarterly state-level

[10] `https://oui.doleta.gov/unemploy/claims_arch.asp`

[11] Total employment means total *non-farm* employment. The Current Employment Survey does not collect farm employment data. Hence, non-farm employment is the most wholesome employment measure.

[12] `https://apps.bea.gov/iTable/index_regional.cfm`

[13] Again, non-farm: `https://beta.bls.gov/dataQuery/find?fq=survey:[sm]&s=popularity:D`

[14] `https://www.ers.usda.gov/data-products/snap-policy-data-sets/`

tax revenues in thousands of US dollars are from the USCB.[15] Because tax revenue and GDP are quarterly time series, we utilise a cubic spline to interpolate them to monthly time series, as specified by Forsythe, Malcolm, and Moler (1977).[16][17]

Using the SNAP data, we create a monthly dummy control variable denoting a state with expanded SNAP in a given month, by example of Hagedorn et al. (2013). The dummy equals one only "when states use broad-based categorical eligibility to increase or eliminate the asset test and/or to increase the gross income limit for virtually all SNAP applicants" (Hagedorn et al., 2013). The SNAP database runs from 1996 until December 2016, whilst productivity only runs from 2005 onwards (state-level GDP data is available from 2005). This disbalances time points around treatment. However, this is not problematic for inclusion of the 5-month leads and lags; it should only, ceteris paribus, marginally affect statistical power of the coefficients' significance tests (Zhang et al., 2011). The alternative of shortening the pre-treatment period by another seven months increases the probability of sketching an unrepresentative picture of the pre-treatment period, problematic for our DID identification strategy. Since there is a trade-off, we opt to use more data rather than less, using the time series from August 2004 until December 2016. The total observation count for this regression is 7344 (144 months by 51 states).

Data on the TEUC program is obtained from the USDL for the state-specific linear time trends.[18] Average per-state TEUC weeks compensated are computed over the complete duration of the TEUC from March 2002 up until and including January 2004. Seasonally adjusted national unemployment data used for creating a state-specific time intercept is obtained from the BLS.[19]

6.3 Seasonality Corrections

To test for delayed effects in total state unemployment, it would be possible to use the BLS unemployment per state time series (BLS does not publish state-level time series per gender). However, as of January 2014 several changes were made to the BLS data cleaning process for the official time series, with "revised population controls, model re-estimation, and new seasonal adjustment" (Bureau of Labor Statistics, 2019e).[20] Since 2014 the BLS switched to the new X-13-ARIMA-SEATS seasonality correction procedure from prior X-12-ARIMA use over 2003-2014. Changes are not radical, but we re-compute and adjust total unemployment rates from the raw unemployment numbers because it avoids systematic measurement error from discontinued processing methods (see Bureau of Labor Statistics, 2019d, for technical details). While the measurement error could bias treatment effects, it would be especially problematic for the analysis of delayed effects as the processing changes occur immediately after EUC08 ends in December 2013. Finally, creating the time series from scratch keeps data processing uniform across subgroups.

To utilise the most advanced processes, all monthly unemployment time series are adjusted using the X-13-ARIMA-SEATS package for `R`, programmed from the official

[15] `https://www.census.gov/programs-surveys/qtax/data/tables.All.html`

[16] Time series graphs show cubic splines fit the data more smoothly than other functions.

[17] Augmented Dickey-Fuller tests were conducted. Productivity and tax were non-stationary. We rely on panel cointegration tests (section 6.4) to see if further transformations are necessary.

[18] https://oui.doleta.gov/unemploy/teuc/.

[19] https://beta.bls.gov/dataViewer/view/timeseries/LNS14000000

[20] `https://beta.bls.gov/dataQuery/find?fq=survey:[la]&s=popularity:D`

USCP hard-code. In addition to selecting the optimal ARIMA (autoregressive integrated moving average) model to correct for seasonality, it accounts for national holidays, trading days, outliers and makes necessary transformations (Sax & Eddelbuettel, 2018). Despite the identical basis, however, our seasonally adjusted unemployment rates may differ from those that would be computed by the BLS, due to minor discrepancies like adjusting unemployment by industry before aggregating, and using more refined trade-day and automatic outlier detection processes (Bureau of Labor Statistics, 2019c). This should not drastically change regression outputs.

6.4 Pre-Estimation Tests

To remove autocorrelation from the models, satisfying the sequential exogeneity assumption, and test whether models (1) and (2) estimated in level unemployment rates are cointegrated, several pre-estimation tests are conducted.

For testing serial correlation and selecting dependent variable lag length, Wooldridge (W) tests for AR(1) serial correlation in panel models, and Breusch-Godfrey (BG) tests for AR(1) serial correlation, adjusted for panels, are performed (Breusch, 1978; Godfrey, 1978; Wooldridge, 2010). We report the results in appendix 12.3, table 8. Both tests are applied as the BG test is classically used to test for autocorrelation, while the W test is heteroskedasticity robust and designed specifically for fixed effects panel regressions (Wooldridge, 2010). Since from the standard four lag minimum on, the W tests consistently show no autocorrelation at every significance level, the maximum number of lags is determined by the BG test for extra precaution against violating sequential exogeneity. As the dependent variables are highly persistent, and adding too many lags of the regressand disbalances the time points around the treatment period substantially, the 1% significance level is sustained as the 'no serial correlation' threshold.[21] Arguably, this begs the question whether autocorrelation remains in the models to bias coefficients, as compared to using the standard 5% significance threshold maintained in this thesis. However, this trade-off must be made for the DID assumptions to hold adequately. Therefore, for further precaution we use autocorrelation (and cluster) robust SEs.

Augmented Dickey-Fuller (ADF) tests are conducted on the seasonally adjusted unemployment rate time series to test stationarity. We find they are nonstationary, whilst first differences are stationary.[22] To prevent spurious regression we test for cointegration in the model (1) and (2) panel residuals, estimated in level unemployment rates. This is done using Im-Pesaran-Shin bounds tests for panels. We use the bounds test as it allows a mix of stationary, and non-stationary (of order one) variables to be included in one model (Im, Pesaran, & Shin, 2003).[23] Technical details of the IPS test and the test results are presented in appendix 12.3. All our models indicate strongly cointegrated residuals, such that level unemployment model errors are stationary and the regressions present non-spurious relationships. It follows, we proceed to estimate (1) - (2) for unemployment in levels, in addition to first differences.

[21]Since the BG test null is *no autocorrelation* we wish to *accept* the null hypothesis.

[22]ADF test results are not reported due to the numerous time series (408: 51 states by for four subgroups, in levels and first differences).

[23]ADF tests showed the treatment variables were stationary, while other right-hand side variables were not, such as the national unemployment rate.

6.5 Gender Differential Effects

Econometric models (1) and (2) are streamlined to provide a clear overview of the distribution of (lagged) EUC08 effects per subgroup. Although it facilitates examination of this study's second aim, and the distribution of effects differentially across sexes, it also makes examining the first aim more difficult. Observing the gender heterogeneous effect of EUC08 is better explored with a figure expressing the overall EUC08 effect per subgroup, and on the gender difference variable in particular. Thus, to supplement the regressions and concisely summarise the gender differential EUC08 effect over its lifetime, a nonlinear Chi-squared Wald test is conducted on the regressions' significant coefficients.[24] We consult the delta method for nonlinear combinations of coefficients as the estimators represent *rates of change* in the unemployment rate, wherefore coefficients cannot simply be added for aggregation. Hence, the null hypothesis of no change in the unemployment rate due to EUC08 ($G(.)$) would be:

$$H_0 : G(\varphi_q) = (1 + \varphi_2) \times (1 + \varphi_3) - 1 = 0,$$

if for instance, only tier 3c lags 2 and 3 are significant. As in models (1) and (2), φ_q is the generic $TIER3c_{st+q}$ coefficient.

6.6 Descriptive Statistics

Table 2 gives summary statistics for all final variables included in the baseline regression as well as the sensitivity analyses. The 'treatment group' is defined by all state-month observations with one or more triggered state-specific EUC08 tier, whilst the control group includes all observations without active EUC08 tiers.

Table 2: Descriptive Statistics

	Treatment (N = 2592)				**Control (N = 5568)**			
Variable	Mean	Std. Dev.	Min.	Max.	Mean	Std. Dev.	Min.	Max.
				Dependent Variable: Unemployment Rate				
Levels								
Total	0.079	0.017	0.022	0.145	0.045	0.012	0.007	0.095
Female	0.072	0.017	0.016	0.147	0.043	0.013	0.005	0.102
Male	0.085	0.021	0.027	0.174	0.047	0.014	0.007	0.109
Gender Difference	0.014	0.019	-0.050	0.096	0.004	0.014	-0.052	0.076
First Differences								
Total	-0.000	0.009	-0.053	0.043	-0.000	0.007	-0.033	0.033
Female	0.000	0.012	-0.055	0.056	-0.000	0.010	-0.047	0.044
Males	-0.000	0.013	-0.059	0.069	0.000	0.010	-0.052	0.048
Gender Difference	-0.000	0.017	-0.069	0.079	0.000	0.014	-0.058	0.068
				State-Level Additional Control Variables				
Productivity	114	19	86	178	110	18	83	171
Tax Revenue	3915	5218	262	44583	3956	5139	54	51861

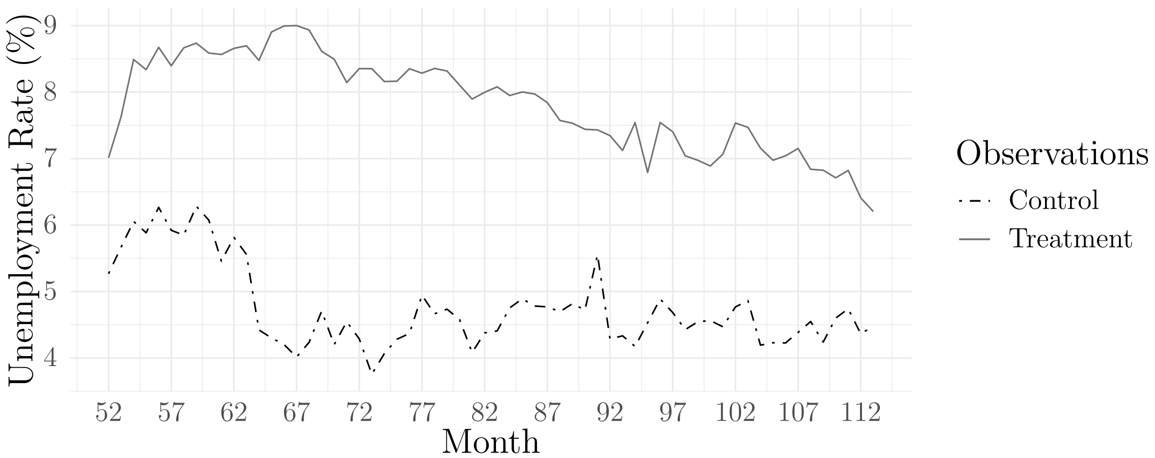
[24] Coefficients significant at the 5% level. The standard significance level sustained in this study.

SNAP Dummy	0.733	0.442	0	1	0.487	0.500	0	1
				State-Specific Intercept/Trend Variables				
TEUC Weeks Compensated	84674	95650	1619	471317	68805	89128	1619	471317
National Unemployment	0.087	0.009	0.067	0.100	0.054	0.111	0.041	0.100

Numbers rounded to three decimal places
TEUC and productivity rounded to the nearest integer
Tax revenue is in millions of US dollars, rounded to the nearest million
Productivity in thousands of US dollars per worker

Some cues emerge from the descriptive statistics. Firstly, control observations' mean unemployment falls approximately two standard deviations below treatment means, suggesting EUC08 had marginally significant, positive effects on unemployment rates across subgroups. For instance, the male unemployment rate in a treated state-month combination is 3.8 percent (0.085 - 0.047) higher than in a control observation. The difference in average unemployment rates between treatment and control observations is lowest for females at 2.9 percent (0.072 - 0.043). This figure suggests they benefit more from receiving extended benefits than men. Alternatively, it demonstrates females might be more employable, or reenter search to counteract household income losses during recessions, supporting the *substitution hypothesis* and the *added worker effect*. Because these figures are simple summary statistics, however, they also reflect policy endogeneity. State-month observations receiving treatment in month t have higher unemployment in prior months, which induces treatment. This can be observed in figure 1, where treated observations' unemployment lies above that of control observations. Remarkably, treatment and control observations' unemployment rates move in opposition, particularly during months 62 until 112 of the treatment period.[25]

Figure 1: Mean Total Unemployment

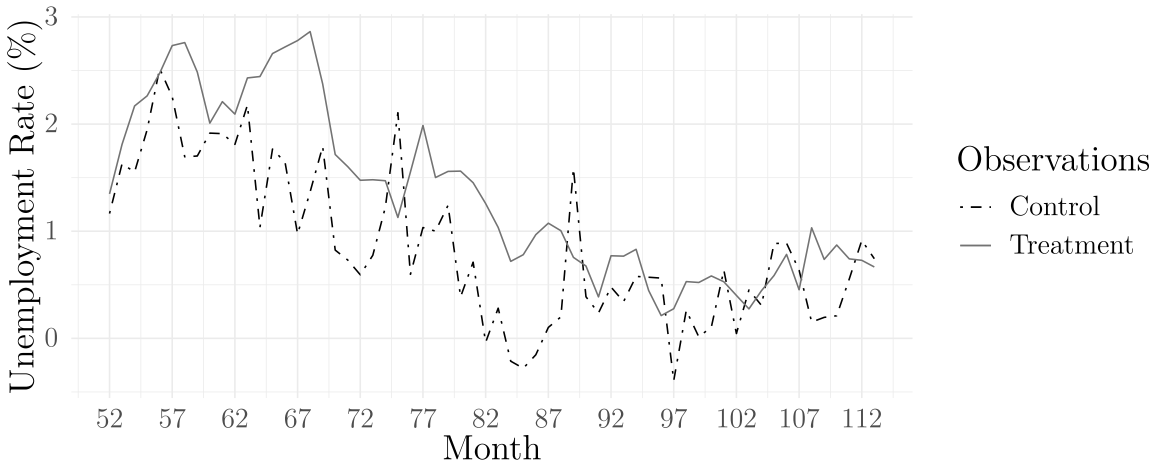


Contrary to the subgroups, the level gender difference variable sees control and treatment means falling within one standard deviation of each other. This suggests EUC08

[25]Similar remarks can be made for the female and male graphs in appendix 12.4.

effects are nondiscriminatory across genders, and challenges the *added worker effect* and *substitution hypothesis* advanced by the gender specific means. Figure 2 supports this, exhibiting no clearly higher or lower average difference in the gender difference variable between control and treatment observations. Similarly, for all subgroups the treated observation means for first difference unemployment rounded to three decimals are zero. Considering standard deviations, treatment observations' unemployment growth falls within one standard deviation of the control means and vice versa. This hints EUC08 may not have had a substantial impact on unemployment growth rates, and therefore on cyclical unemployment. Control group variables appear to have higher maximums, lower minimums, and overall, larger standard deviations for unemployment (growth), suggesting UI extensions reduce unemployment volatility.

Figure 2: Mean Unemployment Gender Difference



Women see lower unemployment maxima and minima, with a range of 13.1% in the treatment period, compared to men's 14.7%. In control observations, males have a 10.2% difference between the maximum and minimum, while women have a range of 9.7%. Ranges are higher in the treatment group, and relatively more so for men than women. This reinforces the literature on gendered differences in unemployment during the Great Recession. Higher male unemployment ranges may also result from stronger *search* and *separation effects* for men, because of EUC08. If true, EUC08 may have been enjoyed more by the distressed male subgroup, which would support the 'insurance' side of the insurance-incentive debate, in favour of UI extensions.

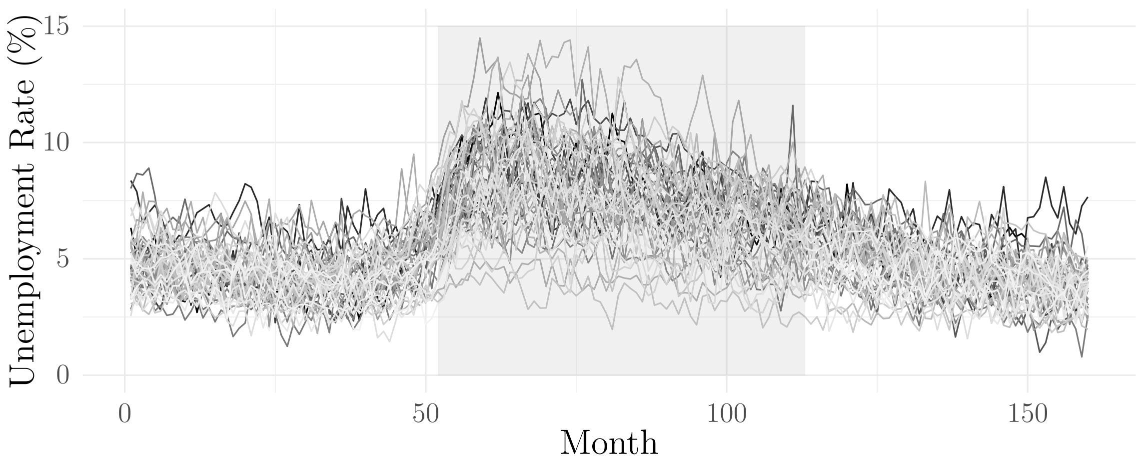
State-level additional controls' means in the treated groups fall within one standard deviation of control means. Productivity is on average four thousand dollars per worker higher for treated observations. Conversely, average tax revenues are higher for control observations. Food-stamps program extensions are, on average, granted more in states receiving extended UI than in those that are not. This may reflect how dire labour market circumstances translate to dire household circumstances. The average number of TEUC weeks compensated over its 2002-2004 lifespan is higher by 15869 (84674 - 68805) weeks for treated state-month combinations. It could mirror how specific states' labour markets are systematically more sensitive to external economic shocks due to characteristic differences, like industrial composition. To limit the influence of such factors we control for the state-specific effects of the national unemployment rate as well. An alternative explanation is that states that needed more TEUC support were also left more impaired in the aftermath of the 2001 recession. Consequently, during the 2008 recession they needed EUC08 more as well. Finally, higher average national unemployment for treated observations display the endogenous relationship between economic circumstances and extended UI.

7 Threats to Identification

7.1 Common Trends

Since we have 51 time series per demographic group (one for each state), it is difficult to draw a clear conclusion on the common trends assumption from multiple time series graphs. It must be cautioned that visual inspections for common pre-trends are more customary than statistically reliable. We proceed with the graphical analysis nonetheless, but caution its interpretation. Note we cannot generate pre-treatment and post-treatment period graphs per control and treatment 'group'. This is because each 'group' is determined by the number of (un)treated *observations*, of which there are none, by definition, in the pre-treatment and post-treatment periods.

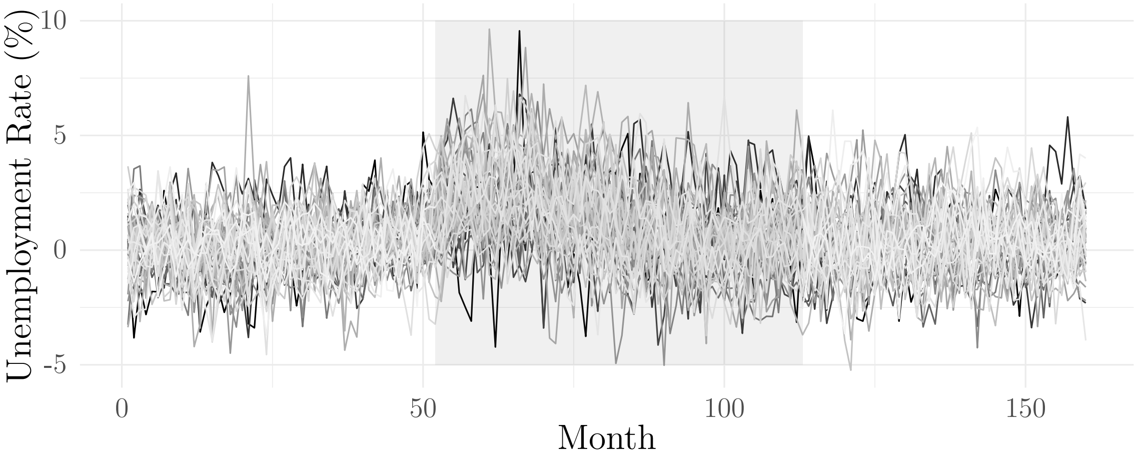
Figure 3: Per State Total Unemployment Rate



As figure 3 demonstrates, most states' total unemployment rate follow an approximately similar, polynomial trend in the pre-treatment and post-treatment periods, which constitute many of the control observations. The shaded area, from month 52 - 113, is the treatment period. Visibly, some states' pre-trends deviate somewhat from the trend, with more short-term volatility in total unemployment, and more unpredictable movements. It is expected these deviations will be accounted for by the FEs, control variables or TEUC trends. The graphs by gender can be found in appendix 12.5. They exhibit similar characteristics to figure 3. Noteworthy is that for males, state unemployment deviates marginally more from the majority common trend than for female and total unemployment. Figure 4 demonstrates gender difference pre-treatment and post-treatment trends are more linear than polynomial, whilst volatility for each *individual* state is also higher.

Notably, unemployment rates for many states (not just those deviating from the common trend) do not converge to the post-treatment majority trend *immediately* after

Figure 4: Per State Gender Unemployment Rate Difference

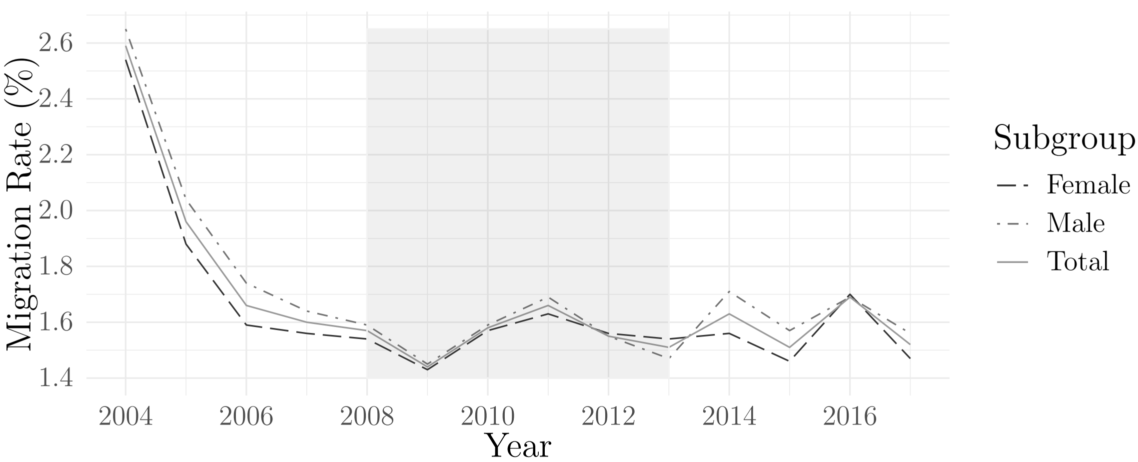


EUC08 ends. Instead of converging after month 113, these time series take approximately until month 120, hinting at the presence of lagged treatment effects.

7.2 Mobility

As treatment differs at the state level, inter-state mobility may confound regression results. It is possible that activation of a subsequent tier in a state may induce unemployed residents from other states (with lower or no triggered tiers) to in-migrate. This endogenously increases the treated state's unemployment rate, and overestimates the true causal effect of UI extensions on unemployment in that state. Figure 5 shows per-year inter-state migration as a percentage of state population with a minimum of 1-year residential status in that state. The shaded area is the treatment period.[26]

Figure 5: Inter-State Migration as Percent of Annual State Population



Average percentages of inter-state migration to population across all subgroups since

[26]Annual mobility data is from the USCB: `https://www.census.gov/topics/population/migration/data/tables.All.html`. The underlying data source is the American Community Survey.

2006 range from 1.44% for the aggregate group, to the maximum of 1.74% for males in 2009. The one year average over the 15 sample years across all groups is 1.68%. The maximum inter-state migration rate in any sample year is that for males in 2004 at 2.65%. Maximum inter-state migration for the other subgroups falls below this value. Graphically, inter-state migration has been on the decline consistently over the sample years. The census data also demonstrates inter-state (and domestic) in-migration to be at historically low levels since 2004 (Frey, 2017). Mobility poses a relatively small threat to identification. Therefore, we reasonably assume average treatment effects generated by this study's DID model are unbiased by inter-state migration.

8 Results

Table 3 gives the baseline regression output. Lag '0' is the contemporary treatment effect. Before discussing results we provide a brief discussion of anticipatory effects.

Appendix 12.6 presents the accompanying leading effects of all results and robustness checks, per table. Several tiers have statistically significant leads in the baseline models and for the sensitivity analyses. Because this indicates violation of the common trends assumption, we preclude interpreting those tiers' contemporary and lagged effects as causal in respective section 8 and 9 regressions. Tiers with non-causal 'significant' coefficients have significance asterisks placed within brackets in tables 3 - 6. Nevertheless, the benefit of including anticipatory effects combined with modelling EUC08 effects in its multi-tiered form, is that it enables observing whether leading effects exhibit intuitive anticipation or not. Indeed, we find it is exclusively the first (2a) and last (2c, 3c, 4d) implemented tiers with significant anticipatory effects. The only exception is tier 4b in the female regression, remedies are further discussed in section 11.

Significance of starting and ending tiers' leads is consistent with intuition. It demonstrates jobseekers and employers preemptively adjust job search and creation behaviour in expectation of UI extensions. US history of providing state differential UI extensions during high unemployment phases generates expectations of upcoming UI extensions ahead of implementation, particularly in states with high pre-extension unemployment rates. This explains pretrends for tier 2a. Pretrends for sequestration tiers indicate individuals in high unemployment areas anticipated EUC08 termination, possibly considering the program had been running exceptionally long already compared to historical UI extensions. This is despite the *official* sequestration term of EUC08 starting March 2013 (Whittaker & Isaacs, 2014), six months after the tiers' start in September 2012. The vast majority of tiers, however, see no significant leading effects. We focus on these henceforth.

Table 3: Baseline Results

	Dependent variable:							
	Unemployment Rate				**Unemployment Growth Rate**			
Tier/Lag	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/0	-0.003 (0.002)	-0.002 (0.001)	-0.004 (0.003)	-0.003 (0.003)	-0.003 (0.002)	-0.002 (0.001)	-0.004 (0.003)	-0.002 (0.003)

1	0.003	0.001	0.004*	0.002	0.002**	0.001	0.004	0.003
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.003)
2	0.000	0.002	0.000	-0.001	0.000	0.002	0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)
3	-0.002	-0.003	-0.003	-0.000	-0.002	-0.003	-0.003*	0.000
	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)
4	0.002	0.001	0.001	0.001	0.002	0.001	0.001	0.001
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)
5	-0.001	0.000	-0.002	-0.003	-0.001	0.000	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.004)
2c/0	0.004	0.007	0.002	-0.005	0.003	0.006	0.001	-0.007
	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.005)	(0.003)	(0.003)
1	-0.003	-0.001	-0.002	-0.002	-0.003**	-0.001	-0.002	-0.001(*)
	(0.002)	(0.004)	(0.003)	(0.005)	(0.002)	(0.004)	(0.003)	(0.005)
2	-0.000	0.001	0.002*	0.001	-0.000	0.001	0.002	0.001
	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)	(0.003)	(0.002)	(0.003)
3	0.001	-0.001	0.001	0.002	0.001	-0.001	0.001	0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
4	-0.001	0.001	-0.002	-0.003	-0.002	0.000	-0.003(*)	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.003)
5	0.001	-0.001	0.002	0.004*	0.000	-0.002	0.001	0.004
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
3a/0	-0.002	-0.003	-0.001	0.002	-0.002	-0.003	-0.002	0.001
	(0.002)	(0.005)	(0.002)	(0.003)	(0.001)	(0.005)	(0.001)	(0.002)
1	-0.001	0.003*	-0.001	-0.004**	-0.001	0.003*	-0.002	-0.004
	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
2	0.003	0.005**	0.001	-0.004**	0.002**	0.005**	0.000	-0.005*
	(0.002)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
3	0.001	-0.001	0.004	0.005	0.000	-0.003	0.003	0.005**
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.001)
4	-0.003*	-0.003**	-0.007*	-0.004*	-0.004*	-0.004**	-0.007***	-0.004*
	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
5	-0.001	-0.001	-0.002	-0.001	-0.001	-0.002	-0.002	-0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.004)
3b/0	-0.004	0.000	-0.006	-0.006	-0.005	-0.001	-0.007	-0.005
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.002)
1	0.000	-0.000	-0.001	-0.001	-0.000	-0.000	-0.001	0.000
	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)	(0.003)	(0.003)	(0.005)
2	0.002	-0.001	0.005	0.006	0.001	-0.001	0.004	0.006
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)	(0.004)	(0.004)
3	0.002	-0.000	0.003	0.003	0.002	0.000	0.002	0.003
	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)
4	-0.005**	-0.001	-0.008**	-0.008*	-0.006***	-0.001	-0.009***	-0.008
	(0.001)	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.005)
5	0.003	0.001	0.005	0.004	0.003	0.000	0.005*	0.004
	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)	(0.004)
3c/0	-0.001	0.002	-0.001	-0.003	-0.002	0.002	-0.002	-0.004(**)
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.001)
1	0.001	-0.001	-0.000	0.001	0.000	-0.002	-0.001	0.001
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)
2	-0.002	-0.003	-0.002	0.001	-0.003**	-0.004	-0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.004)
3	0.001	0.001	0.001	-0.001	0.001	0.002	0.001	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
4	0.000	0.001	-0.001	-0.002	0.000	0.001	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.004)

5	0.001	-0.000	0.002	0.002	0.000	-0.001	0.001	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
4a/0	-0.003	-0.003	-0.003	-0.000	-0.003	-0.004	-0.003	0.002
	(0.003)	(0.003)	(0.002)	(0.005)	(0.002)	(0.003)	(0.003)	(0.003)
1	0.002	0.005***	-0.001	-0.006	0.002	0.005***	-0.001	-0.006
	(0.002)	(0.001)	(0.003)	(0.004)	(0.001)	(0.001)	(0.004)	(0.004)
2	-0.001	-0.006***	0.002	0.008*	-0.001	-0.006***	0.002	0.008*
	(0.002)	(0.001)	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)	(0.003)
3	-0.002	0.002	-0.005	-0.007	-0.002	0.002	-0.005	-0.007
	(0.002)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)	(0.006)
4	0.001	0.001	-0.000	-0.002	0.001	0.000	0.000	-0.001
	(0.001)	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)
5	-0.001	0.001	0.001	-0.001	-0.001	-0.001	-0.000	-0.002
	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)
4b/0	0.001	-0.001(*)	0.002*	0.003	0.001	0.000	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.004)	(0.001)	(0.001)	(0.002)	(0.002)
1	-0.000	-0.003	0.002	0.005	-0.000	-0.002	0.002	0.004*
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
2	0.001	0.002(**)	-0.000	-0.003	0.001	0.003*	-0.000	-0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
3	0.002	0.001	0.003	0.003	0.002	0.001	0.003**	0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
4	-0.002	-0.002	-0.001	0.001	-0.002	-0.002	-0.002	0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
5	-0.000	-0.002	-0.000	0.001	-0.000	-0.001	-0.001	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
4c/0	0.001	-0.005*	0.005	0.009	-0.001	-0.007*	0.002	0.009
	(0.003)	(0.002)	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)	(0.005)
1	-0.003	-0.001	-0.004	-0.003	-0.005**	-0.003	-0.005	-0.003
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.002)
2	0.009**	0.011**	0.005	-0.005	0.008**	0.010**	0.004	-0.006
	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)	(0.005)
3	-0.004**	-0.005*	-0.001	0.005	-0.004***	-0.005*	-0.000	0.004
	(0.001)	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.004)	(0.004)
4	-0.001	-0.001	-0.003	-0.002	-0.002	-0.001	-0.003	-0.002
	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)
5	0.005**	0.003	0.004	0.001	0.004**	0.002	0.003	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.004)	(0.003)
4d/0	0.002	0.002	-0.001	-0.004	0.002	0.003	-0.001	-0.004**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.001)
1	-0.001	-0.001	0.002	0.002	-0.001	-0.001	0.002	0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
2	0.006***	0.007(**)	0.004(*)	-0.003	0.006***	0.007(**)	0.003	-0.003
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
3	-0.004**	-0.005	-0.003	0.001	-0.005**	-0.005	-0.003	0.002
	(0.001)	(0.003)	(0.003)	(0.005)	(0.002)	(0.003)	(0.003)	(0.005)
4	-0.004*	-0.005	-0.003	0.002	-0.004	-0.004	-0.003	0.001
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.005)
5	0.005**	0.007(**)	0.002	-0.006(**)	0.004	0.007(**)	0.001	-0.006
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Nonlinear Wald Test								
Aggregate	0.012***	0.012***	-0.008***	-0.008***	0.004	0.010	-0.013***	0.001***
effect	(0.005)	(0.004)	(0.003)	(0.002)	(0.004)	(0.005)	(0.004)	(0.002)
N	7,548	7,242	7,548	7,344	7,395	7,242	7,242	6,834

Months	153	147	153	149	150	147	147	139
States	51	51	51	51	51	51	51	51
Lags	7	13	7	11	10	13	13	21
FEs	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.562	0.384	0.486	0.238	0.292	0.318	0.306	0.341
Overall	0.903	0.797	0.862	0.440	0.284	0.307	0.297	0.329

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

Table 3 shows, EUC08 has significant causal effects, whether positive or negative, on unemployment and unemployment growth for all subgroups. It follows, EUC08 provides substantial insurance coverage for the unemployed, bolstering the 'insurance' side of the insurance-incentive trade-off. The case is particularly strong for the population total and females. Despite inter-group differences, the absence of significant contemporary effects is notable. EUC08 tiers affect unemployment rates with a minimum delay of one month after implementation. The majority of effects occur two to four months after implementation. Tiers with the most impact (those significant in multiple regressions for several subgroups) are exclusively lags. Tier 3b, for instance, affects male and total unemployment (growth) four months after implementation, whilst tiers 3a and 4c affect female and total population unemployment (growth) two and three months after implementation, respectively. The distribution of effects is, thus, highly partial to delay for all subgroups.

Effects do not appear to consistently grow or fade over time, but fluctuate for all subgroups. The presence of positive *and* negative treatment effects express the plethora of conflicting forces determining short-run UI extension effects. On the one hand the *search*, *separation* and *wage effects* increase unemployment rates by reducing jobseeker search efforts, increasing job separation and raising bargained wages. On the other hand, the *entitlement effect* decreases unemployment as labour force nonparticipants, like students and discouraged workers, re-enter activity and increase search efforts to earn future entitlement rights. EUC08 being enforced during a recession, further complicates inter-channel dynamics, as recessions dampen the moral hazard of UI extensions, leading to reduced search and separation effects. Subsequently, if the magnitude of the entitlement effect and reduced moral hazard are larger for a given tier and lag, it registers as a decrease in unemployment figures. Conversely, if the search, separation and wage effects are larger, it results in a positive EUC08 unemployment effect for the tier lag or instantaneous effect.

These same forces can explain the disparate overall EUC08 effects seen between the sexes. The Great Recession took a disproportionate toll on male unemployment. It follows that EUC08 might aid the more distressed subgroup more, by providing mental and financial support through insurance. This would manifest in the numbers as males having *more* and *more sizeable* effects, positive or negative, on unemployment (growth) rates. However, the results show otherwise: males see fewer and (although not much) smaller unemployment effects than women. Consequently, insurance effects are more prominent for women. This finding questions the effectiveness of UI extensions as insurance, and as (automatic) stabilisers, as the worst off group enjoyed the least of the policy's benefits. Yet, the Wald tests refute this initial impression. The aggregate EUC08 effects show males see a small *decrease* in their unemployment (growth) rates, while females see an *increase* in unemployment and an insignificant effect on unemployment growth. The gender difference variable confirms the gender unemployment gap declines significantly by 0.8% because of EUC08. The UI extension, therefore, relieved the relatively larger unemployment affliction of the Great Recession on males. Even though women enjoy more of the financial support from longer insurance provisions, demonstrating a weaker 'insurance' benefit for men, men enjoy the extension's assistance by regaining employment, demonstrating a net absence of EUC08 disincentive costs for them. On balance, these results provide support of UI extensions being effective (automatic) stabilisers, strengthening the argument in favour of UI extensions in the insurance-incentive trade-off debate.

To delve deeper, negative male unemployment *growth* indicates male cyclical unemployment decreases as result of EUC08. This demonstrates that the vulnerability of male private sector employment to business cycle risk can be counteracted with stabilisers like UI extensions, consistent with Faccini and Rendahl (2016). The difference variable shows EUC08 decreases male unemployment growth by 0.1% less than for females, however (Wald test column 8). This is a sign of higher male unemployment persistence in the private sector.[27] Moreover, the *level unemployment* gender gap reduction (Wald test, column 4) indicates EUC08 reduced structural unemployment differences between men and women. By induction, this finding could be an ode to the severity of the Great Recession: it not only created cyclical unemployment, as is common to recessions, but also induced structural disruption in the economy.

The reduction in male level unemployment rates can be explained by a sizeable entitlement effect. Sahin et al. (2010) remark that during the Great Recession, there was a disproportionate influx of males re-entering the labour market from the inactivity pool. In line with the entitlement effect, male search effort increased leading to male jobseekers finding more employment. Alternatively, the extensions may (also) have provided unemployed men with an opportunity to invest in training and (re-)education, leading to a small reduction in males' structural unemployment (Wald test, column 3).

For females the dominant effect could be any or all of the search, separation and wage effects. Unfortunately, the exclusive use of macrodata prevents us from directly observing which. Nevertheless, the presence of a prominent search effect amongst women is contradicted by Grown and Tas's (2011) finding that women were less selective with job acceptance than men during the Great Recession, also exhibiting higher exit rates into temporary employment while preferring permanent employment. The primary channel being the wage effect is somewhat unlikely as well. Public sector wages tend to be inflexible to bargaining, particularly during recessions (Lewin, 2012). As the public sector largely remains women's preferred employment sector, a dominant wage effect is unlikely. The separation effect could, however, be a valid explanation. Although equalising across sexes over time, women still have higher opportunity costs to work due to persisting gender roles (see e.g. Boeri & Van Ours, 2013; Raymo, Musick, & Iwasawa, 2015).

One outstanding factor for women is, as with men, the increased female labour force participation during the recession. Although the *added worker effect* is typically posited as the outcome of difficult household financial circumstances, the effect can also, in part, be attributed to increased benefits generosity. It can be seen as a specific entitlement effect:

[27]This seems to be contradicted by the insignificant overall female unemployment growth effect. We stress the difference variables test if male and female tier effects are *significantly different from each other*, irrespective of whether they are significant in the individual subgroup regressions or not. As result, the aggregate difference effect may also show discrepancy versus the difference in male and female aggregates.

women enter the labour force to supplement household income, both through earning wages *and* through benefits entitlement. When benefits generosity increases, women on the margin of joining the labour force may be pushed past and enter the labour market.

Yet, in our case, female entitlement effects and subsequent intensive search *do not* lead to higher female employment rates. The first reason is a relative rise in public versus private sector wages over 2007-2011 (Boeing-Reicher & Caponi, 2016). This pay rise caused a reduction in the gender pay gap, as women incline more towards public sector jobs while men more towards private sector jobs. Higher relative wages in the sector in which women search more intensively results in a decrease in vacancy creation in that sector, and so, fewer workers being employed there. Given relatively inflexible female job preferences and job-specific skill sets, women who (re-)enter the labour market to search may not obtain employment status due to fewer positions being open in the public sector.

Moreover, Lewin (2012) and Boeing-Reicher and Caponi (2016) note public sector jobs promise high stability, so there are typically few layoffs during recessions. Although this security is beneficial to existing public sector workers, it also means recessionary budget cuts are realised through taking fewer new recruits. Thus, the public sector's job stability also leads to fewer vacancies being created in that sector, and so fewer women exiting unemployment. As long as women are predisposed to public sector jobs, (re-)entering the labour market and searching intensively is not sufficient for them to find employment. Hence, the female unemployment pool grows, increasing the female unemployment rate.

In sum, the gender unemployment gap induced by the Great Recession decreases because of EUC08 UI extensions. We theorise this for two reasons. Firstly, a substantial male *entitlement effect* boosts men's search intensity and job finding. Moreover, the *added worker effect* leads to an influx of females into the labour force, and so, into the unemployment pool. Yet, due to reduced public sector vacancy creation, females preferring public sector jobs remain unemployed. Hence, men's unemployment rates fall, while women's rise, leading to the negative 0.8% aggregate gender difference effect of EUC08.

9 Robustness Checks

9.1 Additional Control Variables

The first robustness check executes models (1) - (2) with extra state-month control variables that may correlate systematically with state unemployment and EUC08. The first covariate is state-level productivity. Productivity declines during recessions, wherefore it correlates negatively with EUC08, whilst simultaneously negatively impacting job creation. This increases unemployment. If changes in productivity significantly affect unemployment, excluding it from the model can overestimate the effect of EUC08 on unemployment. Secondly, we include state tax revenues to broadly control for alternative support systems extended to the unemployed during dire economic circumstances, which may correlate with UI enrolment. Finally, we control for the expansion of the SNAP program over EUC08's duration. It provides impoverished households with food-stamps to spend at authorised grocers (Economic Research Service United States Department of Agriculture, 2019). Essentially, it is a specific UI, creating effects similar to wage and search effects (although no entitlement effects as the inactive are also eligible). Consequently, excluding significant SNAP effects may overestimate baseline treatment effects.

Table 4: Additional Control Variables

	Dependent variable:							
	Unemployment Rate				Unemployment Growth Rate			
Tier/Lag	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/0	-0.003*	-0.002	-0.004	-0.003	-0.003	-0.002	-0.004	-0.002
	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.004)	(0.003)
1	0.003*	0.001	0.004*	0.003	0.002(**)	0.001	0.004	0.003
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
2	0.000	0.002	0.000	-0.001	0.000	0.002	0.000	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.003)
3	-0.002	-0.003	-0.003	-0.000	-0.002	-0.003	-0.003	-0.000
	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
4	0.002*	0.001	0.001	0.001	0.002	0.001	0.001	0.001
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)
5	-0.001	0.001	-0.002	-0.003	-0.001	0.001	-0.001	-0.002
	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.005)
2c/0	0.004*	0.007	0.001	-0.005	0.003	0.006	0.001	-0.006(*)
	(0.001)	(0.004)	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
1	-0.003	-0.001	-0.002	-0.002	-0.003	-0.001	-0.002	-0.001
	(0.002)	(0.004)	(0.002)	(0.006)	(0.003)	(0.004)	(0.003)	(0.005)
2	0.000	0.001	0.002	0.001	-0.000	0.002	0.002	0.001
	(0.001)	(0.004)	(0.001)	(0.004)	(0.001)	(0.003)	(0.002)	(0.004)
3	0.001	-0.001	0.001	0.002	0.001	-0.001	0.001	0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)
4	-0.001	0.001	-0.002	-0.003	-0.002	0.000	-0.003*	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
5	0.001	-0.001	0.003	0.004	0.001	-0.002	0.001	0.004
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
3a/0	-0.001	-0.003	-0.001	0.002	-0.002	-0.003	-0.002	0.001
	(0.002)	(0.004)	(0.001)	(0.004)	(0.001)	(0.004)	(0.002)	(0.003)
1	-0.000	0.003*	-0.001	-0.003	-0.001	0.003	-0.001	-0.004
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
2	0.003	0.005*	0.001	-0.004	0.002*	0.005*	-0.000	-0.005
	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
3	0.001	-0.001	0.004	0.005*	0.000	-0.002	0.003	0.005**
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
4	-0.003	-0.003	-0.007	-0.004	-0.004**	-0.004*	-0.007**	-0.004
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
5	-0.001	-0.001	-0.002	-0.001	-0.001	-0.002	-0.002	-0.001
	(0.001)	(0.002)	(0.003)	(0.004)	(0.001)	(0.003)	(0.003)	(0.004)
3b/0	-0.004	0.000	-0.007**	-0.006*	-0.005*	-0.001	-0.007	-0.005
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
1	0.000	0.000	-0.001	-0.001	-0.000	-0.001	-0.001	0.000
	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.005)
2	0.002	-0.001	0.005	0.006	0.001	-0.001	0.004	0.005
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)
3	0.002	0.000	0.003	0.002	0.002	0.000	0.002	0.002
	(0.002)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)	(0.003)	(0.004)
4	-0.005**	-0.001	-0.008**	-0.008**	-0.006***	-0.001	-0.009***	-0.008*
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
5	0.003	0.001	0.005	0.004	0.003	0.001	0.006*	0.004

	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
3c/0	-0.001	0.002	-0.001	-0.003	-0.002	0.002	-0.002	-0.004(*)
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
1	0.001	-0.001	-0.000	0.001	0.000	-0.001	-0.001	0.001
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.004)
2	-0.002	-0.003	-0.002	0.001	-0.003**	-0.004	-0.002	0.001
	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.003)
3	0.001	0.001	0.001	-0.001	0.001	0.002	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)
4	0.000	0.001	-0.001	-0.002	0.000	0.001	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(0.002)
5	0.001	-0.000	0.002	0.002	0.000	-0.001	0.001	0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.001)
4a/0	-0.003	-0.003	-0.003	-0.000	-0.003	-0.004	-0.003	0.002
	(0.002)	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)
1	0.002	0.006**	-0.001	-0.006	0.002	0.005**	-0.001	-0.006
	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)	(0.002)	(0.004)	(0.003)
2	-0.001	-0.006***	0.002	0.008**	-0.001	-0.006***	0.002	0.008*
	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.003)	(0.004)
3	-0.002	0.002	-0.005	-0.007	-0.002**	0.001	-0.005	-0.007
	(0.002)	(0.002)	(0.003)	(0.005)	(0.001)	(0.002)	(0.003)	(0.005)
4	0.001	0.001	-0.001	-0.002	0.001	0.001	-0.000	-0.002
	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
5	-0.001	0.001	0.000	-0.001	-0.001	-0.001	-0.000	-0.002
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.003)	(0.002)
4b/0	0.001	-0.001	0.002	0.002	0.001	0.000	0.002	0.000
	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.002)	(0.003)
1	-0.000	-0.003	0.002	0.005	-0.000	-0.002	0.002	0.004
	(0.001)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)
2	0.001	0.002	-0.000	-0.003	0.001	0.003	-0.000	-0.003
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
3	0.002	0.001	0.003	0.003	0.002**	0.001	0.003*	0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
4	-0.002	-0.002	-0.001	0.001	-0.002	-0.002	-0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)
5	-0.000	-0.002	-0.000	0.002	-0.000	-0.001	-0.000	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
4c/0	0.001	-0.005	0.005	0.009**	-0.001	-0.008*	0.002	0.010
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)	(0.003)	(0.006)
1	-0.003	-0.001	-0.004	-0.004	-0.005**	-0.002	-0.006	-0.003
	(0.002)	(0.002)	(0.004)	(0.005)	(0.001)	(0.002)	(0.004)	(0.003)
2	0.009**	0.011***	0.005	-0.006	0.008**	0.010***	0.004	-0.006
	(0.003)	(0.002)	(0.003)	(0.005)	(0.002)	(0.001)	(0.004)	(0.004)
3	-0.003**	-0.005***	-0.000	0.005	-0.004**	-0.005***	-0.001	0.005
	(0.001)	(0.001)	(0.004)	(0.004)	(0.001)	(0.001)	(0.004)	(0.004)
4	-0.001	-0.000	-0.002	-0.002	-0.002	-0.000	-0.003	-0.002
	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.004)
5	0.005**	0.003	0.004	0.002	0.004**	0.002	0.003	0.0001
	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.004)	(0.003)
4d/0	0.002	0.003	-0.001	-0.004	0.002	0.003	-0.000	-0.004(**)
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
1	-0.001	-0.001	0.002	0.002	-0.001	-0.001	0.002	0.002*
	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.004)
2	0.006***	0.007(*)	0.004	-0.003	0.006***	0.006	0.003	-0.003
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)
3	-0.004**	-0.005	-0.003	0.001	-0.005*	-0.005	-0.004	0.002

	(0.001)	(0.004)	(0.003)	(0.004)	(0.002)	(0.004)	(0.003)	(0.004)
4	-0.004*	-0.004	-0.003(*)	0.002	-0.004	-0.004	-0.003	0.001
	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)	(0.003)	(0.004)
5	0.004**	0.007(**)	0.002	-0.006***	0.004	0.006(**)	0.001	-0.006
	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)	(0.003)
				Additional Control Variables				
SNAP	0.002**	0.001	0.002**	0.001	-0.000	0.000	-0.000	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Prod.	-0.000	-0.000	-0.000	0.000	0.000	0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Tax	0.000	0.000	0.000	-0.000	-0.000	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
				Nonlinear Wald Test				
Aggregate	0.012***	0.006**	-0.015***	0.003	-0.004	0.004	-0.016***	0.005***
effect	(0.004)	(0.003)	(0.003)	(0.005)	(0.005)	(0.004)	(0.004)	(0.002)
N	6,783	6,222	6,732	6,477	6,579	6,222	6,477	6,171
Months	154	143	153	148	150	143	148	142
States	51	51	51	51	51	51	51	51
Lags	6	17	7	12	10	17	12	18
FEs	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.552	0.381	0.476	0.243	0.295	0.332	0.309	0.342
Overall	0.904	0.799	0.863	0.445	0.290	0.315	0.305	0.329

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

Some differences are observed in table 4. Several coefficients drop from the regression, whilst others are added. A discernible change is tier 3a lag 2, which had substantial effects across subgroups for both unemployment and the growth rate, but are now void. Nonetheless, these changes are simply the result of minute standard error changes which push the coefficients over the 5% significance threshold. The estimators of these effects are unchanged. The greater part of the effects are as before.

Contrary to Hagedorn et al. (2013) where no additional control variables are significant, SNAP *is* significant in explaining unemployment growth for the male and total unemployment regressions. The results reflect the omitted variables bias as outlined above: SNAP has a positive coefficient, echoing wage and search effects. Consequently, the aggregate EUC08 unemployment effect for males is overestimated by 0.7% in the benchmark regression for men. The aggregate effect on total unemployment, however, shows a minor discrepancy, falling within the benchmark standard error. Moreover, the gender difference variable's effect is not robust to inclusion of the additional control variables. It is no longer significantly different from zero, indicating the EUC08 might, after all, be gender indiscriminate. This would weaken the position of UI extensions in the insurance-incentive debate, because it suggests they do not help the most perturbed subgroups as much as they should based on asymmetric recessionary unemployment impacts. Nonetheless, males were not the sole group affected by the recession. The significant effects across regressions still suggest EUC08 provides widespread unemployment insurance, simply not proportionate to recessionary effects.

The remaining regressions' aggregate effects are minimally overestimated in the benchmark, with the exception of the gender difference in unemployment growth. The gender difference variable shows males enjoy a 0.5% smaller decrease than females, compared to the baseline's 0.1%. This gives stronger support for the persistence of male cyclical unemployment. Furthermore, across regressions, there are still few significant instantaneous effects, as in the baseline. The dominance of delayed effects persists.

9.2 TEUC Trends

Since our sample period starts six months after the TEUC program, there is enough time between TEUC and our sample period to sidestep confounding short-term TEUC *lagged* effects, defined by the five month lag period. Nevertheless, it is uncertain TEUC delayed effects cease after five months. In the baseline models, (1) - (2), possible confounding long-term effects are implicitly assumed to be captured by month FEs and the interaction between state FEs and the national unemployment rate. This is questionable because TEUC was state-differentially administered according to monthly trigger criteria. Hence, TEUC may also have long-term effects that evolve differentially over time per state, influencing unemployment rates in our pre-treatment period differentially across states, and weakening the common trends assumption. If ignored these long-term effects can bias estimators. Including a state-specific linear time trend remedies this problem by relaxing the common trends assumption, which allows treatment and control states to have diverging trends in a restricted way. Table 5 presents a robustness check with state-specific linear time trends for the per-state average TEUC weeks compensated.

Table 5: TEUC Trends

	Dependent variable:							
	Unemployment Rate				**Unemployment Growth Rate**			
Tier/Lag	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/0	-0.003	-0.002	-0.004	-0.003	-0.003	-0.002	-0.004	-0.003
	(0.002)	(0.001)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)
1	0.003(*)	0.002	0.004*	0.002	0.002**	0.001	0.004	0.003
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.004)
2	-0.000	0.002	0.000	-0.002	0.000	0.002	0.000	-0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
3	-0.002	-0.003	-0.003	-0.000	-0.002	-0.003	-0.003*	-0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)
4	0.001	0.001	0.001	0.000	0.002	0.001	0.001	0.001
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)
5	-0.002	-0.000	-0.003	-0.003	-0.001	-0.000	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)
2c/0	0.004*	0.007	0.002	-0.005*	0.003*	0.007	0.001	-0.007(*)
	(0.001)	(0.004)	(0.002)	(0.002)	(0.001)	(0.004)	(0.003)	(0.003)
1	-0.003	-0.001	-0.002	-0.002	-0.003	-0.001	-0.002	-0.001
	(0.002)	(0.004)	(0.003)	(0.005)	(0.003)	(0.004)	(0.003)	(0.007)
2	-0.000	0.001	0.002	0.001	-0.000	0.001	0.002	0.001
	(0.001)	(0.003)	(0.001)	(0.004)	(0.001)	(0.003)	(0.002)	(0.003)
3	0.001	-0.001	0.001	0.002	0.001	-0.001	0.001	0.002

	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
4	-0.001	0.001	-0.002	-0.003	-0.002	0.000	-0.003(*)	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.003)
5	0.001	-0.001	0.003	0.004	0.000	-0.002	0.001	0.004
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
3a/0	-0.001	-0.003	-0.001	0.002	-0.002	-0.003	-0.002	0.001
	(0.002)	(0.005)	(0.002)	(0.003)	(0.001)	(0.005)	(0.001)	(0.003)
1	-0.001	0.003**	-0.001	-0.003**	-0.001	0.003*	-0.002	-0.004
	(0.003)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
2	0.003	0.005***	0.001	-0.004*	0.002**	0.005**	0.000	-0.006(**)
	(0.002)	(0.001)	(0.004)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
3	0.001	-0.001	0.004*	0.005	0.000	-0.003	0.003	0.005*
	(0.001)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)
4	-0.003	-0.003*	-0.007*	-0.004*	-0.004*	-0.004**	-0.007***	-0.003(*)
	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)
5	-0.001	-0.001	-0.002	-0.001	-0.001	-0.002	-0.002	-0.001
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
3b/0	-0.004	0.001	-0.006	-0.006	-0.005	-0.001	-0.007	-0.005*
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)	(0.002)
1	0.000	0.000	-0.001	-0.001	-0.001	-0.002	-0.001	0.000
	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)	(0.003)	(0.003)	(0.004)
2	0.002	-0.001	0.005	0.006	0.001	-0.001	0.004	0.005
	(0.003)	(0.004)	(0.003)	(0.003)	(0.002)	(0.004)	(0.004)	(0.003)
3	0.002	-0.000	0.003	0.003	0.002	0.0001	0.002	0.003
	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.005)
4	-0.005**	-0.000	-0.008*	-0.008*	-0.006***	-0.001	-0.009***	-0.008*
	(0.001)	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
5	0.002	0.001	0.005	0.004	0.003	0.000	0.005	0.004
	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
3c/0	-0.001	0.002	-0.001	-0.003	-0.002	0.002	-0.002	-0.004(*)
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
1	0.001	-0.001	-0.000	0.001	0.000	-0.002	-0.001	0.001
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)
2	-0.002	-0.003	-0.002	0.001	-0.003**	-0.004	-0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.004)
3	0.001	0.001	0.001	-0.001	0.001	0.001	0.001	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
4	0.000	0.001	-0.001	-0.002	-0.000	0.001	-0.001	-0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)
5	0.001	0.000	0.002	0.002	0.000	-0.001	0.001	0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)
4a/0	-0.002	-0.002	-0.003	-0.000	-0.003	-0.004	-0.003	0.002
	(0.003)	(0.003)	(0.002)	(0.005)	(0.002)	(0.003)	(0.003)	(0.003)
1	0.002	0.006***	-0.001	-0.006	0.002	0.005***	-0.001	-0.006
	(0.002)	(0.001)	(0.003)	(0.004)	(0.001)	(0.001)	(0.004)	(0.003)
2	-0.001	-0.006***	0.002	0.008*	-0.001	-0.006***	0.002	0.008**
	(0.002)	(0.001)	(0.003)	(0.004)	(0.001)	(0.001)	(0.003)	(0.003)
3	-0.002	0.002	-0.005	-0.007	-0.002	0.002	-0.005	-0.007
	(0.002)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)	(0.006)
4	0.001	0.001	-0.001	-0.002	0.001	0.000	0.000	-0.001
	(0.001)	(0.003)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)
5	-0.001	0.001	0.000	-0.001	-0.001	-0.001	-0.000	-0.002
	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
4b/0	0.001	-0.001	0.002*	0.003	0.001	0.000	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.004)	(0.001)	(0.001)	(0.002)	(0.003)
1	-0.000	-0.003	0.002	0.005	-0.000	-0.002	0.002	0.004

	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
2	0.001	0.002*	-0.000	-0.002	0.001	0.003(*)	-0.000	-0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
3	0.002	0.001	0.003	0.003	0.002	0.001	0.003*	0.003
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)
4	-0.001	-0.002	-0.001	0.001	-0.002	-0.002	-0.002	0.001
	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
5	-0.000	-0.002	-0.000	0.002	-0.000	-0.001	-0.001	0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
4c/0	0.001	-0.004	0.005	0.009*	-0.001	-0.007**	0.002	0.009
	(0.003)	(0.002)	(0.003)	(0.005)	(0.002)	(0.003)	(0.002)	(0.005)
1	-0.003	-0.001	-0.004	-0.003	-0.005**	-0.002	-0.005	-0.003
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.002)
2	0.009***	0.011***	0.005	-0.005	0.008**	0.010**	0.004	-0.006
	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)	(0.005)
3	-0.003**	-0.005*	0.000	0.005	-0.004***	-0.006*	-0.001	0.004
	(0.001)	(0.003)	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.004)
4	-0.001	-0.000	-0.002	-0.002	-0.002	-0.001	-0.003	-0.002
	(0.003)	(0.005)	(0.003)	(0.003)	(0.004)	(0.005)	(0.005)	(0.004)
5	0.005**	0.003	0.004	0.002	0.004**	0.002	0.002	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.004)	(0.004)
4d/0	0.002	0.002	-0.001	-0.004	0.002	0.003	-0.001	-0.004(**)
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.001)
1	-0.001	-0.001	0.002	0.002	-0.001	-0.001	0.002	0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)
2	0.006***	0.007(**)	0.003(*)	-0.003	0.006***	0.007(**)	0.003	-0.003
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
3	-0.004**	-0.005	-0.003	0.002	-0.005*	-0.005	-0.003	0.001
	(0.001)	(0.003)	(0.003)	(0.005)	(0.002)	(0.003)	(0.003)	(0.004)
4	-0.004*	-0.005	-0.003	0.002	-0.004	-0.004	-0.003	0.001
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)
5	0.005**	0.007(**)	0.002	-0.006(**)	0.004	0.007(**)	0.001	-0.007
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
Nonlinear Wald Test								
Aggregate	0.013***	0.019***	0.000	-0.003***	0.004	0.003	-0.016***	0.008**
effect	(0.003)	(0.004)	(0.000)	(0.001)	(0.004)	(0.004)	(0.003)	(0.003)
N	7,548	7,242	7,548	7,344	7,395	7,242	7,242	6,783
Months	153	147	153	149	150	147	147	138
States	51	51	51	51	51	51	51	51
Lags	7	13	7	11	10	13	13	22
FEs	Y	Y	Y	Y	Y	Y	Y	Y
TEUC	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.580	0.402	0.501	0.251	0.297	0.322	0.311	0.344
Overall	0.904	0.799	0.863	0.447	0.291	0.311	0.303	0.332

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

Several states' TEUC trends are statistically significant, visible from the higher R^2 across subgroup regressions. Nonetheless, the regressions yield no substantial differences versus the baseline, as these trend effects are modest in magnitude at best. Few lagged effects shift from significance to marginal insignificance and vice versa. Overall, lagged effects still dominate EUC08 effects, with only the contemporary effect of tier 4c for the female unemployment growth recession showing significance. Hence, the baseline model's delayed effects conclusion is robust.

The aggregate unemployment effect for males shifts from −0.8% in the baseline, and 1.5% in the additional control variables regression, to insignificance. This is an economically significant difference, suggesting males may have seen no substantial decrease in unemployment after all. It also echoes in the gender difference variable, which shows the gender unemployment gap saw a 0.3% decline because of EUC08, versus 0.8% in the baseline. Yet, this effect is more substantial than the absence of a gender differential effect predicted by the second robustness check. Moreover, the magnitude of the gender difference in unemployment growth, rises to 0.8% compared to 0.1% and 0.5% in the baseline and additional control variables regressions. This furthers our argument of the persistence of male cyclical unemployment.

9.3 Excluding Additional Benefits States

Finally, we exclude the states with state-level AB trigger criteria that *do not* discriminate between individuals based on person-specific characteristics. The states are California, Connecticut and the District of Columbia. The states' AB vary at the same level as treatment (state-month) and so may confound causal interpretation. Table 6 shows AB-state excluded results.

Table 6: Excluding AB States

	Dependent variable:							
	Unemployment Rate				Unemployment Growth Rate			
Tier/Lag	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/0	-0.003	-0.002	-0.004	-0.003	-0.003	-0.002	-0.004	-0.002
	(0.002)	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
1	0.003(**)	0.001	0.004**	0.003	0.003*	0.000	0.004	0.003(*)
	(0.001)	(0.002)	(0.001)	(0.004)	(0.001)	(0.002)	(0.003)	(0.002)
2	0.000	0.002	0.000	-0.002	0.001	0.002	0.001	-0.001
	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)	(0.003)
3	-0.002	-0.003	-0.003	-0.000	-0.002	-0.003	-0.003	0.000
	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)
4	0.002	0.001	0.002	0.001	0.002*	0.001	0.002	0.001
	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)
5	-0.001	0.001	-0.002	-0.003	-0.001	0.001	-0.001	-0.002
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.003)
2c/0	0.004**	0.007	0.001	-0.005**	0.003	0.006	0.000	-0.006(**)
	(0.001)	(0.004)	(0.002)	(0.001)	(0.002)	(0.004)	(0.003)	(0.002)
1	-0.002	-0.001	-0.002	-0.002	-0.003	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(0.006)
2	-0.000	0.001	0.002	0.001	-0.000	0.001	0.002	0.001
	(0.001)	(0.004)	(0.001)	(0.003)	(0.002)	(0.004)	(0.001)	(0.003)
3	0.001	-0.001	0.001	0.002	0.001	-0.001	0.001	0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.002)
4	-0.002	0.000	-0.003	-0.003	-0.002	-0.000	-0.003	-0.003

	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
5	0.001	-0.001	0.003	0.004**	0.001	-0.002	0.001	0.004
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.003)
3a/0	-0.002	-0.003	-0.001	0.002	-0.002	-0.003	-0.002	0.001
	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.001)	(0.005)
1	-0.001	0.003**	-0.001	-0.004	-0.001	0.003*	-0.002	-0.004
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)
2	0.003**	0.005**	0.001	-0.004	0.002	0.005**	0.000	-0.005
	(0.001)	(0.001)	(0.002)	(0.004)	(0.001)	(0.001)	(0.002)	(0.004)
3	0.001	-0.001	0.004*	0.005	0.000	-0.003	0.003	0.005*
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
4	-0.003*	-0.003***	-0.006	-0.004	-0.004	-0.004***	-0.007	-0.004
	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.004)
5	-0.000	-0.001	-0.002	-0.001	-0.001	-0.002	-0.002	-0.001
	(0.001)	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
3b/0	-0.004	0.000	-0.007*	-0.007*	-0.005**	-0.001	-0.008*	-0.006
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)	(0.005)
1	-0.000	0.000	-0.001	-0.001	-0.001	-0.000	-0.001	-0.000
	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.005)
2	0.002	-0.001	0.005	0.006	0.001	-0.002	0.004	0.005
	(0.001)	(0.003)	(0.003)	(0.006)	(0.001)	(0.003)	(0.005)	(0.003)
3	0.002	-0.001	0.003	0.004	0.002	-0.001	0.003	0.004
	(0.001)	(0.003)	(0.002)	(0.004)	(0.001)	(0.003)	(0.003)	(0.002)
4	-0.006*	-0.001	-0.009**	-0.007**	-0.006***	-0.002	-0.009	-0.008***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)
5	0.003	0.001	0.005	0.004	0.004	0.000	0.006	0.004
	(0.002)	(0.003)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.003)
3c/0	-0.001	0.002	-0.001	-0.003	-0.001	0.002	-0.002	-0.004*
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
1	0.001	-0.001	-0.001	-0.000	0.000	-0.001	-0.001	-0.000
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.001)	(0.004)
2	-0.002*	-0.003	-0.002	0.001	-0.003*	-0.004	-0.002	0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
3	0.001	0.002	0.001	-0.001	0.001	0.002	0.002	-0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
4	0.000	0.001	-0.001	-0.002	-0.000	0.001	-0.001	-0.002
	(0.000)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.003)
5	0.001	-0.000	0.002	0.002	0.000	-0.001	0.001	0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)
4a/0	-0.003	-0.003	-0.003	0.001	-0.003	-0.004	-0.002	0.003
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.005)
1	0.002	0.006**	-0.001	-0.006	0.002*	0.005**	-0.000	-0.005
	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)	(0.002)	(0.004)	(0.004)
2	-0.001	-0.006***	0.002	0.008**	-0.001	-0.006***	0.002	0.008**
	(0.002)	(0.001)	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)
3	-0.002	0.002	-0.004	-0.006	-0.002	0.001	-0.005	-0.006
	(0.002)	(0.002)	(0.004)	(0.005)	(0.001)	(0.002)	(0.004)	(0.003)
4	0.001	0.001	-0.001	-0.002	0.001	0.000	-0.001	-0.002
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.001)
5	-0.000	0.001	0.001	-0.001	-0.001	-0.001	0.000	-0.001
	(0.001)	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.003)	(0.003)
4b/0	0.001	-0.001	0.001	0.002	0.001	0.000	0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)
1	0.000	-0.003	0.002	0.005	-0.000	-0.002	0.002	0.004*
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
2	0.001	0.002(*)	-0.000	-0.003	0.001	0.003(*)	-0.001	-0.003

	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
3	0.002	0.001	0.003	0.002	0.002	0.001	0.003	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
4	-0.002	-0.002	-0.001	0.001	-0.002	-0.002	-0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
5	-0.001	-0.002	-0.001	0.001	-0.001	-0.001	-0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
4c/0	0.001	-0.005	0.005*	0.009*	-0.001	-0.007**	0.003*	0.010*
	(0.002)	(0.003)	(0.002)	(0.005)	(0.001)	(0.003)	(0.001)	(0.004)
1	-0.003	-0.001	-0.004	-0.003	-0.005**	-0.003	-0.005	-0.003
	(0.002)	(0.002)	(0.004)	(0.005)	(0.001)	(0.002)	(0.003)	(0.005)
2	0.009	0.011***	0.006	-0.006	0.009***	0.011***	0.005	-0.006
	(0.002)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.004)	(0.006)
3	-0.004**	-0.005**	-0.001	0.005	-0.004**	-0.005**	0.000	0.005
	(0.001)	(0.002)	(0.003)	(0.004)	(0.001)	(0.002)	(0.003)	(0.003)
4	-0.001	-0.000	-0.002	-0.001	-0.002	-0.001	-0.002	-0.002
	(0.004)	(0.005)	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)	(0.002)
5	0.004*	0.003*	0.003	0.000	0.004*	0.002	0.002	-0.001
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.004)	(0.004)
4d/0	0.003	0.004	-0.001	-0.005	0.003	0.005	-0.000	-0.005
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)	(0.005)
1	-0.001	-0.001	0.003	0.004	-0.001	-0.001	0.003	0.004
	(0.001)	(0.002)	(0.002)	(0.004)	(0.001)	(0.002)	(0.002)	(0.003)
2	0.007**	0.007(**)	0.005(*)	-0.002	0.007**	0.007(**)	0.004	-0.003
	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)
3	-0.006**	-0.005	-0.006	-0.001	-0.006***	-0.005	-0.006	-0.001
	(0.002)	(0.003)	(0.003)	(0.004)	(0.001)	(0.003)	(0.004)	(0.005)
4	-0.003	-0.005	-0.003	0.002	-0.003	-0.005	-0.002	0.002
	(0.003)	(0.004)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)	(0.005)
5	0.005**	0.009(***)	0.003	-0.006(*)	0.005*	0.008(**)	0.002	-0.006(**)
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)
Nonlinear Wald Test								
Aggregate	0.018***	0.011**	-0.005*	-0.007	-0.010**	-0.001	-0.016***	-0.000
effect	(0.004)	(0.005)	(0.003)	(0.006)	(0.005)	(0.006)	(0.003)	(0.004)
N	7,104	6,816	7,056	6,960	6,960	6,816	6,720	6,528
Months	153	147	153	150	150	147	145	141
States	51	51	51	51	51	51	51	51
Lags	7	13	7	10	10	13	15	19
FEs	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.562	0.380	0.483	0.238	0.297	0.319	0.312	0.336
Overall	0.901	0.785	0.859	0.438	0.290	0.308	0.302	0.326

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

This last sensitivity analysis asserts the largest divergence from the baseline. Again, several coefficients are pushed into (in)significance due to small SE changes. However, in this robustness check these changes are more numerous than in previous checks. The changes consist of both positive and negative ones, exhibiting the competing wage, search, separation and entitlement effects of the AB programs. As opposed to the other checks, there are also divergences in estimators with significance robust to AB state exclusion.

Yet, these all fall within one SE of their respective baseline estimators. For instance, tier 4c sees two-month lagged effects for total and female unemployment growth (columns 5 and 6) rise by 0.1% compared to the baseline, while respective baseline estimators' SEs are 0.2% and 0.3%. The previously unseen significance of aggregate EUC08 effects on total unemployment growth displays a relatively stronger EUC08 entitlement effect versus contesting search, wage and separation effects, once AB are excluded.

Additionally, more instantaneous effects are observed. For example, tier 2c is significant in explaining total unemployment and the gender differential effect between men and women, as well as tier 3b for total unemployment growth and 4c for female unemployment growth. Notwithstanding, the majority of EUC08 treatment effects occur with a delay of two to five months after tier activation. The absolute value of the delayed effects, consequently, also outweighs the contemporaneous effects, as in the benchmark.

In benchmark table 3, EUC08 results in an overall decrease in male unemployment of 0.8%. Excluding the AB states, however, this number falls to 0.5% and the strength of this effect diminishes to marginal significance. Subsequently, the gender differential unemployment effect attenuates to −0.7%, from −0.8% in the baseline, and becomes insignificant. Furthermore, the aggregate gender difference effect for unemployment growth (column 8) becomes insignificant, even though aggregate female and male unemployment growth is unaltered compared to the other model specifications. Hence, it is possible EUC08 did not necessarily reduce the gender unemployment gap, even though men were more severely affected by the recession. This challenges EUC08 as an effective (automatic) stabiliser somewhat, but does not challenge the effectiveness of UI extensions as automatic stabilisers *overall*: AB are also UI extensions, albeit state-funded ones.

10 Conclusion

This thesis had two goals. It inquired after EUC08 gender heterogeneous unemployment effects, to analyse if the UI extension policy relieved the disproportionate, negative impact of the Great Recession on male unemployment. Furthermore, it intended to uncover the pattern of EUC08 effects over time, establishing whether UI extensions produce (larger) effects with delay, and if so, approximately how long the lag period is.

This study finds EUC08 produced a gender heterogeneous unemployment effect between 0% and 0.8%. The latter figure indicates men see a 0.8% larger (smaller) decrease (increase) in unemployment rates than women because of the UI extensions. Previous research portrayed men as the subgroup most severely affected by the Great Recession. This is partly, due to their pro-cyclical occupation preference, and partly, due to women's increasing labour force participation and employability during recessions. EUC08, as a corrective program, intended to re-stabilise the economy and relieve unemployment stress on individuals. Although small, 0.8% suggests UI extensions rectified the unequal recessionary employment effects across genders. The per sex regressions also support the presence of a gender differential EUC08 effect, with female unemployment increasing 0.6% - 1.9%, and male unemployment rates decreasing by 0% - 1.5%. However, given the 0% lower bound for the gender differential effect, the absence of a significant gender heterogeneous EUC08 effect cannot be ruled out. Although the lower bound challenges EUC08's effectiveness as a short-term unemployment stabiliser, the significant EUC08 effects across subgroups demonstrates the program inarguably reduced short-term financial and psychological strain on the unemployed. Both this finding and the modest 0.8% upper bound suggest UI extensions provide insurance against unemployment at a relatively small moral hazard, or 'incentive' cost in the insurance-incentive debate, particularly during recessions. Hence, our conclusions vie for UI extensions provision.

Finally, we conclude the majority of EUC08's impact on unemployment takes two to four months to effectuate across genders and EUC08 tiers. The prominence of lagged over instantaneous treatment effects results in the size of delayed effects overshadowing the contemporary effects by a large margin. As such, significant contemporary effects are nullified by later tiers' delayed effects. This finding is valuable to policy planning, as policymakers intending to manipulate unemployment figures during recessions should take into account the lag period UI extensions take to produce results. Consequently, economic recovery can be accelerated if UI extensions are triggered by forecasted unemployment figures approximately one quarter forwards, instead of by historical unemployment rates.

11 Discussion

A drawback of this study is that it is conducted on macrodata. Doing so precludes controlling for the interactive effects of gender and other factors, like education. Moreover, it prevents detecting differential EUC08 effects between within-gender subgroups more upset by the Great Recession than the male aggregate. These include single mothers and Afro-American men. Further avenues for heterogeneous UI extensions research could involve ethnicity and socioeconomic class differential effects. It is also of interest to study (unintentional) increases in income and consumption inequality across subgroups caused by UI extensions as suggested by De la Rica and Rebollo-Sanz (2017).

The robust pretrends for the first and last EUC08 tiers and tier 4b are another limitation to this study. Significant pretrends mean those tiers' estimators are not causally interpretable. It follows that if these tiers have significant unemployment effects, the true aggregate EUC08 effects might be stronger or weaker than the effects found in this thesis. As this study pioneered the evaluation of gender heterogeneous UI extension effects, we hope the knowledge of significant anticipation can guide future studies into taking necessary precautions against anticipatory changes in stakeholders' job search and creation behaviour. For future reference, we recommend (semi)parametric difference-in-difference models (e.g. Abadie, 2005). These allow violation of the common trends assumption behind regular difference-in-difference models, such that causal treatment effects can be confidently captured for tiers with strong anticipation.

As this study focused on EUC08 and the Great Recession, we encourage study of other UI extensions programs' lagged and gender heterogeneous effects, to embolden or dispute the external validity and significance of our results. In a similar vein, the extent to which this finding holds in other economies might command inspection, particularly in countries with more rigid labour market institutions that intensify delay. Furthermore, for policy purposes, it could be useful to examine if delayed effects exist for policies that alter other dimensions of benefits generosity, like benefits levels. Finally, unemployment is only one dimension through which UI extensions act as automatic stabilisers. As Bachmann et al. (2015) state: an "analysis of alternative adjustment mechanisms, such as wage and working hours adjustments, is clearly warranted" to permit a more wholesome empiric evaluation of the insurance-incentive trade-off.

12 Appendices

12.1 Appendix 1: Wage-Labour Market Tightness Relation

Expected profits from a posted vacancy (Π_v) can be rearranged in terms of Π_e:

$$\rho\Pi_v = -\kappa + m(\theta)[\Pi_e - \Pi_v] \Leftrightarrow \Pi_e = \frac{1}{m(\theta)}[\rho\Pi_v + \kappa + m(\theta)\Pi_v].$$

Similarly, profits from a filled job (Π_e) can be rearranged:

$$\rho\Pi_e = q - w + \delta[\Pi_v - \Pi_e] \Leftrightarrow \Pi_e = \frac{1}{\rho + \delta}[q - w + \delta\Pi_v].$$

Equating the above expressions for Π_e and imputing the free product market entry condition ($\Pi_v = 0$), we can obtain the wage (w) expression:

$$\frac{1}{m(\theta)}[\rho \cdot 0 + \kappa + m(\theta) \cdot 0] = \frac{1}{\rho + \delta}[q - w + \delta \cdot 0]$$

$$\Leftrightarrow \frac{q - w}{\rho + \delta} = \frac{\kappa}{m(\theta)}$$

$$\Leftrightarrow w = q - \frac{\kappa}{m(\theta)}[\rho + \delta].$$

We can take the derivative of the final expression for wages with respect to labour market tightness:

$$\frac{dw}{d\theta} = -\frac{0 \cdot m(\theta) - m'(\theta) \cdot \kappa(\rho + \delta)}{[m(\theta)]^2}$$

$$= \frac{m'(\theta) \cdot \kappa(\rho + \delta)}{[m(\theta)]^2} < 0.$$

Remembering the vacancy filling rate ($m(\theta)$) is a decreasing function of labour market tightness (θ) so $m'(\theta) < 0$, we can see that a negative relationship between wages and labour market tightness exists.

12.2 Appendix 2: Short-Run ARDL Error Correction Model

First, for demonstrative purposes we derive the ECM of a stylised long-run single time series ARDL model for the unemployment rate U_t:

$$U_t = \delta + \varrho U_{t-1} + \delta_0 TIER_t + \delta_1 TIER_{t-1} + \varepsilon_t, \tag{a}$$

where $TIER_t$ is an explanatory variable, here denoting the EUC08. If U_t and $TIER_t$ have a long-run cointegrated relationship, $U_t = U_{t-1} = U$, $TIER_t = TIER_{t-1} = TIER$, and $\varepsilon_t = 0$ such that the variables' long-run cointegrated relationship is described by:

$$(1-\varrho)U = \delta + (\delta_0 + \delta_1)TIER$$

$$\Leftrightarrow U = \frac{\delta}{1-\varrho} + \frac{\delta_0 + \delta_1}{1-\varrho}TIER, \tag{b}$$

$$= \beta_1 + \beta_2 TIER.$$

To derive the short-run ECM, the long-run ARDL model (a) is manipulated as follows. First, subtracting the lagged dependent variable from both sides:

$$\Delta U_t = \delta + (\varrho - 1)U_{t-1} + \delta_0 TIER_t + \delta_1 TIER_{t-1} + \varepsilon_t.$$

Adding and subtracting $\delta_0 TIER_{t-1}$ yields:

$$\Delta U_t = [\delta + (\varrho - 1)U_{t-1} + (\delta_1 + \delta_0)TIER_{t-1}] + \delta_0(TIER_t - TIER_{t-1}) + \varepsilon_t.$$

Factorising $\varrho - 1$ out of the part in square brackets:

$$\Delta U_t = (\varrho - 1)[\frac{\delta}{\varrho - 1} + U_{t-1} + \frac{(\delta_0 + \delta_1)}{\varrho - 1}TIER_{t-1}] + \delta_0 \Delta TIER_t + \varepsilon_t.$$

Proceeding with rearranging the part in square brackets gives the short-run ECM:

$$\Delta U_t = (1-\varrho)[\frac{\delta}{1-\varrho} - U_{t-1} + \frac{(\delta_0 + \delta_1)}{1-\varrho}TIER_{t-1}] + \delta_0 \Delta TIER_t + \varepsilon_t,$$

$$= -(1-\varrho)[U_{t-1} - \frac{\delta}{1-\varrho} - \frac{(\delta_0 + \delta_1)}{1-\varrho}TIER_{t-1}] + \delta_0 \Delta TIER_t + \varepsilon_t,$$

$$= -\alpha[U_{t-1} - \beta_1 - \beta_2 TIER_{t-1}] + \delta_0 \Delta TIER_t + \varepsilon_t,$$

The part in square brackets is the long-run cointegrated relationship between the dependent and independent variable, (b). Notably, the ECM embeds (b) in (a), the ARDL framework. The term in square brackets and its coefficient, $-\alpha$, is also referred to as the error correction term, as the bracketed section captures the error of U_{t-1} from its long-run value, $\beta_1 + \beta_2 TIER_{t-1}$, whilst the coefficient 'corrects' this error (Hill, Griffiths, & Lim, 2012). If the errors of the bracketed section are stationary, the variables are cointegrated and the long-run ARDL model can be correctly estimated without it being a spurious regression. This is tested for in appendix section 12.3.

To write the dynamic panel ECM corresponding to long-run ARDL equation (1) in section 5, we rewrite the long-run ARDL in terms of the unemployment lags, the fixed effects and a vector containing all explanatory variables, including the lagged EUC08 variables, $\mathbf{Z}_{st}$:

$$U_{st} = \gamma_t + \beta_s + \beta_s \mathbf{X}_t + \sum_{k=1}^{x} \varrho_k U_{st-k} + \sum_{j=-5}^{q=5} \delta_j' \, \mathbf{Z}_{st+j} + \varepsilon_{st}.$$

Here δ_j' is a vector containing explanatory variable coefficients for lag j of each explanatory variable. The maximum number of lags and leads is five, as in equation (1). For the control variables like EB, however, $j = q = 0$. The corresponding ECM can then be expressed as:

$$U_{st} = \gamma_t + \beta_s + \beta_s \mathbf{X}_t + \alpha U_{st-1} + \vartheta' \, \mathbf{Z}_{st} + \sum_{k=1}^{x} \varrho_k^* U_{st-k} + \sum_{j=-5}^{q=5} \delta_j^{*'} \, \mathbf{Z}_{st+j} + \varepsilon_{st}. \quad \text{(c)}$$

Where α captures the short-run deviation or error from the long-run model. Moreover,

$$\varrho_k^* = - \sum_{i=k+1}^{x} \varrho_k,$$

$$\delta_j^{*'} = - \sum_{l=j+1}^{q=5} \delta_j'.$$

The long-run multiplier for the j^{th} lag of, for instance, $TIER3$c is given by $-\vartheta_j/\alpha$. The ARDL and ECM corresponding to equation (2) can be similarly expressed by changing U_{st} to U_{st}^M, U_{st}^F or $U_{st}^M - U_{st}^F$ for males, females, or the gender difference variable, respectively. It must be noted that this reparametrization's (c) coefficients are already averaged over states, wherefore the dependent and independent variable coefficients have no subscript s.

12.3 Appendix 3: Tables Pre-Estimation Tests

Table 7 presents serial correlation test results for each model, respective to tables 3 - 6 in sections 8 and 9.

Table 7: Serial Correlation Test *p*-values

	Dependent variable:							
	Unemployment Rate				**Unemployment Growth Rate**			
Test	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
	Table 3							
W	0.791	0.891	0.647	0.857	0.850	0.863	0.760	0.932
BG	0.126	0.016	0.258	0.094	0.082	0.025	0.103	0.020
Lags	7	13	7	11	10	13	13	21
	Table 4							
W	0.842	0.913	0.622	0.909	0.885	0.942	0.966	0.969
BG	0.222	0.030	0.460	0.019	0.0120	0.019	0.010	0.015
Lags	6	17	7	12	10	17	12	18
	Table 5							
W	0.830	0.983	0.718	0.943	0.882	0.894	0.808	0.880
BG	0.089	0.003	0.117	0.013	0.055	0.017	0.050	0.022
Lags	7	13	7	11	10	13	13	22
	Table 6							
W	0.810	0.873	0.660	0.849	0.892	0.859	0.792	0.894
BG	0.240	0.020	0.276	0.013	0.044	0.017	0.032	0.013
Lags	7	13	7	10	10	13	15	19

All numbers rounded to three decimal places

BG denotes the Breusch-Godfrey Lagrange multiplier test for AR(1) serial correlation in the regression residuals, adjusted for panels. When the null hypothesis of no serial correlation holds, the test statistic has an asymptotic approximate χ^2 distribution (Breusch, 1978; Godfrey, 1978; Wooldridge, 2010). *W* represents the Wooldridge test for AR(1) serial correlation in panel models, with a null of no serial correlation. For the *W* test serial correlation is eliminated at all significance levels. For the *BG* test, lags are included until serial correlation is removed for the minimum 1% significance threshold. The only exception is the female regression for the TEUC regression (table 5), which achieved its minimum *BG* p-value of 0.003 at 13 lags. The *W* p-value for this specification is, however, almost unity at 0.983, indicating confidence in no serial correlation.

Table 8 presents cointegration test results for models with level unemployment rates in tables 3 - 6 in sections 8 and 9, respectively. *IPS* denotes the Im-Pesaran-Shin (Im et al., 2003) bounds test for cointegration in panel models. It is called a bounds test as it tests whether long-run ARDL regression residuals are stationary in levels, so with an I(0) order of integration (stationary), or at an I(1) level (in which case variables are non-stationary of order one but model residuals are stationary due to cointegration). It does this by producing critical values of the standard normal distributed test statistic, when model residuals are I(0) and when they are I(1), both with a null hypothesis of nonstationary errors. If the test statistics fall above the I(1) value, residuals are cointegrated at an I(1) level, and are stationary. If statistics fall below the I(0) values, residuals are not cointegrated because all variables are I(0), stationary at levels, so they their ARDL model gives non-spurious results in any case. If statistics fall between the I(0) and I(1) critical values, variables are fractionally cointegrated and additional tests must be conducted.

All model residuals in this thesis reject the null of nonstationary errors, showing strong cointegrated relationships between included variables at an I(1) level. Hence, errors of the level unemployment models are stationary and models (1) and (2) present non-spurious relationships.

Table 8: Cointegration Test Statistics

	Dependent variable:			
	Unemployment Rate			
Test	Total (1)	Female (2)	Male (3)	Difference (4)
		Table 3		
IPS	-95.188***	-86.112***	-93.284***	-90.554***
		Table 4		
IPS	-88.624***	-79.715***	-87.000***	-83.747***
		Table 5		
IPS	-89.797***	-84.129***	-90.551***	-89.284***
		Table 6		
IPS	-86.075***	-90.071***	-88.185***	-89.622***

All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

12.4 Appendix 4: Graphs Mean Unemployment over Treatment Period

Figure 6: Mean Female Unemployment over Treatment Period

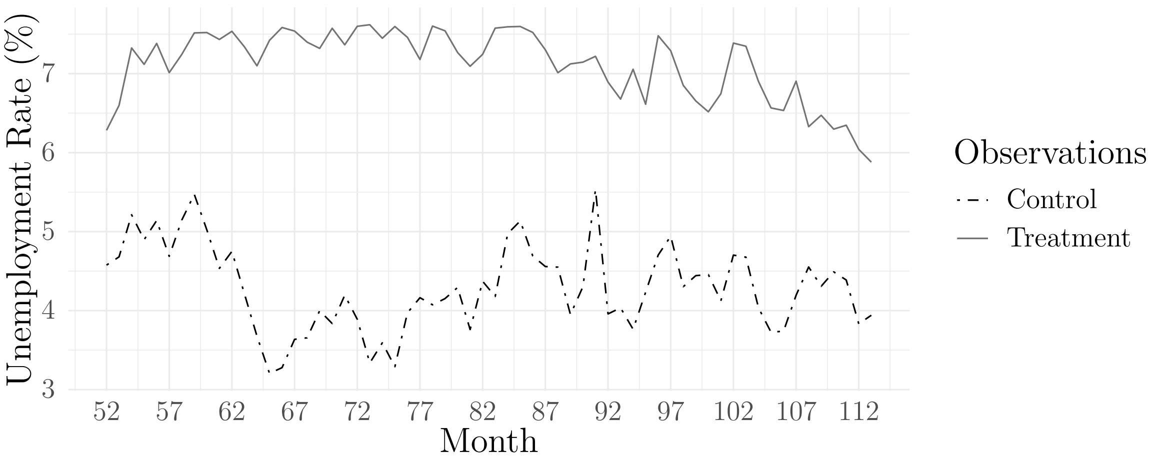
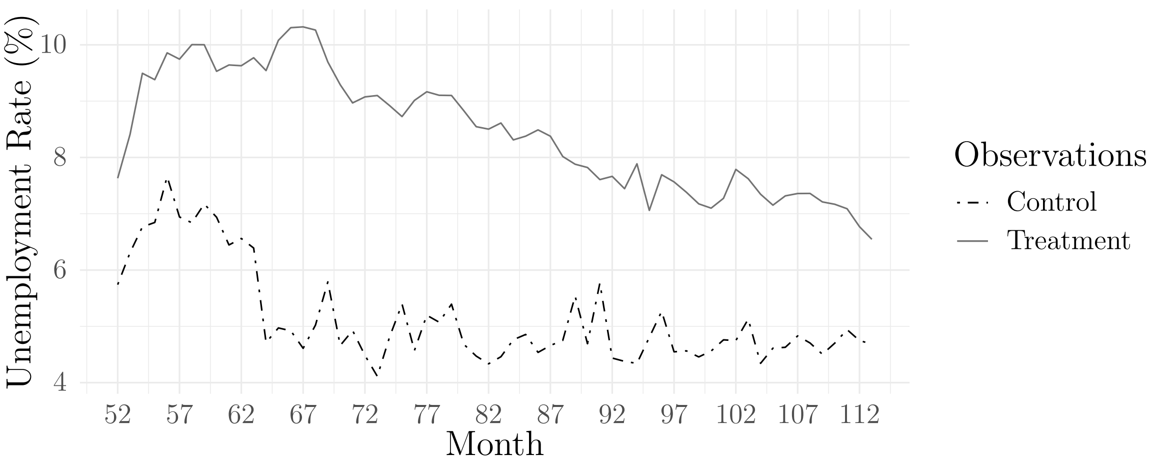


Figure 7: Mean Male Unemployment over Treatment Period



12.5 Appendix 5: Graphs Common Trends

Figure 8: Per State Female Unemployment Rate

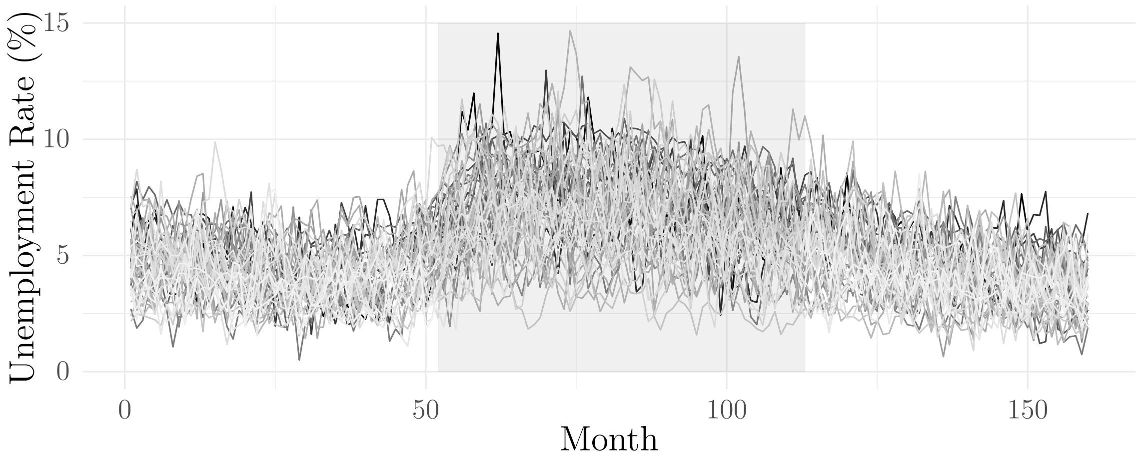
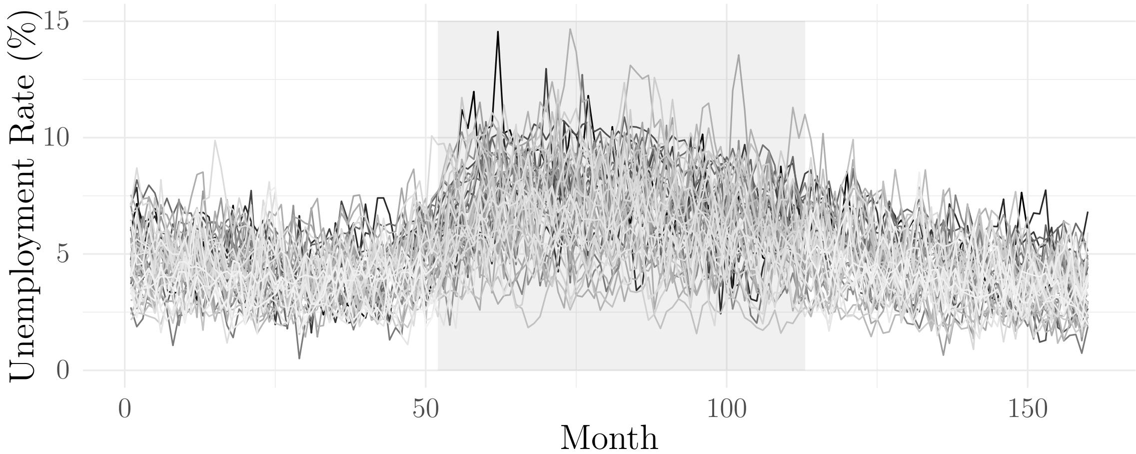


Figure 9: Per State Male Unemployment Rate



12.6 Appendix 6: Anticipatory Effects

Table 9: Leads and EB Table 3

	Dependent variable:							
	Unemployment Rate				**Unemployment Growth Rate**			
Tier/Lead	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/1	0.001	0.001	0.000	-0.001	0.001	0.001	0.001	-0.000*
	(0.001)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(0.002)
2	-0.000	0.002	-0.002	-0.004	-0.001	0.002	-0.002	-0.004
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
3	-0.000	-0.004**	0.002	0.006	-0.000	-0.004**	0.002	0.006**
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)
4	0.001	0.003	0.000	-0.003	0.001	0.003	-0.000	-0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
5	0.002*	0.003	0.001	-0.002	0.002	0.003	0.001	-0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
2c/1	0.001	-0.002	-0.000	0.002	0.001	-0.003	-0.001	0.002
	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.004)
2	-0.001	-0.000	-0.001	-0.001	-0.001	0.000	-0.002	-0.001
	(0.001)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.005)
3	0.003	0.000	0.006	0.006*	0.003	0.001	0.006	0.005*
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)
4	-0.001	0.001	-0.005*	-0.005	-0.001	0.001	-0.005*	-0.006***
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
5	0.001	-0.001	0.003	0.003	0.001	-0.000	0.002**	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
3a/1	-0.001	-0.001	0.001	0.002	-0.001	-0.001	0.001	0.002
	(0.002)	(0.003)	(0.003)	(0.005)	(0.001)	(0.003)	(0.002)	(0.003)
2	-0.001	-0.001	0.000	0.001	-0.001	-0.001	0.001	0.001
	(0.003)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
3	-0.001	-0.001	-0.002	-0.001	-0.001	-0.001	-0.002	-0.001
	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)
4	0.000	-0.001	0.001	0.001	-0.000	-0.000	0.001	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
5	0.002	0.002	-0.001	-0.003*	0.002	0.002	-0.001	-0.003*
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)
3b/1	0.003	0.001	0.007	0.006	0.003	0.000	0.008*	0.008**
	(0.004)	(0.004)	(0.004)	(0.006)	(0.002)	(0.004)	(0.004)	(0.003)
2	-0.002	-0.000	-0.003	-0.002	-0.002	-0.001	-0.003	-0.002
	(0.003)	(0.004)	(0.005)	(0.005)	(0.002)	(0.004)	(0.003)	(0.005)
3	0.001	0.004	-0.000	-0.004	0.001	0.004	-0.001	-0.004
	(0.003)	(0.004)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.005)
4	-0.002	-0.004	-0.002	0.002	-0.002	-0.004	-0.002	0.002
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)
5	0.002	0.004	0.001	-0.004	0.002	0.005	0.000	-0.003
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
3c/1	0.001	-0.003	0.004*	0.006*	0.001	-0.003	0.004**	0.007**
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.002)
2	0.001	0.003	-0.001	-0.004	0.001	0.003	-0.001	-0.003*
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)

3	-0.004	-0.003*	-0.004*	-0.001	-0.004	-0.003**	-0.004	-0.001
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)
4	0.001	-0.001	0.002	0.003	0.000	-0.002	0.002	0.004
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
5	0.002	0.004***	0.001	-0.003	0.002	0.004	0.002	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
4a/1	0.000	0.002	-0.001	-0.003	0.000	0.002	-0.001	-0.002
	(0.002)	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)	(0.003)	(0.004)
2	-0.002	-0.001	-0.002	-0.001	-0.002	-0.002	-0.002	-0.001
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
3	0.000	0.003	-0.002	-0.004	0.000	0.003	-0.001	-0.004
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.004)
4	0.003	0.002	0.004	0.002	0.003	0.002	0.004	0.002
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)
5	0.000	-0.001	0.001	0.003	0.000	-0.002	0.002	0.004*
	(0.002)	(0.003)	(0.002)	(0.004)	(0.001)	(0.003)	(0.001)	(0.002)
4b/1	-0.001	-0.001	-0.001	0.001	-0.001	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
2	-0.000	0.000	-0.001	-0.001	-0.000	0.000	-0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
3	-0.001	-0.003**	0.002	0.005	-0.001	-0.003**	0.001	0.005
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
4	-0.001	0.001	-0.001	-0.002	-0.001	0.000	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
5	0.001	0.002	0.000	-0.002	0.001	0.003	0.000	-0.002
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
4c/1	0.003	0.007	0.001	-0.006	0.002	0.006	-0.000	-0.007
	(0.003)	(0.005)	(0.003)	(0.006)	(0.002)	(0.005)	(0.001)	(0.004)
2	-0.002	-0.002	-0.003	-0.001	-0.003	-0.004	-0.004	-0.001
	(0.003)	(0.004)	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
3	0.001	-0.002	0.004	0.006*	-0.000	-0.003	0.004	0.007*
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
4	0.001	0.000	0.004	0.004	0.001	-0.001	0.003	0.004
	(0.002)	(0.002)	(0.004)	(0.005)	(0.002)	(0.002)	(0.003)	(0.005)
5	0.002	0.002	-0.001	-0.002	-0.000	-0.001	-0.002	-0.001
	(0.002)	(0.003)	(0.004)	(0.004)	(0.001)	(0.003)	(0.003)	(0.004)
4d/1	0.002	0.002	0.003	0.001	0.002	0.003	0.002	-0.000
	(0.003)	(0.003)	(0.003)	(0.005)	(0.002)	(0.004)	(0.003)	(0.005)
2	-0.008	-0.007*	-0.008	-0.001	-0.008*	-0.008*	-0.008	-0.001
	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.005)	(0.004)
3	0.003	0.004	0.002	-0.001	0.003	0.004	0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)
4	0.001	-0.005*	0.006	0.011**	0.002	-0.005*	0.006	0.010*
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.004)
5	-0.001	0.006**	-0.008**	-0.015**	-0.001	0.006**	-0.008*	-0.013**
	(0.001)	(0.002)	(0.003)	(0.005)	(0.001)	(0.002)	(0.003)	(0.005)
EB	-0.000	0.001	0.000	-0.001	-0.001	-0.000	-0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
N	7,548	7,242	7,548	7,344	7,395	7,242	7,242	6,834
Months	153	147	153	149	150	147	147	139
States	51	51	51	51	51	51	51	51
Lags	7	13	7	11	10	13	13	21
FEs	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.562	0.384	0.486	0.238	0.292	0.318	0.306	0.341

Overall	0.903	0.797	0.862	0.440	0.284	0.307	0.297	0.329

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

Table 10: Leads and EB Table 4

	Dependent variable:							
	Unemployment Rate				Unemployment Growth Rate			
Tier/Lead	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/1	0.001	0.001	0.000	-0.001	0.001	0.001	0.000	-0.001
	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.002)
2	-0.000	0.002	-0.002	-0.004	-0.001	0.002	-0.002	-0.003
	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
3	-0.000	-0.004***	0.002	0.006*	-0.000	-0.004***	0.002	0.007**
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
4	0.001	0.003	-0.000	-0.002	0.001	0.003	0.000	-0.002
	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.003)
5	0.002*	0.003*	0.001	-0.002	0.002**	0.003*	0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)
2c/1	0.001	-0.002	-0.000	0.002	0.001	-0.002	-0.001	0.002
	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.005)
2	-0.001	-0.000	-0.001	-0.001	-0.001	-0.000	-0.002	-0.001
	(0.001)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.005)
3	0.002	0.000	0.006	0.006*	0.003	0.001	0.006	0.005*
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.002)
4	-0.002	0.001	-0.005	-0.005**	-0.001	0.001	-0.005	-0.006**
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
5	0.002	-0.001	0.003	0.004	0.001	0.000	0.002*	0.002
	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
3a/1	-0.001	-0.000	0.001	0.002	-0.001	-0.001	0.001	0.003
	(0.002)	(0.002)	(0.003)	(0.006)	(0.001)	(0.003)	(0.003)	(0.003)
2	-0.001	-0.001	0.000	0.001	-0.001	-0.001	0.000	0.001
	(0.003)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.005)	(0.003)
3	-0.001	-0.001	-0.002	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.005)	(0.003)
4	0.000	-0.000	0.001	0.001	-0.000	-0.000	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)	(0.004)
5	0.001	0.002	-0.001	-0.003	0.002	0.002	-0.002	-0.004
	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
3b/1	0.003	0.001	0.007	0.006	0.003*	0.000	0.008*	0.008*
	(0.004)	(0.004)	(0.005)	(0.007)	(0.001)	(0.003)	(0.003)	(0.004)
2	-0.002	-0.000	-0.003	-0.003	-0.002	-0.001	-0.003	-0.002
	(0.004)	(0.002)	(0.004)	(0.005)	(0.002)	(0.003)	(0.002)	(0.006)
3	0.001	0.004	-0.000	-0.003	0.001	0.004	-0.001	-0.004
	(0.003)	(0.002)	(0.005)	(0.006)	(0.003)	(0.002)	(0.003)	(0.005)
4	-0.002	-0.003	-0.002	0.002	-0.002	-0.004	-0.002	0.002
	(0.002)	(0.003)	(0.003)	(0.005)	(0.002)	(0.003)	(0.002)	(0.004)
5	0.002	0.004*	0.001	-0.004	0.002	0.004*	0.000	-0.003
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)

3c/1	0.000	-0.003	0.004**	0.006	0.001	-0.003	0.004	0.007*
	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.003)	(0.002)	(0.003)
2	0.001	0.003	-0.001	-0.004	0.001	0.003	-0.001	-0.004**
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
3	-0.003	-0.003	-0.004	-0.001	-0.004*	-0.003	-0.004	-0.001
	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
4	0.001	-0.001	0.002	0.003	0.00	-0.002	0.002	0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
5	0.002	0.004**	0.001	-0.003	0.002	0.003**	0.001	-0.001
	(0.001)	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
4a/1	0.001	0.002	-0.000	-0.003	0.000	0.002	-0.001	-0.002
	(0.002)	(0.003)	(0.004)	(0.006)	(0.002)	(0.004)	(0.003)	(0.003)
2	-0.002	-0.001	-0.002	-0.001	-0.002	-0.002	-0.003	-0.001
	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
3	0.000	0.003	-0.002	-0.004	0.000	0.003	-0.001	-0.004
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.003)
4	0.003	0.002	0.004	0.002	0.003	0.002	0.004	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.005)
5	0.000	-0.001	0.001	0.003	0.000	-0.002	0.002	0.003
	(0.002)	(0.002)	(0.002)	(0.004)	(0.001)	(0.002)	(0.001)	(0.002)
4b/1	-0.001	-0.001	-0.001	0.001	-0.001	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)
2	-0.000	0.000	-0.001	-0.001	-0.000	0.000	-0.001	-0.001
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)
3	-0.000	-0.003***	0.002	0.005	-0.001	-0.004***	0.001	0.005
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
4	-0.001	0.000	-0.001	-0.002	-0.001	0.000	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
5	0.001	0.002	0.000	-0.002	0.001	0.002	0.000	-0.002
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)
4c/1	0.003	0.007	0.001	-0.006	0.002	0.006	-0.000	-0.007
	(0.003)	(0.004)	(0.003)	(0.005)	(0.003)	(0.005)	(0.002)	(0.005)
2	-0.002	-0.002	-0.003	-0.001	-0.003	-0.003	-0.004	-0.001
	(0.003)	(0.004)	(0.004)	(0.005)	(0.003)	(0.004)	(0.003)	(0.004)
3	0.001	-0.002	0.004	0.006	-0.000	-0.003	0.004	0.007*
	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
4	0.002	0.000	0.004	0.004	0.001	-0.001	0.003	0.004
	(0.002)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)	(0.006)
5	0.002	0.002	-0.001	-0.003	-0.000	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.005)	(0.001)	(0.003)	(0.003)	(0.004)
4d/1	0.002	0.002	0.003	0.001	0.002	0.003	0.002	-0.000
	(0.003)	(0.004)	(0.003)	(0.005)	(0.002)	(0.004)	(0.004)	(0.005)
2	-0.008	-0.007	-0.008	-0.001	-0.008	-0.007	-0.008	-0.001
	(0.004)	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.006)	(0.004)
3	0.003	0.004	0.002	-0.001	0.003	0.004	0.002	-0.001
	(0.001)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.004)
4	0.002	-0.005	0.006	0.011*	0.002	-0.004	0.006	0.010*
	(0.003)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.004)	(0.004)
5	-0.001	0.006**	-0.008**	-0.015*	-0.001	0.006**	-0.008*	-0.013***
	(0.001)	(0.002)	(0.002)	(0.006)	(0.001)	(0.002)	(0.004)	(0.003)
EB	0.000	0.001	0.000	-0.000	-0.001	-0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)	(0.002)
N	6,783	6,222	6,732	6,477	6,579	6,222	6,477	6,171
Months	154	143	153	148	150	143	148	142
States	51	51	51	51	51	51	51	51

Lags	6	17	7	12	10	17	12	18
FEs	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.552	0.381	0.476	0.243	0.295	0.332	0.309	0.342
Overall	0.904	0.799	0.863	0.445	0.290	0.315	0.305	0.329

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

Table 11: Leads and EB Table 5

	Dependent variable:							
	Unemployment Rate				Unemployment Growth Rate			
Tier/Lead	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/1	0.001	0.001	0.000	-0.001	0.001	0.001	0.000	-0.001
	(0.001)	(0.002)	(0.002)	(0.005)	(0.002)	(0.002)	(0.003)	(0.003)
2	-0.000	0.002	-0.001	-0.004	-0.001	0.002	-0.002	-0.004
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
3	-0.000	-0.004*	0.002	0.006	-0.000	-0.004**	0.002	0.007**
	(0.001)	(0.002)	(0.002)	(0.004)	(0.001)	(0.002)	(0.003)	(0.002)
4	0.001	0.003	0.001	-0.003	0.001	0.003	-0.000	-0.002
	(0.001)	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)
5	0.002**	0.003	0.001	-0.002	0.002	0.002	0.001	-0.002
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.003)
2c/1	0.001	-0.002	-0.000	0.002	0.001	-0.003	-0.001	0.002
	(0.002)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
2	-0.001	-0.000	-0.001	-0.001	-0.001	0.000	-0.002	-0.002
	(0.001)	(0.003)	(0.001)	(0.004)	(0.002)	(0.003)	(0.002)	(0.006)
3	0.003	0.000	0.006	0.006	0.003	0.001	0.006	0.005*
	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.004)	(0.003)	(0.002)
4	-0.002	0.001	-0.005*	-0.005	-0.002	0.001	-0.005*	-0.006**
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
5	0.001	-0.001	0.002	0.004	0.001	-0.000	0.002**	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.002)
3a/1	-0.001	-0.000	0.001	0.002	-0.001	-0.001	0.001	0.002
	(0.001)	(0.003)	(0.002)	(0.005)	(0.001)	(0.003)	(0.002)	(0.004)
2	-0.001	-0.001	0.000	0.001	-0.001	-0.001	0.000	0.001
	(0.003)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
3	-0.001	-0.001	-0.001	-0.000	-0.001	-0.001	-0.002	-0.001
	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)
4	0.000	-0.000	0.001	0.002	-0.000	-0.000	0.000	0.001
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
5	0.002	0.002	-0.001	-0.002	0.002	0.002	-0.001	-0.003**
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.003)	(0.003)	(0.001)
3b/1	0.004	0.001	0.007	0.006	0.003	0.000	0.008*	0.008*
	(0.004)	(0.004)	(0.004)	(0.006)	(0.002)	(0.004)	(0.004)	(0.004)
2	-0.001	0.000	-0.003	-0.003	-0.002	-0.001	-0.003	-0.002
	(0.003)	(0.004)	(0.005)	(0.005)	(0.002)	(0.005)	(0.003)	(0.006)
3	0.002	0.004	0.000	-0.003	0.001	0.004	-0.001	-0.003
	(0.003)	(0.004)	(0.005)	(0.003)	(0.003)	(0.004)	(0.004)	(0.006)

4	-0.001	-0.003	-0.001	0.002	-0.002	-0.004	-0.002	0.003
	(0.002)	(0.004)	(0.003)	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)
5	0.003	0.005*	0.001	-0.003	0.002	0.005*	0.001	-0.003
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)
3c/1	0.001	-0.003	0.004*	0.006*	0.001	-0.003	0.004*	0.007**
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.002)
2	0.001	0.003	-0.001	-0.004	0.001	0.003	-0.001	-0.003**
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)
3	-0.003	-0.003*	-0.004	-0.001	-0.004**	-0.003	-0.004	-0.001
	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)
4	0.001	-0.001	0.002	0.003	0.000	-0.002	0.002	0.004
	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
5	0.002	0.004***	0.001	-0.003	0.002	0.004**	0.002	-0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)
4a/1	0.001	0.002	-0.000	-0.003	0.000	0.002	-0.001	-0.002
	(0.002)	(0.004)	(0.004)	(0.004)	(0.002)	(0.004)	(0.003)	(0.004)
2	-0.001	-0.001	-0.002	-0.001	-0.002	-0.002	-0.002	-0.001
	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)
3	0.000	0.003*	-0.001	-0.004	0.000	0.003	-0.001	-0.004
	(0.001)	(0.001)	(0.002)	(0.003)	(0.001)	(0.002)	(0.003)	(0.004)
4	0.003	0.002	0.004	0.002	0.003	0.002	0.004	0.002
	(0.003)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)	(0.004)
5	0.001	-0.001	0.002	0.002	-0.000	-0.002	0.001	0.004
	(0.002)	(0.004)	(0.003)	(0.004)	(0.001)	(0.003)	(0.001)	(0.002)
4b/1	-0.001	-0.001	-0.001	0.001	-0.001	-0.001	-0.001	0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)
2	-0.000	0.000	-0.001	-0.000	-0.000	0.000	-0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.004)
3	-0.000	-0.003**	0.002	0.005	-0.001	-0.003**	0.001	0.005
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.003)	(0.003)
4	-0.001	0.001	-0.001	-0.001	-0.001	0.000	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)
5	0.001	0.002	0.001	-0.001	0.001	0.002	0.000	-0.002
	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)	(0.003)	(0.002)	(0.002)
4c/1	0.004	0.007	0.002	-0.006	0.002	0.006	-0.000	-0.007
	(0.003)	(0.004)	(0.003)	(0.006)	(0.002)	(0.005)	(0.001)	(0.004)
2	-0.001	-0.001	-0.002	-0.001	-0.003	-0.003	-0.004	-0.001
	(0.003)	(0.004)	(0.005)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)
3	0.001	-0.001	0.004	0.006*	-0.000	-0.003	0.004	0.006*
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.004)	(0.003)
4	0.002	0.001	0.004	0.004	0.001	-0.001	0.003	0.004
	(0.002)	(0.002)	(0.004)	(0.005)	(0.002)	(0.002)	(0.003)	(0.005)
5	0.002	0.003	-0.000	-0.002	0.000	-0.001	-0.002	-0.001
	(0.002)	(0.003)	(0.003)	(0.005)	(0.001)	(0.003)	(0.003)	(0.004)
4d/1	0.002	0.002	0.002	0.001	0.002	0.003	0.002	0.000
	(0.002)	(0.003)	(0.003)	(0.005)	(0.002)	(0.004)	(0.003)	(0.004)
2	-0.008	-0.007*	-0.008	-0.001	-0.008*	-0.008*	-0.008	-0.001
	(0.004)	(0.003)	(0.005)	(0.004)	(0.004)	(0.003)	(0.005)	(0.003)
3	0.003	0.004	0.003	-0.001	0.003	0.004	0.002	-0.001
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)
4	0.002	-0.005*	0.006	0.011**	0.002	-0.005*	0.006	0.010**
	(0.002)	(0.002)	(0.004)	(0.003)	(0.002)	(0.002)	(0.004)	(0.003)
5	-0.001	0.007**	-0.008**	-0.015*	-0.001	0.006**	-0.008*	-0.014**
	(0.001)	(0.002)	(0.002)	(0.005)	(0.001)	(0.002)	(0.003)	(0.005)
EB	-0.000	0.001	0.000	-0.001	-0.000	0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)

N	7,548	7,242	7,548	7,344	7,395	7,242	7,242	6,783
Months	153	147	153	149	150	147	147	138
States	51	51	51	51	51	51	51	51
Lags	7	13	7	11	10	13	13	22
FEs	Y	Y	Y	Y	Y	Y	Y	Y
TEUC	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.580	0.402	0.501	0.251	0.297	0.322	0.311	0.344
Overall	0.904	0.799	0.863	0.447	0.291	0.311	0.303	0.332

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

Table 12: Leads and EB Table 6

	Dependent variable:							
	Unemployment Rate				Unemployment Growth Rate			
Tier/Lead	Total (1)	Female (2)	Male (3)	Difference (4)	Total (5)	Female (6)	Male (7)	Difference (8)
2a/1	0.001	0.001	0.000	-0.001	0.001	0.001	0.000	-0.000
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)
2	-0.000	0.002	-0.001	-0.004	-0.001	0.002	-0.002	-0.003
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
3	-0.000	-0.004*	0.002	0.006	-0.000	-0.004*	0.002	0.006**
	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)
4	0.001	0.003	0.000	-0.003	0.001	0.003	0.000	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.004)
5	0.002**	0.003	0.002	-0.001	0.002	0.003	0.001	-0.002
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
2c/1	0.001	-0.002	-0.000	0.002	0.001	-0.002	-0.001	0.002
	(0.003)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.004)
2	-0.001	-0.000	-0.001	-0.001	-0.001	0.000	-0.002	-0.002
	(0.001)	(0.004)	(0.002)	(0.003)	(0.001)	(0.004)	(0.002)	(0.003)
3	0.003	0.000	0.006	0.006	0.003	0.001	0.006	0.005**
	(0.004)	(0.004)	(0.004)	(0.003)	(0.002)	(0.004)	(0.004)	(0.002)
4	-0.001	0.001	-0.005	-0.006	-0.002	0.001	-0.005	-0.006*
	(0.001)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
5	0.002	-0.001	0.003	0.004	0.002	-0.000	0.002	0.002
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)
3a/1	-0.001	-0.000	0.001	0.002	-0.001	-0.001	0.001	0.003
	(0.002)	(0.002)	(0.003)	(0.004)	(0.001)	(0.003)	(0.002)	(0.003)
2	-0.001	-0.001	0.000	0.001	-0.001	-0.001	0.000	0.001
	(0.002)	(0.002)	(0.004)	(0.004)	(0.003)	(0.002)	(0.003)	(0.002)
3	-0.001	-0.001	-0.002	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
4	0.000	-0.001	0.001	0.001	-0.000	-0.000	0.001**	0.000
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
5	0.002	0.002	-0.001	-0.003	0.002	0.002	-0.001	-0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)
3b/1	0.003	0.001	0.007	0.006	0.003	0.000	0.008*	0.008

	(0.002)	(0.003)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)	(0.005)
2	-0.002	-0.000	-0.003	-0.002	-0.002	-0.001	-0.003	-0.002
	(0.002)	(0.003)	(0.006)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
3	0.001	0.004	-0.001	-0.004	0.001	0.004	-0.001	-0.004
	(0.002)	(0.003)	(0.005)	(0.003)	(0.004)	(0.003)	(0.004)	(0.003)
4	-0.002	-0.003	-0.003	0.001	-0.003	-0.004	-0.003***	0.001
	(0.002)	(0.004)	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)	(0.004)
5	0.002	0.005*	0.001	-0.004	0.002	0.005*	0.001	-0.003
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
3c/1	0.001	-0.003	0.004*	0.007***	0.001	-0.004	0.004	0.007
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.004)
2	0.000	0.003	-0.002	-0.005**	0.001	0.003	-0.001	-0.004
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)
3	-0.003	-0.003	-0.003	-0.001	-0.004	-0.003*	-0.003	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.001)	(0.002)	(0.002)
4	0.001	-0.001	0.002	0.003	0.000	-0.002	0.002	0.004
	(0.001)	(0.003)	(0.003)	(0.002)	(0.001)	(0.003)	(0.002)	(0.002)
5	0.002	0.004**	0.001	-0.003	0.002	0.004**	0.002	-0.001
	(0.002)	(0.001)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
4a/1	-0.001	0.002	-0.001	-0.003	-0.001	0.001	-0.001	-0.003
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.006)
2	-0.001	-0.001	-0.002	-0.001	-0.002	-0.001	-0.002	-0.000
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.003)
3	0.000	0.003*	-0.002	-0.004	-0.000	0.003	-0.002	-0.004
	(0.002)	(0.001)	(0.002)	(0.004)	(0.001)	(0.001)	(0.002)	(0.003)
4	0.003	0.001	0.004	0.002	0.003	0.001	0.004	0.003
	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.005)
5	0.000	-0.001	0.001	0.002	-0.000	-0.002	0.001	0.003
	(0.002)	(0.003)	(0.002)	(0.002)	(0.001)	(0.003)	(0.002)	(0.004)
4b/1	-0.000	-0.001	-0.001	0.001	-0.001	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.003)
2	-0.000	0.000	-0.001	-0.001	-0.000	0.000	-0.001	-0.001
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
3	-0.000	-0.003***	0.002	0.005	-0.001	-0.003**	0.001	0.005*
	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.001)	(0.003)	(0.002)
4	-0.001	0.000	-0.002	-0.002	-0.002	0.000	-0.002	-0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)
5	0.001	0.002	0.001	-0.001	0.001	0.002	0.000	-0.002
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
4c/1	0.004	0.008	0.001	-0.007	0.002	0.007	-0.001*	-0.009
	(0.003)	(0.005)	(0.004)	(0.005)	(0.003)	(0.005)	(0.002)	(0.006)
2	-0.000	-0.002	-0.000	0.001	-0.002	-0.003	-0.002	0.001
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.004)
3	0.000	-0.002	0.004	0.006	-0.001	-0.003	0.003	0.007
	(0.002)	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.005)
4	0.001	-0.001	0.004	0.005	0.000	-0.002	0.004	0.005
	(0.002)	(0.001)	(0.004)	(0.006)	(0.002)	(0.001)	(0.003)	(0.006)
5	0.001	0.001	-0.003	-0.004	-0.001	-0.001	-0.004	-0.002
	(0.002)	(0.003)	(0.003)	(0.004)	(0.002)	(0.003)	(0.003)	(0.006)
4d/1	0.002	0.003	0.002	-0.000	0.002	0.003	0.001	-0.002
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.006)
2	-0.009	-0.008	-0.008	0.000	-0.009	-0.009	-0.008	0.000
	(0.004)	(0.004)	(0.006)	(0.002)	(0.005)	(0.004)	(0.006)	(0.003)
3	0.003	0.004	0.003	-0.001	0.003	0.004	0.002	-0.002
	(0.002)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)	(0.003)
4	0.002	-0.006**	0.008	0.014***	0.002	-0.006**	0.008	0.014**

	(0.003)	(0.002)	(0.005)	(0.003)	(0.003)	(0.002)	(0.005)	(0.004)
5	-0.002	0.008**	-0.011**	-0.018**	-0.002	0.007**	-0.010**	-0.016**
	(0.002)	(0.002)	(0.003)	(0.005)	(0.001)	(0.003)	(0.003)	(0.006)
EB	-0.000	0.001	0.000	-0.000	-0.001	-0.000	0.000	0.001
	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
N	7,104	6,816	7,056	6,960	6,960	6,816	6,720	6,528
Months	153	147	153	150	150	147	145	141
States	51	51	51	51	51	51	51	51
Lags	7	13	7	10	10	13	15	19
FEs	Y	Y	Y	Y	Y	Y	Y	Y
R^2								
Within	0.562	0.380	0.483	0.238	0.297	0.319	0.312	0.336
Overall	0.901	0.785	0.859	0.438	0.290	0.308	0.302	0.326

SEs in brackets
All numbers rounded to three decimal places
*/**/*** significant at the 0.1/0.05/0.01 level

References

Abadie, A. (2005). Semiparametric difference-in-differences estimators. *The Review of Economic Studies*, *72(1)*, 1-19.

Acemoglu, D., Naidu, S., Restrepo, P., & Robinson, J. A. (2019). Democracy does cause growth. *Journal of Political Economy*, *127(1)*, 47-100.

Addabbo, T., Rodríguez-Modroño, P., & Gálvez-Muñoz, L. (2013). Gender and the great recession: Changes in labour supply in spain. *Universitá di Modena Reggio Emilia, DEMB Working Paper Series*, *10*.

Angrist, J. D., & Pischke, J. S. (2008). *Mostly harmless econometrics: An empiricist's companion*. New Jersey, United States: Princeton University Press.

Autor, D. H. (2003). Outsourcing at will: The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of labor economics*, *21(1)*, 1-42.

Bachmann, R., Bechara, P., Kramer, A., & Rzepka, S. (2015). Labour market dynamics and worker heterogeneity during the great recession–evidence from europe. *IZA Journal of European Labor Studies*, *4*(1), 19.

Boeing-Reicher, C. A., & Caponi, V. (2016). Public wages, public employment, and business cycle volatility: evidence from us metro areas. *IZA Discussion Papers*, 1-46.

Boeri, T., & Van Ours, J. (2013). *The economics of imperfect labor markets*. Princeton University Press.

Breusch, T. S. (1978). Testing for autocorrelation in dynamic linear models. *Australian Economic Papers*, *17*(31), 334–355.

Bureau of Labor Statistics. (2019a). *Bls information glossary.* Retrieved from `https://www.bls.gov/bls/glossary.htm#L` (Accessed: 11-03-2020)

Bureau of Labor Statistics. (2019b). *Changes in state unemployment insurance legislation in 2003.* Retrieved from `https://oui.doleta.gov/unemploy/euc.asp` (Accessed: 20-03-2019)

Bureau of Labor Statistics. (2019c). *Current employment statistics.* Retrieved from `https://www.bls.gov/web/empsit/cesseasadj.htm` (Accessed: 18-06-2019)

Bureau of Labor Statistics. (2019d). *Seasonal adjustment methodology for national labor force statistics from the current population survey (cps).* Retrieved from `https://www.bls.gov/cps/seasonal-adjustment-methodology.htm` (Accessed: 18-06-2019)

Bureau of Labor Statistics. (2019e). *Unemployment rate california (s).* Retrieved from `https://beta.bls.gov/dataViewer/view/timeseries/LASST060000000000003;jsessionid=66CB05DDE2237DF80A2981AA618ABA89` (Accessed: 18-06-2019)

Caliendo, M., Tatsiramos, K., & Uhlendorff, A. (2013). Benefit duration, unemployment duration and job match quality: A regression-discontinuity approach. *Journal of Applied Econometrics*, *26*, 604-627.

Card, D., Chetty, R., & Weber, A. (2007). Cash on hand and competing models of intertemporal behavior: New evidence from the labor market. *The Quarterly Journal of Economics*, *122(4)*, 1511-1560.

Card, D., & Levine, P. B. (2000). Extended benefits and the duration of ui spells: evidence from the new jersey extended benefit program. *Journal of Public economics*, *78(1-*

2), 107-138.
Chang, A. C. (2018). Tax policy endogeneity: Evidence from r & d tax credits. *Economics of Innovation and New Technology*, *27(8)*, 809-833.
Costain, J. S., & Reiter, M. (2008). Business cycles, unemployment insurance, and the calibration of matching models. *Journal of Economic Dynamics and control*, *32(4)*, 1120-1155.
De la Rica, S., & Rebollo-Sanz, Y. F. (2017). Gender differentials in unemployment ins and outs during the great recession in spain. *De Economist*, *165*(1), 67–99.
Diamond, P. A. (1984). *A search-equilibrium approach to the micro foundations of macroeconomics (vol. 1982).* Cambridge, Massachusetts, United States: MIT press.
Di Maggio, M., & Kermani, A. (2016). *The importance of unemployment insurance as an automatic stabilizer. nber working paper no. w22625.* Retrieved from `https://ssrn.com/abstract=2837637` (Accessed: 16-07-2019)
Economic Research Service United States Department of Agriculture. (2019). *Snap policy data sets.* Retrieved from `https://www.ers.usda.gov/data-products/snap-policy-data-sets/` (Accessed: 18-06-2019)
Executive Office of the President. (2011). *Unemployment insurance extensions and reforms in the american jobs act.* Retrieved from `https://obamawhitehouse.archives.gov/sites/default/files/ui_report_final_121511.pdf` (Accessed: 05-05-2019)
Faccini, R., & Rendahl, P. (2016). Unemployment insurance and unemployment volatility. *2016 Meeting Papers Society for Economic Dynamics*, *910*, 1-19.
Federal Reserve Bank of Cleveland. (2019). *Articles 18, 25-b, 25-c unemployment insurance law.* Retrieved from `https://www.clevelandfed.org/en/newsroom-and-events/publications/economic-commentary/economic-commentary-archives/2011-economic-commentaries/ec-201111-labor-market-rigidity-unemployment-and-the-great-recession.aspx` (Accessed: 25-07-2019)
Federal Reserve Bank of St.Louis. (2019a). *Labor market indicators.* Retrieved from `https://fredblog.stlouisfed.org/2015/08/labor-market-tightness/` (Accessed: 29-09-2019)
Federal Reserve Bank of St.Louis. (2019b). *Nber based recession indicators for the united states from the period following the peak through the trough.* Retrieved from `https://fred.stlouisfed.org/series/USREC` (Accessed: 19-03-2019)
Forsythe, G. E., Malcolm, M. A., & Moler, C. B. (1977). *Computer methods for mathematical computations.* New Jersey, United States: Prentice Hall.
Frey, W. H. (2017). *U.s. igration still at historically low levels, census shows.* Retrieved from `https://www.brookings.edu/blog/the-avenue/2017/11/20/u-s-migration-still-at-historically-low-levels-census-shows/` (Accessed: 17-07-2019)
Fujita, S. (2010). Economic effects of the unemployment insurance benefit. *Federal Reserve Bank of Philadelphia Business Review*, *4*, 1-19.
Fuller, W. A. (2009). *Introduction to statistical time series* (Vol. 428). John Wiley & Sons.
Godfrey, L. G. (1978). Testing against general autoregressive and moving average error models when the regressors include lagged dependent variables. *Econometrica:*

Journal of the Econometric Society, 1293–1301.

Grown, C., & Tas, E. (2011). Gender equality in us labor markets in the great recession of 2007–10. In *Consequences of economic downturn* (pp. 167–186). Springer.

Hagan, J. (1995). *Crime and inequality.* Stanford University Press.

Hagedorn, M., Karahan, F., Manovskii, I., & Mitman, K. (2013). Unemployment benefits and unemployment in the great recession: The role of macro effects. *Federal Reserve Bank of New York Staff Reports*, *Report No. 646*, 1-75.

Hagedorn, M., & Manovskii, I. (2008). The cyclical behavior of equilibrium unemployment and vacancies revisited. *The American Economic Review*, *98(4)*, 1692-1706.

Ham, J. C., & Rea Jr, S. A. (1987). Unemployment insurance and male unemployment duration in canada. *Journal of labor Economics*, *5*(3), 325–353.

Hill, R. C., Griffiths, W. E., & Lim, G. C. (2012). *Principles of econometrics.* John Wiley & Sons.

Hunt, J. (1995). The effect of unemployment compensation on unemployment duration in germany. *Journal of labor economics*, *13*(1), 88–120.

Im, K. S., Pesaran, M. H., & Shin, Y. (2003). Testing for unit roots in heterogeneous panels. *Journal of econometrics*, *115*(1), 53–74.

Karni, E. (1999). Optimal unemployment insurance: a survey. *Southern Economic Journal*, *66*, 442-465.

Katz, L. F., & Meyer, B. D. (1990). Unemployment insurance, recall expectations, and unemployment outcomes. *The Quarterly Journal of Economics*, *105*(4), 973–1002.

Lake, J. E. (2003). Unemployment benefits: Temporary extended unemployment compensation (teuc) program. *Congressional Research Service*, *RS21397*, 1-6.

Lalive, R. (2007). Unemployment benefits, unemployment duration, and post-unemployment jobs: A regression discontinuity approach. *American Economic Review*, *97(2)*, 108-112.

Lalive, R. (2008). How do extended benefits affect unemployment duration? a regression discontinuity approach. *Journal of econometrics*, *142(2)*, 785-806.

Lalive, R., Van Ours, J., & Zweimüller, J. (2006). How changes in financial incentives affect the duration of unemployment. *The Review of Economic Studies*, *73*(4), 1009–1038.

Lancaster, T., & Chesher, A. D. (1983). An econometric analysis of reservation wages. *Econometrica*, *51*, 1661-1676.

Leszczensky, L., & Wolbring, T. (2018). How to deal with reverse causality using panel data? recommendations for researchers based on a simulation study. *Sociological Methods and Research*, *0049124119882473*, 1-29.

Lewin, D. (2012). Effects of deep recession on public sector pay, benefits, and employment. *Public Jobs and Political Agendas: The Public Sector in an Era of Economic Stress*, 13–40.

Lippman, S. A., & McCall, J. J. (1979). *Studies in the economics of search.* Amsterdam, North Holland, Netherlands: Prentice Hall.

Moffitt, R. (1985). Unemployment insurance and the distribution of unemployment spells. *Journal of Econometrics*, *28*(1), 85–101.

Moffitt, R., & Nicholson, W. (1982). The effect of unemployment insurance on unemployment: the case of federal supplemental benefits. *The Review of Economics and Statistics*, 1–11.

Mortensen, D. T. (1977). Unemployment insurance and job search decisions. *Industrial and Labor Relations Review*, *30(4)*, 505517.

Mortensen, D. T., & Pissarides, C. A. (1994). Job creation and job destruction in the theory of unemployment. *The Review of Economic Studies*, *61(3)*, 397-415.

New York State. (2019). *Articles 18, 25-b, 25-c unemployment insurance law.* Retrieved from https://labor.ny.gov/ui/dande/article18.shtm (Accessed: 11-09-2019)

Nickell, S. (1981). Biases in dynamic models with fixed effects. *Econometrica: Journal of the Econometric Society*, *127(1)*, 1417-1426.

Novo, . A., & Silva, A. C. H. (2017). Can a search model predict the effects of an increase in the benefit duration? evidence from the portuguese unemployment insurance reform. *IZA Journal of Labor Policy*, *6(1)*, 1-14.

OECD. (2019). *Articles 18, 25-b, 25-c unemployment insurance law.* Retrieved from https://stats.oecd.org/Index.aspx?DataSetCode=EPL_OV (Accessed: 25-07-2019)

Pustejovsky, J. E., & Tipton, E. (2018). Small-sample methods for cluster-robust variance estimation and hypothesis testing in fixed effects models. *Journal of Business and Economic Statistics*, *36(4)*, 672-683.

Rafferty, A. (2014). Gender equality and the impact of recession and austerity in the uk. *Revue de l'OFCE*(2), 335–361.

Raymo, J. M., Musick, K., & Iwasawa, M. (2015). Gender equity, opportunity costs of parenthood, and educational differences in unintended first births: Insights from japan. *Population research and policy review*, *34*(2), 179–199.

Røed, K., & Zhang, T. (2003). Does unemployment compensation affect unemployment duration? *The Economic Journal*, *113*(484), 190–206.

Sahin, A., Song, J., & Hobijn, B. (2010). The unemployment gender gap during the 2007 recession. *Current Issues in Economics and Finance*, *16*(2).

Sax, C., & Eddelbuettel, D. (2018). Seasonal adjustment by x-13arima-seats in r. *Journal of Statistical Software*, *87(11)*, 1-17.

Schmieder, F., J., Wachter, T. V., & Bender, S. (2012). The effects of extended unemployment insurance over the business cycle: Evidence from regression discontinuity estimates over 20 years. *The Quarterly Journal of Economics*, *127*, 701752.

Starr, M. A. (2014). Gender, added-worker effects, and the 2007–2009 recession: Looking within the household. *Review of Economics of the Household*, *12*(2), 209–235.

Tatsiramos, K., & Van Ours, J. C. (2014). Labor market effects of unemployment insurance design. *Journal of Economic Surveys*, *28*(2), 284–311.

Theodossiou, I. (1998). The effects of low-pay and unemployment on psychological well-being: A logistic regression approach. *Psychological Bulletin*, *17(1)*, 85-104.

United States Department of Labor. (2014). *Extensions and special programs.* Retrieved from https://oui.doleta.gov/unemploy/pdf/uilawcompar/2014/special.pdf (Accessed: 15-03-2019)

United States Department of Labor. (2018). *State unemployment insurance benefits.* Retrieved from https://oui.doleta.gov/unemploy/uifactsheet.asp (Accessed: 13-06-2019)

United States Department of Labor. (2019). *Emergency unemployment compensation 2008 (euc08) and federal-state extended benefit (eb) summary data for state programs.* Retrieved from https://oui.doleta.gov/unemploy/euc.asp (Accessed: 08-03-

2019)

Van den Berg, G. J. (1990). Nonstationarity in job search theory. *The review of economic studies*, *57*(2), 255–277.

Van Ours, J. C., & Vodopivec, M. (2008). Does reducing unemployment insurance generosity reduce job match quality? *Journal of Public Economics*, *92*(3-4), 684–695.

Whittaker, J. M., & Isaacs, K. P. (2012). Extending unemployment compensation benefits during recessions. *Congressional Research Service*, 1-35.

Whittaker, J. M., & Isaacs, K. P. (2014). Unemployment insurance: Programs and benefits. *Congressional Research Service*, 1-28.

Winter-Ebmer, R. (1998). Potential unemployment benefit duration and spell length: lessons from a quasi-experiment in austria. *Oxford Bulletin of Economics and Statistics*, *60*(1), 33–45.

Wooldridge, J. M. (2010). *Econometric analysis of cross section and panel data.* MIT press.

Yoon, B. J. (1983). A model of unemployment duration with variable search intensity. *Review of Economics and Statistics*, *63*, 599-609.

Zhang, F., Wagner, A. K., & Ross-Degnan, D. (2011). Simulation-based power calculation for designing interrupted time series analyses of health policy interventions. *Journal of Clinical Epidemiology*, *64*, 1252-1261.