# The Impact of Unemployment Benefit Extensions on Employment: The 2014 Employment Miracle?*

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## Abstract

We measure the aggregate effect of unemployment benefit duration on employment and the labor force. We exploit the variation induced by Congress' failure in December 2013 to reauthorize the unprecedented benefit extensions introduced during the Great Recession. Federal benefit extensions that ranged from 0 to 47 weeks across U.S. states were abruptly cut to zero. In sharp contrast to their typical dynamics, labor force and employment growth accelerated sharply in states with larger cuts in benefit duration.

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# 1 Introduction

Our objective in this paper is to assess the impact of unemployment benefit extensions on the labor force and employment. Measuring the magnitude of this effect is manifestly important for understanding the economic consequences of this widely used policy instrument. Yet, the existing literature provides little information on the sign, let alone the size of these effects. In the theoretical literature the effect of benefit extensions on employment is generally ambiguous. Basic decision theory suggests that some unemployed may increase their search effort in response to a cut in benefits, while others, who were mainly searching to qualify for benefits, might drop out of the labor force once losing eligibility, leading to offsetting effects on employment. Equilibrium job search theory typically implies a positive effect of a cut in benefit duration on job creation. This makes it easier for the unemployed to find jobs and might induce those previously out-of-labor force to rejoin the labor force, leading to an increase in employment with an ambiguous effect on unemployment since the number of job vacancies and the number of searchers increases at the same time. The empirical micro literature has focused almost exclusively on measuring the effects of benefit eligibility on the search effort of unemployed workers – a focus that is too narrow to infer the total impact of benefit duration on employment. The estimates in the quantitative macro literature vary widely depending on the value of parameters that are notoriously difficult to identify. Moreover, the literature generally ignores the effect of policies on the participation decisions of those out-of-the-labor force, which limits their ability to measure the total effect on employment. Indeed, in the data the flow from non-participation into employment accounts for over 60% of all transitions into employment.

We propose to sidestep these difficulties by directly measuring the employment and labor force impacts of a large nationwide cut in benefit duration in December 2013. The attractive feature of this quasi-natural experiment is that its effects can be measured using standard empirical techniques that do not require imposing assumptions of a particular labor market model on the data. Specifically, we measure the impact of the December 2013 decision by Congress to terminate the Emergency Unemployment Compensation Act of 2008 (EUC08) which abruptly lowered benefit duration in all states to their regular duration of typically 26 weeks. This decision terminated an unprecedented extension of unemployment benefit

durations adopted by policymakers following the onset of the Great Recession. While benefit durations began declining in some of the states starting in 2011, even by the end of 2013, right before the reform and long after the recession had ended, the average benefit duration across U.S. states stood at 53 weeks.

The decision to eliminate benefit extensions at the end of 2013 was quite controversial. Summarizing the conventional wisdom at the time, the Council of Economic Advisers and the Department of Labor (2013) predicted that 240,000 jobs would be lost in 2014 because of the negative impact on aggregate demand. Many economists voiced a concern, first articulated in Solon (1979), that without access to benefits unemployed workers will stop searching for jobs and will exit the labor force instead.

However, the U.S. labor market performance in 2014 surprised many observers (though not all, see e.g. Mulligan (2015)). Figure A-1 in the Appendix reports some basic aggregate statistics. Average employment growth was about 25% higher in 2014 than in the best of several preceding years. The employment-to-population ratio rose. The unemployment rate declined sharply. In contrast to typical predictions, the labor force participation rate suddenly halted its steady secular decline. The number of job vacancies that employers were trying to fill increased sharply.

At the national level, however, it is difficult to ascertain the extent to which these aggregate labor market developments were induced by the elimination of unemployment benefit extensions. The fact that aggregate productivity growth was slower in 2014 than in the preceding years eliminates the most prominent alternative explanation. While that can help explain the low observed wage growth in 2014, it cannot reconcile the low wage growth with the otherwise booming labor market. However, based on aggregate data alone, it appears difficult to rule out the possibility that some other aggregate shocks (coincidental with the decline in benefit duration) suddenly spurred the decisions of firms to create job vacancies and of jobless workers to accept them.

To overcome this difficulty, we take a different route in this paper. In particular, we exploit the fact that, at the end of 2013, federal unemployment benefit extensions available to workers ranged from 0 to 47 weeks across U.S. states. As the decision to abruptly eliminate all federal extensions applied to all states, it was exogenous to economic conditions of individual states. In particular, states did not *choose* to cut benefits based on, e.g. their employment in 2013 or

expected employment growth in 2014. This allows us to exploit the vast heterogeneity in the size of the decline in benefit duration across states to identify the labor market implication of unemployment benefit extensions. Note, however, that the benefit durations prior to the cut, and, consequently, the magnitudes of the cut, likely depended on economic conditions in individual states. Thus, the key challenge to measuring the effect of the cut in benefit durations on employment and the labor force is the inference on labor market dynamics that various states would have experienced without a cut in benefits.

After describing the institutional features of the U.S. unemployment insurance system and the details of the policy change in December 2013, in Section 2 we document several patterns in the data using a graphical analysis and provide simple but informative correlations. The aim is to explore the raw data for the evidence of the potential effects the reform had on employment and the labor force as well as to assess the presence of potential confounders that might affect our subsequent formal inference. We first show that states that witnessed larger cuts in benefit duration experienced a significantly stronger acceleration of employment and labor force growth, suggesting that the reform stimulated the labor market significantly. One concern might be that the high growth of states that experienced larger benefit cuts was just a continuation of a trend that started before the reform and was thus not caused by it. We show that this is unlikely to be the case as high benefit states did not experience significant relative acceleration in the years leading up to the reform. The abrupt reversal in the relative employment growth trend of high benefit states in December 2013, right at the time when the benefit durations were cut, strongly suggests that our analysis indeed identifies the implications of this particular policy change. There were no other policy changes at the turn of 2014 that could have differentially affected states depending on their pre-reform benefit duration and had significant labor market implications. Another concern might be that the strong correlations we find are due to mean-reverting dynamics around state-level trends. This would be the case if states that, although featuring the same medium-run trends, experienced negative shocks started the recovery process before the reform and this recovery continued through 2014. As those states also had high benefits around the time of the reform our analysis would attribute the employment gains of the recovery to the cut in benefits, although those gains would have realized even without the reform. We conduct several placebo tests confirming the conclusion that the effects of the reform are large and that mean reversion is not driving our results.

Indeed, we find no evidence of sizeable mean reversion in the data.

The fact that high benefit states did not experience an acceleration in employment growth in the years prior to the reform suggests that state-level employment follows a highly persistent process. This property was first identified by Blanchard and Katz (1992), who showed that state level employment evolves according to a process statistically indistinguishable from a unit root. Although their 40-year sample period included large booms and recessions, it obviously did not include the Great Recession, which could have been different. Our findings suggest that it was not too different in terms of the persistence of employment.

The results of the graphical analysis suggest clear and large effects of the reform on employment and the labor force and provide no evidence of worrisome dynamics that might be challenging to control for in a formal inference. Building on these results, we conduct a formal analysis where we allow for a flexible dynamic model of state level employment that permits mean-reversion in the variables of interest (as the data suggest a highly persistent process but not necessarily a random walk). This is important to ensure an unbiased estimate of the effect of the reform that is not contaminated by the acceleration in employment and labor force growth that might have occurred in high benefit states even in the absence of the reform. This and various additional challenges to identification are formalized in Section 3 in which we also propose a methodology that can overcome those challenges.

The results of the formal empirical analysis are presented and discussed in Section 4. The baseline specification includes a relatively parsimonious autoregressive model of employment and of the labor force that is extended in various ways in the sensitivity analysis. The results are quite robust, however, because the counterfactual dynamics in the labor force and in employment estimated using all the approaches we consider are fairly similar, and capture quite well state-level dynamics. This allows us to conclude that, conditional on these estimated dynamics, the common trend assumption is satisfied and that mean reversion does not bias our results. We can therefore obtain consistent estimates of the effects of the cut in benefit durations on the labor force and employment. The results indicate that changes in unemployment benefit duration had a large and statistically significant effect on employment: a 1 percent drop in benefit duration led to an increase in employment 4 quarters later by approximately 0.02 log points. Importantly, we also find that more than half of the increase in employment attributed to the cut in benefits was due to an increase in the labor force. Our

analysis thus implies that those previously not participating in the labor market decided to enter the labor force. These effects are not unexpected in light of equilibrium labor market theories which imply an increase in job creation in response to a cut in benefit duration. The increased availability of jobs than draws non-participants into the labor market.[1]

These estimates are based on the differential response of the labor force and of employment across states to changes in benefit durations. To the extent that economic activity reallocates in response to differences in benefit durations across states, the effects of such a reallocation are reflected in our estimates. It would be desirable, however, to be able to aggregate these estimates to obtain the effect of the nation-wide change in benefit duration that precludes the possibility of reallocation of economic activity. To this end, we document that there was no significant change in individuals' state of employment in response to changes in benefits. In addition, we do not find any differential impact of benefit duration changes on employment shares of tradeable and non-tradeable sectors. Using a simple trade model with frictional labor markets, we show that these observations allow us to aggregate state-level elasticities to the nation-wide one. Empirically, we find that our estimates imply that the cut in benefit duration accounted for close to 75 percent of aggregate employment growth in 2014.

As follows from the discussion above, to help guide economic theory, the joint evolution of employment and of the labor force in response to unemployment benefit duration changes is most informative. The only data source in the U.S. that contains long time series of both measures at the state-level at a reasonably high frequency is the Local Area Unemployment Statistics (LAUS). Conveniently for our purposes, both variables are also consistently defined and represent the counts of individuals at each point in time. A complementary data set that is often used to measure state-level job counts covered by the unemployment insurance system is the Quarterly Census of Employment and Wages (QCEW). In Section 5, we find that changes in unemployment benefits have a large and statistically significant effect on QCEW job counts: a 1 percent drop in benefit duration increases the number of jobs four quarters ahead by 0.013 log points. The point estimates for the increase in the number of jobs is somewhat smaller than the estimate of the effect on employment. This might indicate that a cut in benefits leads

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[1]The theoretical prediction that an increase in job availability draws non-participants into the labor market is the standard one in the literature. See Pissarides (2000) Ch. 7 for a textbook treatment and Krusell et al. (2015) for a modern quantitative evaluation and additional references.

to an increase in the number of full-time jobs at the expense of part-time jobs.[2] This is not the only possibility, however, since the covered population also differs across the two data sets.

The only other paper to provide a direct estimate of the total impact of unemployment benefit extensions on employment is Hagedorn et al. (2013). The objective of that paper was to measure the effects of benefits on *un*employment in a way that is consistent with the standard equilibrium labor search model and to assess whether the model provides a coherent rationalization of the joint evolution of various labor market variables in response to unemployment benefit extensions. That paper exploits multiple changes in benefits over time and space which necessitates the development of a novel structural measurement methodology that controls for agents' expectations regarding future policy changes that is consistent with the theoretical model. Our focus in this paper is instead on the measurement of the effects of a one-time permanent change in unemployment benefit extensions on employment. We exploit the variation induced by the policy reform that lends itself to the analysis using the standard tools developed by labor economists. This allows us to conduct the measurement without imposing any theoretical restrictions of a particular labor market model. Nevertheless, we compare the results of the two papers below and find that they imply a quantitatively similar negative impact of benefit extensions on employment. In addition, Mulligan (2015) computes the employment effect of the policy reform based on his measure of the change in implicit marginal tax rates on work associated with the reform and obtains a very similar aggregate employment impact to the one we find. Johnston and Mas (2015) study a similar, albeit smaller, policy reform and find a significant positive employment impact of the abrupt cut of benefit duration in Missouri in 2011. They provide detailed calculations that show that their findings are quantitatively nearly identical to our estimates in this paper that are based on the nation-wide reform. An exhaustive analysis of the other related but less relevant literature is available in Johnston and Mas (2015) and Hagedorn et al. (2016).

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[2]This happens because QCEW counts the number of jobs while LAUS counts the number of individuals who have at least one job. Thus, for example, if a worker holding two part-time jobs secures a single full-time job, the QCEW job count would decline by one while LAUS employment would not be affected.

# 2 Data and the Unemployment Insurance Reform

## 2.1 Policy Environment

Prior to the onset of the Great Recession, unemployed workers in most states qualified for 26 weeks of unemployment compensation paid by the state in which the lost job was located.[3] In response to the deterioration of labor market conditions, the federal Emergency Unemployment Compensation (EUC08) program was enacted in June 2008. The program started by allowing for an extra 13 weeks of benefits to all states and was gradually expanded to have 4 tiers, providing potentially 53 weeks of federally financed additional benefits. The availability of each tier was dependent on state unemployment rates. The EUC08 program was not originally envisioned to last for many years, but was periodically reauthorized by Congress. The last annual reauthorization took place in December 2012.

In addition, the Extended Benefits (EB) program allows for 13 or 20 weeks of extra benefits in states with elevated unemployment rates. The EB program is a joint state and federal program. The federal government pays for half of the cost, and determines a set of "triggers," related to the state insured and total unemployment rates, that the states can adopt to qualify for extended benefits. At the onset of the recession, many states chose to opt out of the program or only adopt high triggers. The American Recovery and Reinvestment Act of 2009 turned this into a federally funded program. Following this, many states joined the program and several states adopted lower triggers to qualify for the program. Most states wrote their legislation implementing their EB program in a way that provided for their participation only as long as federal government paid for 100 percent of the cost. The provision for federal financing of the EB program was reauthorized together with reauthorizations of the EUC08 program.

An important feature of the EB program is that many triggers available to the states under the federal law contain look-back provisions. In particular, the state under those triggers qualified for federal financing only if state unemployment was 110 or 120 percent (depending on a trigger) higher than in the preceding two years. In other words, the EB program could be made available under those triggers only if unemployment is rising. Consequently, starting in 2011 some states began losing eligibility for the EB program.[4] As total duration of available

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[3]Note that benefit eligibility is based on the location of employment, not the residence of the worker.

[4]To mitigate this effect, the federal government temporarily gave states an option of using a three year

Table 1: Benefit Duration across States in December 2013

| Weeks of Benefits | States |
|---|---|
| 73 weeks | Illinois, Nevada, Rhode Island |
| 63 weeks | Alaska, Arizona, California, Connecticut, Delaware, DC, Indiana, Kentucky, Louisiana, Maryland, Massachusetts, Mississippi, New Jersey, New York, Ohio, Oregon, Pennsylvania, Tennessee, Washington |
| 61 weeks | Arkansas |
| 57 weeks | Michigan |
| 54 weeks | Alabama, Colorado, Idaho, Maine, New Mexico, Texas, West Virginia, Wisconsin |
| 49 weeks | Missouri, South Carolina |
| 44 weeks | Georgia |
| 40 weeks | Florida, Hawaii, Iowa, Kansas, Minnesota, Montana, Nebraska, New Hampshire, North Dakota, Oklahoma, South Dakota, Utah, Vermont, Virginia, Wyoming |
| 19 weeks | North Carolina |

unemployment benefits began declining so did the unemployment rate resulting in some states also losing eligibility for some of the tiers of the EUC08 program.

As a result, by December 2013 there was substantial heterogeneity in the actual unemployment benefit durations across U.S. states. As Table 1 shows, 3 states had 73 weeks of benefits available, 20 states had 61-63 weeks, 9 states had 54-57 weeks, 18 states had 40-49 weeks, and one state had 19 weeks. These data on unemployment benefit durations in each state is based on trigger reports provided by the Department of Labor. These reports contain detailed information for each of the states regarding the eligibility and activation status of the EB program and different tiers of the EUC08 program.[5]

In December 2013, Congress chose not to reauthorize the EUC08 program. As there was

look-back period.

[5] See http://ows.doleta.gov/unemploy/trigger/ for trigger reports on the EB program and http://ows.doleta.gov/unemploy/euc_trigger/ for reports on the EUC08 program.

no "phase-out" period for EUC08 payments, all EUC08 payments ceased abruptly in all states when the program ended. Specifically, individuals who exhausted regular state unemployment compensation after December 21, 2013 (December 22, 2013 in NY) were no longer eligible for EUC08. For unemployed individuals already participating in the EUC08 program, the last payable week of EUC08 benefits was the week ending December 28, 2013 (December 29, 2013 in NY)[6]. From the moment the unemployment benefit extensions came to an end in December 2013, newly unemployed individuals could only qualify for the regular state unemployment compensation for a duration of 26 weeks in most states.[7]

An important property of the decision not to renew benefit extensions in December 2013 is that it applied to all states, regardless of their economic conditions. In particular, the states could not choose whether to be treated by this reform, for example, based on their employment in 2013 or expected employment growth in 2014. The fact that the policy change was exogenous from the point of view of an individual state, allows for a relatively straightforward identification of its labor market impact. This contrasts sharply with the gradual decline in benefit durations in many states since 2011. While those declines could have had significant labor market implications, those policy changes were endogenous to a state's labor market conditions, making the identification of the effects of policies more challenging.

While from the outset, the federal unemployment benefit extension program was understood to be temporary, the decision to stop the program came largely as a surprise. Indeed, by December 2013 the program had been re-authorized a dozen of times. By that time it had paid benefits for a record 66 months, over two years longer than any prior discretionary benefit extension program. However, the U.S. unemployment rate was higher and the long-term unemployment rate was at least twice as high as it was at the expiration of every previous unemployment benefit extension program. Moreover, the Council of Economic Advisors, the Congressional Budget Office and others argued forcefully for the reauthorization on the grounds that EUC08 is among policies with "the largest effects on output and employment per dollar of budgetary cost." In light of this, few expected Congress to terminate the program

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[6]All states had triggered off the EB program by the end of 2012, so no states were offering extended benefits under this program in December 2013.

[7]Some states had less than 26 weeks available in 2014, including Arkansas (25), Florida (16), Georgia (18), Kansas (20), Michigan (20), Missouri (20), North Carolina (19) and South Carolina (20). Two states – Massachusetts (30) and Montana (28) – offered more generous benefit durations.

in December 2013. Even following Congress' decision, there was likely some uncertainty regarding the finality of the program throughout the first half of 2014. For example, on April 7, 2014, the Senate narrowly approved a bipartisan bill that would have restored (retroactively to December 2013) federal funding for extended unemployment benefits. The bill faced a determined opposition in the House of Representatives, which refused to hold a vote on it. Note that, to the extent that economic agents were able to forecast the expiration of unemployment benefit extensions prior to December 2013 and adjusted their actions accordingly, and to the extent that they were uncertain about the possibility of the extensions being re-authorized at some point in 2014, our estimates will provide a lower bound on the effects of the policy change.

## 2.2 A First Look at the Data

Before proceeding with the formal econometric analysis, we first present the patterns evident in the raw state-level data. This evidence not only highlights the quantitative significance of the changes in the patterns of employment and labor force growth at the time of the reform, but also helps to assuage any fears that subsequent formal results are driven by the choice of the specification, or are significantly influenced by outliers or a few states with benefit duration changes in a particular range.

The main variables of interest are the state-level ratios of employment or labor force to population, henceforth abbreviated EP and LFP, respectively. State-level data on employment and the labor force are from the Local Area Unemployment Statistics (LAUS) provided by the Bureau of Labor Statistics.[8] We use the most up-to-date version of the data based on the 2015 redesign of LAUS methodology. The measures of employment and the labor-force are seasonally adjusted by the BLS. The data are reported monthly and aggregated to quarterly averages. Quarterly state-level population data are from the Regional Economic Accounts of the Bureau of Economic Analysis (BEA).[9] We then construct our employment to population ratio and labor force participation measures by diving the quarterly values. Henceforth, when

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[8]ftp://ftp.bls.gov/pub/time.series/la/. Data accessed April 28, 2016.

[9]https://www.bea.gov/regional/docs/NewQuarterlyPopulationPCPI.cfm. Data accessed August 8, 2016. The BEA only provides quarterly population estimates from 2010Q1 onwards. Later in the paper we will also use quarterly estimates of population for prior years obtained by linearly interpolating (between years) annual state-level population data from the Census Bureau, http://www.census.gov/popest/data/state/totals/2015/index.html, also accessed August 8, 2016.
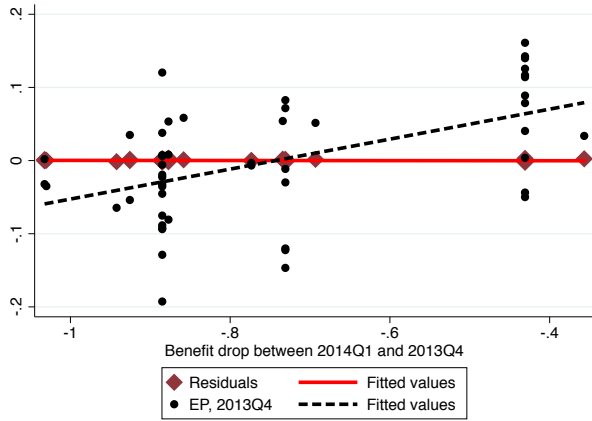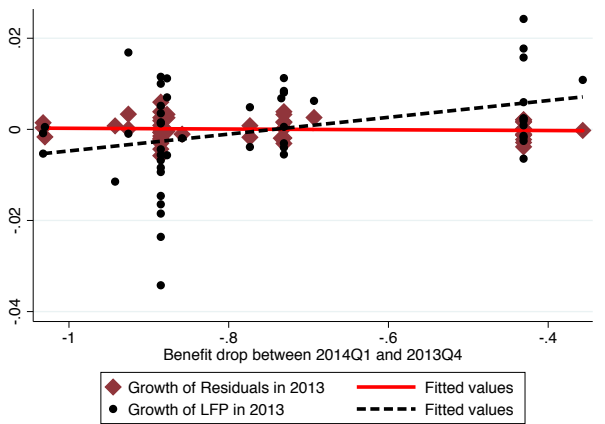
(a) Employment
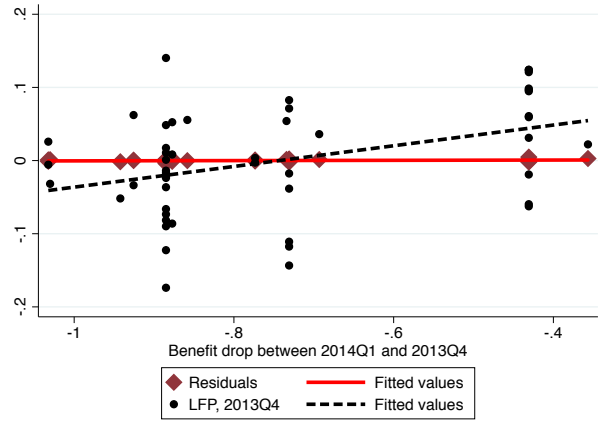
(b) Labor Force
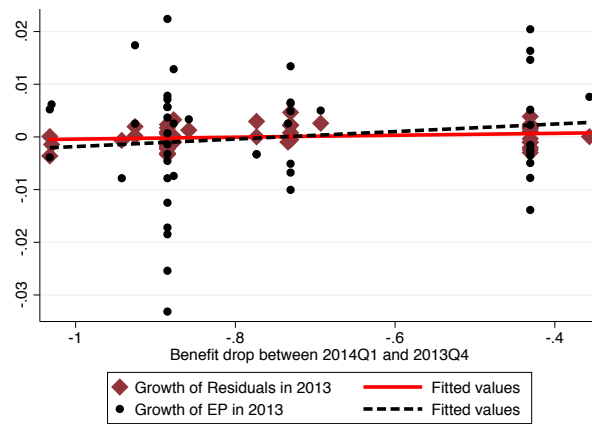
(c) Employment

(d) Labor Force

Figure 1: Panels 1(a) and 1(b): Level of EP or LFP in 2013Q4 and the cut in benefit duration induced by the reform; Panels 1(c) and 1(d): Change of EP or LFP between 2012Q4 and 2013Q4 and the cut in benefit duration induced by the reform. EP and LFP show the relationship between the cut in benefit duration and labor market outcomes across states. The residuals are from our formal analysis in Section 4 where this relationship is not present.

we refer to employment or labor force as arguments in the descriptive or formal analysis, we mean their ratio to population. Weekly state-level unemployment benefit duration data are also aggregated to a quarterly frequency.[10]

Before we describe the relationship between the cut in benefit durations and labor market outcomes across states following the reform, it is important to understand the relationship between the benefit duration cut and state labor market performance before the reform. To this end, we first plot the size of the drop in benefit duration in each state between 2013q4 and

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[10]Based on trigger reports described in Footnote 5.

2014q1 against the 2013q4 state's (cross-sectionally demeaned) level of employment in Figure 1(a) and of labor force in Figure 1(b).[11] There is visible positive relationship, highlighted by the dotted linear regression line, implying that states with a larger drop in benefit duration (and thus generally a higher level of benefit duration at the onset of the reform) also had lower EP and LFP ratios. As expected, this confirms that benefit duration was higher in states with a worse labor market situation.

A more important question for the subsequent analysis, however, is how employment evolved across states prior to the reform. If employment started to accelerate before the reform in high benefit states and if this acceleration continued after the reform, then our results would erroneously interpret the acceleration of employment as a consequence of the reform. Figures 1(c) and 1(d) illustrate this relationship. They plot each state's employment growth between 2012q4 and 2013q4 against the cut in benefit duration it experienced between 2013q4 and 2014q1. We observe a weaker but still noticeable positive relationship, implying that rather than accelerating, EP and LFP in fact grew *slower* prior to the reform in states with larger cuts (and a higher pre-reform level) of benefit duration.

In the formal analysis in Section 4 we will use more flexible models of state-level employment trends to make sure that we measure the effects of the reform and not the continuation of the pre-reform trends. Previewing the results, the red solid line in every panel of Figure 1 indicates, that there is no relationship between the level or growth in EP and LFP in 2013 and the cut in benefit duration during the reform after these trends are accounted for.

Prior to heading to the formal econometric analysis, it is helpful to establish that there is clearly visible evidence in the data indicating substantial effect of the reform. To this aim we apply a simple "difference-in-differences" analysis as the double differencing eliminates potential linear state-specific pre-trends. In Figure 2(a) we plot the difference in the growth rate of employment to population ratio in 2014 and in 2013 against the difference in the growth rate of unemployment benefit duration between those two years for each state.[12] Similarly, Figure 2(b) we plot the difference in the growth rate of labor force participation in 2014 and

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[11]Formally, let $b_{it}$ and $x_{it}$ be the log of benefit duration and the log of EP or LFP in state $i$ in quarter $t$, respectively. The Figures 1(a) and 1(b) then plot $x_{2014Q4}$ against $(b_{2014Q1} - b_{2013Q4})$, while Figures 1(c) and 1(d) plot $(x_{2013Q4} - x_{2012Q4})$ against $(b_{2014Q1} - b_{2013Q4})$.

[12]To focus on the effects of the nationwide cut in benefits in December 2013, we exclude North Carolina from the analysis in this section, since all benefit extensions were eliminated in NC six months before their elimination in the rest of the country.
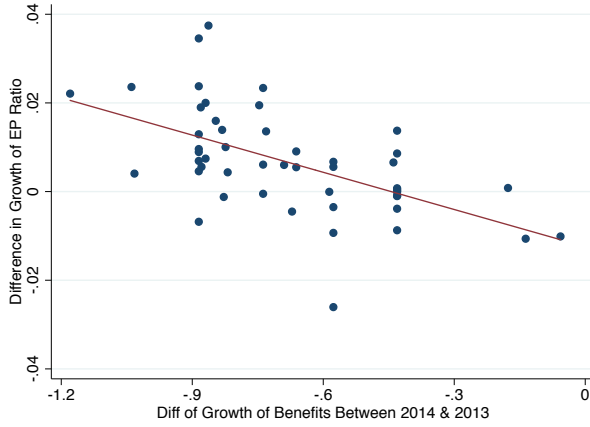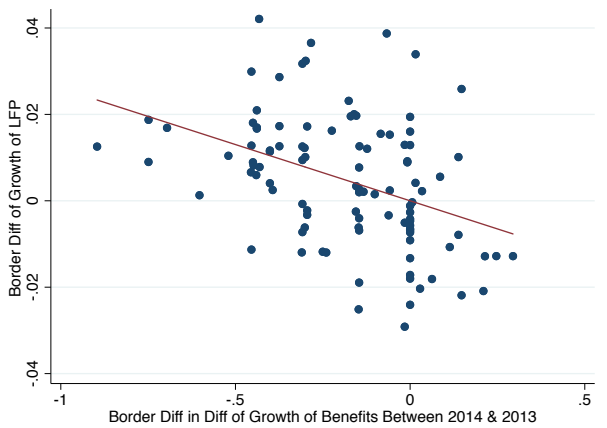
(a) Employment States  (b) Labor Force States
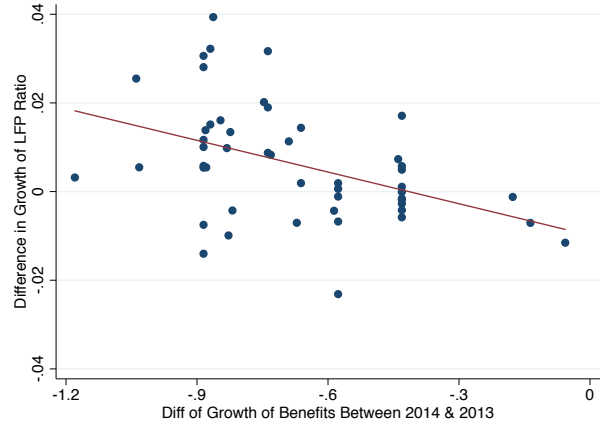
(c) Employment State Borders  (d) Labor Force State Borders

Figure 2: Difference in growth rates of EP or LFP in 2014 and 2013 vs. the difference in growth rates of benefit duration in 2014 and 2013 across states and bordering state pairs.

in 2013 against the difference in the growth rate of unemployment benefit duration between those two years for each state.[13] The standard logic of "difference-in-differences" ensures that the high growth of employment and labor force in the states with high benefit duration drops in 2014 were not a continuation of trends already present in 2013, ruling out this potential bias.

As evident from the figures, states that saw larger declines in benefit duration in 2014 relative to 2013 also experienced an acceleration in employment and labor force growth. While there is heterogeneity in labor market dynamics across states, the overall pattern is unam-

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[13]Formally, let $b_{it}$ and $x_{it}$ be the log of benefit duration and the log of EP or LFP in state $i$ in quarter $t$, respectively. The figure then plots $(x_{2014Q4} - x_{2013Q4}) - (x_{2013Q4} - x_{2012Q4})$ against $(b_{2014Q4} - b_{2013Q4}) - (b_{2013Q4} - b_{2012Q4})$.

biguous with the slope of the linear regression line through these points being negative and highly statistically significant: -0.0284 (s.e. 0.0059) for EP, and -.0241 (s.e. 0.0071) for LFP.

In Figures 2(c) and 2(d), we report the corresponding plots where the unit of analysis is not a state but a border between two adjacent states. Specifically, we first difference employment and labor force growth in a given year between two adjacent states (defined as the difference in EP or LFP in the state with higher benefits at the end of 2013 minus EP or LFP in the state with lower benefits). On the vertical axes of Figures 2(c) and 2(d) we have the difference in these differences in EP and LFP growth, respectively. On the horizontal axes we have the difference between 2014 and 2013 in the differences of benefit duration growth between adjacent states in those years.[14] As neighboring states are expected to have more similar employment and labor force trends than locations that are further apart geographically, such triple differencing helps to eliminate the potential effect of such trends in addition to eliminating linear state-level pre-trends.

The results once again reveal a clear tendency for employment and labor force growth to accelerate in the states experiencing larger benefit declines in 2014 relative to 2013. The negative slope of the linear regression line through these points is slightly larger than in the state-level analysis: -.0329 (s.e. 0.0026) for EP, and -.0260 (s.e. 0.0031) for LFP.

How exceptional are these patterns? In other words, could have we expected the remarkable acceleration of employment and labor force growth in states that experienced an acceleration in the decline in benefit duration in 2014 had benefit duration not been cut? To address this question we perform a placebo analysis where we counterfactually assume that the nationwide benefit cut occurred in some quarter preceding the date of the actual reform. Figure 3 summarizes the slopes of the regression lines of the scatter plots such as in Figure 2 constructed in every quarter between 2011Q1 and 2012Q4 by assuming (counterfactually) that benefit extensions were eliminated in that quarter. In Appendix III we provide a full set of the associated scatter plots. This evidence suggests that the patterns observed during the actual reform at the end of 2013 are indeed exceptional.

What can account for these patterns in the data? One theory is that the cuts in unemploy-

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[14]Formally, let $\Delta b_{ijt} = b_{it} - b_{jt}$ and $\Delta x_{ijt} = x_{it} - x_{jt}$ be the difference in log of benefit duration and the log of EP or LFP between bordering states $i$ and $j$ in quarter $t$, respectively. State $i$ is the one with higher benefit duration in 2013Q4 relative to state $j$. Figures 2(c) and 2(d) then plot $(\Delta x_{ij,2014Q4} - \Delta x_{ij,2013Q4}) - (\Delta x_{ij,2013Q4} - \Delta x_{ij,2012Q4})$ against $(\Delta b_{ij,2014Q4} - \Delta b_{ij,2013Q4}) - (\Delta b_{ij,2013Q4} - \Delta b_{ij,2012Q4})$.
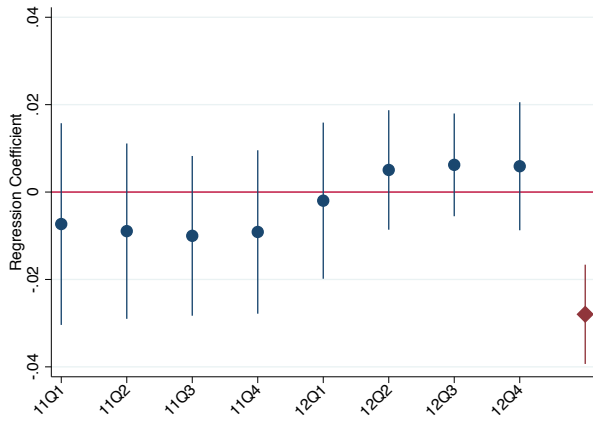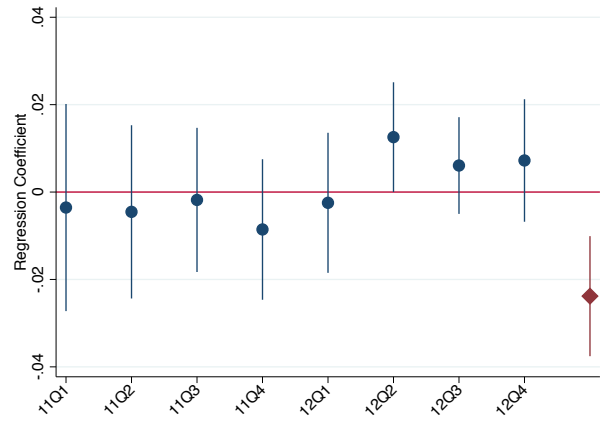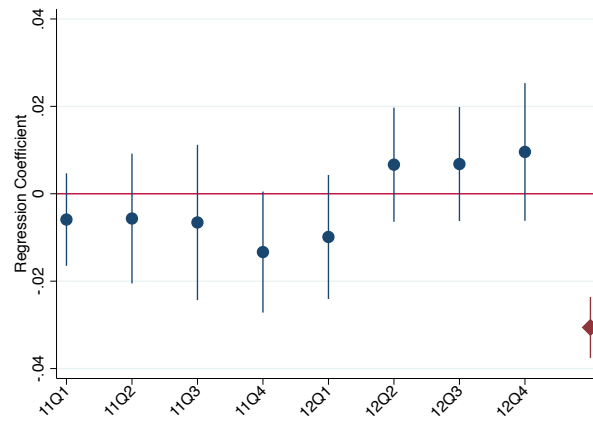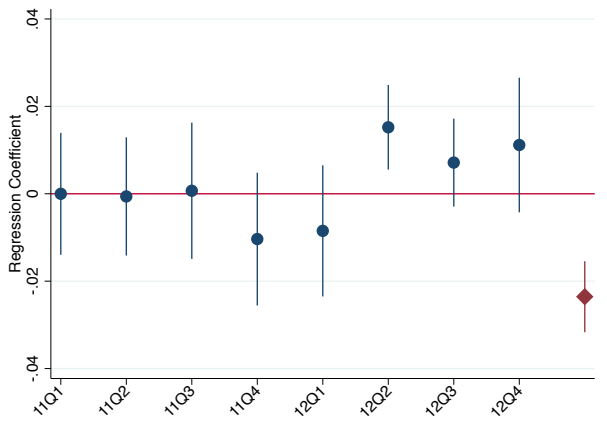
Figure 3: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before the quarter marked on the horizontal axis on the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs. Rightmost point on each panel corresponds to actual reform in 2013q4.

ment benefit duration induce an acceleration of employment and labor force growth. However, these patterns might also be consistent with an explanation based on "mean-reversion" in labor market variables. Such an explanation is based on an idea that shocks to state labor markets tend to revert to the mean. Thus, the lower is, say, a state's employment in some time period, the larger is the increase in employment in the next period. As state unemployment benefit extensions co-move negatively with state employment, this alternative theory predicts an acceleration of employment growth to depend on the level of benefit extensions (and thus the size of the cut), even if benefit durations themselves have no direct impact on
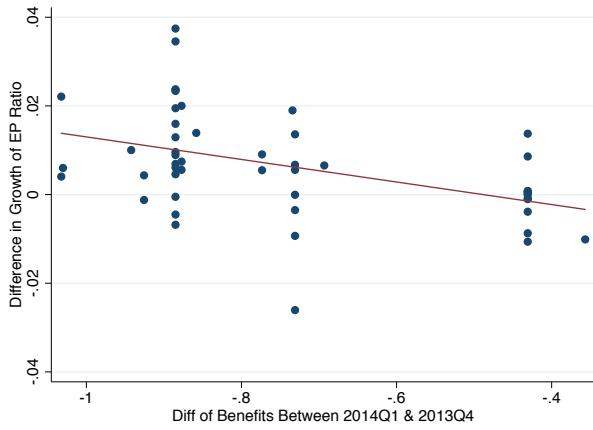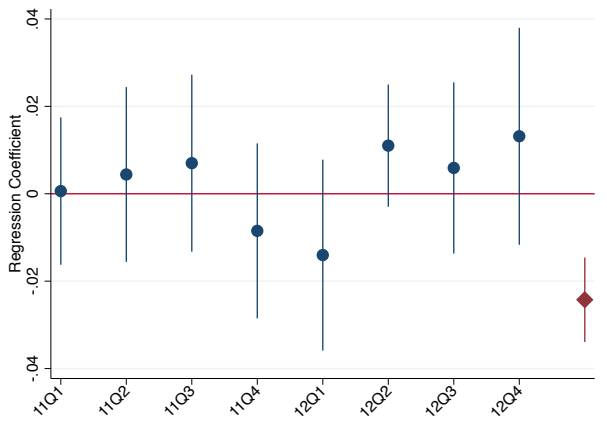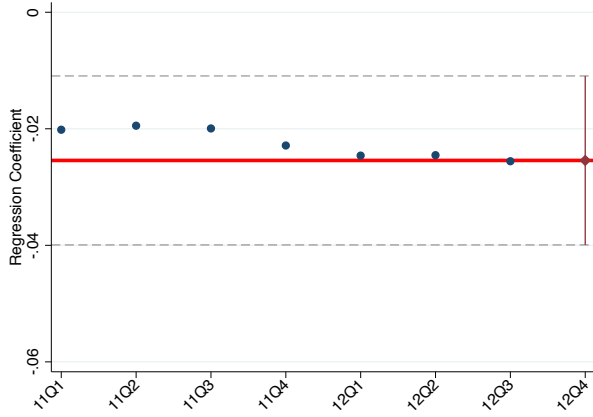
15

(a) Employment States        (b) Labor Force States
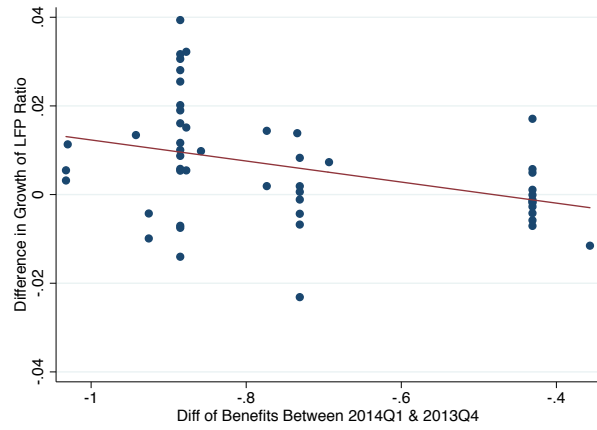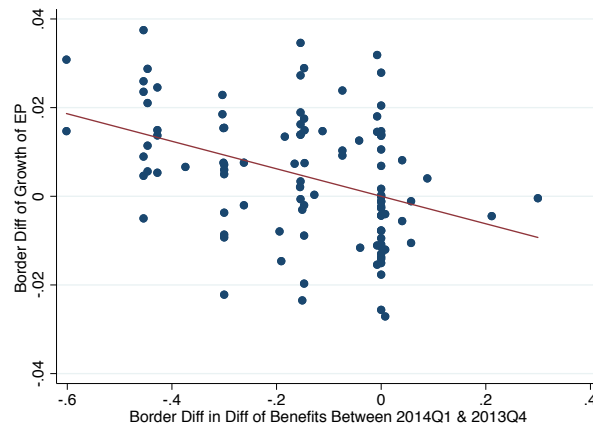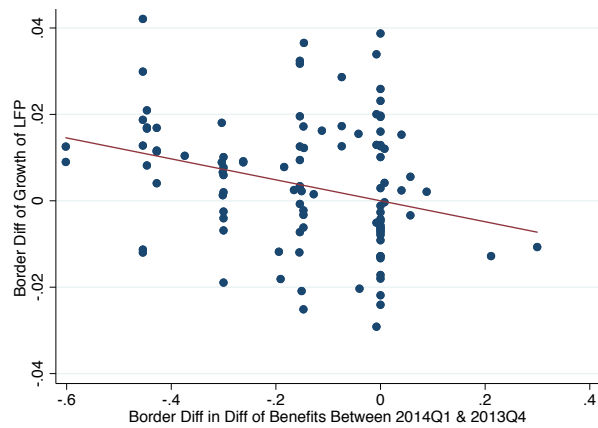
(c) Employment State Borders        (d) Labor Force State Borders

Figure 4: Difference in growth rates of EP or LFP in 2014 and 2013 vs. the benefit duration cut due to the reform, i.e., the difference in benefit duration between 2014q1 and 2013q4; states and bordering state pairs.

employment. Indeed, the scatter plots in Figure 4 illustrate the negative relationship between the difference in the growth rate of employment in 2014 and in 2013 and the cut in benefit duration due to the reform in December 2013.[15] While this figure is consistent with both theories, the one based on mean-reversion is largely discredited by the evidence in Figure 5 which summarizes the slopes of the regression lines of the scatter plots such as in Figure 4 constructed in every quarter between 2011Q1 and 2012Q4 (these scatter plots can be found in Appendix IV). That figure shows that the size of benefit extensions (i.e., the magnitude

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[15]The estimated slopes of the linear regression lines through points in each panel are: -0.0253 (s.e. 0.0074) in Panel 4(a), and -0.0230 (s.e. 0.0085) in Panel 4(b), -0.0307 (s.e. 0.0033) in Panel 4(c), and -0.0231 (s.e. 0.0034) in Panel 4(d).
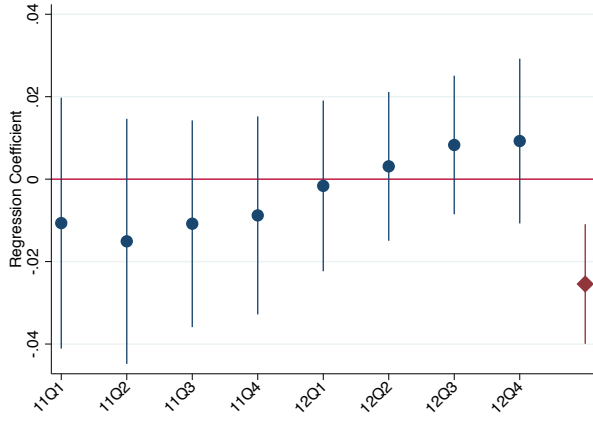
(a) Employment States
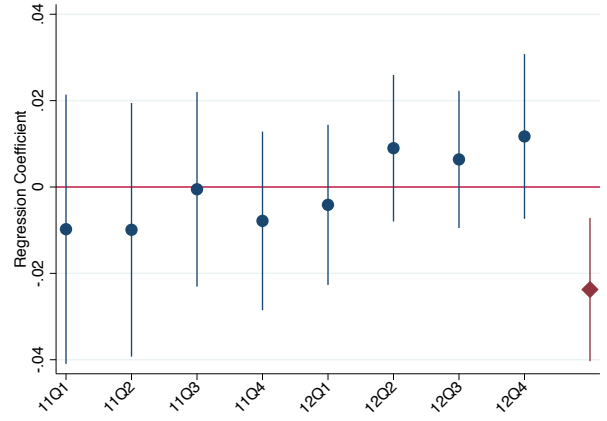
(b) Labor Force States
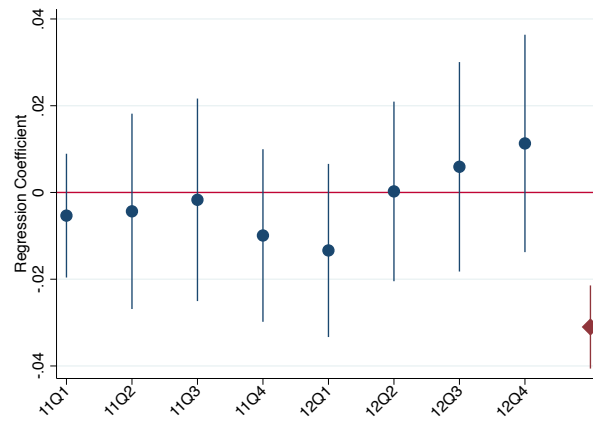
(c) Employment State Borders
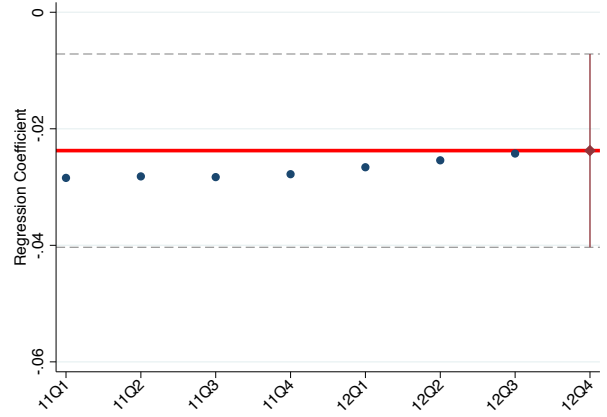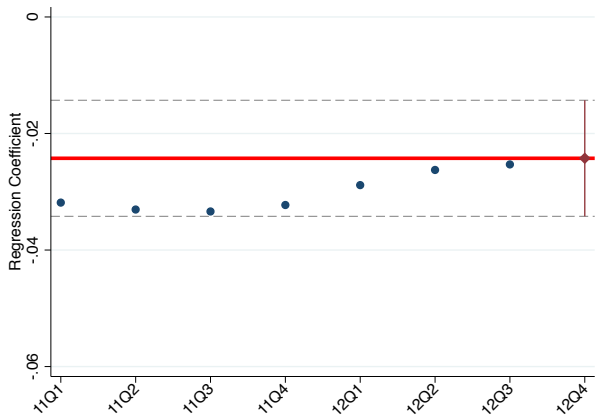
(d) Labor Force State Borders

Figure 5: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before the quarter marked on the horizontal axis on the size of placebo benefit cut in the quarter marked on the horizontal axis. States and bordering state pairs. Rightmost point on each panel corresponds to actual reform in 2013q4.

of the placebo cut) does not predict the acceleration of employment growth in periods when there was no actual reform eliminating extended benefits. These results show that in the years prior to the cut states with high benefits do not have a "tendency" to experience accelerated growth in employment over the subsequent year, strongly suggesting that there is no sign of mean reversion, therefore ruling out that concern

Figure 6 illustrates that the sharp acceleration of employment and labor force growth in 2014 of high benefit duration states in 2013q4 occurred not only relative to the trend in 2013, but also relative to longer term pre-reform trends. Specifically, for each state $i$ and $\tau = 0, ..., 11$ we define $\Delta_{i,\tau} = (x_{i,2014Q4} - x_{i,2013Q4}) - (x_{i,2013Q4} - x_{i,2012Q4-\tau})$, i.e. the deviation of the 2014

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(a) Employment States

(b) Labor Force States
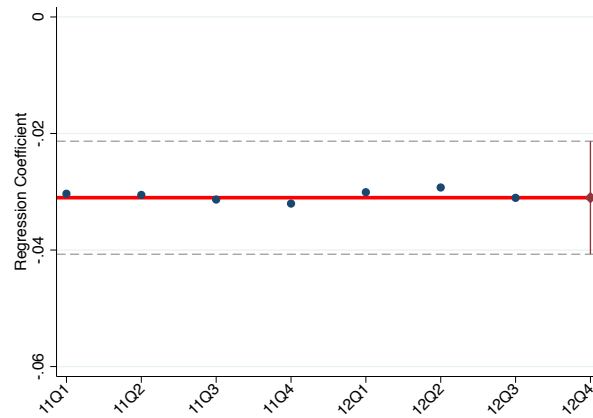
(c) Employment State Borders

(d) Labor Force State Borders

Figure 6: Slopes of the regression line of the difference in growth rates of EP or LFP in 2014 and between 2013q4 and the quarter marked on the horizontal axis on the benefit duration cut due to the reform, i.e., the difference in benefit duration between 2014q1 and 2013q4. States and bordering state pairs.

growth of variable $x$ from its growth over $\tau + 4$ preceding quarters. We then regress $\Delta_{i,\tau}$ on the difference in log of benefit duration in state $i$ between 2014q1 and 2013q4. The resulting coefficients are plotted in Panels 6(a) and 6(b) for employment and labor force, respectively. Panels 6(c) and 6(d) report similar coefficients estimated using the state-border specification. In each of the panels, the estimated coefficients remain similar for all values of $\tau$. This confirms that state-specific employment and labor force dynamics are highly persistent.

This finding extends the classic result of Blanchard and Katz (1992), who used forty years of data to document that state-level employment evolves according to a highly persistent process. While their study predated the experience of the Great Recession and subsequent

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recovery, we find that their conclusions continue to apply during the latter period. In the formal analysis below we will consider richer models of state-level EP and LFP dynamics and confirm that these processes are highly persistent, further ruling out the possibility of the patterns described in this section being driven by mean-reversion of the relevant labor market variables.

# 3  Empirical Methodology

In this section we describe the empirical methodology that utilizes cross-state variation to infer the labor market implications of the nationwide cut in benefit duration in December 2013. In Section 3.1 we describe the baseline empirical specification and the construction of the measure of the policy impact. We then explain the role played by various elements of the specification in Section 3.2 followed by the discussion of aggregation issues in Section 3.3. The results based on estimating the baseline specification and various alternative specifications are presented in Section 4.

## 3.1  Benchmark Specification

The baseline empirical specification is as follows:

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \nu_t \tilde{x}_{i,2013Q4} + \eta_i + \delta_t + \epsilon_{i,t}, \qquad (1)$$

where $x_{i,t}$ is the labor market outcome (i.e., log of the ratio of employment or labor force and population) in state $i$ at time $t$, $b_{i,t}$ is the log of the number of weeks of benefits available in state $i$ at time $t$, the indicator $\mathbf{1}_{t=2014Q\tau}$ equals one in quarter $\tau$ in 2014 and zero otherwise, $n$ is the number of lags included, $\eta_i$ is a state fixed effect, $\delta_t$ is an aggregate time effect, and $\nu_t x_{i,2013Q4}$ is a state-specific time trend where $\tilde{x}_{i,2013Q4}$ is the deviation of the outcome variable (EP or LFP) in each state in 2013Q4 from the the cross-sectional mean in that quarter and $\nu_t$ is a time dummy.

The key object of interest is the estimated cumulative effect of the expiration of the policy on the relevant labor market outcome, $\tilde{\beta}_\tau$ for $\tau = 1, 2, 3, 4$, which takes into account the estimated coefficients $\beta_\tau$ and the dynamic propagation via the estimated lag structure $\gamma_j$.

For example, the effect in the first quarter is simply summarized by the dummy for the first quarter of 2014:

$$\tilde{\beta}_1 = \beta_1. \tag{2}$$

The cumulative effect in the second quarter is the dummy from the second quarter plus the dynamic effect via the lag from the first one:

$$\tilde{\beta}_2 = \beta_2 + \gamma_1\tilde{\beta}_1. \tag{3}$$

More generally, we can define the cumulative effects recursively as

$$\tilde{\beta}_m = \beta_m + \sum_{j=1}^{\min\{n,m-1\}} \gamma_j\tilde{\beta}_{m-j}, \tag{4}$$

where, recall, $n$ is the number of estimated lags in the specification. This sequence is the impulse response function, i.e., the response of future labor force or employment to a current change in policy. The effects of the reform are revealed by the magnitude and the statistical significance of the response at various lags. Standard errors of the cumulative effects $\tilde{\beta}_m$ are estimated using the delta method.

## 3.2 Identification

The identifying assumption is the standard OLS zero conditional mean assumption:

$$E[\epsilon_{it} \mid \{\mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4})\}_{\tau=1}^4, \{x_{i,t-j}\}_{j=1}^n, \nu_t\tilde{x}_{i,2013Q4}, \eta_i, \delta_t] = 0. \tag{5}$$

To understand what this assumption does and which potential endogeneity problems are addressed, it is instructive to consider a simpler specification,

$$x_{i,t} = \sum_{\tau=1}^4 \beta_\tau\mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \xi_{i,t}, \tag{6}$$

where this assumption is less likely to be satisfied and which can therefore serve to illustrate the relevant identification issues.

For this specification to deliver unbiased results, it is required that the shocks $\xi$ to $x$ are uncorrelated with $b_{i,t} - b_{i,2013Q4}$. This simple specification differs from the benchmark one in

that it does not control for lagged values of $x$ through including $\sum_{j=1}^{n} \gamma_j x_{i,t-j}$, does not control for differences across states through including $\eta_i$, does not control for time effects through $\delta_t$ and does not include a state-specific time trend $\nu_t \tilde{x}_{i,2013Q4}$. Since these controls are not included, they are captured by $\xi$. This does not induce a correlation between $\xi$ and $b_{i,t}$ (benefit duration in every quarter of 2014) because the benefit cut affected all states independently of their past employment or unemployment levels or more broadly independently of the economic performance of the state. For the pre-reform duration of benefits $b_{i,2013Q4}$, however, this is not the case and omitting one of the control variables may lead to a bias.

Including the time dummies captures the U.S.-wide evolution of the labor market. A bias may arise if e.g., the US labor market is on a recovery path with employment increasing in all states. When time dummies are not included, this trend in employment would be picked up by the coefficients $\beta_\tau$, delivering an upward biased estimate of the effects of benefits on employment.

State fixed effects control for permanent differences in employment across states which might be correlated with available benefits. Including them in the specification prevents this correlation from being erroneously attributed to a causal effect of benefit duration on employment.

Another bias arises in the simple specification (6) from the mechanical way benefits are set. In contrast to benefits in 2014, the pre-reform benefit duration in 2013Q4 depends on the past employment in the state.[16] If employment crosses a certain threshold (from above) then benefits are automatically increased with a short lag, so that

$$b_{i,2013Q4} = G(\{x_{i,2013Q4-j}\}_{j=1}^{k}). \tag{7}$$

If the economy is hit by an adverse shock in the past, employment decreases and then evolves according to the process $x_{i,t} = \sum_{j=1}^{n} \gamma_j x_{i,t-j}$. If employment is mean-reverting, then after a large adverse shock during the Great Recession employment starts recovering so that subsequent employment gains are the results of this recovery process. As this recovery may continue through 2014, employment gains in 2014 might be a result of mean-reversion as well. A bias

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[16]More precisely, it depends on past unemployment but we will use past employment here as the determinant of benefits to save on switching back and forth between the two.

arises since the initial shock to employment also leads to a rise in benefits which stay at that high level for some time, so that the benefit level is still elevated in 2013Q4. Implementing the simple regression would then suggest a negative effect of benefits on employment even if there were no true causal effect but just because high benefit states are mean-reverting in 2014. In other words, the identifying assumption of the simple model fails:

$$E[b_{i,2013Q4}\ \xi_{it}] = E[G(\{x_{i,2013Q4-j}\}_{j=1}^{k})\ \xi_{it}] \neq 0. \tag{8}$$

This non-zero correlation is a result of not including past employment levels in the regression as this adds them to the error term $\xi_{it}$, resulting in a standard endogeneity problem due to omitted variables as past employment levels move both current benefits according to equation (7) and the current shock $\xi_{it}$. Including $\sum_{j=1}^{n} \gamma_j x_{i,t-j}$ into the specification, as in (1), controls for these dynamic adjustments and thus overcomes this bias, as past shocks do not predict current employment or the current shock $\xi_{it}$ conditional on this lag. Benefits in 2013Q4 can be still be elevated because of a past negative shock but this does not create a bias since this past shock is not correlated with current employment conditional on the included lags. In the implementation we follow the standard practice and include as many lags such that the shocks $\xi_{it}$ are *i.i.d.*

Finally, the traditional specification in the literature that exploits cross-state variation in economic policies (e.g., minimum wages) to infer their impact on employment, commonly includes state-specific time trends as controls. They are included to control for heterogeneity in the evolution of labor markets within states that might be correlated with treatment intensity. In our baseline specification, state-specific trends are captured by $\nu_t \tilde{x}_{i,2013Q4}$. The advantage of this flexible specification relative to several alternatives that we will consider below is that (1) it depends on the pre-reform level of the outcome variable only and (2) it directly addresses the concern that the time of the policy reform coincided with the unusual turning point in employment dynamics (as documented above, such turning was not observed in other time periods), whereby employment and labor force growth accelerated more in states experiencing particularly severe lingering effects of the Great Recession by the end of 2013 (and this acceleration would have occurred even in the absence of the reform). As we report below, however, the estimated effects of the policy change are robust to alternative specifications of

state-specific trends.

## 3.3 Aggregation of State-Level Employment Effects

Our baseline estimates reflect the effect of unemployment benefit extensions on the labor force or on employment at the state-level. While of interest on its own, it is also desirable to be able to use the resulting coefficients to predict the effect of a nation-wide extension. A potential concern is that when some states cut benefits more than others, economic activity may reallocate to states with, say, the larger benefit cut. This reallocation is picked up by our estimates but will be absent when the policy is changed everywhere. Our results in Section 4.2.1 below alleviate such concerns. First, we find large negative effects of unemployment benefit extensions on employment in sectors commonly considered non-tradable and thus not subject to reallocation. Second, we find that, in response to changes in benefits, even unemployed workers living close to state borders do not change the strategy of which state to look for work in. Building on these insights, we show in Appendix I that we can use the estimates obtained at the state level to compute the change in U.S. employment due to the cut in benefits in a model where each state is an open economy in the (closed) U.S. economy and the labor market in each state is governed by a Mortensen-Pissarides search and matching model. Each state produces (and consumes) a nontradable and a tradable good. The two sectors, producing the tradable and the nontradable good, operate in the same labor market and are subject to the same labor market frictions. We then show that our elasticity for the employment response at the state level can be used at the aggregate level as well.

Specifically, due to the absence of reallocation and mobility caused by a change in benefits, we can estimate Equation (1) using log EP or LFP as $x$, recover the cumulative effect of interest, e.g. $\tilde{\beta}_4$, and use it to compute the implied increase in aggregate U.S. labor force or employment that is caused by the cancellation of extended benefits. In particular, in a given state $s$, the drop in benefit duration led to an increase in the ratio of employment or labor force to population by the end of 2014 of

$$\mu_s^x = \tilde{\beta}_4(b_s^{2014Q4} - b_s^{2013Q4})exp(x_s^{2013Q4}). \tag{9}$$

where $b_s^{2013Q4}$ and $b_s^{2014Q4}$ denote the logarithm of the number of weeks of benefits available in

state $s$ in 2013Q4 (just prior to the policy change) and in 2014Q4, respectively, and $x_s^{2013Q4}$ is the logarithm of EP or LFP in state $s$ in 2013Q4. Denoting the population in state $s$ by $P_s$, we obtain the increase in the aggregate level of employment or labor force, $X$, by 2014Q4 due to the policy reform as

$$\pi^X = \sum_{\text{All U.S. states } s} \mu_s^x P_s^{2014Q4}. \tag{10}$$

# 4  Unemployment Benefit Extensions, Employment and Labor Force: Findings.

## 4.1  Benchmark Results

Table 2 contains the results of the estimation of the effect of unemployment benefit duration on employment using the baseline specification in Equation (1) for the period 1990–2014, which is sufficiently long to estimate the coefficients of the dynamic model without bias.[17] The specification includes three lags of the dependent variable as this is the smallest number of lags need to ensure that residuals are serially uncorrelated. We will consider additional criteria below. We find that changes in unemployment benefits have a large and statistically significant effect on employment-population ratio: a 1 percent drop in benefit duration increases employment-population by 0.0214 log points after 4 quarters.[18] We can also use Equation (1) with labor force participation on the left hand side to estimate the percentage change in the labor force participation attributable to the cancellation of policy. Estimating this equation, we find that a 1 percent drop in benefit duration increases the labor force participation rate by 0.0145 log points.

These results of evaluating the actual reform in December 2013 stand in sharp contrast to those obtained when using the same empirical specification to assess the impact of placebo reforms in prior time periods. The associated cumulative effects, $\tilde{\beta}_\tau$, on the employment-

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[17]Econometrically, our study is best described as a "large T, large N" setting. To verify this, we conducted a Monte Carlo study by simulating samples with the dimension of the data used in estimation from specification (1) and making sure to preserve the correlation structure between the treatment variable at the time of the reform and the outcome variable. Estimating the benchmark specification on these synthetic data recovers the estimated coefficients, including the ones on lags and fixed effects, and cumulated effects well. In particular, the bias for the fourth quarter cumulant across simulations is less than 5% of the value of the coefficient.

[18]This is slightly larger but comparable to the corresponding effect estimated in Hagedorn et al. (2013).

Table 2: Benchmark Results

| VARIABLES | $\tilde{\beta}_1$ | $\tilde{\beta}_2$ | $\tilde{\beta}_3$ | $\tilde{\beta}_4$ |
|---|---|---|---|---|
| EP | -0.00414*** | -0.0106*** | -0.0168*** | -0.0214*** |
|  | (0.000857) | (0.00221) | (0.00363) | (0.00498) |
| LFP | -0.00314*** | -0.00675*** | -0.0106*** | -0.0145*** |
|  | (0.000924) | (0.00203) | (0.00352) | (0.00523) |

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

population ratio and labor force participation one through four quarters ahead are reported in the four panels of Figures 7 and 8. Except for the the rightmost point on each panel which corresponds to the actual reform in 2013q4, the estimated effects of the placebo reforms are generally quite small and statistically insignificant.

The internal validity of our baseline empirical specification depends on whether the parsimonious model of EP and LFP dynamics it uses is sufficient to account for state-level pre-reform trends in these variables. We have already seen in Figure 1 that the size of the benefit cut is related neither to the level in 2013q4 nor to the growth between 2012q4 and 2013q4 of the residuals $\epsilon_{i,t}$ recovered from estimated baseline specification in Equation (1). Building on this analysis, we now regress the residuals for each state over the 2011Q1-2013Q4 period on a constant and a linear time trend. Each dot in Figure 9 represents the estimated coefficient on the time trend for each state (on the vertical axis) plotted against the future drop in benefits between 2013q4 and 2014q1 induced by the policy reform. We observe no systematic relationship between residual pre-trends and the cut in benefits induced by the reform. To quantify this relationship we also plot a regression line obtained by regressing the residual slope coefficients for each state on the future drop in benefits. We obtain a slope coefficient of 0.00011 (s.e. 0.00012) for EP and of 0.00019 (s.e. 0.00014) for LFP. These coefficients are statistically insignificant at conventional levels. The fact that they are positive implies some residual divergence, i.e. a relative deterioration in labor market variables of the states with larger cuts in benefits. This implies that our baseline estimates understate the positive effects of the reform EP and LFP. Quantitatively, the effect is miniscule, however. To put its magnitude into perspective, we also plot the estimated coefficient $\pm\tilde{\beta}_1$ on the same figure, which highlights that the potential understatement of the effect of the reform is negligible.

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(a) $\tilde{\beta}_1$

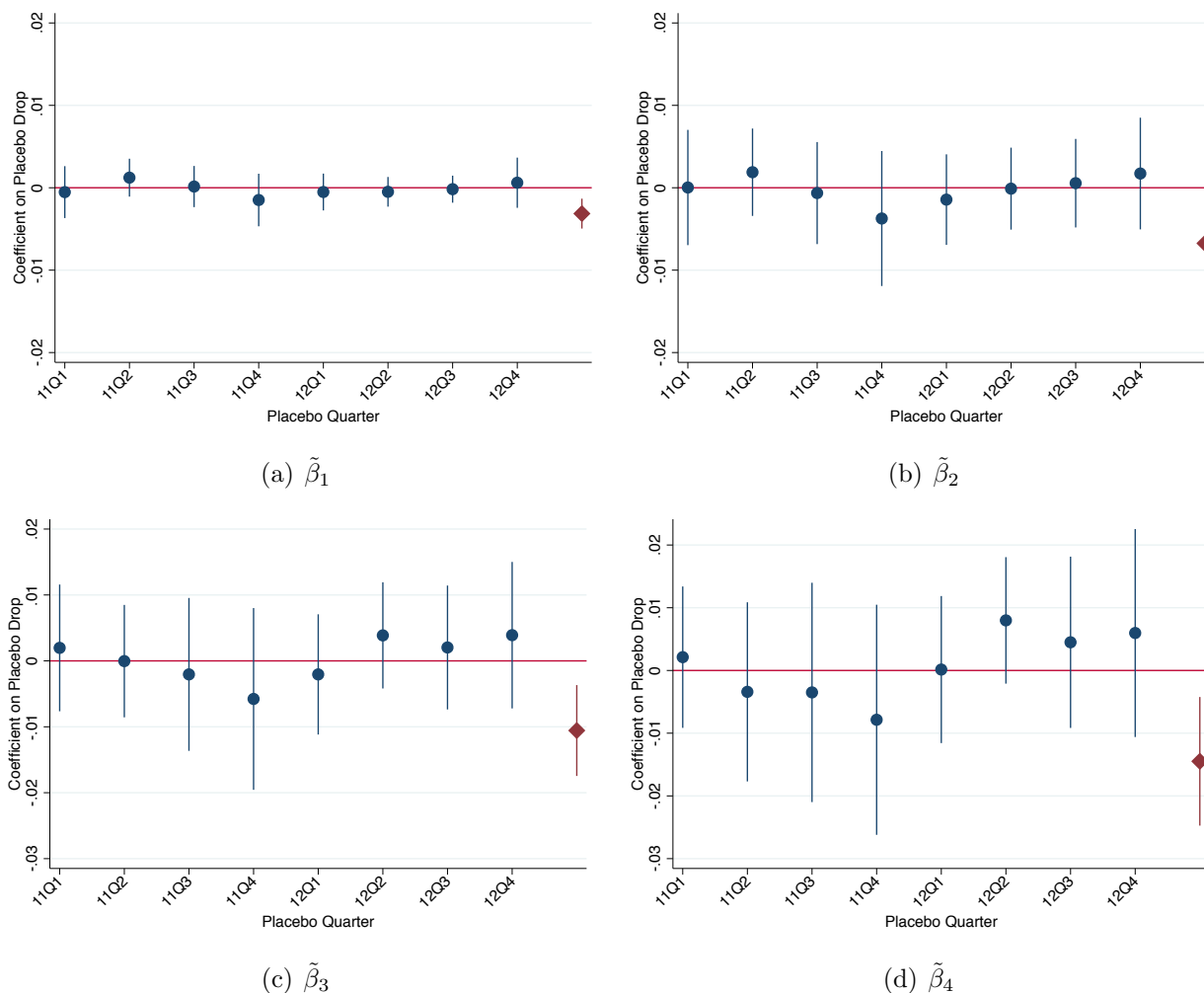(b) $\tilde{\beta}_2$

(c) $\tilde{\beta}_3$

(d) $\tilde{\beta}_4$
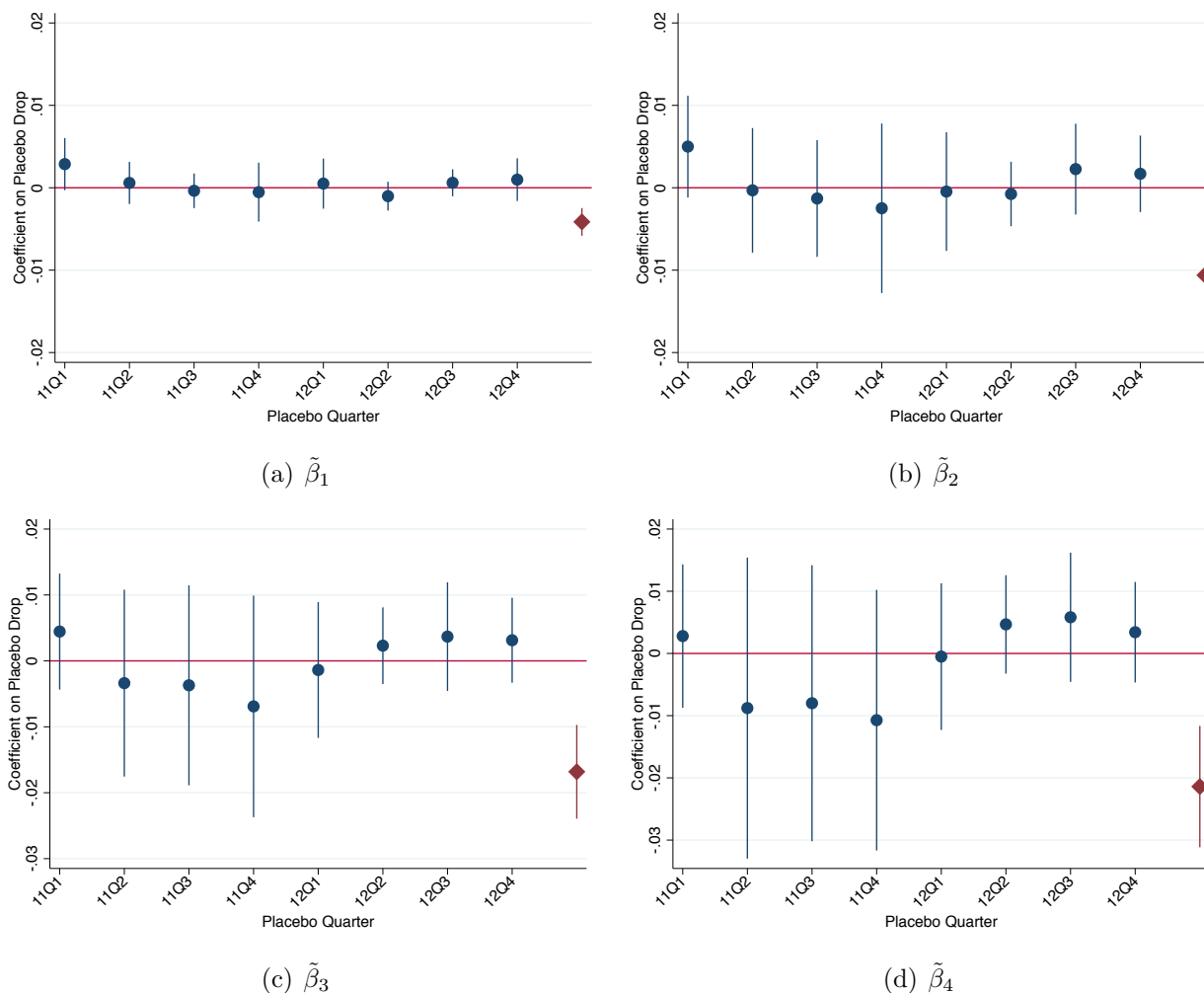
Figure 7: Estimated impacts on employment to population ratio, $\tilde{\beta}_\tau$, of a placebo elimination of benefit extensions in the quarter marked on the horizontal axis. Rightmost point on each panel corresponds to actual reform in 2013q4.

## 4.2 Implications for Aggregate Employment and Labor Force

### 4.2.1 Evidence on Reallocation and Mobility

As discussed above, the degree to which one can rely exclusively on state-level estimates of the effects of unemployment benefit extensions to predict the effects of a nation-wide extensions depends on whether state benefit extensions induce a spatial reallocation of economic activity. In this section we document the extent of such a reallocation.

If the state-level change in employment was driven to an important degree by reallocation, we would expect that benefit extensions have a larger effect on the tradable sector, which can reallocate, than on the non-tradable, which can reallocate to a much lesser degree. Thus, if

(a) $\tilde{\beta}_1$

(b) $\tilde{\beta}_2$

(c) $\tilde{\beta}_3$

(d) $\tilde{\beta}_4$

Figure 8: Estimated impacts on labor force to population ratio, $\tilde{\beta}_\tau$, of a placebo elimination of benefit extensions in the quarter marked on the horizontal axis. Rightmost point on each panel corresponds to actual reform in 2013q4.

there is substantial reallocation of economic activity in response to the cut in benefit duration, we would expect to find a decrease in the ratio of employment in non-tradable to tradable sectors in states with the largest cuts in benefits. To assess this possibility, we apply our empirical methodology to measure the change in employment in sectors producing output that is plausibly non-tradable across states, such as leisure and hospitality, to the change in employment in tradable sectors, such as manufacturing. We find that a cut in benefit duration has no significant effect (a coefficient of $-0.0124$, s.e. $0.0106$) on the relative employment in the two sectors, implying that the null hypothesis of no reallocation induced by benefit extensions cannot be rejected in the data.

| (a) Employment | (b) Labor Force |
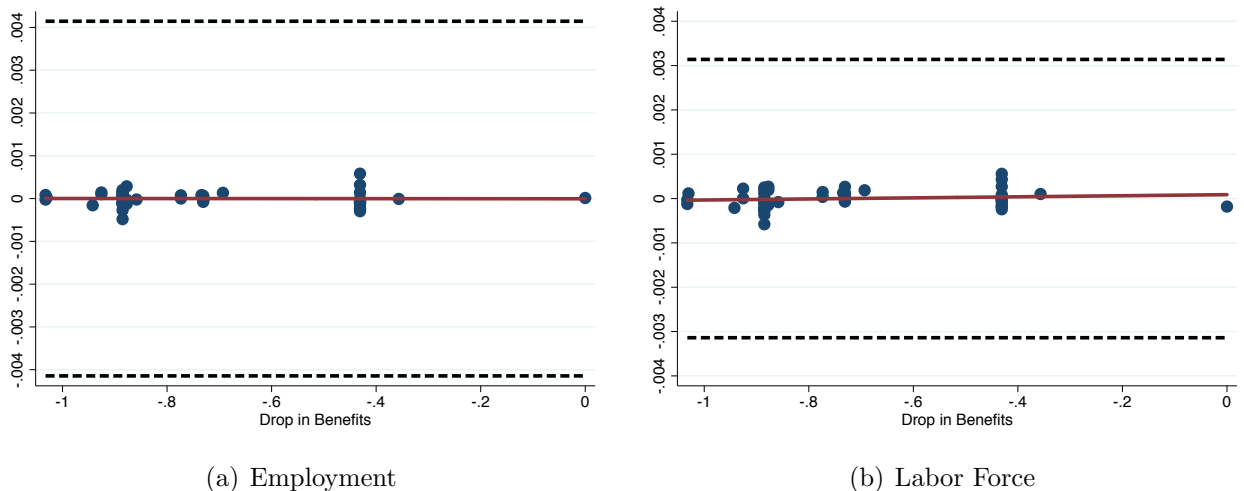
Figure 9: Pre-trends in residuals from baseline specification vs drop in benefit duration between 2013q4 and 2014q1. The dashed lines show the estimated coefficient $\pm\tilde{\beta}_1$.

In addition, Hagedorn et al. (2015) use the Nielsen Consumer Panel Data to measure the responsiveness of cross state border shopping to changes in unemployment benefit generosity. Their results indicate that this effect is also negligible.

Another potential reallocation effect arises because households may live in different states than where they work. Note that this type of worker reallocation would bias even our local estimates if the households systematically change their job search behavior in response to changes in unemployment benefits. For example, suppose households search in states with less generous benefits to take advantage of a higher job-finding rate. As employment is measured based on the place of residence and not on the basis of the location of the job, our estimate of the effect of benefit extensions on employment would be biased *downwards*, since some households residing in high benefit states would face a higher job-finding rate, which would translate into higher employment in their state of residence (despite them actually working in the neighboring state). To investigate whether this is the case, we use direct empirical evidence on where people work relative to where they live. Specifically, we use data from the American Community Survey which is an annual 1% survey of households in the United States conducted by the Census Bureau. The survey contains information on the household's state of residence and state of employment. The share of individuals in our sample who worked in a different state from the one they lived in at the time of the reform is 1.68%. Regressing the difference in log share of individuals working in a different state from the one they live

in on differences in log benefits duration between 2013 and 2014, we find a very small and statistically insignificant coefficient on weeks of benefits available of $-0.038$ (s.e. 0.097).[19] This evidence implies that workers' search behavior does not respond significantly to changes in local unemployment benefit duration.

### 4.2.2 Aggregate Implications of Baseline Empirical Results

The foregoing results that changes in benefit durations induce neither reallocation of economic activity nor worker mobility imply (using the model laid out in Appendix II) that we can rely on our state-level estimate to measure the implications for the aggregate U.S. employment and labor force. Specifically, using the estimate of $\tilde{\beta}_4$ from our benchmark specification in the formulas in Section 3.3, implies that the aggregate employment and labor force have increased by the end of 2014 due to the policy reform by

$$\pi^E = 2,542,625 \qquad \text{and} \qquad \pi^{LF} = 1,846,049. \tag{11}$$

Thus, more than half of the increase in employment was due to the increase in the labor force as a result of the reduction of benefit duration. The remaining increase corresponds to a decrease in the number of unemployed. Our analysis thus shows that the dominant impact of the benefit cut on employment was not driven by a contraction in the labor force – unemployed dropping out of the labor force because they were no longer entitled to benefits – but instead by those previously not participating in the labor market deciding to enter the labor force.

It is also interesting to note that the existing empirical literature has mainly attempted to measure the "micro" effect of unemployment benefit duration on search intensities and job acceptance decisions of individual workers. Clearly, this micro effect is zero for those out-of-labor force who were entitled to benefits neither in 2013 nor in 2014. Yet, it was predominantly movements from out-of-labor force that drove the rise in employment in 2014. Presumably this happened due to a large "macro" effect of the benefit cut on job creation. It is then the availability of jobs that drew non-participants back into the labor force, as is consistent with the standard prediction of labor search models.

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[19]The interpretation is that in response to a cut in benefits from 53 to 25 weeks the share of workers employed in a different state from the one where they live would increase from 1.68% to 1.72%.

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Table 3: Additional Lags

| VARIABLES | $\tilde{\beta}_1$ | $\tilde{\beta}_2$ | $\tilde{\beta}_3$ | $\tilde{\beta}_4$ |
|---|---|---|---|---|
| | | | | |
| | Employment to Population Ratio | | | |
| | | | | |
| 2 Lags | -0.00436*** | -0.0109*** | -0.0170*** | -0.0213*** |
| | (0.000671) | (0.00159) | (0.00268) | (0.00395) |
| 4 Lags | -0.00400*** | -0.0104*** | -0.0165*** | -0.0209*** |
| | (0.000799) | (0.00203) | (0.00331) | (0.00457) |
| 5 Lags | -0.00395*** | -0.0103*** | -0.0163*** | -0.0206*** |
| | (0.000785) | (0.00207) | (0.00350) | (0.00496) |
| 6 Lags | -0.00379*** | -0.0100*** | -0.0159*** | -0.0201*** |
| | (0.000787) | (0.00196) | (0.00319) | (0.00456) |
| | | | | |
| | Labor Force to Population Ratio | | | |
| | | | | |
| 2 Lags | -0.00332*** | -0.00739*** | -0.0117*** | -0.0160*** |
| | (0.000705) | (0.00174) | (0.00334) | (0.00491) |
| 4 Lags | -0.00309*** | -0.00674*** | -0.0108*** | -0.0149*** |
| | (0.000938) | (0.00198) | (0.00343) | (0.00500) |
| 5 Lags | -0.00308*** | -0.00671*** | -0.0107*** | -0.0147*** |
| | (0.000907) | (0.00194) | (0.00343) | (0.00514) |
| 6 Lags | -0.00297*** | -0.00649*** | -0.0104*** | -0.0143*** |
| | (0.000917) | (0.00204) | (0.00347) | (0.00500) |

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

## 4.3 Sensitivity of Baseline Findings

### 4.3.1 Additional Lags

Our analysis of the data reveals that residuals become serially uncorrelated if three or more lags of the dependent variable are included in the specification.[20] We selected the specification with three lags as the benchmark but report in Table 3 that including more lags has little impact on the estimated effects of the cut of benefit duration on employment and the labor

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[20]We select this strategy based on the results of a Monte Carlo study in which we simulate data from the specification in (1) with residuals modeled as an AR(1) process. We then estimate the benchmark specification on these simulated data by choosing the minimal number of lags sufficient to reject that the residuals are serially correlated. We find that this requires estimating additional lags relative to the true underlying DGP. The estimated cumulative effects of treatment, however, are virtually unaffected despite the different estimated lag structure.

force. This indicates that the parsimonious baseline specification is sufficient to control well for the dynamics of the variables of interest.

If we instead rely on the Akaike Information Criterion or the Bayesian (Schwarz) Information Criterion to select the number of lags, we would select only 2 lags. The results corresponding to this specification in Table 3 suggest only a minor impact on the coefficient of interest.

### 4.3.2 Alternative Specifications of State-Specific Time Trends

Controlling for pre-existing state-specific trends in labor force and employment is required to obtain unbiased estimates of policy effects.[21] The traditional specification in the literature that exploits cross-state variation in economic policies (e.g., minimum wages) to infer their impact on employment, typically includes linear state-specific time trends as controls. To assess whether this model of state-level employment dynamics affects our inference of the effect of the change in unemployment benefits, we replace the flexible model of state-specific trends in the benchmark specification with linear state-specific trends $\zeta_i$:

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \zeta_i \times t + \eta_i + \delta_t + \epsilon_{i,t}. \qquad (12)$$

The results of estimating this specification are reported in rows labeled "Linear Trend" in Table 4. The estimated effects are slightly smaller but are not substantively different from those in the benchmark specification.

The fact that the estimated policy effects estimated using this more rigid specification that also uses post-reform data in the estimation are slightly smaller, is consistent with the recent critique of this specification by e.g., Meer and West (2016). Economic theory implies that employment effects of policy changes are not instantaneous so that policy reforms affect the growth rate of employment (at least during the transition). In this case following the traditional approach and estimating state-specific trends will attenuate the estimated policy treatment effect (and inflate standard errors). Or baseline specification avoids these concerns

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[21]However, even downward biased estimates obtained from the specification that does not include any controls for trends, are economically large and statistically significant: $\hat{\bar{\beta}}_4 = -0.014$, s.e. 0.0018 for employment to population ratio and $\hat{\bar{\beta}}_4 = -0.0087$, s.e. 0.0036 for labor force to population ratio.

Table 4: Alternative Specifications of State-Specific Time Trends

| VARIABLES | $\tilde{\beta}_1$ | $\tilde{\beta}_2$ | $\tilde{\beta}_3$ | $\tilde{\beta}_4$ |
|---|---|---|---|---|
| Employment to Population Ratio | | | | |
| Linear Trend | -0.00336*** | -0.00755*** | -0.0124*** | -0.0174*** |
| | (0.000658) | (0.00165) | (0.00297) | (0.00461) |
| 2006 control | -0.00408*** | -0.0104*** | -0.0165*** | -0.0210*** |
| | (0.000877) | (0.00225) | (0.00364) | (0.00492) |
| Both 2013 & 2006 controls | -0.00416*** | -0.0106*** | -0.0169*** | -0.0215*** |
| | (0.000987) | (0.00264) | (0.00473) | (0.00685) |
| Labor Force to Population Ratio | | | | |
| Linear Trend | -0.00267*** | -0.00477*** | -0.00708*** | -0.0103*** |
| | (0.000528) | (0.00141) | (0.00266) | (0.00394) |
| 2006 control | -0.00306*** | -0.00651*** | -0.0101*** | -0.0139*** |
| | (0.000915) | (0.00200) | (0.00348) | (0.00502) |
| Both 2013 & 2006 controls | -0.00363** | -0.00806*** | -0.0129*** | -0.0177*** |
| | (0.00146) | (0.00306) | (0.00462) | (0.00607) |

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

because the inclusion of lagged variables, $\sum_{j=1}^{n} \gamma_j x_{i,t-j}$, already captures the sluggish adjustment of labor market variables emphasized by these authors. Furthermore, our estimation of a flexible state-specific time trend is based on pre-reform values of the outcome variable only.

In the baseline specification, state-specific trends depend on the level of the outcome variable at the end of 2013, i.e., right before the reform. This directly controls for the possibility that the time of the policy reform coincided with the unusual turning point in employment dynamics whereby employment growth unexpectedly accelerated (for reasons unrelated to the reform) in the the states with low employment the eve of the reform.

An alternative that we consider next models state-specific trends as a function of the outcome variable in 2006, i.e., not only pre-reform, but also pre-recession. This addresses the concern that the recession induced different trends across the states, depending on their employment level in 2006 (that reflected, say, the heterogeneous impact of the housing boom). Specifically, we replace $\nu_t \tilde{x}_{i,2013Q4}$ in the baseline specification with $\varsigma_t x_{i,2006}$, i.e., the interaction between the time dummy $\varsigma_t$ with the (cross-sectionally demeaned) average level of the outcome

variable in 2006:

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \varsigma_t x_{i,2006} + \eta_i + \delta_t + \epsilon_{i,t}. \qquad (13)$$

The results, summarized in rows labeled "2006 control" of Table 4, imply that this specification yields very similar estimates to the baseline ones.

Finally, we combine the preceding specification with the baseline one,

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \nu_t x_{i,2013Q4} + \varsigma_t x_{i,2006} + \eta_i + \delta_t + \epsilon_{i,t}. \quad (14)$$

Despite the added flexibility in this model of state-specific trends in labor force and employment, the estimated coefficients of interest remain little changed, as reported in rows labeled "Both 2013 & 2006 controls" of Table 4.

## 4.4 Analysis at a Finer Level of Geographic Variation

A prominent approach in the empirical analysis of the effects of policies is to compare states bordering each other (e.g. New Jersey and Pennsylvania) but having different policies. The idea is that many of the shocks, e.g., weather conditions, affect neighboring states similarly. So far, in our state-based panel analysis, we had to model the impact of such shocks. However, the border state design allows us to control for those common shocks by either differencing between bordering states or using a bordering state time dummy. We now consider such a specification:

$$x_{i,p,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,p,t-j} + \nu_t \tilde{x}_{i,p,2013Q4} + \eta_{i,p,t} + \epsilon_{i,p,t}, \qquad (15)$$

where $\eta_{i,p,t}$ is the border-pair by time dummy.[22] The effects of the cut in benefit duration on labor force and employment estimated using this specification are reported in Table 5. Their similarity to baseline estimates reinforces the conclusion that the benchmark specification includes an adequate model of employment and labor force dynamics.

Another prominent empirical research strategy in the literature, followed in the context of

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[22]Alaska and Hawaii are not adjacent to other states and are effectively excluded from this analysis.

Table 5: Results Based on Border State Specification

| VARIABLES | $\tilde{\beta}_1$ | $\tilde{\beta}_2$ | $\tilde{\beta}_3$ | $\tilde{\beta}_4$ |
|---|---|---|---|---|
| EP | -0.00497*** | -0.0111*** | -0.0154*** | -0.0177*** |
|    | (0.000909) | (0.00221) | (0.00314) | (0.00381) |
| LFP | -0.00357*** | -0.00720*** | -0.00936*** | -0.0112*** |
|     | (0.00101) | (0.00194) | (0.00259) | (0.00315) |

Robust standard errors clustered by state, state pair, and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

unemployment insurance by Hagedorn et al. (2013), involves comparisons between counties that belong to different states but border each other. Their motivation for conducting the analysis at the county rather than the state level is that it helps overcome the challenge of policy endogeneity at the state level. Unemployment insurance policies are generally determined at the state level and respond to a state's economic condition, complicating the identification of the effect of policies on economic conditions. The county-based analysis helps mitigate this difficulty because state policies are less likely to be driven by economic conditions in a given county. State policy endogeneity, however, is not a concern for this study because the policy reform we consider was exogenous to economic conditions of individual states as it applied nationwide. The only challenge for this paper is to ensure that the estimated effects of the reform are not contaminated by pre-existing trends in labor market variables. The border county setup could help in this regard by eliminating the influence of those shocks that affect border counties similarly, potentially making it easier to estimate the remaining trends (although at the cost of introducing some noise due to higher measurement error in county-level data). The forgoing results indicate, however, that this is unnecessary in our setting because the model of state-level dynamics does a very good job describing employment and labor force trends.[23]

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[23]The border-county based strategy cannot be pursued with the latest version of LAUS data that is used in this paper. This is because the 2015 redesign of LAUS provided a historical update to state-level data but not to the county-level data. The results based on the 2014 version of county-level LAUS data were reported in the previous version of this paper and are similar to the results based on states and state borders reported in this draft.

# 5 Unemployment Benefit Extensions and QCEW Payroll Employment

The traditional approach, at least in the macroeconomics literature, to measuring the aggregate effects of policies on employment, defines the latter variable as including all individuals who did any work for pay or profit during a given week. For example, when measuring the aggregate effects, the literature usually does not draw a distinction whether the increase in employment was due to more individuals becoming employees or starting their own businesses. The object of interest is the change in the total number of individuals supplying labor in the market in response to a policy change. This is the established definition of employment adopted by the Current Population Survey and it corresponds to the measure of employment used so far in this paper. The disadvantage of this measure of employment is that some components of employment have to be measured through surveys that are subject to sampling error.

A more narrow notion of employment can, however, be measured through administrative records. These data are called Quarterly Census of Employment and Wages (QCEW) and represent the count of jobs for which a paycheck subject to a UI tax was issued. Due to the nature of these data, this employment measure counts the number of jobs rather than the number of individuals with at least one job so that the same individual may be counted multiple times if he or she receives payments from multiple employers. Moreover, the data excludes most jobs not subject to the UI tax, such as self employed workers, unpaid family workers or employees of schools affiliated with religious organizations, railroad employees, etc. as well as jobs excluded for other reasons, such as employees of national security agencies.

It is well documented that these two measures of employment often diverge significantly even after accounting for the differences in coverage.[24] Of a particular concern to the period we study is the sharp rise in non-traditional employment, or what has become known as the rise of "1099 economy" (the IRS form 1099-MISC must be submitted by all "employers" who pay someone $600 or more a year in nonemployee compensation). Dourado and Koopman (2015) document a sharp rise in the number of these forms submitted to the IRS in recent years while Abraham et al. (2017) consider additional evidence. For example, an Uber driver would be

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[24]See Hagedorn and Manovskii (2011) for a discussion and additional references.

Table 6: QCEW Results, Payroll Employment to Population Ratio

| VARIABLES | $\tilde{\beta}_1$ | $\tilde{\beta}_2$ | $\tilde{\beta}_3$ | $\tilde{\beta}_4$ |
|---|---|---|---|---|
| States | -0.00244*** | -0.00490** | -0.00827*** | -0.0132*** |
|  | (0.000939) | (0.00197) | (0.00311) | (0.00417) |
| Border States | -0.00383*** | -0.00628*** | -0.00994*** | -0.0133*** |
|  | (0.000684) | (0.00114) | (0.00169) | (0.00209) |

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

paid this way. He or she will be classified as being employed according to the CPS definition but will not appear in the QCEW data. An additional complication presented by this rapid ongoing change in the labor market is that Uber drivers may not even classify themselves as being self-employed but to consider themselves as being employed by Uber when replying to the survey. This makes it challenging to interpret the data on self-employment and to use them in conjunction with QCEW data to obtain the picture of total employment changes.

Nevertheless, as QCEW data refer to a well defined segment of employment and are not affected by sampling error, it appears interesting to assess the effects of the cut in benefits on payroll counts as measured by the QCEW. Accordingly, we repeat the analysis above using these data. QCEW data contain non-seasonally adjusted monthly payroll employment counts. We seasonally adjust the monthly series using the X-12 ARIMA procedure. We then aggregate the monthly seasonally adjusted series to quarterly aggregates and divide by the previously constructed population measure.

Table 6 contains the results. We find that these data also reveal a significant positive impact of the reduction in benefit duration on payroll employment. Specifically, the implied $\pi^E = 1,326,187$ so that the policy reform accounted for over 43% of the growth in payroll employment in 2014. The magnitude of the effect of the policy reform on payroll employment is somewhat smaller than its impact on total employment, but the foregoing discussion illustrates the difficulties in interpreting this difference. For example, it might be that non-traditional employment was particularly sensitive to the cut in benefits. Alternatively, it might be that holders of several part-time jobs secured full-time employment as a result of increased job availability following the EUC08 expiration which would be recorded as a decline in the number of jobs in the QCEW.

# 6  Conclusion

In this paper we measure the effect of unemployment benefit extensions on total employment and the labor force. Following the aftermath of the Great Recession, by December 2013 there was wide heterogeneity of federally-financed durations of benefits across U.S. states, ranging from 0 to 47 weeks on top of the regular state-funded benefitss. In December 2013 the U.S. Congress abruptly and immediately eliminated all federal unemployment benefit extensions. The particular usefulness of this policy change for understanding the employment effects of benefit extensions stems from the fact that the policy change at the national level was exogenous to economic conditions of individual states. The available benefit duration in a given state just prior to the reform, however, was endogenous to the economic conditions of the state. Thus the key challenge to a proper inference of the effects of benefits is to ensure that the effects are not confounded by pre-existing differences in employment or labor force dynamics.

The classic findings in Blanchard and Katz (1992) imply that state-level employment follows a highly persistent process, suggesting that pre-trends induced by mean-reversion in state economic conditions are unlikely to play a very important role. A simple descriptive analysis confirms this to be the case in the period we study. Specifically, we document a significant acceleration of EP and LFP growth in states that experienced larger benefit cuts induced by the reform (and thus had higher benefit duration just prior to the reform). The acceleration is quantitatively the same when measured relative to the trend in 2013, or 2012-2013, or 2011-2013, etc. In other words, this implies that pre-reform dynamics are quite stable with little evidence of mean-reversion prior to the reform. Even more reassuringly, we find no evidence of acceleration in EP and LFP growth in high benefit states in pre-reform time periods (when the reform did not actually take place). This once again suggests that the acceleration of EP and LFP growth of high benefit duration states is not a typical feature of the data. Instead, it only happened when benefit durations where exogenously cut in December 2013.

Our formal analysis is based on a rich but parsimonious model of employment and labor force dynamics. Among other things, it allows for mean reversion by measuring the autoregressive components in EP and LFP. Moreover, it allows for the possibility that the time of

the policy reform coincided with the unusual turning point in employment dynamics, whereby employment and labor force growth accelerated more in states experiencing particularly severe lingering effects of the Great Recession by the end of 2013. We find that this model is successful empirically and that there are no significant state-level pre-trends in the residuals of this specification in relation to the size of the benefit duration cut due to the reform. Thus, the common trend assumption required for the validity of our analysis based on the regression discontinuity in the time domain is satisfied.

The results of the formal analysis reveal that changes in unemployment benefits have large and statistically significant effects: a 1 percent drop in benefit duration increases four-quarter-ahead state employment by 0.02 log point and state labor force by 0.014 log points. While these state-level estimates are of independent interest, it is also desirable to be able to use them to infer the effects of a nation-wide policy change. We document several empirical facts and provide a simple model that guides the aggregation of these effects. We find that the cut in benefit duration accounted for about 75 percent of the aggregate employment growth in 2014. Over half of the aggregate employment growth was due the increase in the labor force induced by the policy reform. Considering alternative models of state-level dynamics does not substantively affect the conclusion that the benefit duration cut led to significant employment gains and an increase in the labor force. This is in contrast to the estimated effects of placebo reforms which are generally economically small and statistically insignificant.

While our focus in the paper is on total employment and the labor force for which the only data source is the LAUS program of the BLS, we also replicate the analysis using the payroll counts from the QCEW. The two data sets are different in terms of the notion of employment and in terms of coverage. The conclusions they lead to are nevertheless similar.

Our empirical approach is agnostic about the channels which cause employment and labor force participation to increase. Instead of measuring one specific channel, our objective is to measure the aggregate total effect of all potential channels at the same time, including equilibrium effects. In particular, our measure also includes the effect of changes in demand on employment and the labor force, while remaining agnostic about the reason for the change in demand. For example, it allows for a drop in demand (e.g. in nontradables) across states due to the cut in benefit payments as well as an increase in demand as a result of an increase in employment due to the cut in benefit duration.

While we did not impose any theoretical restrictions of a particular labor market model on our empirical analysis, the findings are consistent with the predictions of the standard equilibrium labor market search model. For example, the primary labor market effect of a cut in unemployment benefit duration in the framework of Mortensen and Pissarides (1994) is the positive impact on job creation (using such a framework, Mitman and Rabinovich (2013) find a quantitatively similar effect of the expiration of benefits on employment in 2014 to the one found in this paper). It is this rise in job creation that leads in equilibrium to the increase in employment. It is also consistent with standard search models that an increase in job availability draws non-participants (who are not eligible for benefits either before or after the reform) into the labor market leading to a positive effect on the labor force (for a textbook treatment see Chapter 7 in Pissarides (2000)), even outweighing the potentially countervailing effect of some unemployed leaving the labor force after losing eligibility for benefits. Remarkably though, such aggregate labor market implications of unemployment benefit policies have been largely neglected by the empirical literature. The findings in this paper suggest that future research and policy analyses should take these aggregate implications into account.

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# I Aggregation: A Simple Trade Model of the US

In this section we show that we can use our estimates based on (border) states to derive the implications for the induced employment changes for the aggregate U.S. economy. To this aim, we develop a standard model where we show that our aggregation methodology of Section 3.3 is exact. In this model each state is an open economy inside the (closed) US economy. The labor market in each state is governed by a Mortensen Pissarides search and matching model. Each state produces (and consumes) a nontradable and a tradable good. Both sectors, the one producing the tradable good and the one producing the nontradable one, operate in the same labor market and are subject to the same labor market frictions as in a standard Mortensen Pissarides model. The evidence provided in Section 4.2.1 implies that the unemployed do not change where to search for a job in response to changes in benefits. In the model, which we use to study a policy change in benefits, we therefore assume that unemployed search for jobs in their own state only.

Specifically, each state is described by a discrete time two sector version of the Pissarides (1985, 2000) search and matching model. There is a measure one of infinitely lived workers and a continuum of infinitely lived firms. Workers maximize their expected lifetime utility:

$$\mathbb{E} \sum_{t=0}^{\infty} \delta^t y_t, \tag{A1}$$

where $y_t$ represents income in period $t$ and $\delta \in (0, 1)$ is workers' and firms' common discount factor. We denote the sectors producing tradable and non-tradable goods by $\Omega \in \{T, NT\}$. Firms in both the tradable ($\Omega = T$) and the non-tradable ($\Omega = NT$) sector have a constant returns to scale production technology that uses labor as the only input (Pissarides (2000) shows that capital can be added to the model leaving all equations unchanged).

Output of each unit of labor in sector $\Omega$ is denoted by $A^{\Omega}$. There is free entry of firms into both sectors. Firms attract unemployed workers by posting a vacancy at the flow cost $c$. The price of the tradable good is normalized to one ($p^T = 1$) and the price of the non-tradable is denoted $p^{NT}$. Once matched, workers and firms separate exogenously with probability $s$

per period. Employed workers in sector $\Omega$ are paid a wage $w^\Omega$, and firms in sector $\Omega$ make accounting profits $p^\Omega A^\Omega - w^\Omega$ per worker each period in which they operate. Unemployed workers get flow utility $z$ from leisure/non-market activity. Unemployed workers can search in either one of the two sectors. In equilibrium they are indifferent in which sector to search. Workers and firms split the surplus from a match according to the generalized Nash bargaining solution. The bargaining power of workers is $\beta \in (0,1)$.

Let $u^\Omega$ denote the unemployed searching in sector $\Omega$, $e^\Omega$ employment in sector $\Omega$ and $v^\Omega$ the number of vacancies posted in sector $\Omega$. We refer to $\theta^\Omega = v^\Omega/u^\Omega$ as the market tightness in sector $\Omega$. The number of new matches in each sector is given by a constant returns to scale matching function $m(u^\Omega, v^\Omega)$. Employment in each sector evolves according to the following law of motion:

$$n_{t+1}^\Omega = (1-s)n_t^\Omega + m(u_t^\Omega, v_t^\Omega). \tag{A2}$$

The probability for an unemployed worker searching in sector $\Omega$ to be matched with a vacancy next period equals $f(\theta_t^\Omega) = m(u_t^\Omega, v_t^\Omega)/u_t^\Omega = m(1, \theta_t^\Omega)$. The probability for a vacancy in sector $\Omega$ to be filled next period equals $q(\theta_t^\Omega) = m(u_t^\Omega, v_t^\Omega)/v_t^\Omega = m(1/\theta_t^\Omega, 1) = f(\theta_t^\Omega)/\theta_t^\Omega$. We restrict $m(u_t^\Omega, v_t^\Omega) \le \min(u_t^\Omega, v_t^\Omega)$.

In each sector $\Omega$, denote the firm's value of a job (a filled vacancy) by $J^\Omega$, the firm's value of an unfilled vacancy by $V^\Omega$, the worker's value of having a job by $W^\Omega$, and the worker's value of being unemployed and searching in sector $\Omega$ by $U^\Omega$.

$$
\begin{align}
J^\Omega &= p^\Omega A^\Omega - w^\Omega + \delta(1-s)J^{\Omega'} \tag{A3} \\
V^\Omega &= -c + \delta q(\theta^\Omega)J^{\Omega'} \tag{A4} \\
U^\Omega &= z + \delta\{f(\theta^\Omega)W^\Omega + (1-f(\theta^\Omega))U^{\Omega'}\} \tag{A5} \\
W^\Omega &= w^\Omega + \delta\{(1-s)W^{\Omega'} + sU^{\Omega'}\}. \tag{A6}
\end{align}
$$

The interpretation is straightforward. Operating firms earn profits $p^\Omega A^\Omega - w^\Omega$ and the matches are exogenously destroyed with probability $s$. A vacancy costs $c$ and is matched with a worker (becomes productive next period) with probability $q(\theta^\Omega)$. An unemployed worker derives utility $z$ and finds a job next period with probability $f(\theta^\Omega)$. An employed worker

earns wage $w^\Omega$ but may lose her job with probability $s$ and become unemployed next period.

Nash bargaining with worker bargaining power $\beta$ implies that a worker and a firm split the surplus $S^\Omega = J^\Omega + W^\Omega - U^\Omega$ such that

$$J^\Omega = (1 - \beta)S \tag{A7}$$

$$W^\Omega - U^\Omega = \beta S^\Omega. \tag{A8}$$

Free entry implies that the value of posting a vacancy is zero: $V^\Omega = 0$ and, therefore,

$$c = \delta q(\theta^\Omega)(1 - \beta)S^{\Omega'}. \tag{A9}$$

As shown in Hagedorn and Manovskii (2008), the steady state surplus equals

$$S^\Omega = \frac{p^\Omega A^\Omega - z}{1 - \delta(1 - s) + \delta f(\theta^\Omega)\beta}. \tag{A10}$$

Plugging this into the free entry condition yields:

$$\frac{p^\Omega A^\Omega - z}{1 - \delta(1 - s) + \delta f(\theta^\Omega)\beta} = \frac{c}{\delta q(\theta^\Omega)(1 - \beta)}, \tag{A11}$$

and, equivalently,

$$\frac{1 - \delta(1 - s)}{\delta q(\theta^\Omega)} + \beta\theta^\Omega = \frac{p^\Omega A^\Omega - z}{c}(1 - \beta). \tag{A12}$$

Since unemployed workers are indifferent between searching in the tradable or in the non-tradable sector, $U^T = U^{NT}$, which implies that $A^T = p^{NT}A^{NT}$ (where we used the normalization $p^T = 1$), $\theta^T = \theta^{NT}$ and $w^T = w^{NT}$. To see this, suppose $A^T \neq p^{NT}A^{NT}$, say $A^T > p^{NT}A^{NT}$. Then, the above equations imply that $\theta^T > \theta^{NT}$ and since wages equal

$$w^T = \beta A^T + (1 - \beta)z + c\beta\theta^T \tag{A13}$$

$$w^{NT} = \beta p^{NT}A^{NT} + (1 - \beta)z + c\beta\theta^{NT}, \tag{A14}$$

we also have $w^T > w^{NT}$. As a a result, $U^T > U^{NT}$, implying by contradiction that $A^T = p^{NT}A^{NT}$, which then implies $\theta^T = \theta^{NT}$ and $w^T = w^{NT}$.

This implies that the employment rate in state $i$ equals $e_i = \frac{f(\theta_i)}{s + f(\theta_i)}$, where $\theta_i = \theta^T = \theta^{NT}$.

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Employment in the U.S. then equals

$$E = \sum_{\text{All U.S. states } i} e_i L_i, \tag{A15}$$

where $L_i$ is labor force in state $i$ (= population in state $i$ in the simple Mortensen Pissarides model we use here. Below we discuss how to extend the analysis to allow for an endogenous labor force).

In the empirical analysis we compare the employment population ratio $e_i$ for state $i$ before and after the reform, and obtain the difference in log employment $\log(e_i^{after}) - \log(e_i^{before}) + \log(L_i^{after}) - \log(L_i^{before})$, where in this simple model the labor force $L_i$ is fixed. Our regression then delivers an estimate of the change in employment in state $i$ w.r.t. an increase in benefit duration in state $i$, since employment in state $i$ does not depend on the benefit level in other states. To see this, note that since $\theta_i$ solves

$$\frac{1 - \delta(1 - s)}{\delta q(\theta_i)} + \beta \theta_i = \frac{A^{T,i} - z_i}{c}(1 - \beta), \tag{A16}$$

it just responds to changes in $z_i$ but not to changes in $z_j$ in some other state $j$.[25] The increase in the employment rate (=employment population ratio) in state $i$ is

$$\mu_i^E = \tilde{\beta}_4(b_i^{2014Q4} - b_i^{2013Q4})e_i^{2013Q4}, \tag{A17}$$

where where $b_i^{2013Q4}$ and $b_i^{2014Q4}$ denote the logarithm of the number of weeks of benefits available in state $i$ in 2013Q4 (just prior to the policy change) and in 2014Q4 and $\tilde{\beta}_4$ is the estimated cumulative effect.

The previous analysis then implies that we can aggregate these employment changes for states $i$. We compute the policy induced change in employment for the whole U.S. as

$$\pi^E = \sum_{\text{All U.S. states } i} \mu_i^E L_i^{2013Q4}. \tag{A18}$$

The same derivations for employment can be applied to the labor force as well. Indeed,

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[25]In the empirical analysis we also compare two neighboring states $i$ and $j$. The same arguments apply to this setup as well. The difference in log employment is $\log(e_i) - \log(e_j) + \log(L_i) - \log(L_j)$. Our regression then delivers an estimate of the elasticity of employment in state $i$ w.r.t. an increase benefit duration in state $i$, since employment in state $i$ does not depend on the benefit duration in other states.

Pissarides (2000) shows in a more complex model, where heterogenous households take a participation decision, that the labor force can be written as a function of market tightness as well. The same derivations as above in the more elaborated model imply that labor force in a state depends on market tightness in that state only (and not on market tightness in other counties). Our regression then delivers again an estimate of the elasticity of the labor force in state $i$ w.r.t. an increase benefit duration in state $i$ (since labor force in state $i$ does not depend on the benefit level in other counties). And we can again use this estimate at the state level to compute the percentage change of the aggregate labor force as well as the change in the total number of labor force participants.

# II    Aggregate U.S. Labor Market Performance
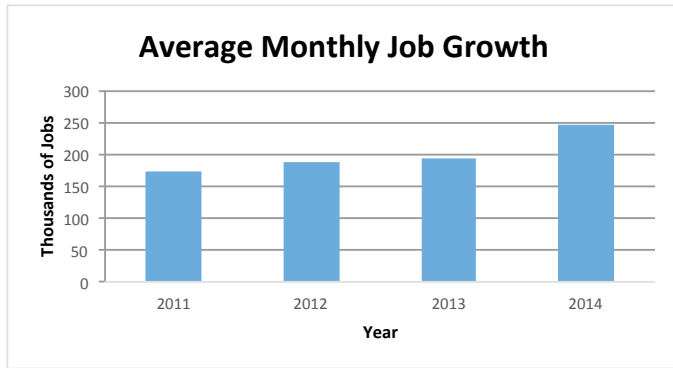


(a) Monthly Job Growth.
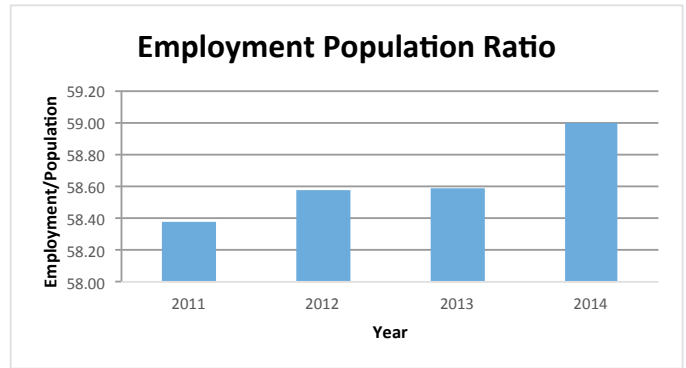
(b) Employment to Population Ratio.
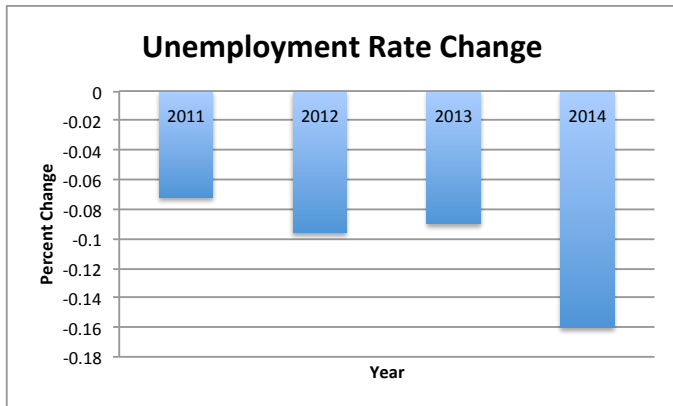
(c) Unemployment Rate Change.
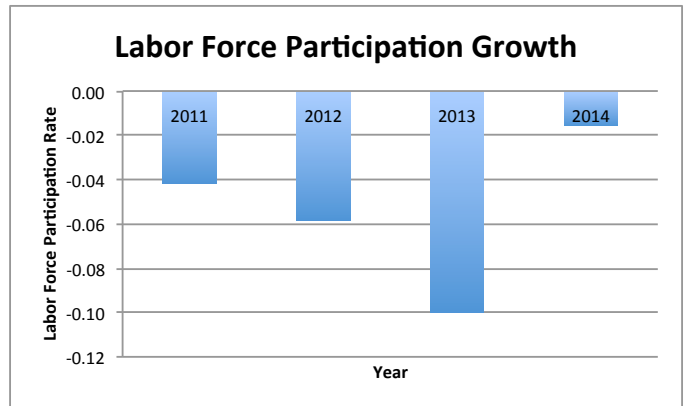
(d) Labor Force Participation Growth.
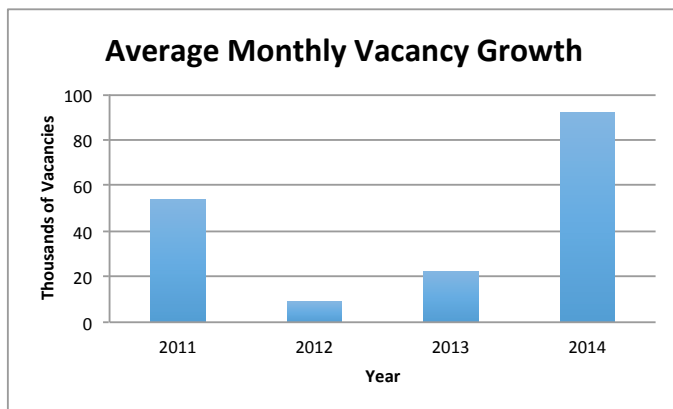
(e) Monthly Vacancy Growth.
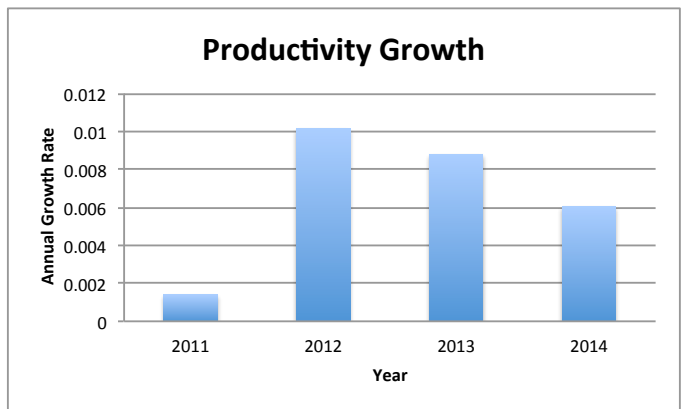
(f) Productivity Growth.

Figure A-1: U.S. Labor Market Performance in 2014.

Note - Data series downloaded from the Bureau of Labor Statistics website http://www.bls.gov/data/ on 01/09/2015 with the following series identifiers:
Panel (a) - CES0000000001, Panel (b) - LNS12300000, Panel (c) - LNS14000000,
Panel (d) - LNS11300000, Panel (e) - JTS00000000JOL, Panel (f) - PRS85006093.

# III   Scatter Plots Underlying Placebo Experiments in Figure 3 in the Main Text
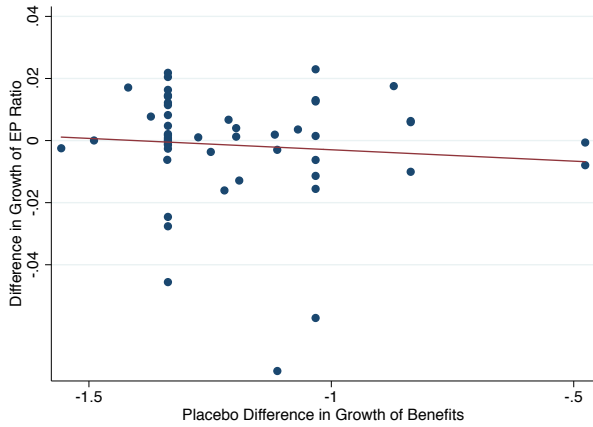


(a) Employment States
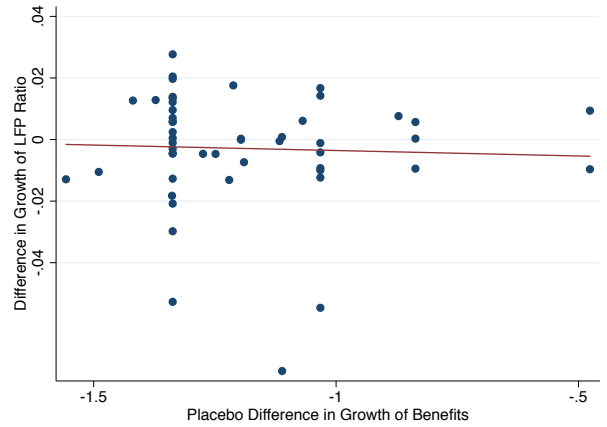
(b) Labor Force States
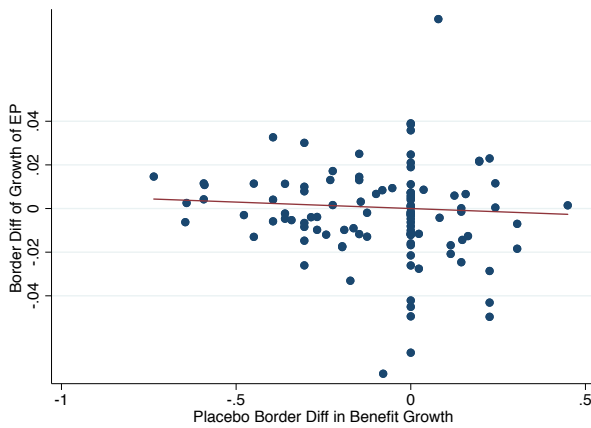
(c) Employment State Borders
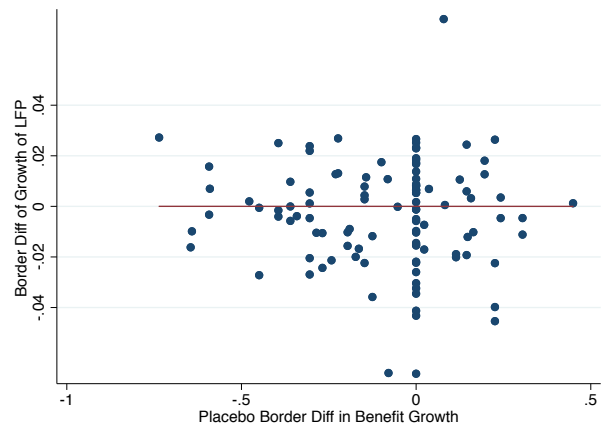
(d) Labor Force State Borders

Figure A-2: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2011Q1 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.
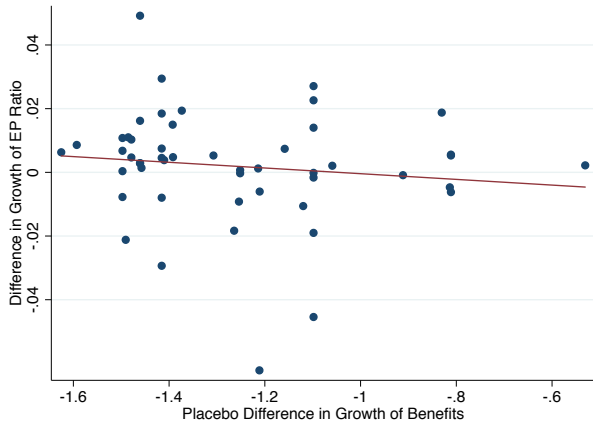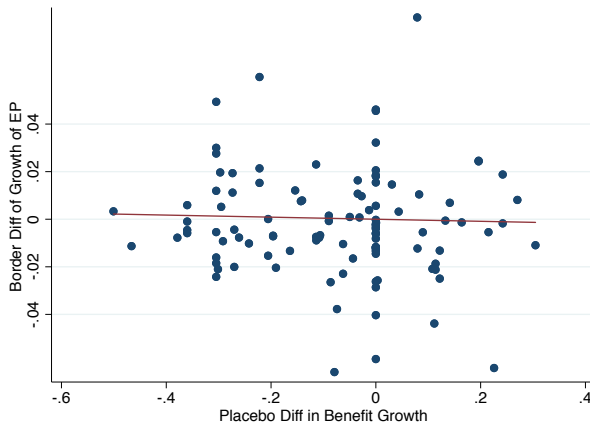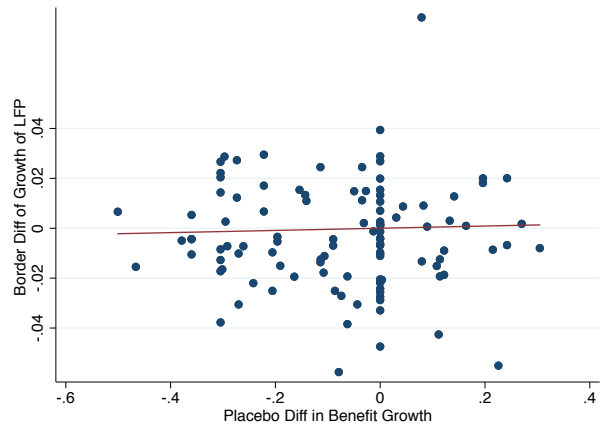
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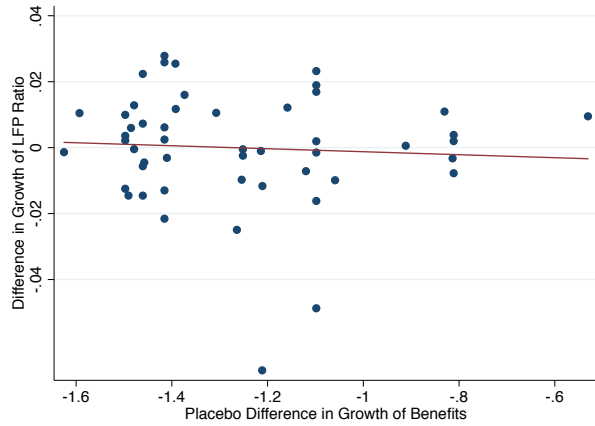
(b) Labor Force States
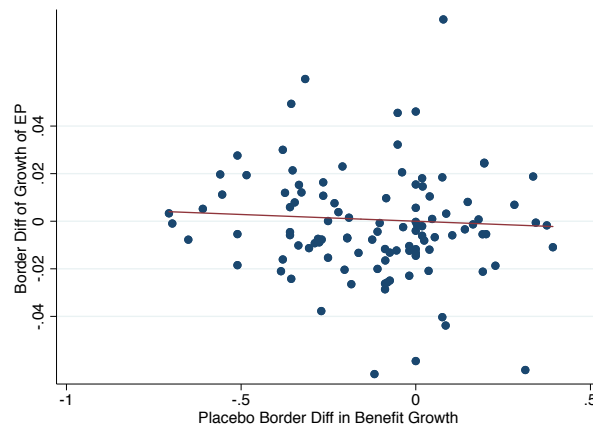
(c) Employment State Borders
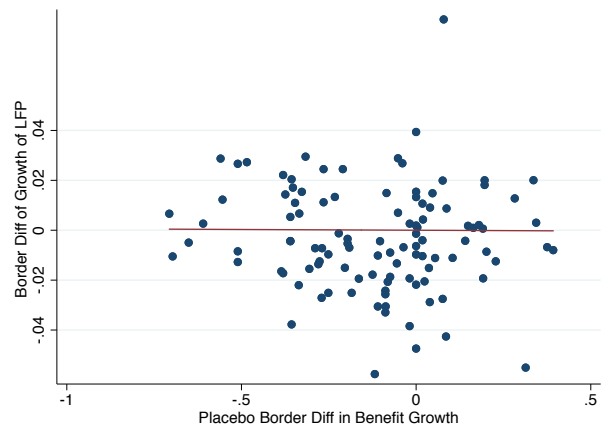
(d) Labor Force State Borders

Figure A-3: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2011Q2 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.
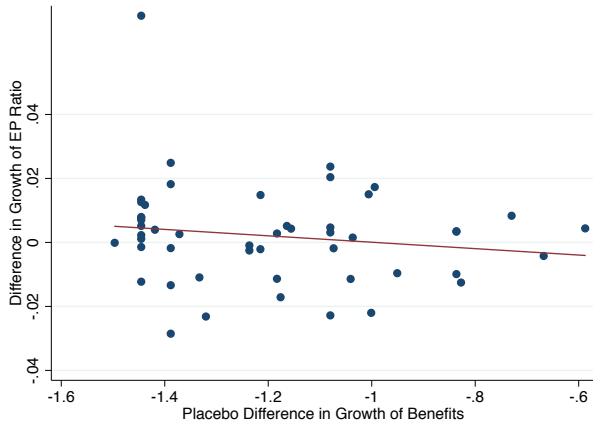
(a) Employment States  (b) Labor Force States
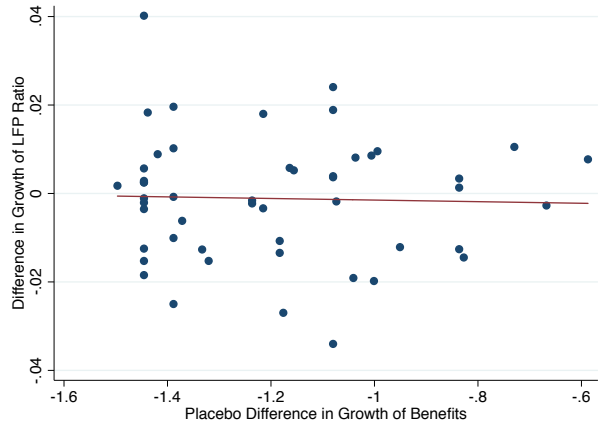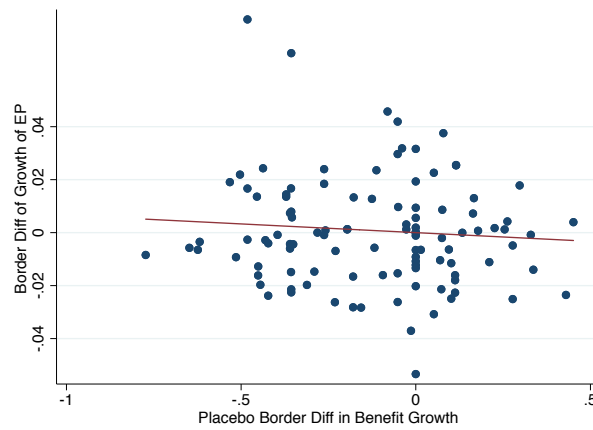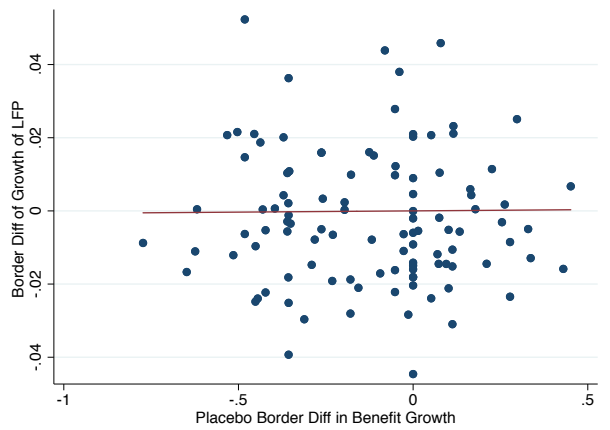
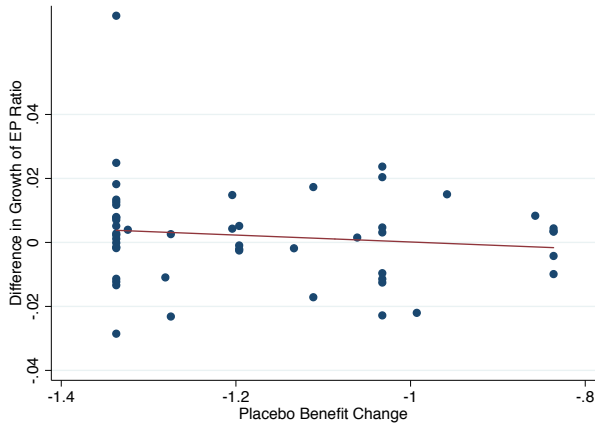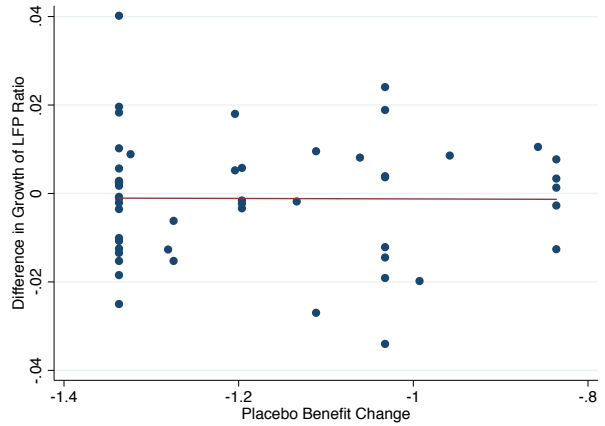(c) Employment State Borders  (d) Labor Force State Borders

Figure A-4: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2011Q3 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.
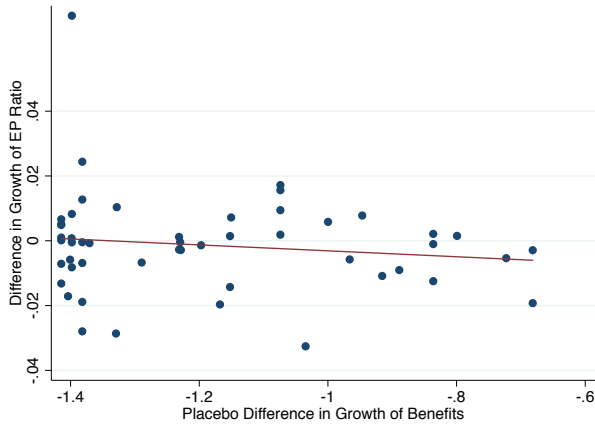
(a) Employment States
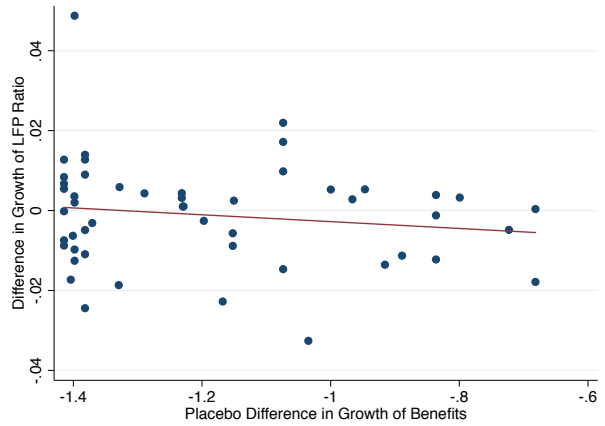
(b) Labor Force States
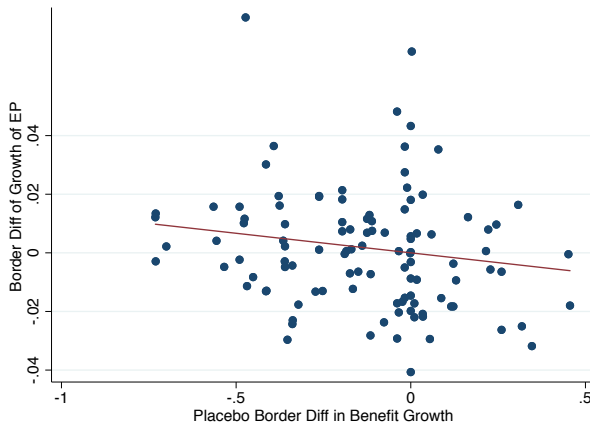
(c) Employment State Borders
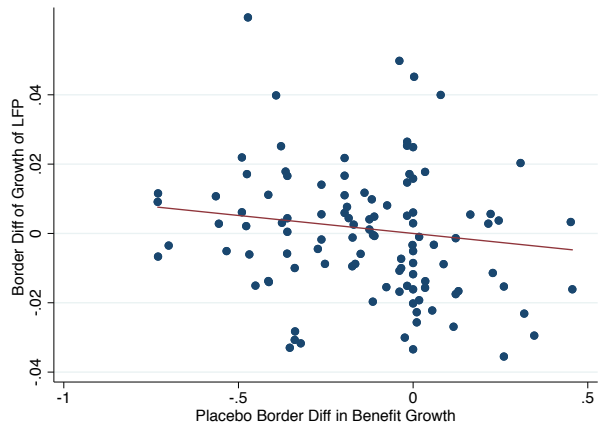
(d) Labor Force State Borders

Figure A-5: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2011Q4 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.
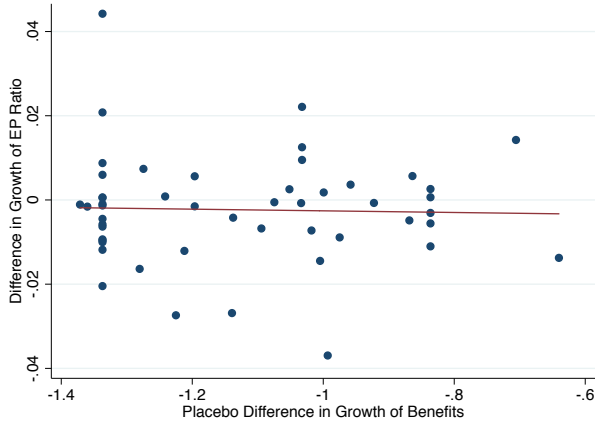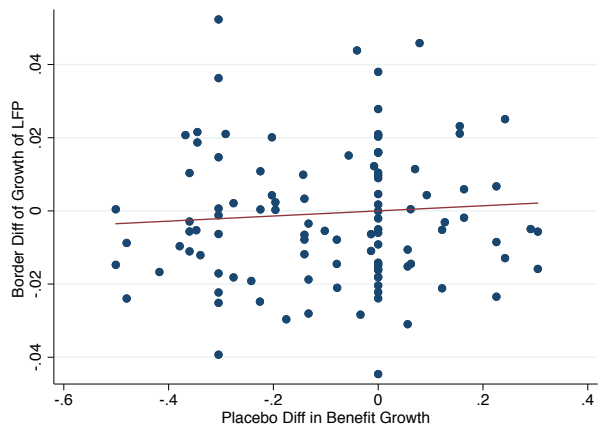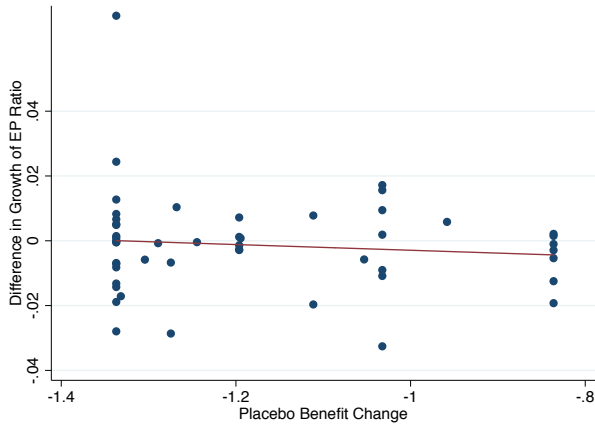
(a) Employment States
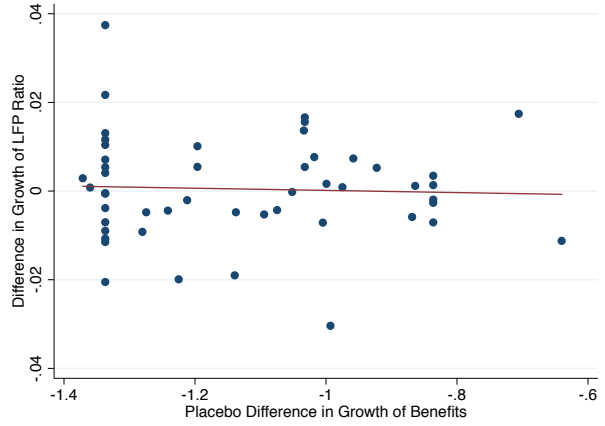
(b) Labor Force States
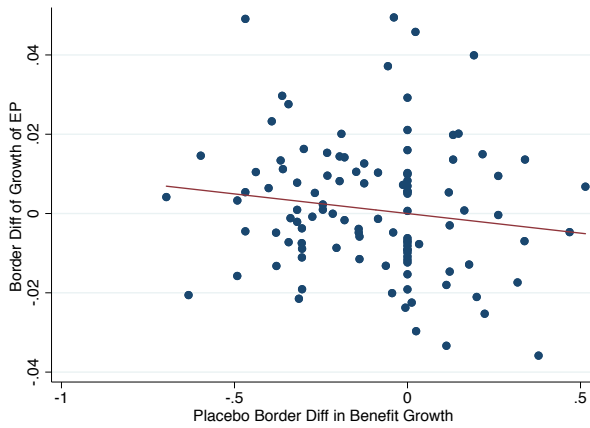
(c) Employment State Borders
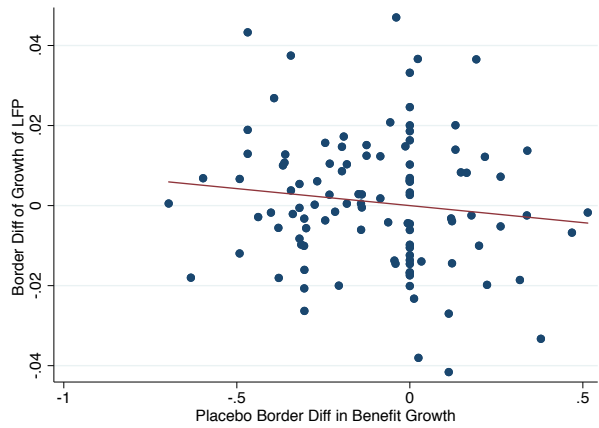
(d) Labor Force State Borders

Figure A-6: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2012Q1 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.
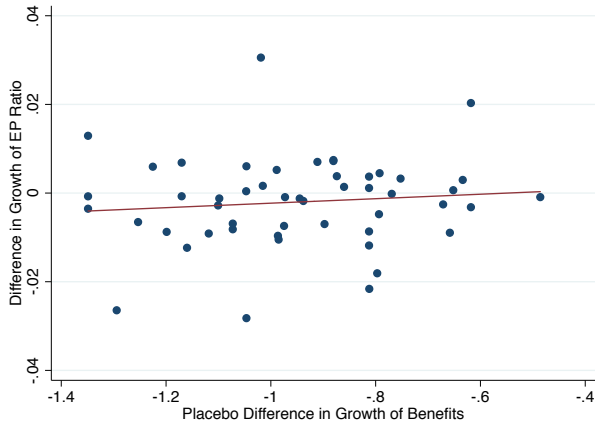
(a) Employment States
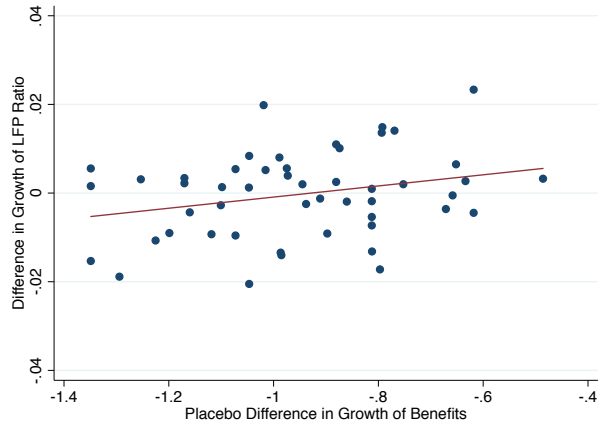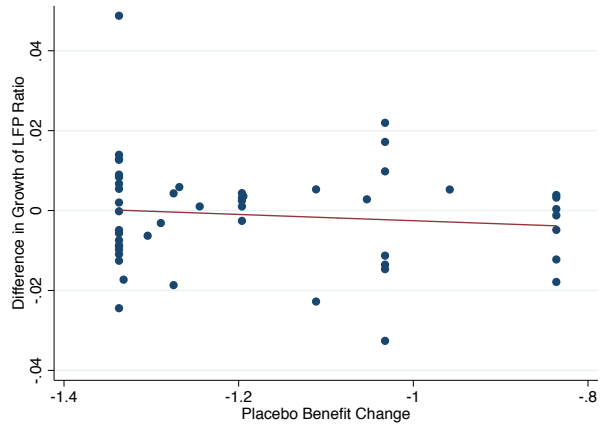
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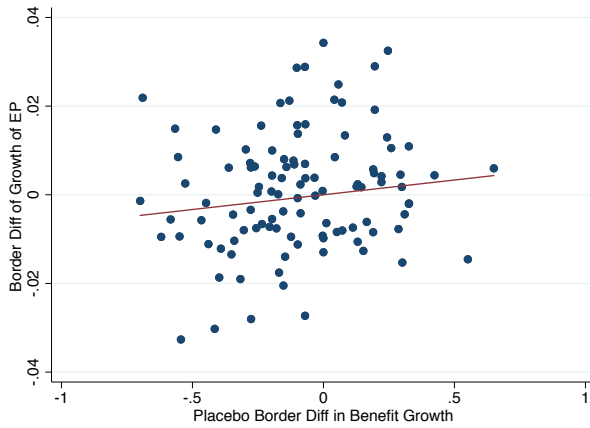
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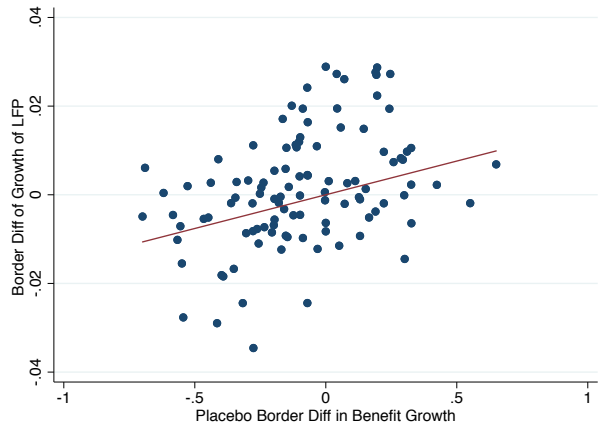
(d) Labor Force State Borders

Figure A-7: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2012Q2 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.
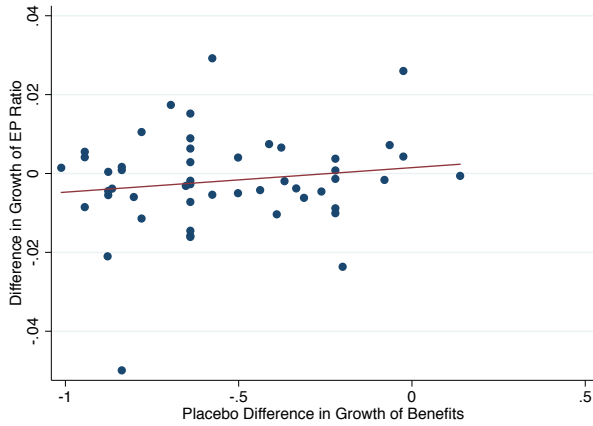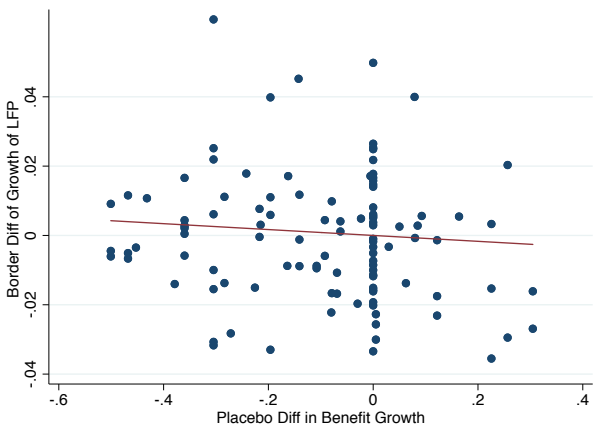
(a) Employment States      (b) Labor Force States
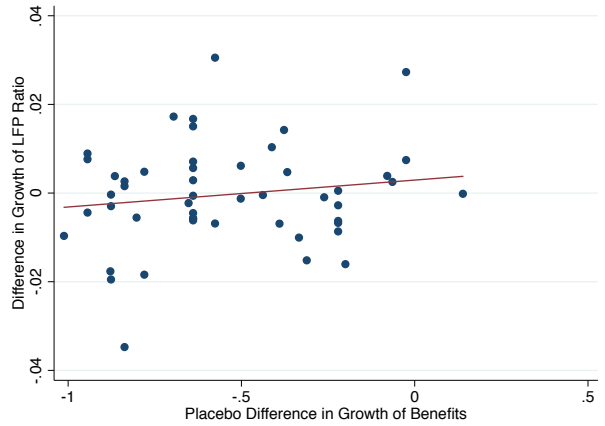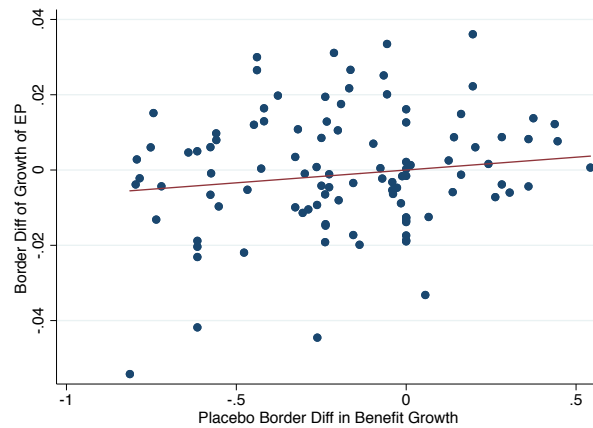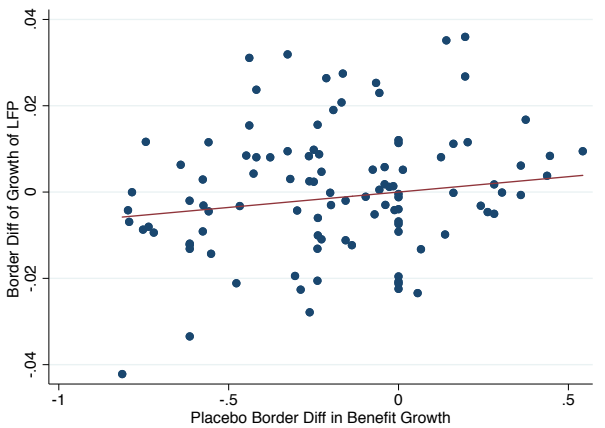
(c) Employment State Borders      (d) Labor Force State Borders

Figure A-8: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2012Q3 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.
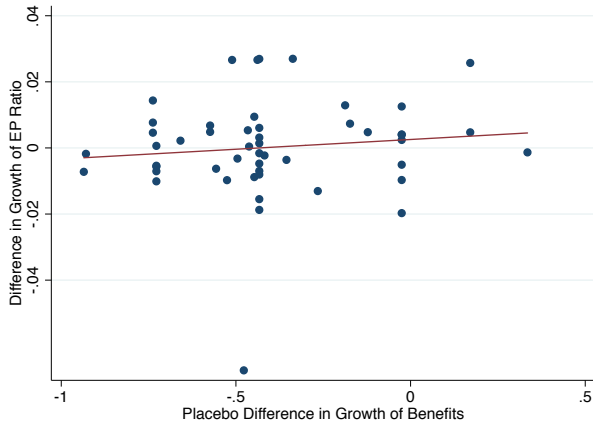
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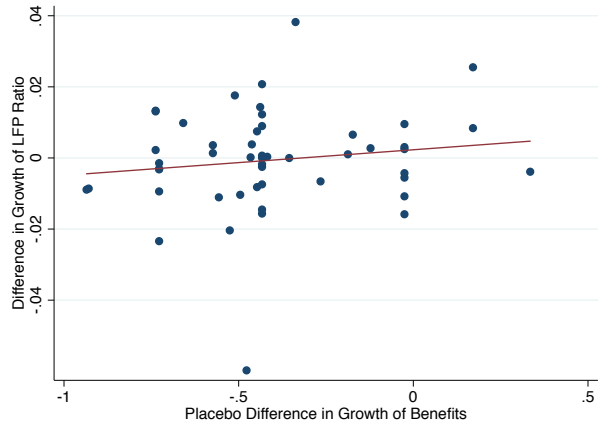
(b) Labor Force States
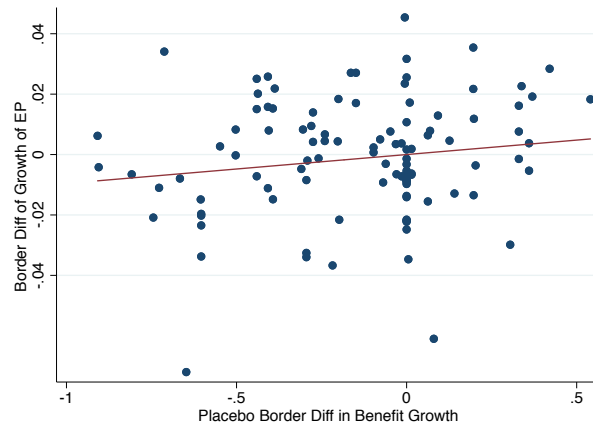
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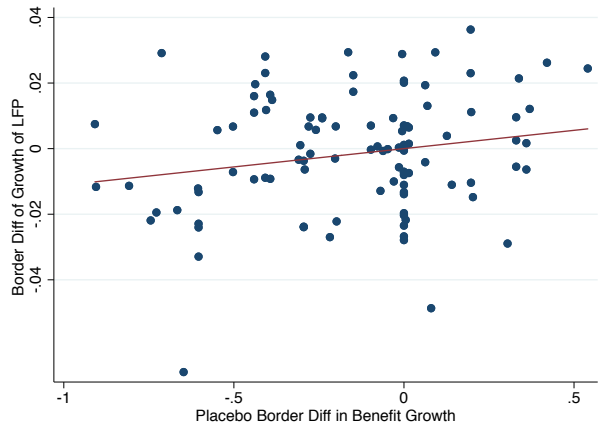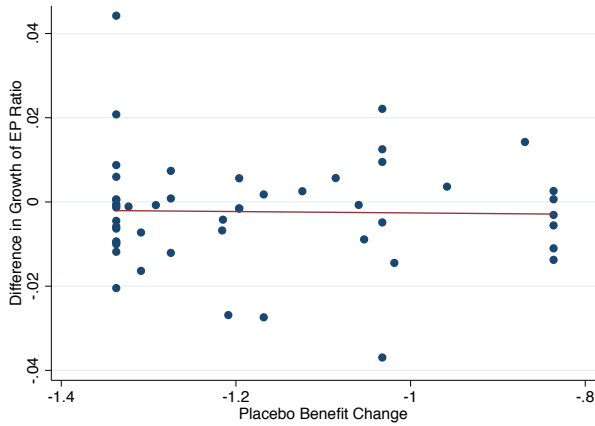
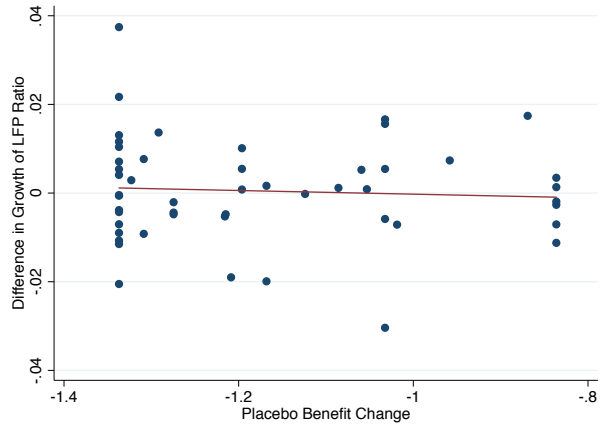(d) Labor Force State Borders

Figure A-9: Difference in growth rates of EP or LFP over 4 quarters after and 4 quarters before 2012Q4 vs. the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs.

# IV Scatter Plots Underlying (Placebo) Experiments in Figure 5 in the Main Text



(a) Employment States

(b) Labor Force States

(c) Employment State Borders
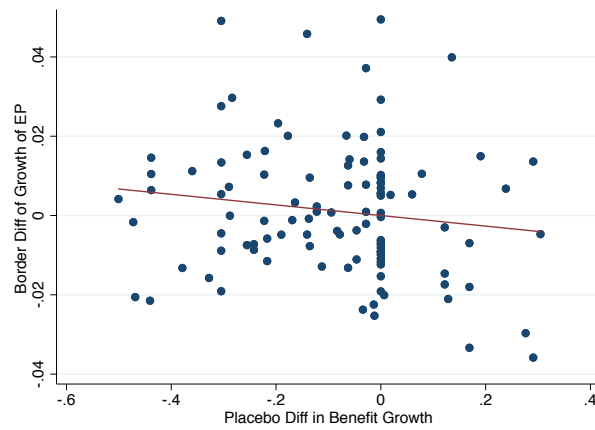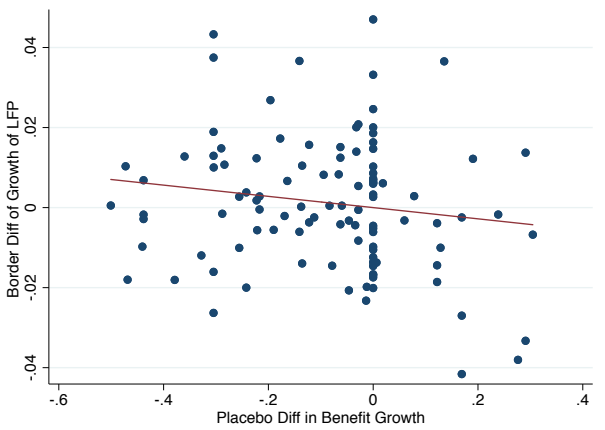
(d) Labor Force State Borders
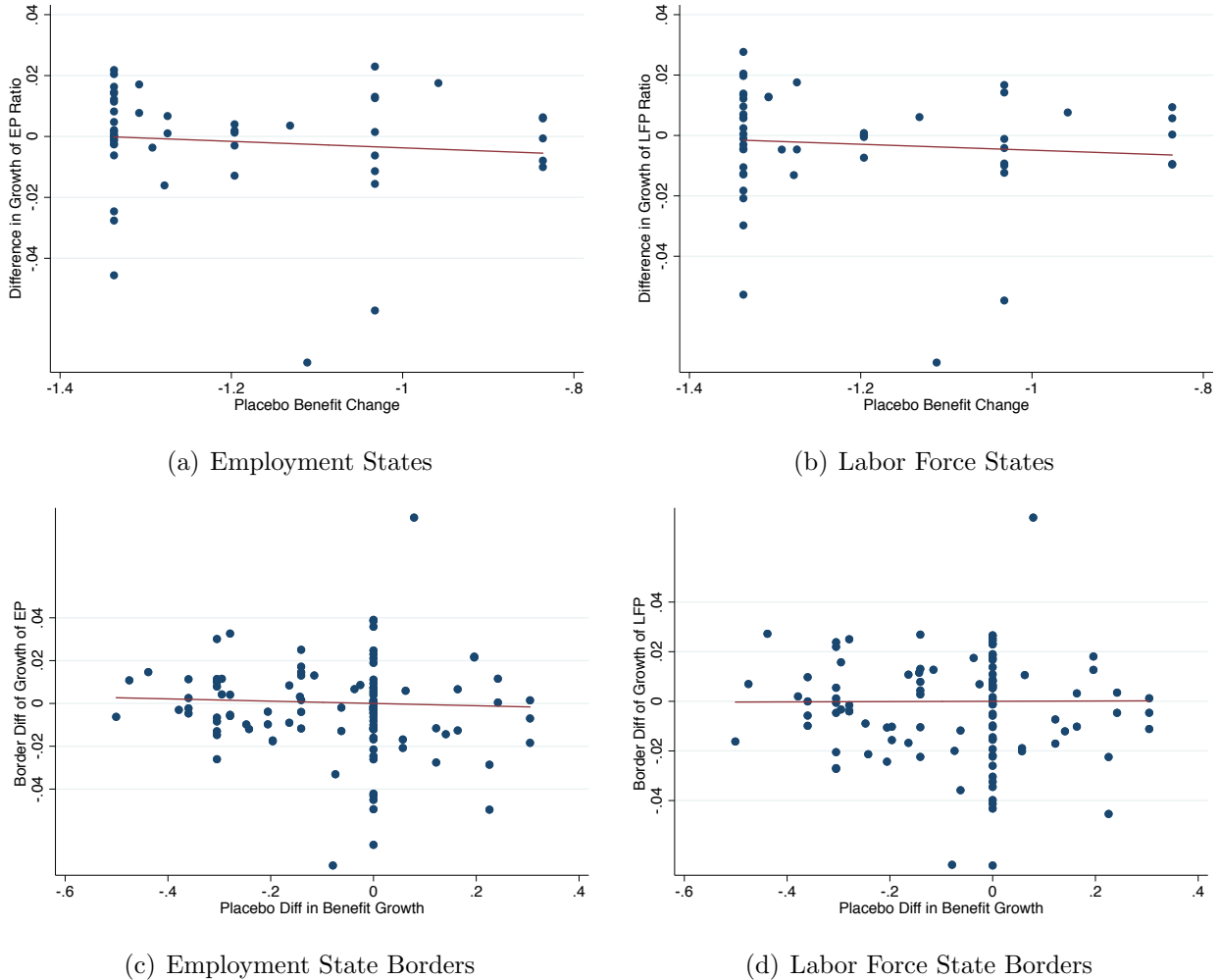
Figure A-10: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2011Q1 on the size of placebo benefit duration cut in 2011Q1 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.
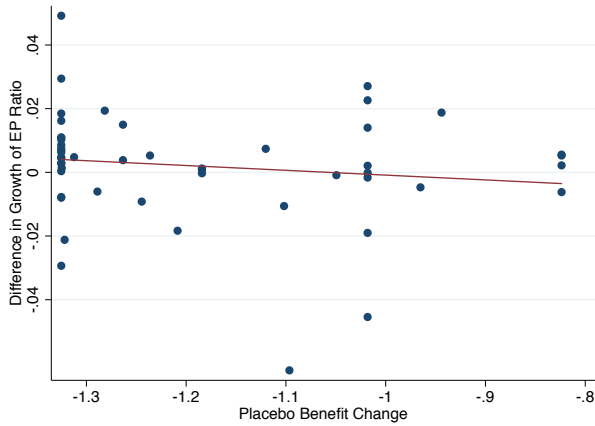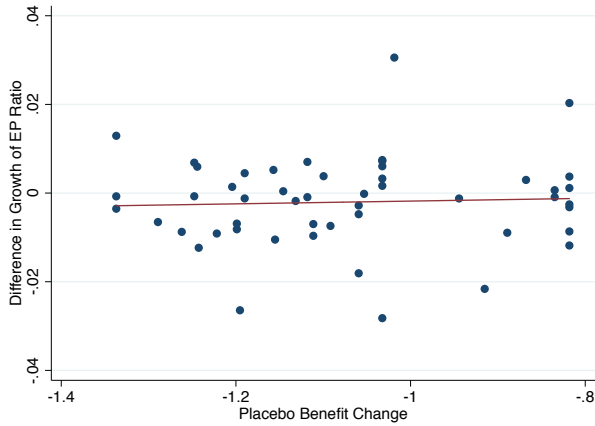
(a) Employment States
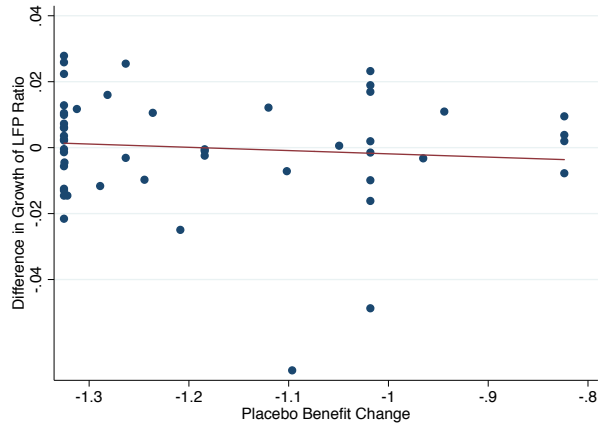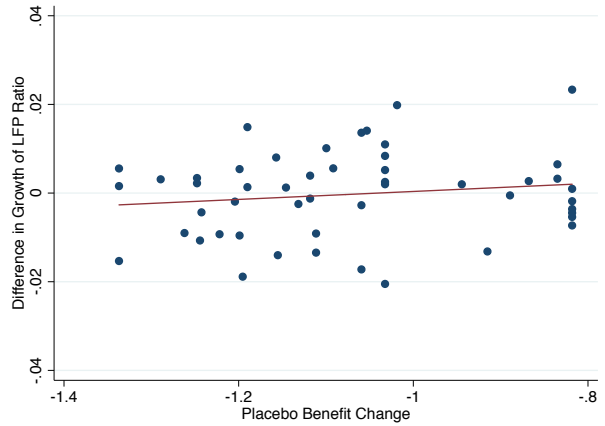
(b) Labor Force States

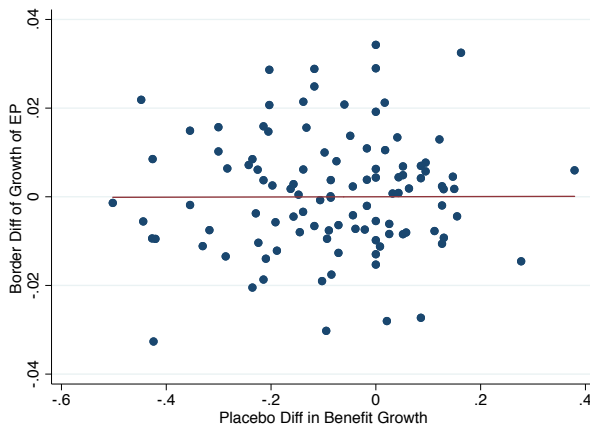(c) Employment State Borders

(d) Labor Force State Borders

Figure A-11: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2011Q2 on the size of placebo benefit duration cut in 2011Q2 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.
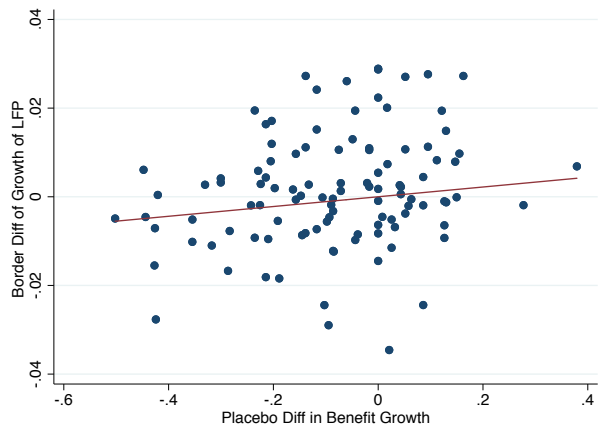
(a) Employment States

(b) Labor Force States
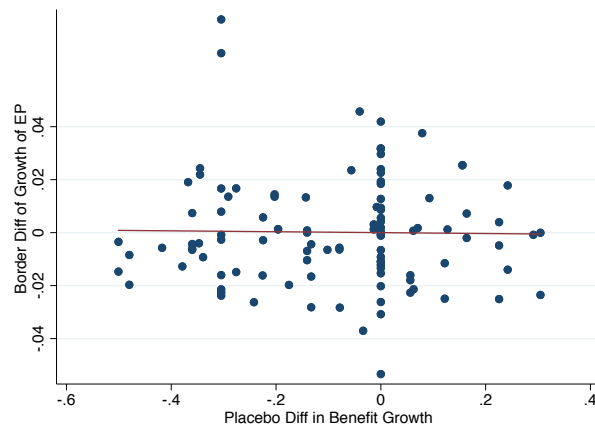
(c) Employment State Borders

(d) Labor Force State Borders

Figure A-12: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2011Q3 on the size of placebo benefit duration cut in 2011Q3 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.

(a) Employment States
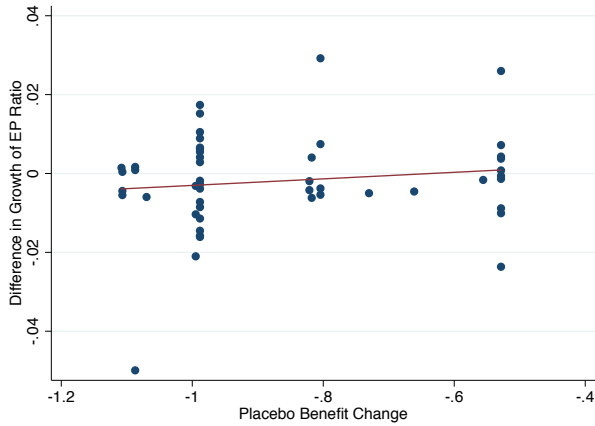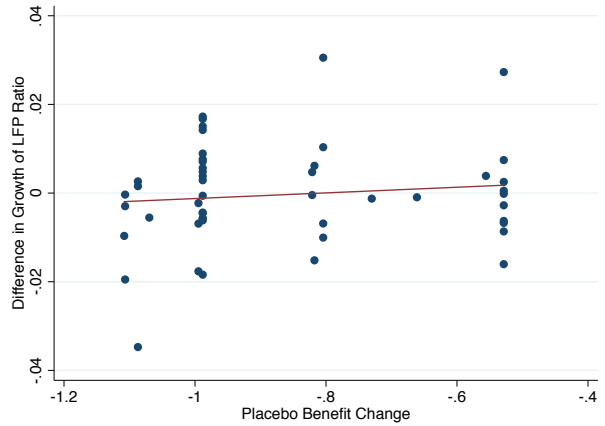
(b) Labor Force States
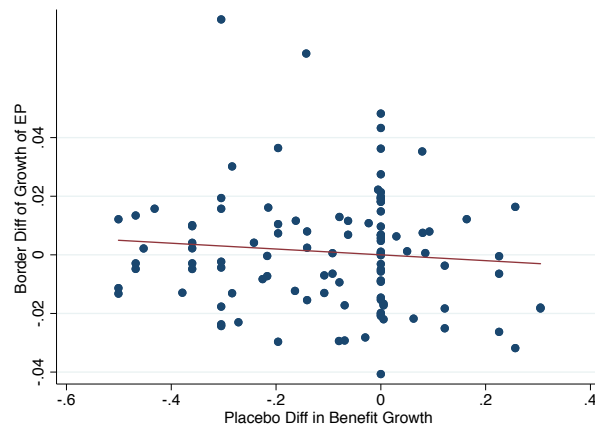
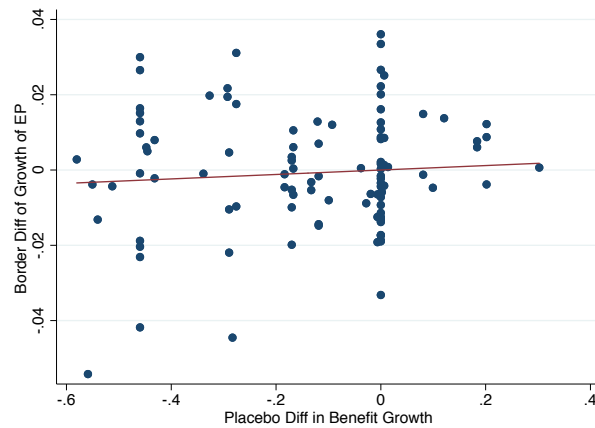(c) Employment State Borders

(d) Labor Force State Borders

Figure A-13: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2011Q4 on the size of placebo benefit duration cut in 2011Q4 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.

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(a) Employment States

(b) Labor Force States

(c) Employment State Borders

(d) Labor Force State Borders

Figure A-14: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2012Q1 on the size of placebo benefit duration cut in 2012Q1 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.
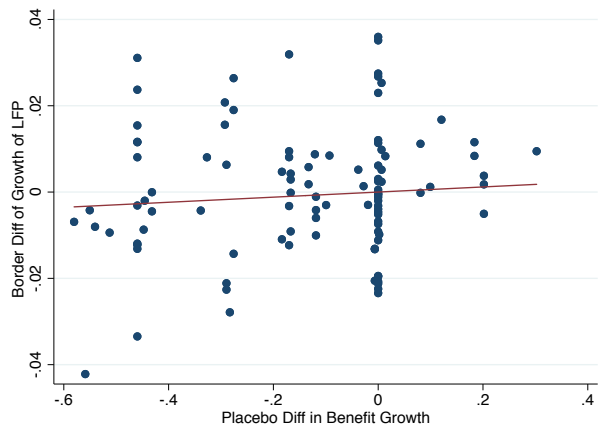
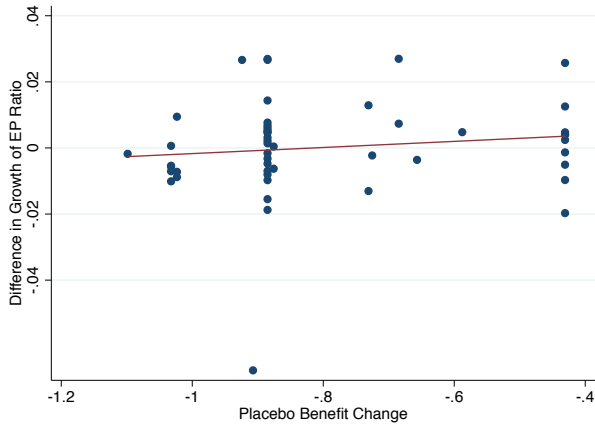(a) Employment States      (b) Labor Force States

(c) Employment State Borders      (d) Labor Force State Borders

Figure A-15: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2012Q2 on the size of placebo benefit duration cut in 2012Q2 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.

(a) Employment States

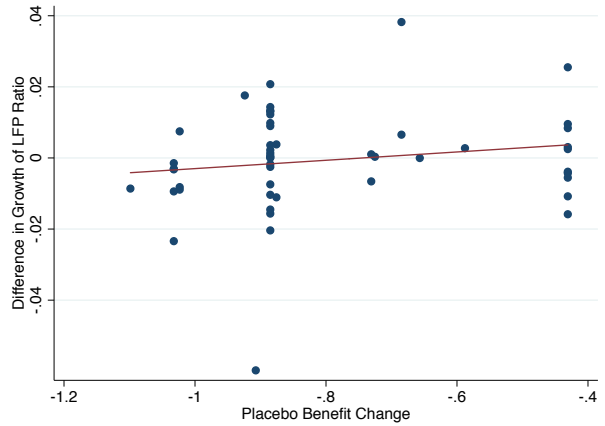(b) Labor Force States

(c) Employment State Borders
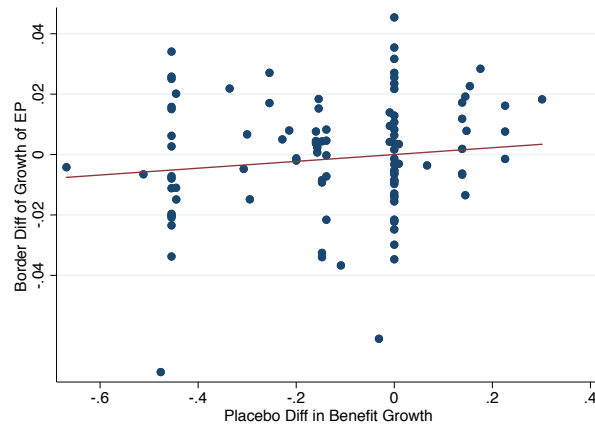
(d) Labor Force State Borders

Figure A-16: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2012Q3 on the size of placebo benefit duration cut in 2012Q3 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.
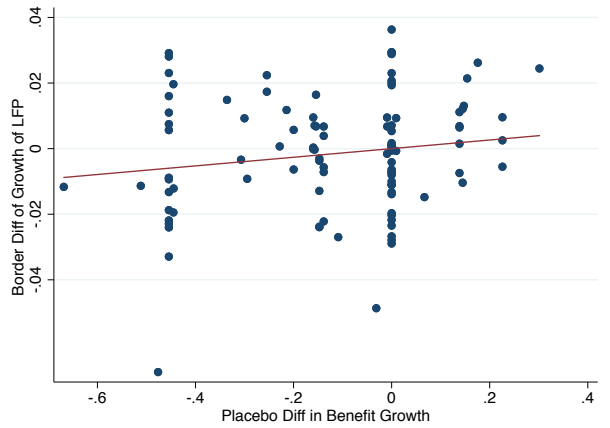
(a) Employment States

(b) Labor Force States

(c) Employment State Borders

(d) Labor Force State Borders

Figure A-17: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before 2012Q4 on the size of placebo benefit duration cut in 2012Q4 (countrfactually assuming that all federal extensions were eliminated in that quarter). States and bordering state pairs.