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The Jobs Effect of Ending Pandemic Unemployment Benefits: A State-Level Analysis *

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Abstract

This paper uses the asynchronous cessation of emergency unemployment benefits (EUB) in 2021 to investigate the jobs impact of ending unemployment benefits. While some states stopped providing EUB in September, others stopped as early as June. Using the cessation month as an instrument, we estimate the effect on employment of reducing unemployment rolls. In the second month following a state's program termination, for every 100 person reduction in beneficiaries, state employment causally increased by about 27 persons. The effect is statistically different from zero and robust to a wide array of alternative specifications.

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1 Introduction

Between January and April of 2020, U.S. employment declined by 24 million people (about 15 percent) as a result of the COVID pandemic. In response, the federal government introduced several temporary programs to lessen the negative impact of this decline. These emergency unemployment benefits (EUB) included providing program eligibility to many individuals who would not otherwise be covered, such as contract and gig workers, extensions of benefit durations and a $600 weekly add-on for recipients.[1] These initial programs were extended by the federal government through September 2021, with the only major adjustment being a reduction of the add-on to $300 per week.

By spring 2021, following a major but incomplete rebound of employment, job growth began to slow and job openings were steadily rising, reaching 9.6 million in May 2021, up 2.5 million from their pre-pandemic level. At the same time, some state governors voiced concerns that the generosity of EUB was contributing to businesses' difficulty filling job vacancies. They announced plans to halt either some or all of these benefits several months before the programs' completion in September. In total 26 governors ended benefits before September, with 24 halting participation between June 12 and July 3.[2] In this paper, we use this asynchronous benefit termination across states to estimate the causal effect of losing unemployment benefits on employment.[3]

There are several important things to note about difference across states. First, a key indicator for each governor's decision to become an early halter or remain a late halter

[1] Throughout the paper, EUB will abbreviate both "emergency unemployment benefits" as well as "emergency unemployment beneficiaries." The meaning that is appropriate in each instance will be clear. For a description of these programs, enabled by U.S. Congress (2020), see U.S. Department of Labor (2020a) and U.S. Department of Labor (2020b).

[2] To fix language, we shall refer to states that ended EUB in September as late halters and state that ended EUB before September as early halters.

[3] It is worth noting the tremendous amount of national news regarding the quasi-natural experiment afforded by the end of EUB and the differential timing of benefit cessation. See for example Horsley (2021), Smialek (2021) and Mollica and Santilli (2021).

was the governor's political party. All but one of the early-halting states' governors was a member of the Republican party. Our identification uses the halt month as a source of exogenous variation, aligning with other economic research that uses political affiliation to identify causal effect.[4]

Instead of assuming that the halt month was strictly exogenous, we make a weaker assumption that each state's halt month was exogenous *conditional on covariates* (i.e., conditionally exogenous). We observe that although political motivation was a primary source of randomness in each state's halt month, the political variable was also potentially correlated with changes in that state's resident attitudes toward the virus and its stringency of public health regulations, both at the state level. Each state's timing of cessation might coincide with that state's reduction in, for example, workplace closures or stay-at-home requirements. In either of these scenarios, the labor supply effect driven by halting benefits might be confounded by labor demand effects from businesses reopening or consumers' expanded propensity to shop.

Fortunately, the episode provides detailed data on these policies and attitudes. In large part to track and help control the virus's spread, media groups, government agencies and NGOs constructed disaggregate, high frequency measures of these covariates. We use these measures as controls in order to ensure conditional exogeneity. We find that—while there were timing differences in policy and attitude changes across states—these differences did not align with the cessation month in a way that had an appreciable effect on our jobs effect estimates.

Despite the important role of political party for determining which states were early halters, the two types of states tended to differ on an additional dimension. On average,

[4]For example, Knight (2002) uses political power of state congressional delegations as a source of exogeneity to identify potential crowd-out of federal highway grants on state government spending. Benhabib and Spiegel (2022) use the comparison of the president's political party to the share of a state's congressional delegation as an instrument to estimate the effect of sentiments on economic activity.

early halters had recovered much closer to their pre-pandemic employment levels by the spring of 2020 than late halters. That is, early halters in general had relatively smaller remaining "employment gaps" immediately before treatment. With smaller remaining gaps, employment growth for early halters had already slowed relative to late halters and EUB recipient rolls in early halting states were generally lower relative to late halting states. In May 2021, for example, EUB per capita in late halting states was nearly double that of early halters. We address this difference by explicitly using the reduction in the number of EUB recipients as our treatment variable, while maintaining exogeneity by instrumenting using the month of program termination.

Finally, we additionally address endogenity concerns by including both early and late halting states in our analysis. Including late halting states strengthens our case for exogeneity. While early halters made the explicit—although politically influenced—decision of when to halt benefits, late halters did not individually choose the timing of benefit termination. The late-halting states' September 2021 termination date was set in December 2020 by federal legislation. Federal lawmakers could not have foreseen states' economic conditions when the termination date was determined.

Our main outcome variable is the state-level three month change in employment. In our benchmark specification, we estimate that for every 100 person reduction in beneficiaries driven by program termination, employment increased by roughly 27 persons comparing the second month following a state's EUB termination to the month before EUB termination. Thus, there was a strong, rapid employment response to the reduction in EUB rolls resulting from the end of emergency benefits.

Next, we show that our results are robust to a battery of controls. In the benchmark specification we control for the mask usage rate and an index for lockdown stringency. We further show that the results are robust to many alternative specifications, such as changing the controls, adjusting the start month of the sample and modifying the sample states. Robustness checks control for pandemic-related health variables such as COVID deaths and COVID cases as well as the leisure and hospitality share of employment. We employ placebo tests to show that our regression results do not reflect spurious findings.

We forgo an extensive literature review due to our intention to present our findings concisely and the number of excellent surveys on the topic of UI and labor markets (e.g., Krueger and Meyer (2002)) and describe only a few most closely related papers.

Holzer, Hubbard and Strain (2021) is perhaps the closest study to ours. They use the June 2021 halt month to conduct an event study using CPS data from roughly the same period as this paper. They find that the national employment-population ratio would have been 0.1-0.2 percentage points higher in July and August had all states ended FPUC and PUA in June. They do not use beneficiary data in their analysis. Our estimates share the same sign, but are larger in magnitude than theirs.

Marinescu, Skandalis and Zhao (2021) use disaggregated data from an online jobs site to estimate the effect of the additional $600 per week benefit on job applications filed and on vacancy creation. They study the March to July 2020 period. They find that higher benefits led to a small decrease in applications and no change in vacancy creation. Their results are not comparable to ours for three reasons. (i) Over their period, jobs were scarce, whereas over our period jobs were plentiful. (ii) They look at micro-level data. Cross-individual spillovers (either positive or negative) will imply that micro estimates differ from macro estimates.[5] (iii) Whereas Marinescu, Skandalis and Zhao (2021) look at variation in benefits levels that are positive before and after treatment, the benefits amounts change from positive to zero for many beneficiaries during the episode we consider.

Finamour and Scott (2021) study the $600 per week EUB add-on program (Federal Pandemic Unemployment Compensation, FPUC) and its effect on employment and unemployment insurance (UI) replacments rates. They analyze labor market trends for the

[5] This is due to a violation of the SUTVA assumption.

period before FPUC, during introduction of FPUC and after FPUC ended. In relation to our paper, the most relevant finding is that the relationship between UI generosity and employment does not change upon expiration of FPUC at the end of July 2020.[6] This result is suggestive that removing EUB had little effect on employment.

Bartik, et.al. (2020) study many aspects of the labor market during the early phase of the pandemic. This includes estimating a state-level event study using the distribution of payment initiation for two of the EUB programs. They motivate exogeneity by appealing to heterogeneous delays in program implementation. They find no evidence that the number of hours worked changed following the payment initialisation. However, similarly to Marinescu, Skandalis and Zhao (2021), jobs were scarce during their period of study compared to ours.

Johnston and Mas (2018) study Missouri's early termination of extended benefits in 2011 following the 2007-2009 recession. While not directly comparable to our results, they likewise find a strong effect on employment. Their estimates imply that a one-month reduction in potential UI duration leads to a reduction in nonemployment of 0.25 months. Next, a federal policy change in December 2013 resulted in the termination (or substantially shortened duration) of benefits for 4.7 million recipients. Using a county-level analysis, Hagedorn, et.al. (2015) find that this abrupt change increased employment by 2.1 million persons in the following year.

2 EUB Payouts Before and After Program Cessation

In May 2021, over 12 million non-working individuals were receiving some form of EUB in the U.S. When a state halted EUB, a natural question at the time was: How many recipients losing benefits would take up work within a short time span? Our approach is

[6]FPUC was reintroduced in December 2020 at the lower amount of $300 per week.

motivated by that question. In this section, we discuss two issues that complicate this approach. First, as mentioned in the introduction, early halting states had substantially fewer beneficiaries per capita than late halters. This difference alone means that one should interpret benefit cessation across states with caution. A state with zero beneficiaries would necessarily have no beneficiaries returning to work.

Second, when states ceased participation in EUB, they often continued to pay benefits to some recipients for unemployment spells that occurred before EUB cessation. From the perspective of individuals looking forward after the state's cessation date, benefits would no longer de-incentivize employment. That is, the relative cost of not working would rise. However, if they were still receiving or anticipating receiving benefits for past EUB, then they might have an income stream that could finance consumption and allow them to remain out of the workforce. Therefore, we will take into account how many people actually stopped receiving benefits when a state halted EUB.

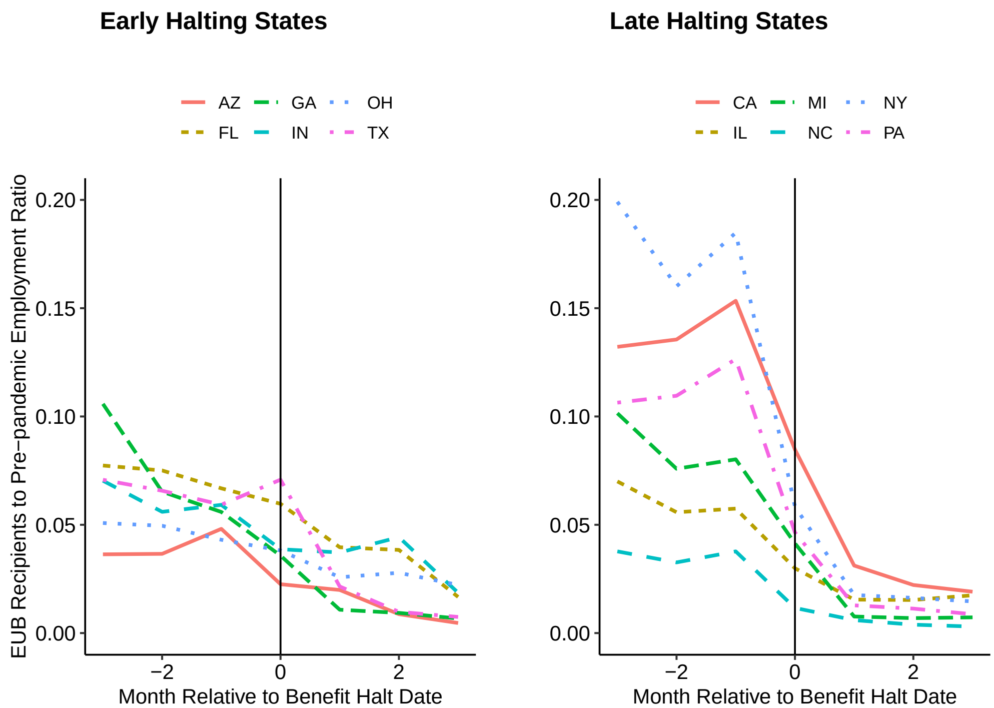
Figure 1 shows the path for the number of beneficiaries by month in twelve large states.[7] The left-hand panel of the figure plots the ratio of EUB recipients to pre-pandemic employment by month for six large early halting states. The horizontal axis marks the number of months before and after the respective state halted benefits. For example, during its cessation month, Arizona (red solid line) had a recipients share equal to roughly 2.3 percent of its pre-pandemic employment. The number of benefit recipients in that state was relatively stable before halting; it began to decline following halting. The right panel presents the analogous data for six large late halters.

Many states saw very high EUB take up. For example, the right panel of the figure indicates that California had a number of beneficiaries equal to about 15 percent of pre-pandemic employment in the month before it halted benefits (August 2021). We discuss

[7] The federal government reports the number of weeks of benefits paid in a month. We approximate the number of beneficiaries by dividing the weeks reported by four.

the reasons for and confirm, using other data, these high beneficiary rates in Appendix A.

Figure 1: Ratio of number of non-working EUB recipients to pre-pandemic employment level, by state and months since benefit halt date



Note: EUB recipient data are from monthly state reports to the Bureau of Labor Statistics and include PUA, PEUC and regular beneficiaries. Pre-pandemic employment levels are from CPS micro data aggregated to the state level for January 2020.

Relative to early halters, late halters tended to have higher EUB-employment ratios in the halting month and saw steeper declines in EUB following halting. This implies that the large majority of the variation in EUB reductions comes from late, rather than early halting states. From the month preceding to the second month following the halt month, non-working EUB recipients fell by 2.4 million persons in early halting relative to 7.5 million persons in September halting states.

Importantly, EUB for two of the main programs, PEUC and PUA, did not collapse to zero in the month following cessation.[8] This may indicate that some EUB beneficiaries

[8]PUA and PEUC stand for Pandemic Unemployment Assistance and Pandemic Emergency Unemploy-

were still receiving EUB for weeks of unemployment preceding EUB program cessation. Given the slow decline, our benchmark model looks at the employment change from the month before to two months after the halting of EUB rather than the impact effect in the first month.

3 Data and Econometric Model

We study monthly data from every state and the District of Columbia covering October 2020 through June 2022. Let $Y_{i,t}$ denote the number of employed persons in state i at month t aged 16+. Our employment data are from the Current Population Survey (CPS) individual-level data, which we aggregate to the state level. We consider two other employment data sources, the CES and the LAUS, in the robustness section and in Appendix Sections E and F. We seasonally adjust our data using the Census Bureau's X-13 procedure in order to control for seasonal changes in employment.

Let $H_{i,t} = 1$ if state i halted in month t, and zero otherwise. Using this measure, we define t_i^* to equal the halt month of state i. To assign a specific cessation month to a state, we record the calendar date that EUB halted in that state. Next, we note that the survey occurs during the calendar week (Sunday through Saturday) containing the twelfth day of the respective month. We choose the treatment month as the one where the twelfth day of the month is closest in time (either before or after) to the date at which the state ended benefits. For example, the earliest four halting states ended benefits on June 12. Thus in these states, we choose June as the treatment month. Appendix B lists the halt month for each state.[9]

Next, let $C_{i,t}$ denote the number of non-working emergency unemployment beneficia-

ment Compensation.

[9] A few states temporarily delayed execution of their cessation plans. Appendix C discusses these cases. Also, in our robustness analysis we show that changing the halt date for these states to reflect the delays does not materially affect our results.

ries in state i in month t, measured in hundreds of persons. We construct this variable using Bureau of Labor Statistics (BLS) data on the number of weeks of emergency unemployment benefits paid per month, which we call $B_{i,t}$. First, we assume that a state resident collecting EUB receives four weeks of benefits per month. We therefore scale $C_{i,t}$ by $\frac{1}{4}$ to measure the number of beneficiaries instead of the number of weeks. Second, from Census Pulse surveys taken during the pandemic, we note that nationwide about 20 percent of beneficiaries were working while collecting benefits. In terms of the effect of program cessation on net employment changes, it is important to consider only the $\frac{4}{5}$ of beneficiaries who were not working while collecting benefits.[10] Finally, we measure the variable in hundreds of persons to ease interpretation of our estimated coefficients by multiplying by $\frac{1}{100}$. Thus,

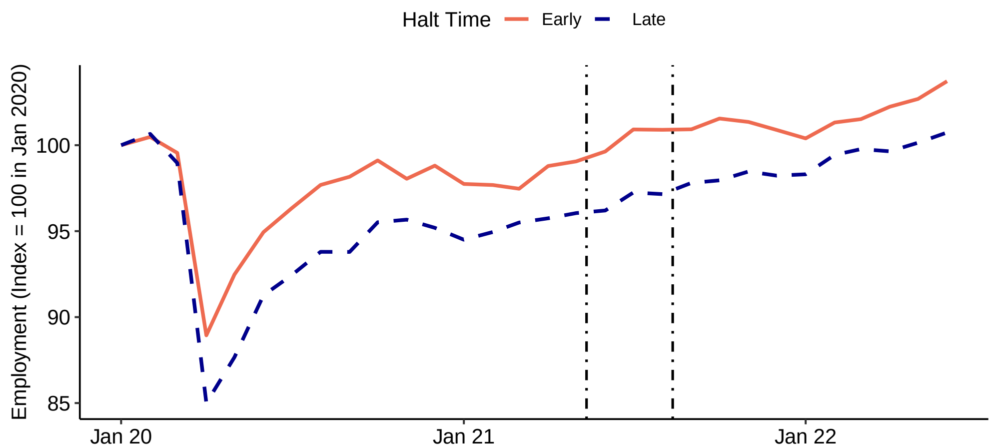
$$C_{i,t} = \frac{1}{4}\left(\frac{4}{5}\right)\left(\frac{1}{100}\right)B_{i,t}$$

Before presenting our full econometric model, we plot the pre-treatment trends in employment for the early halting and late halting groups and estimate a simple event study regression. Figure 2 plots employment as an index (equal to 100 in January 2020) both before and after treatment for June and September halting states. The vertical lines indicate the months immediately preceding the two most common halting months (June and September). Note that in the immediate response to the pandemic (March and April 2020), employment fell more in early halting states relative to late halting states. Employment in both groups increased quickly after April.

For April 2020 through June 2021, the two groups follow roughly parallel trends in which employment growth slows as time progresses. The parallel trends observed in

[10]This adjustment is necessary because if an individual losing benefits had already been working some hours pre-cessation, then their survey response would be “employed” both before and after the state terminated benefits. Throughout the paper, beneficiaries refers to non-working beneficiaries. The 20 percent figure comes from a pulse survey conducted from June 23 to July 5. Data are available on this question from April 14 through July 5 and the figure does not change significantly over that time period. Although it is feasible, we do not use state-specific scaling factors because they would be too noisy given the Census Pulse survey sample sizes.

Figure 2: Employment index for early and late halting states (index equals 100 in January 2020)



Note: Employment in each group is the sum of seasonally adjusted state level employment calculated from the micro-CPS dataset in each month divided by the value in January 2020.

Figure 2 provide additional support for the exogeneity assumption we maintain in order to achieve identification.

We adopt the local projections method, originally developed in Jordà (2005) and extended by Dube, et.al. (2022) to be suitable for our purpose, as a straightforward way to estimate dynamic causal effects in the presence of staggered treatments. However, as a first step, we demonstrate our basic finding in a more traditional event study analysis. Note that we cannot estimate the causal effect via a standard event study approach using all of our data without potentially introducing bias because of the staggered EUB cessation.[11]

To construct an event study that does not suffer from the above potential bias from staggered treatment, we limit the sample by: (i) including only June and September halting months in the regression; (ii) ending the estimation sample with August 2021. Since

[11]See for example Goodman-Bacon (2021) on this point. See Appendix D for a discussion of alternative methods for addressing this potential bias.

benefits had not yet ended for the September halters in August, in absence of anticipation effects, the September states form a valid untreated control group. The June halters make up the treatment group. By excluding states that halt in July and August, we avoid the issue of asynchronous treatments and can apply the basic event study technique.

First, define $\tilde{y}_{i,t} = 100 \times \left(Y_{i,t}/Y_{i,Jan20}\right)$. The estimation equation is:

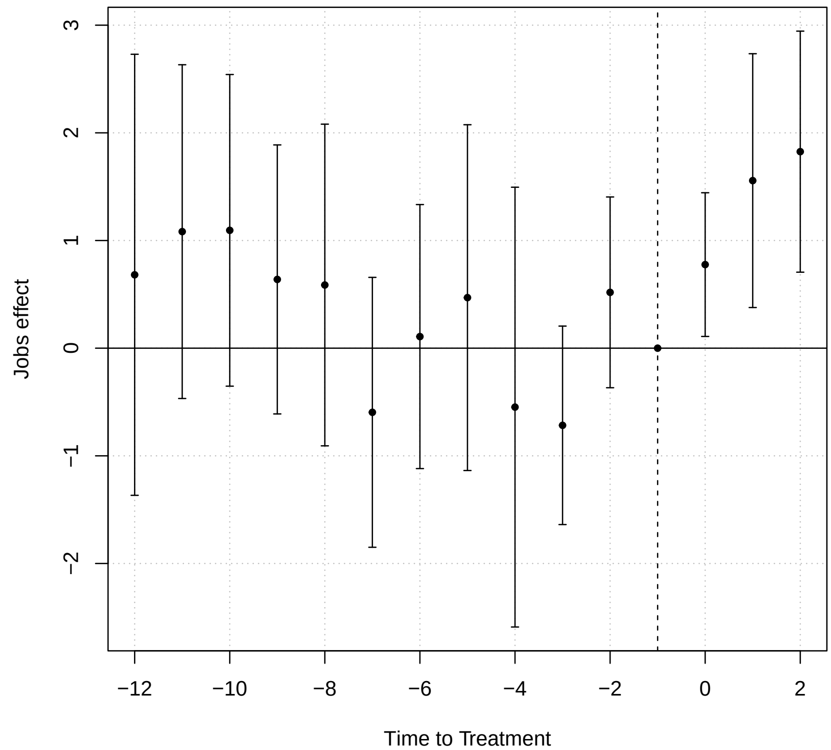
$$\tilde{y}_{i,t} = \psi_i + \mu_t + \sum_{h=-H_1}^{H_2} \alpha_h 1\left(H_{i,t-h} = 1\right) + \phi' W_{i,t} + \eta_{i,t} \tag{1}$$

We scale by January 2020 employment here, as well as in the local projections estimation below, because it provides a simple pre-COVID benchmark. Scaling instead by population would lead to an outcome that did not account for potential differences in the employment rate across states, although presumably this would be corrected for via the state fixed effects used below.

We estimate the model over the period June 2020 through August 2021. $W_{i,t}$ contains two variables: the mask usage rate and a government COVID-19 policy stringency index in state i during month t. The mask usage rate controls for cross-state differences in public attitudes and the course of the pandemic, and the stringency index controls for differential state policies unrelated to EUB policy. The mask usage rate is the percent of the population reporting always wearing a mask when leaving their home reported by the Institute of Health Metrics and Evaluation. The stringency index is measured as the average over indicator variables for containment policies, closure policies and public information campaigns from the Oxford Covid-19 Government Response Tracker. For both controls, we construct a 3-month change ending before $t-1$ and scale the variables to have mean zero and unit variance. ψ_i and μ_t are state and time fixed effects. We weight regressions by pre-pandemic employment and compute standard errors clustering by census division.

Figure 3 plots the regression coefficient α_h for each time-to-treatment h as dots, en-

Figure 3: Effect of halting EUB on employment pre and post treatment, 90 percent pointwise confidence intervals



Note: Outcome variable=100 × Ratio of employment level to pre-pandemic employment level. We weight regressions by pre-pandemic employment and cluster standard errors by census division.

veloped by 90 percent confidence intervals. It indicates a strong employment effect that builds gradually in the months following benefit termination. Two months following cessation, the halting of benefits increases employment by 1.8 percent in the treated relative to the untreated group. To express this effect as a total number of jobs, consider a counterfactual in which all states and DC halted in June relative to another counterfactual in which they halted in September. The point estimate implies that employment would be 2.9 million persons larger in August in the former case relative to the latter.

Next, note that in the twelve months before treatment, the coefficient oscillates around zero. For $h < 0$, the coefficients are statistically indistinguishable from zero. This indicates the same absence of differential pre-trends indicated by Figure 2.

While the event study regression is instructive, it has several limitations. First, we are not using data after August 2021, nor are we using the July or August halting states. Second, the September halting states have a control phase and a treatment phase, the latter of which we are not exploiting above. Third, although we measure the employment effect of terminating benefits, we are not measuring precisely how many people were taken off of EUB as a result of the halting. To address these three concerns, our main econometric specification is a local projections instrumental variables regression. Moreover, unlike the event study approach (and recent extension thereof),[12] the local projections approach allows for a continuous treatment, instrumental variables and staggered adoption.

Our estimation equation is:

$$\frac{Y_{i,t+k} - Y_{i,t-1}}{Y_{i,Jan20}} = \theta_{i,k} - \gamma_k \frac{C_{i,t+k} - C_{i,t-1}}{Y_{i,Jan20}} + \beta_k' X_{i,t-1} + \varepsilon_{i,t+k}^k \tag{2}$$

where we estimate the model at various horizons $k = 0, \ldots, M$. In the above expression, we scale the dependent variable by January 2020 employment for the reasons laid out in the event study analysis above. We scale the (right-hand side) change in beneficiaries variable by the same employment variable to ease interpretation of the main coefficient of interest γ_k. γ_k gives the causal impact on employment of a 100 person reduction in benefit recipients. In most of our specifications, we report the estimate at horizon $k = 2$ and report the estimate at other horizons in the robustness section.

Next, the error term $\varepsilon_{i,t+k}^k$, the second subscript $(t + k)$ indicates the calendar month and the superscript k indicates the horizon of the local projection (of the error term). Here, $\theta_{i,k}$ is a state fixed effect, and $X_{i,t-1}$ is a vector of conditioning variables. We do not include

[12]See Callaway and Sant'Anna (2021) and Sun and Abraham (2021).

month fixed effects, because these are highly collinear with $H_{i,t}$. As we discuss below, adding month fixed effects will result in an imprecisely estimated jobs effect but have little affect on our point estimate.

We estimate (2) using $H_{i,t}$ as an instrumental variable. We assume that beyond period $t + M$ there is no additional effect of EUB cessation on the outcome (where t is the time of benefits halting). This allows us to expand our sample to use some post-treatment months as control months in our estimation. This assumption implies that state i employment following termination adjusts within M months following t_i^*. In the robustness section, we explore how estimates change as we vary M.

The local projection method has several additional advantages over other approaches. First, it easily accommodates including conditioning variables because it is based the linear OLS/IV framework. As such, we can directly use existing tools of inference (e.g., correcting for serial correlation and clustering). Secondly and relatedly, Dube, et.al. (2022) explain that the local projections approach is much faster than other approaches for estimating causal effects with staggered treatment (e.g., Callaway and Sant'Anna (2021)). The local projection approach, more generally, has become a workhorse in macroeconometrics in the past fifteen or so years for estimating dynamic causal effect and forecasting.[13] The only modification to adapt the method to the current environment is the clean control restriction described below.[14]

We draw our sample using the procedure given in Dube, et.al. (2022) to ensure that, for periods close to t_i^*, we do not contaminate our estimates of the horizon k response with observations that are influenced by the treatment. Specifically, for each state i and

[13] See for example Jordà (2005), Montiel Olega and Plagborg-Møller (2021) and Ramey (2016).

[14] Papers that apply this modified Jorda approach include Goda and Soltas (2022), Jordà and Nechio (2022) and Kroen, et.al. (2021).

horizon k, we drop observations

$$t \in \{t_i^* + g\} \text{ for } g \neq 0 \cap g \in \{-k, \ldots, M\} \tag{3}$$

For example, for a state halting in June, we drop observations from April, May, July, August, September and October if $k = 2$ and $M = 4$. Dropping the observations up-to k months before treatment immunizes against using a contaminated control period resulting from overlap induced by the k-month ahead dependent variable. Dropping the up-to M month post-treatment observations immunizes against using a contaminated control period resulting from the delayed effects of the treatment on the outcome. We weight regressions by state pre-pandemic employment. We compute robust standard errors adjusted with a finite-sample correction and also cluster at the census-division level. Restricting the sample via equation (3), ensures that each observation is in either a treatment period or a clean control period.

Our benchmark conditioning variable vector $X_{i,t-1}$ contains three lags of the one month change in EUB and three lags of the one month change in employment, where each variable is scaled by pre-pandemic employment. The benchmark also controls for the mask usage rate and the stringency index.

Our state-level treatment in equation (2) is the reduction in the number of benefit recipients. When a state halted emergency benefits, this affected a reduction in the total number of recipients in two ways. First, PUA and PEUC recipients automatically lost access to future benefits.[15] Second, a portion of regular state program recipients may have chosen to stop collecting benefits as a result of losing the $300 add-on even though their regular state benefits had not yet expired. As such, the size of the treatment pool is attenuated to the extent that regular program recipients continued taking benefits despite

[15] As noted previously, some payments to individuals in this group occurred after the halt month, likely due to administrative delays.

losing the $300.

We calculate the size of the regular program group (pre-halt) as follows: For each state, we calculate the number of total recipients and the number of regular state program recipients during the month preceding that state's halt month. Summing across all states, only 19.1 percent of beneficiaries were regular program recipients. Since a large fraction of the job separations occurred in Spring 2020 at the start of the pandemic, many of the recipients initially in the regular state program timed out of the regular program and moved to PEUC by the summer of 2021. After presenting our main results, we use the variation induced by asynchronous halting to estimate what fraction of regular program recipients ended participation as a result of losing the $300 add on. We show that this fraction was small.

4 Results

This section reports and interprets estimates of equation (2) and its variants. The results deliver a consistent message: Terminating EUB had a quantitatively and statistically significant positive impact on employment, which we show is robust to many sensible alternative statistical specifications.

First, column 1 of Table 1 contains our benchmark two-stage least squares estimate. The coefficient equals 26.60 (SE=9.90), indicating that employment increases by about 27 persons for each 100 person reduction in EUB after two months following a state's EUB cessation (compared to the month prior to cessation). The estimate is statistically different from zero at a five percent level. Note that using state-level data implies that we cannot follow individuals losing EUB benefits over time to see if they specifically boosted employment following EUB termination. Rather, our outcome variable is the net change in total state employment. Parsing the extent to which the employment increase comes

Table 1: Benchmark results for the jobs effect of benefit termination: employment change (over three months) per 100 person reduction in the number of beneficiaries along with reduced form, two-stage least squares and first stage results

	Dep Var: Emp Change			Dep Var: Decl in claims
	(1) 2SLS (Bmark)	(2) Reduced Form	(3) OLS	(4) First Stage
Decl in EUB	26.60^{**}		4.09	
	(9.90)		(4.42)	
Halt month		1.53^{**}		5.75^{***}
		(0.62)		(1.71)
Partial F				11.3^{***}
Num. obs	663	663	663	663

Notes: In columns (1)-(3), the dependent variable is the 3 month change in employment per 100 pre-pandemic employees. In column (4), the dependent variable is the 3-month decline in claimants per 100 pre-pandemic employees. The instrument is an indicator for halting month of EUB. Standard errors are robust, finite-sample corrected and clustered by census division. Regressions include state fixed effects and are weighted by state pre-pandemic employment level (1/2020). See text for additional details.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

from the group of those losing EUB is not possible using state level data.

Column 2 contains the reduced-form estimate, which directly regresses the outcome on the instrument (as well as the additional control variables).[16] The coefficient equals 1.53 (SE=0.62), indicating that benefit cessation causes employment over a three month horizon (inclusive of the impact month) to increase by 1.53 percent of pre-pandemic employment. Since pre-pandemic employment was roughly 157 million, the estimate implies that a nationwide cessation policy would have increased employment by 2.4 million jobs.

This reduced form estimate should be of particular interest to a reader concerned that there may be other channels—besides labor supply increasing through incentive

[16]The regression weights and standard error methodology follow those for the two-stage least squares estimates described above.

effects—operating to influence observed employment growth. For example, an observer could argue that labor demand might increase as firms see their probability of filling vacancies increase as labor supply shifts out. In our view, quantitatively, the main jobs effect channel is the labor supply impact of millions losing benefits, although this is untestable without further econometric structure. Nonetheless, our reduced form estimate provides the total jobs effect of halting benefits, independent of the relative importance of the labor supply versus demand channels. The large, positive reduced form jobs effect is also confirmed by the two-way fixed effects results presented earlier (see Figure 3).

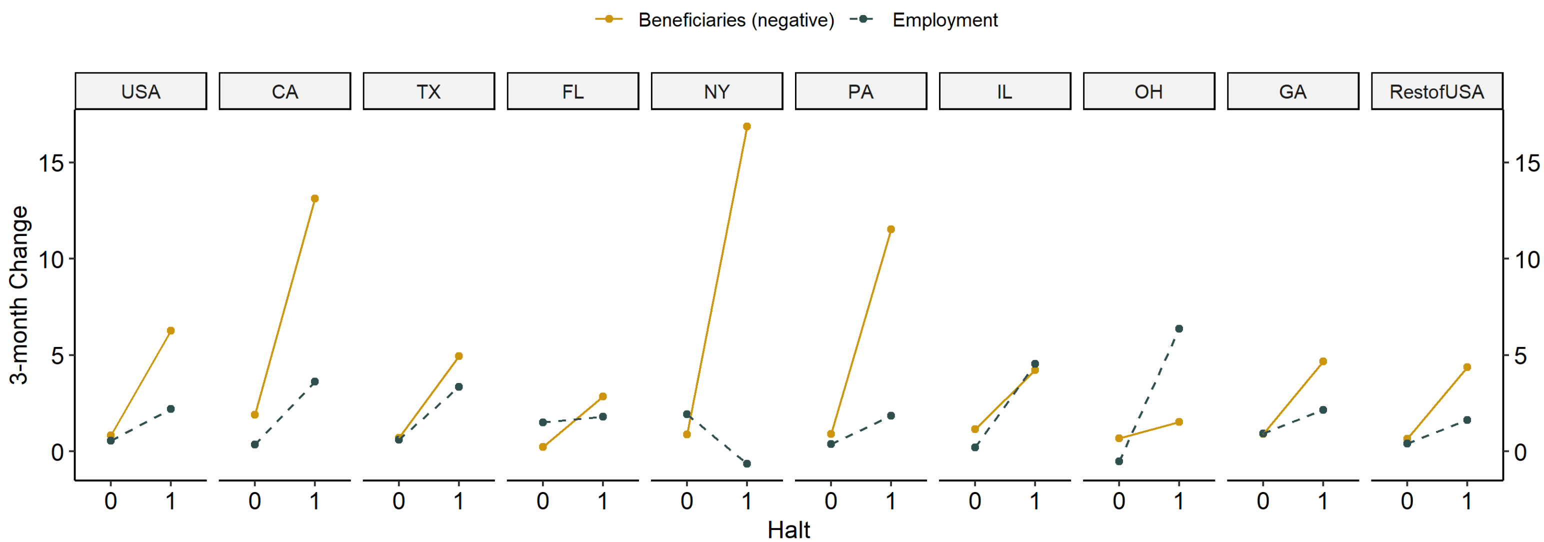
Column 3 presents the least squares analog of the benchmark two-stage least squares coefficient from column 1. Without instrumenting, there is no significant relationship between changes in the number of EUB and the number of employed.

Column 4 contains the first-stage regression estimate, i.e., regressing the scaled change in beneficiaries on the halt indicator, along with the control variables. The point estimate equals 5.75 (SE=1.71), indicating that over a three-month horizon EUB falls by 5.75 percent of pre-pandemic employment. This implies that, had every state simultaneously halted EUB, the number of beneficiaries would have fallen by 9.03 million persons over this horizon, relative to a no-halt counterfactual. The partial F statistic from the first stage (a diagnostic to assess instrument weakness) equals 11.3, which indicates a strong instrument.

Figure 4 presents a visualization of the jobs effect we identify. It consists of ten panels, each of which plots four data points. There are panels for the national total, each of the eight largest states and the sum of the remaining states. The vertical axis indicates the three-month change in either employment or decline in non-working beneficiaries per 100 pre-pandemic workers. The horizontal axis for each panel assigns the value one in the month of cessation and zero to the relevant non-halting months.[17]

[17] The relevant non-halting months are given by equation (3).

Figure 4: 3-month change in non-working beneficiaries and employment comparing halting and non-halting months



Note: USA and RestofUSA are aggregates of state level data using pre-pandemic employment weights. Halt=1 indicates month of program termination; Halt=0 indicates relevant non-halting months, determined by equation (3). 3-month changes are measured per 100 pre-pandemic workers. See text for additional details.

Consider Texas (the third panel from the left). The dashed blue line connects two points. The left point on this line gives the average three month change in employment, per 100 persons, across all clean control months in the sample in which Texas did not initiate the cessation of benefits. The right point gives the three month change in employment starting from the month before cessation. For each state, the left point is an average and the right point is a single observation. The two values for Texas are 0.62 and 3.35 respectively. Thus Texas saw an increase in its 3-month employment change of 2.73 (=3.35-0.62) between its halting month and its average non-halting month. Texas employment increased more rapidly in the halting month relative to the average non-halting months, which is immediately evident from the line's upward slope.

The yellow solid line is constructed similarly except it represents the reduction in the number of beneficiaries in the state between the halting month and the average non-halting months. The figure indicates that the three month decline in beneficiaries was 4.95 in the halting month and 0.70 in the average non-halting month for Texas. The difference equals 4.25. Taking the ratio of the employment change effect and the (reduction in) beneficiary effect, we have 0.64.

The figure's leftmost panel presents the same information as described above, except it presents the weighted sum of the state specific values across the entire U.S. Both lines are upward-sloping, indicating that halting was associated with both faster employment growth and a faster decline in beneficiaries. The ratio of the two slopes equals 0.30. Thus, according to this tabulation, employment increased by 30 persons for every 100 persons losing benefits as a result of program termination.[18]

Next, we examine how the estimate varies as we modify the benchmark specification along several dimensions. We, in turn, change the sample period, sample states and

[18] The difference between the jobs effect from this tabulation and our benchmark estimate is due to our conditioning variables. We discuss the role of conditioning variables below.

Table 2: Jobs effect of benefit termination: employment change (over three months) per 100 person reduction in beneficiaries, various alternative specifications

Vary Controls/horizon	(1) Bmark	(2) Drop St FE	(3) Drop Lag Cntrls	(4) k = 1	(5) k = 3
Decl in EUB	26.60**	24.15**	31.55***	19.21**	30.03
	(9.90)	(10.00)	(8.35)	(8.11)	(28.98)
Partial F	11.3***	11.1**	13.9***	10.9**	15.4***
Num. obs	663	663	663	714	612
NSA/Vary sample months	(6) NSA	(7) M = 3	(8) M = 5	(9) Start Nov	(10) Start Dec
Decl in EUB	28.72**	27.59**	24.96**	27.22**	23.58**
	(12.13)	(10.02)	(10.76)	(10.55)	(9.03)
Partial F	11.3***	11.4***	11.5***	9.6**	11.6***
Num. obs	663	714	612	612	561
Vary sample states	(11) Drop IN, MD	(12) IN, MD in Sept	(13) Drop OH, NY	(14) June Halt	(15) Sept Halt
Decl in EUB	26.36**	27.84**	31.99**	34.19	16.42*
	(10.01)	(10.22)	(10.26)	(38.57)	(8.54)
Partial F	12.2***	10.4**	13.3***	23.1***	11.2**
Num. obs	637	663	637	286	325

Notes: The dependent variable is the 3 month change in employment per 100 pre-pandemic employees. The instrument is an indicator for the EUB halting month. Standard errors are robust, finite-sample corrected and clustered by census division. Regressions include state fixed effects unless otherwise described and are weighted by state pre-pandemic employment level (1/2020). See text for additional details.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

projection horizon and also omit the seasonal adjustment. None of these modifications have a substantial impact on the estimated effect. Table 2 presents 15 sets of estimates. First, entry 1 restates benchmark estimate. Entry 2 and 3 drop the state fixed effects and lagged control, respectively. The estimate size decreases slightly to 24.15 (SE = 10.00) when we remove state fixed effects and increases slightly to 31.55 (SE = 8.35) when we drop the lagged controls.

Next, recall that the benchmark estimate corresponds to the employment change two months following cessation (i.e., $k = 2$). Entries 4 and 5 report the jobs effect at two other horizons: $k = 1$ and $k = 3$. At $k = 1$, the jobs effect is 19.21 (SE=8.11) and at $k = 3$ the jobs effect is 30.03 (SE=28.98). This indicates an increasing employment effect of benefit cessation which begins to dissipate at the last horizon. For the impact month $k = 0$, in results not reported here, the coefficient is small and not statistically different from zero, indicating that the jobs effect occurs with delay.

Entry 6 omits the seasonally adjustment of our outcome variable, which has no material change in the estimate. Next, recall our benchmark case sets $M = 4$, the final horizon for which the treatment influences the outcome. Entries 7 and 8 modify the benchmark so that $M = 3$ and $M = 5$ instead. This results in no substantive change in each case. Entries 9 and 10 vary the start date of the sample (using either November 2020 or December 2020 instead of our benchmark start date of October 2020). The point estimates change slightly, equaling 27.22 (SE=10.55) and 23.58 (SE=9.03), respectively.Further changes in the sample dates are shown in the Appendix.

Next, because Indiana and Maryland (announced early halters) each delayed implementing their EUB cessation, entry 11 drops these two states from the sample and entry 12 changes $H_{i,t}$ to assign these two states to be late halters. As seen in the table, neither adjustment has an appreciable effect on the point estimate or standard error relative to our benchmark.

In Figure 4, we observe that New York had a large decrease in beneficiaries and a decrease in employment comparing halting and non-halting 3-month changes. In Ohio, employment increased more than beneficiaries decreased in the same comparison. Because these states are large and potentially influential, entry 13 drops both states from the sample. The estimate increases by about 5 people to 31.99 (SE = 10.26). Finally, we change the states included in our sample by running our benchmark specification with only June-halting states and then running the benchmark specification with only September-halting states. The samples are likely too small to find precise estimates, but the estimates are positive and larger for June-halting states.

Next, recall that we do not include month fixed effects among our specifications. Month fixed effects are highly collinear with $H_{i,t}$. Adding month fixed effects results in an imprecisely estimated jobs response. Upon including month fixed effects to the benchmark specification, the jobs response is 28.47 (SE=34.93) with a partial F-statistic equal to 21.9. Thus, while the point estimate changes little with this modification, the standard error increases dramatically.

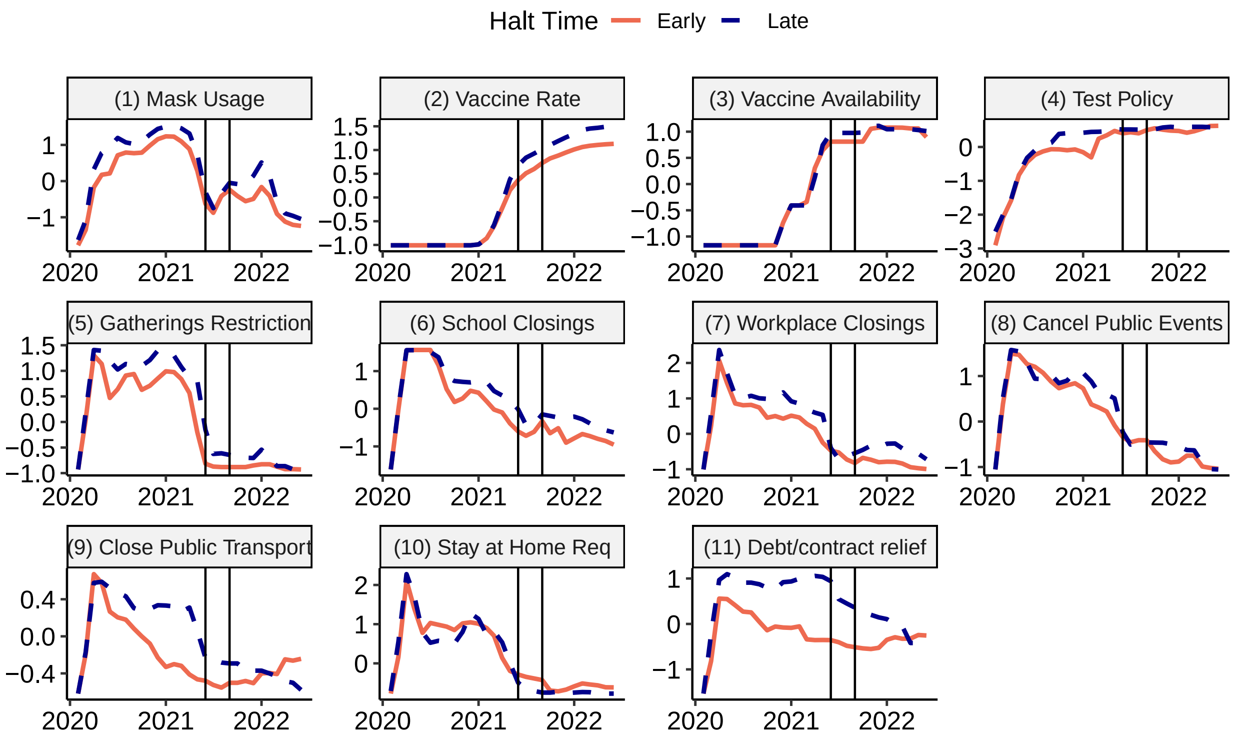
One potential explanation for our large jobs effect might be that early halting states took health policy actions or saw changes in residents' attitudes towards the pandemic that differed relative to late halters. These in turn may have influenced demand for goods and services and thus demand for labor. If the timing of these changes aligned with halting EUB, public health policy or residents' attitudes may have driven the employment increase (relative to late halting states) rather than EUB cessation.

Public attitudes and government policies related to the COVID-19 pandemic differed over time and by state. Fortunately, monthly measures of these policies and attitudes provide conditioning variables one can use to explore this possibility. In early-halting states, the public took fewer precautionary measures against the spread of COVID-19 and government policies were less stringent starting earlier in the pandemic. Figure 5 plots eleven such measures from the Oxford COVID-19 Government Response Tracker.[19] In the figure, we scale each variable to have zero mean and unit variance and average over early and late-halting groups of states, weighted by pre-pandemic employment.

Our measures of public attitudes towards the pandemic (mask usage and the vaccination rate) varied across early and late-halting states. Mask usage rates, shown in panel 1, were persistently higher in late halting states over time. Vaccination rates, shown in panel 2, were similar for early and late halting states for the first few months that vaccines were available, but they had increased to a higher level in late halting states by mid-2021. The figure indicates that several government policy measures tracked closely

[19]More information about each variable can be found at https://github.com/OxCGRT/covid-policy-tracker/blob/master/documentation/codebook.mdcontainment-and-closure-policies

Figure 5: Public attitudes and government policies related to the COVID-19 pandemic



Note: Late-halting state are states that halted on the federal deadline in September 2021 while early-halting states halted before that date. Each variable is scaled across states and months to have mean zero and unit variance. Vertical lines in each graph indicate June 2021 and September 2021.

across the two groups over the entire episode, e.g., vaccine availability (panel 3), public event cancellations (panel 8), and stay-at-home requirements (panel 10). However, the average intensity index of covid testing (panel 4), level of closing of public transport (panel 9), and government debt/contract relief (panel 11) decreased earlier in early relative to late halting states. There were also differences over time in school closings, workplace closings and gatherings restrictions.

Table 3 adds the scaled 3-month change in each policy variable and public attitude

Table 3: Jobs effect of benefit termination: employment change (over three months) per 100 person reduction in beneficiaries, government policy and public perceptions related to COVID-19

Attitudes/Availability	(1) Benchmark	(2) Vaccine Rate	(3) Vaccine Availability	(4) Test Policy
Decl in EUB	26.60**	25.59**	23.49**	26.65**
	(9.90)	(8.79)	(7.37)	(9.53)
Additional Control		0.51	0.31	−0.01
		(0.32)	(0.26)	(0.23)
Partial F	11.3***	15.0***	13.3***	12.1***
Government Policies	(5) Gatherings Restriction	(6) School Closings	(7) Workplace Closings	(8) Cancel Public Events
Decl in EUB	24.49*	26.44**	26.44**	26.58**
	(12.74)	(9.21)	(10.27)	(9.47)
Additional Control	−0.22	0.20	−0.02	0.14
	(0.38)	(0.38)	(0.22)	(0.35)
Partial F	13.5***	11.5***	11.2**	11.4***
Government Policies	(9) Close Public Transport	(10) Stay at Home Req	(11) Debt/contract Relief	
Decl in EUB	26.55**	25.75**	25.33**	
	(9.84)	(10.08)	(10.17)	
Additional Control	0.08	−0.20	−0.18	
	(0.11)	(0.18)	(0.42)	
Partial F	11.3***	12.8***	12.0***	

Notes: The dependent variable is the 3 month change in employment per 100 pre-pandemic employees. Additional controls indicates the control which was added to the benchmark specification identified in the specification header. Instrument is an indicator for halting month of EUB. Standard errors are robust, finite-sample corrected and clustered by census division. Regressions include state fixed effects unless otherwise described and are weighted by state pre-pandemic employment level (1/2020). See text for additional details.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

shown in Figure 5. The benchmark controls, including mask usage, are included in every specification. Each of the other controls (as described in each specification title) are added one at a time to the benchmark specification. Each is included as the variable "Additional Control," in which the coefficent can be interpreted as the change in employment per 100 pre-pandemic employees over three months in response to a one standard deviation increase in the relevant control.

Entry 1 in Table 3 restates the benchmark estimate. Entry 2 adds the vaccination rate to the benchmark controls. Differential time paths of vaccination rates across states might proxy for state residents' concerns about the pandemic and thus willingness to shop and spend. This, in turn, could affect labor demand. If these differences correlated with states' EUB halt month, this could potentially influence our jobs effect estimates. The jobs effect coefficient falls slightly from the benchmark 26.60 to 25.59 and sees a decrease in the standard error. The coefficient on the additional control is positive. Each remaining entry adds a different government policy control.[20] We see that in every case the EUB coefficient is in the range 23 to 27. In all but one case the jobs effect remains statistically significant at a 5 percent level. The partial F statistics indicate that the instrument remains strong across specifications.

Next, we describe the composition of types of those leaving unemployment benefit rolls. If one sums the national number of non-working EUB recipients for the month following each state's respective cessation, this total equals 3.0 million persons. Despite a substantial reduction in beneficiaries, many continued to collect benefits. Two potential reasons account for this effect: (i) As evidenced by Figure 1, some individuals continued to receive benefits for months after program termination (perhaps because of payment delays); (ii) Many individuals on regular state benefits remained on the program despite losing the $300 EUB add on.

With respect to (ii), note that the main two-stage least squares (TSLS) coefficient of interest, i.e., the employment change per 100 persons reduction in beneficiaries, can be expressed as the ratio of a reduced form coefficient to a first stage coefficient.[21] Regressing the employment change on the halt month produces the reduced form coefficient. Regressing the beneficiary reduction on the halt month produces the first stage coefficient. As such, the TSLS coefficient is higher when the employment change coefficient (reduced form) is large and lower when the beneficiary reduction coefficient (first stage) is small. A lower first-stage coefficient means, intuitively, that when a state halts, fewer

[20] Note that the Stringency Index, which is included in the benchmark specification, includes all of these policies except debt/contract relief.

[21] Here, we have already conditioned out the additional covariates.

people are actually leaving the benefit rolls.

Table 4: Reduction in claims in response to halting and pre-halt percent of claims, by type

	Reduction in Claims per 100 workers		
	Total	PUA & PEUC	Regular
Halt month indicator	5.75	5.37	0.38
Expressed as a % of total		93.3%	6.7%
Pre-halt claimant type (%)		80.9%	19.1%

Notes: Each dependent variable is a three-month reduction in EUB recipients per 100 per-pandemic employees (for either total, PUA + PEUC, or regular state programs). The final row gives the share of pre-halt claimant types (PUA + PEUC, or regular state programs) in the month preceding each state's halt date, summed across states. Regressions are weighted by pre-pandemic employment.

Next, our treatment is the reduction in the number of beneficiaries as measured from state claims data. When a state halted EUB, this policy change worked through two channels. First, claimants on PUA (e.g., gig workers) and PEUC (i.e., regular state program claimants whose regular state benefits had expired) lost benefits when their state halted.[22] Second, a portion of claimants on regular state programs lost their $300 add-on and ceased making claims even though their regular benefits had not yet expired.

Those in the first group automatically belong to the treatment. Among the second group, apart from the knife edge case of those whose regular benefits were about to expire, there was a selection into the treated group. An individual who continues to col-

[22]One caveat is, as noted previously, some claims of these continued after the halt month, likely due to administrative delays.

lect regular state benefits—even after losing the $300 add on—would not cease to be a claimant and therefore would not belong to the treated group.

One reason for the large effect, i.e. second-stage coefficient, is that we estimate a relatively small first-stage coefficient. This small coefficient is, in part, due to the participation choice of the regular program recipients: Many of these recipients did not leave the benefit rolls upon losing the $300 add-on but maintained their regular benefits.

To demonstrate this, Table 4 contains the first-stage regression estimate presented earlier, except we additionally break the claimant decline into two types: PUA/PEUC and regular program. The first row contains the reduction in claimants for each type and its total. The total decline coefficient equals 5.75. Observe that a 5.27 claimant reduction comes from the PUA/PEUC pool and 0.38 come from the regular program pool. Thus, as expressed in the second row, only 6.7 percent of the recipient decline came from the pool of regular program participants. In contrast, averaged across states, 19.1 percent of recipients in the month preceding the halt month were regular program participants. As such, many regular program claimants continued to take their regular benefits even after losing their $300 add-on. The overall jobs effect depends on both how the change in employment as well as how the reduction in beneficiaries responded in halting versus non-halting months.

Table 5 reports estimates from several placebo specifications. Each column uses our reduced form specification which regresses our employment change variable on the instrument (halt month). In the placebo specifications, halt months are artificially assigned to different periods.

First, in column 2, we estimate our reduced form specification with halt dates pushed backward by three months (e.g., September halting states counterfactually halt in June and June halting states counterfactually halt in March). This method keeps the groupings of halting and non-halting states the same, but assigns the treatment to a time period in

Table 5: Reduced form placebo tests: Impact of EUB halting on employment change (over three months), placebo tests

	(1) Bmark Reduced Form	(2) Push Back 3 Months	(3) Random Months	(4) Push Back 3 Years
Halt Month	1.53^{**}	0.29	0.12	-0.08
	(0.62)	(0.52)	(0.42)	(0.58)
Num. obs	663	663	663	663

Notes: The dependent variable is the 3 month change in employment per 100 pre-pandemic employees. All columns are variations of our reduced form specification which estimates the impact of our instrument (halt month) on the dependent variable. Standard errors are robust, finite-sample corrected and clustered by census division. Regressions include state fixed effects and are weighted by state pre-pandemic employment level (1/2020). See text for additional details.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

which we expect to find no effect. In column 3, we randomly reassign halt months for each state between January and March. For both of these first two placebo regressions we use the same data and set of cleaned controls as in the reduced form benchmark.

For a third placebo test we push the halt dates backward 36 months to 2018, allowing us to use a set of dates unaffected by COVID-19 to check for effects from seasonality. We construct clean controls using the same method as the benchmark for this new time period. This specification controls for three lags of employment, but not the lagged beneficiary variables or stringency and mask usage indexes. The coefficients for all three placebo specifications are small in size and statistically insignificant.

Table 6 alternatively removes and adds a series of controls that may potentially have impacted employment levels during the COVID-19 pandemic. For each control, we construct a 3-month change ending before $t - 1$ and scale the variables to have mean zero and unit variance. In column 2 we present the benchmark specification estimate, except we drop the mask usage variable and stringency index. In columns 3 and 4, we control for log normalized COVID-19 cases and deaths from *The New York Times*, based on reports from state and local health agencies. Our results are robust to controlling for these mea-

Table 6: Jobs effect of benefit termination: employment change (over three months) per 100 person reduction in beneficiaries, varying the conditioning variables

	(1) Bmark	(2) Drop All Controls	(3) Cases	(4) Deaths	(5) Leis Emp
Decl in EUB	26.60**	30.06*	27.20**	26.62**	27.97**
	(9.90)	(16.14)	(10.22)	(10.05)	(10.57)
Cases			−0.48		
			(0.56)		
Deaths				−0.20	
				(0.47)	
Leis Emp Share					0.37
					(0.59)
Partial F	11.3***	13.2***	10.8**	10.6**	12.3***
Num. obs	663	663	663	663	663

Notes: The dependent variable is the 3 month change in employment per 100 pre-pandemic employees. Instrument is an indicator for halting month of EUB. Standard errors are robust, finite-sample corrected and clustered by census division. Regressions include state fixed effects and are weighted by state pre-pandemic employment level (1/2020). See text for additional details.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

sures of pandemic intensity. In column 5 we control for the leisure and hospitality share of employment to ensure that our results are not driven by differences in labor market dynamics of these sectors across states during the pandemic. The non-farm employment and leisure and hospitality employment data used to construct this share come from the CES (Current Employment Statistics) panel.

We conclude this section by discussing alternative state-level employment measures that one could have used in this study. We use the CPS instead of the CES because the latter excludes gig and contract workers. A large fraction of individuals collecting EUB did so under the Pandemic Unemployment Assistance program — which was established specifically to cover these types of workers. If individuals on this program returned to gig and contract work upon reentry into employment, this would be missed if we had used the employer-based data.

Second, we use the CPS instead of the state-level Local Area Unemployment Statistics (LAUS). Roughly speaking, LAUS employment attempts to control for outliers at the regional level; however, over this two year period, the difference between the LAUS and state-aggregated CPS measures are systematically related to whether or not a state has halted. If the LAUS were simply "controlling for outliers," there is no reason to believe that this noise would be correlated with states' halt months.[23]

5 Conclusion

This paper establishes a strong positive casual link from a reduction in the number of emergency unemployment beneficiaries to state employment growth. We find that for every 100 people in a state that lost unemployment benefits, 27 people became employed in the second month following EUB cessastion. The effect is statistically different from zero and robust to a wide array of alternative specifications.

Note that by focusing on a state-level outcome variable, our estimates are closer to "macro responses" than related studies which instead look at an individual-level outcomes. While interesting from a decision theoretic perspective, using individual-level data may miss important cross-individual spillovers that could be either positive or negative. For example, if a person increases consumption upon losing emergency benefits and then takes a job (e.g., from spending on clothes, fuel and car maintenance for travelling to and from work), then this may drive up demand for goods in the rest of the economy. This may in turn stimulate employment in the state indirectly. This indirect positive effect would be missed in individual-level regressions and thus bias downward the macroeconomic jobs effect of halting unemployment benefits. One could envision

[23] Appendix E and F explain these and other reasons why using either the CES or LAUS datasets would be inappropriate for the question our paper answers.

negative spillovers, on the other hand, that would reverse the direction of the bias. By working with data aggregated to the state-level, we are at least in part immunized against this concern.[24]

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[24]Note that our approach does not take cross-state spillovers into account. If one state halts unemployment benefits there may be positive (or negative) effects on other states’ employment, which would downwardly (or upwardly) bias our estimates.

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Appendix for *The Jobs Effect of Ending Pandemic Unemployment Benefits: A State-Level Analysis*

A Were as many people really on EUB as suggested by Figure 1?

Figure 1 indicates that in California, EUB was roughly 15 percent of prepandemic employment in August 2021. In August 2021, the seasonally adjusted California unemployment rate was 7 percent with 1.34 million unemployed persons according to the LAUS. Is our 15 percent calculation too high?

We compare this estimate to U.S. Department of Labor data on the number of continuing claims in California (which is distinct from the weeks paid out data we use to construct Figure 1 in the paper.). For the week ending August 14, 2021, there were 1.74 million PUA continuing claims and 1.07 million PEUC continuing claims. Together, there were 2.81 million PUA and PEUC claims, more than double the number of unemployed persons in California in that month. This does not include the regular program state claims. In short, our 15 percent calculation for California aligns closely with federal claims data.

One reason that the number of unemployed people was less than the number of recipients is that, in August 2021, some emergency benefit recipients had several ways to collect benefits without conducting a job search (which is a flag for being classified as unemployed in the CPS). For example, according to California Economic Development Department (2021), PUA recipients could meet program requirements by enrolling "in training or education courses that will help the business and does not interfere with an ability to return to full-time self employment." Also, California Economic Development Department (2021) discusses "pandemic-related exemptions to the work search requirement for

PUA recipients such as providing primary care to a person diagnosed with COVID-19 or a child who cannot attend school because of a COVID-19 health emergency."

B For each state and DC, what are the halt months given by your algorithm?

Table 7: Halt dates by state based on our algorithm

Month	States
Jun	AK, AL, AR, FL, GA, IA, ID, IN, MO, MS, MT, ND, NE, NH, OH, OK, SC, SD, TX UT, WV, WY
Jul	AZ, MD, TN
Aug	LA
Sep	CA, CO, CT, DC, DE, HI, IL, KS, KY, MA, ME, MI, MN, NC, NJ, NM, NV, NY, OR PA, RI, VA, VT, WA, WI

Notes: Cutoff dates are the closest CPS survey reference date (the 12th of each month) to the state's official EUB halting dates.

C Didn't a few states delay their implementation of halting EUB?

In our analysis, we set the halt month for Indiana and Maryland as the month each governor's actions initially took place, June 19 and July 3 respectively. In both states, legal challenges and other impediments that followed slowed the actual implementation of the termination of benefits. Both states' actual termination took place in September as the federal dollars ran out.

For robustness, we rerun the regressions using September as the two states' halt month and alternatively drop the two states in the paper's main text. There was no material difference in the estimates relative to the benchmark ones.

D Aren't there other methods (besides local projections) to deal with the staggered treatment issue?

Yes, there are. Unless properly accounted for, using the differences-in-differences method with staggered treatment can induce biased estimates. See Goodman-Bacon (2021) on this point. As such, in the main text of our paper, we adopt the approach of Dube, et.al. (2022), who use local projections. Dube, et.al. (2022) shows that the above bias is not present when local projections are applied correctly.

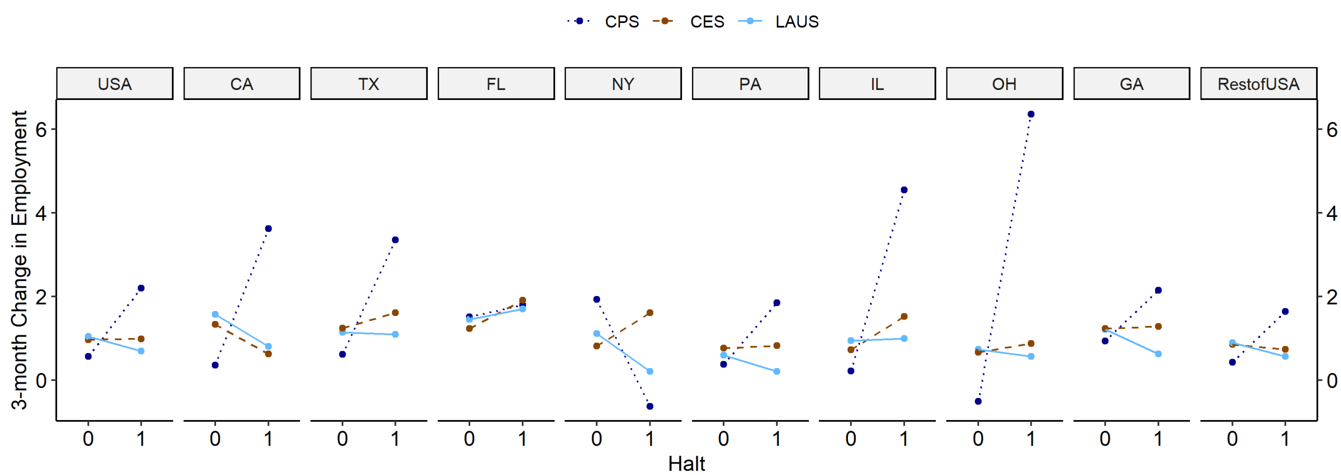
There are other papers that develop approaches to deal with staggered treatment. These include Sun and Abraham (2021) and Callaway and Sant'Anna (2021) as well as a few others not cited here. In our view, Dube, et.al. (2022) is the most straightforward, simplest to implement and easiest to understand.

E Why not construct the outcome variable using the LAUS instead of the CPS?

To estimate our state-level model, one alternative to using the CPS is using the Local Area Unemployment Statistics (LAUS) panel. While the LAUS panel includes a monthly state-level employment measure, as we explain here, the model-based nature of that measure makes it inapplicable in our context.

We start by plotting the data. Figure 6 has the same general setup as Figure 4 from the paper's main body, except here we show 3-month employment changes only and omit the changes in beneficiaries. For each state (or group of states), we plot the employment changes using the CPS, CES and LAUS measure. While the CPS shows substantial changes, the other two do not. For example, the solid light blue line corresponds to the LAUS values. For the US, RestofUS and the plotted states, the LAUS changes are much flatter than the corresponding CPS values. The LAUS sees a much smaller difference across halt and relevant non-halt months.

Figure 6: 3-month change in employment comparing halting and relevant non-halting months, three employment measures (seasonally adjusted)



Note: USA and RestofUSA are aggregates of state level data using pre-pandemic employment weights. Halt=1 indicates month of program termination; Halt=0 indicates relevant non-halting months, determined by equation (3). See text for detailed description. 3-month changes are measured per 100 pre-pandemic workers.

Next, we examine what drives the difference between the CPS and LAUS measures. In particular, we explain how the smoothing procedure used for the CPS systematically removes employment fluctuations that are correlated with employment when states terminate EUB. First, BLS (2020) explains how the LAUS measures are imputed: "Estimates for states are derived from signal-plus-noise models that use the monthly employment and unemployment measures tabulated from the Current Population Survey as the primary inputs."[25]

BLS (2022) explains that the LAUS estimates are "model-based," rather than tabulated from direct sampling. BLS (2022) states:

> The signal-plus-noise model postulates that the observed Current Population Survey estimate consists of a true, but unobserved, labor force value (the signal) plus noise that reflects the error arising from taking a probability sample rather than a complete census of the population.

If the LAUS were simply smoothing out noise that results from taking a probability sample rather than a complete census of working and non-working adults, then we would expect this noise to be uncorrelated with our instrument: each state's halt month.

To assess whether this is the case, we construct the monthly deviation of the LAUS employment from its comparable state-level-aggregated CPS value. That is, define

$$noise_{i,t} = 100 \times \left(Y_{i,t}^{laus} - Y_{i,t} \right)$$

where we use the seasonally adjusted values of both variables. We run the following regression

$$\frac{noise_{i,t+2}}{Y_{i,Jan20}} = \alpha + \beta H_{i,t} + \epsilon_{i,t+2}$$

We run the regression for the same sample as our benchmark specification. We weight regressions and compute standard errors as in our benchmark specification.

[25] BLS (2020) also notes that payroll employment estimates from the CES survey are also used as model inputs.

Table 8: Effect of halt month on CPS "noise" (LAUS employment - CPS employment)

	(1)	(2) State FEs	(3) State and month FEs
Halt month	-0.97^{**}	-0.97^{**}	-0.83^{***}
	(0.37)	(0.39)	(0.29)
Num. obs.	867	867	867
N Clusters	51	51	51

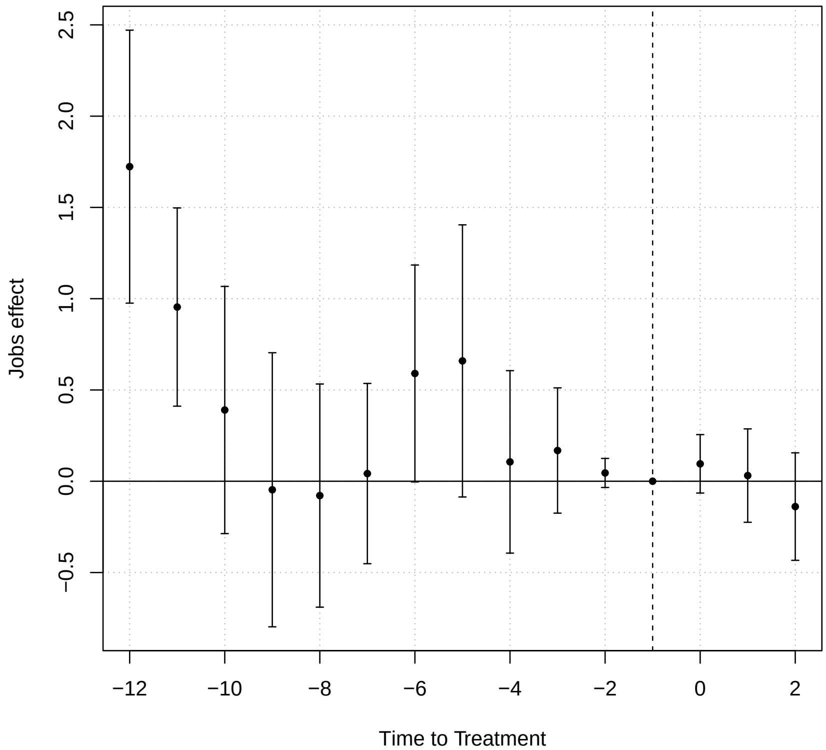
$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Column 1 of Table 8 reports a coefficient of -0.97, which implies that two months following a state's halt month, the noise term is reduced by 0.97 percent. Rather than eliminating noise due to sampling, the coefficient indicates that the "noise" may be resulting from the large employment effect of the cessation of EUB.

This suggests that the signal-to-noise procedure is masking the causal effect of employment that can be seen directly if one uses the (state-level-aggregated) survey-based CPS employment panel. This effect is statistically significant at a five percent level. Column 2 adds state fixed effects. Column 3 adds both state and month fixed effects. In both cases, the effect of halt month on "noise" is negative and statistically different from zero.

BLS (2021) detailed difficulties with constructing the LAUS series during this episode, noting that COVID 19 presented "an unprecedented challenge due to its magnitude and scope." For these reason, we use the state totals from direct CPS respondents as our employment measure.

Figure 7: Effect of halting EUB on CES employment pre and post treatment, 90 percent pointwise confidence intervals



Note: Outcome variable=100 × Ratio of CES employment level to pre-pandemic employment level. We weight regressions by pre-pandemic employment and compute standard errors clustering by census division.

F Why not construct the outcome variable using the CES instead of the CPS?

The CES employment data are not suitable for our purposes. First, as explained in the main text, the CES excludes gig and contractor workers. Pandemic Unemployment Assistance benefits were specially established to provide benefits to workers in this category who lost employment. If individuals on this program returned to gig and contract work upon reentry into employment this would be missed if we had used the employer-based CES data.

Second, a previous literature has found that the household employment survey marks changes (turning points) in employment series more rapidly than the CES. For example, Bolwer and Morisi (2006) discusses the historical discrepancies between the household survey and CES found that after the trough of the 2001 recessions: "establishment survey employment continued to decline while household survey employment began to show signs of growth." The authors cite firm births and deaths, "off-the-books" employment and self employment as potential reasons for the discrepancy. Cajner, et.al. (2022) argue that firm birth and death errors in the CES are typically small but large in recessions using the 2007-2009 Recession as an example.

Nonetheless, one might expect establishment survey employment to show a muted (relative to CPS employment) but positive response to halting EUB. To investigate this, we present an event study plot similar to Figure 3 except we use state-level CES employment to construct our outcome variable. Figure 7 plots the analogous coefficients and confidence regions. The figure indicates no substantive difference between the treated group (June halters) and untreated group (September halters) in the first few months following treatment. While the above discussion of the CES employment construction likely plays a role in explaining the apparent disconnect between the CES and CPS employment responses, the issue merits further investigation.

G What if you control for labor market conditions or change sample dates?

In Table 9 we show several additional specifications of interest. We investigate whether a change in average wages in across states affected employment by controlling for the

Table 9: Jobs effect of benefit termination: employment change (over three months) per 100 person reduction in the number of beneficiaries, additional specifications

Additional Controls	(1) Benchmark	(2) Add Hourly Earnings	(3) Add Job Openings
Decl in EUB	26.60^{**}	26.81^{**}	25.98^{**}
	(9.90)	(10.12)	(8.61)
Hourly Earnings		-0.08	
		(0.14)	
Job Openings			0.23
			(0.24)
Partial F	11.3***	11.6***	11.9***
Num. obs	663	663	663
Additional Start Dates	(1) Start Aug	(2) Start Sept	(3) Start Jan
F2D3claims	26.30^{**}	27.50^{***}	22.65^{**}
	(8.44)	(7.77)	(8.49)
Partial F	14.5***	15.8***	11.9***
Num. obs	714	765	510

Notes: The dependent variable is the 3 month change in employment per 100 pre-pandemic employees. Instrument is an indicator for halting month of EUB. Standard errors are robust, finite-sample corrected and clustered by census division. Regressions include state fixed effects and are weighted by state pre-pandemic employment level (1/2020). See text for additional details.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

change in hourly earnings from the CPS Outgoing Rotation Group/Earner Study. Following Gittleman and Pierce (2012) we use hourly wages for those who are paid on an hourly basis and weekly earnings divided by hours usually worked per week for those paid on a weekly basis. We deflate using CPI. Top coded weekly earnings are multiplied by 1.5 and we drop hourly earnings below 1.675 in 1982 dollars and hourly wages exceeding 1/35 the top-coded value of weekly earnings. We use the earnings weight from the CPS MORG as a weight to aggregate individuals to the state level. We then take the three-month change of the variable and scale it to have mean zero and unit variance. The specification results shown in column (2) show that this control did not have a significant impact on the three-month change in employment and adding it did not impact our benchmark results.

Changes in labor demand across states may also have affected employment in a way that would impact our results. In the third specification of Table 9, we control for labor demand using the scaled 3-month change in job openings by state from the Job Openings and Labor Turnover Survey. Controlling for this variable does not impact our benchmark results. In the second row, we add three specifications with different sample start dates to further demonstrate that the results are not sensitive to changing the sample.

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