

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

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## Abstract

This note 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, other states stopped in June and July. Using the cessation month as an instrument, we estimate the causal effect on employment of reducing unemployment rolls. In the first three months following a state's program termination, for every 100 person reduction in beneficiaries, state employment causally increased by about 35 persons. The effect is statistically different from zero and robust to a wide array of alternative specifications.

## 1 Introduction

Between February and April of 2020, U.S. employment declined by 22 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.<sup>1</sup> 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, employment growth began to slow and job openings were steadily rising, reaching 9.6 million in May 2021, up 2.6 million from their pre-pandemic level. At the same time, some state governors voiced concerns that the historic 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 20 halting participation between June 19 and July 3.

In this paper, we use this asynchronous benefit termination across states to estimate the causal effect on employment of losing unemployment benefits. To fix language, we shall refer to states ending EUB in September as late halters and state ending EUB before September as early halters.

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 halters was the governor's political party. Despite the important role of political party for determining which states were early halters, the two types of states tended to differ on another dimension. On average, 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

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<sup>1</sup>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).

gaps” immediately before treatment. With smaller remaining gaps, employment growth for early halters had already slowed relative to late halters. With smaller remaining gaps, EUB recipient rolls in early halting were generally lower relative to late halting states. In May 2021, for example, EUB per capita in late halters were 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.

Our outcome variable is the state-level three month change in employment. In our benchmark specification, we estimate that in the three months following a state’s EUB termination, for every 100 person reduction in beneficiaries driven by program termination, employment increases by roughly 35 persons. Thus, there was a strong, rapid jobs 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. 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 also show that the results are robust to removing lag controls, seasonal adjustment and fixed effects.

The remainder of this short paper presents the data, econometric model and results. 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)).<sup>2</sup>

## 2 EUB Payouts Before and After Program Cessation

In May 2021, over 10 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 quickly take up work? Our approach is 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 benefits halting 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. For 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

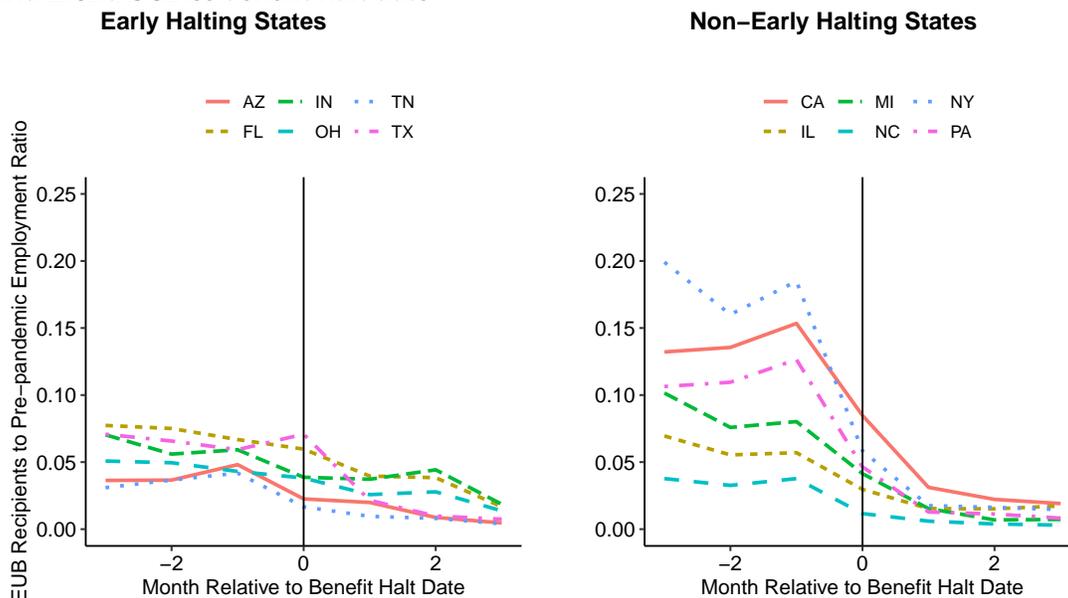
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<sup>2</sup>Several papers have looked at the labor decision effects of the introduction of EUB (e.g., Petrosky-Nadeau and Valletta (2021), Altonji, et.al. (2020) and Coombs, et.al. (2021)). These papers find small behavioral effects of EUB, except Altonji, et.al. (2020), who find no effect. In the 2007-2009 recession, Farber, Rothstein and Valletta (2015) found impacts of halting federal unemployment benefits on labor force attachment but not employment.

that could finance consumption and allow them to remain out of the workforce. As such, 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.<sup>3</sup> 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, Arizona (red solid line) had recipients equal to roughly 2.3 percent of pre-pandemic employment in the halt month. Benefit recipients in that state were relatively stable before halting; they began to decline following halting. The right panel presents the analogous data for six large late halters. 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.

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 Bureau of Labor Statistics and include PUA, PEUC and regular claimants. Pre-pandemic employment levels are for January 2020 and are from CPS micro data aggregated to the state level.

Importantly, continuing EUB for two of the main programs, PEUC and PUA, did not collapse to zero in the month past cessation.<sup>4</sup> This may indicate that some EUB beneficiaries were still

<sup>3</sup>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.

<sup>4</sup>PUA and PEUC stand for Pandemic Unemployment Assistance and Pandemic Emergency Unemployment Compensation.

receiving EUB for weeks of unemployment preceding EUB program cessation. Given the slow decline, our benchmark model looks at the three month employment changes following the halting of EUB rather than the initial month’s effect.<sup>5</sup>

### 3 Data and Econometric Model

We study monthly data from 46 states and the District of Columbia covering December 2020 through December 2021.<sup>6</sup> 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 seasonally adjust our data using the Census Bureau’s X-13 procedure in order to control for seasonal changes in employment.

Importantly, we use the CPS instead of the employer-based Current Establishment Survey 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.<sup>7</sup>

Let  $C_{i,t}$  denote the number of non-working emergency unemployment beneficiaries in state  $i$  in month  $t$ , measured in hundreds of persons. We construct this variable using BLS data on the number of weeks of emergency unemployment benefits paid, which we call  $B_{i,t}$ . First, we assume that a state resident collecting EUB receives four weeks of benefits per month. 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, its important to exclude this 20 percent from the treatment.<sup>8</sup> Finally, we measure the final variable in hundreds of persons to ease interpretation of our estimated coefficients. Thus,

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

We exclude Alabama, Georgia, South Carolina and Vermont because the BLS does not provide the complete recipients data needed for our variable construction.

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<sup>5</sup>In results not reported here (but available on request), we find that the jobs effect of benefit termination was largest about three months after a state ceases EUB.

<sup>6</sup>In results not reported here (but available on request), changing the start month from the benchmark of December 2020 in either direction by two months has only a small impact on the estimated jobs effect.

<sup>7</sup>A second alternative employment measure to the CPS is the Local Unemployment Area Statistics data set. We do not use this alternative data because of problems with the data during this episode, which are discussed in Arbogast and Dupor (2022).

<sup>8</sup>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.

We estimate the jobs effect of EUB termination using a panel estimation equation, given by:

$$\frac{Y_{i,t+2} - Y_{i,t-1}}{Y_{i,Jan20}} = \theta_i + \psi_{t+2} - \gamma \frac{C_{i,t+2} - C_{i,t-1}}{Y_{i,Jan20}} + \beta' X_{i,t-1} + \varepsilon_{i,t+2} \quad (1)$$

where  $\theta_i$  and  $\psi_{t+2}$  are state and time dummies, and  $X_{i,t-1}$  is a vector of conditioning variables. We estimate (1) using instrumental variables where we instrument using a dummy for the month a state ceased EUB provision. We weight regressions by state pre-pandemic employment. We compute robust standard errors adjusted with a finite-sample correction.

The main coefficient of interest is  $\gamma$ , which is interpreted as the causal impact on employment of a 100 person reduction in benefit recipients driven by program termination. Our benchmark condition variable vector  $X_{i,t-1}$  contains three lags of the one month change in EUB and the one month change in employment, where each variable is scaled by pre-pandemic employment. The benchmark controls for the mask usage rate using the percent of the population reporting always wearing a mask when leaving their home from the Institute of Health Metrics and Evaluation. It also controls for COVID-19 lockdown intensity using an index from the Oxford COVID-19 Government Response Tracker that averages over indicator variables for containment policies, closure policies and public information campaigns. For both controls, we construct a 3-month change to align with the timing of the dependent variables and scale the variables to have mean zero and unit variance.

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 CPS employment 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 three states, we choose June as the treatment month. Based on this procedure, three states halted in July whereas the rest halted in either June or September.

## 4 Results

This section reports and interprets estimates of equation (1) and its variants. The results deliver a consistent message: Terminating EUB had a quantitatively and statistically significant positive impact on employment, a finding which we show is robust to many sensible alternative statistical specifications.

First, column 1 of Table 1 contains our benchmark 2SLS estimate. The coefficient equals 35.32 (SE=16.57), indicating that employment increases by 35 persons for each 100 person reduction in EUB in the three months (inclusive of the impact month) following a state's EUB cessation. The estimate is statistically different from zero at a five percent level.

Table 1: Jobs effect of benefit termination: employment change (over three months) per 100 person reduction in the number of beneficiaries

	Dep Var: Emp Change			Dep Var: Decl in claims
	(1) 2SLS (Bmark)	(2) Reduced Form	(3) OLS	(4) First Stage
Decl in EUB	35.32 (16.57)		-0.78 (4.28)	
Halt month		1.06 (0.47)		3.16 (0.73)
Partial F				18.5

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 and finite-sample corrected. Regressions include state and time fixed effects (not reported) and are weighted by state pre-pandemic employment level (1/2020). Sample covers 12/2020 to 12/2021 and excludes AL, GA, SC, and VT because of unavailability of claims data. Each regression includes the mask usage rate, a lockdown intensity index and three lags of the scaled one-month change in recipients and in employment change.

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 from the group of those losing EUB is not possible using our approach.

Column 2 contains the reduced-form estimate, which directly regresses the outcome on the instrument (as well as the additional control variables).<sup>9</sup> The coefficient equals 1.06 (SE=0.47), indicating that benefit cessation causes employment over a three month horizon (inclusive of the impact month) to increase by 1.06 percent of pre-pandemic employment. Since pre-pandemic employment was roughly 150 million, the estimate implies that a nationwide cessation policy would have increased employment by 1.6 million persons relative to a no-cessation counterfactual.

Column 3 presents the least squares analog of the benchmark 2SLS coefficient from column 1. Without an instrument, there is no significant relationship between changes in the number of EUB and the number of employed, likely due to EUB being endogenous.

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 3.16 (SE=0.73), indicating that over a three-month horizon EUB falls by about 3 percent of pre-pandemic employment. This implies that, had every state simultaneously halted EUB, the number of beneficiaries would have fallen by 4.5 million persons over this horizon, relative to a no-halt counterfactual.

Figure 2 presents a visualization of the jobs effect we identify. It consists of ten panels, each of

<sup>9</sup>The regression weights and standard error methodology follow those for the 2SLS estimates described above.

which plots four data points. There is a panel for the nation total, the eight largest states and the sum of the remaining states in our dataset. 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 in all other months (i.e., non-halting months).

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 months in the sample in which Texas did not initiate the cessation of benefits. The right point gives the three month change in employment following the month that cessation occurred. For each state, the left point is an average and the right point is a single observation. The two values for Texas are 1.23 and 3.36 respectively. Thus Texas saw an increase in its 3-month employment change of 2.13 ( $=3.36-1.23$ ) 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 4.91 in the halting month and 1.39 in the average non-halting month for Texas. The difference equals 3.52. Taking the ratio of the employment change effect and the (reduction in) beneficiary effect, we have 0.61.

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 0.23. Thus, according to this tabulation, employment increased by 23 persons for every 100 persons losing benefits as a result of program termination.<sup>10</sup>

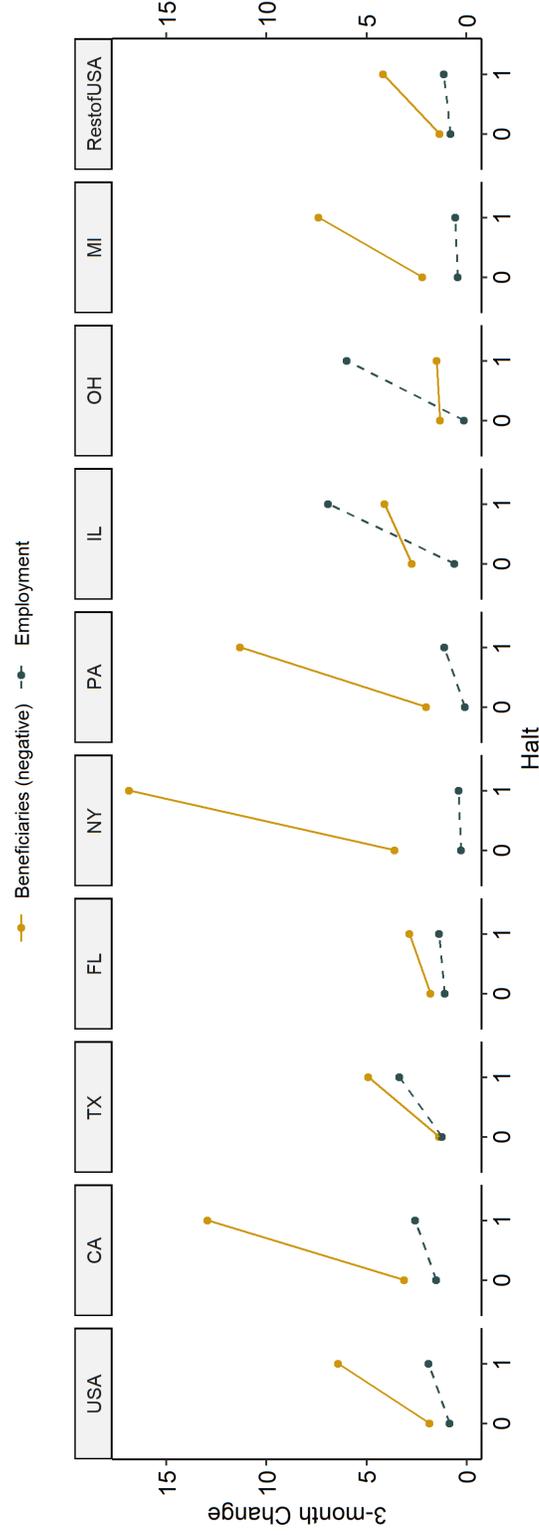
If one sums the number of non-working EUB recipients across states in each state for the month following each state's respective cessation, this total would equal 2.9 million persons. This indicates that, despite a substantial reduction in beneficiaries, many continued to collect benefits. Two potential 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 that program despite losing the \$300 EUB add on. Note that the point estimate is statistically different from zero at a five percent level, indicating that the halt month is a strong instrument.

Table 2 demonstrates the robustness of our results to several additional variants of the benchmark. Column 1 restates the benchmark specification. Column 2 drops all three lags of one month employment change. Column 3 uses non-seasonally adjusted data. Columns 4 and 5, respec-

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<sup>10</sup>The difference between the jobs effect from this tabulation and our benchmark estimate is due to our conditioning variables (mainly the month fixed effect). We discuss the role of conditioning variables below.

Figure 2: 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 all other months. 3-month changes are measured per 100 pre-pandemic workers.

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

	(1) Bmark	(2) Drop Lags	(3) Not Seas	(4) No St FE	(5) No Mo FE
Decl in EUB	35.32 (16.57)	34.97 (17.47)	34.44 (17.10)	37.78 (17.23)	24.66 (12.33)
Emp Change, 1 Mo Lag	-0.34 (0.13)		-0.43 (0.08)	-0.26 (0.11)	-0.39 (0.12)
Emp Change, 2 Mo Lag	-0.30 (0.12)		-0.42 (0.07)	-0.24 (0.10)	-0.35 (0.10)
Emp Change, 3 Mo Lag	-0.13 (0.10)		-0.22 (0.07)	-0.09 (0.09)	-0.13 (0.09)
Partial F	18.5	18.9	17.8	18.6	17.3

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 and finite-sample corrected. Regressions include state and time fixed effects (not reported) and are weighted by state pre-pandemic employment level (1/2020). Sample covers 12/2020 to 12/2021 and excludes AL, GA, SC, and VT because of unavailability of beneficiaries data. Each regression includes the mask usage rate, a lockdown intensity index and three lags of the scaled one-month change in recipients.

tively, drop state and month fixed effects. For columns 2 through 4 the point estimate is nearly unchanged.<sup>11</sup>

Removing the month fixed effect (column 5) has the largest impact on the jobs effect estimate. The jobs effect falls from the benchmark 35 persons to 25 persons. The coefficient remains statistically significant at a five percent level. As such, the modification is consistent with the main lesson from the episode: Terminating benefits had a large stimulative employment effect.

Table 3 adds a series of controls that may potentially have impacted employment levels during the COVID-19 pandemic. In column 2 we present the benchmark specification estimate, except we drop the mask usage 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 measures of pandemic intensity. 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 Bureau of Labor Statistics' Current Employment Statistics program.

<sup>11</sup>In results not reported in the paper but available on request, we also include several lags of the instrument as conditioning variables. This modification tends to increase the jobs effect but also decreases the precision of the estimate of that effect.

Table 3: Jobs effect of benefit termination: employment change (over three months) per 100 person reduction in beneficiaries, varying the conditioning variables

	(1) Bmark	(2) Drop Controls	(3) Cases	(4) Deaths	(5) Leis Emp
Decl in EUB	35.32 (16.57)	32.62 (15.02)	36.68 (17.52)	35.39 (16.59)	35.30 (16.51)
Cases			1.47 (1.04)		
Deaths				0.19 (0.85)	
Leis Emp Share					0.01 (0.57)
Partial F	18.5	17.2	17.3	18.6	18.4

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 and finite-sample corrected. Regressions include state and time fixed effects (not reported) and are weighted by state pre-pandemic employment level (1/2020). Sample covers 12/2020 to 12/2021 and excludes AL, GA, SC, and VT because of unavailability of beneficiary data. Each regression includes three lags of the scaled one-month change in recipients and in employment change.

## 5 Conclusion

Using the cessation of EUB across states in 2021, we establish a strong positive casual link from a reduction in the number of beneficiaries to state employment growth. The effect is statistically different from zero and robust to a wide array of alternative specifications. Future research might examine the effect of cessation on other labor market outcomes, such as labor force participation, and other economic indicators, such as consumption and income.

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 taking 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 UI benefits. One could envision 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.<sup>12</sup>

<sup>12</sup>Note that our approach does not take cross-state spillovers into account. If one state halts unemployment benefits there may be positive effects on employment on surrounding states as workers seek nearby jobs, which would downwardly bias our estimates.

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