Death Without Benefits: Unemployment Insurance, Re-Employment, and the Spread of Covid*

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Abstract

During a pandemic, unemployment insurance (UI) may have externalities for health. Studying variation across states in UI benefits during summer 2021, using longitudinally linked Current Population Survey data, and controlling for individual and state characteristics, we find that unemployment-employment transitions rise 10 percentage points (42 percent of the unconditional mean) in treated states, which cut UI early, relative to states maintaining higher benefits. Estimates of hazards and censored regressions imply a benefit elasticity of unemployment duration of 0.5-0.7. Using an instrumental variables strategy, we find sharp covid rises in states with higher re-employment rates because of the UI cuts: case, hospitalization, and death rates are all estimated to more than double. Consistent with a causal interpretation, the differences between treated and other states in re-employment and covid outcomes are negligible prior to treatment, diverge simultaneously with the policy change, and re-converge quickly after the end of the policy difference. Results are robust to controlling for other relevant factors and policies. We estimate that additional wages of $1.1bln received by re-employed workers offset only one-eighth of the UI losses, and, even from a government budget perspective, UI savings are more than offset by increased hospitalization costs of $14.8bln. Increases in illness-related losses in work time can be valued at $1.5bln. Beyond the monetary and morbidity costs of the UI cuts, we estimate additional deaths at 27,000. The results suggest an important role for UI during infectious stages of pandemics that should be considered for future policy design.

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1 Introduction

The standard approach to studying optimal unemployment insurance policy design focuses on issues of moral hazard and liquidity resulting from capital market imperfections (Baily, 1978; Chetty, 2008). During a pandemic, however, safety concerns may influence job search behavior and, more importantly because contagion has externalities beyond the worker, they may be relevant for policy design. Under these circumstances, how does UI affect re-employment and what are the consequences for pandemic outcomes, including deaths?

This paper addresses these questions in the context of the special UI programs offered in the U.S. during the covid year of 2021: Federal Pandemic Unemployment Compensation (FPUC), Pandemic Emergency Unemployment Compensation (PEUC), and Pandemic Unemployment Assistance (PUA). These programs, which we collectively label "PUI," raised benefits considerably, including $300 per week from December 27, 2020 in all states, and extended eligibility and duration much longer than the 26 weeks normally standard.[1] Our method exploits cross-state variation in the timing of PUI benefit reduction during the summer of 2021: all the programs were terminated by federal law on September 4, 2021, but 19 states ended the program early, around late June that year. We leverage this large difference across states during July and August to estimate the impacts on re-employment behavior and covid outcomes.

Using longitudinally linked Current Population Survey (CPS) data with detailed controls for other characteristics, we find an approximate 10 percentage point increase in the job-finding rate for experienced unemployed in states ending PUI early versus states continuing the additional benefits. The hazard rises by about 30 percent and predicted duration falls by about 22 percent as a result of the policy change. We estimate the elasticity of unemployment duration with respect to the UI benefit at around 0.5-0.7, depending on specification. The differences between the treated and control states appear only during the two-month period of policy difference and are negligible before July and again afterward, from September 2021. The impact is zero for nonparticipants in the labor force, a placebo group that would be ineligible to receive UI benefits.

These estimated effects of UI on re-employment probabilities and unemployment duration are consistent with magnitudes estimated in previous research from non-pandemic periods.[2]

[1] The extra $300 per week is from the FPUC, while PEUC extended benefits an additional 49 weeks and PUA extended eligibility to self-employed, gig workers, and UI exhaustees up to a total of 75 weeks. For more details on these programs, see Whittaker and Isaacs (2022).

[2] Card et al. (2015) and Schmieder and Von Wachter (2016) summarize recent evidence; see also Kroft and Notowidigdo (2016) and Landais (2015). The literature goes back a long ways: Krueger and Meyer (2002) discuss a large number of earlier studies with similar estimates. One of the earliest, Lancaster (1979), reports the elasticity at about 0.6 and states that "an elasticity of this order could now be regarded as

In evaluating UI policy during a pandemic, however, the paramount consideration may be the possible external benefit of encouraging people to isolate in order to reduce the spread of infection. Investigating the policy's effects on covid, both in reduced form and in an instrumental variable framework to explicitly focus on the channel working through re-employment behavior, we find that ending PUI early resulted in large increases in covid cases and hospital admissions, leading with a short lag to higher mortality. Although the policy difference is short-lived, the estimated magnitudes are large. The exact figures depend on the comparison period: relative to the first half of 2021, the rates approximately double, while relative to the immediate pre-treatment months of May-June, they quadruple. We estimate total additional deaths at about 27,000.

These results control for vaccination rates, weather conditions, school openings, pre-treatment obesity, heart disease, and retail and recreation mobility rates interacted with treatment time. Among a number of robustness checks, we also control for state-specific unit trends, and we draw upon a large database of state-level covid-related policies, controlling for those that changed during the summer of 2021; the results with these controls are very similar. Further evidence for a causal interpretation of the impact of UI cuts on covid outcomes, we find no difference between treated and control states before the policy change happened in July, and the difference vanishes quickly after the policy variation ends in September. Thus, if it were the case that some unobserved factors other than the UI policy change account for the differences in re-employment and covid outcomes, those factors must not only have started in the same states at the same time as the UI cuts but also lasted for precisely the same two-month period.

Assessing the benefits and costs of the early ending of PUI, we estimate that the policy created additional wages for workers who accepted jobs at about $1.1bln and reduced UI spending by about $8.9bln, although the latter are transfers; put differently, workers were able to make up only about one-eighth of their losses in benefits. We also estimate the illness-related losses in work time resulting from the policy at $1.5bln and additional hospitalization costs at $14.8bln. Even taking only government spending into account, the early termination of PUI was at best a wash. We also discuss the standard errors around these estimates and show that even under extreme assumptions, the value of a statistical life implied by the policy choice to cut benefits is negative.

Despite its potential importance for policy design in response to a future pandemic, no previous research has examined the implications of UI for the spread of infectious disease. But while this paper is the first to address that question, our analysis of the impact of UI

established beyond reasonable doubt" (p. 956); also cited by Kiefer (1988), p. 648.

benefits on unemployment duration builds on extensive previous literature.[3] Compared with the prior research on that topic, our context and data have some advantages. The policy change we study in summer 2021 provides large variation in UI benefits, and it affected all UI recipients in the 19 treated states. Our data permit us to focus the analysis on the behavior of unemployed individuals, rather than all non-employed as in some studies, and to measure unemployment duration rather than the duration of benefits. On the other hand, we do not observe whether an individual actually receives benefits, so our estimates should be interpreted as "intent-to-treat." In any event, despite differences in data, time period, and source of identifying variation, our estimates of the UI benefit elasticity of unemployment duration lie within the range of previous non-pandemic studies.

A small number of recent papers examine unemployment insurance during the pandemic, most of them focusing on the CARES Act of March 2020 and the early stages of the pandemic. Carey et al. (2021) contains a detailed analysis of the rapid rise in UI claims, while Ganong, Noel and Vavra (2020) calculate the large increases in replacement rates during that period. Marinescu, Skandalis and Zhao (2021) find a decrease in job applications, but Dube (2021), Petrosky-Nadeau and Valletta (2021), and Ganong et al. (2022) find little to no employment effects of the 2020 policies, with the last reporting a duration-benefit elasticity in the range of 0.1-0.2 based on the re-introduction of the FPUC supplement in December 2020.

Two papers study the summer 2021 episode, both finding significant effects of the PUI cut on employment transitions, but differing in magnitude. Coombs et al. (2022) use direct-deposit bank data for a cohort of account holders who received UI payments in late April; they estimate a 6.8 percentage point increase in job-finding (measured from direct wage deposits). Holzer, Hubbard and Strain (2021) use CPS data and find a 14 point increase, although as Coombs et al. (2022) point out, the standard errors are too large to rule out much smaller estimates. Our estimate of a 10 percentage point effect lies in the middle of these, which we compare in greater detail below, but in our paper this result is just the starting point from which we build in several directions, including estimating hazard and duration models and the benefit elasticity of duration; examining the impact of re-employment on covid cases, hospitalizations, and deaths; and assessing costs and benefits of the policy.

Our paper also contributes to an important and much broader literature on lessons from covid for future pandemics. Most of this research focuses on explicit public health measures such as mask mandates and stay-at-home orders. Bollyky et al. (2023) assess a large number of health, political, and economic factors in a cross-sectional accounting of state-level differences in cumulative covid outcomes and economic performance measures from January

[3] A different policy issue, the length of the eligibility period for UI, is the focus of other research, e.g., Katz and Meyer (1990), Schmieder, Von Wachter and Bender (2012), and Boone et al. (2021).

2020 to July 2022. Related to our paper, they provide results from regressions of state-level GDP and employment on each state's federal unemployment insurance benefit duration and amount. None of the four estimated coefficients is statistically significant, but the identification is purely through cross-sectional variation. Nor does it, or any other paper we have found, estimate the impact of UI on covid outcomes, which is a crucial question for policy planning in anticipation of a future pandemic.

Still more broadly, our paper is related to the literature on the interactions of health and labor. Issues in that literature include the impact of unemployment on health, the use and effects of public insurance programs including disability and medicaid, and the impact of health-related work conditions on wages, sometimes used to infer the value of a statistical life. Our analysis of PUI during the covid pandemic involves two-way interactions between labor supply behavior and spread of disease and thus provides an example of the central dilemma in pandemic policy design: the trade-off between safety and economic outcomes.

In the rest of the paper, after describing our data sources and variable construction, we present estimates of the impact of the UI cuts on unemployment-employment transitions (UE) and then take into account unemployment duration using hazard models and censored regressions. Incorporating estimates of UI benefits, we estimate the elasticity of unemployment duration with respect to UI benefits. Next, we present estimates of the impact of the policy change on infections, hospitalizations, and deaths using both a reduced form and an instrumental variables framework, the latter to assess the hypothesized mechanism whereby UI cuts affect covid outcomes by raising re-employment. We assess the plausibility of the covid estimates drawing upon relevant epidemiological research. For both the re-employment and covid analyses, we report a number of robustness checks, placebo tests, and specification checks. Finally, we attempt to measure the benefits and costs of the early termination of PUI, including higher employment and earnings, on the one hand, and illness-related losses in work time, hospitalizations, and deaths, on the other; UI spending is a transfer. Appendix A contains further details on variable construction, Appendix B provides additional analyses, including alternative specifications and further robustness checks, and Appendix C reports estimates of the heterogeneity of re-employment probabilities by age group, teleworking occupation, marital status, race, and gender.

2 Data

This section describes our data sources, variable construction, and summary statistics. Further detail, particularly on variable construction, can be found in Appendix A.

Our basic data source for studying individual behavior is the IPUMS Current Popula-

tion Survey (CPS) (Flood et al., 2023). For the re-employment analysis, we focus on the "experienced unemployed": unemployed individuals who have previously worked (based on a direct CPS question). For some estimates, we distinguish the experienced unemployed by reason for unemployment: job losers, job leavers, and labor force re-entrants. The CPS does not measure unemployment insurance receipt, so our estimates can be interpreted as the "intent-to-treat" impacts of the change in UI benefits on individuals classified by the CPS as unemployed (not working, available for work, and actively searching or expecting recall) rather than on UI recipients.

We measure re-employment behavior of the experienced unemployed using the longitudinally linked version of the CPS, based on its "4-8-4" rotation panel design (Feng, 2001; Rivera Drew, Flood and Warren, 2014). The data structure for this analysis consists of two-consecutive-month pairs, for individuals observed both months. Restricting the sample to individuals who are unemployed in the first month of the pair, we observe whether they have become re-employed in the following month. The main period for the re-employment analysis is February through August 2021, but we also consider other periods for robustness and to investigate behavior after the policy differences end in September 2021. Individuals may appear multiple times (once for each month they are both unemployed and observed in the sample in the next month), but following Chetty (2008), we restrict attention in most of the analysis to those unemployed up to a year.[4] We also restrict the sample to individuals between 20 and 64 years old, in order to reduce complications associated with schooling and retirement.

The treatment we analyze is the early termination of PUI by some states in early summer 2021. The first terminations, by three states, took place on June 12. Seven terminated on June 19, eight on June 26, and one on July 3. We consider these 19 states as treated. Because the reference period for CPS questions on labor force status is the week containing the 12$^{\text{th}}$ day of the month, and June 12, 2021, fell on a Saturday, the CPS questions for June pertain to the workweek before any states had terminated PUI, while the July CPS pertains to a reference week after all the 19 had terminated.[5] Control states are defined as ending PUI by federal law on September 4, 2021.[6] Thus, by the reference week for the September

[4] Chetty (2008) writes "Throughout, I censor durations at 50 weeks to reduce the influence of outliers and focus on search behavior in the year after job loss" (p. 203). For some of the analysis, we further restrict to durations under 26 weeks, the typical maximum duration under state UI programs. Our results, shown in Appendix B and discussed below, are qualitatively robust to these sample changes.

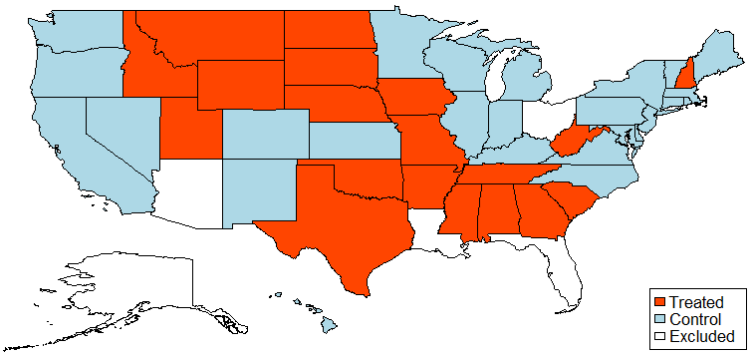
[5] The treated states are Alabama, Arkansas, Georgia, Idaho, Iowa, Mississippi, Missouri, Montana, Nebraska, New Hampshire, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, West Virginia, and Wyoming. Appendix Table A1 contains a list of all states with the date of FPUC, PEUC, and PUA termination and the classification by treated, control, and excluded from our sample.

[6] The control states are California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Indiana, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nevada, New

CPS, all states had terminated PUI.[7]

The differences across states in terminating the PUI programs create both geographic and time variation. Figure 1 contains a map of the states by treated, control, and excluded category. The pre-period is specified as the period before the early ending of PUI, usually from February to June 2021. The treatment period is July and August 2021.

Figure 1: Treated and Control States



Note: Treated states terminated PUI in mid-late June 2021. For detailed information on our classification, see Table A1.

Turning to variables, we define $PUIend_{st}$ as an indicator for states s in months t that equals 1 in July-August 2021 for states ending PUI early, and 0 otherwise. UE_t is an indicator equal to 1 for re-employment, a transition from unemployment in month *t-1* to employment in month t, and 0 otherwise (including remaining unemployed and exiting from the labor force). The hazard rate h_t is the conditional probability of re-employment in t given survival to a given duration in *t-1*. Dur_t is months in the unemployment spell, the answer to a direct question on the CPS asking for how many consecutive weeks a currently unemployed respondent has been jobless and searching; Dur_t is right-censored for incomplete spells (complete spells are measured for $UE_t = 1$).

Control variables include standard demographic characteristics: age, sex, ethnicity, race, education, marital status, child under 12 years old, veteran status, and metropolitan statistical area (MSA). In some specifications, we control for industry and occupation of the unemployed person's last job, but usually we instead control for predicted wage, conserving degrees of freedom. We construct the predicted wage for unemployed individuals in 2020-2022 using data from the CPS Outgoing Rotation Groups (ORGs) in 2015 to 2019 on weekly earnings (reported directly or computed as hourly wage times usual weekly hours) of full-time employed workers aged 20-64 who were unemployed in the previous month. The prediction

Jersey, New Mexico, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, and Wisconsin.

[7]We exclude five states that terminated programs at different times. Alaska, Arizona, Florida, and Ohio ended FPUC early, but continued PUA until September 2021. Louisiana ended the programs on July 31, 2021, and is also excluded from the sample.

equation is a standard log wage function with the same set of demographic characteristics described above plus state, industry, and occupation fixed effects (shown in Appendix Equation (A.1)). The predicted wage thus incorporates previous industry and occupation information.

We also use the predicted wage to construct measures of the unemployment insurance benefit (UIB) and the lost replacement rate (LRR) resulting from the termination of PUI. For this purpose, we need the predicted unlogged wage rather than the logged form, requiring a smearing estimator (Duan, 1983) (Appendix Equation (A.2)). Using the smeared values of the predicted wage, and with the assumptions that the state unemployment insurance replacement rate is 0.5 and that unemployed in the treated states during the pre-treatment period and in the control states throughout receive the FPUC payment of $300 weekly, we compute the UIB for each individual (Appendix Equation (A.3)). We compute the LRR by dividing $300 by $\hat{w}_{ist}$ for observations for whom $PUIend_{st} = 1$.[8]

In analyzing re-employment behavior, we control for not only individual characteristics but also some time-varying state-level characteristics: job openings, state UI program performance, covid vaccinations, and seasonality. The job openings rate, from the Job Openings and Labor Turnover Survey (JOLTS), represents state-month variation in labor demand (or offer arrival rate) for unemployed workers. It is calculated as the ratio of job openings to total employment plus job openings in the nonfarm sector (BLS, 2022). UI program performance may affect UE transitions if payment delays lead discouraged unemployed to return to work earlier than they might (Navarrete, 2023). For this purpose, we draw upon data from the Department of Labor Employment and Training Administration (DOLETA), a Benefits Timeliness and Quality (BTQ) standard, the "continued weeks compensated time lapse" (Employment and Training Administration, 2017). UI timeliness is defined as a proportion of payments made within 3 weeks of receiving the claim, based on the performance standards of payment within 14-21 days.(Employment and Training Administration, 2022). Finally, vaccination rates may matter for unemployed individuals assessing the safety of returning to work and for the extent of subsequent covid spread. The covid vaccination rate, from the Centers for Disease Control and Prevention (CDC) data tracker, is defined as the percentage of the total population with at least one dose of a covid vaccine (CDC, 2023).

Time fixed effects, which we include in our regression specifications, control for nation-

[8]The replacement rate is 0.5 for most individuals in most states. A further analysis could take into account differences across states in the minimum and maximum levels of the state benefits, but while we account for individual differences in predicted wages, the main source of variation in our analysis is the $300 additional benefit from FPUC. Note that, even with information on former wages and UI benefits from the Survey of Income and Program Participation (SIPP), Chetty (2008) prefers to construct predicted wages, with a procedure similar to ours, and to use state-level average or maximum benefits rather than the survey data on the amounts received.

wide seasonal fluctuations in job-finding, but not for systematic state-level variation across different months. Whether seasonality is an important consideration during the volatile period we study is uncertain, but to account for the possibility we derive a state-month varying control variable by estimating the regular monthly variation in UE for each state separately (Appendix Equation (A.4)) based on experienced unemployed in 2015-2019. This variable controls for differences across states not only in weather but also factors such as school year start dates that might affect incentives for re-employment.

Table 1 reports descriptive statistics on individual variables for CPS respondents who are experienced unemployed during the period before the treated states terminate PUI (the pre-treatment period for most of our analysis, from February to June 2021). The table shows means for all states, and for treated and control states, separately.[9] Approximately one-third of observations are in the treated states. The average unemployment to employment transition rate (the mean of UE) is 0.23 overall, slightly higher at 0.25 in the treated states. The mean reported duration of the current spell of unemployment (Dur) is slightly lower in treated states, but this variable is right-censored, so drawing inferences about cross-state differences requires estimation using hazard functions or censored regressions.

While some of our specifications focus on the PUI policy change as a binary treatment, $PUIend$, in others we use the predicted level of the unemployment benefit UIB or the predicted loss in terms of the UI replacement rate LRR. Table 1 shows the benefits tended to be slightly lower in the treated states, even during the pre-treatment period. LRR equals zero before treatment, so it is not shown in the table, but its mean (SD) in the treated states during treatment (July-August) is 0.32 (0.13).

Concerning individual characteristics, the unemployed in treated states tend to have lower predicted wages and less schooling, are more likely to be Black, divorced, or veterans and less likely to be Asian or to live in a metropolitan statistical area. At the state level, job openings rates and timeliness of UI performance tend to be slightly higher in the treated states on average, while vaccination rates are lower. We control for all these variables in most of the re-employment analysis.

Turning to the impact on covid outcomes, the possibility that higher re-employment affects pandemic contagion concerns not only the individuals returning to work but also the externality to others through community transmission of infection: workers may bring the virus home to their households and social networks, resulting sometimes in serious illness, and sometimes in death. To measure these outcomes requires aggregate data and a longer time-series to account for the fact that community spread is not instantaneous, so our regression

[9]The main regression sample includes 10,229 observations, of which 7625 are in the pre-treatment period. We provide statistics for the period from July 2019, used in some specifications, in Appendix Table A2.

Table 1: Descriptive Statistics for Re-Employment Analysis (Pre-Period Means)

Variable	All states	Treated	Control
Unemployment to Employment Transition (UE)	0.23	0.25	0.23
Unemployment duration (Dur, months, right-censored)	5.44	5.03	5.58
	(3.32)	(3.19)	(3.36)
Experienced unemployed by reason:			
Job loser	0.71	0.65	0.74
Job leaver	0.08	0.11	0.07
Re-entrant	0.21	0.24	0.20
Predicted weekly wage ($\hat{w}$, \$)	1242.52	1110.75	1287.85
	(588.74)	(503.52)	(608.81)
Age (years)	38.03	38.51	39.21
	(12.87)	(12.79)	(12.90)
Female	0.44	0.44	0.44
Race/Ethnicity			
Hispanic	0.25	0.23	0.25
White	0.47	0.47	0.47
Black	0.18	0.24	0.15
Asian	0.07	0.02	0.08
Academic Attainment			
< High school	0.12	0.13	0.11
High school	0.33	0.34	0.32
Some college	0.30	0.31	0.29
University	0.19	0.15	0.20
> University	0.07	0.05	0.08
Marital Status			
Never married	0.49	0.44	0.51
Married	0.36	0.37	0.36
Divorced/separated	0.13	0.17	0.11
Widowed	0.01	0.01	0.01
Eldest child age <12	0.16	0.16	0.15
Veteran	0.04	0.06	0.03
Metropolitan area	0.87	0.78	0.91
Predicted unemployment benefit (UIB, \$)	915.14	845.55	940.82
	(291.15)	(242.42)	(303.20)
State-level covariates			
Job opening rate (%)	5.44	5.71	5.34
	(0.97)	(0.89)	(0.98)
UI payment timeliness	0.83	0.85	0.83
	(0.09)	(0.06)	(0.09)
Vaccination rate	0.30	0.26	0.32
	(0.17)	(0.14)	(0.18)
Observations in pre-period (Feb-June 2021)	7625	2497	5128

Note: Calculations based on CPS data from February to June 2021. The sample includes all experienced unemployed aged 20-64, except for Dur and UIB, for which the sample is further restricted to individuals with unemployment duration up to a year (with sample sizes of 5755, 1996, and 3759 for all, treated, and control states, respectively). Variables are defined in text. For continuous variables, standard deviations are shown in parentheses; all other variables are binary dummies. Observations are weighted by the CPS final individual weights.

specification must include lags. Therefore, we construct covid outcome rates by state-month over the period January-November 2021 (which allows us to have three monthly lags after the treatment period) for our main analysis and through October 2022 for a longer event study.

We examine three covid outcomes: infections (new cases), covid-confirmed hospital admissions, and covid deaths.[10] In order to obtain monthly frequencies, we aggregate daily new covid cases and deaths data from the Centers for Disease Control and Prevention (CDC, 2022) and daily hospitalization admissions data from the Department of Health and Human Services (HHS, 2023). The HHS hospital admissions separates adult and pediatric covid-confirmed admissions, so we add these to obtain total hospitalizations. In order to express these numbers per capita, we use April 2020 population from the Decennial Census by state.

In addition to state and month fixed effects and state-time varying vaccination rates, the covid analysis also controls, in some specifications, for the possibility that differences in weather may affect the amount of time spent indoors, and thereby the potential spread of disease. For this purpose, we collect cooling and heating degree-days from National Center for Environmental Information, operated by the National Oceanic and Atmospheric Administration. Cooling and heating degree-days are defined respectively as the sums of the deviations of the monthly average temperature above and below 65°F. We replace missing information from this source on Washington D.C. and Hawaii with data from Weather Data Depot (2023).

Table 2 shows summary statistics for these variables over the pre-period, January-June 2021. Weighted by state populations, the average monthly infection rate across all states is 0.66 percent, the monthly hospitalization rate is 0.06 percent, and the monthly death rate is about 0.01 percent during this period. The means of covid cases and deaths are identical in the treated and control states, while the hospitalization rate is slightly higher in the former. Vaccination rates are lower in the treated states; the time period in this table is longer than in Table 1 so the vaccination rates are higher, but the difference between treated and control states is almost exactly the same. Treated states have higher cooling degree-days than the controls, on average, while control states have higher heating degree-days than the treated. Because vaccinations and time spent indoors are likely correlated with covid outcomes, we control for them in some of the analyses below.

[10]There has been some debate on whether the CDC under- or over-counts covid deaths (Hoffman, 2020; Faust et al., 2021). It seems likely that all the covid variables are measured with error, but for such mismeasurement to affect our results, there would have to be a bias in reporting that emerges exactly in the treatment period in the treated states (because there is essentially no change in covid outcomes in the control states during the treatment period) and that disappears afterwards. We are unaware of any factors that could lead to such biased reporting.

Table 2: Descriptive Statistics for Covid Analysis (Pre-Treatment Means)

Variable	All states	Treated	Control
Covid case rate (%)	0.66	0.66	0.66
	(0.64)	(0.66)	(0.63)
Hospitalization rate (‰)	0.56	0.61	0.53
	(0.46)	(0.53)	(0.43)
Mortality rate (‰)	0.12	0.12	0.12
	(0.11)	(0.12)	(0.11)
Vaccination rate (%)	35.10	30.70	37.12
	(0.18)	(0.15)	(0.19)
Cooling degree days / 100	0.60	0.98	0.43
	(1.06)	(1.37)	(0.83)
Heating degree days / 100	4.61	3.61	5.07
	(3.89)	(3.58)	(4.05)

Note: Calculations based on monthly state-level data, weighted by population, from January to June 2021. Hospitalization and mortality rates are per thousand. Vaccination is the cumulative percentage of the population receiving at least one dose. Cooling and heating degree days are respectively the sums of the deviations of the average temperature above and below 65°F. Standard deviations are shown in parentheses. Covid-related state-level policy variables are listed in Appendix Table A4, and the summary statistics of additional control variables are in Appendix Table A3 .

For additional control variables and robustness checks in the covid analysis, we draw upon several other types of information and sources: school opening dates and the treatment period interacted with the obesity rate, heart disease rate, share of population aged 65 or more, covid case rate, median earnings, leisure and hospitality industry employment ratio, and mobility for recreation, all measured pre-treatment. Further description, sources, and summary statistics for these variables, by treated and control states, are provided in Appendix Table A3. We also make use of a large database of covid-related state-level policies that could affect covid outcomes during summer 2021, focusing on all those that could have exacerbated covid outcomes in treated relative to control states. The policies, which include provisions related to housing (largely evictions), prison visitation, masking, quarantines, re-opening, and state of emergency, are listed in Appendix Table A4, and further described in the Data Appendix A.

A final aspect of data construction concerns the state-month level UE rates used in the covid analysis. For this purpose, we use estimated coefficients of state-by-month fixed effects from a model that controls for individual characteristics (Appendix Equation (A.5)). We denote these estimates as UE_{st} in the covid analysis below.

3 Empirical Analysis of Re-Employment

This section examines the implications of the early termination of Pandemic Unemployment Insurance (PUI) for re-employment and unemployment duration. We start by examining unemployment-employment transitions with the policy change defined as a binary treatment. Next, we estimate hazard and censored regression models, and then we consider the impact of lost UI benefits. A final subsection contains further specification and placebo tests.

3.1 Unemployment to Employment Transitions

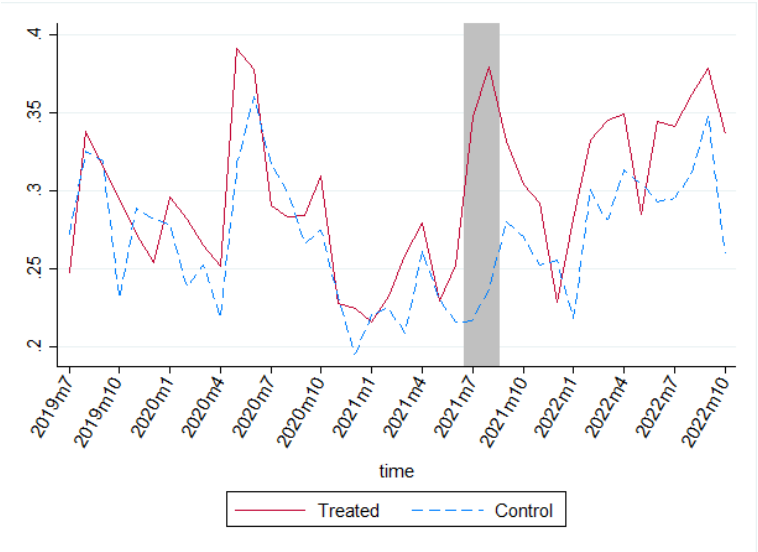
Figure 2 displays the average unemployment-to-employment transition rate UE for treated and control states separately by month from mid-2019 to end-2022. Treatment is defined as early PUI termination, carried out by 19 states in mid-late June 2021, while the 27 control states ended PUI only in September, so the treatment period is July-August, shaded in the figure. Although the rate is usually slightly higher in the treated states, the series tend to fluctuate similarly before July 2021, when the rate jumps in the treated states while increasing only modestly in those remaining untreated.[11] From an identical mean of 0.23 in treated and control states in May, the mean UE reaches 0.38 by August in the treated states but remains little changed at 0.25 in the control states. Afterwards, the series reconverge and again move fairly closely together. Below we examine the statistical significance of the monthly differences in an event study framework while controlling for other factors.

Table 3 aggregates the monthly data into a pre-treatment period of February-June 2021 and a treatment period of July-August 2021. (Similar results obtain with other definitions of the pre-treatment period.) While the average treated-control state difference is about 0.02 before treatment, during the two months of policy difference the mean UE in the treated states jumps from 0.25 to 0.36, while in the control states it remains unchanged. A raw difference-in-differences estimator of the policy effect is therefore 0.11.

Table 3 also shows UE transition rates for the three "reasons" for unemployment among all experienced unemployed: job losers, job leavers, and labor force reentrants. Job losers (accounting for 70 percent of the total) and job leavers (less than 10 percent) have very similar UE behavior, while the UE rates for reentrants are somewhat lower. But the size of the jump from pre-treatment to treatment period in the treated states and the lack of change in the control states is very similar for all three types of unemployment, implying similar difference-in-differences estimates of the policy effect. We therefore include all experienced

[11] The small increase in average UE among treated states in June is entirely driven by the states which ended PUI on the earliest treatment date of June 12, likely reflecting anticipatory behavior in those states; the states ending PUI on June 19, June 26, and July 3 show no increase in UE in June.

Figure 2: Monthly UE Transitions, Treated and Control States



Note: Weighted average rates of UE transition for experienced unemployed by treated and control states. CPS individual weights. Shading in July-August 2021 highlights the two-month period during which PUI policy differed: it had ended in treated states but continued until September in control states.

Table 3: Mean UE Transitions

	Treated States		Control States		Observations
	Feb-Jun 2021	Jul-Aug 2021	Feb-Jun 2021	Jul-Aug 2021	Feb-Aug 2021
Experienced Unemployed	0.251	0.357	0.228	0.227	10229
Job losers	0.258	0.375	0.241	0.247	7169
Job leavers	0.264	0.368	0.251	0.248	927
Reentrants	0.226	0.302	0.174	0.169	2133

Note: CPS-weighted means of UE are shown for treated and control states and by pre-treatment (February-June) and treatment (July-August) periods for all experienced unemployed and by reason for unemployment: job losers, job leavers, and re-entrants.

unemployed in most of our analyses, but also show robustness using only the job loser sample.

In order to control for other factors correlated with re-employment and possibly with the policy treatment, we estimate the following linear probability model (LPM):

$$UE_{ist} = \beta \cdot PUIend_{st} + X_{ist-1} \cdot \gamma + V_{st-1} \cdot \eta + \delta \cdot \widehat{ln(w)}_{ist} + \mu_s + \tau_t + \epsilon_{ist}, \qquad (1)$$

where UE_{ist} is the transition from unemployment$_{t-1}$ to employment$_t$ for individual i in state s at time (month) t, $PUIend_{st}$ is an indicator for the treated states in July and August 2021, and X_{ist-1} is a set of individual characteristics in *t-1*: age, age squared, gender, ethnicity/race, education, marital status, veteran status, and MSA. In some specifications, we also control for a set of state-level covariates V_{st-1} described in Section 2 on data: job openings rate, UI timeliness rate, vaccination rate, and state-level seasonality (described in Appendix A and computed from Equation (A.4)). Except for seasonality, we lag these

variables in Equation (1), and we also report estimates excluding them; we find they make little difference to the results.[12] Again as described in the data section, we usually control for predicted log wages ($\widehat{ln(w)}_{ist}$) instead of previous industry and occupation to conserve degrees of freedom. Finally, we control for state (μ_s) and month (τ_t) fixed effects, and we cluster standard errors on states.[13]

Table 4: Estimated Effects of UI Loss on the UE Transition Probability

	Feb-Aug 2021				Jul 2019-Aug 2021	
	(1)	(2)	(3)	(4)	(5)	(6)
$PUIend$	0.101	0.094	0.106	0.098	0.108	0.105
	(0.023)	(0.023)	(0.022)	(0.022)	(0.020)	(0.020)
Job opening rate		0.030		0.033		0.015
		(0.012)		(0.013)		(0.005)
Timeliness		0.144		0.136		-0.009
		(0.098)		(0.089)		(0.037)
Vaccination		-0.095		-0.122		-0.009
		(0.161)		(0.171)		(0.088)
Seasonality		0.135		0.163		0.344
		(0.151)		(0.147)		(0.078)
$\widehat{ln(\text{w})}$			0.037	0.036	0.021	0.021
			(0.017)	(0.017)	(0.013)	(0.013)
Industry FE	Yes	Yes	No	No	No	No
Occupation FE	Yes	Yes	No	No	No	No
Predicted wage	No	No	Yes	Yes	Yes	Yes

Note: Each column shows estimated coefficients from a separate linear probability model for the UE transition, based on Equation (1). $PUIend$ is an indicator for treated states during the treatment period (states ending PUI by early July 2021 during months July-August). All specifications control for state and month fixed effects and individual characteristics including age, age squared, sex, ethnicity and race, education, marital status, veteran status, and MSA. Industry and occupation FE refer to the former job. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Sample = experienced unemployed, February-August 2021 (columns 1-4) or July 2019-August 2021 (columns 5-6). Observations = 10,229 (columns 1-4) and 38,124 (columns 5-6).

Results from estimating some alternative specifications of Equation (1) are displayed in Table 4. The selected specifications, which are representative of many other variants,

[12]Note that we do not include measures of covid outcomes as regressors because they may be simultaneously determined with UE behavior: higher covid may reduce UE, but higher UE may increase covid, an issue we address below. Consistent with these opposing effects, if covid measures are included in Equation (1), the coefficient estimates on these variables are always small and vary in sign depending on the particular covid variable(s) and other aspects of the specification. Most importantly for current purposes, including these variables has little impact on the estimated coefficient on $PUIend$.

[13]We also considered the possibility of using border county pairs, as in, e.g., Boone et al. (2021), but the CPS variable for county is not disclosed for many respondents and the state-level controls are not available at the county level. Moreover, even if the data were available for the covid analysis below, estimates could be affected by cross-border spillovers in infections, particularly when border county pairs are in the same metropolitan area or commuting zone.

differ in terms of the pre-treatment time period, whether state-level controls are included, and whether predicted wage is included in place of industry and occupation fixed effects. Regardless of these choices, the estimated coefficients on $PUIend$ are highly significant and the magnitudes are very robust, implying an approximate 10 percentage point effect of treatment on the probability of transition from unemployment to employment. This estimate is also very similar to the unadjusted difference-in-differences estimator from the means in Table 3. Relative to a baseline average UE of about 0.24, the implied increase is 42 percent.[14]

Concerning the state-level covariates, only the job openings rate is significant across all specifications: a one percentage point increase in the job openings rate increases the transition by 1.5-3 percentage points. The impact of greater timeliness in UI payments is estimated to be positive in the shorter sample but negative in the longer sample; in both cases it is tiny in magnitude (given a standard deviation 0.1 in Table 1) and statistically insignificant. The vaccination rate enters with a negative sign, but again the magnitude is small (relative to a standard deviation of 0.2) and insignificant. The estimated coefficient on state-level seasonality has the expected positive sign, but again it is small and insignificant, implying that seasonal patterns estimated from 2015-2019 were still weak at this point during the pandemic. But none of these state-level controls matters for the main coefficient of interest on $PUIend$.

The coefficient on the predicted log wage is positive and statistically significant, although not large: a ten percent increase in the predicted wage would raise the UE probability by about 0.2-0.4 percentage points, depending on the specification and pre-period. The positive sign is consistent with the predicted wage reflecting labor demand conditions, possibly a higher offer arrival rate for higher skilled workers. Including the predicted wage and dropping industry and occupation fixed effects raises the estimated coefficient on $PUIend$ about half a percentage point; the standard errors (around 0.02) imply that these results are statistically indistinguishable.[15]

Table 5 shows results for the LPM dividing the sample by reason for unemployment among the experienced unemployed. The magnitudes of the estimated impact of the cut to UI benefits are all close to 0.1, slightly higher for job losers than the other two groups when

[14] As noted in the Introduction, Holzer, Hubbard and Strain (2021) estimate a coefficient of 0.14, while Coombs et al. (2022) estimate it at 0.068. Our estimate lies roughly mid-way between these and the 95 percent confidence interval around our estimate ([.065, 0.147] from column one) contains both.

[15] The coefficient close to 0.1 is remarkably robust across different specifications. In no specification did we find a difference from 0.1 that was greater than the 0.02 standard error on the $PUIend$ variable. Controlling for the duration of the incomplete spell at time *t-1* (in various functional forms, such as polynomials) affects the coefficient only slightly, and duration is better handled through the hazard and censored regression methods of the next subsection. We also estimated the coefficient with alternative time periods, obtaining very similar results, as shown in Appendix Table B1.

state controls are excluded, and slightly lower when they are included. The smaller samples of re-entrants and especially of job leavers are associated with much higher standard errors on all the coefficients in the equations using those samples.[16]

Table 5: Estimated Effects of UI Loss on UE Transitions, by Reason for Unemployment

	Job loser		Job leaver		Reentrant	
	(1)	(2)	(3)	(4)	(5)	(6)
$PUIend$	0.108	0.096	0.098	0.104	0.096	0.109
	(0.029)	(0.030)	(0.069)	(0.067)	(0.049)	(0.051)
State-level covariates	No	Yes	No	Yes	No	Yes

Note: The table shows estimated coefficients from Equation (1), separately by reason for unemployment from February to August 2021. All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2), (4), and (6) condition on state-level covariates including job opening rate, timeliness, vaccination, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 7,169 job losers, 927 job leavers, and 2,133 reentrants.

The key identifying assumption in this analysis so far is a common trend in the outcome variable for treated and control states. That is, the unemployment-to-employment transition rate in the treated states would evolve as in the control states if they had not ended the program early. In order to investigate any differences in pre-treatment trends, we conduct an event study by interacting the treated state indicator $Treat$ with quarter dummies as follows:

$$UE_{ist} = \sum_{k=2019Q3}^{2021Q3} \beta_k \cdot Treat_s \cdot Qtr_k + X_{ist-1} \cdot \gamma + V_{st-1} \cdot \eta + \delta \cdot \widehat{log(w)}_{ist-1} + \mu_s + \tau_t + u_{ist}, \quad (2)$$

where Qtr_k is a set of year-quarter dummies, where the second quarter of 2021 is the base period and the third "quarter" of 2021, the treatment period, defined to include only July and August 2021 (corresponding to $PUIend$, since the control states change policy in September so there is no longer a policy difference). Other variables are the same as in Equation (1). We specify event time in quarters rather than months to save degrees of freedom, while still controlling for state and month fixed effects.[17]

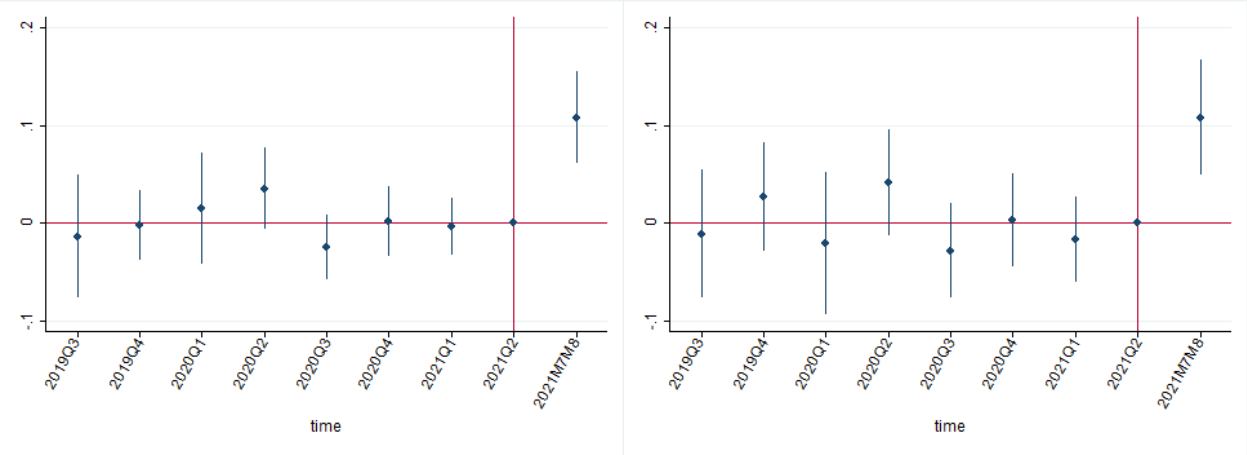
The estimated coefficients from Equation (2) are plotted in Figure 3 for all experienced unemployed and for the subcategory of job losers. None of the pre-treatment coefficients are

[16] Appendix Table B2 contains results for job losers with duration up to 26 weeks, the maximum UI duration under most state programs. The coefficients tend to be somewhat higher, in the range of 0.127-0.143, reflecting greater responsiveness in this group, but still in the general range (and within a standard error or two) of the estimates for the full experienced unemployed population.

[17] Estimating the event study in months instead of quarters produces similar but noisier patterns; we report these results for a longer time period including months after September 2021 in Subsection 3.4, Figure 5 below.

statistically significantly different from zero, and they show no trend. The treatment period coefficient is much larger than all those estimated for the pre-treatment period, and it is statistically significant for all experienced unemployed and for job losers. Thus, we find no evidence contradicting the assumption of parallel trends and no suggestion of anticipatory behavior in advance of the policy change.

Figure 3: Event Study for UE Transitions with Binary Treatment



(a) Experienced Unemployed (b) Job Loser

Note: The graphs show estimated event study coefficients from Equation (2) for experienced unemployed and job losers from July 2019 to August 2021, with the base period defined as the second quarter of 2021, and the last period defined to include only July and August 2021. Specifications control for demographic and human capital characteristics, predicted wage, and state and month fixed effects, as well as state-level covariates including job opening rate, timeliness, vaccination, and seasonality as in Table 4 model (6). The observations are weighted by CPS weights. Vertical lines represent 95% confidence intervals, based on standard errors clustered on state.

How much did employment rise as a result of the early termination of PUI in treated states? We calculate that UE transitions amounted to 421,157 in July and 448,239 in August, but some of these would have taken place in the absence of the policy. To calculate the UE attributable to the policy we apply the ratio of the estimated effect of 0.098 from Column 4 of Table 4 to the mean transition of 0.34 in July and 0.37 in August. These calculations imply that the policy change in the treated states resulted in additional UE transitions of 120,696 in July and 117,551 in August. We will refer back to these estimates in the sections on covid outcomes and on costs and benefits below.

In Appendix C we examine heterogeneity in the estimates of Equation (1). We consider the impact separately for women and Blacks, two population groups whose employment recovery was relatively slow, a pattern sometimes attributed to problems with child care or greater fear of covid creating greater reluctance to return to employment. These supply-side considerations might imply a lower responsiveness of job-finding behavior to changes in unemployment insurance among these groups. In fact, we find slightly higher coefficients

for women and substantially larger for Blacks. We also find variation with marital status, consistent with liquidity constraints, and by age and teleworking occupation, consistent with safety motives of unemployed workers considering a return to work.

3.2 Duration Analysis

The unemployment literature typically estimates hazard functions rather than unconditional UE transitions in order to account for unemployment duration. In the volatile context of a pandemic, it is not clear whether there is a stable exit-duration relationship, but we are interested to quantify the extent to which behavior during the time period we study is similar to non-pandemic times. In this subsection, we start with a nonparametric analysis of survival rates in unemployment, and then provide estimates of hazard and censored duration regressions. Our focus is on all experienced unemployed, but the Appendix also provides results for job losers separately, with results similar to those for experienced unemployed shown here. We maintain a binary treatment framework here, considering the continuous variables of the replacement rate and UI benefit in the following subsection.

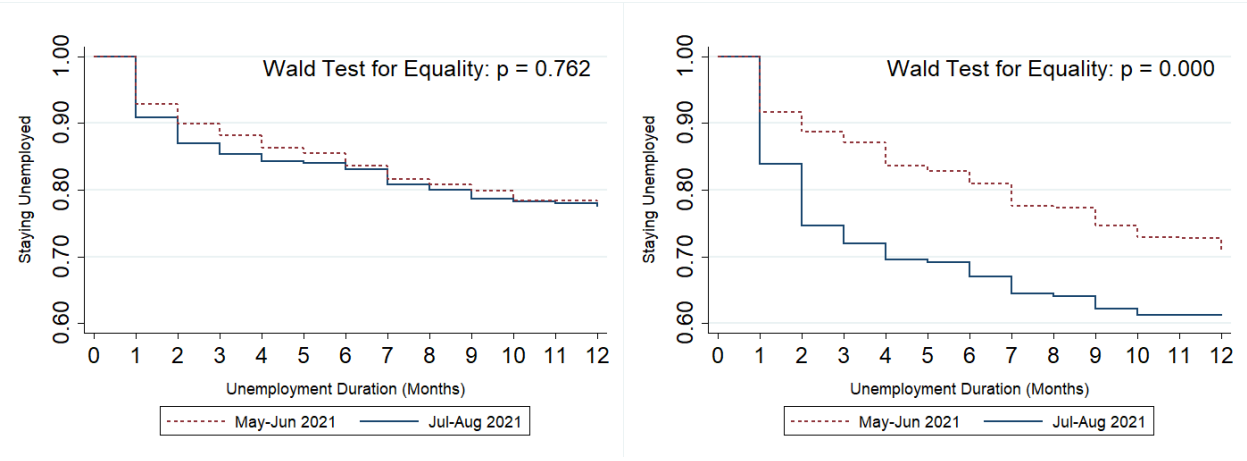
Figure 4 displays Kaplan-Meier survival functions for treated states and control states separately and by pre-treatment period (May-June 2021), when all states had PUI, and treatment period (July-August 2021), when the treated (early termination) states had ended. The graphs show very similar survival rates at all levels of duration for control states in the pre-treatment period and treatment period, and a Wald test fails to reject equality of the survival curves. Compared to these rates for control states, the treated states during the pre-period exhibit only very slightly lower survival rates. During the treatment period, however, the treated states show a sharp drop in unemployment survival of about 10-15 percent at each level of unemployment duration; the Wald test implies that the difference is highly significant. The drop varies little by comparison group: it is very similar relative to the control states in both the pre- and the post-period, and the decrease is only slightly smaller relative to the treated states in the pre-period.

In order to examine the change in behavior in treated states while controlling for other factors, we estimate the unemployment exit hazard using a semi-parametric Cox model:

$$\log h_{isdt} = \alpha_d + \beta_h \cdot PUIend_{st} + X_{ist-1} \cdot \gamma + V_{st-1} \cdot \eta + \delta \cdot \widehat{log(w)}_{ist-1} + \mu_s + \tau_t + \epsilon_{isdt} \quad (3)$$

where h denotes the hazard rate of unemployment exit to employment for an individual i in state s in duration d of an unemployment spell (weeks) in time t. α_d is the baseline hazard rate, and other variables are defined as previously.

Figure 4: Kaplan-Meier Curves, Pre-Treatment and Treatment Periods



(a) Control States (b) Treated States

Note: Survival functions are shown for experienced unemployed during the pre-treatment (before $PUIend$) period of May-June 2021 and treatment ($PUIend$) period of July-August 2021, for control and treated states separately. The Wald tests refer to the null hypothesis of no difference between pre-treatment and treatment period survival functions. Observations are weighted by CPS weights.

An alternative way to account for duration is a censored normal regression model:

$$\log Dur_{ist} = \beta_{Dur} \cdot PUIend_{ist} + X_{ist-1} \cdot \gamma + V_{st-1} \cdot \eta + \delta \cdot \widehat{log(w)}_{ist-1} + \mu_s + \tau_t + \epsilon_{ist} \quad (4)$$

where $\log Dur_{ist}$ is the log of unemployment duration. The specification takes into account the right-censoring of incomplete spells and controls for the sets of individual and state level covariates as well as state and time fixed effects.[18]

Results from estimating the hazard and duration models in Equations (3) and (4) are shown in Table 6. The estimated coefficients on $PUIend$ are highly significant and qualitatively similar across specifications: the early termination of PUI raises the hazard about 30 percent and lowers average duration about 22 percent. If the baseline hazard is constant, then $Dur = 1/h$, and $\beta_{Dur} = -\beta_h$, so the results are substantively similar across the hazard and duration models, and the standard errors suggest that a null hypothesis of equality fails to be rejected.[19]

[18] We estimate with maximum likelihood using the Stata command *cnreg*, which is a type of tobit permitting the censoring value to vary across observations.

[19] Results from estimating these equations restricting the sample to job losers, shown in Appendix Table B3, are qualitatively similar.

Table 6: Estimated Effects of UI Loss on Unemployment Hazard and Duration

Model Dep. Var.	Cox proportional hazard log(Hazard)		Censored normal log(Duration)	
	(1)	(2)	(3)	(4)
$PUIend$	0.305 (0.124)	0.308 (0.110)	-0.221 (0.098)	-0.229 (0.085)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from Equation (3), the Cox proportional hazard model, in Columns (1-2) and from Equation (4), the censored normal regression model, in Columns (3-4). Both account for right censoring because of incomplete unemployment spells and control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2) and (4) condition on state-level covariates including job openings, timeliness, vaccination, and seasonality. The observations are weighted by CPS weight. Standard errors clustered on state are shown in parentheses. Observations = 7,561 experienced unemployed with duration up to a year from February to August 2021.

3.3 UI Replacement Rate and Benefit Analysis

The analysis so far has used a binary treatment framework to estimate the impact of early termination of PUI. But the intensity of the policy varies across individuals, which is relevant for estimating the responsiveness of labor supply behavior. A typical estimand in studies of unemployment and UI is the duration-benefit elasticity. In this subsection, we provide estimates of the elasticity of UE, hazard, and Dur with respect to the UI replacement rate and benefit. For the former, we use the lost replacement rate LRR described in the Data Section, interacting it with $PUIend$ in Equations (1), (3), and (4).

Results of the replacement rate analysis are shown in Table 7. All coefficients are highly significant and imply similar behavioral responses: for $LRR = 0.32$, the mean in the sample, the UE model implies a 7-8 percentage point increase in the unconditional probability of re-employment UE, which compares with the estimates of about 10 percentage points in Table 4. The Cox model implies a 23-25 percent increase in the hazard, and the censored regression implies a 15 percent decrease in unemployment duration.[20]

To estimate the UI benefit elasticities of UE, hazard, and duration, we use Two-Stage Least Squares (2SLS) with $PUIend$ as an instrument for the UI benefit UIB. In the second stage, we use the predicted value from the first stage, $\log \widehat{UIB}_{ist}$, in place of $PUIend$ to estimate the effect on UE transition, hazard, and duration as in Equations (1), (3), and (4), respectively. We present standard errors clustered at the state level and bootstrapped based in each case on 1,000 replications.

Results are shown in Table 8. The estimated UIB elasticity of UE of -0.227 can be compared with Coombs et al. (2022), who report a range of -0.13 to -0.22, using different

[20]Results from estimating the UE, hazard, and duration models restricting the sample to job losers, are qualitatively similar, as shown in Appendix Table B4.

Table 7: Estimating Replacement Rate Elasticities for UE, Hazard, and Duration

Model	LPM		Cox		Censored normal	
Dep. Var.	UE		log(Hazard)		log(Duration)	
	(1)	(2)	(3)	(4)	(5)	(6)
LRR x $PUIend$	0.252	0.215	0.733	0.685	-0.459	-0.425
	(0.082)	(0.076)	(0.318)	(0.287)	(0.255)	(0.225)
State-level covariates	No	Yes	No	Yes	No	Yes

Note: The table shows estimated coefficients on the lost replacement rate LRR from Equations (1), (3), and (4). All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2), (4), and (6) condition on state-level UI timeliness, vaccination, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 7,561 experienced unemployed with duration up to a year from February to August 2021.

data and methods. The first-stage coefficient of -0.463 implies that on average a worker in a treated state lost nearly half their UI benefits. In light of this mean effect, the second stage coefficients, which refer to the impact of a one-unit change, are quite consistent with the results in Tables 4 and 6. A cut of 0.463 leads to a 0.10 increase in the unconditional UE transition, a 0.31 increase in the hazard, and a 0.23 decrease in unemployment duration. The point estimates of the elasticity of unemployment duration with respect to UI benefits are 0.68 from the hazard (under the assumption of a constant benefit elasticity and baseline hazard) and 0.50 from the censored duration regression.[21] Standard errors imply that we fail to reject the null hypothesis of equality of these estimates, and they fall within the general range of previous estimates of the elasticity, even bracketing the 0.6 value suggested by Lancaster (1979) as "established beyond reasonable doubt" (p. 956).[22] The evidence thus implies that unemployed individuals exhibited a similar response to UI benefits during this period of the covid pandemic as has been estimated for nonpandemic periods.

3.4 Specification Tests

The event studies in Subsection 3.1 showed no differences in trends between treated and control states prior to treatment. In this subsection, we consider some additional tests for the re-employment analysis.

We start by estimating the impact of $PUIend$ on a placebo group: those not in the

[21] Chetty (2008) derives an estimate of 0.53 for the duration-benefit elasticity from a hazard estimation for unemployed with duration up to 50 weeks under "the approximation that the benefit elasticity and baseline hazard rates are constant" (p. 217).

[22] If the sample is restricted to unemployed with durations up to 26 weeks (the usual maximum duration for state UI programs), the IV estimates for the three models are -0.29, -0.83, and 0.38, respectively, as shown in Appendix Table B5. Other estimates using samples of job losers and alternative limits on unemployment duration are qualitatively similar, as shown in Appendix Tables B6 and B7.

Table 8: Estimating the Impact of UI Benefits on UE, Hazard, and Duration

	First stage	Second stage		
	(1)	(2)	(3)	(4)
Dep. Var.	log(UIB)	UE	log(Hazard)	log(Duration)
$PUIend$	-0.463			
	(0.008)			
$\widehat{\log(UIB)}$		-0.227	-0.676	0.499
		(0.060)	(0.241)	(0.184)
			[0.241]	[0.175]
Model		2SLS	IV Cox	IV Cens. Normal

Note: The table shows estimated coefficients from the first stage, column (1), and second stage with LPM (2), Cox proportional hazard model (3), and censored normal regression model (4). All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, state and month fixed effects, and state-level job opening rate, UI timeliness, vaccination, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses; bootstrapped standard errors are reported in square brackets. f-statistic from the first stage = 1591.73. Observations = 7,561 experienced unemployed with duration up to a year from February to August 2021.

labor force. Nonparticipants are unlikely to receive UI, therefore their behavior should not be affected by the change in policy in summer 2021. Estimating Equation (1) using the nonparticipant sample produces the results in Table 9. All coefficients are statistically insignificant and precisely estimated at zero.

Table 9: Placebo Effect of UI Loss on Transitions to Employment of Nonparticipants

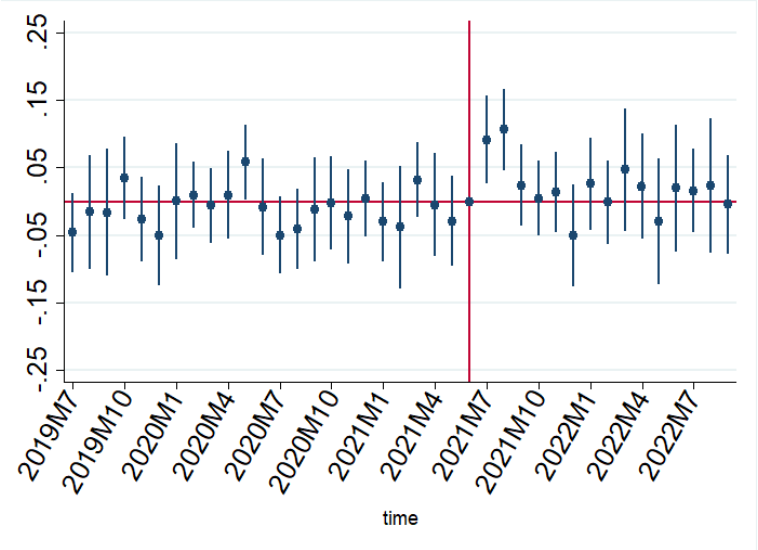
	February - August 2021		July 2019 - August 2021	
	(1)	(2)	(3)	(4)
$PUIend$	0.001	0.003	0.004	0.006
	(0.009)	(0.010)	(0.008)	(0.009)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from Equation (1) for the sample of labor force nonparticipants. All specifications control for demographic and human capital characteristics, predicted wage, and state and month fixed effects, as in Table 4. Models (2) and (4) condition on state-level job openings, UI timeliness, vaccinations, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 59,073 for columns (1-2) and 219,076 for (3-4).

Next, we consider longer-term dynamics, including the period after all states had ended FPUC and PUA in September 2021. For this purpose, we estimate an extended version of Equation (2) with monthly data through October 2022. Results are graphed in Figure 5. The only months during the 39-month period where the coefficient on treated states is statistically significantly different from zero are the two months of July and August of the treatment period during summer 2021. The data show a quick re-convergence of UE transition rates in September 2021, when the PUI policy difference across states ended. This

result supports a causal interpretation of the impact of $PUIend$, because if the much higher re-employment rates we have estimated for treated states in July and August result from some factor other than $PUIend$, then that factor would have to be exactly contemporaneous with the $PUIend$ treatment period, ending simultaneously when the policy difference ends in September.

Figure 5: Monthly Event Study of the Impact of UI Loss on UE Transitions

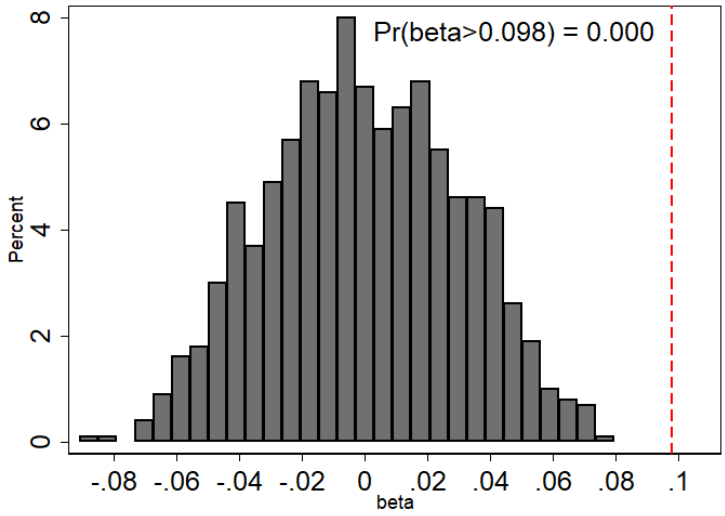


Note: The graphs show a monthly event study of $PUIend$ on UE transition from July 2019 to September 2022 for the experienced unemployed. Specifications control for demographic and human capital characteristics, predicted wage, state and month fixed effects, and state-level job openings, UI timeliness, vaccinations, and seasonality, as in columns 4 and 6 from Table 4. Observations are weighted by CPS weights. Vertical lines represent 95% confidence intervals, based on standard errors clustered on state.

A final type of specification test involves permuting the assignment of treatment among the states. For this purpose, we randomize treatment across states and re-estimate Equations (1), (3), and (4). We carry out 1000 replications of this procedure, re-randomizing treatment each time.[23] Figure 6 shows the results for the binary UE transition and binary treatment (Equation (1)). The result shows that the point estimate of 0.098 from column 4 of Table 4 lies far outside the distribution of the replications, with a p-value < 0.001. Appendix Figure B1 shows the results for the hazard and duration regressions (corresponding to Equations (3) and (4)), and Figures B2 and B3 show the corresponding results for the LRR and UIB. In all cases, the estimated coefficients using the true assignment of treatment have very low p-values and lie far outside relative to the distribution of results from placebos.

[23]We adapt the permutation code from Kuka, Shenhav and Shih (2020).

Figure 6: Permutation Test for Estimated Effect of UI Loss on UE Transitions



Note: The graph shows the distribution of coefficients from 1,000 replications of randomizing treated states in Equation 1. The specification is the same as Table 4, column (4); the estimated coefficient from that column is shown as a dashed vertical line.

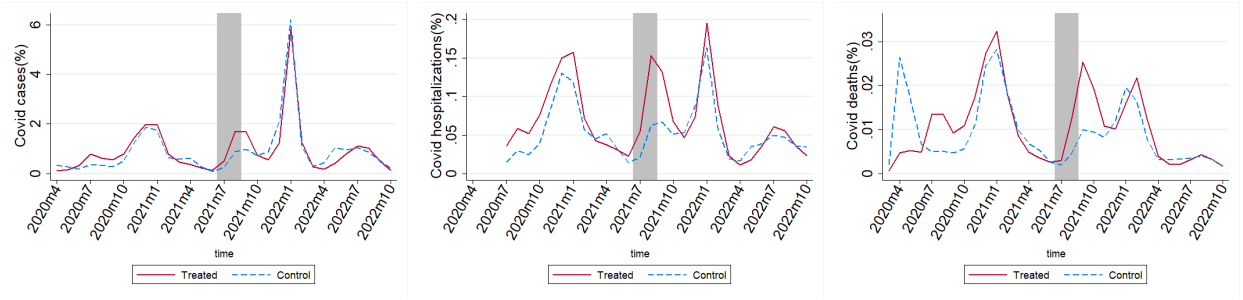
4 Covid Analysis

If early termination of PUI in the summer of 2021 increased the re-employment rate, it could also have implications for the spread of covid. Not only those returning to work could become infected, but also members of their families and social circles. Analyzing such an externality resulting from community spread requires more aggregated data, given the lack of information on familial and social networks, so we turn to state-month level data on covid outcomes. The analysis must also take into account the lagged nature of community transmission, as transmission and outcomes, particularly deaths, hardly occur instantaneously.

Figure 7 plots the population-weighted means of monthly covid outcomes separately for treated and control states from April 2020 until October 2022. The outcomes are new covid cases, hospitalizations (new admissions), and deaths during each month, described in the data section. The graphs show clearly the several waves of covid infections, from the initial strain in spring 2020 to the Alpha wave in late 2020 to the Delta wave in summer-fall 2021 (the period we study), to the Omicron variant at the end of 2021 into 2022 (Lundberg et al., 2023).

The time series for covid cases and hospitalizations tend to move closely together in the treated and control states, with the one exception being the shaded region of July-August 2021. During this time, when UI policies diverged, cases and hospitalizations spiked much higher in the treated states. Afterwards, the graph shows a reconvergence across treated and

Figure 7: Monthly Covid Outcomes, April 2020 - October 2022



(a) Cases (b) Hospitalizations (c) Deaths

Note: The figures show monthly covid outcomes by treated and control states from April 2020 to October 2022. All values are population weighted average.

control states in the level of cases (by October) and hospitalizations (by November).

Covid deaths, also shown in Figure 7, display similar patterns: at the beginning of the pandemic in spring 2020, deaths spike more quickly in the control states followed that summer by a somewhat smaller spike in the treated states. In the next wave, associated with the "Alpha" variant in early 2021, the death rates of treated and control states move closely together. It is in the next wave in summer 2021, associated with the "Delta" variant that rates in the treated states jump faster and higher than in the control states, leading by about two months. By November 2021, the rates reconverge and again move closely together throughout the omicron wave of 2022.

To examine the impact of the policy change in a regression framework, we relate the covid variables to contemporaneous $PUIend$ and its lags over the period from January to November 2021. The next subsection employs a reduced form approach and the following one uses an instrumental variables framework. The last two subsections contain robustness checks and a discussion of the magnitudes of the estimates in the context of epidemiological research on covid.

4.1 Reduced Form Estimates

We estimate the following equation:

$$Y_{st} = \sum_{k=0}^{3} \beta_k PUIend_{st-k} + \mu_s + \tau_t + W_{st} \cdot \phi + u_{st} \tag{5}$$

where Y_{st} represents, in alternative specifications, the number of covid cases, hospital admissions, and deaths as a percentage of the population in state s in month t. We include

only contemporaneous and three lags of $PUIend$ because we found that longer lags have very small and statistically insignificant estimated effects, consistent with the patterns we observed in Figure 7. μ_s are state fixed effects, and τ_t are month fixed effects. W_{st} is a vector of time-varying state-level variables. In the main specification, W_{st} includes cumulative covid vaccination rates and heating and cooling degree-days, while in other specifications and for robustness, it includes controls for school openings, obesity, heart disease, median income, share of old-age population, hospitality industry employment, recreational mobility, pre-treatment covid case rate, unit trends, and other state policies that changed during the summer of 2021 (described in Subsection 4.3 on robustness, with variable descriptions in Appendix A and Table A3). Estimation is weighted by state population, and standard errors are clustered at the state level.

Table 10 contains results from estimating Equation (5). Specifications excluding and including the vaccination and degree-days variables W_{st} are shown, but the results are very similar. The contemporaneous coefficients on $PUIend$ are positive and statistically significant for cases and hospitalizations, but they only become significant for deaths after one lag. At the first lag, the effect grows somewhat for cases and hospitalization and then falls back at the second lag, while for deaths these two months are the largest (and the effect grows from the first to the second lag). At three lags, the impact of $PUIend$ dies out, even becoming negative as the treated and control states reconverge. The timing of the results is consistent with community spread that is rapid for infections and hospitalizations and slightly slower for deaths.[24]

The relative magnitudes of these estimated effects can be assessed for different months and against different benchmarks. Using the estimates with temperature and vaccination controls, if we compare the impact in September 2021 to the mean levels for January-June 2021 in the control states (from Table 2), of 0.66, 0.05, and 0.012 percent for cases, hospitalization, and deaths, respectively, then the monthly covid rates are estimated to double for cases and rise 125 percent for hospitalizations and deaths. During the January-June period, covid was trending downwards, so if the comparison is May-June 2021 (when they were almost equal between the treated and control states at 0.18, 0.025, and 0.0035 percent, respectively), the implied monthly rates for September approximately quadruple.[25]

Figure 8 uses the results from Table 10 to graph the cumulative increase in the covid

[24]Recall that treated states had ended PUI before the beginning of July 2021 (except for one state on July 3), so there was at least a full month for covid to spread even in the first month of treatment.

[25]The exact May-June rates for cases, hospitalizations, and deaths are respectively as follows: 0.1802, 0.0231, and 0.0040 in control states; and 0.1887, 0.0265, and 0.0031 in treated states. Concerning the control variables, the estimated coefficient on the vaccination rate is always negative, and it is statistically significant for cases and hospitalizations, but not for deaths. Heating and cooling degree-days enter with inconsistent signs and are never statistically significant.

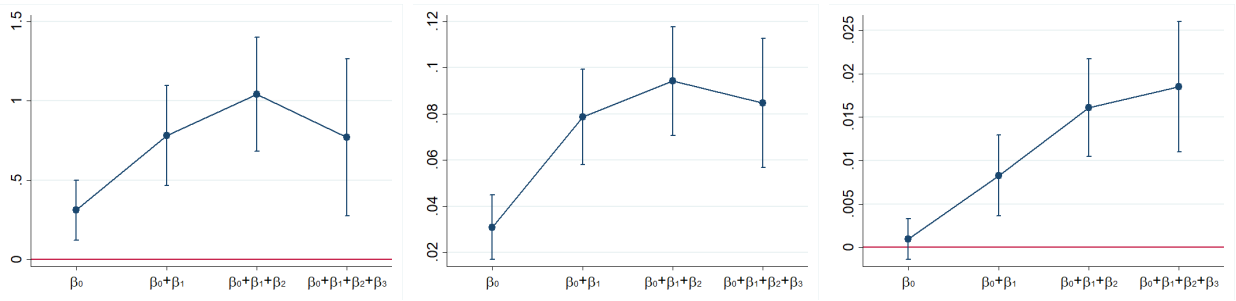
Table 10: Estimated Effects of UI Loss on Covid Outcomes: Reduced Form

	Cases		Hospitalizations		Deaths	
	(1)	(2)	(3)	(4)	(5)	(6)
$PUIend_t$	0.312	0.180	0.031	0.025	0.0009	0.0003
	(0.096)	(0.112)	(0.007)	(0.010)	(0.0012)	(0.0013)
$PUIend_{t-1}$	0.471	0.423	0.048	0.046	0.0073	0.0072
	(0.128)	(0.120)	(0.009)	(0.009)	(0.0015)	(0.0015)
$PUIend_{t-2}$	0.259	0.240	0.016	0.015	0.0078	0.0078
	(0.060)	(0.081)	(0.004)	(0.005)	(0.0018)	(0.0014)
$PUIend_{t-3}$	-0.271	-0.376	-0.009	-0.014	0.0024	0.0019
	(0.160)	(0.113)	(0.010)	(0.007)	(0.0018)	(0.0014)
Controls	No	Yes	No	Yes	No	Yes

Note: The table shows estimated coefficients from Equation (5). All specifications control for state and month fixed effects. Controls in columns (2), (4), and (6) include cumulative vaccination rate, heating degree-days, and cooling degree-days measured at $t-1$. Observations are weighted by population. Standard errors clustered on state are shown in parentheses. Observations = 506 state-months, January-November 2021.

variables based on the sum of coefficients from each equation. The graph also shows 95 percent confidence intervals for the coefficient sums. The impact on hospitalizations is already significant in the month of the policy change, and the cumulative impact becomes significant for the other variables in the month following the policy change; for all variables, it remains significant through three lags. The general shape again shows a faster rise and subsequent decline in cases and hospitalizations than for deaths.

Figure 8: Cumulative Effects of UI Loss on Covid Outcomes: Reduced Form



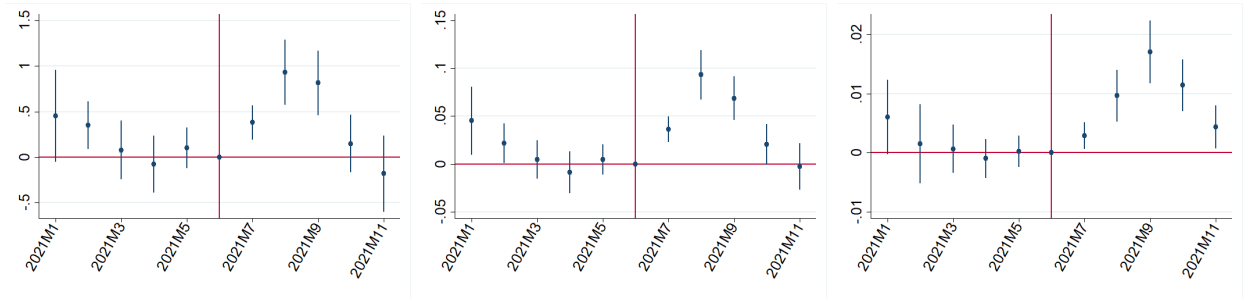
(a) Cases (b) Hospitalizations (c) Deaths

Note: The graphs show the cumulative effects of UI loss on covid outcomes from January to November 2021. Specifications of Equation 5 control for state and month fixed effects and W_{st} as in Table 10 models (2), (4), and (6). Observations are weighted by population. The vertical lines represent 95% confidence intervals, based on standard errors clustered on state.

Event studies of the differences between treated and control states in the rates of covid cases, hospitalizations, and deaths, controlling for vaccination rates, heating and cooling degree-days, and state and month fixed effects, are shown in Figure 9. Up through June

2021 (the reference month for this graph), there are no large differences between treated and control states for any of these variables, and only two instances (February for cases, January for hospitalizations) where the difference is statistically significant at the 95 percent level (neither of these are significant at the 99 percent level). For covid cases, the difference jumps in July, peaks in August, remains high in September, and then disappears in October. Covid hospitalizations show a similar pattern but with a higher jump in August than July. For covid deaths, the impact is more delayed, as the difference is only slight in July, then jumps in August, peaks in September, remains high in October, and drops down in November. These patterns are again consistent with a quick impact on covid cases and hospitalizations, lasting about three months, and a delay of about one month before the impact begins on deaths. We provide a similar analysis over a more extended time period in the specification tests section below.

Figure 9: Estimating Treated-Control Differences in Covid Outcomes, by Month



(a) Cases (b) Hospitalizations (c) Deaths

Note: The graphs show event studies of the impact of UI loss on covid outcomes from January to November 2021. Specifications control for state and month fixed effects as well as cumulative vaccination and heating and cooling degree days at $t-1$ as in Table 10 models (2), (4), and (6). Observations are weighted by population. Standard errors are clustered on state. The vertical lines represent 95% confidence intervals. Observations = 506 state-months.

4.2 Instrumental Variables Estimates

The reduced form results do not account for the mechanism through which the policy change affects covid outcomes. To investigate the mechanism of re-employment following UI loss, we estimate the relationship in an instrumental variable framework where the endogenous variable is the UE transition, which may be jointly determined with the covid situation. The question is whether states that experience increases in UE rates because of the policy change subsequently show worse covid outcomes. The estimating equation is as follows:

$$Y_{st} = \sum_{k=0}^{3} \beta_k \cdot UE_{st-k} + \mu_s + \tau_t + W_{st} \cdot \phi + \epsilon_{st} \tag{6}$$

where UE_{st-k} is the transition rate for state s in month t-k, where we have netted out differences in individual characteristics using Appendix Equation (A.5), and other variables are defined as previously. In order to capture delayed effects of UE transitions on covid outcomes, similarly to the reduced form Equation (5), this equation includes three monthly lags. To estimate the β_k resulting from the policy change $PUIend$, we instrument UE and its lags with $PUIend$ and its lags, estimating two-stage least squares (2SLS) regressions. The four first-stage equations for UE_{t-j} $(j = 0, 1, 2, 3)$ are as follows:

$$UE_{st-j} = \sum_{k=0}^{3} \beta_k PUIend_{st-k} + \mu_s + \tau_{t-j} + \epsilon_{st-j} \tag{7}$$

First-stage estimates are provided in Appendix Table B8; all f-statistics are highly significant. The coefficients on the contemporaneous instruments are similar to those we estimated using individual data based on Equation (1) in Section 3.1.1 above: close to 0.1 for each $PUIend_{t-k}$ associated with UE_{t-k}.[26] The second-stage results are shown in Table 11. The general patterns are similar to those from the reduced form analysis (Table 10) with earlier peaks for cases and hospitalizations than for deaths.

Table 11: Estimated Effects of UI Loss on Covid Outcomes: Two-Stage Least Squares

	Cases		Hospitalizations		Deaths	
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{UE}_t$	0.223	0.206	0.107	0.166	-0.0142	-0.0163
	(1.325)	(0.191)	(0.086)	(0.099)	(0.0263)	(0.0276)
$\widehat{UE}_{t-1}$	3.927	3.790	0.441	0.428	0.0744	0.0739
	(1.258)	(1.243)	(0.114)	(0.101)	(0.0203)	(0.0203)
$\widehat{UE}_{t-2}$	2.270	3.005	0.146	0.216	0.0867	0.0894
	(0.862)	(0.731)	(0.067)	(0.051)	(0.0115)	(0.0155)
$\widehat{UE}_{t-3}$	-3.454	-3.217	-0.209	-0.170	0.0041	0.0047
	(1.373)	(1.524)	(0.099)	(0.119)	(0.0234)	(0.0225)
Controls	No	Yes	No	Yes	No	Yes

Note: The table shows estimated 2SLS coefficients in Equation 6. All specifications control for state and month fixed effects. Controls in columns (2), (4), and (6) include cumulative vaccination rate, heating degree-days, and cooling degree-days measured at $t-1$. Observations are weighted by population and inversely by standard errors from Equation (A.5). Standard errors clustered on state are shown in parentheses. First-stage f-statistics for all the $\widehat{UE}$ are shown in the first-stage results, Appendix Table B8. Observations = 506 from January to November 2021.

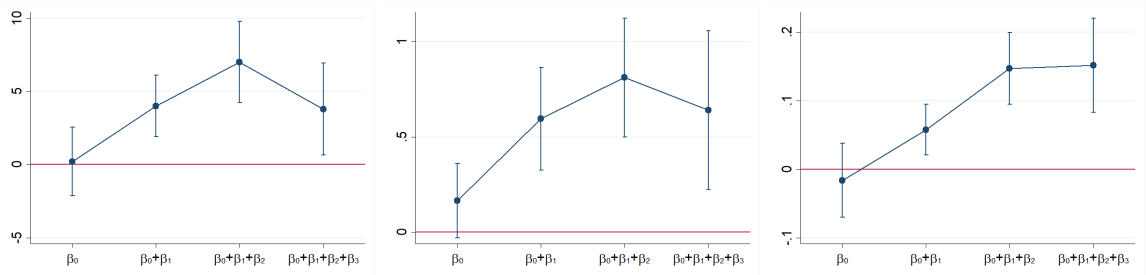
The magnitudes of the second-stage estimates need to be evaluated with respect to the

[26] Equation (7) is an aggregated form of Equation (1). The first-stage results are driven almost entirely by the contemporaneous instruments; the coefficients on non-contemporaneous instruments are nearly always small and statistically insignificant.

relevant range. Applying the estimates from both the individual and state-level analysis that $PUIend$ increased UE by about 0.1, the impact on monthly cases in September 2021 is about 0.7 percentage points, on hospitalizations about 0.06, and on deaths about 0.016. These are very similar to the impacts estimated in the reduced form analysis above.

The 2SLS estimates of the cumulative impact on covid outcomes are plotted in Figure 10. Similar to the reduced form, these show faster increases and subsequent declines in cases and hospitalizations than for deaths. All sums of coefficients are statistically significant from the month following the policy change, and they remain so through three lags afterward.

Figure 10: Cumulative Effects of UI Loss on Covid Outcomes: 2SLS



(a) Cases (b) Hospitalizations (c) Deaths

Note: The graphs show the cumulative effects of UI loss on covid outcomes from January to November 2021, controlling for state and month fixed effects as in Table 11 columns (2), (4), and (6). Observations are weighted by population. The vertical lines represent 95% confidence intervals, based on standard errors clustered on state.

Finally, while our focus is on the impact of the UI policy change in summer 2021, for which we have an identification strategy to estimate causal effects, we have also estimated Equation (6) using OLS. This approach may suffer from simultaneity bias, to the extent that covid outcomes and re-employment decisions are jointly determined. If the feedback from covid to re-employment is negative, then the bias is negative. Consistent with that logic, the OLS results (in Appendix Table B9) show smaller effects than the 2SLS. The sum of the impact on deaths is 0.042 with state-level controls or 0.062 without those controls, which are smaller than the 2SLS estimates, yet the OLS estimates are still highly significant and qualitatively similar to the 2SLS. Results are noisier for cases and hospitalizations, but the OLS estimates suggest that the negative externality of re-employment for health is a more general relationship during the pandemic, not only during the summer of 2021, when we can identify it using a policy experiment.

4.3 Robustness and Specification Tests

To examine the robustness of the estimated effects of early termination of pandemic UI on covid outcomes, we have investigated a large number of alternative specifications. In one

type, we control for state-specific unit trends. Besides covid vaccination rates and heating and cooling degree-days, we have controlled for state-month varying school openings and for the following variables interacted with the treatment period: pre-pandemic levels of obesity, heart disease, median income, the share of the population aged over 64, the ratio of hospitality industry employment in 2021:II to that of 2019:II, mobility to recreational places in May 2021 relative to January 2020, and the May 2021 rate of covid cases. Sources of all these variables are provided in Data Appendix A, and means by treated and control states are shown in Appendix Table A3. Estimates from the unit trends specification and from the specification including all these covariates are shown in Table 12. The results are similar to those in Table 10.

Table 12: Estimated Effects of UI Loss on Covid Outcomes, Other Specifications - Unit Trends and Additional State Controls

	Cases	Hosps	Deaths	Cases	Hosps	Deaths
	(1)	(2)	(3)	(4)	(5)	(6)
$PUIend_t$	0.161	0.027	0.0005	-0.071	0.005	-0.0008
	(0.121)	(0.008)	(0.0010)	(0.120)	(0.009)	(0.0014)
$PUIend_{t-1}$	0.731	0.073	0.0097	0.278	0.027	0.0060
	(0.206)	(0.019)	(0.0024)	(0.129)	(0.009)	(0.0017)
$PUIend_{t-2}$	0.126	0.008	0.0069	0.385	0.030	0.0084
	(0.085)	(0.007)	(0.0020)	(0.095)	(0.007)	(0.0023)
$PUIend_{t-3}$	0.265	0.041	0.0071	-0.408	-0.019	0.0014
	(0.255)	(0.020)	(0.0030)	(0.119)	(0.007)	(0.0014)
Unit Trends	Yes	Yes	Yes	No	No	No
Controls	No	No	No	Yes	Yes	Yes

Note: All specifications include state and month fixed effects, cumulative vaccination rate, heating degree-days, and cooling degree-days measured at $t-1$. Columns (1-3) additionally control for state-specific unit time trends. Columns (4-6) control for school openings and the following state-level variables interacted with treatment period: state-specific obesity in 2020, heart disease rate in 2019, covid case rate in May 2021, age 65-plus share in May 2021, median income in 2020, hospitality industry employment in the second quarter of 2021 as a ratio to the second quarter of 2019, and Google recreational mobility in May 2021 as a ratio to January 2020. Observations are weighted by population. Standard errors clustered on state are shown in parentheses. Observations = 506 state-months, January-November 2021.

The control for the May 2021 rate of covid cases, interacted with the treatment period, accounts for variation across states in immediate pre-treatment covid. In another robustness check, we estimate Equation (5) dropping four treated states that experienced a rise in covid cases from May to June 2021: Arkansas, Missouri, Oklahoma, and Utah. These increases are not enough to produce an upward trend in the event studies of Figure 9 and they may reflect some impact of the PUI termination policy already in June, but to check whether these states are driving the treatment-control differences in July and August, we drop them from the sample. Results are shown in columns (1-3) of Table 13 and are similar, if anything

slightly larger compared with those in Table 10.

Table 13: Estimated Effects of UI Loss on Covid Outcomes, Robustness Checks - Dropping Treated States with June Case Rise and Controlling for Other Covid Policies

	Cases	Hosps	Deaths	Cases	Hosps	Deaths
	(1)	(2)	(3)	(4)	(5)	(6)
$PUIend_t$	0.110	0.020	-0.0004	0.195	0.023	0.0013
	(0.126)	(0.011)	(0.0015)	(0.108)	(0.009)	(0.0015)
$PUIend_{t-1}$	0.506	0.051	0.0069	0.170	0.019	0.0050
	(0.133)	(0.008)	(0.0018)	(0.153)	(0.010)	(0.0017)
$PUIend_{t-2}$	0.253	0.015	0.0093	0.306	0.024	0.0072
	(0.087)	(0.005)	(0.0020)	(0.092)	(0.007)	(0.0022)
$PUIend_{t-3}$	-0.412	-0.016	0.0015	-0.538	-0.031	-0.0009
	(0.113)	(0.007)	(0.0015)	(0.170)	(0.012)	(0.0022)
Sample restriction	Yes	Yes	Yes	No	No	No
Policy controls	No	No	No	Yes	Yes	Yes

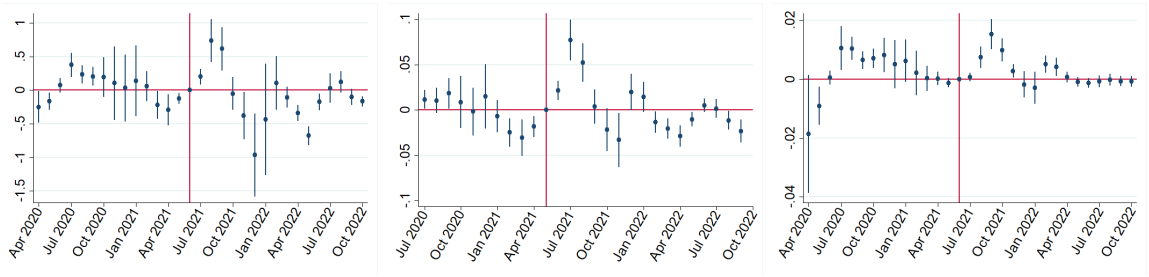
Note: All specifications include state and month fixed effects, cumulative vaccination rate, heating degree-days, and cooling degree-days measured at $t-1$. Columns (1-3) exclude the four treated states with increasing covid case rates May-June 2021 (Arkansas, Missouri, Oklahoma, and Utah), and Columns (4-6) control for covid-related policy changes that could lead to higher covid in treated states, including face mask mandates, school opening interacted with school mask mandate, school opening, and prison visit restrictions. Observations are weighted by population. Standard errors clustered on state are shown in parentheses. Observations = 462 for Columns (1-3) and 506 for Columns (4-6) state-months, January-November 2021.

For a final type of alternative specification, we draw upon a large database of covid-related state-level policies, including 149 separate policies that changed during the pandemic, and analyze 31 that changed during the summer of 2021 (Raifman et al., 2022). The policies, listed in Appendix Table A4, include eviction restrictions and other renters' protections, prison visit regulations, face masking in public spaces, schools, and exemptions based on vaccination, quarantine mandates, re-opening of bars, restaurants and overnight businesses, and state of emergency. From these, we create variables for types of policies whose impact would tend to exacerbate covid rates in treated relative to control states, in order to test the robustness of our estimated effects under the most stringent specification, and we include these as control variables in estimating Equation (5). The results are shown in Table 13, columns (4-6). All the estimates from this exercise are similar to those presented above.

Turning to specification tests, in Figure 9, we provided a type of event study test of the estimated effects of $PUIend$ on covid outcomes in 2021. Figure 11 takes a longer view, including all months from the beginning of the pandemic in April 2020 through October 2022, and controlling for vaccination rates, heating and cooling degree-days, and state and month fixed effects. In the year before treatment, there are only small and generally insignificant differences between treated and control states. During July, the case and hospitalization rates

show a small jump, followed by a much larger one in August, and slight fall in September, and then a drop to zero in October, with some evidence of catch-up by control states that ended PUI in September. The results for covid deaths are similar but with a negligible uptick in July, big jumps in both August and September followed by a slower decline reaching zero in November. All three graphs illustrate the unusual difference between treated and control states that existed only during the immediate aftermath of $PUIend$, the policy difference during summer 2021.

Figure 11: Estimating Treated-Control Differences in Covid Outcomes, Apr 2020-Oct 2022



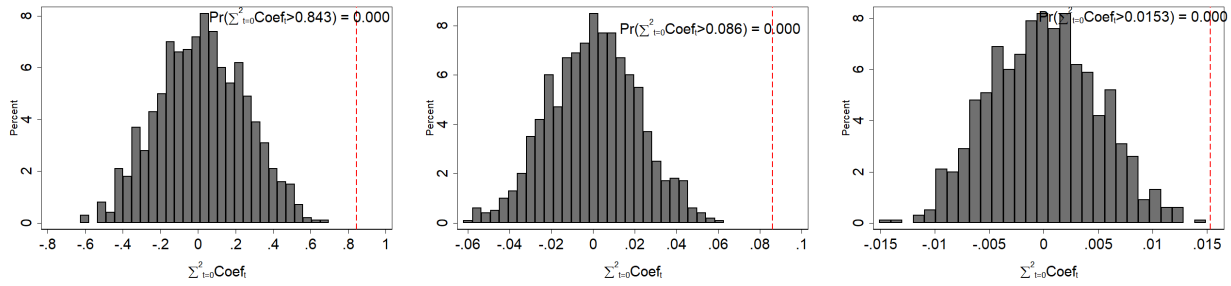
(a) Cases (b) Hospitalizations (c) Deaths

Note: The graphs show event studies of the treated-control state differences in the rates of covid cases and deaths from April 2020 to October 2022 and for hospitalizations from July 2020, controlling for state and month fixed effects as well as cumulative vaccination and heating and cooling degree days at $t-1$. Observations are weighted by population. The reference period is June 2021. Vertical lines represent 95% confidence intervals, based on standard errors clustered on state.

Moreover, the patterns in Figure 11 are very similar to its counterpart for UE in Figure 5. Comparing these event-studies, observe that differences in re-employment rates and covid outcomes between treated and control states move closely together: prior to July 2021 the differences are negligible, then they increase simultaneously in treated relative to control states, and subsequently they reconverge quickly afterwards. This implies that if there is some unobserved factor other than the long list of variables we have considered (including in the robustness checks) that caused the divergences from July 2021, then that factor must have disappeared exactly two months later, coincidental with the ending of PUI in all states.

Permuting the assignment of the policy across states provides another type of test for the estimated results. While this test could be carried out separately for each lag of $PUIend$, we report it more concisely by focusing on the cumulative sum of coefficients through two lags as shown in Figure 8. Calculating this sum for each of 1000 permutations of the treatment assignment across states for each of the covid outcome variables produces Figure 12. The results demonstrate that the estimated effects are extremely unlikely with placebo assignment.

Figure 12: Permutation Tests



(a) Cases (b) Hospitalizations (c) Deaths

Note: This graph shows permutation results from the reduced form Equation (5) for the sum of estimated coefficients on the binary treatment and its lags, using state-month data from February to November 2021. The models include cumulative vaccination rate and heating and cooling degree days measured at $t-1$, and state and month fixed effects. The simulation is conducted 1,000 times with randomized treated states for each dependent variable. Each vertical line indicates the sum of the coefficients from Columns (2), (4), and (6) in Table 10.

4.4 Discussion

What do these estimates imply for the number of additional covid cases, hospitalizations, and deaths in the treated states? We calculate these numbers taking into account the population of 90.6 million in those states, the fact that the policy difference existed for two months, and the estimates of the policy impact in Tables 10) and 11, in the latter case also taking into account the estimated mean UE increase of 0.1 (from Table 5). The results from these calculations are shown in Table 14. Using both the reduced form and IV approaches and with or without covariates, the estimates imply an additional 1.2 million covid cases, 110,000 extra hospitalizations, and additional deaths of 27,000-29,000.

Table 14: Estimates of Additional Covid Cases, Hospitalizations, and Deaths

	Reduced Form		2SLS	
Cases	1,322,760	1,201,356	1,122,896	1,231,254
Hospitalizations	115,968	110,532	106,364	116,693
Deaths	27,361	27,180	29,191	29,590
Controls	N	Y	N	Y

Note: The table shows the additional numbers of people affected by covid in the treated states, obtained by multiplying the treated state population (90.6 mln) with the sum of coefficients (%) of the first and second lags from the specifications in Table 10 and 11 times 2 months of treatment. For IV specifications, we multiply first stage coefficients by 0.1, based on Table 4. All specifications control for state and month fixed effects. Controls include cumulative vaccination, heating degree days, and cooling degree days at $t-1$.

Do these magnitudes make sense given the known patterns of covid infections, transmission, and severity? First of all, the relationships among these estimated impacts on cases, hospitalizations, and deaths are roughly similar to those throughout the covid pandemic: on average, about one in 10-12 new cases is hospitalized, and deaths are around one-fifth to

one-sixth of hospitalizations. Comparing to state-month data from CDC (2022) and HHS (2023), we calculate the total numbers in the treated states from August to October 2021 as follows: cases are 3.7 million, the number of hospitalizations is 317,000, and the covid death count is 51,000. So our estimates represent 32 percent of all cases, 35 percent of hospitalizations, and 53-57 percent of deaths. The Delta variant of covid, by far the dominant strain during the period we study, was much more transmissible than earlier covid variants, and there is also evidence that it led to more severe illness, which may account for the somewhat higher death rate during our period of study (Bast et al., 2021; Bolze et al., 2022; Hart et al., 2022).

Concerning the impact on the infection rate, is it plausible that the increased UE transitions we observe resulting from the policy change raised covid cases by over one million? Calculating the total infections requires estimations or assumptions on several quantities: the amount of additional workplace exposure resulting from $PUIend$ in the treated states, denoted E_0, the rate at which exposure leads to infection of the re-employed workers, r, and two aspects of the subsequent chain of transmissions (through families and social and work networks), the basic reproductive number for secondary infections, R_0, and the number of generations in the chain, n, before the infection rate becomes negligible. Ideally, if these quantities were known and place- and time-invariant (or using appropriate averages over all places for the whole time-period), the total covid cases resulting from the policy could be calculated as $E_0 \cdot r \cdot {R_0}^n$.

In practice, we can estimate E_0 from our analysis above, we can draw upon epidemiological research for estimates of average R_0 and n, and we can make alternative assumptions about r to assess the plausibility of the estimated more than one million additional cases resulting from the policy. For the additional exposure E_0, we have estimated in Subsection 3.1, that the early termination of PUI resulted in UE transitions of 120,696 in July and 117,551 in August. Assuming that all the July UE remained in their jobs in August, this implies that the treated states had 358,943 person-months greater exposure through new employment than they would have had in the absence of the policy.

R_0 varies not only with virus strain but also over time, falling as a population becomes more immune to a new strain, and with local health and social conditions, including the vaccination rate. Several sources provide estimates of average R_0 for the Delta strain of covid at around five, significantly higher than for the original and Alpha strains (Burki, 2021; Liu and Rocklöv, 2021; Tian et al., 2021; Dadras et al., 2022).[27] n varies not only with virus strain, but also stage of the infection wave and other health and social conditions.

[27] Burki (2021) report the R_0 for Delta at "nearly seven;" Liu and Rocklöv (2021) report a mean Delta R_0 of 5.08; Tian et al. (2021) report a range of of 4.04 to 5.0; and Dadras et al. (2022) report "seven or more."

Estimates of n are related to generation time, the number of days from one infection to the next generation, usually approximated by serial interval, the time between the onset of symptoms for one individual and onset for those infected by them in the next generation. For the Delta variant, the serial interval has been variously estimated at 4-7 days.[28] With these estimates, even if lengthening generation time leads to growth in n that decreases over time, an $n = 5$ or 6 seems plausible, so that average ${R_0}^n$ could be over 3000, possibly much higher, although there is a wide confidence interval around any number. Regarding the rate of infection among re-employed workers, r is unknown, but we can consider a range of alternatives: if $r = 0.1$, this would imply $E_0 \cdot r = 36,000$ so that ${R_0}^n$ could be as low as 33 to generate a total of 1.2 mln covid cases. If $r = 0.01$, so that one percent of new workplace exposures are actually infected, then $E_0 \cdot r = 3,600$, then ${R_0}^n = 330$ would be sufficient and if r=0.001, so that only one in a thousand new exposures results in infection, then $E_0 \cdot r = 360$ and ${R_0}^n = 3300$ would result in 1.2 mln cases.[29]

Finally, our results should be understood within the specific context of the treated states and the treatment period. As we have discussed, the populations of treated states have lower average incomes, educational attainment, and vaccination rates, all of which are likely to raise R_0, relative to the control states. The early termination of PUI occurred just when the covid rate was exploding in the Delta wave, also raising R_0 and n (by shortening generation time). The vaccine had been designed for earlier covid strains and was less effective against Delta. Moreover, the rate at which individuals became newly vaccinated decreased sharply from March 2021 in the treated states, implying that immunity may have been waning and providing another reason for the virus to spread more easily. Although the data and context do not permit us to estimate how large the impact of ending PUI would have been in other states at other times, it seems clear that the timing of $PUIend$ was particularly unpropitious and that it took place in relatively more vulnerable states. In the absence of a pandemic, we would expect little to no effects like we estimate here. On the other hand, while it was clear to many in the summer of 2021 that the pandemic was far from over and that more variants, possibly more virulent, were coming (The White House, 2021), the tracking of the Delta variant in the US was slowed by a low rate of genomic sequencing during this period (Asgari, 2021).

[28]Estimates include 3.7 (Ryu et al., 2022), 7 (Hart et al., 2022), and 3.9 days (Madewell et al., 2023). In general, research has documented a shorter serial interval than incubation period, implying that covid transmission often occurs prior to symptom onset (Wu et al., 2022; Xin et al., 2022).

[29]Workers re-employed in remote work would not become susceptible, of course, but the fraction of such workers is small among the re-employed during this period: in our CPS data, only about two percent of employed workers in July and August 2021 who were unemployed the previous month in the treated states report that they are teleworking because of covid.

5 Benefits and Costs

Our analysis implies that the early termination of PUI may have had some benefits, most clearly in the form of increased UE transitions. Costs of the policy are related to health outcomes, including hospitalization, lost work time due to illness, and deaths. UI spending is a transfer, so the reduction resulting from the policy might be a benefit to the government, but it is a loss to households with unemployed workers. Therefore, the benefit-cost calculation depends upon one's viewpoint: unemployed workers, divided into those who find jobs versus those who do not, the government budget, and overall "social welfare." In this section, we provide a rough assessment of the magnitudes of each type of benefit and cost and then discuss how they sum up from the perspective of these different groups.

Placing a dollar value on the additional jobs resulting from $PUIend$, the simplest procedure is to add up the total wages of those becoming re-employed as a result of the policy change, which we estimated in Subsection 3.1 as 120,696 (±54,190) in July and 117,551 (±52,778) in August 2021.[30] Because we cannot know which individuals would make a UE transition in the absence of the policy, we assume equal probability across all re-employed that their re-employment is affected by the policy. The average monthly earnings of those re-employed in July is $3001 (obtained by multiplying their reported weekly earnings by 4.3); assuming their wages remain the same in August, the additional earnings for this group is $724mln (±325mln) over the two months. For those becoming re-employed in August because of the policy, average monthly earnings are $2883, resulting in a total of $339mln (±152mln) for this group. Putting the two groups together, the total additional wages estimated to result from $PUIend$ are $1.063bln (±477mln).[31] These additional wages of the re-employed represent a benefit of the early termination of PUI.

On the other hand, the re-employed workers in the treated states lost UI benefits, which should then be subtracted from their wages. To estimate the magnitude of their UI losses requires a more general estimation of the extent to which early termination of PUI reduced UI spending. For this purpose, we use PEUC, FPUC, and PUA Financial Transactions and state UI benefits paid from the regular monthly programs (Employment and Training Administration, 2023*a*,*b*). We define total benefits, TB_{st}, as the sum of the PUI programs and regular state UI spending for each state and month. Rather than attributing all changes in TB_{st} to $PUIend$, we instead estimate the impact after scaling by the number of unemployed, U_{st} (from the CPS). Appendix Figure B4 plots $(TB/U)_{st}$ separately for treated and control states by month during 2021. UI benefits per unemployed are higher on average in control

[30] ± refers to two standard errors around the point estimate.

[31] Using their data on direct deposits, Coombs et al. (2022) estimate the increased wages at a similar magnitude: $900mln.

states, but the two series move in parallel through June 2021, after which they diverge sharply: there is little change in the control states but the benefit per unemployed in the treatment states plunges in July and August. After PUI is terminated in all states on September 4, 2021, the control states follow with a sharp plunge, and the ratio $(TB/U)_{st}$ becomes similar in the two groups of states. Not all of the declines are immediate, probably because delayed payments were still going out after PUI was ended. Also note that CPS unemployment in the denominator is not the same as the number of UI recipients.

To estimate the effect of the policy change, we use the following regression equation:

$$(TB/U)_{st} = \beta \cdot PUIend_{st} + \mu_s + \tau_t + \epsilon_{st}, \tag{8}$$

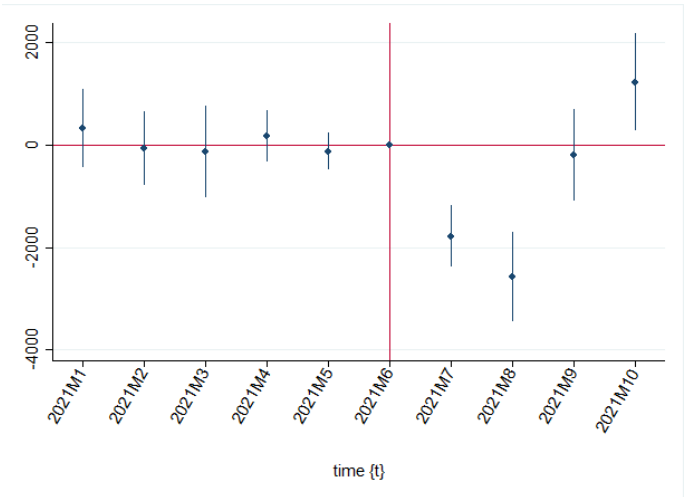
with variables defined as before. The result estimated with data for January-August 2021 is that $\hat{\beta}$ = -2208.83 (SE=376.94). On average, an unemployed person in a treated state lost about $2200 per month as a result of the policy change.

We also carry out an event study of this impact through October 2021, as follows:

$$(TB/U)_{st} = \sum_{k=Jan2021}^{Oct2021} \beta_k \cdot PUIend_{sk} + \mu_s + \tau_t + \epsilon_{st}, \tag{9}$$

with results plotted in Figure 13. Differences between treated and control states prior to July 2021 are negligible, but they become significant in July and August, with reconvergence in September-October. This analysis provides further evidence, from a different point of view using different data, on the large but temporary shock resulting from early PUI termination.

Figure 13: Event Study of Impact of *PUIend* on UI Spending per Unemployed



Note: Total UI cost includes state benefits paid and PUI disbursement to states. The values represent the population-weighted average of treated and control states over months of total UI cost over the number of unemployed.

We calculate the total reduction in UI spending from early PUI termination by multiplying the estimate above of $2200 per unemployed per month by two months of treatment (July and August) and by the number of unemployed in the treated states in June (2,011,601), leading to $8.88bln (±3.03bln).[32] How to evaluate the UI reduction depends on perspective: it is a gain to the government budget but a loss to families with unemployed workers. The magnitude of the cuts dwarfs the increased wages of $1.1bln received by re-employed workers estimated above. From the perspective of all unemployed workers, their increased wages make up for only about one-eighth of their lost UI benefits.[33]

Increased infections and hospitalizations also create economic costs. Infections may lead to lost work time, which we calculate using CPS questions on absence from work during the reference week due to illness and on working part-time during the reference week although usually full-time, again with illness cited as the reason. Appendix Table B10 shows the mean of this variable by two-month groups during 2021, for treated and control states separately. Consistent with covid infections resulting in work absences, treated states show a jump in July-August and a larger one in September-October falling off slightly in November-December. Meanwhile, in the control states, lost hours fall slightly in July-August and then rise slightly in September-October and jump in November-December.

The mimicking by control states of lost work time in the treated states at a two-month lag is very similar to what we have observed for both re-employment and covid outcomes in the analyses above, providing further evidence of the connection of health and labor outcomes during this period. To assess the statistical significance of the treated-control state difference, while taking into account other factors that may affect lost work due to illness, we estimate a regression analogous to Equation (5) with lags in $PUIend$ but using individual data on all civilian employed and controlling for individual characteristics as in Table 4. Results for the $PUIend$ coefficients, with two and three lags, appear in Table 15.

The estimates imply that the early termination of PUI resulted in a loss of 0.2 work hours per week for each employed individual, on average, in the treated states.[34] Multiplying this by the number of weeks per month (4.3), by the two months of treatment, and by the number

[32] We do not include lagged effects in this calculation because the difference in Figure 13 is not significant after August and because (from Figure B4) the effect is nearly immediate in the treated states and the lag is much greater in the control states, which had somewhat worse UI payment timeliness (see Table 1); it seems unreasonable to credit the UI cuts in treated states with the worse timeliness in control states. In any event, an adjustment including lags would have little impact on our qualitative conclusions.

[33] This estimate can be compared with Coombs et al. (2022), who using different data and methods find a very similar ratio: one-seventh.

[34] Appendix Figure B5 graphs the cumulative sums of $PUIend$ coefficients and shows the sums are statistically significant. An event study based on a specification analogous to Equation (9), but estimated with individual data as in Table 15 is reported in Appendix Figure B6, showing the treated-control difference in illness-related losses in work time spiking in September and then reconverging over the next two months.

Table 15: Estimated Effects of UI Cuts on Illness-Related Lost Work Time

	Lost Work Time	
$PUIend$	0.007	0.007
	(0.043)	(0.045)
$PUIend_{t-1}$	0.110	0.114
	(0.036)	(0.047)
$PUIend_{t-2}$	0.083	0.078
	(0.047)	(0.053)
$PUIend_{t-3}$		0.008
		(0.053)

Note: Specifications control for individual characteristics, as in Table 4, and state and month fixed effects. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 431,824, February-November 2021.

of civilian employed in treated states in June 2021 (41,449,423) yields an estimate of the total work-time lost of 71.3mln (±50.6mln) hours. To place a monetary value on this time, we use the average hourly earnings in the treated states of $20, yielding a total value of $1.456bln (±1.012bln). Thus, the gain in additional wages from re-employment are offset by the lost productivity due to increased illness.

Turning to costs of hospitalizations, the IV estimates imply that ending PUI early resulted in extra hospitalizations equal to 126,000 (±37,700), as discussed above. The cost of hospitalization per patient varies widely across patients and conditions: estimates of averages from FAIR Health (2021) in September 2021 are that complex covid hospitalizations (involving either the intensive care unit, ICU, or invasive mechanical ventilation, IMV) had an average cost of $317,810, while non-complex inpatient care for covid cost $74,591, on average.[35] This source does not provide the shares of these among all covid hospitalizations, but Iuliano et al. (2022) (Table 2, p. 150) report that 17.5 percent of hospitalizations during the Delta wave were in the ICU (the number of those with IMV outside the ICU is unclear). Using this number, and with the assumption that hospitalizations caused by the early ending of PUI had the same ICU rate as those that would have occurred in its absence, implies additional ICU stays with covid of 22,050 (±6,600), costing $7.008bln (±2.096bln). Calculating similarly for the non-ICU patients, the cost is $7.754bln (±2.320bln). This implies that the total additional hospitalization cost resulting from ending PUI early is $14.8bln (±4.416bln).

Most of these costs would have been borne by the government, principally through Medicare and Medicaid. Although we do not have exact figures for summer 2021, many sources

[35]This latter figure is consistent with reports of an average cost of about $10,000 per day for all hospitalizations (HealthCare, 2023), given that Iuliano et al. (2022) report the average length of hospital stays for covid patients during the Delta wave as 7.6 days. FAIR Health (2021) also report average outpatient covid cost of $2557, but it is unclear how many covid cases result in outpatient status, so we ignore these costs.

cite figures of about 63-65 percent for the government share in hospitalization costs.[36] Applying 63 percent to the total additional costs gives a point estimate of increased government spending on hospitalization costs of about $9.3bln. Thus, from the government budget perspective, the UI savings resulting from the policy are offset, probably more than offset, by the increased spending on Medicare and Medicaid. From a social perspective, of course, the full costs of covid care should be counted.

Subtracting the total costs (due to lost work hours and hospitalizations) of $15.5bln [10.8bln, 27.7bln] from the benefit of $1.1bln [0.6bln, 1.6bln] from additional wages yields a point estimate of the net benefit-cost equal to negative $14.4bln. The confidence interval formed by two estimated standard errors around this point estimate is [-27.1bln, -9.2bln]. So essentially none of the distribution of estimated net benefit values is positive.

Beyond these monetary calculations, our estimates imply deaths resulting from the policy at 27,000(±6500). From a social point of view, the policy has negative net benefits, so the value of a statistical life (VSL) implicit in the policy is also negative.

It is important to note several caveats. We limit our calculations to the impact of the two-month policy difference and do not extrapolate the estimates to the control states, which ended PUI in September, because we can credibly estimate causal effects only for the policy difference in July and August. This is not a full welfare analysis because we exclude several types of potential benefits and costs that we are unable to measure. We do not measure costs of long covid, the psychological costs of depression and anxiety, or the illness-related work time lost among workers who exit employment or those who do not shift from full-time to part-time status. Nor do we attempt to value leisure of unemployed people, possible job displacement resulting from re-employment of unemployed, or the long-term consequences of changes in unemployment. We do not attempt to measure effects on incumbent workers, producer (employer) surplus, or disutility from illness. The analysis also ignores distributional considerations, but it is likely that the job gains and covid costs of the policy are unequally distributed across the population by income, education, and demographic characteristics.

Other than hospitalizations, we do not measure health expenditures related to covid (outpatient care, vaccinations) nor the reduced health spending for non-covid conditions, probably resulting from fear of infection in health facilities, because it is far from obvious

[36]For example, Liang, Moore and Soni (2020) reports the percentages of hospital costs and hospital stays covered by Medicare and Medicaid as 66.3 and 63.6 percent, respectively, in 2017. The government share seems to have remained roughly the same during covid: Owens (2021) uses the Healthcare Cost and Utilization Project data from 13 states to compute the percentages of hospitalizations paid by Medicare and Medicaid for April-September 2019 at 64.1 percent and for April-September 2020 at 62.4 percent. These figures likely understate costs, if covid hospitalizations are more expensive (e.g., requiring longer stays or more likely involving the ICU or IMV) than non-covid hospitalizations.

that postponement of diagnosis and treatment is a benefit. We do not account for spillovers from UI to other programs, although it is not clear whether these are positive or negative. For example, they could be negative if someone losing their PUI but unable to find a job may suddenly qualify for TANF, SNAP, or other programs, or might go on disability insurance.

Finally, all the types of costs and benefits have large standard errors, which we have reported in order to show the ranges of possible estimates. But no case within these ranges, even at the extremes, would lead to a positive net monetary benefit. None would imply a positive value of a statistical life.

6 Conclusion

The crux of policy decisions during the covid pandemic was the dilemma of how to maximize safety, reducing contagion and its consequences, with the least damage to incomes, education, and economic growth. The trade-offs between lockdown-type regulations and consumer spending and between school closings and student learning remain controversial. Decisions about unemployment insurance face a similar trade-off, even if it was not explicitly stated: extra benefits could increase social distancing and thus reduce contagion by encouraging workers to stay at home, but at the cost of higher public spending and lower employment. Some states, those we have denoted as "treated" in this paper, decided to reduce benefits, largely with the motivation to raise re-employment, implicitly placing a higher weight on economic than public health. This paper assesses the consequences of this policy choice for the trade-off between labor market and health outcomes.

Our first step is to estimate the impact of unemployment insurance benefits on job-finding behavior, for which the pandemic is an unusual context. For one thing, economic conditions in general and labor markets in particular have been so unstable that it may be impossible to uncover systematic relationships. For another, beyond its usual role of providing social insurance and addressing liquidity problems, which could be still more important in the face of the huge unanticipated shock of the pandemic, the unemployment benefit system has an additional, perhaps more important function during this period: slowing contagion by increasing social distancing. Any impact of unemployment insurance on job-finding and unemployment duration may be decidedly secondary to these concerns.

Despite the volatility of the covid period, our analysis finds large, robust, statistically significant impacts of unemployment insurance on the probability of re-employment, on the hazard from unemployment to employment, and on the duration of unemployment. The policy is estimated to raise the re-employment rate about 10 percentage points relative to an unconditional mean of about 23 percent. The estimate is similar across different reasons for

unemployment, and it varies little with the empirical specification. It does vary with marital status (consistent with liquidity effects of UI), age and teleworking occupation (consistent with safety motives), sex, and race. We also estimate hazard functions and duration models, finding benefit elasticities in the range 0.5-0.7, similar to those in nonpandemic studies.

Given the robust finding that ending pandemic UI increased re-employment, we then address the consequences for covid outcomes. We find an immediate rise in covid cases and hospitalizations, followed with a lag by increased deaths. Estimating in a framework with lags and instrumental variables to capture the re-employment mechanism, we find a cumulative impact of 1.2mln cases, 110,000 hospitalizations, and 27,000-29,000 deaths from cutting UI. Relative to the treated states just before treatment or to the control states during treatment, all the covid outcome rates in the treated states quadruple.

A natural question is whether there was some other simultaneous change or some type of vulnerability peculiar to treated states that can explain the sharp divergence in re-employment rates and in the covid outbreak during this period. Our basic regression specifications for covid outcomes control for temperature deviations and vaccination rates, as well as state and month fixed effects, and the estimates are little changed when we control for school openings, for state-specific unit time trends, and for interactions between the treatment period and a set of pre-treatment variables capturing various types of vulnerability: obesity, heart disease, May covid case rate, old age share, median earnings, and recovery of hospitality industry and recreational mobility. As a further robustness check, we draw upon a database of 149 state-level covid-related policies, and control for 31 of those which could have produced worse covid outcomes in the treated relative to the control states and which also changed during the summer of 2021; the results with these controls are again very similar.

Also consistent with a causal interpretation of these results, we find no difference between treated and control states before the treated states cut PUI in late June, and the treated-control difference vanishes after the control states cut PUI in September. Thus, if it were the case that some unobserved factors other than the UI policy change account for the differences in re-employment and covid outcomes, those factors must not only have started in the same states at the same time as the UI cuts but also lasted for precisely the same two-month period. Permutation tests provide an alternative randomization inference approach to assessing statistical significance and imply p-values well below one percent for both the labor and health outcomes.

Our assessment of the benefits and costs of cutting PUI finds that the benefits of higher employment (valued at wages of the re-employed workers) are more than offset by increased work absences due to illness. In total, the added wages amount to only one-eighth the lost

benefits. The impact on the government budget is also negative because UI savings are more than offset by higher hospitalization costs. Our point estimate of the net benefit-cost, ignoring deaths, is negative $14.4bln. Taking the estimated 27,000-29,000 deaths into account would raise the magnitude of this negative net benefit at any conventional value of a statistical life. From a different perspective, if we use the policy decision to infer the VSL, as Ashenfelter (2006) does for changes in speed limits, then the implied value is negative. While our calculations of benefits and costs have large standard errors, even under the most extreme assumptions we do not find a positive VSL associated with the policy. Correspondingly, the decision in the control states to continue PUI saved both lives and treasure.

We should emphasize that the estimates in this paper are ex-post, involving "Monday-morning quarterbacking" of the decisions made by policymakers in early summer 2021. Those choosing to end PUI early probably had little idea that the Delta wave was gathering momentum, although there were voices warning of continued danger that went unheeded. The populations of treated states were probably more vulnerable, based on incomes, education, and vaccination rates. Even for those vaccinated, the available vaccine at the time had been developed based on earlier variants and most individuals had received it several months earlier in spring 2021, so its effectiveness was waning. With the advantage of 20-20 hindsight, it seems clear that ending PUI early was unfortunate in the choice of both time and place. Policymakers may not deserve blame for failure to anticipate the outcomes, but the results in this paper suggest that optimal UI policy design during the most contagious phases of deadly pandemics should take into account not only the tradeoff between work disincentives and social insurance, but also health externalities. Understanding how UI policies worked in the time of covid can provide important lessons for the preparation of policy responses to the next pandemic.

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Appendix A

The Data Section in the main text provides an overview of our data sources, variable construction, and summary statistics. Here we provide further details, particularly on variable construction.

Our basic data source for analyzing individual behavior is the Current Population Survey (CPS). We analyze re-employment behavior of the experienced unemployed, those who previously worked, using the longitudinally linked version of the CPS, based on its rotation panel design (Feng, 2001; Rivera Drew, Flood and Warren, 2014). In each month, the CPS includes a total of eight cohorts of household samples. A selected household is interviewed in the first four months, left out for eight months, and then interviewed again for the subsequent four months. This pattern allows us to link individuals in six cohorts to information from the following month. Each cohort is a random sample, so excluding two cohorts introduces no bias. Restricting the sample to individuals who are unemployed in a previous month, we observe whether they have become re-employed in the following month. Thus, the data structure consists of repeated, overlapping two-month panels. The main period for the re-employment analysis is February through August 2021, but we also consider other periods for robustness and to investigate behavior after the policy differences end. Individuals may appear multiple times (once for each month they are both unemployed and observed in the sample in the next month), but following Chetty (2008), we restrict attention in most of the analysis to those unemployed up to a year.

The first terminations of PUI, by three states, took place on June 12, 2021. Seven terminated on June 19, eight on June 26, and one on July 3. We consider these 19 states as treated. Because the reference period for CPS questions on labor force status is the week containing the 12th day of the month, and June 12, 2021, fell on a Saturday, the CPS questions for June pertain to the workweek before any states had terminated PUI, while the July CPS pertains to a reference week after all the 19 had terminated.[37] Control states are defined as ending PUI by federal law on September 4, 2021.[38] Thus, by the reference week for the September CPS, all states had terminated PUI.[39] Table A1 contains a complete list

[37]The treated states are Alabama, Arkansas, Georgia, Idaho, Iowa, Mississippi, Missouri, Montana, Nebraska, New Hampshire, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, West Virginia, and Wyoming. Appendix Table A1 contains a list of all states with the date of FPUC and PUA termination and the classification by treated, control, and excluded from our sample.

[38]The control states are California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Indiana, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New Mexico, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, and Wisconsin.

[39]We exclude five states that terminated programs at different times. Alaska, Arizona, Florida, and Ohio ended FPUC early, but continued PUA until September 2021. Louisiana ended the programs on July 31,

of all states, the dates on which they terminated FPUC and PUA, and their designation as treated or control for the purposes of this paper, as well as those that are excluded from our sample. (Figure 1 in the text contains a map of the states by treated, control, and excluded category.) The pre-period is specified as the period before the early ending of PUI, ending in June 2021 and starting in February 2021 for most of our estimations, although we consider alternative pre-periods as well. The post-period is July and August 2021.

We define $PUIend_{st}$ as an indicator for states s in months t that equals 1 in July-August 2021 for states ending PUI early, and 0 otherwise. UE is an indicator equal to 1 for re-employment, a transition from unemployment in month *t-1* to employment in month t, and 0 otherwise (including remaining unemployed and exiting from the labor force). The hazard rate h is the conditional probability of re-employment in t given survival to a given duration in *t-1*. Dur is months in the unemployment spell, which is right-censored for incomplete spells.

Control variables include standard demographic characteristics: age, sex, ethnicity and race, education, marital status, child under 12 years old, veteran status, and metropolitan statistical area (MSA). In some specifications, we control for industry and occupation of the unemployed person's last job, but for most specifications we instead use this information to construct a predicted wage, conserving degrees of freedom. The procedure uses data from the CPS Outgoing Rotation Groups (ORGs) in 2015 to 2019 on weekly earnings (reported directly or computed as hourly wage times usual weekly hours). For a sample of full-time employed workers who were unemployed aged between 20 and 64 in the previous month, we estimate the following regression equation:

$$\log w_{ist} = X_{ist} \cdot \alpha_1 + IND_{ist} \cdot \alpha_2 + OCC_{ist} \cdot \alpha_3 + \mu_s + \nu_{ist} \tag{A.1}$$

where $\log w_{ist}$ is the natural log of weekly wages for individual i in state s and month t, X_{ist} is the set of demographic characteristics described above, IND_{ist} and OCC_{ist} are industry and occupation fixed effects, the α are vectors of coefficients, and μ_s are state fixed effects. To predict the log of weekly wages for the currently unemployed sample from February to August 2021, $\widehat{\log w_{ist}}$, we use the estimated coefficients of Equation (A.1), substituting in the demographic characteristics of the unemployed, their state, and their industry and occupation on their last job. The predicted wage thus incorporates previous industry and occupation information.

We also use the predicted wage to construct measures of the unemployment insurance benefit (UIB) and the lost replacement rate (LRR) resulting from the termination of PUI.

2021, and is also excluded from the sample.

Table A1: PUI Termination Dates and Treatment Designation by State

State	FPUC	PEUC	PUA	Treated	Control	Excluded
Alabama	06/19/21	06/19/21	06/19/21	✓		
Alaska	06/19/21					✓
Arizona	07/10/21					✓
Arkansas	06/26/21	06/26/21	06/26/21	✓		
California					✓	
Colorado					✓	
Connecticut					✓	
Delaware					✓	
District of Columbia					✓	
Florida	06/26/21					✓
Georgia	06/26/21	06/26/21	06/26/21	✓		
Hawaii					✓	
Idaho	06/19/21	06/19/21	06/19/21	✓		
Illinois					✓	
Indiana					✓	
Iowa	06/12/21	06/12/21	06/12/21	✓		
Kansas					✓	
Kentucky					✓	
Louisiana	07/31/21	07/31/21	07/31/21			✓
Maine					✓	
Maryland					✓	
Massachusetts					✓	
Michigan					✓	
Minnesota					✓	
Mississippi	06/12/21	06/12/21	06/12/21	✓		
Missouri	06/12/21	06/12/21	06/12/21	✓		
Montana	06/26/21	06/26/21	06/26/21	✓		
Nebraska	06/19/21	06/19/21	06/19/21	✓		
Nevada					✓	
New Hampshire	06/19/21	06/19/21	06/19/21	✓		
New Jersey					✓	
New Mexico					✓	
New York					✓	
North Carolina					✓	
North Dakota	06/19/21	06/19/21	06/19/21	✓		
Ohio	06/26/21					✓
Oklahoma	06/26/21	06/26/21	06/26/21	✓		
Oregon					✓	
Pennsylvania					✓	
Rhode Island					✓	
South Carolina	06/26/21	06/26/21	06/26/21	✓		
South Dakota	06/26/21	06/26/21	06/26/21	✓		
Tennessee	07/03/21	07/03/21	07/03/21	✓		
Texas	06/26/21	06/26/21	06/26/21	✓		
Utah	06/26/21	06/26/21	06/26/21	✓		
Vermont					✓	
Virginia					✓	
Washington					✓	
West Virginia	06/19/21	06/19/21	06/19/21	✓		
Wisconsin					✓	
Wyoming	06/19/21	06/19/21	06/19/21	✓		

Source for program early termination dates: State government documents and websites. All other termination dates are September 4, 2021.

For this purpose, we need the predicted unlogged wage rather than the logged form coming from Equation (A.1), requiring a smearing estimator (Duan, 1983):

$$\hat{w}_{ist} = exp(\widehat{\log w_{ist}}) \cdot \frac{1}{n} \cdot \sum_{i=1}^{n} exp(\hat{\nu}_{ist})^2 \tag{A.2}$$

where $\hat{w}_{ist}$ is the predicted wage, n is the sample size, and $\hat{\nu}_{ist}$ are the residuals from estimating Equation (A.1). Using these results and with the assumption that the state unemployment insurance replacement rate is 0.5, we compute the UIB for each individual using the expected wages from Equation (A.2) as follows:

$$UIB_{ist} = \begin{cases} 0.5 \cdot \hat{w}_{ist} + 300 & \text{if} \quad PUIend_{st} = 0 \\ 0.5 \cdot \hat{w}_{ist} & \text{if} \quad PUIend_{st} = 1 \end{cases} \tag{A.3}$$

and we compute the LRR by dividing \$300 by $\hat{w}_{ist}$ for observations for whom $PUIend_{st} = 1$.[40]

In analyzing re-employment behavior, we control for not only individual characteristics but also some time-varying state-level characteristics: job openings, state UI program performance, covid vaccinations, and seasonality. The job openings rate, from the Job Openings and Labor Turnover Survey (JOLTS), represents state-month variation in labor demand (or offer arrival rate for unemployed workers. It is calculated as the ratio of job openings to total employment plus job openings in the nonfarm sector (BLS, 2022). UI program performance may affect UE transitions if payment delays lead discouraged unemployed to return to work earlier than they might (Navarrete, 2023). For this purpose, we draw upon data from the Department of Labor Employment and Training Administration (DOLETA), a Benefits Timeliness and Quality (BTQ) standard, the "continued weeks compensated time lapse" (Employment and Training Administration, 2017). UI timeliness is defined as a proportion of payments made within 3 weeks of receiving the claim, based on the performance standards of payment within 14-21 days.(Employment and Training Administration, 2022). Finally, vaccination rates may matter for unemployed individuals assessing the safety of returning to work and for the extent of subsequent covid spread. The covid vaccination rate, from the Centers for Disease Control and Prevention (CDC) data tracker, is defined as the percentage

[40]The replacement rate is 0.5 for most individuals in most states. A further analysis could take into account differences across states in the minimum and maximum levels of the state benefits, but while we account for individual differences in predicted wages, the main source of variation in our analysis is the \$300 additional benefit from FPUC. Note that, even with information on former wages and UI benefits from the Survey of Income and Program Participation (SIPP), Chetty (2008) prefers to construct predicted wages, with a procedure similar to ours, and to use state-level average or maximum benefits rather than the survey data on the amounts received.

of the total population with at least one dose of a covid vaccine (CDC, 2023).

In some specifications, we use an extended time period from July 2019 to August 2021 to include a non-pandemic period. Table A2 shows the descriptive statistics by treated and control states over the extended period.

Table A2: Descriptive Statistics, July 2019-August 2021

Variable	All states	Treated	Control
Unemployment to Employment Transition (UE)	0.27	0.29	0.27
Unemployment duration (Dur, months, right-censored)	4.45	4.22	4.54
	(2.68)	(2.61)	(2.70)
Experienced unemployed by reason:			
Job loser	0.73	0.68	0.75
Job leaver	0.08	0.10	0.07
Re-entrant	0.19	0.21	0.18
Predicted weekly wage ($\hat{w}$, \$)	1236.94	1114.42	1279.26
	(570.24)	(502.28)	(585.99)
Age (years)	38.89	38.38	39.06
	(13.08)	(12.91)	(13.13)
Female	0.48	0.48	0.47
Race/Ethnicity			
Hispanic	0.23	0.22	0.23
White	0.50	0.49	0.50
Black	0.17	0.23	0.15
Asian	0.07	0.02	0.08
Academic Attainment			
< High school	0.11	0.12	0.10
High school	0.32	0.35	0.31
Some college	0.30	0.31	0.30
University	0.20	0.17	0.21
> University	0.07	0.05	0.08
Marital Status			
Never married	0.47	0.45	0.48
Married	0.39	0.38	0.39
Divorced/separated	0.13	0.15	0.12
Widowed	0.01	0.02	0.01
Eldest child age <12	0.15	0.16	0.15
Veteran	0.04	0.05	0.03
Metropolitan area	0.87	0.75	0.90
Predicted unemployment benefit (UIB, \$)	910.24	836.57	936.13
	(283.77)	(254.20)	(289.03)
State-level covariates			
Job opening rate (%)	4.66	4.78	4.62
	(1.07)	(1.11)	(1.06)
UI payment timeliness	0.89	0.91	0.88
	(0.10)	(0.06)	(0.10)
Observations	38124	12598	25526

Note: Calculations based on CPS data from July 2019 to August 2021. The sample includes all experienced unemployed aged 20-64, except for UIB, LRR, and Dur, for which the sample is further restricted to individuals with unemployment duration up to a year (with sample sizes of 33230, 11149, and 22081 for all, treated, and control states, respectively). For continuous variables, standard deviations are shown in parentheses; all other variables are binary dummies. Observations are weighted by the CPS final individual weights.

We control for nationwide seasonal fluctuations in job-finding through time effects and for seasonality differences across states by estimating the following specification for each state

separately, using a sample of experienced unemployed individuals from 2015 to 2019:

$$UE_{ismt} = Month_m \cdot \delta_{sm} + \tau_{t \in T} + \epsilon_{ismt} \tag{A.4}$$

where UE_{ismt} is again an indicator for transition from unemployment at time *t-1* to employment at t for individual i in state s, and calendar month m. $Month_m$ is a vector of indicators for the twelve calendar months from January to December, and $\tau_{t \in T}$ is year fixed effects. δ_{sm} captures the seasonality in re-employment rates by state for each calendar month, which we control for in some specifications.

In robustness checks shown in Tables 12 and 13, we also add several other types of variables. School year start date, which might affect incentives for re-employment, is defined as a dummy variable equal to one in August if the average (weighted by student population) of the school district start dates (from Pew Research Center (2023) for a state is before August 15. In order to control for changes in covid outcomes caused by different characteristics of states, we interact the treatment period with the following variables measured pre-treatment: obesity rates in 2020 from BRFSS (2020), heart disease rate (major cardiovascular or coronary) in 2019 from BRFSS (2019), covid case rate in May 2021, share of population aged 65 or more in 2020, median earnings in 2020, leisure and hospitality industry employment ratio in the second quarter of 2021 relative to the second quarter of 2019, and mobility for recreation from Google (2022). Obesity rate is the share of the population with a BMI over 30. Heart disease rate is a sum of major cardiovascular and coronary heart disease. Mobility for recreation is the percentage change in the visits and length of stay at places for retail and recreation compared to the first five weeks of 2020. Summary statistics for these variables, by treated and control states, are provided in Appendix Table A3.

Finally, we draw upon a large database of covid-related state-level policies that could affect covid outcomes during summer 2021 (Raifman et al., 2022). Among 149 policies, we identify 31 that changed that summer, and in robustness checks we control for all types that could have exacerbated covid outcomes in treated relative to control states. The policies, which include provisions related to housing (largely evictions), prison visitation, masking, quarantines, re-opening, and state of emergency, are listed in Appendix Table A4.

A final aspect of the data construction concerns the estimation of state-month level UE rates used in the covid analysis. For this purpose, we use estimated coefficients of state-by-month fixed effects from the model:

$$UE_{ist} = \delta_{st} + X_{ist-1} \cdot \gamma + u_{st} \tag{A.5}$$

where UE_{ist} is the transition from unemployment$_{t-1}$ to employment$_t$ for individual i in

Table A3: Descriptive Statistics for Additional Controls in Covid Analysis

Variable	All states	Treated	Control
School openings (August, 2021)	0.28	0.68	0.09
	(0.45)	(0.48)	(0.29)
Obesity rate (%, 2020)	31.93	35.28	30.40
	(3.76)	(2.35)	(2.35)
Heart disease rate (%, 2019)	9.97	11.88	9.10
	(3.27)	(2.00)	(3.37)
Covid case rate (%, May 2021)	0.26	0.23	0.27
	(0.10)	(0.57)	(0.11)
Age≥65 (%, May 2021)	16.81	15.92	17.21
	(1.96)	(2.29)	(1.64)
Median earnings (2020)	37109.63	33828.04	38613.90
	(3995.97)	(1780.36)	(3826.93)
Hospitality industry ratio (2021Q2/2019Q2)	0.77	0.85	0.74
	(0.13)	(0.14)	(0.12)
Change in recreation mobility (May2021/Jan2020)	-5.49	0.17	-8.09
	(7.42)	(5.82)	(6.59)

Note: Calculations based on monthly state-level data, weighted by population, for the reference period in parentheses. Standard deviations are shown in parentheses. Covid-related state-level policy variables are listed in Appendix Table A4.

state s, and δ_{st} represents state-by-month effects. We include controls for the individual characteristics denoted as X_{ist}, as above. Using this approach, we are able to obtain the UE transition for each state on a monthly level while controlling for individual characteristics. For convenience, we denote $\hat{\delta}_{st}$ as UE_{st} in the covid analysis below.

Table A4: Covid-Related Policy Changes during June-August 2021

Category	Policy	Number of States Changing			Expected	Treated
		Treated	Control	Total	Covid Effect	- Control
Housing	First overall eviction moratorium end	0	6	6	+	-
	Second overall eviction moratorium end	0	1	1	+	-
	First eviction initiation ban end	0	6	6	+	-
	Second Eviction Initiation Ban End	0	1	1	+	-
	First eviction hearing ban end	0	2	2	+	-
	First eviction enforcement ban end	0	5	5	+	-
	COVID-19 hardship limitation end	0	3	3	+	-
	Non-payment limitation start	0	1	1	+	-
	Non-payment limitation end	0	2	2	+	-
	CDC moratorium end	3	8	11	+	-
	Late Fee Ban End	0	3	3	+	-
	Utilities shutoff moratorium expiration	0	5	5	+	-
	Utilities reconnection start	0	1	1	-	+
	Utilities reconnection end	0	2	2	+	-
Incarceration	Resumed visitation in state prisons	5	3	8	+	+
	Resumed visitation in state prisons	1	1	2	+	=
Face mask mandate	In public spaces reimposed	0	2	2	-	+
	In public spaces end for fully vaccinated	0	2	2	+	-
	In public spaces resumed for fully vaccinated	0	3	3	-	+
	In public spaces resumed with exemptions	0	1	1	-	+
	In schools for 2021-22 school year	0	14	14	-	+
	Ended in public spaces	1	6	7	+	-
	Ended the second mandate in public spaces	0	1	1	+	-
	Banned school face mask mandates	3	0	3	+	+
	Banned other local face mask mandates	1	0	1	+	+
	Judicial decision blocked state from enforcing bans	2	0	2	-	-
Quarantines	Date all mandated quarantines ended	0	2	2	+	-
Reopening	Re-open bars	0	2	2	+	-
	Allowed businesses to reopen overnight	0	1	1	+	-
State of emergency	State of emergency lifted	4	12	16	+	-
	State of emergency reinstated	2	0	2	-	-

Note: The table lists 31 covid-related state-level policies that changed from June to August 2021, excluding unemployment insurance policies, from the 149 policies described in Raifman et al. (2022).

Appendix B

Table B1: Estimated Effects of UI Loss on Re-Employment, Alternative Time Periods

	Feb-Aug 2021		Jul 2019-Aug 2021		Apr 2020-Aug 2021	
	(1)	(2)	(3)	(4)	(5)	(6)
$PUIend$	0.106	0.098	0.108	0.105	0.106	0.109
	(0.022)	(0.022)	(0.020)	(0.020)	(0.021)	(0.020)
Job opening rate		0.033		0.015		0.013
		(0.013)		(0.005)		(0.006)
Timeliness		0.136		-0.009		0.007
		(0.089)		(0.037)		(0.039)
Vaccination		-0.122		-0.009		0.041
		(0.171)		(0.088)		(0.093)
Seasonality		0.163		0.344		0.155
		(0.147)		(0.078)		(0.077)
$\ln(\hat{w})$	0.037	0.036	0.021	0.021	0.027	0.026
	(0.017)	(0.017)	(0.013)	(0.013)	(0.015)	(0.015)
State-level covariates	No	Yes	No	Yes	No	Yes

Note: Each column shows estimated coefficients from a separate linear probability model, Equation (1), for the unemployment-to-employment transition by experienced unemployed (which excludes new entrants to the labor force). Columns (1)-(2) are the same as columns (3)-(4) in Table 4. Sample periods are defined over months t, when the dependent variable is measured; all other variables are measured in month $t-1$. $PUIend$ is an indicator for states ending PUI by early July 2021 during July-August 2021. All specifications control for predicted wage, state and month fixed effects and individual characteristics including age, age squared, sex, ethnicity and race, education, marital status, veteran status, and MSA. Models (2), (4), and (6) condition on state-level job opening rate, UI timeliness, vaccination, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 10,229 for columns (1)-(2), 38,124 for (3)-(4), and 29,570 for (5)-(6).

Table B2: Estimated Effects of UI Loss on the Transition into Employment: Job Losers

	Feb-Aug 2021		Jul 2019-Aug 2021		Apr 2020-Aug 2021	
	(1)	(2)	(3)	(4)	(5)	(6)
PUIend	0.142	0.133	0.127	0.142	0.127	0.143
	(0.055)	(0.044)	(0.045)	(0.046)	(0.046)	(0.044)
Job opening rate		0.057		0.016		0.010
		(0.030)		(0.009)		(0.009)
Timeliness		0.307		0.000		0.002
		(0.269)		(0.050)		(0.057)
Vaccination		-0.120		0.167		0.171
		(0.329)		(0.159)		(0.157)
Seasonality		0.473		0.364		0.163
		(0.275)		(0.106)		(0.101)
$\ln(\hat{w})$	-0.033	-0.033	0.016	0.016	0.015	0.014
	(0.033)	(0.033)	(0.020)	(0.020)	(0.022)	(0.022)
State-level covariates	No	Yes	No	Yes	No	Yes

Note: Each column shows estimated coefficients from a separate linear probability model, Equation (1), for the unemployment-to-employment transition by job loser with duration up to 26 weeks. Sample period is defined over months t, when the dependent variable is measured; all other variables are measured in month $t-1$. *PUIend* is an indicator for states ending PUI by early July 2021 during July-August 2021. All specifications control for predicted wage, state and month fixed effects and individual characteristics including age, age squared, sex, ethnicity and race, education, marital status, veteran status, and MSA. Models (2), (4), and (6) condition on state-level job opening rate, UI timeliness, vaccination, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 3,959 for columns (1)-(2), 21,382 for (3)-(4), and 17,517 for (5)-(6).

Table B3: Estimated Effect of UI Loss on Unemployment Hazard and Duration: Job Losers

Model Dep. Var.	Cox proportional hazard ln(Hazard)		Censored normal ln(Duration)	
	(1)	(2)	(3)	(4)
PUIend	0.327	0.310	-0.153	-0.139
	(0.163)	(0.132)	(0.091)	(0.070)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from Equation (3) in columns (1-2) and Equation (4) in columns (3-4). All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2) and (4) condition on state-level job opening rate, UI timeliness, vaccination, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 3,959 job losers with a duration up to 26 weeks in two consecutive months from February to August 2021.

Table B4: Estimated Replacement Rate Elasticity of Unemployment Hazard and Duration

Model	OLS		Cox		Censored normal	
Dep. Var.	UE		ln(Hazard)		ln(Duration)	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Experienced Unemployed, Dur≤12m						
LRR x $PUIend$	0.252	0.215	0.733	0.685	-0.459	-0.425
	(0.082)	(0.076)	(0.318)	(0.287)	(0.255)	(0.225)
B. Job loser, Dur≤26w						
LRR x $PUIend$	0.339	0.292	0.798	0.686	-0.354	-0.275
	(0149)	(0.117)	(0.434)	(0.350)	(0.250)	(0.185)
State-level covariates	No	Yes	No	Yes	No	Yes

Note: The table shows estimated coefficients from Equation (1)in columns (1-2), Equation (3) in (3-4), and Equation (4) in (5-6). All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2), (4), and (6) condition on state-level job openings, UI timeliness, vaccination, and seasonality. Observations are weighted by CPS weights. Standard errors clustered on state are shown in parentheses. Observations = 7,561 in panel A and 3,959 in panel B from February to August 2021.

Table B5: Estimated Effects of UI Benefit on UE Transition, Hazard, and Duration

	First stage	Second stage		
	(1)	(2)	(3)	(4)
Dep. Var.	ln(UIB)	UE	ln(Hazard)	ln(Duration)
A. Experienced Unemployed, Dur≤12m				
$PUIend$	-0.463			
	(0.008)			
$\widehat{\ln(UIB)}$		-0.227	-0.676	0.499
		(0.060)	(0.241)	(0.184)
			[0.241]	[0.175]
B. Job loser, Dur≤26w				
$PUIend$	-0.461			
	(0.013)			
$\widehat{\ln(UIB)}$		-0.287	-0.666	0.301
		(0.095)	(0.290)	(0.152)
			[0.305]	[0.167]
Model		2SLS	IV Cox	IV Cens. Normal

Note: The table shows estimated coefficients from the first stage (1), and second stage with OLS (2), Cox proportional hazard model (3), and censored normal regression model (4). Models (3) and (4) incorporate right censoring for the observations with UE transition equal to 1. All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects as well as state-level covariates including job opening rate, timeliness, vaccination, and seasonality. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses. Bootstrap standard errors are reported in square brackets. f-statistics from the first stage is 1591.73 for panel A and 825.73 for panel B. Observations = 7,561 in panel A and 3,959 in panel B from February to August 2021.

Table B6: Estimated Effect of UI Benefit on Unemployment Hazard, Alternative Samples

	Experienced Unemployed				Job loser					
	All		$Dur \leq$ 12m		All		$Dur \leq$ 12m		$Dur \leq$ 26w	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.	First	Second	First	Second	First	Second	First	Second	First	Second
$PUIend$	-0.466		-0.463		-0.451		-0.456		-0.461	
	(0.009)		(0.008)		(0.090)		(0.012)		(0.0127)	
$\widehat{\ln(UIB)}$		-0.939		-0.676		-0.936		-0.607		-0.666
		(0.209)		(0.241)		(0.272)		(0.273)		(0.290)
		[0.225]		[0.241]		[0.295]		[0.289]		[0.305]
Nobs	10229		7561		7169		5303		3959	

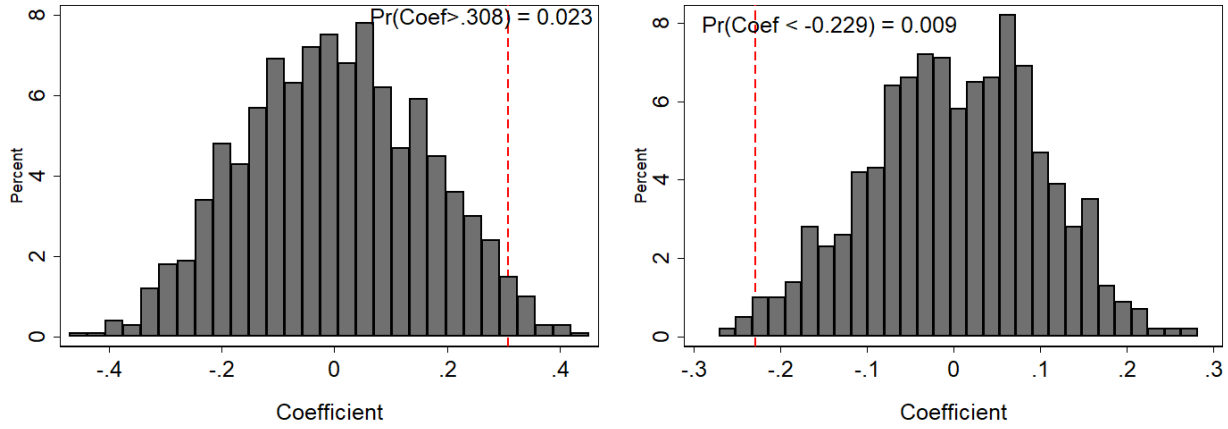
Note: The table shows the IV-Cox coefficient of samples from February to August 2021 of all experienced unemployed (1)-(2), experienced unemployed with duration up to a year (3)-(4), all job losers (5)-(6), job losers with duration up to a year (7)-(8), job losers with duration up to 26 weeks. First stage estimates are reported in (1), (3), (5), (7), and (9). IV-Cox estimates are reported in (2), (4), (6), (8), and (10). All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects as well as state-level covariates including job opening rate, timeliness, vaccination, and seasonality. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses. Bootstrap standard errors are reported in square brackets.

Table B7: Estimated Effect of UI Benefit on Unemployment Duration, Alternative Samples

	Experienced Unemployed		Job loser		
	All	$Dur \leq$ 12m	All	$Dur \leq$ 12m	$Dur \leq$ 26w
	(1)	(2)	(3)	(4)	(5)
$\widehat{\ln(UIB)}$	0.933	0.499	0.971	0.540	0.301
	(0.231)	(0.184)	(0.258)	(0.204)	(0.152)
	[0.213]	[0.175]	[0.267]	[0.216]	[0.167]
Nobs	10229	7561	7169	5303	3959

Note: The table shows the IV-Censored normal regression coefficient of samples from February to August 2021 of all experienced unemployed (1), experienced unemployed with duration up to a year (2), all job losers (3), job losers with duration up to a year (4), and job losers with duration up to 26 weeks (5). First stage estimates are reported Table B6 in columns (1), (3), (5), (7), and (9). All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects as well as state-level covariates including job opening rate, timeliness, vaccination, and seasonality. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses. Bootstrap standard errors are reported in square brackets.

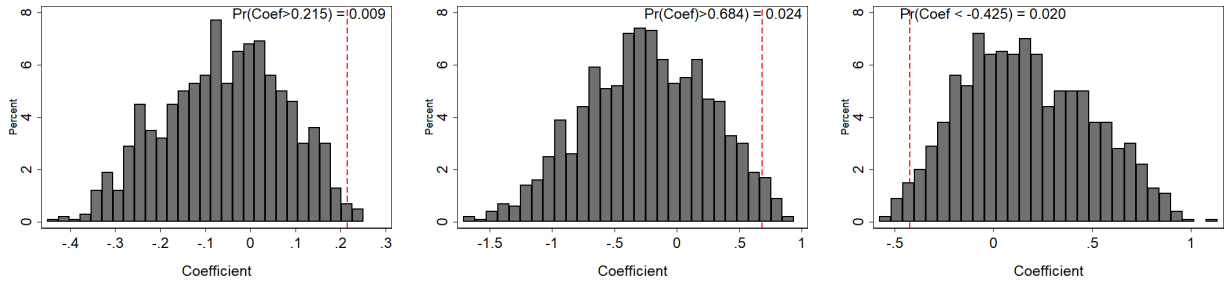
Figure B1: Permutation Test for Effect of $PUIend$ on Unemployment Hazard and Duration



(a) Cox Proportional Hazard Model (b) Censored Normal Regression

Note: The graph shows the distribution of coefficients from 1,000 randomizations of $PUIend$ treatment across states for the hazard and duration of experienced unemployed from February to August 2021. Specifications control for demographic and human capital characteristics, predicted wage, and state and month fixed effects, as well as state-level covariates including job opening rate, timeliness, vaccination, and seasonality as in Table 6, models (2) and (4). The observations are weighted by CPS weights. Standard errors are clustered on state. Observations include all experienced unemployed with unemployment duration up to one year. The dashed vertical line shows the coefficient from Table 6 models (2) and (4).

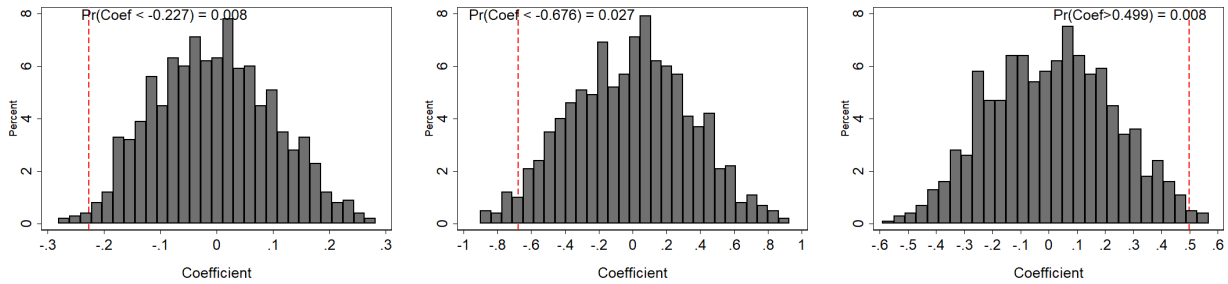
Figure B2: Permutation Test of Lost Replacement Rate on UE, Hazard, and Duration



(a) LPM (b) Cox Hazard Model (c) Censored Normal

Note: The graph shows the distribution of coefficients from 1,000 randomizations of LRR treatment across states for the hazard and duration for experienced unemployed from February to August 2021. Specifications control for demographic and human capital characteristics, predicted wage, and state and month fixed effects, as well as state-level covariates including job opening rate, timeliness, vaccination, and seasonality as in Table 7 model (2), (4), and (6). The observations are weighted by CPS weights. Standard errors are clustered on state. Observations are the experienced unemployed with a duration up to one year. The dashed vertical line indicates the coefficient from Table 7 models (2), (4), and (6).

Figure B3: Permutation Test of UI Benefit on UE, Hazard, and Duration



(a) LPM (b) Cox Hazard Model (c) Censored Normal

Note: The graphs show the distributions of coefficients from 1,000 randomizations of treatment across states of calculating UIB for the UE transition, hazard, and duration for experienced unemployed. Specifications control for demographic and human capital characteristics, predicted wage, and state and month fixed effects, as well as state-level job opening rate, UI timeliness, vaccination, and seasonality as in Table 8. Sample = experienced unemployed with duration up to a year. Results are weighted by CPS weights. The dashed vertical lines indicate the coefficients from Table 8 models (2), (3), and (4).

Table B8: Estimated Effects of UI Loss on Covid Outcomes: First Stage

	UE_t	UE_{t-1}	UE_{t-2}	UE_{t-3}	UE_t	UE_{t-1}	UE_{t-2}	UE_{t-3}
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$PUIend_t$	0.099	0.016	0.029	-0.026	0.093	0.015	0.033	-0.025
	(0.017)	(0.011)	(0.020)	(0.023)	(0.018)	(0.019)	(0.028)	(0.023)
$PUIend_{t-1}$	0.037	0.126	-0.022	0.027	0.035	0.127	-0.023	0.025
	(0.016)	(0.023)	(0.018)	(0.038)	(0.015)	(0.019)	(0.017)	(0.040)
$PUIend_{t-2}$	-0.013	-0.008	0.142	-0.023	-0.014	-0.014	0.151	-0.018
	(0.026)	(0.016)	(0.028)	(0.025)	(0.026)	(0.016)	(0.032)	(0.025)
$PUIend_{t-3}$	0.020	0.012	0.021	0.137	0.017	0.002	0.020	0.137
	(0.017)	(0.018)	(0.016)	(0.022)	(0.017)	(0.026)	(0.018)	(0.023)
SW f-statistics	44.65	28.09	44.45	67.52	23.23	28.70	32.93	60.47
Controls	No	No	No	No	Yes	Yes	Yes	Yes

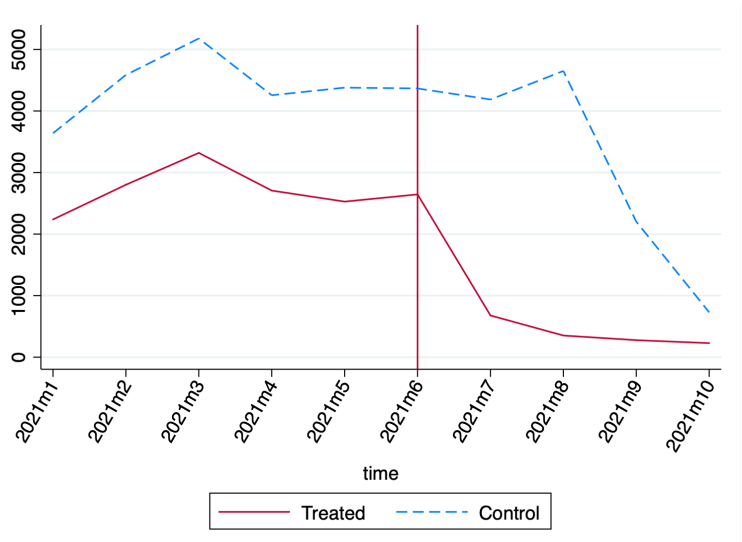
Note: The table shows estimated first stage coefficients in Equation (7). All specifications control for state and month fixed effects. Controls include cumulative vaccination, heating degree-days, and cooling degree-days at t 1 in columns (5-8). Observations are weighted by population and inversely by standard errors from Equation (A.5). Standard errors clustered on state are shown in parentheses. SW f-statistics (Sanderson and Windmeijer, 2016) are clustered on state. Observations = 506 from January to November 2021.

Table B9: Estimated Effects of UE Transition on Covid Outcomes: OLS Estimates

	Cases		Hospitalizations		Deaths	
	(1)	(2)	(3)	(4)	(5)	(6)
$\widehat{UE}_t$	0.594	0.399	0.064	0.051	0.0145	0.0100
	(0.319)	(0.301)	(0.028)	(0.028)	(0.0069)	(0.0047)
$\widehat{UE}_{t-1}$	0.061	0.080	0.058	0.061	0.0232	0.0154
	(0.300)	(0.450)	(0.034)	(0.046)	(0.0095)	(0.0063)
$\widehat{UE}_{t-2}$	0.033	-0.106	0.015	0.006	0.0239	0.0173
	(0.209)	(0.294)	(0.019)	(0.026)	(0.0115)	(0.0093)
$\widehat{UE}_{t-3}$	-0.279	-0.366	-0.366	-0.028	0.0145	0.0095
	(0.294)	(0.240)	(0.019)	(0.021)	(0.0075)	(0.0056)
Controls	No	Yes	No	Yes	No	Yes

Note: The table shows ordinary least squares (OLS) coefficients of UE and its lags on covid outcomes. All specifications control for state and month fixed effects. Controls include cumulative vaccination, heating degree-days, and cooling degree-days at t 1 for columns (2), (4), and (6). The observations are weighted by population and inversely by standard errors from Equation (A.5). Standard errors clustered on state are shown in parentheses. Observations = 506 from January to November 2021.

Figure B4: Average Ratio of UI Spending per Unemployed Person



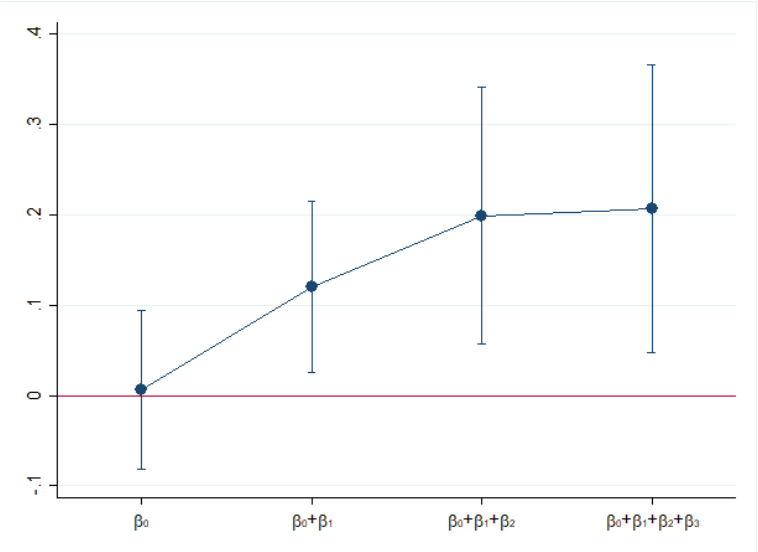
Note: Total UI spending includes state benefits paid and PUI disbursement to states. The values represent the average of treated and control states over months of total UI spending over the number of unemployed. The observations are weighted by population.

Table B10: Lost Working Hours Because of Illness, 2021

	Treated	Control
Feb-Jun	0.41	0.45
	(3.74)	(3.96)
Jul-Aug	0.46	0.42
	(4.08)	(3.76)
Sep-Oct	0.57	0.45
	(4.55)	(3.91)
Nov-Dec	0.53	0.54
	(4.21)	(4.26)
Observations	173,899	257,925

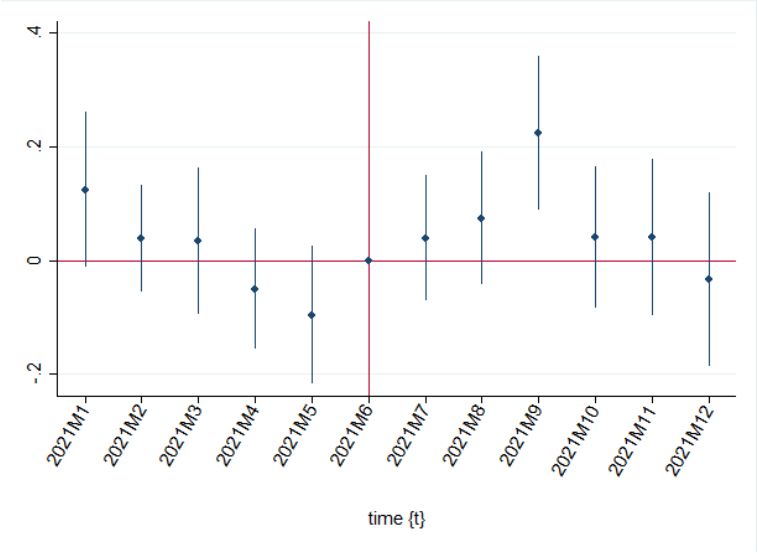
Note: The sample includes all employed civilians aged 15 or above. Lost working hours are measured as usual working hours minus actual working hours (per week) of workers who are absent or part-time due to illness or medical limitations, 0 otherwise. Standard deviations are shown in parentheses. Observations are weighted by the CPS final individual weights.

Figure B5: Cumulative Effects of UI Loss on Lost Working Hours



Note: The graph shows the cumulative effects of UI loss on lost working hours from February to November 2021. Specification controls for demographic and human capital characteristics, and state and month fixed effects in as Table 15. The observations are weighted by CPS final weights. Vertical lines are 95% confidence intervals, based on standard errors clustered on state.

Figure B6: Event Study of Impact of PUI Termination on Lost Working Hours



Note: The graph shows an event study of the treated-control state difference in lost working hours per week. Controls are the same as in Table 15. Vertical lines are 95% confidence interval, based on standard errors clustered on state.

Appendix C

Table C1: Mean UE Transitions by Population Sub-Groups

	Treated		Control		Observations
	Feb-Jun 2021	Jul-Aug 2021	Feb-Jun 2021	Jul-Aug 2021	Feb-Aug 2021
Single	0.250	0.354	0.220	0.213	6346
Married	0.254	0.361	0.244	0.253	3883
Age $\geq$ 50	0.220	0.298	0.214	0.226	2913
Age $<$ 50	0.261	0.375	0.234	0.227	7316
Pr(telew) $<$ medan	0.263	0.370	0.235	0.245	5104
Pr(telew) $\geq$ median	0.238	0.344	0.222	0.212	5125
Black	0.202	0.318	0.172	0.156	1497
White	0.251	0.370	0.239	0.261	5626
Female	0.230	0.370	0.219	0.228	4640
Male	0.268	0.345	0.236	0.226	5589

Table C2: Estimated Effects of UI Loss on the UE Transition by Marital Status

	Single		Married	
	(1)	(2)	(3)	(4)
$PUIend$	0.116	0.104	0.079	0.076
	(0.031)	(0.032)	(0.042)	(0.048)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from linear probability models for the UE transition from February to August 2021, separately by marital status. All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2) and (4) condition on state-level covariates including job opening rate, timeliness, vaccination, and seasonality. Observations = 6,346 for the single and 3,883 for the married. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses.

Table C3: Estimated Effects of UI Loss on the UE Transition by Age 50

	Age $\geq$ 50		Age $<$ 50	
	(1)	(2)	(3)	(4)
$PUIend$	0.061	0.068	0.118	0.104
	(0.044)	(0.050)	(0.022)	(0.025)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from linear probability models for the UE transition from February to August 2021, separately by age 50. All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2) and (4) condition on state-level covariates including job opening rate, timeliness, vaccination, and seasonality. Observations = 2,913 for the age $\geq$ 50 and 7,316 for age $<$ 50. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses.

Table C4: Estimated Effects of UI Loss on the UE Transition by Pr(teleworking)

	Pr(Telew) < Med.		Pr(Telew) ≥ Med.	
	(1)	(2)	(3)	(4)
$PUIend$	0.089	0.074	0.119	0.127
	(0.033)	(0.035)	(0.022)	(0.033)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from linear probability models for the UE transition from February to August 2021, separately by the probability of teleworking. All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2) and (4) condition on state-level covariates including job opening rate, timeliness, vaccination, and seasonality. Observations = 5,104 for the Pr(Telew) < median and 5,125 for Pr(Telew) ≥ median. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses.

Table C5: Estimated Effects of UI Loss on the UE Transition by Race

	Black		White	
	(1)	(2)	(3)	(4)
Feb - Aug 2021				
$PUIend$	0.145	0.133	0.102	0.108
	(0.059)	(0.068)	(0.035)	(0.040)
Jul 2019 - Aug 2021				
$PUIend$	0.150	0.153	0.092	0.094
	(0.049)	(0.050)	(0.033)	(0.035)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from linear probability models for the UE transition from February to August 2021, separately by race. All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2) and (4) condition on state-level covariates including job opening rate, timeliness, vaccination, and seasonality. Observations = 1,497 for the Black and 5,626 for the White population in the sample from February to August 2021; 5,210 for the Black and 21,937 for the White population in the sample from July 2019 to August 2021. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses.

Table C6: Estimated Effects of UI Loss on the UE Transition by Gender

	Female		Male	
	(1)	(2)	(3)	(4)
Feb - Aug 2021				
$PUIend$	0.134	0.127	0.076	0.074
	(0.033)	(0.040)	(0.037)	(0.038)
Jul 2019 - Aug 2021				
$PUIend$	0.113	0.118	0.098	0.088
	(0.031)	(0.035)	(0.029)	(0.033)
State-level covariates	No	Yes	No	Yes

Note: The table shows estimated coefficients from linear probability models for the UE transition from February to August 2021, separately by race. All specifications control for demographic and human capital characteristics, as in Table 4, predicted wage, and state and month fixed effects. Models (2) and (4) condition on state-level covariates including job opening rate, timeliness, vaccination, and seasonality. Observations = 4,640 females and 5,589 males in the sample from February to August 2021; 18,242 females and 19,882 males in the sample from July 2019 to August 2021. The observations are weighted by CPS weight, and standard errors clustered on state are shown in parentheses.