Employment Responses to the Withdrawal of Unemployment Benefits*

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Abstract

We study the short- and medium-run employment responses to the withdrawals of two programs that expanded the coverage and generosity of unemployment benefits in the U.S. from March to August 2021. That 18 states withdrew unemployment benefits earlier than other states offers a unique policy setting to investigate transitions out of unemployment by race/ethnicity, implications for the quality of job matches, and the persistence of employment effects. Difference-in-differences estimates using panel data from the U.S. Current Population Survey demonstrate that states' withdrawals of unemployment benefits increased transitions from unemployment to employment, but with large racial heterogeneity: Black and Asian individuals experienced increases in transitions from unemployment into inactivity as a result of the policy change, while White individuals exiting unemployment generally transitioned into employment. Regarding job quality, the benefit withdrawals increased the take-up of lower-pay routine and manual occupations. One year after the policy change, the positive employment effect of the early benefit withdrawal had disappeared, while the negative effects on job quality outcomes persisted.

Keywords: Unemployment benefits, labor markets, employment
JEL Codes: J45, J65, J68

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1 Introduction

In June 2021, 18 of the 50 U.S. states opted to withdraw early from two programs that the federal government introduced to increase the coverage and generosity of unemployment benefits (UBs). In September 2021, the two programs ended in all remaining states. The differential timing of the benefit withdrawals offers a unique setting to study four questions regarding employment responses to UB withdrawals: how do UB provisions affect employment on the extensive margin? How do employment responses to UB withdrawals vary by race/ethnicity? How do earlier withdrawals affect job quality matches among those transitioning to employment? And to what extent do employment and job quality effects persist a year after the policy change?

The specific policies we study are the Pandemic Unemployment Allowance (PUA), which extended UB eligibility to individuals who are typically not eligible for standard UBs (such as the self-employed, gig workers, workers with insufficient tenure at the time of job-loss and part-timers); and the Federal Pandemic Unemployment Compensation (FPUC), which consisted of a fixed weekly supplement to state-level UBs.[1]

We estimate difference-in-differences (DiD) models using monthly panel data from the Current Population Survey (CPS) to identify the employment consequences of states' early withdrawals from the policies. We document that early and late withdrawal states were on parallel employment trends before the policy change, and that the decision to halt PUA and FPUC earlier than planned derived mostly from partisan considerations.

First, we study the employment responses to the policy change along the extensive margin. This is an area where previous research has generally documented a negative labor supply response to UB generosity, also in the context of this policy (Arbogast and Dupor, 2022; Coombs et al., 2022; Holzer et al., 2021).[2] We find that the early expiration of the two programs increased transitions from unemployment to employment. The effect is large in magnitude, in line with the fact that the labor market was relatively tight at the time of the policy change and that the employment responses to UBs are larger when the economy

[1] This was initially set to \$600 from April to July 2020, but then re-implemented at a weekly rate of \$300 starting in March 2021.

[2] Dube (2021) focuses on the temporary expiration of the FPUC that took place in July 2020 and finds limited employment gains. Differences in the results compared to studies who looked at the expiration of both PUA and FPUC in June 2021 might be associated with differences in (i) the type of support being withdrawn, (ii) the stages of the recovery process, and (iii) the evolution of the pandemic.

is recovering (Schmieder and von Wachter, 2016).[3]

Second, we study heterogeneous responses by race/ethnicity. There are competing perspectives from the literature on how UB withdrawal should affect employment by race/ethnicity. On the one hand, racial/ethnic minorities tend to be more liquidity constrained (Ganong et al., 2020a) and therefore may be more responsive to the withdrawal of UBs. This would imply stronger effects of the UB withdrawals on transitions to employment for non-White adults. At the same time, these adults might have more limited access to UBs, may have worked in industries that are slower to recover as the economy improves, and/or might also face more discriminatory barriers to (re-)employment. This would imply weaker effects of the UB withdrawals on transitions to employment for non-White adults. In practice, we find that early benefit withdrawals disadvantaged racial/ethnic minorities. For White individuals, the decrease in the probability of remaining unemployed was matched by a parallel increase in unemployment-to-employment transitions. However, for Black (Asian) individuals, the decrease in the probability of staying in unemployment partially (fully) resulted into an increase in the likelihood of transitioning into inactivity.

Third, we examine the effects of the policy change on the quality of job matches. Even in this case, it is not clear *ex ante* what effect the policy change could have. More generous UBs might allow individuals more time to look for better jobs. However, employment prospects can also deteriorate with time spent in unemployment. For these reasons, studies on the effects of UB generosity on job outcomes have found mixed results (Card et al., 2006; Lalive, 2007; Nekoei and Weber, 2017; van Ours and Vodopivec, 2006).[4] We find that the early expiration of pandemic UBs has increased the probability of accepting routine and manual occupations compared to non-routine, non-manual occupations. Using data from the Outgoing Rotating Group (ORG) of the CPS matched with wage information from the Annual Social and Economic (ASEC) supplement, we also find suggestive evidence that this is associated with a small wage penalty at the middle of the income distribution.

Fourth, we examine medium-term treatment effects by looking at individuals' employment situation one year after the policy change. This is an area where very limited evidence exists, as most of the literature has analyzed the effects of UB generosity only until

[3] A different stream of research has examined the effects of the introduction of COVID-19 emergency measures in the US (including PUA and FPUC), generally finding large consumption gains and small disincentive effects on labor supply (Bachas et al., 2020; Boar and Mongey, 2020; Garza Casado et al., 2020; Farrell et al., 2020; Ganong et al., 2022; Marinescu et al., 2021; Petrosky-Nadeau, 2020).

[4] Additionally, benefit generosity might also have an impact on employment quantiy and job quality at the macro-economic level, by affecting individuals' consumption and in this way also firms' labor demand.

the time of re-employment (Schmieder and von Wachter, 2016). The few available studies report that short-run reductions in labor supply are at least partially compensated in the medium- to long-run, potentially due to better job matches upon initial re-employment (Schmieder et al., 2012; Scrutinio, 2020). We benefit from a unique setting to study this question, given that late withdrawal states terminated pandemic UBs just a few months later (i.e. in September 2021). Any long-term effect might therefore materialize only if the early withdrawal of pandemic UBs had generated short-term effects that then propagate over time. We find that employment gains of early-withdrawal states disappeared one year after the policy change, while effects on job quality outcomes persisted.

Overall, the evidence suggests that the early withdrawal of pandemic UBs generated an initial increase in employment. This was the main objective of governors in states that withdrew PUA and FPUC early, in line with fears that the recovery was held back by generous welfare transfers. However, that the policy change applied indiscriminately to all workers likely penalized certain groups, such as Black and Asian individuals. Additionally, that early withdrawals generated a sudden and sharp decrease in UB support might have rushed individuals into jobs regardless of the quality of the match. The net labor market gains from the policy are therefore uncertain, especially given that effects on job quality outcomes are the only outcomes to persist one year later.

2 Policy

During 2020 and 2021, the U.S. government provided unprecedented relief to those who lost their jobs as a result of the pandemic. This support largely came in the form of increased generosity of UBs. In particular, the 2020 Coronavirus Aid, Relief and Economic Security (CARES) Act expanded existing state-run unemployment insurance programs to increase benefit levels, expand benefit access, and increase benefit duration.[5]

PUA provided access to UBs to individuals who had lost their job for COVID-19 related reasons and were not eligible for regular UBs, or had exhausted their rights to UBs. In this way, PUA temporarily increased UB eligibility to also cover self-employed individuals (including gig workers), part-timers and employees with insufficient tenure at the time of job loss to qualify for standard UBs.

[5] We discuss the benefit level and access expansions through the PUA and FPUC. Federal funding of the Extended Benefits program and the introduction of the Pandemic Emergency Unemployment Compensation also expanded benefit durations, initially to 13 extra weeks atop the 26 week baseline in most states.

FPUC consisted instead of a weekly federal supplement to state-level UBs. This was set at $600 per week. Given that spring 2020 job losses were concentrated among low-income workers, the $600 supplement translated into replacement rates higher than 100% for around three unemployed out of four (Ganong et al., 2020b).

FPUC initially ended in July 2020, but was reinstated at $300 in January 2021 and was set to expire in September 2021, when PUA was also scheduled to terminate. However, 18 states opted out from both FPUC and PUA in June 2021. Of the other 32 states, 24 maintained instead both programs until its original expiration in September, while the remaining eight states enter a miscellaneous category of different cases and will not be included in the analysis.[6]

The decision by certain states to terminate PUA and FPUC earlier than anticipated came amid concerns of labour shortages holding back the recovery and following a weaker than expected job report in May 2021 (which was later revised upwards). While these concerns might have reflected differences across states in the evolution of the economic recovery, there is evidence that they were also politically motivated.[7]

Notably, the withdrawal of UBs led to abrupt declines in income support: in withdrawal states, all UB recipients lost the $300 weekly supplement, while PUA recipients lost access to UBs altogether. These PUA beneficiaries represented 40 percent of overall UB claims during 2020, and the program's benefits disproportionately went to lower-income adults (Greig et al., 2022).

3 Data

Our primary data source is the panel component of the monthly CPS files obtained from the Integrated Public Use Microdata Series (Ruggles et al., 2021). The CPS has a rotating panel structure, whereby individuals are interviewed for four consecutive months, are not interviewed for the eight following months, and are then interviewed again for four last consecutive months. We exploit the panel structure of the CPS to study short- and medium-run effects, restricting the sample to a balanced panel of individuals who have completed all

[6]The remaining eight states (i) ended only one of the two programs (Alaska, Arizona, Florida and Ohio), (ii) ended both programs but according to a different timing (Tennessee and Louisiana), or (iii) ended both programs but were forced to reinstate them following a court's decision (Indiana and Maryland).

[7]In particular, the Republican party repeatedly called for the early withdrawal of PUA and FPUC. The Democratic administration instead defended the programs, while also acknowledging that it was not appropriate to further extend them beyond September 2021 (NYT, 2021).

interview rounds.

In part of the analysis, we analyze effects on wages. These are obtained from the CPS Outgoing Rotation Group (ORG), which asks information on wages and weekly hours worked to wage and salaried workers during their fourth and eight interview round. We then use data on wage income from the 2022 Annual Social and Economic Supplement (ASEC) supplement of the CPS to compute deciles of the income distribution for wage and salaried workers, and assign each ORG respondent to a specific decile.

To confirm that our results are not driven by variation in state-month COVID-19 restrictions, we also use state-level information on COVID-19 restrictions and support measures by the Oxford COVID-19 Government Response Tracker (OxCGRT). These indexes are measured at the daily level, and we compute their average in each calendar month. The "Containment and health index" reports information on health-related restrictions and progress with the vaccination campaign. The "Economic support index" tracks federal- and state-level support in place to cope with the consequences of the pandemic.

4 Identification

We evaluate the effect of the early withdrawal of PUA and FPUC in a DiD setting, by exploiting cross-state variation in the timing of the policy expiration. Identification requires that early and late withdrawal states would have been on parallel trends in the outcomes of interest absent the policy change. Given that this is a two-period two-group research design, it is not subject to concerns relates with staggered DiD.

Figure 1 provides suggestive evidence that the two groups of states were on parallel trends in the months before the policy change. Panel A presents employment levels for early and late policy withdrawal states, normalized to equal one in January 2020. It shows that, at the time of the policy change, indicated by the vertical dashed line, the early withdrawal states were about to close the employment gap with respect to the pre-pandemic levels, while late withdrawal states where still around 5 percentage points below. However, this gap is completely accounted for by differences in the evolution of employment in the very early phase of the pandemic. After the initial contraction, the two groups showed very similar trends in the recovery, which started strong and then plateaued around the end of 2020.

While the evolution of employment was on parallel trends between early and late withdrawal states in the months before the policy change, it is still important to understand what led certain states to end PUA and FPUC earlier than expected. In particular, a possible concern arises if the policy change is triggered by other time-varying forces that could have an independent effect on the outcomes of interest. However, Appendix Table A1 shows that the best predictor of the early policy termination is whether the state had a Republican governor (column 1). The pre-pandemic employment gap (January 2020-June 2021) is also statistically significant when included in isolation (column 2), but the coefficient becomes close to zero and non-significant when included together with the Republican dummy (column 5). Other measures of the employment gap computed over shorter time periods (i.e. from April 2020 or February 2021 to June 2021) are instead small and non-significant at conventional levels, even when considered alone (Columns 3 and 4).

To provide a first idea of treatment effects, Figure 1 (Panel B) documents the unweighted state-level average employment growth before and after the policy change. Between February and June 2021, the two groups had almost identical employment growth. Employment growth then increased in July and August in early withdrawal states that had ended both PUA and FPUC by then, while remaining stable in late withdrawal states. The pattern is reversed for the period of September and October, when also late withdrawal states had terminated pandemic UBs. Overall, this indicates that the withdrawal of PUA and FPUC is associated with an increase in employment. This effect seems to be driven by the policy change itself, rather than its timing (i.e. whether this happens in June or September).[8]

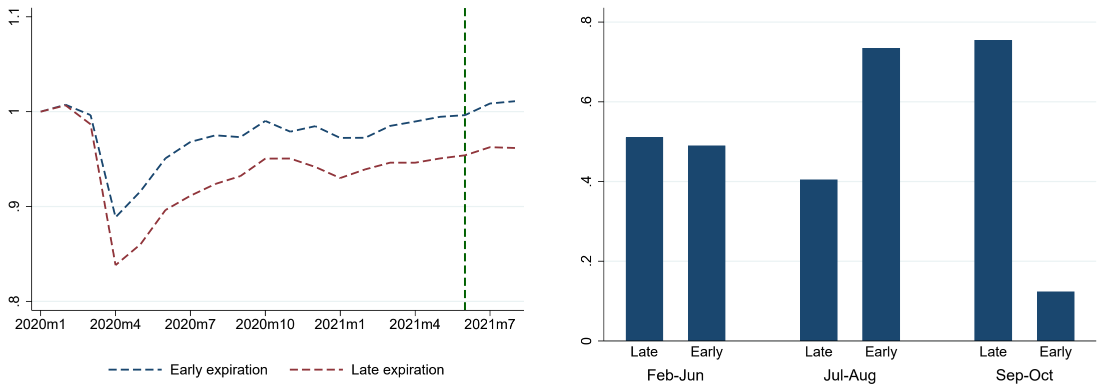
The baseline specification in the paper takes the following form:

$$Y_{ist} = \alpha + \beta_1 Early_s + \beta_2 Post_t + \beta_3 Early_s * Post_t + \beta_4 X_{ist} + c_s + t_t + \epsilon_{ist} \tag{1}$$

where Y_{ist} is the outcome of interest for individual i, living in state s at time t. This will take the form of monthly or yearly transitions across labor market statuses, exploiting the longitudinal structure of the CPS. $Early_s$ is a dummy for whether the state terminated both PUA and FPUC in June 2021 (rather than in September 2021), $Post_t$ is equal to zero in the months between February and June 2021 and to one in July and August of the same year, X_{ist} is a vector of individual-level controls for age, sex and dummies for educational attainments, marital status, racial and ethnic group and presence of children in the household, while c_s and t_t are a full set of time and state fixed effects. The coefficient of interest is the one of the interaction term between the early state status and the post policy dummy, corresponding

[8] However, in the main part of the analysis, we will be looking only at the policy change generated by the early termination of PUA and FPUC. This is because its sudden nature limits the risk of any anticipatory effects, which might instead be happening when PUA and FPUC naturally expired in late withdrawal states.

Figure 1: Employment trends around the policy change, by date of policy expiration



(a) Employment in states with early and late policy expiration, Jan 2020=1

(b) Average employment growth in states with early and late policy expiration

Notes: States are divided between early and late withdrawal according to whether they ended both PUA and FPUC in June or September 2021.

to β_3 in the equation above. Standard errors are clustered at the state level.

5 Results

This section presents the main results of the analysis. Section 5.A introduces the baseline results on short-run labor market transitions, section 5.B discusses the heterogeneous analysis for these baseline results, section 5.C looks at the treatment effects on measures of job outcomes, and section 5.D examines the effects of the policy change one year later.

A. Labor market transitions

Table 1 presents the baseline results for the DiD estimates for all possible combinations of month-to-month transitions from and to employment (E), unemployment (U) and inactivity (I). The table reports coefficient estimates and standard errors of the interaction term between the early withdrawal status and the post policy dummy, corresponding to β_3 in equation 1. Results clearly show a decrease in the probability of remaining in unemployment following the withdrawal of pandemic UBs, which is matched by a parallel increase in the probability of transitioning into employment.

The point estimate (0.150) for unemployment-to-employment transitions is relatively large, corresponding to more than two-thirds of the mean value of such transitions in control states before the policy change (0.229). Coefficients for all other types of transitions

Table 1: Baseline results on labor market status

	EE (1)	EU (2)	EI (3)	UE (4)	UU (5)	UI (6)	IE (7)	IU (8)	II (9)
$\hat{\beta}_3$	0.006	-0.002	-0.004	0.150**	-0.148***	-0.002	-0.003	-0.002	0.005
	(0.004)	(0.002)	(0.003)	(0.063)	(0.050)	(0.030)	(0.003)	(0.002)	(0.004)
Mean of y	0.965	0.01	0.025	0.229	0.551	0.22	0.03	0.018	0.952
Observations	88,921	88,921	88,921	4,604	4,604	4,604	72,951	72,951	72,951
R-squared	0.013	0.004	0.012	0.030	0.036	0.032	0.023	0.019	0.040
State and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates and standard errors for β_3 from separate regression models, where the dependent variables are the different types of monthly labor market transitions from and to employment (E), unemployment (U) and inactivity (I). Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. The mean of the outcome of interest is computed for the control group in the period before the policy change. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

are instead around zero, in line with the fact that the expiration of pandemic UBs affects only transitions from unemployment.

We now present robustness tests to confirm the validity of these baseline results. We focus on the models where the dependent variables are the possible transitions from unemployment to employment, unemployment or inactivity, given that this is the only set of transitions for which we found some statistically significant results.

We start by providing additional evidence in support of the parallel trend assumption (Appendix Figure B1). In Panel A, we plot the coefficients of the interaction term between the early withdrawal state dummy and dummies for the 12 months before the policy change and the two months after. The coefficient for June 2021 is normalized to zero. Results confirm that early and late withdrawal states were on parallel trends before the policy change, and reveal that treatment effects on unemployment-to-unemployment and unemployment-to-employment transitions are similar in July and August. Consistently, Panel B shows that point estimates for β_3 in our baseline specification (i.e. with the post policy dummy, rather than the monthly dummies) do not vary significantly if we change the number of months that we include in the pre-treatment period. In the baseline specification, this is defined as the time between February and June 2021, as the FPUC was reinstated at the federal level only in January 2021. However, results are qualitatively similar for any start date of the pre-treatment period ranging from February 2020 to May 2021.

Even if employment was on parallel trends before the policy change, the identification strategy would still be violated in the presence of time-varying trends differing between early and late withdrawal states. Given the political process that led to the early termination of the policy, this relates in particular to the fact that Republican states who withdrew pandemic UBs early were also more likely to be lifting a series of COVID-19 restrictions. This lifting of restrictions, rather than the withdrawal of PUA and FPUC, might be explaining the increase in employment we observe in the data. Using the "Containment and health index" and the "Economic support index" by OxCGRT, we confirm that late withdrawal states were characterized by a higher stringency of these indicators (Appendix Figure B2, Panels A and B). However, if anything, late withdrawal states were lifting COVID-19 restrictions more rapidly than early withdrawal states around the time of the policy change. In any case, when we add these variables as controls in our baseline model (either separately or jointly), point estimates of treatment effects barely change (Appendix Table A2).

We then address some possible limitations arising from the use of CPS data (Appendix Table A3). The first relates to the fact that we cannot observe UB recipients in the data, and our estimates should therefore be interpreted as intention-to-treat effects on the unemployed. To investigate how this might be biasing our results, we restrict the sample to individuals who are more likely to be receiving UBs.[9] As expected, results are larger in magnitude for this sub-group (Panel A), also because short-term unemployed might be more likely to exit unemployment (see below). Following Holzer et al. (2021), the second correction that we make is to account for the possible mis-measurement of labor force status, potentially generating spurious transitions out of unemployment. We implement the standard re-coding procedure used in the literature (Farber et al., 2015) and consider the transition spurious if the individual moves from unemployment to either employment or inactivity in one month, but then returns to unemployment in the following month. Results remain very similar (Panel B).

B. Heterogeneous analysis

We now examine heterogeneous effects by personal and household characteristics. Previous work has shown that employment and unemployment rates for certain demographic groups, and racial/ethnic minorities in particular, are more volatile over the business cycle (Couch and Fairlie, 2010). As detailed previously, there are competing perspectives on whether we should expect racial/ethnic differences in employment responses after the benefit

[9]These are defined as those (i) who were unemployed since less than 26 weeks, for which UB eligibility should have not expired, and (ii) have lost their job involuntarily (i.e. due to layoff or contract end).

withdrawals: Black and Hispanic individuals, in particular, are more liquidity constrained and therefore may be more responsive to the withdrawal of UBs. Alternatively, we may find weaker responses among racial/ethnic minorities given that these adults might have more limited access to UBs and/or face greater barriers to re-employment.

Appendix Figure B3 (Panels A to E) demonstrates that treatment effects are very similar by age group, sex, educational attainment, presence of children in the household, and family income. Individuals who had spent more than three months in unemployment were instead less likely to transition to employment and more likely to move into inactivity when pandemic UBs expired, compared to those who had been unemployed for less than three months (Appendix Figure B3, Panel F). This is in line with the fact that short-term unemployed are on average more job-ready and the hazard rate out of unemployment declines with the length of the unemployment spell.

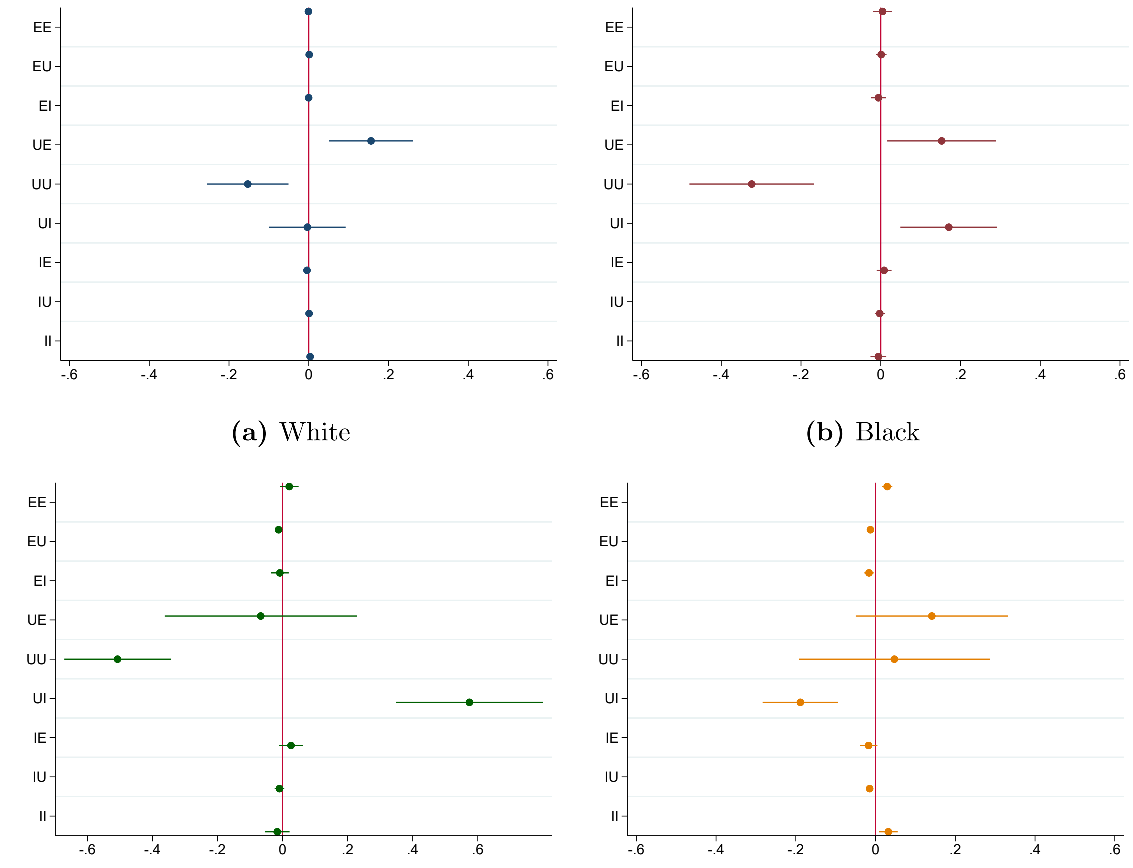
We instead observe strong differences across race/ethnicity (Figure 2).[10] For White individuals, we confirm the pattern observed in the overall sample: the decrease in the probability of remaining unemployed is entirely compensated by an increase in the probability of moving to employment (Panel A). A different picture emerges from Black individuals: for this group, the increase in the outflow out of unemployment is redirected equally into employment and inactivity (Panel B). The labor market response to the policy change is even more atypical for Asian individuals: they experience a larger decrease in the probability of remaining unemployed, but this is entirely compensated by an increase in the probability of transitioning into inactivity (Panel C). Results for Hispanic individuals are slightly different. Their unemployment-to-employment transition is less precisely estimated, but similar in magnitude to the one observed for White individuals. However, this is driven by a decrease in the probability that unemployed individuals transition into inactivity, rather than a decrease in the probability of remaining in unemployment (Panel D).[11]

Previous research has shown that individuals from racial/ethnic minorities are substantially less likely to receive UBs in the US (Kuka and Stuart, 2021), and these gaps persisted also during the pandemic (Forsythe and HesongYang, 2021). However, it is not

[10] We have classified individuals in four mutually exclusive categories: non-Hispanic White (henceforth, White), non-Hispanic Black (Black), non-Hispanic Asian (Asian) and Hispanic.

[11] In addition, for the sample of Hispanic individuals we also observe some small indirect effects (i.e. treatment effects that concern the employed and inactive, rather than the unemployed). In particular, employed Hispanic individuals are less likely to move into both unemployment and inactivity and more likely to remain in employment. Similarly, inactive Hispanic individuals are less likely to transition to unemployment and more likely to remain inactive. This is consistent with the lower returns associated with the unemployment status, following the termination of PUA and FPUC.

Figure 2: Heterogeneous treatment effects by racial and ethnic group



(a) White

(b) Black

(c) Asian

(d) Hispanic

Notes: The figure plots point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). These results are shown for different outcomes of interest, which are displayed on the y axis as the types of transitions from and to employment (E), unemployment (U) and inactivity (I). The different panels present separate sets of results for White (Panel A), Black (Panel B), Asian (Panel C) and Hispanic (Panel D).

clear the extent to which these gaps should systematically differ between early- and late-withdrawal states around the time of the policy change. In an attempt to examine this concern, Appendix Figure B4 presents the same heterogeneous analysis by race/ethnicity, but restricting the sample to likely UB recipients as defined in Section 5.A. Results remain essentially unchanged. Additionally, we find that results still hold even after controlling for one-digit occupation or industry dummies (Appendix Figure B5, Panels A and B respectively).[12]

A final question to be addressed is what drives Asian and Black individuals out

[12] A large set of explanations are compatible with the differential response to the policy change by race/ethnicity that we observe in the data. A plausible possibility is that, at the time of the policy change, White individuals who stopped receiving UBs crowded out jobseekers belonging to minority groups.

of the labor force. To this end, we further differentiate the analysis by age groups (i.e. 30 years old and below, between 31 and 54 years old, and 55 years old and above) (Appendix Table A4). To start with, we find that the increase in inactivity concerns only the youth among Black individuals and both young and prime-age workers among Asian individuals (Panel A). Consistently, we find no increase in transitions from unemployment to inactivity due to retirement (Panel B). Also, there is no increase in the probability of transitioning from unemployment to inactivity due to disability or other health reasons (Panel C). Rather, for Asian individuals, the increase in inactivity among the youth is primarily accounted for by an increased enrollment in education (Panel D), while the increase in inactivity among prime-age workers is explained by individuals taking up family responsibilities (Panel E). For young Black individuals, the increase in inactivity is instead driven by both an increase in the probability of being enrolled in education (Panel D) as well as for other reasons of inactivity (Panel F).[13]

C. Job outcomes

An additional element to be considered refers to the possible effects of the policy change on job outcomes. More generous UBs can help individuals look for better job matches. At the same time, skills can deteriorate with time spent in unemployment. Consistently with the presence of these opposite forces, the literature has found mixed results on the effects of UB generosity on job outcomes (Card et al., 2006; Lalive, 2007; Nekoei and Weber, 2017; van Ours and Vodopivec, 2006).

We examine treatment effects on job outcomes by studying month-to-month transitions from unemployment to different types of jobs. Specifically, we use the standard crosswalk used in Autor and Dorn (2009), Acemoglu and Autor (2011) and Cortes et al. (2020), among others. Using this crosswalk, we convert the three-digit occupation codes into the four broad categories of non-routine cognitive, non-routine manual, routine cognitive and routine manual occupations. We present two sets of baseline results: one assigns zero values to the occupational variables for individuals who are not in employment (unconditional sample), and one assigns missing values to the occupational variables for individuals who are not in employment (conditional sample).

The results demonstrate that the early withdrawal of PUA and FPUC led to an increase in the transitions from unemployment to routine manual jobs (Table 2). In the

[13] Unreported results show that school attendance for Black youth increases for both high school and college (both full-time), while for Asian youth only for college (full-time).

unconditional sample, treatment effects on the transitions from unemployment to all four occupational groups are positive. This in line with the general increase in unemployment-to-employment transitions documented above, which has led to an increase in the take-up of all types of jobs. However, the coefficient for routine manual occupations is much larger in magnitude compared to the others, indicating a shift in unemployment-to-employment transitions towards these jobs. Before the policy change, around three unemployment-to-employment transitions out of ten involved individuals taking up routine manual jobs. After the policy change, this increased to four transitions out of ten. Looking at the conditional sample allows us to examine the effects on the distribution of employment. As expected, in this sample the coefficient for routine manual jobs increases in magnitude, although it is now statistically significant only at the 10 percent. Coefficients for all other occupational groups become instead negative or close to zero, but always non-significant.

We complement these findings with suggestive evidence on transitions from unemployment to jobs in different deciles of the income distribution. We assign individuals in the CPS ORG to different deciles of the income distribution following an imputation strategy similar to the one used in Ganong et al. (2020b). Using data from the ASEC supplement for 2022, we proxy weekly wages by dividing yearly wages by the number of weeks worked.[14] We use this information to compute deciles of the income distribution at the state, year and one-digit occupational level. We eliminate cells for which less than 20 observations are available, and then match information on the wage distribution within each cell to the CPS. We then use self-reported information on weekly wages in the CPS ORG to assign individuals to a given decile of the wage distribution and then compute treatment effects on the probability of transitioning from unemployment to a given wage decile. Appendix Figure B6 presents the results of this exercise, showing that the withdrawal of pandemic UBs led to a small shift towards the bottom of the income distribution for middle-income earners (i.e. from the 7th to the 5th and 6th deciles).

We return to our baseline results on occupational groups to present some robustness tests (Table A5). The main concern relates to the fact that individuals transitioning from unemployment to employment in control and treated states might be different. In the end, treated and control states were at different points of the recovery process. Additionally, transitions from unemployment to employment were much higher in treated than control

[14]Results are obtained by assigning individuals to deciles of the income distribution using data from the 2022 ASEC supplement (reporting information on 2021). However, impact estimates on wages are qualitatively similar when using the 2020 ASEC supplement (reporting information for 2019). We restrict the ASEC sample to wage and salaried workers, in line with the universe sampled in the CPS ORG.

Table 2: Baseline results on job quality outcomes

	U-Non-routine cognitive	U-Non-routine manual	U-Routine cognitive	U-Routine manual
	(1)	(2)	(3)	(4)
	Panel A: Unconditional sample			
$\hat{\beta}_3$	0.039*	0.009	0.026	0.083***
	(0.020)	(0.018)	(0.030)	(0.026)
Mean of y	0.053	0.056	0.049	0.069
Observations	4,604	4,604	4,604	4,604
R-squared	0.061	0.029	0.026	0.062
	Panel B: Conditional sample			
$\hat{\beta}_3$	-0.012	-0.086	0.001	0.134*
	(0.060)	(0.072)	(0.058)	(0.074)
Mean of y	0.231	0.245	0.215	0.301
Observations	1,094	1,094	1,094	1,094
R-squared	0.312	0.127	0.109	0.257
State and time FE	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates and standard errors for β_3 from separate regression models, where the dependent variables are month-to-month transitions from unemployment to non-routine cognitive, non-routine manual, routine cognitive and routine manual jobs. Panel A includes in the sample all individuals in unemployment in month t, while Panel B restricts the sample to those unemployed at time t who had transitioned into a job at time $t+1$. Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. The mean of the outcome of interest is computed for the control group in the period before the policy change. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

states as a result of the policy change. All this might have changed *who* is transitioning out of unemployment. To assess whether this is the case, we first compare different specifications where we change the sets of covariates (i.e. no individual-level controls, with personal level characteristics, adding also industry and occupation dummies). If selection bias was driving our results, treatment effects should decrease as we increase the set of covariates. However, coefficients remain very similar and, if anything, increase in size for routine manual occupations (Panel A). Secondly, we restrict the sample of treated states to those with a relatively large pandemic employment gap (but that still decided to withdraw earlier from PUA and FPUC) and the sample of control states to those with a relatively small employment gap (but that still maintained pandemic UBs until September 2021).[15] Treatment effects barely change as we conduct these different exercises (Panels B to D).

D. Medium-term effects

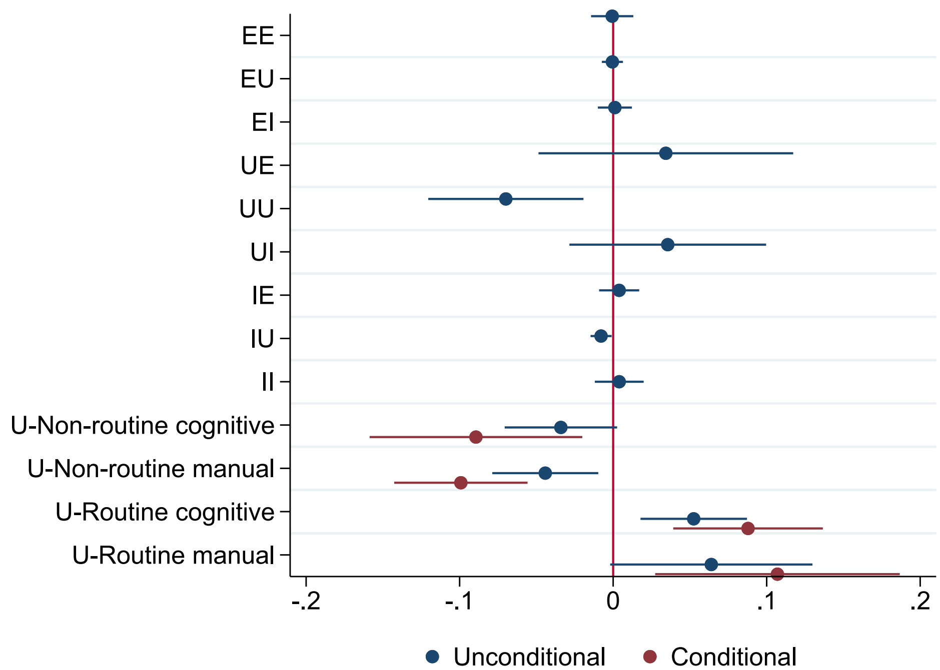
To conclude, we look at the medium-run effects of the policy change. The vast majority of the literature has examined the effects of UB generosity only until the time of first re-employment, but this can either under- or over-state the true costs of UB extensions. Notable exceptions are Schmieder et al. (2012) and Scrutinio (2020), who both find that the short-term negative employment effects of UB generosity are partially compensated by higher employment rates in the medium- to long-run. The final effect will indeed depend on whether benefit generosity also influences the recurrence and length of any other unemployment spell (Schmieder and von Wachter, 2016).

In this part of the analysis, we adopt the same methodology used to obtain the baseline results, but look at year-to-year (rather than month-to-month) transitions. This means that we look at individuals who were employed, unemployed or inactive in control and treated states around the time of the policy change in 2021, and examine their labor market status one year later. Given that control states also withdrew pandemic UBs in September 2021, any long-lasting impact of the early withdrawal of PUA and FPUC would emerge only if the short-term effects of the policy change propagate over time (e.g. due to scarring or spillover effects). Since the short-term results are mixed (i.e. positive effects on employment, but negative effects on job outcomes), it is also interesting to analyze which of these effects persists one year later. For these reasons, we look at the medium-run effects on all type of transitions from and to employment, unemployment and inactivity, as well as from unemployment to the four broad occupational groups introduced above.

Estimation results for the medium-run effects are available in Figure 3. In line with the short-term analysis, treatment effects for all transitions out of employment and inactivity are estimated around zero and statistically non-significant. Treatment effects for unemployment-to-unemployment transitions remain instead negative and statistically significant, but around half in magnitude compared to the short-term results (i.e. 7 percentage points compared to 15 percentage points). However, the decrease in the probability of remaining unemployed is no longer perfectly compensated by a parallel increase

[15] More specifically, we (i) use all treated states, but only control states with a small employment gap (Panel B), (ii) use all control states, but only treated states with a large employment gap (Panel C), and (iii) we only use treated (control) states with a large (small) employment gap (Panel D).

Figure 3: Treatment effects one year after the policy change



Notes: The figure plots point estimates and 90% confidence intervals for the coefficient β_3 in equation (1), this time looking at year-to-year transitions. Results are shown for different outcomes of interest, which are the transitions to and from employment (E), unemployment (U) and inactivity (I) (upper part of the figure) as well as the transitions from unemployment to non-routine cognitive, non-routine manual, routine cognitive and routine manual jobs (lower part of the figure). For job quality outcomes, we present separately results for the unconditional sample (blue dot, where all unemployed at time t are included) and the conditional sample (red dot, where we include only the unemployed at time t who are also employed at time $t+1$).

in unemployment-to-employment transitions. Rather, we see an equivalent increase in both unemployment-to-employment and unemployment-to-inactivity transitions. Both coefficients are around 3 percentage points in magnitude, but imprecisely estimated. This means that any positive employment gain from the early expiration of pandemic UBs has almost disappeared one year after the policy change, and that the mixed employment effects we found in the short-run for racial/ethnic minorities now concern our entire sample.

Finally, we turn to the analysis of the medium-term effects on the types of jobs found (blue and red dots in Figure 3, for the unconditional and conditional samples as defined above). To start with, we observe that, while in the short-term treatment effects for all four occupational groups were positive in the unconditional sample (see Panel A of Table 2), this is no longer the case in the long-run analysis. Rather, the sum of the four point estimates is around zero. This is in line with the result just discussed that there were positive employment gains in the short-run, but these become small and non-significant one year later. Additionally, we find that the positive treatment effect on the probability of transitioning from unemployment to routine manual jobs is similar in magnitude compared to the short-term results. The coefficient for the transitions from unemployment to routine cognitive occupations also becomes positive, while we obtain negative effects on unemployment- to non-routine cognitive and non-routine manual occupations. This suggests that the initial effects on job quality have persisted over time, and have eventually increased.

6 Conclusions

This paper investigates the employment responses to the early withdrawal of PUA and FPUC by some US states in the summer of 2021. The timing of the benefit withdrawals – with 18 states removing the benefits in June 2021, and nearly all other states waiting until the national expiration in September 2021 – allowed us to investigate the short- and medium-term consequences of the early benefit withdrawals. Additionally, we investigated differences in employment responses by race/ethnicity and differences in the quality of job matches.

We find that the early expiration of the PUA and FPUC substantially increased transitions from unemployment to employment. The transitions after the early withdrawal of UBs corresponded to around two-thirds of the mean value of unemployment-to-employment transitions in control states before the policy change. These conclusions are consistent with Holzer et al. (2021). We then qualify these findings along three dimensions.

First, we find strong differences in results across race/ethnicity, with Black and Asian individuals substantially penalized by the policy change. In contrast to White adults, a group that transitioned entirely from unemployment to employment as a result of the policy change, for Black and Asian individuals half (or more) of the increase in exits from unemployment resulted into transitions into inactivity.

Second, we find that the expiration of the policy has increased transitions from unemployment to routine and manual jobs, rather than non-routine, non-manual jobs. Though early-withdrawal states achieved greater transitions to employment, the jobs to which these adults transitioned carried a small wage penalty. This finding contributes to broader understanding of the role of UB generosity in influencing the quality of job matches.

Third, we investigate policy effects one year after the benefit withdrawals and find that employment gains for the early withdrawal states had disappeared, while the negative effects on job type persisted. These findings suggest that the relative employment gains of the early-withdrawal states was short-lived, while the consequences of the poorer job matches was more durable.

The findings emphasize the usefulness of studying both the costs and benefits of changes to the generosity and availability of UBs, both in the short- and medium-term, and by different sub-populations with varying levels of pre-reform (dis)advantage. Sudden and indiscriminate policy changes, such as the early withdrawal of UBs, may be beneficial in increasing employment rates; however, such policy changes may also generate unintended consequences that could be detrimental on equity and efficiency grounds. These consequences include any negative effect that the withdrawal of income support from UBs might have had on consumption and living standards.

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Appendices

A Appendix: Additional tables

Table A1: Determinants of early policy withdrawal

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Republican administration	0.900***				0.863***	0.887***	0.902***
	(0.069)				(0.093)	(0.076)	(0.068)
Employment: Jun2021/Feb2020		-0.052***			-0.009		
		(0.012)			(0.007)		
Employment: Jun2021/Apr2020			0.024*			0.009	
			(0.013)			(0.01)	
Employment: Jun2021/Feb2021				-0.004			0.003
				(0.019)			(0.005)
Observations	43	43	43	43	43	43	43
R-squared	0.828	0.224	0.051	0.001	0.833	0.834	0.829

Notes: The table presents coefficient estimates and standard errors of different regression models, where the dependent variable is equal to one if the state has ended both PUA and FPUC in June 2021, and zero if the termination of both programs happened instead in September 2021. Robust standard errors are in parentheses. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A2: Robustness tests adding COVID-19 restriction measures among the covariates

	UE (1)	UE (2)	UE (3)	UU (4)	UU (5)	UU (6)	UI (7)	UI (8)	UI (9)
$\hat{\beta}_3$	0.126*	0.153**	0.132*	-0.112*	-0.149***	-0.115*	-0.014	-0.004	-0.018
	(0.075)	(0.060)	(0.073)	(0.058)	(0.049)	(0.058)	(0.046)	(0.029)	(0.046)
Mean of y	0.965	0.010	0.025	0.229	0.551	0.22	0.03	0.018	0.952
Observations	4,604	4,604	4,604	4,604	4,604	4,604	4,604	4,604	4,604
R-squared	0.031	0.031	0.031	0.036	0.036	0.036	0.032	0.032	0.032
State and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	All	All	All	All	All	All	All	All	All
COVID-19 controls	Health	Econ	All	Health	Econ	All	Health	Econ	All

Notes: The table presents coefficient estimates and standard errors for β_3 from separate regression models, where the dependent variables are the different types of monthly labor market transitions from and to employment (E), unemployment (U) and inactivity (I). Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity and presence of children in the household. The mean of the outcome of interest is computed for the control group in the period before the policy change. The different models differ for the inclusion of the Health containment index and/or the Economic support index from OxCGRT. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A3: Miscellaneous robustness tests on the baseline specification

	UE (1)	UU (2)	UI (3)	UE (4)	UU (5)	UI (6)
	Panel A: Likely UB recipients			Panel B: Spurious transitions		
$\hat{\beta}_3$	0.179***	-0.113	-0.066	0.163***	-0.170***	-0.027
	(0.066)	(0.081)	(0.042)	(0.044)	(0.045)	(0.040)
Mean of y	0.309	0.479	0.212	0.202	0.551	0.167
Observations	2,247	2,247	2,247	3,081	3,081	3,081
R-squared	0.047	0.050	0.053	0.041	0.039	0.034
State and time FE	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table presents coefficient estimates and standard errors for β_3 from separate regression models, where the dependent variables are the different types of monthly labor market transitions from and to employment (E), unemployment (U) and inactivity (I). Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity and presence of children in the household. The mean of the outcome of interest is computed for the control group in the period before the policy change. In the models in Panel A, the sample is composed only of unemployed individuals who lost their job involuntarily and were unemployed since less than 26 weeks at the time of transition. In Panel B, we correct for spurious transitions following the coding procedure described in the text. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A4: Heterogeneous treatment effects by race and ethnicity: Additional results by age groups and types of inactivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel A: Unemployment to inactivity				Panel B: Unemployment to inactivity for retirement			
β_3	-0.022	-0.197	0.034	0.144	-0.003	-0.008	-0.024	0.048
	(0.053)	(0.134)	(0.079)	(0.142)	(0.018)	(0.009)	(0.016)	(0.064)
$\beta_3 * Black$	0.148	**0.923*** **	0.130	-0.677	-0.036	0.008	-0.008	0.069
	(0.102)	**(0.301)**	(0.145)	(0.409)	(0.033)	(0.009)	(0.037)	(0.211)
$\beta_3 * Asian$	**0.564*** **	**0.898** **	**0.639*** **	-0.372*	0.006	0.008	0.024	-0.121
	(0.118)	**(0.383)**	**(0.126**	(0.204)	(0.034)	(0.009)	(0.016)	(0.096)
$\beta_3 * Hispanic$	-0.100	0.062	-0.127	0.093	0.014	0.008	0.023	0.100
	(0.104)	(0.185)	(0.131)	(0.311)	(0.018)	(0.009)	(0.016)	(0.106)
	Panel C: Unemployment to inactivity for disability or illness				Panel D: Unemployment to inactivity for school attendance			
$\hat{\beta}_3$	-0.010	-0.030	0.00321	-0.003	-0.019	-0.050	-0.007	0
	(0.012)	(0.028)	(0.013)	(0.020)	(0.024)	(0.090)	(0.024)	(0)
$\beta_3 * Black$	0.014	0.037	-0.053	0.020	**0.159** **	**0.580***	0.050	0
	(0.039)	(0.028)	(0.074)	(0.075)	**(0.071)**	**(0.306)**	(0.047)	(2.03e-09)
$\beta_3 * Asian$	0.009	**-0.043**	-0.005	0.062	**0.309***	**0.604***	0.011	-0
	(0.053)	**(0.054)**	(0.025)	(0.074)	**(0.176)**	**(0.318)**	(0.038)	(0)
$\beta_3 * Hispanic$	0.006	**-0.003**	**-0.064***	**0.339***	0.013	0.110	0.016	-0.027
	(0.024)	**(0.029)**	**(0.032)**	**(0.197)**	(0.046)	(0.136)	(0.031)	(0.034)
	Panel E: Unemployment to inactivity for family reasons				Panel F: Unemployment to inactivity for other reasons			
β_3	-0.013	-0.068	-0.015	0.034	0.022	-0.046	0.013	0.060
	(0.031)	(0.047)	(0.053)	(0.046)	(0.0192)	(0.061)	(0.050)	(0.062)
β_3*Black	-0.013	-0.029	0.129	**-0.697*** **	0.019	**0.330** **	0.008	**0.100**
	(0.078)	(0.191)	(0.131)	**(0.204)**	(0.066)	**(0.158)**	(0.075)	**(0.104)**
β_3*Asian	0.106	0.294	**0.976*** **	-0.189	0.109	0.039	**0.158**	-0.138
	(0.171)	(0.183)	**(0.173)**	(0.153)	(0.085)	(0.083)	**(0.112)**	(0.114)
β_3*Hispanic	0.025	0.078	0.077	**-0.205*** **	**-0.133*** **	**-0.096**	**-0.149**	**0.081**
	(0.048)	(0.059)	(0.127)	**(0.067)**	**(0.049)**	**(0.074)**	**(0.104)**	**(0.070)**
Observations	4,604	1,315	2,528	1,286	4,604	1,315	2,003	1,286
R-squared	0.184	0.274	0.236	0.314	0.244	0.094	0.197	0.337
State and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sample	Total	Young	Prime age	Old	Total	Young	Prime age	Old

Notes: The table presents coefficient estimates and standard errors for β_3 and its interaction with the race and ethnicity variable, where the omitted group corresponds to White individuals. Results are from separate regression models, where the dependent variables are unemployment-to-inactivity transitions (Panel A), unemployment-to-inactivity for retirement transitions (Panel B), unemployment-to-inactivity for disability or other illness transitions (Panel C), unemployment-to-inactivity for school attendance transitions (Panel D), unemployment-to-inactivity for family reasons transitions (Panel E) and unemployment-to-inactivity for other reasons transitions (Panel F). For each outcome of interest, we further run the analysis separately for the overall sample as well as for young (30 years of below), prime-age (31 to 54) and older individuals (55 and above). Individual-level controls include age, sex and dummies for educational attainments, marital status and presence of children in the household. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively. Coefficients in bold are those for which the sum of β_3 and the interaction with the race and ethnicity variable is statistically significant at least at the 10 per cent.

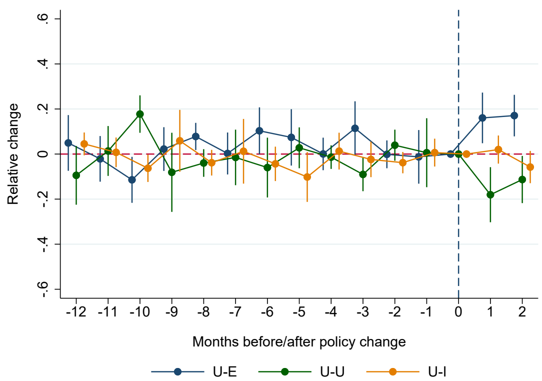
Table A5: Robustness tests on job outcome variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Panel A: Changing covariates											
	Non-routine cognitive			Non-routine manual			Routine cognitive			Routine manual		
$\hat{\beta}_3$	0.040*	0.039*	0.039*	0.010	0.009	0.020	0.029	0.026	0.057*	0.073***	0.083***	0.088***
	(0.022)	(0.020)	(0.021)	(0.017)	(0.018)	(0.023)	(0.030)	(0.030)	(0.031)	(0.025)	(0.026)	(0.025)
Observations	4,604	4,604	3,526	4,604	4,604	3,526	4,604	4,604	3,526	4,604	4,604	3,526
R-squared	0.009	0.061	0.238	0.010	0.029	0.303	0.011	0.026	0.261	0.016	0.062	0.312
State and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Occupation FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
	Panel B: Restricting control states				Panel C: Restricting treated states				Panel D: Restricting control and treated states			
	NRC	NRM	RC	RM	NRC	NRM	RC	RM	NRC	NRM	RC	RM
β_3	-0.0383	0.0332	-0.0209	0.102***	0.0730**	-0.0333	0.0394	0.0614	0.00602	-0.00258	0.000331	0.0928
	(0.0300)	(0.0199)	(0.0405)	(0.0324)	(0.0271)	(0.0430)	(0.0271)	(0.0528)	(0.0323)	(0.0357)	(0.0359)	(0.0584)
Observations	1,897	1,897	1,897	1,897	3,494	3,494	3,494	3,494	787	787	787	787
R-squared	0.103	0.060	0.037	0.067	0.059	0.028	0.030	0.068	0.166	0.100	0.063	0.120
State and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

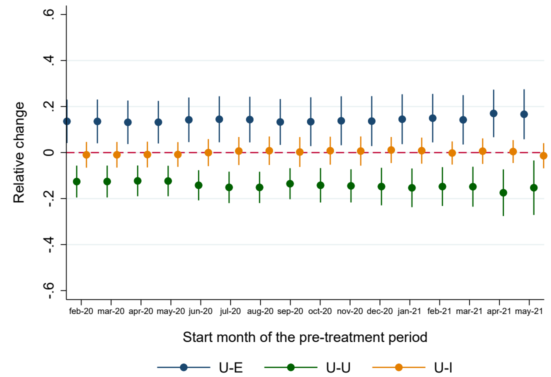
Notes: The table presents coefficient estimates and standard errors for β_3 from separate regression models, where the dependent variables are different types of month-to-month transitions from unemployment to non-routine cognitive (NRC), non-routine manual (NRM), routine cognitive (RC) and routine manual (RM) occupations. Panel A presents for each outcome of interest different models that differ based on the inclusion of (i) only state and year fixed effects, (ii) also individual-level controls, and (iii) also dummies for the occupation and industry of employment at the one-digit level. Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Panels B to D presents instead results where the set of covariates included each time remains fixed, but the sample of treatment and control states varies. In Panel B, we include all treated states but only control states whose pandemic employment gap in June 2021 was below the national median. In Panel C, we include all control states but only treated states whose pandemic employment gap in June 2021 was above the national median. In Panel D, we include only treated (control) states whose employment gap in June 2021 was above (below) the national median. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

B Appendix: Additional figures

Figure B1: Robustness tests for parallel trends



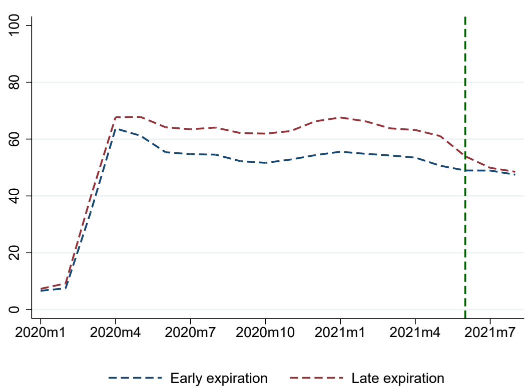
(a) DiD results at the monthly level



(b) DiD results changing the start date of the pre-treatment period

Notes: Panel A presents the DiD results of equation (1), where the post policy dummy is replaced with monthly dummies before and after the policy change. June 2021 is used as the baseline month. Panel B presents the DiD results with the simple post-policy dummy, but where the start month of the pre-treatment period varies from February 2020 to June 2021.

Figure B2: COVID-19 restriction measures in early and late withdrawal states

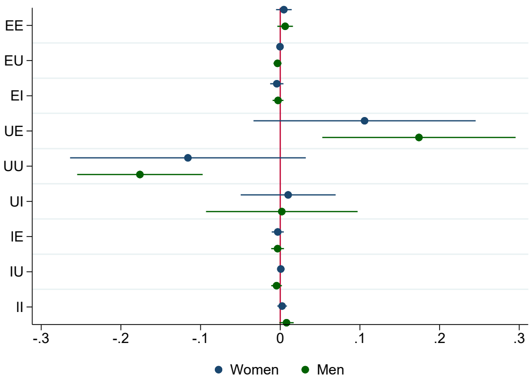


(a) Health containment index

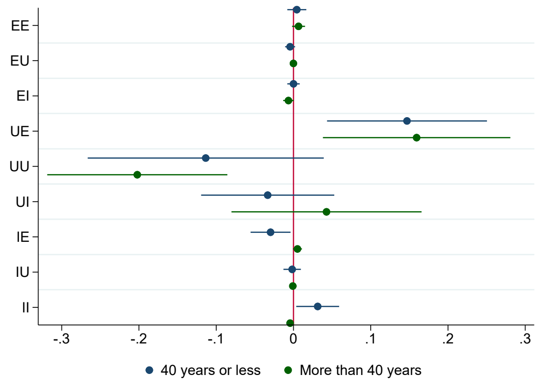
(b) Economic support index

Notes: Panel A presents the monthly average of the Health containment index by OxCGRT in early and late withdrawal states; while Panel B presents the evolution of the Economic support index. The dashed vertical line represents the month of the policy change.

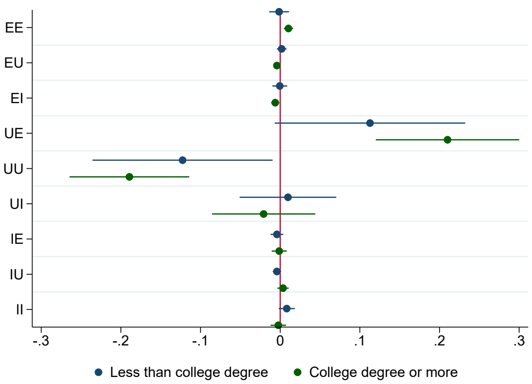
Figure B3: Heterogeneous treatment effects by selected individual and household characteristics



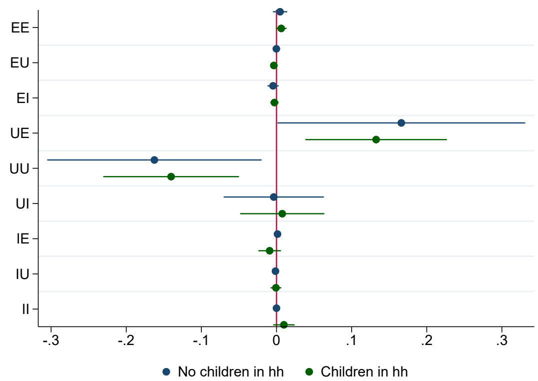
(a) By sex



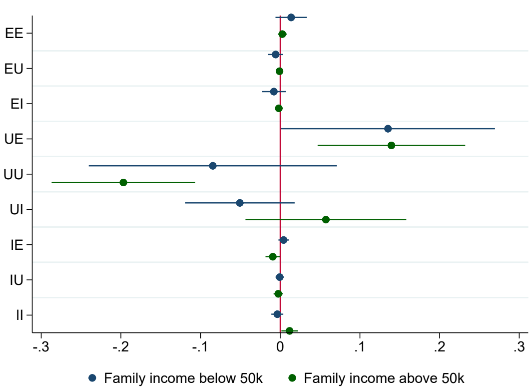
(b) By age group



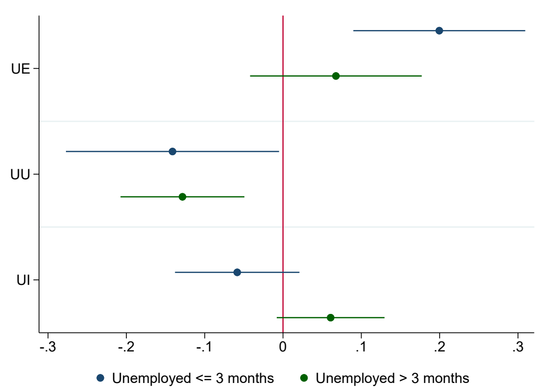
(c) By educational attainment



(d) By children in the household



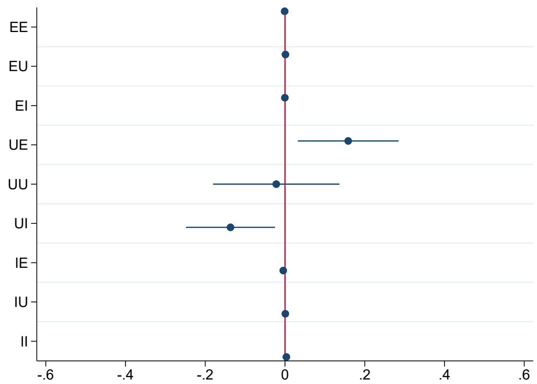
(e) By family income



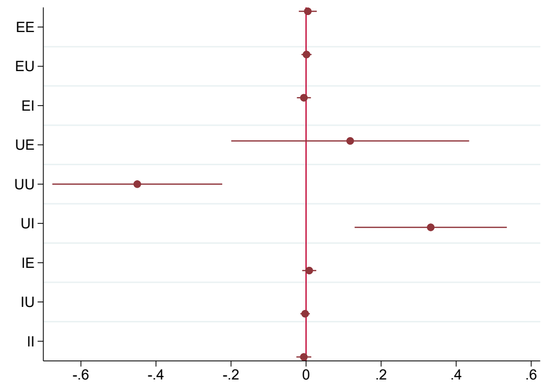
(f) By time in unemployment

Notes: The figure plots point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). These results are shown for different outcomes of interest, which are displayed on the y axis as the types of transitions from and to employment (E), unemployment (U) and inactivity (I). The different panels present separate sets of results by sex (Panel A), age group (i.e. above or below 40 years old, Panel B), educational attainment (i.e. less than college degree or more, Panel C), presence of children in the household (Panel D), family income (below or above 50,000 USD, Panel E) and time spent in unemployment (more or less than three months, Panel F).

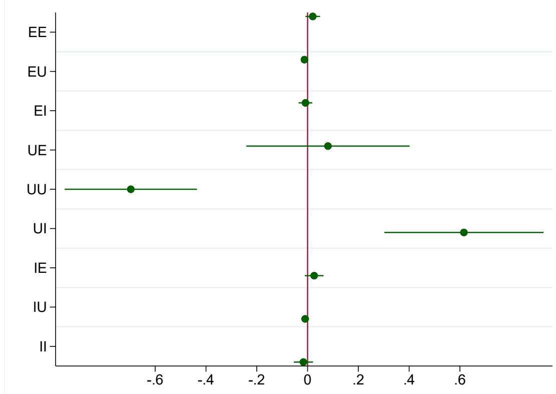
Figure B4: Heterogeneous treatment effects by racial and ethnic group, sample of likely UB recipients



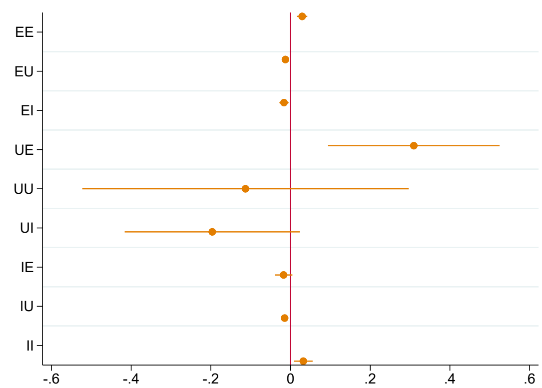
(a) White



(b) Black



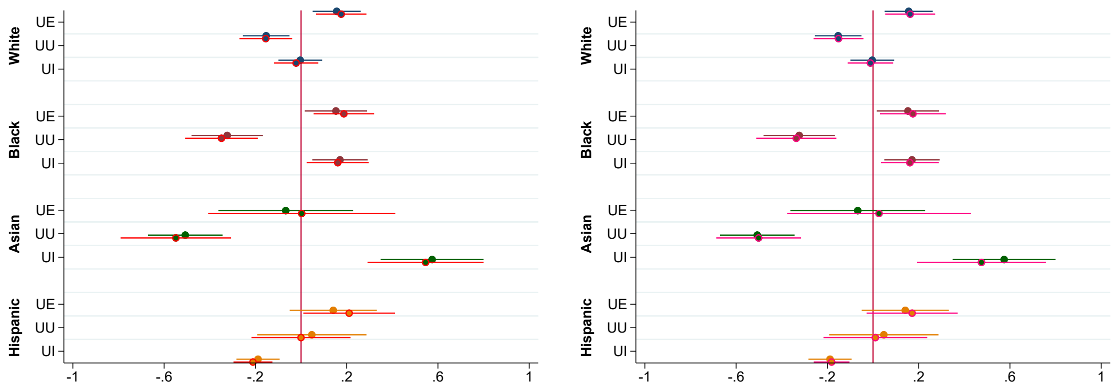
(c) Asian



(d) Hispanic

Notes: The figure plots point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). These results are shown for different outcomes of interest, which are displayed on the y axis as the types of transitions from and to employment (E), unemployment (U) and inactivity (I). The different panels present separate sets of results for White (Panel A), Black (Panel B), Asian (Panel C) and Hispanic (Panel D). Differently from Figure 2 in the main text, results here are obtained only for the sample of individuals who are unemployed since less than 26 weeks and did not quit their job voluntarily.

Figure B5: Treatment effects by racial and ethnic group, different specifications

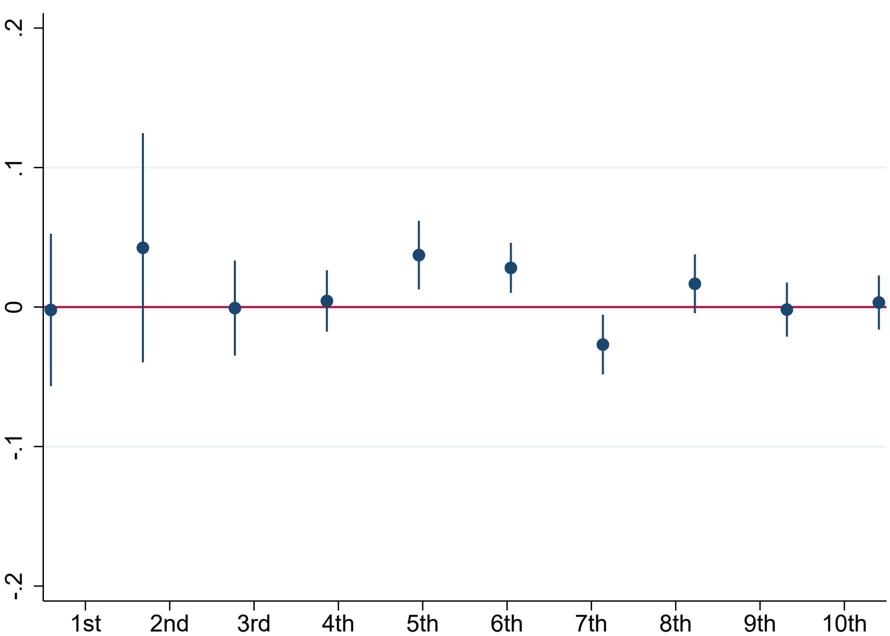


(a) With and without industry dummies

(b) With and without occupation dummies

Notes: The figure plots point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). For ease of exposition, we are presenting only results for the three transitions out of unemployment. Panel A presents the baseline results (with no outline around the marker) and the results that we obtain when adding controls for the industry of employment at the one-digit level. Panel B presents the baseline results (with no outline around the market) and the results that we obtain when adding controls for the occupation of employment at the one-digit level. Information on the industry and occupation of employment refers to the last job held, and is available only for individuals with prior work experience. For inactive individuals (to whom the question on the previous occupation or industry of employment is not asked), we use the information provided when they were unemployed, if this is available.

Figure B6: Treatment effects on transitions from unemployment to jobs in different deciles of the income distribution



Notes: The figure plots point estimates and 90% confidence intervals for the coefficient β_3 in equation (1). These results are shown for different outcomes of interest, which are the transitions from unemployment to jobs in different deciles of the income distribution. The analysis is conducted only for wage and salaried workers in the CPS ORG sample. These individuals are assigned to a specific wage decile based on the imputation procedure described in the text.