Employment and Consumption Responses to the Withdrawal of Unemployment Benefits*

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Abstract

We study the responses to the withdrawal of two generous unemployment benefit (UB) schemes introduced in the US during the COVID-19 pandemic, exploiting variations across states in the timing of the policy change. Using data from the Current Population Survey, we find that the expiration of UBs increased unemployment-to-employment transitions. However, approximately half of this effect was driven by job recalls. We also find evidence that unemployed individuals transitioned into lower quality jobs, compared to their previous occupations, and that young job seekers not eligible for UBs were displaced by higher job-search competition. Using both survey and transaction data, we also provide complementary evidence on the consumption effects of the policy change. We document a small reduction in consumption after the withdrawal of pandemic UBs for some non-essential spending categories.

Keywords: Unemployment Benefits, Labor Supply, Consumption
JEL Codes: J45, J65, J68

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1 Introduction

What are the consequences of the withdrawal of unemployment benefits (UBs)? Specifically, when unemployment support is diminished with little advance notice, how likely are unemployed adults to return to employment? Which types of workers transition to employment, and which types of jobs do they return to? And what are the broader consequences for affected individuals' consumption capabilities? We study these questions in the context of a sudden withdrawal of generous UB support in the United States (US) in 2021.

During the COVID-19 pandemic, governments initiated a series of policy initiatives to rapidly respond to the crisis, which often resulted in abrupt and drastic changes in public support for affected individuals.[1] However, the literature has mostly focused on studying the effects of exogenous variations in policy generosity within a constant policy regime (e.g. age based eligibility criteria determining different benefit levels) or, alternatively, the effects of reforms that are announced in advance and gradually implemented.

The effects of a sudden benefit withdrawal, however, may have different consequences than the gradual phase-out of a benefit, particularly given that affected individuals will have less time and capacity to anticipate the effects of the policy change. Thus, while past research has studied the employment and consumption responses to the gradual and/or anticipated draw back of UBs, we investigate the direction and magnitude of employment responses of state governments' rapid withdrawal of benefits.[2] Moreover, we seek to understand consequences for the quality of job matches and consumption.

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. The two interventions were set to expire in September 2021. However, 18 US states announced in May that they would terminate both PUA and FPUC in June 2021. We exploit the differential timing of the policy change across states to study the short-term effects of this sudden policy change on

[1] These emergency interventions have also become increasingly popular to respond to a large range of shocks in both developed and developing economies, from inflationary pressures to natural disasters.

[2] An important clarification refers to the fact that, while the policy change was indeed sudden and largely unanticipated (see below for details), the overall policy environment was rather volatile during the COVID-19 pandemic. This means that individuals, while not necessarily foreseeing this exact policy change, might have reacted differently compared to what would have happened under a more standard policy context.

labor market and consumption behaviors. We test the validity of our identification strategy by relying on a large variety of administrative and survey data.

Our findings contribute to two main debates in the literature. First, we complement studies that analyzed the impact of the UB schemes introduced in the US during the COVID-19 pandemic. An early wave of papers has examined the effects of their phase-in in early 2020, finding large consumption gains and little disincentive effects on labor supply (Bachas et al., 2020; Boar and Mongey, 2020; Garza Casado et al., 2020; Marinescu et al., 2021; Petrosky-Nadeau, 2020). However, studying policy withdrawal can deliver different insights compared to the analysis of the policy introduction, especially since this happened at the onset of the pandemic.[3] Fewer contributions have examined the effect of the expiration of PUA and FPUC, generally finding an increase in employment levels (Arbogast and Dupor, 2022; Coombs et al., 2022; Ganong et al., 2022; Holzer et al., 2021). We complement these studies by conducting a more detailed analysis of the labor market effects of the withdrawal of PUA and FPUC (e.g. analyzing which types of unemployed individuals transition to employment and what types of jobs they take), and studying consequences for consumption using different data sources (i.e. both transaction and survey data).

Second, we contribute to a classical debate in the labor economics literature, focused on the welfare effects of UBs. Following the standard framework proposed by Baily (1978) and Chetty (2006), these are defined as the difference between their insurance value and efficiency costs. While multiple estimates exist in the literature on the efficiency costs of UBs, relatively little is known with respect to their insurance value (for a review, see Schmieder and von Wachter (2016)). We complement existing studies in two ways. First, we jointly analyze the employment and consumption responses to UB withdrawal within the same setting, something that is rarely done in previous studies. Second, we analyze these questions in the context of a sudden and unanticipated policy change. This is different from the standard setting examined in traditional UB papers, which focus on either exogenous changes in benefit generosity within a constant policy framework, or study the effects of policy reforms that are announced long in advance and/or gradually implemented.

[3]For instance, lack of negative employment effects from the introduction of PUA and FPUC was explained by the exceptionally low labor market tightness that characterized the early phase of the pandemic, also related to the mobility restrictions that were in place to curb the spread of the virus.

2 Policy change and institutional context

During 2020 and 2021, the US 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 lengthen maximum benefit duration.

We study changes to the PUA and FPUC, which were first introduced to the federal UB scheme in March 2020. PUA provided access to UBs to individuals who had lost their job for COVID-related reasons and were not eligible for regular UBs, either because they did not qualify or because they 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. FPUC consisted instead of a weekly federal supplement to state-level UBs. This was set at $600 per week, which had potential to replace 100% of average wages when added to state-level benefits. 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., 2020).

FPUC initially ended in July 2020, but was reinstated at $300 in March 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 both programs until its original expiration in September, while the remaining eight states (i) ended only FPUC but not PUA in June 2021 (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). These eight states will not be included in the main analysis, but we will use states in group (i) above for some additional analysis.[4]

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

[4]The reason for not including these eight states as part of our baseline findings relates to the obstacles of extending our main identification strategy to cover also these specific cases (see below in Section 4 for details). Accordingly, previous contributions that have examined the same policy change have adopted the same sample restrictions (Coombs et al., 2022; Holzer et al., 2021).

concerns might have reflected differences across states in the evolution of the economic recovery, there is evidence that they were also politically motivated.[5] In particular, there is no Democratic state in our sample that withdrew early from both PUA and FPUC. At the same time, almost all Republican states in our sample withdrew early from both interventions, with the exceptions of Massachusetts and Vermont (see Appendix Figure B1).

The political nature of the early withdrawals is also confirmed in a simple regression framework. 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). This is in line with the fact that early- and late-withdrawal states were on parallel employment trends before the policy change.

Appendix Table A2 reports, for all US states, their policy treatment status (i.e. early withdrawal, late withdrawal or outside of the analysis). For early-withdrawal states, it also reports information on the policy announcement date as well as on the date in which the policy change came into effect. We note a the median lag between the policy announcement and the policy change of 40 days. This highlights the difference between this policy change and other reforms that are traditionally investigated in empirical studies. In particular, the short time frame between the policy announcement and the policy change should reduce the risk of any anticipatory effects (i.e. individuals exiting the program after the policy announcement, but before the end of their eligibility).

Notably, the withdrawal of UBs led to abrupt declines in income support for unemployed individuals: in early-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).[6]

[5]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).

[6]While these policy-induced changes in UBs are large, they need to be considered in light of the extremely volatile policy context that characterized the different phases of the COVID-19 pandemic, when a number of social protection benefits were rapidly expanded at the onset of the crisis, and then reduced (or withdrawn)

3 Data

This section presents the data sources used for estimating the employment responses to the policy change, as this represents the main component of the paper. The data sources used for the consumption analysis will be introduced directly in Section 6.

Our primary data source for studying the employment effects of the withdrawal of PUA and FPUC is constituted by the monthly CPS files obtained from the Integrated Public Use Microdata Series (IPUMS) (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 months. We exploit the panel structure of the CPS to study short-run labor market effects, restricting the sample to individuals who have completed all interview rounds.[7] We further restrict the sample to individuals in the working age population (aged 15 to 64) and in our baseline specification we focus our attention on the period between February and August 2021 (i.e. from just before FPUC was reinstated to the time in which both PUA and FPUC expired also in late-withdrawal states). Appendix Table A3 presents descriptive statistics for our sample as just described, overall as well as differentiating between early and late withdrawal states. It shows that the two groups of states are relatively balanced in terms of individual and household characteristics (e.g. age, sex, marital status, racial composition). However, late-withdrawal states have a higher share of individuals with at least a college degree and a lower share of households with an annual income below $50,000.

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 to cope with the consequences of the pandemic.

We also use information from the America Time Use Survey (ATUS) provided by IPUMS. As the CPS, the ATUS is conducted by the US Census Bureau. Eligible individuals are those who have completed all eight CPS interview rounds, and they are interviewed

in its aftermath.

[7] We let individuals complete their eight interviews at any point in time until August 2022, when our data ends. Given how the CPS interview rounds work, there is no new observation entering the sample for the first time after May 2021. While our baseline results are obtained with individuals who have completed all interview rounds, we will also test how results change if we relax this restriction.

between two to five months after their last CPS interview. The ATUS collects information on the amount of time people spend doing various activities, and we will be interested in particular in measuring job-search efforts.

Finally, to check the validity of the identification strategy, we will use two additional data sources. The first one is the Employment and Training Administration (ETA) reports prepared by the US Department of Labor, which contain information on the flows and stock of UB recipients (by type of UBs received) at the state and monthly level. The second one refers to survey data from the Understanding America Survey (UAS), which, unlike the CPS, reports individual-lvel information on actual UB receipt.

4 Identification

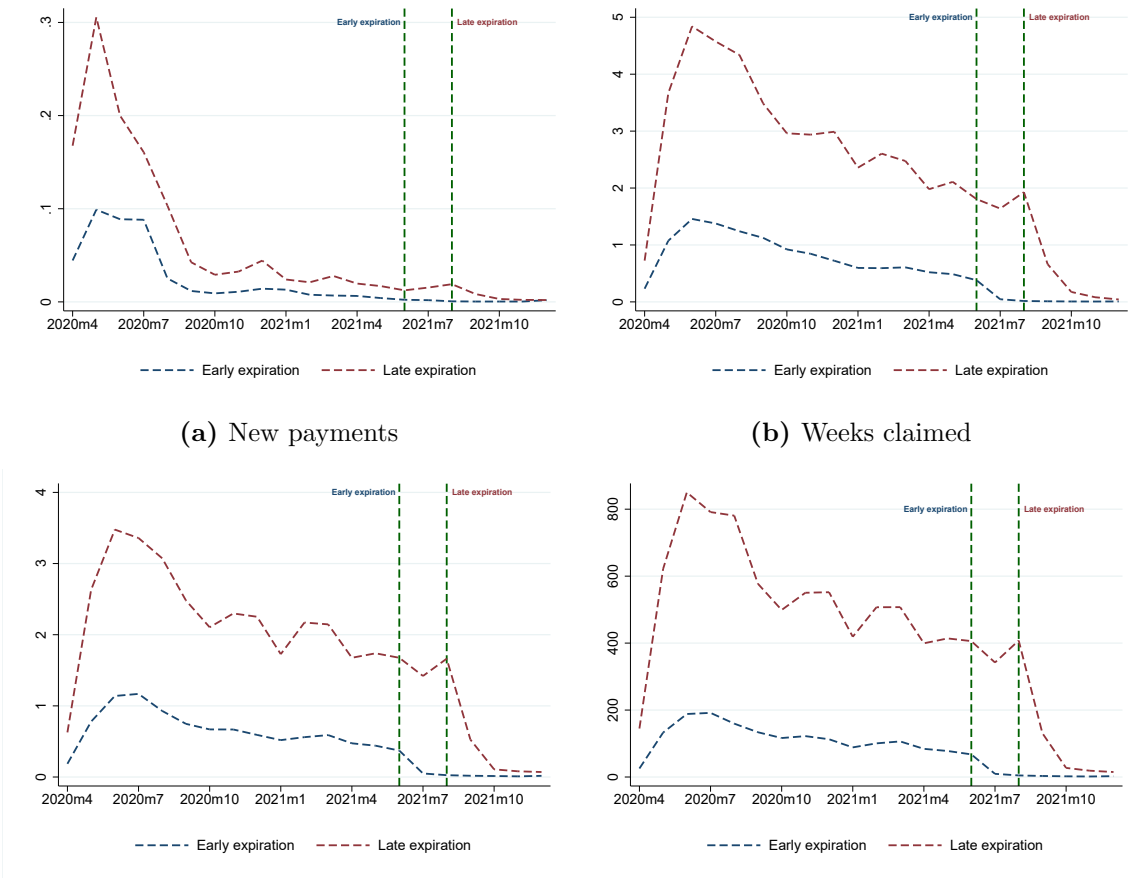
We exploit cross-state variation in the timing of PUA and FPUC expiration in a standard difference-in-differences (DiD) setting. Identification requires that early- and late-withdrawal states would have been on parallel trends in the outcomes of interest absent the policy change. We provide supportive evidence for this identifying assumption in this section.

Figure 1 draws on data from the ETA reports on PUA recipients published by the Department of Labor. Data is available at the monthly and state level, and we normalized it by the state-level population size in 2021. The Figure shows that the number of individuals receiving their first PUA payment (Panel A) sharply increases at the beginning of the pandemic. However, by the time of the policy change, the number of new entrants was relatively low and trending in a similar way between early- and late-withdrawal states. As expected, the differential timing of the policy change leads the number of new entrants to remain higher for a few additional months in late-withdrawal states, but the effect was generally small, as few individuals were joining PUA for the first time in the summer of 2021.[8]

A different picture emerges when looking at the evolution of different indicators of the stock of UB recipients. In particular, the number of weeks benefits were claimed (Panel B), the number of weeks benefits were paid (Panel C) and the total amount of benefit received (Panel D) all present a clear drop exactly at the time of the policy withdrawal (i.e. in July 2021 for early-withdrawal states and in September 2021 for late-withdrawal states). After the policy withdrawal, all these indicators rapidly converge to zero and remain flat.

[8]Panel A reports information on the "number of payments which represent, for any individual, the first week for which assistance is paid". There can be some delay between the time of the policy expiration and the time in which this indicator goes to zero (e.g. retrospective payments, late applications).

Figure 1: Trends in UB claims in early- and late-withdrawal states, 2020-2022



(a) New payments

(b) Weeks claimed

(c) Weeks paid

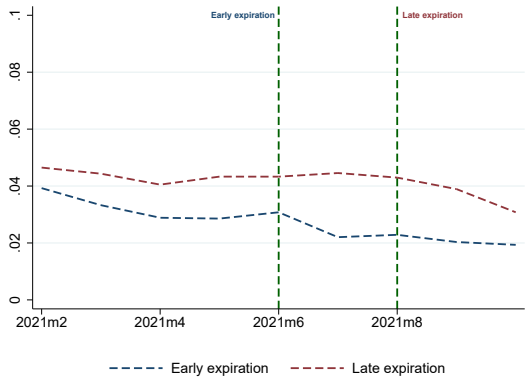
(d) Amount paid

Notes: The figure reports data from the ETA reports collected by the Department on Labor on PUA recipients. Data is reported at the monthly and state level and we have normalized it by population size in 2021. Panel A reports the number of individuals receiving their first PUA instalment in each month, Panel B the number of weeks of benefits that have been claimed in each month, Panel C the number of weeks for which benefits have been paid in each month, Panel D the total amount of benefits that have been paid in each month. In each case, we group states according to whether they ended PUA and FPUC early (blue line) or late (red line).

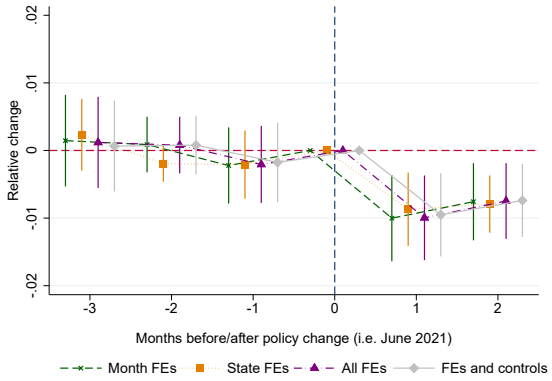
As a following step, we check whether there were any other changes in the functioning of the UB scheme around the time of the policy change. Even in this case, we rely on ETA reports (Appendix Figure B2). Panel A shows that the total number of regular UB recipients (i.e. those claiming regular UBs, rather than special programs such as PUA and FPUC) was trending downwards in both treated and control states, with no signs of discontinuity. Similarly, Panel B shows that the number of recipients of other types of special UB programs, such as the Disaster Unemployment Allowance program, was similar in treated and control states.[9] Finally, trends in the number of UB frauds and appeals were similar in

[9]This is activated in case of major disasters (e.g. hurricanes) and it supports dependent employee and self-employed whose employment was lost or interrupted, and who are not eligible to regular UBs.

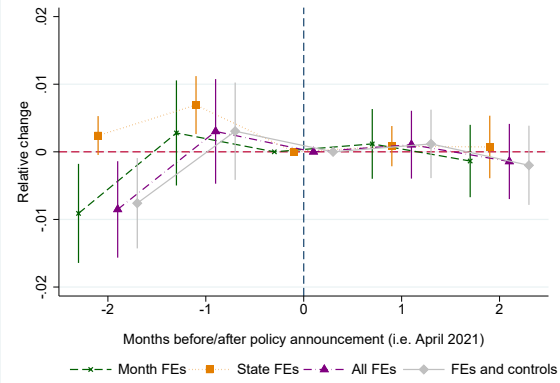
Figure 2: Validating the identification assumptions using survey data



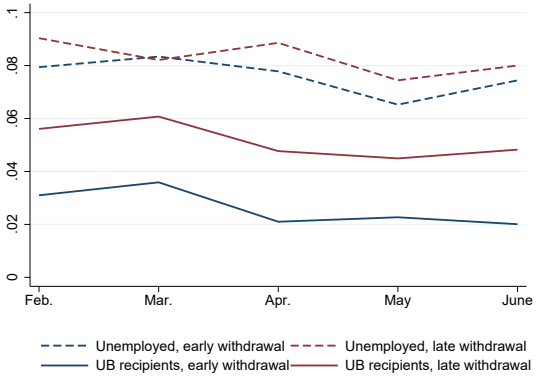
(a) Share of unemployed in treated and control states in the CPS



(b) Effects on the probability of being unemployed, centred around policy change (control and treated states)



(c) Effects on the probability of being unemployed, centred around policy announcement (only treated states)



(d) Unemployed and UB recipients in UAS data

Notes: Panel A reports the share of unemployed individuals in treated and control states around the time of the policy change, computed using the CPS sample that will be employed in the analysis. Panel B reports the results of a series of event study regressions centred around June 2021 (i.e. which corresponds to month 0 on the x-axis), where the dependent variable is the probability of being unemployed and the different specifications differ with respect to the types of covariates included. Treatment corresponds to the early-withdrawal of PUA and FPUC, and the analysis includes observations from both treated and control states (i.e. states that kept both PUA and FPUC until September 2021). Results differ on whether they include only month fixed effects (green line), only state fixed effects (orange line), both month and state fixed effects (purple line), and also individual-level controls (grey line). Panel C reports results of another event study regressions, where the sample is restricted to early-withdrawal states. The analysis is centred around the time of the policy announcement in the three states (i.e. Arkansas, Montana and South Carolina) that announced the early expiration of PUA and FPUC before the May 2021 CPS reference week. This means that April 2021 corresponds to month 0 on the x-axis, and we observe effects for two months between policy announcement and policy implementation. These three states are considered treated in this case, and all the other early-withdrawal states form instead the control group. As in panel B, the different specifications differ with respect to the types of covariates included. Finally, Panel D reports the share of unemployed individuals and UB recipients in treated and control states before the policy change (i.e. from February to June 2021) using data from the UAS.

treated and control states, and trending in parallel (Panels C and D).

We now validate these conclusions using the individual-level data that will be employed in the analysis (Figure 2). This is important as the CPS does not report information on UB receipt and this will be proxied by unemployment status. To start, we plot the evo-

lution of unemployed individuals in the CPS data in treated and control states around the time of the policy change (Panel A). We observe that this is very similar to the evolution of UB recipients as reported in the data from the Department of Labor. Additionally, we observe that the number of unemployed individuals declines in early- and late-withdrawal states exactly after the policy change, as also documented above. This is also confirmed in an event study analysis (Panel B). The figure presents results from different specifications (i.e. with only month dummies, with only state dummies, with both month and state dummies, adding individual-level controls), where the dependent variable is the probability of being unemployed and we compare treated and control states around the time of the policy change (i.e. June 2021 corresponds to month 0 in the Figure).

We also exploit the fact that the employment information provided in the CPS refers to a specific reference week (rather than the entire month), and that early-withdrawal states differed with respect to the time in which they announced the policy change. Only Arkansas, Montana and South Carolina announced the termination of PUA and FPUC before the May 2021 CPS reference week (Appendix Table A2).[10] We can conduct an event-study analysis centered around May 2021, using these three states as the treatment group and the other early-withdrawal states as control. This exercise limits the analysis to early-withdrawal states (that might have been on more similar employment trajectories compared to late-withdrawal states) and exploits only variations in announcement dates. If individuals were reacting to the policy announcement, we would expect a decrease in the probability of being unemployed already before the policy change. However, Panel C of Figure 2 shows very similar unemployment trajectories around the policy announcement.

Finally, we turn to the UAS, which reports information on both UB receipt and unemployment status. Panel D of Figure 2 reports the share of unemployed individuals and the share of UB recipients in treated and control states. The figure shows that both the number of unemployed individuals and the number of UB recipients was higher in late-withdrawal compared to early-withdrawal states, consistent with findings presented above. Additionally, the figure also confirms that trends in both unemployment and UB receipt were similar between treated and control states before the policy change.

[10] All other early withdrawal states announced the policy change after the CPS May reference week (9-15 May 2021), but before the June reference week (6-12 June 2021). In all early-withdrawal states, the policy change took place after the June CPS reference week,but before the July reference week.

5 Consequences for Employment Transitions, Levels, and Quality

The baseline equation for the labor market analysis will take the following form:

$$Y_{ist} = \alpha + \beta Early_s * Post_t + \gamma X_{ist} + c_s + t_t + \epsilon_{ist} \quad (1)$$

Y_{ist} is the outcome of interest for individual i, living in state s at time t. We will look at treatment effects on the probability of being employed, unemployed or outside of the labor force as well as on transitions across labor market statuses and on the use of different job-search methods. X_{ist} is a vector of individual-level controls. In the most complete specification, we control for age, sex and dummies for educational attainments, marital status, racial and ethnic group and presence of children in the household. c_s and t_t are a full set of month and state fixed effects.

$Early_s$ is a dummy for whether the state terminated both PUA and FPUC in June 2021 rather than in September 2021 (see the list of states in Appendix Table A2). $Post_t$ is a dummy equal to one during the period of the policy change, and zero otherwise. We present baseline results with two different definitions of this dummy, which take the value of one (i) between July and August 2021, and (ii) between May and August 2021.[11] In the first definition of the dummy variable, we are capturing only the months during which the two groups of states presented differential policy regimes. In the second definition, we instead also include the months between the policy announcement and the policy introduction.

The coefficient of interest in our DiD setting is the one of the interaction term between the early state status and the post policy dummy, corresponding to β in the equation above. Standard errors are clustered at the state level, where policy variation happens. In all specifications, we will use the standard person-level weights for the monthly CPS files.

Effects on Employment Levels and Transitions

Table 1 presents the 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 results of the DiD interaction term separately for the two definitions of the $Post_t$ dummy introduced above for defining the treatment period (July to August 2021 in Panel A or May to August 2021 in Panel B).

[11] The pre-policy period, when this dummy takes the value of zero, starts instead always in February 2021.

Table 1: Baseline results on labor market transitions

	EE	EU	EI	UE	UU	UI	IE	IU	II
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: Treatment period starting in June and ending in August								
$\hat{\beta}$	0.006*	-0.002	-0.004*	0.164**	-0.144**	-0.020	-0.015*	-0.005	0.019**
	(0.003)	(0.002)	(0.002)	(0.066)	(0.063)	(0.022)	(0.008)	(0.005)	(0.008)
Mean of y	0.972	0.01	0.019	0.235	0.559	0.205	0.053	0.036	0.91
R-squared	0.016	0.005	0.014	0.030	0.041	0.039	0.015	0.012	0.022
	Panel B: Treatment period starting in May and ending in August								
$\hat{\beta}$	0.002	0.000	-0.002	0.049	-0.058**	0.009	-0.001	-0.002	0.004
	(0.003)	(0.001)	(0.003)	(0.030)	(0.026)	(0.030)	(0.006)	(0.003)	(0.007)
Mean of y	0.973	0.01	0.017	0.235	0.561	0.204	0.052	0.035	0.913
R-squared	0.016	0.005	0.014	0.028	0.040	0.039	0.015	0.012	0.022
Observations	86,396	86,396	86,396	4,519	4,519	4,519	32,227	32,227	32,227
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 from Equation (1) 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 analysis is conducted between February and August 2021. In Panel A, the post-policy indicator takes the value of one in July and August 2021. In panel B, the post-policy indicator takes the value of one between May and August 2021. 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.

Results in Panel A 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 (0.164) 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 (0.235). Coefficients for all other types of transitions are instead around zero, in line with the fact that the expiration of pandemic UBs affects only transitions out of unemployment.

Results in Panel B move in the same direction, but are smaller in magnitude for both the unemployment-to-employment and the unemployment-to-unemployment transitions. This is in line with the fact that there were no anticipatory effects. As a result, treatment effects are diluted when including a period of time during which individuals did not modify their behavior. From now on, we will only present results obtained with the definition of the $Post_t$ dummy that takes the value of one from July to August 2021.

We present event study estimates for the different types of labor market transitions

in Appendix Figure B3, for a period that spans from six months before the policy change to 12 months afterwards. The results confirm that, for all outcomes of interest representing all possible transitions by labor market status, pre-treatment trends were parallel in the period before the policy change. Additionally, the effects of the policy on the two types of transitions for which we see statistically significant effects (i.e. unemployment-to-employment and unemployment-to-unemployment) are short-lived, as coefficients are statistically significant only for the two months in which the two groups of states presented different policy regimes (i.e. July and August 2021). After that, trends in the outcomes of interest return parallel between early- and late-withdrawal states, in line with the fact that treated and control states presented differential policy regimes only for two months.

Which types of unemployed individuals transitioned to employment? We look at transitions from different forms of unemployment to employment. In particular, the CPS differentiates between unemployed individuals that (i) were temporarily laid-off (i.e. they expect to be called back from their employer within six months at the latest), (ii) were permanently laid-off, (iii) whose fixed-term contract expired, (iv) quit their job autonomously, (v) were re-entering the labor force from inactivity, or (vi) were entering the labor market for the first time. Appendix Figure B4 shows the distribution of unemployed across these categories between 2019 and 2022, showing the increase in the share of individuals who were temporarily laid-off at the beginning of the pandemic.

Using this categorization, we see that half of the unemployment-to-employment transitions that took place as a result of the exhaustion of PUA and FPUC concerned individuals who were temporarily laid off from their enterprise ("TL to E" in Table 2). Instead, we do not find that individuals who were unemployed due to permanent dismissal from their job transitioned more rapidly into employment ("PL to E"). Rather, we see a similar increase in transitions into employment from individuals whose fixed-term contract had expired ("FT to E"), had voluntarily quit their job ("Q to E"), and had re-entered the labor force after a spell into inactivity ("RE to E"). These three groups of transitions, taken together, account for as much as the transitions of temporarily laid-off individuals who move to employment. This means that around half of the transitions into employment that took place at the expiration of PUA and FPUC was "mechanical" (i.e. it was driven by enterprises recalling their workers) and half of it was instead "behavioral" (i.e. it was driven by individuals accelerating their re-entry in employment when UBs expired).

Instead, we find negative effects on transitions from unemployment to employment

Table 2: Transitions from different types of unemployment to employment

	TL to E (1)	PL to E (2)	FT to E (3)	Q to E (4)	RE to E (5)	FE to E (6)
$\hat{\beta}$	0.095**	0.002	0.031***	0.035**	0.030*	-0.028***
	(0.044)	(0.019)	(0.011)	(0.017)	(0.017)	(0.010)
Mean of y	0.122	0.048	0.015	0.013	0.033	0.004
Observations	4,519	4,519	4,519	4,519	4,519	4,519
R-squared	0.039	0.022	0.019	0.029	0.039	0.049
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 from Equation (1) from separate regression models, where the dependent variables are transitions from different forms of unemployment to employment. In particular, "TL to E" denotes transitions from unemployment due to temporary layoff to employment; "PL to E" denotes transitions from unemployment due to permanent layoff to employment, "FT to E" denotes transitions from unemployment due to the expiration of a fixed-term contract to employment, "Q to E" denotes transition from unemployment due to voluntarily quit the job to employment, "RE to E" denotes transitions from unemployment for individuals who had re-entered the labor market to employment, "FE to E" denotes transitions from unemployment for individuals who had entered the labor force for the first time to employment. Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. The analysis is conducted between February and August 2021, and the post-policy indicator takes the value of one in July and August 2021. 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.

among individuals who were in the labor market for the first time ("FE to E"). It is reassuring that we do not see an increase in the unemployment-to-employment transitions for these individuals, given that these were likely not eligible to UBs. The negative effect we find for this group could instead be explained by congestion in the labor market, where less experienced unemployed individuals are penalized by the competition of more experienced job seekers that rush to new jobs after the expiration of pandemic UBs. This is consistent with models where jobs are rationed, and increasing job-search efforts among unemployed does not lead to an overall increase in employment (Michaillat, 2012).

Appendix Figure B5 presents the event study estimates for the six different types of unemployment-to-employment transitions presented in Table 2. The results show that the parallel trend assumption is likely to be satisfied also with these outcomes of interest, and that treatment effects tend to be concentrated around the time of the policy change.

We also look at the effects of the policy change on overall employment, unemployment and inactivity, which is helpful to quantify the impact of the reform in terms of aggregate labor market stocks. We see an increase in employment in states that withdrew

Table 3: Baseline results on labor market status

	Employed			Unemployed			Inactive		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{\beta}$	0.027***	0.026***	0.024***	-0.010***	-0.010***	-0.009***	-0.017**	-0.016**	-0.014**
	(0.008)	(0.008)	(0.006)	(0.004)	(0.003)	(0.003)	(0.008)	(0.008)	(0.007)
Mean of y		0.698			0.041			0.257	
Observations	155,664	155,664	155,664	155,664	155,664	155,664	155,664	155,664	155,664
R-squared	0.000	0.008	0.134	0.001	0.004	0.012	0.000	0.007	0.134
State and time FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Personal controls	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The table presents coefficient estimates and standard errors for β from Equation (1) from separate regression models, where the dependent variables are the different statuses in the labor market. Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. The analysis is conducted between February and August 2021, and the post-policy indicator takes the value of one in July and August 2021. 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.

PUA and FPUC earlier, and a parallel decrease in both unemployment and inactivity (Table 3). In particular, in the most complete specification, employment increases by 2.4 percentage points (corresponding to an increase of 3.4% compared to the baseline value in the control group before the policy change). This is accounted for by an almost equivalent decrease in unemployment (by 1 percentage point, or 22% of the baseline value) and inactivity (by 1.4 percentage points, or 5.4% of the baseline value).

In order to compare our results to those from previous studies, we compute elasticity estimates. However, this requires focusing on a sub-population of the treatment group for which we have a precise understanding of the size of the policy change. These are individuals who lost their jobs between May and June 2021. Given the functioning of the policy as described in Section 2, this group of UB recipients did not lose UB eligibility in July 2021 (in either early- or late-withdrawal states), but only lost eligibility to the additional $300 provided by the FPUC. Coombs et al. (2022) estimate that this translated into a log point reduction in average benefits between 0.85 and 0.5. Estimating treatment effects for this group, we find that employment increased by around 14.6 per cent following the policy change. These parameters lead to elasticity estimates that range between 0.17 and 0.29.

As a matter of comparison, the median elasticity estimate in the review by Schmieder and von Wachter (2016) for studies on the effects of UB generosity in the US is equal to 0.38 (range from 0.1 to 1.2). We thus interpret our findings (at least those related to the expiration of the $300 supplement) as lower bounds of those generally found in the literature. The relatively large aggregate response in terms of labor market transitions documented above is

thus driven by the large policy change that took place in July 2021, rather than by a large behavioral response to it. This is in line with studies from the beginning of the pandemic, which have found that the introduction of generous UB schemes did not generate large labor supply disincentive effects, while contributing to poverty alleviation (Ganong et al., 2022; Marinescu et al., 2021; Petrosky-Nadeau, 2020).

We next look at heterogeneous effects by plotting event study estimates on the probability of being employed (Figure B6). We find employment gains only among men (panel A), highly educated workers (panel C), people without children living in the household (panel D) as well as low-income workers (panel E). Overall, this suggests that the groups that reacted more to the policy change were either individuals with higher attachment to the labor market (e.g. men, highly-educated individuals as well as individuals without children) and/or those with a stronger risk of out-of-work poverty (e.g. low-income households).[12] Results are instead similar among individuals above and below the age of 40 (panel B). At the same time, we cannot reach any conclusion on differences in treatment effects by race: employment increases more for non-white individuals following the expiration of UBs, but this group also shows a mild positive pre-treatment trend (panel F).

Finally, while our main analysis defined treatment as the combined effect of the expiration of both PUA and FPUC, it is useful to shed light on which specific policy led to the observed effects. We exploit the fact that four states (i.e. Alaska, Arizona, Florida and Ohio) ended only FPUC in June 2021, while leaving PUA until the original end date. In these states, UB recipients were thus subject to a large cut in benefits, but their UB eligibility period was unaffected. We use observations in these four states as the treatment group and compare them with observations in the late-withdrawal states. Appendix Table A4 shows that the results are very similar to those in our baseline findings when it comes to the increase in unemployment-to-employment transitions. However, this seems to be driven by a decrease in unemployment-to-inactivity transitions (rather than a reduction in the probability of remaining unemployed, as it happens in the baseline results).[13]

Effects on Job Quality Outcomes

[12]It is also important to note that, as documented above, many unemployment-to-employment transitions taking place at the expiration of the policy likely involved job recalls. This might explain why certain groups (e.g. highly educated) were more likely to quickly move to employment.

[13]This is consistent with the fact that UB recipients did not lose their eligibility in these four states, hence had more limited incentives to move out of unemployment. However, we also note that the number of observations becomes quite small when looking at unemployed individuals in this group of treated states after the policy change, hence we see this only as suggestive evidence.

How did the policy change affect job quality outcomes? We examine 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, to group three-digit occupations along two main dimensions: (i) routine or non-routine occupations, and (ii) manual or cognitive occupations. We rely on the fact that information on occupation is available in the CPS also for unemployed individuals (provided that they had some previous work experience), and look at the effects of PUA and FPUC expiration on unemployment-to-employment transitions by occupations.

Before presenting the results, it is useful to provide a concrete understanding of working conditions in these occupations. In particular, the median weekly wage was substantially higher in non-routine compared to routine jobs ($1,120 compared to $800) as well as in cognitive compared to manual occupations ($1,130 compared to $751). Accordingly, non-routine and cognitive occupations attract on average more qualified workers. This means that, while different mechanisms can be at play, we can interpret transitions across occupation types in terms of transitions across jobs of different quality.

Starting from the transitions from and to routine and non-routine occupations (upper panel of Table 4), the results show that most unemployment-to-employment transitions that took place in treated states after the expiration of PUA and FPUC concerned movements within the same occupational category (e.g. unemployed individuals previously employed in routine occupations find a new job in routine occupations).[14] A slightly different picture emerges when examining the results by occupation defined based on the cognitive or manual nature of the job (lower panel of Table 4). There, we see that the transitions from unemployment to employment still involve mostly movements within the same occupational category. However, there is also an increase in transitions of unemployed individuals who were previously employed in cognitive occupations and take-up jobs of manual nature. Detailed analysis conducted by overlapping the routine/non-routine nature of the job with its cognitive/manual content shows that this is driven by transitions from routine cognitive jobs to routine manual jobs (Appendix Figure B7).

Appendix Figure B8 presents the event study estimates for the different types of

[14]These outcome variables are defined only for the population of unemployed individuals at $t-1$, irrespective of their employment status at t. As a result, coefficient estimates in each panel do not need to sum to zero (as it would happen if instead we would limit the sample to only individuals who had transitioned from unemployment at time $t-1$ to employment at time t). Rather, the sum of the coefficient estimates will be positive, in light of the fact that we have documented an increase in unemployment-to-employment transitions at the expiration of pandemic UBs (see Table 1).

Table 4: Unemployment-to-employment transitions by occupational category

	Routine to routine	Routine to non-routine	Non-routine to routine	Non-routine to non-routine
	(1)	(2)	(3)	(4)
$\hat{\beta}$	0.146***	-0.011	-0.012	0.078***
	(0.047)	(0.012)	(0.015)	(0.027)
Mean of y	0.095	0.022	0.023	0.088
Observations	4,519	4,519	4,519	4,519
R-squared	0.046	0.019	0.018	0.033
State and time FE	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes
	Cognitve to cognitive	Cognitive to manual	Manual to cognitive	Manual to manual
	(5)	(6)	(7)	(8)
$\hat{\beta}$	0.079***	0.039**	0.021	0.062
	(0.028)	(0.018)	(0.017)	(0.040)
Mean of y	0.077	0.022	0.021	0.107
Observations	4,519	4,519	4,519	4,519
R-squared	0.051	0.028	0.018	0.057
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 from Equation (1) from separate regression models, where the dependent variables are unemployment-to-employment transitions by broad occupational groups. The upper panel looks at transitions to and from routine and non-routine occupations while the lower panel examines transitions from and to cognitive and manual occupations. In all cases, these transition variables are constructed for the sample of individuals who are unemployed at time $t-1$, irrespective of their employment status at time t. This means that these outcomes of interest will take the value of one if an unemployed individual at $t-1$ has transitioned to a job of a given occupation type at time t. Individual-level controls include age, sex and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. The analysis is conducted between February and August 2021, and the post-policy indicator takes the value of one in July and August 2021. 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.

unemployment-to-employment transitions by occupational category. The coefficient of the interaction terms between treatment status and the months before the policy change are, in most circumstances, statistically non-significant.

This last set of results on occupations are compatible with both a labour supply interpretation (i.e. individuals reacting to the withdrawal of pandemic UBs by accepting lower quality jobs) as well as a labour demand interpretation (i.e. changes in the types of jobs requested as a result of economic transformations after the pandemic). While it is impossible to disentangle between these two hypotheses with the available data, we examine these aspects by looking at which specific occupations are involved in the increase in transitions from cognitive to manual jobs documented above. In particular, we find that this is driven by an increase in transitions (i) from managerial to service occupations, and (ii) from office and administrative to transport occupations (Appendix Figure B9). This might suggest that

both labor demand and labor supply mechanisms are at play.[15]

To end this section, we present complementary evidence of the effects of the expiration of PUA and FPUC on job-search methods. To do so, we turn to the ATUS, which reports information on a series of job-search methods used by unemployed individuals, also differentiating between active and passive job-search methods.[16] Overall, we do not find any effect of the policy expiration on the probability that an individual uses any active or passive job-search method (panels A and B in Appendix Figure B10). However, we find a small increase in the number of active job-search methods used (panel C), while the number of passive job-search methods remains unchanged (panel D). Overall, this is in line with models in which individuals increase their job-search intensity (or transition from passive to active job search) when UBs expire (Shimer, 2004). However, the fact that most unemployment-to-employment transitions concerned temporarily laid-off individuals that were recalled likely decreased the magnitude of treatment effects on job-search behavior.

Supporting Analyses and Sensitivity Tests

We present several supporting analyses to corroborate the findings presented above. For ease of exposition, we mostly focus on the three types of transitions from unemployment (i.e. to either employment, unemployment or inactivity).

First, we add state-level measures of the strength of COVID-19 restrictions as additional controls. This is to address concerns related to the fact that early- and late-withdrawal states might have also lifted COVID-19 restrictions at a different speed around the time of UB expiration. However, treatment effects barely change (Appendix Table A5).

Second, we modify the baseline specification by extending the sample period back to the beginning of 2019 and adding year and month fixed effects to control for seasonal trends across states. Appendix Table A6 presents the results of this exercise and shows that results remain largely unchanged.

An additional concern relates to the fact that we cannot observe UB recipients in the CPS. To investigate how this might be biasing our results, we restrict the sample to individuals who are more likely to be receiving UBs. These are defined as those who have

[15]The first type of transitions mentioned above is compatible with occupational downgrading, while the second one can be explained by the sectorial changes that occurred in the aftermath of the pandemic.

[16]According to the ATUS, active job-search methods include: checked with employers, contacted public or private employment agencies, contacted relatives or friends, contacted school or university job centres, sent out CV, checked with trade unions, answered job ads, other active methods. Passive job-search methods instead include: read job ads, attended a training program, other passive methods.

lost their job involuntarily (i.e. due to layoff or contract end).[17] The results are very similar to thosein the baseline specification (Appendix Table A7, Panel A).

We also want 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 (Farber et al., 2015; Holzer et al., 2021) 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 (Appendix Table A7, Panel B).

Similarly, we acknowledge that the longitudinal component of the CPS is very short and does not allow us to observe many individuals both before and after the policy change. We focus on individuals who had their first or fifth interview in April 2021 (i.e. before the policy announcement), and for whom we have consecutive monthly observations until July 2021 (i.e. after the policy change).[18] We replicate the analysis but now include also individual fixed effects and obtain very similar, but imprecisely estimated, results (Appendix Table A7, Panel C).

We then account for the fact that early-withdrawal states had a lower share of long-term unemployed. We follow Coombs et al. (2022) and re-weight observations using inverse probability weighting.[19] The results that we obtain are very similar to those of our baseline specification (Appendix Table A7, Panel D).

As a final test, we re-run our model, but this time not imposing any restriction on the minimum number of interviews that individuals should have completed.[20] With this sample, the results are of the same sign but around half in magnitude (Appendix Table A8). To investigate possible reasons behind this, we look at the descriptive statistics for individuals with (i) only one, (ii) only four, or (iii) all eight CPS interviews (Appendix Table A9). We note that individuals with completed CPS panels are positively selected (e.g. higher income and educational attainments, lower unemployment). We thus interpret the difference

[17] During normal times, it would have been sensible to also restrict the sample to individuals who were unemployed since less than a specific period of time (for instance, Ganong et al. (2020) use the 26 weeks cutoff). However, UB maximum duration was significantly increased during the COVID-19 pandemic, making the restriction related to the length of the UB spell less meaningful in this context.

[18] Recall that the CPS interviews individuals for four consecutive months, then leaves them out of the sample for eight months, and interviews them again for another four consecutive months.

[19] In particular, we run a regression of an indicator equal to one for early policy withdrawal on quintiles of the length of the unemployment spell. We then use the predicted probabilities to re-weight observations.

[20] Remember that our baseline results were obtained by restricting the sample to individuals who had completed all eight CPS interview rounds by the end of our sample period in August 2022.

in results in light of the fact that individuals with completed interviews might have been more attached to the labor market, and thus more likely to respond to the policy change.

6 Complementary Evidence on Consumption

We complete our analysis with estimates of the consumption responses to the early withdrawal of PUA and FPUC. This follows the approach developed by Baily (1978) and Chetty (2006), which define the welfare effects of UBs as a result of the trade-off between their insurance value (i.e. in the form of consumption smoothing) and efficiency costs (i.e. due to longer UB receipt and delayed re-employment).

Analysis with anonymized credit and debit card data

We first use anonymized data on credit and debit card spending. This is provided by Safegraph and covers 316,276 establishments in 2,940 US counties between January 2021 and May 2022. The data is available at the establishment level and we have information on the establishment type (i.e. Restaurants, Alcohol and Tobacco, Personal Care, Family Entertainment and Grocery and General Stores.) For each establishment, we have information on the number of completed transactions, the number of customers and total spending. We aggregate establishment-level information at the county and monthly level, weighting each establishment by the number of transactions. Appendix Table A10 presents descriptive statistics from the SafeGraph database between February and August 2021.

The Safegraph data has been used and validated in previous research (Goolsbee and Syverson, 2021; Parolin and Lee, 2021; Parolin et al., 2022). One advantage of this database is that it does not rely on a single card provider, but it rather gathers information from different spending companies. One limitation, however, is that the consumption data does not capture transactions paid by cash and it will only partially cover spending in places (e.g. child care centers) that are not a site of regular credit card transactions. This can be a problem, given that low-income individuals (including the unemployed) are more likely to make transactions with cash (Shy, 2020). An additional shortcoming relates to the fact that the Safegraph database aggregates information at the level of the establishment, meaning that it does not provide individual-level data on consumption. This can generate concerns, as the majority of individuals making these transactions will not be unemployed.[21]

We begin by validating our consumption measures in the Safegraph database to un-

[21]Note that in July 2021 the overall unemployment rate in the U.S. was equal to 5.4%.

derstand whether the county-level information on consumption obtained from the Safegraph database reacts to changes in unemployment. To this end, we run a series of regressions with three different consumption measures as outcomes of interest (i.e. average spending per transactions, number of transactions and number of customers) and the county-level unemployment rate as the main regressor of interest. We also change the rest of the covariates, ranging from specifications with no controls to specifications with county and time fixed effects together with county-level controls. We find that there is a negative relationship between the unemployment rate and both the number of transactions and the number of customers (Appendix Figure B11). The results on average spending are instead slightly different. In particular, the relationship between average spending and the unemployment rate becomes positive, but close to zero, after including county fixed effects. This might reflect a change in the composition of individuals making these transactions (i.e. away from unemployed individuals who might be doing smaller purchases on average).

We interpret these results as indicative of the fact that the Safegraph database is able to track variations in consumption behavior following changes in unemployment, at least when using the number of transactions and the number of customers.

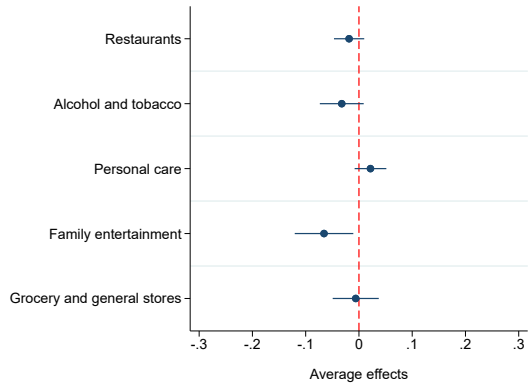
Using these two outcomes of interest, we replicate the DiD strategy introduced in Section 4.[22] The results reveal that, for most establishment types, the early withdrawal of PUA and FPUC did not generate any notable change in the number of transactions (Figure 3, Panel A) or in the number of customers (Figure 3, Panel B). However, we find negative and close-to-significant effects for Alcohol and tobacco as well as Family entertainment. To confirm these results, we also run a triple-difference (DDD) analysis exploiting the fact that counties differed in their pre-treatment unemployment rate.[23] We operationalize this approach by assigning counties to terciles based on their average unemployment rate between March and May 2021.[24] The DDD results are very similar to those from the simple DiD, and show that the number of transactions and customers did not vary for most establishment types (Panels C and D). However, we still see a small decline for the Family Entertainment

[22]The only difference is that now the analysis is conducted at the county level (i.e. by obtaining county-level measures of the outcomes of interested from establishment-level observations, as described above).

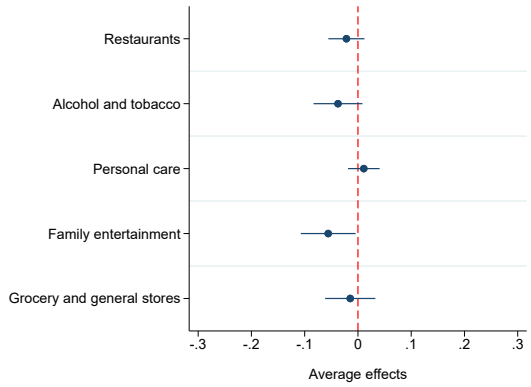
[23]This means that the early withdrawal of pandemic UBs should have had a larger treatment effect on consumption in those counties with an *ex-ante* higher unemployment rate. Identification thus comes from comparing the evolution of the outcomes of interest in counties with high and low unemployment rates in early-withdrawal states with counties with high and low unemployment rates in late-withdrawal states.

[24]Note that, if individuals that make transactions in a county are at least partially not the same as those living in that same county (which are instead used to compute the county's unemployment rate), this could represent an imperfect way to account for differences in treatment intensity.

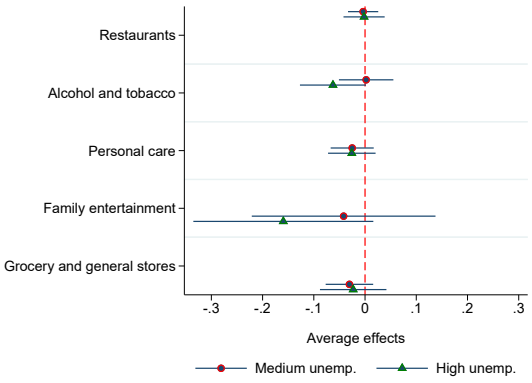
Figure 3: Treatment effects on the number of transactions and the number of customers



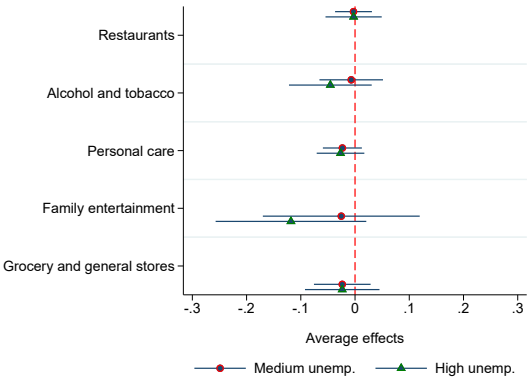
(a) DiD on the number of transactions



(b) DiD on the number of customers



(c) DDD on the number of transactions



(d) DDD on the number of customers

Notes: The figure reports coefficient estimates and 90% confidence intervals for different specifications. In the two upper panels, we present results from simple DiD specifications as introduced in Section 5, with the only difference that the analysis is now run at the county level. These DiD results are presented separately for the logarithm of the number of transactions (Panel A) and the logarithm of the number of customers (Panel B). In the two lower panels, we present results from a triple-difference approach, where we interact the standard DID term with a categorical variable grouping counties in terciles according to their unemployment rate (computed between March and May 2021, using data from the Bureau of Labor Statistics). Counties in the lowest tercile of the unemployment rate represent the omitted category in this specification, and we plot coefficients for counties with medium and high unemployment rates. These DDD results are presented separately for the logarithm of the number of transactions (Panel C) and the logarithm of the number of customers (Panel D). In all specifications, we control for time and state fixed effects as well as county-level controls for the logarithm of population density and the number of COVID-19 infections. County-level aggregates are obtained by using establishment-level information, and weighting each establishment by the number of transactions. Additionally, each regression weights county-level observations with the square root of county population. Standard errors are clustered at the state level.

and Alcohol and tobacco, especially in counties in the upper tercile of unemployment.

Instead, results obtained using mean spending per transaction as the outcome of interest show no effects of the early withdrawal of the pandemic UBs (Appendix Figure B12). This applies to both the DiD or DDD specifications (Panel A and B, respectively).

The results obtained on the number of transactions and customers indicate that households reduced spending in non-essential consumption items, while keeping constant spending in categories that are more difficult to adjust (e.g. grocery). This raises a broader

question of why households wait until benefits are withdrawn before cutting expenditures (Ganong and Noel, 2019; Ganong et al., 2022), at least when it comes to certain spending categories. However, this pattern of altering spending behavior when policies are implemented, rather than when they are announced, is consistent with that observed for other policy interventions during the pandemic, including the temporary expansion of the child tax credit (Parolin et al., 2023). We interpret the differences in results by spending type by noting that households might wait until policy implementation before reducing expenditures in additive substances (i.e. Alcohol and tobacco) and services for which demand was particularly high in the aftermath of the COVID-19 pandemic (i.e. Family entertainment). This behavior might also be driven by imperfect information, especially among marginalized groups, on the exact timing of the policy start and end dates.

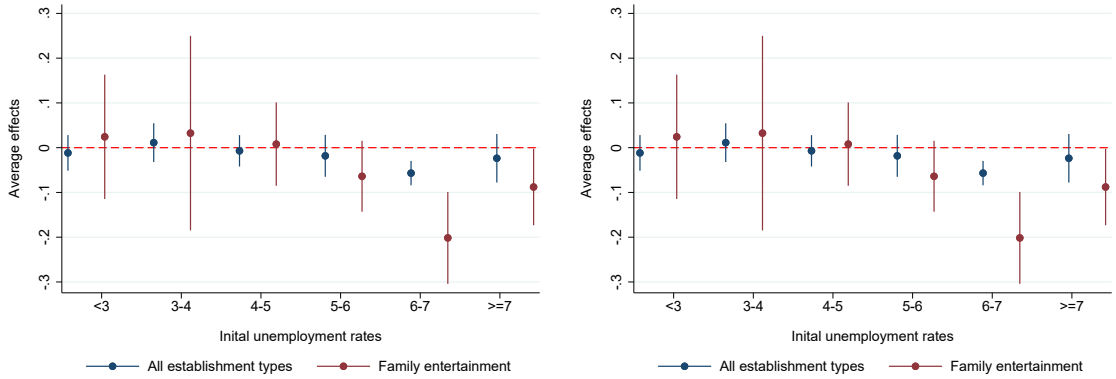
We replicate the DiD analysis by groups of counties characterized by different levels of unemployment. The intuition is that, if the UB withdrawal has reduced consumption among affected individuals, this will emerge more clearly in the Safegraph database in counties with a relatively high unemployment rate. Figure 4 shows the results separately on the number of transactions (Panel A) and the number of customers (Panel B). We plot the coefficients of separate regressions run for counties grouped by their pre-treatment unemployment rates (displayed on the x-axis). In each case, we conduct the analysis for all establishment types (blue dot) and for Family entertainment establishments (red dots). This exercise confirms that the early withdrawal of PUA and FPUC led to a reduction in consumption (as measured by either the number of transactions or the number of customers) in counties characterized by a relatively high unemployment rate (above 6%), and this effect is larger for consumption in Family entertainment establishments.

Analysis with the Census's Household Pulse Survey

We end this Section by presenting complementary evidence on the consumption responses to the early withdrawal of PUA and FPUC using the Census's Household Pulse Survey (Pulse). The Pulse has the advantage of reporting individual-level information on consumption behaviors, together with other individual characteristics, including labor force status. We can thus observe consumption of employed and unemployed individuals in treated and control states before and after the withdrawal of PUA and FPUC.[25] However, the survey is not of longitudinal nature, which implies that we cannot identify individuals who

[25]Phase 3.1 of the Pulse survey reports information also on UB receipt, while Phase 3.2 does not contain this information. For this reason (and also to ensure consistency with the analysis conducted using CPS data), we do not use information on UB receipt and instead look only at the individual's labor force status.

Figure 4: Treatment effects on the number transaction and customers, by counties' pre-treatment unemployment rates



(a) DiD on the number of transactions

(b) DiD on the number of customers

Notes: The figure reports coefficient estimates and 90% confidence intervals for different specifications, where we run the standard DiD analysis presented in Section 5, with the only difference that the analysis is now run at the county level. Additionally, we conduct the analysis separately across counties characterized by a different initial level of unemployment (as reported on the x-axis). This is computed between March and May 2021 using data from the Bureau of Labor Statistics. For all counties within the same bin of the unemployment rate, we present the results of DiD estimates using either the logarithm of the number of transactions (Panel A) or the logarithm of the number of customers (Panel B). For each outcome of interest, we conduct the analysis for all establishment types (blue dot) and for Family entertainment establishments (red dot). All specifications control for state and time fixed effects, and we also include county-level controls for the logarithm of population density and the number of COVID-19 infections. County-level aggregates are obtained by using establishment-level information, and weighting each establishment by the number of transactions. Additionally, each regression weights county-level observations with the square root of county population. Standard errors are clustered at the state level.

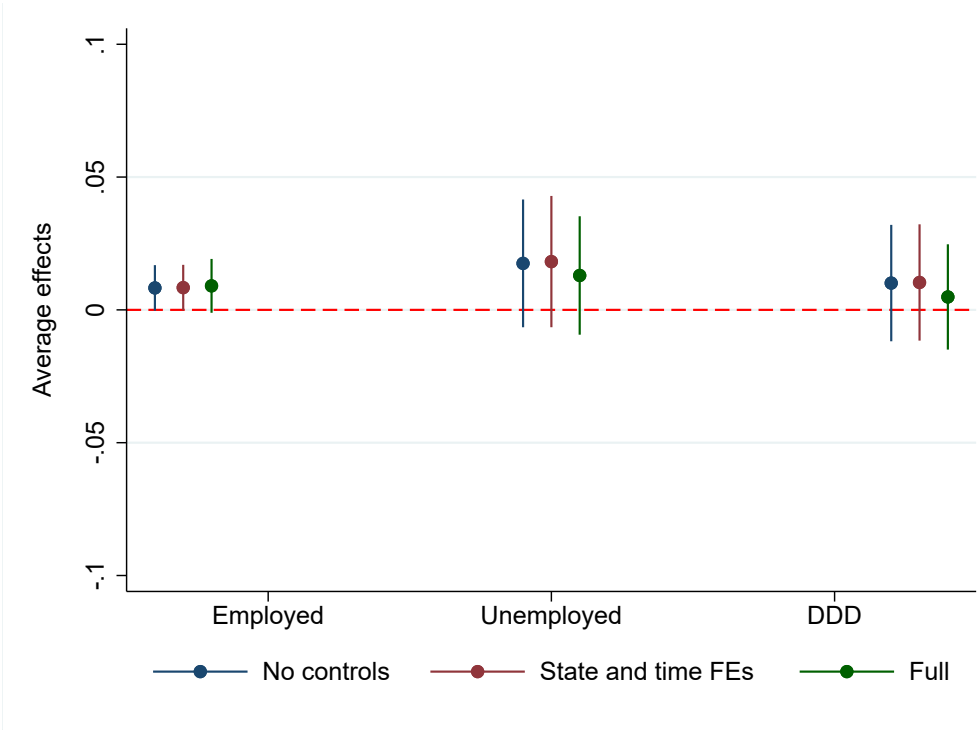
were unemployed before the policy change and track their consumption behavior after the expiration of pandemic UBs. Given the large transitions from unemployment to employment documented in Section 5, this represents a problem if we aim to isolate the treatment effects on consumption from the withdrawal of UBs.

Bearing these caveats in mind, we present results from a simple DiD using information from the Pulse's survey waves 3.1 (April to July 2021) and 3.2 (July to October 2021).[26] To construct the outcome of interest, we use the responses to a question asking "how difficult has it been for your household to pay for usual household expenses". Using this question, we create a dummy variable equal to one if respondents reported that "very difficult" as a response.[27] Figure 5 presents the results of the same DiD approach presented in Section 4, but now with regressions run separately for employed and unemployed individuals (left and central part of the figure). We also conduct a DDD exercise, and plot the interaction term between (i) the dummy for the state's early policy withdrawal, (ii) the post treatment dummy, and (iii) the individual's unemployment status. The results are presented separately

[26]In practice, we end the analysis in August 2021 in the simple DiD specification, as the two groups of states returned to the same policy regime starting in September (see above for details).

[27]During the period of the analysis, this was reported by 9% of survey respondents.

Figure 5: Treatment effects on consumption deprivation using the Pulse survey



Notes: The figure reports coefficient estimates and 90% confidence intervals for specifications run using data from the Census's Household Pulse survey (Phases 3.1 and 3.2). The outcome of interest is always a dummy equal to one if the respondent opts for "very difficult" when answering a question on "In the last 7 days, how difficult has it been for your household to pay for usual household expenses". In the left and central parts of the figure, the coefficients that are plotted are those of the interaction term between the early withdrawal status and the post-policy dummy (standard DiD analysis as introduced in Section 5). The analysis is conducted only for employed and unemployed individuals, respectively. In the right part of the figure, the coefficients that are plotted are those of the triple interaction term between early withdrawal status, the post-policy dummy and unemployment status (DDD analysis, where we use both unemployed and employed individuals). In each case, we present results from specifications with no controls (blue points), with state and time fixed effects (red dots) and also with individual level controls for sex, marital status, race, ethnicity and the presence of a child in the household. Sampling weights are used in all specifications. Standard errors are clustered at the state level.

for three different specifications, without controls (in blue), with state and time fixed effects (in red) and with additional controls for sex, marital status, race, ethnicity and presence of a child (in green). Overall, they confirm that there was a small deterioration in consumption in states that withdrew PUA and FPUC earlier, with the trend being larger in magnitude (but still imprecisely estimated) for the unemployed.

7 Conclusions

In the summer of 2021, some state governments in the US suddenly withdrew access to the PUA and FPUC, two generous supplements to unemployment support. This paper studies the consequences of these policy withdrawals for employment transitions and levels, employment quality among individuals returning to work, and consumption.

Our findings indicate that the labor market adjustment to this sudden policy change was relatively quick. While this can be partially attributed to the specific nature of the crisis that followed the eruption of the COVID-19 pandemic, and in particular the fact that a large share of unemployed individuals were waiting to be recalled by their enterprise, this is a common feature of many crisis responses, when emergency measures temporarily "freeze" the labor market and policy makers need to decide the optimal strategy for phasing-out emergency interventions. We find that the policy withdrawal led to a large employment response. However, this was mostly due a large mechanical response, rather than a behavioral response among UB recipients. Additionally, we document that the withdrawal of UBs led to some unanticipated negative effects on employment probability for non-eligible individuals as well as in terms of job quality and consumption levels.

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Appendices

A Appendix: Additional tables

Table A1: Determinants of early UB 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 robust standard errors of different regressions, 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. We are not including in the analysis the eight US states that followed different rules for terminating PUA and FPUC (i.e. different from either early or late withdrawal, see Section 2 for details). *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A2: List of states by treatment status

	Withdrawal	Announcement	Effective		Withdrawal	Announcement	Effective
Alabama	Early	10 May	19 June	Montana	Early	4 May	26 June
Alaska	Out of sample			Nebraska	Early	24 May	19 June
Arizona	Out of sample			Nevada	Late		
Arkansas	Early	7 May	26 June	New Hampshire	Early	18 May	19 June
California	Late			New Jersey	Late		
Colorado	Late			New Mexico	Late		
Connecticut	Late			New York	Late		
Delaware	Late			North Carolina	Late		
District of Columbia	Late			North Dakota	Early	May 10	19 June
Florida	Out of sample			Ohio	Out of sample		
Georgia	Early	13 May	26 June	Oklahoma	Early	17 May	26 June
Hawaii	Late			Oregon	Late		
Idaho	Early	11 May	19 June	Pennsylvania	Late		
Illinois	Late			Rhode Island	Late		
Indiana	Out of sample			South Carolina	Early	6 May	26 June
Iowa	Early	11 May	12 June	South Dakota	Early	12 May	26 June
Kansas	Late			Tennessee	Out of sample		
Kentucky	Late			Texas	Early	17 May	26 June
Louisiana	Out of sample			Utah	Early	12 May	26 June
Maine	Late			Vermont	Late		
Maryland	Out of sample			Virginia	Late		
Massachusetts	Late			Washington	Late		
Michigan	Late			West Virginia	Early	14 May	19 June
Minnesota	Late			Wisconsin	Late		
Mississippi	Early	10 May	12 June	Wyoming	Early	11 May	19 June
Missouri	Early	11 May	12 June				

Notes: The table reports for each state whether this has withdrawn both PUA and FPUC in June 2021 ("Early") or in September 2021 ("Late"). We are not including in the analysis the eight US states ("Out of sample") that followed different rules for terminating PUA and FPUC (i.e. different from either early or late withdrawal, see Section 2 for details). For early-withdrawal states, the table also reports the announcement date as well as the date in which the policy became effective.

Table A3: Descriptive statistics from the CPS

	Overall		Early withdrawal		Late withdrawal		t-test (p value)
	Mean	SD	Mean	SD	Mean	SD	
Men	0.496	0.500	0.489	0.500	0.498	0.500	0.003
Age	41.361	14.098	41.245	14.094	41.410	14.100	0.056
Race: White	0.785	0.411	0.794	0.404	0.781	0.414	0.000
Race: Black	0.099	0.299	0.135	0.342	0.084	0.278	0.000
Race: Asian	0.076	0.264	0.039	0.195	0.091	0.287	0.000
Race: Other	0.040	0.196	0.031	0.175	0.044	0.205	0.000
Marital status: married	0.540	0.498	0.552	0.497	0.535	0.499	0.000
Marital status: divorced or separated	0.099	0.299	0.113	0.317	0.093	0.291	0.000
Marital status: widow	0.016	0.124	0.019	0.135	0.014	0.119	0.000
Marital status: single	0.345	0.475	0.317	0.465	0.357	0.479	0.000
From none to 12th grade (no degree)	0.128	0.334	0.139	0.346	0.123	0.329	0.000
High school diploma or equivalent	0.240	0.427	0.262	0.440	0.231	0.421	0.000
From some college to PhD	0.632	0.482	0.599	0.490	0.646	0.478	0.000
There are children in the household	0.623	0.485	0.616	0.486	0.626	0.484	0.000
Income less than 50	0.274	0.446	0.317	0.465	0.256	0.436	0.000
Income from 50 to 100	0.316	0.465	0.334	0.472	0.308	0.462	0.000
Income above 100	0.410	0.492	0.349	0.477	0.437	0.496	0.000
N	155664		61968		93696		

Notes: The table reports means and standard deviations (SD) for selected variables, measured in the monthly files of the CPS between February and August 2021. The descriptive statistics are computed separately for the overall sample in the study, and then separately for individuals in early- and late-withdrawal states. The last column reports the p-values of t-tests of equality of means between early- and late-withdrawal states.

Table A4: Complementary analysis using as treatment group states that only withdrew FPUC

	EE	EU	EI	UE	UU	UI	IE	IU	II
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{\beta}$	0.004	0.006**	-0.010**	0.168***	-0.036	-0.132***	-0.046***	-0.001	0.047***
	(0.003)	(0.003)	(0.004)	(0.053)	(0.052)	(0.019)	(0.013)	(0.008)	(0.016)
Mean of y	0.972	0.010	0.019	0.235	0.559	0.205	0.053	0.036	0.910
R-squared	0.014	0.004	0.013	0.025	0.037	0.040	0.017	0.012	0.023
Observations	60,834	60,834	60,834	3,553	3,553	3,553	22,678	22,678	22,678
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 from Equation (1) 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 analysis is conducted between February and August 2021, and the post-policy indicator takes the value of one in July and August 2021. The mean of the outcome of interest is computed for the control group in the period before the policy change. Compared to the baseline results, here we use the four states that withdrew only FPUC (but not PUA) in June 2021 as a new treatment group. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A5: Robustness tests adding COVID-19 restriction measures among the covariates

		UE			UU			UI	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{\beta}$	0.153*	0.167**	0.158*	-0.114	-0.147**	-0.118	-0.0397	-0.0200	-0.0397
	(0.0808)	(0.0643)	(0.0793)	(0.0728)	(0.0611)	(0.0705)	(0.0370)	(0.0219)	(0.0371)
Mean of y		0.235			0.559			0.205	
Observations	4,519	4,519	4,519	4,519	4,519	4,519	4,519	4,519	4,519
R-squared	0.030	0.031	0.031	0.041	0.041	0.042	0.039	0.039	0.039
State and time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID controls	Health	Econ	Both	Health	Econ	Both	Health	Econ	Both

Notes: The table presents coefficient estimates and standard errors for β from Equation (1) 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 analysis is conducted between February and August 2021, and the post-policy indicator takes the value of one in July and August 2021. The mean of the outcome of interest is computed for the control group in the period before the policy change. Compared to the baseline results, here we also add controls for 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 A6: Robustness test adding data from 2019

	UE			UU			UI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{\beta}$	0.146**	0.166**	0.226***	-0.123**	-0.148**	-0.196***	-0.0239	-0.0180	-0.0300
	(0.0594)	(0.0669)	(0.0747)	(0.0553)	(0.0595)	(0.0599)	(0.0247)	(0.0257)	(0.0326)
Mean of y		0.268			0.537			0.195	
Observations	18,713	18,713	18,713	18,713	18,713	18,713	18,713	18,713	18,713
R-squared	0.026	0.030	0.053	0.024	0.029	0.050	0.031	0.035	0.057
State, year and month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Personal controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State*Year FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
State*Month FE	No	No	Yes	No	No	Yes	No	No	Yes

Notes: The table presents coefficient estimates and standard errors for β from Equation (1) from separate regression models, where the dependent variables are different types of month-to-month transitions from unemployment (U) 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. Compared to the baseline results, the analysis here is conducted using data from the beginning of 2019 to August 2021. The different regressions differ with respect to the use of state, month and year fixed effects (first column for each outcome of interest), the inclusion of also year*state fixed effects (second column for each outcome of interest), and the inclusion of also month*state fixed effect (third column for each outcome of interest). Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A7: Miscellaneous robustness tests

	UE (1)	UU (2)	UI (3)	UE (4)	UU (5)	UI (6)
	Panel A: Likely UB recipients			Panel B: Spurious transitions		
$\hat{\beta}$	0.145**	-0.118	-0.0272	0.206***	-0.142**	-0.0555
	(0.0684)	(0.0714)	(0.0242)	(0.0473)	(0.0547)	(0.0337)
Mean of y	0.235	0.559	0.206	0.212	0.558	0.148
Observations	4,213	4,213	4,213	2,878	2,878	2,878
R-squared	0.028	0.040	0.041	0.046	0.043	0.039
	Panel C: With individual Fes			Panel D: Inverse probability weighting		
$\hat{\beta}$	0.049	-0.108	0.059	0.150***	-0.142***	-0.008
	(0.128)	(0.140)	(0.078)	(0.046)	(0.048)	(0.030)
Mean of y	0.241	0.556	0.203	0.235	0.559	0.205
Observations	568	568	568	4,519	4,519	4,519
R-squared	0.166	0.209	0.043	0.042	0.048	0.045
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 from Equation (1) 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 analysis is conducted between February and August 2021, and the post-policy indicator takes the value of one in July and August 2021. The mean of the outcome of interest is computed for the control group in the period before the policy change. In Panel A, the sample is composed only of unemployed individuals who lost their job involuntarily. In Panel B, we correct for spurious transitions following the coding procedure described in the text. In Panel C, we restrict the sample to individuals that have four consecutive interviews between April and July 2021 and include individual fixed effects (FEs). In Panel D, we run our baseline specification but using inverse probability weighting following the approach proposed by Coombs et al. (2022). Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A8: Robustness test including all individuals with at least one interview round

	EE	EU	EI	UE	UU	UI	IE	IU	II
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\hat{\beta}_3$	0.003	-0.002	-0.002	0.075***	-0.082**	0.007	-0.003	-0.001	0.004
	(0.003)	(0.002)	(0.002)	(0.022)	(0.032)	(0.021)	(0.007)	(0.003)	(0.005)
Mean of y	0.965	0.011	0.024	0.227	0.551	0.221	0.06	0.039	0.900
Observations	181,833	181,833	181,833	11,006	11,006	11,006	72,941	72,941	72,941
R-squared	0.019	0.004	0.016	0.021	0.037	0.034	0.015	0.011	0.024
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 from Equation (1) 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 analysis is conducted between February and August 2021, and the post-policy indicator takes the value of one in July and August 2021. The mean of the outcome of interest is computed for the control group in the period before the policy change. Compared to the baseline results, here we include in the sample individuals who have completed at least one (rather than eight) interview round. Standard errors are clustered at the state level. *** , **, and * denote significance at the 1, 5, and 10 percent level, respectively.

Table A9: Descriptive statistics from the CPS, splitting the sample by the number of completed interviews

	One interview		Four interviews		Eight interviews	
	Mean	SD	Mean	SD	Mean	SD
Men	0.497	0.500	0.492	0.500	0.495	0.500
Age	35.485	14.212	36.224	14.477	41.417	14.130
Race: White	0.701	0.458	0.726	0.446	0.789	0.408
Race: Black	0.192	0.394	0.162	0.369	0.106	0.307
Race: Asian	0.056	0.229	0.067	0.250	0.069	0.253
Race: Other	0.052	0.222	0.045	0.206	0.037	0.188
Marital status: married	0.325	0.468	0.378	0.485	0.539	0.498
Marital status: divorced or separated	0.116	0.321	0.113	0.317	0.102	0.302
Marital status: widow	0.016	0.125	0.016	0.124	0.016	0.126
Marital status: single	0.543	0.498	0.493	0.500	0.343	0.475
From none to 12th grade (no degree)	0.152	0.359	0.163	0.370	0.127	0.333
High school diploma or equivalent	0.308	0.462	0.274	0.446	0.245	0.430
From some college to PhD	0.540	0.498	0.563	0.496	0.628	0.483
There are children in the household	0.460	0.498	0.539	0.498	0.621	0.485
Income less than 50	0.455	0.498	0.396	0.489	0.279	0.449
Income from 50 to 100	0.306	0.461	0.320	0.466	0.322	0.467
Income above 100	0.239	0.426	0.285	0.451	0.399	0.490
Employed	0.619	0.486	0.651	0.477	0.697	0.460
Unemployed	0.062	0.240	0.049	0.216	0.038	0.191
Not in labour force	0.320	0.466	0.300	0.458	0.265	0.442
N	8270		73964		185828	

Notes: The table reports means and standard deviations (SD) for selected variables, measured in the monthly files of the CPS between February and August 2021. The descriptive statistics are computed separately for individuals (i) who have completed just one interview round, (ii) four interview rounds, or (iii) eight interview rounds. We let individuals complete their interview rounds until August 2022, when our sample period ends.

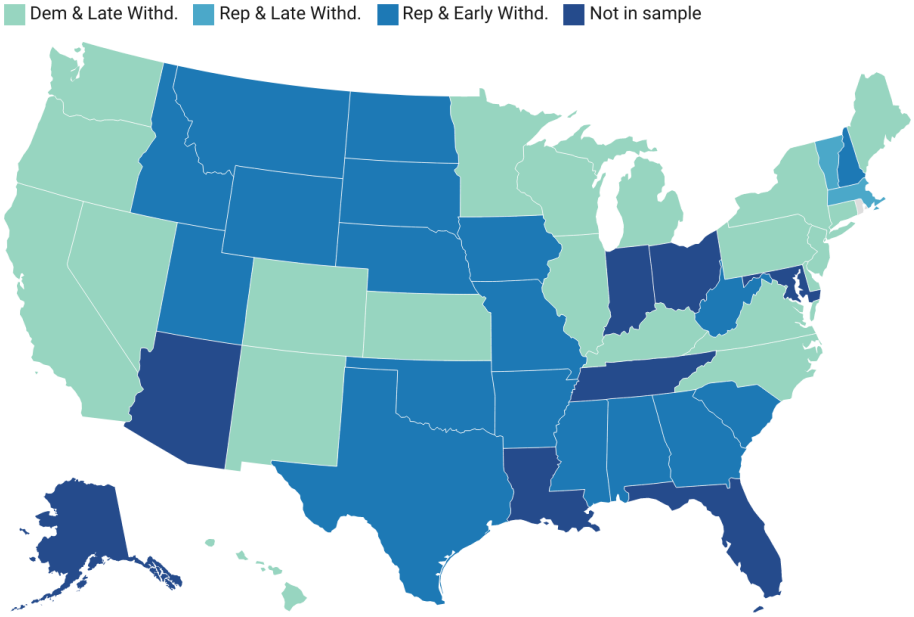
Table A10: Descriptive statistics from the SafeGraph database

	Overall		Early withdrawal		Late withdrawal		t-test (p value)
	Mean	SD	Mean	SD	Mean	SD	
Median number of transactions	123.573	194.885	120.211	241.641	125.643	159.397	0.000
Mean number of transaction	214.169	346.142	201.019	410.616	222.262	299.356	0.000
Median number of customers	77.008	109.991	75.189	136.023	78.128	90.295	0.001
Mean number of customers	116.258	158.809	109.787	185.536	120.241	139.695	0.000
Median spending per transaction	23.426	48.918	22.850	37.145	23.781	54.920	0.001
Mean spending per transaction	36.082	51.024	34.243	39.983	37.214	56.733	0.000
Median spending per customer	36.873	86.205	37.440	88.612	36.524	84.689	0.858
Mean spending per customer	59.362	87.619	58.155	89.714	60.105	86.298	0.005
N	59374		27671		31703		

Notes: The table reports means and standard deviations (SD) for selected variables, measured in the credit and debit card spending database provided by SafeGraph. Data is provided at the establishment level, and then grouped at the county level using the number of transactions to weight establishments. Each observation used to compute these descriptive statistics corresponds to a county/month data point. The descriptive statistics are computed separately for the overall sample in the study, and then separately for individuals in early- and late-withdrawal states. The last column reports the p-values of t-tests of equality of means between early and late withdrawal states.

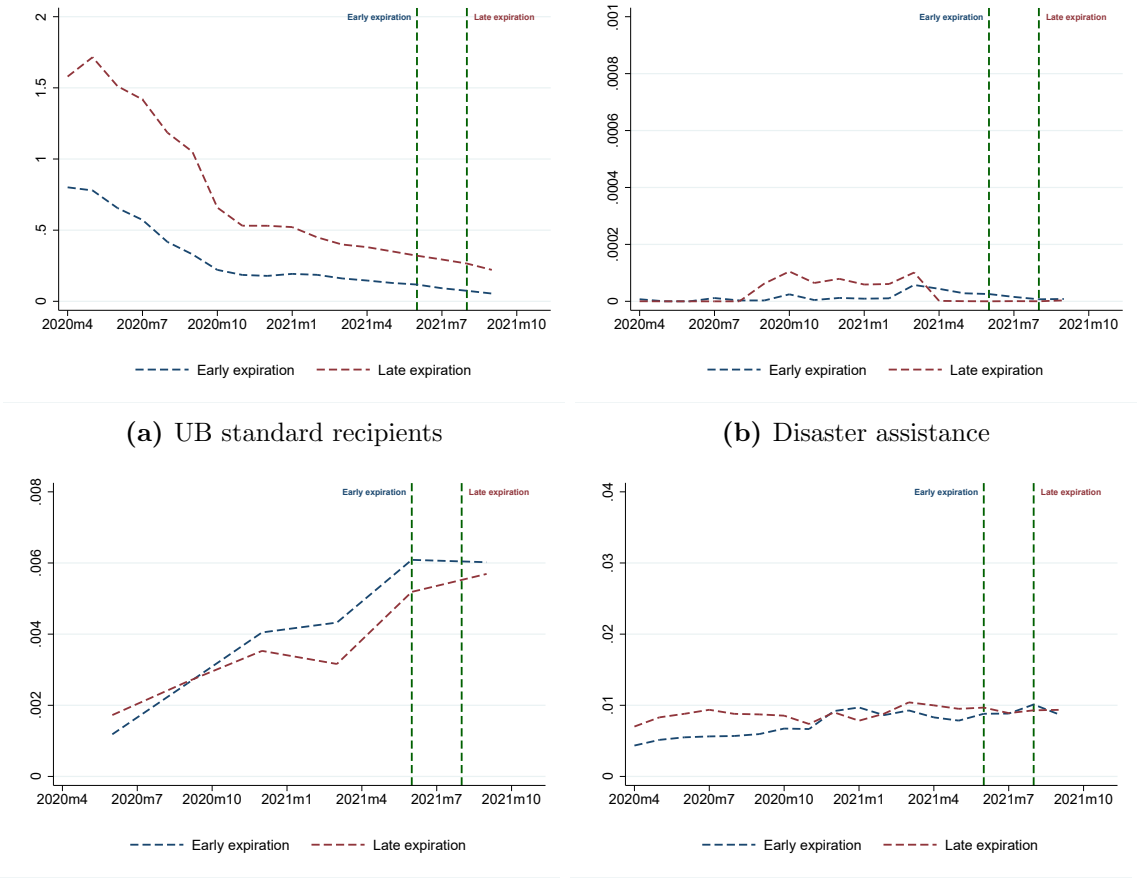
B Appendix: Additional figures

Figure B1: US states by governor's party affiliation and treatment status



Notes: The figure classifies US states by overlapping the party affiliation of the governor in office in June 2021 (i.e. Republican or Democratic), with the policy treatment status (i.e. early- or late-withdrawal states). States not in the sample are those who withdrew PUA and FPUC at some point between June and September 2021, or that withdrew only one of the two intervention in June 2021, or that withdrew both interventions in June 2021 but were forced to reinstate them (see Section 2 for details).

Figure B2: Trends in other types of UB claims in treated and control states, 2020-2022



(a) UB standard recipients

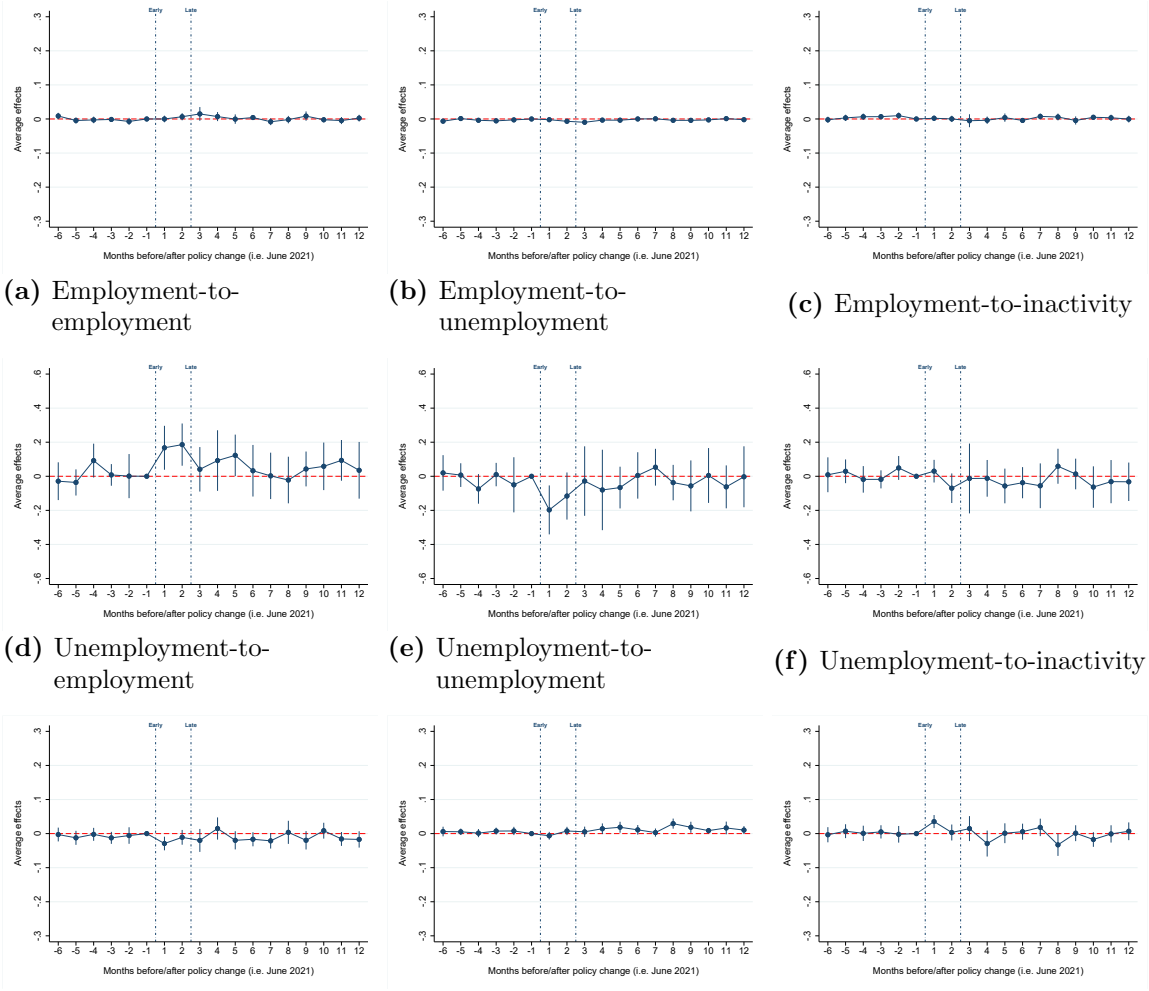
(b) Disaster assistance

(c) Frauds

(d) Appeals

Notes: The figure reports data from the ETA reports collected by the Department on Labor on UI benefit recipients. Data is reported at the monthly and state level and we have normalized it by population size in 2021. Panel A reports information on the number of individuals receiving the standard UBs (i.e. not PUA or FPUC beneficiaries), Panel B shows the number of UB claimants of the Disaster Unemployment Allowance program (ETA 902), Panel C reports the number of UB frauds detected (ETA 227), and Panel D shows the total number of UB appeals (ETA 5130). Information on frauds and appeals is available only for the overall population of UB recipients, and it is not disaggregated by the type of UB program to which the individual is affiliated. In each case, we group states according to whether they ended PUA and FPUC early (blue line) or late (red line). Standard errors are clustered at the state level.

Figure B3: Event-study estimates for the different transitions by labor market status



(a) Employment-to-employment

(b) Employment-to-unemployment

(c) Employment-to-inactivity

(d) Unemployment-to-employment

(e) Unemployment-to-unemployment

(f) Unemployment-to-inactivity

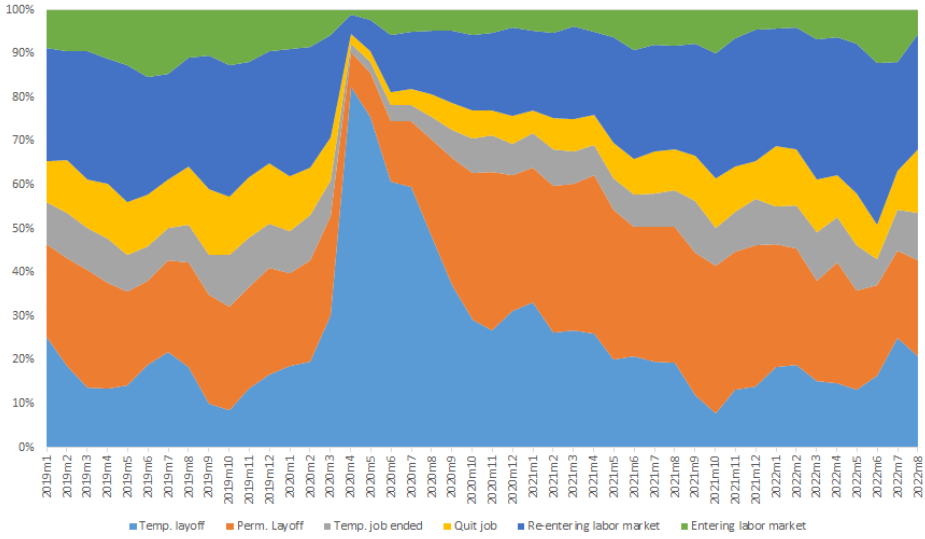
(g) Inactivity-to-employment

(h) Inactivity-to-unemployment

(i) Inactivity-to-inactivity

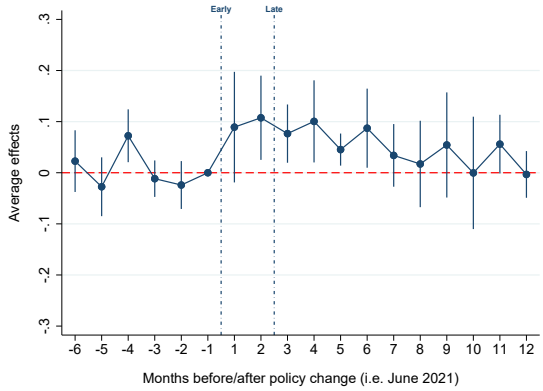
Notes: The figure reports coefficient estimates and 90% confidence intervals for a series of event-study specifications, where the dependent variables are the different types of monthly labor market transitions from and to employment, unemployment and inactivity. In all specifications, we control for state and month fixed effects as well as for age, sex, and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Each panel also plots two vertical lines corresponding to the months of early and late policy withdrawal (June and August 2021, respectively). Standard errors are clustered at the state level.

Figure B4: Composition of the unemployed population in the US by reason for unemployment, January 2019 to August 2022

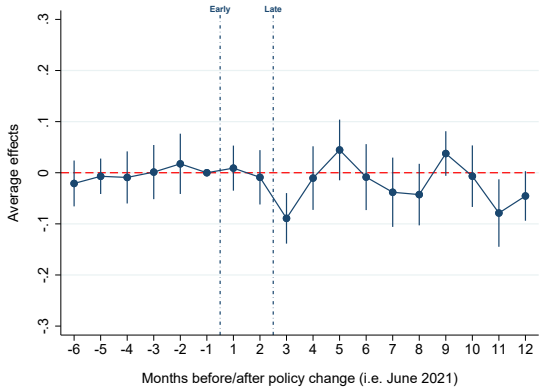


Notes: The figure presents the share of unemployed individuals by reason of unemployment, differentiating among those who were unemployed because of (i) temporary layoff (light blue), (ii) permanent layoff (orange), (iii) termination of a fixed-term contract (grey), (iv) having quit their job (yellow), (v) re-entering the labor market (blue), (vi) entering the labor market (green). Data comes from the monthly CPS files.

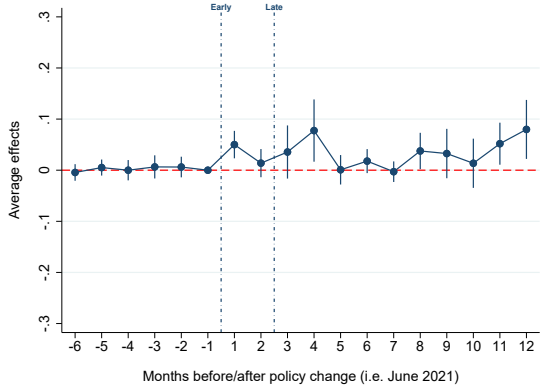
Figure B5: Event-study results for the transitions from different types of unemployment to employment



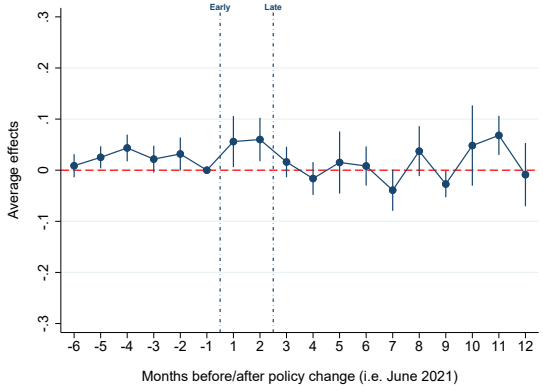
(a) Unemployment due to temporary layoff to employment



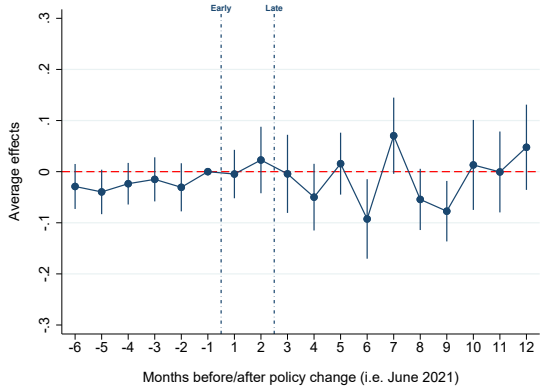
(b) Unemployment due to permanent jobloss to employment



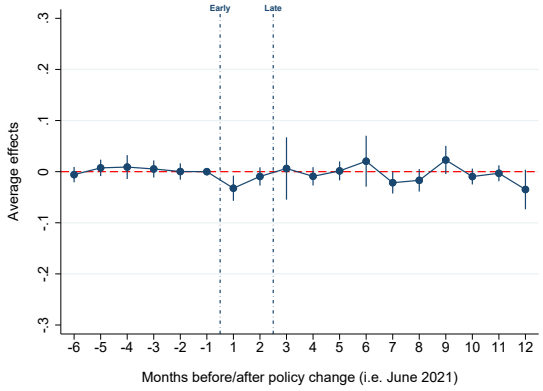
(c) Unemployment due to expiration of temporary contract to employment



(d) Unemployment due to quitting previous job to employment



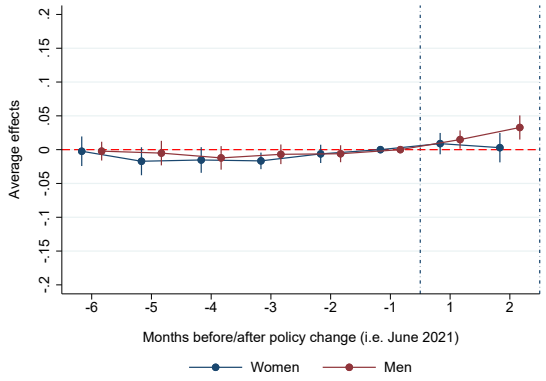
(e) Unemployment due to re-entering the labor market to employment



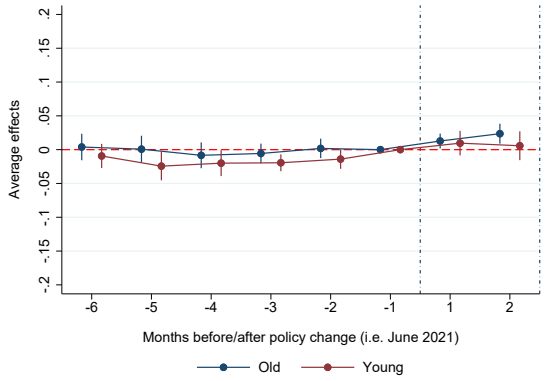
(f) Unemployment due to entering the labor market to employment

Notes: The figure reports coefficient estimates and 90% confidence intervals for a series of event-study specifications, where the dependent variables are the different types of monthly labor market transitions from different types of unemployment to employment. In particular, we differentiate between (i) unemployment due to temporary layoff (Panel A), (ii) unemployment due to permanent jobloss (Panel B), (iii) unemployment due to expiration of temporary contract (Panel C), (iv) unemployment due to quitting previous job (Panel D), (v) unemployment due to re-entering the labor market (Panel E), and (vi) unemployment due to entering the labor market (Panel F). In all specifications, we control for state and month fixed effects as well as for age, sex, and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Each panel also plots two vertical lines corresponding to the months of early and late policy withdrawal (June and August 2021, respectively). Standard errors are clustered at the state level.

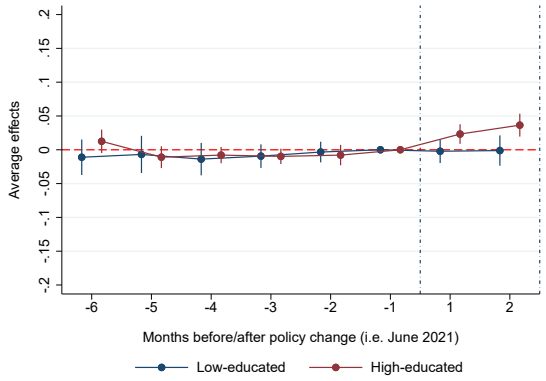
Figure B6: Heterogeneous effects on employment



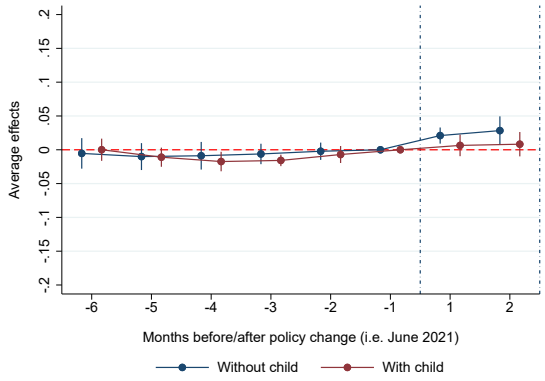
(a) By sex



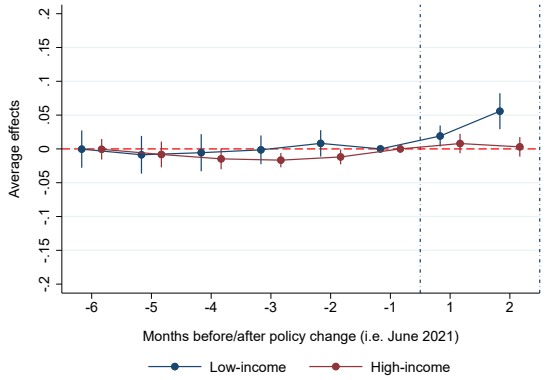
(b) By age



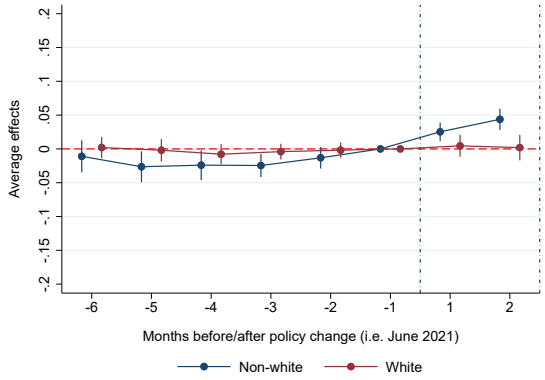
(c) By education



(d) By presence of child



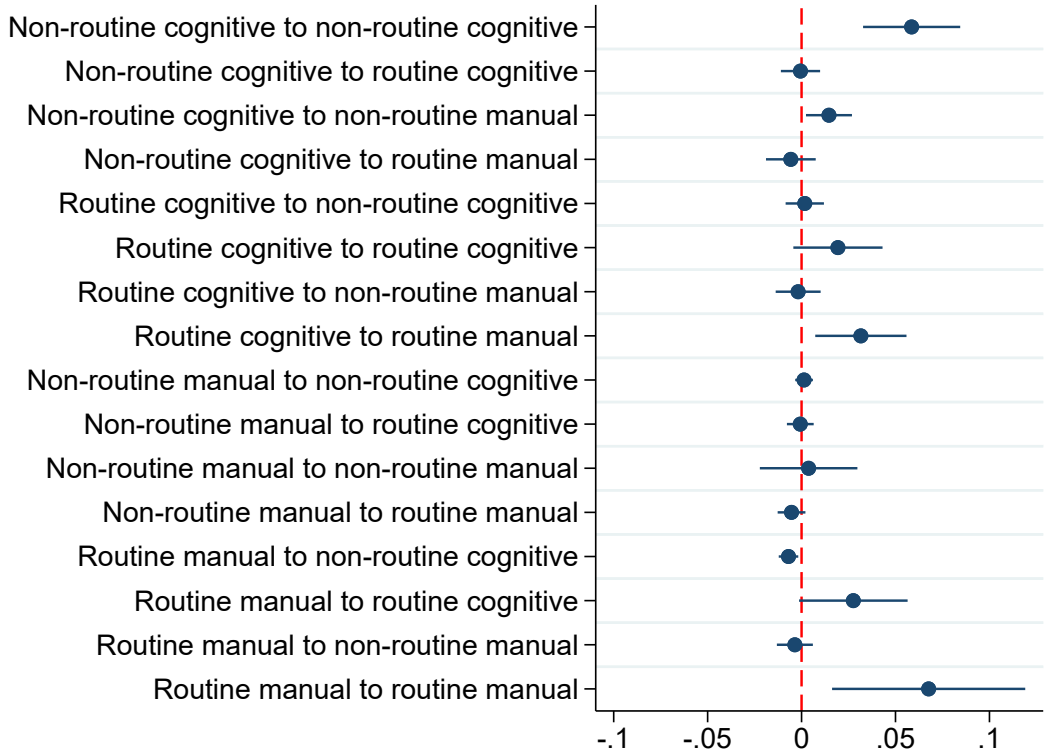
(e) By income



(f) By race

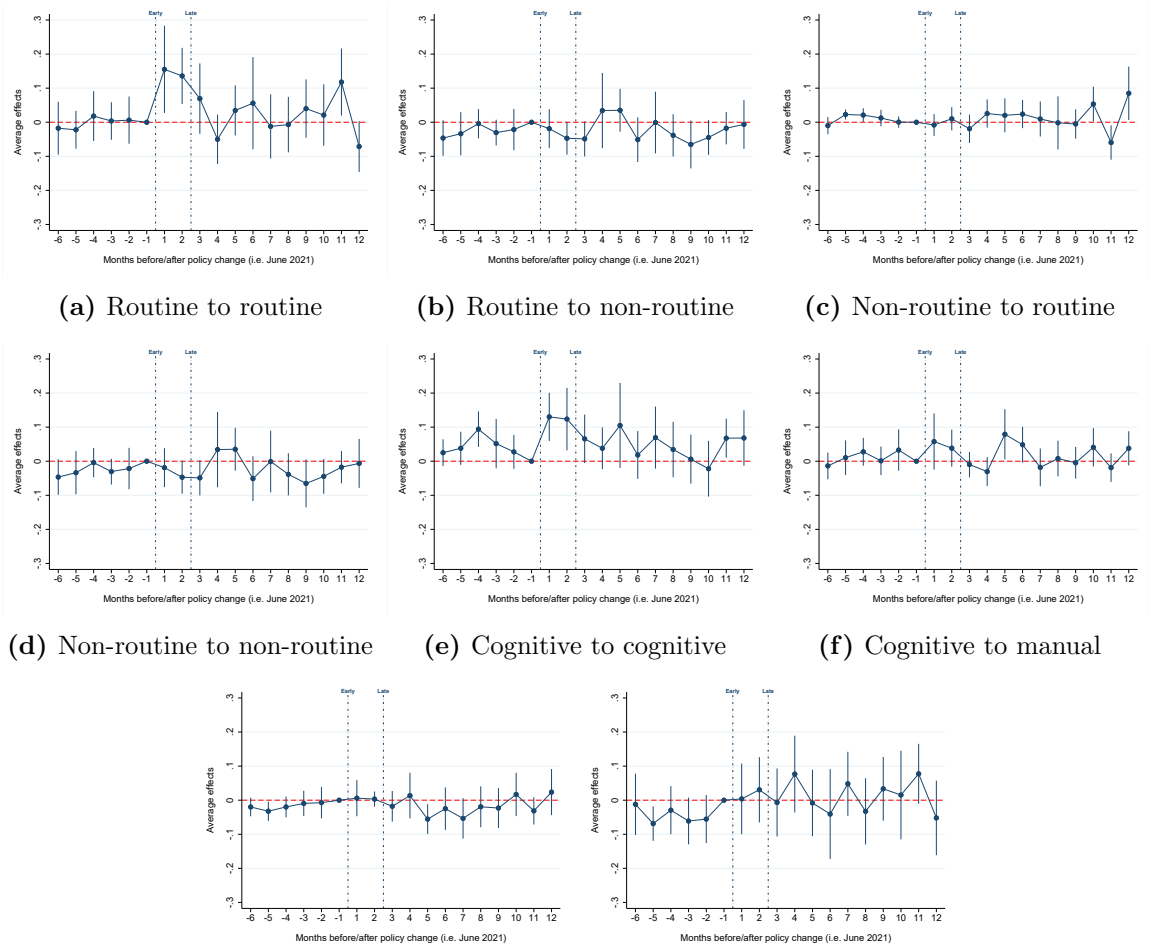
Notes: The figure reports coefficient estimates and 90% confidence intervals for a series of event-study specifications, where the dependent variable is always the probability of being employed. The analysis is presented separately by sex (panel A), age groups (i.e. below or above 40 years old, panel B), by educational attainments (i.e. less than a college or college and more, panel C), by presence of a child in the household (panel D), by household income (i.e. annual household income in the previous year below or above $50,000, panel E) and by race (i.e. White non-Hispanic or other, Panel F). In all specifications, we control for state and month fixed effects as well as for age, sex, and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Standard errors are clustered at the state level.

Figure B7: Treatment effects by occupational group, overlapping the routine vs non-routine classification with the manual vs cognitive classification of occupations



Notes: The figure reports coefficient estimates and 90% confidence intervals for β from Equation (1) from separate regressions, where the dependent variables are the different types of transitions from unemployment-to-employment by broad occupational category. The different groups have been obtained by overlapping the routine vs. non-routine classification with the cognitive vs. manual classification. In all specifications, we control for state and month fixed effects as well as for age, sex, and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Standard errors are clustered at the state level.

Figure B8: Event-study estimates on unemployment-to-employment transitions by occupational category



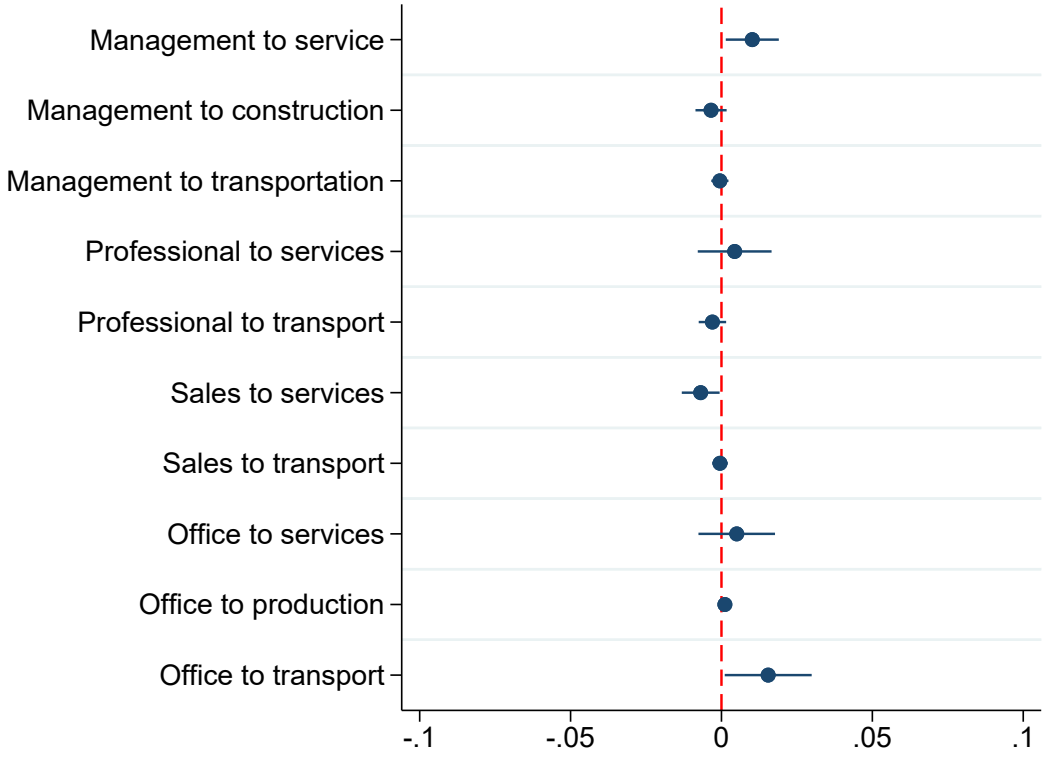
(a) Routine to routine **(b)** Routine to non-routine **(c)** Non-routine to routine

(d) Non-routine to non-routine **(e)** Cognitive to cognitive **(f)** Cognitive to manual

(g) Manual to cognitive **(h)** Manual to manual

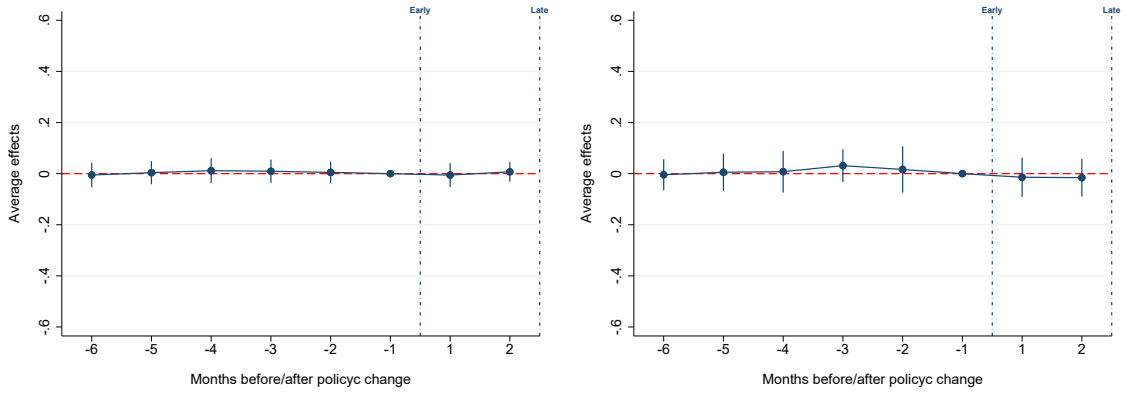
Notes: The figure reports coefficient estimates and 90% confidence intervals for a series of event-study specifications, where the dependent variables are the different types of monthly labor market transitions from unemployment to different occupations, defined by either their routine vs. non-routine or cognitive vs manual nature. In all specifications, we control for state and month fixed effects as well as for age, sex, and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Each panel also plots two vertical lines corresponding to the months of early and late policy withdrawal (June and August 2021, respectively). Standard errors are clustered at the state level.

Figure B9: Treatment effects by one-digit occupations, cognitive to manual transitions



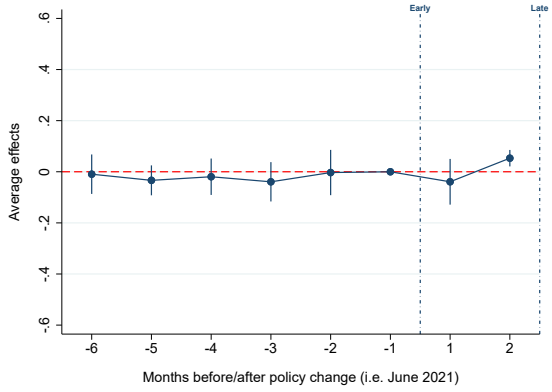
Notes: The figure reports coefficient estimates and 90% confidence intervals for β from Equation (1) from separate regressions, where the dependent variables are the different types of transitions from unemployment-to-employment. Here, we focus only on transitions of unemployed individuals with previous experience in cognitive occupations who transition to new jobs in manual occupations. In particular, we look at which specific occupations were involved in these cognitive-to-manual transitions (using the one-digit occupational classification). For ease of exposition, we only present results from the ten most common transitions. "Management" corresponds to "Management, business, and financial occupations"; "Service" corresponds to "Service occupations"; "Construction" corresponds to "Construction and extraction occupations"; "Transportation" corresponds to "Transportation and material moving occupations"; "Professional" corresponds to "Professional and related occupations"; "Sales" corresponds to "Sales and related occupations"; "Office" corresponds to "Office and administrative support occupations"; "Production" corresponds to "Production occupations". In all specifications, we control for state and month fixed effects as well as for age, sex, and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Standard errors are clustered at the state level.

Figure B10: Treatment effects on active and passive job-search methods

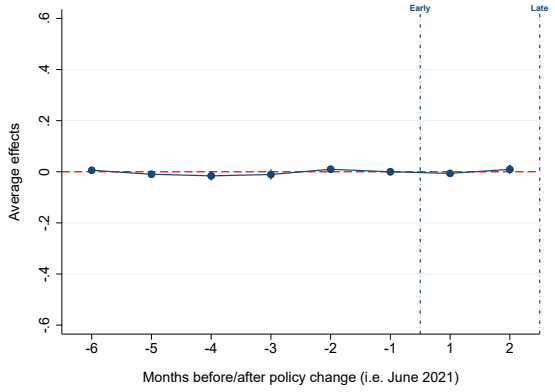


(a) Active job search (0/1)

(b) Passive job search (0/1)



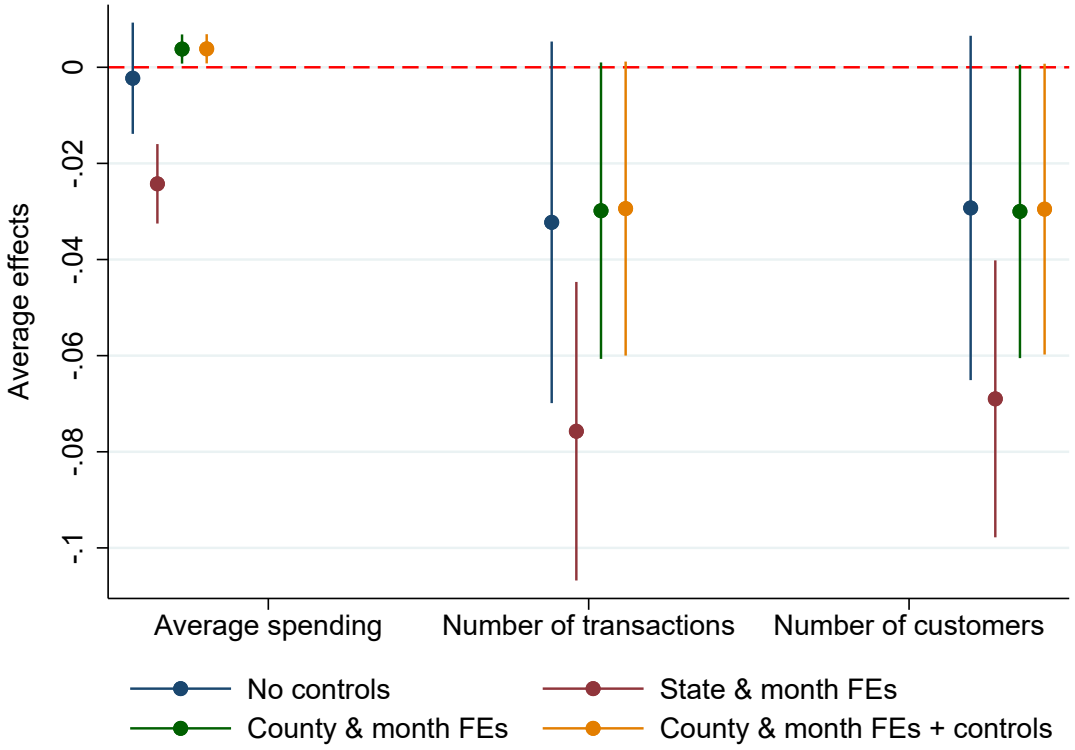
(c) Number of active job-search methods used



(d) Number of passive job-search methods used

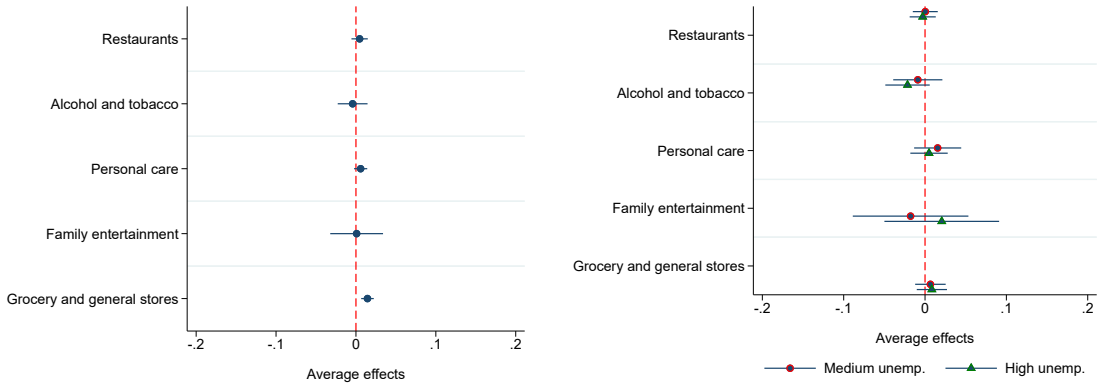
Notes: The figure reports coefficient estimates and 90% confidence intervals for a series of event-study specifications, where the dependent variables are dummies for whether the individual used any active or passive job-search method (panels A and B, respectively) and the number of active and passive job-search methods used (panels C and D, respectively). In all specifications, we control for state and month fixed effects as well as for age, sex, and dummies for educational attainments, marital status, race and ethnicity, and presence of children in the household. Standard errors are clustered at the state level.

Figure B11: Validating the consumption measures in Safegraph



Notes: The figure reports coefficient estimates and 90% confidence intervals for different regressions run using county-level data constructed from the Safegraph database. The outcomes of interest are reported on the x-axis, and they are the logarithm of spending per transaction (in the left of the figure), the logarithm of the number of transactions (in the centre of the figure) and the logarithm of the number of customers (in the right of the figure). In each case, the coefficients that are plotted are those related to the county unemployment rate. The specifications differ in the inclusion of the additional controls. In particular, we present results with no additional controls (blue dots), with state and time fixed effects (red dots), with county and time fixed effects (green dots) and with additional county-level controls for the logarithm of population density and the number of COVID-19 infections (yellow dot). County-level aggregates are obtained by using establishment-level information, and weighting each establishment by the number of transactions. Additionally, each regression weights county-level observations with the square root of county population. Standard errors are clustered at the state level.

Figure B12: Treatment effects on average spending per transaction



(a) DiD **(b)** DDD

Notes: The figure reports coefficient estimates and 90% confidence intervals for different specifications. In Panel A, we present results from simple DiD specifications as introduced in Section 4, with the only difference that the analysis is now run at the county level. In Panel B, we present results from a triple-difference approach, where we interact the standard DID term with a categorical variable grouping counties in terciles according to their unemployment rate (computed between March and May 2021 using data from the Bureau of Labor Statistics). Counties in the lowest tercile of the unemployment rate represent the omitted category in this specification, and we plot coefficients for counties with medium and high unemployment rates. In both the DiD and DDD analyses, the outcome of interest is the logarithm of average spending per transaction. In all specifications, we control for time and state fixed effects as well as county-level controls for the logarithm of population density and the number of COVID-19 infections. County-level aggregates are obtained by using establishment-level information, and weighting each establishment by the number of transactions. Additionally, each regression weights county-level observations with the square root of county population. Standard errors are clustered at the state level.