

The Effects of the Expanded Pandemic Unemployment Assistance on Labor Market Outsiders' Financial and Mental Wellbeing

Abstract (167 words)

During COVID-19, for the first time in the history of the U.S. Unemployment Insurance (UI) system, the UI benefit was extended to non-standard workers who are not typically eligible and often considered to be “labor market outsiders.” Given the outsiders’ high level of insecurity and exclusion from the social security system, this offers an important opportunity to investigate such benefits on their economic and emotional security. To estimate it, I analyze staggered difference-in-differences and triple difference models using the Household Pulse Survey. I found that states’ decisions to terminate their expanded pandemic UI program led to increases in financial and mental difficulties among residents, especially the labor market outsiders likely to be affected by the change with more difficulty in paying expenses and depression. These findings suggest that expanded eligibility for social insurance programs can help alleviate economic insecurity and associated mental health problems for categories of workers who have been excluded from the existing system, particularly in the face of unexpected crises and labor market disruptions.

I. Introduction

The COVID-19 pandemic led to severe economic fallout and job loss, and it hit non-standard workers with limited access to social insurance protection and job security the hardest (OECD 2020; Causa and Cavalleri 2020). To mitigate the economic downturn and protect those who are economically affected by COVID-19, the Coronavirus Aid, Relief, and Economic Security (CARES) Act was signed into law on the 27th of March, 2020 (National Credit Union Administration 2020). The law included a provision called Pandemic Unemployment Assistance (PUA) to extend eligibility toward uncovered non-W2 employees such as the self-employed, freelancers, independent contractors, or gig workers (U.S. Department of Labor, 2020b). These “labor market outsiders” are traditionally excluded from such social insurance programs in the U.S. and other countries (Doeringer & Piore 1971). The expanded UI program would eventually expire on September 6th, 2021. However, beginning in May 2021, at least 16 states announced that they would end these benefits ahead of schedule (Iacurci 2021). This abrupt early withdrawal dramatically changed the policy landscape for workers, especially the aforementioned “outsiders,” in certain states but not others. This presents a unique research opportunity to assess the effects of a social insurance expansion to a large and growing category of economically precarious workers.

In this study, I investigate whether the early termination of expanded UI benefits affected workers’ financial and mental well-being, and whether these effects differed based on their labor market status. Specifically, this paper seeks to answer two research questions. First, “Does the early withdrawal of pandemic UI benefits affect individuals’ financial hardship and mental health?”, and second, “Is the effect more pronounced for non-standard workers?” To answer these questions, I use staggered difference-in-differences (DID) and a difference-in-difference-in-differences (DDD or triple difference) models using data from the Household Pulse Survey (HPS),

which is administered by the U.S. Census Bureau to study the social and economic effects of COVID-19 on American households. The results show that the early termination of pandemic UI benefits increased financial hardships among the general public. Specifically, reported difficulty in paying expenses increased by 0.03 standard deviations and confidence in paying rent declined by 0.017 standard deviations. The policy change also had an adverse impact on mental health, increasing depression by 0.028 standard deviations. Focusing on workers who are not covered by employer-sponsored health insurance, a proxy for non-standard worker or “outsider” status, the early termination of benefits is associated with larger increases in difficulty paying expenses (0.065 standard deviations) and depression (0.045 standard deviations), and a larger decrease in confidence in paying rent (0.051 standard deviations). Findings from triple differences also show that the early termination of benefits is associated with the increase in difficulty paying expenses (0.39 standard deviations) and depression (0.34 standard deviations), and a decrease in confidence in paying rent (0.33 standard deviations). The findings show that early termination of pandemic UI benefits had negative effects for residents in general, but was more harmful for non-standard workers.

This study contributes to our understanding of how expanded eligibility for social insurance programs can help alleviate economic insecurity and associated mental health problems for categories of workers who have been excluded from the existing system, particularly in the face of unexpected crises and labor market disruptions. Given the growth of non-standard work, including the “gig economy,” (Anderson et al. 2021) these findings offer insight on how existing social insurance systems can be updated to provide stability and security for an increasingly precarious workforce.

II. Literature Review

Non-standard Workers as Labor Market Outsiders

The concept of a dual labor market is crucial to understanding the distinction I make in this study between standard and non-standard workers. A dual labor market is divided between an “internal” (ILM) and an “external” labor market (ELM). An ILM provides distinct privileges and a superior position, and the “labor market insiders” in this market enjoy high pay, a high degree of job security, and strong social insurance protections through employer-provided benefits (which are often tax-advantaged) and government benefits (like UI) tied to specific kinds of work (Doeringer & Piore 1971; Piore 1980; Weiss 1980). The labor market “outsiders” in the ELM, by contrast, are offered relatively low wages, less secure employment, and few social insurance protections (Freeman & Katz; Rueda 2005). Notably, labor market dualization tends to be more pronounced in countries with segmented social security systems that privilege some kinds of work over others (Hausermann & Schwander 2012; Burgoon & Dekker 2010). These differences in both private and public benefits mean that labor market outsiders face higher levels of economic risk and high uncertainty for prospective risk than insiders (Standing 2011). For example, in the U.S., employer-sponsored health insurance (ESI) is limited to insiders, which makes outsiders such as temporary workers, the self-employed, freelancers, and gig economy workers more exposed to uncertainty and the risk of medical bankruptcy (Campbell 2014). Thus, these workers’ lack of coverage by ESI is directly related to their limited access to social protection and implies their labor market status as labor market outsiders in the U.S. context, making insurance status a credible proxy variable for outsider status.

The Effects of the Generosity of UI Benefits

Existing literature on the effects of relatively generous UI benefits has mostly focused on whether UI expansion affects job search behavior, and has found that expansion negatively affects the rate of employment (Schmieder et al. 2016; Cortes and Forsythe 2021; Holzer et al. 2021). Notably, Farber et al. (2015) found that the extension of UI benefit durations slightly reduced the exit rate from unemployment by using Current Population Survey (CPS) data during the Great Recession. Also, several studies investigated the effects of the expansion of UI benefits on employment during the COVID-19 pandemic. Coombs et al. (2022) found that ending Pandemic UI benefits increased employment by analyzing 19 states where Pandemic UI expired in June versus 23 states that decided to retain the federal benefits through September using anonymous bank transaction data. Similarly, Ganong et al. (2021) found that job search responses are less than expected when extended benefits are available by using anonymized bank account data. Holzer et al. (2021) also found a sharp rise in transitions from unemployment to employment with the withdrawal of UI benefits. These studies have also investigated how the pandemic UI expansion affected spending. Coombs et al. (2022) found that the loss in UI benefits led to a sharp drop of around 20 percent in spending among beneficiaries. Similarly, Ganong et al. (2021) found that expanded benefits caused an increase in spending, and the expiration of the \$600 supplemental benefit led to an immediate decline. Taken together these findings show that generous UI benefits offer a trade-off between consumption-smoothing benefits and the moral hazard of discouraging job search.

While this work offers useful findings about the effects of the generosity of UI benefits, it leaves some questions unanswered. First, few studies have explored the effect of expansion on non-standard workers or labor market outsiders, meaning that most existing literature assumes

homogenous effects of the policy regardless of work arrangement, even though non-standard workers have not typically been eligible for UI. The few studies that have considered the possibility of heterogeneous effects of social policies by work arrangement have focused on Europe (e.g., Spasova et al. 2021) or the expansion of Medicaid eligibility in the U.S. (e.g., Berdahl and Moriya 2021).

It is also worth noting that previous studies on expanded UI have focused on its effects on labor supply from the macroeconomic perspective. However, since the labor market arrangement of non-standard workers is different from those in traditional jobs, traditional measures of employment do a poor job of accounting for such workers (Risher 2020; Duca 2019; Economic Policy Institute 2017). Instead, in this study I focus on the substantive purpose of UI benefits: improving the well-being of the unemployed, as measured by financial stress and mental health.

Holzer et al. (2021) present evidence about the effect of the early expirations of UI on financial stress. However, they do not distinguish between standard and non-standard employees or consider the dynamic timing of termination in different states. Therefore, this study analyzes whether early withdrawal of the expanded pandemic UI benefits affected workers' financial and emotional situations, and how these effects differ depending on whether they are labor market insiders or outsiders.

III. Background of Pandemic UI Benefits

On March 27th, 2020, the CARES Act expanded the generosity of unemployment benefits to respond to the emerging pandemic.¹ The CARES Act established the Pandemic Unemployment

¹ The law also established Pandemic Emergency Unemployment Compensation (PEUC), which provided an extra thirteen weeks of unemployment benefits on top of the typical period of 26 weeks in most states (Center on Budget and Policy Priorities 2023).

Assistance (PUA) that extended eligibility for unemployment benefits to non-standard workers those who are not W-2 employees such as self-employed workers or gig workers who are typically ineligible for regular UI benefits. Also, the Federal Pandemic Unemployment Compensation (FPUC) program supplemented the existing standard UI benefits of \$600 weekly for those receiving unemployment benefits from April to July and was partially resumed at \$300 in January 2021 (U.S. Department of Labor 2020a).

These benefits originally lasted through July 31st, 2020, but after that, several subsequent relief packages were passed including the Consolidated Appropriations Act (CAA) in December and the American Rescue Plan Act (ARPA) in March 2021 that extended both PUA and FPUC. In March 2021, President Biden signed the American Rescue Plan, which further extended PUA and FPUC (at \$300) through September 6th, 2021. However, increasing concerns about worker shortage led 18 states to opt out of the pandemic UI benefits in June (Committee for a Responsible Federal Budget 2021), and two states opted out of both programs in July (Congressional Research Service 2021). The timeline of the PUA program is summarized in Figure A1 in Appendix A, and based on that, the period of the analysis is from February to August 2021.

Table 1 shows a list of when pandemic unemployment benefits ended in each state. Twenty-four states and the District of Columbia participated in both programs, forming the control group for the analysis presented in this study. The treated states include 18 states that withdrew benefits in June and 2 that did so in July. I exclude four exceptional states (Alaska, Arizona, Florida, and Ohio) that kept PUA while opting out of FPUC early since this policy change could bias financial difficulty in the control group upward. Two other states, Indiana and Maryland, announced the early withdrawal of expanded benefits, but were forced to continue paying benefits by court decisions (Ewall-Wice 2021). I include these two states in the control group.

Early expiration of PUA and FPUC in June (18 states)	Expiration of PUA and FPUC on September 4 th (24 states & Washington, D.C.)	Exception (6 states)
<ul style="list-style-type: none"> - June 12th (3 states) Iowa, Missouri, Mississippi - June 19th (8 states) Alabama, Arkansas, Idaho, Nebraska, New Hampshire, North Dakota, West Virginia, Wyoming - June 26th (7 states) Georgia, Montana, Oklahoma, South Carolina, South Dakota, Texas, Utah 	California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Kansas, Kentucky, Maine, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New Mexico, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, Wisconsin	<ul style="list-style-type: none"> - Keep PUA until Sept., but withdraw FPUC: Alaska (June 12th), Arizona (July 10th), Florida (June 26th), Ohio (June 26th) - Announce to withdraw, but keep it by September Indiana (announced end on June 19th), Maryland: (announced end on July 3rd)
Early expiration of PUA and FPUC in July (2 states)		
<ul style="list-style-type: none"> - July 3rd (2 states) Louisiana, Tennessee 		
<p>Notes:</p> <ul style="list-style-type: none"> - States ending PUA benefits in June 2021 do not include Tennessee and Louisiana, which terminated payments in July 2021 - Indiana and Maryland announced termination of the benefits but a court reinstated payment. - Alaska, Arizona, Florida, and Ohio terminated participation in FPUC early, but not PUA. Except for these four states, FPUC was expired at the same time as PUA was expired. 		

Table 1. List of States Ending PUA²

IV. Data and Methodology

To answer the research questions of whether early withdrawal of the pandemic UI benefits affected workers' financial and mental well-being, and whether it affects non-standard workers

² As a main interest of pandemic UI benefits in the paper is the PUA benefit which expanded to the eligibility toward non-standard workers, I stated the expiration of PUA. However, other states except the four states which have different withdrawal dates of PUA and FPUC programs kept and withdrew the benefits for same time period.

more strongly, I analyze the HPS dataset using staggered difference-in-differences (DID) and triple differences (DDD) models.

HPS Data and Variables

The HPS is a survey administered by the U.S. Census Bureau intended to study how COVID-19 impacts households across the country on a range of economic, social, and health-related measures. The HPS is a repeated cross-section that has been collected biweekly since April of 2020, making it an ideal candidate for event study and DID studies over this time period. The HPS also has a large sample size, with an average of around 72,000 respondents per survey round.

For this study, I use HPS data from February 15th, 2021 to August 30th, 2021, encompassing 13 survey rounds and around 850,000 observations. The sample includes respondents with birth years from 1933 to 2003. Since the sample includes some retirees who are covered by Medicare, I conducted robustness checks using the sample without Medicare recipients and respondents over 65 in Appendix B (Table B4) which show similar results to the main results. Also, the cross-tabulation of ESI and Medicare recipients is provided in Appendix B (Table B5-2). The choice of February as the starting point ensures an adequate number of pre-treatment periods. Selecting August as the endpoint makes the control group a “never-treated group,” which recent methodological literature establishes as important for preventing bias in staggered DID designs (Goodman-Bacon 2021). Staggered DID is for analyzing the effects when the policy is introduced in many different states over multi-time periods (Callaway & Sant’Anna 2021), but without never-treated units in the sample, two relative time indicators need to be omitted to avoid multicollinearity (Borusyak et al 2024; Baker et al 2022).

To measure financial difficulty, I use two dependent variables: ‘to what extent the respondents have difficulty paying expenses in general’ and ‘to what extent they are confident in their ability to make their next rent or mortgage payment on time’. ‘Difficulty in paying expenses’ is phrased, “In the last 7 days, how difficult has it been for your household to pay for usual household expenses, including but not limited to food, rent or mortgage, car payments, medical expenses, student loans, and so on?”, and coded as a 4-point Likert scale from ‘1: not at all difficult’ to ‘4: very difficult’. ‘Confidence in paying rent or mortgage’ is phrased, “How confident are you that the household will be able to pay the next rent or mortgage payment on time?”, and coded as a 5-point Likert scale with ‘payment is/will be deferred.’ as 5. To ensure that the variable measures confidence in paying rent or mortgage, I re-code these values as missing, leaving a 4-point Likert scale ranging from ‘1: no confidence’ to ‘4: high confidence’.³ The full tabulation and the summary statistics of the proportion of people who respond to this option are provided in Appendix A (Table A3). Also, the robustness check with code the response as 1 is provided in Appendix A (Table A3: Panel C).

To capture mental well-being, I use a variable measuring the frequency of feeling depressed over the previous 7 days, which is asked with the question, “Over the last 7 days, how often have you been bothered by feeling down, depressed, or hopeless? Would you say not at all, several days, more than half the days, or nearly every day? Select only one answer.” This variable is coded as from ‘1: not at all’ to ‘4: nearly every day’ with a 4-point Likert scale.

Ideally, I would distinguish between labor market insiders and outsiders with a question that directly asks respondents about the type of work that they do, but none of the questions in the HPS

³ “Payment is/will be deferred” could be also interpreted as a low-level of confidence if the decision to defer the payment is made by the respondents not by the landlord. However, in the HPS data, the detailed reasons behind it are not provided. That is why I recoded this value as missing. The percentage of “Payment is/will be deferred” response is 1.18%.

specifically identifies non-standard workers.⁴ Thus, I use a proxy variable measuring whether the respondent is covered by either employer-sponsored health insurance (ESI) or Medicare.⁵ I added robustness checks by dropping Medicare beneficiaries from the analysis and dropping those over 66 in Appendix B (Table B4). Usually, those covered by employer-sponsored benefits or social insurance programs are considered labor market insiders, whereas those not covered are considered outsiders, especially in the U.S. context. In this sense, this proxy measuring health coverage reflects whether respondents are the target of PUA benefits as non-standard workers. The questions are phrased, “Are you currently covered by insurance through a current or former employer or union (through yourself or another family member)?” and “Are you currently covered by Medicare for people 65 and older, or people with certain disabilities?” I generated a new binary variable coded 1 for labor market outsiders and 0 for insiders based on whether they are covered by Medicare or ESI. A validity check of the ESI variable about whether it is related to sectors of employment is provided in Appendix B (Table B5-1) which shows the percentage of the self-employed among those who are not covered by ESI (31.22%) is higher than that of those with ESI (9.14%). Summary statistics tables about the main variables are provided in Appendix A (Table A2).

⁴ The HPS data has the variable, the sector of employment which asks, “Are you employed by the government, by a private company, a nonprofit organization or were you self-employed or working in a family business?”. Even though it includes some categories of non-standard workers (‘Self-employed’ and ‘Working in a family business’), this variable is not a perfect fit for the research models for a major reason. The concept of non-standard workers is not mutually covered by the variable. Some workers could be non-standard workers even if they choose categories like the government, private, and nonprofit sectors. Thus, I chose the variable based on employer sponsored insurance estimates that the percentage of labor market outsiders is 15.9% (107,896 out of 678,819).

⁵ As a contributory social insurance program, Medicare (public health coverage for people 65 and above), like Social Security and traditional UI, is a benefit for labor market insiders.

The Multiple Periods of Withdrawal in PUA

Table 1 shows the specific date when each state expires the PUA programs according to state government websites and the Congressional Research Service (2021).⁶ Twenty states terminated PUA early, while 24 states and the District of Columbia kept the benefits until September. According to the Congressional Research Service (2021), the 11 states that withdrew from the Pandemic UI programs on June 12th and 19th are considered as early withdrawal states.⁷ The 9 states that terminated the PUA programs on June 26th and July 3rd are considered late withdrawal states.⁸ The 24 states and the District of Columbia that chose to keep the PUA until September and the two states that were compelled to do so by court decisions are the untreated control group.⁹ The 4 exceptional states (Alaska, Arizona, Florida, and Ohio) that kept the PUA program until September but withdrew from the FPUC program early are not included in the analysis, since this policy change could bias financial difficulty in the control group upward. Since UI policy changes only have direct effects on those who are unemployed rather than the public at large, any effects detected by these analyses are best understood as “intent to treat” estimates. Limiting the sample only to the unemployed respondents in each cross-sectional wave of the HPS would introduce bias, since policy change may affect job search behavior and therefore employment status. To check the post-treatment bias introduced by the ESI measurement, a placebo test is provided in Appendix C (Table C1) which shows the estimate is not statistically significant, thus there is no significant concern about the possibility of post-treatment bias.

⁶ Table A1 in Appendix shows the survey weeks, and how each early-treated, late-treated, and control states are set up.

⁷ Early treated states (11 states): Withdrawal on June 12th - Iowa, Missouri, Mississippi. Withdrawal on June 19th - Alabama, Arkansas, Idaho, Nebraska, New Hampshire, North Dakota, West Virginia, Wyoming

⁸ Late-treated states (9 states): Withdrawal on June 26th - Georgia, Montana, Oklahoma, South Carolina, South Dakota, Texas, Utah. Withdrawal on July 3rd - Louisiana, Tennessee

⁹ Control states (26 states and Washington, D.C.): California, Colorado, Connecticut, Delaware, District of Columbia, Hawaii, Illinois, Indiana, Kansas, Kentucky, Maine, Maryland, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New Mexico, New York, North Carolina, Oregon, Pennsylvania, Rhode Island, Vermont, Virginia, Washington, Wisconsin

Pandemic-related control variables

I use a measure of state-level COVID-related policy and the number of COVID-19 cases as control variables. Both could be important confounding factors, since they may affect the outcomes of interest (financial and mental well-being) and correlate with state UI policy choices. The source of this data is the Oxford COVID-19 Government Response Tracker (OxCGRT). Specifically, the COVID-relevant policy is recorded as a stringency index which means the strictness of social distancing policies by states to reflect the severity of restriction designed to deal with COVID-19 (Lee et al. 2021; Holzer et al. 202; Hallas et al. 2021).¹⁰ OxCGRT calculates the index and the number of new cumulative COVID-19 cases for all 50 states and the District of Columbia daily, so I averaged the daily values of the index, and merged them with the HPS dataset.

Empirical Strategy: Staggered DID and Triple Difference

Staggered DID

The objectives of this staggered difference-in-differences (DID) analysis are twofold: 1) to estimate the effect of early termination by comparing treated states and control states; 2) to determine if effects differ by labor market status by conducting staggered DID with subgroup analysis. DID analysis relies on the parallel trend assumption, in which in the absence of treatment, the changes in the outcomes in the treatment group would have been similar to the changes in the outcomes in the control group (Bertrand et al. 2004). Specifically, the identification assumption of the DID framework of this paper is that the financial and mental well-being in the early terminated states and control states that keep the pandemic UI benefits would have shared the same trends in

¹⁰ The stringency index is measured with nine metrics: school closures, workplace closure, cancellation of public events, restrictions on public gatherings, closures of public transport, stay-at-home requirements, public information campaigns, restrictions on internal movements, and international travel controls.

the absence of the policy intervention. As the termination of the policy happened at different times in different states, I apply a staggered DID framework to examine whether and to what extent the early withdrawing pandemic UI programs affect the financial and mental difficulties of those in the early withdrawal states. Staggered DID has the advantage of estimating policy effects at multiple time points across different groups (Goodman-Bacon 2021; Callaway & Sant’Anna 2021). The staggered DID with two-way fixed effects (TWFE)¹¹ model I used is as follows:

$$Y_{ist} = \beta_0 + \beta_1(Expired_{st}) + \delta_1 \ln(Covidcases_{st}) + \delta_2 Social\ distancing\ policy_{st} + \theta_s + \gamma_t + \epsilon_{ist} \quad (1)$$

where i denotes survey respondents, s denotes state, and t denotes survey week. Y_{ist} is the outcomes of interest for the individual i , in state s , and in time t . $Expired$ is an indicator dummy variable, which is one for the treated states after they terminate the policy and zero otherwise. The key parameter of interest is β_1 . δ_1 and δ_2 are the estimates of state-level COVID-related control variables. θ_s and γ_t are the state and time-fixed effects respectively, and ϵ_{ist} is the idiosyncratic error.

One potential concern is a bias of the estimates from staggered DID with TWFE. TWFE is the generalization of canonical DID containing two time periods and two groups for causal inference, and it has been implemented for estimating the effect of a treatment on an outcome with control for group and time-fixed effects (Angrist and Pischke 2009; De Chaisemartin & D’Haultfoeuille 2020; Goodman-Bacon 2021; Wooldridge 2021). However, recently, scholars caution against the

¹¹ The standard “two-way fixed effects (TWFE)” model uses unit fixed effects and time fixed effects. Unit fixed effects remove unit-specific time averages and apply pooled ordinary least squares (OLS) to the transformed data, whereas time fixed effects remove secular changes in the socio-economic environment that have the same effect on all units but vary over time (Wooldridge 2021).

validity and interpretation of the estimated coefficient from TWFE with staggered treatment timing because of the negative weights of some treatment effects across groups and times, even when the parallel trends assumption holds, especially if the effect is heterogeneous and dynamic (Baker et al 2022; Callaway and Sant’anna 2021; De Chaisemartin & D’Haultfoeuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021). To reduce the bias, the treatment should be homogenous across groups and over time, but it is not realistic that the treatment effect is constant (Goodman-Bacon 2021; De Chaisemartin & D’Haultfoeuille 2020). Thus, the estimates from a conventional DID with staggered treatment timing could lead to bias, but this is unlikely in the present case since there is little variation in treatment timing (two different time points). A robustness check with the DID_M estimator, a proposed solution by De Chaisemartin & D’Haultfoeuille (2020) to address the bias from negative weights when the average treatments are heterogeneous, is in Appendix B (Table B2).

Event Studies

I use event study models to capture the dynamic effect of the early withdrawal from pandemic UI benefits, as well as to test whether each group of states (early-treated, late-treated, and control) were on similar trends prior to the policy intervention. Event studies trace each period's effects by including a series of dummy variables (Baker et al. 2022; Borusyak et al. 2024; Sun and Abraham 2021). Specifically, I estimate the following equation:

$$Y_{ist} = \beta_0 + \sum_{e \neq 0} \vartheta_e * D_{st}^e + \delta_1 \ln(Covidcases_{st}) + \delta_2 Social\ distancing\ policy_{st} + \theta_s + \gamma_t + \epsilon_{ist} \quad (2)$$

where ϑ_e is a parameter that captures the dynamic effect of the policy, and D_{st}^e is an indicator for states s being e periods away from their initial treatment (terminating the policy) at calendar time t . Other variables in equation (2) are defined in equation (1).

In terms of the baseline week for the event study, in the case of the financial difficulty in paying expenses and depression, I chose the baseline week when the UI benefit is terminated, period =0 rather than the week right before the policy intervention, period = -1 given the nature of the HPS question. Some existing studies have a reference point of the -1 period because the effect is valid when the policy intervention happens (period =0), but the lookback window of the “previous 7 days” in the HPS questions on paying expenses and depression invalidates the pre-period as the reference period. On the other hand, the question measuring confidence in the ability to pay rent or mortgage references the *next* rent or mortgage payment, so I chose the pre-event period as the baseline for analysis using this dependent variable. A robustness check to see how the announcement itself could affect financial difficulty and mental health is provided in Appendix B (Table B6). The earliest announcement was made in Montana on May 4th. The robustness check of the DID analysis shows that the announcement of early termination mainly increased depression.

Triple differences

The triple differences models test for heterogeneous effects depending on labor market status. Even though the staggered DID with subgroup analyses provides some evidence of heterogeneous effects, it requires two parallel trend assumptions – parallel trends “between non-standard workers across states” and “between standard workers across states.” In contrast, the triple difference estimator is computed as the difference between two difference-in-differences estimators (Olden and Møen 2022). The triple differences do not require two parallel trend assumptions for causal

interpretation because the difference between two biased difference-in-differences estimators will be unbiased as long as the bias is the same in both estimators (Olden and Møen 2022). Rather, the triple difference model requires one parallel trend assumption that the relative outcomes of group A and group B (the gap between the groups) in the treatment state trend in the same way as the relative outcomes of group A and group B in the control state in the absence of treatment. One challenge of the DID model is that there might be some time-variant confounders that bias results. For example, if the controlled states choose to terminate Pandemic UI benefits because of budget constraints, the DID estimates will not provide a pure treatment effect. Thus, having a second control group, standard workers or labor market insiders in the same state under the same economic environment guards against such bias.

The triple difference equation to test is as below:

$$Y_{ist} = \beta_0 + \beta_1(Outsider_i \times Expired_{st}) + \theta_{st} + \gamma_{it} + \sigma_{is} + X_{ist} + \epsilon_{ist} \quad (3)$$

the newly added estimators from equation (1), first, β_1 is the triple difference estimator which is a key parameter of interest indicating the divergent effect of the policy of the withdrawal pandemic depending on the labor market status.¹² θ_{st} , γ_{it} , and σ_{is} are State-Time FE, Time-Work status FE, and State-Work status FE respectively. X_{ist} is the vector of individual-level control variables including gender, race, educational level, and income.¹³

¹² Compared to equation (1) for staggered DID, equation (3) does not include pandemic-related control variables as they are subsumed by the state-by-week FE.

¹³ Income is measured by the question, ‘In 2019 what was your total household income before taxes?’ with 8 ordinal response options. Since the question measures pre-pandemic income, the variable is not affected by pandemic policy changes.

V. Results

I begin by displaying the raw data in a set of figures. Figure 1 shows the mean of each dependent variable over time for early-treated states, late-treated states, and never-treated states. Figure 1 shows that the trends of the control and two treated groups have a similar trajectory prior to the withdrawal of benefits. Also, it shows divergent trajectories after the treatment, with the exception of the early-treated states for the depression measure (Figure 1c). This suggests that it is reasonable to assume that financial difficulty would not have increased in the treated states in the absence of the early withdrawal from pandemic UI programs. In that sense, Figure 1 graphically supports the parallel trends assumption of DID that the treated and untreated states would have shared the same trend in the outcomes of interest in the absence of the treatment.

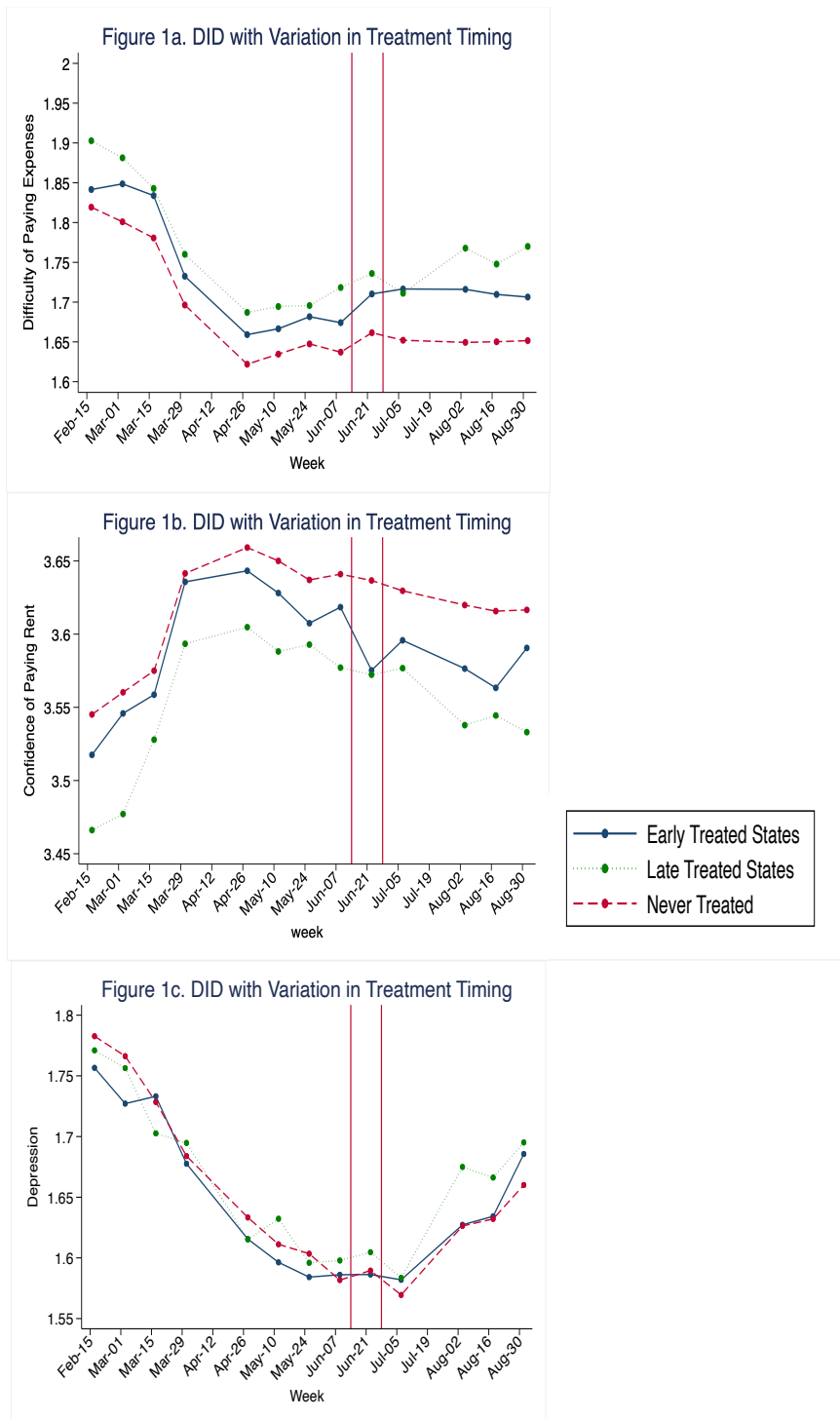


Figure 1

The Average of Financial Difficulty and Depression Before and After Withdrawal PUA, by Week of Withdrawal (note: the first vertical line [on 6/15] specifies the early withdrawal date, and the second vertical line [on 6/29] specifies the late withdrawal date.)

Event Study Results

Figure 2 graphically presents the results of the event study model, which traces the dynamic effect of early withdrawal from PUA on financial and mental difficulties and shows pre-treatment trends to provide evidence for the parallel trend assumption.

Figure 2 shows that the coefficients on the relative time dummies are all centered around zero and are statistically insignificant in the pre-treatment period. However, Figure 2c shows a slight upward pre-treatment trend for the depression effect which suggests that the DID estimate might be upward biased. Also, this upward trend might be due to the anticipation effect of policy termination. This upward trend of depression is aligned with the effect of the announcement of the policy termination on depression in Appendix B (Table B6). Considering the early treatment states terminated the policy in mid-June and that each survey period is two weeks, it may capture the anticipation effect of policy termination as some states announced the withdrawal around the beginning of May 2021 as Table B6-1 in Appendix B shows.

Figure 2 presents evidence of the increases in the difficulty of paying expenses (Figure 2a) and depression (Figure 2c) only after the pandemic UI benefits terminated which indicates the results are not biased by pre-existing trends, but Figure 2b does not strongly confirm the expectation of a negative effect on confidence in paying rent or mortgage because the coefficients after the policy intervention are statistically insignificant.

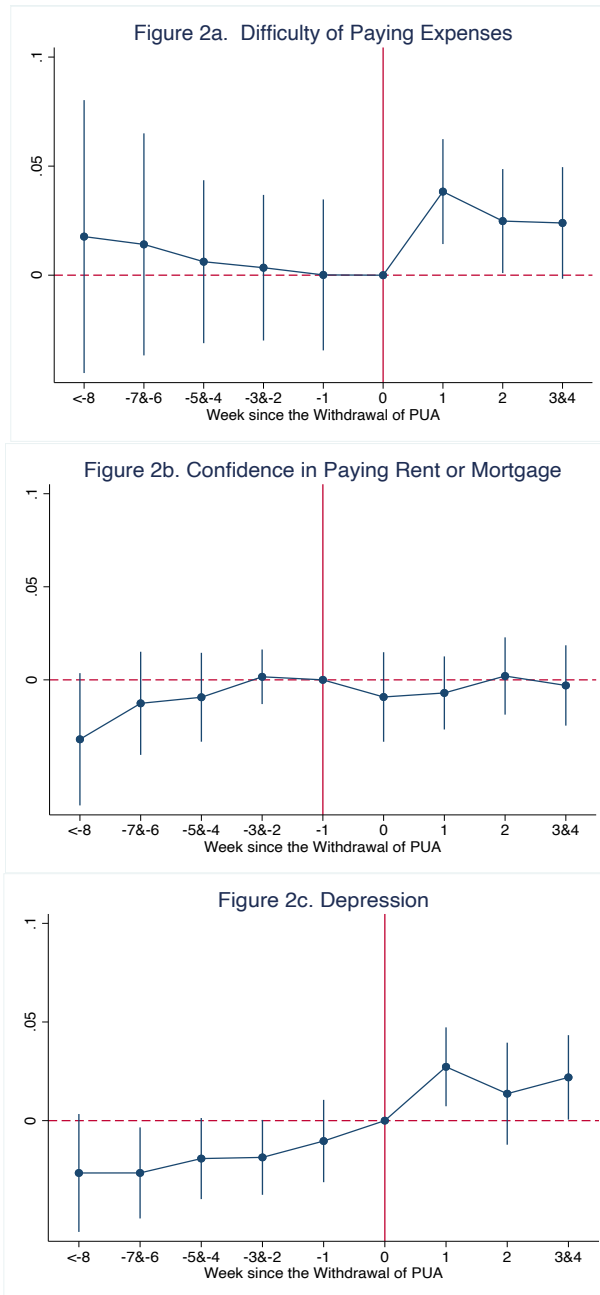


Figure 2
Event Study DID Plots

Difference-in-differences Results

Table 2 presents DID estimates of the impacts of early termination of pandemic UI programs on financial and mental difficulties. Each panel shows results for three different dependent variables. The results in Panel A indicate that early termination significantly increased reported difficulty of paying expenses by 0.0331(0.033 SD), decreased confidence in paying rent by 0.016 (0.02 SD), and increased depression by 0.0307 (0.033 SD). These effects remain statistically significant when controls for COVID-related factors are included, though they are reduced in magnitude. In Appendix B, the results of robustness checks with treatment as a proportion (Table B1) and with respondents dropping Medicare recipients or over 66(Table B4) also show that the effect of policy termination on the financial difficulty of outsiders is larger than that of insiders. In Appendix C (Table C2), a placebo test with dependent variables of ‘expected household job loss’ and ‘housing owned’ is provided. These two variables are supposed to be not affected by policy change, the early termination of the benefit. The results of the placebo test show that the null effect.

Panels B and C estimated the DID models on labor market “outsiders” and “insiders,” respectively. The results are consistent with the argument that early termination had more pronounced negative impacts on outsiders – the effects are larger for the outsider subgroup for each dependent variable, and these differences are visible whether or not COVID-related controls are included. All subgroup effects remain statistically significant with the exception of the housing payment analysis with controls for labor market insiders. I now turn to the triple-differences analysis to determine if these apparent differences in effects are statistically significant.

Table 2 DID Estimates of the Effects of Early Withdrawal on Financial and Mental Difficulties
Panel A. Overall Sample

DVs	(1) Difficulty of paying expenses	(2)	(3) Confidence in paying rent or mortgage	(4)	(5)	(6) Depression
Early Withdrawal States	0.0330*** (0.0063)	0.0296*** (0.007)	-0.0163*** (0.0057)	-0.0136** (0.0067)	0.0317*** (0.0065)	0.0260*** (0.0069)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID-related Controls	No	Yes	No	Yes	No	Yes
Observations	677,526	677,526	458,777	458,777	677,044	677,044

Panel B. Subgroup Analysis of Labor Market Outsiders

DVs	(1) Difficulty of paying expenses	(2)	(3) Confidence in paying rent or mortgage	(4)	(5)	(6) Depression
Early Withdrawal States	0.0613*** (0.0182)	0.0640*** (0.0205)	-0.0419** (0.0182)	-0.0375 (0.0234)	0.0404*** (0.0148)	0.0417** (0.0165)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID-related Controls	No	Yes	No	Yes	No	Yes
Observations	107,671	107,671	76,852	76,852	107,550	107,550

Panel C. Subgroup Analysis of Labor Market Insiders

DVs	(1) Difficulty of paying expenses	(2)	(3) Confidence in paying rent or mortgage	(4)	(5)	(6) Depression
Early Withdrawal States	0.0277*** (0.0061)	0.0235*** (0.0068)	-0.0121** (0.0057)	-0.00998 (0.0066)	0.0298*** (0.0064)	0.0231*** (0.0070)
Week FE	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID-related Controls	No	Yes	No	Yes	No	Yes
Observations	569,855	569,855	381,925	381,925	569,494	569,494

Notes: COVID-related controls include state-level stringency index and COVID-confirmed cases from OxCGRT. Standard errors are clustered at the state level. Standard errors in parentheses. The number of observations varies across columns as the numbers of respondents to each dependent variable vary.
*** p<0.01, ** p<0.05, * p<0.10 (two-tailed p-values)

Triple differences Results

The underlying logic of DDD specifications relies on scenarios where some units in the treated states are theoretically not as affected by a given treatment (Olden and Møen 2022). If standard workers in the treated states are not affected by the termination of the benefits as much as non-standard workers are, the estimators of triple difference will have some advantages to reduce bias.

Table 3 presents the triple difference estimates. As the subgroup analyses in Table 2 established, the effects are all in the expected directions, consistent with the hypothesis that the negative effects of early termination were stronger for labor market outsiders. For the dependent variables measuring the difficulty of paying expenses, confidence in making housing payments, and depression, the effect is statistically significant whether or not demographic controls are included ($p < .01$ in both models). The results imply that the termination of pandemic UI benefits increased financial and mental difficulties for labor market outsiders to a greater degree than insiders.

Table 3. Triple difference Estimates of the Effects of Early Withdrawal on Financial and Mental Difficulties

DV	(1) Difficulty of paying expenses	(2) Difficulty of paying expenses	(3) Confidence in paying rent or mortgage	(4) Confidence in paying rent or mortgage	(5) Depression	(6) Depression
Labor market outsiders in early withdrawal states	0.507*** (0.0219)	0.387*** (0.0159)	-0.316*** (0.0227)	-0.268*** (0.0184)	0.363*** (0.0132)	0.313*** (0.0118)
State and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Time and Work Status FE	Yes	Yes	Yes	Yes	Yes	Yes
State and Work Status FE	Yes	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	Yes	No	Yes	No	Yes
Observations	677,526	677,526	458,777	458,777	677,044	677,044

Notes: Demographic controls include gender, race, educational level, and income. Standard errors in parentheses are clustered at the state level. The number of observations varies across columns as the numbers of respondents to each dependent variable vary. *** p<0.01, ** p<0.05, * p<0.10 (two-tailed p-values)

Discussion and Conclusion

This paper empirically investigates how the early termination of pandemic UI benefits affects an individual's financial and mental difficulties, and how it is divergent depending on their labor market status. Especially, given the non-standard workers' economic vulnerability by being excluded from the regular UI system and the PUA benefit was the first to expand the eligibility of UI benefits, I expect the effects of the early termination of the benefits would differ. The findings from DID with sub-group analysis present that the early termination of the UI benefits increases financial difficulty and depression. Also, non-standard workers in early withdrawal states more struggle than those in the states which retain the PUA benefits until September. It shows that the early termination of UI benefits leads to greater financial hardship for non-standard workers than standard workers. The findings from the triple difference also support that the withdrawal of the pandemic UI benefits increases the non-standard workers' financial struggle as the withdrawal of the benefit increases their difficulty in paying expenses.

The results imply that the expanded pandemic UI benefits successfully assisted the marginalized precarious workers financially and mentally. Especially, considering half of all UI recipients were PUA recipients, which are eligible for non-standard workers, about 14.6 million workers at its peak in August 2020 (Traub 2021), it implies that millions of PUA recipients would have more suffered financially when the benefit was abruptly terminated. In this sense, this paper implicitly shows how big the blind spots of the existing social insurance systems are for non-eligible groups.

Despite the contributions, the limitation of distinguishing non-standard workers still exists. Even though having employer-sponsored insurance (ESI) as a fringe benefit is a reasonable proxy variable for labor market insider status, the variable I used measures whether the respondents are

covered by insurance by a current or former employer through themselves or another family member. If they are covered by another family member's ESI or insurance by a former employer, it is not sure whether they are currently insiders even though they answered that they have ESI and are categorized as insiders in the analysis. In that case, the sizes of insiders in the sample could be overestimated. Additionally, since ESI is tied to current employment status, it may be affected by the policy change which means the subgroup and triple-differences analyses condition on partially post-treatment variable. However, as checked in Table C1, the post-treatment bias is not too worrisome as the estimate is not statistically significant.

Despite the limitation of this paper, this paper contributes to the policy implications of how to renovate the unemployment insurance system given the increase of non-standard workers. Contrary to previous research which focuses on the moral hazard effects of UI benefits that UI benefits discourage job searching, this paper more focuses on the original purpose of UI benefits and the policy blind spot for non-standard workers who were relatively overlooked under the typical UI system. The magnitudes of financial and mental difficulty of the early termination UI benefit on non-standard workers as labor market outsiders indicate how many are excluded from the regular system despite their needs. Accordingly, the findings are not just confined to the effects of the expanded benefits in the Pandemic era but reveal the overall weakness of the UI system in the future of work. Thus, this study suggests narrowing the social benefits disparities by the labor market status.

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