Unemployment Insurance and Labor Market Transitions: Evidence from the Covid-19 Pandemic

By: Aiqi Sun

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Economics Department

The University of North Carolina at Chapel Hill

Thesis Advisor: Prof. Klara Peter

Faculty Advisor: Prof. Klara Peter

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Abstract

To help the unemployed, the Federal Government expanded the weekly amount (Federal Pandemic Unemployment Compensation) and potential benefit duration (Pandemic Emergency Unemployment Compensation) of Unemployment Insurance (UI) benefits during the pandemic, along with the eligibility for benefits (Pandemic Unemployment Assistance). While these programs were set to expire in September 2021, around half of the states terminated some or all programs in advance. This thesis investigates whether the termination of each temporary UI program incentivized re-employment using difference-in-difference estimations. I document four new facts. Firstly, the early termination of all programs in June resulted in a 6-7 percentage point increase in the Unemployment-to-Employment (U-E) transition rates. The early termination of Federal Pandemic Unemployment Compensation (FPUC2) alone had negligible effects. Secondly, under strong assumptions, terminating Pandemic Emergency Unemployment Compensation and Pandemic Unemployment Assistance in advance (in addition to FPUC2) caused a roughly 5 percentage point increase in U-E transition rates. Thirdly, the employment effects of the early termination were short-termed and diminished by the end of 2021. Finally, the scheduled termination of these programs did not increase the U-E flow. Overall, the results infer that UI's expanded eligibility and longer potential duration, not the high benefit amount, reduced re-employment in 2021.

Keywords: Unemployment insurance; Covid-19; Labor market transition; Difference-in-differences

JEL classification: J64; J65

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This work is dedicated to the memory of Lanlan.

1 Introduction

During the COVID-19 global pandemic and the synchronous economic recession, many workers in the U.S. became unemployed. According to the U.S. Bureau of Labor Statistics (BLS), the unemployment rate rose to as high as 14.7% in April 2020[1]. In response, the Federal Government rolled out pandemic-relief policies, several of which were extensions to the statewide unemployment insurance (UI) programs. These extensions not only increased the potential benefit duration (PBD) and weekly benefit amount (WBA) of UI but also expanded UI eligibility. The three corresponding programs were the Pandemic Emergency Unemployment Compensation (PEUC), Federal Pandemic Unemployment Compensation (FPUC2[2]), and Pandemic Unemployment Assistance (PUA). The benefits were generous, and 76% of workers eligible for regular UI could collect benefits that exceeded their lost wages between April and July 2020 Ganong, Noel, and Vavra (2020).

Unemployment insurance is vital to both unemployed workers and the economy as a whole. Mortensen (1977) demonstrates with a search model that UI could reduce the income loss that would otherwise occur and provide workers with insurance while they search for new jobs. Gruber (1994) finds that UI helps smooth consumption across periods of joblessness and is especially effective when the job loss is unanticipated[3]. At a macro level, Acemoglu and Shimer (1999) argue that insured workers seek high-wage jobs with high unemployment risk; therefore, an economy with risk-averse workers requires a positive level of unemployment insurance to maximize output. However, the benefits of UI come with the costs associated with subsidizing unemployment, and the costs could be high when benefits are generous.

As the country recovers from the pandemic economically, it is widely reported that the U.S. is experiencing a labor shortage. As reported by U.S. News (2021)[4], an overwhelming majority of businesses report difficulty hiring workers and retaining existing employees. Unemployment benefits subsidize continued unemployment. Thus, valid speculation is that the enormous UI extensions during the

1. https://www.bls.gov/charts/employment-situation/civilian-unemployment-rate.htm
2. Typically, FPUC refers to the $600/week Federal Pandemic Unemployment Compensation available in 2020, whereas FPUC2 refers to the $300/week Federal Pandemic Unemployment Compensation in 2021
3. Well, we all should have anticipated a global pandemic when there was an outbreak in Wuhan, China, but we didn't.
4. https://www.usnews.com/news/national-news/articles/2021-06-02/overwhelming-majority-of-businesses-report-difficulty-hiring-workers-and-retaining-existing-employees

pandemic contributed to the labor shortage to some degree. The fact that the average unemployment duration fell considerably after federal UI assistance programs ended (see Figure A.1) further fueled the speculation, suggesting that those programs sustained long-term unemployment spells. In May 2021, citing concerns about "worker shortages", around half of the states announced that they would terminate the provision of federal UI assistance early. This research utilizes this "early termination" as a natural experiment and asks a critical question: Did the termination of each temporary UI program in 2021 incentivize people to become re-employed? If so, how large was its magnitude? The goal of the thesis is to extend the currently limited literature on pandemic UI benefits and decompose the effects of the cancellation of different policies on the unemployment-to-employment (U-E) transition probabilities.

By estimating a difference-in-difference model with multiple treatment groups, I find evidence that the early termination of all provisional UI benefits in June resulted in a 6-7 percentage point increase in the U-E transition rates. This effect is economically significant (a 25%-28% increase) but less as large as previous literature indicates. Meanwhile, the effects were short-lived and diminished in the last quarter of 2021. Several DID estimates suggest that the early termination of FPUC2 alone had nearly negligible effects. In addition, under strong assumptions, the impact of terminating PEUC and PUA in advance, in addition to the early termination of FPUC2, is a 3-5 percentage point increase in U-E transition rates. The results infer that UI's expanded eligibility and longer potential benefit duration, not the high replacement rates, generated disincentives and reduced re-employment. Interestingly, the scheduled termination of the temporary UI benefits did not increase the flow from unemployment to employment once statewide labor market conditions and pandemic severity were accounted for.

Economists share a consensus that more generous UI benefits are usually associated with a prolonged unemployment spell. A leading factor behind the longer unemployment duration is the "moral hazard" effect of UI[5]: UI benefits can lead recipients to reduce their search effort and substitute leisure for work, slowing the transition into employment. This channel constitutes a disincen-

5. Some empirical evidence of the moral hazard effect includes Vodopivec (1995) and Katz and Meyer (1990), which find sharp increases in the exit rate from unemployment just before the potential exhaustion of UI.

tive to work and can harm economic efficiency if vacant positions are left unclaimed. Nevertheless, economists disagree on whether an expansion of UI generosity will generate an economically significant disincentive to work, resulting in economic inefficiency. For instance, while both studying UI expansions during the Great Recession, Farber, Rothstein, and Valletta (2015) demonstrated that the phase-out of the extra benefits had a near-zero effect on U-E transitions, whereas Fujita (2010) found that the availability of large-scale extended benefits had a significant adverse effect on the U-E hazard function. Therefore, the magnitude of the "moral hazard" effect remains an important empirical question, especially when presented with different types of UI extensions. This thesis documented that the size of the moral hazard effects can be large when the UI benefits are generous. More importantly, it highlights that a "similar" increase in WBA and PBD yields different amounts of disincentives: the latter is much stronger.

In addition, this paper fills two gaps in the existing literature about federal UI assistance. To the best of my knowledge, no previous studies have examined the effect of the scheduled expiration of benefits on re-employment. I show that the scheduled expiration of FPUC2, PEUC, and PUA in September 2021 did not encourage employment in affected areas. I also found that the incentives provided by the early termination were short-lived. Secondly, this research is also the first to study the effects of reinstating the job search requirement during the pandemic. I document an unexpected result and urge future researchers to explore the issue.

The empirical method of this thesis, difference-in-difference (DiD), has also caught attention in recent times. Namely, when the treatment is delivered staggeringly and (or) when the treatment is not binary, then the standard DiD approach can introduce bias (e.g., Callaway, Goodman-Bacon, and Sant'Anna (2021)). Since the early determination can be deemed as a non-binary staggeredly-delievered treatment, I properly defined treatment paths and use strong assumptions to produce unbiased and interpretable estimations. My results affirm their arguments: the employment effect of early termination documented in the existing literature is 100% larger than my results due to their failures to account for these issues.

The rest of this proposal is organized as follows: Section 2 summarizes the existing literature on unemployment insurance, explains the UI system before and during the pandemic, and highlights related recent developments in the DiD literature. In Section 3, I introduce the data sources and present summary statistics as well as descriptive analysis. Section 4 presents the theoretical model, and Section 5 presents the empirical specifications. Estimation results are tabulated and discussed in Section 6. Section 7 concludes.

2 Background

2.1 Literature Review: Unemployment Insurance

The topic of this paper is most relevant to the body of literature on the effects of UI on labor market outcomes (especially re-employment), job search models, and the consequences of federal UI assistance programs. This thesis is closely aligned to each category and contributes to them uniquely.

The primary goal of the unemployment insurance system, as suggested by its name, is to support workers during job loss and subsequent income loss. Theoretically and empirically, researchers provide critical evidence of the importance of UI. Firstly, theoretical papers, including Baily (1977), demonstrate that UI reduces the income loss that would otherwise occur to workers; some (e.g., Mortensen (1977)) also highlight the importance of providing job seekers with insurance. Empirically, the most common approach to test this theory-based hypothesis is to examine the effects of UI on food and other consumptions (the so-called consumption smoothing benefits of UI). Gruber (1994) estimated that the fall in food consumption expenditure among the unemployed would have been three times larger without UI. Meanwhile, Browning and Crossley (2001) found that UI's consumption smoothing effect is concentrated among those who report zero wealth at the time of job loss. Empirical evidence also suggests that a more generous UI leads to a higher post-unemployment wage rate (Ehrenberg and Oaxaca 1976). Overall, as summarized in East and Kuka (2015), the literature on the benefits of UI is limited.

A more extensive set of literature focuses on UI's (negative) influences on re-employment, especially the "moral hazard" effect. The moral hazard effect is the commonly discussed "distortion effect" of unemployment benefits, and many argue against a generous UI system because of it (e.g., Feldstein (1973)). Since UI recipients' unemployment spells are subsidized, they have less pressure or urge to exit unemployment. Theoretically, the mechanism through which UI affects job search depends on the model. More generous UI benefits raise reservation wages in a simple search model that assumes fixed search intensity. If the search intensity is endogenized, a model would also predict a lower search intensity as claimants substitute leisure for work. Herefore, UI benefits constitute a disincentive to work and slow the transition into employment, which can harm economic efficiency if vacant positions are unclaimed. The more generous the unemployment benefits are (in terms of wage placed and potential benefit duration), the stronger the disincentives there is. Theoretical models of efficient unemployment insurance in an economy with moral hazards often find that the UI system is too generous in the US (see Hansen and Imrohoroğlu (1992) & Davidson and Woodbury (1998)).

Consistently, empirical analysis proved the existence of the moral hazard effect and demonstrated that a more generous UI system is associated with a prolonged unemployment spell or a decreased probability of re-employment. Card and Levine (2000) showed that when Extended Benefits were available in New Jersey between 1995 and 1997, the fraction of UI claimants who exhausted their regular benefits rose by 1–3 percentage points. Lalive (2007) studied Austria's REBP program and concluded that large benefit extensions increased the duration until a new job was taken.

Even so, economists have differing views on whether UI generates an economically significant disincentive to work. Supporters of a considerable moral hazard effect include Vodopivec (1995)), a study that discovers that the escape rate (from unemployment) of the recipients of UI to employment increased dramatically just before the potential exhaustion of UI and decreased equally dramatically after benefits were exhausted. The author concludes that the observed pattern proves the "waiting behavior" of UI recipients. Similarly, Uusitalo and Verho (2010) demonstrated that a 15% benefit increase led to a 17% decrease in re-employment hazard at the beginning of unemployment spells. However, several recent studies of the UI expansion during the Great Recession question the magnitude of the moral hazard effect; they pointed out that several earlier studies that focused on exit hazards overestimated the disincentive effect because they mixed U-E exits and U-N exits. Notably, Farber, Rothstein, and Valletta (2015) estimated a logistic model with competing risk and found that the UI extensions during the Global Financial Crisis have not had sizeable disincentive effects on recipients' job-finding rate, thus no impact on labor market efficiency. Studying the same period, Rothstein (2011) infers that the moral hazard effect of UI extensions raised the unemployment rate in early 2011 by no more than 0.25 percentage points. I contribute to this debate by providing evidence that the unprecedentedly generous UI system in the US in 2021 led to a substantial increase in re-employment rates among states that terminated all federal UI assistance in advance but not among states that let those programs expire naturally. Another contribution of this paper is that I presented rare evidence that expanding UI eligibility to more unemployed individuals can lead to a sizable disincentive effect.

Besides the commonly discussed moral hazard effect, unemployment insurance may affect employment status via other channels. For instance, studies including Farber, Rothstein, and Valletta (2015) and Solon (1979) highlight a "labor force attachment" effect: UI encourages continued job search by people who would otherwise become "discouraged workers" and stop looking for work. While raising the "nominal" unemployment rate, this effect may encourage people to search for work and boost labor force participation. Chetty (2008) argues for a liquidity effect and explains the increase in unemployment duration following a UI expansion among credit-constraint individuals partially by the incompetence of the credit and the insurance market.

The theoretical model used in this paper took the strength from a well-established set of search-theoretic models of the labor market. Rogerson, Shimer, and Wright (2005) conducted a detailed survey of these models. McCall (1970) developed a single-agent search model in which the worker compares the marginal costs and marginal benefits of continuing searching; the model I propose builds upon a simplified one-period McCall Model. Lucas Jr and Prescott (1978) developed the Islands Model and expanded the search to a continuum of workers. Mortensen (1977) 's model underscores the importance of unemployment resources and demonstrates that UI could reduce the income loss that would otherwise occur and provide workers with insurance while they search for new jobs. Motivated by models surveyed in Rogerson, Shimer, and Wright (2005), I introduce unemployment benefit amount, expected benefit duration, and eligibility to a simplified static McCall model. The model allows us to predict the effect of each specific pandemic UI program on search intensity (thus U-E transitions) and enriches the existing literature.

My research also supplements a rapidly growing literature on the effectiveness of the Covid-19 unemployment benefits offered by the U.S. Federal government during the Covid-19 pandemic. Cox et al. (2020) found that the marginal propensity to consume out of the $600 FPUC supplements was large and argued that these supplements thus helped insure households against material hardship and stimulated aggregate demand. Bachas et al. (2020) showed that the spending of low-income households recovered most rapidly after the start of government stimulus and income support programs, even though low-income households lost more labor income during the recession. These papers imply that the liquidity provision and income support provided by the pandemic UI programs likely played an important positive role in stimulating aggregate spending. On the other hand, Bitler, Hoynes, and Schanzenbach (2020) explored the rising food insecurity rates during the pandemic and argued that one contributor was the substantial delay of relief payments due to the overwhelmed UI systems. Baker (2021) found that while FPUC (in 2020) reduced food insufficiency by 3 percentage points for non-working households, the effect was offset after FPUC expired on July 31st, 2020.

Regarding federal UI assistance programs' disincentive effects, Mitman and Rabinovich (2021) argued that a sizable transitory increase in UI would be optimal in response to a COVID-type shock. Marinescu and Skandalis (2021) discovered that each 10% increase in benefit amount caused a 3.6% decline in job applications while not decreasing job vacancy creation. The high benefits reduced job competition, likely welfare-improving in 2020's tight labor market. Another pivotal piece of literature is Ganong, Noel, and Vavra (2020), a paper that computed how the expansion of UI changed the expected income of a typical worker in a group during the pandemic. They reported that the median statutory replacement rate reached a record high of 145% between April and July 2020. Ganong et al. (2022) documented minor negative job-finding impacts of federal UI assistance in 2020.

While much research has studied the federal UI assistance in 2020, not as many studies have focused on what happened when benefits expired in 2021. To the best of my knowledge, no research has examined the effects of the scheduled expiration of federal UI assistance or the suspension of the job search requirement. This paper fills the gap in the literature and shows that the scheduled expiration had no significant employment effect. Research on the effects of the early termination of these benefits is also scarce, with a few notable exceptions. Coombs et al. (2022) studied low-income UI recipients and found that the early expiration led to a major reduction in UI recipiency, a fall in income, and a small increase in the job-finding rate among those who lost benefits. They did not attribute the documented disincentive effects to FPUC2, and I affirm their findings. Deshmukh (2022) analyzed all nine possible labor market transitions using multinomial logit regression. She found that the probability of becoming re-employed rose after FPUC2 was terminated early, while the probability of remaining unemployed or of leaving the labor force fell. Holzer, Hubbard, and Strain (2021) inspired this thesis; the authors used Current Population Survey data and estimated that the flow of unemployed workers into employment increased by around two-thirds following early termination. They found that expanded eligibility (PUA) and generosity of UI (FPUC2) may have both slowed transitions from unemployment to employment. Nonetheless, the treatment effect they estimated was unconventionally large, and they ignored the PEUC program (up to 53 additional weeks of PBD) and did not decompose the effect of each program. My thesis solves these issues by estimating a DID model with multiple levels of treatment (and multiple periods).

Decomposing the effects of each program is especially of interest. Empirical evidence suggests that the effects of a change in UI depend on how the change was introduced: did it change the maximum benefit amount, the maximum duration, or the replacement rate? Anderson and Meyer (1997) documented that a 10 percent increase in the WBA would increase UI's take-up rate by 2 to 2.5 percentage points, while a similar increase in PD would increase take-up by 0.5 to 1 percentage point. Lalive (2007) highlighted the heterogeneity in UI's effects by the maximum duration extended by Austria's REBP program. These sets of literature underscore the potential usefulness of studying and "dissecting" the effect of a drastic change in the amount, duration, and replacement rate of unemployment insurance, such as that generated by FPUC2 and PEUC during the Covid-19 pandemic. I, therefore, turn to a detailed discussion of these programs.

2.2 An Overview of the US's UI System Before and During the Pandemic

Before the pandemic, the regular state UI program in the United States did not protect all unemployed workers. Only those involuntarily unemployed with a sufficiently long work history and (or) enough earnings in the required periods were eligible for benefits[6]. In all states, a UI claim's weekly benefit amount (WBA) is typically a function of an individual's earnings in (the whole or a fraction of) the base period and is bounded by a maximum and minimum WBA. The maximum UI duration is limited to 26 weeks in most states. In addition, in order to keep their eligibility, UI claimants must perform and report active job searches each week[7]. The median replacement rate, or the ratio of unemployment insurance benefits to prior earnings, ranged from 34 percent in Arizona to 65 percent in Oregon at the onset of the pandemic Ganong, Noel, and Vavra (2020). Most states were pulling back from their commitment to UI before 2020 Vroman and Woodbury (2014) and had to rely on substantial federal emergency funding to finance their UI programs amid the COVID-19 shock Moffitt and Ziliak (2020).

During the Covid-19 pandemic, several extensions to the regular UI benefits increased both the WBA and the maximum UI duration. Firstly, in response to the mass unemployment created by the COVID-19 pandemic, the Coronavirus Aid, Relief, and Economic Security Act (CARES) Act established an emergency program, the Federal Pandemic Unemployment Compensation (FPUC). The FPUC expanded state UI benefits by providing an extra $600 weekly benefit for all weeks of unemployment from April 4 through July 25, 2020. The median statutory replacement rate under FPUC grew to 145 percent, and three out of four unemployed workers became eligible for benefits that exceeded their lost wages Ganong, Noel, and Vavra (2020). Such high replacement rates should have provided critical resources for unemployed individuals to smooth consumption. The Consolidated Appropriations Act of 2021 restarted the FPUC program (FPUC2) and authorized $300 FPUC2 payments beginning December 26, 2020; the American Rescue Plan Act extended the FPUC2 program until September 4, 2021. Twenty-six states, however, terminated FPUC2 in advance. Between the

6. For instance, in the first half of 2018, UI eligibility in Maryland is limited to involuntarily unemployed individuals who, in the Base Period (four full quarters before a UI application), 1) have earned at least $1800 in two quarters, 2) have earned at least $1176.01 in the highest earning quarter, and 3) have a total base period earnings of at least 1.5 time the highest quarterly earnings

7. However, it is difficult to enforce the work search requirements O'Leary (2006).

two rounds of FPUC, two other federal UI programs, the Lost Wages Assistance (LWA) and the Increased benefit assistance (IBA), provided additional weekly unemployment benefit payments to unemployed US workers.

Two programs provided additional weeks of unemployment benefits. The Extended Benefits (EB) is a permanent project to provide UI extensions automatically during recessions. EB has two triggers conditioned on statewide unemployment rates and the trends in unemployment rates; states and the federal government jointly pay an extra 50% of the total regular benefits payable to an individual with the first trigger (up to 13 weeks) and pay an extra 80% with the second trigger. To address the prolonged unemployment spells caused by the pandemic, the CARES Act established the Pandemic Emergency Unemployment Compensation (PEUC) program to allow people who had exhausted their unemployment compensation benefits to receive up to 53 additional weeks of benefits[8]. Just like FPUC2, PEUC was available through Sept. 4, 2021; 20 states opted out early. With both EB and PEUC available, per the New York State's Department of Labor[9], residents in a typical state (such as NY) may be eligible for a total of 99 (26+20+53) weeks of unemployment benefits.

Moreover, the Pandemic Unemployment Assistance (PUA) program was a special federal program and an unprecedented supplement to the UI system. PUA benefits were payable up to 79 weeks to individuals who were unable to work as a direct result of COVID-19 and were ineligible for regular unemployment benefits; these include the self-employed, independent contractors, gig economy workers, people who do not meet state UI program's minimum monetary requirement or people who have exhausted their unemployment benefits. No minimum monetary requirement exists for an individual to be eligible for PUA. PUA benefit calculation uses the same formula for calculating regular benefits, and the minimum weekly PUA benefit amount is 50% of the average weekly payment of regular compensation in the state. A unique feature of the Covid-19 UI benefits was that the self-employed, gig-economy workers and independent contractors were, for the first time, eligible for benefits under the PUA program. The 20 states that opted out of PEUC early also opted out of PUA together.

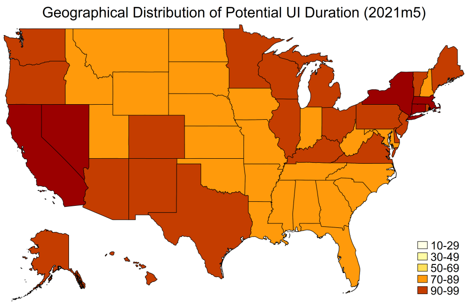
8. The initial amount approved in the CARES Act was 13 weeks. It was later first extended to 24 weeks then 53 weeks. However, it was impossible to claim the full 53 weeks, as PEUC expired on Sep 6, 2021.

9. Source: https://dol.ny.gov/system/files/documents/2021/06/cd2-extended-benefits-faqs-06-09-21.pdf

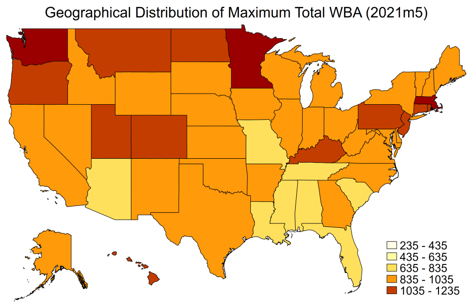
Finally, another unique feature of the UI system during the pandemic was the temporary removal of the work search requirement. In March or April 2020, all states and D.C. suspended the requirement that workers applying for new unemployment benefits need to be actively searching for work if the COVID-19 pandemic directly impacts them. The pandemic turned out to have affected every single sector of the economy, so almost all eligible unemployed individuals could claim unemployment benefits without actively searching for a job. States reinstated the job search requirement in a scattered manner; Arkansas reinstated it as early as June 28, 2020, whereas New Jersey waited until all provisional UI programs expired on September 4, 2021, to bring the requirement back. The governor of Louisiana, John Bel Edwards, shed light on why some states rushed to reinstate the search requirement in 2020[10]: aside from the goal of encouraging re-employment, states were running out of their unemployment savings.

10. Source: https://www.wafb.com/2020/08/07/work-search-requirements-unemployment-money-resume-sunday-la/

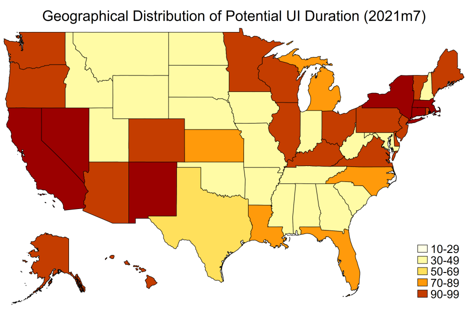
Figure 1: Geographical Visualization of UI Generosity in May, July, September 2021



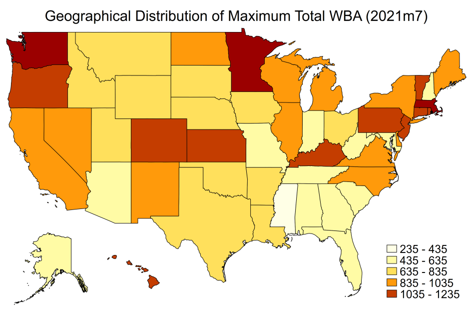
(a) Total PBD in May 2021



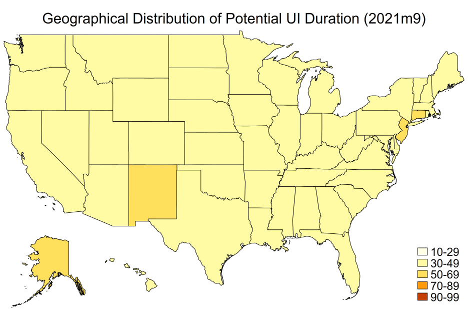
(b) Max Total WBA in May 2021



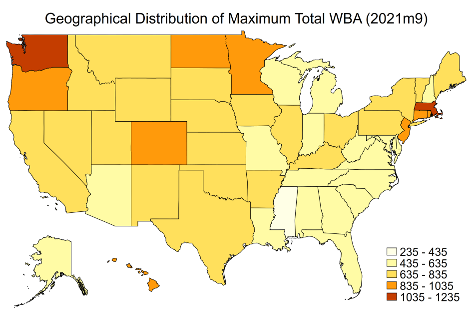
(c) Total PBD in July 2021



(d) Max Total WBA in July 2021



(e) Total PBD in September 2021



(f) Max Total WBA in September 2021

Note: These figures display the geographical distribution of UI benefits available in 2021 in selected months. The left column plots the *effective*[a] total PBD (state + EB + PEUC), and the right column plot the *maximum* WBA (state + FPUC2) in all fifty states. The top row reflects UI generosity in May 2021, the middle row reflects that in July, and the bottom reflects that in September. Data is from the UI Tracker.

a. Discarding all weeks after Sep. 4th, 2021.

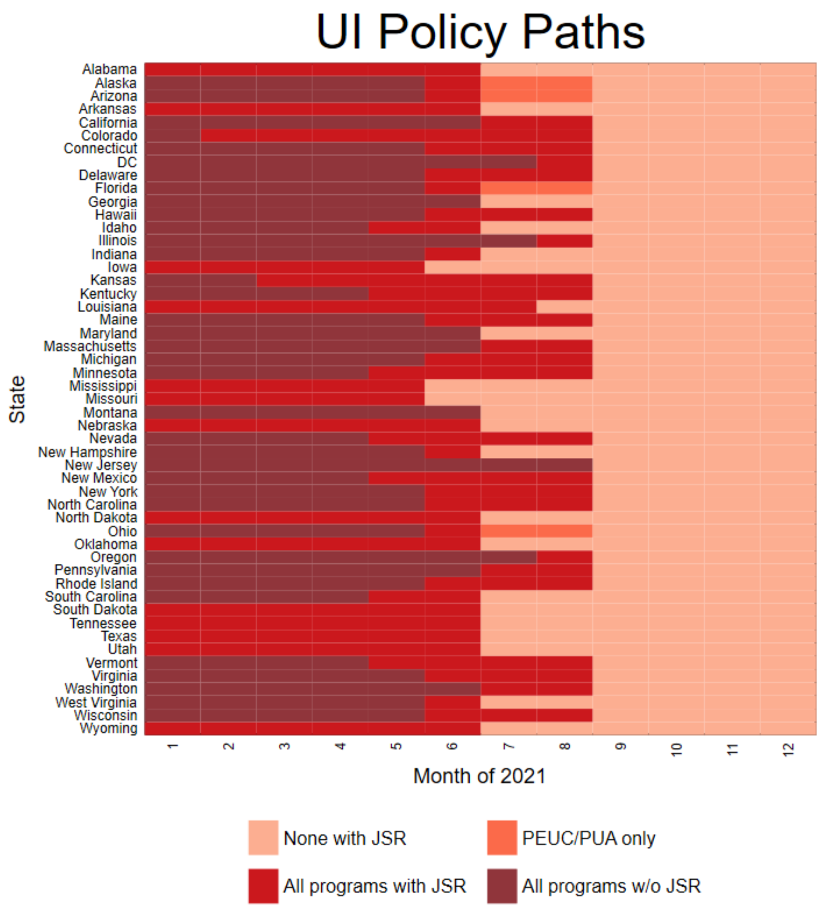
Figure 1 visualizes the changes in UI generosity in terms of PBD and max total WBA in 2021. Eligible individuals in all states could claim at least seventy weeks of unemployment benefits in May 2021 (before any state terminated any federal UI assistance), and the maximum WBA in most states was above \$800. UI generosity plummeted in the withdrawal states in July 2021. As of September, besides a few states that triggered EB, UI's PBD and WBA returned to their pre-pandemic levels across the US[11].

2.2.1 Early Withdrawal from Pandemic UI Benefits

A handful of states withdrew from pandemic UI benefits early, citing reasons including strengthening labor markets and business concerns about "worker shortages". Most "withdrawal" states announced their plan to terminate pandemic UI benefits early in late April and May. Figure 2 draws all 50 states and D.C. policy paths in 2021. Eighteen states opted out of PUA, PEUC, and FPUC in June. Tennessee[12] and Louisiana opted out of all three programs in July. Alaska, Arizona, Florida, and Ohio opted out of FPUC between CPS's June and July survey rounds but continued participating in PUA and PEUC. Indiana and Maryland filed to withdraw early but were required by court order to continue paying benefits for both programs. Twenty-four states and the District of Columbia remained in all three programs until the designated expiration date of September 4th, 2021. I shall denote the states that terminated FPUC2 "partial-withdrawal" states and those that withdrew from all federal UI assistance "full-withdrawal" states.

11. A few states also revised their UI system in 2021.
12. Tennessee withdrew on July 3rd, before CPS's July survey round.

Figure 2: Policy Path of all 50 States and D.C.



Note: These two figures report the policy paths in all fifty states regarding both the availability of federal UI assistance programs and the status of the job search method. The sample period is the whole year of 2021. Data is from the UI Tracker.

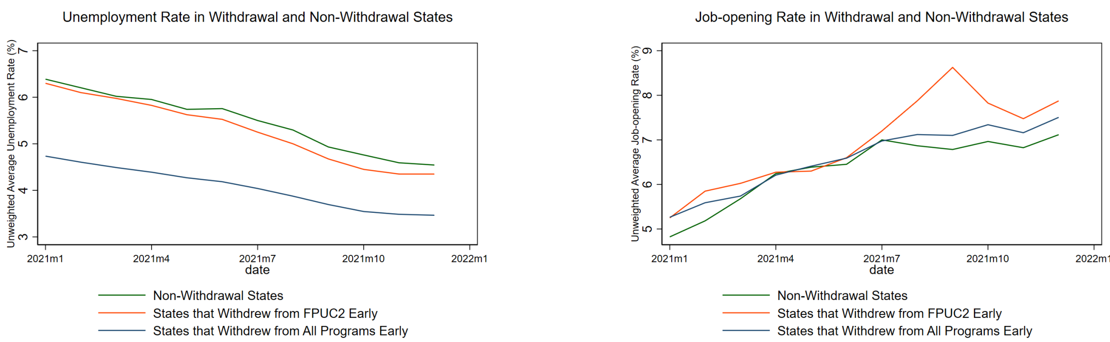
A natural question to ask is: Why did some states withdraw from UI programs in advance but not others? Both economic factors and political factors could potentially explain the different decisions. If unemployment was higher in the "withdrawal" states[13], and if there were many unfilled vacancies in those states, the decision to withdraw from generous UI programs was out of economic motives. On the other hand, it is possible that the decisions made by state governments were determined by their political ideologies. For example, if residents and the governments of the withdrawal states

13. Unless specified, "withdrawal" states include both "partial-" and "full-" withdrawal states.

believed that UI claimants lived off the toil of other people and that the federal government should not interfere with state unemployment insurance systems, they would opt out of federal UI programs early, even in the lack of economic motives.

To examine the hypothesis that economic factors led to the withdrawal decisions, let us compare the labor market conditions in withdrawal and non-withdrawal states. I calculated the (unweighted) average unemployment rates and job-opening rates[14] in withdrawal and non-withdrawal states[15], plotted in Figure 3.

Figure 3: Labor Market Conditions in Withdrawal and Non-withdrawal States



(a) Unemployment Rates

(b) Job-opening Rates

Note: The figure on the left reports the average monthly unemployment rates in non-, partial, and full-withdrawal states, and the figure on the right plots the average monthly job-opening rates in these three groups of states, respectively. All data is calculated as an unweighted mean. The sample period is the whole year of 2021. Data is from the UI Tracker.

According to Figure 3a, non-withdrawal states had higher unemployment rates than withdrawal states throughout 2021. If eliminating unemployment was the motive behind terminating benefits in advance, non-withdrawal states would have had more incentives to withdraw instead of withdrawal states. Figure 3b demonstrates that before the early withdrawal took place, withdrawal and non-withdrawal states had similar levels of job opening rates[16]. There is no evidence that businesses

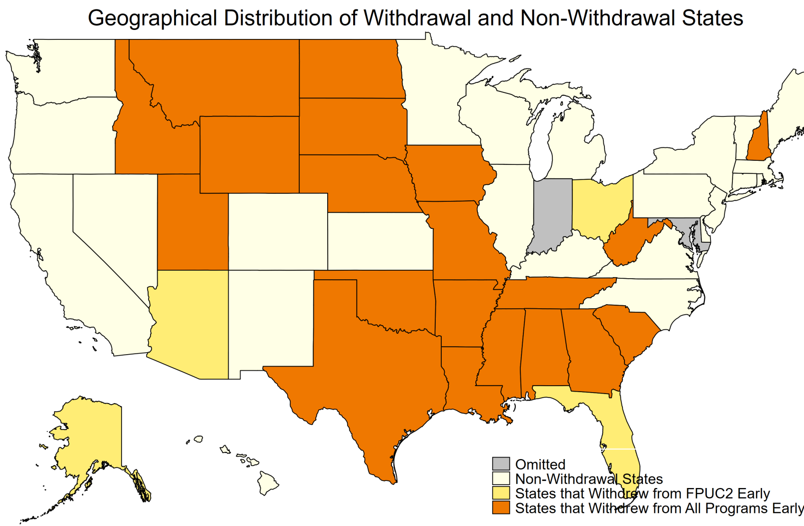
14. The job openings rate is the number of job openings on the last business day of the month as a percent of employment plus job openings. (U.S. Bureau of Labor Statistics, 2023)

15. Indiana and Maryland are omitted because it's difficult to put them in any category.

16. Meanwhile, it is worthwhile to note that the job opening rate in the second half of 2021 was higher in withdrawal states, and the divergence occurred after the early termination. Potentially, this reflects the demand-side effects of

in withdrawal states experienced more difficulties filling vacant job openings. Overall, macro-level data does not suggest that withdrawal states are treated due to their labor market conditions.

Figure 4: Location of Withdrawal and Non-Withdrawal States

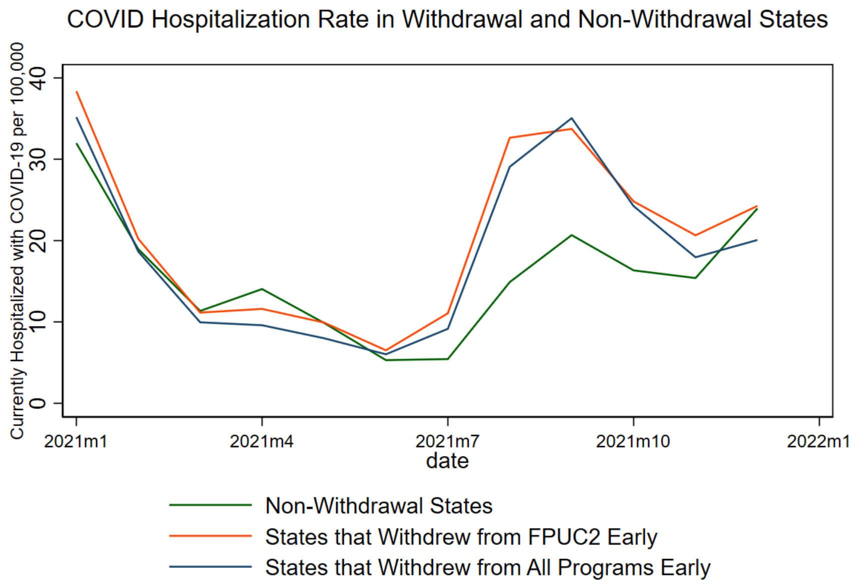


Note: This figure plots the locations of non-, partial-, and full-withdrawal states. The two omitted states are Indiana and Maryland because local court orders reversed their decisions to terminate benefits in advance.

Meanwhile, the geographical distribution of withdrawal and non-withdrawal states almost mirrors the 2020 election map. States that opted out of some or all pandemic UI programs coincide with the stereotypical Republican or Republican-leaning states, where a greater share of residents hold conservative or libertarian values and repel the idea of a "welfare state". Differences in opinions towards UI in general between withdrawal and non-withdrawal states were also evident from the word choices of their governors. For instance, Gov. Asa Hutchinson of Arkansas (a withdrawal state) described unemployment benefits as "paying people not to work" (CNN, 2021). In short, there is some evidence that differences in politics rather than economic situations fueled the withdrawal decisions.

early termination: with more people incentivized to look for jobs, businesses were expanding operations and hiring more workers.

Figure 5: Hospitalization Rates



Note: This figure reports the mean (seven-day moving average) Covid-related hospitalization rates (out of 100,000) at CPS's monthly survey dates in non-, partial, and full-withdrawal states, respectively. All data is calculated as an unweighted mean. The sample period is the whole year of 2021. Data is from the UI Tracker.

One also may wonder if the severity of the pandemic had a role in the decision (not) to withdraw from benefits. Indeed, if the pandemic was still very severe in non-withdrawal states, pushing people back to the workplace could have serious public health consequences. If so, public health concerns could have prevented those states from terminating the benefits early. Nonetheless, as plotted in Figure 5, withdrawal and non-withdrawal states had almost identical Covid-related hospitalization rates and very similar trends: hospitalization was high in all three groups of states in January due to the Omicron wave and decreased after that. The higher hospitalization rates among the two types of withdrawal states in the 3rd quarter reflect that Covid was transmitting faster in those states as more people there worked and interacted. Herefore, pandemic severity does not explain the withdrawal decision, either.

2.3 Developments in the Diff-in-Diff Literature

This paper exploits the early termination of federal UI assistance programs in June 2021 as a "natural experiment" as its identification strategy. In the previous subsection, I presented evidence that the selection of control (non-withdrawal states) and treated group (partial- and full-withdrawal states) was somewhat exogenous: the decision was motivated by political instead of economic reasons. Assuming the above argument is valid, the incidence that some individuals were treated but not others provides an excellent opportunity to conduct a difference-in-difference (DiD) analysis, which is indeed the principal econometric method applied in this thesis. A great proportion of the UI literature also uses DiD, including but limited to Solon (1979), Card and Levine (2000), and Holzer, Hubbard, and Strain (2021).

Despite its popularity, DiD estimates in empirical studies are not necessarily robust. The "doubt" on DiD can be traced back to Bertrand, Duflo, and Mullainathan (2004), who demonstrated that a DiD with panel data could seriously underestimate the standard errors and produce type-1 errors. Recently, a wave of new studies has cast doubt on the unbiasedness of the commonly used two-way fixed-effect (TWFE) DiD method when there is variation in treatment timing (i.e., the treatment assignment is "staggered"). Athey and Imbens (2022) show that under random assignment of the adoption date, the standard DiD estimator is an unbiased estimator of a particular weighted average causal effect. Baker, Larcker, and Wang (2022) explain that TWFE estimates could be biased if the timing of treatment is staggered and the treatment effect is heterogeneous because it introduces a "bad comparison" problem: already-treated units can act as effective comparison units. Callaway, Goodman-Bacon, and Sant'Anna (2021) discuss multi-period DiD setups with a "continuous-enough" treatment (including multi-valued discrete treatment): TWFE will 1) introduce a fundamental "selection bias" problem under regular parallel trend assumptions and 2) lead to the negative-weighting problem as it is sensitive to heterogeneous causal responses across groups.

Echoing these findings, researchers are developing alternative estimators or methods to deliver interpretable unbiased causal inferences. Callaway and Sant'Anna (2021) and Sun and Abraham (2021) develop two closely-related estimators separately; their method relies on first estimating the individual cohort-time-specific treatment effects, allowing for treatment effect heterogeneity, then aggregating them to produce measures of overall treatment effects. Some other studies adopt a "stacked regression estimator" (e.g., Cengiz et al. (2019)), which addresses the "bad comparison" problem by carefully picking the control group. Borusyak, Jaravel, and Spiess (2022)'s estimator, meanwhile, performs an "imputation-based" estimation. Callaway, Goodman-Bacon, and Sant'Anna (2021) propose a "strong parallel trend" assumption that will make estimates interpretable if satisfied.

In the case of pandemic UI programs, the treatment is non-binary, and the treatment timing is not homogenous, so both the "bad comparison" problem and the "selection bias" could arise. Hence, in addition to verifying the standard parallel trend assumption, following Callaway, Goodman-Bacon, and Sant'Anna (2021), I provide some evidence that the "strong parallel trend assumption" is not violated. In addition, I estimate the ATTs on different groups and periods separately (with well-defined treatment paths), as I anticipate heterogeneous responses to the treatment. While it is not the main focus of this paper, the fact that the policy effect I documented is significantly lower than that in the existing literature (e.g., Holzer, Hubbard, and Strain (2021)) proves the importance of correcting using DiD.

3 Data

3.1 Current Population Survey

This project's primary data source is the Current Population Survey (CPS)[17], a panel dataset of monthly labor force statistics (household survey) sponsored jointly by the U.S. Census Bureau and the U.S. Bureau of Labor Statistics (and provided by the Integrated Public Use Microdata Series). All observations are at the individual-month level, and the sample period is Feb - Dec 2021, depending on the empirical specification. Each monthly selected sample of the CPS is about 65,000 occupied households across all 50 states and the District of Columbia; participants are in the survey for four

17. The sample design descriptions, questionnaires, description of variables, and codebook of CPS can all be found on IPUMS.org.

consecutive months, out for eight, and then return for another four months before leaving the sample permanently.

As one of the most comprehensive data sets about individual characteristics and labor force status in the United States, the CPS is widely used by demographers, economists, sociologists, and other population-related researchers. The CPS is particularly suitable for this research project because its panel structure allows us to identify individual-level labor market transitions in consecutive months. For a similar reason, an extensive set of literature about the effects of UI extensions in the US, including but not limited to Card and Levine (1994) and Farber, Rothstein, and Valletta (2015), also based their analysis on samples from the CPS. In addition, several recent papers on the effects of pandemic UI extensions (such as Holzer, Hubbard, and Strain (2021)) also use CPS data in the same sample period, and readers can directly compare the results of our estimations. Finally, the CPS is the basis upon which federal statistics on unemployment are calculated monthly, and the results in this paper can potentially be used in a counterfactual exercise to infer the effects of UI programs on the unemployment rate.

There are also two significant limitations of the CPS dataset. Firstly, CPS's longitudinal observations of each individual come with an eight-month gap. Secondly, the employment status variable and the unemployment duration variable exhibit many cases of inconsistencies. Namely, an individual who was employed two months ago shall not report their unemployment duration to be, say, 16 weeks. Economists widely well document this issue: both Poterba and Summers (1986) and McGovern and Bushery (1999) highlighted that both unemployment status and unemployment duration are often misreported. I prefer to "trust" the employment status variable over the unemployment duration variable because it is easier for an individual to know their *current* employment status as opposed to when they became unemployed *in the past*. I also re-estimated the regressions with a commonly-used recoding procedure [18].

Several sample constraints are applied to the data. First, the main sample is restricted to the sample to people of prime working age (25-54) because their responses are of interest to economists and

18. For details, see Appendix B.

policymakers. Many influential publications, including Card and Levine (2000), employ the same age cut-off. Moreover, since this paper studies the labor market transitions of unemployed people, the effective sample in a month is the subset of individuals who reported being unemployed in the previous month and remained in the sample in the current month. After applying the above restrictions, the effective sample size becomes 9,829, or about 894 observations per month. Among them, 25.62% were employed in the current survey round, 53.41% stayed unemployed, and 20.97% were out of the labor force. Slightly more than half of the respondents are male, the average age is 38.54 years old, and 7% had at least one disability. About 40% of all observations are married, and 17% had children aged 5 or younger. The most common level of education is high school graduates. For detailed summary statistics of the characteristics of individuals in the CPS sample, see table A.1 in the Appendix.

3.2 External Data Sets

I first supplement the CPS with the *UI tracker*. The *UI Tracker*, developed by Dr. Klara Peter and Katie Baker, is a rich data set that contains detailed information about the temporary UI programs available in each state. For example, it tracks the dates when specific states opted out of the FPUC2 program as well as the dates when the job search requirement in each state was triggered on or off. I contribute to the *UI Tracker* by extending it to 2022. I also expand the *UI Tracker* to incorporate the formulas of the Maximum Benefit Amount (MBA) in states where applicable[19]. Ganong, Noel, and Vavra (2020), a pioneer study in measuring the generosity of UI benefits during the pandemic, collects similar UI data; our UI tracker dataset encompasses a wider time period and contains more accurate information regarding monetary eligibility requirements.

Other data merged include state-level monthly unemployment data from the Local Area Unemployment Statistics (LAUS) by the Bureau of Labor Statistics (BLS), non-farm job opening data from the Job Openings and Labor Turnover (JOLTS) by the BLS, Covid-19 hospitalization rates from US Dept

19. In the process of updating the dataset, Dr. Klara Peter and I noticed that no previous study has differentiated between the max possible duration and statutory duration based on eligibility (the de facto individual-level maximum potential duration). The lack of attention between these two concepts may introduce an error when simulating the number of weeks of regular UI benefits as well as EB benefits claimable to qualified unemployed individuals.

of health and human services, and the changes in time spent outside of residential locations from Google's COVID-19 Community Mobility Report. These are the four state-level control variables. Firstly, the monthly unemployment rate is a proxy for the local economic conditions as well as labor market conditions. Following Fujita (2010) and Farber, Rothstein, and Valletta (2015), I control for the slackness of the local labor market, using the ratio of total non-farm sector job openings over the number of unemployed. The higher this ratio is, the higher the labor demand is unmet, and the more job-finding opportunities an unemployed individual has. The rationale behind this is that extension of the unemployment insurance program could have larger effects on job finding in a relatively tight labor market Kroft, Lange, and Notowidigdo (2013). Both the hospitalization rates and the changes in time spent outside of residential locations control for the severity of the pandemic and the stringency of policy responses.

3.3 Unadjusted Differences in U-E Transition Rates

This thesis's most vital summary statistics are undoubtedly the unadjusted differences in average U-E transition rates among states on different policy paths. If the raw differences in U-E transition rates visibly increased following the termination of federal UI assistance or the reinstatement of job search requirements, we'd have speculative evidence that the moral hazard effect of pandemic UI programs is economically significant and worthy of research.

I first represent a table of raw average re-employment rates in non-withdrawal states, states that withdrew only from FPUC (partial-withdrawal states), and states that withdrew from all three federal UI assistance programs (full-withdrawal states). I calculate the average in three different periods in 2021 (table 1): pre-early termination (Feb - Jun), post-early termination[20] (Jul - Aug), and post-scheduled expiration (Sep - Dec). In July and August, unadjusted U-E transition rates increased by 4.77 percentage points in partial-withdrawal states, 8.32 percentage points in full-withdrawal states, and did not change much in non-withdrawal states. A "raw" DID-style back-of-the-envelop calculation hints that the withdrawal from pandemic UI programs increased U-E transition by roughly 8 percentage points (or 31.67%). Meanwhile, it's interesting to observe that the unadjusted re-

20. Louisiana is omitted because it's the only state that withdrew after CPS's July survey round.

employment rates in full-withdrawal states actually fell after September. Overall, table 1 suggests that the early termination of all federal UI assistance programs had significant employment incentive effects. On the other hand, the increase in re-employment after the scheduled expiration in non-withdrawal states was much smaller.

Table 1: Unadjusted Differences in Re-employment Rates Across Sets of States

	(1) Feb - Jun	(2) Jul - Aug	(3) Sep - Dec	(4) change 1	(5) change 2
Non-Withdrawal States	22.81	22.09	27.63	-0.71	5.54
Partial-Withdrawal States	22.69	24.55	30.42	1.87	5.87
Full-Withdrawal States	23.74	36.96	26.92	13.22	-10.04

Note: The table reports the average re-employment rates (weighted by CPS's final basic weights) in non-, partial, and full-withdrawal states in three periods: February to June (column 1), July to August (column 2), and September to December (column 3). Column 4 is the changes in average re-employment rates from period 1 to period 2, and column 5 is the changes from period 2 to period 3. The sample is the Basic Monthly CPS from February to December 2021.

Next, let us compare the raw average U-E transition rates at different stages of UI policy paths, as presented in table 2. The rate is around 28.67% in states that participated in no federal UI assistance program and had reinstated job search requirements. States in the other three stages had a similar unadjusted re-employment rate of around 22%-24%. Notably, this means that there was no sizable difference between states that terminated FPUC2 and states that did not. A somewhat unexpected observation is that states that did not require UI claimants to search for remaining eligible had a slightly higher average transition rate.

Table 2: Unadjusted Differences in Re-employment Rates Across Stages at Policy Path

	(1) N	(2) Mean
None with JSR	3814	28.67
PUA and PEUC only	221	24.55
All programs with JSR	2771	22.06
All programs w/o JSR	3023	23.31

Note: The table reports the average re-employment rates (weighted by CPS's final basic weights) at the four policy stages (as in figure 2), respectively. The sample is the Basic Monthly CPS from February to December 2021.

4 Theoretical Model

This section proposes a simple job search model that models how an individual's job search behaviors respond to changes in UI eligibility, the job search requirement, benefit amount, and potential benefit duration (PBD). The rationale is that pandemic UI programs increased both the eligibility and generosity (amount and PBD) of UI. The model is motivated by the McCall Model McCall (1970) and inspired by several other research surveyed in Rogerson, Shimer, and Wright (2005). The economic agent in my model is an unemployed individual, and their objective is to maximize utility. Their choice variable is their job search intensity (denoted by s). Having a higher job search intensity, for example, could be spending more hours looking for vacant positions or editing one's resume carefully. A higher job search intensity translates into a higher probability of being employed; for individuals in my sample, this corresponds to a higher U-E transition rate.

This model focuses on the moral hazard effect of unemployment benefits. Since unemployment insurance subsidizes continued unemployment, it is natural to speculate that UI reduces an individual's search effort. Indeed, UI benefits can lead recipients to reduce their search effort and substitute leisure for work, slowing the transition into employment. Many pieces of literature (e.g., Marinescu, Skandalis, and Zhao (2021)) have argued theoretically and documented empirically that when unemployment benefits replace a higher proportion of an individual's expected income (or pre-unemployment income), people have smaller incentives to search for a job and thus will remain unemployed for a longer duration. Similarly, a longer maximum potential UI duration may reduce search intensity (e.g., Marinescu and Skandalis (2021)); unemployed individuals can live off benefits and only search for a job near benefit exhaustion. This type of unfavorable effect can be harmful as it reduces labor supply and, therefore, the economy's overall efficiency. Nonetheless, studies do not reach a consensus on the magnitude of this moral hazard effect, especially during the Covid-19 pandemic. This model makes predictions regarding the signs and magnitudes of the effects of each pandemic UI program.

4.1 A Model of Job Search without UI

In this section, let us set up a one-period job search model in the absence of UI. The economic agent in this model is an unemployed individual who wishes to maximize their utility $\max U = U(x, Z)$, where x is the total income received and Z is a set of individual attributes to allow for individual heterogeneity. We do not observe the full set of Z, as we cannot know the extent of an individual's preference towards not working or the education they receive from their family. We make the assumption of homogeneous taste among all individuals in the following analysis.

In this model, individuals choose an amount (continuous) of job search intensity s ($s \geq 0$) at a cost $c(s)$. The cost $c(s)$ includes the monetary fixed cost, monetary variable cost, and time cost of searching for a job. We assume a convex cost function, i.e., $c'(s) > 0$ and $c''(s) > 0$. Let us denote by A the exogenous factors that affect the job-finding probability (the higher the better); A is a measure of how efficient the job search mechanism is as well as the aggregate economic conditions (e.g., the number of job openings). Let X be a vector of job-finding probability factors; X includes observed individual characteristics, including race, gender, educational attainment, etc. Finally, let d be the unemployment duration of the individual. Previous literature, including Marinescu and Skandalis (2021) and Shimer (2008), has consistently documented the "duration dependence" of unemployment spells: the longer an individual stays unemployed, and less likely one will find a job.

An individual finds a job with a probability $\phi(s, A, X, d)$, where $\frac{\partial(\phi)}{\partial(s)} > 0$, $\frac{\partial(\phi)}{\partial(A)} > 0$, and $\frac{\partial(\phi)}{\partial(d)} < 0$. To simplify the model, we assume that all workers are homogeneous. We also assume that the labor market is competitive, and thus wage is given at w[21]. If an individual finds a job, they receive wage w, utility $u(w)$; if an individual does not find a job, they receive w^h (household production), utility $u(w^h)$. We assume the utility function u to be concave (risk-averse agents).

The worker's expected utility is

$$-c(s) + \phi(s, A, d)u(w) + (1 - \phi(s, A, d))u(w^h) \tag{1}$$

A worker chooses s to maximize their expected utility. By solving for the first-order condition, the optimal s is such that:

$$\frac{\partial(\phi(s, A, d))}{\partial(s)}(u(w) - u(w^h)) = c'(s) \tag{2}$$

A clear and simple interpretation of the optimal search intensity is that the worker chooses a level of search intensity at which the marginal cost of searching equates to the marginal benefits of searching. This model predicts that a higher level of (exogenous) wage rate w induces a higher search effort, while a higher valuation of home production discourages job search. The latter is of relevance during the Covid-19 pandemic: during the pandemic, the value of home production increased for some individuals because they needed to stay at home and take care of family members who were 1) infected by the virus, 2) part of the vulnerable population (e.g., senior citizens), 3) required care-taking service after the closure of institutions such as daycare centers. Ceteris paribus, given a higher w^h, unemployed workers are expected to put in a fewer amount of search efforts.

4.2 Job Search Model with UI

In this subsection, unemployment benefits are introduced to the model. Let b denote the benefit; let γ amount denote the UI eligibility of an individual ($= 1$ if eligible). We assume that both b and γ are exogenous[22], i.e., any variation results from different statewide or federal UI programs.

21. This assumption may or may not be violated during the pandemic. While it is widely reported that employers were experiencing difficulties with hiring, Marinescu and Skandalis (2021) found that employers did not experience greater difficulty finding applicants for their vacancies after the CARES Act, despite the large increase in unemployment benefits.
22. γ, in reality, is a function of employment history and earning history.

Furthermore, instead of granting an eligible worker with benefits b if they fail to find a job (with a probability of $1-\phi(s,A,d)$), let τ denote the probability that this worker would receive benefits. $0 \leq \tau \leq 1$; $\tau = 0$ corresponds to the model without UI. The rationale behind this parameter is that it proxies an infinite-horizon setting in which an eligible individual has a probability τ of maintaining eligibility. This implies that the expected duration of benefits payable is $\frac{1}{1-\tau}$ periods. By raising the τ parameter, we are essentially increasing the UI duration, which was an important part of the UI extension during the pandemic. Finally, to model the search requirement of a typical statewide UI program in the US, suppose that a UI-individual ($\gamma = 1$) can only receive benefits b with probability τ if they perform a minimum intensity of job searching s_0. If the minimum requirement is not satisfied, then the individual cannot claim benefits (income = w^h).

The utility of being unemployed for a UI-ineligible individual is:

$$u(w^h)$$

The utility of being unemployed for a UI-eligible individual is:

$$u(\tau b + w^h)$$

The expected utility of an unemployed individual is, therefore, the following maximization problem[23]:

$$\max_{s \geq s_0}(-c(s) + \phi(s,A,d)u(w) + (1-\phi(s,A))u(\gamma\tau b + w^h)), u(w^h) \tag{3}$$

Once again, a worker chooses s to maximize their expected utility. When the first term dominates, by solving for the first-order condition, the optimal s is such that:

$$\frac{\partial(\phi(s,A,d))}{\partial(s)}(u(w) - u(\gamma\tau b + w^h)) = c'(s); s \geq s_0 \tag{4}$$

The model makes four vital predictions regarding the responses in people's search behaviors toward changes in the UI program. Notably:

23. We can ignore the scenario in which an individual searches but searches with intensity below the minimum requirement, as such an action is almost certainly sub-optimal.

1. Suppose $(-c(s) + \phi(s, A, d)u(w) + (1 - \phi(s, A, d))u(\gamma\tau b + w^h))$ dominates equation (3) so that the optimal s is positive and determined by equation (4). The equation predicts that a higher level of benefits b or a longer PBD ($\uparrow \tau$) will lower the marginal benefit of searching, thus lowering the job search intensity. This reflects the moral hazard effect of UI, as workers in the model search with less effort and reduce their job-finding probabilities. Critically, the model suggests that if the amount of unemployment benefits is extraordinarily generous and replaces more than $w - w^h$, UI-eligible individuals have the incentive to search at a bare minimum (to maintain eligibility), collect benefits, and not try to become employed. This hints that UI programs with high replacement rates are inefficient, and the disincentive effects shall be large quantitatively. Nonetheless, if the replacement rate of the UI program is not high enough, then the magnitude of the disincentive effects is ambiguous. Similarly, the model predicts that a significant extension of the PBD will strongly discourage job search.

2. An immediate observation is that UI affects search effort only if the worker is UI-eligible. If $\gamma = 0$, equation (3) becomes equation (1), and the equation for optimal s, equation (4), is the same as equation (2). Suppose that a policy expands UI eligibility to include a greater share of unemployed individuals. For these individuals, the marginal benefit of performing job searches falls by $\frac{\partial(\phi(s,A,d))}{\partial(s)}(u(\tau b + w^h) - u(w^h))$, so we predict the new equilibrium search intensity to become lower. The size of this effect depends on the magnitude of the reduction in the marginal benefit of job search, which depends on the generosity of the UI policies (b and τ).

3. Suppose the search requirement is relaxed, and a UI-eligible worker who does not find a job can claim b (with probability τ) regardless of their search effort s. Then, the optimal s is still obtained by equation (4) but without the constraint $s \geq s_0$. Since $c(s)$ is increasing and concave, the marginal cost of choosing a search effort usually outweighs the marginal benefit, so adopting a relatively lower search effort is ideal when there is no search requirement. Therefore, we predict that the *direct effect* of the removal of the search requirement is to lower search effort; similarly, the reinstatement of the search requirement will raise search effort.

   - The magnitude of this effect depends on the composition of the cost function: if the fixed cost is high while the variable cost is low, individuals are likely to refrain from searching at all; if it is the other way around, then individuals are likely to reduce their search intensity but not necessarily stop performing a job search.

4. Suppose $(-c(s) + \phi(s, A, d)u(w) + (1 - \phi(s, A, d))u(\gamma\tau b + w^h))$ dominates equation (3) so that the optimal s is positive and determined by equation (4). A more generous UI system ($\uparrow b$ or $\uparrow \tau$) leads to a larger unemployment population, a fiercer competition for vacant positions (if it is required to search for remaining eligible for benefits), and a lower wage level. Based on predictions made in section 4.2, the lower w induced by a more generous UI program reduces search intensity.

    - However, if the job search requirement is waived, a larger unemployment population induced by a UI expansion may or may not intensify the competition for jobs. Indeed, if existing and new unemployed individuals both perform no or minimal job search, the equilibrium w may actually become higher relative to the pre-expansion level. Therefore, the removal of the job search requirement could potentially mitigate the indirect disincentive effects of a more generous UI system. Thus, the overall effect of the removal of the job search requirement is ambiguous when it accompanies a UI expansion.

We can apply these predictions from the theoretical model to the context of the Covid-19 pandemic and predict the responses in search behaviors toward the various Covid-19 unemployment benefit policies. Firstly, the PEUC (and the EB) prolonged the maximum UI duration; based on predictions ①, the longer PBD would reduce the search intensity of UI-eligible unemployed individuals. The magnitude of this disincentive is expected to be large since PEUC (combined with EB) expanded the PBD in a typical state from 26 weeks to a record high of 99 weeks. This would translate into a lower re-employment probability in terms of labor market transitions. On the other hand, the termination of PEUC ($\downarrow \tau$) is expected to be a strong but not as strong re-employment incentive: since the expiration date and the early withdrawal dates of the benefits were publicly known, unemployed individuals' *effective* PBD was the difference between the date of unemployment and the date of planned benefit termination[24]. Meanwhile, FPUC, FPUC2, IWA, and IBA provided additional benefit amounts; similar to PEUC and EB, we expect these policies to reduce search effort and result in

24. I will further discuss this in Section 6.4.

a lower re-employment likelihood as well. Since only FPUC2 ($300) was available in 2021, it is reasonable to expect the total amount of unemployment benefits (before termination) not to replace the potential incomes for many individuals, and the withdrawal from FPUC2 in most cases did not reduce the replacement rates from above 100% to a level well below 100%. Thus, the magnitude of the disincentive effect of removing FPUC2 is ambiguous and of empirical interest. In addition, from prediction ④, we'd expect PEUC and FPUC2 to discourage job search and re-employment further when the job search requirement was in place.

One unique feature of the Covid-19 unemployment benefits was the expanded eligibility granted by PUA. Under the CARES Act, a large group of ineligible workers' γ changed from 0 to 1. The model predicts they would adopt a lower search intensity and have a lower U-E transition rate (prediction ②). When PUA expired, these individuals became ineligible for UI again, and their optimal search effort increased. As discussed in prediction ②, the disincentive effect of expanding UI eligibility is strong if benefits are generous. If only FPUC2 is terminated, the remaining UI program (PEUC) is still very generous, and the availability of PUA will still pose a significant disincentive effect to many unemployed individuals. Therefore, the early termination of FPUC2 is expected not to indirectly affect U-E transitions via lowering the effects of the PUA benefits.

Another unique feature of the pandemic UI package was removing (and reinstating) the job search requirement. Predictions ③ and ④ highlight the complications. While suspending the job search requirement has a direct disincentive effect, it accompanied an expansion in UI generosity (FPUC2 and PEUC). Since few jobs were available amid the pandemic, it is natural to suspect that many UI claimants minimized their search effort when they were not required to search. The equilibrium wage w may have gone up instead, and people were encouraged to re-employ. Therefore, the overall effect of suspending the job search requirement is ambiguous. Similarly, reinstating the job search requirement may have strengthened the indirect disincentive effects of PEUC and FPUC2 described in prediction ④. Thus, it requires empirical examination to determine the sign of the net effect of the job search requirement reinstatement on U-E transition probabilities.

To summarize, I construct a job-search model that allows us to separate the effects of UI eligibility, the job search requirement, benefit amount, and potential benefit duration on job search intensity. The model predicts the effects of FPUC2, PEUC, PUA, and the removal of the job search requirement on search intensity, thus U-E transitions. While most policies are predicted to reduce re-employment, the magnitudes of the effects of some policies are ambiguous and require empirical examination. The model is especially suitable for this research setting, as it allows us to make specific testable predictions regarding the effect of (the expiration of) each temporary UI program during the pandemic. Nonetheless, unrealistic assumptions were made to keep this model simple, and some of the concerns will be addressed or discussed in the empirical section.

5 Empirical Strategy

In this section, I introduce several empirical models that examine how unemployed individuals across the U.S. responded to the termination of temporary UI programs during the pandemic. As discussed previously, the empirical specifications of this thesis are motivated by Holzer, Hubbard, and Strain (2021) and other emerging literature on the effects of the pandemic UI programs. This paper first re-estimates a similar specification. Then, a multiple-treatment DID model is introduced as the main model, and several extensions of the baseline are discussed. In all equations, the subscript i indicates an individual, t denotes the month of the observation, and s indicates the state of individual residence.

5.1 Preliminary Model - a simple DID in Holzer, Hubbard, and Strain (2021)

Following Holzer, Hubbard, and Strain (2021), I estimate a difference-in-difference (DID) linear probability model of the following form

$$\begin{aligned} Pr(y_{i,s,t} = 1) = \beta_0 + \beta_1 * withdrawal_s + \beta_2 * post_t + \beta_3 * (withdrawal * post)_{s,t} \\ + \alpha X_{i,s,t} + \epsilon_{i,s,t} \end{aligned} \tag{5}$$

where:

- $Y_{i,s,t}$ is an indicator variable that equals one if an individual i (living in state s) is employed (E) at time t.[25] In essence, since only individuals who are unemployed at time $t-1$ enter the regression, the variable reflects whether an unemployed individual transitioned into employment.

- $post_t = 0$ is a dummy variable that equals zero from February to June 2021 and equals one in July and August 2021. Since most of the early terminations of pandemic UI programs took place between the CPS survey round in June and July 2021, we can simplify the scenario by imposing a treatment timing that does not vary by state.

- $withdrawal_s$ is a time-invariant indicator variable that equals one if the state s terminated all three temporary UI programs (FPUC2, PEUC, and PUA) in June 2021. $withdrawal_s = 0$ for those states that did not terminate any program in advance. In this model, we ignore states that only terminated FPUC2 in June and states that withdrew in July. We also exclude Maryland and Indiana since their termination of pandemic UI programs was later reversed.

- $X_{i,s,t}$ is a vector of control variables expected to influence labor market transitions. It consists of time-constant individual characteristics X_i including the respondent's gender, race/ethnicity, educational attainment, and any disability; characteristics which may vary over time X_{t+1}, including marital status (and its interaction term with gender), age group, and having a child below seven years old; and monthly state-wide economic and pandemic conditions $X_{s,t}$ including the unadjusted unemployment rate, the number of non-farm job openings per unemployed, the seven-day moving average of covid-related hospitalization rates, and the seven-day moving average of changes in time spent outside of residential locations (from the Google Community Mobility Report). State fixed effects and month fixed effects are also included. [26]

- Standard errors are clustered at the state level since the treatment is assigned by state.

Model (5) offers an intuitive measure of the employment effect of the early termination of all three pandemic UI programs. If the termination was effective in encouraging re-employment, we'd observe $\beta_3 > 0$, i.e., a rise in the U-E transition rates. The model can be slightly modified to measure

25. $Y_{i,s,t} = 0$ if the individual is either unemployed or out of the labor force.

26. The vector $X_{i,s,t}$ in Holzer, Hubbard, and Strain (2021) only includes age, education, state and month fixed effects, the (logged) number of new cumulative Covid-19 cases, and the Oxford Coronavirus Government Response Tracker (OxCGRT) stringency index. I estimate all models with both sets of covariates (denoted as "*preferred* covariate" and "*alternative* covariates". All reported regressions used a seven-day moving average of hospitalization rates and the Google Community Mobility Report index to control for pandemic severity and mobility even if it was estimated with the vector of "*alternative* covariates". The rationale behind this is the inconsistencies with the OxCGRT data **INSERT CITATION LATER**.

the employment effect of only terminating FPUC2 in advance[27]. To verify the parallel trend assumption critical to model (5), and to capture the dynamic effect of the early withdrawal, I estimate the following event study equation:

$$Pr(y_{i,s,t} = 1) = \tau_0 + \sum_{t!=6} \tau_t * (withdrawal_s * \phi_t) + \alpha X_{i,s,t} + \epsilon_{i,s,t} \tag{6}$$

, where τ_t is an indicator variable for each of the eight months in the estimation sample.

5.2 Main Specification

As discussed, the goal of this paper is to extend the relatively limited current literature on pandemic UI benefits and decompose the effects of different temporary UI programs on U-E transition probabilities. Model (5), therefore, does not reveal sufficient information.

A useful assumption is that terminating all three pandemic UI programs will create a re-employment incentive that is at least as large as the incentive created when only one program (FPUC2) is terminated. This assumption is plausible as the theoretical model predicts all three programs to induce a higher job search intensity (thus a higher re-employment probability). In other words, *terminating only FPUC2* in advance is a "small dose" treatment, whereas *terminating all three programs* is a "large dose" treatment. Let us denote treatment status by d, and let d_0 (d_1) represent the treatment status before (after) the treatment timing ($post_t$):

$$d = \begin{cases} 0 & \text{Not treated (All benefits are available)} \\ 1 & \text{Treated with a "small dose" (FPUC2 is terminated)} \\ 2 & \text{Treated with a "large dose" (FPUC2, PEUC, and PUA are all terminated)} \end{cases}$$

The goal, therefore, is to identify the ATT (and partially identify the ATE) of the treatments $d = 1$ and $d = 2$, and to estimate the effects of moving from $d = 1$ to $d = 2$. Following the latest econometrics literature[28], I employ a "well-specified" two-way fixed effect DID model with multiple treatment

27. Indeed, one can simply redefine $withdrawal_s$ as a time-invariant indicator variable that equals one if the state s terminated only FPUC2 in June 2021; $withdrawal_s = 0$ for those states that did not terminate any program in advance.
28. Papers including Callaway, Goodman-Bacon, and Sant'Anna (2021) highlighted that the "canonical" TWFE with a time-varying treatment status variable produces inconsistent estimates.

levels.

To begin with, let us define a time-invariant[29] multi-level treatment variable D_s, where

$$D_s = \begin{cases} 0 & d_0 = 0 \; \& \; d_1 = 0 \text{ (States that did not withdraw early from any program)} \\ 1 & d_0 = 0 \; \& \; d_1 = 1 \text{ (States that terminated FPUC2 early)} \\ 2 & d_0 = 0 \; \& \; d_1 = 2 \text{ (States that terminated all three programs early)} \end{cases}$$

We regress the following multiple-treatment DID model:

$$Pr(y_{i,s,t} = 1) = \sum_{d=1}^{2} \sigma_d \mathbb{1}\{D_s = d\} + \beta post_t + \sum_{d=1}^{2} \theta_d \mathbb{1}\{D_s = d\} * post_t + \alpha X_{i,s,t} + \epsilon_{i,s,t} \quad (7)$$

where $y_{i,s,t}$, $post_t$, and the vector X are defined as they were defined in model (5). Observations from February 2021 are excluded from entering the regression to avoid the confounding effects of the Omicron wave. In the above equation, θ_1 is the ATT of moving from $d = 0$ to $d = 1$ (conditioning on the group $D_s = 1$), whereas θ_2 is the ATT of moving from $d = 0$ to $d = 2$ (conditioning on the group $D_s = 2$). Again, to verify the parallel trend assumptions, we estimate a corresponding event study specification:

$$Pr(y_{i,s,t} = 1) = \tau_0 + \sum_{t!=6} \tau_t * (D_s * \phi_t) + \alpha X_{i,s,t} + \epsilon_{i,s,t} \quad (8)$$

, where τ_t is an indicator variable for each of the eight months in the estimation sample.

5.3 Do Scheduled Termination and Early Termination Have the Same Effect?

The sample period of the above empirical specifications finished in August. This is in line with the current literature[30], which examines the employment effects of pandemic UI programs by studying the impact of the early withdrawal from these programs by some states (in July and August). It is natural to ask, nonetheless, whether the early termination and the scheduled termination of pan-

29. Therefore, we again ignore Maryland and Indiana, as well as the other two states that withdrew from pandemic UI programs in July.

30. Examples include Holzer, Hubbard, and Strain (2021), Coombs et al. (2022), Albert et al. (2022), etc. One exception is Arbogast and Dupor (2022), but their model also assumes a homogeneous effect across time.

demic UI programs had similar or different influences on U-E transitions. To explore this question, I modify and extend my main specification beyond September.

Let us define a new time variable T_t, where $T_t = 0$ between March and June, $T_t = 1$ between July and August, and $T_t = 2$ between September and December. For simplicity, denote d (the treatment status) at $T_t = j$ by d_j. We can divide states by the three "treatment paths"[31]:

$$D_s = \begin{cases} 0 & \text{if the treatment path is } \{d_0 = 0, d_1 = 0, d_2 = 2\} \\ 1 & \text{if the treatment path is } \{d_0 = 0, d_1 = 1, d_2 = 2\} \\ 2 & \text{if the treatment path is } \{d_0 = 0, d_1 = 2, d_2 = 2\} \end{cases}$$

Notice that this is the same D_s as in model (7).

We estimate:

$$\begin{aligned} Pr(y_{i,s,t} = 1) = \sum_{i=1}^{2} \sigma_i \mathbb{1}\{D_s = i\} + \sum_{j=1}^{2} \beta_j \mathbb{1}\{T_t = j\} \\ + \sum_{i=1}^{2}\sum_{j=1}^{2} \theta_{i,j}(\mathbb{1}\{D_s = d\} * \mathbb{1}\{T_t = j\}) + \alpha X_{i,s,t} + \epsilon_{i,s,t} \end{aligned} \tag{9}$$

where the definitions of $y_{i,s,t}$, $post_t$, and the vector X are consistent with previous models. Each $\theta_{i,j}$ measures the ATT of switching from treatment path $D_s = 0$ to $D_s = i$ in period $T_t = j$. We expect a positive $\theta_{i,j}$ if UI became less generous in period j in group s.

In addition, we are also interested in β_2 (changes in transition rates in non-withdrawal states after September), and we'd expect $\beta_2\, 0$. Nonetheless, this poses a challenge when we introduce month fixed effects (FE). Since the month FE and T_t vary at the same level, the coefficients on T_t would become uninterpretable. Therefore, I also estimate a version of model 9 without the time control (so the differences by time are purely captured by T_t).

31. We ignore omit those "anomalous" states.

5.4 The Suspension and the Reinstatement of the Search Requirement

In previous models, the job search requirement (JSR) was ignored. The "excuses" are that, firstly, JSR was never effectively enforced before the pandemic (O'Leary 2006); secondly, the reinstatement of the JSR by each state happened at various different times, so it's impossible to properly define treatment paths. However, as suggested by the theoretical model, the effect of the termination and subsequent reinstatement of the JSR on U-E transitions is ambiguous and of interest. The omission of this policy change may lead us to either under- or overestimate the effects of other policies. Therefore, in this subsection, I incorporate the JSR into the specification.

First, let us define a policy variable ($policy_{s,t}$) that reflects the four policy stages observed in figure 2.

$$policy_{s,t} = \begin{cases} 1 & \text{All programs available; JSR waived} \\ 2 & \text{All programs available; JSR reinstated} \\ 3 & \text{PEUC/PUA; JSR reinstated} \\ 4 & \text{No program; JSR reinstated} \end{cases}$$

$policy_{s,t}$ is a typical time-variant treatment status variable in a canonical TWFE model. We estimate:

$$Pr(y_{i,s,t} = 1) = \beta + \gamma_j policy_{s,t} + \alpha X_{i,s,t} + StateFE * month_t + \epsilon_{i,s,t} \tag{10}$$

The base category of $policy_{s,t}$ in model 10 is $policy_{s,t} = 2$ (All programs available; JSR reinstated). Comparing the first and the second category suggests the effect of suspending the JSR, while comparing the second and the third (last) informs us of the effect of terminating FPUC2 (all programs).

The major difference between model 10 and a canonical TWFE model is that model 10 allows each state to have a different time trend[32]. This method does not "force" states to have the same unobservable re-employment propensities (and unobserved covariates) over time thus reducing bias. While the approach certainly does not eliminate all the bias with TWFE estimations, empirical evidence underscores its usefulness: for instance, Friedberg (1998) studied the effect of unilateral di-

32. I also present estimation results of model 10less the state trends in the next section.

vorce laws on divorce rates. The author compared estimation results with and without state-specific time trends and found that the inclusion of state trends significantly impacted the estimated coefficients.

6 Results

6.1 Re-estimating Model (5) from Holzer, Hubbard, and Strain (2021)

To begin, let us attempt to replicate the findings in Holzer, Hubbard, and Strain (2021) by estimating equation (5). Table 3 displays the estimated coefficients on the variables of interest. The top row of table 3 presents the impact of terminating all pandemic UI programs for three different groups of unemployed workers: (1)-(3) those ages 25-54; (4)-(6) those ages 25-54 who do not have college degrees; (7)-(9) those in ages 16-64. Those ages 25-54 are the main sample because they are less likely to be affected by the confounding effects of changes in enrollment in higher education or in retirements[33] in response to the pandemic.

For each category of workers, table 3 presents three estimates — the original estimates reported in Holzer, Hubbard, and Strain (2021), regression results with the "alternative" set of covariates[34], and regression results with the "preferred" set of covariates.

33. For example, Faria-e-Castro (2021) documented that more than 3 million people likely retired earlier than they would have otherwise during the pandemic.
34. Recall that this set of covariates is similar to the covariates used in their paper.

Table 3: Estimates from Model (5)

VARIABLES	(1) Original	(2) Replication w/o Recode	(3)	(4) Original	(5) Replication w/o Recode	(6)	(7) Original	(8) Replication w/o Recode	(9)
	Ages 25-54			Ages 25-54 not Graduated College			Ages 16-64		
Full-Withdrawal		-0.189***	-0.191***		-0.233***	-0.229***		-0.182***	-0.193***
		(0.011)	(0.019)		(0.012)	(0.025)		(0.011)	(0.014)
Post		-0.050	-0.020		-0.082**	-0.028		-0.034	-0.026
		(0.030)	(0.028)		(0.034)	(0.033)		(0.029)	(0.023)
Interaction	0.144***	0.095***	0.087***	0.116**	0.066**	0.054	0.133***	0.062**	0.047
	(0.045)	(0.029)	(0.028)	(0.052)	(0.033)	(0.034)	(0.024)	(0.027)	(0.028)
Constant		0.294***	0.508***		0.281***	0.515***		0.339***	0.624***
		(0.028)	(0.143)		(0.031)	(0.167)		(0.026)	(0.103)
Observations	4,419	6,682	6,682	3,237	4,856	4,856	7,219	10,913	10,913
R-squared		0.020	0.027		0.022	0.030		0.017	0.024
Covariates	Alternative	Alternative	Preferred	Alternative	Alternative	Preferred	Alternative	Alternative	Preferred

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports regression results from model 5 that measure the effects of ending participation in FPUC2, PEUC, and PUA early on U-E transitions. Data is from the Basic Monthly CPS from February to August 2021. The estimation sample in columns (1)-(3) consists of unemployed individuals in their prime working-age, the estimation sample in columns (4)-(6) consists of unemployed prime working-age individuals without college degrees, and that in columns (7)-(9) is made up of unemployed individuals between 16 to 64 years old. Columns (1) (4) (6) report the results presented in Holzer, Hubbard, and Strain (2021). The "alternative" covariates are the set of covariates used in Holzer, Hubbard, and Strain (2021), whereas the "preferred" covariates are a more comprehensive set of covariates used in the main analysis of this thesis. Standard errors are clustered at the state level.

An immediate observation from table 3 is that the replication results are qualitatively consistent but are much smaller in terms of magnitudes. While column (1) indicates a 14.4 percentage point increase in transitions from unemployment to employment among prime-age workers, the size is around 9 percentage points in columns (2) and (3). Regardless of the choice of covariates, it is consistently found that the individuals in the second and the third samples did not respond as much to the early termination. The coefficients on the DID estimator also became statistically insignificant once I controlled for additional individual characteristics and local economic conditions in columns (6) and (9).

One might point out that the failure to replicate the results is because the sample sizes reported in columns (1) (4) (6) are roughly 2/3 of the sample sizes reported in columns with my regression outputs. This is because Holzer, Hubbard, and Strain (2021) recoded the EMPSTAT variable in CPS and removed spurious transitions from unemployment spells, a popular approach in the literature that the author advises against[35]. Nonetheless, even after I perform the same data-cleaning procedure and re-estimate model (5), the magnitudes of the DID estimators are still at least 30% smaller (see table A.2). With the same sample and the same model specification, the only explanation seems to be that the estimations reported in Holzer, Hubbard, and Strain (2021) are extremely sensitive to the choice and the definition of the covariates[36]. It is advised that readers should be extra cautious when interpreting results from table 3.

6.2 Main Analysis

The unsatisfactory performance of model (5) inspired my main specification and underscored model 7's importance. Besides the advantages of a multiple-treatment DID model discussed in section 4, the fact that it considers (almost) all states avoid the bias that arose when excluding states that only withdrew from FPUC2 in June.

35. For a detailed explanation of the recoding process, see Appendix C

36. In addition to the fact that I use a somewhat different measure of pandemic severity and mobility, Holzer, Hubbard, and Strain (2021) did not specify how age and educational attainment enter the regression

Table 4: Multiple-Treatment DID Results

	(1)	(2)	(3)
	Ages 25-54	Ages 25-54	Ages 16-64
Sample		not Graduated College	
Partial-Withdrawal	-0.097***	-0.057	-0.104***
	(0.034)	(0.046)	(0.031)
Full-Withdrawal	-0.211***	-0.248***	-0.209***
	(0.016)	(0.021)	(0.012)
Post	0.004	-0.004	-0.018
	(0.025)	(0.031)	(0.021)
Partial#Post	0.011	-0.069**	0.026
	(0.027)	(0.034)	(0.042)
Full#Post	0.065**	0.024	0.029
	(0.026)	(0.036)	(0.026)
Constant	0.485***	0.506**	0.620***
	(0.126)	(0.206)	(0.106)
Observations	6,286	4,582	10,179
R-squared	0.030	0.035	0.025
Covariates	Preferred	Preferred	Preferred

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports regression results from model 7 and measures the effects of both ending participation in all three programs and withdrawing only from FPUC2 early on U-E transitions. Data is from the Basic Monthly CPS from February to August 2021. The estimation sample in column (1) consists of unemployed individuals in their prime working age, the estimation sample in column (2) consists of unemployed prime working-age individuals without college degrees, and that in columns (3) is made up of unemployed individuals between 16 to 64 years old. Standard errors are clustered at the state level.

I present multiple-treatment DID estimates of the effects of terminating only FPUC2 or all pandemic UI programs on transitions from unemployment to employment in Table 4. The estimated effect of removing all temporary benefits on the treated group $D_S = 2$, θ_2, is statistically significant and equals 6.5 percentage points. The result is qualitatively and quantitatively consistent when I instead use the "alternative" set of covariates. While it is not the main focus of this paper, the fact that this estimate is significantly lower than that in the existing literature (e.g., Holzer, Hubbard, and Strain (2021)) proves the importance of correcting using DiD. Nevertheless, the estimated effect is still economically large: the early termination of FPUC2, PEUC, and PUA led to a 27.34% increase in U-E transitions. The estimated employment effect of only withdrawing from FPUC2, on the other hand, is statistically insignificant and quantitatively negligible[37]. There were no changes in the U-E flow in untreated states, as desired.

There is also evidence of effect heterogeneity. Columns (2) and (3) document a much smaller and imprecisely estimated θ_2. In addition, prime working-age individuals without college degrees in states that only terminated FPUC2 experienced a surprising decrease in U-E transition rates. This is somewhat worrying as these workers were hardest hit by the pandemic and were slower to recover from it.

Inferring the Effects of Terminating PEUC and PUA Early

The θ_is in table 4 document the effects of $d = 1$ (terminating FPUC2) and $d = 2$ (terminating all programs). One natural question to ask is what the effects of terminating PEUC and PUA are. Here, I provide a suggestive argument to measure it with $\theta_2 - \theta_1 = 0.54$.

Recall that for $i = 1, 2$, $\theta_i = E[y_1(d = i) - y_1(d = 0)|D = i]$. Assuming the conventional parallel trend assumption holds, we can decompose and simplify[38] the difference $\theta_2 - \theta_1$ into

$$
\begin{aligned}
&\theta_2 - \theta_1 \\
&= E[y_1(d = 2) - y_1(d = 1)|D = 2] + E[y_1(d = 1) - y_0(d = 0)|D = 2] - E[y_1(d = 1) - y_0(d = 0)|D = 1]
\end{aligned}
$$

37. This is consistent with the results reported in table A.3, in which I re-estimated model (5) after I redefined $withdrawal_s$ to equal one for states that only terminated FPUC2 in June 2021.

38. See the Appendix for details.

Specifically, $E[y_1(d=2) - y_1(d=1)|D=2]$ is the ATT of moving from $d=1$ to $d=2$ for the treatment group $D=2$. In other words, it measures the additional employment effect of ending PEUC and PUA, in a counterfactual scenario in which the states that canceled all pandemic UI programs only terminated FPUC2 by July. $\theta_2 - \theta_1 = 0.54$ is an unbiased estimate of this ATT if and only if $E[y_1(d=1) - y_0(d=0)|D=2] - E[y_1(d=1) - y_0(d=0)|D=1] = 0$. This holds if the individuals are equally likely to be selected into states in $D_s = 1$ and states in $D_s = 2$.

The claim that the selection into the two treatment groups is exogenous is a strong yet not entirely implausible assumption. Both groups $D_s = 1$ and $D_s = 2$ are mostly made up of Republican states. Moreover, both moderate and extreme Republican states are scattered in $D_s = 1$ and $D_s = 2$. If one assumes that homophily[39] is true, then residents in Republican states shall be similar. I further verified the assumption by performing a two-sample t-test comparing the average individual characteristics of observations in $D_s = 1$ and $D_s = 2$ before the treatment. At the 5% significance level, the only apparent difference is that the group $D_s = 1$ has more mixed-race people and more people of Hispanic origin (reported in table (A.4)). The fact that neither mixed-race people nor people of Hispanic origin are the dominant racial/ethnicity group among these treated states mitigates the concern. In addition, as shown in Section 2.2.1, both groups of states had similar trends in unemployment rates and job opening rates before the treatment.

If one is willing to make the strong assumption, then we can interpret $\theta_2 - \theta_1$: terminating PEUC and PUA early would lead to an additional 5.4 percentage points increase in the U-E transition rates (conditioned on the early termination of FPUC2). Therefore, the results infer that UI's expanded eligibility and longer potential benefit duration, not the high replacement rates, contributed more to the low U-E transitions during the pandemic.

Do Scheduled Termination and Early Termination Have the Same Effect?

Previous studies did not explore the effect of the scheduled termination of federal UI assistance on re-employment probabilities. I estimate that effect with model 9 and present the results in table 5.

39. Homophily is a fundamental concept in sociology describing the tendency of individuals to associate and bond with similar others.

Across columns (1) - (3), consistent with table 4, the effects of the early termination of all benefits are precisely estimated at around 6 percentage points, and the effects of the early termination of FPUC2 are negligible. There were no changes in the U-E transition rates of unemployed individuals in non-withdrawal states after withdrawal states canceled benefits in advance.

Perhaps more interesting are the following observations. Firstly, as shown in column (1)[40], unemployed individuals in non-withdrawal states did not have a higher U-E transition after the scheduled termination of the benefits. Also, the effects of early termination are short-termed: after September 2021, there was no difference in the re-employment probabilities of unemployed individuals in full-withdrawal states and non-withdrawal states. This isn't totally unexpected, since the unadjusted re-employment rate in full-withdrawal states plummeted in the last period, as shown in table 1. Both results can be potentially explained by a lack of vacant job positions. As shown in figure 3b, the job opening rate in non-withdrawal states did not increase after the scheduled expiration of federal UI assistance, and the job opening rate in full-withdrawal states decreased after being higher between July and September. These findings suggest that the impact of UI depends on macro economic conditions and the demand for labor from employers.

Finally, the estimated coefficient $\theta_{1,2}$, which measures the effects of terminating PEUC and PUA in September, is only statistically significant at 10% in column (3) when monthly FEs are included. The magnitudes are also smaller than the 5.4 percentage points anticipated in the previous subsection. Yet, it is worthwhile to note that the number of observations from the three partial-withdrawal states is very limited, and the statistical power may increase if there was a large sample size.

40. Column (1) of table 9 presents the estimation results when I do not control for monthly trends (so the time effects are purely captured by T_t). The coefficients on T_t in the other two columns are not interpretable because of the inclusion of time FE or quadratic time trends.)

Table 5: Effects of Early and Scheduled Termination of Benefits

	(1)	(2)	(3)
Sample		Ages 25-54	
Partial-Withdrawal	-0.134***	-0.095***	-0.090***
	(0.022)	(0.022)	(0.024)
Full-Withdrawal	-0.210***	-0.222***	-0.227***
	(0.009)	(0.010)	(0.011)
Period 1 (Jul-Aug)	-0.001	0.036**	-0.000
	(0.013)	(0.015)	(0.037)
Period 2 (Sep-Dec)	0.011	0.086**	-0.095*
	(0.029)	(0.033)	(0.051)
Partial#Period 1	0.009	0.004	0.007
	(0.020)	(0.023)	(0.026)
Partial#Period 2	0.021	0.027	0.032*
	(0.019)	(0.017)	(0.017)
Full#Period 1	0.059**	0.059**	0.060**
	(0.024)	(0.026)	(0.026)
Full#Period 2	-0.019	0.000	0.004
	(0.019)	(0.020)	(0.022)
Constant	0.421***	0.542***	0.640***
	(0.054)	(0.075)	(0.065)
Observations	9,252	9,252	9,252
R-squared	0.028	0.030	0.031
Covariates	Preferred	Preferred	Preferred
Monthly Trends	NO	Quadratic	FE

Robust standard errors in parentheses

*** p$<$0.01, ** p$<$0.05, * p$<$0.1

Note: This table reports regression results from model 9. They measure the effects of 1) ending participation in all three programs, 2) withdrawing from only FPUC2, 3) the scheduled expiration of PEUC and PUA, and 4) the scheduled expiration of all three programs on U-E transitions. Data is from the Basic Monthly CPS from February to December 2021, and the estimation sample consists of unemployed individuals in their prime working age. Column (1) had no additional control for time trends, column (2) employs a quadratic function of time, and column (3) employs month FE. Standard errors are clustered at the state level.

The Suspension and the Reinstatement of the Job Search Requirement

Finally, let us turn to the suspension and the reinstatement of the job search requirements. Table 6 lists selected estimated coefficients from model 10, with and without the inclusion of state-specific time trends. Overall, the coefficients of interest do not differ by much in columns (1) and (2). By comparing with the base category (All programs + JSR), I find that terminating all benefits led to a 4.4 percentage point increase in re-employment rates. This estimate is a weighted average of the effects of early termination and scheduled expiration of federal UI assistance (Borusyak, Jaravel, and Spiess 2022), so its magnitude is smaller (than those in previous tables). As expected, the termination of FPUC2 had no effects.

What's intriguing is that unemployed individuals were around five percentage points more likely to become re-employed when the job search requirement was suspended. The magnitude of this coefficient is economically large and unexpected. While the theoretical model in Section 5 predicts that the removal of the JSR would have a somewhat ambiguous overall effect, such a large positive coefficient would require the indirect effect to be higher than the direct effect by magnitudes. Therefore, this paper advises against interpreting this coefficient as the "effect" of waiving the JSR. Rather, the coefficient could reflect some systemic differences (in space and time) between different policy stages. For instance, it could be the case that states without the JSR were less affected by the omicron wave. Another speculation is that observations on the policy stage $policy_{s,t} = 1$ were concentrated on a few specific months with good economic conditions, whereas observations on the policy stage $policy_{s,t} = 2$ were more spread out throughout the sample period. Additionally, the underlying mechanism could also be a reverse causality: states that had trouble with encouraging people to get re-employed decide to reinstate the JSR earlier, and states that had high U-E transitions did not bother to reinstate the JSR at first. Further work is required to investigate this issue and properly quantify the effects of the suspension of the JSR.

Table 6: Estimated Effect of Each Stage on the Policy Paths

	(1)	(2)
VARIABLES	TWFE	State Trend
All programs w/o JSR	0.048***	0.052***
	(0.012)	(0.014)
PEUC/PUA + JSR	0.001	-0.015
	(0.025)	(0.028)
No program + JSR	0.051**	0.044*
	(0.024)	(0.022)
Constant	0.409***	0.605***
	(0.071)	(0.125)
Observations	11,180	11,180
R-squared	0.029	0.034
Covariates	Preferred	Preferred
State FE	Yes	Yes
Month FE	Yes	Yes
State Trend	No	Yes

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports regression results from model 10. They measures the differences in U-E transition rates between states that had all benefits available and had reinstated the JSR with states that 1) had all benefits available and had not reinstated the JSR, 2) states that had terminated only FPUC2, and 3) states that had terminated all federal UI assistance. Data is from the Basic Monthly CPS from February to December 2021, and the estimation sample consists of unemployed individuals in their prime working age. Column (1) is a canonical two-way fixed-effect model, while column (2) controls for a linear state-specific time trend. Standard errors are clustered at the state level.

6.3 Robustness Check

6.3.1 Endogeneity

As with all difference-in-difference models, one of the most significant concerns is that the treatment is endogenous. If individuals more likely to get re-employed were selected for the treatment, then the treatment effect would be overestimated. While I cannot entirely rule out the possibility of endogeneity, two pieces of information suggest that the selection into treatment is arguably exogenous. Firstly, the treatment was at the state level, and an individual's characteristics could not affect the policy decision of the state government. Secondly, as illustrated in Section 2.2.1, non-withdrawal, partial-withdrawal, and full-withdrawal states exhibited similar trends in their unemployment (figure 3a), job-opening (figure 3b), and Covid-related hospitalization rates (figure 5) up to the early termination of benefits. Therefore, neither pandemic severity nor labor market conditions explain the selection of treatment states, and instead, political differences mirror the selection well (figure 4).

6.3.2 Parallel Trend Assumption

A critical assumption underlying in model (7) is the parallel trend assumption (PTA). A common approach to validate the PTA is to estimate an event study model (by replacing the interaction $D_s * post_t$ with $D_s * monthFE_t$). I plot the coefficients of interest in figure (A.3); neither full-withdrawal states nor the partial-withdrawal states had a different pre-trend in re-employment rates compared to the non-withdrawal states.

Since the outcome variable $y_{i,s,t}$ is binary, there is an additional test to perform to rule out the violation of the PTA. The parallel trend assumption states that:

$$E[y_1(0) - y_0(0)|D = i] = E[y_1(0) - y_0(0)|D = 0]$$

, where $y_1(0)$ ($y_0(0)$) is the employed status in the post (pre) period of someone not treated, as indicated by the 0 in the brackets. We can rearrange the equation into:

$$E[y_1(0)|D=i] = E[y_0|D=i] + E[y_1 - y_0|D=0]$$

Notice that the left-hand side (LHS) of the equation is a counterfactual bounded between 0 and 1, whereas the right-hand side (RHS) can take values from 0 to 2. Therefore, if RHS < 0, the LHS must not be equal to the RHS, and the PTA is clearly violated. In my main sample, $E[y_0|D=1] + E[y_1 - y_0|D=0] = 0.234$ and $E[y_0|D=2] + E[y_1 - y_0|D=0] = 0.258$, so there is no evidence that the PTA is breached.

6.3.3 Placebo Test

Are the results in table 4 caused by noise? To mitigate the concern, I conducted ten placebo tests. Namely, I randomly drew half of the non-withdrawal states in each test and assigned them as the "pseudo" treated group. I then estimated a simple DID model (analogous to model 5) with the pseudo-treatment indicator. In only one of the ten tests, the coefficient on the DID estimator was statistically significant. Moreover, half of the estimated coefficient is negative. Therefore, the placebo test is passed, and the validity of the results in table 4 is supported.

Table 7: Placebo Tests

VARIABLES	(2) Seed 1	(3) Seed 2	(4) Seed 3	(5) Seed 4	(6) Seed 5	(7) Seed 6	(8) Seed 7	(9) Seed 8	(10) Seed 9	(11) Seed 10
Pseudo	0.016	0.016	-0.074***	-0.019	-0.002	-0.073***	0.005	0.009	0.000	-0.002
	(0.016)	(0.028)	(0.019)	(0.043)	(0.040)	(0.022)	(0.041)	(0.041)	(0.040)	(0.042)
Post	-0.010	-0.024	-0.027	-0.050	-0.026	-0.026	-0.018	-0.019	-0.021	-0.026
	(0.035)	(0.043)	(0.040)	(0.038)	(0.041)	(0.045)	(0.039)	(0.034)	(0.037)	(0.039)
Interaction	-0.035	-0.005	0.001	0.047**	-0.001	-0.002	-0.021	-0.022	-0.019	-0.002
	(0.023)	(0.024)	(0.024)	(0.022)	(0.023)	(0.025)	(0.023)	(0.026)	(0.021)	(0.023)
Constant	0.298***	0.308***	0.309***	0.321***	0.310***	0.309***	0.305***	0.301***	0.310***	0.309***
	(0.059)	(0.073)	(0.069)	(0.071)	(0.069)	(0.073)	(0.071)	(0.034)	(0.069)	(0.071)
Observations	4,513	4,513	4,513	4,513	4,513	4,513	4,513	4,513	4,513	4,513
R-squared	0.014	0.013	0.013	0.014	0.013	0.013	0.013	0.013	0.013	0.013
covariates	all	all	all	all	all	all	all	all	all	all

Note: This table reports regression results from the ten placebo tests. The interaction term attempts to capture a "pseudo" treatment effect. Data is from the Basic Monthly CPS from February to August 2021, and the estimation sample consists of unemployed individuals in their prime working age only in non-withdrawal states. Standard errors are clustered at the state level.

6.3.4 State-Specific Trends

One may also question if the results found in this section are driven by upward time trends in some populous states in the treatment groups. To address this concern, I additionally control for a state-specific linear time trend in model (7) and model (9). Results are, in general, consistent with table 4 and table 5: the early termination of all three federal UI programs led to a 6-7 percentage points increase in U-E transition probabilities, whereas the withdrawal from only FPUC2 had zero effects. The weaker statistical significance in table 8 can be explained by that the specifications with state trends are too flexible and the sample is not sufficiently large.

Table 8: Model 7 and 9 with State-Specific Time Trends

	(1)	(2)
VARIABLES	Model 7	Model 9
Partial-Withdrawal	-0.232***	-0.153***
	(0.041)	(0.034)
Full-Withdrawal	-0.390***	-0.314***
	(0.130)	(0.061)
Period 1 (Jul-Aug)	-0.035	-0.071**
	(0.063)	(0.031)
Period 2 (Sep-Dec)		-0.294***
		(0.080)
Partial#Period 1	-0.017	-0.037
	(0.039)	(0.038)
Partial#Period 2		-0.031
		(0.035)
Full#Period	0.064	0.070**
	(0.044)	(0.031)
Full#Period 2		0.039
		(0.062)
Constant	0.702***	0.683***
	(0.178)	(0.148)
Observations	6,286	9,252
R-squared	0.038	0.036
Covariates	Preferred	Preferred
State Linear Trend	Yes	Yes

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports regression results from models 7 (column 1) and 9 (column 2), after the inclusion of a state-specific linear time trend. Data is from the Basic Monthly CPS, between February and August 2021 in column (1) and between February and December 2021 in column 2. The estimation sample consists of unemployed individuals in their prime working age. Standard errors are clustered at the state level.

6.4 Discussion

Before I turn to a detailed discussion of specific topics, it is vital to reiterate that this study shall not be read as a welfare analysis. While this thesis documents new evidence of UI's disincentive effects, it in no way suggests that UI extensions during the pandemic or UI, in general, should be eliminated because they harm the economy. Unemployment insurance programs are necessary for both individuals and the economy. During the pandemic, as mentioned in the lit review, federal UI assistance programs provided a safety net for many people and helped smooth consumption (Cox et al. 2020). It requires further research to measure to overall welfare implications of FPUC, PEUC, and PUA.

A Comparison of PEUC and FPUC2

The main analysis subsection suggests that terminating PEUC and PUA before the designated expiration date provided strong employment incentives, whereas withdrawing early from FPUC2 had little effect. But why? It might appear that the extraordinarily huge increase in PBD (from 26 weeks to 99 weeks) caused by PEUC generated a substantial amount of disincentive effects and dwarfed the increase in WBA of only $300. However, for unemployed individuals after June, their *effective* PEUC duration was at most 13 weeks (the number of weeks between July 12th and September 4th), as all benefits would expire on September 4th. Consider a UI-eligible unemployed individual, A, living in a partial-withdrawal state whose regular WBA is $300. Terminating FPUC2 in June decreased A's WBA by $300 for at most 13 weeks. Nonetheless, if A's state of residence decided to also withdraw from PEUC, A would lose at most another $300 * 13 weeks of benefits[41]. Therefore, A would lose the same amount of non-labor income from the withdrawal from FPUC2 and PEUC; the effects of the withdrawal from FPUC2 and PEUC (plus PUA) on re-employment, however, differed starkly, according to the estimation results.

One explanation behind the observed "paradox" is that the coefficient estimate represents the *average* treatment effect (on the treated). For workers ineligible for regular UI, the termination of

41. This is an overestimate of the loss in benefits. If A had more than 13 weeks of their regular UI PBD left, the termination of PEUC would not affect A's *effective* total PBD at all.

PUA (which coincided with the termination of PEUC) took away *all* their unemployment benefits and "forced" them to become employed immediately. Therefore, the considerable magnitude of the combined effect of withdrawing from PEUC and PUA partially reflects the effect of PUA on the self-employed and gig-economy workers.

More importantly, the law of diminishing marginal utility also offers an explanation. Removing FPUC2 reduced A's WBA from $600 to $300, while removing PEUC reduced A's WBA from $300 to nothing. The marginal utility from the first dollar is much higher than the marginal utility from the 301st dollar because the first dollars are essential for buying necessities such as food.

Essentially, the WBA in a partial-withdrawal state equaled the pre-pandemic level. Since the state UI programs are designed to support workers during job loss, the pre-pandemic level of WBA should enough for making a living and meeting basic needs. As a result, UI claimants can afford to[42]choose to remain unemployed and replace work with leisure or household production. Therefore, even without FPUC2, the regular benefits induce a sizable moral hazard effect. On the other hand, when PEUC was cut, the benefits available to unemployed individuals who had already exhausted their regular benefits (and EB, if applicable) immediately dropped to $0. With imperfect credit markets, these individuals would have to rely on their savings to pay for essential goods and services if choosing to remain unemployed. Considering the fact that half of the job losers in the United States report zero liquid wealth at the time of job loss (Chetty 2008), we would expect individuals who lost their benefits to be strongly motivated to find a job. Thus, the employment incentives provided by the withdrawal from PEUC were larger than those provided by the withdrawal from FPUC2, even when the total amount of benefits lost was the same.

The policy implication of the above findings is that a moderate increase in WBA does not harm the job-matching process as much. If a similar[43] extension in PBD provides similar protection for job losers, future policymakers shall prioritize increasing the WBA when expanding UI during recessions because it generates fewer distortion effects. Nevertheless, whether a comparable increase in WBA

42. UI claimants may also have additional sources of non-labor income (e.g., spouses' incomes).
43. By similar, I refer to an extension to the PBD that would result in a similar total amount of benefits increased.

and PBD provides equally adequate support during job loss remains an unexamined hypothesis.

Limitations

There are several areas for improvement in this thesis. For instance, as mentioned earlier, the estimated "effect" of suspending the job search requirement is unexpectedly large and positive. Below are several other limitations of this research; I plan to address them (if possible) in an updated version of this paper.

Firstly, we do not observe whether participants in the basic monthly CPS panel claim UI benefits. Suppose individuals in withdrawal and non-withdrawal states may have different propensities to take up benefits, and their political differences also drive that difference. In that case, the selection into the intended-to-treat and the selection into the actual treatment is correlated and would introduce bias. If withdrawal states' residents view UI as a "reap-off" from others, they may be less likely to file a UI claim, and the *average* effect of (the termination of) federal UI assistance would be smaller in withdrawal states when my sample includes all unemployed observations (instead of only UI claimants). The different degrees of urbanization between withdrawal and non-withdrawal notes may also affect UI take-up: land can provide insurance to job losers, as they can produce on their farms. If unemployed individuals in withdrawal states are more likely to own land, they may be less likely to file UI claims. Nonetheless, given the generosity of UI during the pandemic, especially in 2020[44], individuals in both withdrawal and non-withdrawal both should have had strong motives to collect unemployment benefits. Also, even if the bias exists, it would put downward pressure on the coefficient estimates and would not result in type-I errors.

Additionally, while this thesis offers a method to isolate the impact of terminating PUA and PEUC, we still cannot separate the effects between them. This is because, in each state, the termination (or expiration) of PUA and PEUC happened on the same date. However, the separate effect of PUA and PEUC, respectively, is of interest, notably because the extension provided by PUA was unprecedented. To identify the effect of PUA separately, one would need information about unemployed

44. If one lost one's job in 2020, and if one stayed unemployed, one's UI claim would automatically continue to 2021.

individuals' eligibility for regular state benefits. While constructing a proxy for UI eligibility is possible via CPS's ASEC Supplement, no early termination or scheduled expiration of PUA occurred in the first half of 2021. Therefore, it would be impossible to address this issue with CPS data. Potential researchers can utilize banks' or other financial institutions' transaction-level data to infer unemployed individuals' eligibility for regular UI and observe whether they collected benefits (in their cases, the benefits would be from the PUA program). Then, a diff-in-diff design analogous to the one in Coombs et al. (2022) would provide a good estimate of PUA's effect on reemployment and post-unemployment earnings.

Finally, the identification of this thesis relies on a "natural experiment": the early termination of federal UI programs in June 2021. In most of the main analysis, I divided states into three groups: non-withdrawal, partial, and full-withdrawal states. While I have presented evidence that 1) the selection of treatment states is not endogenous with the outcome variable and 2) the two treated groups are similar, the latest developments in the econometrics literature (e.g., Callaway, Goodman-Bacon, and Sant'Anna (2021)) suggest that even stronger assumptions may be required to interpret the results as the ATE since the treatment is not binary and its delivery was staggered. My next step is to verify the "strong PTA" discussed in the literature and use newly-developed econometric methods to correct for the selection bias.

7 Conclusion

This thesis project explores the role of the temporary federal UI assistance programs in the labor shortage of 2021. Using a sub-sample of prime working-age unemployed individuals in the CPS dataset, I quantify the effects of the termination of each temporary UI program on unemployment-to-employment transitions using difference-in-difference estimations. This research builds upon sparse preexisting literature that exploits the early termination of FPUC2, PEUC, and PUA as a natural experiment. Additionally, the paper presents the first attempt that I know of to estimate the effect of the scheduled expiration of benefits in September 2021 and the impact of reinstating the job search requirement.

By estimating several DiD models with multiple treatment levels (and multiple periods in some cases), I find evidence that the early termination of all programs in June resulted in a 6-7 percentage point increase in the Unemployment-to-Employment transition rates. This effect is not as large as the previous literature indicates and was short-termed. The early termination of Federal Pandemic Unemployment Compensation (FPUC2) alone had negligible effects. In addition, under strong assumptions, terminating Pandemic Emergency Unemployment Compensation and Pandemic Unemployment Assistance in advance (in addition to FPUC2) caused a roughly 5 percentage point increase in U-E transition rates. Finally, the scheduled termination of these programs did not increase the U-E flow. Overall, the results infer that UI's expanded eligibility and longer potential duration, not the high benefit amount, reduced re-employment in 2021. The policy implication of this research is that extending WBA instead of PBD may produce fewer disincentives to employment.

This study has several limitations, as discussed in the previous subsection. Unfortunately, the basic monthly Current Population Survey does not allow us to observe UI take-up or infer UI eligibility for regular state benefits. Future research may use more detailed datasets, especially transaction-level datasets from financial institutions, to separately identify the effect of PEUC on benefit claimants who are UI-eligible and the impact of PUA on claimants who are UI-ineligible. More importantly, to interpret the above results as causal ATE, we may need to make even stronger assumptions that are not proven here rigorously. Despite these limitations, this thesis offers perceptive insight into the unemployment benefits facet of the developing COVID-19 pandemic literature. In the future, I hope to expand this research by verifying the "strong Parallel Trend Assumption" proposed in Callaway, Goodman-Bacon, and Sant'Anna (2021) and utilizing newly developed difference-in-difference estimators.

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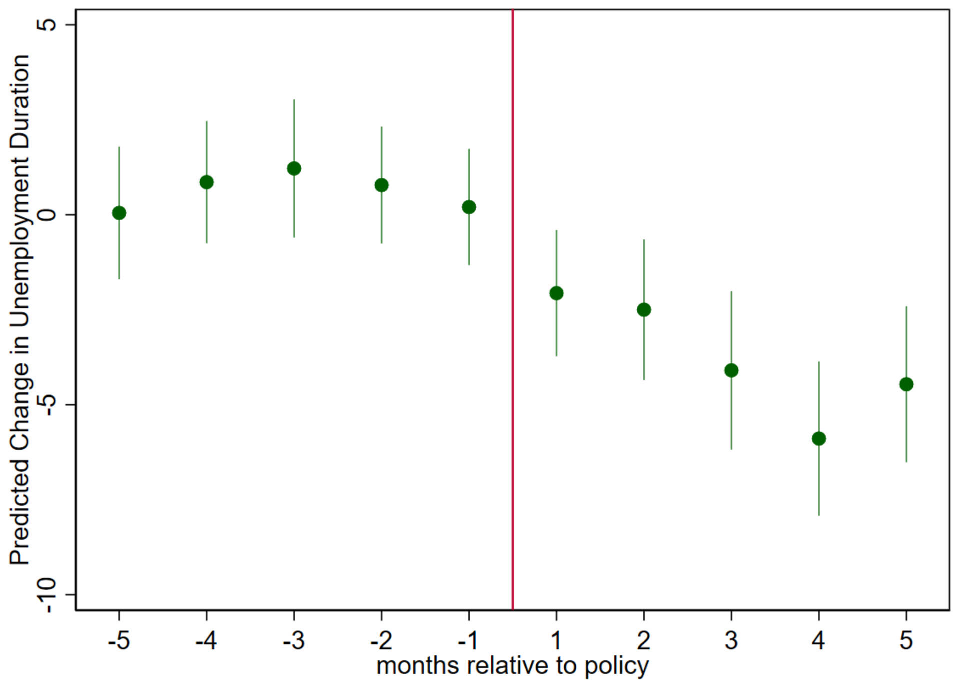
Appendix A: Figures & Tables

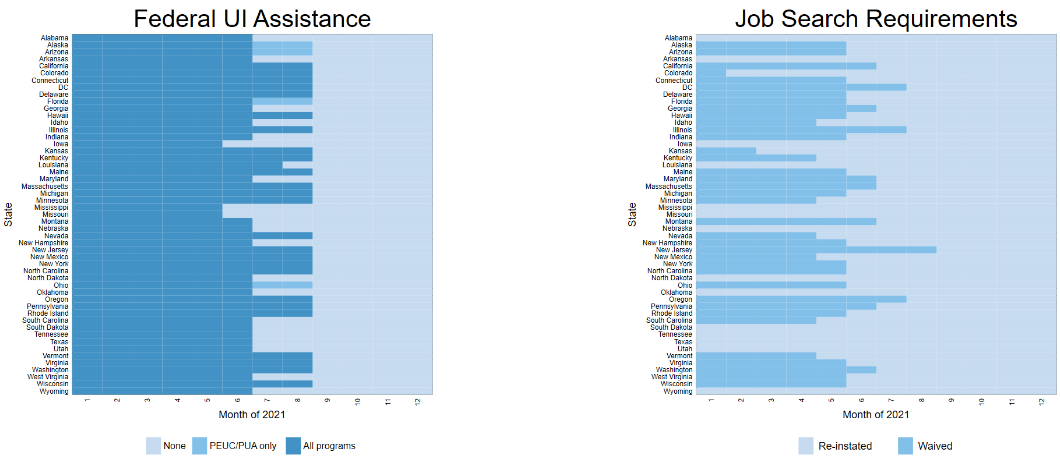
Figures

Figure A.1: Changes in Average Unemployment Duration around the Federal UI Assistance Expiration



Note: This figure reports the changes in average unemployment duration (in weeks) around the termination of federal UI assistance in each state, estimated by a simple event study analysis. The sample is from the Basic Monthly CPS in 2021. The red vertical line represents the month when the benefits were cut; the mean unemployment duration in that month serves as the baseline value.

Figure A.2: Separate Policy Paths for Federal UI Assistance and Job Search Requirements

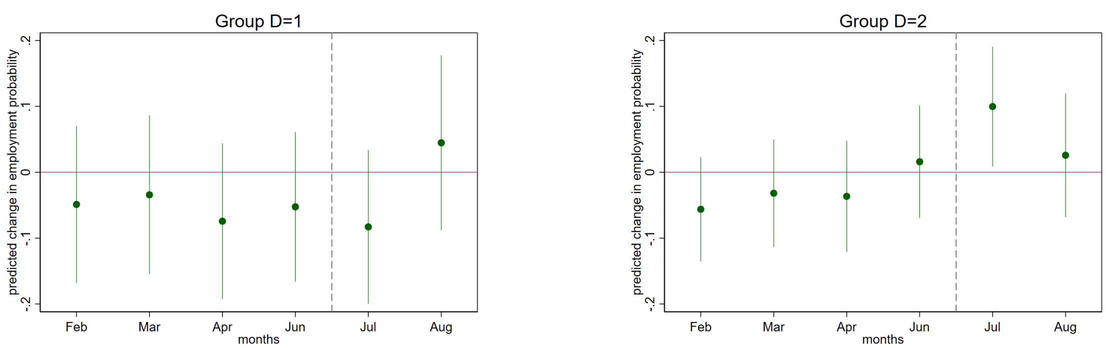


(a) Policy Paths for Federal UI Assistance

(b) Policy Paths for Job Search Requirements

Note: These two figures report the policy paths in all fifty states regarding the availability of federal UI assistance programs and the status of the job search method, respectively. The sample period is the whole year of 2021. Data is from the UI Tracker.

Figure A.3: Event Study Analysis: Monthly Re-employment Rates



(a) Partial-Withdrawal v.s. Non-Withdrawal

(b) Full-Withdrawal v.s. Non-Withdrawal

Note: This figure reports the differences in monthly re-employment rates between non-withdrawal states and partial- or full-withdrawal states, respectively. The plotted results are coefficients estimates obtained by estimating model 8 (an event study analysis). The sample is from the Basic Monthly CPS from February to August 2021. The vertical dashed line divides the graph into two periods: between v.s. after early termination. The baseline category is May 2021.

Tables

Table A.1: Pooled Summary Statistics

VARIABLES	(1) mean	(2) sd
Female	0.474	0.499
Age	38.54	8.670
Hispanic origin	0.204	0.403
Married	0.390	0.488
Any disability	0.0721	0.259
Own children age 5 or younger	0.173	0.378
Educ: Less than HS	0.107	0.309
Educ: HS degree	0.628	0.483
Educ: Bachelor's degree	0.189	0.392
Educ: Graduate degree	0.0756	0.264
Race: White	0.708	0.455
Race: Black	0.175	0.380
Race: American Indian	0.0218	0.146
Race: Asian	0.0714	0.258
Race: Mixed	0.0233	0.151

Note: This table displays the pooled summary statistics of prime working-age unemployed individuals in the US, from February to December 2021. Data is from the Basic Monthly CPS.

Table A.2: Re-estimating Model5 after Removing Spurious Transitions

	(1)	(2)	(3)	(4)	(5)	(6)
			Replication after Recoding			
			Ages 25-54			
Sample	Ages 25-54		not Graduated College		Ages 16-64	
Full-Withdrawal	-0.300***	-0.310***	-0.295***	-0.298***	-0.261***	-0.282***
	(0.017)	(0.024)	(0.018)	(0.033)	(0.015)	(0.019)
Post	-0.064	-0.028	-0.074	0.002	-0.050	-0.039
	(0.043)	(0.038)	(0.055)	(0.053)	(0.036)	(0.030)
Interaction	0.103***	0.089***	0.037	0.024	0.087**	0.067**
	(0.035)	(0.032)	(0.046)	(0.049)	(0.033)	(0.031)
Constant	0.377***	0.557***	0.354***	0.545**	0.433***	0.763***
	(0.040)	(0.188)	(0.042)	(0.214)	(0.034)	(0.140)
Observations	4,061	4,061	2,956	2,956	6,615	6,615
R-squared	0.033	0.040	0.040	0.045	0.029	0.038
Covariates	Alternative	Preferred	Alternative	Preferred	Alternative	Preferred

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports regression results from model 5 that measure the effects of ending participation in FPUC2, PEUC, and PUA early on U-E transitions, after removing spurious transitions using the method documented in Appendix C. The sample is the Basic Monthly CPS from February to August 2021. The "alternative" covariates are the set of covariates used in Holzer, Hubbard, and Strain (2021), whereas the "preferred" covariates are a more comprehensive set of covariates used in the main analysis of this thesis. Standard errors are clustered at the state level.

Table A.3: DID Estimates of the Effects of Terminating FPUC2 Early Only

	(1)	(2)	(3)
	DID	DID	DID
VARIABLES	25-54	25-54	25-54
Partial-Withdrawal	0.005	-0.144***	-0.027
	(0.017)	(0.012)	(0.089)
Post	0.002	-0.015	0.012
	(0.010)	(0.034)	(0.028)
Interaction	0.028	0.053*	0.027
	(0.025)	(0.027)	(0.029)
Constant	0.228***	0.287***	0.509***
	(0.007)	(0.032)	(0.156)
Observations	5,336	5,336	5,336
R-squared	0.000	0.013	0.021
Covariates	No	Alternative	Preferred

Robust standard errors in parentheses

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Note: This table reports regression results that measure the effects of ending participation in FPUC2 (only) early on U-E transitions. The sample is the Basic Monthly CPS from February to August 2021. The "alternative" covariates are the set of covariates used in Holzer, Hubbard, and Strain (2021), whereas the "preferred" covariates are a more comprehensive set of covariates used in the main analysis of this thesis. Standard errors are clustered at the state level.

Table A.4: Two Sample T-test of Individual Characteristics between Full-Withdrawal States and Partial-Withdrawal States

	Mean		t-test	
Variable	Treated	Control	t	p>t
Female	.42901	.45183	-0.97	0.335
Married	.40636	.37043	1.54	0.124
Educ: HS degree	.67136	.6794	-0.36	0.719
Educ: Bachelor's degree	.14871	.17608	-1.58	0.114
Educ: Graduate degree	.05386	.07309	-1.71	0.087
Own children age 5 or younger	.17381	.19767	-1.30	0.193
Age group: 25-34	.37148	.38206	-0.46	0.647
Age group: 35-44	.30355	.30731	-0.17	0.864
Age group: 45-54	.28213	.27409	0.38	0.707
Race: Black	.18299	.15615	1.48	0.139
Race: American Indian	.02815	.04153	-1.60	0.110
Race: Asian	.0257	.02326	0.33	0.743
Race: Mixed	.0104	.04485	-5.23	0.000
Hispanic origin	.18115	.22425	-2.29	0.022
Any disability	.08752	.06977	1.35	0.177

Note: This table reports the differences in the mean individual characteristics between full- and partial-withdrawal states and the corresponding t-test results. The sample is the Basic Monthly CPS from February to August 2021.

Appendix B: Math Behind "Inferring the Effect of Terminating PEUC and PUA Early"

The θ_is in table 4 document the effects of $d = 1$ (terminating FPUC2) and $d = 2$ (terminating all programs). One natural question to ask is what the effects of terminating PEUC and PUA are. Here, I provide a suggestive argument to measure it with $\theta_2 - \theta_1 = 0.54$.

Recall that for $i = 1, 2$, $\theta_i = E[y_1(d = i) - y_1(d = 0)|D = i]$. We can re-write the difference between θ_2 and θ_1 as:

$$
\begin{aligned}
\theta_2 - \theta_1 &= E[y_1(d=2) - y_1(d=0)|D=2] - E[y_1(d=1) - y_1(d=0)|D=1] \\
&= E[y_1(d=2) - y_1(d=1) + y_1(d=1) - y_1(d=0)|D=2] - E[y_1(d=1) - y_1(d=0)|D=1] \\
&= E[y_1(d=2) - y_1(d=1)|D=2] + E[y_1(d=1) - y_1(d=0)|D=2] - E[y_1(d=1) - y_1(d=0)|D=1] \\
&= E[y_1(d=2) - y_1(d=1)|D=2] + E[y_1(d=1) - y_0(d=0) + y_0(d=0) + y_1(d=0)|D=2] \\
&- E[y_1(d=1) - y_0(d=0) + y_0(d=0) + y_1(d=0)|D=1]
\end{aligned}
$$

Assuming the conventional parallel trend assumption holds ($E[y_0(d = 0) + y_1(d = 0)|D = 2] = E[y_0(d = 0) + y_1(d = 0)|D = 1]$), one can simply the equation into:

$$\theta_2 - \theta_1 = E[y_1(d=2) - y_1(d=1)|D=2] + E[y_1(d=1) - y_0(d=0)|D=2] - E[y_1(d=1) - y_0(d=0)|D=1]$$

Specifically, $E[y_1(d = 2) - y_1(d = 1)|D = 2]$ is the ATT of moving from $d = 1$ to $d = 2$ for the treatment group $D = 2$. In other words, it measures the additional employment effect of ending PEUC and PUA, in a counterfactual scenario in which the states that canceled all pandemic UI programs only terminated FPUC2 by July. $\theta_2 - \theta_1 = 0.54$ is an unbiased estimate of this ATT if and only if $E[y_1(d = 1) - y_0(d = 0)|D = 2] - E[y_1(d = 1) - y_0(d = 0)|D = 1] = 0$. This holds if the individuals are equally likely to be selected into states in $D_s = 1$ and states in $D_s = 2$.

The claim that the selection into the two treatment groups is exogenous is a strong yet not entirely implausible assumption. Both groups $D_s = 1$ and $D_s = 2$ are mostly made up of Republican states. Moreover, both moderate and extreme Republican states are scattered in $D_s = 1$ and $D_s = 2$. If one assumes that homophily[45] is true, then residents in Republican states shall be similar. I further verified the assumption by performing a two-sample t-test comparing the average individual characteristics of observations in $D_s = 1$ and $D_s = 2$ before the treatment. At the 5% significance level, the only apparent difference is that the group $D_s = 1$ has more mixed-race people and more people of Hispanic origin (reported in table (A.4)). The fact that neither mixed-race people nor people of Hispanic origin are the dominant racial/ethnicity group among these treated states mitigates the concern. In addition, as shown in the summary statistics, both groups of states had similar trends in unemployment rates and job opening rates before the treatment.

If one is willing to make the strong assumption, then we can interpret $\theta_2 - \theta_1$: terminating PEUC and PUA would lead to an additional 5.4 percentage points increase in the U-E transition rates (conditioned on the early termination of FPUC2). Therefore, the results infer that UI's expanded eligibility and longer potential benefit duration, not the high replacement rates, contributed to the labor shortage.

45. Homophily is a fundamental concept in sociology describing the tendency of individuals to associate and bond with similar others.

Appendix C: Removing Spurious Transitions out of Unemployment in CPS

As discussed in Section 3, the inconsistency between unemployment duration and employment status is of concern to many economists. An example of an inconsistent observation would be an individual who was employed two months ago reporting their unemployment duration to be 16 weeks. Economists widely well document this issue: both Abowd and Zellner (1985) and McGovern and Bushery (1999) highlighted that both unemployment status and unemployment duration are often misreported. I prefer to "trust" the employment status variable over the unemployment duration variable because it is easier for an individual to know their *current* employment status as opposed to when they became unemployed *in the past*.

Nevertheless, a large collection of empirical research that uses CPS as its primary data set chooses to recode "suspicious" between-month movements in an individual's employment status. Poterba and Summers (1986) documented that many CPS survey respondents cannot correctly determine their employment status, and errors are especially prone to occur when someone has been unemployed for an extended period (unemployed or NILF?). Recent UI literature, including Rothstein (2011), Farber, Rothstein, and Valletta (2015), and Fujita (2010), consistently choose to regard temporary transition out of unemployment spells as spurious and removed those movements. Holzer, Hubbard, and Strain (2021) described how they removed spurious transitions out of unemployment in CPS: "For individuals who transition out of unemployment and into employment or nonparticipation in one month, but then return to unemployment in the following month (i.e., U-E-U or U-N-U), we consider the transition spurious, and recode the respondent as having been unemployed for each of the three months. " We followed this procedure when replicating their results in Section 6.1.

There are, however, two other important caveats with this procedure. The above procedure requires us to observe respondents in three consecutive months of the four-month CPS rotation, reducing the size of the matched sample by approximately one-third. The sample size is already too limited for the regression analysis, and removing an additional 33% of observations will further increase the standard errors of the estimated coefficients. Secondly, while previous literature found that U-N-U movements are often the result of misreporting, the accuracy of U-E-U movements is relatively higher, as many of such movements reflect a temporary job. Removing those noisy (yet genuine) movements could lead us to overestimate the policy effect.