

Policy Memo:

“Balancing Reemployment Incentives and Poverty
Prevention: Evidence from Early Termination of
Pandemic Unemployment Benefits”

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Economics 101: The Economics of Aging and Social Insurance in the
United States

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June 6, 2025

I. Executive Summary

Unemployment insurance (UI) serves a dual purpose in economic policy: it acts as a critical automatic stabilizer that helps households avoid poverty and enables workers to smooth consumption during job loss, while simultaneously incentivizing workers to find new jobs. During COVID-19, the U.S. federal government offered a \$300 weekly UI supplement to support displaced workers, in addition to states' existing UI benefits. In May 2021, 25 states announced plans to end this supplement early, by June. The remaining states maintained benefits through early September, as scheduled. This staggered policy change provides a natural experiment to ask two critical questions: first, how do job-finding rates respond when unemployment benefits are cut or when cuts are announced? Second, how much do reductions in UI benefits impact poverty for low- and middle-income recipients?

We reach three main findings. First, the combination of announcing and then implementing the early termination significantly increased job-finding rates by 6.13 percentage points (108%) through August 2021. Second, the combined effect of the announcement and the policy termination had a larger and more immediate impact than termination alone, suggesting that expectations played a critical role in shaping worker behavior. Third, within states that announced and exited UI early, the family income-to-poverty ratio for low- to middle-income households increased by 8.33%, although we note that this occurred in the context of high job vacancies conducive to easier reemployment and higher wages. Therefore, we recommend that policymakers should reduce supplemental UI benefits instituted during recessions once job vacancies are high. More broadly, they should maintain clear, advanced communication followed by fast implementation—as well as careful monitoring of job vacancy rates—to ensure faster workforce reentry without an increase in poverty rates.

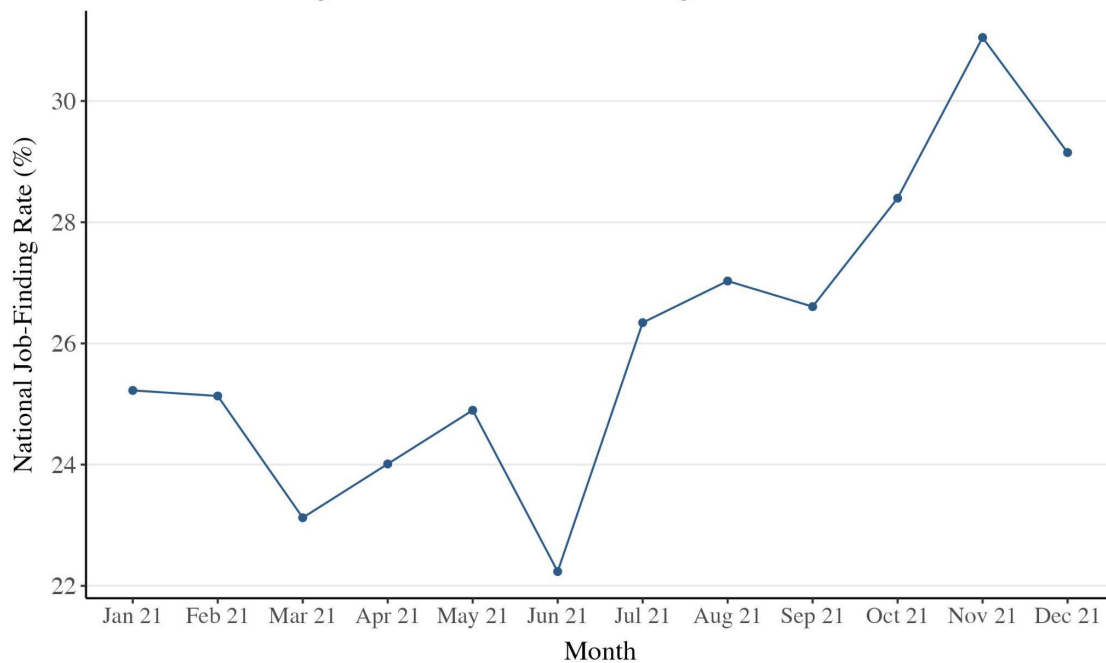
II. Introduction

Designing UI policy involves a careful act of balance—ensuring adequate support for jobseekers without reducing incentives to return to work. Unemployment benefits remain a critical source of income

for those near the poverty level. However, if benefits delay reemployment, they may lower overall productivity by keeping capable workers out of the labor market for longer, slowing the reallocation of productive labor to firms and the regaining of an income source for individuals. Striking this balance is essential for designing UI policy; however, few natural experiments exist to support where it lies. UI policy during the COVID-19 pandemic provides answers. To support unemployed workers, the federal government introduced flat-rate UI supplements of \$300 per week, beginning in January and set to expire in September 2021 (Whittaker & Isaacs). However, in May 2021, 25 states announced plans to end the \$300 supplement a month later, in June, creating a unique natural experiment to evaluate how expectations and benefit generosity shape labor market behavior and poverty.

Coincidentally, the national job-finding rate rose sharply when COVID UI benefits began to expire early after June and had another peak after they expired as scheduled in September (see Figure 1). Understanding whether these spikes resulted from policy changes or wider labor market trends remains unclear.

Figure 1: National Job-Finding Rate in 2021



Job-finding rate defined as the percent of unemployed people finding a job in a given month.
Source: FRED.

Therefore, our paper examines this moment to answer two key policy questions: (1) How does the job-finding rate respond to cuts in unemployment benefit levels and to announcements of those cuts? (2) how much do reductions in UI benefits impact poverty for low- and middle-income recipients? Using a nationally representative dataset, a difference-in-differences framework, and an event-study design, we find that states that announced early termination of the \$300 federal UI supplement saw a 6.13 percentage point (108%) increase in job-finding rates through August 2021. We isolate the effects of solely the announcement by using an event study model, and find an increase of 3.87 percentage points (59.6%) in the job-finding rate. Although this result is not statistically significant, it suggests that, directionally, the policy announcement generated some response among workers. We further find an increase of 0.13 (8.33%) in the family income-to-poverty ratio for low- and middle-income households.

III. Literature Review

Our paper makes original contributions to the existing literature on unemployment in two ways. First, whereas most research focuses on the effect of expanded unemployment benefits on the job-finding rate, we estimate this effect during extraordinary times, where income from unemployment benefits exceeded income from working for many individuals. This result is key for policymakers since it allows them to directly calculate the administrative and moral hazard costs of benefit expansions amid strained government finances. Second, we estimate the effect of reducing UI benefits on family income relative to the poverty line, providing a direct measure of how benefit generosity influences household economic well-being. This is especially valuable for understanding UI's capacity to prevent poverty during unemployment, a dimension that has received less attention in the COVID-19 policy context. Answering both questions using the same, nationally-representative dataset allows policymakers to directly weigh the trade-off between supporting unemployed workers and encouraging reentry into the labor market.

We complement the existing literature associating UI generosity with moral hazard, which focuses on the effect of more generous UI benefits on longer durations of unemployment. Meyer (1990)

supports this hypothesis, finding that a 10% increase in UI benefits raises unemployment duration by 4-5%. Other studies have supported this conclusion, like Leung, Mas, and Pei (2015), who also find that higher UI benefits extend unemployment spells, with elasticities between 0.3 and 0.5. Bell, Hedin, Schnorr, and von Wachter (2024) find that while the behavioral effects of UI generosity on labor supply are stable, the elasticity of unemployment duration rises during recessions. However, some researchers attribute this to liquidity constraints rather than reduced job search effort. Chetty (2008) found that liquidity-constrained individuals extend their unemployment durations when UI is available. These effects are especially pronounced among individuals with low assets or limited access to credit, who would otherwise be forced to accept lower-quality jobs rapidly. As a result, UI benefits serve a critical consumption-smoothing role beyond mere disincentives to work. We build upon this literature by using a sharp difference-in-difference model exploiting variations in benefit reduction during extraordinary times while maintaining relatively stable liquidity conditions. This design helps us better isolate the causal effect of UI generosity on unemployment duration in this recent scenario.

A smaller but growing body of research examines how unemployment benefits affect family income and poverty levels. Bitler and Hoynes (2016) find that UI plays a critical role in stabilizing income and reducing poverty during economic downturns, especially among low-income households. Ganong and Noel (2019) suggest that households behave myopically when receiving UI benefits and maintain roughly stable consumption levels until those benefits suddenly end, instead of being forward-looking and adapting their spending as UI expiration nears. Similarly, Rothstein and Valletta (2017) conclude that UI helps households smooth consumption and avoid immediate material hardship, even if it does not fully prevent longer-term income losses. This literature complements existing research on job-finding and labor supply by emphasizing the protective, anti-poverty function of UI. This prior work provides important context for our analysis of how early benefit termination shaped family income-to-poverty ratios during COVID-19. However, we add to it by using the same dataset to observe

UI generosity cuts' impact on both job-finding and poverty levels simultaneously, allowing policymakers to fairly weigh this trade-off.

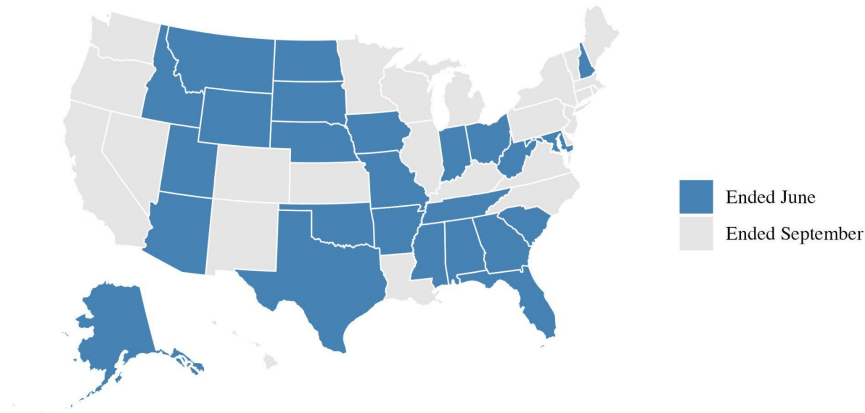
Our paper also extends the literature studying how expanded unemployment benefits impacted the U.S. labor market during the COVID-19 pandemic. Much of this research focuses on how flat-rate benefit supplements discouraged job-searching. For example, Ganong, Noel, and Vavra (2020) find that three-fourths of unemployed workers received more in benefits than their previous wages, reducing incentives to find a new job. Similarly, Petrosky-Nadeau and Valletta (2024) report that a \$600-per-week expansion of benefits led to a moderate, 6.5 percentage-point decline in weekly job-finding rates. When the \$600 expansion expired and was later replaced with a \$300 supplement, Ganong *et al* (2024) found that it caused a smaller decrease in the weekly reemployment rate — between a 0.59 to 1.18 percentage-point drop. This is relevant when considering that between April and July 2020, the median statutory replacement rate for unemployed workers is 145%, far exceeding workers' pre-job loss wages (Ganong *et al*, 2020). Conversely, in unpublished research similar to ours, Coombs *et al* (2022) use a difference-in-differences approach to find that the early expiration of the \$300 supplement in some states caused a 6.8 percentage-point (28%) increase in weekly job-finding rates. While Coombs *et al* (2022) use a small sample of low-income households with limited credit access, we use a much larger, nationally-representative dataset, which lends credence to our estimates.

IV. Institutional Details & Data

Beginning the first week of January 2021, the Biden administration activated the Federal Pandemic Unemployment Compensation (FPUC) program. Aiming to support families in staying above the poverty line and smoothing consumption to avoid macroeconomic collapse during the pandemic-induced recession, FPUC added a \$300 per-week supplement to the UI benefits that each state already offered. Although the supplement was set to expire in September 2021, a key policy change occurred when 25 states announced in May that they would terminate the federally-funded \$300 weekly

UI supplement early, in June or the first week of July (see Figure 2), while the remaining states continued payments as scheduled through early September (Whittaker & Isaacs, 2021).

Figure 2: States Ending UI Benefits Early



To analyze the effect of this policy divergence, we use monthly panel data from the 2021 wave of the Survey of Income and Program Participation (SIPP), which provides detailed information on individual unemployment spells, UI benefit receipt, wages, and demographic characteristics. Our sample includes working-age individuals (18–64) who experienced unemployment at any point during the year and received UI benefits. Our age sample restriction excludes those likely retired or too young to have been actively engaged in the labor market. We further exclude individuals who received UI benefits but did not report either the beginning or end of their unemployment spell to ensure that we accurately identify the duration of unemployment, as benefit cessation does not necessarily indicate reemployment—it may reflect expiration of eligibility or exit from the program for other reasons. To capture post-policy labor market responses, we restrict the sample to individuals with non-missing data from April through December 2021. These restrictions reduce our sample from approximately 1,000 to 576 individuals. Per SIPP weights, they represent an estimated 5 million people in the U.S. labor force.

Table 1 below presents summary statistics for the main analytical sample of 576 individuals. Although our data source is meant to be nationally representative, we notice that it overrepresents White respondents—71% early termination and 66% later termination, versus 60.5% national average—and underrepresents those with a college degree and union members, relative to the national average (US Census, 2020). This is consistent with our sample being focused on those enrolling in government welfare programs. The overall job-finding rate is lower than expected, hovering around 8-9% per month, whereas national rates exceed 20% (FRED, 2021). Our results, however, are similar to prior research by Ganong and Noel (2021), who use transaction-level banking data to find a roughly 3% weekly reemployment rate in 2021. In addition, we note that much of our sample likely skews toward individuals with weaker labor force attachment—such as long-term unemployed, lower-income households, or those more reliant on public assistance—who tend to have slower reemployment rates regardless of economic conditions.

Table 1: Summary Statistics

	States Terminating Early <i>N = 2,048</i> <i>No. individuals: 176</i>		States Terminating in September <i>N = 4,843</i> <i>No. of individuals: 410</i>	
	Mean	Std. Dev.	Mean	Std. Dev.
Job-Finding Rate (%)	9.07	28.6	8.03	27.0
First Month Unemployed	1.87	2.25	1.76	2.18
Last Month Unemployed	9.96	2.82	10.33	2.62
Monthly UI Received (\$)	523.05	979.53	508.41	965.63
Family Income-to-Poverty Ratio	3.16	3.69	3.39	4.73
Age	41.7	12.9	40.0	12.4
Union Member	0.01	0.11	0.04	0.20
Male	0.50	0.50	0.46	0.50
<i>Race</i>				
White	0.71	0.45	0.66	0.47
Black	0.25	0.43	0.20	0.40
Asian	0.02	0.15	0.08	0.28
Other	0.02	0.15	0.07	0.24
Hispanic	0.22	0.41	0.26	0.44
<i>Education</i>				
No high school	0.01	0.10	0.02	0.13
Less than high school	0.06	0.24	0.07	0.25
High school graduate	0.26	0.44	0.31	0.46
Some college	0.42	0.49	0.31	0.46
College+	0.25	0.43	0.29	0.45

Notes: *N* refers to person-months in our panel dataset.

V. Empirical Analysis

a. *Effect of Supplement Cut and Cut Announcement on Job-Finding Rates*

In our empirical analysis, we employ a standard differences-in-differences specification. This strategy leverages the staggered policy change, such that our treated group are states that announced an early exit in May and implemented it in June, while our control are those that exited in September. Specifically, we aim to identify the causal effect of early UI expiration—both its announcement and implementation—on the probability that an unemployed individual in month $t-1$ finds a job in month t . We run two versions of our difference-in-differences model: the first uses the May announcement as treatment and combines the effects of both announcing and then implementing the early exit; the second uses the June exit as treatment and thus only captures the effect of the termination. We use the following specification:

$$Y_{it} = \alpha + \beta_1 \text{EarlyExit}_i + \beta_2 \text{Post}_t + \beta_3 (\text{EarlyExit}_i \times \text{Post}_t) + \gamma_t + \delta_i + \epsilon_{it} \quad (1)$$

Where:

- Y_{it} : outcome variable—in this case, the job finding rate—for individual i in month t .
- EarlyExit_i : indicator = 1 if the individual is in a state that ended UI early.
- Post_t : indicator = 1 for months after the beginning of treatment (May or later for the exit's announcement in our first regression, June or later for the exit's enactment in our second).
- $(\text{EarlyExit}_i \times \text{Post}_t)$: interaction term capturing whether an individual lived in a state that exited early, after either the announcement of or the exit.
- β_3 : Difference-in-differences estimator — the causal effect of early UI expiration on job finding. This answers, “how much more did job-finding rates change in states that ended UI early compared to those that did not, after treatment?”
- γ_t : month-level fixed effects.

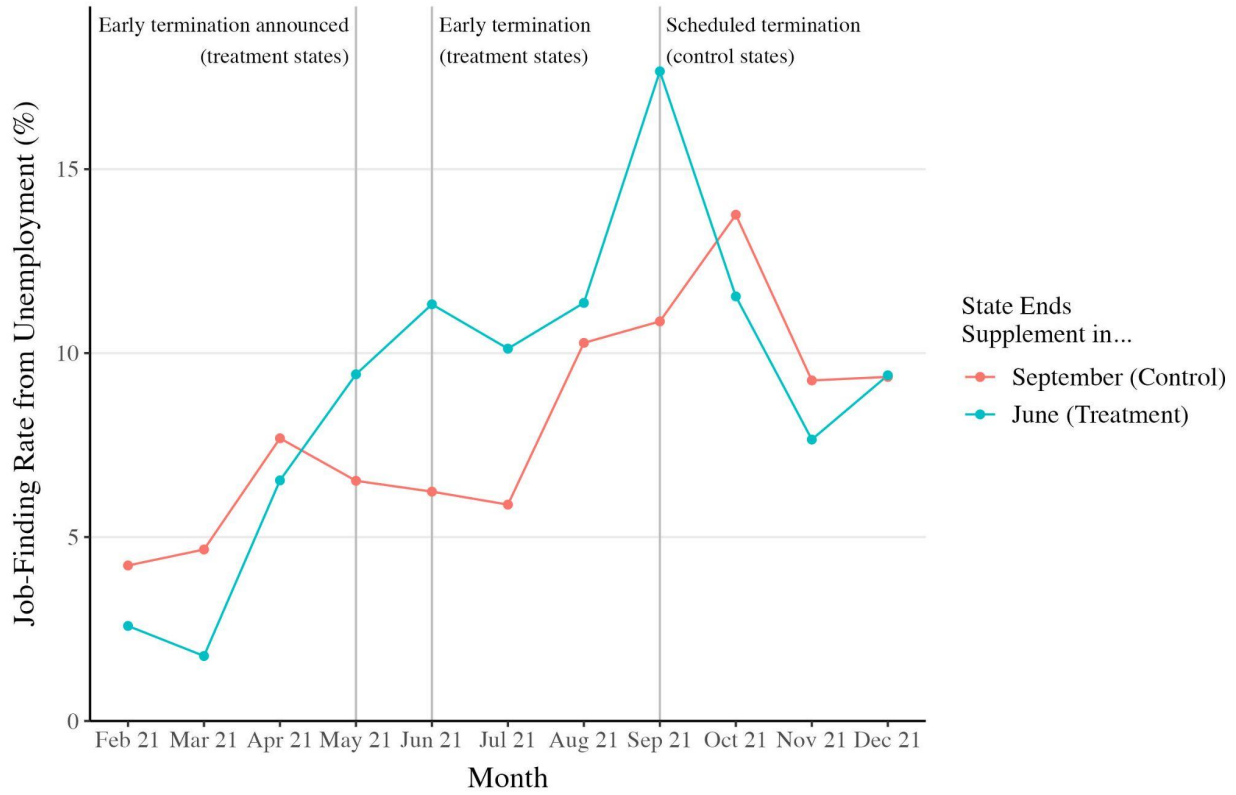
- δ_i : state-level fixed effects.
- ϵ_{it} : error term.

This strategy compares changes over time in job-finding rates between treated and untreated groups. It controls for baseline differences between early-exit and other states as well as common shocks over time. We include state-level fixed effects to control for time-invariant differences across states—such as baseline reopening policies, political attitudes, or economic structures—and month-level fixed effects to account for nationwide shocks or seasonal trends affecting all states equally. This strategy helps us isolate the policy’s impact from broader economic trends or state-specific factors.

The main effects, β_1 and β_2 , control for differences between early-exit and non-exit states (regardless of timing) and for time trends common to all states (regardless of policy status), respectively, ensuring that only the interaction term captures the unique effect of early UI termination.

The identifying assumption of our difference-in-differences design is that, absent the policy intervention, job-finding rates in early-exit and non-exit states would have followed parallel trends. Figure 3 below supports this assumption: between February and April 2021—prior to the May policy announcement—job-finding rates in both groups follow similar upward trajectories. This pre-trend similarity suggests that both treated and control states were on comparable labor market paths before the early UI termination decision, lending credibility to our use of non-exit states as a control group. Here, we did not do a staggered treatment effect analysis for the states exiting in September because we did not observe parallel trends past April. Instead, we focus on a restricted time window (February to August) to avoid policy interference: benefits in the remaining states expired in early September, potentially confounding treatment and control groups in the latter part of the year. Additionally, this period most closely reflects the window in which policy expectations were clear and divergence in benefit generosity was sharpest.

Figure 3: Monthly Job-Finding Rate by Treatment Group



Since the job-finding rate is calculated as the ratio between the number of individuals who found jobs in month t , to the unemployed population in month $t-1$, this rate can be measured starting in February of 2021.

While we see very strong effects of the early termination policy on job-finding rates, this could be attributed either to the announcement of the policy or the termination itself. In order to isolate the effects of the announcement that the policy would terminate—rather than the combined effects of announcement and termination—we used an event study model with uniform treatment timing. Assuming that variation in job-finding rates came from differences in the timing of treatment across states, we estimated the dynamic effects of the announcement on job-finding rates each month. We used the below model:

$$Y_{ik} = \alpha + \sum(\beta_k \times EventTime_{ik}) + \delta_i + \varepsilon_{ik} \quad (2)$$

where:

- Y_{ik} : the probability that an individual i finds a job from unemployment in time t .
- β_k : the estimated effect of the announcement k months from treatment May (e.g., $k = 1$ in June), for $k \in [-3, 1]$, $k \neq -1$. This coefficient represents how much the job-finding rate differed between treatment and control groups in month k relative to month $k-1$.
- $EventTime_{ik}$: a vector of dummies = 1 for an individual in a state that exited early, k months relative to treatment (except for $k = -1$).
- δ_i : state-level fixed effects.
- ε_{ik} : error term.

In this specification, there are no separate main effects for treatment or time since treatment is fully absorbed by the event-time dummies. Each β_k measures the difference in the job-finding rate between treated and control states at month k relative to one month before treatment. This lets us visualize both pre-trends and the dynamic effects of the announcement over time and answer whether the policy announcement alone had an immediate effect on labor supply. We do not include month-level fixed effects since our $EventTime_{ik}$ variable already accounts for these.

b. Effect on Poverty Levels for Low- to Middle-Income Households

To estimate the effect that early exit had on poverty levels, we use a similar difference-in-differences specification to equation (1) for the job-finding rate. Instead, here, Y_{it} is the family income-to-poverty ratio for individual i in month t , and β_3 captures either the combined effect of the announcement and early exit or early exit alone since we, similarly, run two regressions. We note that this ratio includes all income, including earned income and government transfers. State fixed effects δ_i control for unobserved, time-invariant differences across states—such as return-to-work policies and state

transfer programs—allowing us to isolate the causal impact of the policy announcement on household income relative to the poverty threshold. Month fixed effects control for common shocks over time, such as COVID trends.

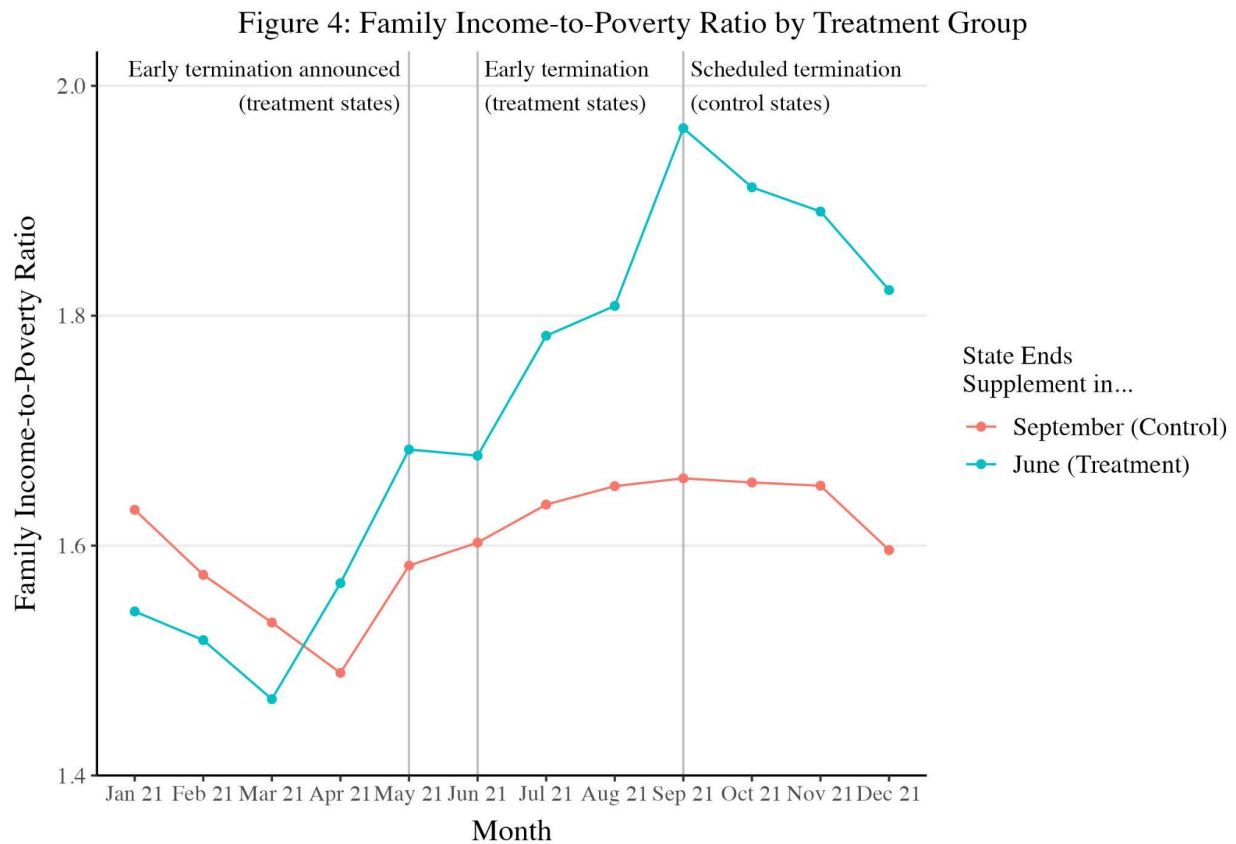


Figure 4 above shows the family income-to-poverty ratio over time for a subsample of low- to middle-income individuals, defined as those with ratios between 0.5 and 3. This range captures most of our sample and targets households most affected by unemployment benefits. The lower bound ensures the inclusion of individuals earning at least the equivalent of a full-time minimum-wage job, while the upper bound includes moderate-income families without skewing toward high-income outliers. As the figure shows, pre-treatment trends between treatment and control states are similar, supporting the parallel trends assumption. Because the ratio includes both earned and transferred income, it serves as a proxy for UI replacement rates—the extent to which benefits offset lost earnings. Ganong and Noel (2019) estimate that around three-fourths of unemployed workers received UI benefits exceeding prior wages during the

pandemic, implying replacement rates often over 100% for this group. Thus, we expected the family income-to-poverty ratio to decrease post-termination, but we actually observed the opposite effect.

VI. Results

Overall, our research yields three main findings. First, we find that the most powerful incentive to return to work came from a combination of announcing the early termination of the \$300 supplement, then implementing it shortly thereafter. This approach more than doubles the monthly job-finding rate—an effect larger than only ending the supplement. Second, unemployed workers likely responded to this announcement alone when changes were imminent. This suggests that even in the short term, workers are forward-looking and adjust their job search behavior in anticipation of policy changes. Lastly, poverty did not rise among low- and middle-income recipients when states cut the \$300 supplement.

a. Effect of Supplement Cut and Cut Announcement on Job-Finding Rates

We find that the combination of the announcement and early termination of federal UI supplements led to a statistically significant increase in job-finding rates, as Table 2 lays out. As Panel A shows, states that ended the \$300 federal UI supplement early saw a 6.13 percentage point (108%) increase in job-finding rates through August, relative to states that continued benefits ($p = 0.007$). This represents more than a doubling of the control group's pre-treatment mean, at 5.70 percentage points, and is our preferred estimate of job-finding rates given that trends between treatment and control groups start diverging in May (Figure 3). This finding is roughly consistent with estimates of the general US population from FRED survey data (2021).

Table 2: Effect of Early Exit from \$300 UI Supplement Program on Monthly Job-Finding Rates

	(1)	(2)
	Intercept	Monthly Job-Finding Rate
<i>Panel A: Difference-in-Differences Estimates</i>		
<i>Treatment: Announcement plus Early Exit</i>		
EarlyExit $\times$ post-Announcement	10.67 (5.98) [0.075]	6.13 (2.28) [0.007]
Control group pre-treatment mean: 5.70		
F-statistic's p-value: < 0.001		
R ² : 0.015		
<i>Panel B: Difference-in-Differences Estimates</i>		
<i>Treatment: Early Exit Only</i>		
EarlyExit $\times$ post-Exit	10.16 (5.99) [0.090]	4.79 (2.56) [0.062]
Control group pre-treatment mean: 5.95		
F-statistic's p-value: 0.001		
R ² : 0.013		
Overall mean of dependent variable: 7.01		
Observations: 4,022		
Number of individuals: 576		

Notes: Numbers without parentheses in columns (1) and (2) show variable coefficients. Standard errors appear in parentheses and are clustered by individual ID. P-values appear in square brackets and are also clustered by individual ID. Panel A shows the combined effect of announcing the exit in May and then exiting in June. Panel B shows the effect of only exiting in June. Value for dependent variable and control group pre-treatment mean: the probability that an unemployed individual in month $t - 1$ begins a job in month t . Control group pre-treatment mean differs from difference-in-differences intercept because the intercept includes fixed effects. Individual observations in our sample are weighted per SIPP specifications to improve representativeness. Both panels use fixed effects for the month and the state. Value for R² is adjusted-R².

Panel B shows the effect of actually terminating the UI benefit alone on job-finding rates. The estimated effect through August 2021 is 4.79 percentage points (81%), statistically significant at the 10% level. As our results show, the combination of early announcements plus implementation of UI cuts had stronger and more immediate effects on job-finding than the actual terminations themselves.

Table 3 below shows the dynamic effects of the May announcement of early termination. Each row represents the difference in job-finding rates between treatment and control groups, relative to the difference between both in April—the month before treatment. Our data shows a constant trend in February and March leading up to the announcement of early termination, which lends credence to our parallel trends assumption and allows us to perform a difference-in-difference analysis. Crucially, we find

an immediate increase in the job-finding rate of 3.87 percentage points (68%) following the announcement in May, which is over half of the control group's pre-treatment mean of 5.70%. Given that terminations occurred in late June, we also include results for that month. We see the effect on job-finding rates through June rising to a 5.93 percentage-point (104%) increase relative to before the announcement. Although our results are not statistically significant and make it difficult to isolate the effect of the announcement, this might be because of the small number of observations for each month. However, they are directionally consistent with our difference-in-difference results, which leads us to conclude that it is likely that workers had some reaction to the announcement itself.

Table 3: Dynamic Effect of Announcing Early Exit from \$300 UI Supplement Program on Monthly Job-Finding Rates

	(1) Monthly Job- Finding Rate Difference (Treatment vs. Control)	Standard Error	p-value
<i>Event-Study Estimates</i>			
<i>Treatment: Announcement of Early Termination</i>			
February 2021 (3 months pre-treatment)	-4.05	(3.09)	[0.203]
March 2021 (2 months pre-treatment)	-4.83	(3.25)	[0.150]
May 2021 (treatment month)	3.87	(3.04)	[0.216]
June 2021 (1 month post-treatment)	5.93	(4.66)	[0.215]
Control group pre-treatment mean: 5.70			
R ² : 0.015			
Overall dependent variable mean: 7.01			
Observations: 2,874			
Number of individuals: 576			

Notes: Reference period: April 2021 (month before policy announcement in May). Numbers without parentheses in column (1) show the difference in monthly job-finding rates for states that announced an early exit in May (treatment) versus those that kept the UI supplement (control), compared to their difference in April 2021. Standard errors appear in parentheses and are clustered by state. P-values appear in square brackets and are also clustered by state. We restrict the sample until June to capture the effect of announcing the exit in May only. Value for control group pre-treatment mean and overall dependent variable mean: the probability that an unemployed individual in month $t - 1$ begins a job in month t . Individual observations in our sample are weighted per SIPP specifications to improve representativeness. Model uses fixed effects for the state only, since month fixed effects are already captured by the event-time indicator. Value for R² is adjusted-R².

b. Effect on Poverty Levels for Low- to Middle-Income Households

Finally, we analyze the effects of early policy termination on poverty. While we expected family income-to-poverty ratios to decrease post-termination—meaning families would get closer to the poverty line because of lost benefit income—we actually found the opposite effect.

Table 4: Effect of Early Exit from \$300 UI Supplement Program on Family Income-to-Poverty Ratio

	(1) Intercept	(2) Family Income-to- Poverty Ratio
<i>Panel A: Difference-in-Differences Estimates</i>		
<i>Treatment: Announcement plus Early Exit</i>		
EarlyExit × post-Announcement	2.33 (0.02) [<0.001]	0.13 (0.06) [0.020]
Control group pre-treatment mean: 1.56		
F-statistic's p-value: < 0.001		
R ² : 0.082		
<i>Panel B: Difference-in-Differences Estimates</i>		
<i>Treatment: Early Exit Only</i>		
EarlyExit × post-Exit	2.33 (0.02) [<0.001]	0.13 (0.06) [0.038]
Control group pre-treatment mean: 1.56		
F-statistic's p-value: < 0.001		
R ² : 0.082		
Overall mean of dependent variable: 1.60		
Observations: 1,860		
Number of individuals: 347		

Notes: Numbers without parentheses in columns (1) and (2) show variable coefficients. Standard errors appear in parentheses and are clustered by state. P-values appear in square brackets and are also clustered by state. Panel A shows the combined effect of announcing the exit in May and then exiting in June. Panel B shows the effect of only exiting in June. Control group pre-treatment mean differs from difference-in-differences intercept because the intercept includes fixed effects. Individual observations in our sample are weighted per SIPP specifications to improve representativeness. Both panels use fixed effects for the month and the state. Value for R² is adjusted-R².

Per Table 4, in states that announced and exited UI early, the family income-to-poverty ratio for low-middle income households increased by 0.13 points (8.33%), relative to states that continued benefits. This effect is statistically significant at the 5% level, suggesting that early UI termination led to a modest increase in reported family income relative to poverty thresholds. We see the exact same effect in both our regressions for the combined effect of the policy announcement and implementation as well as implementation alone. This finding suggests that the rise in income levels likely took place after the termination of the benefit supplement only, not after announcement of the policy change.

Since family income is calculated through both earned wages and transfers, these results suggest that UI beneficiaries actually earn more in wages at their new job than through unemployment benefits. These results present a win-win situation: higher incomes for families and lower government spending on UI. What might explain this counterintuitive conclusion is that the benefit reduction occurred in a tight labor market—where jobs were plentiful—allowing unemployed workers to return to higher-paying jobs.

VII. Policy Recommendation

Based on our findings, we recommend a simple strategy: during periods of high job vacancies, policymakers should scale back unemployment benefit supplements—and those reductions should be announced shortly in advance. More broadly, policymakers aiming to drive the strongest behavioral response to UI policy should prioritize changes that are announced early, communicated clearly, and implemented quickly.

We base these conclusions on three key findings. First, the most powerful incentive to return to work comes from a combination of announcing a benefit reduction, then implementing it shortly after. This approach more than doubles the monthly job-finding rate—an effect larger than only ending the supplement. Second, it is likely—although not certain—that unemployed workers respond to announcements alone when changes are imminent. This suggests that even in the short-term, workers are forward-looking and adjust their job search behavior in anticipation of fast-approximating policy changes. Third, poverty does not rise among low- and middle-income recipients when the supplement ends. This is critical: our strongest evidence is that scaling back benefit supplements during tight labor markets encourages reemployment without harming the most financially vulnerable. Clear, advance notice and careful monitoring of labor market tightness can prompt faster reentry into the labor force without requiring abrupt income loss.

Our study is limited by two major factors. First, our relatively small sample size of 576 individuals may reduce the precision of estimates and limit generalizability, especially when analyzing

subgroups. Although the Survey of Income and Program Participation (SIPP) is nationally representative, our sample may overrepresent individuals with weaker labor market attachments which could bias job-finding rates downwards. Second, we cannot fully disentangle the effects of concurrent economic trends, such as rising job vacancies or labor market elasticity, despite using fixed effects. Furthermore, while we focus on job-finding and income-to-poverty ratios, we cannot directly measure job quality or matching, long-term employment stability, or the psychological and financial stress caused by early UI termination. We urge future researchers to repeat this study with a larger sample to observe some of these related metrics.

In short: reducing UI generosity in tight labor markets—when jobs are plentiful—can help accelerate reemployment without raising poverty. And even outside of pandemic conditions, a phased, well-communicated approach to UI changes, combined with close monitoring of labor market tightness, generates the strongest immediate results.

VIII. Conclusion

In designing unemployment benefits and their duration, our findings indicate that policymakers should scale back benefit supplements during times of tight labor markets and should announce those reductions shortly in advance. We use a difference-in-differences and an event study model to investigate the effect of announcements of and cuts in supplemental UI benefits on the job-finding rate and poverty levels of unemployed persons who received benefits in 2021. We examine variation in states' termination of a federal \$300 per-week supplement to existing UI benefits. We find that the announcement and quick, early termination of supplemental UI benefits had a larger and more immediate effect on job-finding rates than the withdrawal of benefits alone (108%), more than doubling the baseline rate through August 2021. We find evidence, albeit inconclusive, that unemployed workers respond to policy announcements on their own when implementation is imminent. We find stronger evidence that the termination of UI supplements did not raise poverty levels for low- and middle-income individuals. As policymakers consider adjustments to unemployment insurance in future downturns, our results underscore the

importance of timing supplemental benefit cuts during a labor market conducive to reemployment and wage growth. More broadly, integrating clear notice periods and a study of job market tightness into UI policy design could help achieve both fiscal discipline and labor market responsiveness, without compromising economic security for unemployed workers.

IX. Works Cited

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