Early Withdrawal of Pandemic Unemployment Insurance: Effects on Earnings, Employment and Consumption [*]

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Abstract

In June 2021, 22 states ended all supplemental pandemic unemployment insurance (UI) benefits, eliminating benefits entirely for over 2 million workers and reducing benefits by $300 per week for over 1 million workers. Using anonymous bank transaction data and a difference-in-differences research design, we measure the effect of withdrawing pandemic UI on the financial and employment trajectories of unemployed workers in states that withdrew benefits, compared to workers with the same unemployment duration in states that retained these benefits. In our data through August 6, we find that ending pandemic UI increased employment by 4.4 percentage points while reducing UI recipiency by 35 percentage points among workers who were unemployed and receiving UI at the end of April 2021. Through the first week of August, average UI benefits for these workers fell by $278 per week and earnings rose by $14 per week, offsetting only 5% of the loss in income. Spending fell by $145 per week, as the loss of benefits led to a large immediate decline in consumption.

1 Introduction

The Federal Pandemic Unemployment Compensation (FPUC), which passed under the CARES Act, originally added a $600 supplement to weekly UI benefits in March 2020. This supplement lapsed in July, 2020, and was partially reinstated at $300 in January 2021. Most recently, this $300 supplement was withdrawn by 26 states. Importantly, 21 of these states additionally ended the Pandemic Unemployment Assistance (PUA)–which extended benefits to uncovered workers such as the self employed–and the Pandemic Emergency Unemployment Compensation (PEUC)–which extended coverage to those who have exhausted regular state benefits. In this paper, we use new bank transaction-level data to study the effect of early withdrawal of pandemic UI benefits on UI recipiency, earnings from employment, and consumption. Our de-identified transaction-level data comes from Earnin, a financial services company that provides users with products such as access to their income before their payday. These users are predominantly low-income workers with low access to credit.

Of the 26 states announcing a withdrawal, 22 withdrew in June 2021. We focus on the June withdrawal states in this brief, allowing sufficient time to detect behavioral changes. Furthermore, we are able to identify UI benefit transactions in 19 of these "Withdrawal" states. We compare these 19 states to the 23 "Retain" states that decided to continue the pandemic UI programs until they expire in early September at the federal level, and where we can observe UI benefits in our data, for a total of 42 analysis states. 16 of the June withdrawal states in our sample additionally ended the PUA and the PEUC, which accounted for two thirds of all continuing claims in these states just prior to the withdrawals (based on data from the Department of Labor's weekly claims reports).

We follow a group of individuals who were unemployed and receiving UI in the last week of April—just before the withdrawal announcements were made. We find a sharp drop in the share of individuals receiving UI in Withdrawal states relative to Retain states by the end of July (amounting to 35 percentage points). We find that unemployed individuals in Retain states saw a 4.4 percentage point (20 percent) increase in the probability of having found a job through the first week of August. Also, through the first week of August there was a roughly $14 gain in weekly

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earnings, which was small compared to the $278 loss in weekly benefits, and was associated with a $145 (20 percent) reduction in weekly spending. In addition, our evidence suggests that most of the employment gains were due to the mechanical exhaustion of UI benefits, as opposed to through greater incentives for job finding from the loss of $300/week supplement.

2   Data and Sample Construction

Our data comes from Earnin, a financial services company that provides earned wage access services when users connect their bank accounts. Through this connection, Earnin maintains a database containing user tags with information about each user, transactions-level data, balance data, and observed earnings data. Each of these datasets contains the user tags, and we use these tags to construct "proxy IDs"; this process is explained further in the Data Appendix. For simplicity, we will call each proxy ID unit an "individual" or a "user" below.

We first begin with all individuals living in our analysis states in the last week of April who had a transaction between December 18, 2020 and August 6, 2021. Next, we limit our attention to individuals who, in the last week of April (immediately prior to the withdrawal announcements), were classified as receiving UI benefits (i.e., accounts had a UI deposit in the past 3 weeks) and were not at a job in the last week of April (i.e., accounts without a paycheck deposit in the past 3 weeks). We follow this cohort of 18,648 individuals separately by Withdrawal and Retain states over the following 12 weeks and document how their outcomes evolved over time.

We classify individuals as receiving UI in a given week as follows: A UI spell starts at time t when the first UI payment is deposited in the bank account. The spell continues until three weeks pass without any UI payments. In the case where the last UI payment is deposited in week $t + k$, and no additional UI payment is received in dates $t + k + 1$ through $t + k + 3$, we define the spell to have ended at date $t + k$.

We classify individuals as being employed using paycheck deposit information. In particular, an employment spell starts at time t when the first paycheck is deposited at date t. Similarly, it ends when three weeks go by without a paycheck deposit. This three week window is appropriate given that very few (less than 5 percent) of workers in our sample receive monthly paychecks.

Our estimation sample is a balanced weekly panel stretching from the first week of January

through the first week of August, and we focus on five key outcomes. These include: 1) whether an individual is classified as being a UI recipient that week; 2) whether an individual is classified as employed that week; 3) weekly UI deposit amount; 4) weekly earnings deposit amounts; and 5) weekly spending amounts.

The details on our data construction, including our methods for detecting UI payments and paychecks, as well as our construction of our spending measure are provided in the Data Appendix.

3 Research Design: Estimating the Effect of Withdrawals

3.1 Re-weighting the "Retain" sample

Our research design compares the 19 Withdrawal states to 23 Retain states using a difference-in-differences methodology. The key assumption is that the Withdrawal states' outcomes would evolve in parallel to the Retain states' outcomes absent the policy change. A major threat to identification when it comes to analyzing the behavior of unemployed individuals is that these two groups of states may have very different shares of short and long term unemployed. This reflects, among other things, the fact that the Retain states tended to be Democratic leaning, and instituted more restrictive pandemic mitigation measures in 2020—including restrictions in the hospitality sector—which was likely to have built up a larger set of long-term unemployed in the Retain states.

As the following figure shows, the UI spell durations at the end of April were, indeed, longer in the Retain states than in the Withdrawal states. 57.8 percent of the spells originated in 2020 in the Retain states, while the analogous share in Withdrawal states was 52.8 percent.

Figure 1. Histogram of Starting week of UI spells in April by State Withdrawal Status
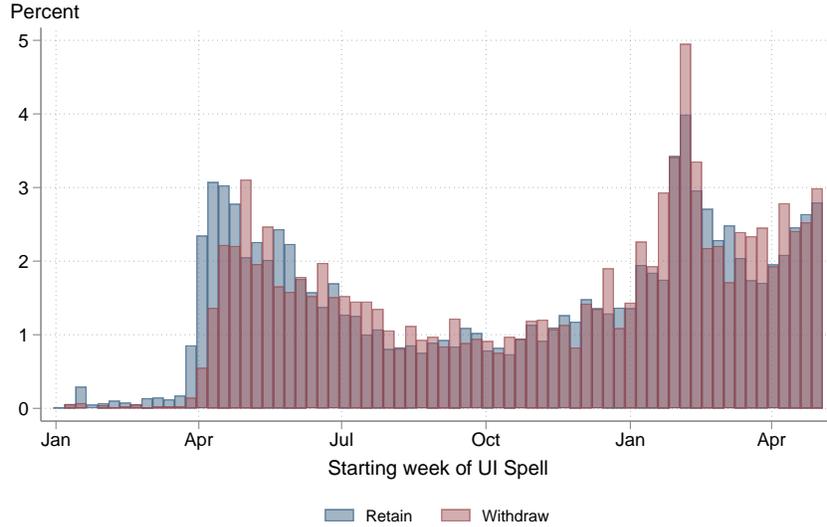
Notes: The above figure plots the histograms of the starting week for each users' unemployment insurance recipiency spell that runs through the end of April by retain and withdraw states. The sample is restricted to those 18,648 Earnin users whom we track as receiving UI benefits and no earnings in the final week of April. In our sample, the Retain cohort contains 23 states and the Withdraw cohort contains 19. Within this sample, 57.8 percent of users in Retain states started this spell in 2020, while the analogous share in Withdraw states is 52.8 percent.

Given the well-known duration dependence of the job finding rate, we re-weight our Retain sample to match the distribution of duration in the Withdrawal sample. In particular, we use inverse-propensity-weighting, where we regress a *Withdrawal* indicator variable on deciles of spell start date. Then using the predicted probabilities $p(S)$, we assign the observations in the Retain sample with spell duration S the weight of $\frac{p(S)}{1-p(S)}$. All of our analyses use this weighted sample.

To address lingering concerns that re-weighting our sample is not enough to ensure parallel trends, we show estimates using a "placebo-in-time" approach after our main results. These robustness checks provide strong evidence against pre-existing trends prior to the withdrawal announcements.

3.2 Regression Specifications

We estimate the following regressions.

$$y_{it} = \sum_{k \neq April 30} \beta_t (I_{t=k} \times Withdraw_i) + \alpha_i + \gamma_t + \epsilon_{it}$$

Here y_{it} is our outcome of interest, β_t is the estimated treatment effect in week t, and α_i and γ_t are person and week fixed effects, respectively. We cluster standard errors at the state level, the level of treatment. The weekly sample extends from the first week of January 2021 through the first week of August. We exclude the last week prior to announcement (i.e., last week of April) whose coefficient is normalized to zero.

Note that the coefficients β_t trace out the difference-in-differences estimates of the dynamic response to Withdrawal over time. We plot these coefficients and associated confidence intervals for our key outcomes. In some figures, we subtract the β_t estimates and confidence intervals from the Withdrawal time series to show the point estimate and confidence interval around the counterfactual and the actual Withdrawal outcomes over time.

Finally, we are also interested in understanding the *cumulative* impact of Withdrawal on outcomes. For this calculation we sum the weekly coefficients in the weeks after the first implementation of the policy begins on June 12: $\sum_{t > June 12} \beta_t$.

4 Main Findings

Figure 1 shows that the early withdrawals led to a substantial reduction in UI recipiency. The timing of these changes line up precisely with the timing of when the states withdrew from pandemic UI (i.e., June 12, 19, or 26). The sharp drops occurring in the week of withdrawal are consistent with benefit exhaustion from these policies. For our sample of UI recipients at the end of April, there was a 35 percentage point decline in UI recipiency by the end of July in the Withdrawal states as compared to Retain states. This is a 46% decline relative to the share of workers who would still be receiving unemployment benefits in the last week of July absent the policy change.

Figure 2. Mean Share Receiving UI Benefits by June Withdrawal State Cohorts

Share Receiving UI Benefits

Unemployed insured end of April Users:
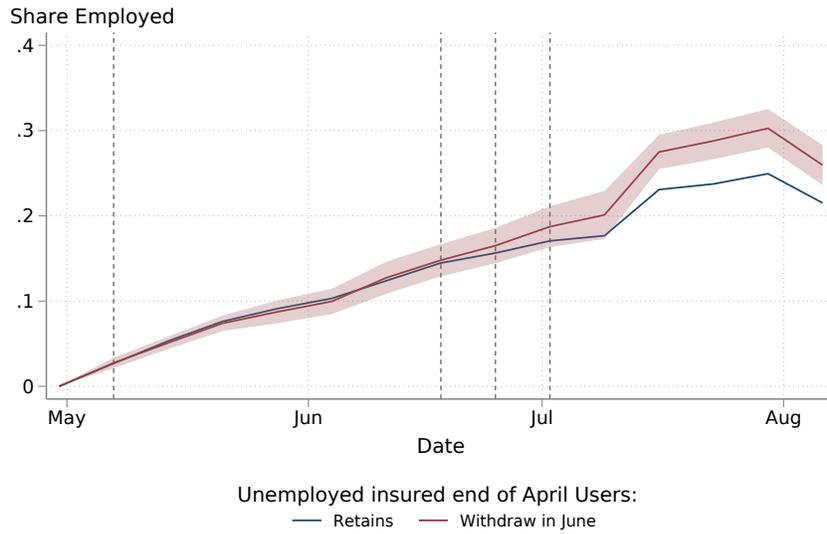Retains — Ends June 12 — Ends June 19 — Ends June 26
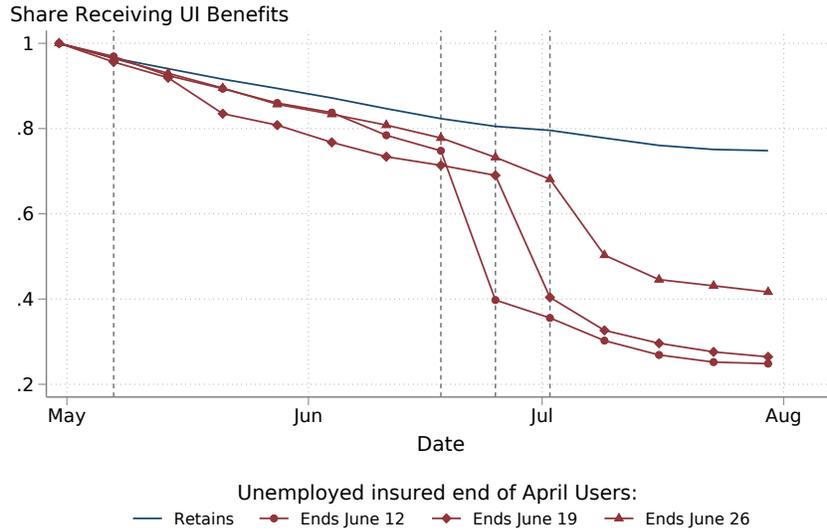
Notes: The above figure plots the mean share of users who are receiving UI benefits in a given week. The sample is restricted to those 18,648 Earnin users whom we track as receiving UI benefits and no earnings in the final week of April, resulting in the first observation being mechanically 1 for all states. In our sample, the Retain cohort contains 23 states, June 12 contains 4 states, June 19 contains 7 states, and June 26 contains 8.

What happened to the 46% of the cohort who lost unemployment insurance? For this cohort, we see very little difference in employment probabilities between January 2021 and early June 2021 (Figures 2 and 3). However, by the first week of August, we find a modest, but precisely measured increase in the probability of job finding in the Withdrawal states. While around 21.5% of the cohort had jobs by the end of July in Retain states, around 24.9% of the cohort did so in Withdrawal states, a 4.4 percentage point (20 percent) increase.

Figure 4 highlights that the job gains (4.4 percentage point increase) were substantially smaller compared to the loss in UI coverage (35 percentage points). Roughly 1 out of 8 (or around 13 percent) of those who lost UI coverage from the policy change had jobs in the first week of August. At the same time, our estimates of job gains are precise enough to rule out effect sizes smaller than 2.1 percentage point, or larger than 6.8 percentage point at the 95 percent confidence level.

The employment boost in July following the exhaustion of benefits is consistent with findings in Ganong and Noel (2019) who find a temporary, 38 percent, increase in the monthly job finding rate following benefit exhaustion in the pre-pandemic period.

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Figure 3. Mean Share Employed by June UI Withdrawal

Notes: The above figure plots the mean share of users who are employed in a given week. The sample is restricted to those 18,648 Earnin users whom we track as receiving UI benefits and no earnings in the final week of April, resulting in the first observation being mechanically 0 for all states. In our sample, the Retain cohort contains 23 states and June Withdrawal contains 19.

Figure 4. Difference-in-differences Estimates: Effect of June UI Withdrawal on Share Receiving UI, and Share Employed
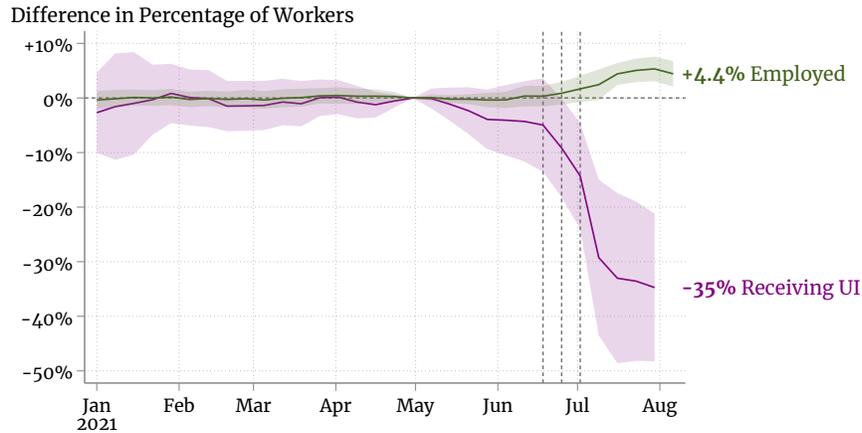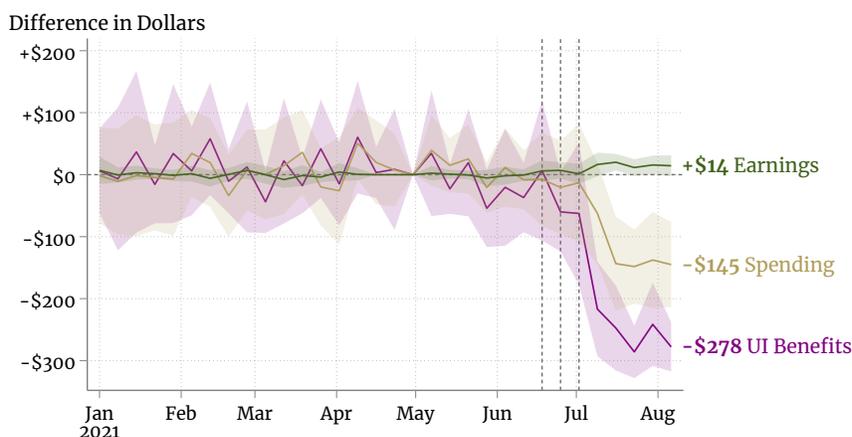

Notes: The above figure plots the difference in (1) mean share of users who are receiving UI in a given week, and (2) mean share of users who are employed in a given week between June withdrawal and Retain states. Values for the last week of April are normalized to zero. The sample is restricted to those 18,648 Earnin users whom we track as receiving UI benefits and no earnings in the final week of April. In our sample, the Retain cohort contains 23 states and June Withdrawal contains 19.

How did the policy affect income and spending? As Figure 5 shows, UI benefits fell by \$278/week

by early August. We see a small (but precisely estimated) rise in earnings of \$14/week, making up around 5 percent of the shortfall from benefit decline. The reduced income was accompanied by a \$145/week (20 percent) fall in spending. This spending fall is larger than the 12 percent drop following benefit exhaustion found in Ganong and Noel (2019) using pre-pandemic data. However, this is consistent with having higher replacement rate in the current period, as Ganong and Noel (2019) found the drops were larger for part of their sample with higher replacement rates.
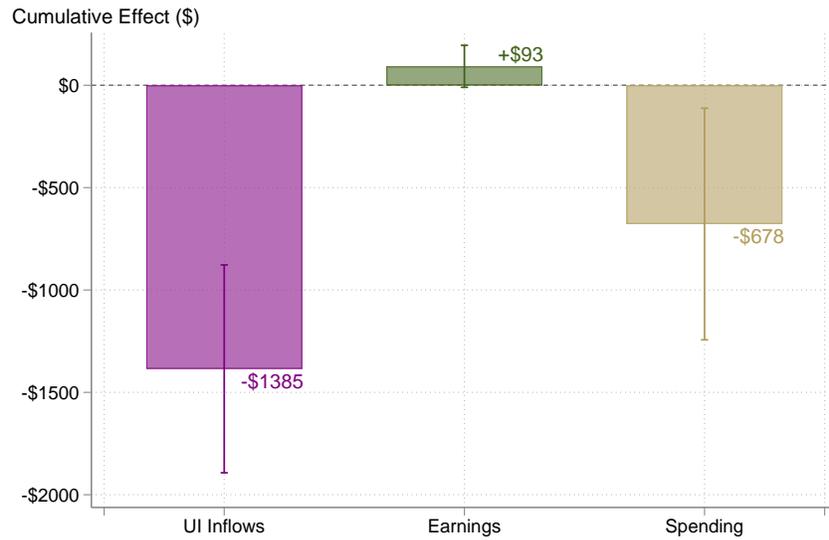
Figure 5. Difference-in-differences Estimates: Effect of June UI Withdrawal on UI Inflows, Earnings, and Spending Levels

Notes: The above figure plots the difference in means between June Withdrawal and Retain states for three separate dollar measures: UI Inflows, Spening, and Earnings. The sample is restricted to those 18,648 Earnin users whom we track as receiving UI benefits and no earnings in the final week of April. Values for the last week of April are normalized to zero. In our sample, the Retain cohort contains 23 states and June Withdrawal contains 19.

We can also aggregate the earnings gain, UI transfer losses, and spending reduction after the policy changes occurred starting mid June. Aggregated over the eight weeks, we find that for this cohort, UI transfers fell by \$1,385, while earnings rose by \$93, and total consumption dropped by \$678. The implied marginal propensity to consume out of net income was 0.52.

Figure 6. The Cumulative Effect of June UI Withdrawal on UI Inflow, Earnings, and Spending

Notes: The above figure plots the cumulative difference in means between June Withdrawal and Retain states starting June 18, 2021 for three separate dollar measures: UI Inflows, Earnings, and Spending. The sample is restricted to those 18,648 Earnin users whom we track as receiving UI benefits and no earnings in the final week of April. Values for the last week of April are normalized to zero. In our sample, the Retain cohort contains 23 states and June Withdrawal contains 19.

5 Separating the Impact of Benefit Exhaustion from the $300 Supplement

The withdrawals were a bundle of policy changes that both hastened benefit exhaustion for around 2/3 of the UI recipients in these states and cut $300 in weekly payments to recipients that continued to receive UI payments (typically through regular state benefits). Benefit exhaustion mechanically ends UI spells and may lead some of those individuals to transition to employment. There are three distinct ways in which the policy changes affect employment. First, around 2/3 of UI beneficiaries saw their benefits end due to the expiration of PEUC and PUA; this exhaustion of benefits may make them look for jobs, either prior to or following the expiration. Second, those who are on state benefits that do not expire following the withdrawal see a loss of $300/week in benefits; this reduced replacement rate may incentivize individuals to get a job faster, even if they continue to receive some benefits. Third, this latter group also now face a lower potential benefit duration (PBD) which might hasten how quickly they take a job. The latter two effects are based on incentives (through both the benefit level and PBD channels) while the former group's actions are based on

the mechanical effect of benefit exhaustion.

To assess how much of the job creation was through the mechanical exhaustion channel, as opposed to the disincentive effect of a high replacement rate (or lower PBD), we separately consider individuals who lost their jobs in more recent months (which allow them to continue to receive regular state benefits with a high probability). Specifically, we divide our sample into those who began the end-of-April UI spell who lost their jobs (1) in February 2021 or earlier and (2) between March and April 2021. For example, those who began their UI spells in the first week of March would quality for regular state benefits at least through late August (in most states); these individuals would not be subject to a mechanical benefit exhaustion effect when benefits expired in June. Still, this group faces more than the loss of $300, since the policy also reduces their PBD by ending PEUC. In this sense, we see the impact on this group as an upper bound for the impact of the loss of the $300 supplement. To get a further sense of how the loss of the $300 supplement may be affect job finding rates, we additionally consider a separate sample of individuals who lost their job in May. For this group, the expected date of benefit exhaustion is around November in *both* Withdrawal and Retain states (since the pandemic benefits are slated to expire at the federal level on September 6, 2021). Therefore, for this sample there are no differences in PBD or benefit exhaustion across the two groups of states, allowing a cleaner assessment of the impact of the $300 supplement.

Because the baseline job finding probabilities for these groups in the Retain states are so different (0.205 for the February-and-prior group, 0.349 for the March-April group, and 0.498 for the May group), we calculate the percentage change in job finding rate from treatment (and not the percent point change). Specifically, we estimate

$$\frac{b_{Aug6}}{Prob(Employed_{Aug6}|Retain=1)}$$

separately for each of these three groups.

As we show in Figure 7, job finding rates rose by 23 percent by August 6 for the group that had lost jobs prior to March 2021 and were subject to full benefit exhaustion. The effects from July 16 through August 6 are all distinguishable from zero. In contrast, the impact on the second

group (those losing jobs in March or April) is only around a half of the size of the rst. While the con dence interval in this group contains the point estimates in the rst group (and vice versa), this provides some evidence that much of the impact was through the mechanical bene t exhaustion e ect rather than the incentive e ects of the $300 supplement. The even lower estimate of job nding e ect in the May group supports this explanation. The estimates for this supplementary sample are close to zero, not signi cant, and the associated con dence intervals do not include the point estimates from the February and earlier group. These ndings suggest that the impact of removing the $300/week supplement per se played a small role in explaining the job gains, which seem to have been driven largely by bene t exhaustion.

Figure 7. Percentage Change in the Job Finding Rate from June Withdrawal: Heterogeneity by the Month of Job Loss

Notes: The above gure plots the estimated treatment e ect of withdrawing from federal UI bene ts on employment as a percentage di erence from our baseline, the employment rate in states that retained additional federal UI bene ts. To do this, we divide our sample by month of job loss, requiring a job loss to be followed by UI receipt within eight weeks of a job loss. We rst take a subsample of our End-of-April Unemployed sample and split it into two groups: those who lost their jobs in February 2021 or earlier and those who lost their jobs between March and April of 2021. For additional context, we supplement our primary sample with those who lost their jobs in May 2021, regardless of their end-of-April employment states. We present estimates for four weeks for each cohort; from left to right, these are the weeks ending July 16, July 23, July, 30, and August 6. The sample sizes for the February, March/April, and May groups are 9,459, 3,156, and 2,597 and the the baseline estimates are .205, .349, and .498, respectively.

6 Assessment of Pre-existing Trends using Placebo Policies

Our analysis is based on the assumption that once we adjust for the distribution of UI spell duration (as of end of April), the key outcomes for our sample of UI recipients (job nding rate, earnings at new jobs, spending) would have followed parallel trends in the absence of the policy changes. It is reassuring that outcomes for our re-weighted sample appear to follow similar path during January to April of 2021. However, especially for job nding, this look back does not settle the issue of pre-existing trends for two reasons. First, the re-weighting of our Retain sample by UI spell length tends to mechanically ensure that the share of individuals who were working in January will be broadly similar in the Retain and Withdrawal states. A second factor is that the look back is about transition out of employment into UI; however, the key outcome we are interested in is the transition out of UI into employment. To assess whether job nding rates were moving in parallel requires us to devise a di erent test.

We take a \placebo-in-time" approach, where we assign placebo announcement dates in every week in 2021 prior to the true announcement that is consistent with a \clean" 14 week post-announcement period (i.e., so that the \post" period does not contain the third week of June when the rst cohort of states withdrew expanded bene ts). Additionally, we require a 3-week period form the start of 2021 to assess whether someone had been working and/or were on UI. This allows us to use the weeks ending January 22 through March 12 as placebo announcement dates. For each of these 8 placebo-treatment dates, we re-do our analysis just as in the primary analysis, but pretending that the announcement and the subsequent implementation of the policies happened on this ctitious date. Then we look at the outcomes of the 14-week post-announcement period, also just as in our primary analysis. We plot the 8-, 10-, 12-, and 14- week placebo-treatment e ects for four[1] of our ve key outcomes by date alongside the actual treatment e ect for the true announcement date of rst week of May. For the March 12 placebo, we omit the 14-week e ect as this is the third week of June. If, for example, it were easier to nd jobs in Retain states as compared to Withdrawal states in 2021, then|even absent any policy change|we would see a positive placebo-treatment e ect on jobs. The same argument holds for other outcomes.

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[1] We omit the placebo plot for Share Receiving UI to save space, but it is available upon request. As the Sharing Receiving UI is a function of UI in ow levels, the placebo tests look similar.

The figure below shows the estimates for the four key outcomes (UI inflows, employment, earnings, and spending). We find that all of the 124 placebo-treatment effects are close to zero and not statistically significant, consistent with a lack of a pre-existing trends. For UI inflows, employment, and spending, the actual estimates are much larger in magnitude and statistically distinguishable from zero. For earnings, the the actual earnings estimate is still larger than the placebo-treatment effects, but only modestly so; moreover, the actual treatment effect is marginally statistically significant, consistent with a relatively small impact of the policy change on earnings.

Figure 8. Assessing Pre-existing Trends: Effects of Actual versus Placebo Treatments

Notes: The figures above present placebo estimates on the effect of federal benefit withdrawal on state share employed, share insured, average earnings, UI inflows, and spending in the 8th, 10th, 12th, and 14th weeks after placebo announcements in each week from January 22nd to March 12th and the actual week before the announcement, April 30. Placebo estimates with 95% confidence intervals are shown in yellow, and the true estimate with 95% confidence intervals are shown in blue.

In Appendix B, we provide additional placebo exercises for the cumulative estimates of benefit loss, earnings gains, and spending reductions, all of which also are consistent with lack of pre-existing trends.

7 Discussion

What do these estimates imply about the aggregate employment and spending impacts of early with-drawal policies? Note, there are several reasons why we might expect our estimates to overstate any such macro estimates. First, our sample is composed entirely of low-income and credit-constrained workers who are likely to respond more strongly to a loss of benefits than higher-income workers affected by the same policy. Second, our job finding estimates miss some of the "congestion" effect created by increased job search. This arises from the fact that people who lost their UI benefits are applying to the same job postings as others in the labor market, and some of those other people (e.g. teenagers) are now passed over for jobs they would have taken. Third, our estimates do not directly account for aggregate demand effects of lost spending. Reduced spending by people who lost pandemic unemployment benefits fell substantially, which will lower business revenue and affect job destruction and creation (outside of jobs in our cohort).

With those caveats and if the only impact of the policy change were through the labor supply of the previously unemployed, extrapolating from our job finding estimates (along with the 2.9 million individuals receiving UI in end of April) suggests an additional 35 thousand additional jobs were created in June and 135 thousand in July, but 25 thousand fewer (so far) in August. This implies that the unemployment rate in Withdrawal states would have been 4.8 percent in the absence of the change, as opposed to 4.5 percent in reality. Note, however, that since the federal pandemic unemployment insurance programs are slated to expire on September 6, 2021, these approximately 145 thousand additional jobs (adding June, July and August together) would have likely been created a few months later without the early withdrawal.

Because our sample tends to have lower income and have more limited access to credit than the general population, these policy changes may elicit larger behavioral responses when it comes to spending.[2] If we do assume that other individuals losing UI behave similarly, we estimate that the early withdrawal translated to an approximately $4 billion reduction in federal UI payments to the Withdrawal states aggregated over June through early August. This was offset by an increase of only $270 million in earnings and accompanied by a fall in consumer spending of $2 billion. This

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[2]Our estimated MPC of 0.52 is slightly larger than the estimate of 0.42 in Ganong et al. (2021a) for the $300 supplement; however, the policy treatments we study are also different since ours involves benefit exhaustion.

reduction in spending may have had an adverse e ect on employment through increased layo s or reduced hiring of non-recipients that is not captured in our analysis.

What are the implications for the federal expiration on September 6? Extrapolating the e ect of expiration to Remain states involves additional sources of uncertainty beyond the caveats noted above, as the labor market conditions may di er between these groups of states. For this reason, the calculations below are best thought of illustrative. There are currently 12 million continuing UI claims, the majority of whom are on PEUC and PUA which are set to expire. This compares to around 3 million claims in the case of June withdrawals. This means the impact may be roughly four times as large as from the June withdrawals. If we were to extrapolate from our estimates with all of the caveats above, it would suggest that the federal expiration would likely lead to an additional half a million new jobs spread over September and October, while most of the approximately 4 million recipients losing UI due to the expiration would take much longer to nd jobs. As a result, we could see around $8 billion in reduced spending during September and October. The spending losses are likely to continue further as additional workers take time to enter the workforce, although we are not able to assess those magnitudes based on the evidence in this paper. As before, the loss in spending may limit any macro job gains from the increase in labor supply.

A Appendix: Data Construction

A.1 Datasets

The database of anonymized data we receive from Earnin includes separate datasets containing bank transactions, daily checking and savings account balances, transactions classified as earnings, and user information in the form of \tags". None of the data we receive contains personally identifying information, and all data is stored and processed on secure servers.

The user tags are weekly datasets at the level of de-identified individuals that contain both time-variant (earnings in the past 14 days, work ZIP code, etc.) and time-invariant (Earnin sign-up date, January 2020 earnings, etc.) variables for each Earnin user. The other datasets contain these tags in addition to their respective banking data.

The full transactions data cover January 1, 2020 to August 6, 2021 and include transaction-specific information on the amount of each transaction, a memo describing the source or destination of a transaction, and a categorization of the type of transaction from Plaid, a third party that connects users' bank accounts to Earnin's database.

The bank balance data also cover January 1, 2020 to August 6, 2021. Balance data include the number and total balance of checking, savings, and \other" bank accounts connected to Earnin.

The earnings transactions data is a subset of the transactions data covering the earnings inflows of each of the jobs reported to Earnin by the user, from January 1, 2020 to August 6, 2021. These data include the date of payment, posted date of the transaction, the amount of earnings, and whether those earnings are from unemployment benefits. These data are a direct subset of the transaction data conditional on the memo satisfying a regex search, summed to the user-job-week level.

A.2 Creating Proxy User IDs Using Tags

While the datasets we receive do not contain user identifiers, each dataset does contain Earnin's \tags" that allow us to categorize users across datasets. We use these tags to construct panels

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based on the sign-up date, gender estimated by rst name, and con dence in that estimate|which are included in each dataset. Using these tags, we construct \proxy IDs" and measure the panel outcomes for each proxy ID in each dataset. For simplicity, we sometimes refer to each proxy ID as a \user" or an \individual".

A.3   ZIP Codes

We create a single ZIP code variable for each proxy ID in order to assign a state. This ZIP code variable is equal to the job ZIP code unless missing, in which case it equals the \pip ZIP code", which is the ZIP reported most frequently to the Earnin app. We default to the job ZIP code rst because unemployment bene ts are associated with the state of employment instead of residence.

A.4   De ning Panel, Sample Restrictions

A.4.1   Transaction Coverage

We require that each individual in our sample have transaction data coverage leading up to and following relevant dates for our analyses. We begin with a sample of Earnin users with transactions spanning January 1, 2021 through August 6, 2021, the focus of our main analysis. We re ne this sample further based on transaction memos, state, and earnings tracking.

A.4.2   Uninformative Transaction Memos

For each proxy ID, we count the number of memos that do not o er information about the transaction, which are `CREDIT or' `DEBIT' or memos that are entirely missing. We remove users who have any these types of memos, as it is rare to have only a few of these uninformative memos.

A.4.3   State

There are six states for which our coverage of UI receipt is considerably lower than in other states due to a lack of direct deposit UI disbursement. These states are California, Maryland, Nevada, Arizona, Oklahoma, and Mississippi and are colored in red in the following gures. While it appears that some of those states have measures of UI receipt that match Department of Labor estimates in Figure 9, we attribute this to the fact that these states had low unemployment rates to benchmark.

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Figure 9. Insured Unemployment Rate Comparison

Figure 10 allows us to leverage our 2020 survey in which we asked respondents to report the amount of bene ts they received in July of 2020. In this gure, the lack of coverage of UI receipt is clear, with those six states having over 70% false negative UI receipt tracking, de ned as the fraction of users who report receiving UI in our survey who we do not track through Earnin's administrative data. We remove those states from this analysis.

Figure 10. UI False Negative UI Rate

Notes: The gure above compares the false negative rate of our Earnin UI tracking in July
2020 with the Department of Labor estimate of unemployment rate in July of 2020. We de ne
a false negative as a user reporting receiving UI in our survey and us not tracking UI in their
transactions. To create a rate, we divide this number by the total number of users reporting
receiving UI in July 2020 in our survey in that state. The states colored in red are those
that we exclude from our analyses due to an inability to track unemployment bene ts via
direct deposit. Because we use our survey results here to get a rate of false positives, we use
a less-restricted sample of 4,497 Earnin users with transactions from January 2020 through
August 6, 2021 and who reported receiving bene ts in July 2020 in our survey to estimate the
false negative rate.

We also exclude from this analysis users from states who withdrew from additional federal
unemployment bene ts in July and August. These states are Arizona, Louisiana, Maryland, and
Tennessee; additionally we drop users from Indiana, since that state withdrew from additional
federal unemployment bene ts in June but subsequently restarted those bene ts in July due to a
court order.

The product of applying these restrictions is a sample of 401,812 proxy IDs from states with well-
tracked UI payments, who have no uninformative transaction memos, and who have transactions
from January 1, 2021 through July 23, 2021.

A.5   Identifying UI Payments

We identify those UI payments that are paid through direct deposit based on their memos. Earnin
maintains a list of transaction memos that indicate that an in ow is a UI payment, and we supple-

ment this list with other memos that we identify as attached to UI payments.

We de ne an individual as a UI recipient in week t if they received any UI bene ts in weeks t through $t + 2$.

The gure below shows the number of UI spell starts and ends by week between January 2020 and August 2021 for a sample of users with transactions throughout this period. These patterns of starts and ends are similar to what is shown in Ganong et al. (2021a).

Figure 11. Employment Rate Trend

Notes: The above gure plots the number of UI spell starts and ends by week for our Earnin sample through 2020 to 2021. These estimates are based on the Earnin users from our analysis states from the week ending January 24, 2020 through August 6, 2021.

A.6 Categorizing Consumption

We categorize consumption using transaction categories added by the data processor, Plaid. Plaid uses over 500 categories to describe transactions, so we create a crosswalk between these categories and 19 broader categories that allow us to compare our spending estimates to the Consumer Expenditure Survey and recent work from Ganong and Noel (2019).

First, we correct for variation in Plaid categorization over time; to do so, we rst remove any non-alphabetic characters from transaction memos. Then, we use our 2020 transactions data for those users who lled out our survey to create a modal category for each cleaned memo. We replace the Plaid categorization with this modal categorization.

Then, we merge these stable Plaid categories with our crosswalk to 19 broader consumption categories, further grouped into strict nondurable, other nondurable, and durable consumption based on the categorization developed by Lusardi (1996) and used by Ganong and Noel (2019). We also observe other transfers from bank accounts in this data, including internal and external transfers, checks, credit card payments, mortgage and rent payments, and other bill payments, and we exclude these categories from our measure of total consumption. These other transfers make up a sizeable fraction of out ow transactions (between 30% and 40% of all out ows), a fraction in line with prior work from Ganong and Noel (2019).

Consumption at some vendors includes di erent consumption categories, spanning durables and nondurables. For example, purchases at a discount store can include items in groceries or home improvement. To account for this, we use weights developed in Ganong and Noel (2019) to reallocate spending amounts from Department Stores (80% to other retail, 10% to home improvement, 10% to professional and personal services); Drug Stores (30% to drug stores, 40% to professional and personal services, 30% to other retail); Discount Stores (50% to groceries, 10% to drug stores, 15% to home improvement, 10% to entertainment, 15% to other retail); Grocery Stores (75% to groceries, 25% to other retail); and Wholesale Stores (60% to groceries, 5% to medical copayment, 15% to other retail, 10% to professional and personal services, 10% to home improvement).

Finally, we aggregate these categories into strict nondurable, other nondurable, and durable consumption. Strict nondurables include ights, food away from home, transportation, professional and personal services, groceries, telecom, and utilities; other nondurables include department stores, other retail, online, drug stores, discount stores, and medical copayments; durables include hotels and rental cars, entertainment, retail durables, home improvement, auto repair, insurance, and miscellaneous durables.

A.7 Identifying Earnings

In order to identify transactions as earnings, we leverage multiple aspects of the transactions and observed earnings data. We start by cleaning transaction memos to remove any non-alphabetic characters. This helps make it possible to sum amounts from multiple transactions from the same source, even where memos include dates of payment.

First, we compare transaction amounts to Earnin's observed earnings database. Earnin's observed earnings database includes three earnings variables per week for each proxy ID, representing different sources of earnings. If a user has only one earning, the two remaining variables are missing. If we match a transaction to the amount of one of these three observed earnings from Earnin in a week, we consider those matched transactions to be earnings. If no match to a single transaction exists, we consider matches between observed earnings and the sum of transactions in a week with the same memo to be earnings. For a user with a matched memo, we also consider any other instance of that transaction memo to be earnings. We then track memos over the entirety of the database and consider a given memo to be earnings if it is tracked as earnings more than 5 times globally and is tracked as earnings over 90% of the time it appears.

Second, we perform straightforward searches of transaction memos; we flag any transaction with a memo containing the phrases \PAYROLL," \ACHPAY," \PAYRL," or \SALARY" as earnings.

Finally, we use Plaid's categorization transactions as Payroll or Income. Upon inspection, we find Plaid's categorization of Earnings and Income to be susceptible to false positives. To account for this, we require the memo to occur in more than two unique weeks and with a modal frequency once every one or two weeks and not be identified as unemployment benefits and either include the phrase \DIRECT DEPOSIT" (or derivates) or have a median weekly amount between $50 and $5,000.

After the above earnings identification process, 12,986 proxy IDs have more than five earnings in at least one week of the panel. We omit these proxy IDs from our analysis sample.

We define someone as *employed* in week t if they received any earnings in weeks $t - 2$ through t.

Figure 11 shows the employment rate of our Earnin sample from January 2020 through July 2021. The dips reflect those users that have monthly earnings, again making up less than 5 percent of our sample. Even with these dips, we can see that earnings are tracked well for users both in Withdraw and Retain states.

Figure 12. Employment Rate Trend

Notes: The above gure plots the rate of employment for our Earnin sample through 2020 and
2021. These estimates are based on the Earnin users from our analysis states with transactions
from January 2020 through August 6, 2021.

A.8   Final Sample for Analysis of June UI Withdrawals

We additionally compare the characteristics of our unemployed population to those in the Current
Population Survey. Speci cally, we compare the pre-pandemic earnings distribution of those who
were unemployed in January and February of 2021; as expected, our Earnin sample has lower
earnings than the estimates from the CPS. Furthermore, our insured unemployment rate estimates
track those from the Department of Labor from the beginning of 2020 through July 2021.

Figure 13. Earnings Distributions

Notes: The above gure compares distributions of the average weekly earnings in January and February of 2020 for those who were unemployed in January and February of 2021 between Earnin users with transactions from January 2020 through August 6, 2021 and estimates from the CPS.

Figure 14. Insured Unemployment Rate Trends

Notes: The above lines plot the insured unemployment rates for states that retained additional federal bene ts and those that withdrew them in June of 2021 for our Earnin sample and estimates from the Department of Labor. These estimates are based on the Earnin users from our analysis states with transactions from January 2020 through August 6, 2021.

We also compare the demographic characteristics of our August 2020 sample of unemployed to

those in the CPS. As described, the Earnin sample of those employed or receiving UI benefits in August of 2020 is younger, more female, less likely to have received a college degree, and less white than the CPS estimates of the labor force.

Table 1. Demographic Summary Statistics

	CPS	Earnin
Age	42.181	33.464
Female	0.469	0.666
College degree	0.506	0.200
Race: White	0.765	0.609
Race: Black	0.138	0.336
Race: Asian or Paci c Islander	0.068	0.042
Spanish, Hisp. or Latino	0.191	0.202

Notes: The sample for the above table includes CPS full labor force estimates and estimates for 11,402 Earnin users who completed the survey and were either employed or receiving UI benefits in August of 2020 and had transactions from January 2020 through July 2021.

B   Appendix: Additional Results

The following figure presents the placebo estimates for the cumulative effects of federal benefit withdrawal from June 18th to August 6th. These placebo effects were calculated in the same manner as our main placebo estimates, here using the cumulative effect as our outcome. The placebo estimates are also similar to those prior estimates, with UI inflows ans spending having significant true estimates and nonsignificant placebo estimates as well as all placebo estimates being statistically indistinguishable from zero. The cumulative effect on earnings is close to its placebo estimates, pointing again to a diminshed effect of ending pandemic UI on average earnings.

Figure 15. Placebo Sum
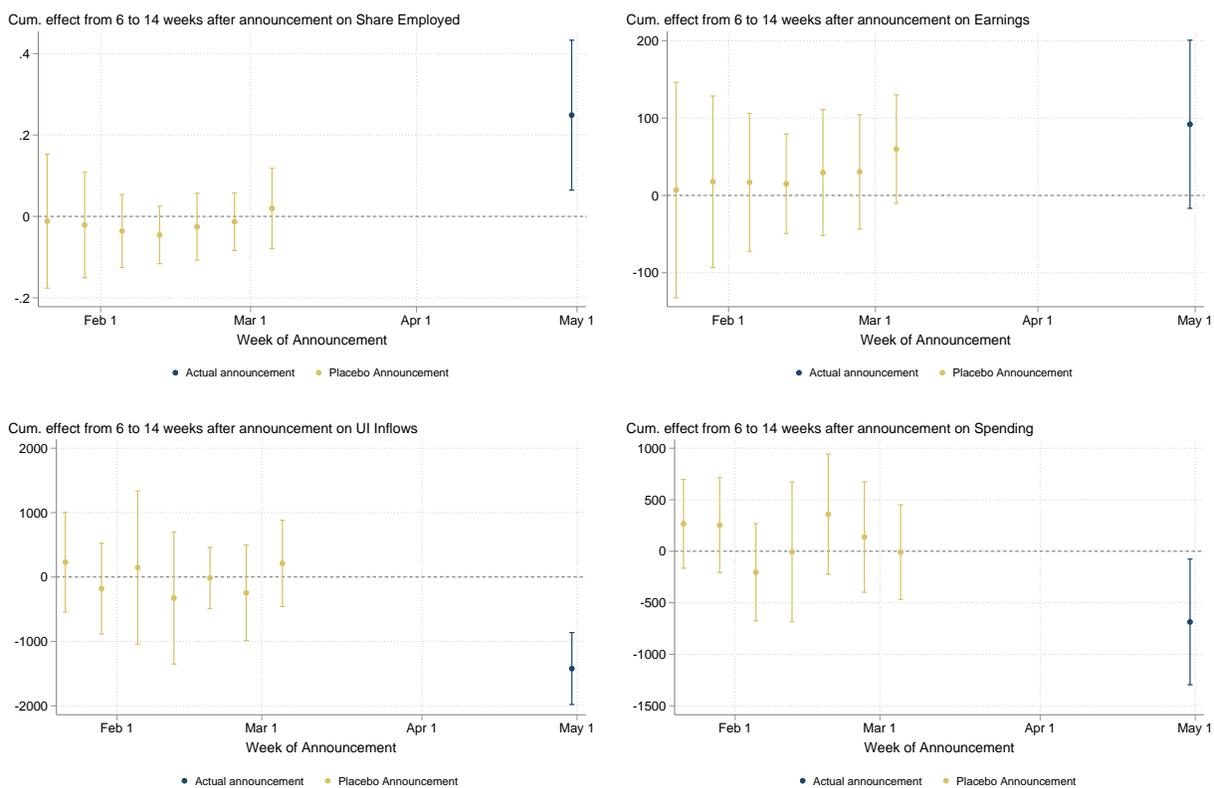
Notes: The figures above present placebo estimates on the cumulative effect of federal benefit withdrawal on state share employed and average earnings, UI inflows, and spending from six to 14 weeks after placebo announcements in each week from January 22nd to March 5th and the week before the actual announcement, April 30th. Placebo estimates with 95% confidence intervals are shown in yellow, and the true estimate with 95% confidence intervals are shown in blue.

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