Crowding out crowd support? Measuring substitution between formal and informal insurance*

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Abstract

I present new results on the informal insurance role of person-to-person (P2P) platforms during unemployment spells in 2019 and 2020. I use a new dataset of administrative bank account balances and transactions with 217,102 users who experience at most one unemployment spell between June 2019 and December of 2020, which are linked to two large-scale surveys (N = 24,671 & N = 12,287) on expectations and economic preferences. During unemployment, I estimate informal insurance as the P2P inflows in excess of pre-job loss inflows, which makes up 14% and 22% as a share of public and informal insurance for UI recipients in 2019 and 2020, respectively. Event study estimates suggest that inflows from all P2P platforms increase to a peak of $25 per month on average after job loss for the first three months before falling in the long run and with the decline in inflows closely tracking the timing of UI receipt suggesting a crowd out effect. I use these crowdout estimates to calculate that welfare falls less than thre cents for the marginal dollar of expanded pandemic UI benefits using the formula provided in **?**. To address the possibility that P2P inflows are informal earnings, I use the formula provided in **?** to sign whether this miscategorization leads to an over- or underestimate. Additionally, I present event studies that show gig earnings increase an average of $10 per month after job loss, which serves as a benchmark for the extent that P2P inflow responses are informal earnings instead of insurance.

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1 Introduction

In March and April 2020, 22.4 million jobs were lost and weekly jobless claims peaked at 6.1 million mostly due to slowdowns caused by the COVID-19 pandemic (?, ?). In response, Congress introduced a weekly $600 supplement to benefit payments, nearly tripling the average earnings replacement rate, and expanded the eligibility and duration of UI under the Coronavirus Aid, Relief, and Economic Security (CARES) Act (?). While the CARES Act included the most generous UI expansion in US history, political frictions led to lapses in certain expansions and delays in payments (?) and mutual aid groups stepped in to fill the gaps.[1] Mutual aid groups were fueled in part by an estimated 9.3-percentage point increase in the share of households giving COVID-19 relief directly to individuals, businesses, or organizations between May 2020 and May 2021 often as direct cash assistance over person-to-person (P2P) payment platforms as private informal insurance (?). This informal insurance role of P2P platforms is common in the developing world. ? shows that the spread of mobile money platform M-PESA in Kenya reduced transaction costs of sending money and coincided with increased income pooling. While relatively little is documented on the relationship between public and private UI, the transition to P2P platforms makes it possible to estimate informal social insurance responses that previously operated via cash.

Within a social insurance framework, more generous public UI should crowd out private UI as the two are substitutes. Given this predicted crowdout, it is somewhat counterintuitive that both the generosity of public UI and private informal insurance increased during the pandemic. In this paper, I use new bank transaction-level data to perform event studies indicating that monthly P2P inflows increase by up to $25 on average in the short-run after job losses in 2019 and 2020, but decline with the onset of public UI benefits, as if UI crowds out P2P. In addition to reconciling simultaneous public and informal UI increases, I use my estimate to show how crowdout changes the optimal UI benefit following ?, filling a gap in the literature.

Early empirical work on optimal UI sought to identify the consumption drops at job loss (?) or re-employment at benefit exhaustion (? and ?). These empirical approaches reflected the canonical Baily-Chetty formula for optimal UI, which balances the consumption smoothing role of UI with

[1] See https://www.nytimes.com/2022/02/19/opinion/pandemic-charity-remittance.html

possible unintended consequences of reducing labor supply (? and ?). ? showed that in the absence of private insurance markets and under a few additional assumptions, the labor supply elasticity with respect to benefits and the consumption change are sufficient statistics to calculate the optimal UI benefit given any coefficient of relative risk aversion. While consumption and labor supply are relatively easy to measure, ? shows that in the presence of private insurance markets, the optimal UI benefit formula includes the extent of public-private crowdout unlesss private insurance generates zero moral hazard.

There is little evidence about the moral hazard of private UI in the US as these markets are relatively thin. Households instead use credit (?) and increase spousal labor supply (?) as alternatives to public UI, and it has proven challenging to measure the role of informal transfers in social networks as insurance, particularly in the unemployment risk context. ? document that when living hand-to-mouth or facing large expeses, users of the P2P platform Zelle are less likely to overdraft than non-users, but their work does not isolate unemployment spells nor observe multiple P2P platforms. In contrast, I observe all major P2P platforms, enabling me to more comprehensively catalog how people send and receive payments within their social networks in the US.

Unlike the US, informal insurance arrangements are widespread and well-documented in small village economies within the developing world, where families and friends use informal credit, gifts, and charity to partially income pool within social networks (?, ?, ?, ?). Theoretical work rationalizes partial income pooling as the product of limited commitment to reduce moral hazard (?, ?), which ? show requires a very particular "sparse" network structure to guarantee stability, suggesting moral hazard persists in these informal insurance arrangements. Recent empirical work in Côte d'Ivoire work suggests informal insurance networks produce moral hazard, as workers who receive private savings accounts increase their labor supply likely because they are able to hide any earnings increases and avoid paying a "social tax" to their friends and family (?).

Given likely moral hazard in informal insurance, how does it interact with formal insurance? First, households have been slow to adopt formal index insurance against aggregate risk in small village economies, suggesting crowdout by informal insurance (?). Within sub-caste networks in rural India, ? finds informal insurance crowds out take up of indexed rainfall insurance when it acts as a substitute for aggregate risk coverage, but also crowds in take up when covering the basis risk that index insurance fails to pay out because the two are complements. Evidence on the effect of formal insurance on informal insurance is similarly ambiguous. **?** show that households in Kenya with a poor informal insurance network smooth consumption better when given health insurance, but health insurance did not seem to crowd out informal insurance during health shocks. In contrast, a lab-in-the-field experiment in Kenya showed adults that purchased formal index insurance participated less in informal group sharing (**?**).

In addition to informal insurance, formal insurance can also affect informal labor supply in unexpected ways. **?** show that increases in the benefit duration of public UI in Brazil lead to small, precisely estimated behavioral increases in the duration of formal unemployment. The authors link this moral hazard effect to widespread informal employment suggesting that increased UI crowds in informal labor supply to further delay formal re-employment.

This informal labor supply response raises a potential issue with my crowdout estimates: P2P inflows may be predominantly informal earnings instead of informal insurance, suggesting that my results measure the informal earnings elasticity with respect to UI benefits instead of crowdout. This hypothetical informal labor supply reduction upon UI runs counter to the crowdin relationship suggested by **?**, which is possible if workers perceive the US as more capable of detecting fraud than workers in Brazil. I handle this potential miscategorization in two ways. First, I use the setup in **?** to provide a formula of sufficient statistics that determines whether my estimate of benefits is an over- or underestimate. Second, I present event studies of monthly earnings from common gig work platforms. These event studies indicate that gig earnings increase by an average of $10 per month in the short-run and also taper off with the onset of UI benefits, providing a benchmark elasticity for the informal earnings role of P2P.

This paper builds on a growing literature that uses de-identified individual bank transactions data to more precisely measure consumption and labor supply, and can measure other behavioral responses to UI that the Bailey-Chetty approach assume are neglible. **?** document the consumption response to UI checks and benefit expiration using rich, transactions-level data from JP Morgan Chase checking account holders during the Great Recession, and **?** use the same data to estimate the impact of the expiration of Federal Pandemic Unemployment Compensation (FPUC) on consumption. **?** also use these data to measure delays in UI receipt from job loss, documenting another advantage in bank transaction data that I will use in this paper. Last, **?** use the same bank transactions-level dataset as this paper to estimate how early withdrawal from expanded pandemic benefits over the summer of 2021 affected spending, job finding, and earnings. Together these papers show that spending is highly sensitive to income during unemployment and falls sharply after expected exhaustion of UI benefits with relatively small labor supply increases after benefit loss, but do not assess the role of informal transfers as part of income.

The rest of this paper proceeds as follows. In section 2, I present the policy context of the pandemic. In section 3, I describe the Earnin dataset. In section 4, I present the two-way fixed effects methodology used to measure informal insurance responses after unemployment and the associated event study. In section 5, I outline the **?** model of public and private insurance, and introduce a test of how miscategorizing P2P would affect my estimates. In section 6, I present robustness checks of my results. In section 7, I provide concluding thoughts.

2 Pandemic Unemployment Assistance Policy Setting

The CARES Act was signed into law on March 27, 2020 and included $2.2 trillion in economic stimulus. The bill included one-time, untargeted cash payments of $1,200 to individuals, expanded unemployment benefits through the Federal Pandemic Unemployment Compensation (FPUC) program, forgivable small business loans through the Paycheck Protection Program, and hundreds of billions of dollars in aid to large corporations and state and local governments. In this paper, we are focused on the initial introduction of the FPUC program, its expiration in late July 2020, and the subsequent modified continuation of the expanded benefits which followed the FPUC payments in some states following an executive order.

FPUC provided an additional $600 per week for those receiving unemployment benefits. The supplementary benefits first arrived in unemployed workers bank accounts in early April and ran through through July 26, 2020. The $600 benefit was in addition to regular weekly unemployment compensation and more than doubled the average replacement rates (**?**).

Following the expiration of the additional $600 per week from the FPUC program at the end of July, President Trump authorized the Federal Emergency Management Agency (FEMA) to provide supplementary payments in the form of Lost Wages Assistance (LWA). Individuals who were eligible for unemployment payments of at least \$100 per week after the expiration of the FPUC program were made eligible for LWA payments of up to \$400 per week with \$300 per week provided by up to \$44 billion of funding allocated through FEMA.[2] The initial rollout of the LWA program produced some confusion, as it was unclear what was required of the states—many facing budget shortfalls as a byproduct of the pandemic—in order to access the additional funding from FEMA (?). In January 2021, the Congress passed the Biden administration's American Recovery Program Act, which restarted the weekly UI payments at a lower \$300 rate, which would continue until the week ending September 3, 2021. Starting in May 2021, 26 states announced that they would withdraw from some or all of the pandemic UI expansions during the summer of 2021. These expansions, terminations, and withdrawals provide a number of useful natural experiments over the first 18 months of the pandemic to evaluate the relationship between unemployment benefits and various outcomes of interest like consumption smoothing and labor force re-entry as done in ? and ?, as well as inflows from various informal insurance options as done in this paper.

3 Individual bank transaction data matched to experimentally-validated survey measures

This paper uses a new dataset of bank account balances and transactions of individuals disproportionately impacted by the pandemic's economic fallout, including over 200,000 UI recipients first introduced in ?. These data come 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 Appendix B. For simplicity, I will call each proxy ID unit an "individual" or a "user" below.

In addition to having a large sample of UI recipients, I tied financial outcomes to welfare and policy-relevant factors not previously observed in conjunction with administrative bank data of this

[2] See FEMA's Lost Wages Supplemental Payment Assistance Guidelines for additional details: https://www.fema.gov/disasters/coronavirus/governments/supplemental-payments-lost-wages-guidelines.

scale. I observed these factors by implementing two large-scale surveys in August 2020 and July 2021 of 24,671 and 12,287 individuals, respectively, linked to bank account information.

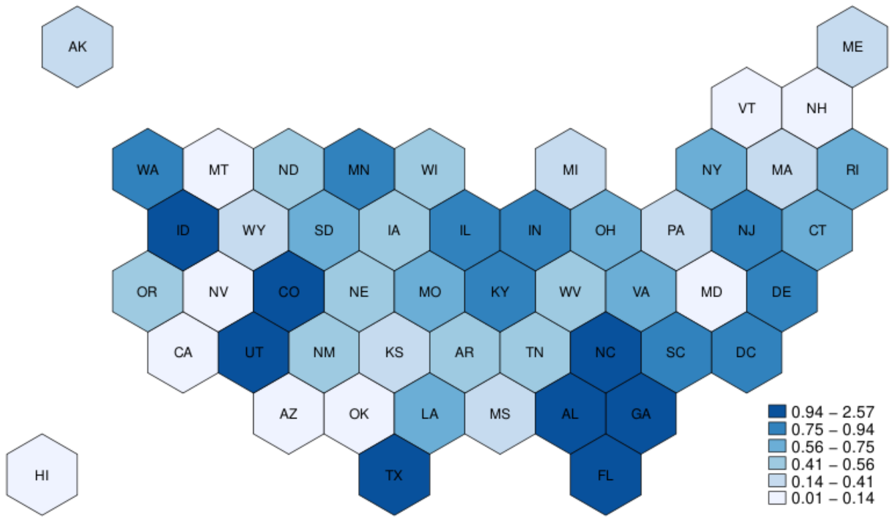
These de-identified transaction-level data come from Earnin, a financial-management platform that provides users who link their bank accounts with products that include accessing their income before payday. The transactions include paychecks and UI payments as well as purchases, allowing me to measure category-level and aggregate consumption. In addition to transactions, I also have end-of-day bank account balances to easily monitor accumulated savings.

The full data include all Earnin users from December 29, 2018 to June 3, 2022, but I make a number of sample restrictions so the data are representative of a user's full financial picture and are a relevant sample for each research question. These restrictions include requiring that a user's first and last bank transaction date fall outside of a question-specific window, among other restrictions.

Earnin users are unrepresentative of the general population, but the data cover a large number of low-wage workers (**?**). One key advantage of this dataset over similar datasets is it appears to be more representative of workers affected by the economic disruptions of the pandemic. **?** use Current Population Survey data to show that mean pre-job loss earnings were $886. The distributions in Appendix Figure 20 are close to this national benchmark suggesting the Earnin data may be more representative of the workers most likely to become unemployed during the pandemic.

A handful of states do not have easy to flag UI transaction memos. These states, California, Nevada, Maryland, Azrizona, Mississippi, and Oklahoma, are dropped from analysis that depends on identifying UI. 175,000 of the million users I observe received UI payments in July 2020, and I see roughly 1,000 UI recipients in the median state. The dataset covers 0.7 percent of the 30 million UI recipients nationwide, with coverage reaching between 1 and 2 percent in states where UI benefits are more commonly dispensed through direct deposit as shown in Figure 1 for state-by-state coverage of UI recipients. For additional details on the Earnin data, see Appendix B.

Figure 1. Fraction of UI Recipients by State



Notes: Hexmap of the fraction of Earnin recipients receiving UI in July 2020.

The transactions and end-of-day balance datasets with two surveys of Earnin users. The surveys ask questions about recent earnings, employment, UI benefits, and consumption for the month of July 2020 and June 2021, respectively. I also ask respondents about their expectations for each of those outcomes for September 2020 and August 2021, respectively. In addition to these questions, I gather demographic information, and elicit risk aversion and discount rates using questions from the Global Economic Preferences Survey (**??**). The survey samples are drawn from the universe of Earnin users who received at least one UI check and an equal-sized sample of users who did not receive a UI check between January and July 2020.[3] Sampling users who received UI lets me analyze the effect of federal benefit changes.[4] Results from these survey groupings are forthcoming as the survey results are validated.

3.1 Unemployment Definitions & Sample Construction

First, I drop any users for whom more than one percent of bank memos are "uninformative," that is are the words, "CREDIT," "DEBIT," or missing. Second, I require users to have at least one full

[3]The sample is additionally restricted on our ability to observe bank transactions on or before January 1, 2020 and on or after July 1, 2020. Potential respondents in the survey sample were offered an incentive of a $5 Amazon gift card.

[4]Ideally our July 2021 survey sample would have been indexed to UI receipt in 2021, but administrative burdens required the team to use the 2020 sampling frame.

year of transactions data. Third, I require that users experience one unemployment spell between June 2019 and December 2020 (I explain how I measure employment and UI receipt below). Fourth, I follow **?** by requiring that all users have at least five outflows in the three months before and after they become unemployed. This yields samples of 217,102 users overall with 153,892 in 2020 and 63,210 in 2019, of these 47,614 and 4,853 received UI during their unemployment spell, respectively.

I 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 21 days pass without any UI payments. In the case where the last UI payment is deposited on day $t+k$, and no additional UI payment is received in dates $t+k+1$ through $t+k+21$, we define the spell to have ended at date $t+k$.

I classify individuals as being employed using paycheck deposit information. First, I define users as having a weekly, biweekly, monthly, or undefined paycycle. In particular, an employment spell starts at time t when the first paycheck is deposited at date t. Depending on the paycycle, the employment spell ends after 21, 28, or 42 days without a paycheck deposit, for weekly, biweekly, and monthly and undefined paycycle users. A job is defined as lost on the date t of the last paycheck deposit of the employment spell.

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

3.2 Identifying P2P and Gig platforms

I measure P2P and gig memos using a combination of regular expression flags and transaction categories provided by the financial services company, Plaid[5], to measure monthly P2P inflows and outflows for all major P2P platforms including Venmo, Cash App, PayPal, and Zelle[6] and gig inflows for a selected sample of 20 platforms including Uber, Lyft, Taskrabbit, and Upwork[7].

Several P2P platforms are used to pay for goods and services. Consequently, the regular expressions I use will pick up transactions that are not transfers between friends and family, but

[5] Plaid uses natural language processing to categorize bank memos to allow financial service companies better track how users spend money.

[6] The full list of regular expressions is available in the appendix.

[7] The full list of regular expressions and gig platforms is avaiable in the appendix.

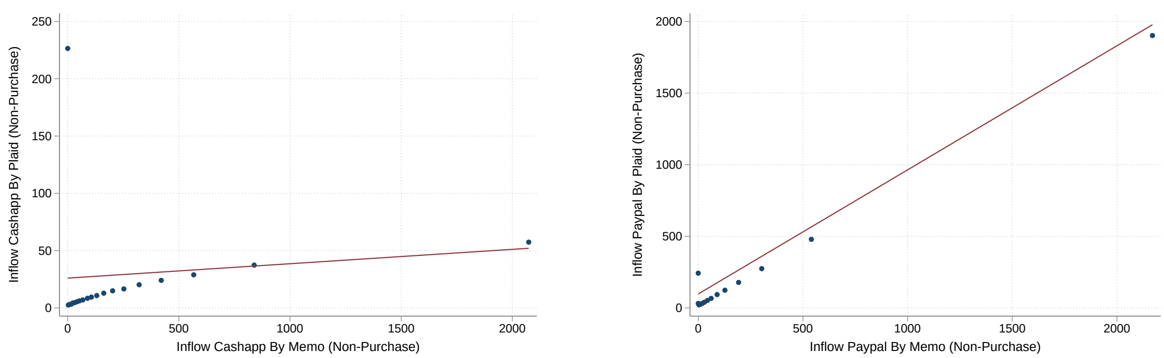
instead payments associated with consumption, refunds, or running a small business. I exclude memos that include a variety of regular expressions associated with sales of goods or services like "POINT.*OF.*SALE" or "POS DEBIT," "OVERDRAFT," and several others listed in the appendix. Similarly, I drop all those memos that mention "EARNIN," as these are likely false positives. These omissions do not change the results I identify below.

To validate the P2P regular expressions, I compare to the Plaid categorization for PayPal and Venmo. Figure 2 presents a binscatter of the monthly inflows associated with both totals follow a nearly exact 45-degree line, building confidence in the flagging of memos.

Figure 2. Binscatter of PayPal and Cashapp Plaid categories versus memos

(a) Cashapp

(b) PayPal



Notes: Binscatters comparing P2P inflows as flagged by memo regular expressions and Plaid categorization.

4 Event studies of informal insurance response during unemployment

I start by presenting event studies around first job loss in a twoway fixed effects model. Equation 1 shows the format of this event study where α_i represent user fixed effects, λ_t are calendar-month fixed effects, and D_s^t is an indicator for being s months since unemployment in month t.

$$y_{it} = \alpha_i + \lambda_t + \beta_{-6} \sum_{s \leq -6} D_s^t + \sum_{s \in [-5,-3]} \beta_s D_{it}^s + \sum_{s \in [-1,9]} \beta_s D_{it}^s + \beta_{10} \sum_{s \geq 10} D_{it}^s + \varepsilon_{it} \qquad (1)$$

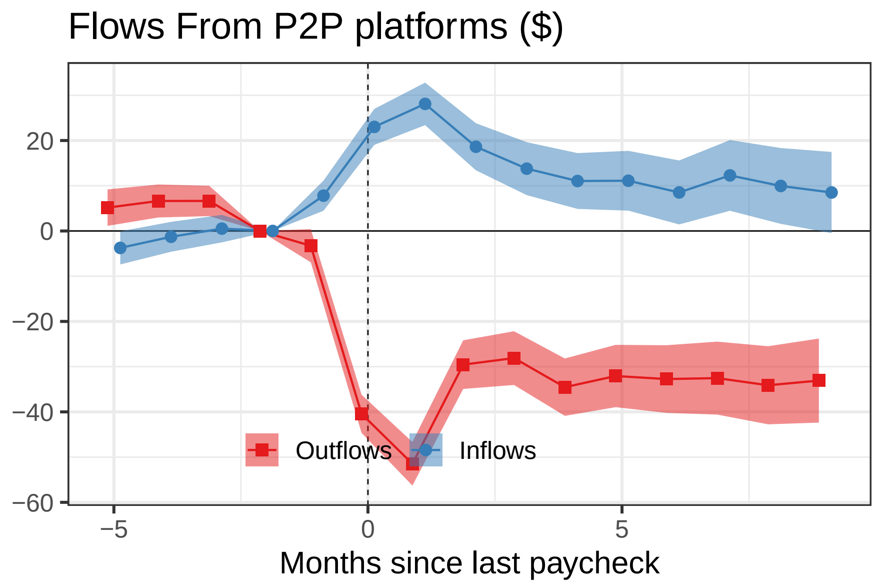
where y_{it} is any outcome like P2P inflows and outflows and Gig employment inflows. I choose to omit month -2 because the last paycheck may come several weeks after someone has lost their job such that the unemployment event is indexed to the next calendar month, but friends may have started sending money in advance of the last paycheck. In some event studies, I present event studies by subset such as months to UI, length of unemployment, tercile of the lost earnings replacement rate, or tercile of bank balances before becoming unemployed.

4.1 P2P inflows and outflows

Figure 3 shows an event study of P2P inflows and outflows leading up to the last paycheck relative to two months prior to losing the job. The pretends of both are relatively flat, indicating little differential use of P2P in anticipation of job loss. In the month prior to the last paycheck, monthly P2P inflows increase by about \$5, then peak around \$25 per month in the first month after the job loss before dropping to \$5 extra per month by month 3. By month 4 and on, P2P inflows continue to drop off to well below zero. Figure 4 shows a similar relationship for most major platforms, albeit with different magnitudes and precision. Zelle shows the largest increase, while the Venmo and CashApp increases are just a few dollars. Meanwhile, PayPal shows a fairly noisy monthly increase of \$5 per day. All platforms follow the same dynamic path of inflows and outflows around job loss.

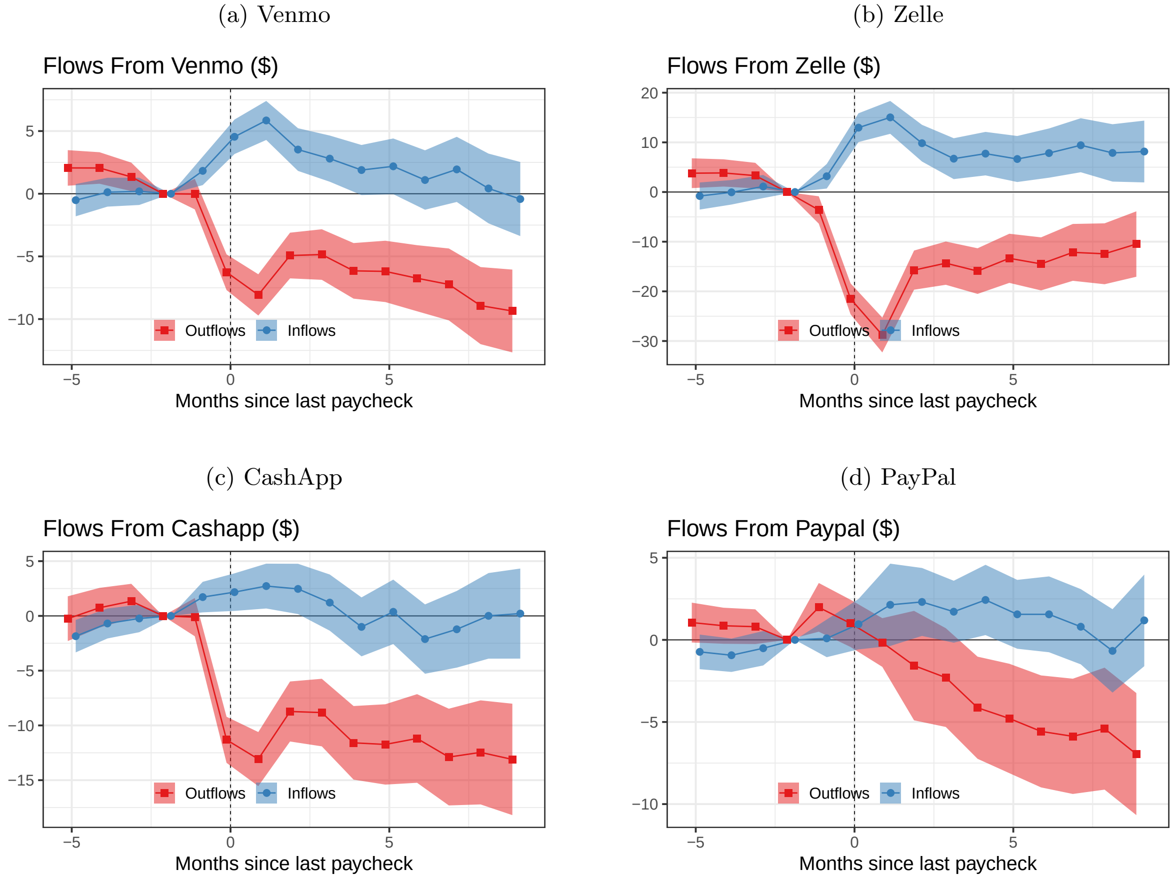
This dynamic path suggests that informal support from friends and family occurs after the initial job loss, but does not last, possibly because a job has been found or possibly because people drop support after a certain duration.

Figure 3. P2P inflows & Outflows



Notes: Within-person event study of P2P inflows and outflows for users with a single unemployment spell around month of job loss. Standard errors clustered at user-level.

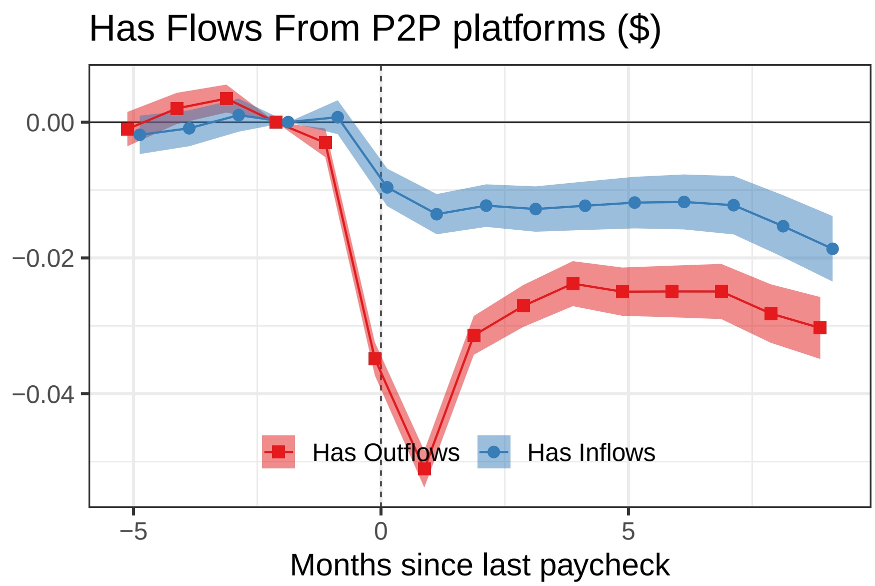
Figure 4. P2P inflows by platform



Notes: Within-person event studies for each P2P platforms inflows and outflows around the month of job loss. Sample restricted to users that had at most unemployment spells. Standard error's clustered at the user-level.

Does this increase in average P2P track an increase in use of P2P, but relatively low amounts of money? Or is this average increase from a concentrated group of users who see more P2P during unemployment? Figure 5 suggests I measure the latter case. This figure plots an event study for the extensive margin of using P2P, literally whether there is any inflow or outflow, around a job loss. There is an initial pretrend indicating some take up and then an immediate drop off in use after job loss. Together figures 3 and 5 suggest that P2P inflows after a job loss spike for a subset of workers receiving support through this means.

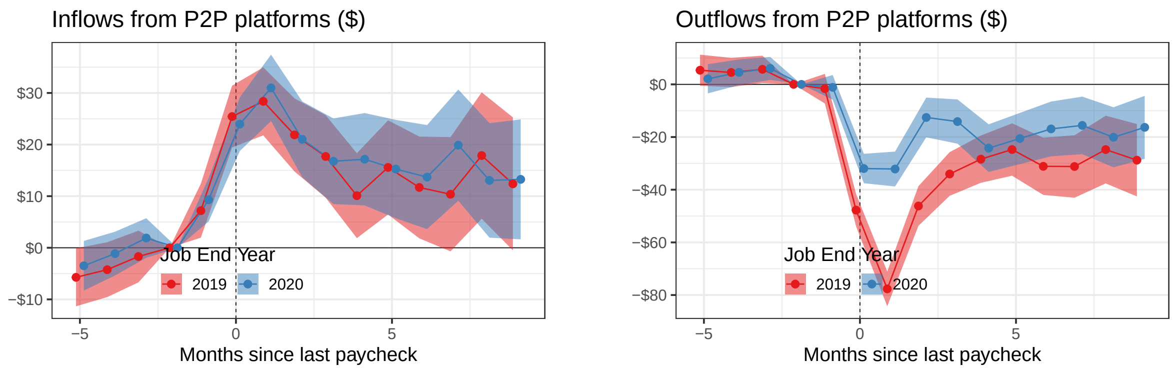
Figure 5. Extensive Margin of P2P



Notes: Event study of whether the user had inflows, outflows, or either transactions from a P2P platform. The sample is restricted to users with a single unemployment spell. coefficients Standard error's clustered at the user-level.

While the increase in P2P inflows is concentrated among those with longer unemployment spells, how do things differ between 2019 and 2020? Unemployment spells were typically longer during the pandemic. Additionally, P2P use increased during the pandemic – so do the earlier results capture the correlation between unemployment timing and the increase in P2P beyond monthly fixed effects? Figure 6 shows similar inflow and outflow paths during the pandemic regardless of the year of job loss. While there are fewer users with a job loss in 2019 in my sample, leading to slightly less precision, the point estimates are essentially the same with one another with two exceptions: a slight pretrend and a lower reduction in post-job loss outflows during 2020. Both may reflect the increase in unemployment insurance during 2020, which create greater liquidity immediately around job losses due to higher benefits.

Figure 6. P2P inflows & Outflows by Year of Job Loss



Notes: Each figure shows event study coefficients of the relative months since job loss interacted with the year a worker experienced their job loss. Sample restricted to users with a single job loss. Standard errors clustered at user-level.

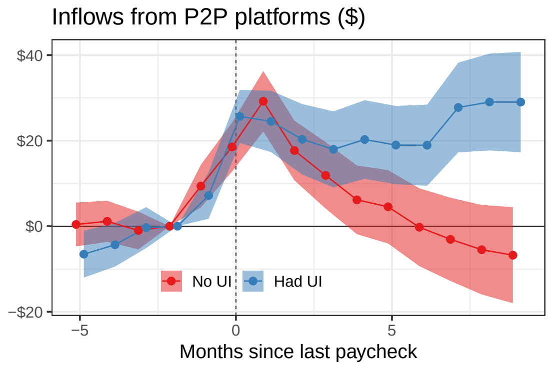
Given the unemployment increase in 2020 does not lead to major changes in inflow paths, but does affect outflows – what is the relationship between receiving UI and P2P inflows? Figure 7 shows event study coefficients from interacting the relative time dummies with whether or not a worker received UI within six months of their job loss. In order to reduce any false negatives, I restrict to those states in which I am able to relatively accurately flag UI payments.

First, P2P outflows increase, suggesting a "marginal propensity to consume" effect consistent with past work on UI receipt. Second, P2P inflows increase for both groups, but last longer for those who receive UI. This longer increase for UI recipients might counterintuitively suggest that UI crowds in informal insurance or simply reflect longer periods of unemployment for recipients.

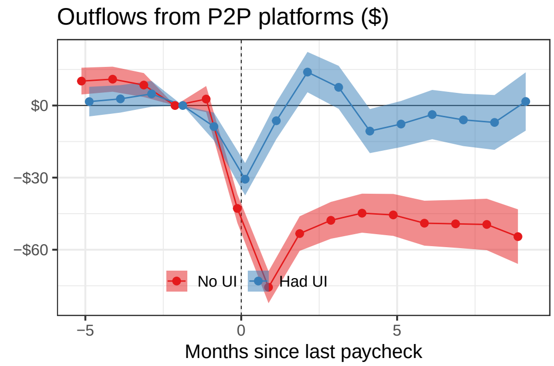
Therefore, I further explore the relationship between P2P and UI in Figure 8, which plots three separate event study coefficients taken from interacting the relative time dummies with whether it took 0-1 month, 2-3 months or 4-6 months to receive unemployment insurance, again eliminating users in states where I do a poor job flagging UI payments. The outflows further show the "marginal propensity to consume" story, as they increase in clear "check marks" patterns at the onset of UI. Meanwhile, those who receive UI immediately receive much less in P2P inflows and for a shorter duration, while those who receive UI after a delay receive more P2P and for longer as the length of uninsured unemployment increases, consistent with a crowdout relationship between UI and P2P.

Figure 7. P2P Inflows & Outflows by UI receipt during unemployment

(a) P2P Inflows



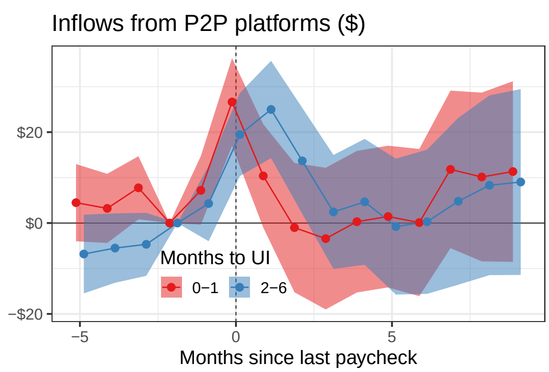
(b) P2P Outflows



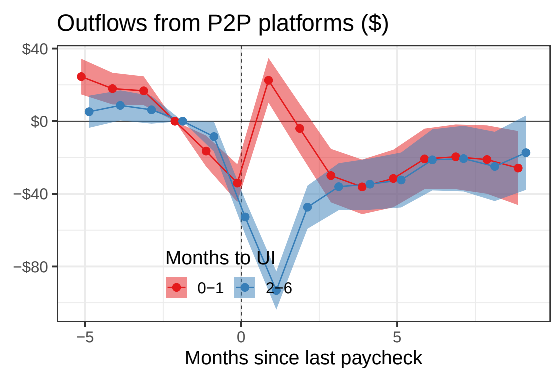
Notes: Each figure shows event study coefficients of the relative months since job loss interacted with whether the user received UI during that period. Sample restricted to users with one job loss and excluding users in states that do not have easily identifiable UI deposit memos. Standard error's clustered at the user-level.

Figure 8. P2P Inflows & Outflows by Months to UI Receipt

(a) P2P Inflows



(b) P2P Outflows



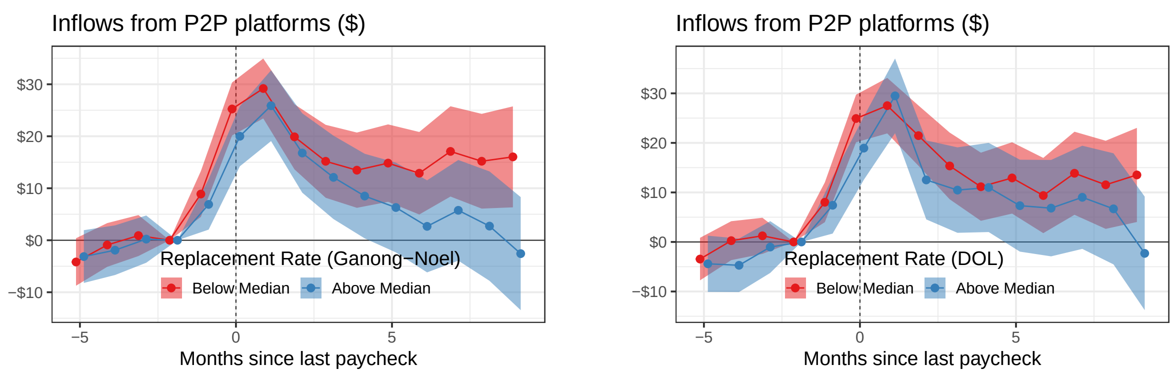
Notes: Within-person event study coefficients interacting the months since job loss with bins for the months to UI since job loss. Sample restricted to users with a single unemployment spell and excluding users in states that do not have easily identifiable UI deposit memos. Standard error's clustered at the user-level.

While UI timing and potential UI receipt during unemployment show a crowdout relationship, both are partially the consequence of whether a user chooses to get UI and how soon to apply. These UI receipt choices are endogenous to a variety of factors including the supportiveness of informal insurance arrangements. Figures 9 show event study coefficients interacted with whether a user lives in a state with above median state statutory replacement rate of pre-job loss earnings.[8]

I calculate the median using two series of the 2019 statuory replacement rates. First is the median 2019 statutory replacement rates calculated by **?** and second is the average 2019 statutory replacement rate, calculated as part of the Department of Labor Benefit Accuracy Measurement (BAM). In many states, the median tends to exceed the BAM rates because UI formulas cap max benefits for earnings with higher pre-job loss earnings, but these are irrelevant for median earners. As my sample is predominantly made up of low-income workers, this makes the median formula preferable.

Figure 9. P2P Inflows by Above Median Replacement Rate

(a) Median Statutory Rate (b) Average Statutory Rate



Notes: Within-person event study coefficients are interacted with whether a user lives in a state that is above or below the median statutory replacement rate of pre-job loss earnings. Figure (a) groups states by median replacement rate calculated by **?**, while Figure (b) groups states by state average replacement rate calculated by the Department of Labor Benefit Accuracy Measurement program. Sample restricted to users with a single job loss. Standard error's clustered at the user-level.

The event studies in Figure 9 show the same short-term increase, but also show that users in states with lower replacement rates tend to receive more P2P suggesting a higher UI crowds out informal insurance. During this period, UI increased under the CARES Act in both states, but P2P support followed a very similar path for those that lost their job in 2019 and 2020 as shown in Figure 6. In Figure **??**, I present event study coefficients interacted with groups for the median replacement rate and year of job loss.[9] These event study coefficients show very little difference

[8]The user-specific replacement rate can also be calculated, but is also endogenous. I put the same results by tercile in Appendix Figure 12 which show an imprecise version of the state-based results.

[9]The BAM average replacement rate groupings can be found in appendix figure 13

between 2019 and 2020 in either high or low UI states by either measure, but this is partially driven by imprecise measurement of P2P inflows in 2019 in states with above median replacement rate.

4.2 Benchmarking P2P as a share of unemployment insurance support

The unemployment event studies show a clear, short-term increase in P2P payments during early unemployment, which suggests these payments play an informal insurance role. It is challenging to assess the magnitude of this role for a variety of reasons. Ideally, I would take a month of P2P payments as a share of public UI to benchmark the role of informal insurance. Unfortunately, there are a variety of barriers that make this approach inaccurate.

First, people receive P2P payments for a variety of reasons, while public UI is paid (statutorily) after facing an employment shock – typically a job loss. Another contrast between P2P payments and public UI is the latter is paid in regular installments of the same amount determined by a state-specific formula for the duration of the unemployment spell, while P2P payments are not paid in predetermined amounts or on a fixed frequency or duration.

In order to put P2P informal insurance, denoted b^p, and public UI, denoted b, in comparable terms for a single unemployment spell, I first calculate the total b from the start of a spell to the two months just after the spell ends. Next, I assume any monthly P2P in excess of the pre-unemployment average monthly P2P is informal insurance. Excess P2P can be estimated using a regression of P2P inflows for the four months on either side of the unemployment spell with time and user fixed effects.

$$b^p = \lambda_t + \lambda_i \times \text{Unemployed}_{it} + \epsilon_{it} \tag{2}$$

I predict excess P2P with the user-unemployed fixed effect $\lambda_i \times \text{Unemployed}_{it}$, which measures the average monthly P2P paid to the individual during unemployment less average monthly P2P levels. I multiply these user-unemployed fixed effects by four to get the total excess P2P received during the spell. Together the sum of total informal and formal insurance quantifies total unemployment insurance.

For those users with negative excess P2P, it is possible for the share of excess P2P of total insurance to be outside of zero and one. In order to account for this, I bin the share at zero and one. In Figure 10, I plot histograms of this binned share by whether the job loss was in 2019 or 2020 with vertical lines for the (unbinned) average of the ratio. The first figure is unconditional UI receipt, while the second conditions on UI receipt.

In 2019 and 2020, 91% and 60% of total insurance came from excess P2P, respectively, unconditional on UI receipt. Furthermore, the histogram shows over 90% with nearly all of their total insurance coming from excess P2P. This amount shrinks to nearly 50% in 2020 with the introduction of the additional $600 and expanded eligibility of the CARES Act. Also, roughly 20% receive effectively zero excess P2P as a share of total insurance. Conditional on UI receipt, these percentages fall to 22% and 14% in 2019 and 2020, respectively. Additionally, the histograms show both 33% and 45% receive effectively zero excess P2P, in 2019 and 2020, respectively, and declining long right-tail. In 2019, there is relatively more mass above 50% than in 2020, again reflecting the CARES Act. Altogether these histograms show wide heterogeneity in the excess P2P share of insurance.

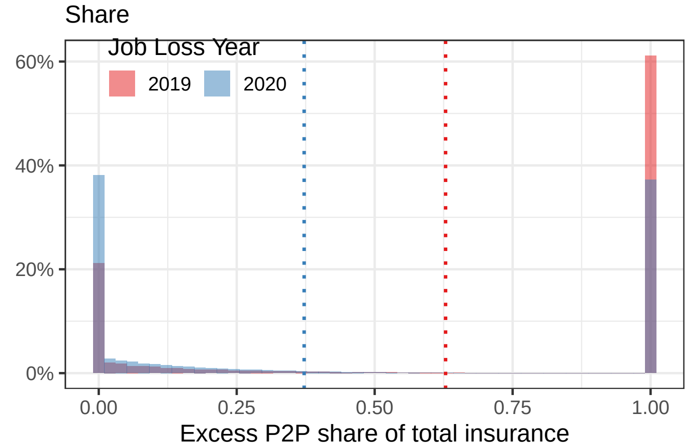
4.3 Crowdout of private insurance by public insurance

If informal insurance is a relatively small share of total insurance, to what extent does public insurance crowd out informal insurance or gig work? The event studies in figure 8 show that P2P inflows continue for longer if UI receipt is delayed, which is highly suggestive of crowdout from public insurance. These results suggest that public insurance may crowd out informal insurance, reducing the associated welfare and changing the optimal UI formula (**?**).

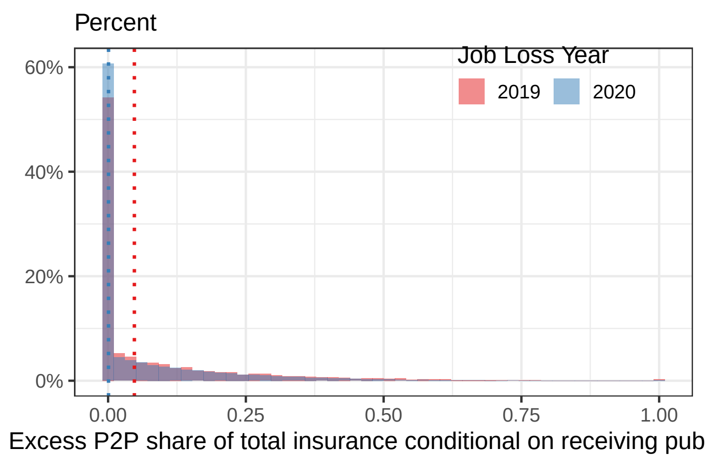
A naive measure of crowd out would be to regress P2P inflows on public UI inflows with user and monthly fixed effects to measure the relationship db^p/db, but the two forms of insurance are endogenous as public UI starts getting paid during unemployment, which corresponds to an increase in P2P. Similarly, those who receive relatively large early P2P payments may be slower to apply for UI, creating reverse causality. Why not restrict the regression to only the unemployment spell? In that case, I would not measure P2P inflows residualized of individual fixed effects calculated for the period before being unemployed, which is our relevant measure of informal insurance.

Figure 10. Histograms of excess P2P share of total formal and informal UI support

(a) All



(b) UI Recipients



Notes: Histograms of the excess P2P during unemployment as a share of excess P2P plus total UI received during the first four mouths after a job loss. Restricted to a sample of users with at most one job spell and excluding users in states that do not have easily identifiable UI deposit memos.

Instead of relying on this naive regression, I exploit the cross-state variation in UI generosity and the variation generated by pandemic unemployment expansions. To implement this strategy, I separate workers into year-of-job-loss cohorts and conduct a triple difference-in-differences. I also account for the fact that excess P2P payments typically come in the first few months of unemployment by separating the unemployed period into "short" and "long" run employment, where "short" is defined as the first three months of unemployment.

$$\begin{aligned} \text{P2P}_{it} = \alpha_i + \lambda_t + \beta_1 \text{SR}_{it} + \beta_2 \text{LR}_{it} + \beta_3 \text{High UI}_i \times \text{SR}_{it} + \beta_4 \text{High UI} \times \text{LR}_{it} + \\ \beta_5 \text{High UI} \times \text{LR}_{it} \times 1\{t \geq 2019\} + \beta_6 \text{High UI} \times \text{LR}_{it} \times 1\{t \geq 2019\} + \epsilon_{it} \end{aligned} \tag{3}$$

Equation 3 shows the triple DID specification where i and t are worker and month indicators. SR and LR are dummies for whether this observation is within the first month of job loss, i.e. the short-run, or longer, i.e. the long-run. High UI refers to whether the worker is in a state where the pre-job loss earnings replacement rate of UI is above the country median, again using the **?** and BAM calculated median and average rates. Last I interact with an indicator for the year of job loss as a proxy for whether the worker was eligible for the CARES-expanded unemployment insurance schemes.

Table 1 shows the triple DiD coefficients for each of the P2P platforms in addition to the overall measure of P2P. These coefficients track with the results in the event studies – P2P inflows increase in the short-run after a job loss for all platforms and are precisely estimated, but the long-run effects are often negative and less precisely estimated. Meanwhile, the marginal effect of being in a state with a higher replacement rate is less precisely estimated, but negative in the long-run for the full zeries and Zelle. Meanwhile, the marginal effect of losing your job in 2020 in a less generous UI leads to increase in P2P in both the short and long-run, but these increases drop off in the more generous states – suggesting a crowdout relationship.

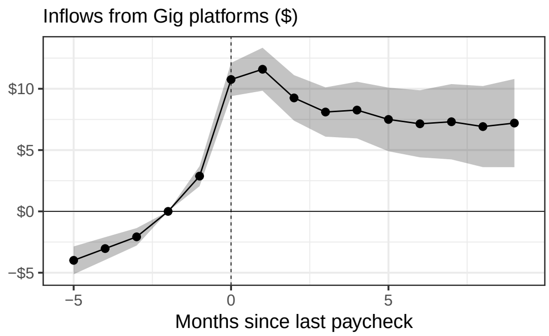
At the bottom of the table, I present calculations of $\beta_5 - \beta_3$ and $\beta_6 - \beta_4$ from equation 3, which are the average difference in P2P inflows from 2019 to 2020 received during unemployment for workers that live in states with high statutory replacement rates. Workers that lost their job in 2020 were much more likely to receive an additional \$600, so I expect this relationship to be negative, suggesting that greater UI crowded out P2P in areas with relatively generous pre-pandemic UI replacement rates. Unfortunately, these results are imprecisely estimated for all series, but for the **?** median replacement rates – P2P inflows fall an average of \$16 in the short-run. For Zelle, the most precisely estimated platform, the decline is \$8-\$9 per month under both definitions. Given that statutory UI increased \$600 per week, or \$2400 per month, this makes short-run crowdout neglible. In fact, the positive results suggest that more generous UI may crowdin more P2P support – possibly due to those receiving UI giving to those who are still waiting on UI to show up due to delays in a coinsurance relationship. Unfortunately, I am unable to assess the possibility of coinsurance as I cannot see links between users in my dataset.[10]

4.4 Gig work earnings

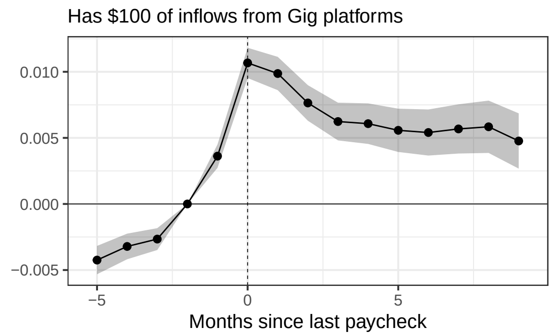
An alternative reason that P2P inflows increase after a job loss is that a worker has transitioned to working in the "informal" sector. In that case, these results would suggest a labor supply response instead of a crowdout relationship between public and informal UI. To that end, I also flag "gig" work earnings, which are likely similar to those in the informal P2P sector and repeat the event studies above. Specifically, I look at both total gig work inflows and the probability of receiving at least $100 in gig inflows, as $100 is often the minimum threshold for payout on platforms like Twitch, OnlyFans, etc.

Figures **??** and 11 show the overall relationship and the interactions with the months to receiving UI. These show a temporary increase in gig earnings during unemployment similar to the P2P inflows increase, as well as, a similar substitution relationship with the time to UI – though the earnings increase peaks at a lower amount of only $10. Similarly, the extensive margin results suggest a temporary increase in P2P peaking around one percentage point, which also increases with the uninsured duration. These results suggest that gig work follows a similar pattern as P2P – I attempt to diagnose the extent that this problem will affect any welfare implications in the next section.

[10]This is a potential avenue for future work given access to a P2P platform's specific user payment network.



(c) Gig Earnings



(d) $\geq$ 100 Gig Earnings

Notes: Within-person event study of gig work earnings and the probability that gig work earnings exceed $100 around month of job loss. Standard error's clustered at the user-level.

Figure 11. Intensive and Extensive Margin of Gig Work With Time to UI Receipt

Table 1. Triple difference-in-difference of P2P inflows during unemployment in 2019 vs 2020

	Median Replacement Rate						Average Replacement Rate					
	All P2P	Zelle	PayPal	Venmo	Cashapp	Other P2P	All P2P	Zelle	PayPal	Venmo	Cashapp	Other P2P
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
0-1 mths post-job loss	-39.7***	-18.3***	-2.0	-5.5***	-11.7***	-2.8***	-40.6***	-19.9***	-1.3	-5.2***	-11.9***	-2.9***
	(3.3)	(2.7)	(1.8)	(0.47)	(0.70)	(0.45)	(3.1)	(2.3)	(1.7)	(0.49)	(0.72)	(0.44)
2+ mths post-job loss	-50.7***	-19.6***	-4.0***	-7.6***	-15.0***	-3.8***	-53.0***	-21.7***	-3.5**	-8.4***	-14.5***	-4.5***
	(3.4)	(3.1)	(1.4)	(0.83)	(1.4)	(0.67)	(3.1)	(2.8)	(1.5)	(0.91)	(1.2)	(0.59)
Lost Job in 2020 × 0-1 mths post-job loss	-2.0	3.4	-0.01	-1.1	-2.3**	-1.9***	-1.5	3.3	-0.58	-1.2*	-1.4	-2.0***
	(4.2)	(2.8)	(1.6)	(0.70)	(1.0)	(0.50)	(3.6)	(2.2)	(1.6)	(0.64)	(0.95)	(0.50)
Lost Job in 2020 × 2+ mths post-job loss	-1.1	1.9	1.3	-0.51	-3.2**	-0.67	-0.85	1.7	0.80	-0.13	-2.8**	-0.51
	(4.8)	(2.7)	(1.5)	(1.1)	(1.4)	(0.76)	(4.6)	(2.4)	(1.4)	(1.0)	(1.3)	(0.71)
Above Med. Rep. Rate × 0-1 mths post-job loss	-1.4	-2.5	0.43	0.77	-0.27	0.46	1.2	1.7	-1.5	0.20	0.31	0.72
	(3.8)	(2.5)	(2.4)	(0.72)	(1.5)	(0.45)	(3.7)	(2.4)	(1.9)	(0.90)	(1.4)	(0.44)
Above Med. Rep. Rate × 2+ mths post-job loss	-8.3	-13.0*	2.1	-0.003	1.5	-0.93	-3.1	-9.8	0.83	2.3	0.07	0.95
	(9.0)	(7.7)	(2.1)	(1.3)	(3.0)	(1.1)	(9.1)	(8.0)	(1.9)	(1.4)	(3.3)	(1.3)
Lost Job in 2020 × Above Med. Rep. Rate × 0-1 mths post-job loss	1.5	-7.3	0.12	0.87	2.6	0.19	0.58	-8.7	1.9	1.4	0.49	0.58
	(8.3)	(6.2)	(2.5)	(1.4)	(1.9)	(0.75)	(7.7)	(6.2)	(2.0)	(1.7)	(1.9)	(0.79)
Lost Job in 2020 × Above Med. Rep. Rate × 2+ mths post-job loss	3.8	-1.3	-0.15	1.7	1.1	0.06	3.8	-0.94	1.2	0.88	0.32	-0.31
	(7.0)	(3.9)	(2.2)	(1.5)	(1.9)	(0.89)	(6.8)	(4.0)	(2.1)	(1.5)	(2.1)	(0.81)
Observations	9,322,173	9,322,173	9,322,173	9,322,173	9,322,173	9,322,173	9,364,777	9,364,777	9,364,777	9,364,777	9,364,777	9,364,777
R^2	0.34986	0.42857	0.19111	0.36617	0.27273	0.28337	0.34997	0.42857	0.19116	0.36632	0.27286	0.28361
0-1 months Difference	2.8855	-4.7991	-0.31258	0.09594	2.8681	-0.27004	-0.60600	-10.409	3.3319	1.1589	0.17625	-0.14305
0-1 months Difference SD	11.257	7.6547	4.8833	1.9970	2.5957	1.0744	10.489	7.5223	3.8131	2.4176	2.6291	1.0549
2+ months Difference	12.042	11.714	-2.2207	1.6799	-0.32002	0.98287	6.9015	8.8620	0.37415	-1.4280	0.24194	-1.2644
2+ months Difference SD	12.284	8.1225	4.0006	2.2776	3.8580	1.4922	12.171	8.5952	3.6639	2.1434	4.4390	1.6333
Proxy ID fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Triple difference-in-difference results from equation 3 for each P2P inflow series less purchase and Earnin memos. (1)-(6) use ? calculation of median replacement rate. (7)-(12) use Department of Labor Benefit Accuracy Measure of average replacement rate calculations. Standard errors clustered at state-level.

5 Model: Optimal unemployment insurance with private insurance

In this section, I build on the model presented in **?** of optimal insurance benefits in the presence of unoptimized private insurance with some moral hazard. The intuition of the model is as follows, there is some private insurer who fails to fully optimize around government UI such that crowdout is incomplete. Additionally, workers exhibit moral hazard responses to both public and private insurance benefits – neither mechanism is able to fully punish behavioral changes. These relationships reduce optimal UI benefits for two reasons: (1) to directly offset the amount of UI provided privately and (2) to balance the moral hazard cost of both private and public insurers against the value of public insurance. As a result, the optimal public UI is lower and is set to make the sum of private and public insurance equal to optimal UI with a single provider.

The model consists of a continuum of workers with ability level n drawn from a distribution $F(n)$. These workers can choose to earn one of two earnings levels, $z \in \{z_L, z_H\}$ after observing ability n.All workers have the same separable utility function that makes empirical applications easier:

$$U(c, z; n) = u(c) - h(z/n) \tag{4}$$

Before observing n, the worker signs a contract with a private insurer to smooth utility across the two states. The worker is also enrolled in unemployment insurance by the government. Both the government and insurer seek to insure against the two possible earnings levels. When earning z_L, the worker receives b^p and b from the insurer and government respectively. Those earning z_H must pay τ_P and τ to the insurer and government, respectively. These two insurance contracts and earnings levels can be plugged into the utility function above to solve for a threshold n^* above which workers will choose to z_H and below which they will choose z_L:

$$u(z_h - \tau - \tau_P) - u(z_l + b + b^p) = h(z_H/n^*) - h(z_L/n^*) \tag{5}$$

The government applies a social welfare function to aggregate all utility over all agents:

$$W = \int_0^{n^*} [u(z_L + b + b^p) - h(z_L/n)]dF(n) + \int_{n^*}^{\infty} [u(z_H - \tau - \tau_P) = h(Z_h/n)]dF)n) \quad (6)$$

If we denote $e = 1 - F(n*) = \int_{n^*}^{\infty} dF(n)$, and define F^{-1} as the inverse of F, then we know that $n^* = F^{-1}(1-e)$, and social welfare can be written as a function of e:

$$W(e) = eu(z_H - \tau - \tau_P) + (1-e)u(Z_L + b + b^p) - \varphi(e) \quad (7)$$

where

$$\varphi(e) = \int_0^{\infty} h(z_L/n)dF(n) + \int_{F^{-1}(1-e)}^{\infty} [h(z_H/n) - h(z_L/n)]dF(n)$$

gives the total disutility associated with working to earn z_H. Effectively, the social planner takes the private and government contracts as given and chooses the fraction e that earn z_H to maximize welfare, W. This setup is the one presented in **?**, which is isomorphic to **?** in which agents' effort level e choice determines their likelihood of having low/no earnings.

The government considers b^p and e functions of b, $b^p(b)$ and $e(b)$, respectively and thus sets b to optimize $B = b + b(p)$, the total insurance level where $b(p)$ may or may not be set optimally. **?** define $\tau(b)$ to guarantee that $\tau + \tau_p = \frac{1-e}{e}(b^p(b) + b)$, the actuarially fair tax rate, yielding the welfare equation that the government maximizes over b:

$$W = eu\left(z_H - \frac{1-e}{e}\left(b^p(b) + b\right)\right) + (1-e)u\left(z_L + b^p(b) + b\right) - \varphi(e) \quad (8)$$

Before solving for the welfare gain from changing b, I will define two further parameters. First, the extent that public insurance crowds out private insurance is best defined with the crowdout parameter $r = -db^p/db$ in this setting. This crowdout parameter is useful for defining the second parameter, $\varepsilon_{1-e,B} = \varepsilon_{1-e,b}/(1-r)$, the unemployment elasticity with respect to total benefits, B.Together these parameters simplify the formula for the welfare change from raising b[11]:

[11]Proof in **?**

$$\frac{dW}{db} = (1-e)(1-r)u'(c_H)\left[\frac{u'(c_L) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1+b^p/b}{1-r}\right] \tag{9}$$

Equation 9 is analogous to the Baily-Chetty formula for optimal UI, but with private insurance included. The first term measures the marginal value of insurance for smoothing consumption across states through the gap in marginal utilities across the two states. If zero, the user perfectly smooths utility across states. The second term measures the behavioral response to private and public insurance, summarizing the cost of insurance. In contrast to the Baily-Chetty formula, private insurance increases the cost of public insurance through two channels. First, crowdout scales up the elasticity $\varepsilon_{1-e,b}$ to measure the elasticity of labor with respect to total benefits B. Second, the elasticity is scaled up by b^p/b, which captures the necessary decrease in b to reach the optimal level of B.

I follow **?** and convert this welfare function to a money metric by dividing the welfare gain from a \$1 increase in b to the welfare gain from increasing z_H earnings by a \$1:

$$\begin{aligned} G(b) &= \frac{dW}{db}\frac{1}{1-e} / \frac{dW}{dz_H}\frac{1}{e} \\ &= (1-r)\left[\frac{u'(c_L-) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1+b_p/b}{1-r}\right] \end{aligned} \tag{10}$$

Equation 10 allows me to tractably measure the welfare change associated with increases and decreases in unemployment insurance benefits caused by delays during the pandemic.

5.1 Calculating Welfare Loss

Equation 10 shows the sufficient statistics and parameters needed to estimate the value of the change in welfare associated with raising benefits. Given that the current crowdout estimates are essentially negligible, the welfare loss remains unchanged whether included or not. Instead of showing the results of these calculations, I explain where I will pull the estimates for these results.

For the crowdout parameters r and $1 + b_p/b$, I use my own calculations. I measure r using the crowdout estimates in table 1 of subsection 4.3 divided by \$2400 as the monthly statutory increase in UI, which means $r \approx 0.00692$. Next, I calculate $1 + b^p/b$ using the ratio of total private and public unemployment insurance, $b^p/(b + b^p)$ presented in subsection 4.2. Specifically, I restrict my estimate of $b^p/(b+b^p)$ to those users to those users who are unemployed in 2019 and receiving UI as the pre-increased UI benefit level, with a value of approximately 0.22. Then I note that $1 - b^p/(b+b^p) = b/(b^p+b) \approx 0.78$, which I invert to get $1 + b^p/b \approx 1.28$.

For the other statistics and parameters of the model, I follow **?** for each. The change in utility, is best approximated by the expression $\frac{u'(c_l)-u'(c_H)}{u'(c_H)} = \frac{c_L}{c_H}^{\gamma} - 1$, which γ is the coefficient of relative risk aversion per work and c_e/c_u is the ratio of consumption while employed to unemployed per work. Next, I use the share employed in April 2020 to get $e = 0.85$ from **?**. Last, I use two estimates of the labor elasticity of unemployment benefits, which are 0.33 in a baseline model and 0.01 in a model that incorporates increased job search costs (**?**). Together, these parameters yield the formulae in equation **??**.

$$\begin{aligned}
e &= 0.85 \text{ from Current Employment Statistics in } \textbf{?} \\
r &= -\frac{db_p}{db} = 0.04 \text{ from calculations above} \\
1 + \frac{b_p}{b} &= 1.02 \text{ from calculations above} \\
\frac{c_e}{c_u} &= 1/0.92 \text{ from } \textbf{?} \\
\gamma &\in \{2\} \text{ from } \textbf{?} \\
\varepsilon_{1-e,b} &\in \{0.07, 0.05\} \text{ from } \textbf{?}
\end{aligned}$$

Table **??** presents estimations of $G(b)$ without private insurance and with private insurance, with a variety of values of γ, the CRRA.[12] These results suggest that increasing b by one dollar from 2019 current levels led to between 34 cents less welfare, which was reduced to 37 cents using my largest crowdout estimate. During the pandemic the crowdout welfare measure is also indistinguishable from the standard model welfare effect, which is positive. The results suggests the size of the informal market would need to be $b_p/b = 1.12$ to get a negative welfare sign from raising benefits during the pandemic, which is well outside the range that I estimate or can be found in the Survey of Income and Program Participation.

[12] I am currently reworking with values of the elasticity with P2P considered to be earnings.

Context	ε	e	r	b_p/b	Standard	With crowd-out
Pandemic	.07	.85	-.008	.01	.10	.10
Pandemic	.07	.85	.04	.01	.10	.09
Pre-pandemic	.5	.95	-.008	.06	-.34	-.36
Pre-Pandemic	.5	.95	.04	.06	-.34	-.37

Table 2. Money metric welfare effects of UI with and without crowdout. Elasticities from **?**. Employment share from **?** and CPS. Consumption change (8%) taken from **?** and CRRA $\gamma = 2$ from **?**.

6 Robustness

There are a variety of potential pitfalls in this paper. The main issues have to do with whether I have properly identified insurance with my signature of P2P. In this section, I address these pitfalls in turn.

First, what if the unemployment events I detect are users quitting their jobs to start a job that paid via a P2P platform. In that case, I would obviously see P2P increases after the job loss. Given that my results persist for those who receive UI at some point during their unemployment spell in Figures 7 and 8, this seems unlikely. Similarly, Figure 11 suggests that the P2P receipt is greater than that from P2P. Furthermore the decline in outflows shown in each of the unemployment graphs suggests these job loss events are actual unemployment periods. Still, as a robustness check, I could look at other adverse events like health shocks, bank balances hit zero, or Earnin users spend unexpectedly large amounts of money similar to **?**. These results are forthcoming.

What if I am identifying temporary layoffs or holidays instead of job losses because I allow too short a window to identify a job loss of at minimum 21 days for weekly workers or 28 days for biweekly workers? In that case P2P use my increase if workers use the time off to go out with friends more often. In Figure 14, I present the same event studies interacting the relative time dummies with an indicator for whether the unemployment spell lasted more or less than six weeks. These results indicate that the short-term unemployed only have a drop off in P2P inflows, while those who stay unemployed for longer see in an uptick of \$20-\$30 per month that lasts for four months. Meanwhile, outflows for both drop off, but mostly recover for the short-term unemployed within two months.

Second, perhaps I have not properly measured informal insurance with P2P and instead measured exchanges due to people going out for meals. To that end, I reformed my P2P series to only focus on memos that are easily divisible amounts and above \$25 – these are less likely to be exchanges for food and much less likely to be purchases of goods and services. These results yield qualitatively and quantitatively similar event study plots and are available upon request.

Third, my event studies rely on staggered event timing where treatment is being out of a job, which is itself a (hopefully) short-term treatment that declines over time. As a result, I use early job losers (job losers in 2019) as controls for later job losers. If P2P support increases in the long-run after early job losses, then this could bias short-run treatment effects to zero. Alternatively, if P2P support falls in the long-run, meaning job losers are less likely to spend on P2P, then this could bias short-run estimates up. In a future iteration of this paper, I will present twoway fixed effect event study bias correction presented in **?**.

6.1 Calibration if miscategorizing P2P as informal insurance

One possible issue with the preceding analysis is that I may miscategorizing some P2P payments as informal insurance when in reality these are earnings under the table or the product of selling assets. In this section, I present conditions that determine whether miscategorization of informal earnings[13] leads to a lower or upper bound bound on the optimal benefit level in the presence of private informal insurance.

The intuition of these conditions is as follows. Informal work reduces leisure and asset sales reduce future consumption, such that both produce less welfare than gifts or interest-free loans from friends. As a result, decreasing benefits crowds in these lower welfare activities and the welfare-maximizing benefit level would be higher. In fact, if all P2P is informal earnings, then the traditional Baily-Chetty formula applies, which yields a higher benefit level than the formula with private insurance (**?**).

As a more formal "proof," consider the Baily-Chetty formula for welfare optimization with a single insurer (the government), dW^{BC}/db, which is adapted from **?**:

[13] Miscategorization of receiving P2P payments for selling assets requires a dynamic model, which is available upon request. I present a written argument instead.

$$\frac{dW^{BC}}{db} = (1 - e^{BC})u'(c_H)\left[\frac{u'(c_L) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e^{BC}}\right] \tag{11}$$

The relationship between dW/db and dW^{BC}/db captures the extent that raising the benefit b changes welfare. To determine which welfare change is larger, first I need to determine how the threshold n^* ability-level changes from the case with private insurance and the corresponding share e with that ability level. Consider the relationship presented in equation 5, but now the maximum and minimum earnings are shifted by the amount of informal P2P transfers. For example, the low earnings level, so workers spend $(z_L + b^p)/n$ working at the low end and $(z_H - \tau_p)/n$ at the high end. The low earnings expression captures that b^p are informal earnings earned at a rate of ability n. The higher earnings are reduced by P2P transfers out, which makes the algebra somewhat easier below, but is somewhat more difficult to rationalize. **?** describe a relationship similar to this one as a firm that redistributes earnings to reduce inequality while preserving the mean. Plugging these new earnings levels into their respective $h()$ on the RHS of equation 5 yields a new optimal threshold for n, which I call n^{BC}. As these new earnings levels are now closer together by $\tau_p + b^p$, the new optimal $n^{BC} \geq n^*$, i.e. when earnings are raised at the low end and reduced at the high end, the marginal ability level shifts up. Intuitively, mid-level ability workers can swork less for a smaller drop in earnings, so shift their hours down.

Given that $n^{BC} \leq n^*$, it follows from the CDF, $F(n)$, that the share of high earners, $e^{BC} \geq e^*$ is larger. With these inequalities in mind, I can assess and sign the relationship between dW/db and dW^{BC}/db.

$$\begin{gathered}
\frac{dW}{db} - \frac{dW^{BC}}{db} \leq 0 \\
(1-e)(1-r)u'(c_H)\left[\frac{u'(c_L) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1+b^p/b}{1-r}\right] \leq (1-e^{BC})u'(c_H)\left[\frac{u'(c_L) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e^{BC}}\right] \\
\underbrace{\frac{\frac{u'(c_L)-u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1+b^p/b}{1-r}}{\frac{u'(c_L)-u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e^{BC}}}}_{\leq 1} \leq \underbrace{\frac{1}{1-r}}_{\geq 1}\underbrace{\frac{1-e^{BC}}{1-e}}_{\leq 1}
\end{gathered} \tag{12}$$

Note at the optimum where $dW/db = 0$ in both setups, the RHS is undefined as the denominator and numerator equal zero. The inequalities reflect that $e^{BC} < e$ and $1-r \leq 1$ such that $\epsilon_{1-e,b}/e^{BC} \leq \epsilon_{1-e,b}/e\,(1 + b^p(b)/b)\,/\,(1-r)$ assuming a constant elasticity with respect to benefits. Given that consumption across the two earnings levels is equal in both settings, also by assumption, this means the gap in marginal utilities is equal and the LHS of the final inequality has a numerator smaller numerator than denominator.

The above expression gives bounds under which the standard Baily-Chetty change in welfare with respect to benefits exceeds that in the case of private insurance. I can measure all terms but the utility gap, which is best represented by the coefficient of relative risk aversion, allowing me to assess the degree that I underestimate the welfare change and as such underestimate the optimal benefit level.

The results from this calibration exercise are forthcoming.

7 Conclusion

In this paper, I presented results about the relationship between P2P inflows, outflows, and gig earnings, and job loss and how it varies based on receipt of public, formal UI. These results show that short-run P2P inflow increases during unemployment before dropping in the long-run, suggestive of an informal insurance role. Next, I show that these P2P increases are larger when recipients are slower to receive UI, indicative of a crowdout relationship between the two. Finally, I present a way to estimate the optimal benefit level based work in **?** along with a calibration exercise in the case that I have miscategorized excess P2P as informal insurance instead of informal earnings.

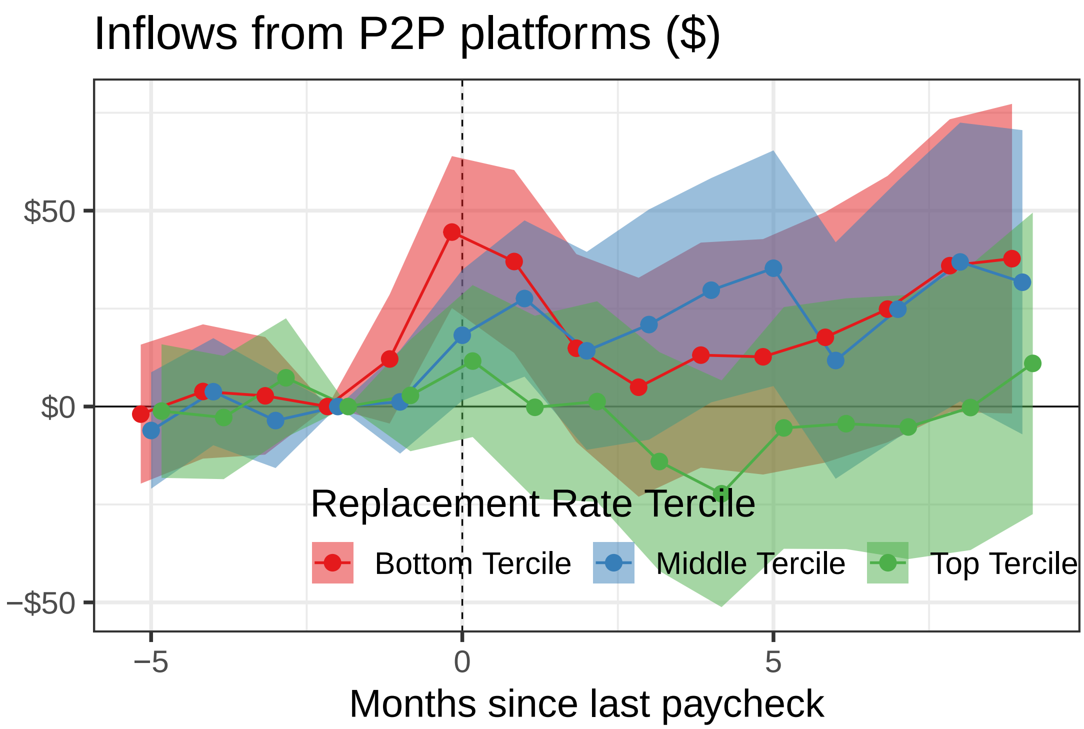
In future iterations of this paper, I will add a handful of extensions including calculations of the optimal UI benefits if P2P captures is categorized as informal earnings as described in section 5.1 as well as the calibration from section 6.1. Additionally, I will add heterogeneity analysis by the risk and discount preferences, and expectations gathered in the linked survey instrument once validated. Furthermore, I will include the robustness checks presented in section 6.

Altogether, I document a crowdin relationship between P2P and formal unemployment insurance that suggests a previously undocumented relationship between informal, private and formal public UI. The next step requires that I develop a more robust estimation strategy of the relationship between P2P and UI increases during the pandemic.

A Appendices

A.1 Figures

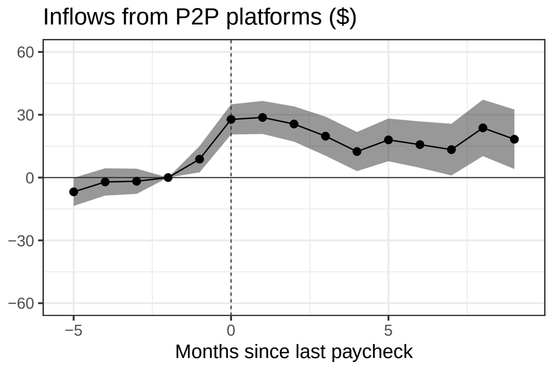
Figure 12. P2P Inflows by Replacement Rate Tercile



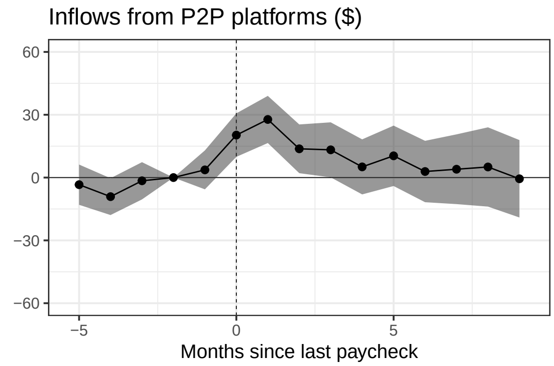
Notes: Within-person event study coefficients are interacted with tercile of user pre-job loss earnings replacement rate. Sample restricted to users with a single job loss and and excluding users in states that do not have easily identifiable UI deposit memos. Standard error's clustered at the user-level.

Figure 13. P2P Inflows by Above Median State Average Replacement Rate and Year of Job Loss

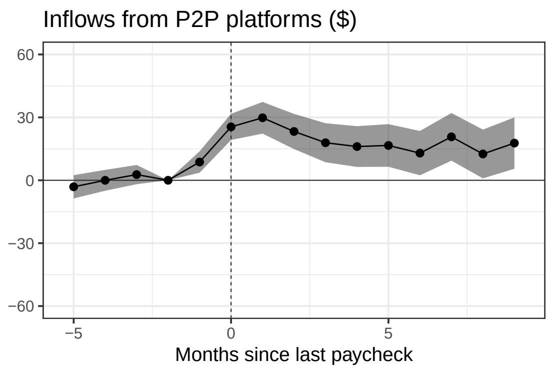
(a) 2019, Below Median



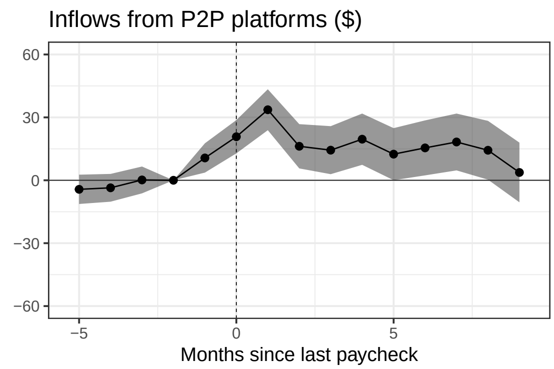
(b) 2019, Above Median



(c) 2020, Below Median

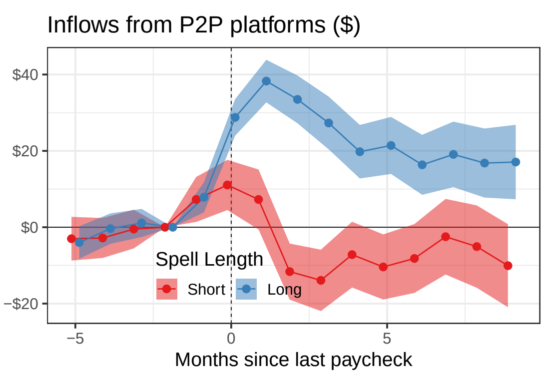


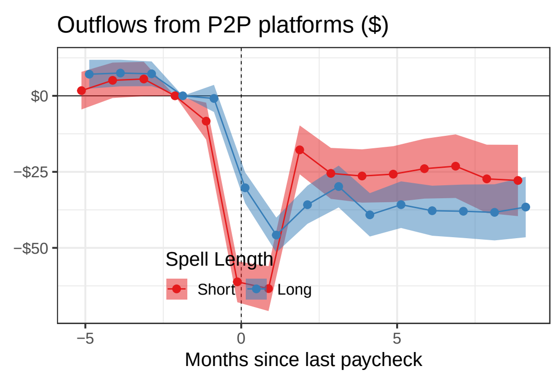
(d) 2020, Above Median



Notes: Each figure shows event study of P2P inflows with coefficients of the relative months since job loss interacted with one level from each of two indicators: (1) whether above or below the median replacement rate for a state and (2) the year of job loss. Replacement rates pulled from Department of Labor's calculations. Sample restricted to users with a single job loss. Standard errors clustered at user-level.

Figure 14. P2P inflows & Outflows by Length of Unemployment Spell





Notes: P2P inflows and outflows broken out by whether the unemployment spell lasted six weeks or less, or over six weeks.

B Appendix: Data Construction

B.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.

B.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 based on the sign-up date, gender estimated by first name, and confidence 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".

B.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 first because unemployment benefits are associated with the state of employment instead of residence.

B.4 Defining Panel, Sample Restrictions

B.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 refine this sample further based on transaction memos, state, and earnings tracking.

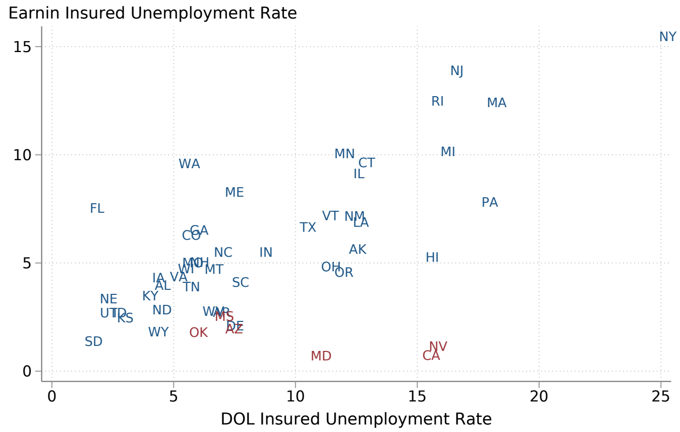
B.4.2 Uninformative Transaction Memos

For each proxy ID, we count the number of memos that do not offer 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.

B.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 figures. 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.

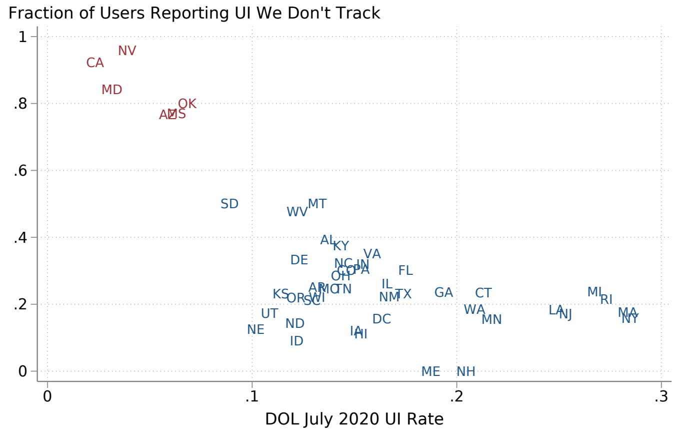
Figure 15. Insured Unemployment Rate Comparison



Notes: The figure above compares the insured unemployment rate from Earnin with the same from the Department of Labor for April 30, 2021, defined as the fraction of the labor force unemployed and receiving unemployment benefits. The states colored in red are those that we exclude from our analyses due to an inability to track unemployment benefits via direct deposit. These estimates are based on the Earnin users from all states with transactions from January 2020 through August 6, 2021.

Figure 10 allows us to leverage our 2020 survey in which we asked respondents to report the amount of benefits they received in July of 2020. In this figure, the lack of coverage of UI receipt is clear, with those six states having over 70% false negative UI receipt tracking, defined 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 16. UI False Negative UI Rate



Notes: The figure 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 define 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 benefits 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 benefits 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 benefits 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 benefits in June but subsequently restarted those benefits 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.

B.5 Identifying UI Payments

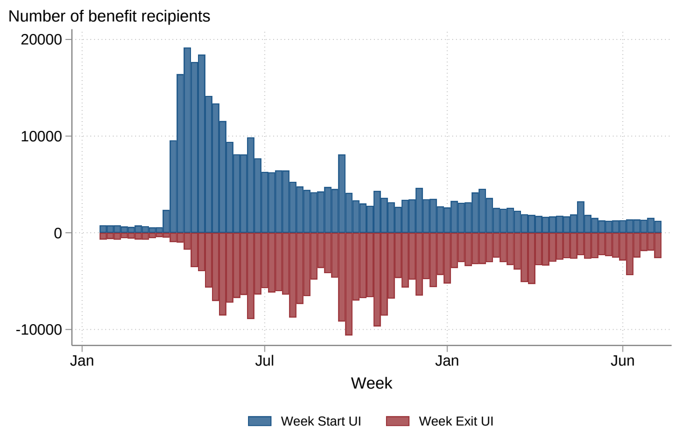
We identify those UI payments that are paid through direct deposit based on their memos. Earnin maintains a list of transaction memos that indicate that an inflow is a UI payment, and we supplement this list with other memos that we identify as attached to UI payments.

We define an individual as a *UI recipient* in week t if they received any UI benefits in weeks t

through $t + 2$.

The figure 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 **?**.

Figure 17. Employment Rate Trend



Notes: The above figure 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.

B.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 **?**.

First, we correct for variation in Plaid categorization over time; to do so, we first remove any non-alphabetic characters from transaction memos. Then, we use our 2020 transactions data for those users who filled 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 **?** and used by **?**. 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 outflow transactions (between 30% and 40% of all outflows), a fraction in line with prior work from **?**.

Consumption at some vendors includes different 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 **?** 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 flights, 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.

B.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.

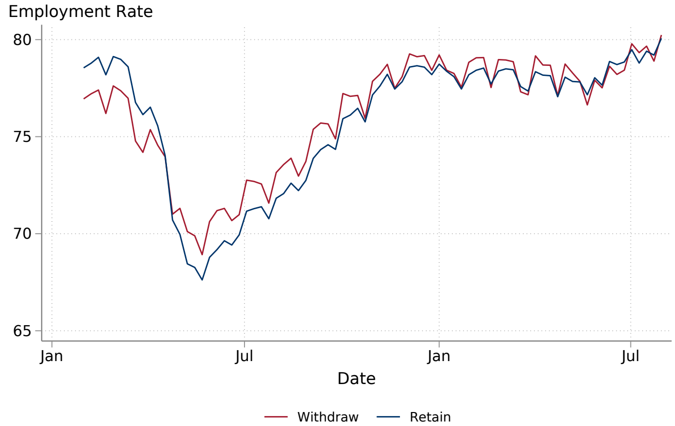
Finally, we use Plaid's categorization transactions as Payroll or Income. Upon inspection, we find Plaid's categorization of Earnings and Income to be susceptible to false positives. To account for this, we require the memo to occur in more than two unique weeks and with a modal frequency once every 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 18. Employment Rate Trend

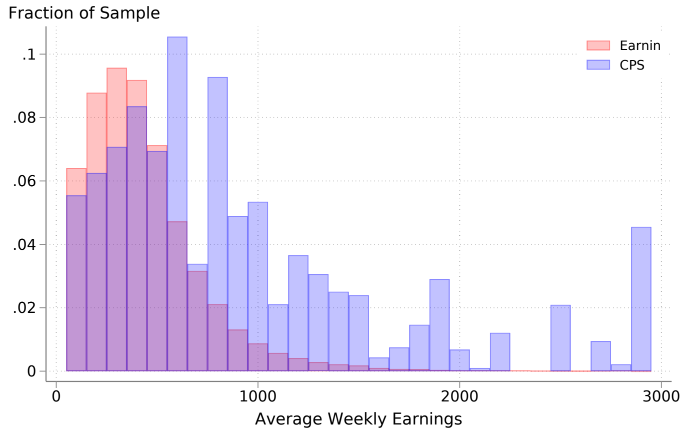


Notes: The above figure plots the rate of employment for our Earnin sample through 2020 and 2021. These estimates are based on the Earnin users from our analysis states with transactions from January 2020 through August 6, 2021.

B.8 Final Sample for Analysis of June UI Withdrawals

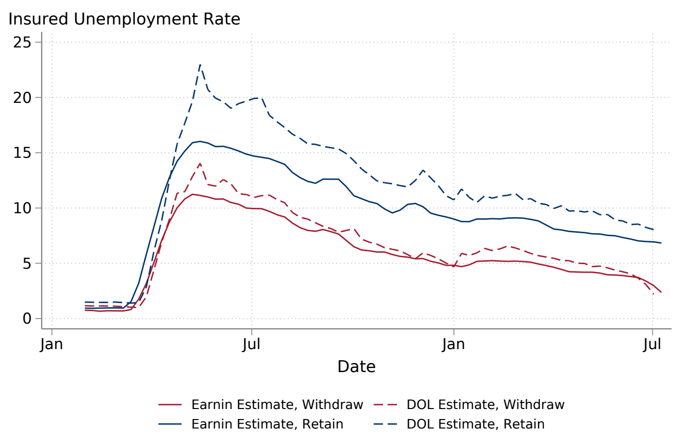
We additionally compare the characteristics of our unemployed population to those in the Current Population Survey. Specifically, we compare the pre-pandemic earnings distribution of those who were unemployed in January and February of 2021; as expected, our Earnin sample has lower earnings than the 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 19. Earnings Distributions



Notes: The above figure 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 20. Insured Unemployment Rate Trends



Notes: The above lines plot the insured unemployment rates for states that retained additional federal benefits 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 3. 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 Pacific 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.