Crowding out crowd support?
Substitution between formal and informal insurance*

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Abstract

Gifts and loans from friends and family play a largely unstudied informal insurance role in high-income countries, making it difficult to assess their implications for social insurance policy. I present new results on informal insurance paid via person-to-person (P2P) payment platforms using a survey-linked administrative bank transaction dataset covering 130,502 low-income users from the US who were unemployed at least once between July 2019 and September 2020. Event study estimates show average monthly inflows from all P2P platforms increase by \$30, or 2% of lost earnings, one month after job loss before returning to baseline over 10 months. Single mothers and the long-term unemployed receive the largest increases, as do those living in high-income areas. I exploit three plausibly exogenous changes to federal pandemic unemployment insurance (UI) policy to estimate that UI benefits crowd out at most \$0.04 of informal P2P transfers. Using the social insurance framework introduced in Chetty and Saez (2010), my crowd-out estimates indicate negligible welfare consequences for an additional dollar of benefits. Altogether these results imply that public UI benefits can raise welfare by pooling risk across networks without reducing within-network targeting of informal insurance.

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1 Introduction

During times of economic strife people rely on financial support from family and friends as a form of informal insurance. Public insurance can further distribute assistance across these networks, but might cause friends and family to reduce their support. This tradeoff between pooling risk across networks and crowding out within-network informal insurance has welfare implications for optimal unemployment, health, and other forms of insurance (Chetty and Saez, 2010). While there is a wealth of literature studying informal insurance in low-income countries (Townsend (2016), Chiappori et al. (2014), Auriol et al. (2020), Angelucci and De Giorgi (2009)), it is unclear whether the same patterns hold in high-income countries. Measurement of informal insurance in high-income countries has eluded researchers because this form of transfer has historically been made via cash, which is prohibitively expensive to track sufficiently for welfare analysis.

This paper uses new survey-linked, de-identified bank transaction-level data to estimate informal insurance paid via person-to-person (P2P) platforms around job loss events in 2019 and 2020. I also exploit three plausibly exogenous changes to unemployment insurance (UI) benefits during the pandemic to estimate crowd-out of informal insurance. The data come from Earnin, a financial services company that provides pre-dominantly low-income and liquidity-constrained users with products including access to earnings before payday. This rich dataset provides a unique window into who receives the most informal support. I impute family composition from user level-information such as pandemic stimulus payment amounts to document targeting of informal insurance. Furthermore, I use local economic data from the American Community Survey and Social Capital Atlas (Chetty et al., 2022) to explore how access to high income networks affects transfers. Finally, I use my estimates to calculate the welfare implications of crowd-out using the sufficient statistics framework provided in Chetty and Saez (2010).

My empirical analysis yields four key findings, which taken together suggest that informal insurance varies based on demographic and local economic characteristics, but not in response to public UI policy. First, monthly P2P inflows increase up to a peak of $30 on average in the first month after job loss or about 2% of pre-job loss earnings before returning to zero over 10 months. Second, single mothers, the long-term unemployed, and those living in high income areas receive the most informal transfers, suggesting limited targeting due to within-network resource constraints. Third, I show that average monthly consumption falls \$40-\$70 less after job loss for people that actively use P2P platforms prior to job loss, suggesting that informal transfers are largely used to smooth consumption. Fourth, across three pandemic-induced, plausibly exogenous changes to public UI, an additional dollar of public benefits crowds out at most \$0.04 of informal transfers, suggesting that UI is not holding back insurance within informal networks. In short, these facts suggest that within-network informal insurance varies based on local resources, and that public UI can increase welfare by redistributing support across networks without excessively crowding it out.

These results are useful to policymakers for two reasons. They suggest that first, policymakers should document network- or geography-specific changes in consumption to better insure across networks, and that second, policymakers can focus on measuring what is comparatively easy to measure — consumption and labor supply responses — to assess the value of UI, avoiding the collection of highly-detailed and sensitive information on P2P transactions among UI recipients.

To calculate a crowd-out elasticity for my welfare analysis, I scale my reduced form crowd-out estimates by the ratio of average public and informal assistance. With an elasticity, I can extrapolate to percent changes in total informal insurance from the observed shifts in the level of P2P inflows, which include a subset of total insurance as well as other forms of payments. This extrapolation holds to the extent that payments by cash, check, or other instruments respond similarly to shifts in public UI benefits. Given that payment instrument choices are primarily driven by fixed technology adoption and transaction costs, such extrapolation is likely to hold locally.

In addition to extrapolating with an elasticity, I use a combination of empirical and data cleaning methods to isolate the informal insurance response of P2P inflows. First, I use within-person event studies around job loss to track changes in P2P inflows relative to their level two months prior to job loss, which differences out P2P use related to splitting household bills, restaurant checks with friends, and payments to businesses. Second, I omit bank transactions with memos that mention sales, gig work, or taxes to eliminate memos that are clearly linked to non-insurance uses.[1] Third, I assess the extent that P2P inflows capture an informal labor response by comparing them to earnings on gig work platforms, an alternative informal labor channel. Event studies of gig

[1]Results are robust to keeping these transactions.

earnings imply that at most $10 of the post-job loss increase in P2P is an informal labor supply response. Taken together, these three steps isolate informal insurance paid via P2P.

Having measured the informal insurance role of P2P, I ask whether public UI crowds out informal insurance. UI benefits are likely endogenous to informal support, which biases the causal identification of a crowd-out relationship. For example, an unemployed worker who receives large informal transfers from friends might delay applying to or opt out of UI benefits, which would bias up the estimated degree that UI crowds out informal insurance. Alternatively, high earners may have high-income friends, receiving both large informal transfers and higher statutory UI benefits, which would bias down crowd-out estimates.

I address these opposing sources of bias by leveraging three sources of plausibly exogenous variation in UI benefits driven by the COVID-19 pandemic. I use this variation to motivate an instrumental variable difference-in-differences (IV-DID) approach laid out by Ganong et al. (2022). First, I compare the P2P inflows of March 2020 job losers who receive UI benefits in April 2020 to the P2P inflows of those who receive benefits in June 2020. Second, I compare P2P inflows of the same March job losers to a group of employed users before and after the July 31, 2020 expiration of $600 per week in UI benefits. Third, I compare unemployed workers in 19 states that withdrew from federally-expanded UI benefits in June 2021 to unemployed workers with the same unemployment duration in states that retained these benefits through September 2021. The first natural experiment exploits that early pandemic UI delays were primarily driven by an overload of applications instead of application timing decisions by job losers. The second two experiments exploit government-initiated changes in benefits that are independent of user characteristics like pre-job loss earnings or network support. In each natural experiment, I compare the relevant treated and control groups in the month before and after each policy event, regressing P2P inflows on UI inflows instrumented with an indicator for being in the relevant treated group during the month after the policy change. Across these policy experiments, I find that a $1 increase in UI benefits causes small but precisely estimated changes in P2P inflows ranging from -$0.04 to $0.008, the latter of which suggesting negligible crowd-in.

My estimate of informal crowd-out is the first measured outside a lab for a high-income country (Lin et al., 2014). The lack of crowd-out estimates persists despite Chetty and Saez (2010) showing that crowd-out of informal insurance can have large consequences for the welfare of public insurance. Chetty and Saez (2010) extend the canonical Baily-Chetty formula (Baily (1978) and Chetty (2006)), which calculates the optimal replacement rate of lost earnings to balance the consumption smoothing role of insurance with an unintended reduction in labor supply. The extension addresses that increases to public insurance are offset to the degree that they crowd out private insurance. Unlike crowd-out, the other components of the Baily-Chetty formula — the consumption drop at job loss (Gruber (1997), Ganong and Noel (2019), Ganong et al. (2022)) and re-employment at benefit exhaustion (Meyer (1990), Card et al. (2007), Ganong and Noel (2019)) — have proven easier to measure. Most welfare analyses use these components and assume crowd-out effects are second order, and thus negligible, because workers have already optimized for the level of private insurance. Chetty and Saez (2010) show that this assumption only holds if private insurance does not exhibit moral hazard on labor supply, i.e. through perfect monitoring of effort.

Despite this assumption, evidence from low-income countries suggests moral hazard persists in informal insurance arrangements.[2] Such arrangements in small village economies function similarly to my findings: families and friends use informal credit, gifts, and charity to partially income pool within social networks (Townsend (2016), Chiappori et al. (2014), Auriol et al. (2020), Angelucci and De Giorgi (2009)). Theoretical work rationalizes partial income pooling as the product of limited commitment to reduce moral hazard (Coate and Ravallion (1993), Ligon et al. (2002)), which Bloch et al. (2008) show requires a very particular "sparse" network structure to guarantee stability, suggesting moral hazard persists in informal insurance arrangements. Furthermore, experimental evidence finds that informal networks in Côte d'Ivoire produce moral hazard via a "social tax" paid to friends and family out of earnings (Carranza et al., 2021).

Given informal insurance likely creates moral hazard, how does it interact with formal insurance in the developing world? In small village economies, informal insurance crowds out take-up of formal index insurance, unless it covers basis risk that index insurance does not pay out. Such complementary coverage can lead to crowd-in of formal insurance (Mobarak and Rosenzweig (2012), Mobarak and Rosenzweig (2013)). Evidence on the effect of formal insurance on informal insurance

[2] There is sparse evidence on moral hazard of informal insurance in high-income countries because informal insurance is poorly measured.

is similarly ambiguous. Geng et al. (2018) shows that households in Kenya with public health insurance do not receive less informal support after health shocks. In contrast, crowd-out effects are quite large in laboratory settings (Hample (2021), Lin et al. (2014)). My results further suggest that outside the lab, informal support is minimally crowded out by formal insurance options.

My paper also adds to a growing literature documenting that informal insurance networks expand on P2P platforms because they have a lower fixed cost per transfer than cash or checks. The rise of the P2P platform M-PESA in Kenya has driven increases to the number of members and transfers in informal insurance networks (Jack and Suri, 2014). Furthermore, Balyuk and Williams (2021) document that Zelle users in the US with more Zelle users in their social networks manage periods of financial instability more easily than other Zelle and non-Zelle users. I advance this literature by not only documenting the informal insurance role of multiple P2P platforms, but also using it to identify crowd-out, a parameter relevant to policy.

This paper also builds on a literature that uses large, de-identified administrative government or individual bank transactions data to precisely measure behavior during unemployment (Ganong and Noel (2019), Farrell et al. (2020), Bell et al. (2022), Johnston and Mas (2018), Card et al. (2015)). Ganong et al. (2022) use rich, transactions-level data from JP Morgan Chase checking account holders to measure delays in UI receipt, documenting another advantage in bank transaction data used in this paper. Coombs et al. (2021) 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, while labor supply responds modestly to benefit exhaustion, but neither assesses the role of informal transfers as part of income.

The rest of this paper proceeds as follows. Section 2 presents the policy context of the pandemic. Section 3 describes the Earnin dataset and my analysis sample. Section 4 presents the two-way fixed effects event study methodology used to estimate informal insurance responses after unemployment. In section 5, I present IV-DID results estimating the extent that public UI benefits crowd out P2P informal insurance. Section 6 outlines the Chetty and Saez (2010) model of public and private insurance, and introduce a test of how miscategorizing P2P would affect my estimates. Section 7 presents robustness checks of my results. In section 8, I provide concluding thoughts.

2 The policy setting of pandemic unemployment assistance

During the pandemic, Congress passed three spending bills aimed at bolstering the economy and supporting unemployed and underemployed workers through the pandemic, the CARES Act on March 27, 2020, the Consolidated Appropriations Act (CAA) on December 27, 2020, and the American Rescue Plan (ARP) on March 11, 2021. Each included an untargeted cash payment, scaled by the number of adults and dependents claimed on the previous year's tax returns. Each bill included hundreds of billions of dollars in support for large corporations and small businesses, for example, the forgivable small business loans under the CARES Act's Paycheck Protection Program. Last, the ARP introduced monthly payments of the Child Tax Credit (CTC), which lasted for six months of 2021.

Also introduced under the CARES Act were the Pandemic Unemployment Assistance (PUA) program, which extended coverage to self-employed and gig workers, the Pandemic Emergency Unemployment Compensation (PEUC) program, which expanded coverage beyond the traditional six-month expiration window to 99 weeks, and the Federal Pandemic Unemployment Compensation program (FPUC), which provided an additional $600 per week to all those receiving unemployment benefits. The additional $600 per week started arriving in unemployed workers' bank accounts in early April, though high application volumes led to delays, before expiring on July 26, 2020. The Lost Wage Assistance program provided an additional $400 per week in additional benefits, but lasted only four weeks between August and September 2020 in most states.[3] The CAA restarted an additional $300 per week in January 2021, which the Biden administration extended through September 6, 2021 with the ARP. In this paper, I will leverage plausibly exogenous variation to UI benefits driven by these delays, expirations, and withdrawals during the pandemic to estimate how UI benefits crowd-out informal insurance.

I focus on these large changes to the UI benefit schedule because PUA and PEUC were in place for most of my sample period and prevent more traditional exogenous shifters of UI benefits like timing out of benefits or ineligibility. To assess informal insurance responses to these more common reasons workers lose UI benefits, one would need to study a sample period that is not covered by

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

my dataset, but that will be in the years following the pandemic.

In addition to variation in UI benefits, I also use these spending bills to impute information about family composition. Under the CARES Act, a household received $1200 for each adult and $500 for each child. Under the CTC, households received $300 per child under six and $250 per child between the ages of six and 17. Together these payments allow me to impute whether a user in my dataset is single, in a couple, and either a single or coupled parent. This information is used to look for evidence of targeting in informal insurance networks.

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 Coombs et al. (2021). 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 access to their income before payday. Through this connection, Earnin maintains a database containing tags with information about each user, transactions-level data, balance data, and observed earnings data. The bank transactions contain memos and categories provided by the financial services company Plaid.[4] I categorize transactions into P2P inflows and outflows, spending, earnings, and UI inflows using user-specific information from Earnin, bank memos, and Plaid categories. Each of the datasets contains user tags, which allow me to construct "proxy IDs" and sum all earnings, inflows, and outflows at the user-monthly level. For additional details on categorizing and summarizing the Earnin data, see appendix C. For simplicity, I will call each proxy ID unit an "individual," "worker," or "user."

In addition to having a large sample of UI recipients, these bank transaction data are linked to a large-scale survey of 24,671 users conducted in August 2020 as part of work on Coombs et al. (2021). This survey links financial outcomes to welfare and policy-relevant behavioral and demographic characteristics not previously observed in conjunction with administrative bank data of this scale.

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

The survey ask questions about recent earnings, employment, UI benefits, and consumption for the month of July 2020. It also asks respondents about their expectations for each of those outcomes for September 2020. In addition to these questions, the survey gathers demographic information, and elicit risk aversion and discount rates using questions from the Global Economic Preferences Survey (Falk et al., 2016, 2018). 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.[5]

The full data include all Earnin users from December 29, 2018, to October 15, 2021, but I make a number of sample restrictions to ensure the data are representative of a user's finances and relevant to the research question. First, I require that users have at least five outflows per full month to ensure that I am analyzing the user's primary bank account. Second, I drop any users for whom more than one percent of their bank memos are "uninformative," because they just contain the word "CREDIT," "DEBIT," "TRANSACTION," "TRANSFER," or a list of numbers and symbols, or are altogether missing. Uninformative memos reduce the reliability of tracking and categorizing types of transactions. Third, I require users to have at least five outflows in each month between their first and last observed month, showing that their bank account is regularly used. Fourth, I restrict my sample to users who are continuously employed for at least six months and experience their first job loss at some point between July 2019 and September 2020 — my "treated" group, or after September 2021 with no prior insurance — my "not-yet-unemployed" group.[6] The "not-yet-unemployed" provide a clean control group for my two-way fixed effects event study estimates, which reduces possible bias from comparing later-treated group to early-treated groups in a setting with staggered treatment timing and heterogeneous treatment effects. Fifth, the sample is restricted to users with transactions for at least six months prior to and 10 months after their first job loss[7] balance my analysis sample in relative time for the event studies. Altogether,

[5]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.

[6]I also omit months August through October 2021 to ensure the "not-yet-unemployed" do not have any anticipation treatment effects, which seem to start about a month prior to job loss. The total sample relative to the analysis sample can be found table A2.

[7]I except September 2021 job losers from this requirement because their job loss is at the end of the observed panel.

my restrictions leave me with 130,502 “treated” job losers and 4,245 “not-yet-unemployed” users covering calendar months January 2019 through July 2021.

To measure crowd-out of informal UI by public UI, I look at each user’s UI inflows as flagged from regular expression searches of their bank transaction memos. I track UI inflows by searching for 200+ different regular expressions within these memos. Prior to the restrictions above, 175,000 of one million users observed in July 2020 received UI payments, and there are roughly 1,000 UI recipients in the median state. These 175,000 users cover 0.7% of the 30 million UI recipients nationwide, with coverage reaching between one and two percent in states where UI benefits are more commonly dispensed through direct deposit as shown in figure A.1 for state-by-state coverage of UI recipients.

UI transaction memos are not easy to flag in a handful of states that do not direct deposit their UI benefits or with deposit memos do not have clean regular expressions. Figure A.4 shows a scatterplot of the share of users who reported having UI benefits that I do not observe, i.e. the false positive rate by state against the insured rate as reported by the Department of Labor in July 2020. Highlighted in maroon are California, Nevada, Maryland, Arizona, Mississippi, and Oklahoma, which have both a high false positive rate and a relatively low UI rate. I drop these states from all analysis that involves directly observed UI inflows to reduce measurement error leaving a sample of 108,181 users, 51,850 of whom receive UI.[8]

While restrictions based on account activity, unemployment, and UI flagging minimize noise in the sample, Earnin users are themselves unrepresentative of the general population. They are primarily low-wage and liquidity-constrained workers (Chetty et al., 2020). One key advantage of this dataset is that it appears to be more representative than other datasets of workers affected by the economic disruptions of the pandemic. Ganong et al. (2020a) use Current Population Survey data to show that mean pre-job loss earnings were $886. The distributions in Appendix Figure C.47 are close to this national benchmark suggesting the Earnin data might be more representative of the workers most likely to become unemployed during the pandemic.

In addition to the benefit of oversampling from a group affected by the pandemic, this dataset represents people who opted to use Earnin, a digital payment technology. As a result, I am likely

[8] Table A3 shows the analysis sample counts of UI recipients in good and bad states.

to capture more of their financial activity, and specifically, informal insurance payments via P2P payment platforms like Venmo, Zelle, Cashapp, or PayPal than for the median worker in the US.

3.1 Flagging earnings and UI inflows and creating unemployment and insurance spells

The event studies in this paper track P2P inflows, outflows, and overall spending around first job loss. To flag job losses, I first develop labor market histories for each user's UI inflows and earnings. While UI inflows are relatively simple to track because UI regular expressions are uniform across users, earnings tracking involves a more complicated algorithm that isolates user-specific earnings. The algorithm employs a series of "observed earnings" provided by Earnin, regular expression searches of memos for words like "PAYROLL" or "SALARY," and categories provided by Plaid that mention "PAYROLL" or "DIRECT DEPOSIT." First, the algorithm groups all transactions by user and memo. If Earnin labels a transaction as "observed earnings," then the algorithm flags all transactions within the same user-memo group as earnings. Second, if at least 90% of the transactions associated with a memo are "observed earnings," the algorithm classifies the rest of these transactions as earnings across all users. Third, the algorithm flags any regular-occurring transactions with memos or Plaid categories that correspond to paychecks as earnings. Further details can be found in appendix C.10.

Armed with UI inflows and earnings series, I define UI and unemployment spells for each worker. A UI spell starts at week t when the first UI payment is deposited in the bank account. The spell continues until 3 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 week $t+k+1$ through $t+k+3$, I define the spell to have ended at date $t+k$. An unemployment spell starts in week t of the worker's last paycheck deposit before five consecutive weeks without a deposit. It continues until the next paycheck deposit.

Outside the earnings flagging shown above, I also flag inflows for a selected sample of 20 gig platforms including Uber, Lyft, Taskrabbit, and Upwork[9] using regular expression memo searches. These isolated series allow me to track how gig work becomes an alternative form of risk management during unemployment. Additionally, I am able to benchmark changes to P2P inflows against

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

changes in gig earnings, in order to rule out that P2P inflows are just gig earnings disguised on another platform.

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

3.2 Identifying P2P

The primary data outcomes of interest here are P2P inflows and outflows, which I use as proxies for informal insurance provided during unemployment. To measure these flows, I supplement Plaid categories for Venmo, Cashapp, PayPal, and Chase QuickPay with Zelle with regular expression searches of memos,[10] which allow me to pick up additional platforms and fill in holes in the Plaid series that seem to be inconsistently applied over the sample period. For example, Figure A.5 shows that memos containing the word "ZELLE" go from being regularly categorized as "Chase QuickPay" to "Credit" after March 2021.

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 instead payments associated with consumption, refunds, or running a small business. I exclude memos that have a variety of regular expressions associated with sales of goods or services like "POINT.*OF.*SALE" or "POS DEBIT," or "OVERDRAFT." Some accountants send tax refunds and other payments via digital payment platforms, so I drop any transactions with memos mentioning "TAX." Furthermore, a greater share of P2P transaction amounts around stimulus deposit dates are equal to economic impact payments, likely due to intrahousehold splitting of stimulus checks, which are not insurance, but also due to people gifting their whole stimulus check to a friend, which is insurance. As I cannot separate these uses, I drop transactions equal to stimulus amounts around these deposit dates in April 2020, January 2021, and March 2021.[11] Similarly, I drop all those transactions with memos that mention "EARNIN," or other fintech platforms like Dave Inc., Chime, Coinbase, or Brigit, as these are likely false positives. Furthermore, I drop any memos mentioning a gig platform as some users might receive payments for Etsy or other services

[10] The full list of regular expressions is available in appendix C.13.

[11] I further find that event study point estimates do not change when omitting these months entirely from the analysis.

via PayPal. Last, I drop any transactions of less than \$5 and above \$15,000, the untaxed maximum for family gifts. Most transactions fall within this range, but this approach greatly reduces noise in the results as it effectively winsorizes the data.

After removing these transactions, I sum the remaining P2P inflows and outflows at the user-month level.[12] I plot these series in Figure 1 for both my combined series and the Plaid categories only, which shows that the Plaid categories remain stable over time with some clear holes in coverage, while my expanded series shows a clear increase over the sample period – highlighting that P2P payment platforms have been used more frequently over the sample period. Additionally, the inflows and outflows show similar spikes around the stimulus payout months of April 2020, January 2021, and March 2021, even after removing stimulus amounts, suggesting that during these periods users likely sent money out of their stimulus checks to one another.

Most Earnin users also employer P2P payment platforms, though some more actively.[13] Figure 2 shows histograms of user-months of total P2P inflows and outflows and the P2P share of total inflows or outflows summed at the user level across the sample period. About 25% and 40% of user-months have zero outflows and inflows, respectively, but less than 10% and 5% of users have \$0 of P2P inflows or outflows, respectively. Additionally, P2P outflows have a much longer right-tail than P2P inflows. This is because P2P platforms can be used to pay businesses, leading to a greater total amount of outflows per month, while P2P inflows to a person are less commonly associated with business payments. As a consequence, P2P inflows are more informative than outflows for payments between two people, making them a better proxy of informal support after job losses. Appendix B.2 presents the monthly counts of P2P transactions, and their overall shares, which tells a consistent story to the flows.

One potential threat to the external validity of my results is that P2P makes up only a subset of informal insurance. Informal insurance is only partially made-up of cash gifts from friends, which are only partially captured in P2P platforms. The rest is primarily paid out through cash and checks. That said, my sample is selected based on use of Earnin, suggesting an inclination for

[12] I also keep all P2P inflows and outflows and find the point estimates in my event study analysis are effectively unchanged by removing these transactions that are unlikely to be real informal insurance payments.

[13] I present the P2P inflows, outflows, UI inflows, earnings, gig earnings, and overall outflows and inflows in the appendix table A4.

digital payment systems over cash. Furthermore, the overall growth of P2P platforms and their role in expanding informal insurance suggests my crowd-out estimates are helpful for assessing the overall welfare consequences.

According to the 2021 Diary of Consumer Payment Choice (DCPC), the share of transactions between two people paid on P2P platforms in the US increased from 11% in 2019 to 15% in 2020, then to 29% in 2021 (Cubides and Shaun, 2022), in part because P2P platforms have a lower fixed per transaction cost than cash. These results are pulled from a sample of the entire population, while my sample is a group of digitally-inclined adopters of Earnin, who likely use P2P even more than they do cash. To benchmark use of P2P among Earnin users, I follow the methodology outlined by the Federal Reserve Bank of Atlanta. I restrict transactions to those between October 2019 and October 2020 and categorize transactions by payment instrument, including cash, check, debit, P2P platform, or other. I drop cash transactions because the DCPC reports all cash transactions, while I can only observe cash transactions related to ATMs in the Earnin data. Figure A.2 shows density plots of the within-user share of cumulative non-cash flows that are P2P transactions by dataset in both October 2019 and October 2020. The histogram shows a large mass near zero P2P transactions in the DCPC, while Earnin users have a much longer and fatter right-tail, suggesting many of them conduct more of their transactions on P2P platforms relative to the nationally-representative DCPC sample.

P2P platforms reduce the fixed cost of making transfers, which means informal insurance networks expand to include newer members who exclusively send digital payments (Jack and Suri (2014), Balyuk and Williams (2021)). As a result, a greater share of P2P transfers are likely made by "marginal" network members, while cash and check transfers are primarily made by network members. This relationship between network proximity and payment choice was even stronger during the pandemic when social distancing minimized opportunities to exchange cash. If P2P transfers are disproportionately made by "marginal" network members, they might be more likely to get crowded out by public UI benefits relative to cash payments. If P2P transfers are more responsive to public benefits, I probably overestimate the actual crowd-out of all informal insurance. Given how small my crowd-out estimates are, this would suggest the welfare consequences of crowd-out are even smaller than I report and further support the case that public UI is not holding back a superior informal insurance regime.

An alternative argument that would question the validity of my crowd-out estimates is that cash and check transfers are disproportionately given by close friends and family in person, especially during the pandemic. Friends and family may have better information on personal finances, and thus may be more likely to learn someone has received UI benefits and withhold support than someone sending money via P2P. That said, close friends and family tend to care more than marginal network members about someone who is out of work and commit to sending transfers regardless of formal UI support, counterbalancing the information effect on crowd-out. A data-driven approach to address information-driven crowd-out is out of the question, because I do not observe the sender of P2P inflows at a sufficiently high quality to estimate heterogeneity in crowd-out by relationship. Altogether, I do not expect information-driven crowd-out to dominate commitment between close-knit network members who pay via cash. This is a limitation to analyzing P2P payments only.

4 Event studies of informal insurance response and use during unemployment

I start by presenting event studies around first job loss estimated with a two-way fixed effects model. Equation 1 shows the formula for 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.

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

where y_{it} is any outcome like P2P inflows and outflows, consumption, and gig employment inflows. I omit month -2 because the last paycheck might come several weeks after someone has lost their job, with the unemployment event indexed to the next calendar month, but friends might have started sending support in advance of the last paycheck.

4.1 P2P inflows and outflows

Figure 3 shows the event studies of P2P inflows and outflows. The pretends of both are relatively flat, indicating little differential use of P2P in the months leading up to job loss, which might occur if a worker shifts to an informal career instead of losing their job. In the month prior to the last paycheck, P2P inflows increase by about \$5, before peaking around \$30 in the first month after the job loss and then returning to baseline over the next 10 months. In contrast, P2P outflows drop by \$50 by the month after job loss and never fully recover. Appendix figure A.6 shows a similar dynamic path of inflows and outflows across the major P2P platforms, albeit with different magnitudes and precision. Zelle shows the largest changes, while the Venmo, PayPal, and Cashapp inflows increases are just a few dollars.

The dynamic path of P2P inflows suggests that informal support starts prior to the last paycheck possibly because workers anticipate and try to prepare for the income loss by asking friends for support. This anticipation effect is consistent with reductions in consumption prior to job loss identified by Gruber (1997). However, increased support continues for only a short duration either because workers have found a job or insurance networks can only offer limited support.

In addition to being temporary, P2P inflows replace very little of pre-job loss earnings. Figure 4 shows event studies of "static" and "dynamic" P2P replacement rates to contextualize the magnitude of support. The "static" replacement rate in figure 4 is the share of median monthly earnings prior to job loss replaced by P2P inflows — about two percent per month. The "dynamic" replacement rate is the contemporaneous P2P share of total income, defined broadly as all inflows less internal account transfers. This definition includes P2P inflows; the replacement rate measures the change in the P2P share of income around a job loss. The share is quite low, peaking at four percent before falling to two percent in the long run. Together these graphs suggest that P2P inflows replace relatively little income during unemployment. In appendix B.4, I further explore the relationship between P2P inflows and income using instruments for the timing of job loss and public UI receipt to isolate changes in income related to downturns. These results suggest that P2P inflows are largely unresponsive to changes in income.

4.1.1 Heterogeneous support

This relatively small average increase in P2P inflows obscures underlying heterogeneity in the level of support. Figure 5 shows that after a job loss, the probability of receiving or sending any money via P2P falls because users reduce spending, and thus any shared expenses. On the other hand, the probability of having at least \$100 of P2P inflows increases by 0.75 percentage points in the month after job loss implying that a subset of users receives a large windfall of support from P2P platforms.

One possibility is that only individuals who already use P2P platforms receive any informal support on them, while the rest exclusively receive informal support through cash, checks, or in-kind gifts. If so, then much larger average increases should be seen among these individuals. Figure 6 shows coefficients from an event study in which the relative time dummies are interacted with the tercile of the median monthly share of a user's financial cumulative inflows and outflows, conducted on P2P platforms more than three months before job loss. The coefficients show pretrends because these groups are created based on differential use prior to job loss, making them somewhat uninformative. After job loss, inflows from the top tercile group peak at \$60 before falling to roughly \$40 per month. While this peak is double the unconditional average of \$30 and the continued support does not return to zero, \$60 is still a fairly low amount of monthly support relative to lost earnings, considering P2P transactions capture far more of the overall financial activity of these users. Appendix B.3 shows alternative specifications for "prior use," but none of these results show greater informal support for users who disproportionately use a large amount of P2P.

This increase in P2P is not necessarily informal insurance. Despite removing bank memos linked to informal work or consumption, the increase could correspond to increased "social spending" or informal earnings. Given the majority of job losses in my data occurred during the pandemic when people were isolating, it seems unlikely that these results are driven by increased social spending. Furthermore, average monthly P2P outflows fall after job loss, which would only be consistent with a story of increased social spending, for example, if a recently laid off worker chose to go out to eat and covered the full bill and be paid back by friends. In appendix B.1, I present event studies documenting that P2P inflows tend to be made up of larger transactions that are multiples of \$25, suggesting a compositional change toward "lumpier" payments like gifts. The path of P2P inflows differs from earnings on gig platforms in both magnitude and dynamics, suggesting that the former is not a proxy for informal earnings. Together these results are most consistent with a story that increases in P2P inflows are driven by informal support after job loss.

4.2 Heterogeneous support by demographics and economic conditions

Several heterogeneous results are most consistent with a story of informal insurance that targets the neediest, but is constrained by the available resources of their network. I explore heterogeneity with respect to user and local economic characteristics. To compare P2P use across groups, I present event studies of coefficients that are interacted with a group indicator G_{it}, as shown in equation 2. As a result, the month fixed effects are pooled across groups and the event study coefficients isolate within group variation.

$$y_{it} = \alpha_i + \lambda_t + G_{it} \times \left(\beta_{-6} \sum_{s \leq -6} D_{it}^s + \sum_{s \in [-5,-3]} \beta_s D_{it}^s + \sum_{s \in [-1,9]} \beta_s D_{it}^s + \beta_{10} \sum_{s \geq 10} D_{it}^s \right) + \varepsilon_{it} \quad (2)$$

Figure 7 summarizes the amount of support that individuals receive over the ten months after job loss by summing the 11 lagged interacted event study coefficients after relative time -2 and plotting them in a bar chart. Each of these coefficients is taken from a regression of P2P inflows on relative time dummies interacted with an indicator for being in the group of interest. The baseline total from the main event study in 1 is provided as a reference. All the associated event studies, as well as results related to worker industry and risk aversion, can be found in appendices B.7-B.11.

Figure 7 looks at heterogeneity by gender, parenthood, marital status, length of unemployment, assets at job loss, whether the user lives in a count with above-median per capita income, and measurements of social capital taken from Chetty et al. (2022). I identify parenthood and marital status by flagging the presence of monthly Child Tax Credit payments and the first Economic Impact Payment, which paid \$1200 per adult and \$500 per dependent claimed on 2019 taxes. Local economic indicators are matched on the user's zip code or county in the month prior to their job loss. Further details are available in appendices B.7 and B.8.

Informal support by gender and parenthood is considered for three reasons. First, women tend to have a harder time returning to work after job loss, especially single mothers. Second, women tend to have more friendships than men, from which they might draw greater support. Third, children had unpredictable school schedules during the pandemic making it much harder for parents to get a job with regular hours despite having a high marginal propensity to consume out of income to feed their family. The results in 7 suggest that women and parents receive far more informal support, but that these effects are overwhelmingly driven by support for single mothers who receive $500 in extra P2P after job loss. Altogether, these results suggest that informal insurance networks target those who have the hardest time finding work and greatest marginal propensity to consume.

The long-term unemployed receive even more support than single mothers. Figure 7 shows that those unemployed for longer than six weeks receive $650 more P2P inflows on average after job loss. Figure 8 shows that the long-term unemployed not only receive for longer periods, but also get more initial support. Together these dynamics suggest that insurance networks target and have accurate expectations about which workers will be unemployed longest and thus struggle the most to smooth consumption out of precautionary savings and government benefits.

As figure 7 suggests, while informal insurance networks target single mothers and the long-term unemployed, those with the least precautionary savings to self-insure after job loss also receive the least informal support. I measure the level of precautionary savings using the tercile of month-end bank balance prior to the last paycheck deposit. After job loss, the top, middle, and bottom terciles receive $100, $200, and $300, respectively, in increased P2P inflows. While the differences are not precise, they imply that those who have the most liquidity to manage unemployment receive even more liquidity through P2P platforms.

One reason wealthier people might receive support is their networks are wealthier or higher income, and can offer more support. The bars for above- and below-median per capita income from the 2019 American Community Survey shows that users living in poorer counties receive negative P2P inflows over the ten months after job loss, or essentially zero informal support. Intuitively, a worker with high-income friends or friends of friends might draw more support after a job loss or any adverse economic event, but friendship networks tend to cross county lines, making the ACS measure an imperfect proxy.

To get closer to measuring each individual's own network, I use the zip code level Social Capital Atlas provided by Chetty et al. (2022). The Atlas measures economic connectedness as the share of Facebook users in a given zip code who are friends with high socioeconomic status (SES) users in all zip codes.[14] Furthermore, Chetty et al. (2022) separate economic connectedness into Exposure,

[14]High socioeconomic status measures income, wealth, educational status, as well as other characteristics detailed

the share of high SES users linked to a given zip code, and Friending Bias, the likelihood that low SES people to not befriend high SES individuals linked to their zip code. Exposure proxies for the chance that a Facebook user would interact with a high SES person on Facebook, while Friending Bias captures whether interactions lead to connections.

The results in figure 7 suggest that above median economic connectedness is associated with greater support over ten months, which is explained by a person both living in an area with greater exposure to and lower friending bias against the high SES group. Together these results suggest that an unemployed person benefits more if they are either friends with a high SES person or have weak ties to high SES individuals because they occupy similar social circles. Given that weak ties tend to increase employment mobility (Rajkumar et al., 2022), there appears to be an overlap between opportunity and support during jobless spells, possibly reflecting that within-network resource constraints lead to imperfect targeting.

4.3 Consumption smoothing from P2P

How much of this informal support from P2P platforms do households pass through to consumption? Ideally, one could regress consumption on P2P inflows through this period to measure the marginal propensity to consume out of informal support. Unfortunately, there are several issues with this approach. First, households that cannot cut consumption may be more likely to seek out and receive support from their community, which would bias up any estimate of the MPC of informal insurance. Second, P2P is often used to split bills and expenses, so non-informal insurance P2P is positively correlated with consumption, a further endogeneity.

Instead of the regression outlined above, I present the same event study as shown in figure 6 with total spending as an outcome. This event study estimates the extent to which being linked to a P2P network facilitates consumption smoothing possibly through the informal insurance channel established above. The results show spending drops of about \$200 immediateley after job loss for all users; after that, users in the top tercile of P2P use see consumption fall by \$40 to \$70 less over the next ten months.

Given that these same users also see \$40 to \$60 more in monthly P2P inflows in the months after

in the paper.

job loss, this suggests users pass through most of this support indicating a large marginal propensity to consume out of informal support. Appendix B.5 presents similar results in specifications related to whether a user previously used any or a specific P2P platform prior to job loss. While these results are purely descriptive, they do suggest a clear consumption smoothing role of P2P.

5 Crowd-out of P2P by unemployment insurance

Given the low dollar amount, but precisely estimated informal insurance role of P2P transfers, how do they interact with more formal UI payments? Federal policy during the pandemic included the largest expansion of social insurance since the Great Depression, so it is an ideal setting to test the extent that UI crowds out informal insurance. If UI increases during the pandemic were associated with large drops in unemployment P2P transfers, this might suggest that public UI is preventing a more robust informal system from taking shape, which would have large welfare consequences for UI policy. Instead, I find little evidence for such crowd-out across a variety of specifications detailed in this section.

Before moving into more complex specifications, consider workers who lose a job in 2020 and receive more UI benefits due to expanded eligibility and weekly benefits. If expanded UI crowds out informal support, this should be associated with lower P2P inflows. Instead, in figure 7 the bars for job loss show no difference in total support after job losses in 2019 and 2020.[15] That said, the pandemic accelerated a shift towards P2P inflows, which could bias estimates for 2020 in either direction.

Instead of comparing based on broad groupings, I look at user-specific differences in UI benefits to estimate crowd-out of informal support. In the ensuing analysis, I condition on the sample of UI recipients because UI enrollment is related to informal support and labor force participation which could bias my estimates up or down. For example, a worker might not enroll in UI if they have sufficient informal support, suggesting that informal support crowds out formal insurance. Alternatively, non-UI recipients might exit the labor force or take an extended break before returning to work, both situations that might not require informal support. I explore the relationship between UI receipt and P2P in appendix B.12, finding UI recipients tend to receive much more support.

[15] The event study in appendix A.11 shows P2P inflows also follow a similar path around job loss in both years.

Within UI recipients, I look at how delays and the user-specific replacement rate of formal benefits affect the amount of informal support. UI delays are widespread both before and after the pandemic, which I detail further in appendix B.14. Intuitively, those who receive UI immediately after job loss likely have an easier time smoothing consumption than those who receive UI weeks or months later. Thus the late recipients have a greater need for informal insurance.

Figure 10 shows the coefficients of the event study relative time dummies interacted with an indicator for whether a user received UI in 0 to 1 or 2 to 6 months. Early UI recipients receive P2P inflows for a shorter duration, while later UI recipients receive transfers over a slightly longer horizon amounting to a larger cumulative total. These timing differences are consistent with a small, but imprecisely estimated "crowd-out" relationship. In addition to inflows, outflows indicate a spike immediately after UI onset that is roughly equal to the initial increase in P2P inflows. This spike falls back to zero and is consistent with a story that initial transfers are paid back after people receiving UI inflows.

Further supporting a "crowd-out" story is figure 11, which shows event study coefficients interacted with an indicator for the tercile of the replacement of pre-job loss earnings. Tercile is calculated by year of job loss, to account for the increase in UI benefits during 2020. While the differences are imprecise, the plot suggests that informal support increases as the UI replacement rate falls. This relationship could reflect that UI crowds out informal support or that users with higher pre-job loss earnings have higher income networks. Appendix B.12 shows state level replacement rates, which may be more exogenous to the income in an insurance network, have a similar relationship with P2P inflows.

5.1 Plausibly exogenous evidence of crowd-out

There is descriptive evidence that UI benefits crowd out a small amount of informal insurance, this does not account for various sources of bias. Workers might apply for UI benefits later if they expect to receive generous UI support early on, which would explain the longer horizon of P2P inflows for this group. Similarly, statutory UI rules typically mean that the highest earners have the lowest replacement rate, but these high earners might have richer friends supporting them with larger P2P inflows. While these alternative explanations are still interesting, they suggest different policy conclusions than if UI crowds out informal insurance. Furthermore, none of the above results provide a dollar-for-dollar estimate of crowd-out.

To get a well identified estimate of dollar-for-dollar crowd-out, I present instrumental variable difference-in-difference results from three separate plausibly exogenous changes to UI during the pandemic, following Ganong et al. (2022) and Coombs et al. (2021). The first isolates exogenous variation in the timing of UI receipt tied to delays caused by an overload of UI application rolls at the start of the pandemic. The second isolates the effect of the expiration of the $600/week UI benefits in July 2020. While the first two natural experiments compare UI recipients to non-UI recipients, the third compares those receiving UI across states that do and do not withdraw from $300/week in federal UI benefits earlier than planned. Together these three quasiexogenous changes suggest relatively little crowd-out of P2P by UI leading me to conclude that in the short run, UI is not holding back informal insurance networks. Figure 12 shows the relevant monthly UI inflows and P2P inflows for the cohorts associated with each natural experiment event.[16]

5.1.1 Natural experiment #1: March 2020 UI delays

In the first natural experiment, I isolate a cohort of workers that lost their jobs in March 2020 that receive UI in April 2020 or June 2020. The April recipients form a "treated" group, while the June recipients act as a "control" group. I then compare the two groups UI and P2P inflows across March and May 2020, specifically instrumenting UI inflows using an indicator for being a UI recipient in the month of May. Equation 4 shows the exact specification.

$$\text{P2P}_{it} = \gamma \hat{\text{UI}}_{it} + \lambda_i + \lambda_t + \epsilon_{it} \tag{3}$$

$$\text{UI}_{it} = \beta \text{March UI recipient}_{it} \times (\text{May 2020})_{it} + \lambda_i + \lambda_t + \nu_{it} \tag{4}$$

This analysis relies on the assumption that UI delays during the pandemic are exogenous to P2P support. Given that most of the delays were caused by overloaded application systems and administrative backlogs, this assumption seems very likely to hold. Even if application timing is

[16]Figure A.32 shows the spending and P2P outflows around each natural experiment event.

not purely exogenous, i.e. that those with better network support tend to wait longer, the delays in question probably greatly exaggerated any differences in application timing, blurring any selection effects.

Figures 12(a) and 12(b) show the relationship between UI and P2P inflows for March job losers based on whether they received UI in April 2020, May 2020, or June 2020.[17] To a lesser degree, UI inflows, spending, and P2P outflows show a clear relationship with the timing of UI receipt. UI increases in the month of benefit onset with higher spikes for the May and June receipt cohorts, representing backfilled payments. Meanwhile, spending and outflows similarly rebound around the timing of UI receipt representing a marginal propensity to consume. In contrast, P2P inflows increase for all three groups simultaneously with only the slightest flattening for the April receipt cohort in April relative to the other two cohorts.

5.1.2 Natural experiment #2: July 2020 expiration of $600/week

In the second natural experiment, I compare March 2020 job losers who received UI by June 19 to a group of workers who became unemployed at some point in 2021. Job losers are compared to the "not yet unemployed" instead of the "not yet received UI" for two reasons: (1) UI delays lasting through August were unlikely and (2) August recipients who did not experience the $600 drop received backfilled UI payments of thousands of dollars. That said, there are some drawbacks to comparing the employed to the "not yet employed." For example, it would not make sense to compare these groups immediately after the March 2020 job loss, but figures 12(c) and 12(d) show that by June 2020, these groups had very similar trends in P2P inflows and spending. Equation 6 shows the exact specification I run for the month of June and August 2020.

$$\mathrm{P2P}_{it} = \gamma \hat{\mathrm{UI}}_{it} + \lambda_i + \lambda_t + \epsilon_{it} \tag{5}$$

$$\mathrm{UI}_{it} = \beta \text{March job loser, receiving } \mathrm{UI}_{it} \times (\text{August 2020})_{it} + \lambda_i + \lambda_t + \nu_{it} \tag{6}$$

[17]Figure A.32 shows the changes in P2P outflows and spending exhibit a clear marginal propensity consume out of UI benefits.

5.1.3 Natural experiment #3: June withdrawal from UI benefits

In the third natural experiment, I isolate to a cohort of those unemployed and insured on April 30, 2021 living in one of 43 states in which I accurately flag UI benefits that either retained expanded UI benefits through September 2021 or 19 that withdrew in June 2021. Equation 8 shows the exact specification comparing users in retain states to withdrawal states in April 2021 versus August 2021. The underlying assumption is that P2P inflows do not markedly change for some other reason in the withdrawal states around the June withdrawal. There are no clear changes in any of the P2P platforms' policies or other noteworthy press releases from these platforms at this point, so this seems like a safe assumption.

$$\text{P2P}_{it} = \gamma \hat{\text{UI}}_{it} + \lambda_i + \lambda_t + \epsilon_{it} \tag{7}$$

$$\text{UI}_{it} = \beta \text{Retain}_{it} \times (\text{August 2021})_{it} + \lambda_i + \lambda_t + \nu_{it} \tag{8}$$

Figures 12(e) and 12(f) show the UI and P2P inflows for the unemployed and insured across retain and withdraw states. UI inflows fall in the withdrawal states in June 2021, then also fall in retain states in September 2021 with the national expiration. Furthermore, average UI inflows are not perfectly aligned prior to April 2021 indicating a difference in duration of benefits across states. Specifically, those in retain states had been receiving UI for longer, so I re-weight these estimates using inverse propensity weighting described in appendix C.12.

Spending follows UI, while P2P outflows are too noisy to discern a meaningful pattern. In this case P2P inflows in retain states close the gap relative to withdrawal states, indicating the slightest crowd-in.

5.1.4 Crowd-out results

Table 1 shows the dollar-for-dollar crowd-out results from these three IV-DID specifications and their OLS counterparts. Each regression yields the same basic result: very small values or precisely estimated zeroes. To underscore the precision, I show the lower-bound of the confidence interval for each point estimate multiplied by $100 in UI benefits, which is associated with at most

\$8.69 of crowded-out P2P inflows in the June 2021 withdrawal. Appendix tables A5 through A8 show reduced form results, logged variables, a Poisson regression, and conditioning on using P2P throughout the experiment months. Tables A9 through A12 show the results by P2P platform. None yield meaningfully different results to the mainline outcomes.

Given that P2P support is higher in counties above median per capita income, I also run the same regressions, but separating the sample into high and low income counties. These results, shown in table 2 shows that negative crowd-out is concentrated in high income counties, but the point estimates are still small and precisely estimated.

Overall, the results indicate that UI does not greatly hold back within-network informal insurance via crowd-out. Furthermore, they indicate that the welfare consequences associated with crowd-out of informal insurance are negligible, which I will expand on in section 7.1.

6 Model: Welfare of unemployment insurance with crowd-out of informal insurance

This section builds on the model presented in Chetty and Saez (2010) of optimal insurance benefits in the presence of unoptimized private insurance with some moral hazard. I present the simplest model in which there is only one informal insurance network as my results do not indicate highly variable crowd-out by network. An extension and its welfare consequences appear in appendix section B.15.

The intuition of the model is as follows: there is some private insurer who fails to fully optimize for government UI such that crowd-out 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, which makes empirical applications easier:

$$U(c, z; n) = u(c) - h(z/n) \tag{9}$$

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:

$$u(z_h - \tau - \tau_P) - u(z_l + b + b^p) = h(z_H/n^*) - h(z_L/n^*) \tag{10}$$

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

$$W = \int_0^{n^*} [u(z_L + b + b^p) - h(z_L/n)]dF(n) + \int_{n^*}^{\infty} [u(z_H - \tau - \tau_P) = h(Z_h/n)]dF(n) \tag{11}$$

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

$$W(e) = eu(z_H - \tau - \tau_P) + (1-e)u(Z_L + b + b^p) - \varphi(e) \tag{12}$$

where

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

is 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 who earn z_H to maximize welfare, W. This setup is the one presented in Chetty and Saez (2010), which is isomorphic to Baily (1978) 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 $b(p)$ might or might not be set optimally. Chetty and Saez (2010) 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:

$$W = eu\left(z_H - \frac{1-e}{e}\left(b^p(b) + b\right)\right) + (1-e)u\left(z_L + b^p(b) + b\right) - \varphi(e) \tag{13}$$

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 crowd-out parameter $r = -db^p/db$ in this setting. This crowd-out 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[18]:

$$\frac{dW}{db} = (1-e)(1-r)u'(c_H)\left[\frac{u'(c_L) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1 + b^p/b}{1-r}\right] \tag{14}$$

Equation 14 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, crowd-out 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 Chetty and Saez (2010) 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:

[18]Proof in Chetty and Saez (2010)

$$
\begin{aligned}
G(b) &= \frac{dW}{db}\frac{1}{1-e} / \frac{dW}{dz_H}\frac{1}{e} \\
&= (1-r)\left[\frac{u'\left(c_L-\right)-u'(c_H)}{u'\left(c_H\right)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1+b_p/b}{1-r}\right]
\end{aligned} \tag{15}
$$

Equation 15 allows me to tractably measure the welfare change associated with increases and decreases in unemployment insurance benefits caused by delays during the pandemic.

6.1 Calculating welfare loss

Equation 15 shows the sufficient statistics and parameters needed to estimate the value of the change in welfare associated with raising benefits. Given that the current crowd-out 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 crowd-out parameters r and $1 + b_p/b$, I use my own estimations. I calculate welfare for the values of $r \in \{-0.008, 0.04\}$ taken from the field estimates in table 1. Next, I calculate $1 + b^p/b$ using the excess P2P ratio of private and public unemployment insurance, detailed in appendix B.6.[19]. Before the pandemic $b^p/b = 0.08$ conditional on UI receipt, but during the pandemic it falls to 0.07 as UI levels rose.

One caveat is that my estimates are restricted to changes in informal insurance which might differ from crowd-out of other sources of private insurance like severance pay, estimated by Chetty and Saez (2010) to be $r = 0.14$. Given that severance pay is likely set on an annual basis, and this study focuses on month-to-month changes to informal insurance, the absence of severance pay is unlikely to change the short-run interpretation of my welfare calculations. This brings up another caveat: the long-run effect of public insurance on informal insurance could be much larger, as larger public social insurance programs might reduce the density and extent of informal support in the long run. As such, it is best to interpret my calculations as short-run welfare effects of UI dollars.

For the other statistics and parameters of the model, I follow Chetty (2006) for each. The change in utility, is best approximated by the expression $\frac{u'(c_l)-u'(c_H)}{u'(c_H)} = \frac{c_L}{c_H}\gamma - 1$, where 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 Ansell

[19]Formally, I calculate $1 + b^p/b = 1 - b^p/(b + b^p)$

and Mullins (2021). Last, I use two estimates of the labor elasticity of unemployment benefits, which are 0.5 in a baseline model and 0.07 in a model that incorporates increased job search costs (Ganong et al., 2021b).

$$
\begin{aligned}
e &= 0.85 \text{ from Current Employment Statistics in Ansell and Mullins (2021)} \\
\frac{c_e}{c_u} &= 1/0.92 \text{ from Ganong et al. (2021a)} \\
\gamma &= 2 \text{ from Chetty (2006)} \\
\varepsilon_{1-e,b} &\in \{0.07, 0.5\} \text{ from Ganong et al. (2021a)}
\end{aligned}
$$

Table 3 presents estimates of $G(b)$ without private insurance and with private insurance under pre-pandemic and post-pandemic levels of UI. These results suggest that increasing b by one dollar from 2019 current levels led to 34 cents less in welfare, reduced to 39 cents using my largest crowd-out estimate. The crowd-out welfare measure during the pandemic is also indistinguishable from the standard model welfare effect, which is positive. Notably, labor supply elasticity drives the extent of the welfare consequences associated with crowd-out, but as labor supply elasticity increases, crowd-out will only change the sign of the welfare estimate if the unemployment consumption gap grows sufficiently large.

One potential shortcoming of my results is that P2P inflows represent only a subset of the private insurance that makes up b_p. Other sources could include cash, checks, in-kind support, and severance pay. That said, my crowd-out estimates are so low, that the magnitude of the private market would have to be infeasibly large for crowd-out to have substantial welfare consequences. For example, equation 16 shows the money metric in terms of b and b_p with the relevant pandemic values of the elasticity and employment rate and "normal" consumption change and CRRA from above plugged in. Some quick algebra yields that b_p/b, the ratio of informal to public insurance would need to be 1.125 or that private insurance would need to be more than double the level of public insurance to yield zero welfare during COVID. This ratio is also well outside the range that I estimate or that can be found in the Survey of Income and Program Participation.

$$
\begin{aligned}
G(b_p/b) &= 0.96 \times \left(\left(\frac{1}{.92} \right)^2 - 1 - \frac{.07}{.85} \times \frac{1 + b_p/b}{0.96} \right) \\
&= (0.17 - .08(1 + b_p/b))
\end{aligned} \tag{16}
$$

7 Robustness

There are a variety of potential pitfalls to the approaches in this paper. The main issues have to do with whether I have properly identified insurance with P2P.

First, what if the unemployment events I detect are users quitting their jobs to start a job that pays 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 unemployment in figures A.25 and 10, this seems unlikely. Similarly, figure A.9 suggests that the P2P inflows exceed earnings from gig platforms. Furthermore, the decline in outflows shown in each of the unemployment graphs suggests these job loss events are actual unemployment periods.

What if I am identifying temporary layoffs or holidays instead of job losses because five weeks is too short a window to identify a job loss? In that case, workers might use the time off to go out with friends more often and P2P use could increase. Figure 8 presents event study coefficients of relative time dummies interacted with an indicator for whether the unemployment spell lasts 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 to \$30 per month that lasts for four months. Meanwhile, outflows for both drop off, but within two months they mostly recover for the short-term unemployed.

A second potential pitfall is that I might not have properly measured informal insurance with P2P and instead measure exchanges reflecting people going out for meals. To that end, I reformed my P2P series to focus solely 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.

7.1 Calibration if miscategorizing P2P as informal insurance

One possible issue with the preceding analysis is that I might miscategorize 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[20] leads to a lower or upper 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 (Chetty and Saez, 2010).

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 Chetty and Saez (2010):

$$\frac{dW^{BC}}{db} = (1 - e^{BC})u'(c_H)\left[\frac{u'(c_L) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e^{BC}}\right] \tag{17}$$

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 10, but now the maximum and minimum earnings are shifted by the amount of informal P2P transfers. Low earners spend $(z_L + b^p)/n$ and high earners spend $(z_H - \tau_p)/n$ 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 transferred out, which makes the algebra somewhat easier below, though it is somewhat more difficult to rationalize. Chetty and Saez (2010) describe a relationship similar to this one: a firm redistributes earnings to reduce inequality while preserving the mean. Plugging these new earnings levels into their respective $h()$ on the RHS of equation 10 yields a new optimal threshold

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

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 workers can work less for a smaller drop in earnings, hence 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.

$$
\begin{gathered}
\frac{dW}{db} - \frac{dW^{BC}}{db} \leq 0 \\
(1-e)(1-r)u'(c_H)\left[\frac{u'(c_L)-u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1+b^p/b}{1-r}\right] \leq (1-e^{BC})u'(c_H)\left[\frac{u'(c_L)-u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e^{BC}}\right] \\
\underbrace{\frac{\frac{u'(c_L)-u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1+b^p/b}{1-r}}{\frac{u'(c_L)-u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e^{BC}}}}_{\leq 1} \leq \underbrace{\frac{1}{1-r}}_{\geq 1}\underbrace{\frac{1-e^{BC}}{1-e}}_{\leq 1}
\end{gathered}
\tag{18}
$$

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, the gap in marginal utilities is equal and the LHS of the final inequality has a 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.

7.2 Robust to heterogeneous treatment timing

This paper relies on identification derived from event studies on heterogeneous treatment timing produced with two-way fixed effects (TWFE) regressions. As summarized by Goodman-Bacon (2021), the issues arise because early treated groups end up serving as controls for later treated groups. If treatment effects increase in relative time after treatment, this can bias down estimates of the effects. Alternatively, if treatment effects go negative in the long-run, the TWFE estimator can bias treatment estimates up. Any fixes must undo the effect of "forbidden" comparisons of the later- to early-treated observations, while accounting for selection bias into treatment timing.

In general, TWFE analysis is aided by having a larger "clean control" group that is not treated. To increase the robustness of my baseline results, I include the 4,245 users who become unemployed in September 2021. At the same time, this might be insufficient to fully residualize out the contamination from later treated groups which make up a much larger sample.

Borusyak et al. (2021) propose one possible fix: imputing month and user fixed effects from the purely "untreated" observations, which Gardner (2022) implements as a two-stage estimation strategy, which provides more efficient estimates than other methods. I implement the Gardner (2022) method as shown in equation 19. The intuition is simple: estimate month and user fixed effects using untreated observations, assuming that untreated observations provide efficient estimates of time trends and each person's overall use. In my case, I define "treatment" as starting at relative time -1 to account for anticipation of job loss.

$$\begin{aligned} y_{it}(0) &= \lambda_i + \lambda_t + \nu_{it} \\ \tilde{y}_{it} &= y_{it} - \hat{\mu}_t - \hat{\mu}_t \\ \tilde{y}_{it} &= \beta_{-5} \sum_{s \leq -5} D_s^t + \sum_{s \in [-4,-2]} \beta_s D_{it}^s + \sum_{s \in [0,9]} \beta_s D_{it}^s + \beta_{10} \sum_{s \geq 10} D_{it}^s + \varepsilon_{it} \end{aligned} \tag{19}$$

I present the event study results in figure A.40, which are consistent with the earlier event study estimates. The standard errors are bootstrapped at the user-level as the analytical standard errors associated with Gardner (2022) were infeasible.

7.3 Placebo test of spurious correlation between unemployment months and P2P bank runs

Another threat to the validity of my results is that the increase in P2P inflows is driven by a "run" on balances held in Venmo, Cashapp, and Paypal accounts at the start of the pandemic. As the plurality of job losses in my data occur at the start of the pandemic, this could artificially generate an increase in P2P inflows after job loss in my event study plots. Given that 2019 job losers receive more P2P inflows after job loss, this hypothetical spurious correlation between job losses and P2P inflows at the start of the pandemic seems unlikely to drive my results. Still, the pandemic starts within 10 months of any 2019 job losses, which might explain P2P inflow increases. Furthermore, if everyone made a run on "P2P savings" right at the start of the pandemic this would be differenced out by the month fixed effect, but if only a subset of those who expected to lose their job did, then that could also generate the same effect.

To assess the extent that the pandemic is driving results, I implement a placebo test. Using the observed counts of unemployment start months in the analysis sample, I assign each user a placebo job loss month t and estimate the average increase in P2P inflows after month $t-1$. I choose $t-1$ to match the "expectations of job loss." I produce these 10,000 such placebo estimates of the average increase in monthly P2P inflows after placebo job losses. Figure A.41 shows the 10,000 placebo estimates against the true treatment effect for P2P inflows, P2P outflows, and gig earnings. These plots indicate that the placebo estimates are centered around zero, while the true estimates are distinct from zero.

8 Conclusion

This paper is the first attempt to use the growth of P2P platforms in the United States to document informal insurance and the extent that it is crowded out by formal unemployment insurance. Overall, I find that informal insurance in the United States exists, but it is a short-run form of support that imperfectly targets those with the most need. Furthermore, crowd-out estimates suggest that formal insurance is keeping informal insurance networks from flourishing in the short run.

The results presented here address 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 off in the long-run, suggestive of an informal insurance role. Additionally, this study shows that users in the top tercile of transactions made via P2P, for whom P2P captures a much larger share of transfers, had P2P inflows peak at $60 after job loss, suggesting that informal insurance still tends to pay out fairly little.

Next, I showed that those workers that actively use P2P prior to job loss tend to see consumption fall less after job loss relative to non-users of P2P, consistent with the inflow increases I measure. These results suggest that P2P facilitates consumption smoothing via P2P, but only by a marginal amount on average.

I then looked into heterogeneous informal insurance during job loss. Specifically, I find that single mothers tend to receive the most inflows after job loss. At the same time, those living in high income areas or in zip codes that are better networked with high SES people, are also more likely to receive high P2P inflows. Together these results suggest that people are more generous to those in greater need, but that the support a network can offer is directly tied to its overall income. The fact that those who are better connected to high income network members are likely to have more re-employment opportunities suggests a misallocation of insurance, though further research is needed to rule out alternative explanations.

After establishing that informal insurance via P2P offers fairly insufficient replacement of lost earnings, I moved onto ask whether this is because public UI is crowding it out. Initial heterogeneity analysis suggests P2P inflows are greater among those for whom UI replaces less of lost earnings or who receive UI payments with a delay of at least two months after job loss. Given that these two groups could be endogenous, I exploit quasiexogenous variation in UI inflows driven by the pandemic to show that an addition dollar of UI benefits crowds out at most $0.04 of P2P inflows. This low crowd-out estimate suggests that UI inflows are not holding back informal insurance within the United States, instead most people simply do not receive much support after job losses.

I bring these crowd-out estimates to a framework from Chetty and Saez (2010) to calculate the marginal welfare of UI benefits with and without crowd-out. My calculations show crowd-out has negligible welfare consequences unless the informal insurance market is unrealistically large relative to public UI. Given that it is extremely difficult and invasive to measure informal insurance, even as we move towards an increasingly digital economy, my results suggest that policymakers make reasonably accurate estimates when ignoring informal insurance in their analysis without appealing to an assumption grounded in zero moral hazard in informal insurance, which is unlikely to hold.

While I find relatively little crowd-out, this is only documented in the short run. It is possible that the robust safety net in the United States has led to relatively weak informal support structures over the long run, but that is a subject for future research. Additionally, my results hint that people repay at least some of their initial support after job loss out of their UI inflows. However, one would need to match payments by sender and receiver to confirm repayment, an increasingly feasible area for future research.

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A Tables

Table 1. Crowd-out of P2P Inflows by UI during various pandemic policy events

Method	OLS			IV		
Policy Change	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration	June Withdrawal
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	0.003	0.004***	-0.01*	-4.6×10^{-5}	0.008	-0.04*
	(0.004)	(0.002)	(0.006)	(0.005)	(0.006)	(0.02)
Standard-Errors	User		State	User		State
Lower bound × $100 in UI	-0.46312	0.10852	-2.3191	-1.0837	-0.38761	-8.6962
Observations	35,392	31,312	28,546	35,392	31,312	28,546
R^2	0.73820	0.75872	0.71917	0.73819	0.75869	0.71886
F-test (1st stage), UI Inflows				28,828.8	7,541.0	5,083.0
User and Month fixed effects	✓	✓	✓	✓	✓	✓

Instrumental variable difference-in-difference estimates of crowd-out of P2P Inflows by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of $600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.

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

Table 2. Crowd-out of P2P Inflows by UI during various pandemic policy events subset by county income

Method	OLS						IV					
Policy Change	March Delays		July Expiration		June Withdrawal		March Delays		July Expiration		June Withdrawal	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Median PCI	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above	Below	Above
UI Inflows	0.006	0.002	0.004***	0.003	0.03	-0.02**	0.007	-0.002	0.02	0.006	0.06*	-0.05**
	(0.01)	(0.004)	(0.0009)	(0.005)	(0.02)	(0.007)	(0.01)	(0.006)	(0.01)	(0.007)	(0.03)	(0.02)
Standard-Errors	User				State		User				State	
Lower bound × $100 in UI	-1.5252	-0.57658	0.25696	-0.59143	-0.95837	-3.1032	-2.0306	-1.3347	-0.52163	-0.76482	-0.45494	-9.9202
Observations	5,508	29,874	4,676	26,628	3,880	24,650	5,508	29,874	4,676	26,628	3,880	24,650
R^2	0.69883	0.74509	0.78470	0.75459	0.80977	0.70188	0.69883	0.74507	0.78242	0.75458	0.80948	0.70142
F-test (1st stage), UI Inflows							4,844.1	23,976.8	246.90	30,430.8	578.11	4,388.6
User and Month fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

Instrumental variable difference-in-difference estimates of crowd-out of P2P Inflows by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Odd columns are users in counties below median per capita income and even columns are users in counties above median per capita income. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of $600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.

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

Table 3. Money metric estimates of marginal welfare of additional dollar of UI before and after pandemic with and without crowd-out included

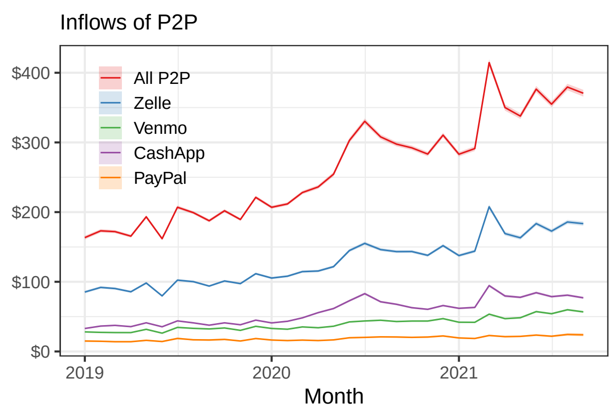
Context	ε	e	r	$b_p/(b+b_p)$	Standard	With crowd-out
Pandemic	0.07	0.85	-0.008	0.08	0.10	0.09
Pandemic	0.07	0.85	0.04	0.08	0.10	0.09
Pre-pandemic	0.5	0.95	-0.008	0.07	-0.34	-0.39
Pre-Pandemic	0.5	0.95	0.04	0.07	-0.34	-0.40

Money metric welfare effects of UI with and without crowd-out. Elasticities from Ganong et al. (2022). Employment share from Ansell and Mullins (2021) and CPS. Consumption change (8%) taken from Ganong and Noel (2019) and CRRA $\gamma = 2$ from Chetty (2006).

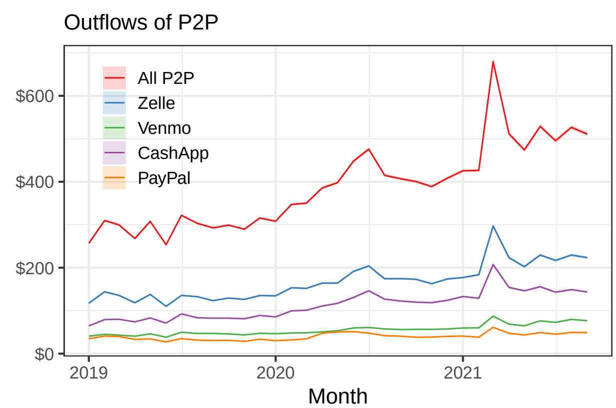
B Figures

Figure 1. Timeline of P2P inflows and outflows flagged using memos and Plaid categories

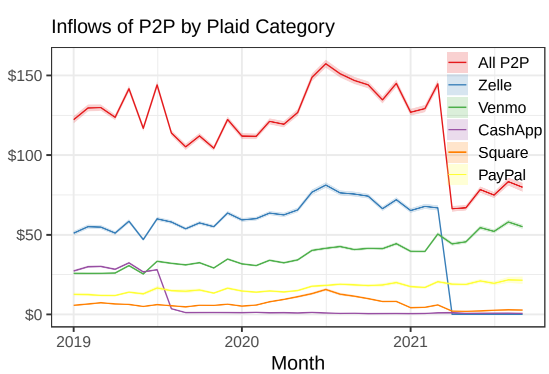
(a) Inflows (plaid categories or memos)



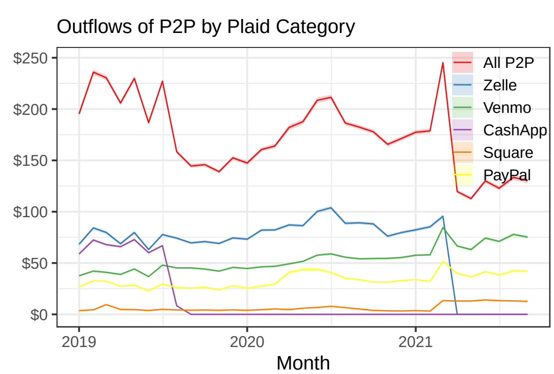
(b) Outflows (plaid categories or memos)



(c) Inflows (plaid categories and memos)



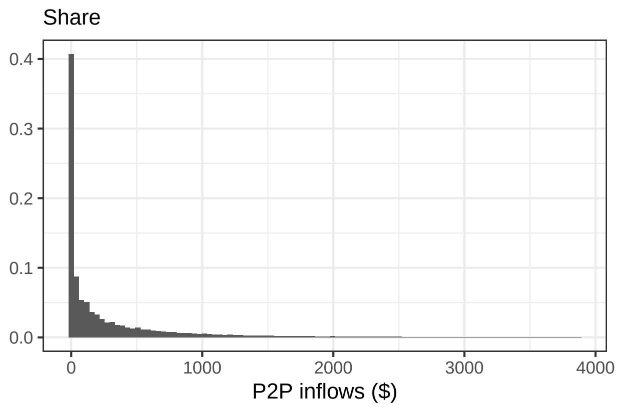
(d) Outflows (plaid categories and memos)



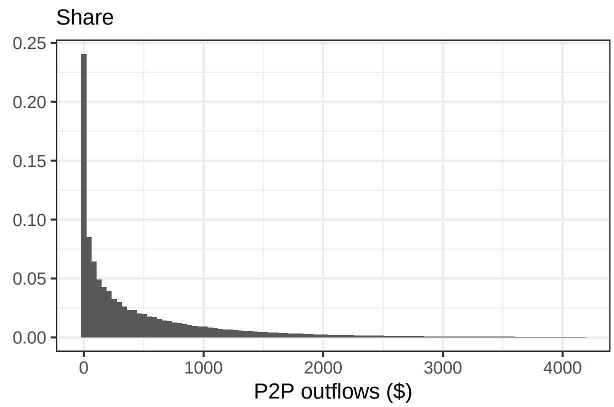
Notes: P2P inflows and outflow of transactions between \$5 and \$15,000, not linked to purchases, gig platforms, or stimulus payments over time. The top two figures show inflows and outflow from transactions with memos that meet regular expression search criteria or Plaid categories. The bottom are those that meet the Plaid category search criteria and memo regular expression searches. The sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure 2. Histograms of monthly inflow and outflow dollars and counts

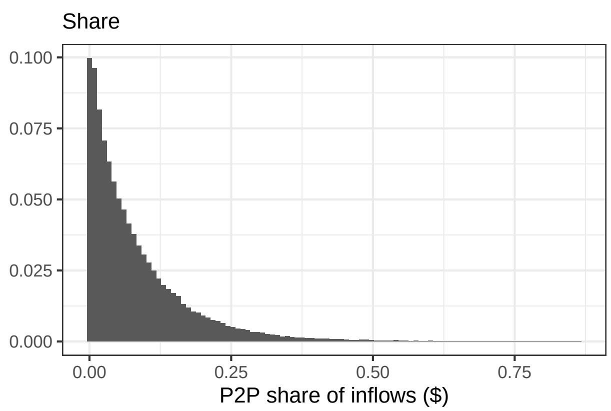
(a) Monthly P2P inflows ($)



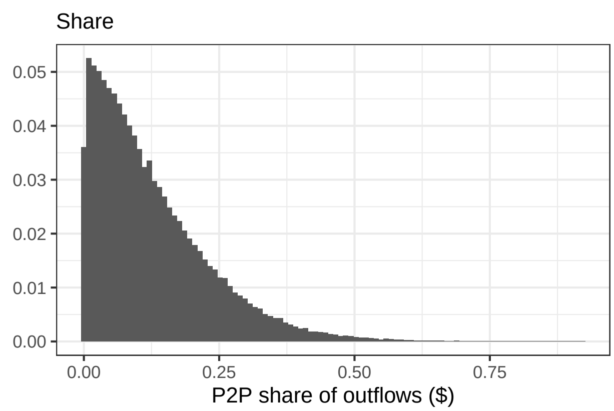
(b) Monthly P2P outflows ($)



(c) P2P share of total inflows ($)

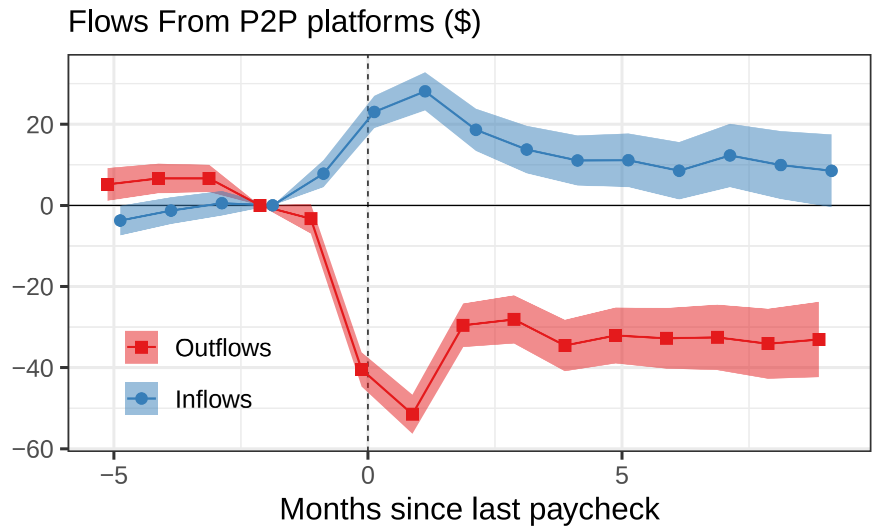


(d) P2P share of total outflows ($)



Notes: Figures (a) and (b) show the total P2P inflows and outflows, respectively, for each user-month in the data. Figures (c) and (d) show the share of total inflows and outflows, respectively, exchanged on P2P platforms. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

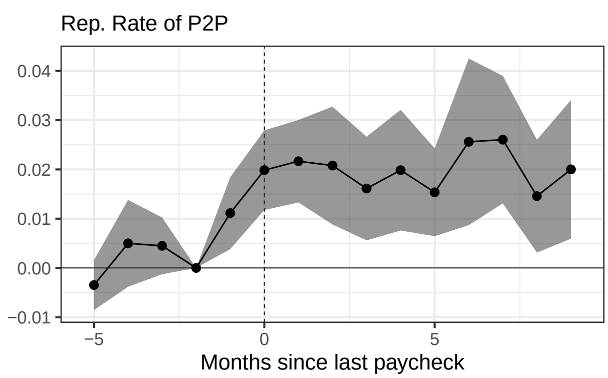
Figure 3. P2P inflows & Outflows



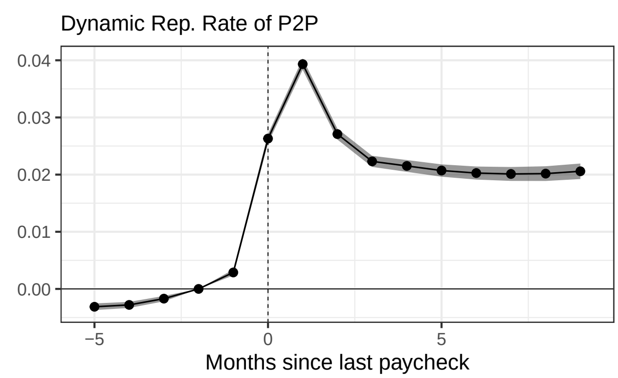
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure 4. Event study of the static and dynamic replacement rate of P2P inflows after unemployment

(a) Static Replacement of Earnings



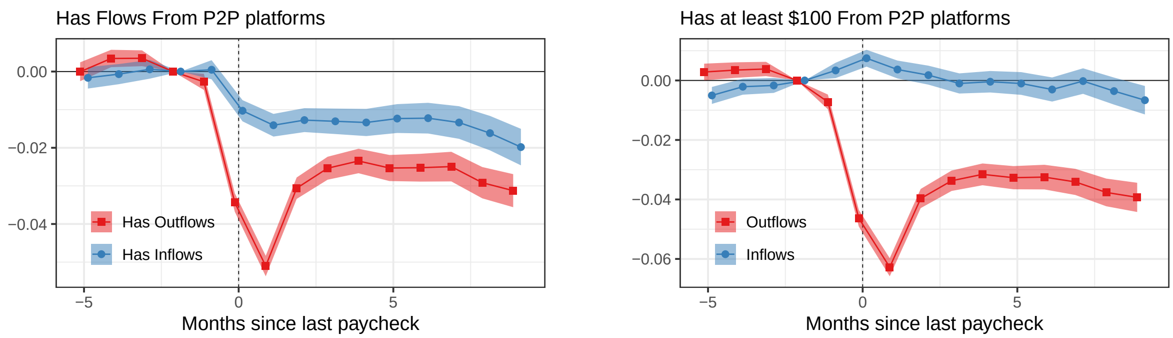
(b) Dynamic Replacement of Earnings



Notes: Within-person event study of the share of median monthly pre-job loss earnings replaced by monthly P2P inflows around a user's first unemployment spell. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure 5. Extensive Margin of P2P

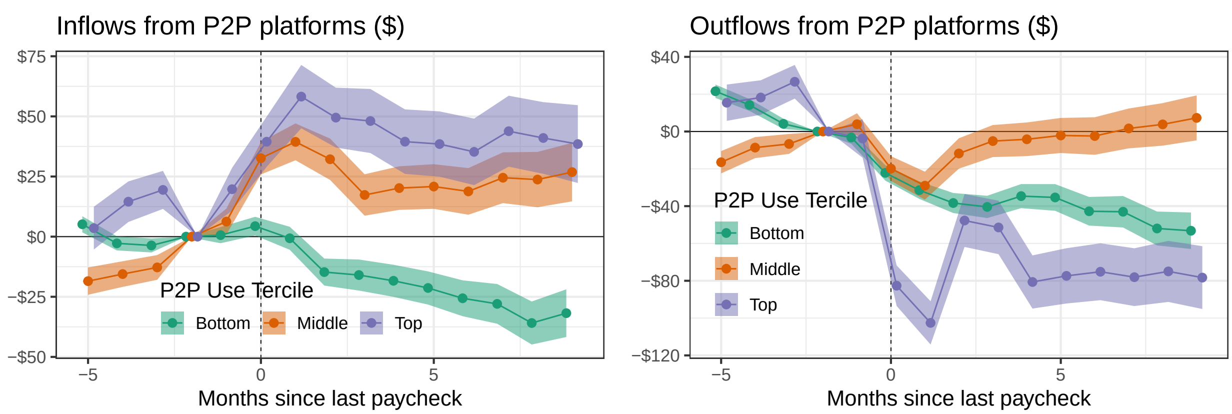
(a) $\mathbb{P}$(Monthly P2P > 0) (b) $\mathbb{P}$(Monthly P2P > 100)



Notes: Within-person event study whether a user had inflows or outflows from a P2P platform around the user's first unemployment spell. Figure (a) is having any flows, while figure (b) is having at least $100 of the relevant flow. The sample is restricted to users with a single unemployment spell. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure 6. Event study of inflows and outflows interacted with tercile of P2P share prior to job loss

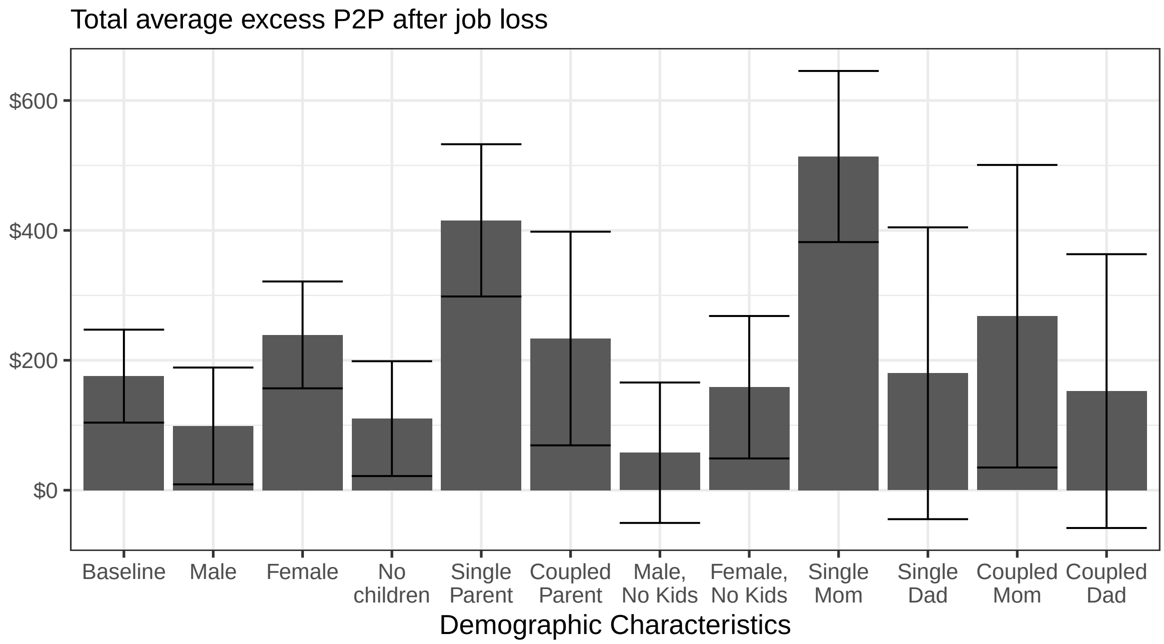
(a) Inflows, cumulative share (b) Outflows, cumulative share



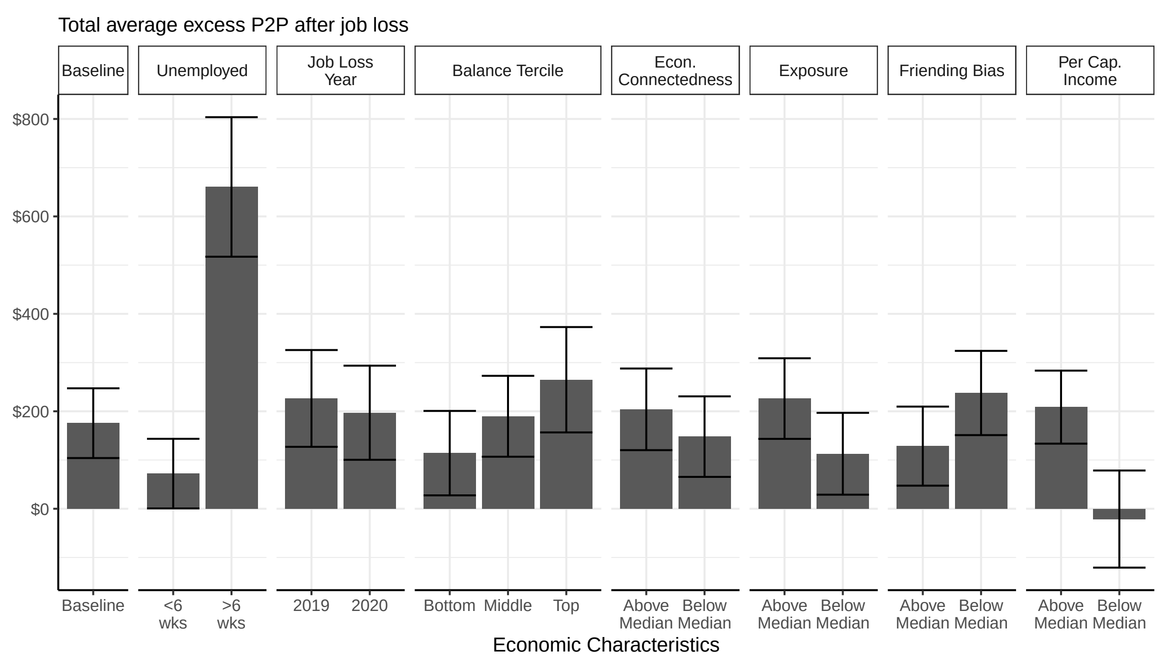
Notes: Event studies of P2P inflows and outflows around a user's first unemployment spell. Two months prior to job loss is omitted. The event study coefficients are relative time dummies interacted with indicators with tercile of the median monthly P2P share of cumulative flows two or more months before job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure 7

(a) Demographics

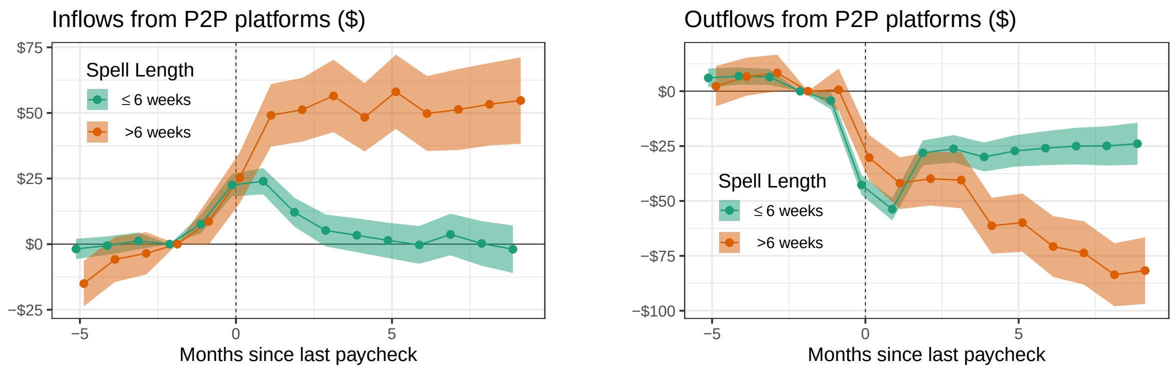


(b) Economic Characteristics



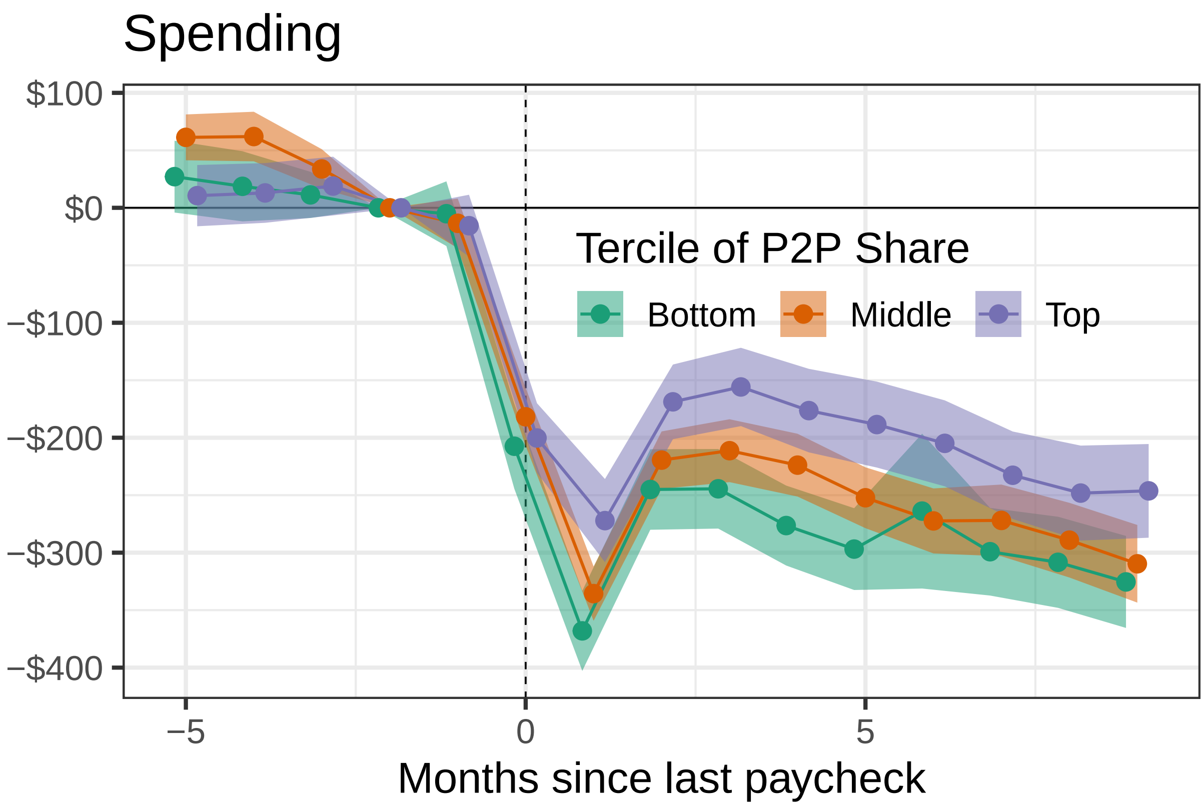
Notes: Aggregate total increases to P2P inflows after job loss from event study with relative time dummies interacted with the indicator group specified in the x-axis. Each bar plot is a linear combination of the coefficients on relative time dummy -1 forward. Baseline estimates are from an event study without interacting relative time dummies. Gender comes survey response or first name prediction performed by Earnin. Relationship status and presence of children imputed from CARES Act stimulus payment and receipt of child tax credit. Economic connectedness, exposure, and friending bias come from the Social Capital Atlas (Chetty et al., 2022). Balance tercile is tercile of bank balance in month before job loss. Per capita income based on the 2019 American Community Survey county level per capita income. Unemployment length longer or shorter than 6 weeks based on spells defined as starting unemployment after five consecutive weeks without a paycheck. Standard errors are clustered at the user level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure 8. P2P inflows & Outflows by Length of Unemployment Spell



Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator for whether the spell lasted more or less than six weeks. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

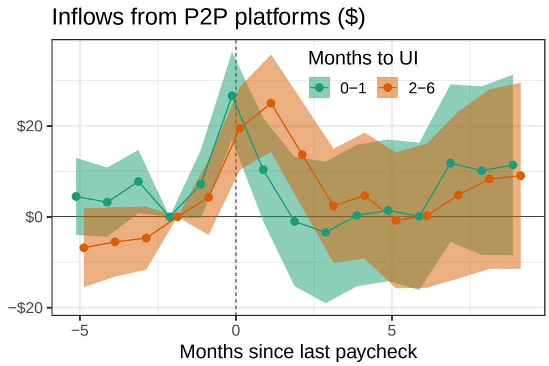
Figure 9. Event study of consumption by tercile of P2P share of inflows and outflows prior to job loss



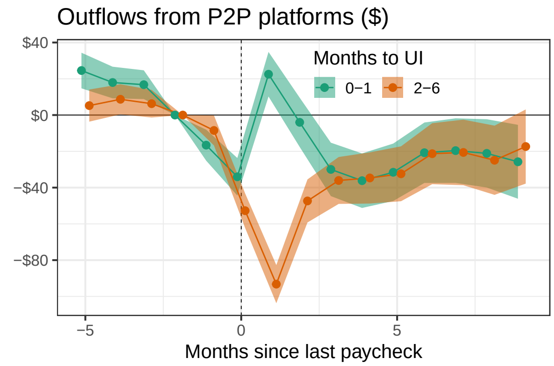
Notes: Within-person event studies of spending a user's first unemployment spell. Changes are relative to the level two months prior to job loss. Coefficients are relative time dummies are interacted with tercile of median monthly P2P share of cumulative flows more than one month before job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure 10. P2P Inflows & Outflows by Months to UI Receipt

(a) P2P Inflows



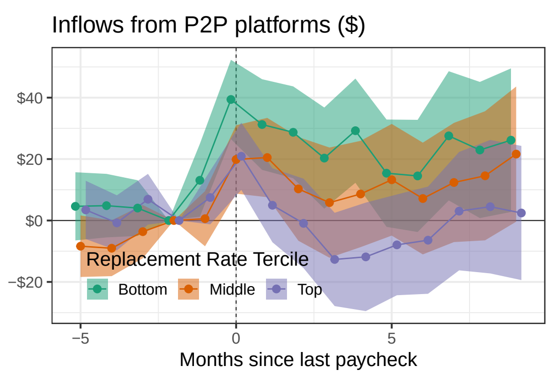
(b) P2P Outflows



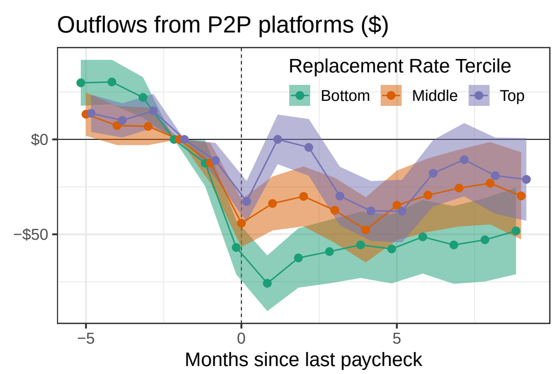
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of bins for the months to UI since job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

Figure 11. P2P Inflows Replacement Rate Tercile

(a) P2P Inflows



(b) P2P Outflows

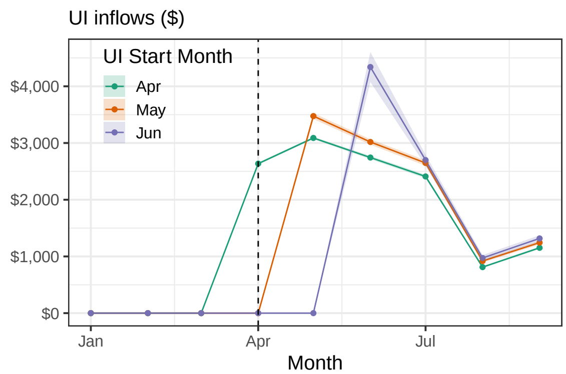


Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with tercile of user pre-job loss earnings replacement rate. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

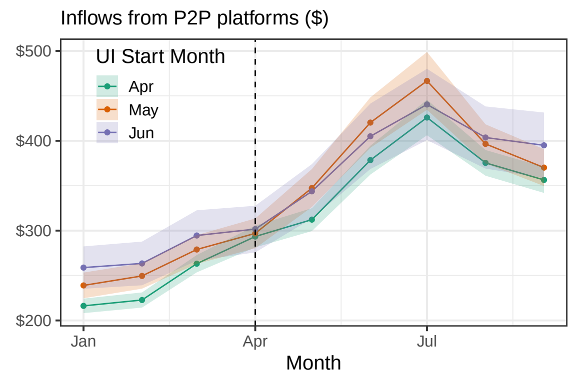
Figure 12. UI and P2P inflows across different pandemic-related changes to UI benefit payments

March 2020 job losers by cohorts of job receipt

(a) UI Inflows

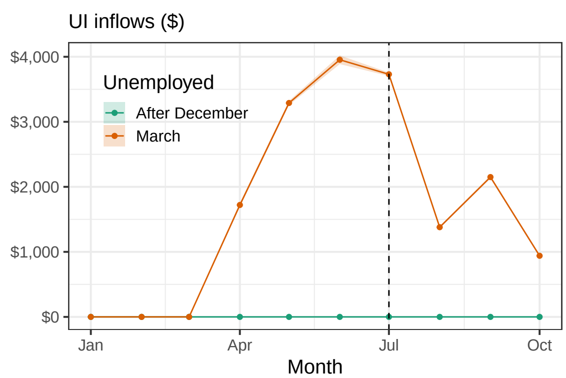


(b) P2P Inflows

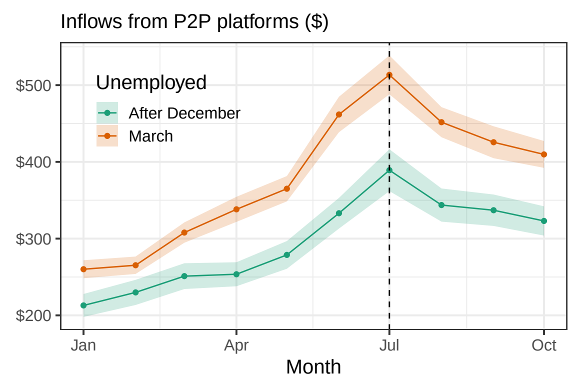


March 2020 job losers receiving UI vs. continuously employed through December 2020

(c) UI Inflows

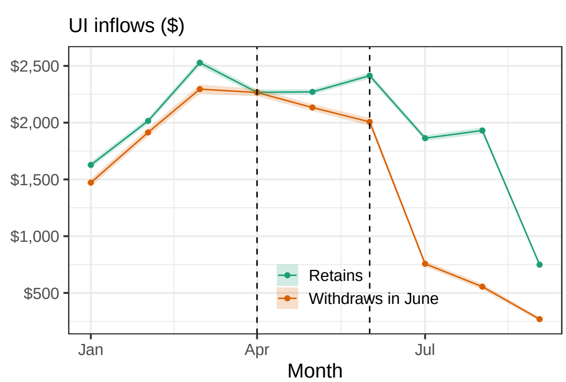


(d) P2P Inflows

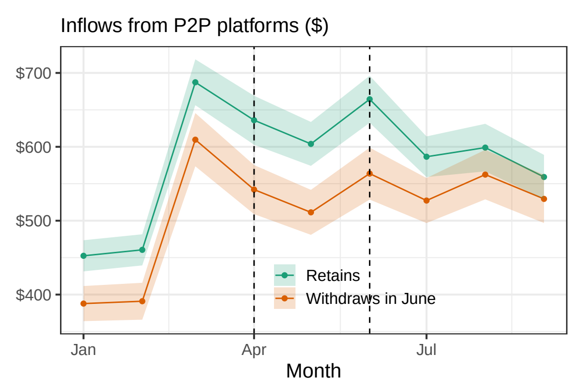


Unemployed and insured as of April 30 in Withdrawal vs. Retain States

(e) UI Inflows



(f) P2P Inflows



Notes: UI and P2P inflows for different cohorts around three pandemic-related changes to UI benefits. Figures (a) and (b) show plots for cohort that lost jobs in March 2020 broken out by month they received UI benefits. Figures (c) and (d) compare the same cohort of March job losers workers that remained continuously employed through December 2020 around the July 2020 expiration of expanded UI benefits. Figures (e) and (f) compare workers and were unemployed and insured as of April 30, 2021 in states that withdrew from federally-expanded UI benefits in June 2021 to states that retained these benefits through September 2021. UI inflows are all inflow transactions containing a set of UI-related regular expressions. P2P inflows have P2P-related regular expressions and are restricted to transactions between $5 and $15,000. Sample also includes those with at most five outflows per month in the policy experiment.

C Appendices

A Appendix Tables

Table A1. P2P Inflows In Response to Income or Spending Changes and Local Unemployment Rate

Method	OLS (1)	Logged OLS (2)	IV (3)	Logged IV (4)	Pois (5)
Income	-0.003***	0.01***	-0.003	0.001	-7.1×10^{-6}***
	(0.0007)	(0.0007)	(0.002)	(0.003)	(1.5×10^{-6})
Local Unemployment Rate	9.3***	-0.004***	9.3***	-0.004***	-0.006***
	(0.42)	(0.001)	(0.42)	(0.001)	(0.001)
Observations	3,799,973	3,799,973	3,799,973	3,799,973	3,498,575
R^2	0.39342	0.54568	0.39342	0.54563	
F-test (1st stage), Income			55,825.3		
F-test (1st stage), Income				165,923.2	
User and Month fixed effects	✓	✓	✓	✓	✓

OLS and 2SLS estimates of the change in P2P inflows in response to changes in income for the analysis sample of Earnin users that become unemployed between July 2020 and September 2021. 2SLS estimates are instrumented using an indicator for the month being after the first month of unemployment and an indicator for the month being after the first month of unemployment insurance. Odd columns are levels, while even columns are logged. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Spending is all consumption spending with cash, card, or check. Standard errors clustered at the user level.

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

Table A2. Summary Statistics of various series

	No UI	Had UI	Total
Has job loss	929,193	344,268	1,273,461
	(72.97)	(27.03)	(100.00)
Continuously employed	445,949	58,887	504,836
	(88.34)	(11.66)	(100.00)
Total	1,375,142	403,155	1,778,297
	(77.33)	(22.67)	(100.00)

Cross tab of the continuously employed by those that ever received UI. Continuous employment defined by never having more than five weeks without earnings.

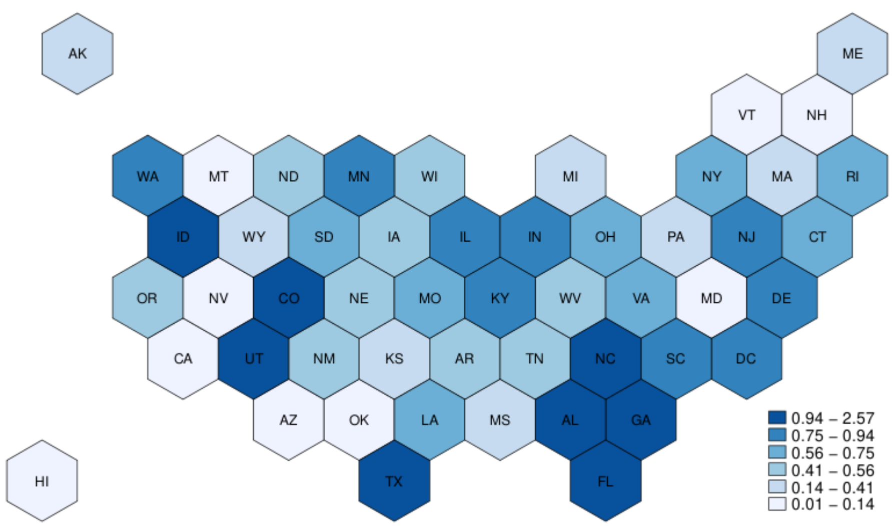
Table A3. Analysis sample accounting

	No UI after first job loss	UI after first job loss	Total
Good UI tracking state	56,331	51,850	108,181
	(52.07)	(47.93)	(100.00)
Bad UI tracking state	24,923	1,643	26,566
	(93.82)	(6.18)	(100.00)
Total	81,254	53,493	134,747
	(60.30)	(39.70)	(100.00)

Cross tab of those in good versus bad UI flagging states by those that do and do not receive UI in the analysis sample. The bad states are those with hard to identify UI inflows based on the level of false negatives from the survey design. UI receipt based on having at least one inflow memo equal with an unemployment insurance regular expression within six months of job loss.

A Appendix Figures

Figure A.1. Fraction of UI Recipients by State



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

Table A4. Summary Statistics of various series for analysis sample

	Mean	SD	Min	Max	N
P2P Platforms					
P2P inflows	375.51	998.95	0.00	437,828.22	3951446
Zelle inflows	181.14	639.24	0.00	100,000.00	3951446
Cashapp inflows	84.45	382.20	0.00	90,747.50	3951446
Venmo inflows	50.75	258.61	0.00	28,651.95	3951446
Paypal inflows	28.20	450.81	0.00	422,891.22	3951446
P2P outflows	545.47	977.81	0.00	259,316.20	3951446
Zelle outflows	220.21	643.92	0.00	56,441.64	3951446
Cashapp outflows	176.95	496.57	0.00	34,713.00	3951446
Venmo outflows	68.70	276.97	0.00	17,862.00	3951446
Paypal outflows	60.34	384.80	0.00	253,996.20	3951446
P2P flows less Sales, Earnin, Taxation Memos					
P2P inflows	328.78	866.91	0.00	164,199.45	3951446
Zelle inflows	168.22	584.70	0.00	70,936.87	3951446
Cashapp inflows	67.28	345.03	0.00	52,875.71	3951446
Venmo inflows	50.49	257.83	0.00	28,651.95	3951446
Paypal inflows	22.43	290.75	0.00	163,743.14	3951446
P2P outflows	462.85	911.03	0.00	223,741.84	3951446
Zelle outflows	212.30	630.25	0.00	43,041.64	3951446
Cashapp outflows	121.16	413.25	0.00	34,712.00	3951446
Venmo outflows	68.18	275.46	0.00	17,862.00	3951446
Paypal outflows	47.83	345.51	0.00	223,741.84	3951446
Other memos					
Gig inflows	25.34	223.46	0.00	79,382.82	3951446
UI inflows	248.77	900.38	0.00	126,095.99	3951446
Non-UI inflows	4367.87	6248.63	0.00	1,768,090.36	3947942
Outflows	4551.14	5584.87	0.00	1,646,458.32	3947942
Earnings+UI	1979.06	1928.21	0.00	1,136,246.43	3951446
Earnings	1729.97	1872.94	0.00	1,136,246.43	3951446
Observations	3951446				

Summary statistics of P2P inflows for the analysis sample. Analysis sample includes those with at least four outflows per month who become unemployed between July 2019 and September 2020 and those who do not become unemployed until after September 2021 and are balanced in calendar time. P2P variables come from bank transactions that meet regular expression search criteria of bank memos orcategories by financial services company, Plaid, of the P2P platforms. The second set of P2P variables are those from transactions between $5 and $15,000 and not including a list of regular expressions associated with Earnin and other pay advance companies, sales, informal earnings, or other criteria. Furthermore, during the stimulus payment months, those payments that are stimulus amounts of money are excluded. UI inflows are those transactions for which regular expression searches of memos for words related to unemployment insurance. Non-UI inflows are the rest of inflows, while outflows are all outflows. Earnings are those inflows flagged as earnings from the earnings algorithm.

Table A5. Reduced form of P2P Inflows by UI during various pandemic policy events

Policy Change	March Delays (1)	July Expiration (2)	June Withdrawal (3)
Post x Treat	-0.14	-20.7	57.4*
	(16.7)	(15.3)	(30.2)
Standard-Errors	User		State
Observations	35,392	31,312	28,546
R^2	0.73819	0.75872	0.71922
User and Month fixed effects	✓	✓	✓

Reduced form difference-in-difference estimates of crowd-out of P2P Inflows UI receipt related to exogenous changes to UI benefits during the pandemic. Column (1) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (2) compares June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of $600 per week in UI benefits at the end of July 2020. Column (4)shows a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Standard errors in (1) and (2) are clustered by user and (3) by state.
$p < 0.1$, $p < 0.05$, *** $p < 0.01$

Table A6. Crowd-out of P2P Inflows by UI during various pandemic policy events

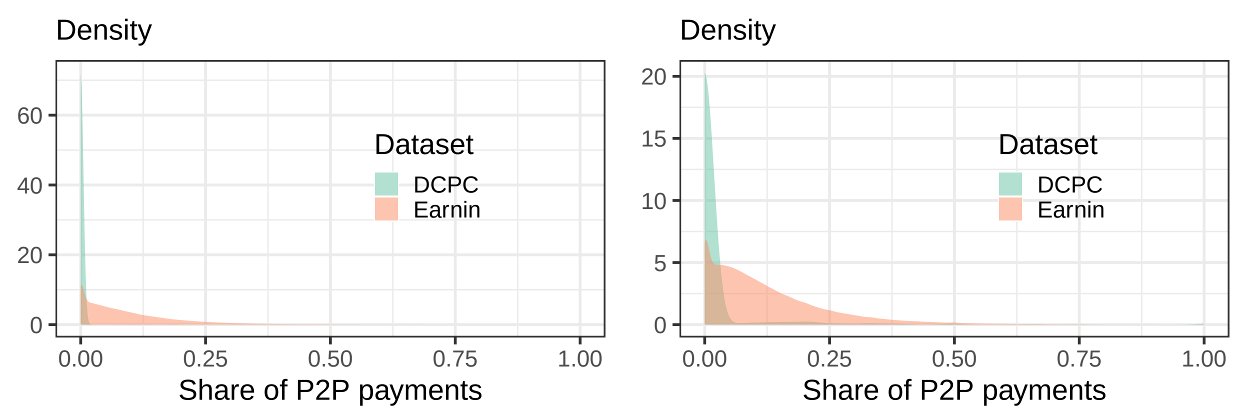
Method	OLS			IV		
Policy Change	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration	June Withdrawal
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	-0.02***	-0.05*	0.001	-0.03***	-0.09**	-0.03*
	(0.006)	(0.03)	(0.008)	(0.007)	(0.04)	(0.02)
Standard-Errors	User		State	User		State
Lower bound × \$100 in UI	0.86796	0.62501	0.93267	-4.1724	-16.200	-6.2903
Observations	35,392	31,312	28,546	35,392	31,312	28,546
R^2	0.79996	0.82404	0.80841	0.79994	0.82401	0.80804
F-test (1st stage), UI Inflows				98,485.7	45,058.2	4,680.8
User and Month fixed effects	✓	✓	✓	✓	✓	✓

Instrumental variable difference-in-difference estimates of crowd-out of logged P2P Inflows by logged unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of \$600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.

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

Figure A.2. Density of P2P share of financial transactions in Diary of Consumer Payment Choice vs. Earnin users

(a) 2019 (b) 2020



Notes: These show a comparison of the share of total non-cash flows that were P2P transactions in the months of October 2019 and 2020 in the Earnin dataset and the Diary of Consumer Payment Choice (DCPC). These shares are after cash transactions are removed from both datasets as these are not accounted for similarly between the two datasets.

Table A7. Crowd-out of P2P Inflows by UI during various pandemic policy events

Policy Change	March Delays (1)	July Expiration (2)	June Withdrawal (3)
UI Inflows	6.1×10^{-6} (1.2×10^{-5})	$5.4 \times 10^{-6*}$ (2.9×10^{-6})	$-1.8 \times 10^{-5**}$ (9.4×10^{-6})
Standard-Errors	User		State
Lower bound × \$100 in UI	-0.00177	-4×10^{-5}	-0.00371
Observations	24,226	22,220	21,500
User and Month fixed effects	✓	✓	✓

Poisson estimates of crowd-out of P2P Inflows on the extensive margin by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Column (1) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (2) compares June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of \$600 per week in UI benefits at the end of July 2020. Column (4)shows a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Standard errors in (1) and (2) are clustered by user and (3) by state.

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

Table A8. Crowd-out of P2P Inflows by UI during various pandemic policy events conditional on using service

Method	OLS			IV		
Policy Change	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration	June Withdrawal
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	0.01**	0.01	0.07***	-0.0003	0.09***	0.08
	(0.006)	(0.009)	(0.02)	(0.009)	(0.02)	(0.05)
Standard-Errors	User		State	User		State
Lower bound × \$100 in UI	0.07919	-0.63889	3.0632	-1.8868	4.4405	-2.0573
Observations	16,230	15,862	15,582	16,230	15,862	15,582
R^2	0.00026	0.00081	0.00213	-1.3×10^{-5}	-0.02873	0.00204
Month fixed effects	✓	✓	✓	✓	✓	✓

Difference-in-difference estimates of crowd-out of P2P Inflows on the extensive margin by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic conditional on receiving P2P Inflows in both months. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of \$600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.

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

Table A9. Crowd-out of Zelle by UI during various pandemic policy events

Method	OLS			IV		
Policy Change	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration	June Withdrawal
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	-0.0002	0.004***	-0.01***	0.003	0.010**	-0.03
	(0.003)	(0.001)	(0.004)	(0.004)	(0.004)	(0.02)
Standard-Errors	User		State	User		State
Lower bound × \$100 in UI	-0.55803	0.20356	-2.0389	-0.50927	0.07745	-5.8857
Observations	35,392	31,312	28,546	35,392	31,312	28,546
R^2	0.75283	0.76855	0.71318	0.75281	0.76845	0.71308
F-test (1st stage), UI Inflows				28,828.8	7,541.0	5,083.0
User and Month fixed effects	✓	✓	✓	✓	✓	✓

Instrumental variable difference-in-difference estimates of crowd-out of Zelle by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of \$600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.

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

Table A10. Crowd-out of Venmo by UI during various pandemic policy events

Method	OLS			IV		
Policy Change	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration	June Withdrawal
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	-0.0003	-0.0002	0.0008	-0.002	-0.002	-0.0007
	(0.001)	(0.0005)	(0.002)	(0.001)	(0.002)	(0.004)
Standard-Errors	User		State	User		State
Lower bound × \$100 in UI	-0.23075	-0.11733	-0.21291	-0.42956	-0.61093	-0.93799
Observations	35,392	31,312	28,546	35,392	31,312	28,546
R^2	0.75126	0.73472	0.74650	0.75124	0.73464	0.74649
F-test (1st stage), UI Inflows				28,828.8	7,541.0	5,083.0
User and Month fixed effects	✓	✓	✓	✓	✓	✓

Instrumental variable difference-in-difference estimates of crowd-out of Venmo by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of \$600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.

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

Table A11. Crowd-out of PayPal by UI during various pandemic policy events

Method	OLS			IV		
Policy Change	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration	June Withdrawal
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	0.001	0.0005	-0.0004	0.002	0.003	-0.004
	(0.002)	(0.0007)	(0.003)	(0.004)	(0.002)	(0.006)
Standard-Errors	User		State	User		State
Lower bound × \$100 in UI	-0.19415	-0.09568	-0.56997	-0.47396	-0.08267	-1.5952
Observations	35,392	31,312	28,546	35,392	31,312	28,546
R^2	0.56681	0.65054	0.61552	0.56679	0.65026	0.61546
F-test (1st stage), UI Inflows				28,828.8	7,541.0	5,083.0
User and Month fixed effects	✓	✓	✓	✓	✓	✓

Instrumental variable difference-in-difference estimates of crowd-out of PayPal by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of \$600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.

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

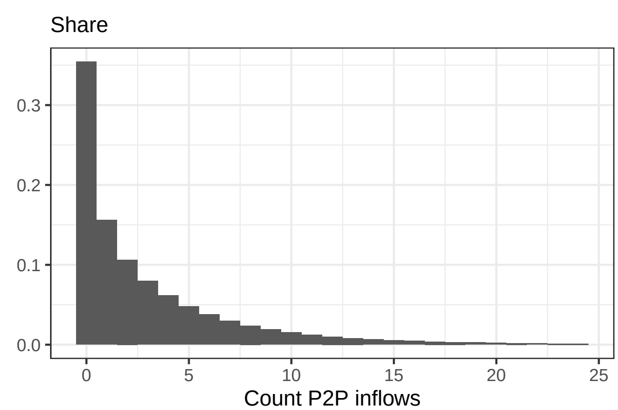
Table A12. Crowd-out of Cashapp by UI during various pandemic policy events

Method	OLS			IV		
Policy Change	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration	June Withdrawal
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	0.003*	0.0002	0.0003	0.002	0.003	-0.008
	(0.002)	(0.0008)	(0.004)	(0.002)	(0.003)	(0.008)
Standard-Errors	User		State	User		State
Lower bound × \$100 in UI	-0.01475	-0.13757	-0.66499	-0.25755	-0.25662	-2.3797
Observations	35,392	31,312	28,546	35,392	31,312	28,546
R^2	0.67741	0.71108	0.67830	0.67741	0.71100	0.67812
F-test (1st stage), UI Inflows				28,828.8	7,541.0	5,083.0
User and Month fixed effects	✓	✓	✓	✓	✓	✓

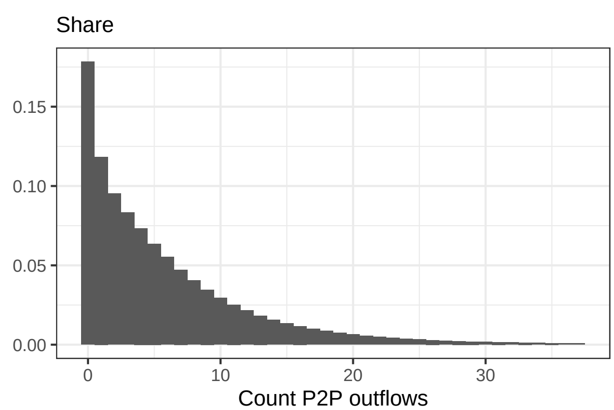
Instrumental variable difference-in-difference estimates of crowd-out of Cashapp by unemployment insurance (UI) using different plausibly exogenous changes to UI benefits during the pandemic. Columns (1) and (2) show a sample of March 2020 job losers in March 2020 and May 2020 comparing those who receive UI in April 2020 vs. June 2020, (3) and (4) compare June 2020 and August 2020 P2P inflows for March 2020 job losers receiving UI by June 2020 to those that become unemployed after December 2020 to leverage the expiration of \$600 per week in UI benefits at the end of July 2020. Columns (5) and (6) show a sample of users that are unemployed and insured at the end of April 2021 comparing P2P inflows in April 2021 to August 2021. P2P inflows as measured using regular expression flags of bank memos. Income is equal to earnings plus UI inflows plus other government payments like stimulus checks. Odd columns are OLS results, even columns are IV results. Standard errors in (1)-(4) are clustered by user and (5)-(6) by state.
$p < 0.1$, $p < 0.05$, *** $p < 0.01$

Figure A.3. Histograms of monthly inflow and outflow dollars and counts

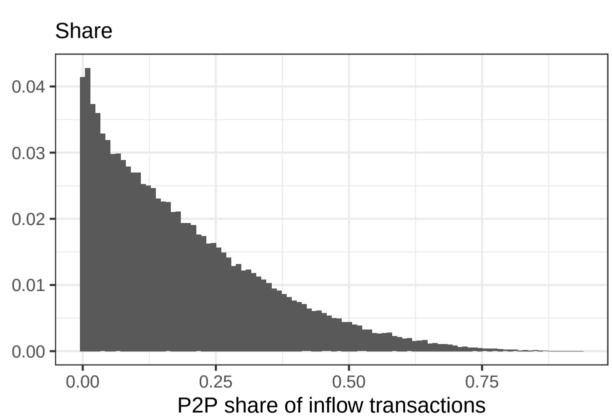
(a) Monthly P2P inflow transaction count



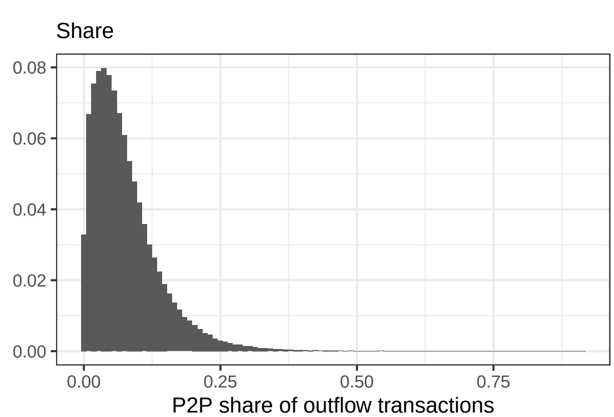
(b) Monthly P2P outflows transaction count



(c) P2P share of total inflows transaction count

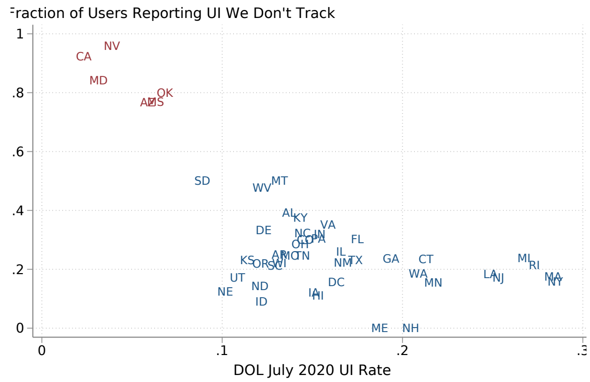


(d) P2P share of total outflows transaction count



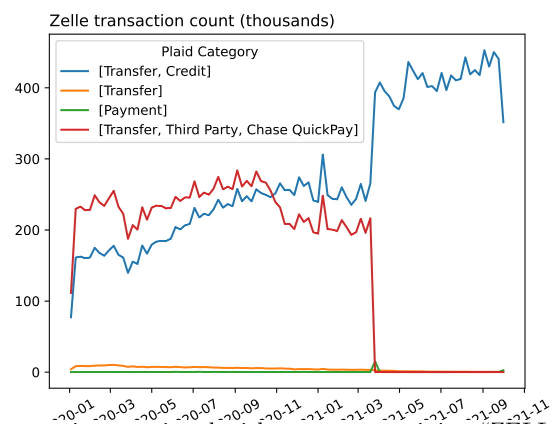
Notes: Figures (a) and (b) show the total P2P inflows and outflows, respectively, for each user-month in the data. Figures (c) and (d) show the share of total inflows and outflows, respectively, exchanged on P2P platforms.

Figure A.4. False negative UI from survey by population insured rate by state



Notes: July 2020 DOL UI rate vs. fraction of false negatives based on survey – drop states in red.

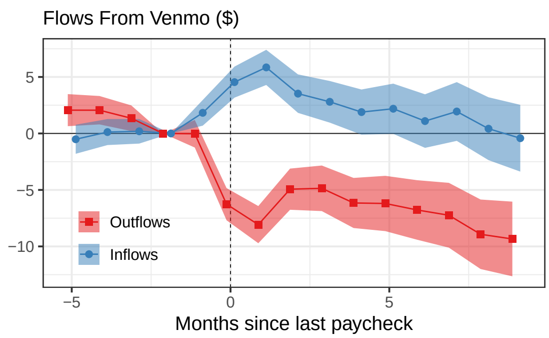
Figure A.5. Plaid Categories associated with memos mentioning Zelle over time



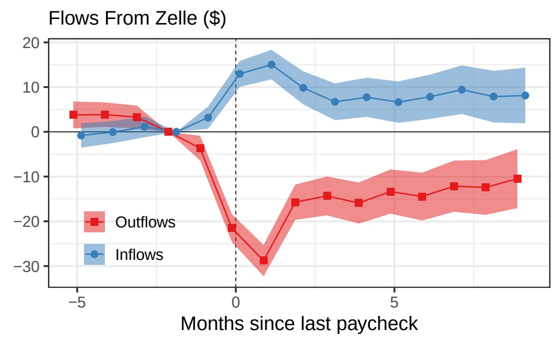
Notes: Isolating common categories associated with memos containing "ZELLE" around early 2021. The drop off in [Transfer, Third Party, Chase QuickPay], suggests that Plaid abandoned the category in late 2021.

Figure A.6. P2P inflows by platform

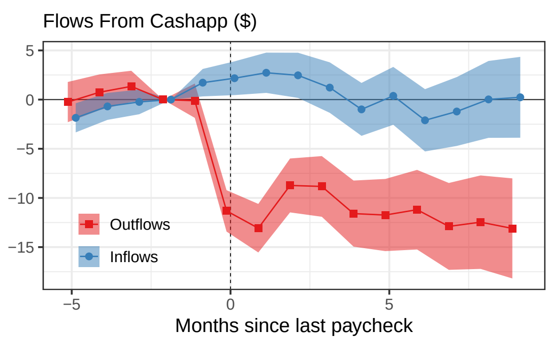
(a) Venmo



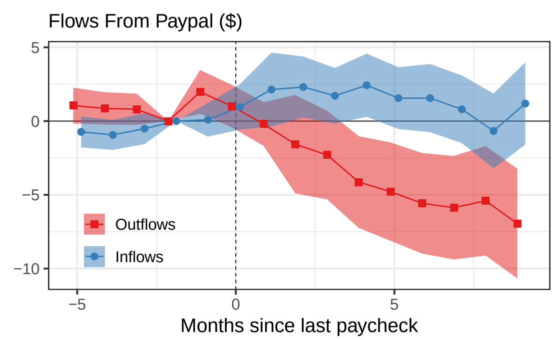
(b) Zelle



(c) CashApp



(d) PayPal

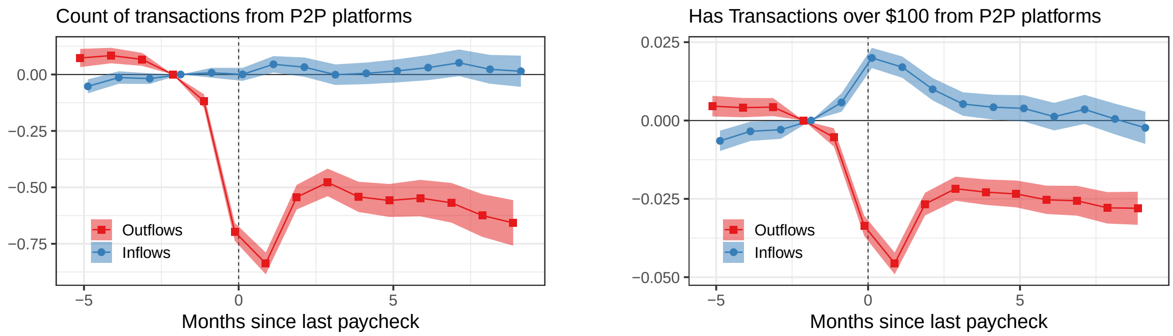


Notes: Within-person event studies of major P2P platform inflows and outflows around a user's first unemployment spell by platform. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.7. Extensive margin of individual P2P transactions

(a) Count of P2P transactions

(b) $\mathbb{P}$(P2P transaction > 100)

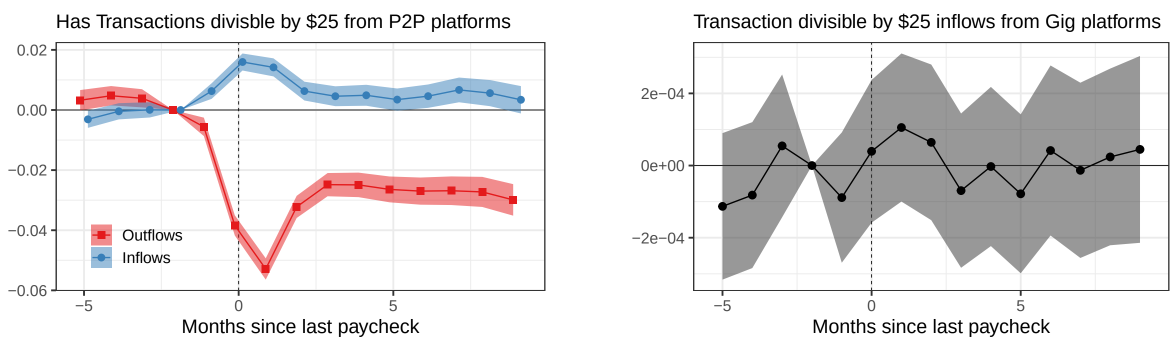


Notes: Within-person event studies of the count of inflow and outflow transactions and probability of having transactions greater than $100 in a given month. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.8. Extensive margin of individual P2P transactions

(a) $\mathbb{P}$(P2P transaction divible by 25)

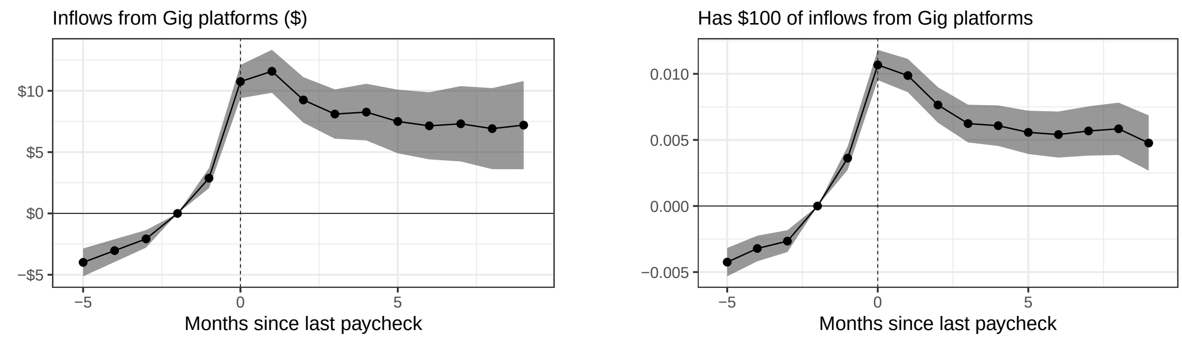
(b) $\mathbb{P}$(Gig earning transaction divible by 25)



Notes: Within-person event studies of the probability that user has P2P inflows, P2P outflows, or gig earnings divisible by $25 in a month. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.9. Intensive and Extensive Margin of Gig Work

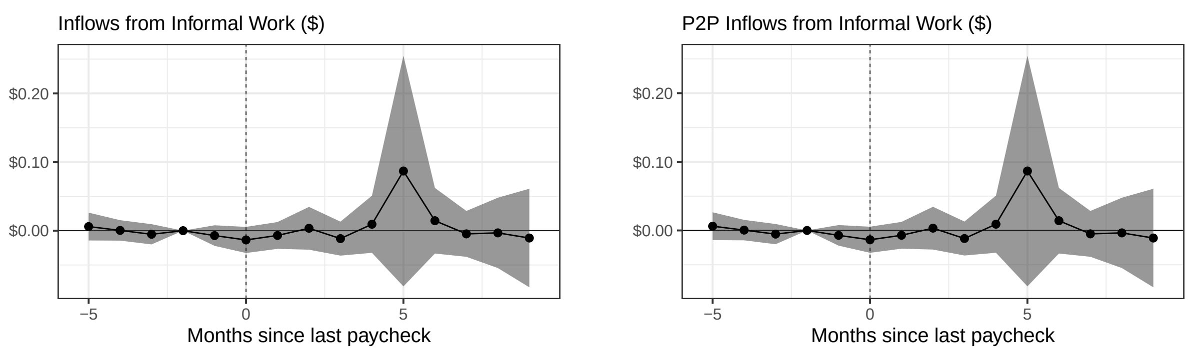
(a) Gig Earnings (b) $\geq$ 100 Gig Earnings



Notes: Within-person event studies of gig earnings and probability that user has at least $100 of gig earnings around a user's first unemployment spell. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.10. Event studies of informal work earnings around job loss

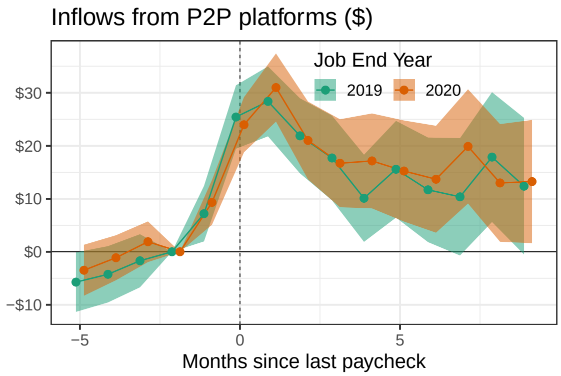
(a) Informal Work (b) P2P Informal Work



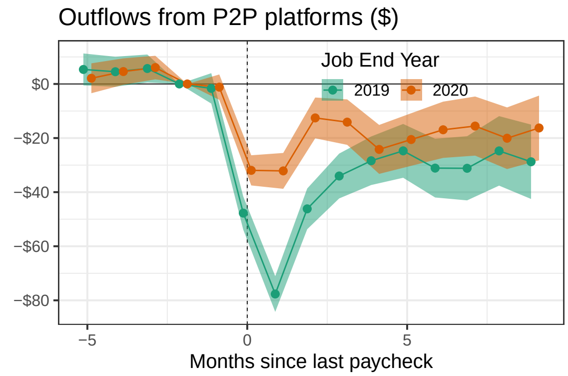
Notes: Within-person event studies of inflows from transactions that are likely informal earnings around a user's first unemployment spell. Informal earnings transactions mention "hours worked," "babysitting," or "yard work." Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.11. Event studies of all P2Ps by year of job loss

(a) Inflows



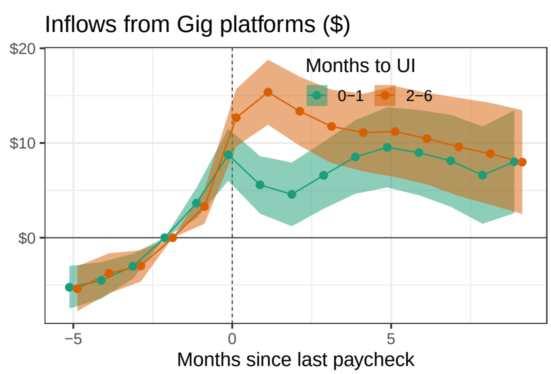
(b) Outfows



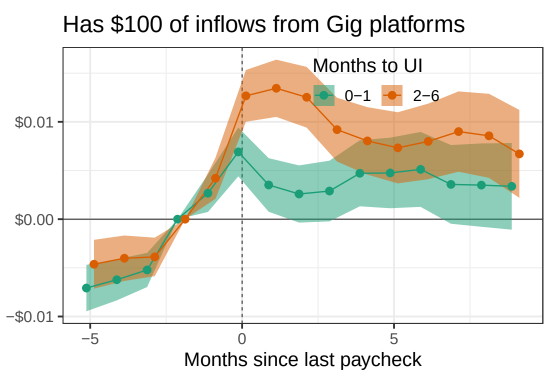
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of year of user's first job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have.

Figure A.12. Intensive and Extensive Margin of Gig Work With Time to UI Receipt

(a) Gig Earnings

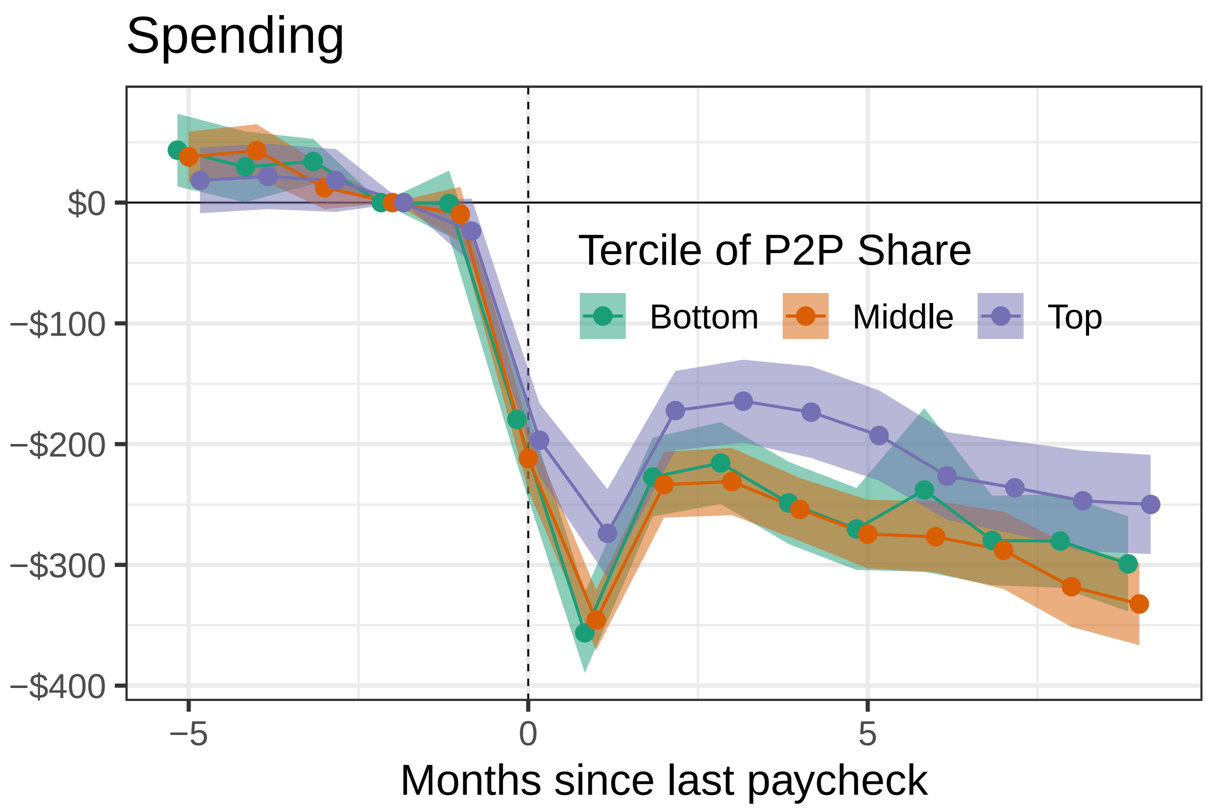


(b) $\geq$ 100 Gig Earnings

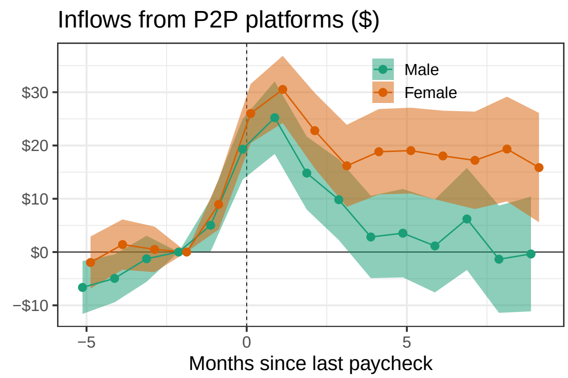


Notes: Within-person event studies of gig earnings and probability that gig earnings exceed $100 around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of bins for the months to UI since job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

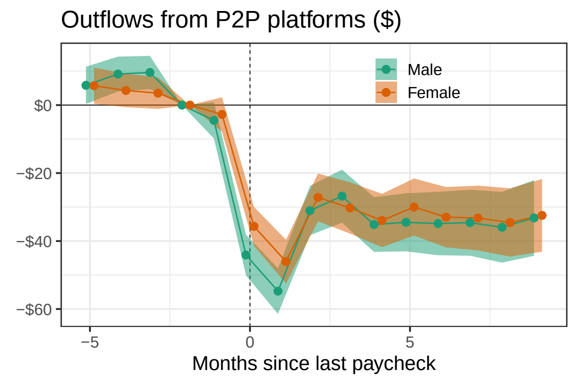
Figure A.13. Event study of consumption by tercile of P2P share of inflows and outflows prior to job loss



Notes: Within-person event studies of spending a user's first unemployment spell. Changes are relative to the level two months prior to job loss. Coefficients are relative time dummies are interacted with tercile of median monthly P2P share of all inflows more than one month before job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.



(a) Inflows

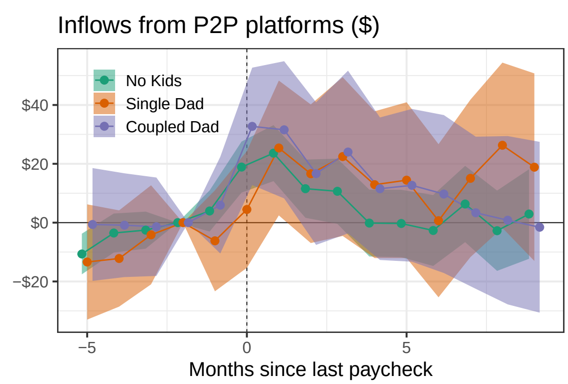


(b) Outflows

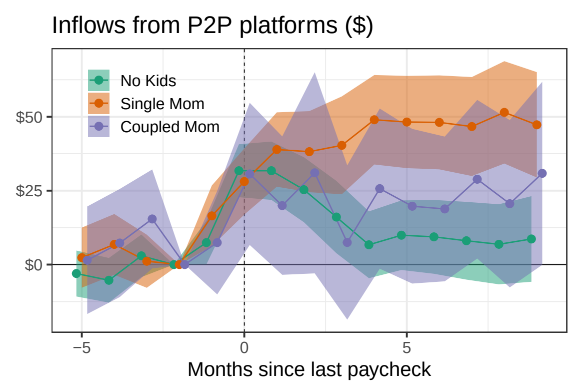
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with gender of user. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

Figure A.14. Event study coefficients based on family composition and gender

(a) Men



(b) Women

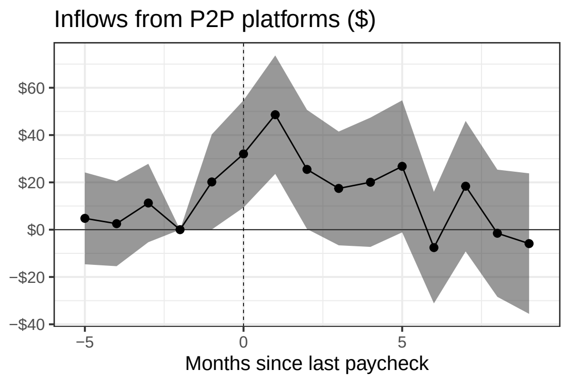


Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with gender of user and whether the user is single, a single parent, or a coupled parent. Gender and family composition as determined by survey response, observed receipt of CTC, or stimulus payment amount. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

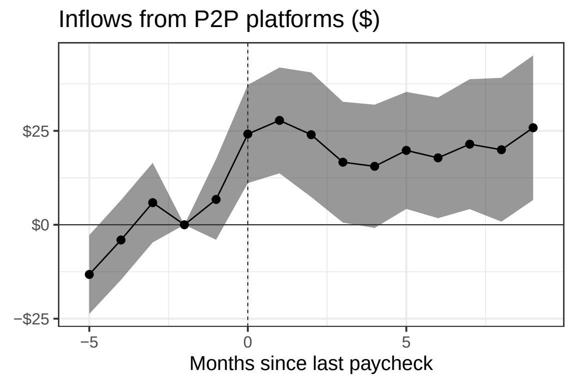
Notes: P2P inflows and outflows by gender and family composition as determined by survey response, observed receipt of CTC, or stimulus payment amount.

Figure A.15. Event studies of P2P inflows by NAICS codes

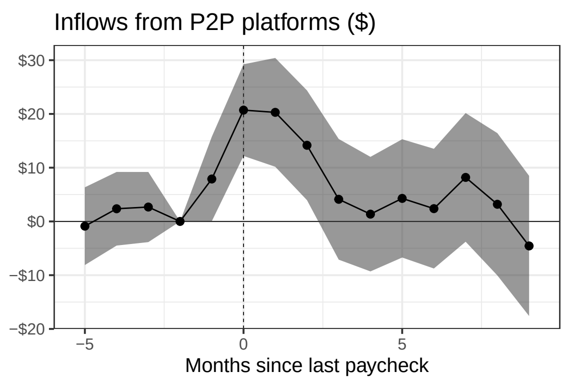
(a) Educational Services



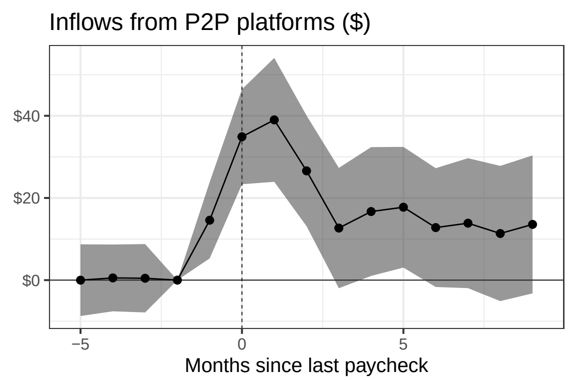
(b) Healthcare & Social Services



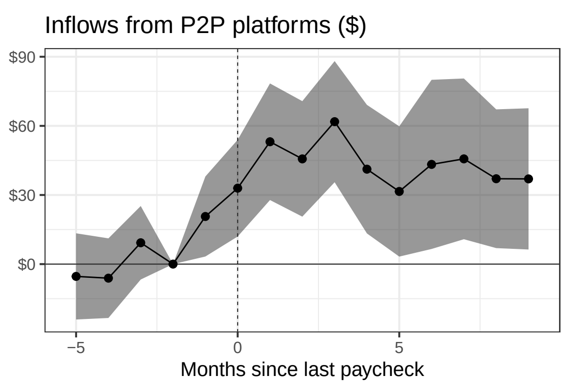
(c) Retail Trade



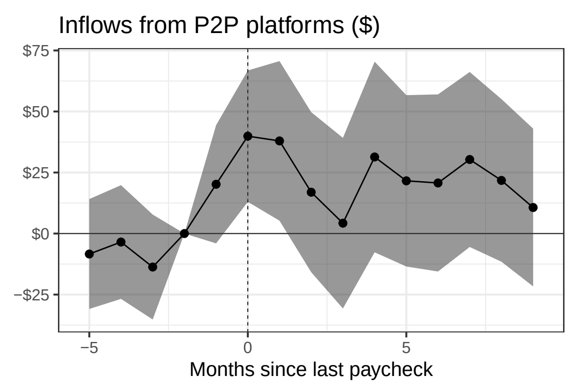
(d) Accommodation and Food Services



(e) Transportation & Warehousing



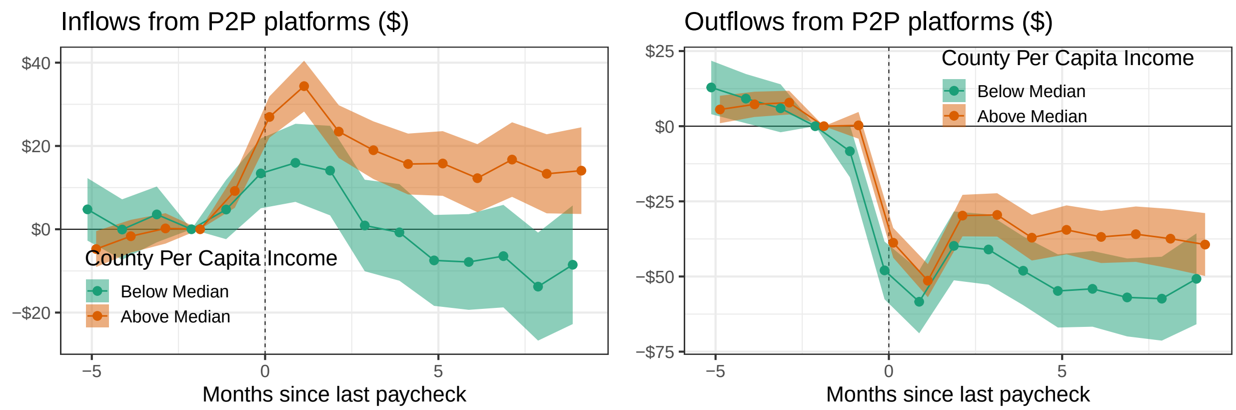
(f) Arts, Entertainment, Recreation



Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator for NAICS code of user's last job before job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.16. Event study coefficients by whether user lives in county above or below median per capita income

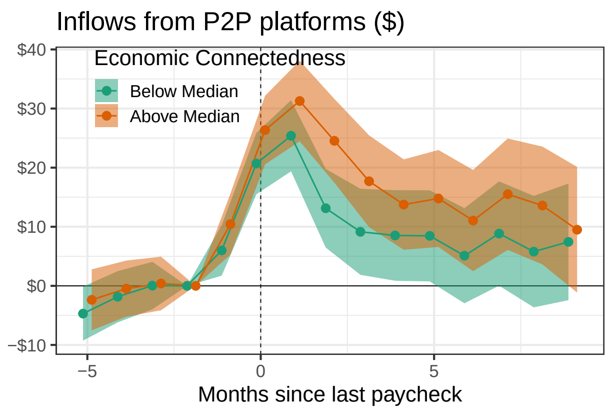
(a) Inflows (b) Outflows



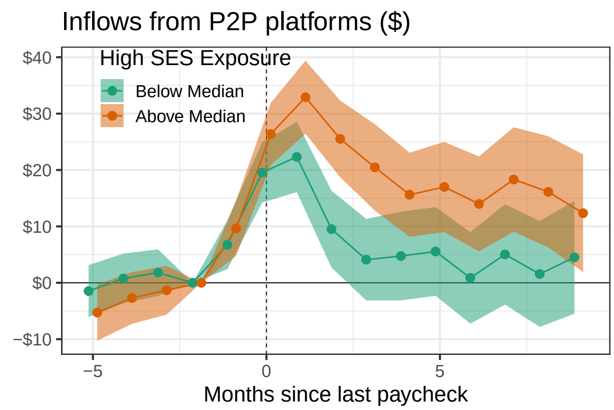
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator for living in an above or below median per capita household income county as measured by the American Community Survey 2019 5-year. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.17. Event study coefficients by indicators of being in above median measures of economic connectedness in Chetty et al. (2022)

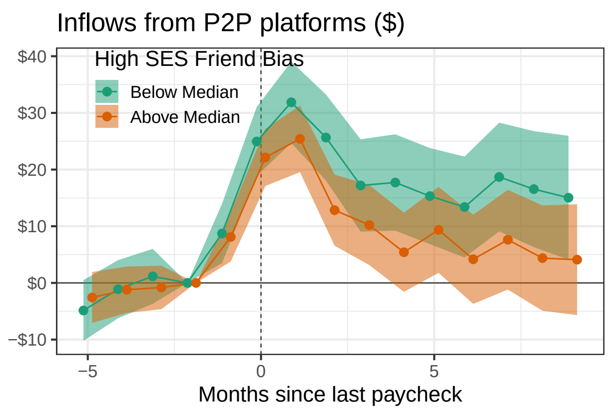
(a) Economic Connectedness



(b) Exposure



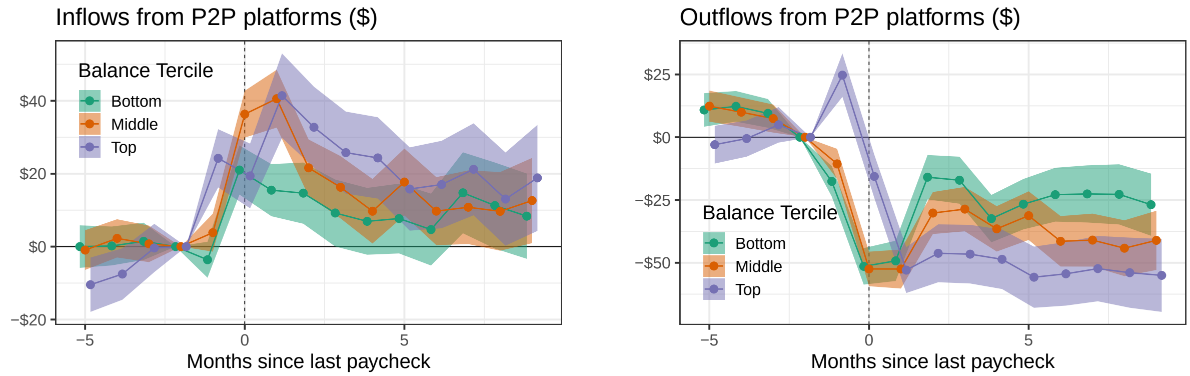
(c) Friending Bias



Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator for whether the user lives in area that is above or below the median value of economic connectedness, exposure, and friending bias from the social capital dataset produced by Chetty et al. (2022). Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.18. Event studies of P2P inflows by tercile of bank balance at job loss

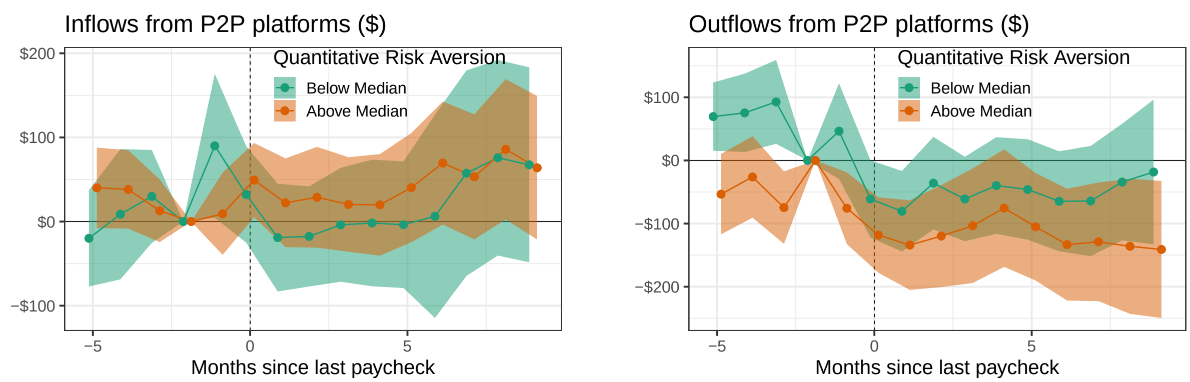
(a) Inflows (b) Outflows



Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with tercile of bank balance in the month before job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.19. Event studies of P2P inflows by whether user has above or below median risk aversion

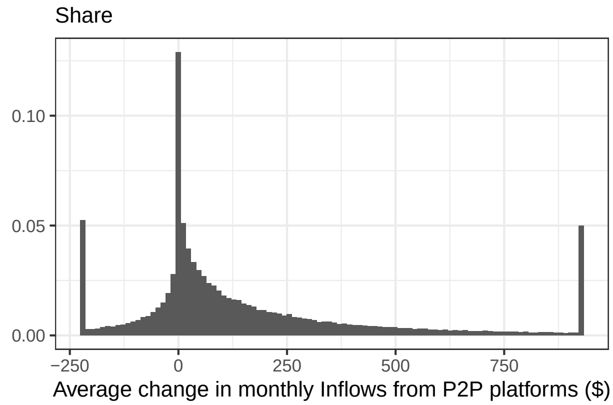
(a) Inflows (b) Outflows



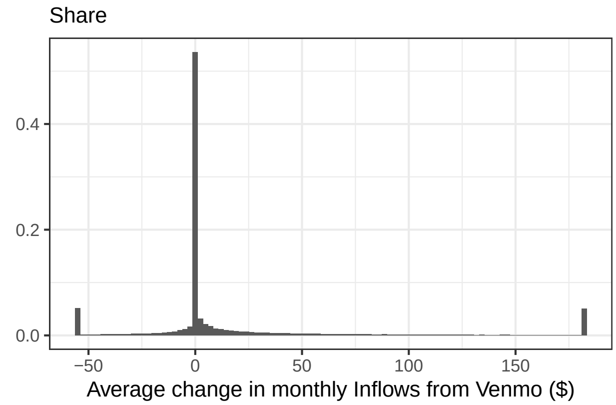
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with whether user has above or below median risk aversion based on telescoping question in survey of Earnin users conducted in August 2020. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.20. Distributions of average change in monthly P2P inflows after job loss

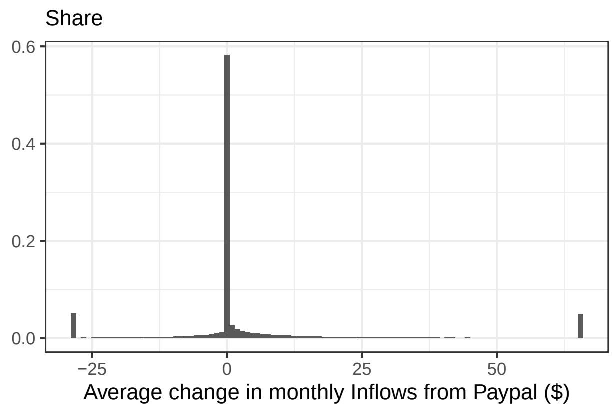
(a) All P2P inflows



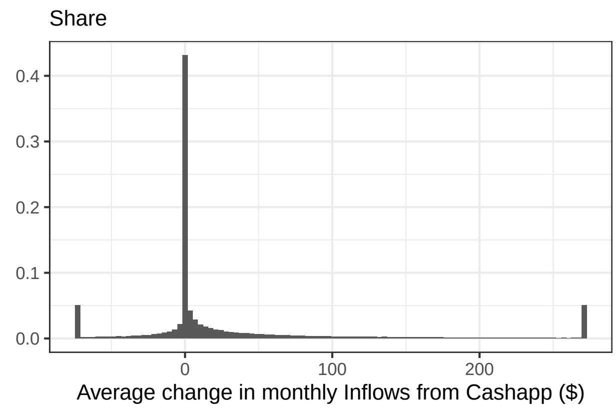
(b) Venmo



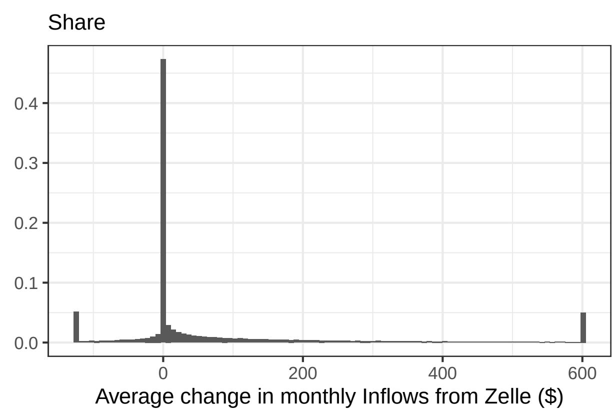
(c) Paypal



(d) Cashapp



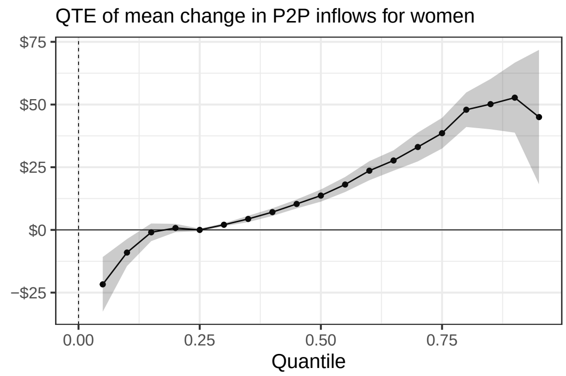
(e) Zelle



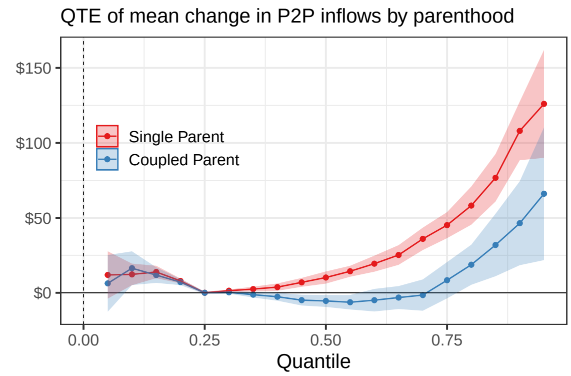
Notes: Each observation is the average change in a user's monthly inflows on different P2P platforms after they lose their job. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.21. Quantile treatment effects of different group indicators on P2P inflows

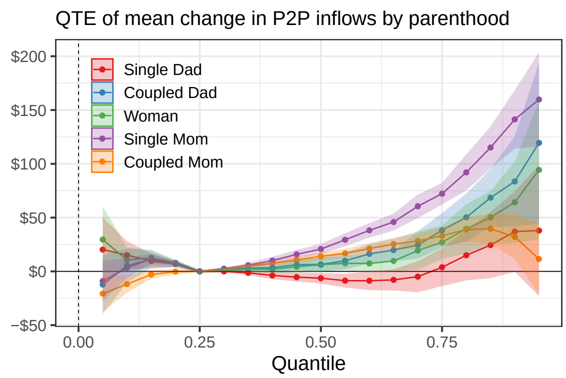
(a) Gender



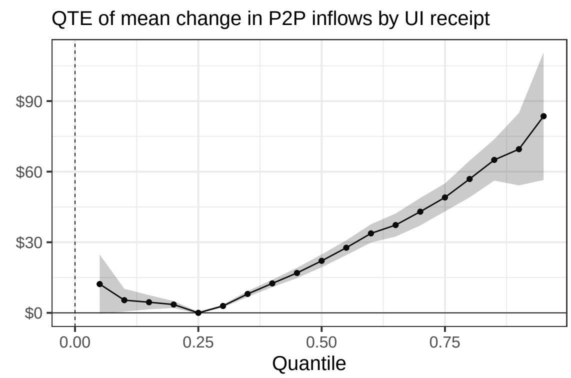
(b) Parents



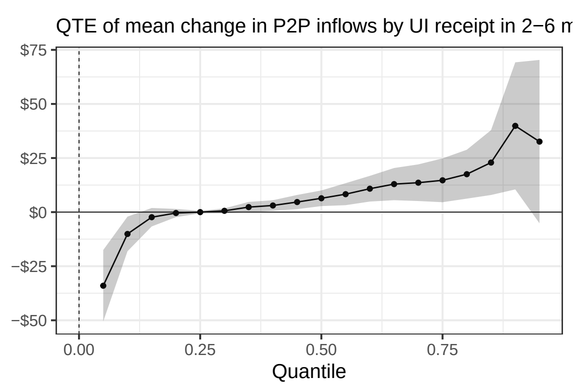
(c) Parent × Gender



(d) UI Receipt



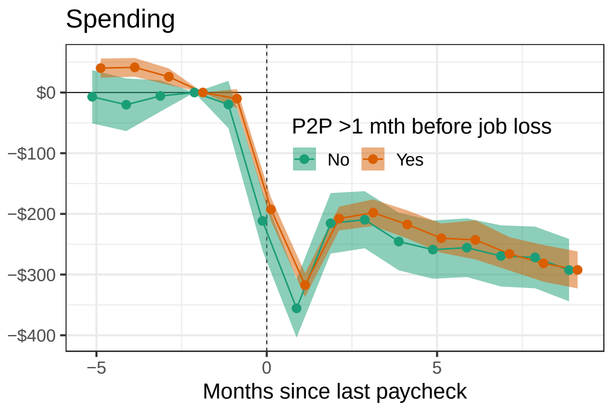
(e) Time to UI



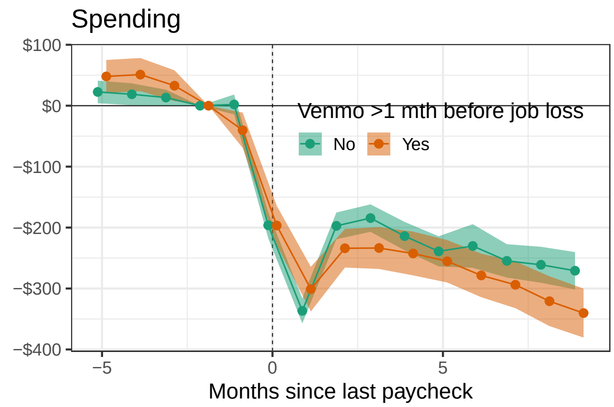
Notes: Quantile treatment effects of different group indicators on the average change in P2P inflows before and after job loss. Standard errors bootstrapped at user level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.22. Event study of consumption by use of P2P prior to job loss

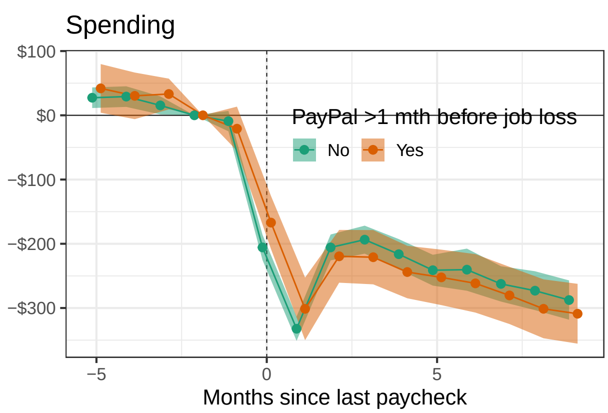
(a) Any P2P Platforms



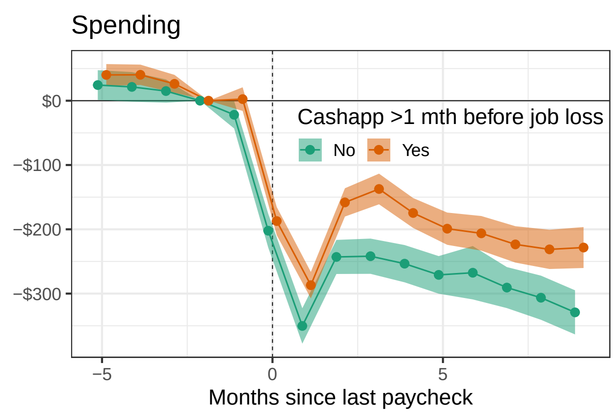
(b) Venmo



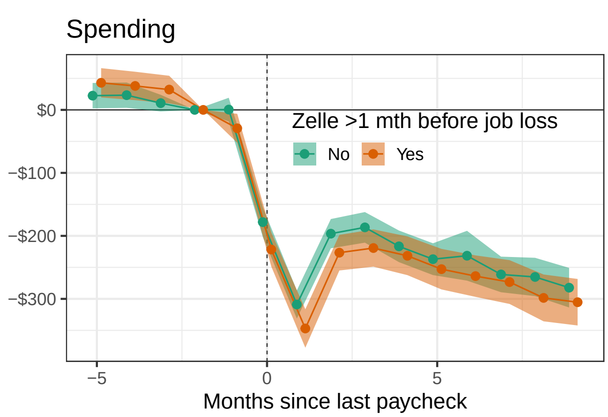
(c) Paypal



(d) Cashapp



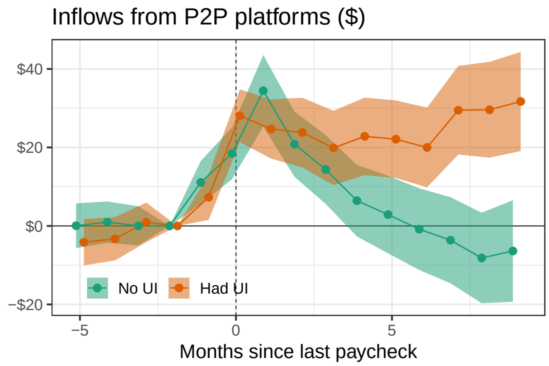
(e) Zelle



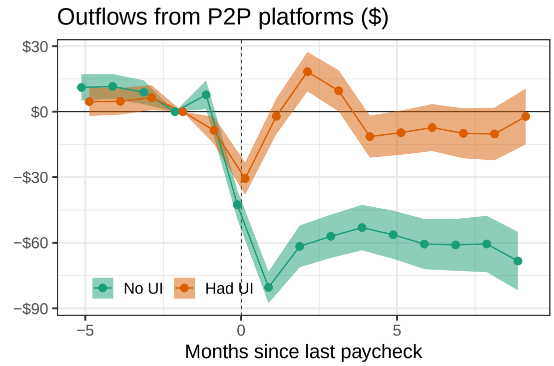
Notes: Within-person event studies of spending a user's first unemployment spell. Changes are relative to the level two months prior to job loss. Coefficients are relative time dummies are interacted with indicators for whether the user had inflows from the relevant platform in at least month prior to job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.23. P2P Inflows & Outflows by UI receipt during unemployment

(a) P2P Inflows

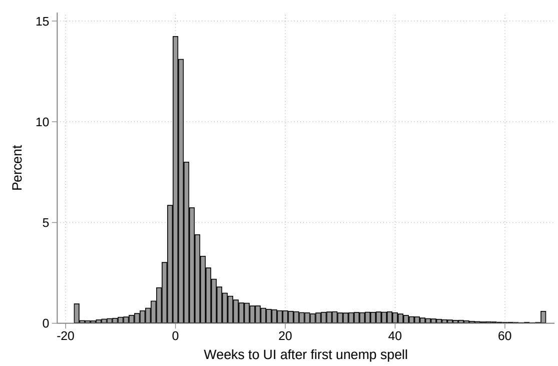


(b) P2P Outflows

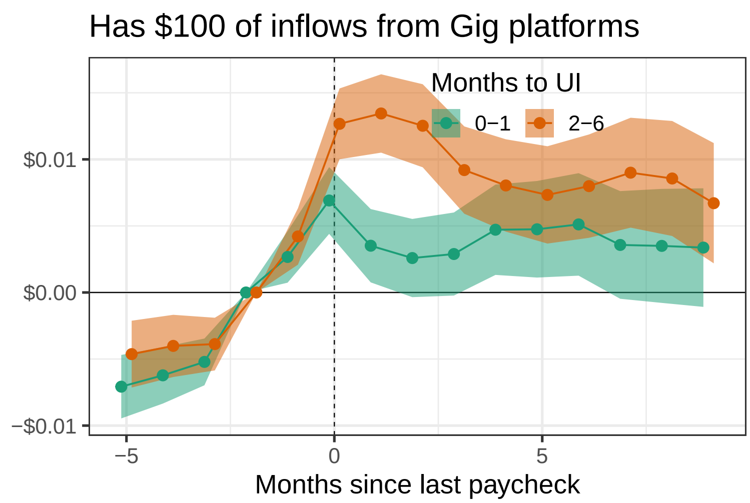


Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of for whether user received UI within six months of first job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

Figure A.24. Weeks between first job loss and nearest unemployment insurance spell start



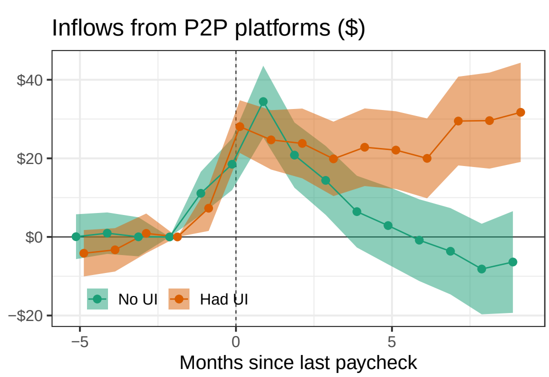
Notes: Weeks to nearest unemployment insurance (UI) spell after first job loss. Job loss timing defined as going five weeks since last paycheck. UI spell start defined as going three weeks without a UI benefit deposit. The weeks are to the UI spell that starts in the fewest weeks relative to the job loss date. Sample includes users with at least five outflows per month and a job loss in the months July 2019 through September 2020 or in September 2021.



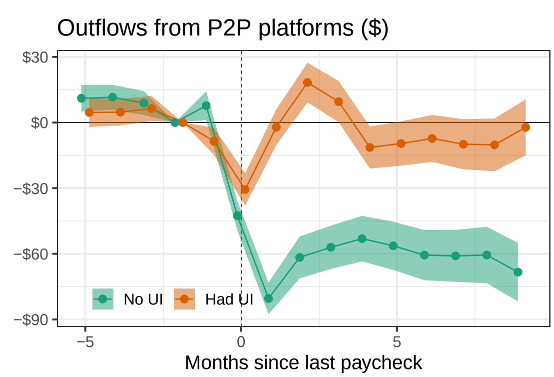
Notes: Within-person event studies of gig earnings around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of bins for the months to UI since job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

Figure A.25. P2P Inflows & Outflows by UI receipt during unemployment

(a) P2P Inflows



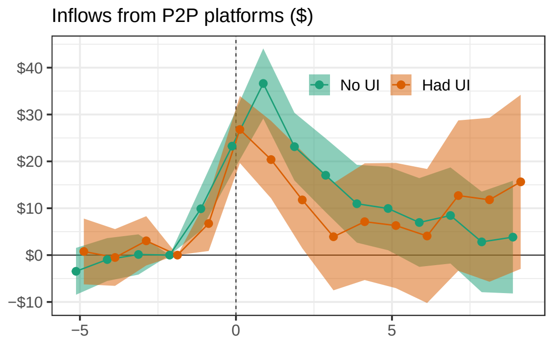
(b) P2P Outflows



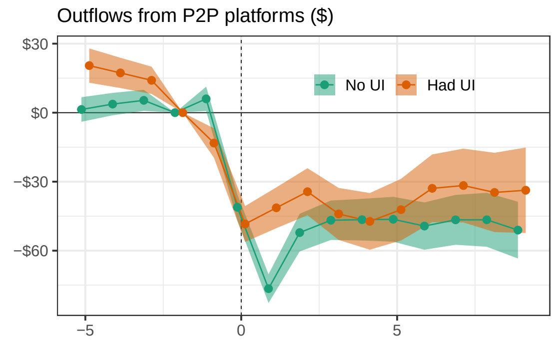
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of for whether user received UI within six months of first job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

Figure A.26. P2P Inflows & Outflows subset by UI receipt during unemployment

(a) P2P Inflows

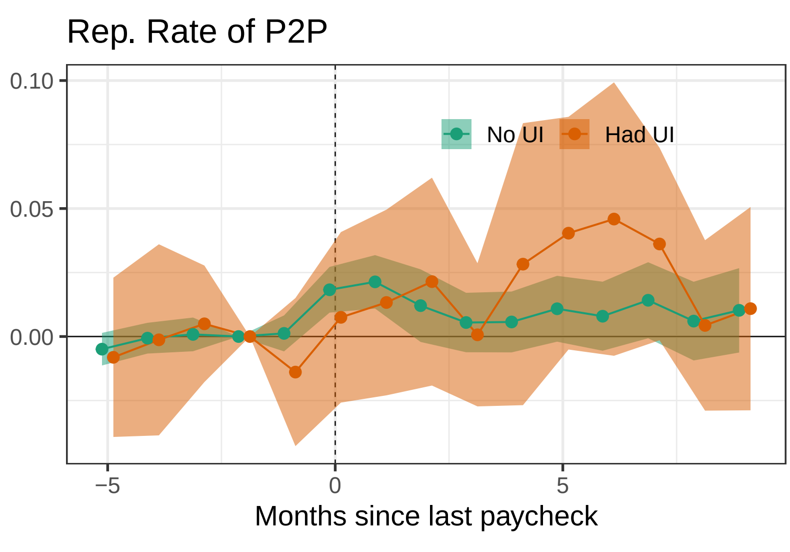


(b) P2P Outflows



Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies from regressions run on subsets based on whether user received UI within six months of first job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

Figure A.27. Event study of P2P replcaement rate by UI recipient

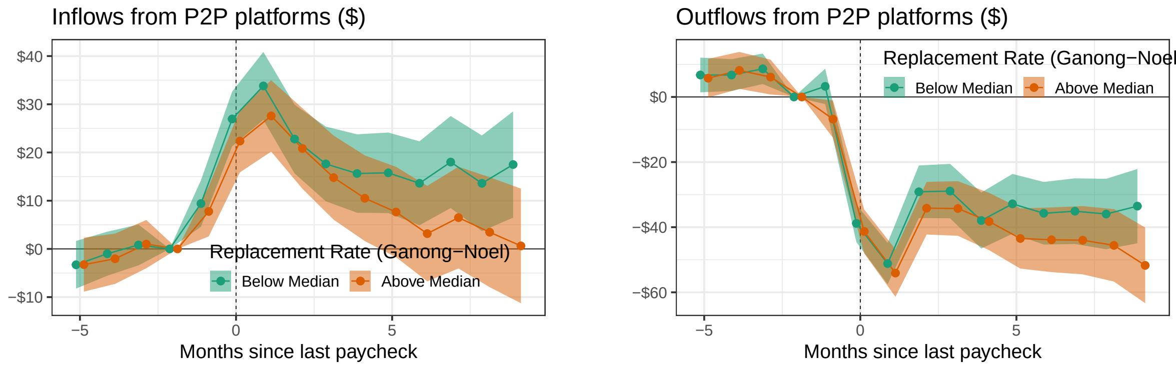


Notes: Within-person event studies of P2P inflows as a share of median monthly earnings prior to job loss around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of bins for the months to UI since job loss. Changes are relative to the level two months prior to job loss. Standard errors clustered at user-level. Sample includes users that become unemployed in six months with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021 and excluding users in states that do not have easily identifiable UI deposit memos.

Figure A.28. P2P Inflows by Above Median Replacement Rate

(a) Inflows

(b) Outflows

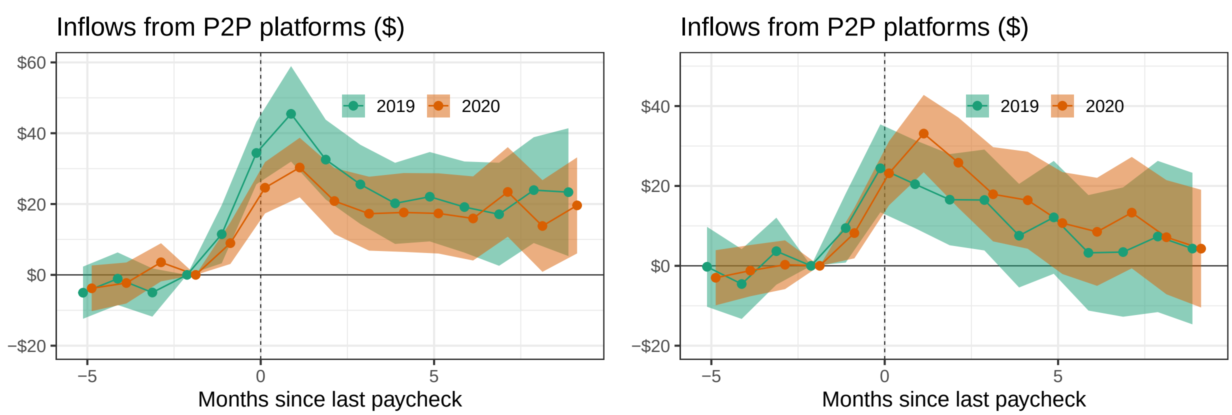


Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with whether user is above or below the median statutory replacement rate of pre-job loss earnings. Replacement rates are taken from calculations by Ganong et al. (2020a) using JP Morgan Chase Institute data and the Department of Labor Benefit Accuracy Measurement program. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.29. P2P Inflows by State Replacement Rate and Year of Job Loss

(a) Below Median

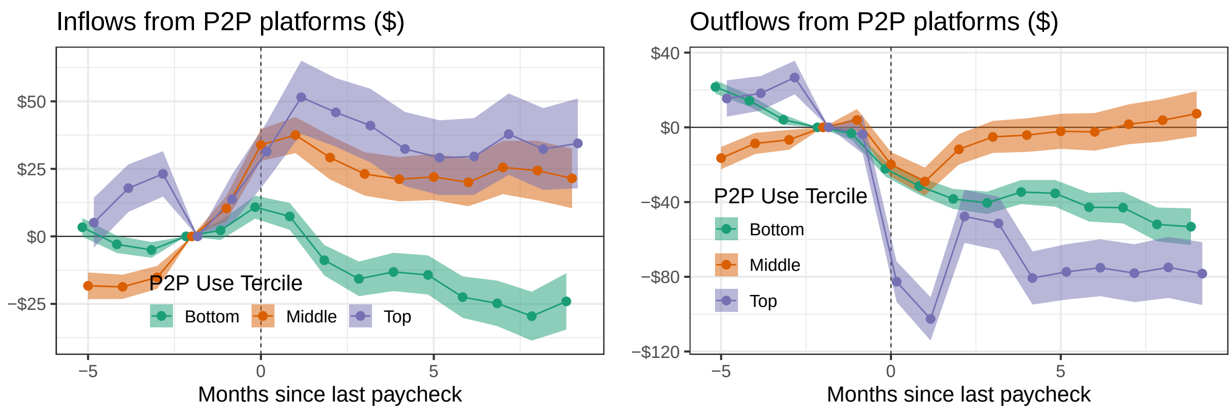
(b) Above Median



Notes: Figures shows event study of P2P inflows with coefficients 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. Median pre-job loss earnings replacement by Ganong et al. (2020b). Standard errors clustered at user-level.

Figure A.30. Event study of inflows and outflows interacted with tercile of P2P share prior to job loss

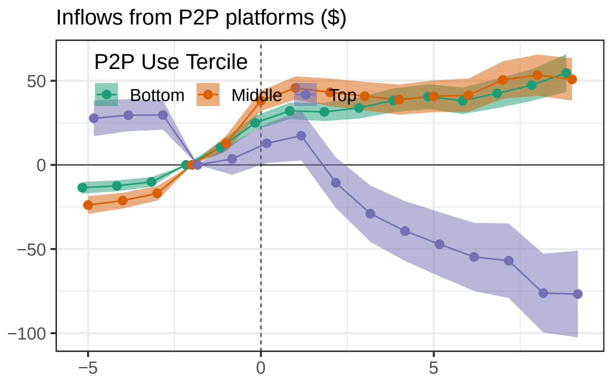
(a) Inflows, inflow share (b) Outflows, inflow share



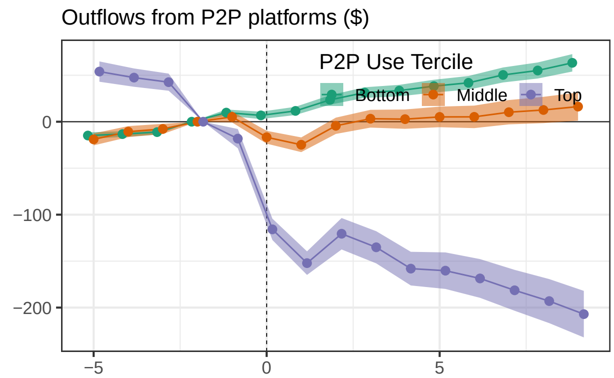
Notes: Event studies of P2P inflows and outflows around a user's first unemployment spell. Two months prior to job loss is omitted. The event study coefficients are relative time dummies interacted tercile of the median monthly P2P share of inflows two or more months before job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.31. Event study of inflows and outflows by subset of tercile of P2P share prior to job loss

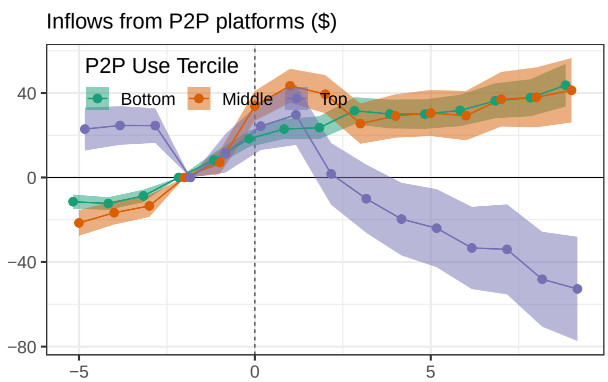
(a) Inflows, inflow share



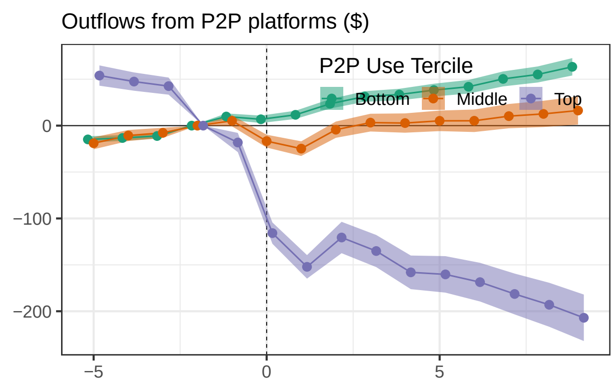
(b) Outflows, inflow share



(c) Inflows, cumulative share



(d) Outflows, cumulative share

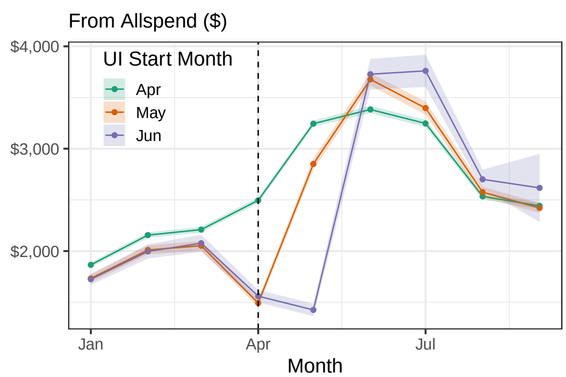


Notes: Event studies of P2P inflows and outflows around a user's first unemployment spell. Two months prior to job loss is omitted. The event study coefficients on relative time dummies from regressions on subsets tercile of the median monthly P2P share of cumulative flows ((a)-(b)) or inflows ((c)-(d)) two or more months before job loss. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

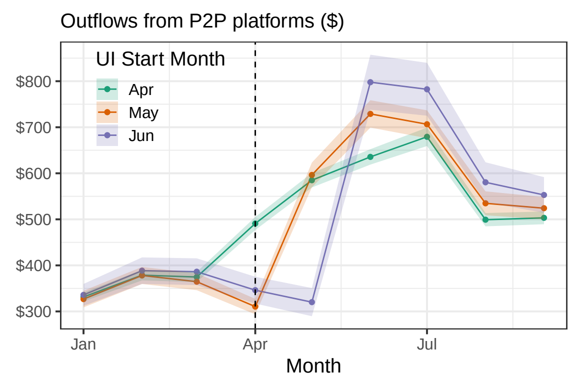
Figure A.32. Spending and P2P Outflows across different pandemic-related changes to UI benefit payments

March 2020 job losers by cohorts of job receipt

(a) Spending

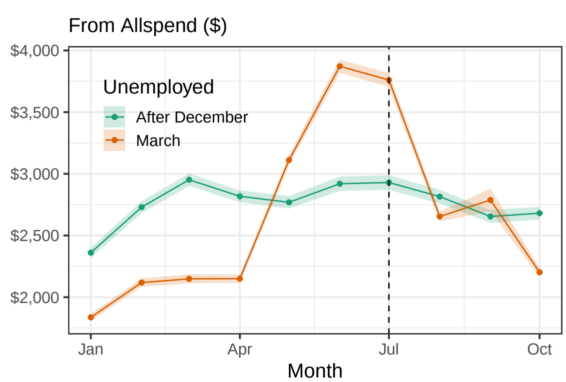


(b) P2P Outflows

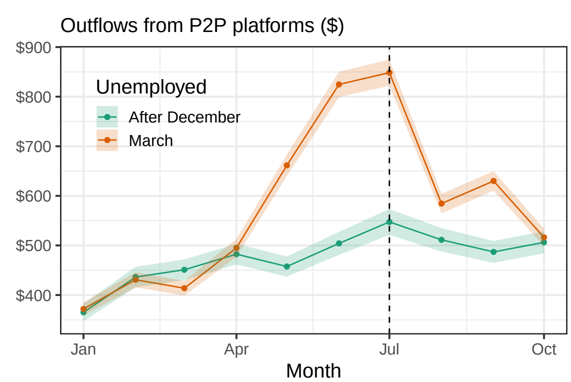


March 2020 job losers receiving UI vs. continuously employed through December 2020

(c) Spending

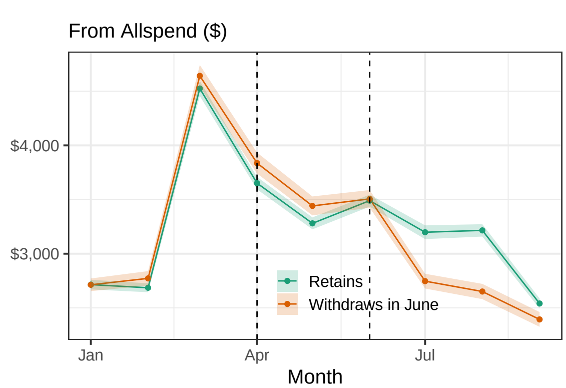


(d) P2P Outflows

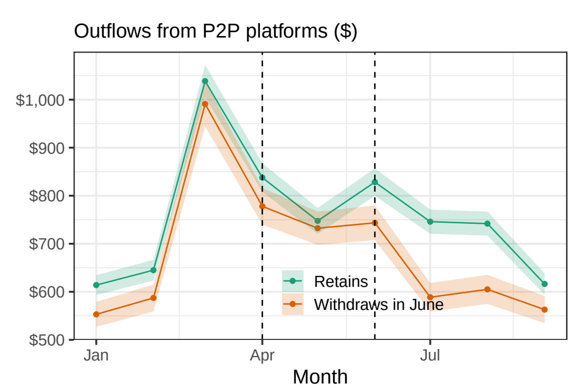


Unemployed and insured as of April 30 in Withdrawal vs. Retain States

(e) Spending



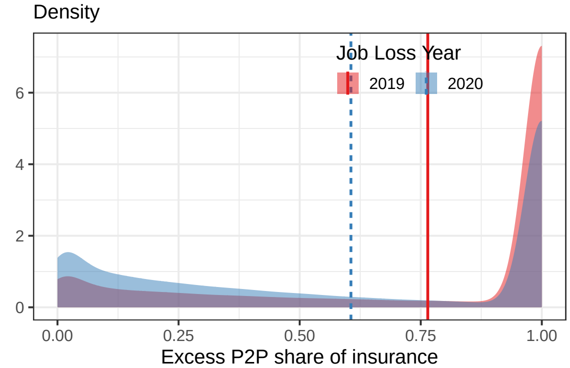
(f) P2P Outflows



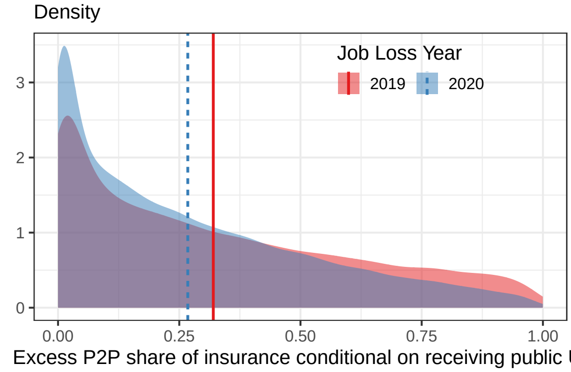
Notes: UI and P2P Outflows for different cohorts around three pandemic-related changes to UI benefits. Figures (a) and (b) show plots for cohort that lost jobs in March 2020 broken out by month they received UI benefits. Figures (c) and (d) compare the same cohort of March job losers workers that remained continuously employed through December 2020 around the July 2020 expiration of expanded UI benefits. Figures (e) and (f) compare workers and were unemployed and insured as of April 30, 2021 in states that withdrew from federally-expanded UI benefits in June 2021 to states that retained these benefits through September 2021. Spending are all inflow transactions containing a set of UI-related regular expressions. P2P Outflows have P2P-related regular expressions and are restricted to transactions between $5 and $15,000.

Figure A.33. Density of post-P2P inflows over UI

(a) Unconditional on UI receipt



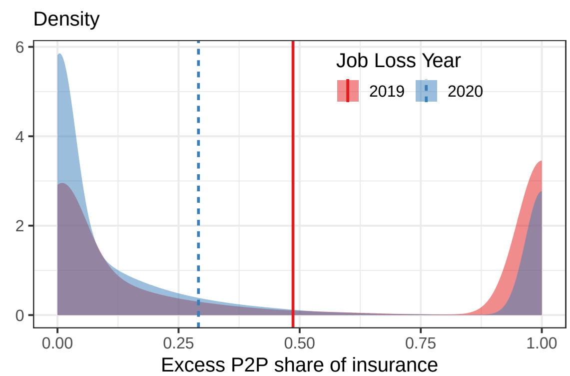
(b) Conditional on UI receipt



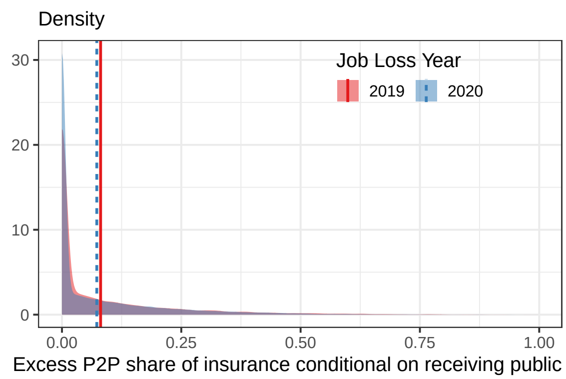
Notes: Share of P2P and UI inflows made up by P2P during unemployment conditional on receiving UI. The numerator is the total P2P inflows after job loss and the denominator is total UI inflows and P2P inflows after job loss.

Figure A.34. Density of excess share of P2P

(a) Unconditional on UI receipt



(b) Conditional on UI receipt

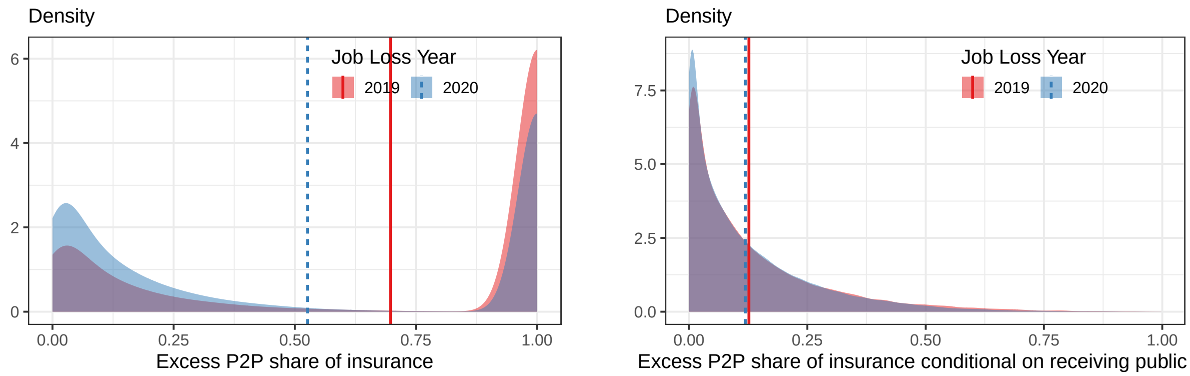


Notes: Share of P2P and UI inflows made up by P2P during unemployment conditional on receiving UI. The excess share as calculated as the within user average increase in P2P inflows from a user-unemployment spell fixed effect. The denominator is average UI inflows in months receiving UI plus the excess P2P inflows. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.35. Density of post-P2P inflows over UI inflows

(a) Unconditional on UI receipt

(b) Conditional on UI receipt

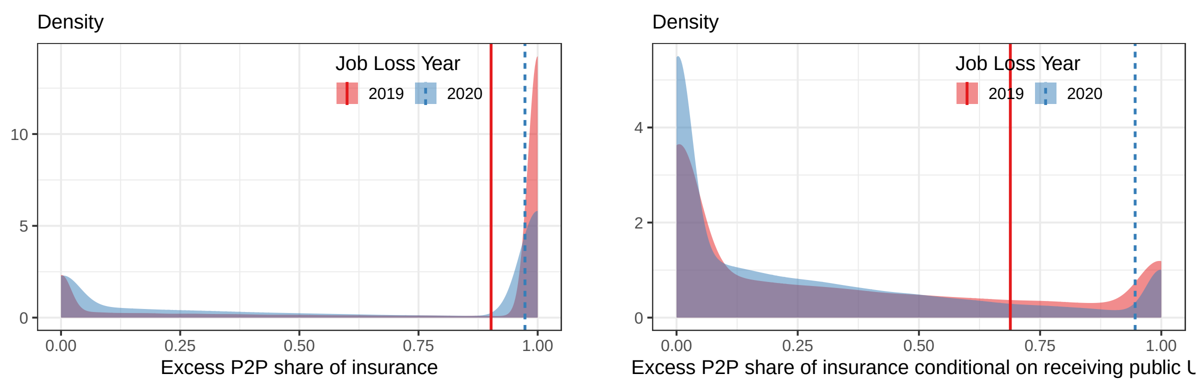


Notes: Share of P2P and UI inflows made up by P2P during unemployment conditional on receiving UI. The numerator is the average P2P inflows after job loss and the denominator is average UI inflows and P2P inflows after job loss. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.36. Density of post-P2P inflows over UI inflows

(a) Unconditional on UI receipt

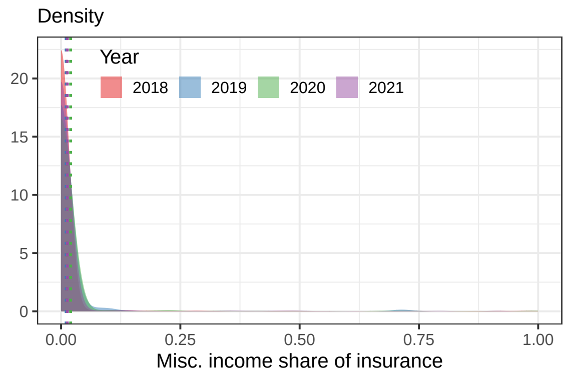
(b) Conditional on UI receipt



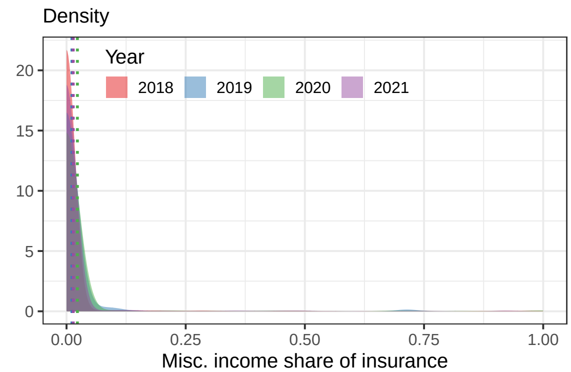
Notes: Share of P2P and UI inflows made up by P2P during unemployment conditional on receiving UI. The numerator is the difference in P2P inflows from the potential outcome P2P inflows calculated under the two-stage imputation procedure in Gardner (2022). The denominator is average UI inflows and these same excess P2P inflows after job loss. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.37. Density of post-P2P inflows over UI inflows

(a) Unconditional on UI receipt



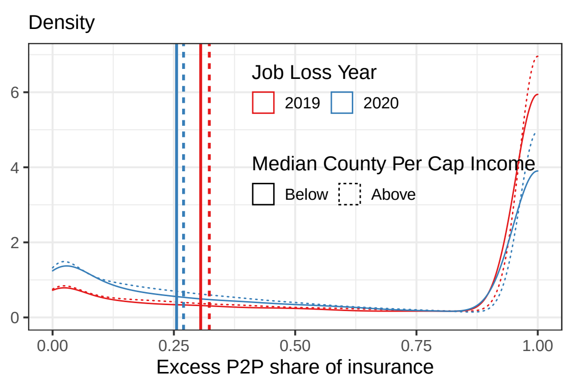
(b) Conditional on UI receipt



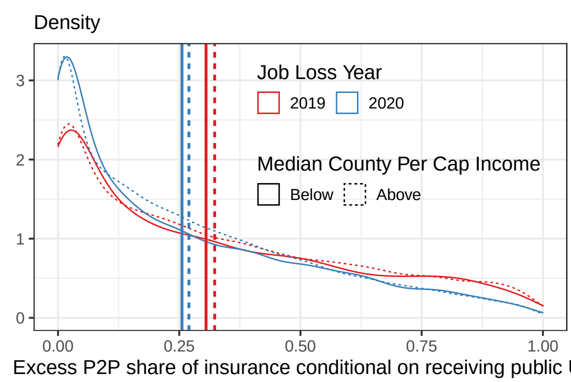
Notes: Share of total formal and informal support that is gifts from friends according to the Survey of Income and Program Participation. The population is all those user's who report receiving only money from friends or charity under their "miscellaneous income" and the denominator is this same amount plus the total unemployment insurance received by the user in the year. The vertical line show the average in each year.

Figure A.38. Density of post-P2P inflows over UI

(a) Unconditional on UI receipt



(b) Conditional on UI receipt

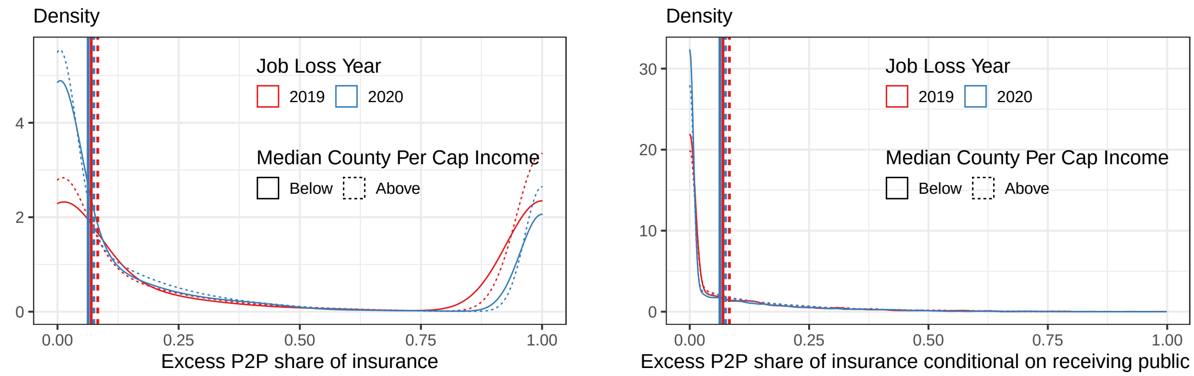


Notes: Share of P2P and UI inflows made up by P2P during unemployment conditional on receiving UI. The numerator is the total P2P inflows after job loss and the denominator is total UI inflows and P2P inflows after job loss. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.39. Density of excess share of P2P by year and income of county

(a) Unconditional on UI receipt

(b) Conditional on UI receipt

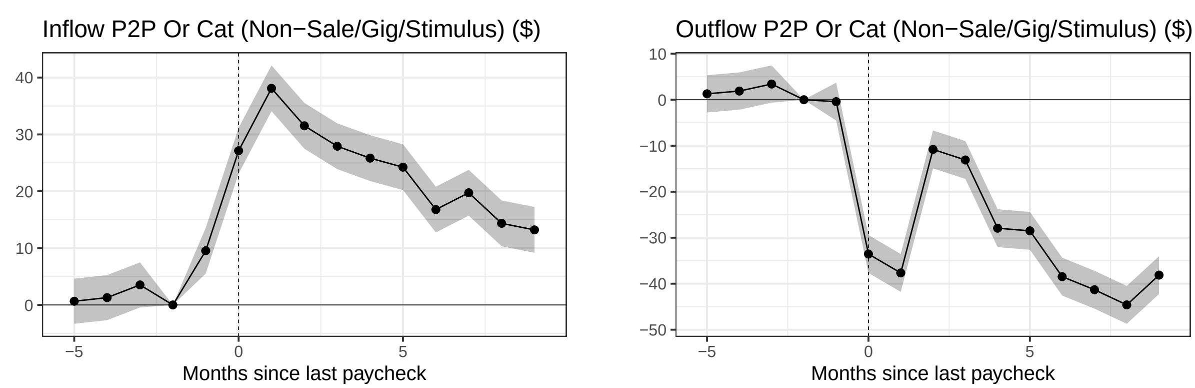


Notes: Share of P2P and UI inflows made up by P2P during unemployment conditional on receiving UI. The excess share as calculated as the within user average increase in P2P inflows from a user-unemployment spell fixed effect. The denominator is average UI inflows in months receiving UI plus the excess P2P inflows. Colors indicate the year of job loss. The line type indicates if user lives in an above or below median per capita income county.

Figure A.40. Gardner Imputation Event Study Estimates of P2P Inflows and Outflows Around Unemployment

(a) Inflows

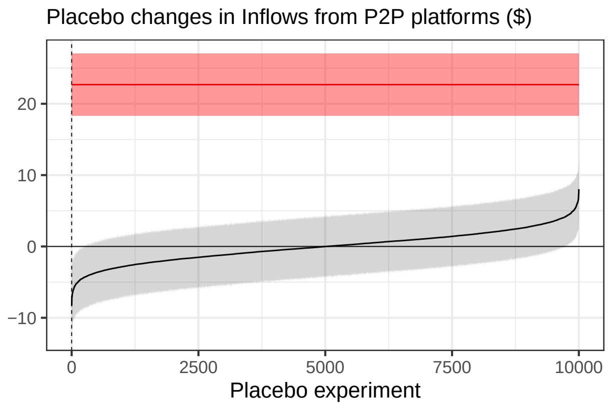
(b) Outflows



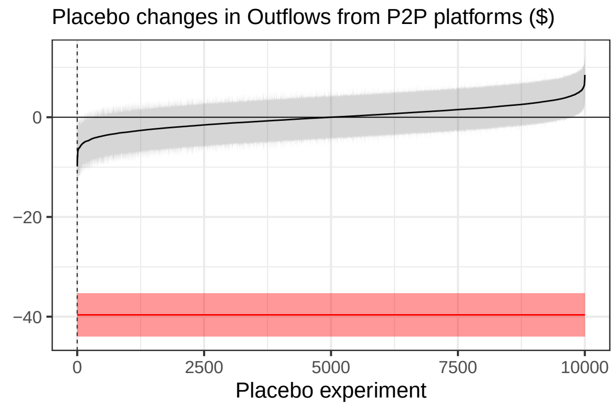
Notes: Within-person Gardner (2022) two-stage event study of P2P inflows and outflows from a P2P platform around the user's first unemployment spell. This approach corrects for "bad" comparisons by excluding treated units from calculations of user and monthly fixed effects. The sample is restricted to users with a single unemployment spell. Standard errors clustered at user-level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.41. Placebo Tests of average increase in P2P inflows and outflows and gig earnings during unemployment

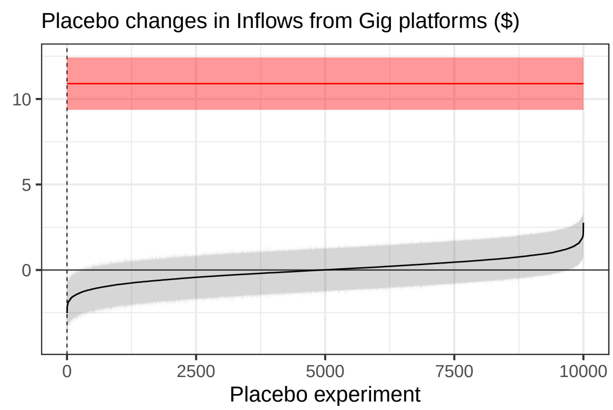
(a) P2P Inflows



(b) P2P Outflows



(c) Gig Inflows



Notes: Placebo estimates of the average increase in P2P inflows, outflows, and gig earnings during unemployment. Placebo estimates are derived from randomly assigning job loss months using weights pulled from the observed distribution of job loss months in the analysis sample. Data shows 10,000 placebo estimates in black against the true treatment effect shown in red. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021. Standard errors clustered at user level.

B.1 Appendix: Ruling out "social spending" and informal earnings

In this section, I rule out that increases to P2P after job loss are representative of increased "social spending" or a shift toward informal employment.

Figure 5 shows the extensive margin of any inflows or outflows and the extensive margin of having \$100 in inflows and outflows around job loss. These show that after job loss, workers are less likely to use P2P platforms for either inflows or outflows, but conditional on using these platforms they clear much larger monthly inflows, which does not translate to outflows. Together these facts suggest that the size and or number of inflow transactions increased after job loss. Figure A.7 show fewer individual inflow and outflow transactions for inflows and outflows after job loss, but increased probability of inflow transactions greater than \$100, consistent with a shift towards larger transfers from friends after job loss and not splitting of "social spending" expenses.

While larger inflow transactions rules out a "social spending" story, it does not rule out increased informal earnings. A recent job loser may cut back on other "social spending," but start getting paid wages via P2P that total more than \$100. I present two pieces of evidence to suggest that P2P inflows are representative of informal transfers instead of informal earnings.

First, I compare P2P inflows to changes in inflows from 20+ gig platforms after job loss. If P2P inflows are informal earnings, then they should behave similarly to earnings on gig platforms, an alternative form of informal work. Figure A.9 shows the same event study for gig earnings around job loss. Gig earnings show a clear pretrend leading up to job loss, suggesting that some job losses may in fact be career switches to gig work, but the same pattern does not emerge in P2P inflows. Furthermore, gig earnings peak at \$10 or 1/3 of P2P inflows suggesting that if informal and gig earnings have similar labor supply elasticities, at most \$10 of my informal insurance measure would be gig earnings.

Second, I compare gig and P2P inflows at the transaction level. Consider that informal insurance is likely to paid in lumpy amounts, whereas gig or informal earnings are not. To that end, figure A.8 presents the extensive margin for having transaction amounts that are multiples of \$25. The results indicate a clear increase in P2P transactions that are multiples of \$25 and no such increase in gig earnings, against indicating that P2P inflows are distinct and more likely to capture informal insurance.

Third, I address the possibility that informal earnings paid out via P2P inflows are more likely to be for tasks like yard work and babysitting, which are disproportionately paid in "lumpy" amounts. To that end, I create an alternative P2P series that isolates P2P memos that mention "yard work," "hours worked," and "babysitting," which I remove from my main P2P inflows series. Figure A.10 shows the same event study with this series does not show increases commensurate with my main P2P inflows series.

B.2 Counts of P2P transactions

In figure 2, I show that outflows make up a greater dollar share than inflows, but most users that at least one P2P inflow during the sample period. Figure A.3 has histograms of the counts of P2P transactions per month and as a share of a user's total number of transactions. Subfigures A.3 and A.3 show only %35 and %20 of months have zero P2P inflows and outflow transactions, respectively. Furthermore, subfigures A.3 and A.3 show that more than 95% of users have either a P2P inflow or outflow during the sample period.

Curiously, figures inflows as a share of total transactions and outflow counts have similarly fat tails, while the opposite is true for outflows as a share and inflow counts. These unexpected histogram shapes reflect that while there are more P2P transactions in a given month in terms of payments to businesses than for inflows, there are far more other outflows that reduce the overall share of P2P outflows. Similarly, there are fewer non-P2P inflow transactions in the denominator of figure A.3 leading to a fatter right-tail.

B.3 Informal insurance by intensity of P2P use

Section 4.1 suggests a small, but precise informal insurance role of P2P after job losses. However, these low estimates may be partially driven by users that only minimally use P2P, while those for whom the majority of their financial activity happens via P2P see much larger increases. In this section, I assess whether those with prior use of P2P for whom I may see more of their transfers see much larger inflows after job loss.

First, figure shows the P2P inflows and outflows conditional on job users using P2P inflows or outflows six or months prior to job loss. These users do not need to start using P2P platforms upon job loss in order to start receiving transfers and thus are more likely to use these platforms to get cash. Interestingly, this group only receives a maximum of $40 per month.

While these prior users of P2P have a modest increase in P2P, what about those users for whom P2P makes up a greater share of their financial activity prior to job loss tend to see much larger increases in P2P? I calculate P2P shares of both total bank account inflows and cumulative flows prior to job loss. Then I calculate the tercile of the median monthly share of these two measures of P2P. I peorm event studies in which I interact the relative time dummies with indicators for each tercile.[21] Figure A.30 shows the top tercile P2P share of inflows group peaks at $60 per month before falling off. Notably, the pre-trends in this plot are unlikely to be flat because the groups are calculated based on having more or less P2P flows prior to job loss.[22] All the same, the fact that even the highest tercile group for whom much more informal transfers

[21] I do not repeat this exercise by platform because several platforms have more than a third of users with zero pre-job loss flows, making terciles unbalanced and equally as informative as having any use prior to job loss.

[22] Figure A.31 shows the event studies calculated as subsets by each tercile. These within-group month fixed effects reverse the gradient in the interacted event studies because the earlier treated groups are compared to use with much higher monthly P2P use in later months.

are captured by P2P inflows see fairly small dollar amounts on a monthly basis suggesting informal insurance is fairly small after job losses.

B.4 Income Replacement

The "static" and "dynamic" replacement rate event studies in figure 4 suggest that P2P inflows replace relatively little income during unemployment, but neither isolates how responsive P2P inflows are to changes in income. On one hand, the "static" replacement rate isolates changes in P2P inflows, but ignores changes in income after job loss. On the other hand, the "dynamic" replacement rate corresponds to changes in income, but it can increase because income falls, increasing the share of a non-zero amount of P2P inflows. A simple OLS regression of P2P inflows on income is insufficient to measure responsiveness because P2P inflows also increase if users have more shared expenses with friends because they have more social outings after getting a raise at their job, which would yield a positive correlation between earnings and P2P inflows.

To isolate changes in income associated with job loss and receiving public UI, I use the timing of these events as instruments in a 2SLS specification shown in 20. The underlying assumption here is that these instruments isolate changes in income that are related to economic downturns. A negative coefficient in this regression would mean that when income falls one dollar, P2P inflows increase to replace some portion of lost income.

I also control for the local monthly county unemployment rate as a proxy for time-varying changes in a friends' income. I do this for two reasons. First, I want to control for whether someone's community also experienced simultaneous job losses and may be able to offer less support. Second, I want to mimic the income pooling tests found in the development literature (Chiappori et al. (2014),Townsend (1995)), which control for local community per capita income in a regression of consumption on income. Unfortunately, the unemployment rate forms a fairly poor proxy for two reasons (1) it is a noisy signal of the share with income falling to zero, not the actual per capita income and (2) informal insurance networks in the US are not county-based as people have friends from all over the country.

$$\begin{aligned} \text{P2P}_{it} &= RR\hat{\text{Income}}_{it} + \beta \text{UR}_{it} + \lambda_i + \lambda_t + \epsilon_{it} \\ \text{Income}_{it} &= \gamma_1 1\{\text{Month} \geq \text{Unemp Month}\} + \gamma_2\{\text{Month} \geq \text{Insured Month}\} + \lambda_i + \lambda_t + \nu_{it} \end{aligned} \tag{20}$$

The results in table A1 show that P2P inflows fall three-tenths of a cent when income increases by one dollar, or essentially zero in both the OLS and IV specification. The logged results, which involve added one to the dependent and independent variables before taking logarithms, are also small, though positive, suggesting noisiness in the underlying logged variables. I also estimate a Poisson regression following Wooldridge (2013).

A Poisson estimate can more appropriately handle the mass of zeros in P2P inflows and income and is an even more precise zero. Altogether, these results are consistent with the story that P2P inflows replace very little of lost earnings and are minimally crowded out by increase in UI. They suggest instead that people receive a lump sum from friends at job loss that is divorced from the amount of money lost.

B.5 Consumption smoothing from P2P

As an alternative to using the tercile of the P2P share of cumulative flows as an indicator for the type of P2P support, I look at two additional specifications to assess the consumption smoothing effect of P2P. First, I look at the tercile of the P2P share of all inflows and second, I condition on having used a P2P platform prior to job loss.

Figure A.22 shows event studies interacted by whether the user had P2P inflows at least one month prior to job loss across platforms. Figure A.22 shows that those with prior use of any P2P platforms see consumption drop about \$10-\$30 less just after job loss. Taking prior P2P use as as a proxy for informal network support, this suggests having prior informal support facilitates consumption smoothing. Interestingly, there is a great deal of platform heterogeneity in consumption smoothing with Cashapp associated with consumption falling \$100 less per month, while Zelle is not associated with any consumption smoothing. Venmo and PayPal are associated with short-run smoothing, but long-run reductions in consumption.

Figure A.13 shows the tercile of median monthly P2P share of total inflows for each user. These results are consistent with figure 9, that users see consumption fall by \$40-\$70 less after job loss.

B.6 Benchmarking P2P as a share of UI support

The event studies show a clear, short-term increase in P2P payments which play an informal insurance role during unemployment. This short-term replaces a small share of lost earnings on average, much less than the average replacement rate of UI. At the same time, these averages mask a large amount of heterogeneity. Some people likely receive far more informal insurance than public UI during jobless spells. Put another way, what is the distribution of the informal share of total insurance in the population.

Ideally, I could divide the informal insurance each person received in a month by the total UI and informal insurance they received to benchmark individual informal insurance against UI. Figure A.33 shows the P2P share of total UI and P2P inflows after job loss in 2019 and 2020, unconditional and conditional on getting UI. In both density plots, more mass shifted to the left for 2020 because the CARES Act added \$600 per week to UI benefits and expanded UI eligibility, increasing UI on the intensive and extensive margin and inflating the denominator. These suggest that unconditional on receiving UI, the bulk of users receive almost all P2P with 76% and 61% on average in 2019 and 2020, respectively. When conditioning on UI receipt, the mass shifts to the left for averages of 32% and 27% in 2019 and 2020. The shift occurs because (1) many receive zero UI inflows and (2) UI crowds out at least some P2P.

While these raw ratios are informative, they are likely overestimates of the informal insurance share of total insurance because I do not have an earkmarked series of "informal insurance" P2P payments. Instead, I have all P2P payments after a job loss – even after finding a new job. In contrast, UI inflows are explicitly insurance payments by statutory definition.

In order to get a more user-specific measure of "informal insurance" from P2P inflows, I measure average individual monthly informal insurance, b^p as a the excess P2P an individual receives during unemployment. 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 from the regression in equation 21. This fixed effect is the post-job loss excess P2P after differencing out overall user and calendar month fixed effects.

$$\begin{gathered} \text{P2P} = \lambda_t + \lambda_i + \underbrace{\lambda_i \times \text{Unemployed}_{it}}_{\text{Excess P2P}} + \epsilon_{it} \\ \text{Excess P2P Insurance Share} = \begin{cases} \frac{\text{Excess P2P}}{\text{Excess P2P} + \mathbb{E}[UI_{jt} | UI_{jt} > 0, j \in I]} \text{ if user receives UI} \\ 1 \text{ if user never receives UI} \end{cases} \end{gathered} \tag{21}$$

The second line of equation 21 shows that I divide this excess P2P by the total average P2P and UI inflows in months when receiving UI. I condition on months when UI inflows are positive because users usually receive UI for only a subset of their unemployed months by design. If the user never receives UI after a job loss, then the excess P2P share of insurance is exactly one as the user only receives P2P during unemployment.

As the excess P2P share is the average within-user change in P2P inflows before and after job loss, it can be negative. In such cases, the excess P2P insurance share has a negative numerator, while the denominator can be positive or negative of a smaller or greater magnitude depending on the magnitude of UI inflows. Altogether, this means that the excess P2P insurance share is not strictly within zero and one. In order to make the plots cleaner, I bin the share at zero and one. In figure A.33, 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, 49% and 28% of total insurance came from excess P2P on average, respectively, unconditional on UI receipt. Furthermore, the density plots show mass shifted to the left relative to A.33 in both 2019 and 2020 because average excess P2P is a much smaller share than total P2P for most users. Conditional on UI receipt, these percentages fall to 0.08% and 0.07% in 2019 and 2020, respectively. Altogether these histograms show excess P2P makes up a small share of total insurance for most users.

Appendix figure A.37 shows a similar calculation done using the Survey of Income and Program Participation (SIPP). The SIPP asks users to name their total miscellaneous income, which includes money from friends and money from charity, but also includes money from boarders, lottery winners, miscellaneous work, national guard service, and all other sources. I restrict to those users that receive only money from friends or family during the year and calculate its share of total support including UI. The sample without any other source of income contaminating the measure is quite small ($N = 411$), but it should contain *all* sources of pecuniary support including cash, checks, and P2P payments. All told, the average friend share of total support in the SIPP is less than 0.05 across all years from 2018 to 2021, which is even less than what I measure in the average excess P2P share. If anything then, I end up with a slight overestimate of support from friends during job loss.

In addition to breaking up by job loss year, I further break up by job loss year crossed with whether the user lives in an above or below median per capita income county. Figure A.38 shows the raw shares of P2P versus P2P and UI inflows by job loss year and counter per capita income. This indicates that P2P inflows relative to UI do not differ much between the two types of counties across the years. Figure A.38 shows the same "fixed effect" approach as in A.33 and finds the same pattern. This suggests that informal support is slightly higher in richer counties, but the levels are not extensively different from one another.

I also try other alternative definitions of excess P2P. First, I substitute the within-user unemployment fixed effect for average monthly P2P inflows after job loss. This approach does not difference out regular P2P use from before job loss. If a user's P2P inflows are all informal insurance after job loss, then this method is more accurate than the "fixed effects" method, which subtracts out non-UI P2P. Second, I adapt the imputation procedure in Gardner (2022), which restricts user and time fixed effects to those that are pre-treatment. These pre-treatment user and time fixed effects are used to impute a potential outcome for each user in the absence of treatment, which I subtract from the observed P2P inflows. These two approaches can be found in figures A.35 and A.36, yielding the same qualitative results.

B.7 Local economic conditions: P2P support higher in richer areas

Given that informal insurance via P2P seems to make up a fairly small amount of money after job loss, does this vary based on local economic conditions? Those in poorer areas are likely to have a tougher time finding work after a job loss, so would benefit from greater informal insurance. At the same time, their neighbors may have less disposable income with which to support them.

Figure A.16 shows event studies of coefficients from relative time dummies interacted with whether a user lives in a county with above median per capita household income per the 2019 American Community Survey five-year survey. Notably, those in the above median per capita income counties receive double informal insurance inflows after job loss of about $40, while those in below median counties receive a peak of about $20 per month. These results suggest that while those in poorer areas deserve more support, the lack of disposable income in their community dominates.

Of course, the friendship networks that informal insurance networks follow are not explicitly tied to county of residence. Instead, people have friends all over the world that they can reach out to. Chetty et al. (2022) document such networks using Facebook connections among the 18-49 year-old population, which they show is highly representative of the actual real-life networks of these Facebook users. I use their zip code level dataset, which includes a measure of economic connectedness, the share of high-socioeconomic[23] status to low-socioeconomic status friendships out of all low-socioeconomic status individuals in a zipcode. I further utilize the decompositions of economic connectedness into "exposure" and "friending bias," which capture that a high degree of connectedness is either due to a large number of high-SES people in zip code or a higher rate of cross-SES friendships being made in an area.

In figure A.17, I show the event studies of the coefficients on relative time dummies interacted with indicators for being in a zip code with above or below median "economic connectedness," "exposure," or "friending bias." Figure A.17 is consistent with A.16 – living in area with more connections to high-SES individuals leads to greater informal insurance after job loss. But Figure A.17 and A.17 color the mechanism by which these transfers occur. Interestingly, those in areas with greater exposure to high-SES individuals received more support, while those in areas where they are more likely to become friends with high-SES individuals see slightly smaller increases in informal insurance after job loss. Together these plots suggest that high-SES individuals send more money to those at an arm's length in their network, but less money to those with whom they are more likely to share a direct connection.

Altogether, the higher average informal transfers found among job losers in richer areas and those with better connections to high-SES individuals suggest that informal support is directly tied to the disposable income of a job loser's network. As a result, informal insurance is only as robust as a worker's network, suggesting a weakness relative to broad-based public UI.

[23]Socioeconomic status is defined as a mix of a variety of factors like local average income, educational attainment, etc.

B.8 User demographics: Single mothers targeted

If informal insurance support levels are linked to the economic status of a user's network, how well do they target? To that end, I look for targeting based on whether a user is a mother, father, and in a relationship or not. In general, women and mothers tend to have a tougher time returning to the labor force after a job loss such that women, mothers, or specifially single mothers receive the most support during downturns. In order to assess whether any group is more likely to receive support, I rely on demographic information provided Earnin, the surveys, and imputed from IRS payment amounts.

Within the fixed tags, Earnin provides an indicator for user gender as predicted by user first name. The survey also asked about user gender, which I use when it disputes the gender predicted by Earnin. Next, I use the family compositions imputed from the CARES Act and CTC payments as explained in section 2.

Figure A.14 shows that among all groups, single mothers received the most P2P inflows after a job loss – reaching \$50 per month, which continues throughout the 10 months after a job loss. The figure further shows that women tend to receive more support than men across all family compositions, while there is essentially zero differentiation in support by whether a man has children, is a single parent, or a coupled parent. Altogether, this suggests clear targeting by informal networks to single mothers, which tracks with social norms and matches the demographic group that likely has the toughest time returning to the workforce.

B.9 Targeting by NAICS code of former job

Another characteristic associated with higher and lower P2P inflows is the industry of a job loser's former occupation. Figure A.15 shows P2P inflows for selected NAICS codes of the last job that workers had. These indicate that P2P inflows increase across occupations, but have different shapes and cumulative payments. For example, those working in Healthcare and Retail receive the least inflows after a job loss, possibly because these were "essential" workers during the pandemic and tend to have short unemployment durations. In contrast, those in areas that were hardest hit by pandemic slowdowns like Accommodation and Food Services (i.e. restaurant service) and Educational Services saw increases as high as \$40 immediately after job loss. The most surprising result is that those in Transportation & Warehousing saw P2P inflows reach as high as \$60 after job loss and stay elevated, again likely due to production slowdowns that meant transportation of goods and people fell through the pandemic. Unfortunately, I do not have more granular NAICS information, which I could use to separate out good transportation from passenger transportation jobs. Altogether, these results suggest that those who were unlikely to find work received more support, but again, only modestly support on average.

B.10 Bank balances: Mistargeting based on balances

Given that informal networks target based on demographics, how well do the target based on assets at job loss? Those with higher assets should have an easier time smoothing consumption through the pandemic, while those with lower balances should struggle, and thus may need more support from their communities. On the other hand, those with higher balances may have be of higher SES, and thus have higher SES friends that can support them.

In order to answer, I interact an indicator for the tercile of a user's bank balance at job loss with the relative time dummies. Figure A.18 shows the interacted event study coefficients based on tercile for P2P inflows and outflows.

The data indicate those with the lowest bank balances receive the least in informal insurance after job loss, while those with the highest bank balances receive the most. Interestingly, outflows show a greater recovery for the lowest tercile of bank balances at job loss, possibly indicating that this group has a higher level of spending across states, which explains is low bank balances at job loss. In general, these results suggest an example of mistargeting by informal insurance networks – those with the most assets to fall back on during a job loss, receive the least support.

B.11 Risk aversion

While sections B.8 and B.10 suggest that informal insurance networks exhibit targeting, users may be more or less likely to seek out support during job loss based on behavioral characteristics. For example, the risk averse may seek out more informal insuranace in order to maximize consumption smoothing across states. Alternatively, if networks can only provide support temporarily, the risk averse may see support spike earlier before tapering off.

In order to assess these possibilities, I restrict my analysis to those that responded to the 2020 survey question on risk aversion. To maximize power, I separate my regression into those with above and below median risk aversion based on a telescoping question that asks for the willingness to take different 50/50 gambles versus a sure bet of receiving \$240. The exact text of the question can be found in the appendix.

Figure A.19 shows the event study coefficients interactions of relative time dummies with indicators for whether the user has above or below median risk aversion. These results are fairly imprecise, but indicate that the less risk averse tend to receive P2P inflows earlier, relative to the less risk averse who receive more steady support over the course of their job loss spell. These dynamic paths are consistent with an idea that the more risk averse seek to consumption smooth, while risk lovers are unbothered by large swings in consumption and may seek out less regular support.

B.12 Relationship between UI receipt and P2P inflows

Given that informal support and formal UI benefits fulfill a similar purpose during job losses, one might expect P2P inflows to be lower for UI recipients. Figure A.25 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.

Counterintuitively, UI recipients receive more P2P inflows after job loss than non-recipients. This unexpected relationship occurs because these groups are fundamentally different. Furthermore, the P2P outflows increase more for UI recipients consistent with a marginal propensity to spend money out of UI benefits, suggesting that I am correctly identifying UI receipt.

Non-UI recipients are disproportionately drawn from users who either quit their job and took over five weeks to receive their next paycheck or exited the labor force entirely. Both groups are unlikely to apply for UI benefits and also unlikely to need support from friends and family. Figure A.27 shows event studies for the replacement rate of pre-job loss earnings by P2P. These show that if the replacement rate increase is much less precisely measured for UI recipients relative to non-UI recipients, likely because non-UI recipients likely have lower earnings prior to job loss, suggesting another compositional difference between the two groups.

As a consequence of this compositional difference, the pooled month fixed effects difference out a smaller amount of P2P flows for UI recipients. Figure A.26 shows the event study for the two groups as subsets, so the month fixed effects only use within UI receipt variation. These results show that P2P inflows increase for a shorter duration for UI recipients, as expected, implying the two have separate time trends. Given the complicated econometric interpretation, I focus on crowd-out within UI recipients based on changes to benefits or delayed receipt.

In addition to differences in UI receipt, I also look at P2P inflows and outflows by whether the user lives in a state with a 2019 UI system that has an above or below median replacement rate of lost earnings as calculated by Ganong et al. (2020a). Figure A.28 plots the event study interaction coefficients with the replacement rate. These coefficients show users in below median replacement rate states get slightly more P2P inflows, but only marginally.

Next, I look at how these inflows change for 2020 job losers who are disproportionately able to draw on federally-expanded UI benefits. Figure A.29 shows that P2P inflows were highest for 2019 job losers in below median UI states and dropped off for 2020 job losers, coinciding with increased benefits. Oddly though, 2020 job losers received more P2P inflows than 2019 job losers in above median states, the opposite of what one might predicted.

B.13 Quantile Treatment Effects

The results thus far have shown a clear average increase in P2P inflows, but these may disguise that only a subset of users experience major increases in P2P inflows, while the rest experience zero change. One way to see the distribution of P2P increases would be to estimate event studies with quantile treatment effects. Unfortunately, linear user and month fixed effects in a two-way fixed effects regression behind event studies do not work with non-linear quantile regression.

Instead, I instead summarize the data to the user-level by calculating the change in average monthly P2P inflows before and after the month before job loss. I show the quantiles of this average change in figure A.20. These show that most user's see a positive change in P2P inflows after job loss across platforms.

Next, I analyze how the heterogeneous effects differ based on quantile treatment effects. Specifically, I estimate equation 22 for different group indicators used above like gender, parenthood, gender crossed with parenthood, UI receipt, and months to UI receipt. I present these quantile treatment effect estimates in figure A.20.

$$Q_{y_{it}|G_{it}}(\tau) = \alpha + G_{it}\beta_\tau + \epsilon_{it} \tag{22}$$

First, figure A.21 shows that women experience greater P2P inflows at all quantiles above the 25th percentile. Figure A.21 shows that single mothers also have a greater change at all but the lowest quantiles below the 10th percentile. These further emphasize that single women receive more P2P inflows than their peers at all levels.

Meanwhile, figure A.21 shows that UI recipients receive more P2P inflows at all quantiles. Furthermore, figure A.21 shows that recipients of UI in 2-6 months after job loss receive more P2P inflows at all quantiles of P2P. These results suggest that UI delays are associated with more P2P inflows for nearly all levels of increases in P2P after job loss and not isolated to a specific set of users.

B.14 Delays in UI

Not all workers receive UI immediately upon application. In normal times, there is a week waiting period to start benefits after job loss and then bureaucratic delays in processing applications. In 2020, the CARES Act allowed states to waive the waiting period and expedite processing of applications. At the same time, application systems were overwhelmed with applicants offsetting these attempts to expedite the process.

Figure A.24 shows the number of weeks between first job loss and the nearest UI spell start among states where I can accurately identify UI spells. These show that users receive UI in an average of nine weeks after job loss, but that there is a large amount of variation across users. Some workers receive UI before their last paycheck likely due to delayed receipt of last paycheck and on-time payment of benefits or because of limitations in properly identifying spell start and end times. While about 50% receive benefits within a month of application, 25% receive benefits in 2-6 months.

B.15 Welfare of unemployment insurance with crowd-out of informal insurance with separate networks

In this section, I build on the work presented in section 6 to show a sufficient statistics approach to documenting the welfare effects of UI with crowd-out of informal insurance when there are many networks. Again this builds on work in Chetty and Saez (2010). There are K exogenous groups $k = 1, \ldots, K$, which represent county of residence but could represent any other grouping. Each group contains p^k of the population and has a conditional density of ability $f^k(n)$, so the population density of ability is $f(n) = p^k f^k(n)$. Following the procedure outlined in section 6, the marginal welfare of a dollar of UI is:

$$\frac{dW}{db} = \sum_k p^k(1-e^k)(1-r^k)u'(c_H^k)\left[\frac{u'(c_L^k) - u'(c_H^k)}{u'(c_{Hk})} - \frac{\varepsilon^k_{1-e,b}}{e}\frac{1+b_p^k/b}{1-r^k}\right] \tag{23}$$

I follow Chetty and Saez (2010) 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:

$$\begin{aligned} G(b) &= \sum_k p^k \frac{dW}{db}\frac{1}{1-e^k} / \frac{dW}{dz_H}\frac{1}{e^k} \\ &= \sum_k (1-r^k)\left[\frac{u'\left(c_L-\right) - u'(c_H^k)}{u'\left(c_H^k\right)} - \frac{\varepsilon^k_{1-e,b}}{e^k}p^k\frac{1+b_p^k/b}{1-r^k}\right] \end{aligned} \tag{24}$$

Appendix table A13 shows the welfare calculations by whether county has income above or below the median. I use the b_p^k calculated in section B.6 and the r_k values from table 2. Otherwise, I use the same values for e^k, ε^k across all counties because I do not have a cleanly identified county-specific estimated of each within the pandemic.

Neither set of counties sees largely different welfare gains from a \$1 increase in UI implying that crowd-out has negligible consequences for UI.

Table A13. Money metric estimates of marginal welfare of additional dollar of UI before and after pandemic with and without crowd-out included by welfare

Income	Context	ε	e	r	$b_p/(b+b_p)$	Standard	With crowd-out	Total
High	Pandemic	0.07	0.85	0.05	0.08	0.10	0.08	
High	Pre-Pandemic	0.5	0.95	0.05	0.07	-0.34	-0.40	
Low	Pandemic	0.07	0.85	-0.06	0.07	0.10	0.10	
Low	Pre-Pandemic	0.5	0.95	-0.06	0.06	-0.34	-0.47	

Money metric welfare effects of UI with and without crowd-out. Elasticities from Ganong et al. (2022). Employment share from Ansell and Mullins (2021) and CPS. Consumption change (8%) taken from Ganong and Noel (2019) and CRRA $\gamma = 2$ from Chetty (2006). High-income refers to having per capita income from the 2019 American Community Survey above the median, while low-income refers to having per capita income below the median.

C Appendix: Data Construction

C.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.

C.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".

C.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.

C.4 Defining Panel, Sample Restrictions

C.5 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.

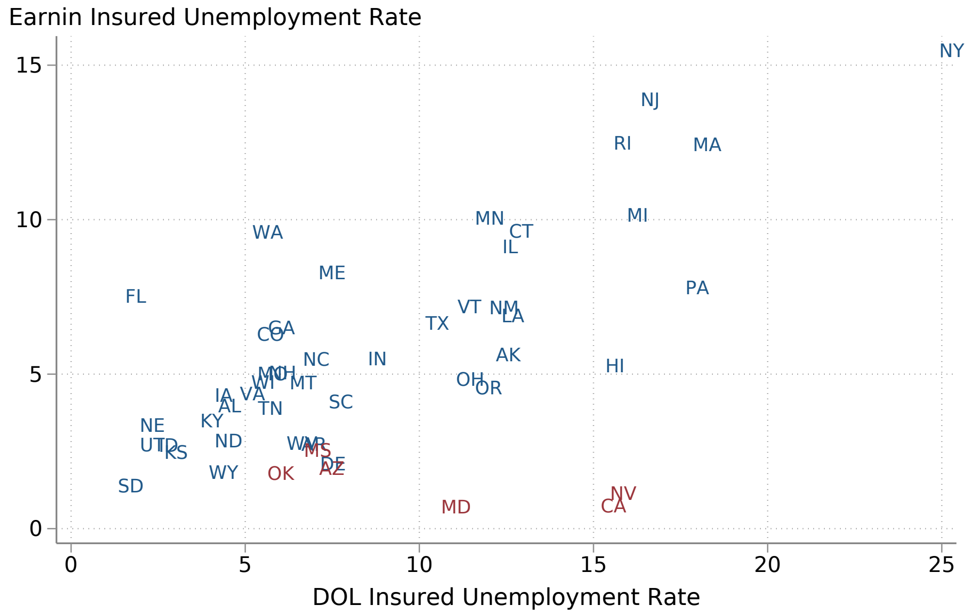
C.6 Uninformative Transaction Memos

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.

C.7 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.

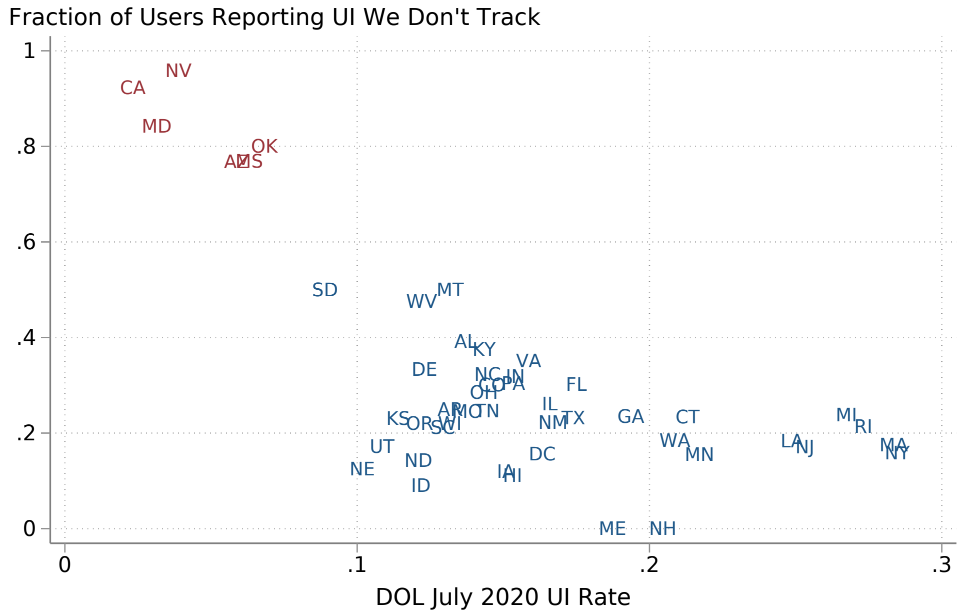
Figure C.42. Insured Unemployment Rate Comparison



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 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 C.43. 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.

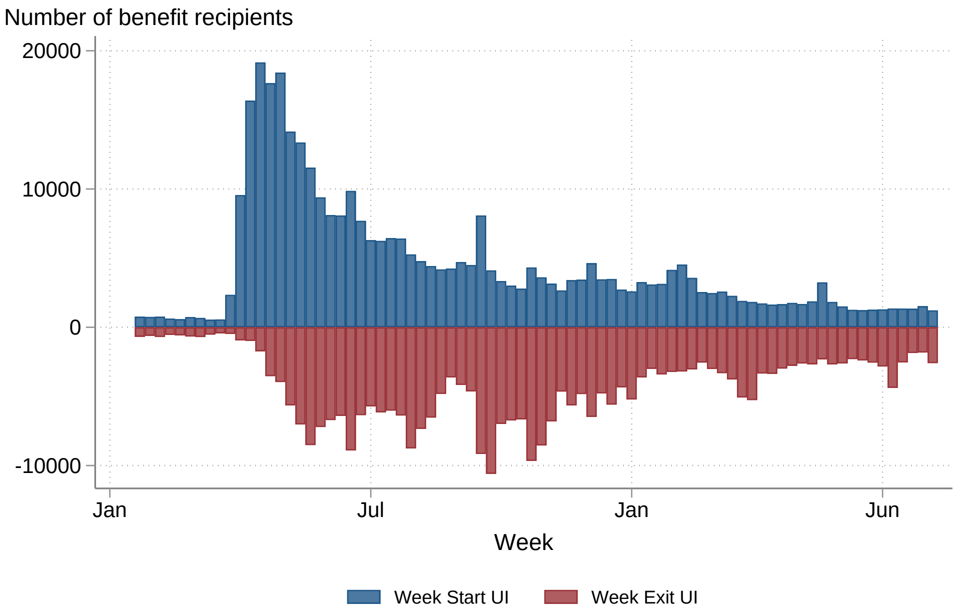
C.8 Identifying UI Payments

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 Ganong et al. (2021a).

Figure C.44. 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.

C.9 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 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 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 outflow transactions (between 30% and 40% of all outflows), a fraction in line with prior work from Ganong and Noel (2019).

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 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 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.

C.10 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.

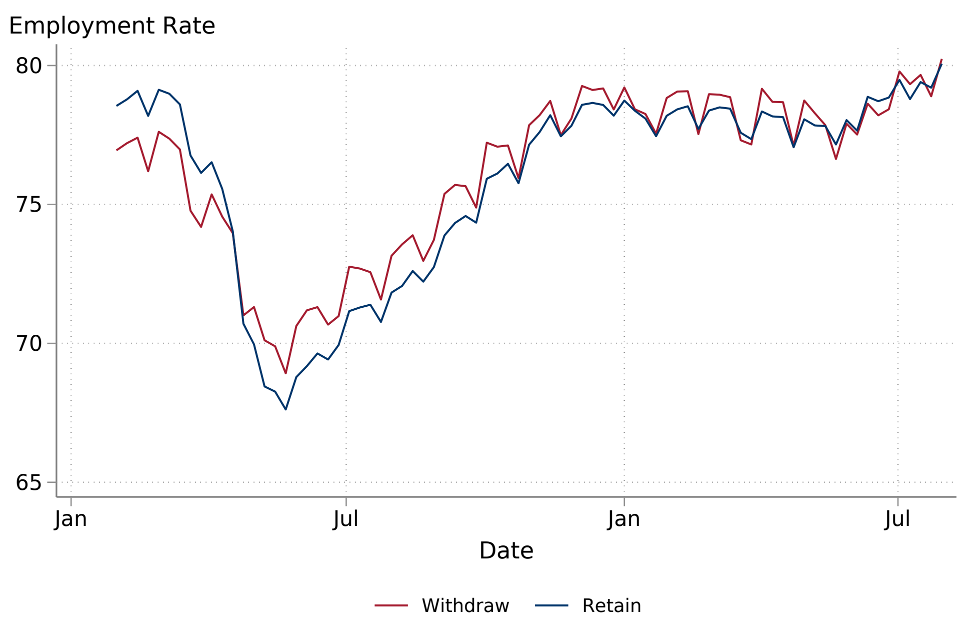
Finally, we use Plaid's categorization transactions as Payroll or Income. Upon inspection, we find Plaid's categorization of Earnings and Income to be susceptible to false positives. To account for this, we require the memo to occur in more than two unique weeks and with a modal frequency once every 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 C.45. Employment Rate Trend

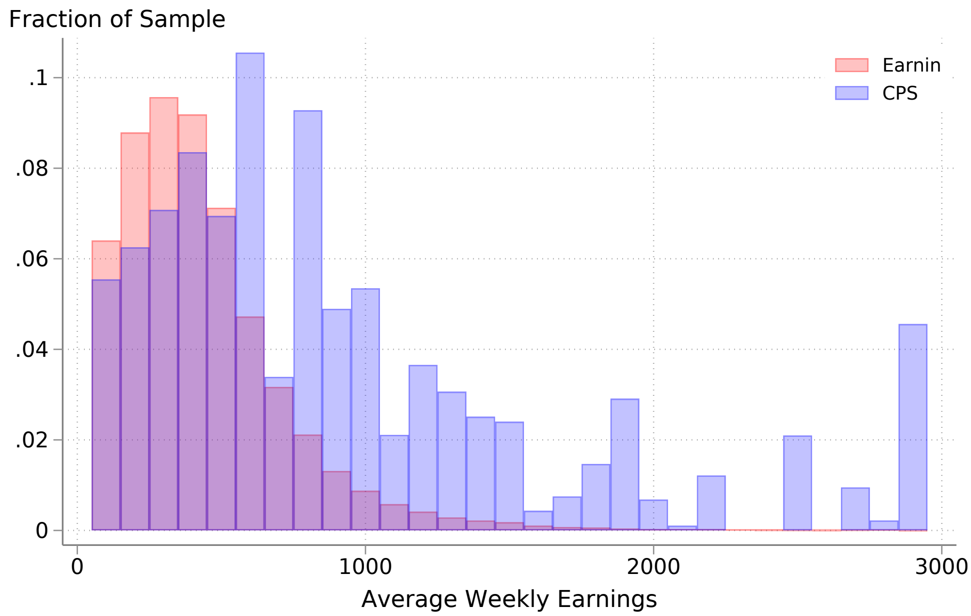


Notes: The above figure plots the rate of employment for our Earnin sample through 2020 and 2021. These estimates are based on the Earnin users from our analysis states with transactions from January 2020 through August 6, 2021.

C.11 Final Sample for Analysis of June UI Withdrawals

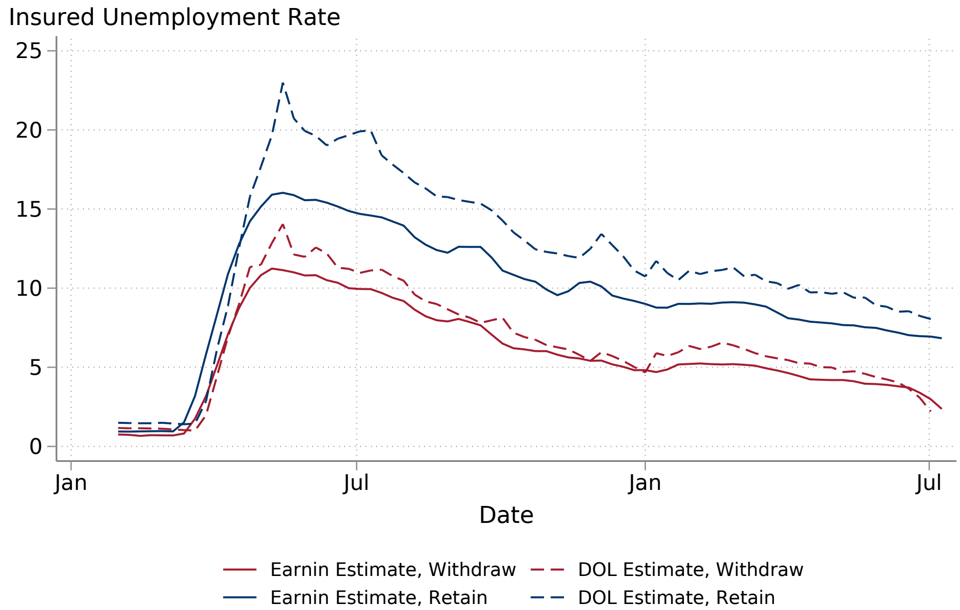
We additionally compare the characteristics of our unemployed population to those in the Current Population Survey. Specifically, we compare the pre-pandemic earnings distribution of those who were unemployed in January and February 2021; as expected, our Earnin sample has lower earnings than the estimates 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 C.46. 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 C.47. 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 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 A14. 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

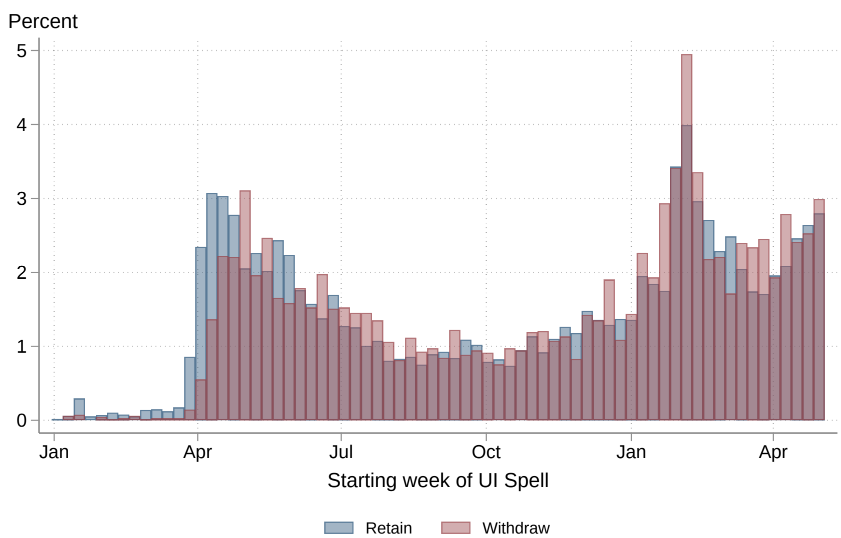
Notes: The sample for the above table includes CPS full labor force estimates and estimates for 11,402 Earnin users who completed the survey and were either employed or receiving UI benefits in August of 2020 and had transactions from January 2020 through July 2021.

C.12 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 C.48. 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

Table A15. Types of P2P Platforms and their regular expressions or Plaid categories.

Platform	Regular Expression	
Venmo	VENMO—VENM	[Tr
PayPal	PAYPAL	[Tra
Zelle	ZELLE	[Transfe
Square Cash App	SQC*CASH, SQUARE CASH, CASH APP	[Transfer, Third Party
Apple Pay	APPLE PAY	
Chase Pay	CHASEPAY, CHASE.*QUICK.*PAY	
P2P	P2P, PERSON.*TO.*PERSON,PERSON.*2.*PERSON, P.*TO.*P	
Google Pay	GOOGLE.*PAY	
Facebook	PAY.FB.COM, FACEBOOK	
Moneysend	MONEY.*SEND	
Monet Network	MONEY NETWORK	
Cashout	CASHOU?T?	

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.

C.13 P2P and Gig Platform Regular Expressions

I flag several difference P2P platforms and Gig platforms using a variety of regular expressions. I provide the lists in tables A15 and A16.

For P2P inflows and outflows, I include all transactions with a matching Plaid category or memo containing an associated regular expression. For gig earnings, I include all transactions with a memo containing a regular expression from the list below.

Table A16. Types of Gig Platforms and their regular expression searches.

Platform	Regular Expression
Taskrabbit	TASK RABBIT
Etsy	
bETSY	
b	
Uber	
bUBER	
b—UBEREATS	
b	
Lyft	
bLYFT	
b	
Fiverr	
bFIVERR?	
b	
Doordash	DOORDASH
Grubhub	GRUBHUB
Instacart	INSTA CART
FreshDirect	FRESH DIRECT
Peapod	PEAPOD
Upwork	
bUPWORK	
b	
Freelancer	FREELANCE
Flex Jobs	FLEX JOBS
Seamless	SEAMLESS
AirBNB	AIR BNB
Postmates	POSTMATES
Amazon Flex	AMAZON FLEX
OnlyFans	ONLY FANS
Manyvids	MANY VIDS
Twitch	
bTWITCH	
b	
YouTube	ADSENSE—YOUTUBE
Patreon	
bPATREON	
b	
Facebook	BUY FB COM
Other Gig	GIGSMART—
bGIG	
b	