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

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Abstract

Gifts and loans from friends and family play an underexplored informal insurance role in high-income countries, posing challenges to assessing 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 with at least one job separation between at least once between July 2019 and September 2020. Event study estimates show average monthly P2P inflows increase by \$30, or 1.4% of lost earnings, one month after job loss before returning to baseline over 10 months. Single mothers receive the largest increases, as do those with high prior earnings or 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 \$0.05 of informal transfers, suggesting 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 by 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 1.4% of average pre-job loss earnings before returning to zero over 10 months. Second, single mothers, the long-term unemployed, and those with high prior earnings or 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 \$0.05 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 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[2] 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 during 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. (2024). First, 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. Second, 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. Third, I compare P2P inflows of the same March job losers to a group of employed

[1] Results are robust to keeping these transactions.

[2] In this paper, I use "unemployed" to refer to works who are not working. Technically a worker who is not working may have exited the labor force and would instead be "non-employed." Given that Earnin requires a regular paycheck deposit from an employer to use its services and that the majority of job separations during the pandemic were involuntary and temporary, this terminology choice seems appropriate for expositional simplicity.

users before and after the July 31, 2020 expiration of $600 per week in UI benefits. The first and third experiments exploit government-initiated changes in benefits that are independent of user characteristics like pre-job loss earnings or network support. The second natural experiment exploits that early pandemic UI delays were primarily driven by an overload of applications instead of application timing decisions by job losers. 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 monthly UI benefits causes small but precisely estimated changes in P2P inflows ranging from -$0.05 to $0.01 per month, the latter of which suggests negligible crowd-in.

My estimate of informal crowd-out is the first measured using high-frequency administrative data in a high-income country. It is also smaller, but more precisely estimated than those found using the Panel Study of Income Dynamics (PSID) survey. Both Edwards (2020) and Schoeni (2002) use the PSID to estimate crowd-out,instrumenting for UI receipt with variation in state-level policy. Schoeni (2002) finds an additional dollar of UI crowds out 24 to 40 cents of annual family support in 1987, but his 2SLS analysis has weak instruments[3]. Edwards (2020) finds small crowd-out along the extensive margin similar to my estimates, but with much less precision. The imprecision in these papers underscores the value of high frequency, administrative data for estimating crowd-out.

The lack of high frequency crowd-out estimates persists despite theory that crowd-out of informal insurance can have large welfare consequences for the design of public insurance (Chetty and Saez (2010), Di Tella and MacCulloch (2002), Thomas and Worrall (2007)). Chetty and Saez (2010) extend the canonical Baily-Chetty formula (Baily (1978), Chetty (2006)), which calculates the optimal replacement rate of lost earnings to balance the consumption smoothing role of insurance with a 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)) and re-employment at benefit exhaustion (Meyer (1990), Card et al. (2007), Ganong and Noel (2019)) — have proven easier to measure. Most welfare analyses 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 on risk-sharing suggests moral hazard persists in informal insurance arrangements. In the United States, there is incomplete risk-sharing within family units in the PSID (Altonji et al. (1997, 1992) and Hayashi et al. (1996)). In small village economies, families and friends use informal credit and gifts to incompletely income pool within social networks (Townsend (2016), Chiappori et al. (2014), Auriol et al. (2020), Angelucci and De Giorgi (2009)). Incomplete income pooling is consistent with limited commitment, which facilitates punishment to eliminate moral hazard in informal arrangements (Coate and Ravallion (1993), Ligon et al.

[3] $F - statistic$ of 2.8, which is well below the rule of thumb of 10 (Andrews et al., 2019).

(2002)). Bloch et al. (2008) show that limited commitment requires a very particular "sparse" network structure to guarantee stability, suggesting moral hazard persists in informal insurance arrangements. Furthermore, Carranza et al. (2021) finds that informal networks in Côte d'Ivoire produce moral hazard via a "social tax" paid to friends and family out of earnings.

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, 2013)). Evidence on the effect of formal insurance on informal insurance 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 transactions data to precisely measure behavior during unemployment (Ganong and Noel (2019); Ganong et al. (2024), Farrell et al. (2020), Bell et al. (2022), Johnston and Mas (2018), Card et al. (2015)). Ganong et al. (2024) 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. Andersen et al. (2023) use linked high-frequency bank transaction and tax data to identify and measure insurance strategies in unemployed Danish households. Andersen et al. (2020) specifically find that family and friends replace seven cents for every dollar of lost income, which is roughly five times my estimate. 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 a job separation. 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 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 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 included monthly payments of the Child Tax Credit (CTC), which lasted for six months.

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 31, 2020.[4] 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 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. I use this imputed family composition to look for evidence of targeting in informal insurance networks.

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

3 Individual bank transaction data linked to 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 paycheck advances. Through this connection, Earnin maintains a database containing tags with information about each user, transactions, bank balances, and flagged earnings. The bank transactions contain memos and categories provided by the financial services company Plaid.[5] I categorize transactions into P2P inflows and outflows, spending, earnings, and UI inflows using user-level 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 proxy ID-monthly level. For simplicity, I will call each proxy ID unit an "individual," "worker," or "user." For further details on data construction and categorization, see appendix C.

These bank transaction data are linked to a large-scale survey of 24,671 users conducted in August 2020 by 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. The survey ask questions about earnings, employment, UI benefits, and spending for the month of July 2020 and expectations of these same outcomes for September 2020. In addition to these questions, the survey elicits risk aversion and discount rates using questions from the Global Economic Preferences Survey (Falk et al., 2016, 2018).[6]

The full data include all Earnin users from December 29, 2018, to October 15, 2021, but I make a number of sample restrictions consistent with Coombs et al. (2021) 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, showing that their bank account is regularly used. Second, I drop any users for whom more than one percent of their bank memos are uninformative, meaning they only contain a generic word (e.g. "Credit") or a list of numbers and symbols, or are altogether missing. Uninformative memos reduce the reliability of categorizing transactions. Third, 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.[7] The "not-yet-unemployed" provide a clean control group for my two-way fixed effects event study estimates, which

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

[6]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. 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. Further information provided in appendix C.4.

[7]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.

reduces possible bias from comparing later-treated group to early-treated groups in a setting with staggered treatment timing and heterogeneous treatment effects. Fourth, the sample is restricted to users with transactions for at least six months prior to and 10 months after their first job loss[8] to balance my analysis sample in relative time for the event studies. Altogether, 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.[9]

While restrictions based on account activity, unemployment, and UI flagging minimize noise in the sample, Earnin users are 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 earnings distributions in figure A.2 are close to this national benchmark suggesting the Earnin data is more representative of the workers most likely to become unemployed during the pandemic. Similarly, table A1 shows that the Earnin survey sample is younger, less educated, more racially diverse, and has more women compared to the CPS.

In addition to the benefit of oversampling from a group affected by the pandemic, the users in my dataset opted to use Earnin, a digital payment technology. As a result, I am likely 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 UI inflows and earnings and creating unemployment and insurance spells

In this paper, I present event studies to track P2P inflows, outflows, and overall spending around first job loss. I also estimate the extent that public UI benefits crowd out informal insurance. To do this, I need to flag earnings and UI inflows, creating labor market histories for each user.

I flag UI inflows by searching for regular expressions in bank transaction memos that correspond to UI payments. In total, I search for over 200 regular expressions. Prior to the sample restrictions describe above, 175,000 users received UI in July 2020, with 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.3.

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 that are indistinguishable from other deposits. In Appendix C.5.4, I present evidence that California, Nevada, Maryland, Arizona, Mississippi, and Oklahoma have exceptionally high miss rates for flagging UI payments based on survey responses. I drop these states from all analysis that involves directly observed UI inflows to reduce measurement

[8] I exempt September 2021 job separations from this requirement because their job loss is at the end of the observed panel.

[9] I am unable to provide a clear breakdown of users cut by each restriction, due to privacy restrictions within my data user agreement. However, I provide a table of my analysis sample counts in table A2. I do provide a histogram of the weekly entrances and exits to the labor market in figure A.1.

error leaving a sample of 108,181 users, 51,850 of whom receive UI.[10]

While UI inflows are relatively simple to track because deposit memos are uniform across users, earnings tracking involves a more complicated algorithm to isolate each user's earnings. The algorithm employs the 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 "observed earnings," then the algorithm flags all transactions within its 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 transactions with memos or Plaid categories that correspond to paychecks as earnings. Further details can be found in appendix C.6.

Armed with UI inflows and earnings, I define UI and unemployment[11] 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. These isolated series allow me to track gig work as an alternative form of risk management after a job separation. Additionally, I am able to benchmark changes to P2P inflows against changes in gig earnings, in order to rule out that P2P inflows are just gig earnings disguised on another platform.

3.2 Categorizing informal support of P2P

My primary outcomes of interest are P2P inflows and outflows, which I use as proxies for informal support provided during unemployment. To measure these flows, I take of the union of all transactions marked "Venmo," "CashApp," "PayPal," or "Chase QuickPay" by Plaid or with bank memos containing regular expressions mentioning these P2P platforms. These regular expression searches allow me to pick up additional platforms and fill in holes due to inconsistent Plaid categorization during the sample period. For example, memos containing "Zelle" go from being marked as "Chase QuickPay" to "Credit" after March 2021.[12]

Several P2P platforms are used to pay for goods and services. Consequently, the some P2P transactions are not transfers between friends and family, but payments associated with consumption, refunds, or a small business. I exclude transactions with memos that contain regular expres-

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

[11] I use the term "unemployment" to refer to all jobless spells and not just involuntary separations to simplify exposition. As the majority of these spells occur during the pandemic, the majority are likely to be involuntary initial separations.

[12] Figure A.4 shows the dropoff for "Chase QuickPay" and the rise of "Credit" in March 2021.

sions associated with sales of goods or services, tax refunds, or other fintech platforms like Earnin as these are false positives. Furthermore, a greater share of P2P transactions equal stimulus check amounts (e.g. $1200) around economic impact payment deposit dates. These transactions are either due to households dividing up stimulus checks or people giving their entire stimulus check to a friend. While the latter is a form of insurance, the former is not. As I cannot separate these uses, I drop transactions equal to stimulus amounts on deposit dates in April 2020, January 2021, and March 2021.[13] 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. I further explain P2P categorization in appendix C.6.4.

After removing these transactions, I sum the remaining P2P inflows and outflows at the user-month level.[14] I plot these series in Figure 1, which show 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 users likely sent money out of their stimulus checks to one another.

Most Earnin users employ P2P payment platforms, though some more actively.[15] Figure 2 shows histograms of total P2P inflows and outflows by user-month and the P2P share of total inflows or outflows by user. About 25% (40%) of user-months have zero outflows (inflows), but less than 10% of users never have P2P inflows. Additionally, P2P outflows have a much longer right-tail than P2P inflows because P2P platforms can be used to pay businesses, leading to more outflows per month, while P2P inflows 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.1 presents the same histograms for the number of P2P transactions, telling a similar story to the dollar amounts.

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 digital payment systems over cash. In appendix B.3, I show that Earnin users receive increases in P2P inflows during unemployment spells that are similar in magnitude to the increases in transfers reported by respondents in the PSID during unemployment spells.[16] These similar magnitudes suggest that P2P inflows make up a majority of support received by Earnin users. 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

[13]Event study point estimates do not change when omitting these months entirely from the analysis.

[14]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 informal support.

[15]I present summary statistics for P2P inflows, outflows, UI inflows, earnings, gig earnings, and overall outflows and inflows in table A4.

[16]Specifically, Earnin users receive $175, while PSID respondents receive $293 on average.

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). 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 in October 2019 and October 2020 and categorize transactions by payment instrument. 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.5 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 conduct more transactions on P2P platforms relative to the nationally-representative DCPC sample.

Beyond the increased adoption of P2P, these platforms also help expand informal insurance networks. P2P platforms reduce the fixed cost of making transfers, which means informal insurance networks expand to include newer members (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 with stronger ties. 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 are 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 challenges the validity of my crowd-out estimates is that cash and check transfers are disproportionately given in person by close friends and family, 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 model 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 to 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.

Given the well-documented challenges associated with two-way fixed effect event study designs (Goodman-Bacon, 2021), I implement the two-stage difference-in-difference by Gardner (2022) and the Local Projections Approach by Dube et al. (2023) in Section 7. Results are quantitatively and qualitatively similar.

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 11 months, for a total of \$175. 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 inflow 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 on average. Figure 4 shows an event study of the share of average monthly earnings replaced by P2P inflows. Specifically, I subset to observations taken two months prior to job loss for each user and calculate the average earnings across these user-months. I then divide the raw P2P inflows received by each user by this average earnings to get the share matched by P2P inflows before and after job loss.[17] The results indicate that P2P inflows replace 1.4% of lost earnings on average in the month after job loss before falling in the long-run as inflows taper off in the long run.

It is possible that I am underestimating the replacement rate of informal support by focusing on

[17] I use the average earnings across all user months as this is the most straight-forward calculation. In appendix B.2, I present the same event study with alternative measures of the replacement rate by P2P inflows, which tell a similar story about the role of P2P.

P2P inflows, which are only one way to receive transfers. To address this concern, I follow Edwards (2020) to estimate the change in annual informal transfers from family and friends reported by respondents to the PSID in a year when they experience at least one week of unemployment. She finds informal transfers from family increase by \$77[18] over the sample period of 1976 through 2013. In appendix B.3, I extend her analysis and find an average annual increase of \$293 in transfers during 2020, while my estimates show \$175. While the PSID shows a larger change in annual informal support, Earnin users tend to be poorer than than the PSID sample, which is representative of the US population. In section 4.3, I show Earnin users with the highest pre-job loss earnings receive even more support, consistent with the PSID results. Altogether, these results suggest that P2P inflows capture at least 60% of the total change in informal support.

4.2 Heterogeneous support

This relatively small average increase in P2P inflows obscures underlying heterogeneity in 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.[19]

To explore which users receive more support, I look at how P2P inflows differ by economic and demographic groups using equation 2 to estimate coefficients on relative dummies interacted with a group indicator G_{it}. I pool month fixed effects across groups so the event study coefficients isolate within group variation.

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

4.2.1 Heterogeneous support by P2P use

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 these individuals will see larger average increases.

To assess heterogeneity due to P2P use, I create three groups using the tercile of the median monthly share of each user's cumulative transaction flows, conducted on P2P platforms more than three months before job loss. Figure 6 shows event study coefficients for each group estimated using equation 2. The coefficients show pretrends because these groups have differential use prior to job loss. 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,

[18]Adjusted for inflation. Edwards (2020) finds an increase of \$70 in 2013 USD.

[19]In appendix B.4, I estimate the quantile treatment effects of job loss on P2P inflows across platforms showing the top 5% of changes in monthly P2P inflows are over \$900s.

considering P2P transactions capture far more of the overall financial activity of these users.

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.5, 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.

4.3 Heterogeneous support by demographics and economic conditions

I next consider heterogeneity by various demographic and economic characteristics. The results suggest that informal insurance networks target the neediest, but are somewhat inequitable likely because socioeconomic status (SES) tends to be correlated within social networks. This targeted, but inequitably distributed informal support has somewhat ambiguous welfare implications.

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 alongside total support by gender, parenthood, marital status,[20] length of unemployment, earnings prior to job loss, assets at job loss, whether the user lives in a county with above-median per capita income, and zipcode-level measurements of social capital taken from Chetty et al. (2022).[21]

I consider informal support by gender and parenthood 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 figure 7(a) 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.

[20] I impute family composition using the Child Tax Credit payments and the first Economic Impact Payment, detailed further in appendix B.6.

[21] The associated data construction and event studies, as well as additional results related to risk aversion, are discussed in appendices B.6-B.10.

The long-term unemployed receive even more support than single mothers. Figure 7(b) shows that those unemployed for seven or more 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.

While informal insurance networks target single mothers and the long-term unemployed, figure 7(b) shows that poorer workers receive less informal support after job loss. For each worker, I calculate their median monthly earnings prior to job loss and record their bank balance in the month before their last paycheck deposit. I then group workers by tercile of their prior earnings and bank balance.[22] After a job loss, workers in the bottom tercile of prior earnings receive only $40 compared to $380 for those in the top tercile – a precisely estimated difference. Similarly, those with the least precautionary savings, based on tercile of bank balance before job loss, tend to receive the least support. After job loss, the bottom, middle, and top terciles receive $100, $200, and $280, respectively, in increased P2P inflows though the differences are not precisely estimated.

These results suggest informal insurance networks target inequitably, with ambiguous welfare implications. On one hand, higher earners often have more consumption to smooth, making the additional informal support well-targeted. On the other hand, those with lower precautionary savings would benefit more from the additional liquidity provided by informal support.

One reason higher income and wealthier people might receive more support is their networks are wealthier or higher income, and can offer more support. The bars for above- and below-median per capita county 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 SES users in all zip codes.[23] Furthermore, Chetty et al. (2022) separate economic connectedness into Exposure, 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(b) 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

[22]Further details on these measures can be found in appendices B.8 and B.9.

[23]High SES measures income, wealth, educational status, as well as other characteristics detailed in the paper.

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.

4.4 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 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, I present the same event study as shown in figure 6 with total spending as an outcome in figure . 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 job loss, this suggests users pass through most of this support indicating a large marginal propensity to consume out of informal support. Appendix B.11 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 P2P transfers, public UI may be 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.

Before moving into more complex specifications, consider workers who lose a job in 2020 and receive more UI benefits due to FPUC. If expanded UI crowds out informal support, this should be associated with lower P2P inflows. Instead, in figure 7(b) the bars for job loss show no difference in total support after job losses in 2019 and 2020.[24] That said, the pandemic accelerated a shift

[24]The event study in appendix A.8 shows P2P inflows also follow a similar path around job loss in both years.

towards P2P inflows, which could bias estimates for 2020 in either direction.

Instead of comparing across time periods, 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.

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 were 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 event study coefficients from equation 2 grouped by whether a user received UI early (in 0 to 1 months) or late (in 2 to 6 months). Early recipients receive P2P inflows for a shorter duration, while later 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. P2P outflows also exhibit differences by UI timing after initially dropping of for both groups. After UI onset, early recipients' P2P outflows spike, while P2P outflows remain diminished for later recipients. This spike roughly matches the P2P inflow increase suggesting that initial transfers are paid back after receiving UI.

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. This latter argument is consistent with the finding that those with higher pre-job loss earnings receive more P2P inflows after job loss. Figures A.32 shows P2P inflows are similar in states with higher and lower average UI replacement rates, which are arguably exogenous to individual earnings.

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 (IV-DID) results from three separate plausibly exogenous changes to UI during the pandemic, following Ganong et al. (2024) and Coombs et al. (2021). The first compares UI recipients in states that withdrew early from $300/week in expanded federal UI benefits to those in states that retained benefits through September 2021. The second compares those who have received UI to those who have not yet received UI, exploiting exogenous payment delays caused by an overload of UI applications at the start of the pandemic. The third isolates the effect of the expiration of $600/week PUA benefits in July 2020. I summarize the three separate designs in table 1. Together these three quasiexogenous changes to UI benefits 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.[25]

5.1.1 Natural experiment #1: June withdrawal from UI benefits

In the first natural experiment, I isolate a cohort of those unemployed and insured during the week ending April 30, 2021 living in one of 43 states in which I accurately flag UI benefits. Of these 43 states, 23 retained FPUC, PUA, and PEUC through September 2021 and 19 withdrew in June 2021.[26] I restrict my sample to the unemployed and insured at the end of April 2021 because governors started to announce withdrawals in the first week of May 2021 to reduce contamination from anticipation of lost benefits among the newly unemployed.

Equation 3 shows the specification comparing users in retain states to withdrawal states in April 2021 versus August 2021. The main 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 announced changes in any of the P2P platforms' policies during this period, so this seems like a safe assumption. I also assume that there are no differential trends in P2P inflows between the withdrawal and retain states. The main difference between these states was political in 2021: withdrawal states were predominantly led by Republican governors while retain states were run by Democratic governors. As such, I specifically assume that changes in individual social network P2P transfers are uncorrelated with state-level politics. Given social networks change slowly, this assumption is likely reasonable. Furthermore, figure 12(b) shows that average P2P flows track closely between both groups of states prior to June 2021 suggesting similar P2P trends.

[25] Figure A.9 shows the spending and P2P outflows around each natural experiment and are largely consistent with a marginal propensity to consume out of UI benefits.

[26] Indiana also announced a withdrawal, but the government was court ordered to reinstate benefits through September 2021 and restarted benefits in July. It is removed from the sample to reduce contamination of estimates, but results are robust to its inclusion. (Source: AP News July 12, 2021)

$$
\begin{aligned}
\text{P2P}_{it} &= \gamma \hat{\text{UI}}_{it} + \lambda_i + \lambda_t + \epsilon_{it} \\
\text{UI}_{it} &= \beta \text{Retain}_i \times (\text{August 2021})_t + \alpha_i + \alpha_t + \nu_{it}
\end{aligned} \tag{3}
$$

Figures 12(a) shows the average UI inflows for the unemployed and insured across retain and withdrawal 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, more users in retain states had received UI for longer, so I re-weight these estimates using inverse propensity weighting described in appendix B.13.

While re-weighting ensures that I compare similar users across states, it does not account for different economic conditions across withdrawal and retain states. Ganong et al. (2022) find that withdrawal states had higher average employment growth rates than retain states leading up to the withdrawal announcements in the Spring 2021 before converging over the summer. Greater employment growth might mean that a person's friends and family are more likely to be employed, and thus offer more support after UI benefits fall. If so, this approach would overestimate crowd-out. Given that my estimates are already close to zero, this would not change my conclusions. Furthermore, figure 12(b) shows the average P2P inflows across retain and withdrawal states exhibited parallel trends throughout 2021 suggesting that any differential employment growth did not lead to differential P2P inflows trends.

5.1.2 Natural experiment #2: March 2020 UI delays

In the second 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.

$$
\begin{aligned}
\text{P2P}_{it} &= \gamma \hat{\text{UI}}_{it} + \lambda_i + \lambda_t + \epsilon_{it} \\
\text{UI}_{it} &= \beta \text{March UI recipient}_i \times (\text{May 2020})_t + \alpha_i + \alpha_t + \nu_{it}
\end{aligned} \tag{4}
$$

This analysis relies on the assumption that UI delays during the pandemic are exogenous to P2P support. Since the majority of delays resulted from overwhelmed application systems and administrative bottlenecks, preventing UI systems from prioritizing specific claims (Bitler et al., 2020), this assumption appears highly plausible. Furthermore, if those with more generous informal support tend to apply for benefits more slowly (or vice versa), the massive delays through this period would blur any selection effects. Still, I repeat the same difference-in-difference design, but instrument for UI benefits using the Department of Labor's ETA 9050 First Time Payment Time Lapse data. Results discussed in appendix B.14 and do not indicate crowd-out.

Figures 12(c) and 12(d) 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. UI increases in the month of benefit onset with higher spikes for the May and June receipt cohorts, representing backfilled payments. In contrast, P2P inflows increase for all three groups simultaneously with only a slight slowdown in April for the April receipt cohort, suggesting little crowd-out.

Instead, this period may have been a time of increased informal support in the form of "informal credit." The unemployed are instead borrowing against their expected UI benefit payments, suggesting a type of intertemporal crowding in of informal insurance when expected formal insurance increases as considered in Mobarak and Rosenzweig (2012). Still, the results are not sufficiently precise to make any definitive statements.

5.1.3 Natural experiment #3: July 2020 expiration of $600/week

In the third natural experiment, I exploit the sudden expiration of the $600/week FPUC benefit in July 2020 to estimate the change in UI and P2P inflows between June and August 2020. Following Ganong et al. (2024), my treatment group is a cohort of March 2020 job losers who received a UI payment by June 19, 2020 and my control group is a cohort of workers who are continuously employed through 2020, but experience a job separation in 2021. This approach isolates the effect of the sudden withdrawal of expanded UI benefits in July 2020 on informal transfers.

I select the continuously employed in 2020 as a control group instead of those still awaiting UI benefits as in natural experiment #2 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. Equation 5 shows the exact specification I run for the month of June and August 2020.[27]

$$\begin{aligned}\text{P2P}_{it} &= \gamma\hat{\text{UI}}_{it} + \lambda_i + \lambda_t + \epsilon_{it} \\ \text{UI}_{it} &= \beta\text{March job loser, receiving UI}_i \times (\text{August 2020})_t + \alpha_i + \alpha_t + \nu_{it}\end{aligned} \tag{5}$$

There are drawbacks to comparing the unemployed to the continuously employed. For example, it would not make sense to compare these groups immediately after the March 2020 job loss, but figures 12(e) and 12(f) show that by June 2020, these groups had very similar trends in P2P inflows resembling a seasonal increase in economic activity in the summer. These figures also show that P2P inflows decline for both groups after in August, and by more for UI recipients, which is more consistent with a crowd-in relationship.

Still, these two groups likely have different pandemic experiences. For example, workers that stayed employed in 2020 were more likely to see others (via Zoom or in-person) and potentially maintain social ties. Still P2P inflows for employed workers may be more stable through the summer of 2020 than the unemployed who are largely homebound during the pandemic. In appendix B.15, I use a triple difference design that extends this analysis through 2021 to compare the continuously

[27] While I use 2021 to determine whether a user is continuously employed until 2021, I do not use 2021 data in this specification.

employed to the unemployed in June and August 2021. These results also suggest negligible crowd-out.

5.1.4 Crowd-out results

Table 2 shows the dollar-for-dollar crowd-out results from these IV-DID specifications and their OLS counterparts. Each regression yields the same result: very small values or precisely estimated zeroes.[28] 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 \$10 of crowded-out P2P inflows in the June 2021 withdrawal. I further add the average P2P inflows and UI inflows, which are \$633.65 and \$1,851.40 for June 2021, respectively. Together, back of the envelope calculations imply an elasticity of -0.15 in June 2021, which is one-fifth of the elasticity of severance pay found by Chetty and Saez (2010).[29]

There are few comparable estimates in the literature to contextualize my results. Schoeni (2002) offers the most comparable estimate finding heads of household in the 1988 PSID received \$0.24 to \$0.40 less in informal support for every dollar of UI benefits. As mentioned, this estimate suffers from weak instruments and the confidence interval overlaps with my own estimates. Additionally, it covers a period when UI benefits were lower and P2P platforms did not exist, making it difficult to compare directly. Edwards (2020) expands this PSID analysis to cover 1976 to 2013, finding a crowd-out elasticity of -0.04, on the extensive margin, which is consistent with my own estimates in Table A5.[30]

It is possible that crowd-out may be greater among those with more P2P inflows. As a result, I repeat these regressions, but add the additional sample criteria that all users have used P2P inflows in the first month of the analysis period. I condition on P2P use in the first month because it is consistent with the sample criteria that users be unemployed (and insured in the case of June 2020) in the first month of the analysis period. Table 3 shows less precise point estimates due to a smaller sample size, but are otherwise consistent with my main results with the point estimate for June 2021 implying an elasticity of -0.11.

Given that P2P support is elevated in higher income counties, I repeat this analysis, but split the sample by whether the user lives in a county with per capita income above or below the national median. Table 4 shows that 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 crowd out informal insurance. Furthermore, they suggest that the welfare consequences associated with crowd-out of informal insurance are negligible.

[28] These results are robust to controlling for monthly county-level COVID case counts and deaths from the CDC as shown in table A6. Appendix tables A5 through 3 show extensive margin, reduced form, log, and conditional results, which tell a consistent story of minimal crowd-out.

[29] Calculating the elasticity yields $1851/634 \times -.06 = -0.15$

[30] Extensive margin estimates are not possible in July as no UI recipients in my sample stopped receiving UI after the expiration of the \$600/week benefit by construction.

6 Model: Unemployment insurance with crowd-out of informal insurance

This section presents the framework provided by 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.

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. 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, $U(C, z; n) = u(c) - h(z/n)$.

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. 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 yield a threshold n^* above which workers will choose to z_H and below which they will choose z_L shown in equation 6.

$$u(z_h - \tau - \tau_P) - u(z_l + b + b^p) = h(z_H/n^*) - h(z_L/n^*) \tag{6}$$

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

$$W = \int_0^{n^*} [u(z_L + b + b^p) - h(z_L/n)]dF(n) + \int_{n^*}^{\infty} [u(z_H - \tau - \tau_P) = h(Z_h/n)]dF(n) \tag{7}$$

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:

$$W(e) = eu(z_H - \tau - \tau_P) + (1-e)u(Z_L + b + b^p) - \varphi(e) \tag{8}$$

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:

$$W = eu\left(z_H - \frac{1-e}{e}\left(b^p(b) + b\right)\right) + (1-e)u\left(z_L + b^p(b) + b\right) - \varphi(e) \tag{9}$$

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:[31]

$$\frac{dW}{db} = (1-e)(1-r)u'(c_H)\left[\frac{u'(c_L) - u'(c_H)}{u'(c_H)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1 + b^p/b}{1-r}\right] \tag{10}$$

Equation 10 is analogous to the Baily-Chetty formula for optimal UI with private insurance included. The first term measures the marginal value of insurance for smoothing consumption. If zero, the user perfectly smooths utility across states. The second term measures the moral hazard of insurance, summarizing its cost. 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:

$$\begin{aligned} G(b) &= \frac{dW}{db}\frac{1}{1-e}/\frac{dW}{dz_H}\frac{1}{e} \\ &= (1-r)\left[\frac{u'\left(c_L-\right) - u'(c_H)}{u'\left(c_H\right)} - \frac{\varepsilon_{1-e,b}}{e}\frac{1 + b_p/b}{1-r}\right] \end{aligned} \tag{11}$$

Equation 11 allows me to tractably measure the welfare change due to changes in unemployment insurance benefits. I contrast the measure of welfare with and without crowd-out to assess the additional welfare loss due to crowd-out.

This sufficient statistics approach rests on two key assumptions (1) a constant crowd-out elasticity and (2) equivalent labor supply elasticities with respect to formal and informal transfers. These assumptions work together to simplify the welfare calculation to apply regardless of the informal share of insurance and the extent of crowd-out. Under assumption (1), a large crowd-out elasticity implies that, regardless of their initial level, informal transfers would largely replace a reduction in formal UI benefits leaving total insurance effectively stable, such that UI has a small consumption smoothing benefit. Under assumption (2), the moral hazard effect, captured by the labor supply

[31] Proof in Chetty and Saez (2010).

elasticity of total insurance, is left unchanged because the level of total insurance remains stable.

I cannot test the second assumption directly, so I follow the Chetty and Saez (2010) in assuming that the elasticity of labor supply is constant across insurance types. In contrast, I have some empirical evidence for the second assumption. Table 3 shows that the crowd-out effect is relatively small even when I condition on P2P use. Back of the envelope calculations, suggest a crowd-out elasticity of -0.11, which is on par with the unconditional elasticity of -0.15.[32] While the results are less precise with a smaller sample, they suggest that the crowd-out elasticity is relatively constant across various ranges of informal insurance, making my second assumption more plausible.

6.1 Calculating welfare loss

Equation 11 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.

For the crowd-out parameters r and $1+b_p/b$, I use my own estimates. I calculate welfare for the values of $r \in \{-0.001, 0.05\}$ taken from the estimates in Table 2. Next, I calculate $1+b^p/b$ using the excess P2P ratio of private and public unemployment insurance, detailed in appendix B.16.[33] In 2019, $b^p/b = 0.08$ among UI recipients, but falls to 0.07 as UI levels rose after 2020.

As a caveat, I have to restrict my crowd-out estimates to informal insurance, which may differ from crowd-out of other sources of private insurance like severance pay, estimated by Chetty and Saez (2010) to be $r = 0.14$. Severance pay is determined by slow-moving company policies, making it unlikely to be crowded out in the short-run and thus unlikely to affect my estimates of month-to-month crowd-out. A further caveat is that the long-run crowd-out of informal insurance could be much larger, as more generous social insurance programs could reduce the formation of informal support networks. As such, it is best to interpret my calculations as short-run welfare effects.

For the other statistics and parameters of the model, I follow Chetty (2006). The change in utility, is best approximated by the expression $\frac{u'(c_l)-u'(c_H)}{u'(c_H)} = \left(\frac{c_e}{c_u}\right)^\gamma - 1$, where γ is the coefficient of relative risk aversion and $c_e/c_u = 1/0.92$ (Ganong et al., 2024) is the ratio of consumption while employed. Next, I use the share employed in April 2020 from Ansell and Mullins (2021) of $e = 0.85$. 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 job search costs (Ganong et al., 2024).

$$
\begin{aligned}
e &= 0.85 \text{ from Current Employment Statistics in Ansell and Mullins (2021)} \\
\frac{c_e}{c_u} &= 1/0.92 \text{ from Ganong et al. (2024)} \\
\gamma &= 2 \text{ from Chetty (2006)} \\
\varepsilon_{1-e,b} &\in \{0.07, 0.5\} \text{ from Ganong et al. (2024)}
\end{aligned}
$$

[32] The mean of P2P inflows is \$929, while the mean of UI inflows is \$1,851. $1851/929 \times -.06 = -0.11$

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

Table 5 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 40 cents using my largest crowd-out estimate. During the pandemic, the welfare money metric is positive and equal to eight and 10 cents with and without crowd-out. Notably, labor supply elasticity drives the extent of the welfare consequences due to 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 may represent only a subset of the informal insurance in 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 12 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 the pandemic. This ratio is also well outside the range that I estimate or found in the PSID.[34]

$$\begin{aligned} G(b_p/b) &= 0.96 \times \left(\left(\frac{1}{.92} \right)^2 - 1 - \frac{.07}{.85} \times \frac{1 + b_p/b}{0.96} \right) \\ &= (0.17 - .08(1 + b_p/b)) \end{aligned} \tag{12}$$

7 Robustness

There are several 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.29 and 10, this seems unlikely. Similarly, figure A.10 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 seven 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

[34] See appendix B.16 for more details.

\$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.

7.1 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.

7.1.1 Two-stage difference-in-difference

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 13. 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.

$$\begin{aligned} y_{it}(0) &= \lambda_i + \lambda_t + \nu_{it} \\ \tilde{y}_{it} &= y_{it} - \hat{\mu}_i - \hat{\mu}_t \\ \tilde{y}_{it} &= \beta_{-5} \sum_{s \leq -5} D_s^t + \sum_{s \in [-4,-2]} \beta_s D_{it}^s + \sum_{s \in [0,9]} \beta_s D_{it}^s + \beta_{10} \sum_{s \geq 10} D_{it}^s + \varepsilon_{it} \end{aligned} \tag{13}$$

I present the event study results in figure A.11, 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.

7.1.2 Local Projections Approach to difference-in-differences

Dube et al. (2023) present an alternative solution to heterogeneous treatment timing, the local projection (LP) based difference-in-differences approach. Effectively, this method puts in place a "clean control" condition, i.e. that none of the control units for a given treatment have experienced a prior treatment. This "clean control" condition paired with the standard "no anticipation" and "parallel trends" assumptions provide an estimate that is robust to bias related to negative weighting.

The estimation approach is shown in equation 14.

$$y_{i,t+s} - y_{i,t-2} = \lambda_t^s + \beta_s^{LP} \Delta D_{it} + \varepsilon_{it}^s; \quad \text{for } s = -S, -S+1, \ldots, 1, \ldots, S \tag{14}$$

As the "clean control" condition changes the sample composition for each relative time period s, the LP method estimates a separate regression to recover each β_s^{LP}. As separate regressions render unit fixed effects impossible, first differences are taken, in this case, relative to period $t-2$.

The results are shown in Figure 14 with standard errors clustered at the user-level. Despite a slight pre-trend in period $s = -3$, these results are qualitatively and quantitatively similar to the baseline results.

7.2 Placebo test of correlation between unemployment timing and P2P withdrawals

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 account for job loss expectations. Figure A.13 shows 10,000 placebo estimates of the average increase in monthly P2P inflows against the true treatment effect for P2P inflows 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 the extent that it is crowded out by formal unemployment insurance. Overall, I find that informal insurance is a short-run form of support that imperfectly targets those with the most need. Furthermore, crowd-out estimates suggest that formal insurance is not keeping informal insurance networks from flourishing in the short run.

The results 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 workers who actively use P2P 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 with high prior earnings or living 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 little replacement of lost earnings, I asked whether 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 $0.05 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 by 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 pandemic era social safety net in the United States will weaken informal support structures in 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.

Use of Artificial Intelligence

During the preparation of this work the author used GitHub CoPilot in order to edit code and copyedit. After using this tool/service, the author reviewed and edited the content as needed and takes full responsibility for the content of the publication.

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Tables

Table 1. Guide to Natural Experiments

Experiment	Sample Selection	Treatment Group	Control Group	Pre Period	Post Period
Natural Experiment 1	Unemployed and insured as of April 30, 2021	States that withdraw benefits before September 2021	States that retain benefits through September 2021	May 2021	August 2021
Natural Experiment 2	Unemployed in March 2020, receiving UI by June 2020	April 2020 UI recipients	June 2020 UI recipients	March 2020	May 2020
Natural Experiment 3	Unemployed in March 2020, receiving UI by June 2020 or continuously employed until 2021	Unemployed & Insured	Continuously employed	June 2020	August 2020

This table summarizes the treatment and control groups and the pre and post-period associated with each natural experiment's difference-in-difference design in the paper.

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

Method		OLS			IV	
Policy Change	June Withdrawal	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	-0.01*	0.005	-0.002	-0.05*	0.003	0.01*
	(0.006)	(0.004)	(0.003)	(0.03)	(0.007)	(0.007)
Standard-Errors	State	User		State	User	
Dependent variable mean	633.65	313.05	441.30	633.65	313.05	441.30
Mean of UI Inflows	1,851.4	1,328.9	1,734.7	1,851.4	1,328.9	1,734.7
Observations	28,546	34,508	31,746	28,546	34,508	31,746
R^2	0.72320	0.72837	0.75514	0.72283	0.72837	0.75480
F-test (1st stage), UI Inflows				5,083.0	27,825.4	7,683.9
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 (4) 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. Columns (2) and (5) 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 (6) 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. P2P inflows as measured using regular expression flags of bank memos. Odd columns are OLS results, even columns are IV results. Standard errors in (1) and (4) are clustered by state and the rest by user.

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

Table 3. Crowd-out of P2P Inflows by UI during various pandemic policy events conditional on using P2P in first month

Method		OLS			IV	
Policy Change	June Withdrawal	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	-0.01	0.01	-0.002	-0.06	0.01	0.02**
	(0.009)	(0.007)	(0.003)	(0.04)	(0.01)	(0.01)
Standard-Errors	State	User		State	User	
Dependent variable mean	924.84	498.43	672.71	924.84	498.43	672.71
Mean of UI Inflows	1,851.4	1,328.9	1,734.7	1,851.4	1,328.9	1,734.7
Observations	18,786	20,540	20,092	18,786	20,540	20,092
R^2	0.70826	0.71025	0.73770	0.70772	0.71024	0.73680
F-test (1st stage), UI Inflows				3,597.7	18,312.7	3,242.9
User and 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 the first month of the difference-in-difference design. Columns (1) and (4) 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. Columns (2) and (5) 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 (6) 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. P2P inflows as measured using regular expression flags of bank memos. Odd columns are OLS results, even columns are IV results. Standard errors in (1) and (4) are clustered by state and the rest by user.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Crowd-out of P2P Inflows by UI during various pandemic policy events subset by county income

Method	OLS						IV					
Policy Change	June Withdrawal		March Delays		July Expiration		June Withdrawal		March Delays		July Expiration	
	(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	0.04*	-0.02***	0.01	0.003	-0.005***	0.005	0.05	-0.06**	0.02	-0.0005	0.01	0.01
	(0.02)	(0.007)	(0.01)	(0.005)	(0.001)	(0.005)	(0.04)	(0.03)	(0.01)	(0.007)	(0.01)	(0.007)
Standard-Errors	State		User				State		User			
Dependent variable mean	609.32	637.69	274.84	319.96	393.10	449.79	609.32	637.69	274.84	319.96	393.10	449.79
Mean of UI Inflows	1,851.4	1,851.4	1,328.9	1,328.9	1,734.7	1,734.7	1,598.3	1,891.0	1,276.5	1,338.5	1,731.9	1,735.4
Observations	3,880	24,650	5,328	29,170	4,774	26,962	3,880	24,650	5,328	29,170	4,774	26,962
R^2	0.78852	0.71054	0.69718	0.73365	0.79048	0.74886	0.78850	0.71007	0.69713	0.73363	0.78726	0.74884
F-test (1st stage), UI Inflows							578.11	4,388.6	4,764.8	23,051.2	253.06	30,543.1
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), (2), (7), and (8) 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. Columns (2) and (5) 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), (4), (11), and (12) 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. P2P inflows as measured using regular expression flags of bank memos. Odd columns are OLS results, even columns are IV results. Standard errors in (1), (2), (7) and (8) are clustered by state and the rest by user.

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

Table 5. Money metric estimates of marginal welfare of additional dollar of UI before and after pandemic with and without crowd-out included

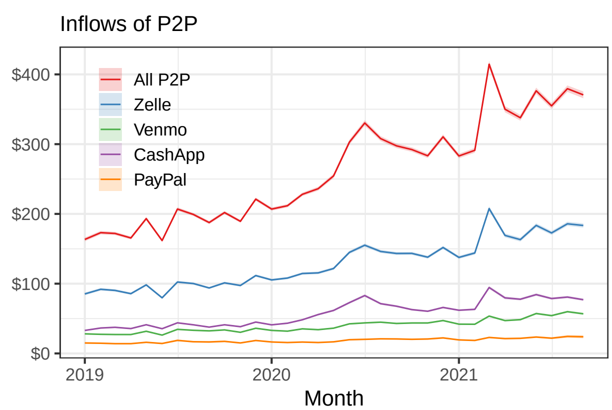
Context	ε	e	r	$b_p/(b+b_p)$	Standard	With crowd-out
Pandemic	0.07	0.85	-0.002	0.08	0.10	0.09
Pandemic	0.07	0.85	0.05	0.08	0.10	0.08
Pre-pandemic	0.5	0.95	-0.002	0.07	-0.34	-0.39
Pre-Pandemic	0.5	0.95	0.05	0.07	-0.34	-0.40

Money metric welfare effects of UI with and without crowd-out. Elasticities from Ganong et al. (2024). 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). Other numbers are taken from author's estimates in this paper.

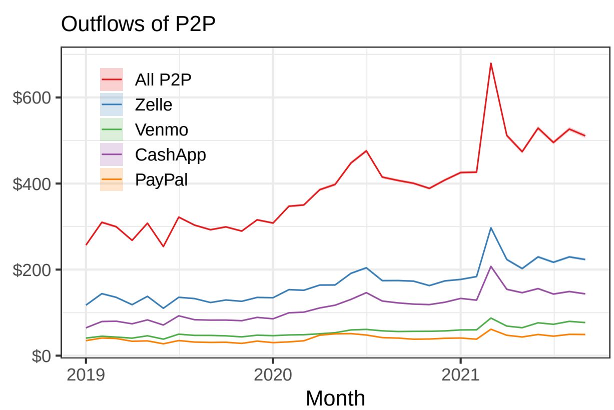
Figures

Figure 1. Timeline of P2P inflows and outflows

(a) Inflows

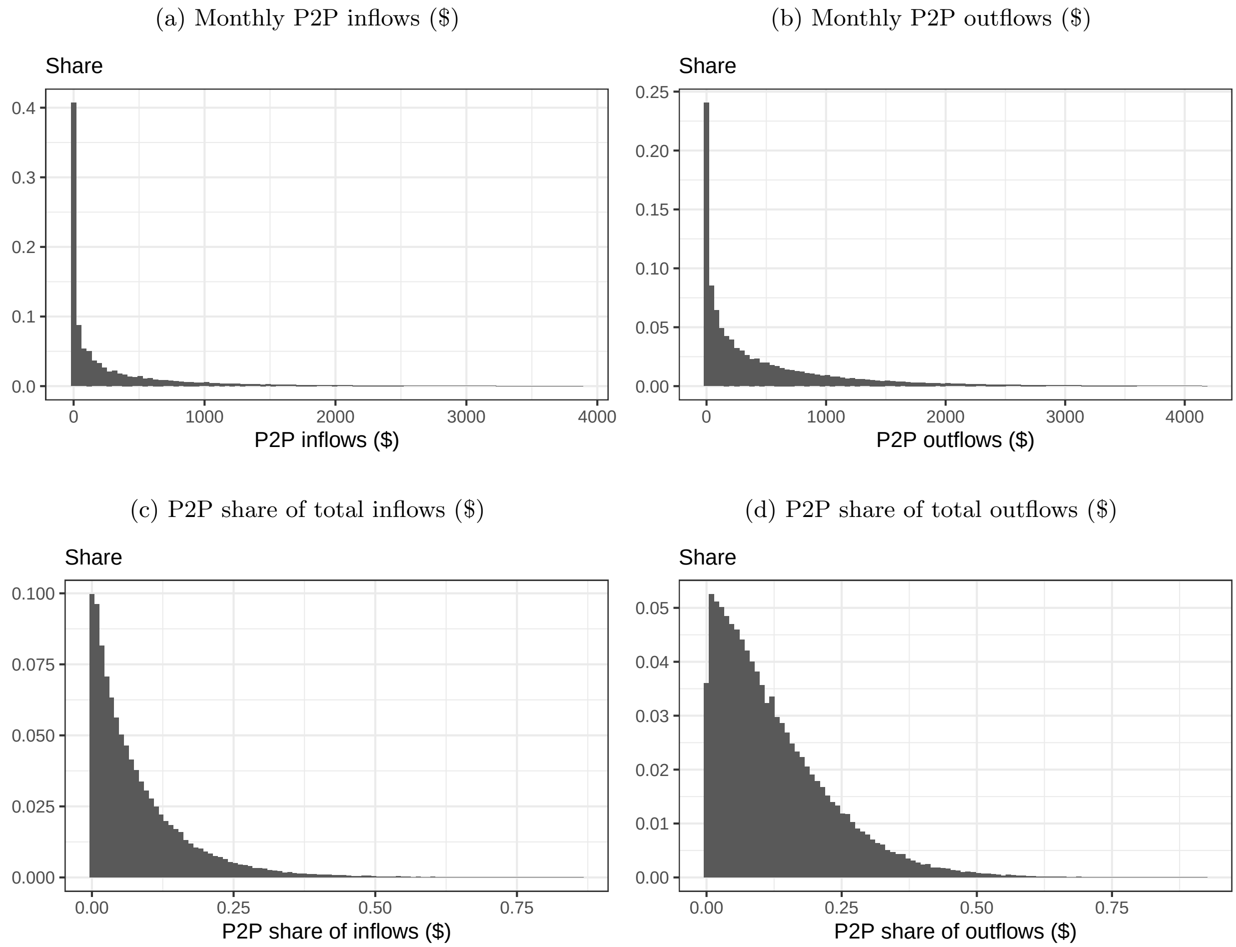


(b) Outflows



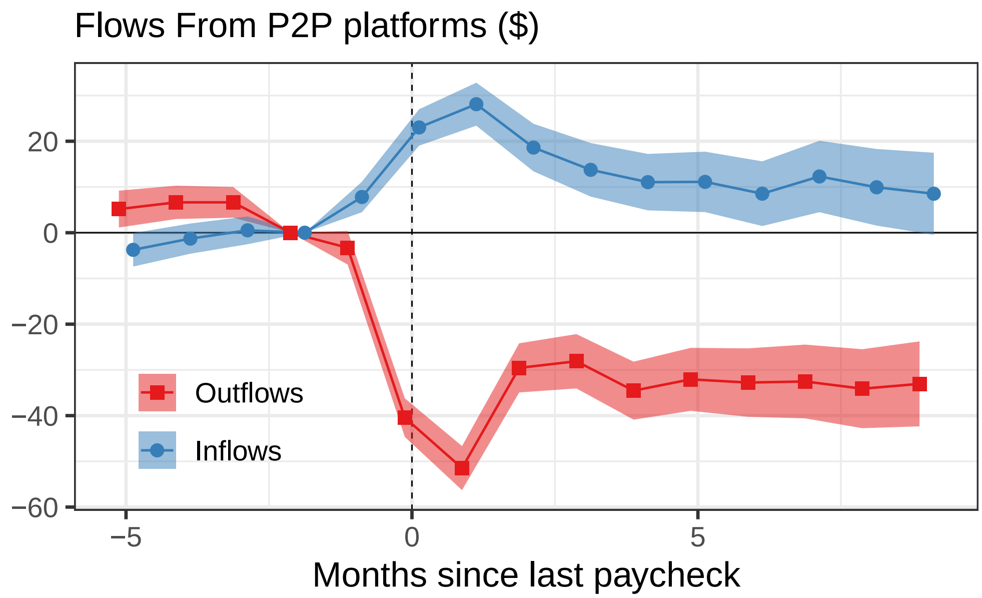
Notes: P2P inflows and outflow of transactions between \$5 and \$15,000, not linked to purchases, gig platforms, or stimulus payments over time. The series are constructed from all transactions with memos that meet regular expression search criteria or Plaid categories. The sample includes users with 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



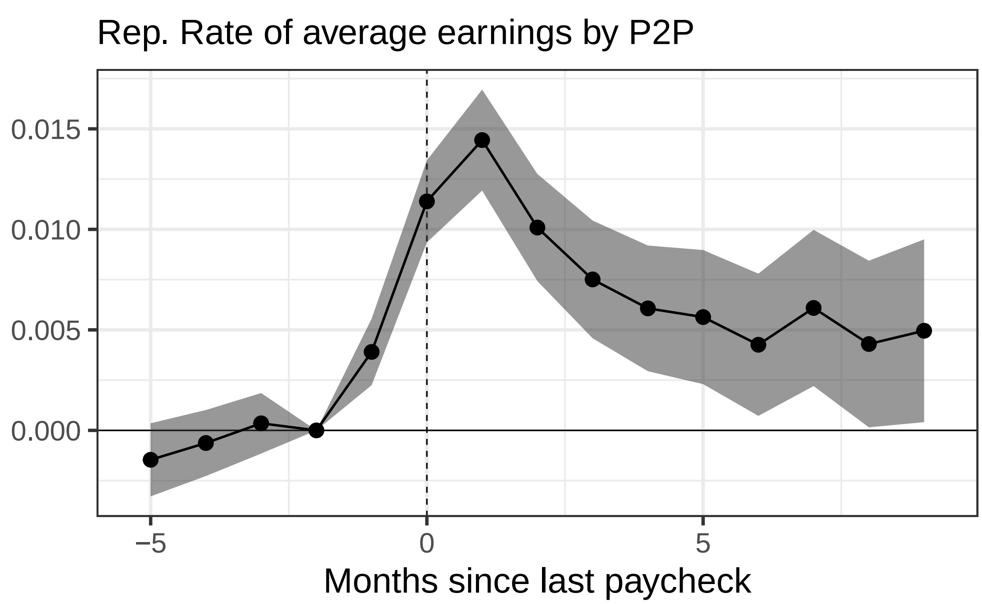
Notes: Figures (a) and (b) show the total P2P inflows and outflows, respectively, for each user-month in the data. Figures (c) and (d) show the share of total inflows and outflows, respectively, exchanged on P2P platforms. Sample includes users with at least five outflows per month and a job loss in 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 flows in or out of the user's bank account 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.

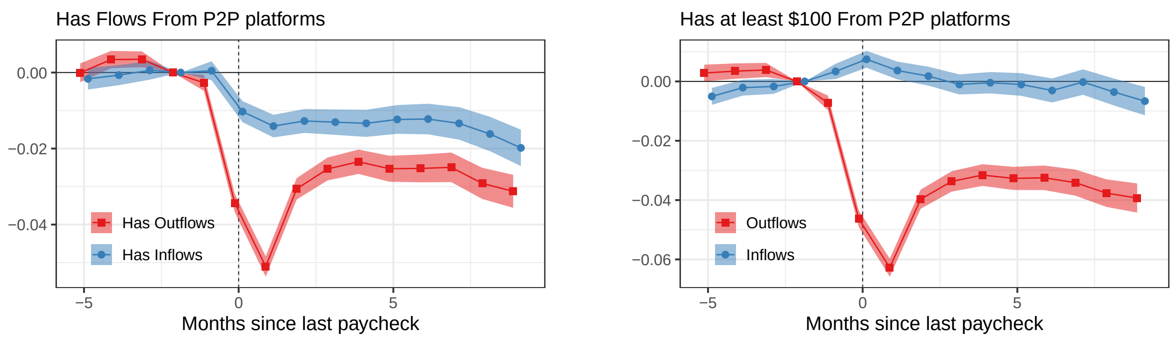
Figure 4. Event study of average earnings replaced by P2P inflows



Notes: Within-person event studies of the share of average monthly earnings prior to job loss replaced by P2P inflows. The series is the raw P2P inflows received by each user divided by the average monthly earnings prior to job loss. Changes are relative to the flows in or out of the user's bank account 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

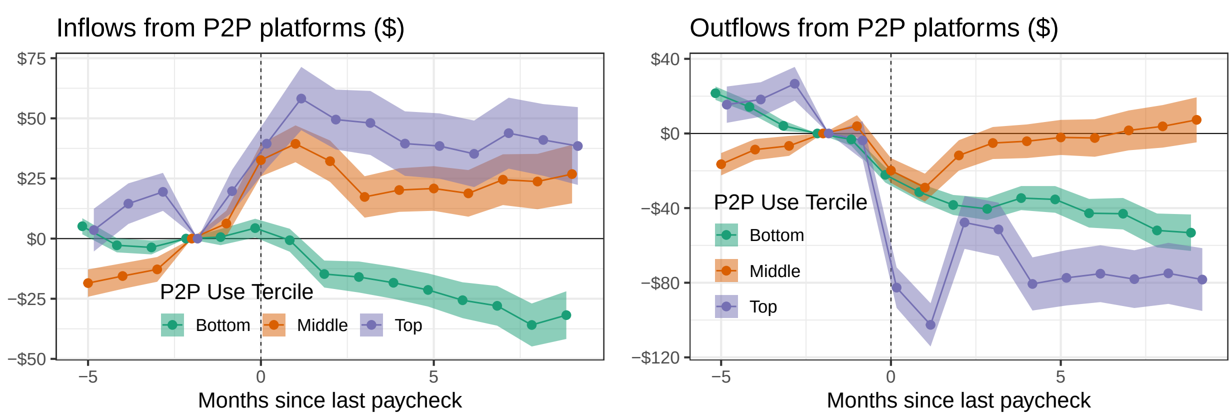
(a) $\mathbb{P}$(Monthly P2P > 0) (b) $\mathbb{P}$(Monthly P2P > 100)



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

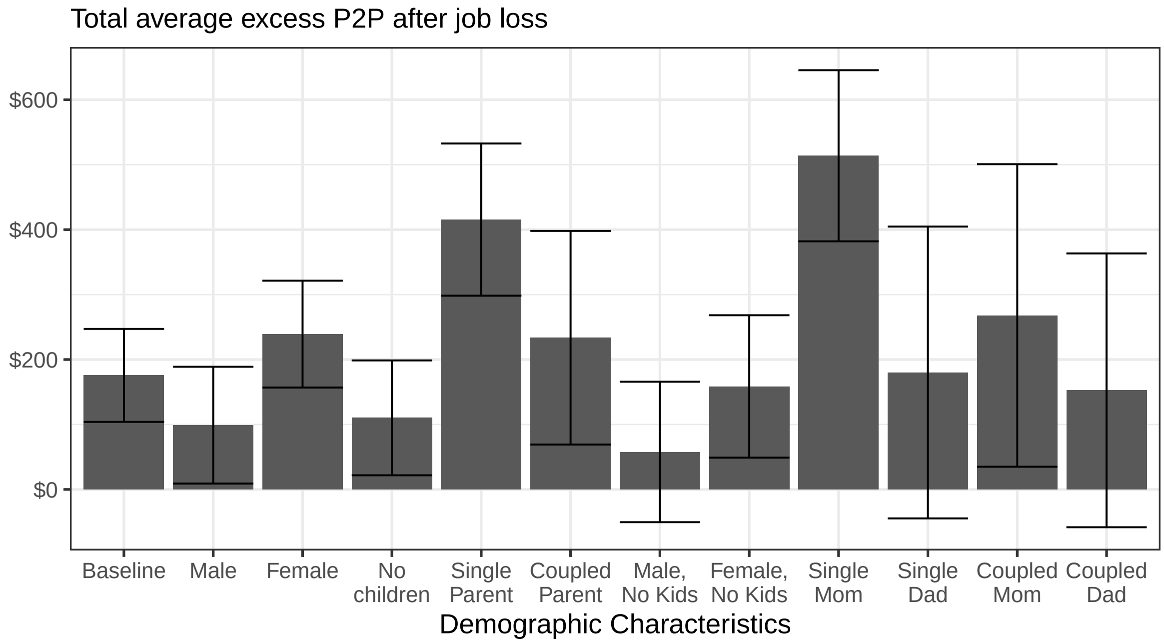
(a) Inflows, cumulative share (b) Outflows, cumulative share



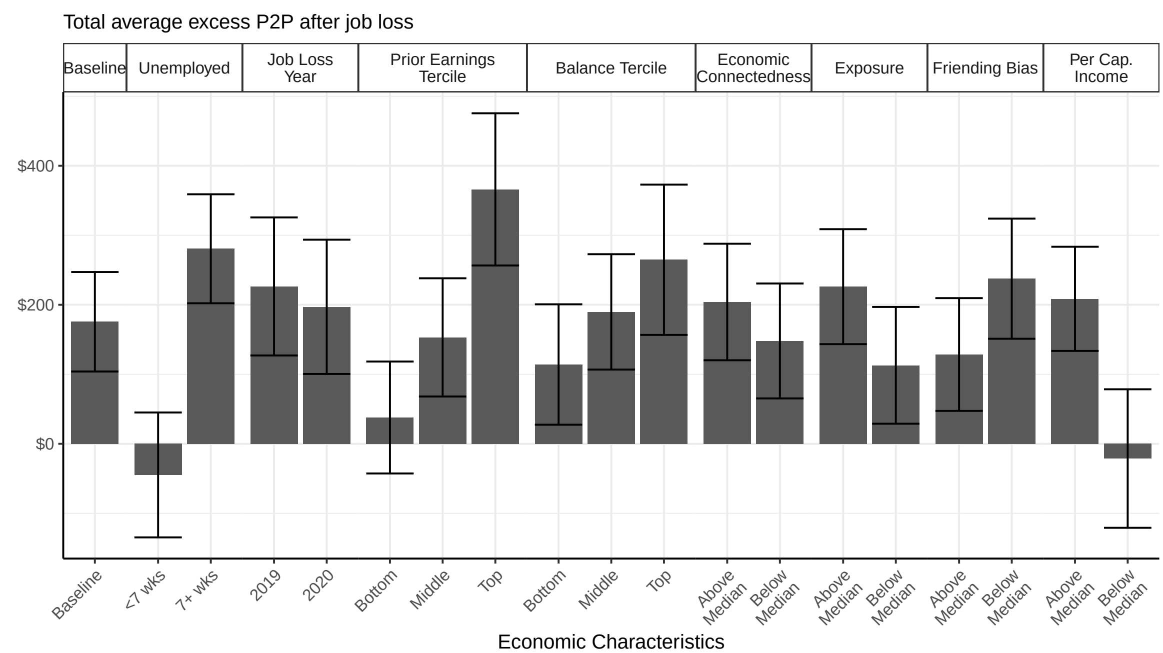
Notes: Event studies of P2P inflows and outflows around a user's first unemployment spell. Two months prior to job loss is omitted. The event study coefficients are relative time dummies interacted with indicators with tercile of the median monthly P2P share of cumulative flows two or more months before 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. Total average excess P2P inflows after unemployment by group

(a) Demographics

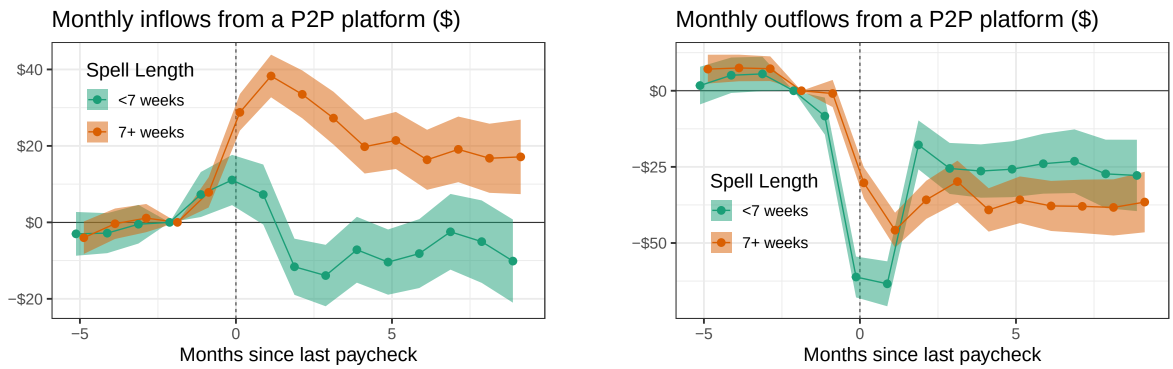


(b) Economic Characteristics



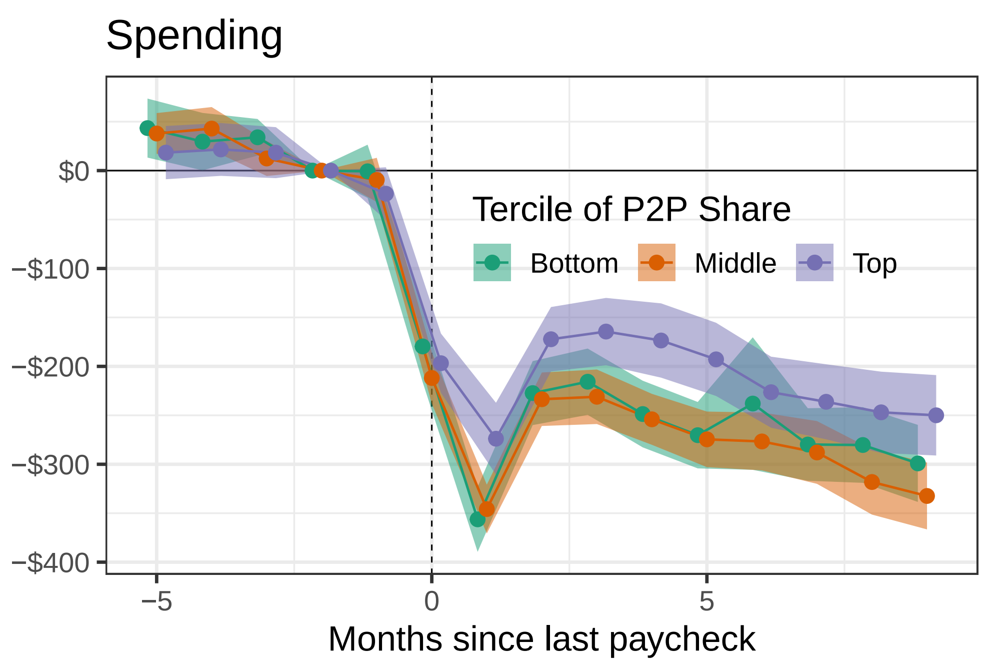
Notes: This figure shows the increase in P2P inflows after job loss by group. Each bar shows the sum of event study coefficients from one month before to 10 months after job loss. Gender comes from survey responses or name prediction. Relationship and parental status are inferred from economic impact payment and child tax credit formulas. Economic Connectedness, Exposure, and Friending Bias are from Chetty et al. (2022). Prior earnings tercile is based on median monthly earnings prior to job loss. Balance tercile is based on bank balance before job loss. Per capita income is from the 2019 American Community Survey. Standard errors are clustered at the user level. The sample includes users with at least five outflows per month and a job loss between July 2019 and September 2020 or September 2021. See appendix A for corresponding event studies.

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 seven weeks. changes are relative to the flows in or out of the user's bank account 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.

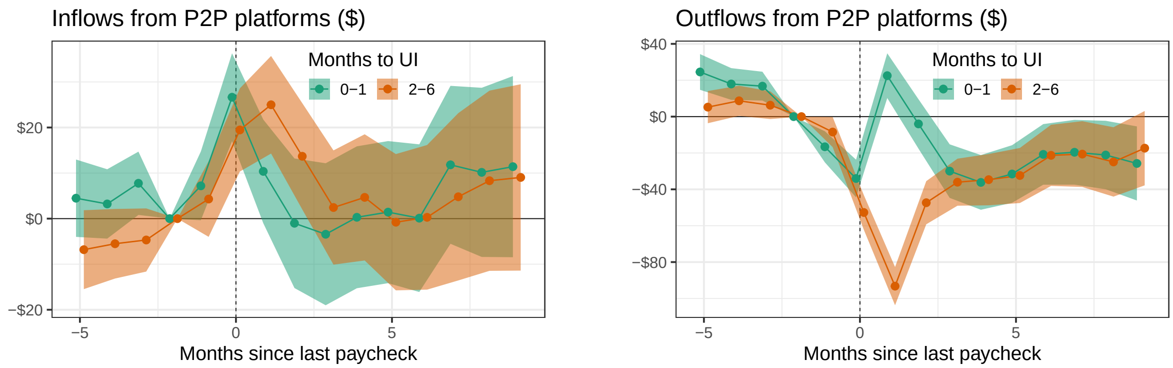
Figure 9. Event study of consumption by tercile of P2P share of inflows and outflows prior to job loss



Notes: Within-person event studies of spending a user's first unemployment spell. changes are relative to the flows in or out of the user's bank account 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.

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

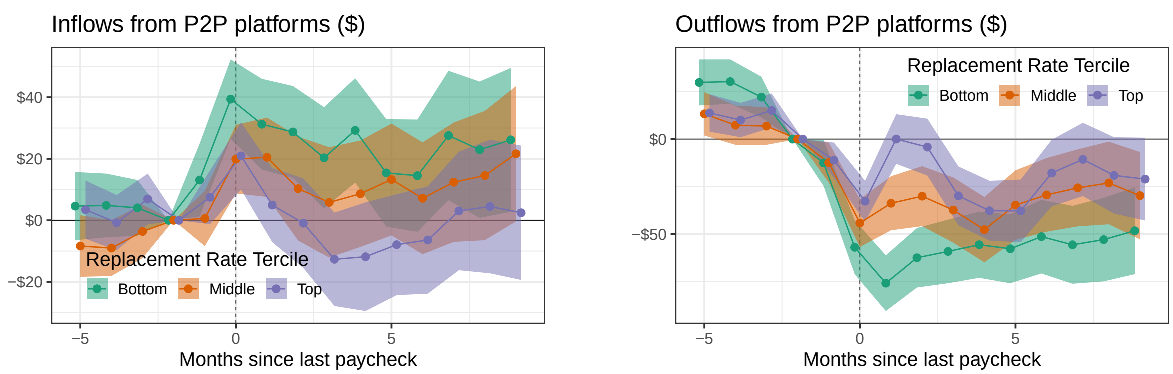
(a) P2P Inflows (b) P2P Outflows



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 flows in or out of the user's bank account 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

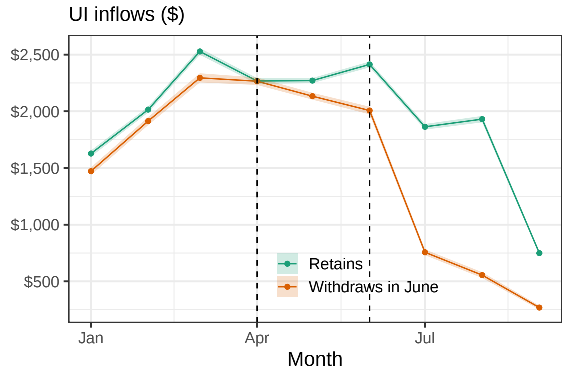


Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with tercile of user pre-job loss earnings replacement rate. changes are relative to the flows in or out of the user's bank account two months prior 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.

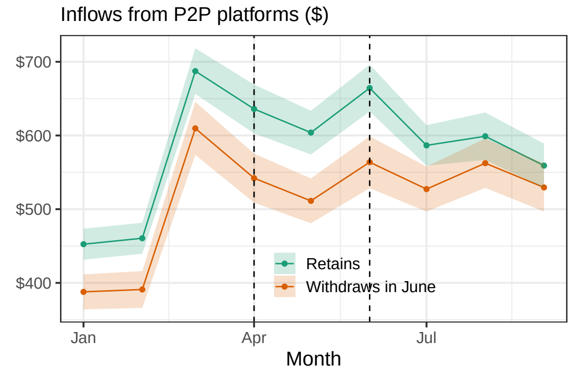
Figure 12. UI and P2P inflows across different pandemic-related changes to UI benefit payments

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

(a) UI Inflows

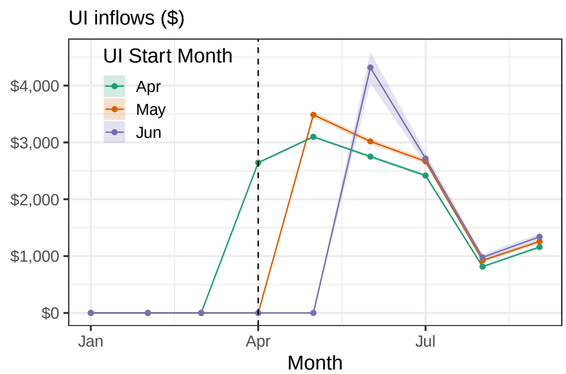


(b) P2P Inflows

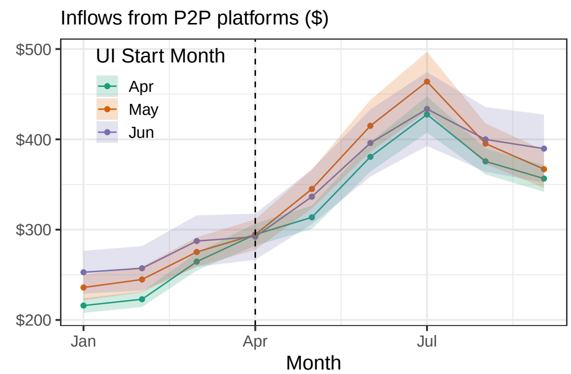


March 2020 job losers by cohorts of job receipt

(c) UI Inflows

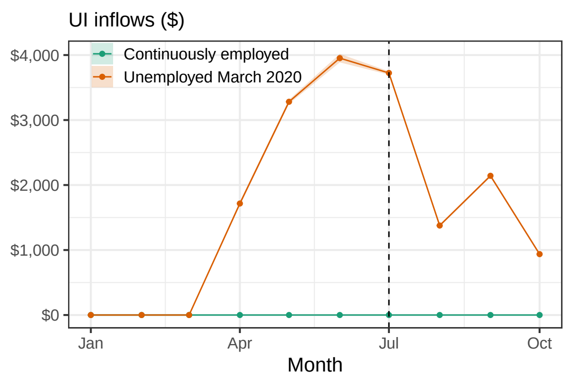


(d) P2P Inflows

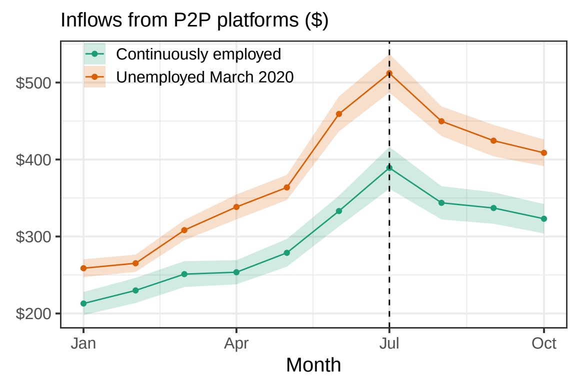


March 2020 job losers receiving UI vs. continuously employed through December 2020

(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.

Appendices – For Online Publication

A Appendix Tables

Table A1. 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.

Table A2. Counts of various cuts of employment

	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.

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

Method	OLS		IV	
Policy Change	June Withdrawal	March Delays	June Withdrawal	March Delays
	(1)	(2)	(3)	(4)
Received UI	0.004	-0.03***	-0.007	-0.03***
	(0.010)	(0.010)	(0.02)	(0.010)
Standard-Errors	State	User	State	User
Dependent variable mean	0.67001	0.59731	0.67001	0.59731
Mean of UI Inflows	1,851.4	1,328.9	1,851.4	1,328.9
Observations	28,546	34,508	28,546	34,508
R^2	0.76172	0.75556	0.76168	0.75556
F-test (1st stage), Received UI			3,488.4	
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 (4) 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. Columns (2) and (5) 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 (6) 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. P2P inflows as measured using regular expression flags of bank memos. Odd columns are OLS results, even columns are IV results. Standard errors in (1) and (4) are clustered by state and the rest by user.

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

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

Method	OLS			IV		
Policy Change	June Withdrawal	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration
	(1)	(2)	(3)	(4)	(5)	(6)
UI Inflows	-0.01**	0.004	-0.002	-0.05**	0.002	0.01*
	(0.005)	(0.004)	(0.003)	(0.02)	(0.007)	(0.007)
Cumulative Cases	0.002	-0.0003	0.0003	0.002	-0.0003	0.0001
	(0.002)	(0.0008)	(0.001)	(0.002)	(0.0008)	(0.001)
Cumulative Deaths	-0.46	0.02	0.005	-0.39	0.02*	0.01
	(0.30)	(0.01)	(0.08)	(0.30)	(0.01)	(0.08)
Standard-Errors	State	User		State	User	
Dependent variable mean	633.83	312.99	441.26	633.83	312.99	441.26
Mean of UI Inflows	1,851.4	1,328.9	1,734.7	1,851.4	1,328.9	1,734.7
Observations	28,530	34,498	31,736	28,530	34,498	31,736
R^2	0.72361	0.72855	0.75509	0.72317	0.72855	0.75473
F-test (1st stage), UI Inflows				3,539.1	27,851.3	7,719.1
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 (4) 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. Columns (2) and (5) 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 (6) 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. P2P inflows as measured using regular expression flags of bank memos. Odd columns are OLS results, even columns are IV results. Standard errors in (1) and (4) are clustered by state and the rest by user. County-level controls for COVID cases and deaths reported by the CDC included.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7. Reduced form of P2P Inflows by UI during various pandemic policy events

Policy Change	June Withdrawal (1)	March Delays (2)	July Expiration (3)
Post x Treat	63.3*	8.6	-31.2*
	(36.3)	(20.5)	(17.0)
Standard-Errors	State	User	
Observations	28,546	34,508	31,746
R^2	0.72326	0.72835	0.75519
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) 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. Column (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) 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. P2P inflows as measured using regular expression flags of bank memos. Standard errors in (1) is clustered by state, the other two by user.

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

Table A8. Crowd-out of logged P2P Inflows by UI during various pandemic policy events

Method	OLS			IV		
Policy Change	June Withdrawal	March Delays	July Expiration	June Withdrawal	March Delays	July Expiration
	(1)	(2)	(3)	(4)	(5)	(6)
Log UI Inflows	0.002	-0.02***	-0.06**	-0.02	-0.03***	-0.11***
	(0.007)	(0.006)	(0.03)	(0.01)	(0.007)	(0.04)
Standard-Errors	State	User		State	User	
Dependent variable mean	3.9774	3.2047	3.5737	3.9774	3.2047	3.5737
Mean of UI Inflows	1,851.4	1,328.9	1,734.7	1,851.4	1,328.9	1,734.7
Observations	28,546	34,508	31,746	28,546	34,508	31,746
R^2	0.80150	0.79127	0.81560	0.80125	0.79124	0.81557
F-test (1st stage), Log UI Inflows				4,680.8	96,543.0	45,168.0
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 (4) 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. Columns (2) and (5) 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 (6) 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. P2P inflows as measured using regular expression flags of bank memos. Odd columns are OLS results, even columns are IV results. Standard errors in (1) and (4) are clustered by state and the rest by user.

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

Table A9. Increase in tranfers during unemployment in the Panel Study on Income Dynamics

Margin	Intensive		Extensive	
	(1)	(2)	(3)	(4)
Unemployed	170.8***	161.9***	0.06***	0.06***
	(35.6)	(37.2)	(0.004)	(0.004)
Unemployed × Year=2020		130.7		-0.007
		(96.3)		(0.01)
Excess transfers in 2020		292.56***		0.05***
Observations	189,759	189,759	189,759	189,759
R^2	0.12833	0.12834	0.30121	0.30121
Individual fixed effects	✓	✓	✓	✓
Year fixed effects	✓	✓	✓	✓
State fixed effects	✓	✓	✓	✓
State by Year fixed effects	✓	✓	✓	✓

Results from regressions of total annual informal transfers on whether the user experienced at least one week of unemployment.The sample comes from the Panel Study of Income Dynamics waves 1977 through 2021, which cover years 1976 through 2020. Columns (1)-(2) are dollar amounts, while columns (3)-(4) are indicator variables. Dollar amounts are adjusted for inflation to 2020 USD. All data are survey responses. Transfers defined as those from family and friends. Sample includes heads of household and spouses from the same family units interviewed in the PSID. Sample further restricted to those being the ages of 18 and 59. Excess transfers in 2020 is the linear combination of the coefficient on unemployment and the interaction between unemployment and the year being 2020. Regressions control for age, age squared, whether a household member is disabled, whether the respondent became disabled, student status, if the respondent left school, got married, got divorced, homeownership, and whether the respondent became a homeowner. Models include fixed effects for individuals, year, state by year and household where appropriate. Standard errors clustered by family. Sample sizes vary due to non-reporting.

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

Table A10. Crowd-out of P2P Inflows due to delays in state UI payments

	Reduced Form (1)	IV DID (2)
Post x Above Median Payment Delays	-9.2	
	(18.9)	
UI Inflows		0.06
		(0.10)
Dependent variable mean	313.05	313.05
Mean of UI Inflows	1,328.9	1,328.9
Observations	34,508	34,508
R^2	0.72835	0.72535
F-test (1st stage), UI Inflows		83.396
User and Month fixed effects	✓	✓

Reduced form and instrumental variable estimates of crowd-out of P2P Inflows by unemployment insurance (UI) using the March 2020 cohort. The instrument is an interaction of a treatment indicator and month indicator. The treatment indicates whether the state had an above median share of its June UI benefit payments paid with more than a 63-day delay. Delays measured using the Department of Labor's ETA 9050 time to first benefit payment to measure delay in state UI benefits.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

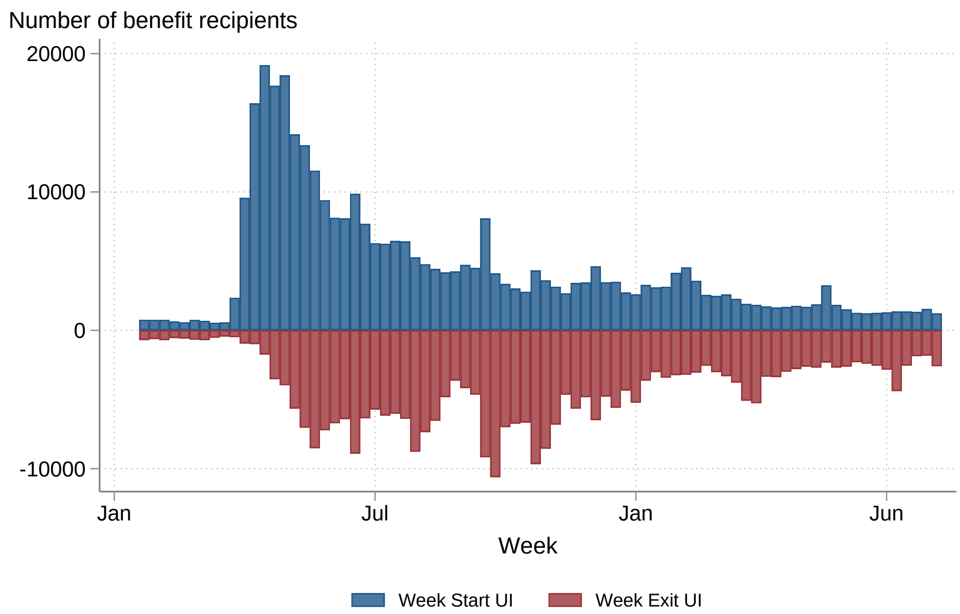
Table A11. Triple difference estimates of P2P Inflows

	OLS (1)	Reduced Form (2)	IV DDD (3)
UI Inflows	0.003		0.02
	(0.009)		(0.01)
Treat × Post × Year=2021		46.1	
		(34.0)	
Dependent variable mean	482.52	482.52	482.52
Mean of UI Inflows	1,480.0	1,480.0	1,480.0
Observations	41,313	41,313	41,313
R^2	0.92238	0.92239	0.92233
F-test (1st stage), UI Inflows			15,139.3
User, Month, User-Month, Month and User-Year fixed effects	✓	✓	✓

Triple difference estimates of crowd-out of P2P Inflows by unemployment insurance (UI). The treatment group includes those who are unemployed in March 2020 and insured by June 2020. The control group are those continuously unemployed that experience unemployment in 2021. For these groups, I perform a difference-in-difference of P2P inflows in June and August in both 2020 and 2021, then take the difference in these difference-in-difference estimates. I the OLS, reduced form and instrument variable result using the triple difference as the instrument. I include fixed effects for user, month, and year as well as the two-way interaction of each of these.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

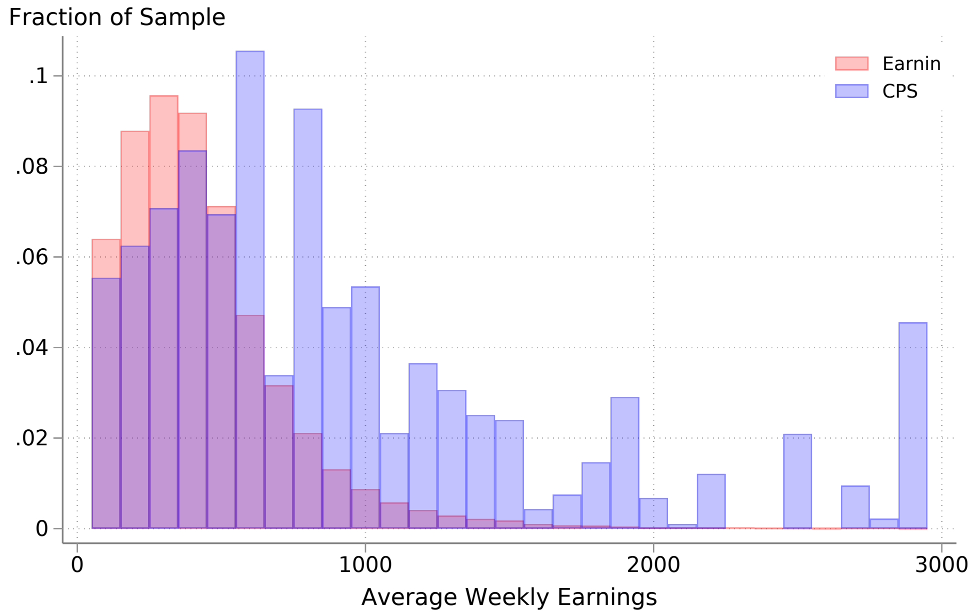
A Appendix Figures

Figure A.1. Employment Rate Trend



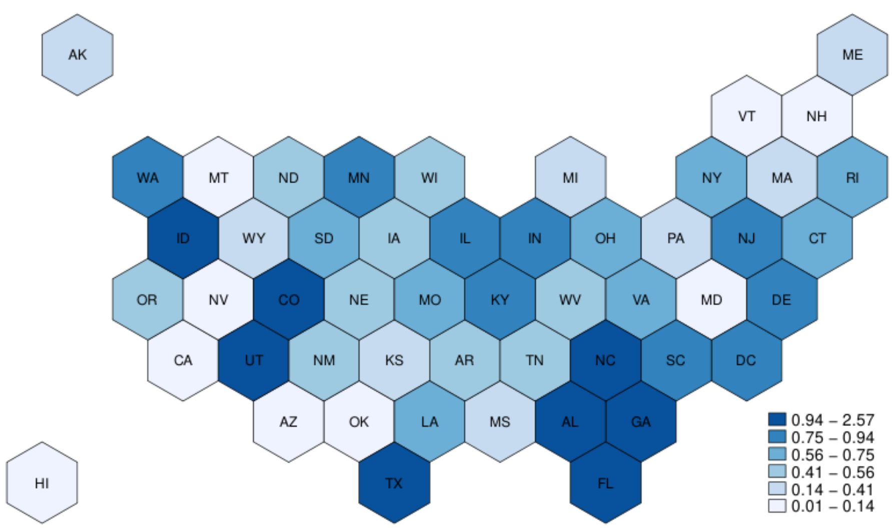
Notes: The above figure plots the number of UI spell starts and ends by week for our Earnin sample through 2020 to 2021. These estimates are based on the Earnin users from our analysis states from the week ending January 24, 2020 through August 6, 2021.

Figure A.2. 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.

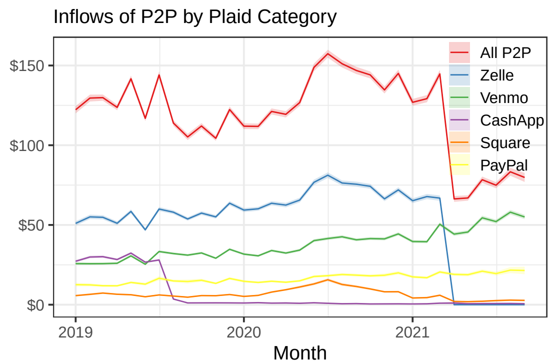
Figure A.3. Fraction of UI Recipients by State



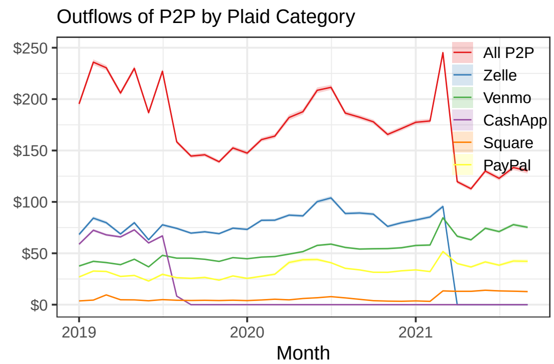
Notes: Hexmap of the fraction of Earnin recipients receiving UI in July 2020.

Figure A.4. Timeline of P2P inflows and outflows only Plaid categories

(a) Inflows



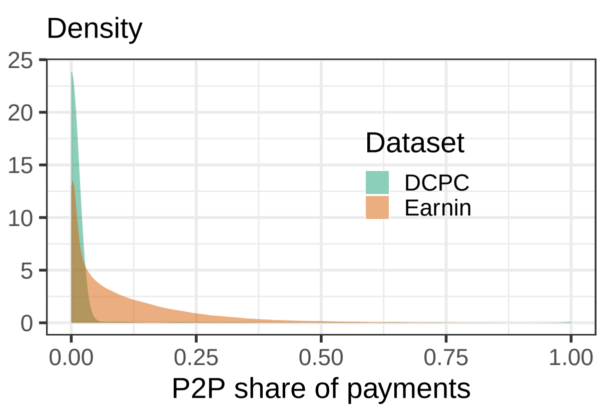
(b) Outflows



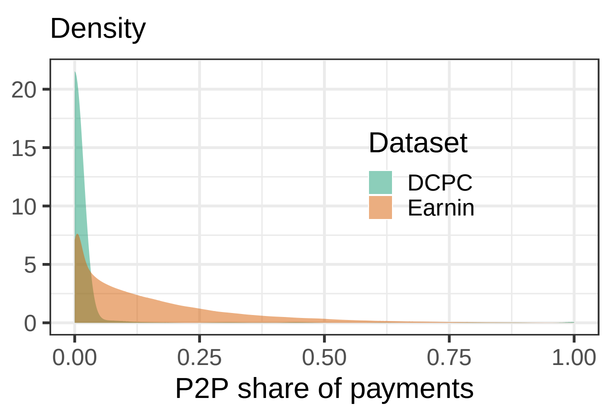
Notes: P2P inflows and outflow of transactions between $5 and $15,000, not linked to purchases, gig platforms, or stimulus payments over time. The series are constructed from all transactions with Plaid categories matching P2P platforms. The sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.5. Density of P2P share of financial transactions in Diary of Consumer Payment Choice vs. Earnin users

(a) 2019



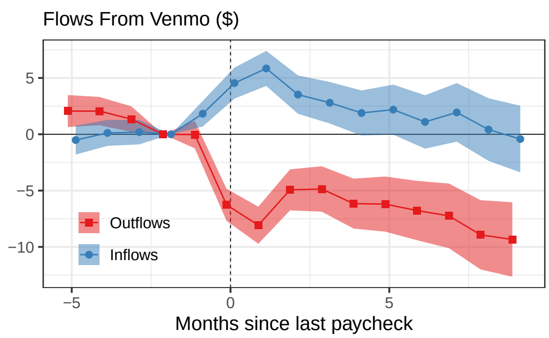
(b) 2020



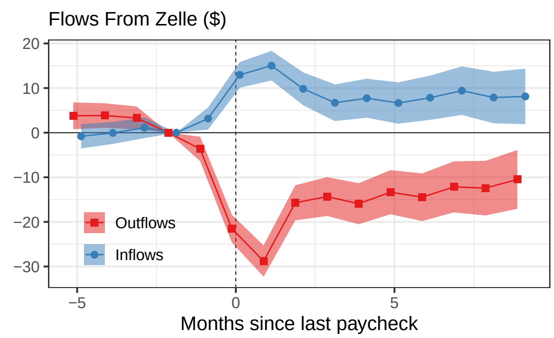
Notes: These show a comparison of the share of total non-cash flows that were P2P transactions in the months of October 2019 and 2020 in the Earnin dataset and the Diary of Consumer Payment Choice (DCPC). These shares are after cash transactions are removed from both datasets as these are not accounted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

Figure A.6. P2P inflows and outflows by platform

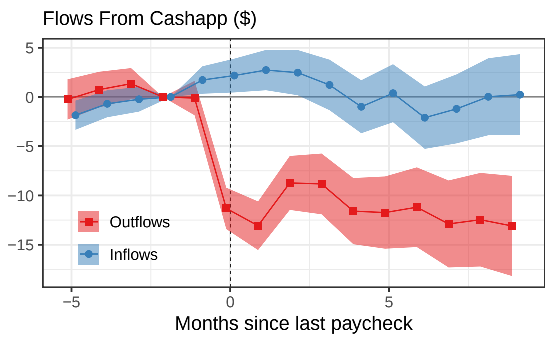
(a) Venmo



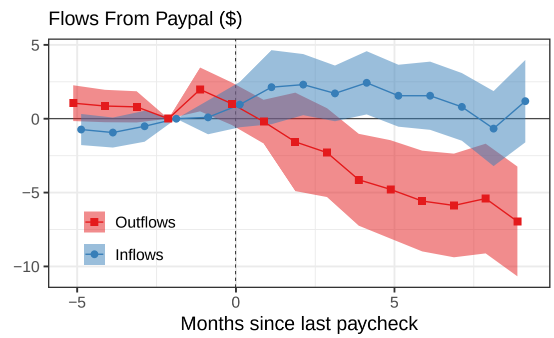
(b) Zelle



(c) CashApp



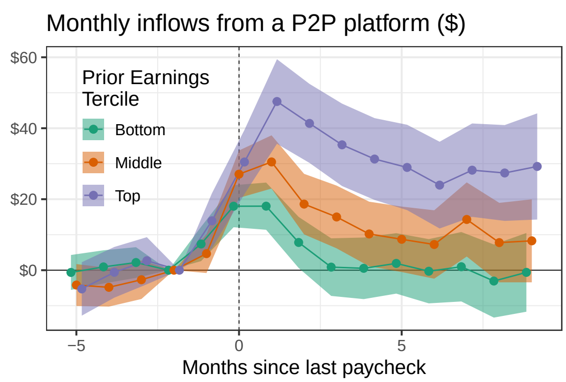
(d) PayPal



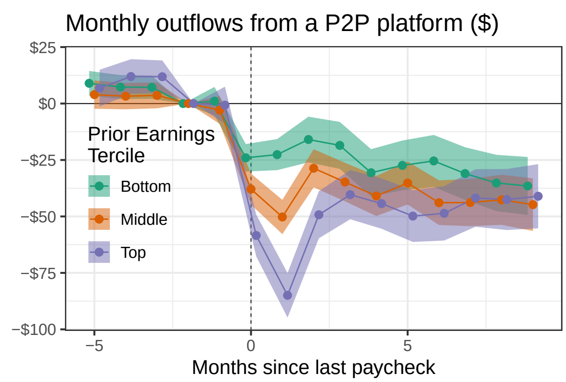
Notes: Within-person event studies of major P2P platform inflows and outflows around a user's first unemployment spell by platform. changes are relative to the flows in or out of the user's bank account two months prior to job loss. Standard errors clustered at user-level. Sample includes users with 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. Event studies of P2P inflows by tercile of bank balance at job loss

(a) Inflows



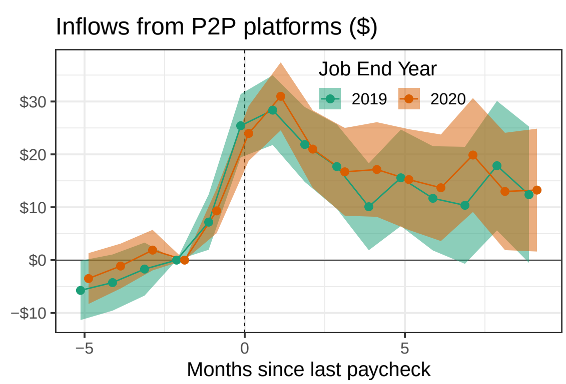
(b) Outflows



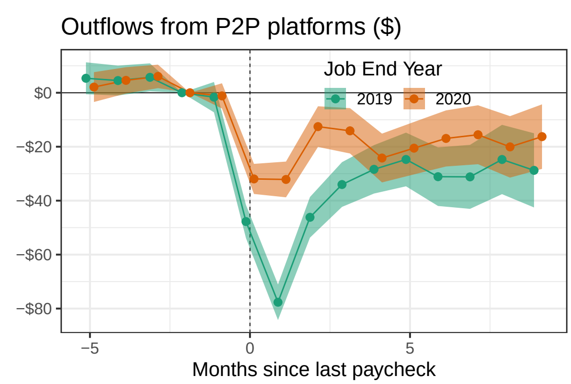
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with tercile of earnings prior to job loss. changes are relative to the flows in or out of the user's bank account two months prior to job loss. Standard 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. Event studies of P2P inflows and outflows by year of job loss

(a) Inflows



(b) Outfows

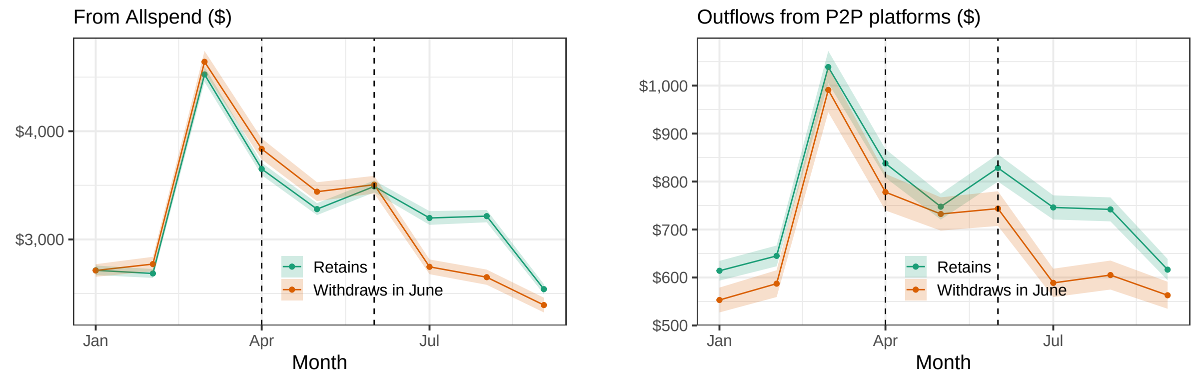


Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of year of user's first job loss. changes are relative to the flows in or out of the user's bank account two months prior to job 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.9. Spending and P2P Outflows across different pandemic-related changes to UI benefit payments

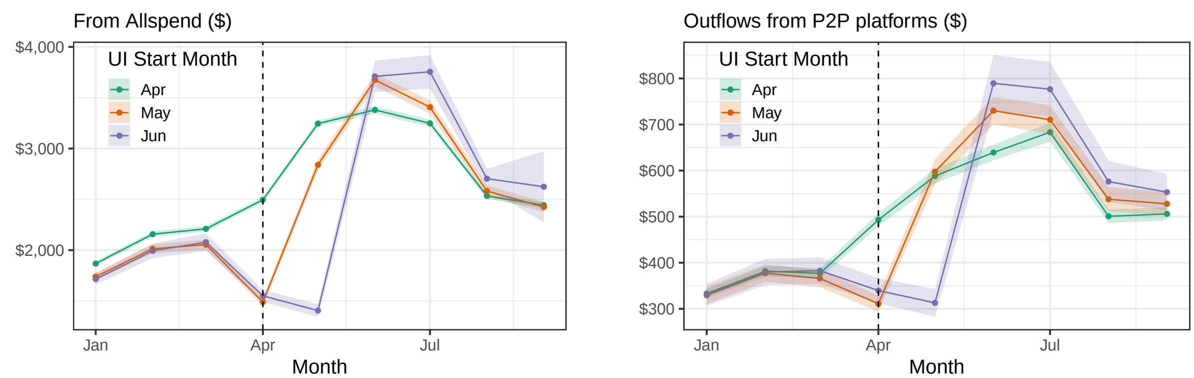
Unemployed and insured as of April 30 in Withdrawal vs. Retain States

(a) Spending (b) P2P Outflows



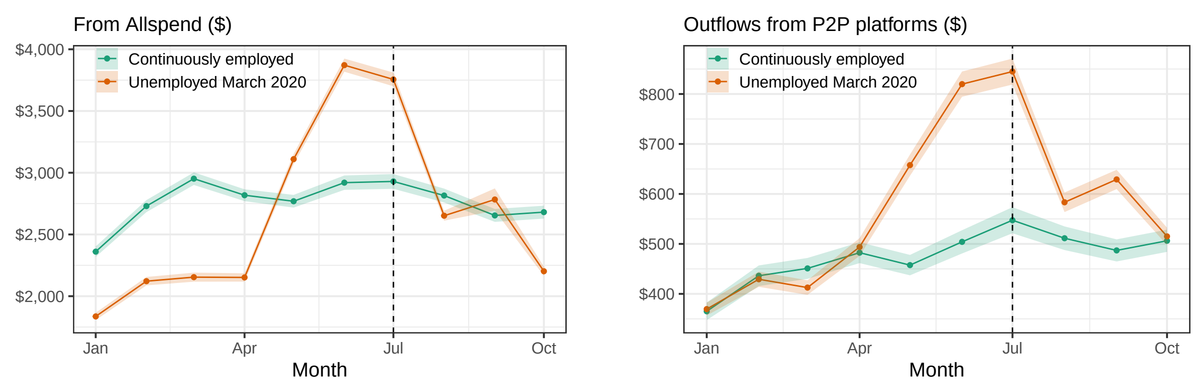
March 2020 job losers by cohorts of job receipt

(c) Spending (d) P2P Outflows



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

(e) Spending (f) P2P Outflows

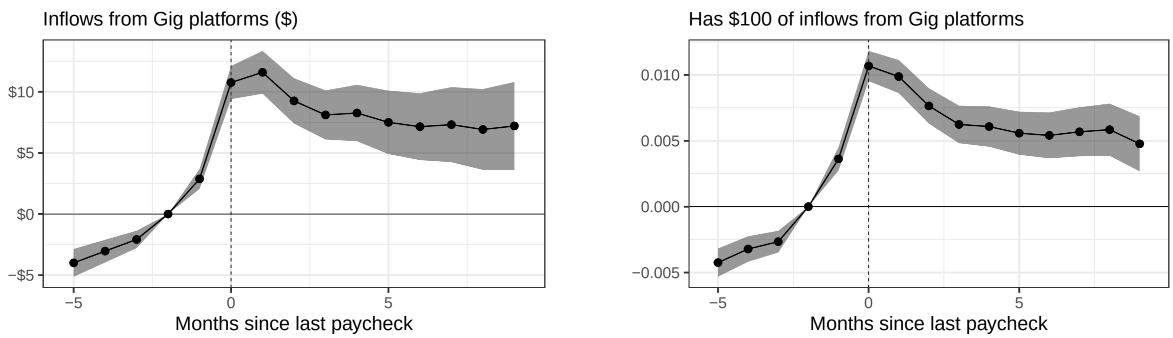


Notes: UI and P2P Outflows for different cohorts around three pandemic-related changes to UI benefits. Figures (a) and (b) show plots for cohort that lost jobs in March 2020 broken out by month they received UI benefits. Figures (c) and (d) compare the same cohort of March job losers workers that remained 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.10. Intensive and Extensive Margin of Gig Work

(a) Gig Earnings

(b) ≥ 100 Gig Earnings

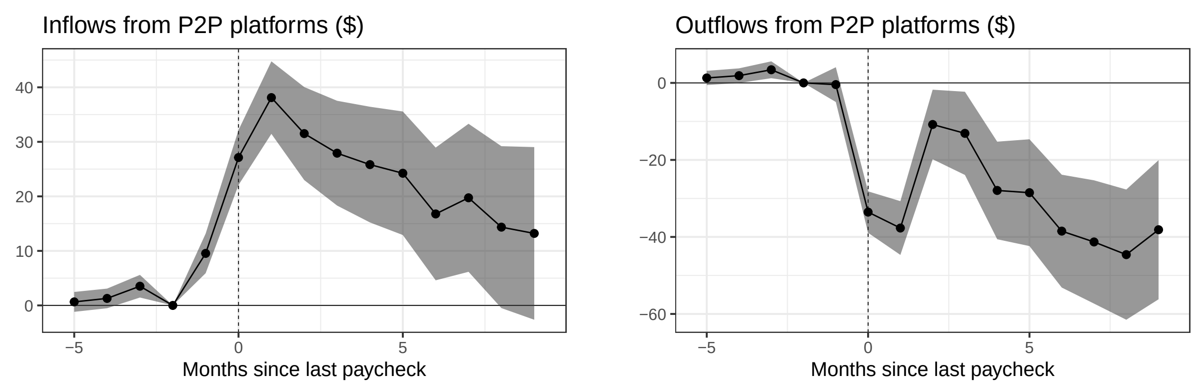


Notes: Within-person event studies of gig earnings and probability that user has at least $100 of gig earnings around a user's first unemployment spell. changes are relative to the flows in or out of the user's bank account two months prior to job loss. Standard errors clustered at user-level. Sample 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. Gardner Imputation Event Study Estimates of P2P Inflows and Outflows Around Unemployment

(a) Inflows

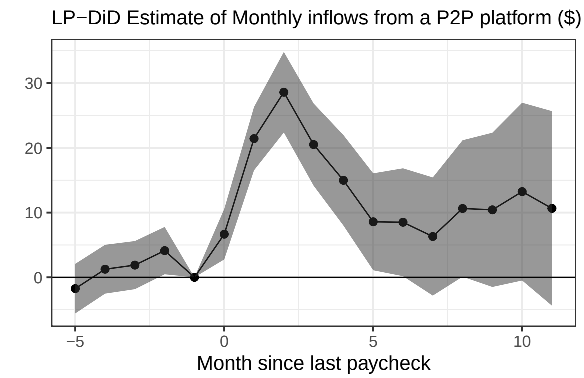
(b) Outflows



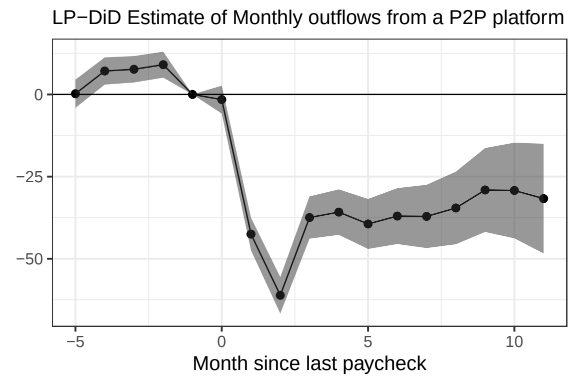
Notes: Within-person Gardner (2022) two-stage event study of P2P inflows and outflows from a P2P platform around the user's first unemployment spell. This approach corrects for "bad" comparisons by excluding treated units from calculations of user and monthly fixed effects. The sample is restricted to 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.12. Local Projections Approach to Difference-in-difference Event Study Estimates of P2P Inflows and Outflows Around Unemployment

(a) Inflows



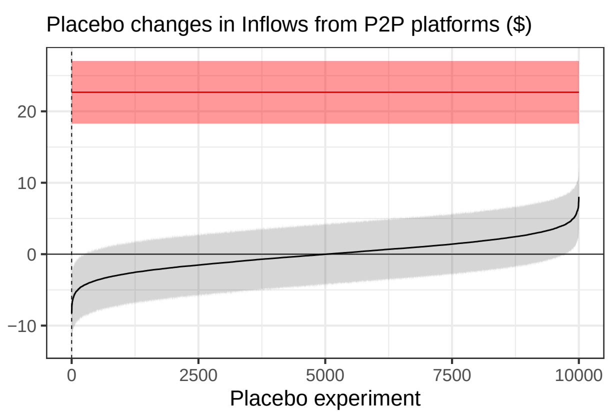
(b) Outflows



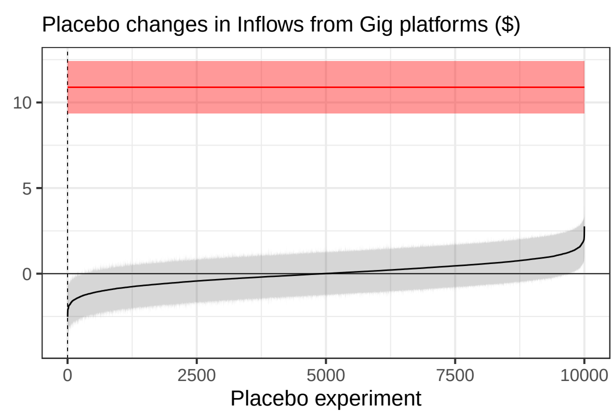
Notes: These figures show a Local Projections approach to difference-in-difference event study (Dube et al., 2023) of P2P inflows and outflows from a P2P platform around the user's first unemployment spell. This approach implements a "clean control" condition, which restricts the control group for each relative time period to exclude treated units. Each relative time period coefficient is estimated in a separate first-differences regression. 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.13. Placebo Tests of average increase in P2P inflows and outflows and gig earnings during unemployment

(a) P2P Inflows



(b) Gig Inflows

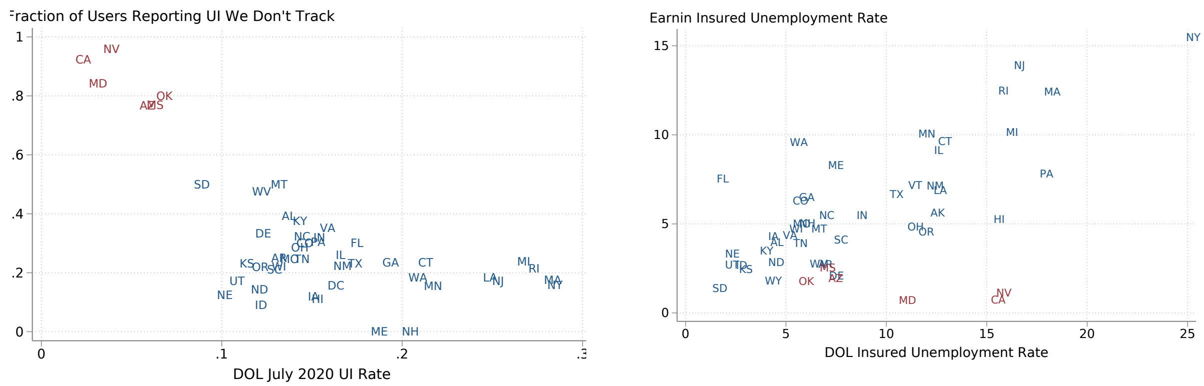


Notes: Placebo estimates of the average increase in P2P inflows, outflows, and gig earnings during unemployment. Placebo estimates are derived from randomly assigning job loss months using weights pulled from the observed distribution of job loss months in the analysis sample. Data shows 10,000 placebo 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.

Figure A.14. False negative UI from survey by population insured rate by state

(a) False Negative UI from survey

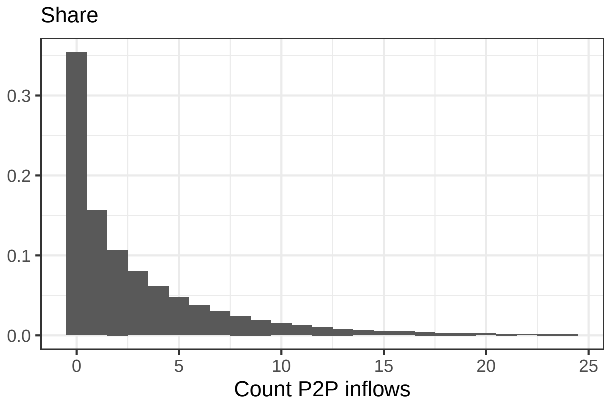
(b) Insured Unemployment Rate Comparison



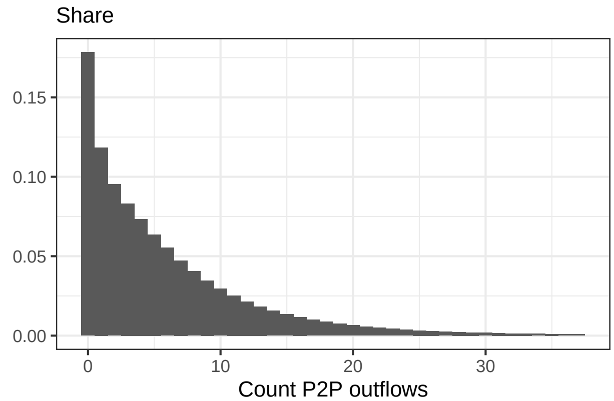
Notes: Figure (a) shows the July 2020 DOL UI rate vs. fraction of false negatives based on survey – drop states in red. Figure (b) compares the insured unemployment rate from Earnin with the same from the Department of Labor for April 30, 2021, defined as the fraction of the labor force unemployed and 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 A.15. Histograms of monthly inflow and outflow dollars and counts

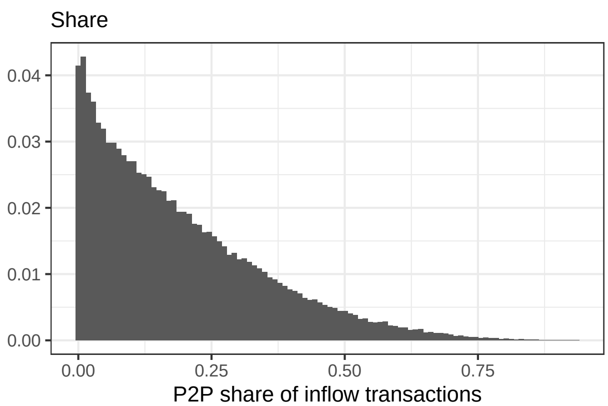
(a) Monthly P2P inflow transaction count



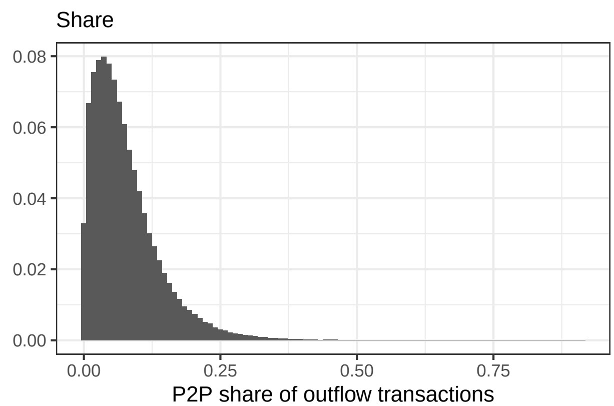
(b) Monthly P2P outflows transaction count



(c) P2P share of total inflows transaction count



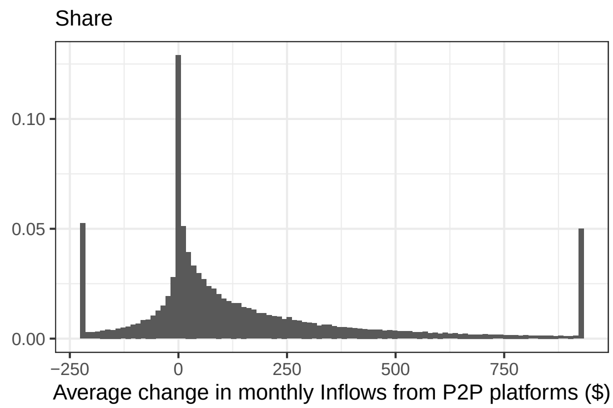
(d) P2P share of total outflows transaction count



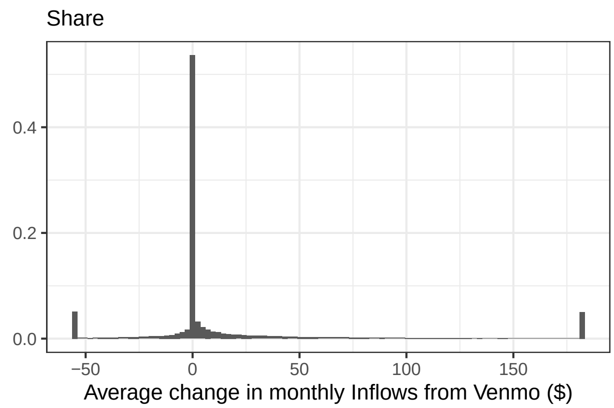
Notes: Figures (a) and (b) show the total P2P inflows and outflows, respectively, for each user-month in the data. Figures (c) and (d) show the share of total inflows and outflows, respectively, exchanged on P2P platforms.

Figure A.16. Distributions of average change in monthly P2P inflows after job loss

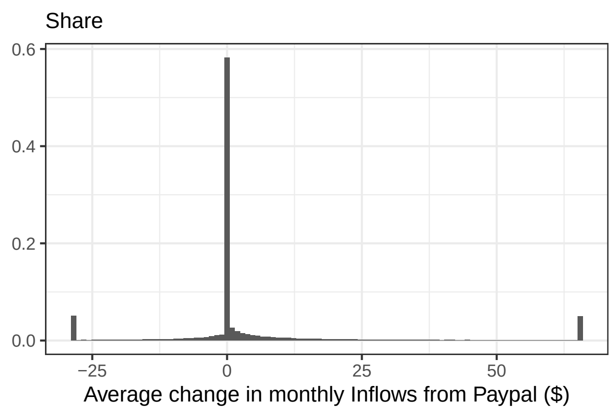
(a) All P2P inflows



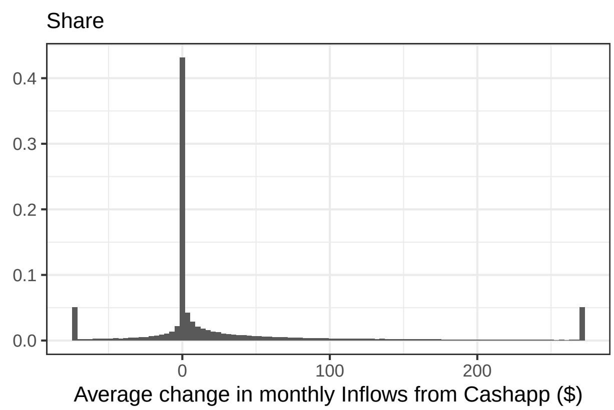
(b) Venmo



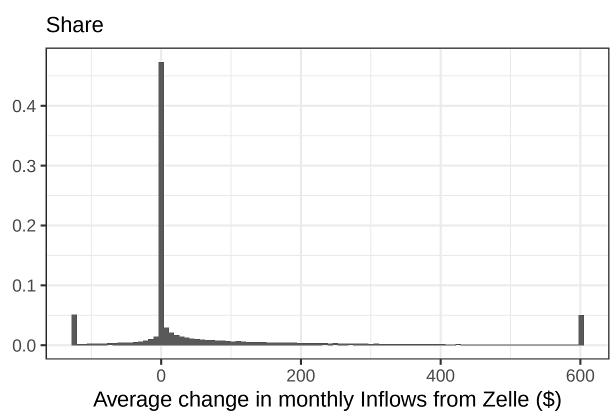
(c) Paypal



(d) Cashapp



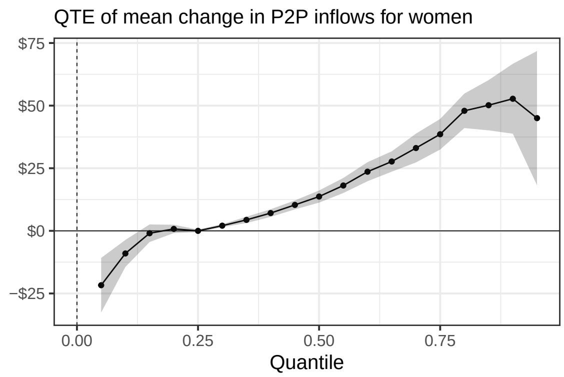
(e) Zelle



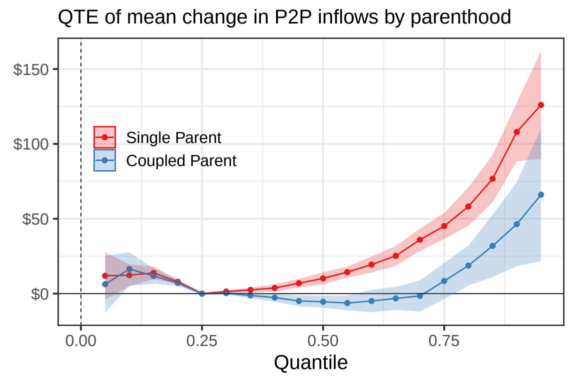
Notes: Each observation is the average change in a user's monthly inflows on different P2P platforms after they lose their job. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September 2021.

Figure A.17. Quantile treatment effects of different group indicators on P2P inflows

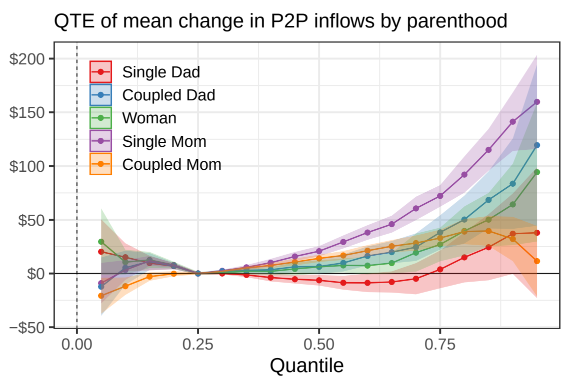
(a) Gender



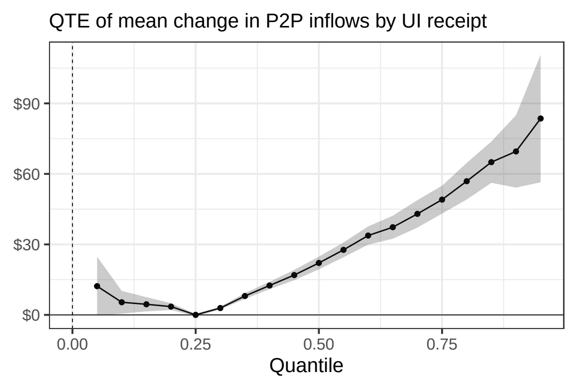
(b) Parents



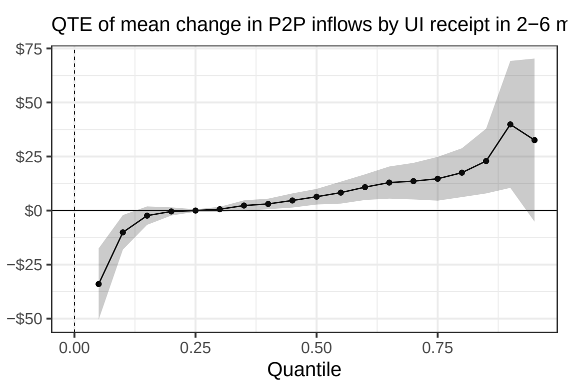
(c) Parent × Gender



(d) UI Receipt



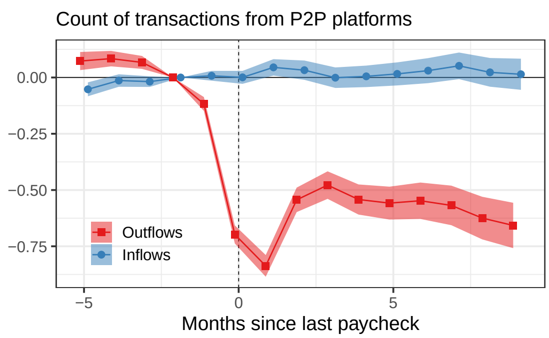
(e) Time to UI



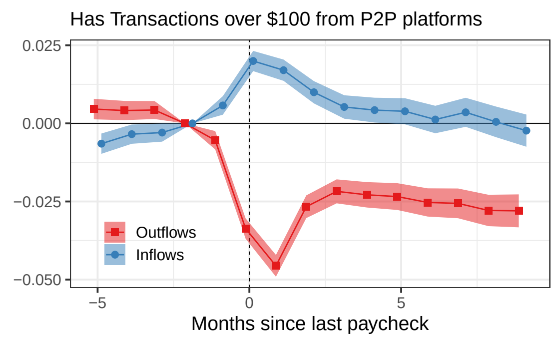
Notes: Quantile treatment effects of different group indicators on the average change in P2P inflows before and after job loss. Standard errors bootstrapped at user level. Sample includes users with at least five outflows per month and a job loss in a month between July 2019 and September 2020 or September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

Figure A.18. Extensive margin of individual P2P transactions

(a) Count of P2P transactions



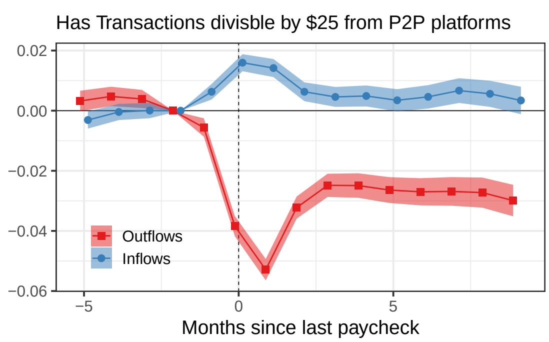
(b) $\mathbb{P}$(P2P transaction > 100)



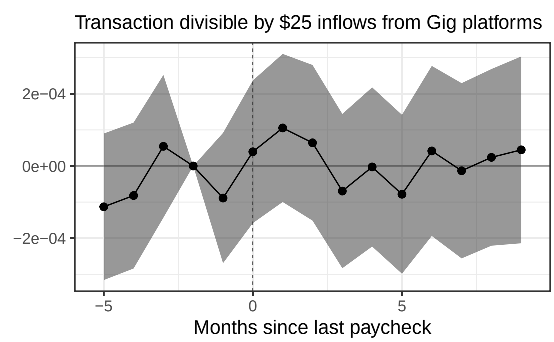
Notes: Within-person event studies of the count of inflow and outflow transactions and probability of having transactions greater than $100 in a given month. changes are relative to the flows in or out of the user's bank account two months prior to job loss. Standard errors clustered at user-level. Sample 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. Extensive margin of individual P2P transactions

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

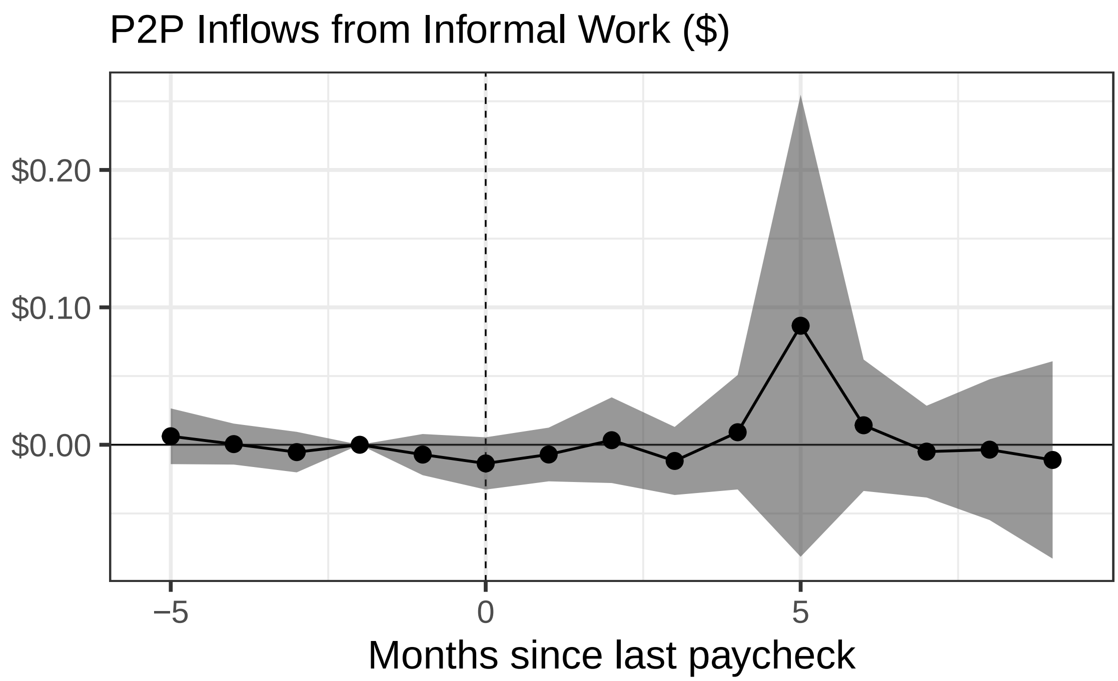


(b) $\mathbb{P}$(Gig earning transaction divible by 25)



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 flows in or out of the user's bank account 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.

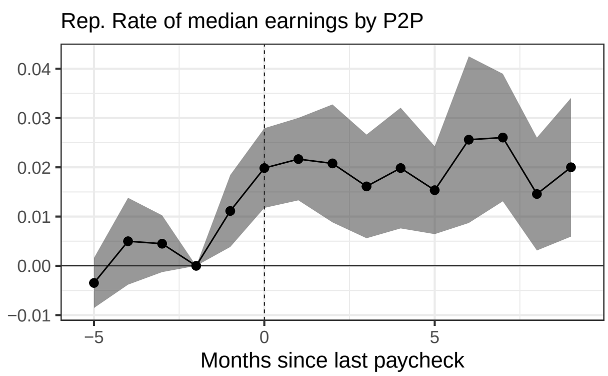
Figure A.20. Event study of P2P inflows from informal work earnings around job loss



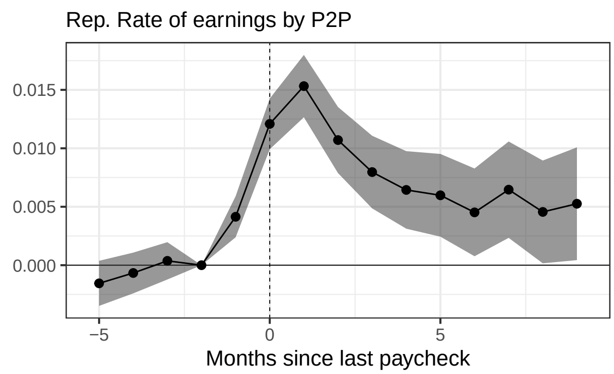
Notes: Within-person event studies of inflows from transactions that are likely informal earnings around a user's first unemployment spell. Informal earnings transactions mention "hours worked," "babysitting," or "yard work." changes are relative to the flows in or out of the user's bank account two 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.21. Event studies of alternative measures of replacement rate of P2P inflows after unemployment

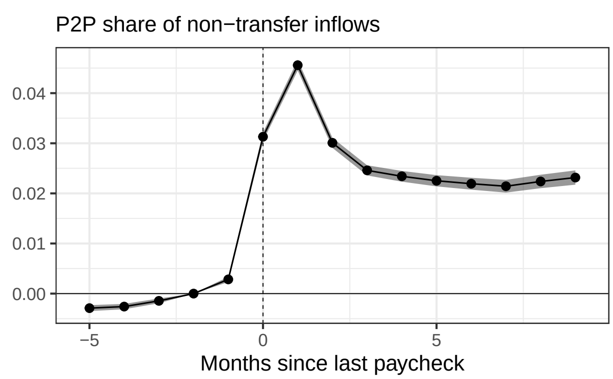
(a) Replacement of within-user median earnings



(b) Replacement of average of within-user median earnings



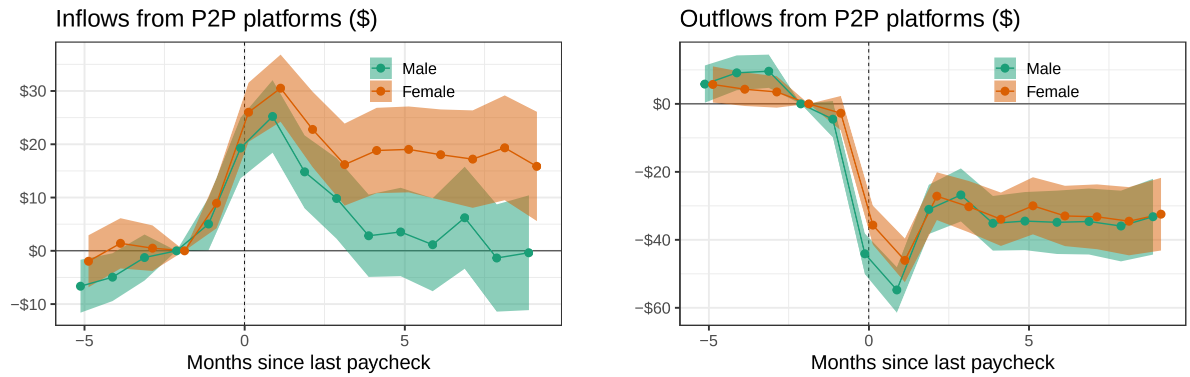
(c) P2P share of total inflows



Notes: Within-person event studies of different measures of the replacement rate of P2P inflows after job loss. The outcome in figure A.21(a) is the P2P inflows divided by median monthly pre-job loss earnings. The outcome in figure A.21(b) is the P2P inflows divided by the average of the median monthly pre-job loss earnings. The outcome in figure A.21(c) is the share of P2P inflows in total inflows. The x-axis is the number of months relative to the month of job loss. The y-axis shows the coefficient estimate from an event study regression around first job loss. Changes are relative to the flows in or out of the user's bank account 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.22. Event studies of P2P inflows by gender

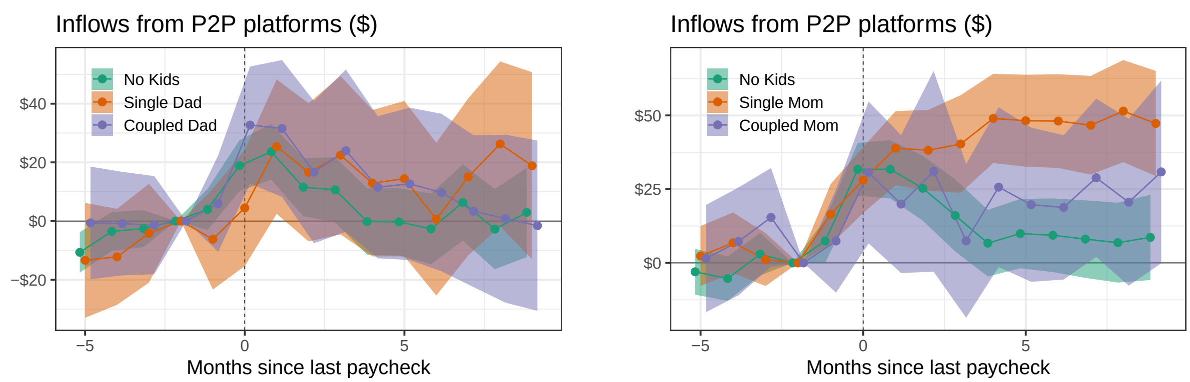
(a) Inflows (b) Outflows



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 flows in or out of the user's bank account 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.23. Event study coefficients based on family composition and gender

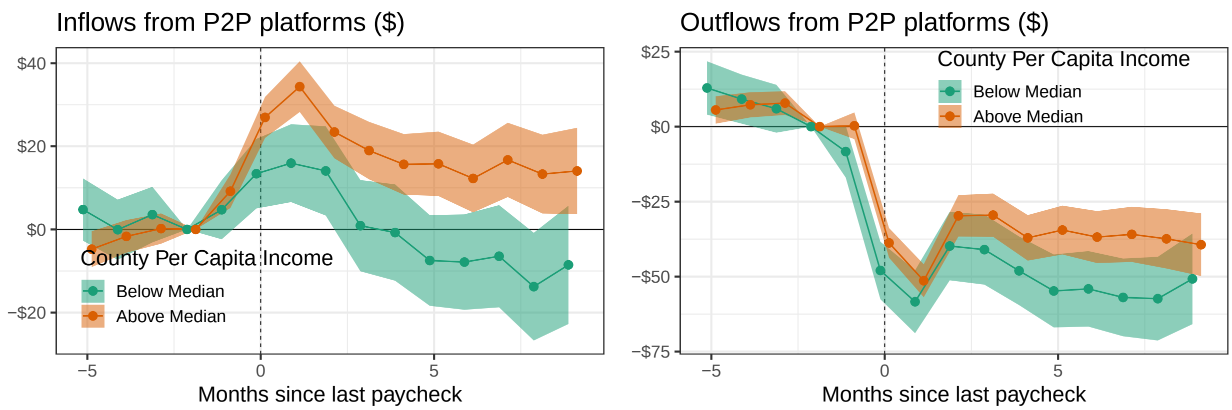
(a) Men (b) Women



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 flows in or out of the user's bank account 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.

Figure A.24. Event study coefficients by whether user lives in county above or below median per capita income

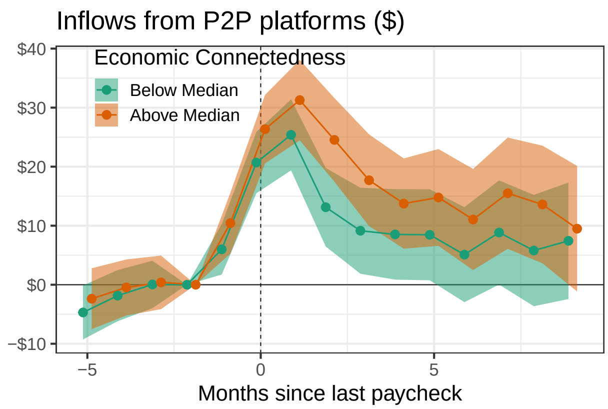
(a) Inflows (b) Outflows



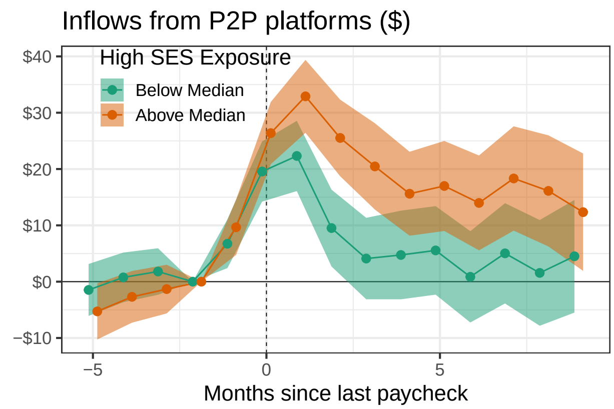
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator for living in an above or below median per capita household income county as measured by the American Community Survey 2019 5-year. 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.25. Event study coefficients by indicators of being in above median measures of economic connectedness in Chetty et al. (2022)

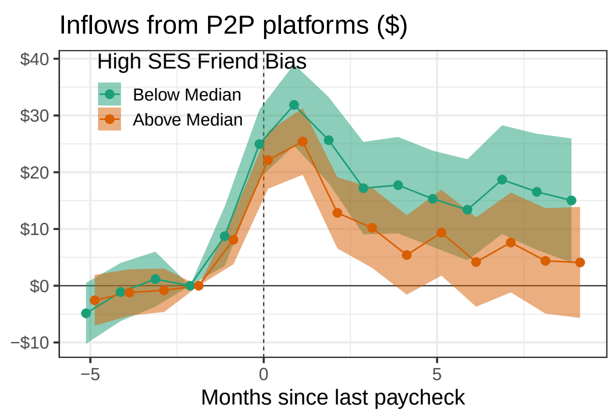
(a) Economic Connectedness



(b) Exposure



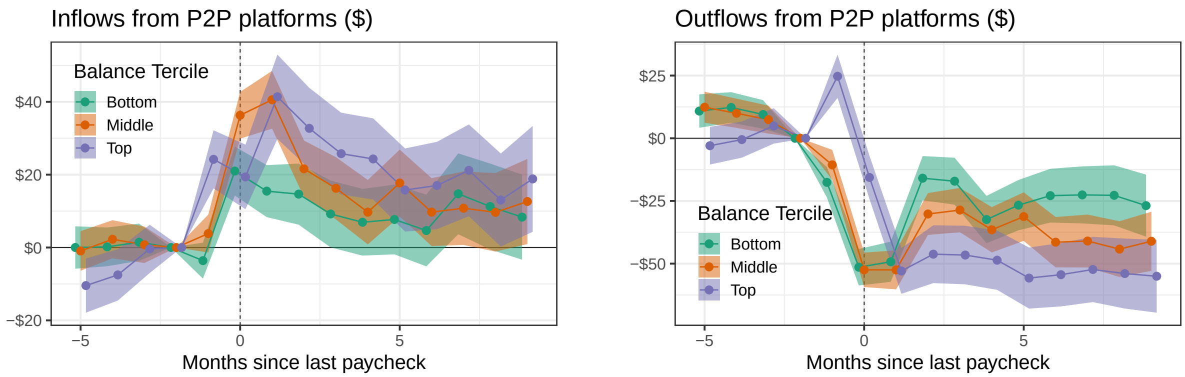
(c) Friending Bias



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 flows in or out of the user's bank account 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.26. Event studies of P2P inflows by tercile of bank balance at job loss

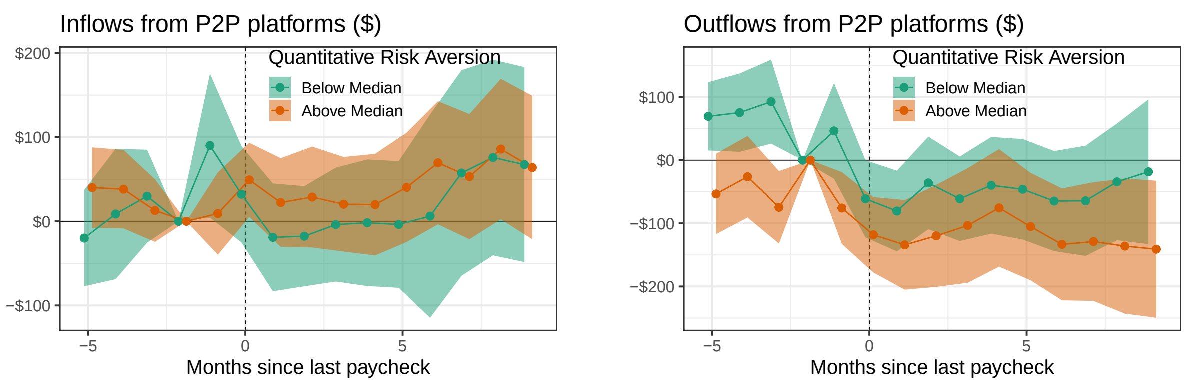
(a) Inflows (b) Outflows



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 flows in or out of the user's bank account 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.27. Event studies of P2P inflows by whether user has above or below median risk aversion

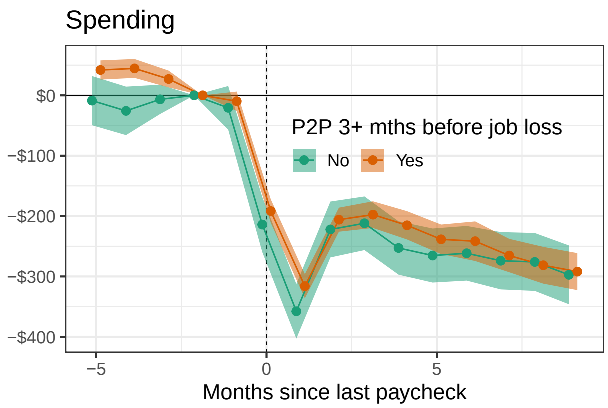
(a) Inflows (b) Outflows



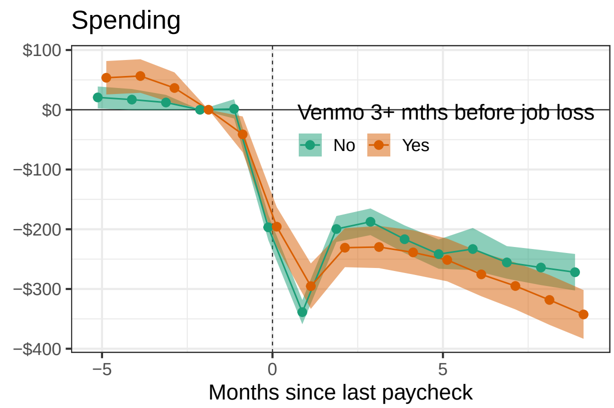
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with whether user has above or below median risk aversion based on telescoping question in survey of Earnin users conducted in August 2020. changes are relative to the flows in or out of the user's bank account 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.28. Event study of consumption by use of P2P prior to job loss

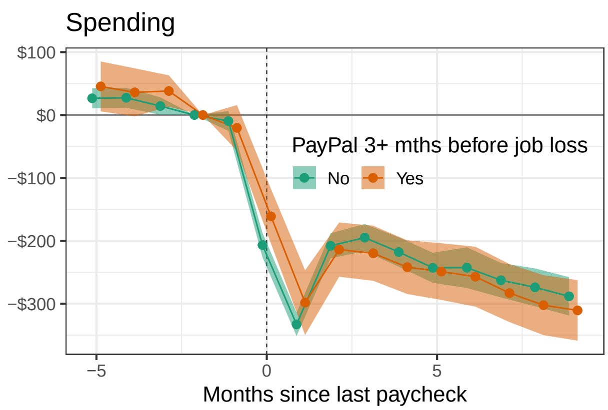
(a) Any P2P Platforms



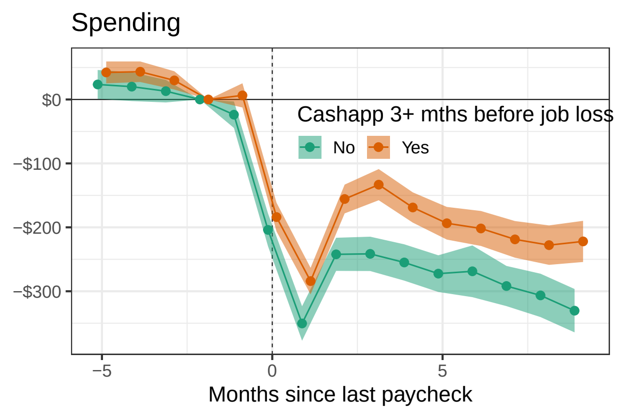
(b) Venmo



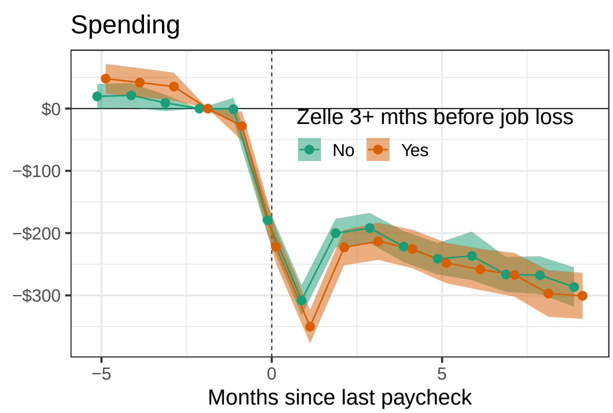
(c) Paypal



(d) Cashapp



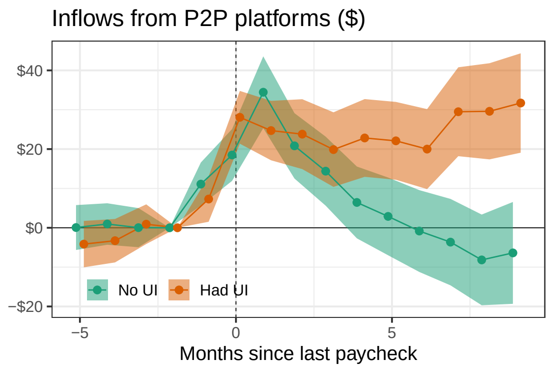
(e) Zelle



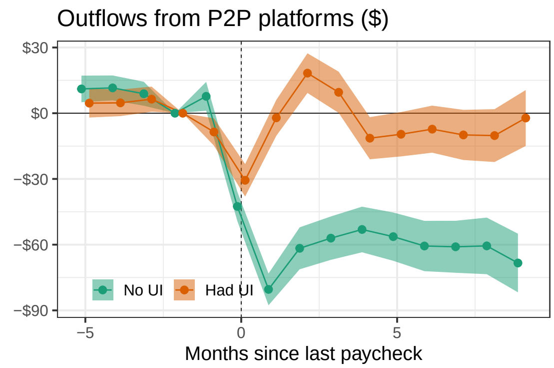
Notes: Within-person event studies of spending a user's first unemployment spell. changes are relative to the flows in or out of the user's bank account two months prior to job loss. Coefficients are relative time dummies are interacted with indicators for whether the user had inflows from the relevant 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.29. P2P Inflows & Outflows by UI receipt during unemployment

(a) P2P Inflows



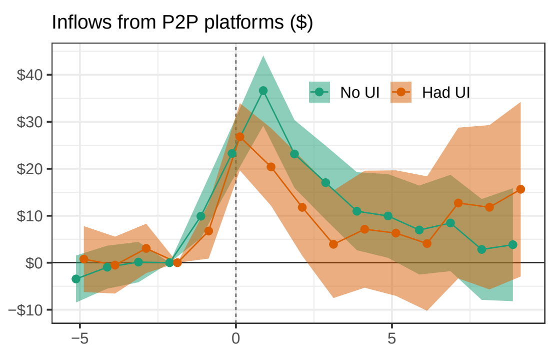
(b) P2P Outflows



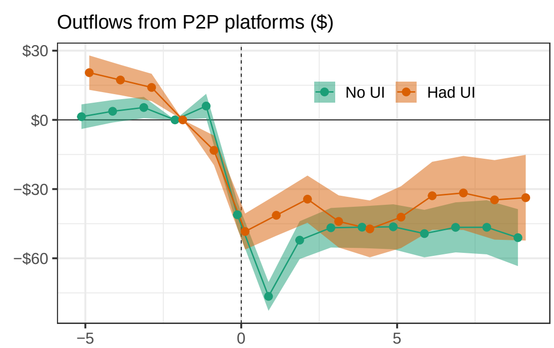
Notes: Within-person event studies of P2P inflows and outflows around a user's first unemployment spell. Coefficients are relative time dummies interacted with an indicator of for whether user received UI within six months of first job loss. changes are relative to the flows in or out of the user's bank account 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.30. P2P Inflows & Outflows subset by UI receipt during unemployment

(a) P2P Inflows



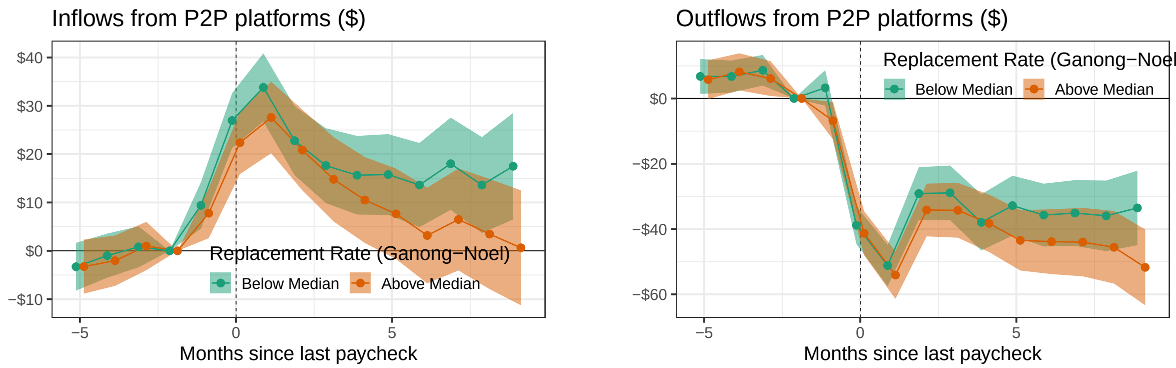
(b) P2P Outflows



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 flows in or out of the user's bank account 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.31. P2P Inflows by Above Median Replacement Rate

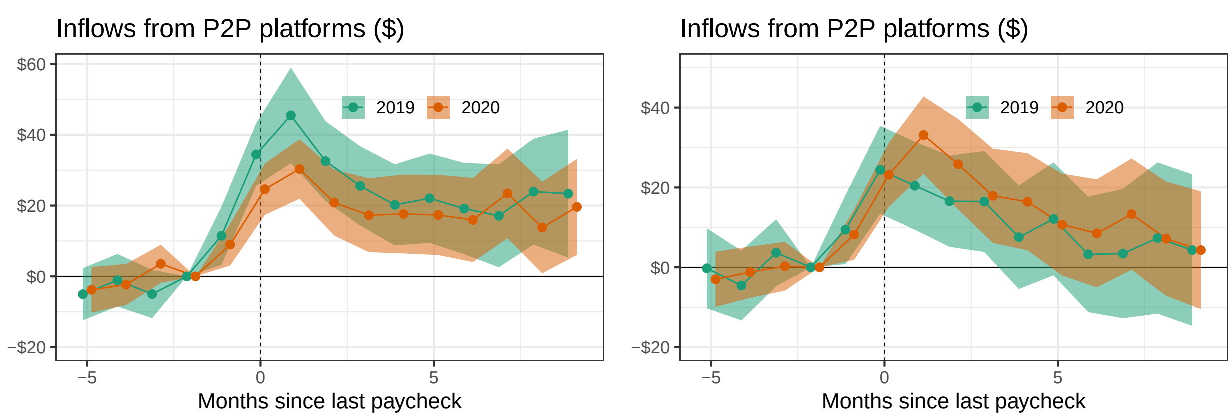
(a) Inflows (b) Outflows



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.

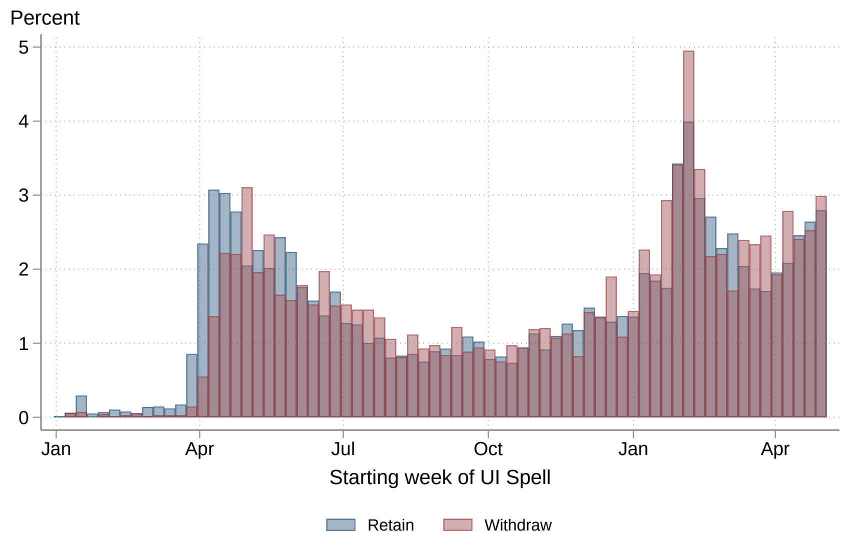
Figure A.32. P2P Inflows by State Replacement Rate and Year of Job Loss

(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.33. Histogram of Starting week of UI spells in April by State Withdrawal Status

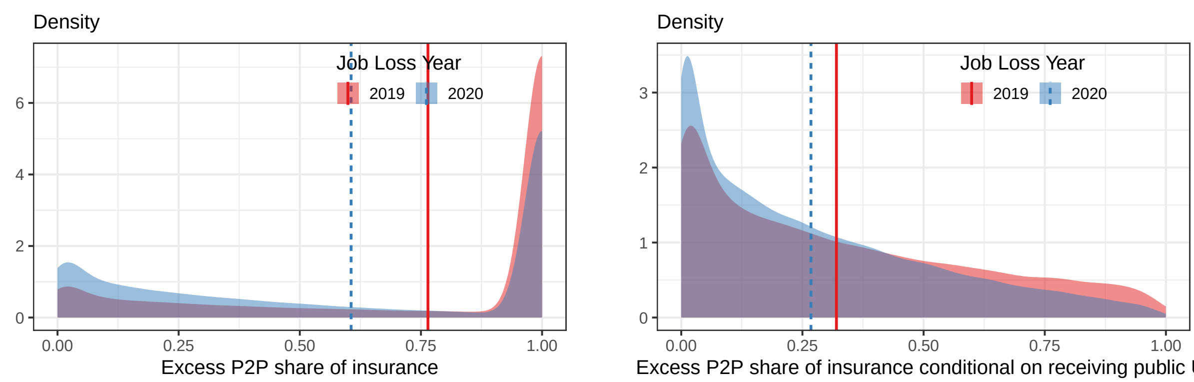


Notes: The above figure plots the histograms of the starting week for each users' unemployment insurance recipiency spell that runs through the end of April by retain and withdraw states. The sample is restricted to those 18,648 Earnin users whom we track as receiving UI benefits and no earnings in the 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.

Figure A.34. Density of post-P2P inflows over UI

(a) Unconditional on UI receipt

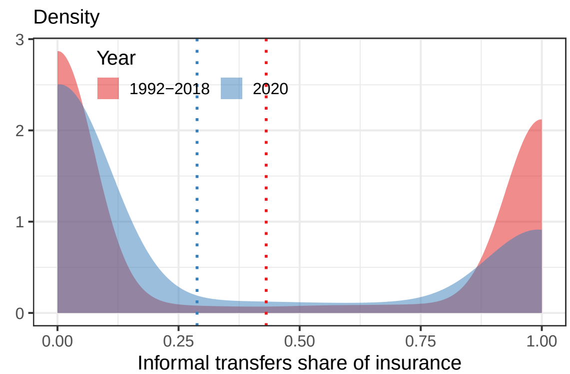
(b) Conditional on UI receipt



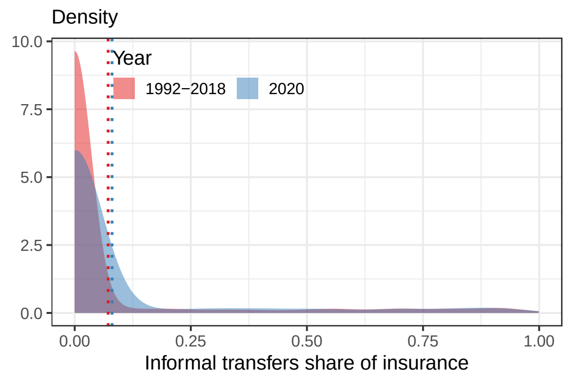
Notes: Share of P2P and UI inflows made up by P2P during unemployment conditional on receiving UI. The numerator is the total P2P inflows after job loss and the denominator is total UI inflows and P2P inflows after job loss.

Figure A.35. Density of informal support 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 Panel Study of Income Dynamics. Each data point is a person in a year they experience unemployment from 1976 to 2020. The calculation divides the transfers from friends and family by the total of unemployment insurance payments and transfers from friends and family. The vertical line show the average in each year.

B.1 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.15 has histograms of the counts of P2P transactions per month and as a share of a user's total number of transactions. Subfigures A.15 and A.15 show only 35% and 20% of months have zero P2P inflows and outflow transactions, respectively. Furthermore, subfigures A.15 and A.15 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.15 leading to a fatter right-tail.

B.2 Income replacement by P2P

In figure 4, I present evidence that P2P inflows replace a small share of the overall average earnings lost after job loss. The series in this event study scales the main series by an average and then the event study reports the average across all users – effectively providing a ratio of averages. In this section, I explain and report other attempts to measure the replacement rate of P2P inflows – each of which tells a similar story: P2P inflows replace a small share of earnings after job loss.

Figure A.21 shows three separate alternative specifications of the replacement rate. The outcome in figure A.21(a) includes the P2P inflows divided by the within-user median monthly earnings prior to job loss. To construct the denominator, I calculate the median monthly earnings prior to job loss for each user. The results show that the share of this earnings measure replaced by P2P inflows increases to and remains at two percent after job loss instead of returning to zero over 10 months. This elevation over 10 months is a somewhat surprising result given that figure 3 shows that P2P inflows rise and fall over this period. This oddity is related to using a denominator that varies by user with a long right tail alongside user fixed effects and month fixed effects, which lead to counterintuitive and somewhat suspect results due to outliers.

Figure A.21(b) shows a similar measure, but I average this same user-specific median monthly earnings across all users to construct a denominator that does not vary by user. This event study largely parallels figure 4, though the peak is 0.1 percentage points higher as the average of the median monthly earnings is lower.

Figure A.21(c) shows a dynamic measure of the replacement rate, which is calculated using a contemporaneous measure of the P2P share of total inflows. Total inflows are defined as all inflows less internal account transfers. P2P inflows are always a subset of this measure, so the series is constrained at one. When there are no inflows, I fix the series at 1 – indicating that all inflows are also P2P inflows. Effectively, the replacement rate measures the change in the P2P share of inflow around a job loss. This event study shows the highest replacement rate measure, which peaks at four percent before falling to two percent in the long run. This replacement is highest as P2P inflows make up a larger show of total inflows by definition immediatley after job loss. Still, all three of these graphs suggest that P2P inflows replace relatively little income during unemployment.

B.3 Comparison to the Panel Study of Income Dynamics

In order to provide a benchmark for my estimates of the increase in informal transfers during unemployment, I follow Edwards (2020) and use the PSID to estimate the change in transfers from relatives and friends during unemployment spells. Following Edwards (2020), I restrict my analysis to years 1992 through 2020 – the period when both transfers from family and non-family were recorded. I further restrict my sample to users who were in the sample in 2020. I then estimate the change in transfers from friends and relatives during unemployment spells as well as a regression in which I interact this indicator with whether it is 2020 to estimate the change in transfers during the pandemic. Formally, I perform equation 15 in which λ_i, λ_t, and λ_{st} are user, year, and state-year fixed effects. X_{it} includes a host of demographic controls taken from Edwards (2020) including the respondent age, age squared, whether a household member is disabled, whether the respondent became disabled, student status, if the respondent left school, got married, got divorced, homeownership, and whether the respondent became a homeowner.

$$\text{Transfer}_{it} = \beta \text{Unemployed}_{it} + \beta_1 \text{Unemployed}_{it} \times \text{Year}_{it} = 2020 + \delta X_{it} + \lambda_i + \lambda_{st} + \lambda_t + \varepsilon_{it} \quad (15)$$

I deviate from Edwards (2020) on three major points. First, I measure the change in all transfers, not just those from family members. I consider transfer from friends and family because I am unable to differentiate between friends and family that send payments via P2P in my own analysis. Second, I require that respondents answered the survey in 2021 concerning their payments in 2020, but do not require a balanced panel. This ensures precision in 2020, but does not require respondents to have been a head of household or reference person since 1992. Third, I use state-year fixed effects to account for all state level changes over time, whereas she accounts for the state unemployment rate over time.

I report these results in appendix table A9. Columns (1) shows average increase in transfers of \$171 during unemployment, while (2) shows an average increase of \$293 during the pandemic. Along the extensive in columns (5) and (6), margin respondents are six percentage points more likely to get a transfer throughout this period, which remains unchanged during 2020.

B.4 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 experiences 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 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.16. 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 16 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.16.

$$Q_{y_{it}|G_{it}}(\tau) = \alpha + G_{it}\beta_\tau + \epsilon_{it} \tag{16}$$

First, figure A.17 shows that women experience greater P2P inflows at all quantiles above the 25th percentile. Figure A.17(c) 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.17(d) shows that UI recipients receive more P2P inflows at all quantiles. Furthermore, figure A.17(e) 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.5 Ruling out "social spending" and informal earnings

In this section, I rule out that P2P increases after job loss are representative of increased "social spending with friends" 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. 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.18 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.10 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.19 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.20 shows the same event study with this series does not show increases commensurate with my main P2P inflows series.

B.6 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, linked surveys, and imputed from IRS payment amounts. Specifically, 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.

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.22 shows that women tend to receive more support than men. Figure A.23 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. Furthermore, 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.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.

To assess the relationship between informal support and local economic indicators, I match record each user's zip code or county in the month prior to job loss. I use the month prior to job loss to prevent endogenous moving due to unemployment. Then I match users to county-level per capita income data from the American Community Survey and zipcode-level measures of social capital taking from the Social Capital Atlas (Chetty et al., 2022).

Figure A.24 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 5-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[35] 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 numerous high-SES people in zip code or a higher rate of cross-SES friendships being made in an area.

In figure A.25, 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.25 is consistent with A.24 – living in area with more connections to high-SES individuals leads to greater informal insurance after job loss. But Figure A.25 colors 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

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

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.

B.8 Prior Earnings: High earners get more support

In this subsection, I detail the extent to which high earners tend to receive more support after unemployment. For each unemployed user, I calculate their median monthly earnings during the period two or more months prior to job loss. I then separate users into groups based on tercile of earnings prior to job loss. I interact an indicator for user's prior earnings tercile with the relative time dummies in my event study. Figure A.7 shows the interacted event study coefficient estimates for the change in P2P inflows and outflows.

The results indicate that those with earnings in the bottom tercile receive the least in informal insurance after job loss – peaking at just under $20 per month. Those in the top tercile receive upwards of $40 per month, which stays elevated throughout the sample period. P2P outflows show the opposite pattern, reflecting that higher earners need to drop consumption more after a job loss. In general, these results suggest that informal support is distributed inequitably based on prior earnings likely because people tend to befriend those of similar socioeconomic statuses. The corresponding decline in P2P outflows suggests that this may aid consumption smoothing.

B.9 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.26 shows the interacted event study coefficients based on tercile for P2P inflows and outflows.

The results 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.10 Risk aversion

While sections B.6 and B.9 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 insurance 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.27 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.11 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.28 shows event studies interacted by whether the user had P2P inflows at least one month prior to job loss across platforms. Figure A.28(a) 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 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.

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.29 shows event study coefficients from interacting the relative time dummies with whether 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.

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.30 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.31 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.32 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 predict.

B.13 Re-weighting the "Retain" sample

My 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. Figure A.33 shows a greater share of those in retain states had been receiving UI for about a year than in Withdrawal states. 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.

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

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

B.14 Delays in UI

Not all workers receive UI immediately upon application. In section 5.1.2, I describe my second policy experiment to estimate crowd-out using the delayed receipt of UI benefits in the spring of 2020. In that setting, I subset to a group of March 2020 job losers and compare those who received benefits in April 2020 to those who received them in June 2020 under the hypothesis that delays are due to exogenous state-level policy. 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.

In my primary results reported in table 2, I assume that first payment timing is exogenous for the individuals in my dataset in Spring 2020. A potential concern is that individuals with more generous networks apply later (or vice versa), which explains all of the variation in payment timing. To address this concern, I use the Department of Labor's state-level ETA 9050 First Payment Time Lapse data.

For each state I calculate the share of first-time June benefit payments paid more than 63 days after application, meaning that the applications were largely completed in late March and early April. Then I separate groups into above and below median share of first-time June benefits paid in more than 63 days. Those in the above median states are in the treatment group, while those in below median states serve as a control in the same instrumental variables difference-in-difference design that I present in the paper.

In addition to the above/below median approach, I also instrument UI inflows using the shares of benefits paid out after a 63 day delay or longer. The logic is that benefit receipt should change based on how likely a state is to pay benefits on time. This is a deviation from the specification outlined in the main paper, so I present it in equation 17.

$$\begin{aligned} \text{P2P Inflows}_{it} &= \beta \text{UI Inflows}_{it} + \lambda_i + \lambda_t + \varepsilon_{it} \\ \text{UI Inflows}_{it} &= \gamma \text{Share payments with 63 day delay}_{it} + \alpha_i + \alpha_T + \nu_{it} \end{aligned} \tag{17}$$

Results in table A10 suggest that the delayed receipt of UI benefits does not crowd out P2P inflows. The columns show imprecise estimates that P2P inflows are \$9 lower in states with above median delays, but this implies a imprecisely estimated crowd-in of \$0.06 in P2P inflows from a dollar of UI benefits.

B.15 Triple difference July Expiration

In section 5.1.3, I describe my third policy experiment to estimate crowd-out using the expiration of \$600 per month paid out by the CARES Act on July 31, 2020. I compare the unemployed and insured to those who are continuously employed until 2021 and specifically estimate the change in P2P inflows between June and August 2020 for these two groups.

One drawback of comparing these two groups is that they have remarkably different pandemic experiences. For example, workers that stayed employed in 2020 were more likely to see others (via Zoom or in-person) and potentially maintain social ties. As such P2P inflows for employed workers may be more stable through the summer of 2020 than the unemployed who are largely homebound during the pandemic. As such, I would misattribute any changes in P2P inflows to the change in UI, when in reality these are due to a decay in social ties.

To address the drawbacks of comparing the unemployed and the continuously employed during 2020 I extend my difference in difference analysis to a triple difference that subtracts out the same difference-in-difference estimate for June and August 2021 as the continuously employed in 2020 are unemployed in 2021. Equation 18 breaks down the triple difference estimator where i indexes users, m indexes calendar month, and y indexes calendar year and α and λ represent fixed effects along each dimension. I instrument UI inflows using a triple interaction of an indicator for being in the treatment group (unemployed and insured in March 2020), an indicator for whether it is August in 2020 or 2021, and an indicator for whether the year is 2021. I require a balanced panel across the four months of June and August in 2020 and 2021. As a result, the sample size shrinks due to attrition among those who were unemploed in the summer of 2020.

$$\begin{aligned}
\text{P2P}_{imy} &= \gamma\hat{\text{UI}}_{imy} + \lambda_i + \lambda_m + \lambda_y + \lambda_{im} + \lambda_{iy} + \lambda_{my} + \epsilon_{imy} \\
\text{UI}_{imy} &= \beta\text{March job loser, receiving UI}_i \times (\text{Month=August})_m \times \text{Year=2021} \\
&\quad + \lambda_i + \alpha_m + \alpha_y + \alpha_{im} + \alpha_{iy} + \alpha_{my} + \nu_{it}
\end{aligned} \tag{18}$$

By differencing out the change in 2021 inflows, I difference out changes in informal support related to weakened social connections shortly after a job loss. At the same time, 2021 provides an imperfect placebo as it overlaps with the period when some states started to withdraw from expanded UI benefits early, which motivates my third natural experiment. Nonethless, many experienced stable UI payments through this period.

I report OLS and IV results in Table A11. The sample size does not double compared to the baseline experiment because I drop the 2020 unemployed users who attrit by 2020. Despite the drop in sample size, the results are precisely estimated around zero indicating negligible crowd-out (or crowd-in) of P2P inflows by UI benefits.

B.16 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.34 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 19. This fixed effect is the post-job loss excess P2P after differencing out overall user and calendar month fixed effects.

$$\begin{aligned} \text{P2P} &= \lambda_t + \lambda_i + \underbrace{\lambda_i \times \text{Unemployed}_{it}}_{\text{Excess P2P}} + \epsilon_{it} \\ \text{Excess P2P Insurance Share} &= \begin{cases} \frac{\text{Excess P2P}}{\text{Excess P2P} + \mathbb{E}[UI_{jt} | UI_{jt} > 0, j \in I]} & \text{if user receives UI} \\ 1 \text{ if user never receives UI} \end{cases} \end{aligned} \tag{19}$$

The second line of equation 19 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 equal to one by construction.

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.34, 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.34 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 8% and 7% 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.35 shows a similar plot based on the PSID. The PSID asks whether respondents are unemployed and for both UI inflows and transfers from friends and family. I calculate the share of total insurance that comes from friends and family for each user in the PSID from 1976 to present. In the PSID, the average share of total insurance coming from friends and family is 42% prior to 2020 and 29% in 2020, which is right in line with my estimates of excess P2P – unconditional on UI receipt. Many of these transfers may not occur during unemployment, so to guarantee I am looking at those who are unemployed and insured, I condition on receipt of UI. When I condition on receipt of UI, these numbers fall to 7% and 8% before and during 2020, quite a bit closer to my estimates based on the excess P2P share conditional on receipt of UI.

C Appendix: Data Construction

C.1 Datasets

The database of anonymized data I 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". These data cover January 1, 2020 to October 15, 2021. None of the data I 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 include 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 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 October 15, 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 I receive do not contain user identifiers, each dataset does contain Earnin's "tags" that allow me to categorize users across datasets. I 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, I construct "proxy IDs" and measure the panel outcomes for each proxy ID in each dataset. For simplicity, I sometimes refer to each proxy ID as a "user" or an "individual".

C.3 ZIP Codes

I 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. I default to the job ZIP code first because unemployment benefits are associated with the state of employment instead of residence.

C.4 Survey Data

These transaction data are linked to a survey conducted in August 2020 as part of 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. 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 race, gender, age, and marital status, and elicits risk aversion and discount rates using experimentally-validated 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. 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.

C.5 Defining Panel, Sample Restrictions

C.5.1 Transaction Coverage

I require that each individual in my sample have transaction data coverage leading up to and following relevant dates for my analyses. I begin with a sample of Earnin users with transactions spanning January 1, 2021 through October 15, 2021, the focus of my main analysis. I refine this sample further based on transaction memos, state, and earnings tracking.

C.5.2 Uninformative Transaction Memos

For each proxy ID, I count the number of memos that do not offer information about the transaction, which are 'Credit,' 'Debit+,' 'Transaction,' 'Transfer,' a list of numbers and symbols, or memos that are entirely missing. I remove users when more than 1% of their memos are uninformative, as it is rare to have only a few of these uninformative memos.

C.5.3 Minimum outflow count

Following Ganong and Noel (2019), I drop any users with fewer than five outflows per month. I do this to ensure that I do not include users who are not regularly transacting out of the account linked to Earnin and tracked in my dataset.

C.5.4 States without easily trackable UI memos

There are six states for which my 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 A.14, I attribute this to the fact that these states had low unemployment rates to benchmark.

Figure A.14 allows me to leverage the 2020 survey in which Coombs et al. (2021) 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 my survey who I do not track through Earnin's administrative data. I remove those states from this analysis.

I 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 I 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 108,181 proxy IDs from states with well-tracked UI payments, who have no uninformative transaction memos.

C.6 Categorizing transactions

C.6.1 Identifying UI Payments

I 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 I supplement this list with other memos that I identify as attached to UI payments.

Figure A.1 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. (2021).

C.6.2 Categorizing Consumption

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

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

Then, I merge these stable Plaid categories with my 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). I 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 I exclude these categories from my 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).

C.6.3 Identifying Earnings

In order to identify transactions as earnings, I leverage multiple aspects of the transactions and observed earnings data. I 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, I 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 I match a transaction to the amount of one of these three observed earnings from Earnin in a week, I consider those matched transactions to be earnings. If no match to a single transaction exists, I 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, I also consider any other instance of that transaction memo to be earnings. I 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, I perform straightforward searches of transaction memos; I flag any transaction with a memo containing the phrases “PAYROLL,” “ACHPAY,” “PAYRL,” or “SALARY” as earnings.

Finally, I use Plaid's categorization transactions as Payroll or Income. Upon inspection, I find Plaid's categorization of Earnings and Income to be susceptible to false positives. To account for this, I 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.

C.6.4 Identifying, clean, and categorize P2P as informal support

In this section, I outline the specific steps I use to identify, clean, and categorize P2P payments as informal support payments. First, I collect all bank transactions for which that Plaid categorizes as “Venmo,” “CashApp,” “PayPal,” or “Chase QuickPay with Zelle.” Second, I search transaction bank memos for regular expressions that mention these P2P platforms add these transactions. I use these additional memos to fill in holes in the Plaid series that seem to be inconsistently applied over the sample period. For example, memos containing “Zelle” go from being regularly categorized as “Chase QuickPay” to “Credit” after March 2021.

Next, I flag and drop any transactions with bank memos that indicate a payment for a service, good, taxes, refund, small business, other fintech pay advance platforms, gig work, or other payment unrelated to informal support. These regular expressions include ‘point.*of.*sale” or “pos debit,” or “overdraft.” Some accountants send tax refunds and other payments via digital payment platforms, so I flag any transactions with memos mentioning “tax.” Similarly, I flag all 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 via PayPal.

I flag and drop any transactions for amounts below $5 or over $15,000, which is the untaxed maximum for family gifts. These are all unlikely to be informal support for friends. I further flag and drop any transactions equal to economic impact payment amounts in the week surrounding the stimulus deposit dates during the pandemic. I drop these payments because they are likely an indication that households are splitting up stimulus payments that came in one check between family members rather than an informal payment. At the same time, some of these payments are likely to be informal support. I confirm that the event study point estimates are unchanged when omitting these months from analysis or leaving in these payments.