Friends and Family Money: P2P Transfers and Financially Fragile Consumers*

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ABSTRACT

This paper examines the impact of real-time payments on financially fragile individuals in the United States. We do so by using detailed transaction data that allows us to carefully identify income, expenditures and liquid assets holdings. We then investigate how digital payments technology that allows for instant transfer of funds between people at low or no cost, affects person-to-person transfers and consumer outcomes after negative income shocks. We find that instant access to funds reduces uncertainty in cash flow timing, enabling better alignment of income and expenditures, which in turn reduces the likelihood of negative consequences such as overdraft/late payment fees. We confirm our results using a novel instrument for access to digital payments technology. These findings contribute to the active policy debate on real-time payments, highlighting their potential to enhance financial stability for vulnerable individuals.

Keywords: Consumer Credit, FinTech

JEL classification: G50, G51, G41

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Real-time or instantaneous payments that provide prompt access to funds, have gained widespread recognition as a crucial lifeline for individuals with limited financial buffers. This accessibility is believed to play a significant role in mitigating the potential costs associated with income receipt delays, including burdensome late payment penalties and onerous overdraft fees, especially when liquid asset holdings are minimal. In the United States, where many people hold minimal liquid assets, the faster movement of funds between accounts has emerged as a central focus of policy discussions on financial inclusion. However, the empirical evidence supporting the impact of real-time payments on typically marginalized communities, particularly in developed countries, remains limited. While foundational work by Jack and Suri (2014) demonstrates the substantial improvements in access to financial services and long-term implications for poverty reduction resulting from the adoption of mobile money technology in Kenya, it remains unclear whether and how real-time payments can affect individuals within highly developed financial systems, such as that of the U.S.

This paper studies the significance of instant payments in the United States, by investigating how peer-to-peer payments apps, that allow for the instantaneous transfer of funds directly between people at zero or low cost, impact the use of peer-to-peer transfers and consumer outcomes after negative income shocks. We find that during these periods, when liquid assets are unusually low, people rely on peer-to-peer transfers to replace lost income. However, different from research conducted in developing countries, such as Jack and Suri (2014), we do not find evidence that use of peer-to-peer payments apps in the United States results in smaller and more frequent transfers from a larger network as a result of a material reduction in direct person-to-person transfer costs. We do find that while the amount of person-to-person transfers is not different when obtained via peer-to-peer payments apps or not, the timing of the transfers *is* different for those who receive funds via these methods. Specifically, the likelihood of receiving a transfer immediately before large important payments such as housing payments increases more if the user receives funds via peer-to-peer payments apps. Receiving timely transfers before consumption commitments also reduces the likelihood of overdrafts in the future. Collectively our results suggest that real time availability of funds can have a significant impact on consumers with low liquidity.

We begin our analysis by pinpointing periods of income loss, making use of rich micro-data comprised of transaction-level bank account and credit card data for millions of individuals in the United States. We observe all incoming and outgoing transactions from bank accounts and credit cards, which allows us to carefully document income, expenditures and liquid assets holdings. Importantly, we are able to flag person-to-person sources of credits to bank accounts via check, deposit, or digital transfers. We identify endogenous income loss across the broad population, as well as a transitory but exogenous income shock for Federal employees during the 2018/2019 government shutdown.

Our analysis proceeds in five steps. First, we document that during these periods of income loss, people receive more inflows in the form of peer-to-peer transfers. These findings are consistent with survey evidence that suggests that friends or family are one of the most commonly mentioned resources for coping with a financial emergency such as an unexpected income or expense shock. [1] We also find that people with less liquid resources are even more likely to draw down on liquid savings and receive person-to-person transfers.

Second, we investigate how the timing of peer-to-peer transfers shifts relative to expenditures during negative income shocks. During periods of income loss, people receive overall more targeted transfers, which are defined as peer-to-peer transfers into an account closely followed in time by payments out of the account. Making use of the granularity of our data, at the weekly level, we find that the likelihood of these targeted transfers increases when large important consumption commitments such as housing payments are due. Specifically, we find that after income shocks, the likelihood of matched inflows/outflows increases for people living hand-to-mouth, when large payments are due. These findings suggest that matching of timing of income to expenditures increases when account balances are more likely to be close to zero. These targeted transfers might be more likely at these critical times possibly to overcome self-control problems when the stakes are high,[2] or to increase the likelihood of receiving the transfer in the first place.[3] These findings offer new insights into the intricacies of informal social insurance markets, highlighting how moral hazard necessitates the alignment of informal social support with the timing of expenditures. This timing alignment underscores the critical need for funds to be received promptly, especially when liquid resources are particularly low.

Third, we investigate how access to digital payments technology, and specifically peer-to-peer payments apps, impacts the use of person-to-person transfers and consumer outcomes after negative income shocks. We focus on the largest peer-to-peer payments apps in the United States whom account for approximately 95% of U.S. digital person-to-person transaction volume, and allow users to transfer funds instantaneously at low or no cost[4].

Recognizing that peer-to-peer payments app user status could be correlated with unob-

[1]For example Lusardi, Schneider, and Tufano (2011) show that people turn to person-to-person transfers before they seek formal debt, and more recently, surveys conducted by Zelle show that during the Covid crisis, nearly two thirds of consumers have sent financial aid to someone. See www.zellepay.com/sites/default/files/2020-09/Consumer_Payment_Behaviors.pdf.

[2]See Parsons and Van Wesep (2013) who show that when people have self-control problems, matching income to expenditures can be welfare improving.

[3]Targeted amounts might be easier to obtain from social networks if earmarked for a particular purpose, as a result of an interaction of social norms and mental accounting type decision making. See Zelizer (2012).

[4]See https://www.emarketer.com/content/mobile-payment-apps-dominate-p2p-transfers

servable characteristics that also affect the ability of households to smooth risk, we propose an instrument for peer-to-peer payments app access. Our instrument makes use of three important features of peer-to-peer payments apps. First, these platforms are network goods, and hence your own use of the service is a function of the use of others. Second, the use of one platform leads to use of other similar technologies, through financial technology adoption spillovers such as cross-side network effects documented in Higgins (2019) or increased awareness about these technologies. Finally, use of Zelle specifically is also a function of whether or not your bank partners with Zelle.

Making use of these attributes, we use variation in individual exposure to Zelle caused by the staggered adoption of Zelle partnerships with different banks through time as plausibly exogenous variation in access to and use of peer-to-peer payments apps. The idea is, the greater the number of local banks that offer Zelle, the more likely it is that a person will use Zelle and in turn other peer-to-peer payments apps as a result of direct access effects, direct and indirect network effects and technology adoption spillover effects.

Extending this logic, we argue that Zelle/bank partnership exposure at the location of the users' close social circle should also predict peer-to-peer payments apps use. We provide a novel method of identifying the location of close social circle by observing locations of transactions around major holidays such as Thanksgiving and Christmas, and confirm these effects: exposure to Zelle/bank partnerships at the location of close social circle not only predicts Zelle use, but also the use of other peer-to-peer payments apps. Using variation in Zelle/bank partnerships at the location of users' close social circle to instrument for their own peer-to-peer payments app use allows us to control for hyper local time-varying factors. We saturate our tests with city of residence x time (week/year or month/year) fixed effects and hence absorb any time-varying local economic trends that might be impacting both use of the service and outcome variables for users with similar observable characteristics but different social circle exposure to Zelle/bank partnerships.

While the impact of access to peer-to-peer payments apps on households has been well documented in developing countries, it is unclear if payment technologies that improve the efficiency of transfer of funds, would have any impact at all on consumers in a highly developed financial system such as that in the U.S. Using our instrument, we find that those that have access to transfers via peer-to-peer payments apps, do not obtain more or less cash transfers after negative income shocks, in both number and amount. These findings differ from the typical outcomes documented in research conducted in developing countries. In contrast to the observed material reduction in direct transfer costs facilitated by peer-to-peer payments systems in developing countries, our findings suggest that the use of such technology in the United States does not significantly impact transaction costs and in turn enable smaller and more frequent transfers from a larger network.

We do, however, find that while the amount of person-to-person transfers is not different for consumers with better access to peer-to-peer payments apps, the timing of the transfers *is* different. Specifically, the likelihood of receiving a transfer before large important payments such as housing or other bills, increases more if the user has more access to peer-to-peer payments apps. These consumers are also more likely to avoid overdrafts down the line.

Collectively, our results suggest that the timing of receipt of funds matters: people who lose income, are more likely closer to their zero-balance bound and large payments mechanically bring account balances down. While payments such as those related to housing and bills are very likely regular and expected, uncertainty around the availability of funds from person-to-person transfers or the ability to delay or cut these payments, introduces uncertainty around the sufficiency of funds to meet these expenditures. Peer-to-peer payments apps that facilitate the instantaneous transfer of funds, reduce this uncertainty in timing, which helps people manage expenditures better when they are closer to the lower bound, and in turn avoid negative knock on effects such as low balance, overdraft, and non-sufficient funds (NSF) fees.

Our work directly contributes to the active policy debate on whether or not the lack of access to real time payments in the U.S. adversely impacts the millions of Americans who live paycheck to paycheck. For these people, delays in receipt of funds are thought to have severe negative consequences such as triggering overdrafts and NSF fees. We find evidence consistent with this hypothesis: more certainty around the timing of receipt of funds when account balances are closer to zero, can reduce the likelihood of negative knock on effects such as incurring overdraft fees.

We make three additional contributions to the academic literature. First, we add to the literature on financial fragile households in the United States and how they cope via informal social insurance. Theory suggests that people who are risk averse, and who face uninsurable shocks, accumulate wealth in a precautionary way to help smooth consumption (e.g., Deaton, 1992). However, copious survey and anecdotal evidence indicates that many households have few or no emergency funds and are often financially fragile as a result. For example, Larrimore, Durante, Kreiss, Park, and Sahm (2018) suggests that households rely on methods of managing financial shocks outside of the use of savings or the formal credit system. Indeed, Lusardi et al. (2011) show that when people are financially fragile, they turn to informal social insurance provided through friends and family transfers well before they turn to other more formal sources of credit. However, while informal social insurance has been widely studied in developing countries, such as in Chetty and Looney (2006), there are no systematic studies conducted in the United States and what we know on the topic stems solely from survey evidence such as that in Lusardi et al. (2011) and Larrimore et al. (2018). We add to this literature by providing visibility into informal social insurance in the United States. Additionally, we provide new evidence on how digital payments technologies impact the use of informal social insurance through person-to-person transfers and affect consumer outcomes during periods of financial fragility.

Second, we contribute to the emerging literature on the impact of the timing of receipt of funds. In frictionless markets, the timing of receipt of income should not matter: consumers can save or borrow to create any timing profile they desire. However an emerging empirical literature has documented that the timing of receipt of funds does matter for consumer choices and outcomes. For example, Parsons and Van Wesep (2013) show that that impatient households are better off when the timing of their pay aligns with the timing of their expenditures because this matching reduces self-control problems. Additionally, Baugh and Correia (2022) find that the timing of receipt of income impacts people's savings, borrowing, and consumption decisions because paycheck frequency determines liquidity, which in turn drives illiquid savings decisions and ultimately consumption. Consistent with these studies, we show that when liquidity is low, households prefer to match receipt of income to expenditures, and increased certainty around the timing of this match results in a lower likelihood of negative knock on effects such as overdrafts.

Finally, we add to work related to understanding how technology adoption can impact transaction costs and in turn risk sharing. The closest paper to our study is Jack and Suri (2014) who show that adoption of M-Pesa – a mobile technology first rolled out in Kenya that allows users to transfer money via SMS and lowers the direct transaction costs of remittances – helps users increase and smooth consumption. Jack and Suri (2014) supports a wider body of work focused in developing countries that demonstrate how high transaction costs can hinder risk sharing and economic development and that reducing transaction costs through institutional and technological innovations can facilitate risk sharing and promote economic growth. We study the adoption of a cost-reducing instantaneous person-to-person transfer technology by millions of consumers in the U.S., and show that direct transactions costs do not seem to be of first-order importance in facilitating risk sharing through person-to-person transfers in highly developed financial systems.

The rest of the paper is organized as follows: Section I contains background information and describes our data, Section II shows how individuals use friends & family transfers to cope with income shocks. SectionIII provides evidence of transfer–expense matching during these shocks. Section IV investigates the effects of real-time transfers through P2P payment apps on the timing of transfers vis-a-vis expenses. Section V examines how P2P app-based transfers affect consumer outcomes such as low liquidity fees. Section VI concludes.

I. Background and Data

In this section, we first discuss institutional details that are relevant for our study. We then describe the key characteristics of our data, the measures we use, and construction of our analysis samples. Finally, we provide the summary statistics and discuss validation of some of our key measures using external data sources and studies.

A. Friends & family money transfers

Money transfers between people have been around for centuries, and they have historically been physical transfers of cash or checks delivered in person or via the postal service. Later, non-physical transfers through bank wire services or Automated Clearing House (ACH) transfers became widespread. These legacy methods have high (direct and indirect) costs, low processing speed, and high uncertainty as to when the transfer completes. Consider Western Union, which launched the first widely used wire transfer service on its existing telegraph network in 1872. To use the service, a sender would bring money to a telegraph office, and the telegraph operator would then transmit a message and "wire" the money to another office, using passwords and code books to authorize the release of the funds to a recipient at that location. Bank wires are still widely used to this day,[5] and they typically require large flat fees to make the funds immediately available to the recipient.[6] Other widely used non-physical money transfers are electronic transfers through the ACH bank-to-bank network founded in 1972, which is largely used for low-value transfers. While the cost of ACH transfers is typically low, these transfers are not instantaneous, with funds taking anywhere from 3 to 7 business days to become available in the recipient's bank account.

Recent innovations in digital payments technology have significantly reduced the direct and indirect costs associated with money transfers between people. Peer-to-peer (P2P) app-based money transfers, a major innovation in this space, link digital wallets to bank accounts and allow instant transfer of funds at zero or very low cost. For example, transfers between users of wallet applications such as CashApp, Venmo, or PayPal are typically instantaneous and free. Transfers from these wallet applications to bank accounts are also free if the recipient can wait for 1–3 business days (i.e., faster than ACH transfers) and cost up to 1.75% of the transaction amount if transferred instantaneously (i.e., cheaper than wire transfers).

[5]E.g., *Statista.com*'s survey of 4,180 consumers in September–October 2018 found that 13.97% of 18–29-year-olds, 16.63% of 30–49-year-olds, and 7.24% of 50–64-year-olds used a wire transfer service in the last 12 months. See "Share of Americans who used a wire transfer service in the last 12 months in 2018, by age," *Statista Survey*, October 31, 2018.

[6]Wire transfer fees are typically around $35 per transaction, and the fees are sometimes incurred by both the sender and the receiver. See Internet Appendix Table IA.1 for money transfer costs and times to clear.

We will further refer to both traditional and P2P app-based transfers between people as "friends & family transfers."

Zelle is a notable development in the P2P transfer market because it provides both free usage and access to transferred funds within minutes. Zelle is built on its own Real-Time Payment (RTP) network operated by Early Warning Services, a financial company owned by a consortium of seven major U.S. banks (i.e., Bank of America, Truist Bank, Capital One, JPMorgan Chase, PNC Bank, U.S. Bank, and Wells Fargo).[7] Zelle's transaction volume grew rapidly from $55 billion in 2016 to $307 billion in 2020 largely due to its availability through partner institutions' mobile banking applications, which enhances consumer awareness, reduces setup costs, and increases convenience.[8] Additionally, this integration fosters trust in the service, as users perceive it to be more reliable if offered through banking network.[9]

One important feature of Zelle for our study is that it requires at least one party in a transaction to bank with a Zelle partner institution.[10] The adoption of Zelle by banks is hence crucial for consumer adoption, and network effects further incentivize its use among consumers. We leverage this feature in our identification strategy by making use of the correlation between Zelle use and the (staggered) adoption of Zelle in cities with consumers' close social circles. This approach allows us to isolate network effects from local variations in preferences, while controlling for time-varying economic conditions and peer-group effects.

B. Data

B.1. Consumer transaction data

We construct our analysis data set by combining two series of data – consumer transaction data and Zelle–bank partnership data – in weekly and monthly representative consumer-level panels. The first data series are constructed from transaction data provided by a large U.S. data aggregation and analytics platform. The platform uses advances in machine learning to clean and categorize transaction data, which are offered as a product to institutional investors and investment managers in aggregated and disaggregated forms. Access to these

[7] While Zelle is free for consumers, it costs participating banks from $0.50 to $0.75 per transaction. Banks likely monetize on Zelle through curtailing competition, retaining customers by creating switching costs, and earning revenue from interchange fees. E.g., see "Zelle Costs Bankers Money, Venmo Can Make Bankers Money," *BankDirector.com*, November 30, 2018 and "How Does Zelle Make Money & Who Owns It?" *productmint.com*, July 29, 2021.

[8] E.g., see Sarah Perez, "Zelle Forecast to Overtake Venmo This Year," *TechCrunch*, June 15, 2018.

[9] See Dave Johnson, "Zelle Is a Safe Way to Send and Receive Money – but Only from People You Trust," *Business Insider*, May 6, 2022.

[10] Consumers can access Zelle through a standalone application if their bank does not offer Zelle. However, doing so is limited to users with certain types of debit cards and is less convenient. Transactions through the Zelle application are also subject to low limits (e.g., $500 per week) and are not instantaneous.

data is provided pursuant to agreements between the platform and its partners – financial institutions and FinTech firms – rather than directly by consumers, which overcomes active selection issues inherent in aggregator data sets when consumers can opt in to share their data for a specific purpose (e.g., financial planning, purchase of a specific FinTech product).

We obtained access to de-identified data comprising bank account, debit card, and credit card transactions and demographics data (income and geographical location[11]) for an unbalanced panel of approximately 10 million active consumers from January 2010 to May 2021.[12] We observe a number of fields for each transaction, such as transaction date, amount, type (e.g., debit, credit), account type, and transaction memo. We also observe fields derived by the data provider from these memos, including the assigned transaction category, transaction location, and merchants associated with the transaction.

B.2. Friends & family transfers

We construct two broad classes of variables from the raw data. The first class is created by aggregating the data (e.g., summing, averaging) at the person/time unit/category level. The second class is created by aggregating at the person/time unit/derived class level. We create derived classes by making use of the merchant names and memo text. The data provider strikes out any personal identifiable information from the memos, and we are then able to identify additional characteristics of transactions by searching for text in the remaining characters. For example, we are able to identify the incidence of overdrafts by searching the memo field for keywords such as "overdraft fee." We outline the key variables used in this study and details of how these variables are constructed in Internet Appendix Section IA.VI.

Using the derived class approach, we identify two groups of money transfers between people. The first group is traditional transfers such as those made via checks, cash (i.e., ATM withdrawals and deposits), ACH transfers, and wire transfers. The second group is P2P app-based transfer systems such as Venmo, Zelle, CashApp, and PayPal. Collectively, we refer to these two groups of money transfers as "friends & family money transfers." We recognize that some of the money transfers we identify can reflect income from informal work rather than transfers between friends and family. However, several facets of our analysis make it less plausible that the variation in money transfers we are capturing during liquidity

[11] City names in the transaction data require extensive cleaning. We create a custom crosswalk between these city names and those in the U.S. Census Bureau data to maximize the match rate between data sources. Internet Appendix Section IA.I details the procedure.

[12] While some consumers enter and exit the panel at different times, we observe roughly 10.6 million distinct consumers on average on a monthly basis. The entire data set comprises 59 million U.S. consumers. There is some attrition in the data in the early years, but attrition is minimal after 2014. Additionally, demographics data are only available from January 2014. We thus drop transactions before 2014 from the data.

shocks is coming from increased informal sector labor rather than from friends and family money. We come back to this point later (e.g., see Section II).

We create our measures of friends & family money transfers (e.g., likelihood, dollar amount, frequency) by making use of transaction categories and merchant names provided by the data aggregator, as well as information contained in transaction descriptions. Specifically, we manually search for the names of payment systems and their variations in primary and secondary merchant classifications, filtering transaction categories to include "Transfers," "Deposits," "ATM/Cash Withdrawals," and "Check Payment," as appropriate. Since we are interested in money *received* by consumers from their friends & family, we only consider transaction types marked as credit (rather than debit). To improve on the merchant classifications and in cases when merchant names are left blank in the data, we supplement this procedure by extracting similar information directly from transaction memos. See Internet Appendix Section IA.II for an illustration of synthetic transactions flagged as friends & family transfers and more details of how we identify specific money transfers.

B.3. Analysis samples

We construct our analysis samples as follows. First, we select four equally sized groups of consumers from the data who form all combinations of two variables: (1) P2P transfer apps user/not a P2P transfer apps user and (2) federal employee during the 2018/2019 government shutdown/never a federal employee. A consumer is defined as a P2P transfer app user if she has at least one P2P app-based transfer transaction in the data. We outline how we identify these transactions earlier in this section. We identify federal employees as individuals who have "fed sal" in the transaction memos.[13] We also flag federal employees with lost income during the shutdown as those who receive a federal paycheck in December 2018 but do not receive a federal paycheck during the first three weeks of January 2019.[14] Second, we require that all consumers in analysis data have on average greater than two transactions per day, totalling greater than $20 per day, and are active in the data for at least 180 days. This restriction ensures that we are capturing active consumers for whom the data provider has high-quality transaction data.

We next combine all transactions by the selected consumers in two data sets, one monthly and one weekly. In the monthly data set, each of the four groups of consumers is sampled to resemble the U.S. population along income and geographical (i.e., city/place) dimensions, as documented in the 2020 U.S. Census. Our final representative monthly data set comprises

[13] We follow the Treasury Green Book, which outlines ACH payment types for various federal payments, including salary payments for federal employees. See "Green Book: A Guide to Federal Government ACH Payments," *U.S. Department of the Treasury*.

[14] Also see Herpfer, Maturana, and Teodorescu (2023) for labor costs of government shutdowns.

monthly level data for 195,331 consumers, spanning January 2014 to December 2020. In unreported analysis, we visually check that the income distribution in our monthly representative sample closely resembles that of the 2020 U.S. Census, which gives us comfort that our sampling technique successfully draws a representative sample of U.S. consumers along the income dimension. We also inspect the geographic spread of people in our representative sample and confirm that we are able to match the location of people in the U.S. fairly well. In the weekly data set, we relax the U.S. population sampling criteria to make sure we have a meaningful analysis sample. We pull a random sample from each of the two groups of consumers – P2P transfer apps user/not a P2P transfer apps user – and condition on consumers being federal government employees. Additionally, we ensure that we randomly sample federal employees who lost income in the 2018/19 shutdown and those who earned federal income in December 2018 but did not lose income during the shutdown. Our final weekly data set comprises 75,077 federal employees and spans the period from January 2017 to December 2020, of which 28,149 lost income during the shutdown.

B.4. Zelle partnership data

The second data series comprises data on Zelle's partnerships with "network financial institutions" (e.g., banks, credit unions), which we hand-collect from public sources. We first identify current and past Zelle partner institutions by combining 144 historical Zelle partner lists (see Internet Appendix Section IA.III for more details). We carefully match these Zelle partners to identifiers (RSSD IDs) using the Federal Financial Institutions Examination Council (FFIEC) database, taking into account variations in name spellings, dates of incorporation, and locations of headquarters of these financial institutions.[15] We then manually collect Zelle roll-out dates by partner institutions and dates when institutions stopped partnering with Zelle, when applicable.[16]

Our master list of Zelle partners contains 1,113 partner institutions, including banks (75.4%), state and federal credit unions (22.8%), and savings and loan associations or financial companies (1.8%). We further restrict this list to bank partners because of availability of bank branch data from the FDIC's Summary of Deposits (SOD) database and because we are concerned that Zelle adoption decisions of credit unions and other nonbanks such as savings and loan associations may be endogenous to local economic conditions. We also distinguish between big banks and small banks, treating banks with branches in at least 12 cities as big banks (e.g., Bank of America, Wells Fargo).

[15] Only 1.7% of Zelle partners remain unmatched to RSSD IDs.

[16] Several banks stopped partnering with Zelle during our sample period, mostly for relatively exogenous reasons to consumers such as bank mergers.

There is significant variation in Zelle adoption by financial institutions across time and type of institution, although banks dominate Zelle partner lists even in later years (Internet Appendix Figure IA.6). Importantly, our sample contains both periods of economic growth and crises (i.e., Covid-19). Yet, there is a lot of Zelle adoption during 2020–2021 as well as during 2017–2019. We note that most early adoption is by big banks, but there is visible time variation in Zelle adoption even for big banks. We find noticeable geographical variation in Zelle partnerships, as well as variation across time (Internet Appendix Figure IA.7).

We then combine our Zelle partnership data with bank branch data from SOD data set to construct a city-level measure of consumers' exposure to Zelle, which provides a source of exogenous variation in P2P transfer systems' use. Specifically, our measure *Zelle Exposure* as the number of bank branches owned by Zelle partner banks to the total number of bank branches in a city, where partnership roll-out dates are lagged by one month and bank branch data are measured as of the most recent (relative to partnership dates) June release.

We merge this time-varying measure of Zelle exposure to our monthly and weekly analysis data sets by consumers' city of residence and city of social circle. We use a novel approach to identifying the city of consumers' close social circle from transaction data by geocoding the locations of consumers' spending during major holidays such as Thanksgiving and Christmas when individuals tend to visit close friends and family (see Internet Appendix Section IA.IV). We note that Zelle exposure at the city of residence may not be fully exogenous to the outcomes we study because changes in local Zelle exposure may be correlated with not only local economic trends but also characteristics of consumers who reside in a city. Indeed, there seems to be some heterogeneity in Zelle exposure by consumer characteristics such as income when we examine Zelle exposure at the city of residence (not reported). However, this heterogeneity disappears when we examine Zelle exposure at the city of social circle.

We take a closer look at the correlation of Zelle exposure at the city of social circle with city of residence and consumer characteristics in bin scatter plots reported in Figure A.II. We examine local Zelle exposure, employment, and population. We also plot regular income, income volatility, and a dummy for income being usually below <\$1,000. We focus on income because it is the first-order determinant of other outcomes (e.g., low-liquidity fees), it is highly correlated with other characteristics (e.g., spending), and it can proxy for certain unobservables such as education level. Figure A.II demonstrates that exposure to Zelle at the city of social circle is virtually uncorrelated with Zelle exposure at the city of residence and other local characteristics (Panels A–C). Additionally, comparing consumers who reside in the same city, there is no relationship between consumers' income measures and Zelle exposure at the city of social circle (Panels D–F).

C. Summary statistics

We report summary statistics of both monthly and weekly analysis samples over a one-month snapshot in September 2018 in Table I. Column (1) describes consumer characteristics for the full monthly representative sample, while Column (2) subsets this population to consumers who we flag as likely constrained due to living hand to mouth (defined in Internet Appendix Section IA.VI). Columns (3)–(5) describe the weekly data set of government employees for the same month, where we appropriately aggregate all variables originally defined on a weekly basis over the entire month of September 2018. Column (3) reports statistics for the full set of government employees. In Columns (4) and (5), we restrict the sample to federal employees who lost income during the 2018/19 federal government shutdown and further to those who are also likely liquidity constrained, respectively.

Table I shows that between 20% and 30% of consumers in our data are living hand to mouth (Panel A). We refer to these consumers as "constrained users" because they exhibit features of liquidity constrained population such as higher likelihood to incur overdrafts or late fees and higher use of alternative high-cost credit (see Internet Appendix Table IA.2). This fraction is consistent with other statistics documented in the existing literature. For example, Kaplan, Violante, and Weidner (2014) find that around 30% of U.S. households live hand to mouth, consuming all of their disposable income. More recently, Aguiar, Bils, and Boar (2023) estimate that 40% of households in the U.S. live hand to mouth, based on the Panel Study of Income Dynamics. It is noteworthy that we have a less severe measure of constrainedness requiring that an individual is a hand-to-mouth consumer if she on average consumes within $1,000 of all her income each month, which can explain our slightly lower incidence of hand-to-mouth consumers of 20–30%. Additionally, Table I Panel B shows that individuals who live hand to mouth on average have lower incomes, which is also consistent with Kaplan et al. (2014) and Aguiar et al. (2023) who report that hand-to-mouth households have approximately half the income of unconstrained households.

Panel A of Table I also documents that unconstrained consumers are more likely to occasionally use their savings and their spending can fluctuates downward more than spending by constrained consumers. Put differently, those who likely live hand to mouth are much less likely to have flexibility of adjusting their spending downward in any given month, relative to the same month in the previous year. We also note that federal employees seem to earn on average higher median income than a representative person in our monthly sample. This result is consistent with data from the U.S. Bureau of Economic Analysis.[17]

We next examine several key liquidity-related variables reported in Table I and compare

[17]See, e.g., the U.S. Bureau of Economic Analysis, National Income and Product Accounts, Table 6.6D.

their distributions to external studies. Around 3–4% of people in our data incur overdraft and/or low balance fees in any given month, and those who are likely liquidity constrained are significantly more likely to incur these fees. In Appendix Figure A.I, we compare to what extent our overdraft fee incidence aligns with similar outcomes documented in a representative sample of the U.S. population from the Report on the Economic Well-Being of U.S. Households released by the Federal Reserve Board in 2021. Notwithstanding the fact that our data comprises individuals rather than households and that the Fed Survey buckets individuals into different income groups than our data provider, we find the distribution of overdraft fee incidence in our study to be similar to that documented in the Fed Survey. Similarly, the distribution of credit card usage by consumers in our data closely resembles that in the Fed study (see Appendix Figure A.I). These results suggest that the variables that we use in our study are likely representative of characteristics of the general population in the U.S. Additionally, these comparisons give us confidence that we are able to identify and measure well key outcomes and characteristics used in this paper.

Finally, we find that in any given month, 30% of people receive a friends and family transfer and around 2/3 of these transfers are likely ear-marked for specific expenditures. These findings are consistent with recent survey evidence suggesting that around 30% of Americans are owed money at any given point from friends and family.[18] It is also consistent with smaller-scale evidence from the U.K. documenting that 60% of adults have borrowed money from friends and/or family at some point, and 37% and 26% of people who borrowed funds from friends and family did so specifically to pay a bill or buy groceries.[19]

II. Coping through liquidity shocks

In this section, we document broad dynamics of how people cope after income shocks, with an emphasis on friends & family transfers. We begin by focusing on federal employees who did not receive paychecks during the government shutdown of 2018/2019 and later provide a parallel set of results for endogenously identified income shocks in our data.

A. Exogenous income shocks: The Federal Government Shutdown of 2018/19

The Federal Government Shutdown of 2018/2019 resulted from an impasse between Congress and the White House on December 22, 2018 and lasted for a record-breaking period of 35 days. The shutdown was unanticipated and large in scale, affecting approxi-

[18] "31% of Americans Say a Friend or Family Member Owes Them Money," *LendingTree*, November 2021.
[19] See "The Invisible Debt of Borrowing from Friends and Family," *NerdWallet*, August 16, 2023.

mately 800,000 federal employees across multiple government agencies. Federal employees were furloughed or required to work without pay, missing their regular paychecks starting from the week of January 7, 2019 until the week of January 28, 2019 when the shutdown was terminated and some federal employees received back pay. We treat the 2018/2019 Federal Government Shutdown as an exogenous shock to income of Federal employees working at affected agencies. We also note that the shutdown was mostly a liquidity shock to affected federal employees due to the timing of pay rather than a change in income. There was also substantial uncertainty around when the shutdown would be resolved and paychecks would continue as normal. We describe the 2018/2018 government shutdown episode in more detail in Internet Appendix Section IA.V.

A.1. Income shock

We start by documenting the extent of the income shock, by tracing out regular income during the months of December 2018 to February 2019 for federal government employees. We flag Federal employees as treated for those who did not receive paychecks – either through furloughing or working without pay – during the first two weeks of January 2019. The control group are federal employees who continued to receive paychecks through the shutdown. We trace out the income shock by estimating the following event-study specification:

$$y_{it} = \alpha_i + \zeta_{ct} + \sum_{k=-5}^{6} \gamma_k \mathbb{1}\{k = Onset_t - t\} \times T_i + \varepsilon_{it} \tag{1}$$

where y_{it} is an outcome variable, T_i is an indicator variable equal to one for treated federal employees and zero otherwise, i indexes federal employees, and t indexes calendar weeks. $Onset_t$ is an indicator variable for the week ending December 31, 2018, which is the week before affected federal employees missed their regular paychecks for the first time after the shutdown. The coefficients α_i and ζ_{ct} represent person and city of residence by week fixed effects, respectively. These fixed effects absorb consumer heterogeneity and calendar time trends that vary by city.

Figure 1 Panel A report γ_k coefficients for y_{it} equal to regular income such as salary income, part-time income, and other regular income. The graph shows that federal employees affected by the shutdown lost roughly \$3,600 in regular income during the first three weeks of January 2019. This drop in income represents a close to 100% weekly decline relative to income at the peak and on average a 43% decline over the 4 weeks of January 2019. Around \$1,200 of the lost income was replaced in the week following the reopening of the government post January 25, 2019, and paychecks seem to resume as normal two weeks after the reopening. Hence, while the income shock was temporary, it was sizable.

A.2. Methods of coping

We next document how federal employees cope with income shocks such as that resulting from the 2018/19 Federal government shutdown, focusing on consumer reliance on friends & family transfers. Figure 1 Panels B–D show that the likelihood of receiving a transfer from friends and family jumps by up to 2.5 percentage points (pp) during the first three weeks after missed paychecks, which represents an increase of approximately 25% relative to the mean likelihood of receiving a credit in any given week. These transfers account for approximately \$35 in additional money a week and conditional on receiving a transfer, represent a credit of roughly \$140 a week, on average. The frequency of friends & family transfers in a given week increase by about 4 pp after the shock. Figure 1 also shows that the likelihood, amount, and frequency of friends & family transfers drops below the pre-shutdown levels in the weeks of February 4–18, 2019, which indicates that at least some consumers were able to shift the receipt of money from friends and family to earlier, match the timing of these transfers to their liquidity needs.

We also assess two other methods of coping, namely the draw down of liquid savings and cuts in total expenditure. We define the use of liquid savings by first calculating the rolling difference between the sum of income and the sum of expenditures over the previous 6 weeks and flagging the use of liquid savings when expenditures exceed income over this window. These instances identify weeks when it is likely the consumer is dipping into account balances in order to fund expenditures. Additionally we flag credits to the bank account classified as savings as the use of liquid savings. Figure 1 Panels E–F show that federal employees are 20% more likely to draw down on their liquid savings at the peak of income loss in the week of January 21, 2019 and up to 12% more likely to cut total spending. Panels B, E, and F of Figure 1 also shed some light on potential pecking order of coping. For example, the likelihood of cutting spending peaks earlier than the likelihood of drawing down on liquid balances and receiving friends & family transfers, which indicates that spending cuts might be preferable as a first choice.

We summarize these estimates by averaging over the period of income loss in Panel A of Table II, where post is a dummy variable taking a value of one for the first three weeks in January 2019 and 0 for the remaining weeks around the shutdown. These results provide more conservative estimates due to the shifts in the timing or certain methods of coping – such as friends & family transfers as documented earlier – between the weeks immediately following the shutdown shocks and subsequent weeks after federal employees received their back pay and could replenish their savings, meet consumption needs, or repay their friends and family if needed.

A.3. Heterogeneity

Income shocks like the government shutdown are particularly concerning for consumers living paycheck to paycheck who typically have little liquid savings and are liquidity constrained. These hand-to-mouth consumers can find it difficult to cover expenses, especially large non-discretionary expenses such as housing payments, when their income unexpectedly dry out. We assess to what extent the coping behaviors documented above differ as a function of ex-ante constrainedness. We flag federal employees in our sample as liquidity constrained users if these consumers on average spend within $1,000 of their income over a 6 week window, before December 2018. We than estimate a regression specification with triple interaction terms between a post-shutdown indicator, an indicator for treated federal employees, and an indicator for liquidity constrained consumers in Table III.

Table III Panel A shows that constrained hand-to-mouth individuals are more likely to receive friends & family transfers, draw down on liquid savings, and cut spending when they are affected by the income shock compared to unconstrained federal employees. However, affected constrained employees received similar amounts of friends & family transfers during he shock as those who also lost income during the shutdown but were not living hand to mouth. These findings suggest that the less liquid savings you have, the more likely you are to reach out to friends and family for transfers indicating that these transfers are received at times when those who are liquidity constrained are most in need.

B. Endogenous income shocks: Large-scale evidence

We next perform a parallel analysis and document coping responses to income loss using our monthly data set. The data set contains a representative sample of the US population along income and geography dimensions and spans a broader time period than the Federal Government Shutdown of 2018/19. Therefore, this analysis supplements and validates the government shutdown results by providing consistent large-scale evidence of the role of friends & family transfers in helping consumers cope with income shocks.

B.1. Income shock

Consumers experience income shocks from time to time due to various reasons such as salary cuts or job loss. We flag income loss as a month in which regular income drops below the 50^{th} percentile of regular income for that person, remains below the 50^{th} percentile in the following month, and is not below the 50^{th} percentile in the previous month. We then keep income loss events where there are no other events in the previous 3 months. We document the evolution of regular income around the month of income loss by estimating the following event-study specification:

$$y_{it} = \alpha_i + \zeta_{ct} + \sum_{k=-4}^{4} \gamma_k \mathbb{1}\{k = \textit{Income Loss}_{it} - t\} + \varepsilon_{it} \tag{2}$$

where y_{it} is an outcome variable, $\textit{Income Loss}_{it}$ is an indicator variable equal to one for the month of income loss for a given consumer and zero otherwise, i indexes consumers, and t indexes calendar months. The coefficients α_i and ζ_{ct} represent person and city of residence by month fixed effects and absorb time-invariant heterogeneity across consumers and calendar time trends that vary by city, respectively.

We plot γ_k coefficients for regular income as an outcome variable in Panel A of Figure 2. The graph shows a large decline in income of roughly \$2,000 at the time of income loss, which is a shock of a similar magnitude to the income loss recorded during the Federal Government Shutdown of 2018/19. The income loss continues for another month and then partly but not fully reverses. It is worth nothing that the income spike in the month prior to the income loss and subsequent income loss is similar in both pattern and magnitude to that documented in Gerard and Naritomi (2021), which precisely identifies layoffs using de-identified high-frequency expenditure data from VAT receipts originally linked to individual identifiers and matched large-sale employee–employer data in Brazil. We argue that given the strikingly similar pattern of income loss we document, we are likely picking up layoffs similar to those captured in Gerard and Naritomi (2021).

B.2. Methods of coping

We next examine how consumers cope with these occasional income shocks on a large scale. The patterns we observe in Panels B–F of Figure 2 are strikingly similar to those contained in Figure 1 Panels B–F. During income shocks, the receipt of friends & family transfers increases in likelihood, count and overall dollars received. People also draw down on their liquid balances and cut expenditures.

While the broad patterns of coping remain the same, Figure 2 shows that the magnitude of reliance on friends & family transfers is significantly smaller during these periods of income loss, possibly because some of this income loss can be anticipated. Specifically, the likelihood of receiving a friends & family transfers increases by approximate 5 pp in likelihood of receiving a transfer and \$40 in dollar terms per month. Conditional on receiving a friends \$ family transfer, the median (average) transfer size is approximately \$485 (\$2,000). In other words, it appears that people obtain larger friends & family transfers less frequently during these likely more persistent periods of income loss than they would during a more transitory income shock such as the Federal Government Shutdown of 2018/19 as documented above. We summarize these results in tabular form in Panel B of Table II.

B.3. Heterogeneity

In Panel B of Table III, we further examine heterogeneity in response to income loss by the degree of liquidity constraints. The coefficient of interest is that of the interaction term between the income loss dummy and an indicator for a hand-to-mouth consumer. Similar to the government shutdown results presented in Panel A, we find that constrained people who live hand to mouth rely more on friends & family transfers during the time of income loss. They are also more likely to draw down on liquid savings and cut spending than consumers who do not live paycheck to paycheck but also experience income loss.

These findings – spanning both temporary exogenous shocks and endogenous more persistent shocks – show consistently that people receive more friends & family transfers after losing income, especially if the person is more likely to be liquidity constrained. We next dig deeper into how people use friends & family transfers around periods of exogenous and endogenous income loss, by exploring how the timing of receipt of friends & family transfers changes during these periods.

III. Timing of friends & family transfers around income shocks

In this section, we leverage our extensive data to document patterns in the timing of receipt of friends & family transfers relative to expenditures around exogenous and endogenous income shocks. We first examine general matching transfer – expense patterns around these shocks and then focus on how P2P app-based transfer technology facilitates transfer–expense matching.

A. *Income-expense matching framework*

We start by discussing the conceptual framework for the timing of income. While expenditures, and especially consumption commitments such as bills and housing payments, are relatively fixed and predictable, receipt of income can be substantially less certain. This is especially true for ad-hoc income such as money from friends and family. This uncertainty is due in part to delays in transaction posting, which are a function of the end-of-day batch clearing that is still present in the U.S.[20] In fact, the timing of receipt of income is part of an active and long-standing policy discussion on the positive impact real-time payments could have on consumers who live hand to mouth.

Additionally, an emerging literature has documented that the timing of income relative to expenditures matters for consumer outcomes, especially during periods of low liquid resources (e.g., Parsons and Van Wesep, 2013; Baugh and Correia, 2022). If people time income to match expenditures to overcome self-control problems, then the actual timing of receipt of the income can have significant impact on consumer outcomes. For example, if income is received *after* the expenditure is due, this delay can lead to increased likelihood of incurring low balance, overdraft, and late fees, with possible longer-term negative knock on effects (e.g., Leinonen, 2005).

Given that we are able to determine the precise dates when income was received and expenditures were incurred, we are able to identify the sequence of the receipt of friends & family transfers vis-a-vis expenditures. Hence, we are able to document if individuals indeed match the timing of income to expenditures where possible, and if and how these sequences change during periods of income loss. Such transfer–expense matching would be consistent with the presence of moral hazard in the friends & family transfer market, in line with the theoretic model on the optimal pay timing in Parsons and Van Wesep (2013).

B. Transfer–expense matching around income shocks

We proceed to our empirical analysis on the timing friends & family transfers using our two panel data sets – weekly data on federal employees during the Federal Government Shutdown of 2018/19 and full monthly representative sample. We estimate regressions of the form reported in Equations (1) and (2). We define a new outcome variable to capture the timing of receipt of friends & family transfers relative to expenses. *Matching* is a dummy variable that takes the value of one if a friends & family transfer is received within 3 days prior to a housing, utilities, credit card bill payments, car, or grocery expenditure and 0 otherwise. We chose these expenditures because they are costly to miss. This variable captures the extent to which people might time the receipt of transfers and allows us to document how this timing might differ during income shocks relative to normal times.

We report the results on transfer–expense matching in Fig. 3. Panel A is based on the government shutdown weekly data and Panel B is based on the full monthly sample. Focusing first on the left-hand graphs, we observe a sharp increase in transfer–expense matching at the time of income shocks. The increase is about 2 pp at the peak for the government shutdown

[20] See e.g., *"Guide to the Federal Reserve's Payment System Risk Policy on Intraday Credit"* for more details on clearing in the U.S.

sample and approximately 0.5% for the monthly sample. Using the monthly average of the matching variable of 22.9% for the representative sample as a conservative estimate (due to potentially endogenous nature of these shocks), the economic magnitude of the effect is at least a 2.2% increase in matching. The effect for the government shutdown on transfer–expense matching appropriately aggregated to the monthly level is more than two times larger. We note that the transfer–expense matching patterns closely resemble friends & family transfer patterns in Figures 1 and 2. To rule out that the incidence of these transfers themselves might be driving the matching results, we condition our matching variable on receiving a friends & family transfer in left-hand graphs. The results hold.

We also present these estimates in tabular form by estimating regressions similar to the ones in Section II. As before, the person and city of residence by time fixed effects absorb time-invariant within-user heterogeneity in transfers and expenditures as well as calendar time trends that vary by city. We report consistent results in Columns (1) and (3) of Table IV. We also examine if the likelihood of receiving a matching transfer increases more for people who live hand to mouth during periods of income loss in Table IV Columns (2) and (4). This is exactly what we find. These results indicate that people are receiving more targeted liquidity from friends and family during income shocks and this is especially true for people who are more likely to receive these transfers during periods of low liquidity.

This alignment of income to expenditures underscores the need for funds to be received promptly, especially for people who have very little liquid balances and cash buffers. But, are we necessarily capturing friends & family money or some sort of informal income due to consumers increasing labor supply after the shocks? We provide several pieces of evidence that our results primarily reflect the effects of friends and family transfers. First, both the incidence of friends & family transfers and transfer–expense matching increase sharply at the time of income shocks. It would be more difficult to increase labor supply sharply and align it as well with income shocks as we document. Second, it is less plausible that individuals – and especially constrained hand-to-mouth consumers – can match the timing and amount of informal income to expenses very well.

We provide additional evidence to support the latter point by examining *exact* matching of transfers to expenses around income shocks, where we define exact transfer–expense matching as a friends & family transfer being followed by an expense of almost exactly the same amount[21]. The results of this robustness test reported in Appendix Table A.I show that exact matching increases for liquidity constrained consumers after both exogenous and endogenous income shocks. These results again speak to potential presence of moral hazard in friends & family money markets, in particular during income shocks when stakes are particularly

[21] Where almost is defined as between 95 and 100% of the outgoing expenditure

high. For example, Parsons and Van Wesep (2013) show that when people have self-control problems, matching income to expenditures can be welfare improving. Additionally, requests for amount-matched transfers may increase the likelihood of receiving the transfer in the first place. Targeted amounts might be easier to obtain from social networks if earmarked for a particular purpose, as a result of an interaction of social norms and mental accounting type decision making (e.g., Zelizer, 2012). Self-control problems could then necessitate the alignment of friends & family transfers with the timing and amount of expenditures, so that the recipient is better positioned to use the funds for the designated purpose.

IV. Peer-to-peer payment apps and income timing

In this section, we investigate how peer-to-peer (P2P) app-based transfers, whereby funds are sent and received instantaneously at very low to no cost, affect the timing of friends & family transfers relative to important expenditures. Recognizing that P2P payments app user status – which represents both access to the apps and take-up – may be correlated with unobservable user characteristics (e.g., financial sophistication, tech "savviness," bank-consumer or consumer-city sorting), we exploit exogenous variation in access to P2P payments apps through *Zelle Exposure* at the city of close social circle.

A. *Instrument for P2P app-based payments*

To construct our instrument, we make use of two key features of P2P payments apps. First, P2P apps are network goods because the ability to send or receive money via an app is a function of whether consumers on the other side of the transaction also use the same app. Second, the use of one P2P payments app can have spillovers to the use of other apps. For example, when consumers are exposed to Zelle, they may be more likely to also start using other apps such as Venmo and CashApp. Third, the likelihood of using Zelle specifically is a function of whether an individual's bank offers Zelle (see Section I). Combining these points, a consumer should be more likely to use Zelle through network effects when their friends and family are more exposed to branches of banks that offer Zelle. Hence, we use the exposure of a consumer's social circle to Zelle as an instrument for consumer's own access to P2P payments apps. Empirically, this instrument allows us to include both person and city of residence by time fixed effects, and hence absorb not only time-invariant consumer characteristics but also time-varying local factors that might impact consumer decisions.

For Zelle exposure to be a valid instrument, it has to be relevant to consumers' use of Zelle and P2P payments app more broadly. We document this relevance empirically with a strong first stage in our weekly and monthly and data sets in Table V. The results show that bank adoption of Zelle at the city of consumers' social circle is positively and significantly correlated with consumers' own use of Zelle (Columns (1)–(2)) and their use of other P2P payments apps (Columns (3)–(4)). In particular, a one standard deviation increase in *Zelle Exposure* is associated with a 14.5% increase in the likelihood of Zelle use and an 5% increase in all P2P payments systems use in any given week in the government shutdown sample (Panel A). In the full monthly sample, a 1 standard deviation increase in the city of consumers' social circle exposure to Zelle partnerships is associated with a 10% increase in the likelihood of Zelle use and an 3% increase in the use of all P2P systems.

These results are robust to constraining the distance between the city of residence and the city of social circle to the interquartile range for each sample. The effects are similar for liquidity constrained users (Appendix Table A.II).[22] In Appendix Table A.III, we zoom in on periods of the shocks themselves (i.e., post-shutdown weeks and income loss months) and find even somewhat stronger positive relation between Zelle exposure and the use of P2P app-based transfers.

Another identifying assumption is that bank adoption of Zelle is uncorrelated with unobserved consumer, local, or macroeconomic variables that are possibly time varying. In other words, bank adoption of Zelle should affect consumer outcomes only through P2P payments app use and not because Zelle adoption by banks is correlated with omitted variables that are also affecting consumer outcomes. We argue that this is likely true because the decision of banks to partner with Zelle is a choice made at the bank level that occurred in a staggered fashion through time and is hence likely uncorrelated with individual time-varying user characteristics and local characteristics, especially when Zelle exposure is defined at the city of social circle. See Section I.B.4 for additional discussion and tests.

B. *P2P app transfer exposure and transfer–expense matching*

Embracing the quasi-exogenous nature of the variation in Zelle exposure at the location of close social circle, we assess to what extent P2P transfer app technology affects the likelihood of matching incoming friends & family transfers to expenditures during periods of liquidity stress. We first estimate reduced-form regressions with triple interaction terms at the weekly level for the sub-population of federal employees around the government shutdown of 2018/2019. The regressions specification is as follows:

[22] These results are also robust to alternative definitions of instruments using branch deposits rather than the number of bank branches, restricting Zelle partnership data to only Zelle adoption by big banks, and restricting the sample to only cities with at least one Zelle-adopting bank (not reported).

$$y_{it} = \alpha_i + \zeta_{ct} + \beta Post_t \times T_i \times Zelle\ Exposure_i + \varepsilon_{it} \tag{3}$$

where $Post_t$ is a dummy variable taking a value of one for the first three weeks in January 2019 and zero otherwise, T_i is an indicator for federal employees who lost income during the shutdown, $Zelle\ Exposure_i$ is close circle exposure to Zelle–bank partnerships for person i as of December 1, 2018. As above, the coefficients α_i and ζ_{ct} represent person and city of residence by calendar week fixed effects, respectively.

We estimate Equation (3) for the full sample of federal employees, the liquidity constrained users, and the constrained users at the time of housing payments. We report the results in Table **??**. We do not find strong evidence that instrumented P2P transfer access is associated with differences in the likelihood of transfer–expense matching during the shutdown when we consider all expenses (Columns (1)–(2) and (4)–(5)). However, when we subset our analysis to constrained consumers, we find that instrumented P2P transfer access is associated with an increase in the likelihood of matched transfers specifically at the time of large consumption commitments such as housing payments (Columns (3) and (6)).

We also analyse the broader population with monthly data. We estimate the following regression where we interact income loss dummy with time-varying Zelle exposure:

$$y_{it} = \alpha_i + \zeta_{ct} + \gamma Income\ Loss_{it} \times Zelle\ Exposure_{it} + \varepsilon_{it} \tag{4}$$

where $Income\ Loss_{it}$ is defined in Equation (2) and $Zelle\ Exposure_{it}$ is the average social circle exposure to Zelle for person i over the four months prior to income loss. Coefficients α_i and ζ_{ct} represent person and city of residence by calendar month fixed effects, respectively.

Table VII shows that in the full sample, instrumented access to P2P app-based transfers is indeed associated with higher likelihood of matched transfers, especially for hand-to-mouth consumers who are likely constrained. One possible explanation for differences in results between the government shutdown sample and the full monthly sample is that the larger sample size allows us to more precisely estimate the effects of P2P payments apps on the timing of friends & family transfers relative to expenditures. On the other hand, the higher frequency of our weekly data set allows us to more precisely pin-point the timing of matching in specific circumstances, such as housing payments by liquidity constrained consumers.

V. P2P payments, transfer–expense matching, and consumer outcomes during income shocks

In this section, we examine how P2P payments affect consumer outcomes such as low liquidity fees *during* income shocks when constraints are more likely to bind, especially for people who have very little liquid buffers. If individuals rely on P2P transfers during periods of financial fragility, and these transfers are timed to match outgoing expenditures, then access to real-time payments may have material consequences.

A. *P2P transfer exposure and matching during negative shocks*

We start by replicating the results on the relevance of Zelle exposure to transfer–expense matching documented in Tables VI and VII in situations when constraints likely bind. In line with this focus, we restrict our analysis to constrained consumers at the time of negative income shocks who lost income and received friends & family transfer at some point during these shocks.

Given much smaller sample sizes and short time periods in this empirical exercise, do not include person fixed effects in our regressions. Thus, the sources of variation we exploit are mostly cross-sectional in nature. We use the following regression specification, for both weekly and monthly data:

$$y_{it} = \zeta_c + \beta \mathit{Zelle\ Exposure}_i + \varepsilon_{it} \tag{5}$$

where $\mathit{Zelle\ Exposure}_i$ is exposure to Zelle–bank partnerships at the city of social circle, y_{it} is the use of Zelle or use of any P2P transfer apps, and coefficient ζ_c represents city of residence fixed effects.

We document similarly strong results in Table VIII. Zelle exposure at the location of consumers' social circle strongly predicts transfer-expense matching for constrained consumers regardless of the distance of close social circle in all but one regression specification.

B. *P2P payments app access and risk sharing*

We next assess to what extent access to P2P transfer systems impacts the amount and number of transfers received around income shocks. If the use of peer to peer transfers systems significantly lowers direct and indirect transactions costs of transferring funds, then access to these systems would increase the likelihood of receiving smaller transfers more frequently and receiving transfers from a broader network of friends and family. Access to these systems could in turn improve risk sharing amongst social groups, consistent with evidence on the introduction of M-Pesa in Kenya in Jack and Suri (2014).

While households can be more likely to receive smaller money transfers from a more diverse pool of senders due to reduction in high transaction costs in developing countries (see, e.g., Jack and Suri, 2014; Suri and Jack, 2016; Sy, Maino, Massara, Saiz, and Sharma, 2019; Suri, 2021), it is unclear if reduction in transactions costs would materially affect consumers in a highly developed financial system such as in the U.S., where transaction costs are usually lower. We test this hypothesis by estimating Equation 5 using the dollar amount and count of friends and family transfers as the left hand side variable.

Results for the dollar amount of friends & family transfers reported in Table IX Panel A are mixed. We do not find conclusive evidence that access to P2P transfer apps as proxied by Zelle exposure is associated with smaller amounts of friends & family transfers received more frequently during periods of liquidity stress in the U.S. We estimate a negative coefficient in Column (1) and a positive coefficient in Column (2). In both cases, the coefficients are noisy and small in magnitude. Likewise, we find no evidence that access to P2P transfer apps is associated with greater count of friends & family transfers for the government shutdown sample (Column (3)–(4)).

We find some statistically significant results in the larger monthly sample, reported in Panel B of Table IX. Similar to Panel A, access to P2P payment apps is not associated with smaller amounts of funds received from friends and family during periods of income loss. However, we do find some evidence of more frequent friends & family transfers in this sample. Overall, these results are not entirely consistent with access to P2P payment apps facilitating risk sharing, although some results are suggestive that this channel may also operate in developed countries like the U.S. and not just in developing countries like Kenya.

C. *P2P payment app access and low liquidity fees*

Our evidence above consistently suggests that P2P payment app access impacts the timing of receipt of transfers, particularly in times of need. Specifically, for hand-to-mouth consumers who are often liquidity constrained, P2P payment app access is associated with an increased likelihood of receiving transfers in the immediate window prior to large expenses during income shocks, which could otherwise push account balances below zero. We proceed by examining the effect that P2P transfer app technology may have on other consumer outcomes, such as the likelihood of incurring low liquidity (e.g., low balance, overdraft, non-sufficient funds) fees.

Given our earlier findings that friends & family transfers are timed to arrive in the immediate vicinity of expenditures, and access to P2P payment apps facilitates this matching, one should expect to also observe reduced incidence of low liquidity fees when consumers have the better access to these apps at critical times such as during income shocks. Such outcomes would be consistent with both lower uncertainty around when the funds will clear due to real-time nature of P2P app-based transfers and receipt of smaller more frequent transfers that can be more tailored to specific expenditures due to improved risk sharing. Additionally, better transfer–expense matching facilitated by P2P transfer apps could prevent negative knock-on consequences of operating bank accounts close to zero.

To test this hypothesis, we first estimate a reduced-form regression of low liquidity fees on Zelle exposure at the location of close social circle at the time of income loss. As before, we subset our analysis to hand-to-mouth consumers who lost income and received friends & family transfers during these times, because timing is likely to matter most for these consumers in time of need. The regression specifications are the same as in Equation 5, but with the likelihood of incurring low balance, overdraft, and non-sufficient funds (NSF) fess as outcomes.

The results for government shutdown sample reported in Table X Panel A show that better access to P2P payment apps as proxied by Zelle exposure at city of social circle is negatively associated with the likelihood of incurring low liquidity fees, except for NSF fees. In Panel B of Table X, we find that social circle exposure to Zelle partnerships is associated with lower likelihood of incurring all three types of low liquidity fees in our full monthly sample. Our point estimates are generally similar across both sets of analyses. The results across both panels imply that a one standard deviation increase in *Zelle Exposure* (0.23) reduces the likelihood of incurring low liquidity fees by around 20% at times of income loss, which is a sizeable effect. These estimates represent an intention-to-treat (ITT) effect, or put differently capture the effect of access to peer-to-peer payment apps on low balance fee outcomes rather than use of the apps themselves.

D. P2P payment app use and low liquidity fees

Finally, we assess to what extent take-up (i.e., actual use) of P2P payment apps is correlated with the likelihood of incurring low liquidity fees, and specifically through their impact on matching. We perform this analysis in two ways. First, we regress the incidence of low balance, overdraft, and NSF fees directly on the use of P2P app-based transfers. Second, we estimate 2-stage least squares (2SLS) regressions, where the first stage regresses a dummy for transfer–expense matching on P2P transfer app use and the second stage regresses low balance fees, overdraft fees, and NSF fees on predicted matching from the first stage. This specification aids us in establishing the empirical link between access to P2P payment apps, their impact on income timing, and ultimately on the incidence of overdraft and NSF fees.[23] As before, we perform this analysis for the sub-sample of constrained consumers who lost their income and received friends & family transfers during income shocks.

We begin this set of tests by exploiting our weekly data for constrained federal employees who lost income during the 2018/19 shutdown in graphical analysis. The granularity of these data enables us to examine the dynamic weekly relation between P2P app-based transfers and low liquidity fees over various time lags using a distributed lag model. This specification helps us capture possible delayed or knock-on effects of changes in the use of P2P transfer systems on the likelihood of incurring these high-cost fees, without enforcing that the relation is contemporaneous. Specifically, we estimate the following cross-sectional specification for the government shutdown sample:

$$y_{it} = \zeta_c + \sum_{k=0}^{5} \beta_k \mathit{P2P\ Transfer\ App\ Use}_{i,t-k} + \varepsilon_{it} \tag{6}$$

where $\mathit{P2P\ Transfer\ App\ Use}_{i,t}$ is a dummy variable taking a value of 1 if the person received a transfer via peer-to-peer payment app and ζ_c represents city of residence fixed effects. Of note, we do not estimate the dynamic effects based on our monthly representative sample because of the less granular nature of these data.

We report the linear sum of the coefficients from Equation (6) as black lines in Figure 4, which captures the cumulative effect of receiving a transfer via P2P payment apps for constrained users of friends & family transfers during the government shutdown. This figure captures the dynamic relationship between P2P payment app use and the incidence of low liquidity fees. We find that if a friends & family transfer was received via a P2P payment app, the consumer is approximately 50% less likely to incur a low liquidity fee in the following weeks. The effect is not immediate, indicating that the timely receipt of transfers prevents cascading negative knock-on effects at times when account balances are likely close to zero.

We then assess to what extent transfers via P2P payment systems prevent low liquidity fees such as overdraft fees through their impact on income timing by estimating Equation 6 in a two-step process. Specifically, we estimate how the variation in matching driven by the variation in P2P payment app use affects the likelihood of incurring low liquidity fees. The

[23] We recognize that P2P payment app user status is likely correlated with user characteristics that could in turn could affect the ability to time income to expenditures and avoid low liquidity fees. However, we do not have enough time variation in *Zelle Exposure* neither at the monthly nor at the weekly level to instrument for P2P payment app use or transfer–expense matching in similar 2SLS regressions. We thus reply on the preponderance of evidence in the totality of our analysis to argue for the causal chain from P2P transfer app access to take-up and transfer–expense matching and finally to the incidence of low liquidity fees.

gray lines on Figure 4 show that variation in matching that stems from P2P payment app use *does* reduce the likelihood of these fees. It is noteworthy that this line is very close to the black line representing reduced-form estimates of the impact of P2P payment app use, which suggests that transfer–expense matching is the first-order mechanism behind the reduction in high-cost fees associated with low liquidity due to P2P app-based transfers.

We confirm these relations in regression analysis presented in Table XI, which estimate the effects of receiving friends & family transfers through P2P payment apps over the full income loss period, for both government shutdown sample and the full monthly sample. Panel A reports the results of the reduced-form analysis, where we directly regress low liquidity fees on the use of P2P app-based transfers. Panel B reports the results from the second stage of the 2SLS estimation, where we regress low liquidity fees on predicted transfer–expense matching. The results of regressing matching on P2P payment app use are included in Appendix Table A.IV.

Columns (1)–(3) of Table XI report the regression estimates for the government shutdown sample. These results closely mirror estimates in Figure 4. The receipt of friends & family transfers via P2P payment apps by constrained individuals is associated with reduced likelihood of incurring low balance, overdraft, and NSF fees (Panel A). The variation in transfer–expense matching driven by the use of P2P payment apps is negatively correlated with the incidence of these low liquidity fees (Panel B). We confirm these results in the broader monthly sample in Columns (4)–(6). We find strong negative effects of P2P transfer app use on low liquidity fees, in particular through the transfer–expense matching channel.

In totality, the results presented in this section are consistent with transfer–expense matching being the first-order channel behind the ability of P2P transfer technology to reduce the likelihood of costly low balance fees, which can also have significant knock-on effects. Our findings speak to the recent policy debate on expending real-time transfers in the U.S. The above results suggest that real-time transfers have positive outcomes for consumers through their impact on the timing of the receipt of income relative to expenditures.

VI. Concluding Remarks

Our research offers novel insights into the functioning of informal social insurance mechanisms through P2P transfers, particularly in the context of a highly developed financial system like that of the U.S. We provide empirical evidence of how individuals rely on informal social insurance from friends and family during periods of financial duress, and align incoming transfers with key outgoing expenditures. This behavior not only reflects the adaptive strategies employed in the face of economic hardship, but also highlights a potential mechanism for mitigating moral hazard – a concept central to the discussion of traditional insurance markets.

In traditional insurance markets, moral hazard refers to the risk that the insured party may alter their behavior when they are protected from the costs of that behavior, as described in Arrow (1963)'s seminal paper on the welfare economics of medical care. This change in behavior can occur because the individual does not bear the full cost of their actions, leading to potential inefficiencies, increased costs within the insurance market or the breakdown of the market entirely. This is particularly problematic in ex-post scenarios, where after the realization of uncertainty, individuals may exhibit behaviors that could lead to exaggerated claims or misuse of funds, since the insurer cannot perfectly observe how the funds are spent.

In contrast, the informal social insurance observed in our study operates differently. We document that transfers from friends and family often occur close to the time of actual expenditures, which could indicate an evolved system designed to overcome this ex-post moral hazard. By timing the transfers to coincide with the expenditures they are meant to cover, the likelihood of the funds being diverted to other uses is reduced. This mechanism is consistent with a form of insurance that has adapted to the limitations of observation and control inherent in more formal arrangements.

Importantly, we find that this pattern of matching inflows with outflows is amplified during income shocks: when faced with low liquidity, individuals align transfers with their spending needs more, indicating that the timing of receipt of income might matter for other consumer outcomes during income shocks. We argue that these settings of low liquidity, either endogenously determined or due to external shocks such as the Federal Government Shutdown of 2018/19, provide an ideal setting, in which to study whether the timing of receipt of funds matters for people living close to zero liquid funds. Put differently, by focusing on periods of income shocks, and more specifically on the exact timing of friends & family transfers, we are able to piece together the impact real-time low-cost payments technology has on other consumer outcomes.

We find that access to P2P payment apps that facilitate instantaneous transfers at very low to no cost enhances the synchronization of income with expenditures. This alignment could be due to the fact that the instantaneous nature of the transfer reduces the uncertainty in the timing of receiving the income or because the low cost of the transfer enables smaller more frequent transfers that are better matched to outgoing expenditures. While we find evidence of both channels, with more support for the reduction in the uncertainty of timing mechanism, we find that the ability to receive more targeted funds in a timely manner diminishes the probability of subsequent financial difficulties, such as low balance, overdraft, and NSF fees.

These findings contribute meaningfully to the ongoing policy discussion regarding the benefits of real-time payments, especially for individuals operating with thin financial margins who are most vulnerable to income volatility. Prior to this study, the potential impact of real-time payments was largely speculative. Our work provides empirical evidence to inform this debate, highlighting the importance of timing in the receipt of funds for those navigating financial vulnerabilities.

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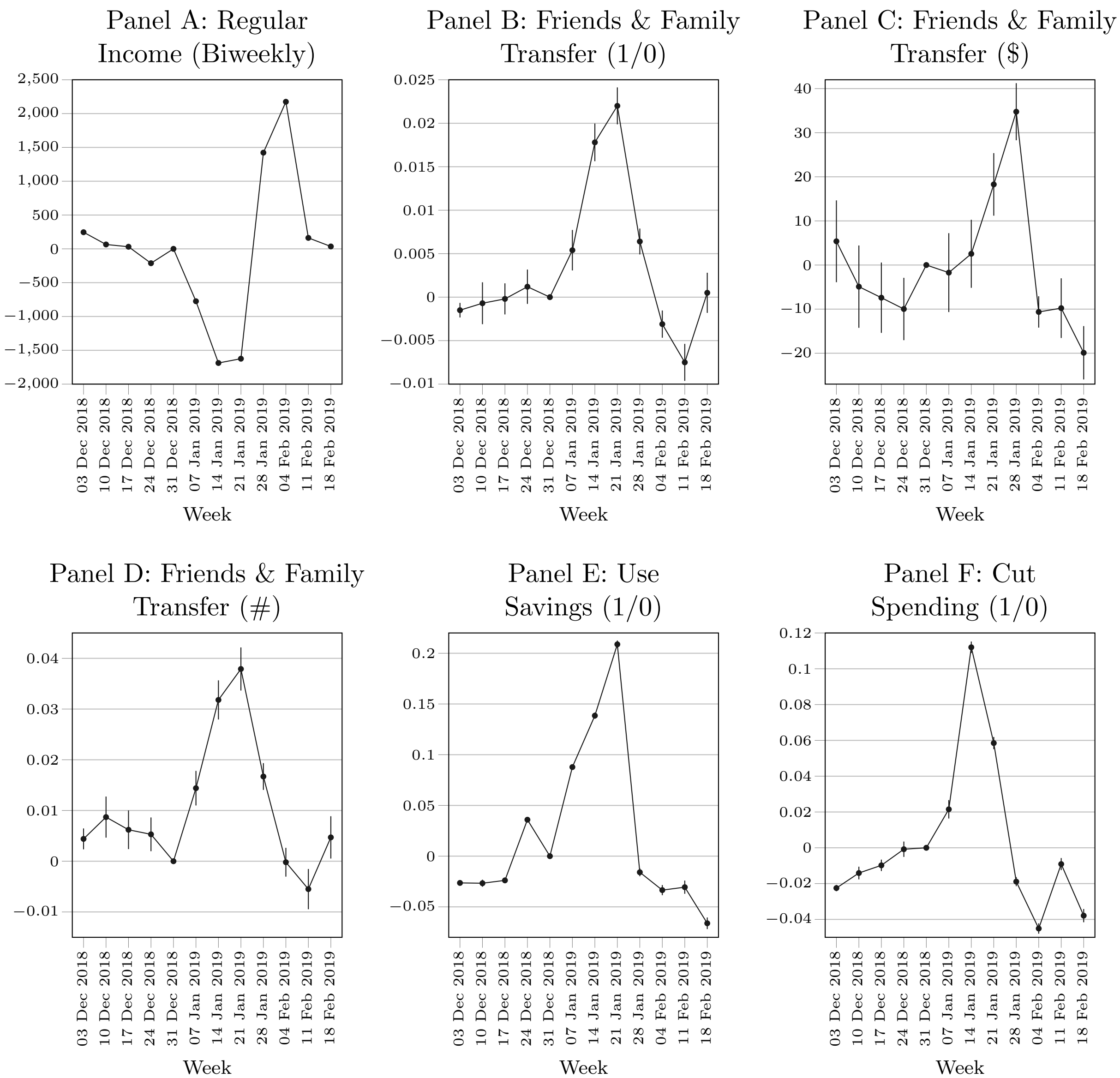


Figure 1. Dynamics of coping with income shocks – government shutdown. This figure reports the coefficients and their confidence intervals from regressing various methods of coping with negative income shocks on week dummies for a window of Week −5 to Week +6 (i.e., December 3, 2018 to February 24, 2019) around the onset of the Federal Government Shutdown of 2018/19. We report the dynamics of regular income around the shock in Panel A. We then report the evaluation of friends and family money transfers (likelihood of receiving a transfer, dollar amount received, and number of transfers) in Panels B–D. Lastly, we examine the likelihood of drawing down on liquid savings or cutting total spending in Panels E and F.

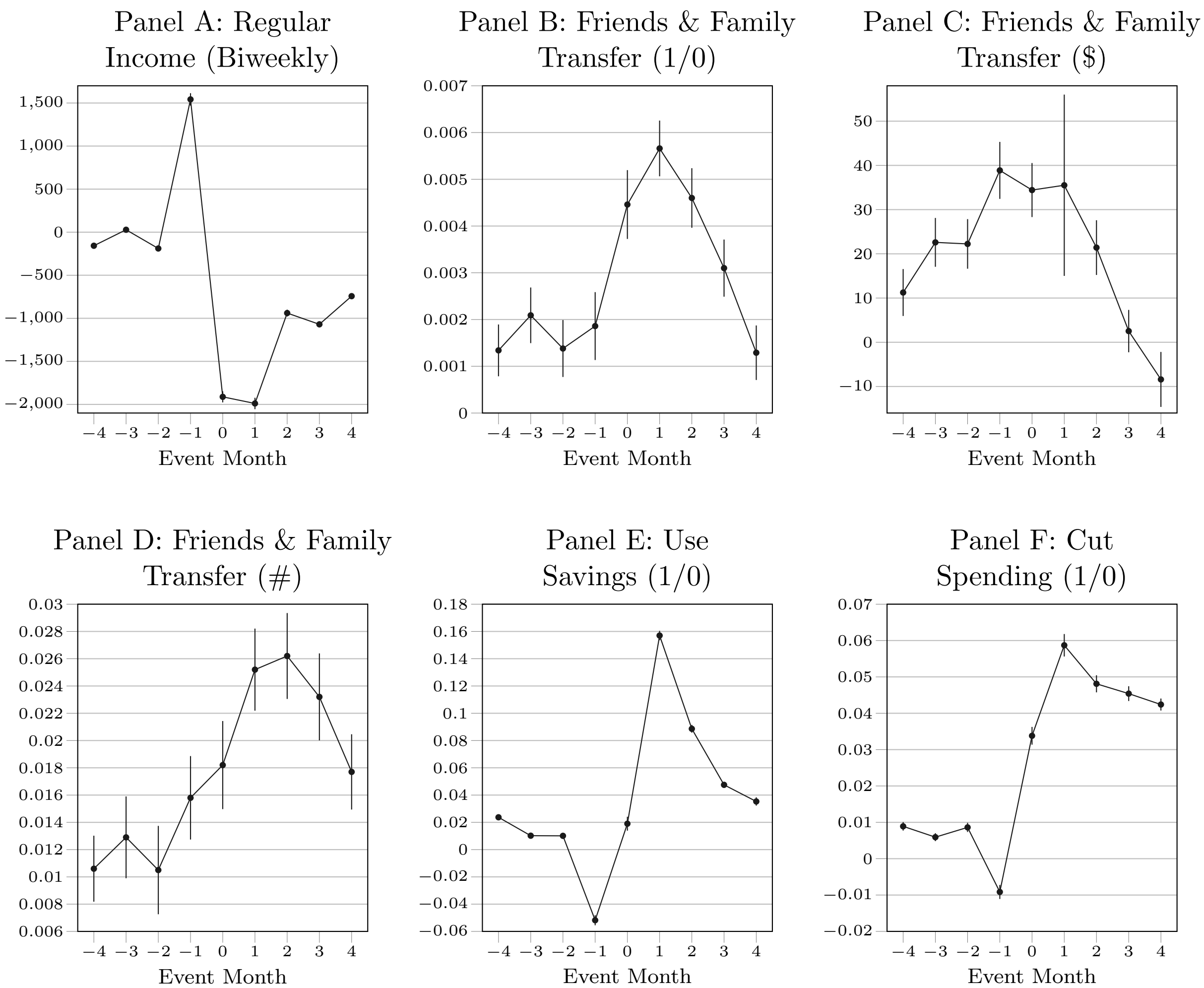


Figure 2. Dynamics of coping with income shocks – full sample. This figure reports the coefficients and their confidence intervals from regressing various methods of coping with negative income shocks on month dummies for a window of Month −4 to Month +4 around income loss for the full representative sample. We define income loss event as a month when a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months (Event Month = 0). We report the dynamics of regular income around the shock in Panel A. We then report the evaluation of friends and family money transfers (likelihood of receiving a transfer, dollar amount received, and number of transfers) in Panels B–D. Lastly, we examine the likelihood of drawing down on liquid savings or cutting total spending in Panels E and F.

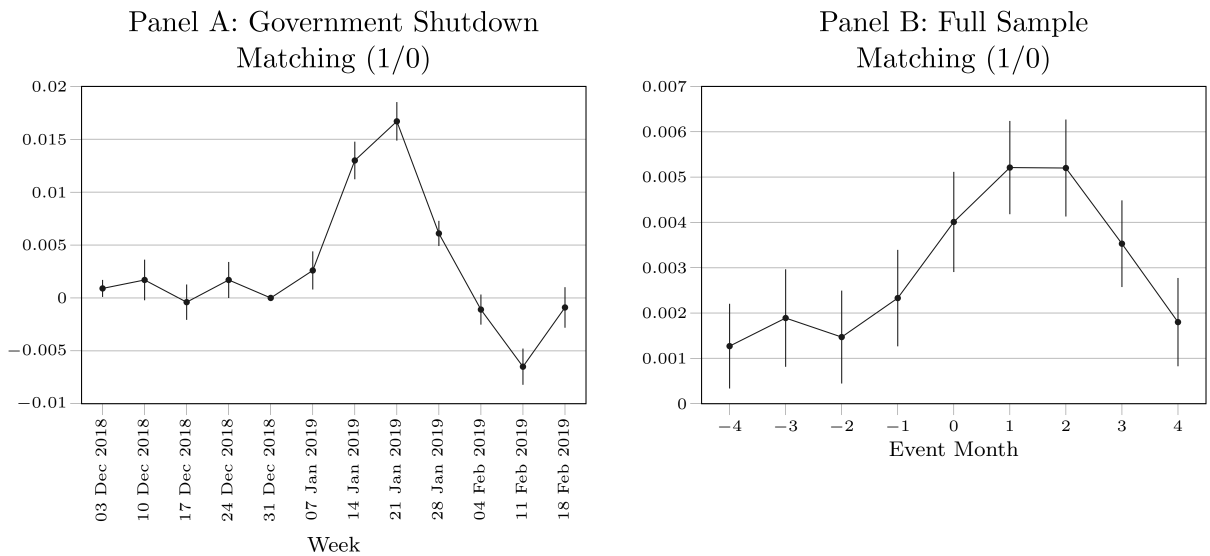


Figure 3. Dynamics of transfer–expense matching around income shocks. This figure reports the coefficients and their confidence intervals from regressing a dummy for matching of transfers to expenses on time dummies for two samples. Panel A reports the results for a window of Week −5 to Week +6 around the onset of the Federal Government Shutdown of 2018/19. Panel B reports the results for a window of Month −4 to Month +4 around income loss for the full representative sample. We define income loss event as a month when a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months (Event Month = 0). *Matching* is a dummy for a friends & family transfer occurring within 3 days of an expense.

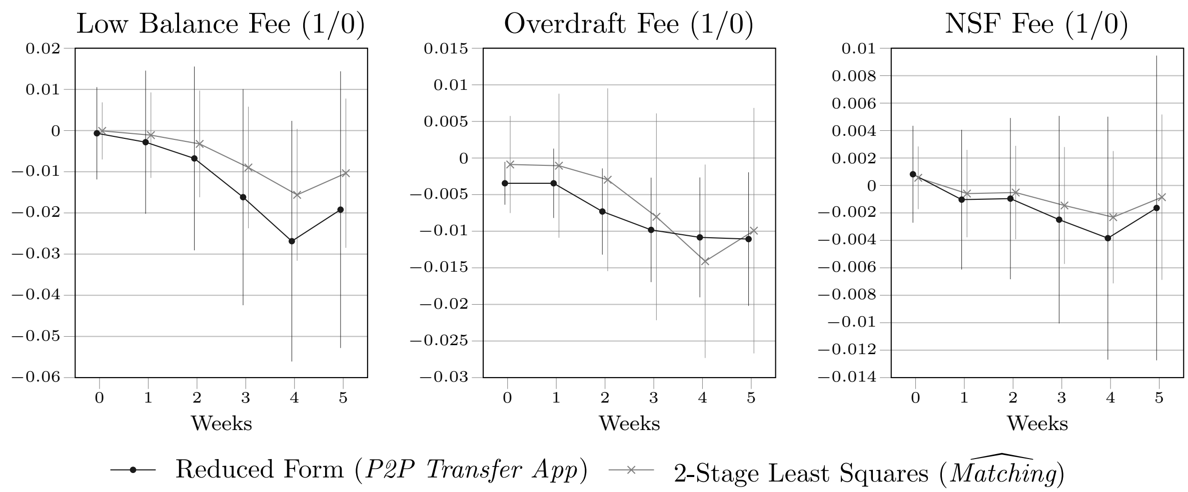


Figure 4. Cumulative dynamics of low-liquidity fees post government shutdown. This figure reports the coefficients and their confidence intervals from estimating a distributed lag model with dummies for low balance fees, overdraft fees, and non-sufficient funds (NSF) fees as outcomes during the first six weeks of the Federal Government Shutdown of 2018/19. The sample is liquidity constrained federal employees who missed a paycheck during the Federal Government Shutdown of 2018/19 and received friends & family transfers at some point during this negative income shock. The black lines correspond to the results from a reduced-form model, where we regress the outcomes variables directly on *P2P Transfer App*, which is a dummy for consumer receiving a friends & family transfer through a P2P transfer app. The gray lines correspond to the results from a 2-stage least squares model, where the first stage regresses a dummy for transfer–expense matching on P2P transfer app use and the second stage regresses the outcomes on predicted matching from the first stage. *Matching* is a dummy for a friends & family transfer occurring within 3 days of an expense.

Table I. Summary Statistics

		Monthly Representative Sample		Federal Government Employees Sample		
		All Users	Liquidity Constrained	All Employees	Employees with Lost Income	Constrained Employees with Lost Income
		(1)	(2)	(3)	(4)	(5)
Panel A: Mean						
Friends & Family Transfer	(1/0)	32.3%	26.7%	29.7%	30.4%	27.1%
Friends & Family Transfer	($)	673	207	513	588	159
Friends & Family Transfer	(#)	0.89	0.68	0.78	0.79	0.65
Use Savings	(1/0)	64.5%	55.4%	49.3%	51.3%	47.8%
Cut Spending	(1/0)	41.0%	37.0%	41.6%	41.5%	36.8%
Low Balance Fee	(1/0)	4.2%	4.3%	3.4%	3.4%	5.9%
Overdraft Fee	(1/0)	3.9%	4.1%	3.3%	3.3%	5.8%
NSF Fee	(1/0)	0.5%	0.4%	0.2%	0.3%	0.3%
Liquidity Constrained	(1/0)	31.5%	100.0%	23.0%	19.3%	100.0%
Matching	(1/0)	22.9%	21.3%	20.7%	20.6%	22.7%
Panel B: Median						
Regular Income	($)	3,261	1,955	5,400	6,288	2,763
Total Income	($)	4,562	2,284	6,786	7,645	3,106
Total Spending	($)	4,098	1,989	5,737	6,108	3,034
N		196,331	61,918	75,077	28,149	17,260

This table summarizes our analysis samples: full monthly representative sample, monthly sample subsetted to liquidity constrained users, weekly sample of federal government employees, a subsample of federal employees who lost income during the 2018/2019 government shutdown, and a subsample of liquidity constrained federal employees who lost income during the shutdown. Panel A reports means of friends % family transfer measures and liquidity measures (e.g., incidence of overdrafts, transfer-expense matching). Panel B provides median income and spending, and the bottom row provides the number of observations in each sample. Definitions of variables are provided in Internet Appendix Section IA.VI.

Table II. Coping with Negative Income Shocks

	Friends & Family Transfer (1/0)	Friends & Family Transfer ($)	Friends & Family Transfer (#)	Use Savings (1/0)	Cut Spending (1/0)
	(1)	(2)	(3)	(4)	(5)
Panel A: Government Shutdown					
Post × T	0.0156***	8.800	0.0235***	0.166***	0.0816***
	(0.00156)	(6.326)	(0.00272)	(0.00251)	(0.00244)
Person Fixed Effects	Y	Y	Y	Y	Y
City of Res × Week Fixed Effects	Y	Y	Y	Y	Y
Observations	889,960	889,960	889,960	889,960	889,960
R-squared	0.258	0.126	0.327	0.432	0.110
Panel B: Full Sample					
Income Loss	0.00742***	35.11***	0.0324***	0.0798***	0.0402***
	(0.000503)	(10.43)	(0.00235)	(0.00406)	(0.00232)
Person Fixed Effects	Y	Y	Y	Y	Y
City of Res × Month Fixed Effects	Y	Y	Y	Y	Y
Observations	14,088,259	14,088,259	14,088,259	14,088,259	14,088,259
R-squared	0.364	0.230	0.453	0.330	0.134

This table reports the results of OLS regressions for methods of coping with negative income shocks. Specifically, we examine friends and family money transfers (likelihood of receiving a transfer, dollar amount received, and number of transfers) and the likelihood of drawing down on liquid savings or cutting total spending. Panel A reports the results for a window of Week −5 to Week +6 (i.e., December 3, 2018 to February 24, 2019) around the onset of the Federal Government Shutdown of 2018/19. *Post* is a dummy variable that takes the value of 1 for the first 3 weeks in January 2019 and 0 for the remaining weeks. T takes the value of 1 for employees who earned federal income in December 2018 but missed at least one paycheck during the shutdown and 0 for employees who earned federal income in December 2018 and did not miss any paycheck during the shutdown. Panel B reports the results for the full sample. *Income Loss* is a dummy variable that takes the value of 1 if a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months and the value of 0 otherwise. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are double-clustered at the person and time levels, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table III. Coping with Negative Income Shocks & Liquidity Constraints

	Friends & Family Transfer (1/0)	Friends & Family Transfer ($)	Friends & Family Transfer (#)	Use Savings (1/0)	Cut Spending (1/0)
	(1)	(2)	(3)	(4)	(5)
Panel A: Government Shutdown					
Post × T	0.0138***	10.69	0.0208***	0.152***	0.0765***
	(0.00174)	(7.730)	(0.00308)	(0.00276)	(0.00275)
Post × T × Liquidity Constrained	0.00996***	-5.936	0.0152**	0.0572***	0.0267***
	(0.00376)	(9.448)	(0.00630)	(0.00634)	(0.00573)
Post × Liquidity Constrained	0.00299	14.65**	0.00483	-0.0470***	-0.000353
	(0.00196)	(6.223)	(0.00332)	(0.00342)	(0.00317)
Person Fixed Effects	Y	Y	Y	Y	Y
City of Res × Week Fixed Effects	Y	Y	Y	Y	Y
Observations	889,960	889,960	889,960	889,960	889,960
R-squared	0.258	0.126	0.327	0.431	0.110
Panel B: Full Sample					
Income Loss	0.00639***	41.88***	0.0284***	0.0771***	0.0351***
	(0.000587)	(14.26)	(0.00246)	(0.00356)	(0.00205)
Income Loss × Liquidity Constrained	0.00377***	-24.75	0.0145***	0.00975***	0.0189***
	(0.000978)	(15.19)	(0.00425)	(0.00275)	(0.00188)
Person Fixed Effects	Y	Y	Y	Y	Y
City of Res × Month Fixed Effects	Y	Y	Y	Y	Y
Observations	14,088,259	14,088,259	14,088,259	14,088,259	14,088,259
R-squared	0.364	0.230	0.453	0.330	0.134

This table reports the results of OLS regressions for methods of coping with negative income shocks for liquidity constrained vs. unconstrained consumers. Specifically, we examine friends and family money transfers (likelihood of receiving a transfer, dollar amount received, and number of transfers) and the likelihood of drawing down on liquid savings or cutting total spending. Panel A reports the results for a window of Week -5 to Week $+6$ (i.e., December 3, 2018 to February 24, 2019) around the onset of the Federal Government Shutdown of 2018/19. *Post* is a dummy variable that takes the value of 1 for the first 3 weeks in January 2019 and 0 for the remaining weeks. T takes the value of 1 for employees who earned federal income in December 2018 but missed at least one paycheck during the shutdown and 0 for employees who earned federal income in December 2018 and did not miss any paycheck during the shutdown. Panel B reports the results for the full sample. *Income Loss* is a dummy variable that takes the value of 1 if a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months and the value of 0 otherwise. *Liquidity Constrained* consumers are individuals identified as likely living hand-to-mouth. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are double-clustered at the person and time levels, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table IV. Transfer–Expense Matching around Negative Income Shocks

Dependent Variable =	Matching (1/0)			
	Unconditional		Conditional on Transfer	
	(1)	(2)	(3)	(4)
Panel A: Government Shutdown				
Post × T	0.0106***	0.00822***	0.0199***	0.0162***
	(0.00131)	(0.00143)	(0.00262)	(0.00281)
Post × T × Liquidity Constrained		0.0127***		0.0217***
		(0.00335)		(0.00706)
Post × Liquidity Constrained		0.00162		0.00368
		(0.00175)		(0.00386)
Person Fixed Effects	Y	Y	Y	Y
City of Res × Week Fixed Effects	Y	Y	Y	Y
Observations	889,960	889,960	417,146	417,146
R-squared	0.261	0.261	0.205	0.205
Panel B: Full Sample				
Income Loss	0.00634***	0.00572***	0.00178***	0.00100*
	(0.000369)	(0.000390)	(0.000527)	(0.000547)
Income Loss × Liquidity Constrained		0.00226***		0.00329***
		(0.000679)		(0.00112)
Person Fixed Effects	Y	Y	Y	Y
City of Res × Month Fixed Effects	Y	Y	Y	Y
Observations	14,088,259	14,088,259	4,240,171	4,240,171
R-squared	0.399	0.399	0.665	0.665

This table reports the results of OLS regressions with matching of transfers to expenses as the outcome, unconditionally (Columns (1)–(2)) and conditionally on friends & family transfer (Columns (3)–(4)). Panel A reports the results for a window of Week −5 to Week +6 around the onset of the Federal Government Shutdown of 2018/19. *Post* is a dummy variable that takes the value of 1 for the first 3 weeks in January 2019 and 0 for the remaining weeks. T takes the value of 1 for employees who earned federal income in December 2018 but missed at least one paycheck during the shutdown and 0 for employees who earned federal income in December 2018 and did not miss any paycheck during the shutdown. Panel B reports the results for the full sample. *Income Loss* is a dummy variable that takes the value of 1 if a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months and the value of 0 otherwise. *Liquidity Constrained* consumers are individuals identified as likely living hand-to-mouth. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are double-clustered at the person and time levels, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table V. Zelle Exposure and P2P Transfer Apps Use

Dependent Variable =	Zelle Use (1/0)		P2P Transfer Apps Use (1/0)	
	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}
	(1)	(2)	(3)	(4)
Panel A: Government Shutdown				
Zelle Exposure	0.00315***	0.00599***	0.0141***	0.00932***
	(0.000664)	(0.00105)	(0.00152)	(0.00240)
Person Fixed Effects	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y
Observations	898,838	443,523	898,838	443,523
R-squared	0.0214	0.0288	0.0244	0.0360
F-Stat	22.58	32.72	86.00	15.01
Panel B: Full Sample				
Zelle Exposure	0.0245***	0.0226***	0.0161***	0.0180***
	(0.00320)	(0.00432)	(0.00382)	(0.00572)
Person Fixed Effects	Y	Y	Y	Y
City of Res × Month Fixed Effects	Y	Y	Y	Y
Observations	14,088,259	6,980,357	14,088,259	6,980,357
R-squared	0.362	0.367	0.455	0.455
F-Stat	58.80	27.33	17.83	9.936

This table reports the results of OLS regressions with dummies for the use of Zelle and P2P transfer apps as outcomes as a function of Zelle exposure at the city of social circle, for all users in the sample. Panel A reports the results for a window of Week −5 to Week +6 around the onset of the Federal Government Shutdown of 2018/19. Panel B reports the results for the full sample. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1) and (3), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (2) and (4), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are heteroskedasticity-robust Huber-White errors in Panel A and double-clustered at the person and time levels in Panel B, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table VI. Transfer–Expense Matching & P2P Exposure – Government Shutdown

Dependent Variable =	Matching (1/0)					
	Any Social Circle Dist			Social Circle Dist P_{25}–P_{75}		
	All Users	Liquidity Constrained	Constrained/ Housing	All Users	Liquidity Constrained	Constrained/ Housing
	(1)	(2)	(3)	(4)	(5)	(6)
Post × T	0.0119***	0.0203**	-0.0233	0.0112***	0.0183**	-0.0848*
	(0.00340)	(0.00912)	(0.0233)	(0.00201)	(0.00803)	(0.0446)
Post × T × Zelle Exposure	-0.00313	0.00377	0.0846*	0.000679	0.0106	0.199***
	(0.00394)	(0.00753)	(0.0424)	(0.00613)	(0.0106)	(0.0634)
Post × Zelle Exposure	0.00310	-0.00203	0.0289	0.000826	-0.00341	0.00230
	(0.00190)	(0.00526)	(0.0246)	(0.00342)	(0.00928)	(0.0421)
Person Fixed Effects	Y	Y	Y	Y	Y	Y
City of Res × Week Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	889,960	196,417	12,804	434,153	94,781	5,659
R-squared	0.261	0.238	0.237	0.257	0.233	0.225

This table reports the results of OLS regressions with matching of transfers to expenses as the outcome as a function of exposure to P2P transfer apps, for a window of Week −5 to Week +6 around the onset of the Federal Government Shutdown of 2018/19. *Post* is a dummy variable that takes the value of 1 for the first 3 weeks in January 2019 and 0 for the remaining weeks. T takes the value of 1 for employees who earned federal income in December 2018 but missed at least one paycheck during the shutdown and 0 for employees who earned federal income in December 2018 and did not miss any paycheck during the shutdown. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1)–(3), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (4)–(6), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are double-clustered at the person and time levels, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table VII. Transfer–Expense Matching & P2P Exposure – Full Sample

Dependent Variable =	Matching (1/0)			
	Any Social Circle Dist		Social Circle Dist P_{25}–P_{75}	
	All Users	Liquidity Constrained	All Users	Liquidity Constrained
	(1)	(2)	(3)	(4)
Income Loss	0.00536***	0.00612***	0.00643***	0.00723***
	(0.000490)	(0.000815)	(0.000580)	(0.00108)
Income Loss × Zelle Exposure	0.00352***	0.0109***	0.00167	0.00712**
	(0.00111)	(0.00232)	(0.00149)	(0.00311)
Zelle Exposure	0.00447*	0.00388	0.00965**	0.00215
	(0.00259)	(0.00434)	(0.00398)	(0.00664)
Person Fixed Effects	Y	Y	Y	Y
City of Res × Month Fixed Effects	Y	Y	Y	Y
Observations	14,088,259	4,311,113	6,980,357	2,183,995
R-squared	0.399	0.365	0.401	0.368

This table reports the results of OLS regressions with matching of transfers to expenses as the outcome as a function of exposure to P2P transfer apps, for the full sample. *Income Loss* is a dummy variable that takes the value of 1 if a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months and the value of 0 otherwise. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1)–(2), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (3)–(4), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are double-clustered at the person and time levels, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table VIII. Transfer–Expense Matching & P2P Exposure during Income Shocks
Constrained Users of Friends & Family Transfers

Dependent Variable =	Matching (1/0)		Matching/Housing (1/0)	
	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}
	(1)	(2)	(3)	(4)
Panel A: Government Shutdown/Post Shutdown				
Zelle Exposure	-0.00146	0.0608***	0.00306***	0.00999***
	(0.0110)	(0.0174)	(0.00110)	(0.00241)
Person Fixed Effects	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y
Observations	17,485	8,866	17,485	8,866
R-squared	0.415	0.459	0.0471	0.158
Panel B: Full Sample/Time of Income Loss				
Zelle Exposure	0.0272***	0.0230***	0.000852**	0.000430
	(0.00578)	(0.00686)	(0.000390)	(0.000560)
Person Fixed Effects	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y
Observations	122,960	65,858	122,960	65,858
R-squared	0.131	0.165	0.027	0.040

This table reports the results of OLS regressions with matching of transfers to expenses for any expenses (Columns (1)–(2)) or specifically housing expenses (Columns (3)–(4)) as outcomes, for liquidity constrained consumers who received friends & family transfers at some point during a negative income shock. Panel A reports the results for federal employees who missed a paycheck during the Federal Government Shutdown of 2018/19, at the time of income loss post shutdown. Panel B reports the results for the months of income loss in the full sample of income shocks defined as the two consecutive months when the consumer earned less than the median of their life-time income, provided that they did not earn less than the median of their life-time income in the previous 3 months. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1) and (3), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (2) and (4), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are heteroskedasticity-robust Huber-White errors in Panel A and clustered at the month level in Panel B, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table IX. Friends & Family Transfers and P2P Exposure during Income Shocks
Constrained Users of Friends & Family Transfers

Dependent Variable =	Friends & Family Transfer ($)		Friends & Family Transfer (#)	
	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}
	(1)	(2)	(3)	(4)
Panel A: Government Shutdown/Post Shutdown				
Zelle Exposure	-1.058	5.083	0.00480	0.0398
	(2.253)	(3.666)	(0.0157)	(0.0263)
Person Fixed Effects	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y
Observations	17,485	8,866	17,485	8,866
R-squared	0.0345	0.0398	0.0365	0.0379
Panel B: Full Sample/Time of Income Loss				
Zelle Exposure	11.30	12.74	0.195***	0.186***
	(8.464)	(9.264)	(0.0153)	(0.0185)
Person Fixed Effects	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y
Observations	122,960	65,858	122,960	65,858
R-squared	0.0353	0.0435	0.0230	0.0270

This table reports the results of OLS regressions with the dollar amount of received friends and family transfers (Columns (1)–(2)) or the number of received friends and family transfers (Columns (3)–(4)) as outcomes, for liquidity constrained consumers who received friends & family transfers at some point during a negative income shock. Panel A reports the results for federal employees who missed a paycheck during the Federal Government Shutdown of 2018/19, at the time of income loss post shutdown. Panel B reports the results for the months of income loss in the full sample of income shocks defined as the two consecutive months when the consumer earned less than the median of their life-time income, provided that they did not earn less than the median of their life-time income in the previous 3 months. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1) and (3), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (2) and (4), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are heteroskedasticity-robust Huber-White errors in Panel A and clustered at the month level in Panel B, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table X. Low-Liquidity Fees & P2P Exposure during Income Shocks
Constrained Users of Friends & Family Transfers

Dependent Variable =	Low Balance Fee (1/0)		Overdraft Fee (1/0)		NSF Fee (1/0)	
	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Government Shutdown/Post Shutdown						
Zelle Exposure	-0.0255***	-0.0361***	-0.0191**	-0.0290**	0.00152	0.0104***
	(0.00806)	(0.0117)	(0.00786)	(0.0115)	(0.00296)	(0.00395)
Person Fixed Effects	N	N	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	17,485	9,434	17,485	9,434	17,485	9,434
R-squared	0.314	0.395	0.318	0.397	0.2289	0.239
Panel B: Full Sample/Time of Income Loss						
Zelle Exposure	-0.0315***	-0.0125**	-0.0309***	-0.0290***	-0.00240***	-0.00250**
	(0.00506)	(0.00524)	(0.00495)	(0.00611)	(0.000889)	(0.00121)
Person Fixed Effects	N	N	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	122,960	65,858	122,960	65,858	122,960	65,858
R-squared	0.0454	0.0649	0.0455	0.0494	0.0195	0.0218

This table reports the results of OLS regressions with dummies for low balance fees, overdraft fees, and non-sufficient funds (NSF) fees as outcomes, for liquidity constrained consumers who received friends & family transfers at some point during a negative income shock. Panel A reports the results for federal employees who missed a paycheck during the Federal Government Shutdown of 2018/19, at the time of income loss post shutdown. Panel B reports the results for the months of income loss in the full sample of income shocks defined as the two consecutive months when the consumer earned less than the median of their life-time income, provided that they did not earn less than the median of their life-time income in the previous 3 months. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1), (3), and (5), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (2), (4), and (6), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are heteroskedasticity-robust Huber-White errors in Panel A and clustered at the month level in Panel B, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table XI. Low-Liquidity Fees & P2P Transfer App Use during Income Shocks
Constrained Users of Friends & Family Transfers

Sample =	*Government Shutdown/Post Shutdown*			*Full Sample/Time of Income Loss*		
Dependent Variable =	Low Balance Fee (1/0)	Overdraft Fee (1/0)	NSF Fee (1/0)	Low Balance Fee (1/0)	Overdraft Fee (1/0)	NSF Fee (1/0)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Reduced Form						
P2P Transfer Apps Use	-0.0102***	-0.0102***	-0.000399	-0.0604***	-0.0587***	-0.00533***
	(0.00281)	(0.00267)	(0.000971)	(0.00239)	(0.00229)	(0.000576)
User Fixed Effects	N	N	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	18,583	18,583	18,583	122,960	122,960	122,960
R-squared	0.0651	0.0681	0.00158	0.0539	0.0538	0.0203
Panel B: 2-Stage Least Squares						
$\widehat{\text{Matching}}$ (P2P Transfer Apps Use)	-0.0355***	-0.0358***	-0.00139	-0.647***	-0.628***	-0.0571***
	(0.00986)	(0.00938)	(0.00339)	(0.0441)	(0.0424)	(0.00712)
User Fixed Effects	N	N	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y	Y	Y
Observations	18,583	18,583	18,583	122,960	122,960	122,960

This table reports the results of OLS and 2SLS regressions with dummies for low balance fees, overdraft fees, and non-sufficient funds (NSF) fees as outcomes as a function of P2P transfer app use, for liquidity constrained consumers who received friends & family transfers at some point during a negative income shock. The sample is federal employees who missed a paycheck during the Federal Government Shutdown of 2018/19, at the time of income loss post shutdown in Columns (1)–(3). The sample is the months of income loss in the full sample of income shocks defined as the two consecutive months when the consumer earned less than the median of their life-time income, provided that they did not earn less than the median of their life-time income in the previous 3 months, in Columns (4)–(6). Panel A reports the results for reduced-form OLS regressions where outcome variables are regressed directly on P2P transfer app use. Panel B reports the results for the second stage of 2-stage least squares (2SLS) regressions, where the first stage reported in Table A.IV regresses a dummy for transfer–expense matching on P2P transfer app use and the second stage regresses low balance fees, overdraft fees, and NSF fees on predicted matching from the first stage. *P2P Transfer App* is a dummy for consumer receiving a friends & family transfer through a P2P transfer app. *Matching* is a dummy for a friends & family transfer occurring within 3 days of an expense. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are heteroskedasticity-robust Huber-White errors in Panel A and clustered at the month level in Panel B, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Appendix A. Supplementary Results

This appendix includes additional tests to supplement the main analysis in the paper.

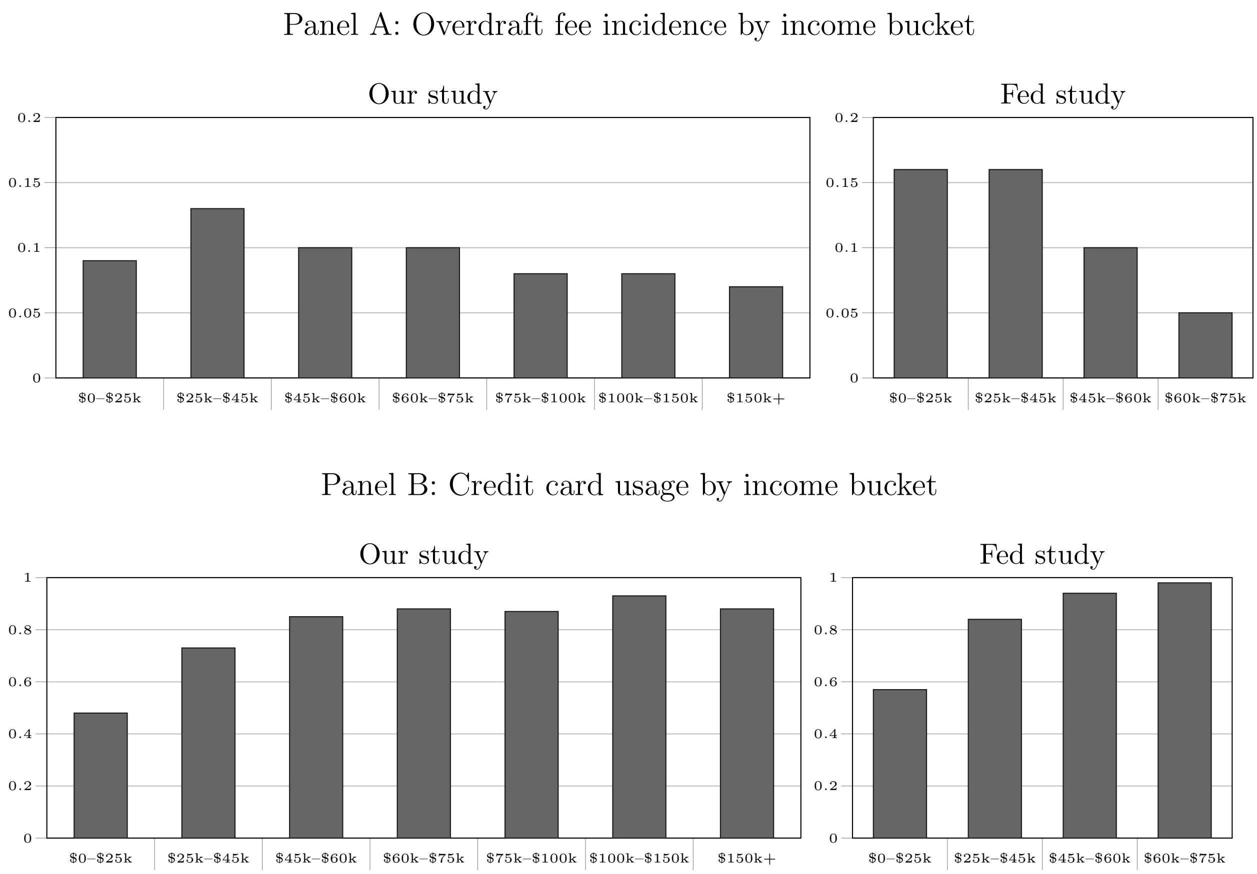


Figure A.I. Comparison of key variables to external studies. This figure documents the distribution of key liquidity-related variables across income groups and compares them to those obtained from the Federal Reserve Board's Report on the Economic Well-Being of U.S. Households in 2021. The left hand panels plot distributions of individuals across income class as defined by the data provider. The right hand panels contain distributions of households across income groups defined by the Federal Reserve Board.

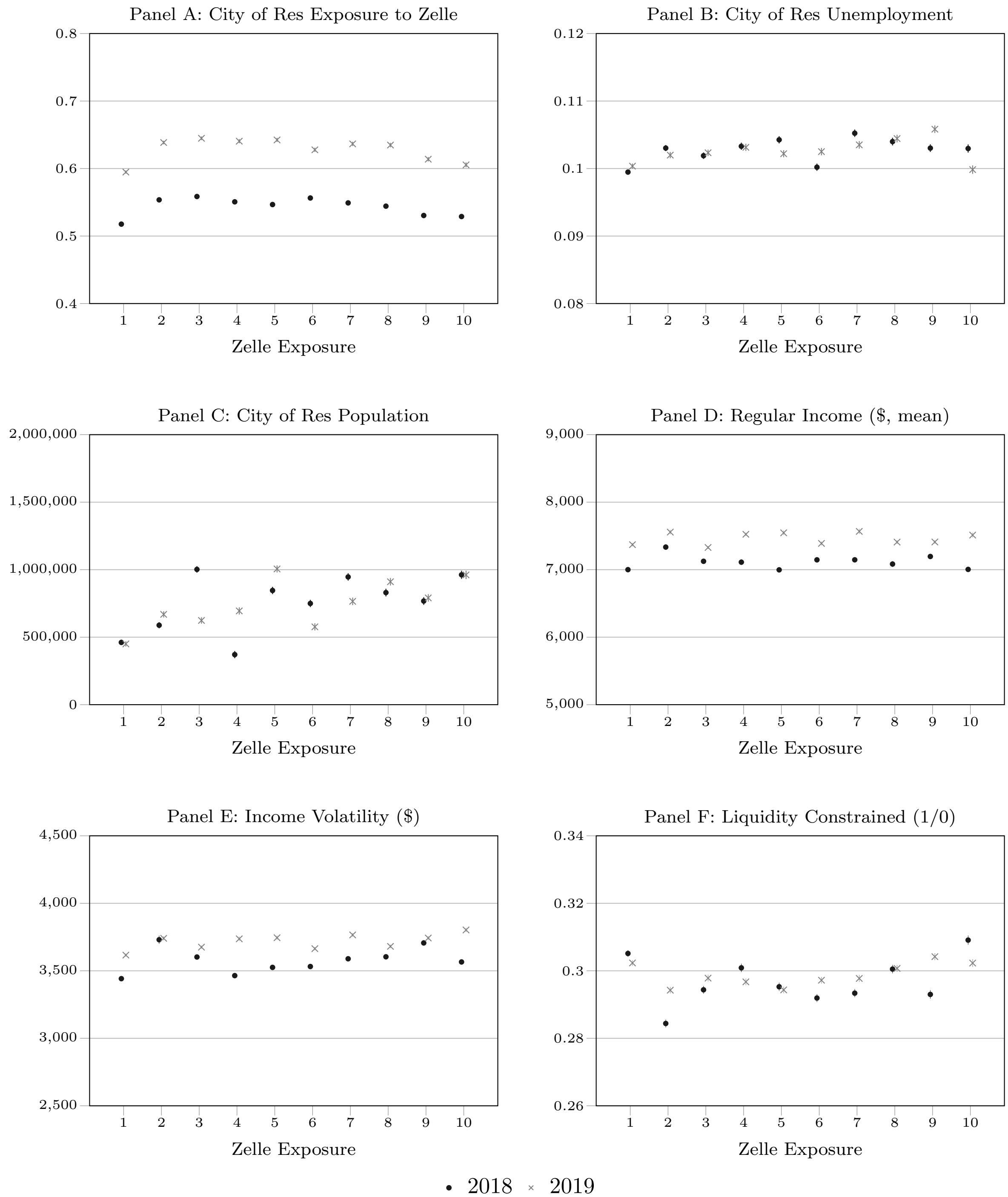


Figure A.II. Zelle exposure vs. city and consumer characteristics. This figure shows bin scatter plots of several city-level and individual-level characteristics against *Zelle Exposure* deciles, for 2018 and 2019. We measure Zelle exposure at consumers' city of social circle and combine it in 10 bins. In Panels A–C, we plot means of city-level characteristics for each of these deciles. In Panels D–F, we plot individual-level characteristics for each of these deciles after taking out city of residence fixed effects.

Table A.I. Exact Transfer–Expense Matching around Negative Income Shocks

Dependent Variable =	Exact Matching (1/0)	
	(1)	(2)
Panel A: Government Shutdown		
Post × T	-0.000847	-0.00142**
	(0.000606)	(0.000560)
Post × T × Liquidity Constrained		0.00293**
		(0.00116)
Post × Liquidity Constrained		-0.000244
		(0.000881)
Person Fixed Effects	Y	Y
City of Res × Week Fixed Effects	Y	Y
Observations	889,960	889,960
R-squared	0.101	0.101
Panel B: Full Sample		
Income Loss	0.00146***	0.00116***
	(0.000285)	(0.000310)
Income Loss × Liquidity Constrained		0.00108**
		(0.000479)
Person Fixed Effects	Y	Y
City of Res × Month Fixed Effects	Y	Y
Observations	14,088,259	14,088,259
R-squared	0.171	0.171

This table reports the results of OLS regressions with matching of transfers to expenses as the outcome, where exact matching is defined as a friends & family transfer being followed by an expense of almost exactly the same amount. Panel A reports the results for a window of Week -5 to Week $+6$ around the onset of the Federal Government Shutdown of 2018/19. *Post* is a dummy variable that takes the value of 1 for the first 3 weeks in January 2019 and 0 for the remaining weeks. T takes the value of 1 for employees who earned federal income in December 2018 but missed at least one paycheck during the shutdown and 0 for employees who earned federal income in December 2018 and did not miss any paycheck during the shutdown. Panel B reports the results for the full sample. *Income Loss* is a dummy variable that takes the value of 1 if a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months and the value of 0 otherwise. *Liquidity Constrained* consumers are individuals identified as likely living hand-to-mouth. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are double-clustered at the person and time levels, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.II. Zelle Exposure and P2P Transfer Apps Use
Constrained Users

Dependent Variable =	Zelle Use (1/0)		P2P Transfer Apps Use (1/0)	
	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}
	(1)	(2)	(3)	(4)
Panel A: Government Shutdown				
Zelle Exposure	0.00497***	0.00549**	0.00670**	0.00389
	(0.00149)	(0.00239)	(0.00308)	(0.00494)
Person Fixed Effects	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y
Observations	205,614	103,137	205,614	103,137
R-squared	0.0545	0.0683	0.0567	0.0832
F-Stat	11.05	5.277	4.719	0.622
Panel B: Full Sample				
Zelle Exposure	0.0247***	0.0171***	0.0101*	0.00470
	(0.00414)	(0.00612)	(0.00580)	(0.00877)
Person Fixed Effects	Y	Y	Y	Y
City of Res × Month Fixed Effects	Y	Y	Y	Y
Observations	4,311,113	2,183,995	4,311,113	2,183,995
R-squared	0.354	0.359	0.436	0.437
F-Stat	35.55	7.824	3.032	0.286

This table reports the results of OLS regressions with dummies for the use of Zelle and P2P transfer apps as outcomes as a function of Zelle exposure at the city of social circle, for subsample of liquidity constrained users. Panel A reports the results for a window of Week −5 to Week +6 around the onset of the Federal Government Shutdown of 2018/19. Panel B reports the results for the full sample. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1) and (3), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (2) and (4), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are heteroskedasticity-robust Huber-White errors in Panel A and double-clustered at the person and time levels in Panel B, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.III. Zelle Exposure and P2P Transfer Apps Use
Time of Negative Income Shocks

Dependent Variable =	Zelle Use (1/0)		P2P Transfer Apps Use (1/0)	
	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}	Any Social Circle Dist	Social Circle Dist P_{25}–P_{75}
	(1)	(2)	(3)	(4)
Panel A: Government Shutdown/Post Shutdown				
Zelle Exposure	0.00368***	0.00633***	0.0153***	0.00969***
	(0.000817)	(0.00129)	(0.00186)	(0.00294)
Person Fixed Effects	N	N	N	N
City of Res Fixed Effects	Y	Y	Y	Y
Observations	598,046	295,135	598,046	295,135
R-squared	0.0219	0.0294	0.0241	0.0353
F-Stat	20.23	23.98	67.94	10.86
Panel B: Full Sample/Time of Income Loss				
Zelle Exposure	0.0351***	0.0392***	0.0341***	0.0327***
	(0.00320)	(0.00330)	(0.00300)	(0.00529)
Person Fixed Effects	N	N	N	N
City of Res × Month Fixed Effects	Y	Y	Y	Y
Observations	1,626,476	775,990	1,626,476	775,990
R-squared	0.114	0.116	0.0761	0.0782
F-Stat	120.7	141.7	129.2	38.20

This table reports the results of OLS regressions with dummies for the use of Zelle and P2P transfer apps as outcomes as a function of Zelle exposure at the city of social circle, for all users in the sample at the time of negative income shocks. Panel A reports the results for federal employees during the Federal Government Shutdown of 2018/19, at the time of income loss post shutdown. Panel B reports the results for the months of income loss in the full sample of income shocks defined as the two consecutive months when the consumer earned less than the median of their life-time income, provided that they did not earn less than the median of their life-time income in the previous 3 months. *Zelle Exposure* is the Zelle bank branch exposure at the location of the consumer's close social circle. In Columns (1) and (3), we use the full set of consumers with identified city of social circle regardless of the distance between the city of residence and the city of social circle (where the city of social circle is *not* the city of residence). In Columns (2) and (4), we subset to the sub-sample of consumers who have a social circle located within the interquartile range of all social circle distances in the sample. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are heteroskedasticity-robust Huber-White errors in Panel A and double-clustered at the person and time levels in Panel B, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table A.IV. Low-Liquidity Fees & P2P Transfer App Use during Income Shocks
First Stage of 2-Stage Least Squares (2SLS)

Dependent Variable =	Matching (1/0)	
	All Users	Liquidity Constrained
	(1)	(2)
P2P Transfer Apps Use	0.0826***	0.0934***
	(0.00481)	(0.00493)
Person Fixed Effects	N	N
City of Res Fixed Effects	Y	Y
Observations	516,456	122,960
R-squared	0.107	0.141
F-Stat	294.44	358.31

This table reports the results of the first stage of 2-stage least squares (2SLS) regressions for consumers who received friends & family transfers at some point during a negative income shock. The first stage regresses a dummy for transfer–expense matching on P2P transfer app use and the second stage reported in Table XI regresses low balance fees, overdraft fees, and NSF fees on predicted matching from the first stage. *P2P Transfer App Use* is a dummy for consumer receiving a friends & family transfer through a P2P transfer app. *Matching* is a dummy for a friends & family transfer occurring within 3 days of an expense. The estimates of the intercept and fixed effects are omitted for brevity. Standard errors are clustered at the month level, and presented in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Internet Appendix to
"Friends and Family Money: P2P Transfers and Financially Fragile Consumers"

FOR ONLINE PUBLICATION

IA.I. City Name to Census Place Matching

This section provides details on procedure we use to match city names in the consumer transaction data to city names in the U.S. Census Bureau data. City names in the transaction data are noisy, and 12,726 out of 25,978 of places (or 49.0%) remain unmatched if one uses exact city-state matching. The following discrepancies are primarily responsible for the low match rate:

- Communities within city boundaries are not separately reported in the U.S. Census data (e.g., Manhattan in New York, NY; Eagle River in Anchorage, AK).
- Unincorporated territories are also not separately reported in the U.S. Census data, but are aggregated into "the balance" of the respective county (e.g., Churchton, MD; San Quentin, CA).
- Some small cities are not separately reported in the U.S. Census data but are aggregated as above (e.g., Kasigluk, AK; Coyanosa, TX).
- Transaction data contain variations in city names (e.g., Sandy Springs, GA vs. Sandy Sprgs, GA), and some names are misspelled.

We employ three algorithmic methods and extensive manual effort to minimize discrepancies in city names. We first utilize the OpenStreetMap API to find the coordinates for all cities in both the list of unmatched cities from consumer-month panel and all cities in the U.S. Census Bureau's Population Estimates data set. The matching logic is quite straightforward: We locate all cities in both data sets and match the closest cities by computing the geodesic distance between cities located in the same state.

Applying OpenStreetMap is similar to searching for a location by city and state name in Google and getting the best result with Google's internal searching algorithms. These requests to OpenStreetMap return location information for each unmatched place: latitude, longitude, bounding box, and detailed display name. For example, one gets the following result when requesting OpenStreetMap to search for "AFOGNAK, AK, USA" by querying https://nominatim.openstreetmap.org/search/AFOGNAK,AK,USA?format=json:


```
[{'place_id': 335361, 'licence': 'Data © OpenStreetMap contributors,
ODbL 1.0. https://osm.org/copyright', 'osm_type': 'node', 'osm_id':
150919119, 'boundingbox': ['57.9872168', '58.0272168', '-152.7894096
', '-152.7494096'], 'lat': '58.0072168', 'lon': '-152.7694096', 'dis
play_name': 'Afognak, Kodiak Island, Alaska, United States', 'class'
: 'place', 'type': 'hamlet', 'importance': 0.45, 'icon': 'https://no
minatim.openstreetmap.org/ui/mapicons//poi_place_village.p.20.png'}]
```


After fetching location information for unmatched cities both in our transaction data and the U.S. Census Bureau data from the above API, we apply two methods to find the best match cities. We also use a third, fuzzy matching, method that does not rely on OpenStreetMap. The three methods are as follows:

1. For all U.S. Census Bureau cities in the same state, we compute the geodesic distance from an unmatched city in the transaction data (from city center to city center) and choose the closest one to represent the unmatched city. Following this method, we find closest matched for 12,317 out of 12,725 unmatched places (i.e., 96.8%). We flag the ones with the bounding box within the unmatched city boundaries as matched. This approach allows us to get 1,378 out of 12,725 unmatched cities' best match with the U.S. Census Bureau cities. These 1,378 matches seem relatively accurate after several checks. Further manual checks of the ones with the bounding box not within the unmatched city boundaries indicate that in most cases the match does represent the closest city to the unmatched one. These matches are less accurate than the ones above but manually checking all the matches does not seem feasible. Therefore, we do not attempt to check all these matches manually, unless our second and third methods or the manual review of the unmatched city's display name raise a flag about the accuracy of these matches.
2. For each unmatched city in the transaction data, we find cities from the U.S. Census Bureau data that have an intersected part with the unmatched city. We use the coordinates to compute whether two cities are intersected (see Figure IA.1 for an example). For all intersected U.S. Census Bureau cities, we compute the geodesic distance to an unmatched city and choose the closest one to represent the unmatched city. Following this method, we are able to get 5,488 out of 12,725 unmatched cities' best match with the U.S. Census Bureau cities. This method appears less accurate than the first method because most cities are not well-shaped. However, it allows us to pick better matches for cases when an unmatched city is close to the center of another small city but it is even closer to the boundary of a large city despite being far from its city center. We manually review all matches where this method suggests a different match to the above method.
3. We also use a fuzzy matching algorithm to account for variations and misspellings in the city names. We use the generalized edit distance (COMPGED) function in SAS to calculate linguistic distance between city names in the transaction data and city names in the U.S. Census Bureau data. For each unmatched city in the transaction data, we select up to three best matches based on this linguistic distance. We then manually review these best matches. We closely review matches with the linguistic distance of 110 or less, and a cursory review of all other matches. This method allows us to match 817 cities, with most matches having the linguistic distance of 400 or less. If the fuzzy

matching method gives a precise match that contradicts matches from the above two methods, we give priority to the fuzzy matched name.

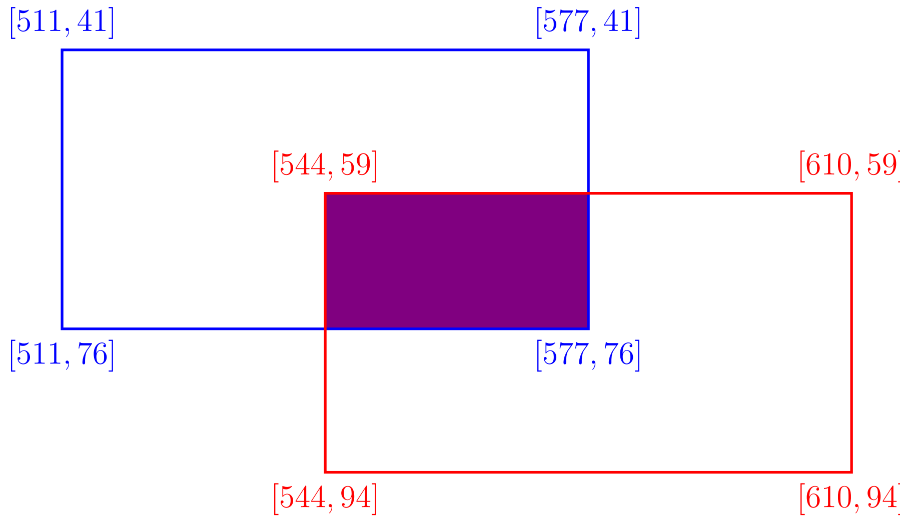


Figure IA.1. Intersected city bounding boxes. This figure shows an example of intersected bounding boxes of two cities and their coordinates. We use these intersections to refine our matching procedure.

We supplement these algorithmic methods with extensive manual review of the list of unmatched cities and the outputs from the above three methods. When deciding which matches to check, we focus on the most unreliable matches (e.g., the display name does not match the name of the unmatched city well). If a match based on the above three methods is missing or appears of low quality, we attempt to find a good match via manual search using Google Maps. Otherwise, we keep the city unmatched. Specifically, we check matches with implausibly large geodesic distance from one another.[1] We also look through the neighborhoods or large cities (e.g., Bronx in New York) and check if these neighborhoods are matched correctly. In addition, if manual checking suggests a better match in terms of travel distance, then we go with that name.[2] Overall, we obtain reliable matches for 12,134 out of 12,725 unmatched places (i.e., 95.3%).

[1] We do not apply a fixed cutoff for the geodesic distance because the perception of whether the distance is large depends on the geographic landscape of a state. For example, distances between cities are generally larger in AK or AZ than in many other states.

[2] The additional check is helpful in detecting cases when there are natural boundaries between cities (e.g., rivers without bridges or mountains).

IA.II. Friends & Family Transfer Measurement

This section summarizes the costs and times to clear for different types of friends & family money transfers. We also provide more details and how we identify these transfers in our consumer transaction data. Table IA.1 Panel A describes traditional money transfers, which include ACH transfers, ATM withdrawals and deposits, checks, and wire transfers. None of these methods of transferring money, except for bank wires, is instant. Wire transfers clear in real time (within banking hours), but they typically come with fees for both the sender and the receiver. Given the flat fee structure for wire transfers, this transitional method of moving money is especially expensive for small-sized transfers. If a consumer can afford to wait, she can lower the transaction costs by choosing ACH, ATM, or check alternatives. Thus, legacy money transfer methods entail a trade-off between cost and speed.

Table IA.1. Direct Costs and Times to Clear by Money Transfer Type

Transfer	Cost ($)		Num. Days
System	if Instant	if not Instant	if not Instant
Panel A: Traditional money transfers			
ACH	N/A	\$0–\$10 (median cost = \$0.29)	5–7 business days
ATM	N/A	ATM withdrawal fee and postage fee if applicable	US postal service delivery time
Checks	N/A	Check purchase fee and postage fee if applicable	US postal service delivery time +2–7 business days to clear
Wire	\$35 for the sender and up to \$20 for the receiver	N/A	N/A
Panel B: Peer-to-peer (P2P) payment apps			
CashApp	0.5%–1.75% fee (with a minimum fee of \$0.25)	0	1–3 business days
PayPal	0 for linked account/2.9% of the transaction amount plus a fixed fee of .30 for a non-linked account	0	1–3 business days
Venmo	1.75% fee (a minimum fee of \$0.25 and a maximum fee of \$25 is deducted from the transfer amount for each transfer)	0	1–3 business days
Zelle	0	N/A	N/A

This table provides a summary of the average direct costs of instant versus non-instant money transfers and times to clear, for traditional transfers (Panel A) and P2P payment systems (Panel B). The table highlights that the timing of receipt of funds via legacy methods is either extremely uncertain or very high cost. Note: Domestic money transfers only. Sources: mybanktracker.com, nerdwallet.com, businessinsider.com, gocardless.com.

Panel B of Table IA.1 describes the average costs and times to clear for peer-to-peer (P2P) transfer systems, which include new payment apps such as CashApp, PayPal, Venmo, and Zelle. These innovative systems are typically cheaper than traditional methods, with Zelle being free of charge to consumers. Transfers through CashApp, PayPal, and Venmo are instant for a small fee or free of charge if a consumer can afford to wait for 1–3 days. Transfers through Zelle are completed in real time and are instant at no cost.

In the data, friends & family transfers are a subset of credits in the following transaction categories: transfers, deposits, check payments, and ATM withdrawals/deposits, as appropriate (see Section IA.VI for each transfer system). We identify traditional money transfers as receipt of funds where transaction memos contain keywords such as "ACH," "ATM," "check," or "wire." We identify transfers through P2P payment apps as credits from transfers where either the primary or the secondary merchant name contains the name of the respective P2P transfer system (i.e., CashApp, PayPal, Venmo, or Zelle) or any of its variations, such as abbreviations. We follow the same process for searching within transaction memos. Fig. IA.2 provides an illustrative example of the transactions containing friends & family transfers we identify in our consumer transaction data.

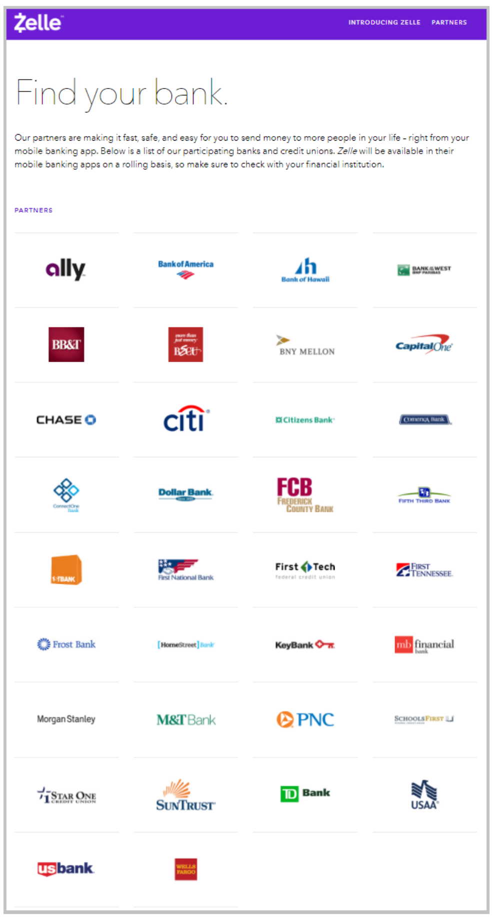
Payment System	Date	Amount	Type	Merchant	State	City	Description
Zelle	30/01/2017	400	credit	Zelle			Zelle XXXXXXXX XXXXX XXXXXXXXXXXX XXXXXXXXXXX XXXXXXX
Zelle	10/06/2018	485	credit	Zelle			ZEL FROM XXXXXX XXXXX
Zelle	17/03/2021	60	credit				Zelle XXXXXXXX XXXXX XXXXXXXXXX X X X XXXXXX
Venmo	17/01/2021	296	credit		NY	New York City	VENMO*XXXXXXXX-XXXXX NEW YORK CITY NYXXXXXX 01/17
Venmo	20/09/2016	82.75	credit		NY	New York Mills	VENMO*XXXX XXX 09/20 #XXXXXX PMNT RCVD VENMO*XXXX XXXX New York City NY
Venmo	21/12/2020	51.15	credit		NY	New York Mills	VENMO XXXXXXX XXXXXX NEW Y - NA
PayPal	28/11/2015	913.63	credit				Details Payment From XXXXX XXXXXXX XXXXXXXXXXXXXXXXX XXXXXX XXXXXXXXXX
PayPal	20/03/2016	14.35	credit				PAYPAL DES:TRANSFER ID:48D229XJRG2US INDN:XXXXXXXX XXX CO ID:PAYP
PayPal	13/10/2017	86.31	credit				PAYPAL DES:TRANSFER ID:5JYJ1AB2NZ9PY INDN:XXXX XXXXXX CO ID:PAYP
Cash App	03/04/2021	17	credit		CA	San Francisco	1 04/03 #XXXXXXXXXX PMNT RCVD Cash App*Cash Out San Francisco CA
Cash App	01/05/2020	118.2	credit				CASH APP XXXX XXXXXXX XXX X - NA
Cash App	21/08/2019	27.6	credit		CA	San Francisco	1 08/19 #XXXXXXXXX PMNT RCVD Cash App*Cash Out San Francisco CA
Wire	03/02/2017	40,000	credit				WIRE TYPE:WIRE IN DATE: XXXXXX TIME:1628 ET TRN:XXXXXXXXXX SEQ:RFIXXXXX/0
Wire	03/01/2019	2,500	credit				WIRE TYPE:WIRE IN DATE: XXXXXX TIME:1109 ET TRN:XXXXXXXXXX SEQ:XXXXXXXXX/
Wire	08/12/2018	779.66	credit				WIRE TYPE:WIRE IN DATE: XXXXXX TIME:0953 ET TRN:XXXXXXXXXX SEQ:XXXXXXXXXX
Wire	30/10/2017	2,520	credit				WIRE TYPE:WIRE IN DATE: XXXXXX TIME:1241 ET TRN:XXXXXXXXXX SEQ:DXXXXXXXX/
Checks	30/09/2018	1,101.56	credit		IL	Lombard	EXPRESS FUNDS CHECK D XXXXXX EXP DEP 2810 S HIGHLAND LOMBARD IL
Checks	28/12/2021	100	credit		MI		EXPRESS FUNDS CHECK D XXXXXXX EXP DEP XXXXXXX GRATIOT A NEW HAVEN MI
Checks	21/02/2020	0.01	credit				Mobile Check Deposit
Checks	30/08/2019	225	credit		VA	Elkton	09-30-19 16:28 AC77 MID-VALLEY-ELKTON ELKTON VA XXXXXXX 24 CHECK DEPOSIT
ATM	14/09/2017	300	credit		OH		ATM DEPOSIT XXXXXXXX DEPOSIT 4600 GRT NRTHRN N OLMSTED OH
ATM	28/03/2014	100	credit		VA	Richmond	XXXXXXXXX ATM 03/28 #XXXXXXXXX FR SAV WEST BROAD SHOPP RICHMOND VA
ATM	03/02/2018	725	credit		MO	Independence	XXXXXXXXXXX ATM 01/03 #XXXXXXXXX DEPOSIT EASTLAND INDEPENDENCE MO
ATM	28/05/2018	100	credit		IN		ATM DEPOSIT XXX XXXXXXXX DEPOSIT XXXXX MAYSVILLE FORT WAYNE IN
ATM	19/07/2014	500	credit		MA	Norwood	XXXXXXXXX ATM 03/19 #XXXXXXXXX DEPOSIT HANNAFORD MARKET NORWOOD MA
ATM	03/03/2021	1,000	credit		PA		XXXXXXX ATM DEPOSIT XXXXXXXX DEPOSIT 200 SOUTH 40TH PHILADELPHIA PA
ACH	01/07/2018	234	credit				X XXXXX ACH - TRANSFER XXXXXXXX
ACH	20/02/2019	200	credit				XXXX 529 ACH DEPOSIT ***********3371 XXXXX XXXXX 0 0032
ACH	05/10/2015	1,500	credit				ACH CREDIT XXXXX6442 XXXXXXXXTRANSFR TRANSFER
ACH	24/02/2016	35	credit				ACH CREDIT FPAQZFZVR4 XXXXX XXXXXXX DDA TO DDA

Figure IA.2. Snapshot of friends & family transactions. This figure provides a snapshot of synthetic transaction data containing friends & family transfers. There data are provided for illustrative purposes only. The data have been modified and do not represent the actual data.

IA.III. Zelle Partnership Data Collection

This section describes the procedure we used to hand-collect Zelle partnerships data. We start by identifying the universe of current and past Zelle partners (referred to as "network financial institutions") such as banks, credit unions, savings and loan associations, and other financial companies. We first save and compare 144 historical lists of Zelle partners obtained from Zelle's official website through direct download in July 2021 and their archived snapshots using the Wayback Machine.[3] We do extensive work to reconcile the lists taking into account variations in names, name changes, and bank mergers with the help of bank web sites and logos linked to Zelle lists. Fig. IA.3 provides an example of a Zelle partner list with logos and a Zelle-dedicated web-page on a Zelle partner's website.

Panel A: Early Zelle Partners



Panel B: Zelle Web-page on Bank's Website

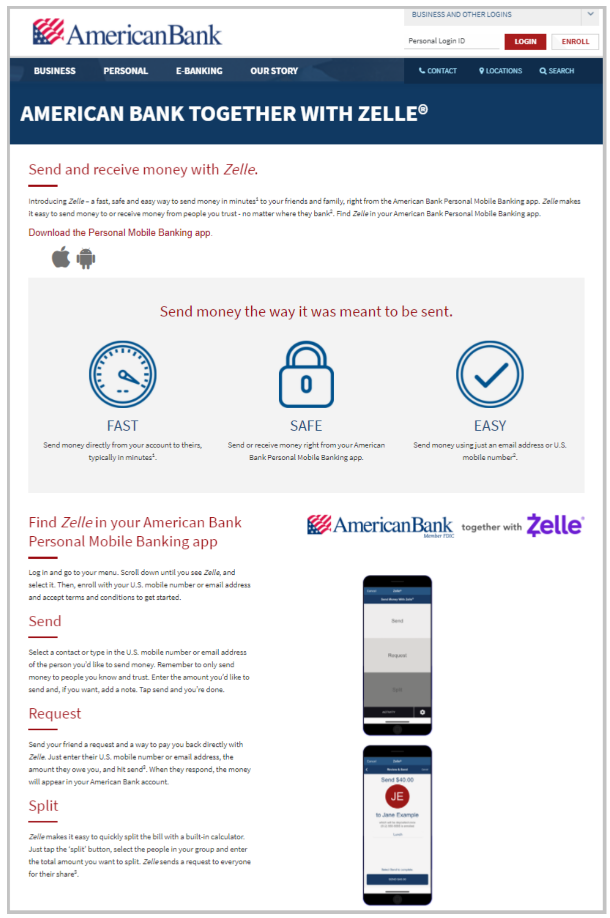


Figure IA.3. Zelle partner example. This figure provides a Zelle partner list as of June 24, 2017 (Panel A) and an example of a Zelle web-page on a partner's website (Panel B).

[3]We use the following web pages containing Zelle partner lists: https://www.zellepay.com/participating-banks-and-credit-unions, https://enroll.zellepay.com, and https://www.zellepay.com/get-started.

We then manually collect Zelle partnership announcement and roll-out dates from banks' official websites, press releases, social media pages (e.g., Facebook, Twitter), and general media mentions. We use Wayback Machine to capture historical data. One complication is some missing official announcement dates, especially for banks that are no longer partners or smaller banks. Another complication is time lapses between when banks announce partnering with Zelle and when they roll out Zelle in their mobile banking applications. These delays in roll-outs appear to differ across time (longer delays in early years) and across banks. We do extensive search for these two dates. Since our empirical analyses require us to know when consumers could start using Zelle in their mobile banking applications, our focus is on the roll-out date when determining the partnership start date.[4] Fig. IA.4 provides examples.

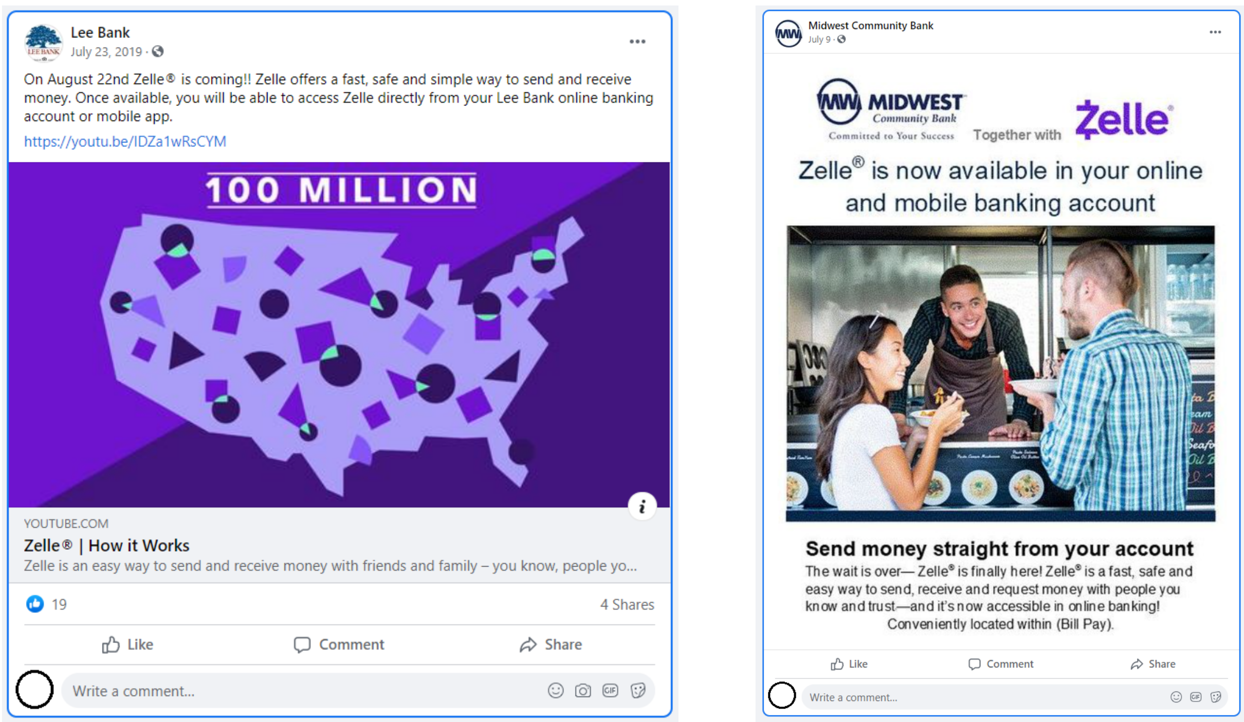


Figure IA.4. Banks' Zelle roll-out announcements on social media. This figure provides examples of banks announcing Zelle roll-out on banks' official Facebook web-pages.

We use the following approach to determine the Zelle roll-out date (in that order):

1. If an official roll-out date is announced by a bank or if we can reliably conclude from the bank's social media advertisement that the bank has just rolled out Zelle,[5] we use that date as the roll-out date.

[4] Despite all our efforts, some of these dates might remain inaccurate. We believe that our rigorous approach to identifying Zelle partners through historical snapshots of Zelle partner lists and comprehensive and unbiased collection of partnership dates minimizes any measurement errors.

[5] We look for phrases in the advertisements such as "now available," "can now use," and the like.

2. If the official roll-out date is not available, we infer the roll-out date. We compare the date of the first Zelle advertisement that the bank posted, the date Zelle's availability was first mentioned on the bank's web site, and the date the bank was first mentioned as a partner in Zelle's lists. We set the roll-out date to the earliest of these three dates.
3. We then examine dates when banks, Zelle, or mass media explicitly state that the bank has partnered with Zelle but has not rolled out the service yet (e.g., "coming soon" pre-announcements). If this date is later that the inferred roll-out date, we use the day after this date as the roll-out date. E.g., see Fig. IA.5.

We also record the last date of partnership for banks that stopped partnering with Zelle based on the last time these banks appear in Zelle partner lists. It is noteworthy that 49 financial institutions (4.4%) drop from Zelle partner lists during our sample period, mostly for exogenous reasons such as bank mergers. Some of these institutions continue to offer Zelle service after the merger while other institutions either decide to stop partnering or never roll out Zelle in the first place.

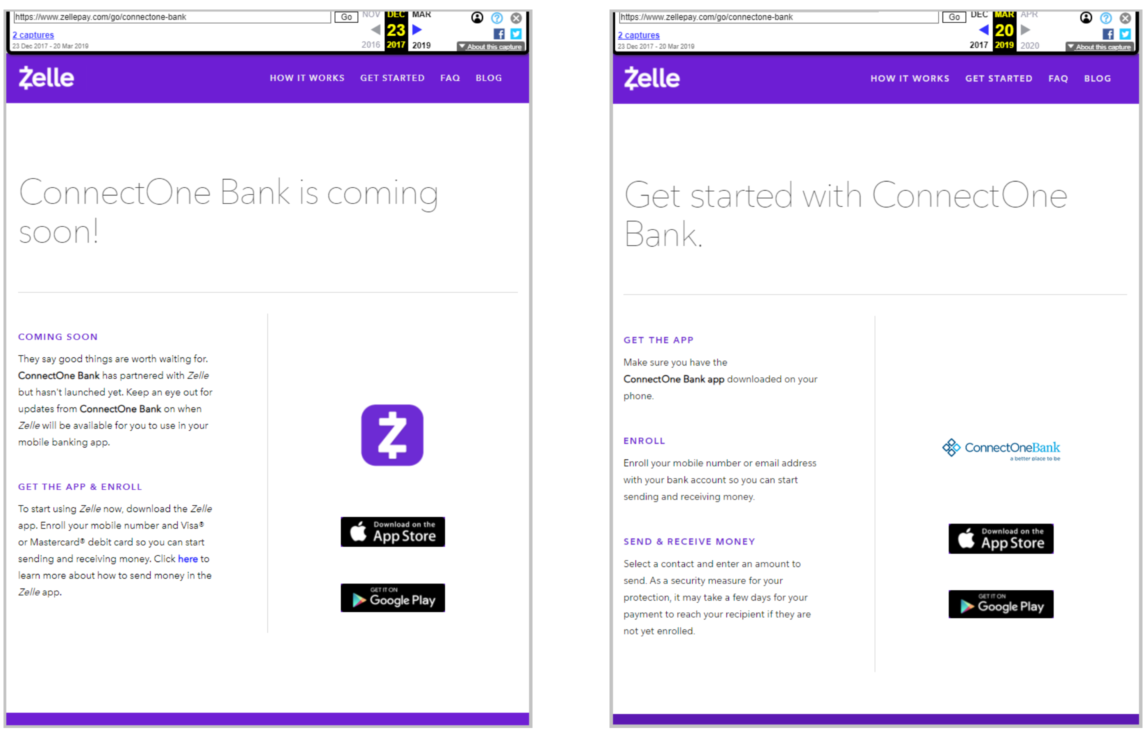


Figure IA.5. Banks' web-pages on Zelle's website. This figure provides examples of web-pages on Zelle's website, which help distinguish between partnership and roll-out dates.

Figure IA.6 reports the composition of Zelle partners over time. Panel A provides clear evidence of staggered adoption and shows that most Zelle partners are banks. Panel B zooms in on bank partners. We focus on banks because of availability of bank branch data from the

FDIC's Summary of Deposits (SOD) data set, data on bank characteristics from the Call Reports provided by the Federal Financial Institutions Examination Council (FFIEC), and because we are concerned that Zelle adoption decisions of credit unions and other nonbanks may be endogenous to local economic conditions. We distinguish between banks of different size, treating banks with branches in at least 12 cities as big banks (e.g., Bank of America, Wells Fargo). Figure IA.6 Panel B shows that most early adoption of Zelle is by big banks. Yet, there is visible time variation in Zelle adoption even within the sample of big banks.

Panel A: Zelle Partners (#) Panel B: Zelle Bank Partners (#)

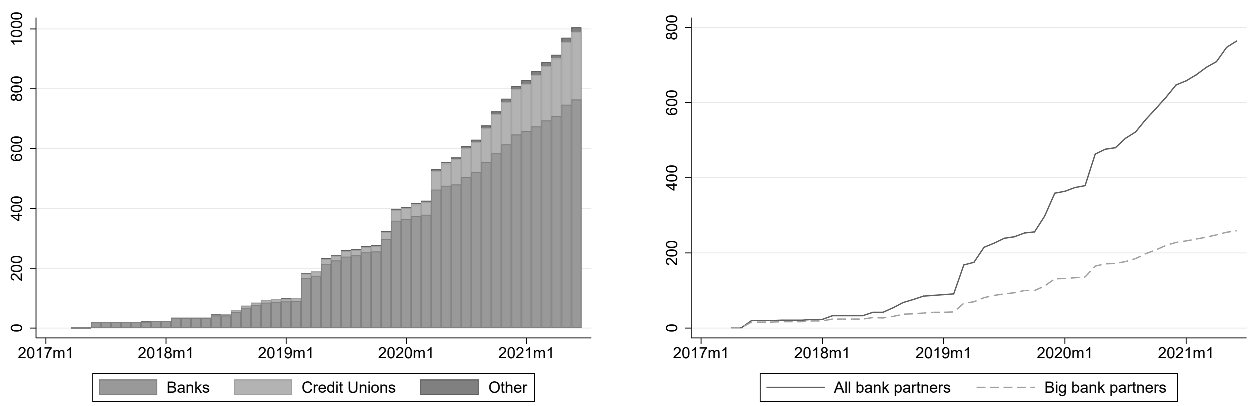


Figure IA.6. Zelle partners over time. This figure plots the cumulative number of financial institutions that partnered with Zelle over time. Panel A reports the number of Zelle partners by type. Panel B reports the number of bank partners, including big banks. We conservatively define big banks as banks that have branches in at least 12 cities (e.g., Bank of America, Wells Fargo).

We then merge our hand-collected Zelle partnership data with the SOD data to create local measures of consumers' access to P2P money transfers through Zelle bank partnerships. Figure IA.7 shows the geographical distribution of Zelle bank partnership intensity across U.S. counties in 2017 (Panel A) and 2020 (Panel B), on the same scale. We measure partnership intensity as the number of county's bank branches owned by Zelle partner banks divided by the total number of branches in the county. There is noticeable geographical variation in Zelle partnerships, as well as variation across time.

Similarly, we create a monthly measure of consumers' Zelle access at the city/state level, which we use to identify exogenous variation in access to P2P transfers apps by consumers in the spirit of the intention-to-treat (ITT) effects. We define *Zelle Exposure* as the number of bank branches owned by Zelle partner banks divided by the total number of bank branches in a city, where Zelle partnership data are lagged by one month and bank branch location data are as of most recent SOD release. We use bank branches as opposed to bank deposits in constructing this measure because the number of bank branches mostly captures the

Panel A: Zelle Partnerships in 2017 Panel B: Zelle Partnerships in 2020

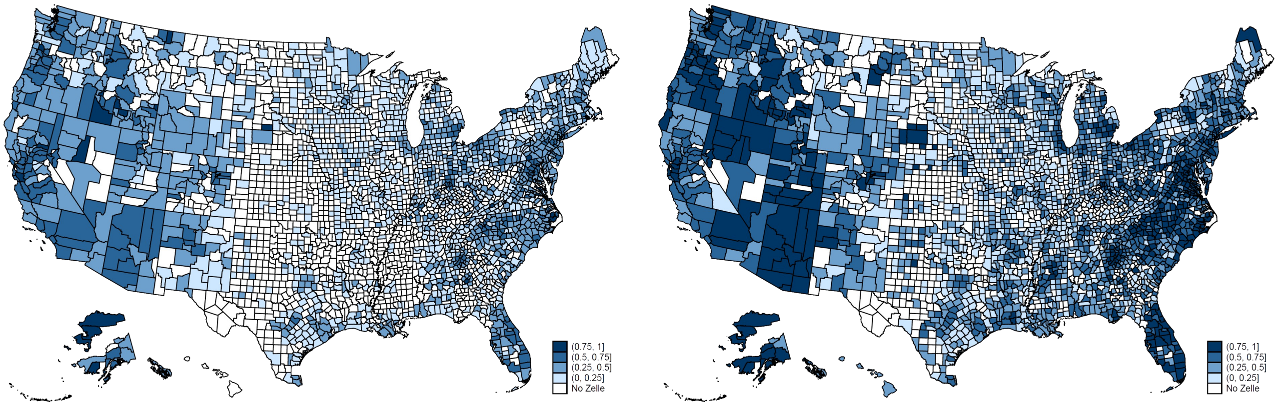


Figure IA.7. Zelle partners over geography. This figure shows the geographical distribution of Zelle–bank partnerships, in 2017 (Panel A) and 2020 (Panel B). For each county, we calculate the share of branches owned by banks that partner with Zelle to the total number of bank branches. Darker counties represent geographies with more Zelle bank partners.

supply of banking services whereas the value of deposits is more likely to capture both the supply and the demand. We further merge *Zelle Exposure* to consumer transaction data by consumers' city of residence and city of social circle (see definition below). Figure IA.8 provides an example of the variation in Zelle exposure we use, with the focus on the variation in Zelle exposure at the city of social circle after controlling for consumers' city of residence.

Our identification strategy relies on the staggered bank roll-out of Zelle and exposure of consumers to these banks in the city in which consumers' close friends and family reside. We focus on Zelle exposure at the city of social circle because bank adoption of Zelle at the city of residence may be correlated with trends consumer characteristics if bank adoption decisions are also correlated with time-varying local factors. For example, more populated cities might have a bigger large bank presence and hence more adoption of Zelle or more uptake of other technologies given that large banks were early Zelle partners. Additionally, large cities might also be populated with higher-income individuals who may have higher willingness to adopt or use a new technology. We show the relevance of Zelle exposure to the use of friends and family transfers through Zelle and P2P transfers apps more generally in the main part of the paper. We also discuss the correlation of Zelle exposure with city-level and consumer characteristics in Section I.B.4 and Appendix Fig. A.II. We show that there is no meaningful correlation of Zelle exposure at the city of social circle with city of residence and consumer characteristics once we control for city of residence characteristics. We describe the identification of the location of close social circle next.

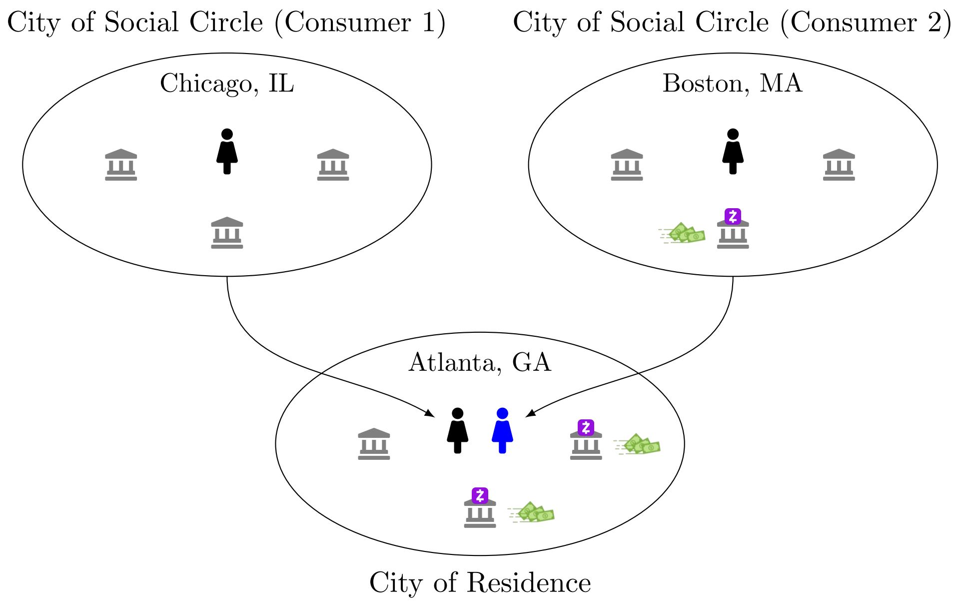


Figure IA.8. Identification strategy diagram. This figure presents a simplified version of our identification strategy. We relate consumers' Zelle use to staggered Zelle adoption by banks in consumers' city of residence (e.g., Atlanta). We also compare Zelle use by consumers who reside in Atlanta but have differential exposure to variation in Zelle adoption by banks in cities of their social circle (e.g., Chicago, Boston). We identify the city of social circle from transaction data using consistent location of consumer spending during family holidays.

IA.IV. Location of Close Social Circle

We identify the city in which each consumer's circle reside by using our detailed transaction data to find the geographical location of the consumer's spending during three major holidays when consumers tend to visit friends and family (i.e., Independence Day, Thanksgiving, and Christmas). We take the following approach. First, for each consumer and each holiday, we create a list of cities where transactions take place during each of these holidays and one day before and after the holiday. Second, we rank these cities by transaction count and retain the top city that is not the same as the consumer's city of residence.

We note that this method will not identify the city of social circle as the city of residence. Instead, we define the next most frequently visited holiday location as the city of close social circle. Put differently, this method implicitly sets the distance between the city of residence and the city of close social circle as >0. We argue that in this setting, this restriction is appropriate because we are primarily interested in pinpointing variation in exposure to Zelle that does not come from the exposure to P2P app-based transfer technology in the person's city of residence. In fact, in our most stringent specification, we include city of residence $\times$ time fixed effects to absorb any local time-varying factors that might impact consumer outcomes. If the consumers' location of close social circle were the same as the city of residence, we would pick up weaker variation in close friends and family exposure to Zelle.

We calculate the distance between the city of residence and city of close social circle, and document the distribution of these distances in Fig. IA.9. Panel A reports histograms of the distance for the government shutdown and the full representative samples, while Panel B reports histograms of the distance measures that fall within the interquartile ranges of this measure within each of these samples. In unreported tests, we run robustness checks conditioning on the consumers' close social circle being further away from their city of residence, which we define in two ways. First, we define further away as the city of social circle being $> 50\,\text{km}$ away from the city of residence.[6] Second, we define further away as the city of social circle being in a different state to the consumers' city of residence. All our results hold.

IA.V. Federal Government Shutdown of 2018/2019 and Constrained Consumers

In this section, we provide some relevant background information on the Federal Government Shutdown we examine and hand-to-mouth consumers most affected by the shutdown shock. The 2018/2019 Federal Government Shutdown resulted from an impasse between Congress and the White House concerning funding, with the primary point of contention being the allocation of funds for a proposed U.S.–Mexico border wall, a pivotal campaign pledge by President Donald Trump. The inability to reach an agreement on a spending bill that incorporated wall funding ultimately triggered the shutdown.

Commencing on December 22, 2018, the government shutdown lasted for a record-breaking 35 days, making it the longest in U.S. history. Numerous federal departments and agencies, including Agriculture, Commerce, Homeland Security, Housing and Urban Development, Justice, State, and Treasury, among others, were affected. Additionally, entities such as the Environmental Protection Agency (EPA) and the Internal Revenue Service (IRS) also experienced repercussions. Throughout the shutdown, a considerable number of federal employees were furloughed, meaning they were placed on temporary unpaid leave. The number of affected employees fluctuated during the course of the shutdown, peaking at approximately 800,000 individuals from diverse agencies who were either furloughed or required to work without pay.

Subsequently, on January 25, 2019, a temporary funding bill was enacted, effectively terminating the shutdown. This legislation provided funding to reopen the government

[6] We choose 50 km as a distance that can reasonably be traveled within one day.

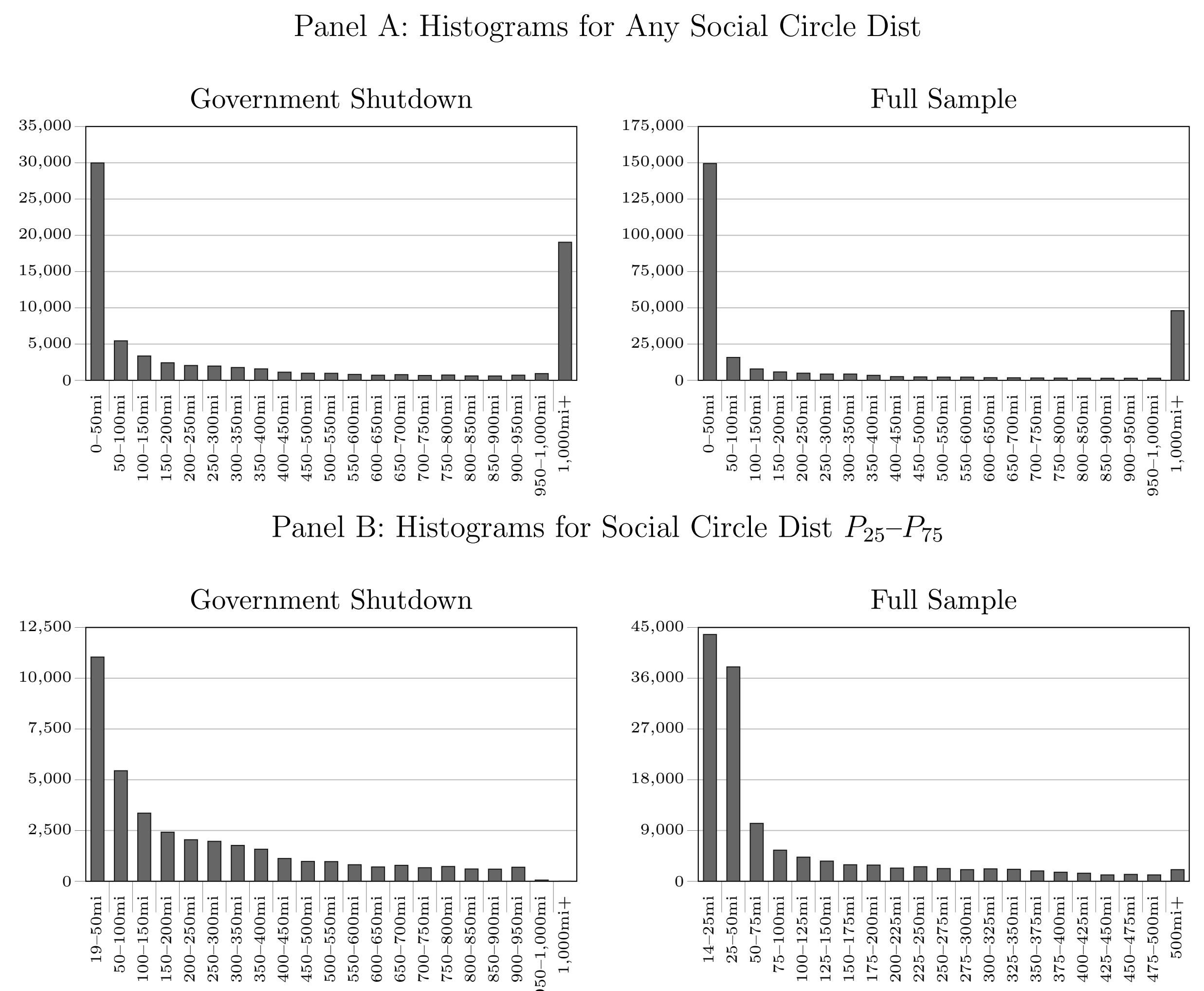


Figure IA.9. Distance between city of residence and city of social circle. This figure plots the geographical distance between consumers' city of residence and their city of social circle, where the city of residence is not the city of social circle. Panel A plots the distance for all observations for the government shutdown sample (left) and the full representative sample (right). Panel B plots the distance for observations where the distance between these cities is within the interquartile range (i.e., P_{25}–P_{75}) for each sample, the government shutdown sample (left) and the full monthly representative sample (right).

for a three-week period, facilitating ongoing negotiations between Congress and the White House pertaining to border security funding. Following the conclusion of the government shutdown, federal employees who had not received paychecks during the shutdown began to receive their back pay. The process of disbursing back pay varied across agencies and payroll systems; however, generally, the distribution commenced shortly after the government reopened. Therefore, the shutdown could be thought of as mostly a liquidity shock to affected federal employees due to the timing of pay rather than a change in income.

The timing of back pay varied among agencies, contingent upon factors such as payroll system complexity, administrative processes, and the extent of employee impact. Some employees received their back pay within the initial payroll cycle following the shutdown's conclusion, while others encountered delays due to logistical challenges associated with processing and disbursing the payments. Anecdotal evidence suggests that pay-cycles did not return to normal until at least one cycle after the reopening on January 25, 2019.

Income shocks such as the government shutdown are particularly concerning for consumers living paycheck to paycheck. Negative shocks for these consumers can exacerbate their existing reliance on high-cost debt and other costs associated with insufficient funds available to cover expenses, especially large non-discretionary expenses such as housing payments. We thus identify consumers who are hand-to-mouth in our data as those whose expenses closely mimic income (i.e., almost all money coming to the account is spent in the same month). In Table IA.2, We compare characteristics of constrained and unconstrained consumers associated with liquidity for our entire sample as of September 2019.

Table IA.2. Liquidity Constraints of Hand-to-Mouth Consumers

	As of Sep 2019			
	Full sample unconstrained	Full sample constrained	Difference in means	Difference in means t-stat
	(1)	(2)	(3)	(4)
Income (weekly)	2,076	731	−1,344***	85.7
Income CV (sd/mean)	1.33	1.42	0.089***	16.6
Overdraft User Ever	14.0%	25.8%	11.8%***	35.7
NSF Incurred Ever	2.6%	2.4%	−0.2%	−1.5
Late Fee Incurred Ever	14.0%	14.6%	0.6%**	2.2
Alternative Loan User Ever	25.2%	29.2%	3.95%***	10.2

This table compares income and other characteristics of constrained (i.e., hand-to-mouth) and unconstrained consumers for the full sample within a particular month (September 2019). We present differences in means between the samples in Column (3) and the corresponding t-statistics in Column (4). ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively. Note: CV = coefficient of variation (cross sectional).

Table IA.2 provides evidence consistent with liquidity constrains being more binding for hand-to-mouth consumers. Constrained consumers have significantly lower income, although the variability of income as measured by the coefficient of variation (CV) is somewhat higher. Constrained consumers are 11.8% more likely to incur an overdraft and 0.6% more likely to incur a late fee. These consumers are also more likely to take out alternative loans, which are typically very expensive. We show how our results differ for constrained versus unconstrained consumers in the empirical analysis presented in the main part of the paper.

IA.VI. Definitions of Variables

Variable:	Regular Income ($)
Transaction base type:	credit
Transaction category name(s):	Salary/Regular Income, Interest Income, Investment/Retirement Income, Other Income, Sales/Services Income
Variable:	Income Volatility ($)
Formula/description:	annual volatility of regular income
Variable:	Federal Income ($)
Transaction base type:	credit
Transaction description contains:	Fed Sal
Variable:	Total Income ($)
Transaction base type:	credit
Variable:	Total Spending ($)
Transaction category name(s):	Atm/Cash Withdrawal, Automotive/Fuel, Cable/Satellite/Telecoms, Charitable Giving, Check Payment, Deposits, Education, Electronics/General Merchandise, Entertainment/Recreation, Expense Reimbursement, Gifts, Groceries, Healthcare/Medical, Home Improvement, Mortgage, Office Expenses, Other Expenses, Personal/Family, Pets/Pet Care, Postage/Shipping Refunds, Rent, Restaurants, Rewards, Service Charge Fees, Services/Supplies, Subscription/Renewals, Travel, Utilities, Credit Card Payments
Variable:	Housing Expenditure ($)
Transaction base type:	debit
Transaction category name(s):	Mortgage, Rent
Variable:	Credit Card Spending ($)
Transaction base type:	debit
Transaction category name(s):	Credit Card Payments
Variable:	Cut Spending (1/0)
Formula/description:	(Total Spending$_t$/Total Spending$_{t-52}$) < 1
Variable:	Credit Card Usage (1/0)
Formula/description:	Credit Card Payments > 0
Variable:	Savings ($)
Formula/description:	$\sum_t^{t-6}$ Regular Income $-\sum_t^{t-6}$ Expenditures
Variable:	Use Savings (1/0)
Formula/description:	$\sum_t^{t-6}$ Regular Income $-\sum_t^{t-6}$ Expenditures < 1
Variable:	Constrained User (1/0)
Formula/description:	
Variable:	Friends & Family Transfer (1/0)
Formula/description:	Friends & Family Transfer > 0]
Variable:	Friends & Family Transfer ($)
Formula/description:	Traditional Transfer + App-Based Transfer
Variable:	Friends & Family Transfer (#)
Formula/description:	count(Traditional Transfer + P2P App-Based Transfer)
Variable:	Traditional Transfer ($)
Transaction base type:	credit
Derived category name(s):	(1) ACH, (2) ATM, (3) Check, (4) Wire
Transaction category name(s):	(1) Deposits, Transfers; (2) ATM/Cash Withdrawals, Deposits, Transfers; (3) Check Payment, Deposits, Transfers; (4) Deposits, Transfers
Transaction description contains:	(1) ACH, (2) ATM, (3) Check, (4) Wire
Variable:	Peer-to-Peer (P2P) App-Based Transfer ($)
Transaction base type:	credit
Derived category name(s):	(1) CashApp, (2) PayPal, (3) Venmo, (4) Zelle
Transaction category name(s):	Transfers
Transaction description contains:	(1) CashApp, (2) PayPal, (3) Venmo, (4) Zelle
Variable:	Peer-to-Peer (P2P) Transfer Apps Use (1/0)
Formula/description:	P2P App-Based Transfer > 0

Variable: **Formula/description:**	Zelle Use (1/0) Zelle > 0
Variable: **Formula/description:**	Zelle Exposure number of bank branches of Zelle bank partners to total number of bank branches in the city of social circle
Variable: **Formula/description:**	Matching (1/0) takes the value of 1 if friends & family transfer occurs within 3 days (before) any of the following payments: Automotive, Cable Satellite, Check Payment, Credit Card Payment, Groceries, Loans, Mortgage, Rent, Restaurants, Utilities, and 0 otherwise
Variable: **Formula/description:**	Matching/Housing (1/0) takes the value of 1 if friends & family transfer occurs within 3 days (before) any of the following payments: Mortgage, Rent, and 0 otherwise
Variable: **Formula/description:** **Variable:** **Transaction base type:** **Formula/description:**	Exact Matching (1/0) takes the value of 1 if friends & family transfer occurs within 3 days (before) any of the following payments and is between 95 and 100% of the outgoing payment: Automotive, Cable Satellite, Check Payment, Credit Card Payment, Groceries, Loans, Mortgage, Rent, Restaurants, Utilities, and 0 otherwise Low Balance Fee (1/0) debit (Overdraft Fee + NSF Fee) > 0
Variable: **Transaction base type:** **Transaction description contains:**	Overdraft Fee (1/0) debit overdraft AND fee, overdraft AND charge, overdraft AND interest, OD fee, OD charge, OD Item, OD itm
Variable: **Transaction base type:** **Transaction description contains:**	NSF Fee (1/0) debit NSF, NS Fee, Non Sufficient, Returned Fee, Returned Check, Returned Item, non-sufficient, insufficient
Variable: **Formula/description:**	City of Res Consumer's city of residence
Variable: **Formula/description:**	City of Res Exposure to Zelle Zelle exposure at consumer's city of residence
Variable: **Formula/description:**	City of Res Unemployment rate of unemployment at consumer's city of residence
Variable: **Formula/description:**	City of Res Population population of consumer's city of residence
Variable: **Formula/description:**	City of Soc Consumer's city of social circle defined as city where consumer most frequently transacts over major family holidays, where City of Soc $\neq$ City of Res
Variable: **Formula/description:**	Any Social Circle Dist (mi) geographical distance between city of social circle and city of residence
Variable: **Formula/description:**	Social Circle Dist P_{25}–P_{75} (mi) geographical distance between city of social circle and city of residence if within interquartile range for each sample
Variable: **Formula/description:**	Post (1/0) dummy variable that takes the value of 1 for the first 3 weeks in January 2019 and 0 for the remaining weeks within the window of Week -5 to Week $+6$ (i.e., December 3, 2018 to February 24, 2019) around the onset of the Federal Government Shutdown of 2018/19
Variable: **Formula/description:**	T (1/0) takes the value of 1 for employees who earned federal income in December 2018 but missed at least one paycheck during the Federal Government Shutdown of 2018/19 and 0 for employees who earned federal income in December 2018 and did not miss any paycheck during the shutdown
Variable: **Formula/description:**	Income Loss (1/0) dummy variable that takes the value of 1 if a consumer earned less than the median income of their life-time income in that month and the following month, but did not earn less than the median of their life-time income in the previous 3 months and the value of 0 otherwise