When Is It Hard to Make Ends Meet?*

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Abstract

We analyze how predictable variation in the timing of income affects household financial health. Exploiting quasi-random variation in the disbursement of benefits by the Social Security Administration, we document that households are more likely to face financial shortfalls during 35-day versus 28-day pay periods. Households are also more likely to experience shortfalls if they have a greater mismatch between the timing of income and expenditure commitments. These patterns are difficult to reconcile with the lifecycle / permanent income hypothesis, and suggest that households do not perfectly adjust to predictable variation in income timing.

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I Introduction

The timing of regular income is often set by arbitrary and inflexible administrative rules. At the same time, nearly half of all Americans hold little liquid wealth, living from paycheck to paycheck.[1] Together, these two facts suggest that small differences in income timing may affect real economic outcomes for many households. Consistent with binding short-term budget constraints, previous studies have shown that consumption declines over the pay cycle.[2] But there has been limited evidence on the effect of income timing on the daily dynamics of credit, delinquency, and financial health.[3] Studying these outcomes is important for understanding consumer behavior, optimal income timing, and the regulation of short-term credit.

Our study exploits predictable variation generated by the Social Security Administration (SSA) to estimate the causal effect of income timing on household finances. For about 28 million beneficiaries, the SSA assigns benefits payments to the second, third, or fourth Wednesday of each month based on the day of the month they were born. About four months per year have five Wednesdays, generating further variation in whether pay cycles have 28 or 35 days. Under the lifecycle / permanent income hypothesis (LCPIH), neither the length of the pay period nor the timing of pay within a month should affect financial health.

We test for the effects of income timing using two distinct datasets that cover the payday loan, bank account, and credit card transactions of Social Security beneficiaries. We measure the daily incidence of financial shortfalls in the form of online and storefront payday borrowing, bank overdrafts, and bounced checks. Our results contrast with the predictions of the LCPIH. First, we find that financial shortfalls are higher during 35-day compared with 28-day pay periods. Because our outcomes are measured at a daily level, this effect is not driven mechanically by the greater likelihood of experiencing a negative liquidity shock during a longer measurement period. Second, we find that households are less likely to experience shortfalls when they are quasi-randomly

[1] See Lusardi, Schneider and Tufano (2011) and Board of Governors of the Federal Reserve (2016).

[2] Stephens (2003) finds that Social Security recipients spend more on instantaneous consumption in the days following a benefits payment. Wilde and Ranney (2000), Shapiro (2005), and Mastrobuoni and Weinberg (2009) document food consumption cycles for SNAP and Social Security recipients. Hastings and Washington (2010) show that the declines in food expenditures among SNAP recipients are neither driven by changes in quality nor prices. Olafsson and Pagel (2016) show that expenditures decline over the pay cycle even for high-liquidity households.

[3] Baker and Yannelis (2015) and Gelman, Kariv, Shapiro, Silverman and Tadelis (2015) examine the effects of the temporary shift in income timing caused by the 2013 federal government shutdown. Bos, Le Coq and van Santen (2016) study the effects of variation in public benefits timing on pawn borrowing in Sweden.

assigned to receive income on the fourth Wednesday compared with the second Wednesday each month.

To shed light on the mechanisms behind our findings, we construct a novel measure of the timing mismatch between income and expenditure commitments. We find little evidence that households try to avoid financing fees by strategically timing their expenses to coincide with income receipt. The timing of bill payments is very similar regardless of income timing. Instrumenting timing mismatch with Wednesday group assignment, we find that a one week greater timing mismatch is associated with a 10% per day increase in overdrafts, a 34% increase in bounced checks, and a 47% increase in online payday borrowing. Together, our findings suggest that some consumers fail to perfectly budget monthly cashflows, even from highly predictable sources of income.

This paper contributes to the large literature on the effects of income receipt on borrowing and consumption.[4] In particular, it is closely related to papers examining the relationship between the timing of government benefits and household expenditures.[5] Some recent papers have also examined the high-frequency effects of wage income receipt on expenditures and delinquencies.[6] This study is distinguished by our broad coverage of household income and expenditures, and our focus on financial shortfalls and financial health.

This study is also related to the literature on the drivers and welfare implications of short-term credit use, which largely focuses on payday loans. Morse (2011), Dobridge (2014) and Zaki (2013) find evidence that payday loans are used to smooth shocks after natural disasters and to smooth income between paychecks. Allen, Wang, Swanson and Gross (2017) document a decrease in payday borrowing following Medicaid expansions in California, consistent with borrowing to smooth medical shocks. The broader literature on payday loans has been mixed on whether access to credit makes consumers better or worse off.[7] In contrast to much of this literature which lacks

[4]See Jappelli and Pistaferri (2010) for a review of the theory and evidence.

[5]See Stephens (2003), Wilde and Ranney (2000), Shapiro (2005), Mastrobuoni and Weinberg (2009), Hastings and Washington (2010) and Hastings and Shapiro (2017)

[6]See Olafsson and Pagel (2016), Bos et al. (2016), Gelman et al. (2015), and Baker and Yannelis (2015)

[7]Melzer (2011), Carrell and Zinman (2014), Gathergood, Guttman-Kenney and Hunt (2016), and Baugh (2017) document that access to payday loans makes it more difficult to pay bills, increases bank overdrafts, and worsens job performance. In contrast, Zinman (2010), Morse (2011), Morgan, Strain and Seblani (2012), and Zaki (2013) find that payday access is associated with better financial outlooks, lower rates of foreclosure and larceny after natural disasters, lower overdrafts and bounced checks, and lower consumption volatility in military commissaries. Bhutta, Skiba and Tobacman (2012) and Carter and Skimmyhorn (2015) show that eligibility for payday loans has no effect on credit scores and few measurable effects on the work performance of service members.

direct measures of payday loan use, our data link payday borrowing to the timing of income receipt at the individual level. We document a novel relationship between income timing and short-term borrowing, which suggests that at least some consumers use credit to cope with predicable variation in liquidity that could be smoothed cheaply using short-term saving.

This paper proceeds as follows. Section II describes our datasets and the institutional features of Social Security benefits payments. Section III presents our empirical approach and results main results, Section IV describes our measurement and analysis of timing mismatch between income and expenditure commitments, and Section V concludes.

II Data Sources and Background on Social Security Benefits

In the first section below, we describe the Social Security benefits schedule and our source of variation in income timing. The next two sections describe our datasets. Our first dataset includes bank and credit card transactions for consumers who signed up to an online account aggregator. Our second dataset includes storefront payday loans made by several multi-state lenders. We detail the nature of each of these datasets and our methodologies for identifying the analysis sample of Social Security recipients within each dataset.

II.A Social Security Benefits Timing

The Social Security Administration disburses benefits according to five distinct pay schedules, which are based on the nature of benefits, the date of benefit onset, and beneficiary birth dates. Figure 1 shows the SSA disbursement schedule for 2011.[8] Supplemental Security Income (SSI) beneficiaries and beneficiaries that began benefits before May 1997 receive payments near the beginning of each month. We exclude these groups from our analysis for several reasons.[9] First, because they are always paid near the beginning of the month, we have limited ability to disentangle pay cycle effects from day-of-month effects. Secondly, these groups have little variation in the lengths of their pay periods, and the variation that exists is correlated with seasonality and the timing of weekends in a

[8] Our sample periods also cover parts of 2010 through 2015, and the disbursement calendar follows similar patterns in each of these years.

[9] In an earlier draft of this paper, we show that these two groups exhibit storefront payday borrowing patterns consistent with the Wednesday groups we currently focus on.

month. Finally, SSI recipients are demographically distinct from retirees and disability recipients, so differences between the SSI and pre-1997 groups and the post-1997 groups could be driven either by these demographic differences or by differences in income timing.

We focus our analysis on beneficiaries who began receiving benefits on May 1, 1997 or later, and who do not also receive SSI benefits. The timing of benefit income for this group is based on the primary beneficiary's date of birth. Individuals born between the 1st and 10th, 11th and 20th, and 21st and 31st of the month are assigned to pay dates on the second, third, and fourth Wednesday of each month. We term these three groups of beneficiaries the "Wednesday groups."

The SSA disbursement schedule for the Wednesday groups generates pay periods that are either 28 or 35 days long. Figure A-1 shows the distribution of 35-day ("long") pay periods during the years of our sample period. A month is marked as "long" in the figure if the Wednesday group pay periods that begin in that calendar month are 35 days long. Most years include four long pay periods, and we do not observe any systematic seasonal pattern in the occurrence of long pay periods during our sample period.

The pay dates in the SSA disbursement schedule reflect both the date of direct deposits and, for beneficiaries who have not signed up for direct deposit, the date that checks should arrive in the mail. According to Congressional testimony by the SSA, as of September, 2012, 94% of Social Security benefits payments were made through direct deposit. The prevalence of direct deposit is likely to be even higher within our samples. Payday borrowers must have bank accounts in order to obtain a loan, so it is likely that the vast majority of consumers in our storefront payday sample receive benefits through direct deposit. We identify Social Security beneficiaries in the account aggregator dataset using direct deposit transactions, so all individuals in that sample receive direct deposits from SSA by construction. For ease of exposition, we refer to benefits payments as "paychecks" and disbursement dates from the SSA calendar as "paycheck dates" or "pay dates" in the remainder of the paper.

II.B Account Aggregator Dataset

Our first dataset comes from an online account aggregation service. Account aggregators allow households to monitor their financial activities from across multiple financial institutions and accounts on a single webpage or smart-phone app. These services often include features such as budgeting, expense tracking, and bill payment. Dozens of companies currently provide such services, and our data come from one of these firms. Users of the service can enter the usernames and passwords to financial accounts from any financial institution into their aggregator account (e.g. checking, savings, credit card, brokerage, retirement, mortgage, and student loan). Our particular dataset is limited to checking account, savings account, and credit card transactions. The service automatically and regularly pulls data from the user's linked financial accounts. The result is a transaction-level dataset containing information similar to what is found on bank or credit card statements, including the amount, date, and description of each transaction.[10]

A significant limitation of this dataset is sample self-selection. Users of our account aggregation service voluntarily sign up for the service. Prior studies have shown that such self-selected users tend to be younger, more likely to be male, and higher-income than the general population (Baker 2014, Gelman et al. 2015). Furthermore, those who sign up may only link a subset of their bank and credit card accounts. We refer to users of the account aggregation service as "households" in this paper. Users may choose to include all financial accounts used by their household, but they may also choose to include only accounts for a subset of household members.

We construct our sample from a universe of 2.7M households that signed up with an undisclosed account aggregator. The sample begins in July of 2010 and ends in May of 2015. We identify a subset of households to use in our analysis based on their receipt of Social Security income. We identify Social Security transactions by querying bank transaction descriptions for the phrase "social security" or "soc sec." In order for a household to enter our sample, we require at least fifteen Social Security transactions. We then restrict to households that belong to one of three Wednesday groups based on the timing of their Social Security transactions. To be assigned to one of these groups, we require that at least 95% of Social Security receipts for a household occur within 1 day of one of the Wednesday schedules as indicated by the SSA disbursement calendar. In order to simplify the interpretation of the results, we exclude households with multiple Social Security recipients. After applying the above filters, we are left with 33,825 households.

Panel A of Table 1 describes the account aggregator sample. The average household receives

[10]See Baugh (2017) and Baugh, Ben-David and Park (2014) for more details on the data.

\$4,535 in income per month, \$1,346 of which comes from Social Security. Panel A of Figure 2 shows the distribution of Social Security direct deposit amounts for the three Wednesday groups. The distributions align very closely across the three groups, validating our sample identification method and the assumption of quasi-random assignment to a pay schedule. Because both of our datasets contain very limited demographic information, these income distributions provide our main test of covariate balance across Wednesday group assignment.

Average household spending is \$6,705 per month. We dis-aggregate spending into four subcategories: recurring bills, cash and check, discretionary expenses, and non-discretionary expenses. We consider three major categories of recurring monthly bills, and describe our procedure for identifying bills, bill due dates, and late bill payments in Section IV below. Seventy-eight percent of households have a recurring bill of any type in a given month according to our measure. Seventy percent have a recurring credit card payment, 32% have a mortgage, and 23% have a car payment. According to our measure, 12% of households are late on at least one recurring bill in a given month, defined as being at least seven days past the normal payment date. We aggregate remaining household expenditures into three broad categories. Cash and check payments add up to \$2,021 per month on average. Remaining "discretionary" expenditures, which we define as entertainment, restaurant, retail, and travel, are \$869 on average. We categorize other bills, gas, groceries, health and loan payment expenses as "non-discretionary," and they are \$1,209 per month on average.[11]

II.C Payday Loan Dataset

Our second data source is a multi-lender administrative dataset of payday loans that was collected by the Consumer Financial Protection Bureau.[12] Payday loans are a common form of short-term credit used by low and middle-income consumers, with principal amounts typically ranging between \$300 and \$500, and costs ranging from \$10-20 per \$100 borrowed.[13] To obtain a payday loan, borrowers submit a pay stub to the lender and provide either a post-dated check or electronic

[11] In the table, we only count credit card payments in total expenses. We exclude credit card purchases from the discretionary and non-discretionary expense categories to avoid double-counting. Thus, the expenditures shown in the table diverge from total household spending for those that are accumulating or decumulating credit card debt.

[12] Because the data are Confidential Supervisory Information, this paper only presents results that are aggregated and do not identify specific lenders. As a further precaution, we do not reveal how many lenders are included in the analysis.

[13] About 5% of U.S. households report ever having used a payday loan (Current Population Survey, June 2013).

debit authorization for the principal plus fee amount, due on an upcoming payday.[14] Although the duration of a typical loan is only 14-30 days, it is very common for borrowers to roll over or reborrow within a few days of the due date, leading to longer-term debt sequences.[15]

The dataset includes several large payday lenders, and covers information on all payday loans extended via brick-and-mortar storefronts by each lender. Each lender's sample covers a 12-month period between 2010 and 2012. Because borrowers must present a pay stub in order to obtain credit, lenders are able to observe both the source and level of income. We observe the recorded income source and income amount of each customer in our dataset, and restrict to borrowers who report income from Social Security benefits when applying for loans.[16]

For each loan, we observe the principal and fee amounts, origination date, payment due date, and actual payment date. An anonymized customer identifier allows us to identify all loans made by a given lender to the same consumer during the sample period.[17] One limitation of the data is that income information is typically only recorded the first time a borrower applies for a loan, so it may be less accurate for consumers who have been borrowing from the same lender for an extended period of time. We also cannot observe whether borrowers have more than one income source individually or within their households.

Because of the prevalence of roll-overs or renewals of payday loans (Skiba and Tobacman 2008, Carter, Skiba and Sydnor 2013, Burke et al. 2014), we limit our analysis to "fresh" loans. Renewal loans are typically both originated and due on pay dates, and result in little to no new funds to borrowers. Thus, renewals are uninformative about the timing of liquidity needs with respect to income timing. We define fresh loans as those to borrowers who have gone at least one pay cycle without borrowing.[18] Overall, we observe several hundred thousand fresh loans taken out by several hundred thousand borrowers who report income from Social Security benefits in our sample.[19]

[14]Most loans are due on the borrower's next payday. Loans made just prior to payday are often due on the following payday.

[15]See CFPB (2013) and Burke, Lanning, Leary and Wang (2014) for more details on payday loans, borrowing patterns, and our dataset.

[16]As reported in CFPB (2013), 18% of borrowers in the underlying dataset report income from public assistance and benefits payments, the majority of which is comprised of Social Security payments.

[17]The data used in this analysis contain no direct consumer identifiers, such as names or addresses.

[18]Most renewals occur within seven days of repayment of the previous loan, with much of the variation in the timing of reborrowing driven by state laws that impose cooling-off-periods between loans. Alternative definitions of fresh loans based on borrowers not having a prior loan within the previous 30 or 60 days yield very similar results.

[19]Exact numbers have been shrouded to protect the confidentiality of lender identities.

We match payday borrowers to one of the five Social Security disbursement groups based on their loan maturity dates. We find that 75% of all loans made to borrowers who report Social Security income are due exactly on a benefits disbursement date. The vast majority of the remainder fall within three days before or after a disbursement date. By using the modal disbursement group for the due dates of each borrower's loans, we are able to categorize 97% of all borrowers who report Social Security income into one of the five disbursement groups.[20] The remaining 3% of borrowers typically have only a single loan in the sample, and we exclude them from the analysis. In the remainder of the paper, we focus only on borrowers in one of the three Wednesday groups as identified by this method.

Panel B of Table 1 shows summary statistics for our sample of Social Security beneficiaries. The first set of rows presents summary statistics for loan terms at origination for fresh loans. Mean loan principal is $305 and the mean fee is $47, for an average total repayment amount of $352. The mean cost per one hundred dollars borrowed is $16 and the mean contract duration is 21 days, resulting in typical APRs around 350%. The next set of rows describes borrower income and annual loan usage. The average net monthly income from benefits is $962, significantly lower than in our account aggregator dataset. Panel B of Figure 2 shows the distributions of net monthly benefits income in the storefront payday sample. While the distributions are shifted significantly to the left compared with the account aggregator sample, they align closely across the three Wednesday groups.

Borrowers take out an average of seven loans per year, consisting of a single fresh loan followed by six renewals. Total fresh credit measures the amount of credit taken out that is not used to repay a prior loan, i.e. that is available for consumption. For each borrower, total fresh credit is calculated as the sum of the loan principal amounts of the largest loan in each loan sequence.[21] The average amount of fresh credit is $427, on which borrowers pay $370 in fees. The borrower-level statistics show that many payday loan sequences extend over multiple months, and total annual fees average more than one third of a month's net benefits income. Thus, factors that influence

[20] Using the modal group also disambiguates the SSI and mixed groups. In most months, payment dates are unique to each of the disbursement groups, but in 2011 there were two months in which the SSI group and the mixed group received benefits on the same day.

[21] A loan sequence is defined as a set of consecutive loans taken out within one pay period of the due date of the previous loan.

the initial borrowing decision, the focus of our study, can have substantial economic effects on borrowers.

The storefront payday dataset has both advantages and disadvantages compared to the account aggregator dataset. Because consumers self-select into signing up with the account aggregator, we cannot directly extrapolate from this sample to the financial behavior of retail bank consumers overall. In contrast, the storefront payday sample includes the universe of loans from several large lenders covering a significant fraction of the market. Because payday loans are a simple and fairly homogeneous product, our sample is more likely to be broadly representative of the payday borrowing patterns for the general population of Social Security beneficiaries.

III Income Timing and Financial Shortfalls

III.A Identifying Variation and Econometric Model

As described in Section II, Social Security beneficiaries who started receiving benefits after May 1, 1997 and who do not also receive SSI are assigned to pay dates on one of three Wednesdays each month based on their date of birth. Our key identification assumption is that Wednesday group assignment is as good as random with respect to ex ante household characteristics. And furthermore, we assume that Wednesday group assignment only affects financial outcomes through its impact on income timing.

Because we have very limited demographic information on households, the main test of our identification assumption is to observe whether the distribution of monthly Social Security income is identical across Wednesday groups. While Social Security income is a function of lifetime earnings history and age at claiming, the effect of Wednesday group assignment on income timing does not come into play until after a beneficiary begins claiming benefits. Thus, pre-existing differences across Wednesday groups that are unrelated to benefits timing should show up in these income distributions. We present these distributions in Figure 2 for both datasets, and they indeed line up very closely across Wednesday groups.

The Social Security disbursement schedule generates variation in both the length of pay periods and the timing of income within a month. Because the pay dates for the Wednesday groups are spread throughout the calendar month, we are able to separately identify these income timing effects from other sources of calendar-time variation. The main specifications for our account aggregator analysis take the following form:

$$Y_{gt} = \beta_1 Long_{gt} + \beta_2 WedGroup_g + \beta_3 PayCycle_{gt} + \gamma X_t + \epsilon_{gt} \tag{1}$$

where Y_{gt} is a measure of the average incidence of overdrafts, bounced checks, or online payday borrowing for recipient group g on day t. Since income timing varies at the disbursement group level, we collapse the data into group-day cells for efficient estimation. $Long_{gt}$ is a dummy variable which equals 1 if day t is in a 35-day pay period, and 0 otherwise. $WedGroup_g$ is a vector of dummies for the third and fourth Wednesday groups, with the second Wednesday group as the omitted category. $PayCycle_{gt}$ is a vector of dummies for the number of days since last paycheck, and X_t is a vector of fixed effects for day of month, calendar month, and calendar year. The β's represent the set of coefficients of interest, measuring the effect of pay period length, pay date timing, and daily patterns over the pay period. Under the lifecycle / permanent income hypothesis, the values of all of the β terms should be zero.

The specification differs slightly for the storefront payday analysis:

$$Ln(Y_{igt}/N_g) = \beta_1 Long_{gt} + \beta_2 WedGroup_g + \beta_3 PayCycle_{gt} + \gamma X_{it} + \epsilon_{igt} \tag{2}$$

Since the lenders in our sample span different time periods, we collapse the data into group-day-lender cells instead of group-day cells, and include lender fixed effects in X_{it} to absorb differences in lender size. In contrast to the account aggregator data, the storefront payday dataset does not allow us to directly measure the propensity to take out payday loans among some underlying sample of consumers. We only observe consumers who actually borrow. To impute the effect of Wednesday group assignment on borrowing propensity, we measure storefront payday borrowing in logs, and normalize by the number of recipients N_g in each disbursement group, which we obtained from SSA. This specification allows us to measure the effect of income timing on the log changes in the propensity to borrow, and by exponentiating the β coefficients we are able to obtain percentage changes which are roughly analogous to the β's from the account aggregator results.

III.B Main Results

Table 2 presents our results for the effects of income timing on the daily incidence of four measures of financial shortfalls. Overdrafts, bounced checks, and online payday borrowing are measured in the account aggregator dataset, while storefront payday borrowing comes from the storefront payday dataset. The first row reports the sample means for each outcome. For Social Security beneficiaries in the account aggregator dataset, an average of 0.7% experience an overdraft on a given day. The daily incidence of bounced checks is 0.2% per day, online payday borrowing is 0.01% per day, and storefront payday borrowing is 0.05% per day.[22] All of the coefficients in the table are reported in terms of relative percentage changes from the baseline borrowing rates.[23]

The results show that the incidence of financial shortfalls is significantly higher during long versus short pay periods. From a baseline of 0.7% per day, households in the aggregator sample are 5% more likely to experience an overdraft on a given day in a 35-day pay period compared to a 28-day pay period. They are 3% more likely to experience a bounced check, and 16% more likely to take out an online payday loan during long pay periods. Individuals are 31% more likely to take out a storefront payday loan during long pay periods. Because our outcomes are measured at a daily level, this effect is not driven mechanically by the greater likelihood of experiencing a negative liquidity shock during a longer measurement period.

We next turn to the effects of paydate timing as proxied by Wednesday group assignment. Under our identifying assumptions, the only factor that should affect financial outcomes across groups is the timing of income within the month. The coefficients on the Wednesday group dummies indicate that while the third Wednesday has the same or higher levels of financial shortfalls compared to the second Wednesday group, the fourth Wednesday group have significantly fewer shortfalls. Compared to the second Wednesday group, those in the third Wednesday group are 3% less likely to overdraft, 10% less likely to have a bounced check, 14% less likely to take out online payday

[22]To estimate the baseline storefront payday borrowing propensity, we inflate the observed loan volume per day in our sample by the market share of firms in our sample in 2010 using estimates from Stephens (2011). We then divide by the total number of Social Security recipients in each disbursement group using exact counts provided by the Office of Research, Evaluation, and Statistics at the Social Security Administration. Our estimate of borrowing propensity relies on the assumption that borrowing rates within our storefront payday sample are representative of borrowing by Social Security beneficiaries as a whole.

[23]The account aggregator results are reported by normalizing the β coefficients from Equation (1) by the baseline borrowing rates. The storefront results are reported by exponentiating the coefficients from Equation (2).

loans, and 4% less likely to take out storefront payday loans. We describe potential explanations for these results in the next section.

While both the account aggregator and storefront payday results show consistent directional effects of income timing, we caution against directly comparing the magnitudes of the effects across the two datasets. As described above, the the account aggregator sample is composed of a self-selected group of individuals who chose to link their accounts to an online personal financial management service. While we believe that our research design ensures that the magnitudes are internally valid within our sample, we cannot directly extrapolate to the effects of income timing on financial shortfalls within bank accounts for the general population of Social Security beneficiaries. We would expect overall that the aggregator results under-estimate baseline levels of bank account shortfalls in the general population. Our storefront payday sample covers all payday loans made by several large lenders, which constitute a significant share of the market, so they are likely to be more representative of the payday borrowing behavior of beneficiaries as a whole.

To examine the differences between long and short pay periods in more detail, we repeat the specification from Table 2, but instead of including a dummy for long months, we estimate the effects for each day of long and short pay periods separately. Figure 3 plots the results of these regressions. The first day of short pay periods is the omitted category, so all coefficients can be interpreted as differences relative to that date. Consistent with the positive coefficient on the long pay period dummy in Table 2, the estimated coefficients for financial shortfalls during long pay periods (red triangles) are generally larger than during short pay periods (blue circles).

The figures also show pronounced weekly cycles, especially for the account aggregator outcomes. Overdrafts, bounced checks, and online payday transactions decline significantly on Fridays, Saturdays, and Sundays, before jumping up on Mondays. We interpret the weekly cycles as a combination of weekly cycles in bank transactions initiated by consumers and idiosyncrasies of the way bank transactions are posted and processed on weekends.

Independently of the weekly cycles, the incidence of financial shortfalls also evolves over the pay cycle. While overdrafts and bounced checks increase over the pay cycle, both online and storefront payday lending decrease over the pay cycle, evidence of substitution from active borrowing toward (intentional or unintentional) banking shortfalls as liquidity declines. A number of factors could account for this pattern. Since payday loan fees are constant for each loan cycle and do not depend on the duration of the loan, households may rationally borrow at the beginning of the pay cycle to minimize their implicit periodic interest rates.[24] Overdrafts and bounced checks can be triggered unintentionally as bank transactions are processed, and mechanically increase as bank account liquidity declines over the pay cycle. Thus, the substitution patterns we observe could also be a "passive" side effect of automated bank account features that mitigate the need for active borrowing late in the pay cycle. With transaction data alone, we have limited ability to distinguish between these explanations. The effect of product substitution over the pay cycle on overall household well-being is ambiguous, and is an interesting question for future research.

IV Income Timing and Expenditure Commitments

One of the main results from Section III is that individuals assigned to receive income at different times of the month experience systematically different rates of financial shortfalls. In this section, we explore whether an interaction between income timing and expenditure commitments can explain these results. In particular, we hypothesize that households facing a greater mismatch between the timing of income and expenditure commitments within a month may find it harder to budget their discretionary cashflows during the interim, and have a higher likelihood of running out of liquidity by the time lumpy bills need to be paid.

This hypothesis is motivated by our finding that the fourth Wednesday group experiences fewer financial shortfalls than the second Wednesday group. Prior studies have found that large expenditure commitments disproportionately cluster near the beginning of the month (Evans and Moore 2012, Vellekoop 2012), so we posit that the smaller gap between the fourth Wednesday of the month and expenditures due at the beginning of the next month could account for this group's relative financial health. An important corollary to this hypothesis is that households do not strategically adjust the timing of their expenditure commitments to match their income. If households were able to fully adjust their expenditure timing to match their income flows, then we should not observe any effects of income timing through this channel.

[24]However, as shown in Figure A-2, loans taken out near the end of pay periods are typically due one pay date later, so implied APR is non-monotonic over the pay cycle, and cannot fully account for the monotonic decline in payday borrowing we observe.

To test this timing mismatch mechanism, we first construct novel measures of household expenditure commitments and timing mismatch between income and expenditures. We then test whether our timing mismatch measure has explanatory power for household exposure to financial shortfalls. Because we can only measure expenditures in the account aggregator data, we focus exclusively on that dataset for the analysis in this section.

IV.A Measuring Recurring Expenditure Commitments and Timing Mismatch

We measure expenditure commitments among the three largest categories of recurring bills that we can identify in the account aggregator sample: credit cards, mortgages, and car payments.[25] As shown in Table 1, these three categories make up $2,607 out of $6,705 in total monthly expenditures, and 79% of households have at least one of these recurring expenditures in a given month. We identify repeated observations of the same bill within a household using the transaction category (i.e. credit card, mortgage, or car) and the text string description. For instance, we would classify a given transaction as a recurring credit card bill if it is part of a series of transactions from the same household that share the description "Citi Credit Card." We restrict to bills that appear consistently on a monthly frequency by requiring a given bill to appear at least 10 times, and with an average number of bill payments per month between 0.5 and 1.5. Using this method, we identify 127,263 individual bills associated with 29,854 households.

We do not observe the contractual terms of expenditure obligations, so we cannot directly observe bill due dates. Since we observe multiple observations of each bill, we impute due dates using the modal day of the month in which a bill is paid. The left column of Figure 4 plots histograms of the imputed due dates for each bill category across the three Wednesday groups. The figures present plausible empirical distributions of bill due dates, with a large fraction of bills clustered around the first of the month in each category. Credit card due dates are more dispersed throughout the month, while mortgage due dates nearly always occur close to the first of the month. Car payment due dates cluster around both the first and fifteenth of the month. Strikingly, the distribution of due dates is similar across the Wednesday groups for all three bill categories.

[25] Car payments include both loans and leases. While rent payments are also likely to be substantial, we are unable to differentiate them in our data because most rent payments are paid by check, and we do not have payee descriptions associated with check transactions.

The uniformity across Wednesday groups provide initial evidence that most households do not strategically adjust bill due dates to match the timing of income – either because they do not want to, because they are not able to, or both.

Because due dates are roughly similar across Wednesday groups, their systematic differences in income timing drive variation in the mismatch between pay dates and bill due dates. We define "timing mismatch" as the number of days between a bill's imputed due date and the household's most recent Social Security paydate. The right column of Figure 4 presents the distributions of timing mismatch by bill type. The differences in timing mismatch across Wednesday groups is most striking for mortgages, with the third and fourth Wednesday groups showing progressively less mismatch compared with the second Wednesday group. The distributions of timing mismatch are more dispersed for credit cards and car payments, with bimodal patterns that vary less distinctively across Wednesday groups.

Table 3 presents formal tests of differences in due dates and timing mismatch across Wednesday groups. We collapse the data to the bill level, and regress the bill due date and average timing mismatch on dummy variables for the third and forth Wednesday groups, with the second Wednesday group as the omitted category. Columns (1) through (3) of the table present regressions for each bill category separately, and column (4) pools all bills together and adds dummies for bill category to the regressions.

In Panel A, the dependent variable is the normalized bill due date:

$$NormDueDate = mod(DueDate - 24, 31).$$

Because many due dates cluster around the beginning of the month, directly using the day of the month a bill is due as the dependent variable can lead to misleading results, since a due date on the 30st is effectively very similar to a due date on the 1st. To correct for this issue, we normalize bill due dates by 24 since 24th is the least common due date.[26] The results suggest that households engage in little strategic adjustment of bill due dates based on the timing of their income. While the pay dates for the second and fourth Wednesday groups are fourteen days apart, the average difference in bill due dates is only about one day. Nonetheless, the negative and significant coefficients on the

[26]Only 1.7% of bills are due on the 24th, compared with 9.3% on the 1st.

fourth Wednesday dummy are consistent with limited adjustment of due dates toward pay dates.

Panel B shows regressions where the dependent variable is the average number of days between a bill's due date and the previous Social Security payment, averaged across all observations of a bill. As shown in the first row, the average timing mismatch is about 15 days. The results show that the fourth Wednesday group has systematically less timing mismatch compared with the second Wednesday group. Based on the pooled regression in column (4), the fourth Wednesday group has an average of 2.7 fewer days of timing mismatch per bill compared with the second Wednesday group. Timing mismatch does not vary consistently between the second and third Wednesday groups, owing to the prevalence of credit card and car payment due dates in the middle of the month.

IV.B The Effect of Timing Mismatch on Financial Shortfalls

In this section, we test whether timing mismatch between income and expenditure commitments plays a role in household financial shortfalls. As discussed above, timing mismatch is systematically related to Wednesday group assignment, so it can potentially explain why the Wednesday groups experience different levels of financial shortfalls.

To test for the role of timing mismatch, we run regressions of the following form:

$$Y_{ht} = \beta_1 Long_{ht} + \beta_2 TimingMismatch_h + \beta_3 PayCycle_{ht} + \gamma X_t + \epsilon_{ht} \tag{3}$$

where $TimingMismatch_h$ for household h is defined as

$$TimingMismatch_h = \sum_{b \in bills(h)} \frac{Amount_b(DueDate_b - LastPaydate_b)}{\sum_{b \in bills(h)} Amount_b}$$

$TimingMismatch_h$ captures the weighted-average number of days between a household's bill due dates and the income payments, weighted by the dollar amount of each bill. For households with no recurring bills, we set $TimingMismatch_h = 0$. Figure 5 shows the distribution for this household-level measure of timing mismatch.

We run both OLS results from equation (3) and IV regressions where $TimingMismatch$ is instrumented by Wednesday group assignment in the following first stage regression:

$$Timing\hat{M}ismatch_h = \beta WedGroup_h + \epsilon_h \tag{4}$$

Since timing mismatch varies at the household level, we run these regressions on household-day cells instead of collapsed group-day cells. The results are shown in Table 4. Panel A of Table 4 shows the OLS results. While timing mismatch is positively related to overdrafts, as expected, it is negatively related to bounced checks and online payday borrowing in the OLS specification. When we instrument for timing mismatch using Wednesday group assignment, we find the expected positive relationship between timing mismatch and financial shortfalls. A one-week increase in timing mismatch is associated with a 10% increase in the incidence of overdrafts per day, a 34% increase in bounced checks, and a 47% increase in online payday borrowing. These magnitudes are larger than the reduced-form effect of Wednesday group assignment we found in Table 2, consistent with the exclusion restriction that Wednesday group assignment acts through the timing mismatch channel. However, given that our instrument relies entirely on assignment to three discrete groups, our standard errors are large, and the coefficients on overdrafts and online payday borrowing in the IV specification are statistically indistinguishable from zero.

V Conclusion

This paper sheds light on the nature of intra-month household budgeting, providing evidence that consumers suffer from systematic financial shortfalls in response to predictable variation in income timing. Households suffer more shortfalls during long pay periods compared with shorter ones, even though the pay schedule is known in advance. Households assigned to receive income at different times of the calendar month also have permanently different levels of financial shortfalls. By constructing novel measures of the timing of household expenditure commitments, we find evidence that the latter effect is driven by mismatch between the timing of income and expenditures. Despite these patterns, most households do not strategically adjust bill due dates to match the timing of their income – either because they do not want to, because they are not able to, or both.

Our results highlight the need for better tools and policies to help consumers match the timing of their income and expenditures. Such tools are increasingly available through traditional banks, payroll providers, and financial technology firms, and could help consumers avoid high-cost borrowing and other costs of financial shortfalls.

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Figure 1: Social Security Administration Disbursement Calendar

Schedule Of Social Security Benefit Payments 2011

JANUARY 2011

S	M	T	W	T	F	S
						1
2	3	4	5	6	7	8
9	10	11	12	13	14	15
16	17	18	19	20	21	22
23	24	25	26	27	28	29
30	31					

FEBRUARY 2011

S	M	T	W	T	F	S
		1	2	3	4	5
6	7	8	9	10	11	12
13	14	15	16	17	18	19
20	21	22	23	24	25	26
27	28					

MARCH 2011

S	M	T	W	T	F	S
		1	2	3	4	5
6	7	8	9	10	11	12
13	14	15	16	17	18	19
20	21	22	23	24	25	26
27	28	29	30	31		

APRIL 2011

S	M	T	W	T	F	S
					1	2
3	4	5	6	7	8	9
10	11	12	13	14	15	16
17	18	19	20	21	22	23
24	25	26	27	28	29	30

MAY 2011

S	M	T	W	T	F	S
1	2	3	4	5	6	7
8	9	10	11	12	13	14
15	16	17	18	19	20	21
22	23	24	25	26	27	28
29	30	31				

JUNE 2011

S	M	T	W	T	F	S
			1	2	3	4
5	6	7	8	9	10	11
12	13	14	15	16	17	18
19	20	21	22	23	24	25
26	27	28	29	30		

JULY 2011

S	M	T	W	T	F	S
					1	2
3	4	5	6	7	8	9
10	11	12	13	14	15	16
17	18	19	20	21	22	23
24	25	26	27	28	29	30
31						

AUGUST 2011

S	M	T	W	T	F	S
	1	2	3	4	5	6
7	8	9	10	11	12	13
14	15	16	17	18	19	20
21	22	23	24	25	26	27
28	29	30	31			

SEPTEMBER 2011

S	M	T	W	T	F	S
				1	2	3
4	5	6	7	8	9	10
11	12	13	14	15	16	17
18	19	20	21	22	23	24
25	26	27	28	29	30	

OCTOBER 2011

S	M	T	W	T	F	S
						1
2	3	4	5	6	7	8
9	10	11	12	13	14	15
16	17	18	19	20	21	22
23	24	25	26	27	28	29
30	31					

NOVEMBER 2011

S	M	T	W	T	F	S
		1	2	3	4	5
6	7	8	9	10	11	12
13	14	15	16	17	18	19
20	21	22	23	24	25	26
27	28	29	30			

DECEMBER 2011

S	M	T	W	T	F	S
				1	2	3
4	5	6	7	8	9	10
11	12	13	14	15	16	17
18	19	20	21	22	23	24
25	26	27	28	29	30	31

Benefits paid on	Birth date on
Second Wednesday	1st – 10th
Third Wednesday	11th – 20th
Fourth Wednesday	21st – 31st

Supplemental Security Income (SSI)

Beneficiaries receiving benefits prior to May 1997 or receiving both Social Security benefits and SSI payments

Please allow three additional mailing days before contacting the Social Security Administration to report nonreceipt of your payment.

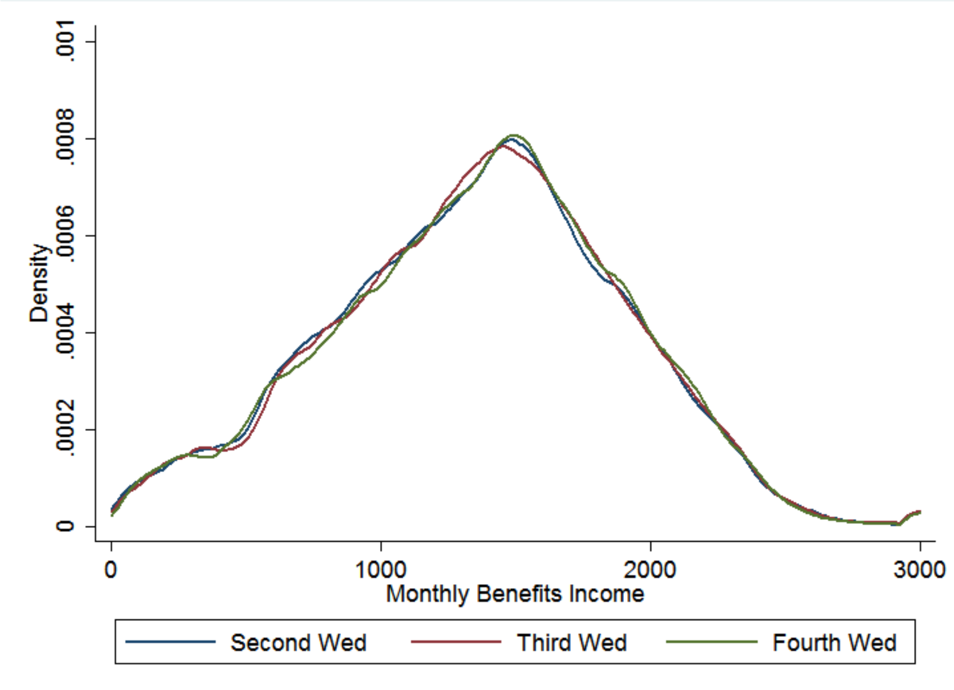
w w w . s o c i a l s e c u r i t y . g o v

Social Security Administration
SSA Publication No. 05-10031
ICN 456100
Unit of Issue - HD (one hundred)
January 2011 (Destroy prior editions)

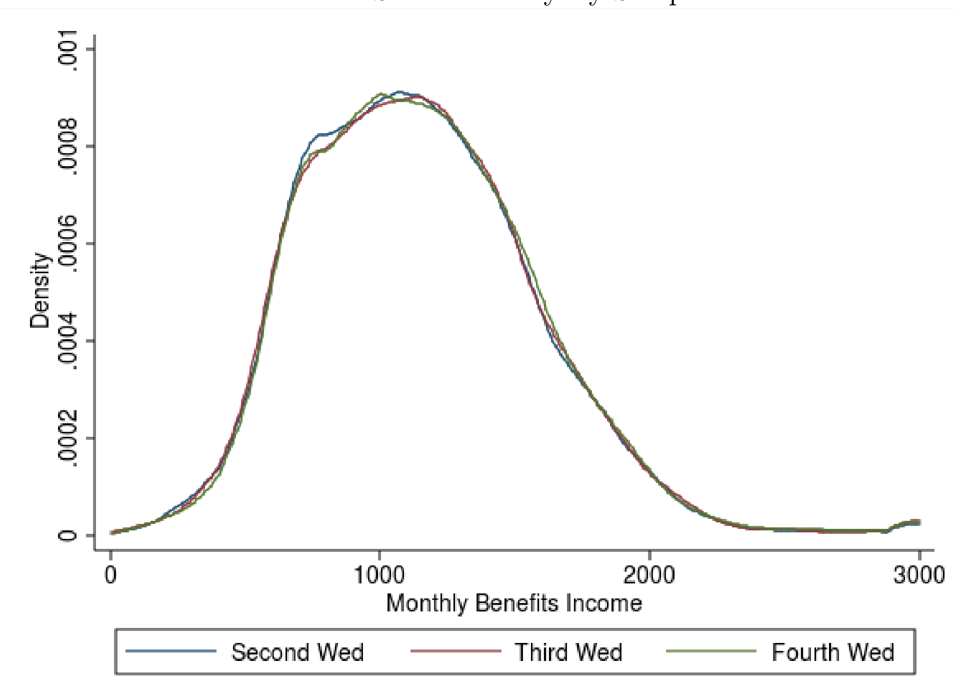
Source: www.socialsecurity.gov/pubs/calendar2011.pdf

Figure 2: Income Distributions By Disbursement Group

Panel A: Account Aggregator Sample

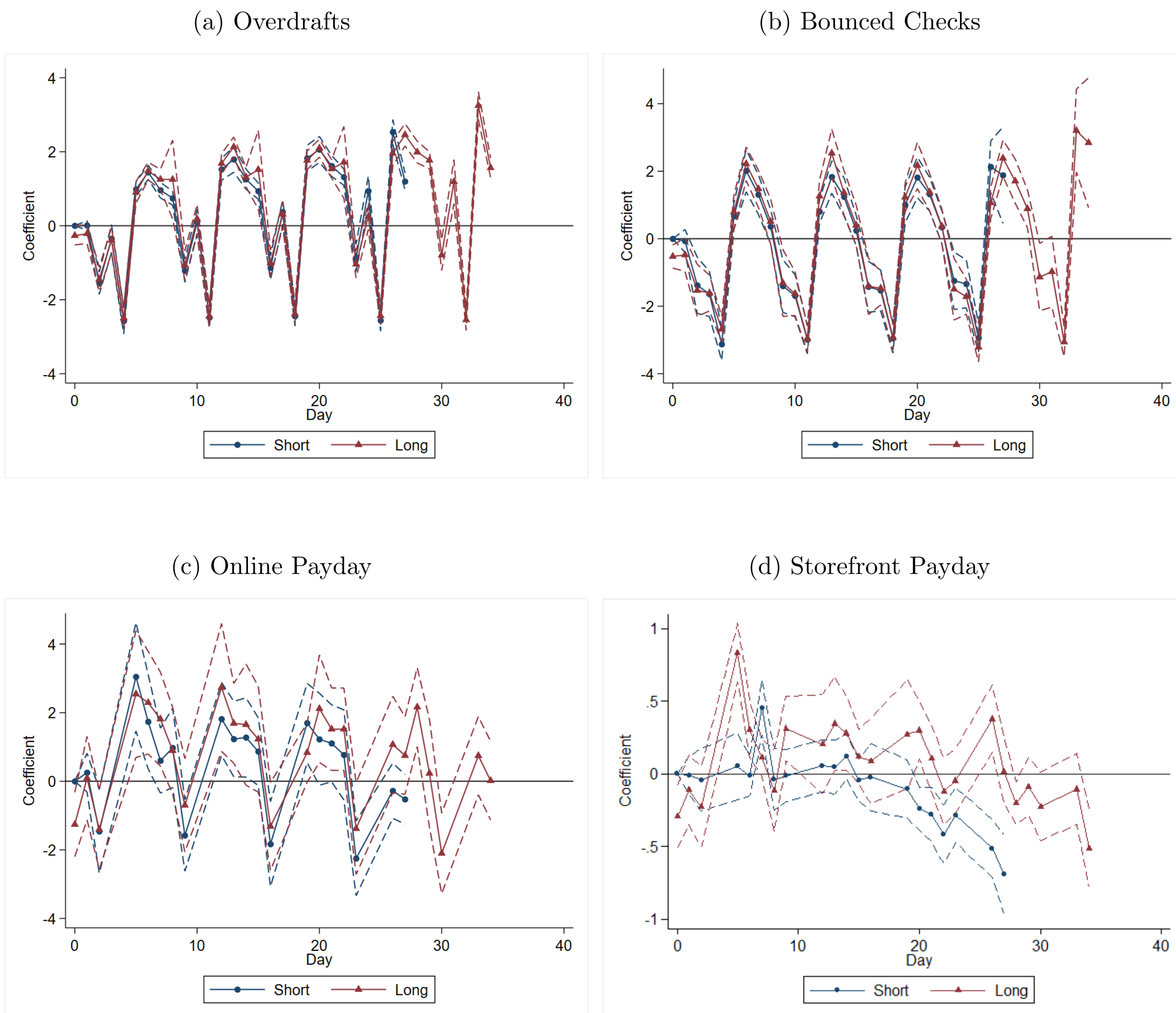


Panel B: Storefront Payday Sample



Note: The figure shows the distribution of net monthly benefits income for the three Wednesday Social Security disbursement groups in the account aggregator sample and storefront payday sample.

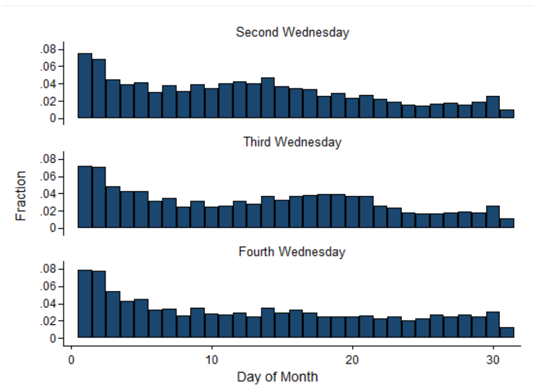
Figure 3: Financial Shortfalls Over Short and Long Pay Periods



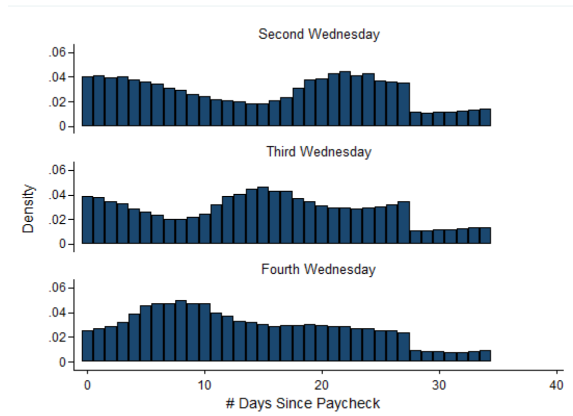
Note: The figure shows coefficient estimates and 95% confidence intervals corresponding to regressions of the daily incidence of financial shortfalls on indicators for the number of days since the last SSA paycheck. The regressions also include fixed effects for calendar year, calendar month, day of month, and Wednesday group. For the storefront payday outcome, the regression also include fixed effects for lender. Standard errors are clustered by Wednesday group interacted with calendar year. The coefficient magnitudes are shown as percentage changes relative to the baseline mean incidence of each outcome.

Figure 4: Due Dates and Timing Mismatch for Recurring Bills

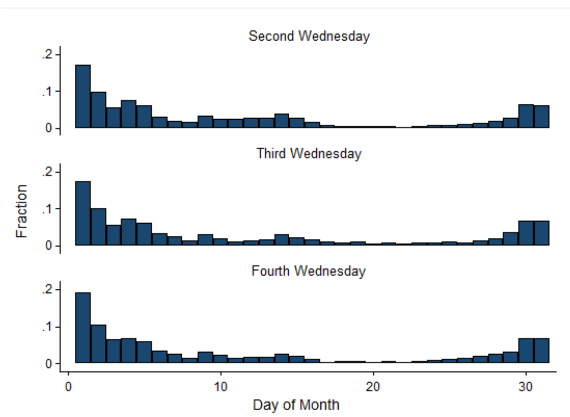
(a) Credit Card Due Date



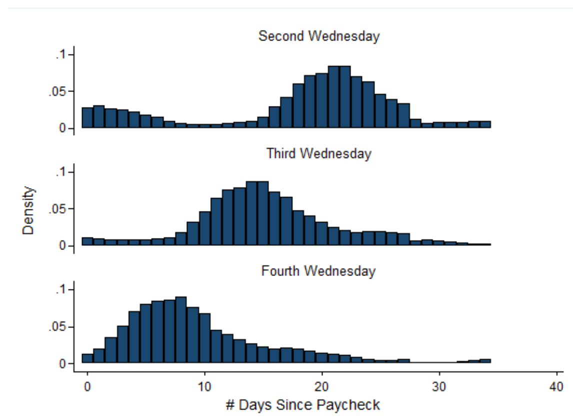
(b) Credit Card Timing Mismatch



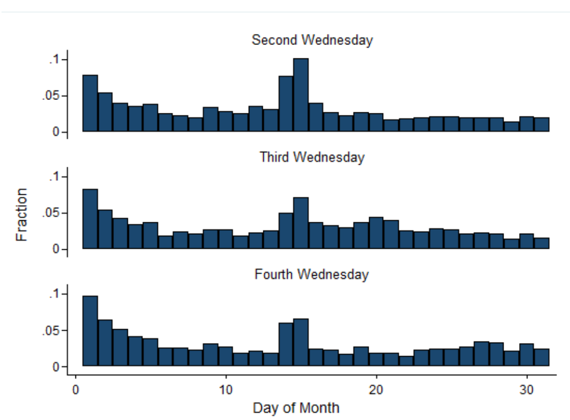
(c) Mortgage Due Date



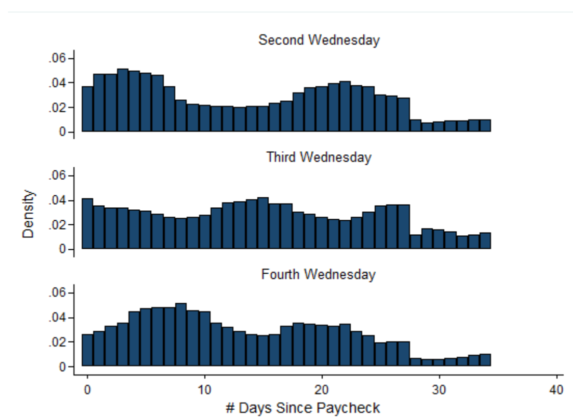
(d) Mortgage Timing Mismatch



(e) Car Payment Due Date

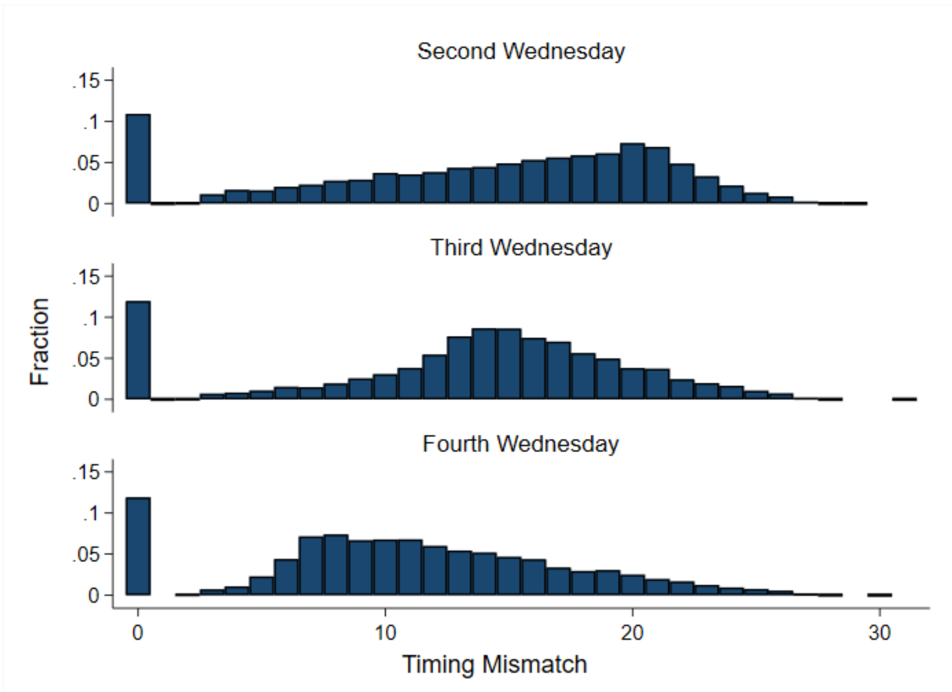


(f) Car Payment Timing Mismatch



Note: The figure shows histograms of bill due dates and timing mismatch between income and due dates, split by the Wednesday each accountholder receives Social Security income. Timing mismatch is defined as the number of days between a bill's due date and the most recent Social Security income payment.

Figure 5: Timing Mismatch Between Income and Expenditure Commitments



Note: This figure shows the distribution of timing mismatch between expenditure commitments and Social Security income, split by the Wednesday each accountholder receives Social Security income. Timing mismatch is defined as the average number of days between a bill's due date and the most recent Social Security check, weighted by the dollar amount of each bill. The bill categories included are credit cards, mortgages, and car payments. Households with no recurring bills are defined as having timing mismatch equal to zero. The distribution of due dates and mismatch for each subcategory of bills is shown in Figure 4. See text for details.

Table 1: Summary Statistics

Panel A: Account Aggregator Data					
	Mean	Median	Std. Dev.	Has Recurring	Has Late
Income	$4,535	$3,347	$4,139		
Social Security Income	$1,346	$1,387	$562		
Salary	$1,249	$0	$2,936		
Retirement	$857	$0	$1,708		
Benefits and Other	$1,084	$0	$2,298		
Expenses	$6,705	$5,150	$6,801		
Recurring	$2,607	$1,761	$2,874	79%	12%
Credit Card Payment	$1,788	$1,000	$2,225	70%	12%
Mortgage	$647	$0	$1,149	32%	2%
Car Payment	$172	$0	$358	23%	3%
Cash and Check	$2,021	$945	$4,659		
Discretionary	$869	$557	$1,007		
Non-discretionary	$1,209	$960	$1,038		
Financial shortfalls	13%	0%	34%		
Overdraft	11%	0%	32%		
Bounced Check	3%	0%	18%		
Online Payday	0.2%	0%	4%		
Panel B: Storefront Payday Data					
Loan characteristics:					
Loan amount total	$352	$306	$169		
Principal	$305	$255	$149		
Finance charge	$47	$45	$25		
APR	352%	282%	260%		
Cost per 100	$16	$15	$4		
Contract duration (days)	21.0	20	10.5		
Customer characteristics:					
Monthly benefits income	$962	$864	$503		
Total # of loan cycles	7.0	7	4.2		
# of fresh loans	1.1	1	0.4		
# of rollover cycles	5.9	5	4.2		
Total fresh credit	$427	$400	$224		
Total fees	$370	$320	$288		
Total days indebted	196	195	121		

Table 2: Income Timing and Financial Shortfalls

	(1)	(2)	(3)	(4)
Dataset:	Account Aggregator			Storefront Payday
LHS:	OD	Bounced	Online Payday	Storefront Payday
Sample Mean:	0.70%	0.19%	0.01%	0.05%
Long pay period	0.047	0.028	0.162	0.308
	(0.011)	(0.011)	(0.061)	(0.062)
	[0.001]	[0.020]	[0.017]	[0.000]
Third Wednesday Dummy	0.041	0.110	0.159	0.002
	(0.011)	(0.017)	(0.058)	(0.022)
	[0.001]	[0.000]	[0.014]	[0.925]
Fourth Wednesday Dummy	- 0.026	- 0.104	- 0.142	- 0.036
	(0.013)	(0.021)	(0.061)	(0.021)
	[0.059]	[0.000]	[0.033]	[0.107]
Fixed effects included:				
Lender	N	N	N	Y
Calendar year	Y	Y	Y	Y
Calendar month	Y	Y	Y	Y
Day of month	Y	Y	Y	Y
R^2	0.682	0.571	0.189	0.916

Note: Table shows results of regressions of daily overdrafts, bounced checks, online payday borrowing, and storefront payday borrowing on the length of pay periods and the timing of income. Standard errors clustered by Wednesday group interacted with calendar year are shown in parentheses, and p-values are shown in brackets.

Table 3: Income Timing and Bill Due Dates

	(1)	(2)	(3)	(4)
Bill Type:	Credit card	Mortgage	Car	Any
	Panel A: Bill Due Date (Days)			
Third Wednesday Dummy	0.302	- 0.288	0.136	0.188
	(0.074)	(0.119)	(0.167)	(0.061)
	[0.000]	[0.016]	[0.416]	[0.002]
Fourth Wednesday Dummy	- 1.149	- 0.991	- 1.793	- 1.201
	(0.075)	(0.115)	(0.165)	(0.062)
	[0.000]	[0.000]	[0.000]	[0.000]
N	91,797	20,358	15,108	127,263
R^2	0.006	0.005	0.011	0.038
	Panel B: Timing Mismatch Between Due Date and Previous Paycheck (Days)			
Sample Mean:	15	15	14	15
Third Wednesday Dummy	- 0.080	- 2.794	1.136	- 0.374
	(0.063)	(0.109)	(0.147)	(0.053)
	[0.200]	[0.000]	[0.000]	[0.000]
Fourth Wednesday Dummy	- 1.760	- 8.434	- 0.709	- 2.704
	(0.068)	(0.114)	(0.148)	(0.057)
	[0.000]	[0.000]	[0.000]	[0.000]
N	91,797	20,358	15,108	127,263
R^2	0.014	0.291	0.011	0.031

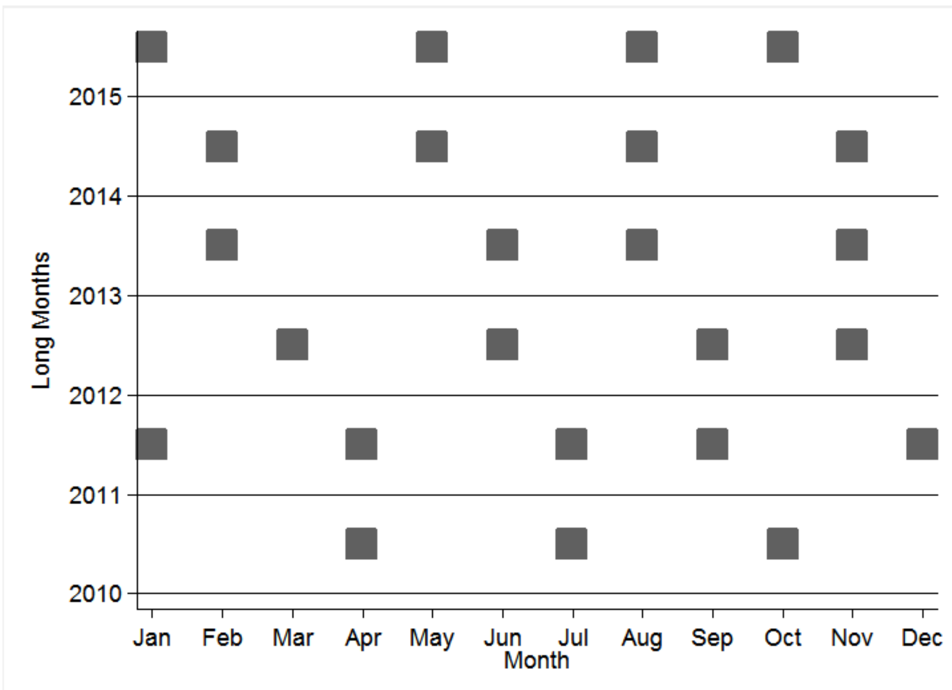
Note: Table shows results of regressions of due dates for recurring monthly bills on Wednesday group assignment. The dependent variable in Panel A is the normalized day of month each bill is due. The dependent variable in Panel B is the average number of days between each bill's due date and the previous paycheck date. Standard errors clustered by Wednesday group interacted with calendar year are shown in parentheses, and p-values are shown in brackets.

Table 4: Timing Mismatch and Financial Shortfalls

	(1)	(2)	(3)
LHS:	OD	Bounced	Online Payday
Sample Mean:	0.70%	0.19%	0.01%
Panel A: Ordinary Least Squares			
Long pay period	0.047	0.028	0.160
	(0.005)	(0.010)	(0.049)
	[0.000]	[0.004]	[0.001]
Timing Mismatch (weeks)	0.037	- 0.162	- 0.563
	(0.014)	(0.025)	(0.120)
	[0.010]	[0.000]	[0.000]
R^2	0.002	0.001	0.000
Panel B: Instrumental Variables			
Long pay period	0.047	0.029	0.163
	(0.005)	(0.010)	(0.049)
	[0.000]	[0.003]	[0.001]
Timing Mismatch (weeks)	0.096	0.336	0.467
	(0.071)	(0.108)	(0.398)
	[0.178]	[0.002]	[0.240]
R^2	0.002	0.000	0.000
Fixed effects included:			
Calendar year	Y	Y	Y
Calendar month	Y	Y	Y
Day of month	Y	Y	Y

Note: Table shows results of regressions of daily overdrafts, bounced checks, and online payday borrowing on the timing mismatch between income receipt and bill due dates. Timing mismatch is measured as the weighted average number of weeks between mortgage, credit card, and car payment due dates and Social Security pay dates, weighted by the dollar amount of each bill for each household. In Panel A, timing mismatch is directly included as a covariate in OLS regressions. In Panel B, timing mismatch is instrumented by Wednesday group assignment. Standard errors clustered by household are shown in parentheses, and p-values are shown in brackets. There are 45,119,445 household-day observations in all regressions

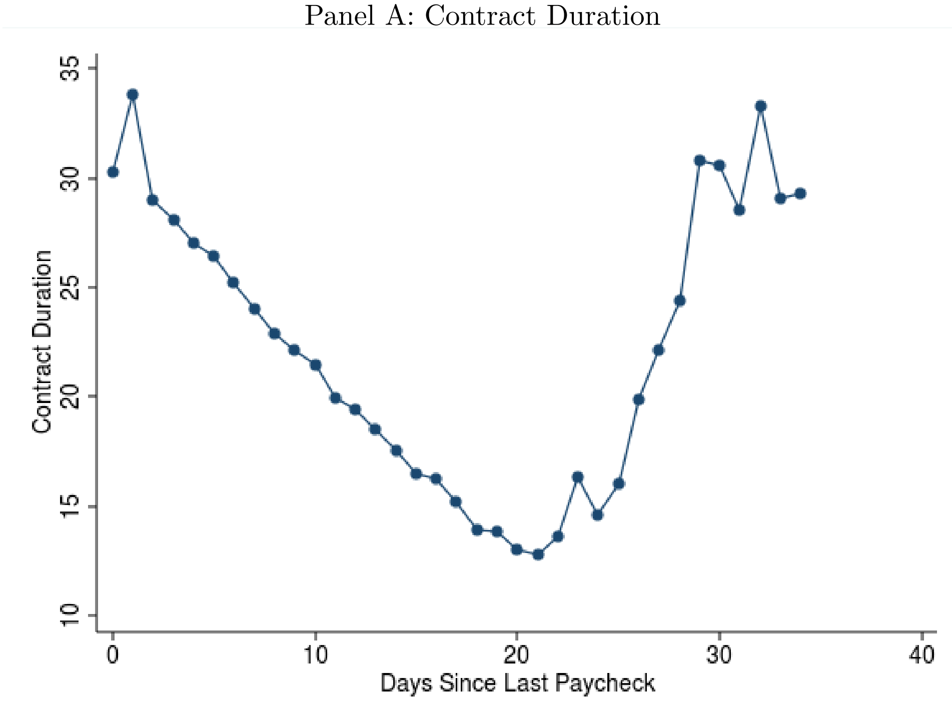
Figure A-1: Distribution of Long Pay Periods



Note: This figure shows the distribution of 35-day (i.e. "long") pay periods during the years of our sample period. A month is marked as "long" if the pay periods starting in that calendar month have 35 instead of 28 days for Social Security beneficiaries paid on Wednesday.

Figure A-2: Storefront Payday Contract Terms by Days Since Check

Panel A: Contract Duration



Panel B: APR