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. The results suggest that policies and technologies that help consumers align the timing of their income and expenditure streams would improve financial health.

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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 could affect real economic outcomes for many households. Consistent with binding short-term budget constraints, previous studies have shown declines in consumption 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 consumption behavior, optimal income timing, and the regulation of short-term credit.

Our study exploits predictable variation in the timing of Social Security disbursements to estimate the causal effect of income timing on household finances. For about 28 million current beneficiaries, the Social Security Administration (SSA) assigns income 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 additional 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.

However, if consumers have behavioral biases or follow imperfect budgeting heuristics, they could face more financial shortfalls during long pay periods, when they have to stretch the same paycheck over a longer period of time. Biased consumers may also be affected by interactions between the timing of income and expenditure commitments. For example, if all households have housing payments due on the 1st of the month, a household paid on the 1st may face fewer shortfalls than one paid on the 15th, since there are fewer opportunities to mis-budget or over-spend when income and lumpy expenditure commitments are aligned.

We test for these two potential effects of income timing using a dataset that covers the bank and credit card transactions of Social Security beneficiaries. We measure the daily incidence of

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

[2]See Stephens (2003), Wilde and Ranney (2000), Shapiro (2005), and Mastrobuoni and Weinberg (2009), and Hastings and Washington (2010) for work documenting expenditure cycles for government benefit recipients. 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.

financial shortfalls in the form of bank overdrafts, bounced checks, and online payday borrowing. Our results contrast with the predictions of the LCPIH. First, we find that financial shortfalls are 9% higher per day 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, by using Wednesday group assignment as an instrumental variable (IV), we find that households are 18% more likely to experience shortfalls for every one-week increase in the timing mismatch between income and expenditures. The second effect is particularly striking, because it represents a cross-sectional difference across otherwise identical households, and cannot be explained by intertemporal substitution.

We also use our IV strategy to evaluate how income timing affects spending, saving, and credit card utilization, to help pin down potential mechanisms for our findings. We find evidence consistent with budgeting heuristics that concentrate both discretionary and committed spending at the beginning of pay periods. Spending peaks on pay dates and declines steadily over the course of the pay period. Spending per day is lower during long pay periods, but not enough to compensate for the lower income per day, hence leading to financial shortfalls. Households with greater timing mismatch spend more per day, especially on discretionary categories such as retail, which further exacerbates financial shortfalls. Savings flows do not seem to help households cope with income timing variation, in contrast to rational theories of consumption smoothing. While we feel that simplistic budgeting heuristics are the most parsimonious explanation for these findings, behavioral theories such as inattention and present bias can also partially explain our results.

This paper contributes to the large literature on the effects of income receipt on borrowing and consumption, including work that examines the relationship between the timing of government benefits and household expenditures.[4] It is particularly closely related to recent papers on the high-frequency effects of income receipt on expenditures and delinquencies that use data from account aggregation firms. Our results complement those of Olafsson and Pagel (2016), who find that spending is correlated with the income cycle even for high-liquidity households. Gelman et al.

[4]See Jappelli and Pistaferri (2010) for a review of the income and consumption literature. See Stephens (2003), Wilde and Ranney (2000), Shapiro (2005), Mastrobuoni and Weinberg (2009), Hastings and Washington (2010), and Hastings and Shapiro (2017) for papers on the relationship between government benefits and expenditures.

(2015), and Baker and Yannelis (2015) examine the effects of a one-time, unexpected delay in income payments due to the 2013 government shutdown. This study is distinguished from the literature by our novel hypothesis and test of the timing mismatch channel, and our focus on financial shortfalls in addition to consumption.[5]

This study is also related to the literature on the usage of short-term credit, including payday loans and bank overdrafts, to smooth both anticipated and unanticipated liquidity variation. 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. Parsons and Van Wesep (2013) show theoretically that the welfare implications of short-term loans depend crucially on income timing, specifically paycheck frequency. Stango and Zinman (2014) and Alan, Cemalcılar, Karlan and Zinman (2015) find evidence for inattention as a driver of overdraft use, which is broadly consistent with our findings. We find that both payday loans and bank overdrafts are higher during long pay periods and for households with greater timing mismatch. Thus, at least some use of high-cost credit can be attributed to predictable variation in liquidity that could be smoothed inexpensively with short-term saving. Nonetheless, our findings do not preclude credit responses to unexpected shocks.

II Data Sources and Background on Social Security Benefits

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.[6] Supplemental Security Income (SSI) beneficiaries and beneficiaries that began benefits before May 1997 receive payments near the beginning of each month. We exclude these first two groups from our analysis for several reasons. 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

[5]Contemporaneous papers by Bos et al. (2016) examine the effects of government benefits in Sweden on pawn loans, and Leary and Wang (2016) estimate the effects of Social Security benefits on storefront payday loans.

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

of their pay periods, and the variation that exists is correlated with seasonality and the timing of weekends in a month. Finally, SSI recipients are demographically distinct from retirees and disability recipients, so differences between the SSI and retirement / disability recipients could be driven either by 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 through 10th, 11th through 20th, and 21st through 31st of the month are assigned to pay dates on the second, third, and fourth Wednesday of each month, respectively. 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 eight short pay periods, and we observe no systematic seasonal pattern in the distribution of long pay periods over the year.

As described below, all beneficiaries in our dataset are identified through their direct deposit transactions, and 80% of all such transactions fall exactly on the dates shown on the publicly available Social Security disbursement calendars. Most of the remainder fall one day prior to the official disbursement date, which is likely due to differences in the speed of transaction processing across different financial institutions. 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 data comes from an online account aggregation service. Account aggregators allow households to monitor their financial activities across multiple financial institutions and accounts on a single webpage or smartphone app. These services often include features such as budgeting, expense tracking, and notification of upcoming bills. 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.[7]

A 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). However, since we condition on the receipt of Social Security income, this alleviates the bias towards younger individuals in our sample. Another limitation of the dataset is that those who sign up for the aggregation service may only link a subset of their bank and credit card accounts, though we are confident we capture the user's primary checking account since this is where Social Security income would likely be received. In the event that a user has unlinked credit cards, we would fail to see the exact purchases made with such cards but we would see the resultant payments to the credit card from the primary checking account. In this regard, we have a fairly comprehensive snapshot of a user's financial situation even if every account is not linked to the service. For simplicity, we refer to each user of the account aggregation service as a "household", even though a user may choose to include all financial accounts used by their household or only those belonging to 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

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

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 33,722 households.

Table 1 provides basic summary statistics for the account aggregator sample. The average household receives $4,562 in income per month, $1,347 of which comes from Social Security receipts. We present three other types of household inflows in addition to income. Net savings inflows include transfers from savings accounts and brokerages, which can be either inflow or outflows, but on average is a net inflow into the accounts in our sample (which consist primarily of checking accounts). These transfers represent net inflows of $144 per month. In order to consistently account for cash flows, we count proceeds from credit cards, payday loans, and other loans as inflows. These yield an average inflow of $947 per month, and are dominated by credit card spending. Other net inflows, which consist primarily of transfers from unlinked checking and savings accounts, average $3,365 per month.

The next series of variables presented in Table 1 are outflows associated with loan repayment and consumption. Total household outflows average $7,372 per month. For use in measuring expenditure commitments later in our analysis, we separate loan outflows into “recurring” expenditures, which are the three largest categories of loan outflows that typically occur on a monthly basis, and “other” loan outflows, which include student loans and other less-common forms of debt. Recurring loan outflows average $2,874 per month, while other loan outflows average $42 per month. Ninety percent of households have a recurring bill under this classification in a given month. Eighty-six percent have a recurring credit card payment, 39% have a mortgage, and 29% have a car payment. All remaining outflows are classified as consumption spending, which includes categories such as cash, check, gas, restaurants, retail, etc.

The last series of variables we report in Table 1 are proxies for financial shortfalls, our key outcome measures. In a given month, 13% of households experience some form of financial shortfall, 11% have an overdraft, 3% bounce a check, and 0.2% borrow from an online payday lender.

It is worth noting a slight difference in how we account for the flows of households with linked credit cards and households with unlinked credit cards. For those with linked credit cards, we observe each credit card spending transaction (a debit) as well as each monthly credit car payment, which we observe as two transactions - a credit to the credit card and a debit from the checking account. For households with an unlinked card, we only observe debits from their checking account to pay off the credit card. The gross flows for the two scenarios are handled differently in Table 1, but they both net out to the amount of the credit card payment. For a household with a linked credit card who pays the card in full, $1 of credit card spending will result in a $1 increase in loan inflows, a $1 increase in recurring loan outflows, and a $1 increase in consumption spending, netting out to a $1 outflow. For a household with an unlinked credit card, the same set of transactions will only result in a $1 increase in recurring loan outflows, since we do not observe the actual spending transaction.

We present two sets of statistics on the representativeness of our sample. Table A-1 shows the geographic distribution of households in our dataset relative to the 2010 U.S. Census. The first two columns show the geographic distribution of households in our entire dataset as well as those who are identified as social security recipients. As shown, both our unfiltered dataset and our Social Security subsample are geographically diverse and map reasonably well to the U.S. distribution. The largest discrepancy between our Social Security subsample and the U.S. Census arises in the state of Florida, which constitutes 13.9% of our social security sample while only comprising 6.1% of the U.S. population. This discrepancy can be reconciled by recognizing first that only 7.6% of our unfiltered dataset resides in Florida and further that Florida is a popular retirement destination for many retirees. The other outlier is California, which reflects the higher propensity of residents in these states to use the account aggregator.

In the final two columns of Table 1, we present summary statistics on income for the sample of Social Security recipients from the 2010-2015 March Current Population Survey (CPS). We compute these statistics by choosing only households in the CPS who receive disability and retirement income from the SSA and exclude those receiving SSI benefits. Within the CPS, we cannot identify and filter out households who began receiving benefits after 1997 as we do in our account aggregator sample, so on average CPS households are likely to be slightly older than those in our sample.

As shown in the table, average income in our sample more closely resembles that of CPS households than CPS individuals, with a total income of $4,562 in the account aggregator sample compared to $4,240 for households in the CPS. We plot the distribution of average monthly income from Social Security for both the account aggregator sample and the CPS in Panel B of Figure 2. The figure confirms that income in the aggregator sample is slightly higher than in the CPS. Since we restrict to households with only one source of Social Security income for ease of interpretation, the aggregator distribution lacks the right tail of CPS households which is driven by households with multiple benefits recipients.

Overall, the summary statistics suggest that our dataset is well-dispersed geographically and has a total household income similar to that a nationally-representative sample from the CPS. If anything, our sample may be slightly higher income and more financially sophisticated than the general public, which would bias us against finding any relationship between income timing and financial shortfalls.

III Theoretical Background and Hypothesis Formation

In this section, we describe potential hypotheses for how financial shortfalls should respond to the two sources of income timing variation generated by the Social Security disbursement calendar: the length of the pay period and the timing of pay within a month.

There is no income uncertainty in our setting, and the two sources of income timing variation are plausibly exogenous to any other economic differences within or across households. Under these two conditions, the lifecycle / permanent income hypothesis (LCPIH) yields a stark prediction: income timing should have no effect on costly financial shortfalls. Since the implicit interest rates on bank overdraft, bounced checks, and payday loans are several hundred to over one thousand percent APR, rational consumers should save a small amount during short pay periods and draw down those savings during long pay periods, rather than resort to borrowing. Similarly, households can undo any effects of income timing within a month through short-term saving, avoiding systematic differences in financial shortfalls due to which Wednesday group they ar assigned to.

In contrast to the LCPIH, behavioral theories can generate greater financial shortfalls during long pay periods. One such theory is inattention - although the income schedule is publicly available, consumers may be unaware of or ignore this information, and are "surprised" by long pay periods.

Related to inattention, households may follow simplistic budgeting heuristics that ignore the length of pay periods. For example, a family surveyed as part of the U.S. Financial Diaries Project reports that "Sarah manages the family's finances with a straightforward system: each paycheck goes to a bill. One goes to the mortgage, another to car payments, and 'everything else is covered by what's left over' " (The U.S. Financial Diaries 2013). Alternatively, present bias and overconfidence about cash flows can both cause overspending in short pay periods, leaving insufficient savings to smooth consumption during long pay periods.

What about the effect of income timing within the month? If all other cash flows were uniformly distributed throughout the month, then this dimension of income timing should have no effect, even under standard behavioral theories. However, as we show below, large monthly expenditures tend to cluster near the 1st of the month. In particular, a significant fraction of household expenditures are "committed" (Chetty and Szeidl 2016, Chetty and Szeidl 2007) - i.e. they have fixed due dates and amounts that are either fixed in the short-run or difficult to control. Examples include rent payments, mortgages, credit card bills, and other regular bills.

If income occurs simultaneously with large expenditure commitments, then households have no opportunity to mis-budget or overspend the funds needed for these commitments. However, households facing a mismatch between income and expenditure timing must budget their discretionary cash flows in the interim. Behavioral households with timing mismatch would be more likely to face financial shortfalls due to the same mechanisms described above. An important caveat to this hypothesis is that households might strategically adjust their expenditure timing to match their income flows. If such adjustment were perfect, then we should not observe any effects of income timing through this channel.

To summarize, we have described three sets of hypotheses about the effect of income timing on financial shortfalls:

1. Under the LCPIH, the length of pay periods and the timing of income within a month should have no effect.

2. Behavioral theories such as heuristic budgeting, inattention, present bias, and overconfidence about cash flows predict more financial shortfalls during long pay periods.

3. The same behavioral theories predict that households with a greater mismatch between the timing of income and expenditure commitments would have more financial shortfalls.

While we will not be able to distinguish every possible mechanism for the relationship between income timing and financial shortfalls, we will be able to sharply distinguish between the LCPIH's null hypothesis and behavioral alternatives. Furthermore, both the rational and behavioral models generate predictions for both the path of consumption and the pattern of financial shortfalls over the pay period. We will examine the joint patterns of shortfalls as well as spending and saving in order to identify the most likely classes of explanations.

IV Empirical Methodology

IV.A Measuring Expenditure Commitments and Timing Mismatch

We construct novel measures of household expenditure commitments and timing mismatch in order to test the timing mismatch hypothesis described above. This section describes the construction of our measures using the transactions in our account aggregator sample.

We measure expenditure commitments using the three largest categories of recurring bills that we can identify in the dataset: credit cards, mortgages, and car payments.[8] As shown in Table 1, these three categories make up $2,874 out of $7,372 in total monthly expenditures. Ninety percent of households have a bill in at least one of these categories in a given month. We identify repeated observations of the same bill within a household using the combination of transaction category (i.e. credit card, mortgage, or car) and text string description. For instance, we classify a given transaction as a recurring credit card bill if it is part of a series of transactions from the same household that shares 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 132,038 bills associated with 30,325 households.

One limitation of our dataset is that we do not observe the contractual terms of each expenditure

[8] 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.

obligation, so we cannot directly observe bill due dates. However, since within each household we observe multiple payments of each bill, we can estimate a bill's due date as the modal day of the month it is paid. For example, if we observe a household making 20 mortgage payments on the 1st of the month, one on the 4th of the month, and one on the 11th of the month, we would infer a due date of the 1st of the month for this bill.

The left portion of Figure 3 (Panels A, C, and E) plots histograms of the imputed due dates for each bill category across the three Wednesday groups. The distribution of due dates maps intuitively to what one would expect. The figures show a large fraction of bills clustered around the first of the month in each category. Credit card due dates show bunching at the beginning of the month, but also some spread throughout the month. Mortgage payments are much more concentrated at the beginning of the month. Car payments have a bimodal distribution, with peaks on both the 1st and 15th of the month. Importantly, the distribution of due dates is similar across the Wednesday groups within all three bill categories. The uniformity across Wednesday groups provides evidence that 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.

Because due dates are roughly similar across the three Wednesday groups, the 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 payment. The right portion of Figure 3 (Panels B, D, and F) presents the distributions of timing mismatch by bill type. The variation in timing mismatch is most striking for mortgages, which show progressively lower mismatch among the third and fourth Wednesday groups compared with the second. 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 2 presents formal tests of differences in due dates and timing mismatch across Wednesday groups. We collapse the data into one observation per bill, and regress each bill's normalized due date and average timing mismatch on dummy variables for the third and fourth 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 30th is effectively very similar to a due date on the 1st. To correct for this issue, we normalize bill due dates by 24 since the 24th is the least common due date.[9]

The results from Panel A confirm 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 fourth Wednesday dummy are consistent with limited adjustment of due dates toward pay dates.

Panel B of Table 2 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 table, the average timing mismatch is about 15 days for each of type of bill. 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 2.4 fewer days of timing mismatch per bill compared with the second Wednesday group. Timing mismatch is also slightly lower for the third Wednesday group compared to the second, although this pattern is muted for credit cards and auto loans which have more due dates in the middle of the month.

We next construct a household-level measure $TimingMismatch_h$ for each household h:

$$TimingMismatch_h = \sum_{b \in bills(h)} \frac{DollarAmountBill_b * (DueDate_b - LastPaydate_b)}{\sum_{b \in bills(h)} DollarAmountBill_b} \tag{1}$$

$TimingMismatch_h$ captures the weighted-average number of days between a household's bill due dates and Social Security receipt, weighted by the dollar amount of each bill. Our measure

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

of timing mismatch intuitively assigns more weight to larger bills within each household. For households with no recurring bills, we set $TimingMismatch_h = 0$, although our results are robust to the exclusion of such individuals as we discuss in Section V.B below.

Figure 4 shows the distribution of this household-level measure of timing mismatch. Consistent with our bill-level results described above, the timing mismatch distributions shift progressively to the left for the third and fourth Wednesday groups. Overall, the results in this section confirm that Wednesday group assignment is correlated with the timing mismatch between income and expenditures. The following section describes how we use Wednesday group assignment as an instrument to explore the effects of timing mismatch on household financial health.

IV.B Identification Assumptions and Econometric Model

As described in Section II.A, 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 the three Wednesdays each month based on their date of birth. This payment schedule generates two key sources of variation that are the focus of our analysis: pay period length and Wednesday group assignment. Our key identification assumptions are that pay period length and Wednesday group assignment are as good as random with respect to ex ante household characteristics. And furthermore, that they affect financial outcomes only through their impact on income timing. As we established in the previous section, Wednesday group assignment is strongly correlated with timing mismatch, and we use it as an instrumental variable in this setting.

Figure A-1 shows the timing of long pay periods, which are evenly distributed across calendar months during our sample. Because the three Wednesday groups span different parts of each calendar month, and we are able to fully control for calendar month, calendar year, day of month, and day of week in our regression specifications, a dummy variable that equals one during long pay periods can isolate the effect of pay period length independently of any other calendar time effects.

Because we have very limited demographic information on households, the main test of covariate balance by Wednesday group assignment 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 an individual 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 Panel A of Figure 2. As shown, the income distributions align very closely across Wednesday groups.

Our main specifications take the following form, where timing mismatch is instrumented by Wednesday group assignment in the first stage:

$$\begin{aligned} TimingMismatch_{gt} &= \alpha_1 Long_{gt} + \alpha_2 WedGroup_g + \alpha_3 X_t + \eta_{gt} \\ Y_{gt} &= \beta_1 Long_{gt} + \beta_2 Timing\hat{M}ismatch_g + \gamma X_t + \epsilon_{gt} \end{aligned} \tag{2}$$

Y_{gt} is a measure of financial outcomes for households in 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. By weighting the collapsed data by the number of households in each cell, the point estimates from these collapsed regressions are identical to those using the underlying microdata. $Long_{gt}$ is a dummy variable that equals 1 if day t is in a 35-day pay period, and 0 otherwise. $Timing\hat{M}ismatch_g$ is the fitted value from the first-stage regression of timing mismatch on Wednesday group assignment. $WedGroup_g$ is a set of dummy variables for the third and fourth Wednesday group, where the second Wednesday group is the omitted category. X_t is a vector of fixed effects for calendar month, calendar year, day of month, and day of week. The coefficients of interest are β_1 and β_2, which measure the effect of pay period length and timing mismatch, respectively. Under the lifecycle / permanent income hypothesis, $\beta_1 = \beta_2 = 0$. Under the behavioral models discussed above, $\beta_1, \beta_2 > 0$.

To summarize our argument that Wednesday group assignment is a valid instrument for timing mismatch, it is important to remember that a valid instrument must satisfy two conditions: relevance and the exclusion restriction. We show that our instrument is relevant by presenting the first stage estimation results in Table 2, Panel B, Column (5). As shown, the coefficients for the Wednesday group dummies are economically and statistically significant at the 1% level. Relative to the omitted second Wednesday group, the third Wednesday group exhibits an average of 0.6 days lower timing mismatch and the fourth Wednesday group exhibits an average of 2.8 days lower timing mismatch. The coefficients on $Long_{gt}$ and our control variables are suppressed in this table for brevity.

The remaining hurdle is to demonstrate that our instrument of Wednesday group assignment also satisfies the exclusion restriction. In order to do so, we must establish that Wednesday group assignment only influences our dependent variables of interest through the channel of timing mismatch. In Section IV.A, we described that assignment to Wednesday groups is assigned by the day of the month in which a person is born. It is unlikely that such an arbitrary assignment into Wednesday groups would have a direct influence on household outcomes such as financial distress through a channel other than timing mismatch. While we cannot test this formally, in Panel A of Figure 2 we dispel concerns that Wednesday group assignment influences the size of Social Security payments. In this figure we show that the income distribution of the three Wednesday groups is almost identical, which is expected given the quasi-random nature of the assignment. Given our setting, it stands to reason that our instrument only influences household outcomes, such as financial distress, through the timing mismatch channel.

V The Effect of Income Timing on Financial Shortfalls

V.A Main Results

Table 3 presents the results of the IV estimation from Equation (2). The coefficients show the effect of income timing on the incidence of financial shortfalls per household, per day. The dependent variables in columns (1) through (3) are the fraction of households who have a bank overdraft, bounced check, or online payday transaction on a given day. In column (4), the dependent variable is the fraction of households with any of the three types of financial shortfalls on a given day. Sundays are dropped from all regressions, since many banks do not post transactions on Sunday and instead process them on the following Monday.

To benchmark the magnitude of the results, the first row of Table 3 presents the unconditional sample means of the outcome variables. On a given day, 0.7% of households experience an overdraft, 0.2% experience a bounced check, 0.01% take out an online payday loan, and 0.85% experience any form of financial shortfall. Panel A shows the unadjusted IV coefficients in percentage points - i.e. a coefficient of 0.01 means that the independent variable causes a one percentage point increase in the fraction of households who experience a shortfall. Panel B normalizes the coefficients by the unconditional mean in each category to aid in the economic interpretation of the results.

Two main findings emerge from Table 3. First, long pay periods cause an increase in all three measures of financial shortfalls. From column (4), the incidence of all financial shortfalls increases by 7 basis points per day during long pay periods, an 8.6% increase from the mean. Relative to their sample means, households experience 9.2% more overdrafts, 6.0% more bounced checks, and 12.3% more online payday loans per day during long pay periods compared with short pay periods. Because our outcomes are measured at a daily level, these findings are not driven mechanically by the greater likelihood of experiencing a negative liquidity shock during a longer measurement period.

The second main finding is that higher timing mismatch leads to more financial shortfalls. A one-week increase in timing mismatch results in 13% more overdrafts, 42% more bounced checks, 37% more online payday loans, and 18% more financial shortfalls overall. These effects are economically large and all statistically significant at the 1% level. In sum, the results of Table 3 provide convincing evidence that the timing of pay matters, both within and between otherwise identical households. Households have a higher propensity to experience financial shortfalls not only during months when there is a longer duration between paychecks, but also when there is a lag between income receipt and expenditure due dates. This evidence soundly rejects the LCPIH in favor of behavioral theories.

To examine the differences between long and short pay periods in more detail and to shed light on potential mechanisms, we repeat the specification from Table 3, but instead of including a dummy variable for long months, we estimate the effects for each day of long and short pay periods separately. Figure 5 plots the coefficients from these regressions. The indicators for the second week of short pay periods are omitted, so all coefficients can be interpreted as differences relative to those dates. Since a day of the pay period is perfectly correlated with a day of a week, and bank outcomes exhibit a very strong weekly pattern due to processing rules, we have to omit seven dummies instead of just one in order to estimate the cyclical day-of-week effects. Consistent with the positive coefficient on the long pay period dummy in Table 3, the estimated coefficients for financial shortfalls during long pay periods (red triangles) are larger on average than during short pay periods (blue circles), especially at the end of the pay period.

Figure 5 shows how financial shortfalls evolve over the pay cycle. Overdrafts and bounced checks both increase over the pay cycle, consistent with households running out of liquidity as they draw down their paychecks. Compared with the mean incidence on day 1 of the pay period, overdrafts increase by 85% over the course of short pay periods and by 128% over the course of long pay periods.[10] The dramatic magnitudes of the increase over the pay period are similar for bounced checks and overall shortfalls. In contrast, online payday lending remains relatively constant over the pay cycle. Given that payday lenders charge a fixed amount of interest per loan, borrowers could minimize their effective interest rates by taking out loans at the beginning of the pay cycle. However, the beginning of the pay cycle is when there is the least need for a loan. These two factors make the pattern of payday borrowing over the pay cycle more ambiguous to interpret than for overdrafts and bounced checks. Since online payday loans are relatively rare, the combined shortfall results in Panel D show an increasing trend of over the pay cycle.

Another important pattern is that financial shortfalls follow the same upward-sloping path over the course of short and long pay periods. The level of shortfalls on a given day depends only on the number of days since the last paycheck, not on total pay period length. This implies that households seem to start each pay period with the same amount of liquidity regardless of pay period length. Although our dataset does not include direct measures of liquid asset balances, we further validate this pattern by exploring spending and saving flows below.

V.B Robustness to Alternate Mismatch Measures

We next turn to assessing the robustness of our timing mismatch results. In Table 4, we reproduce the regressions from Table 3 using different variations of the timing mismatch measure in Equation (1). Columns (1) through (4) show the results of the IV regression in absolute terms, while columns (5) through (8) normalize the coefficients by the sample mean of each outcome to aid in the economic interpretation of the results.

In Panel A, we drop households with zero timing mismatch (i.e. those who have no recurring bills). In Panel B, we reconstruct our timing mismatch measure so that so that each bill is weighted equally, instead of weighting by the dollar value of each bill. In Panel C, we take into account the fact that bill dates can change within a household over time, and recalculate Equation (1) by

[10] Day 0 shows elevated levels of overdrafts and bounced checks, which are likely the result of processing lags for these transactions from the end of the previous pay period.

household-year rather than household. Finally, in Panel D we recompute timing mismatch using the weighted-average number of days between bills and all income payments, not just Social Security income payments.[11] All other aspects of the regressions remain unchanged from Table 3. For brevity, we suppress the coefficients for the long pay period in this table, although they remain very similar to those in Table 3.

The table shows that our main results are robust to alternative formulations of the timing mismatch measure. The coefficients in Panels A and B are of similar magnitude to our main specification in Table 3, while the coefficients in Panels C and D are larger than our initial estimates. The increased economic significance of our results in Panels C and D are intuitive since the timing mismatch measures in these panels is more precise, resulting in increased power of our instrument. Overall, the results of Table 4 give us confidence that our main results are robust to alternative formulations of timing mismatch.

V.C Saving and Spending

The previous sections showed that long pay periods and timing mismatch cause increases in financial shortfalls. To explore why this is the case, and to distinguish between some of the behavioral models that could give rise to these patterns, in this section we explore the effects of incoming timing on saving and spending. In particular, we replicate our main regression specifications to estimate the effect of long pay periods and timing mismatch on savings flows, loan payments, and outflows for consumption spending in various categories. We also explore how income timing affects the method of payment and debt accumulation by examining changes in the fraction of payment transactions made on credit cards.

Table 5 summarizes our results on savings flows, loan payments, and spending flows. The specification in this table is identical to that in Table 3, but the dependent variables are the average dollar amounts of each type of flow per household per day. Columns (1) through (4) present the results for four summary categories representing different types of household flows: net inflows from savings, loan outflows, consumption spending, and consumption spending on linked credit cards.

[11] Panel A of Figure A-2 shows the distribution of non-Social Security income. Since these distributions are identical across Wednesday groups, it does not change the qualitative pattern of decreasing timing mismatch across Wednesday groups. However, as shown in Panel B, including all income flows decreases the level of timing mismatch and dampens the variation in the timing mismatch measure.

The first three categories are mutually exclusive, while consumption spending on credit cards is a subset of all consumption spending in column (3). Columns (5) through (8) present the results for the four largest subcategories of consumption spending, which are components of the consumption spending measure in column (3). These categories, like column (3), include both credit card and non-credit card spending in these categories.

We first describe the effects of long pay periods. While ideally, we would want to examine household balances of both liquid and illiquid assets to distinguish theories of consumption, our dataset only allows us to infer outcomes based on transaction flows. Thus, column (1) presents the results for net flows from savings and brokerage accounts, which in our dataset are on average net inflows of \$5 per day (i.e. net inflows into the accounts we observe, which consist primarily of checking accounts). We find no significant change in net savings flows during long pay periods. In column (2), we examine loan outflows, which include credit card, mortgage, and automobile payments, as well as repayments for other types of loans such as student loans. As shown in Table 1, the majority of these flows are credit card payments. We find that households reduce loan payments by \$2.33 (2.4%) per day during long pay periods.

Column (3) shows the results for total consumption spending, which is \$1.52 per day (1.0%) lower during long pay periods. Total consumption spending can be disaggregated by either merchant category (as listed in Table 1) or by method of payment. We can distinguish spending on linked credit cards from bank account spending (which includes checks, debit card transactions, ATM withdrawals, and ACH payments). Column (4) presents the coefficients for total consumption spending on linked credit cards, which is higher but insignificantly different during long pay periods. The remainder of the table presents results for the four largest consumption spending categories, which are check, retail, utilities, and miscellaneous bills. In these columns, we combine spending on credit cards and bank accounts. Similar to the overall effect in column (3), spending in all of these categories is lower during long pay periods, and all categories except check are significant at the 1% level.

For completeness, we present the long pay period coefficients for all subcategories of consumption in Panels A and B of Figure A-3, which shows that retail, utilities, and miscellaneous bills seem to exhibit the most economically significant reductions both in absolute and relative terms.

Overall, the results show that households reduce outflows across the board during long pay periods, including committed categories such as loans. The only exception is spending with credit cards, which we examine in more detail below. Importantly, however, the spending reductions do not fully compensate for the reduction in income per day during long pay periods. Based on rough calculations from Table 1, Social Security income is about $10 per day lower during long pay periods, which is substantially larger than the $4 reduction in spending on loans and consumption.[12] This gap explains why households encounter financial shortfalls in long pay periods.

We next turn to the effects of timing mismatch. In column (1) of Table 5, we find that timing mismatch significantly reduces net savings inflows. A one-week increase in mismatch leads to a $3.09 (65%) per day reduction in net savings inflows. While one might expect higher-mismatch households to draw down their savings more, not less, they may have less savings to draw from if they habitually spend more. Indeed, the remainder of the table shows that higher mismatch leads to higher outflows in all categories except check, with statistically significant effects in four out of seven outflow categories.

Panels C and D of Figure A-3 shows the timing mismatch coefficients for all subcategories of consumption spending, and shows that retail exhibits one of the largest effects in both absolute and relative terms. Thus, timing mismatch causes increases in consumption holding income constant, which helps to explain the associated higher level of financial shortfalls in our main results.

Returning to Table 5, the increase in consumption is associated with greater reliance on credit cards, illustrated by a shift away from checks (column 5), an increase in loan outflows (column 2, which is driven primarily by credit card payments), and an increase in credit card consumption spending (column 4). Note that the increase in loan outflows is significantly larger than the increase in credit card spending - this is likely due to that fact that while some households use credits cards that are not linked to the account aggregation service (and therefore are not observable to us), we are likely to observe payments for these non-linked credit cards as long as they are made through the household's main bank account.

To shed further light on saving, loan payment, and spending patterns, we examine how these outcomes evolve over the course of short and long pay periods. Figure A-4 shows the coefficients

[12]This calculation is based on the average of $1,347 per month in income from Table 1. $10 = 1347/28-1347/35.

from specifications that include dummies for each day of short and long pay periods, analogous to those in Figure 5. Savings flows in Panel A show no clear trend over the pay period, which is surprising given the increase in financial shortfalls over the pay period. One explanation for the lack of significant savings response is that households lack liquid savings to draw from. Another is that they prefer to conserve their savings for true emergencies or asset purchases instead of everyday shortfalls.

The remaining panels of Figure A-4 show that while both loan outflows and consumption spending systematically decline over the pay period, credit card spending is flat or weakly increasing over the pay period. Interestingly, the paths of all major flow categories are indistinguishable between short and long pay periods, consistent with the results for financial shortfalls. This pattern supports the theory that households treat short and long pay periods equally in terms of saving and spending decisions, either because they are unaware of the variation or due to budgeting heuristics that ignore pay period length. In contrast, the LCPIH would predict flat spending profiles in Panels B and C, supported by increased inflows from savings at the end of long pay periods in Panel A. Such a pattern is soundly rejected by the results.

One of the key results we have shown so far is that credit card spending behaves differently from overall consumption spending, suggesting that variation in income timing leads to substitution from bank-based transactions to credit card transactions. We explore this further in Table 6, which presents results for the fraction of spending transactions made on credit cards as opposed to through bank accounts. Column (1) presents the results for all consumption spending transactions, while columns (2) through (5) present the results for the four largest spending categories other than cash and check: retail, utilities, miscellaneous bills, and groceries. While the results are imprecisely estimated, they show that both long pay periods and timing mismatch are associated with small increases of between 1-5% in the fraction of credit card transactions. Figure A-5 shows that credit card use across all categories increases over the pay period, and consistent with patterns in financial shortfalls and spending, that it evolves similarly across both short and long pay periods.

Overall, this section paints a picture of household financial behavior that helps to explain why long pay periods and timing mismatch cause households to face more financial shortfalls. We soundly reject the LCPIH, which predicts that households should save during short pay periods and draw down these savings during long pay periods, generating smooth consumption across both short and long pay periods. Even behavioral models that incorporate rational expectations such as sophisticated present bias would predict different consumption paths during short and long pay periods, which we do not find evidence for.

The most parsimonious explanation for our results seems to be that households follow budgeting heuristics based on the pay period. These heuristics generate spending on both committed and discretionary expenses at the beginning of each pay period, leaving households to "make do" with what's left over for the rest of the pay period, regardless of differences in pay period length. In other words, they live "paycheck to paycheck." When timing mismatch is low, expenditure commitments crowd out discretionary spending at the beginning of the pay period, contributing to to fewer shortfalls on average. Inattention to income timing can also explain many of the patterns we find.

VI Conclusion

This paper sheds light on the nature of intra-month household budgeting, providing evidence that households 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 with a greater mismatch between the timing of their income and expenditure commitments have permanently higher levels of financial shortfalls. Despite the second effect, 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. We shed light on the mechanisms behind these findings by examining the effect of income timing on spending and saving, and find evidence in favor of budgeting heuristics that lead households to live "paycheck to paycheck," regardless of variation in the timing of payment.

Our results highlight the need for better tools and policies to help consumers match the timing of their income and expenditures. In particular, our results suggest that financial shortfalls and credit card debt could be reduced by providing households with an income stream that includes both lumpy disbursements that match their stream of lumpy expenditures, and a smooth stream of constant income per day for regular expenditures. In other words, an income stream that matches both the timing and lumpiness of their optimal expenditure stream. This type of income stream could be achieved through a number of existing technologies and could be provided by a mix of government agencies, payroll processors, banks, and financial technology firms. While our results are based on a sample of government benefits recipients, they in principle could extend to wage income and other forms of income payments, which are a topic for future research.

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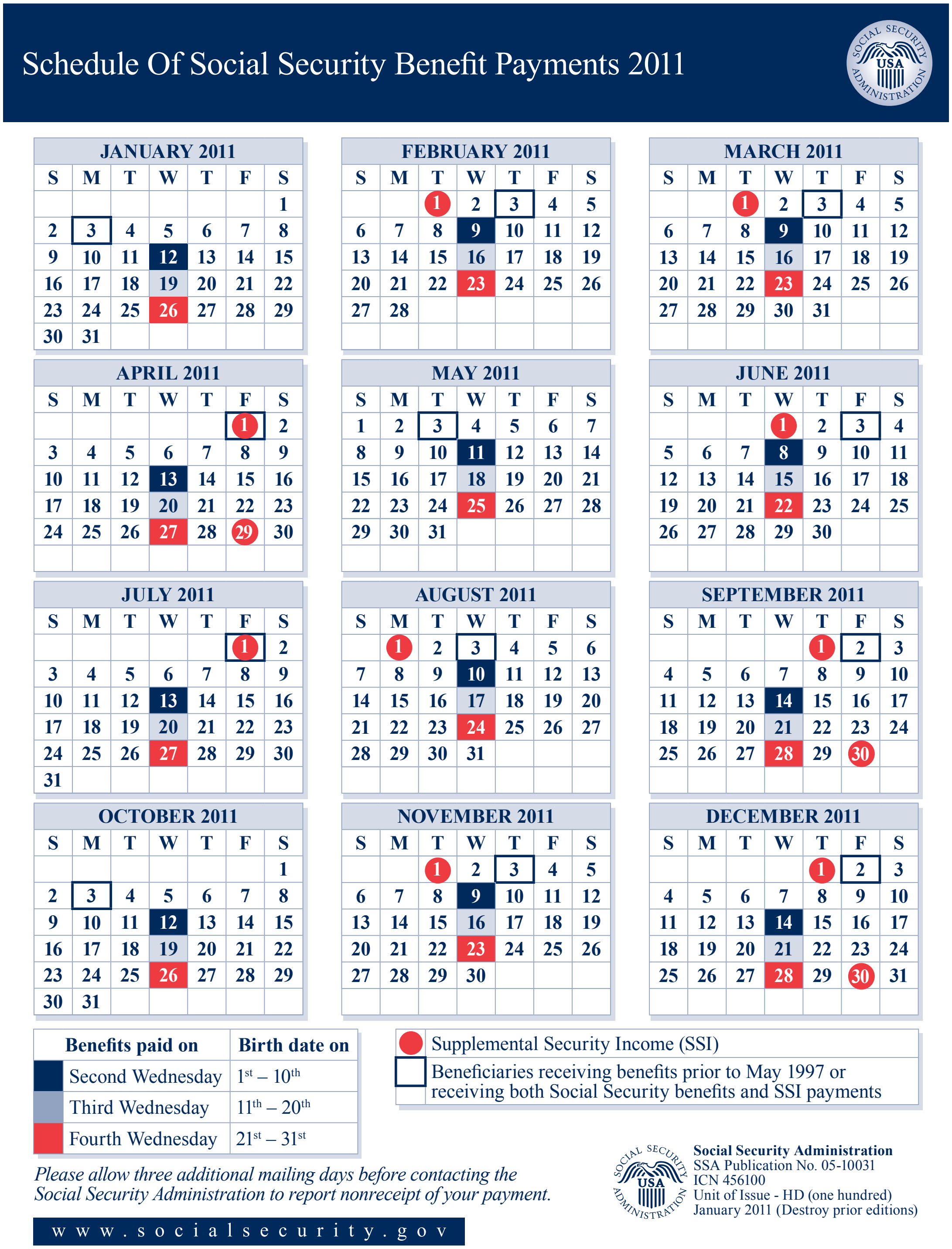
Figure 1: Social Security Administration Disbursement Calendar



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.

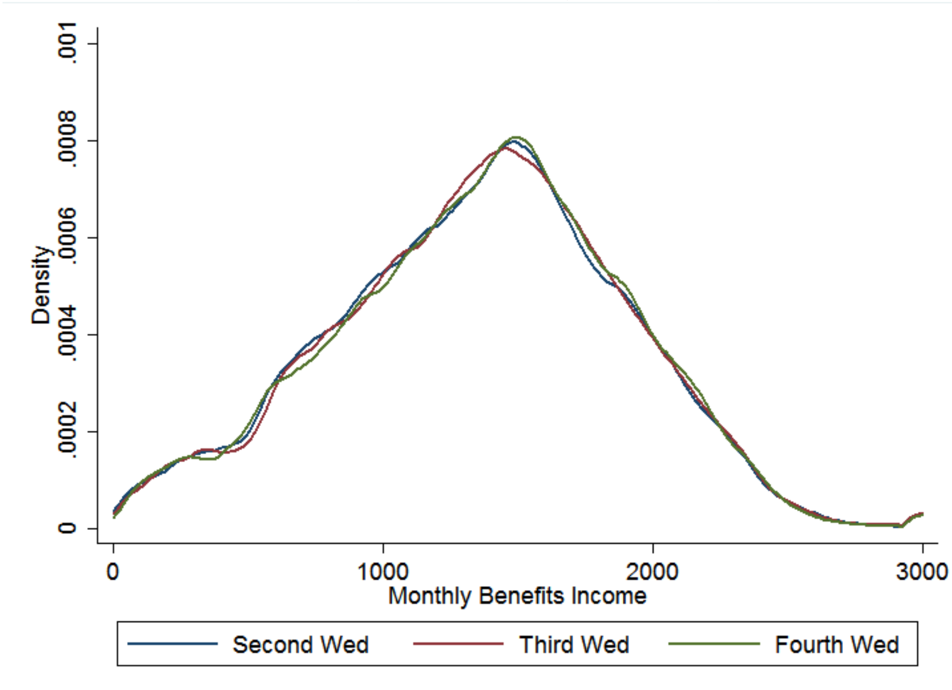
w w w . s o c i a l s e c u r i t y . g o v

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

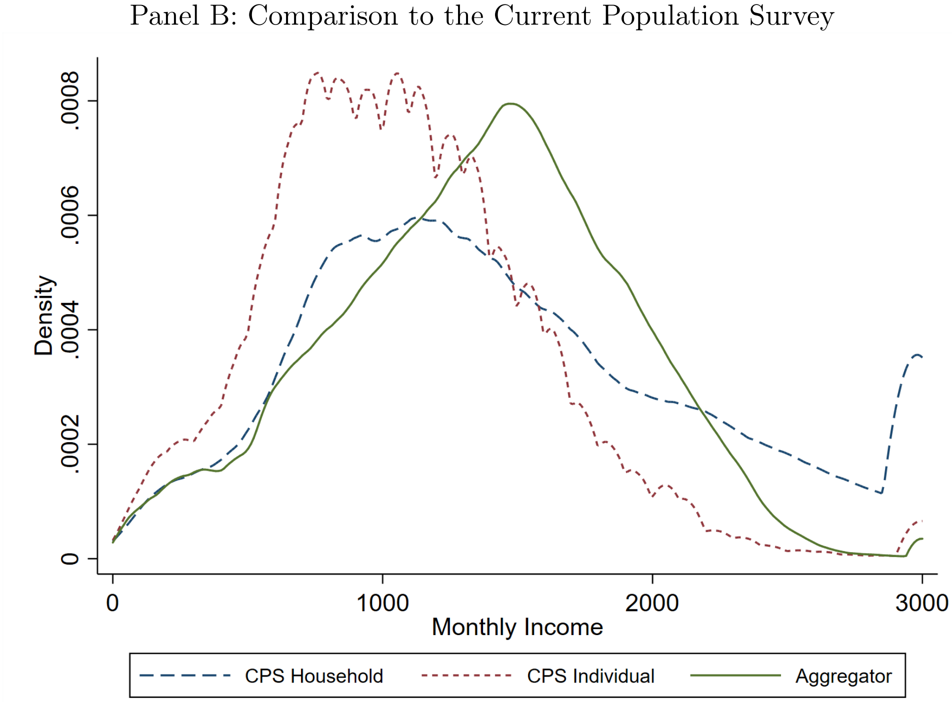
Note: The figure shows the variation in Social Security receipts across Wednesday groups in 2011. Source: www.socialsecurity.gov/pubs/calendar2011.pdf.

Figure 2: Social Security Income Distributions

Panel A: Monthly SSA Income By Wednesday Group



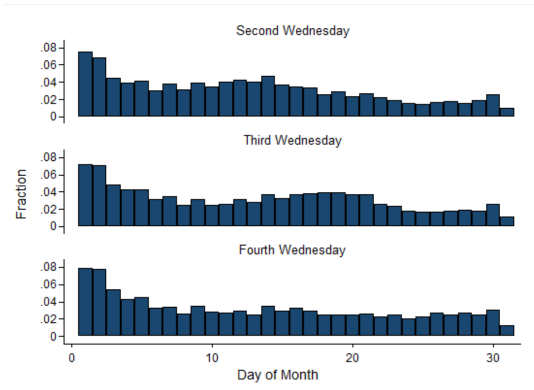
Panel B: Comparison to the Current Population Survey



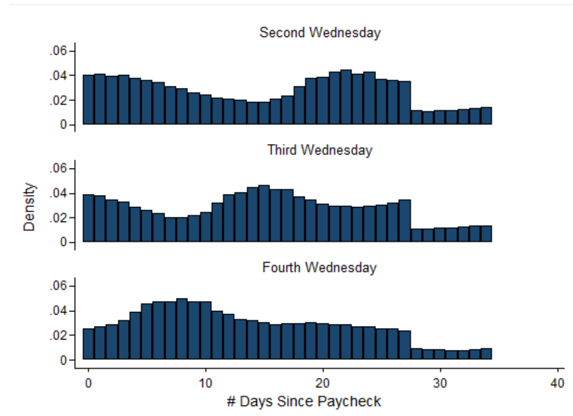
Note: The figure shows distributions of net monthly income from Social Security. Panel A compares the distributions for the three Wednesday disbursement groups in the account aggregator sample. Panel B compares the distribution in the account aggregator sample to individual and household Social Security income for benefit recipients in the CPS.

Figure 3: Due Dates and Timing Mismatch for Recurring Bills

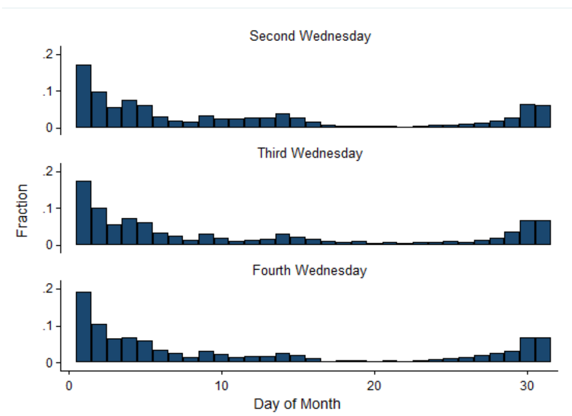
(a) Credit Card Due Date



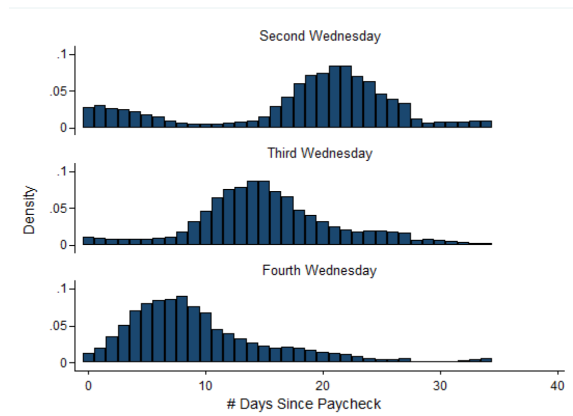
(b) Credit Card Timing Mismatch



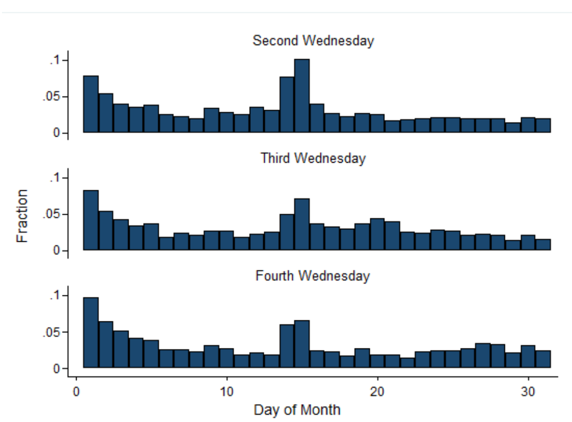
(c) Mortgage Due Date



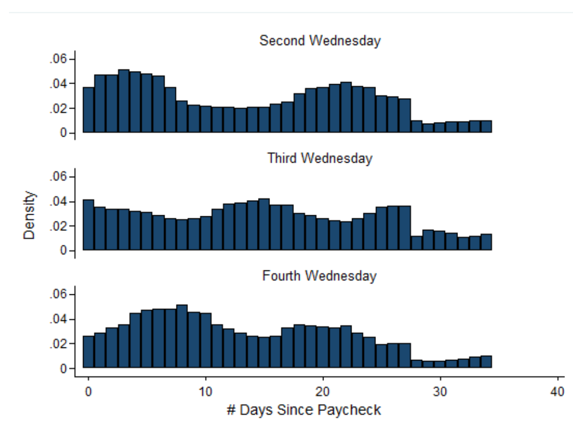
(d) Mortgage Timing Mismatch



(e) Car Payment Due Date

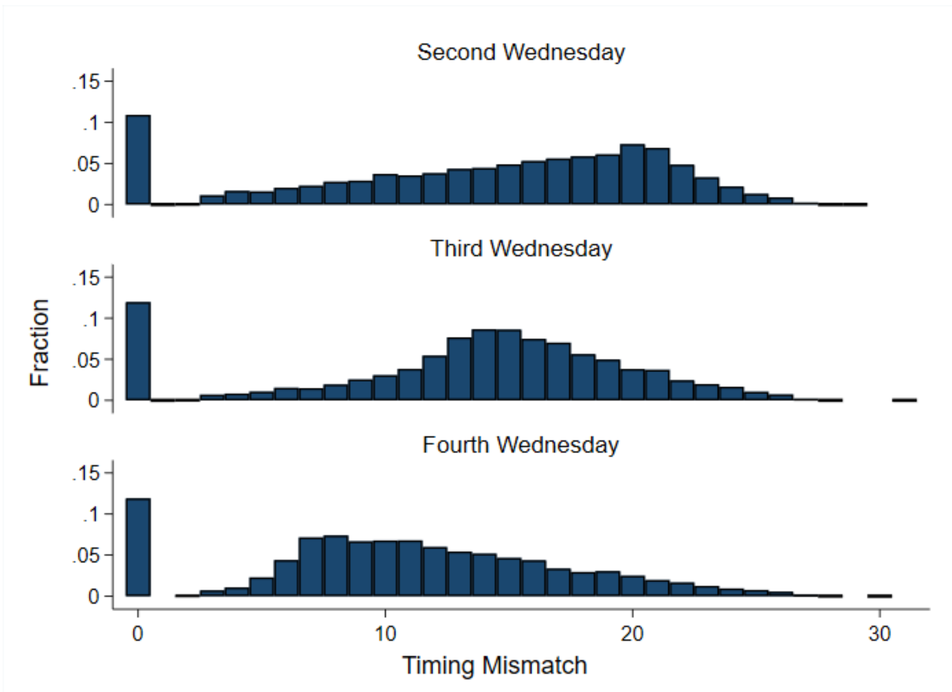


(f) Car Payment Timing Mismatch



Note: The figure shows histograms of due dates and timing mismatch between income and due dates for recurring bills in the three largest categories: credit cards, mortgages, and car payments. The histograms are 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.

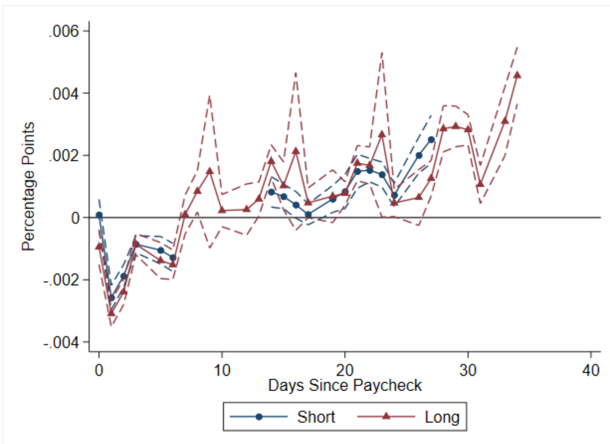
Figure 4: Household Timing Mismatch Between Income and Expenditure Commitments



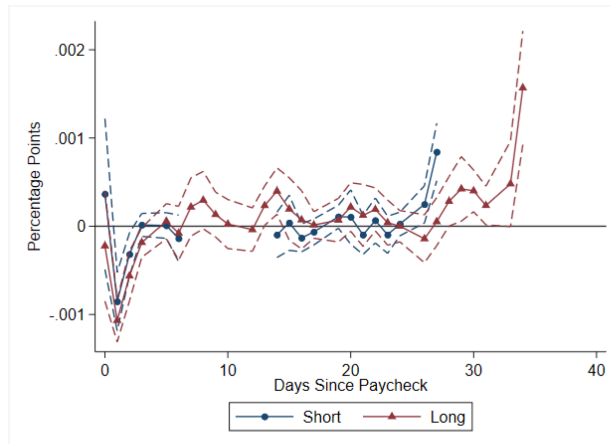
Note: This figure shows the distribution of household-level timing mismatch, split by the Wednesday each household receives Social Security income. Timing mismatch is defined as the average number of days between a household's bill due dates and their Social Security checks, weighted by the dollar amount of each bill. Timing mismatch is computed for each household across all of their credit card, mortgage, and car payment bills. Households with no recurring bills in any of these categories are defined as having timing mismatch equal to zero.

Figure 5: Financial Shortfalls Over Short and Long Pay Periods

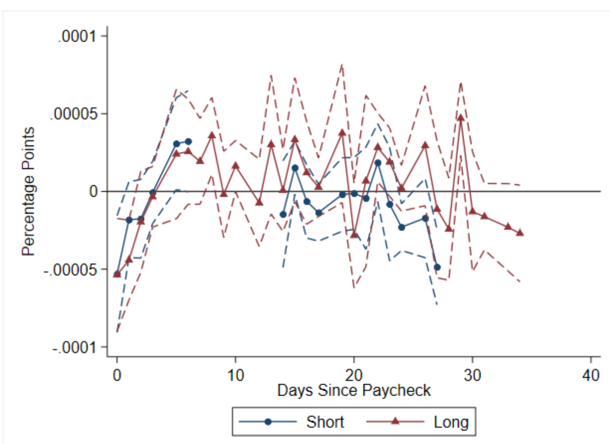
(a) Overdrafts



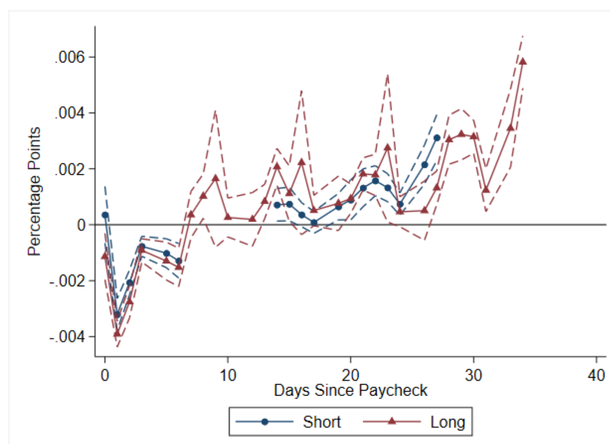
(b) Bounced Checks



(c) Online Payday



(d) All Shortfalls



Note: The figure shows coefficient estimates and 95% confidence intervals corresponding to IV regressions of the daily incidence of financial shortfalls on indicators for the number of days since the last SSA paycheck. The regressions also include an instrumented measure of timing mismatch and fixed effects for calendar year, calendar month, day of week, and day of month. The coefficient magnitudes are shown as the average incidence per day of each outcome, in percentage points (1 = 100%), where the daily incidence for days 7-13 of short pay periods are normalized to 0.

Table 1: Summary Statistics

	Account Aggregator				CPS	
	Mean	Median	Std. Dev.	Has Recurring	Indiv Mean	HH Mean
Inflows	$9,018	$5,962	$11,690			
Income	$4,562	$3,344	$4,290		$2,062	$4,240
Social Security income	$1,347	$1,388	$562		$1,100	$1,522
Salary, benefits & other	$3,216	$2,000	$4,261		$962	$2,718
Net savings inflows	$144	$0	$3,085			
Loan inflows	$947	$140	$1,874			
Other net inflows	$3,365	$1,010	$9,368			
Outflows	$7,372	$5,261	$9,300			
Recurring loan outflows	$2,874	$1,761	$3,492	90%		
Credit Card Payment	$2,041	$1,000	$2,865	86%		
Mortgage	$662	$0	$1,209	39%		
Car Payment	$172	$0	$357	29%		
Other loan outflows	$42	$0	$169			
Consumption spending	$4,456	$2,973	$7,685			
Cash	$232	$0	$625			
Check	$2,177	$736	$7,131			
Gas	$81	$30	$126			
Restaurant	$134	$51	$203			
Retail	$593	$365	$722			
Groceries	$215	$101	$294			
Entertainment	$44	$0	$113			
Healthcare	$72	$14	$136			
Travel	$104	$0	$332			
Insurance	$214	$123	$302			
Utilities	$310	$245	$305			
Misc Bills	$281	$83	$534			
Financial shortfalls	13%	0%	34%			
Overdraft	11%	0%	32%			
Bounced Check	3%	0%	18%			
Online Payday	0.2%	0%	4%			

Note: The table shows summary statistics at the monthly level for income, expenditures, and financial shortfalls for households in our sample. The financial shortfall variables indicate the fraction of households that experience any overdrafts, bounced checks, or online payday loans in a given month. The first three columns show the mean, median, and standard deviation of each variable. The next column shows the average percentage of household that have each type of recurring expense in a given month. The final two columns show the average individual and household income for Social Security recipients in the Current Population Survey from 2010-2015.

Table 2: First Stage: Wednesday Group Assignment and Timing Mismatch

	(1)	(2)	(3)	(4)	(5)
Bill Type:	Credit card	Mortgage	Car	Any	Collapsed
	Panel A: Bill Due Date (Days)				
Sample Mean:	13	8	13	12	
Third Wednesday Dummy	0.176	- 0.419	- 0.192	0.038	
	(0.081)	(0.121)	(0.185)	(0.067)	
	[0.030]	[0.001]	[0.298]	[0.572]	
Fourth Wednesday Dummy	- 1.337	- 1.038	- 1.901	- 1.355	
	(0.082)	(0.118)	(0.178)	(0.067)	
	[0.000]	[0.000]	[0.000]	[0.000]	
N	95,087	21,321	15,630	132,038	
R^2	0.007	0.005	0.010	0.030	
	Panel B: Timing Mismatch Between Due Dates and Previous Paycheck (Days)				
Sample Mean:	15	15	14	15	13
Third Wednesday Dummy	- 0.117	- 2.505	0.624	- 0.420	- 0.605
	(0.069)	(0.117)	(0.157)	(0.058)	(0.023)
	[0.092]	[0.000]	[0.000]	[0.000]	[0.000]
Fourth Wednesday Dummy	- 1.450	- 8.034	- 0.589	- 2.405	- 2.794
	(0.074)	(0.123)	(0.160)	(0.062)	(0.015)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
N	95,087	21,321	15,630	132,038	4,536
R^2	0.009	0.265	0.005	0.024	0.999

Note: The table shows the results of regressions of due dates and timing mismatch on Wednesday group assignment. Each recurring bill represents one observation, and a household can have multiple recurring bills. 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 most recent SSA pay date. In columns (1) through (3), we consider recurring bills in the three largest categories: credit cards, mortgages, and car payments. Column (4) represents pooled results from all three bill types, with dummy variables for bill type included in the regressions. Column (5) presents the first-stage results for our instrumental-variables analysis in Equation (2) using collapsed Wednesday group X day cells. For columns (1) through (4), standard errors clustered by household. For column (5), standard errors are clustered by Wednesday group X year. Standard errors are shown in parentheses, and p-values are shown in brackets.

Table 3: The Effect of Income Timing on Financial Shortfalls

	(1)	(2)	(3)	(4)
			Online	All
LHS:	OD	Bounced	Payday	Shortfalls
Sample Mean:	0.70%	0.19%	0.01%	0.85%
	Panel A: Percentage Points			
Long pay period	0.0006	0.0001	0.0000	0.0007
	(0.0001)	(0.0000)	(0.0000)	(0.0001)
	[0.000]	[0.000]	[0.014]	[0.000]
Timing Mismatch (weeks)	0.0009	0.0008	0.0000	0.0015
	(0.0003)	(0.0002)	(0.0000)	(0.0004)
	[0.003]	[0.000]	[0.009]	[0.000]
	Panel B: Relative Percentage Change			
Long pay period	0.092	0.060	0.123	0.086
	(0.012)	(0.017)	(0.050)	(0.011)
	[0.000]	[0.000]	[0.014]	[0.000]
Timing Mismatch (weeks)	0.133	0.422	0.370	0.179
	(0.045)	(0.095)	(0.142)	(0.047)
	[0.003]	[0.000]	[0.009]	[0.000]
R^2	0.423	0.419	0.154	0.468
Fixed effects included:				
Year and month	Y	Y	Y	Y
Day of week and month	Y	Y	Y	Y

Note: The table shows results of IV regressions of the daily incidence of overdrafts, bounced checks, and online payday borrowing on variables measuring long pay periods and timing mismatch between income receipt and bill due dates. Long pay period is an indicator for whether a given day is a part of a 35-day versus 28-day pay period. 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. Timing mismatch is instrumented by Wednesday group assignment. Panel A shows the results as incidence per day, i.e. the effect of each variable on the percent of household-days that financial shortfalls occur. Panel B shows the same results normalized by the sample mean incidence of each outcome. Standard errors clustered by Wednesday group X year are shown in parentheses, and p-values are shown in brackets. There are 38.7 million household-day observations underlying all regressions

Table 4: Robustness Checks: Timing Mismatch and Financial Shortfalls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LHS:	OD	Bounced	Online Payday	All Shortfalls	OD	Bounced	Online Payday	All Shortfalls
Sample Mean:	0.70%	0.19%	0.01%	0.85%				
	Percentage Points				Relative Percentage Change			
Panel A: Drop Zeros								
Timing Mismatch (weeks)	0.001	0.001	0.000	0.001	0.113	0.380	0.243	0.153
	(0.000)	(0.000)	(0.000)	(0.000)	0.048	0.070	0.119	0.047
	[0.020]	[0.000]	[0.041]	[0.001]	[0.020]	[0.000]	[0.041]	[0.001]
Panel B: Equal-weighted								
Timing Mismatch (weeks)	0.001	0.001	0.000	0.002	0.160	0.508	0.443	0.216
	(0.000)	(0.000)	(0.000)	(0.000)	0.052	0.111	0.167	0.054
	[0.002]	[0.000]	[0.008]	[0.000]	[0.002]	[0.000]	[0.008]	[0.000]
Panel C: Mismatch By Year								
Timing Mismatch (weeks)	0.002	0.001	0.000	0.002	0.228	0.545	0.562	0.281
	(0.001)	(0.000)	(0.000)	(0.001)	0.087	0.107	0.622	0.090
	[0.009]	[0.000]	[0.367]	[0.002]	[0.009]	[0.000]	[0.367]	[0.002]
Panel D: Mismatch Using All Income								
Timing Mismatch (weeks)	0.003	0.002	0.000	0.004	0.379	1.143	0.930	0.504
	(0.001)	(0.000)	(0.000)	(0.001)	0.088	0.194	0.308	0.088
	[0.000]	[0.000]	[0.003]	[0.000]	[0.000]	[0.000]	[0.003]	[0.000]
Fixed effects included:								
Year and month	Y	Y	Y	Y	Y	Y	Y	Y
Day of week and month	Y	Y	Y	Y	Y	Y	Y	Y

Note: The table shows results of IV regressions of the daily incidence of overdrafts, bounced checks, and online payday borrowing on variables measuring long pay periods and 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, with different weights and definitions as described below. The specifications are identical to those in Table 3 except for the definition of the mismatch variable. In Panel A, we drop households that have no recurring bills. In Panel B, the timing mismatch measure of equally-weighted across all bills, instead of by the dollar amount of each bill. In Panel C, mismatch is defined for each household in each year, instead of the whole sample period. In Panel D: the timing mismatch measure is based on the weighted average timing of all forms of income and recurring bills, instead of Social Security income only. Columns (1) through (4) show the results as incidence per day, i.e. the effect of each variable on the percent of household-days that financial shortfalls occur. Columns (5) through (8) show the same results normalized by the sample mean incidence of each outcome. Standard errors clustered by Wednesday group X year are shown in parentheses, and p-values are shown in brackets. There are 38.7 million household-day observations underlying all regressions.

Table 5: The Effect of Income Timing on Saving and Spending

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Summary categories				Four largest spending categories			
LHS:	Inflows from savings	Loan outflows	Consumption spending	CC consp spending	Check	Retail	Utilities	Misc bills
Sample Mean:	$5	$96	$147	$31	$72	$20	$10	$9
	Panel A: Dollar Amount							
Long pay period	0.049	- 2.331	- 1.519	0.186	- 0.432	- 0.316	- 0.199	- 0.246
	(0.221)	(0.332)	(0.485)	(0.169)	(0.401)	(0.121)	(0.050)	(0.048)
	[0.825]	[0.000]	[0.002]	[0.274]	[0.281]	[0.009]	[0.000]	[0.000]
Timing Mismatch (weeks)	- 3.087	8.876	2.688	3.430	- 1.134	1.876	0.685	0.240
	(0.697)	(1.085)	(1.750)	(0.659)	(1.771)	(0.221)	(0.069)	(0.190)
	[0.000]	[0.000]	[0.125]	[0.000]	[0.523]	[0.000]	[0.000]	[0.208]
	Panel B: Relative Percentage Change							
Long pay period	0.010	- 0.024	- 0.010	0.006	- 0.006	- 0.016	- 0.020	- 0.027
	(0.047)	(0.003)	(0.003)	(0.005)	(0.006)	(0.006)	(0.005)	(0.005)
	[0.825]	[0.000]	[0.002]	[0.274]	[0.281]	[0.009]	[0.000]	[0.000]
Timing Mismatch (weeks)	- 0.651	0.093	0.018	0.110	- 0.016	0.096	0.067	0.026
	(0.147)	(0.011)	(0.012)	(0.021)	(0.025)	(0.011)	(0.007)	(0.021)
	[0.000]	[0.000]	[0.125]	[0.000]	[0.523]	[0.000]	[0.000]	[0.208]
R^2	0.131	0.531	0.606	0.840	0.649	0.572	0.508	0.557
Fixed effects included:								
Calendar year and month	Y	Y	Y	Y	Y	Y	Y	Y
Day of week, day of month	Y	Y	Y	Y	Y	Y	Y	Y

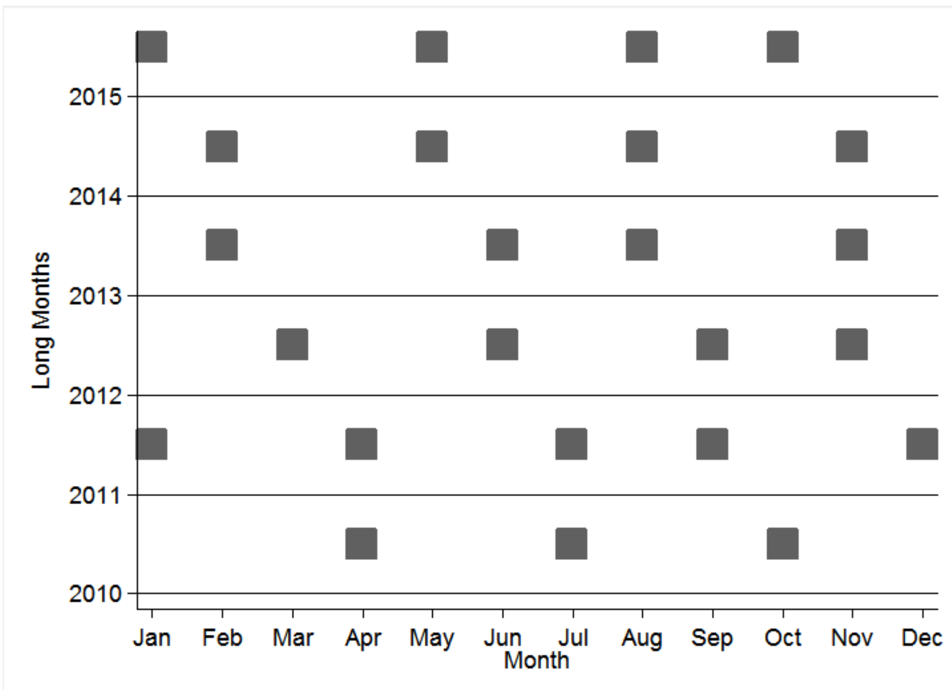
Note: The table shows results of IV regressions of household saving and spending flows on variables measuring long pay periods and the timing mismatch between income receipt and bill due dates. The long pay period is an indicator of whether a given day is a part of a 35-day versus 28-day pay period. 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. Timing mismatch is instrumented by Wednesday group assignment. Panel A shows the results as average dollar amounts per day, while Panel B shows the same results normalized by the unconditional mean of each flow category. Columns (1) through (4) present results for net flows in four summary categories: *Inflows from savings* captures net inflows from savings and brokerage accounts; *Loan outflows* captures outflows to pay down debt, including mortgage, credit cards, and car payments; *Consumption spending* captures spending on consumption; and *CC Spending* captures spending on linked credit card accounts (which is a subset of *Consumption spending*). Columns (5) through (8) present results for the four largest spending categories by average dollar amount: *Check*, *Retail*, *Utilities*, and *Misc Bills*, which captures bills not otherwise classified in other categories. Standard errors clustered by Wednesday group X year are shown in parentheses, and p-values are shown in brackets. There are 38.7 million household-day observations underlying all regressions.

Table 6: Effect of Income Timing on Credit Card Usage

	(1)	(2)	(3)	(4)	(5)
LHS:	Total spending	Retail	Utilities	Misc bills	Groceries
Sample Mean:	15%	33%	16%	22%	32%
			Panel A: Percentage Points		
Long pay period	0.002	0.001	0.000	0.002	0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
	[0.036]	[0.571]	[0.852]	[0.287]	[0.188]
Timing Mismatch (weeks)	0.004	0.004	0.008	0.008	0.016
	(0.003)	(0.007)	(0.002)	(0.004)	(0.008)
	[0.188]	[0.533]	[0.001]	[0.073]	[0.064]
			Panel B: Relative Percentage Change		
Long pay period	0.015	0.004	0.002	0.008	0.008
	(0.007)	(0.006)	(0.011)	(0.008)	(0.006)
	[0.036]	[0.571]	[0.852]	[0.287]	[0.188]
Timing Mismatch (weeks)	0.024	0.013	0.051	0.036	0.049
	(0.019)	(0.021)	(0.015)	(0.020)	(0.026)
	[0.188]	[0.533]	[0.001]	[0.073]	[0.064]
R^2	0.584	0.565	0.380	0.472	0.463
Fixed effects included:					
Calendar year and month	Y	Y	Y	Y	Y
Day of week, day of month	Y	Y	Y	Y	Y

Note: The table shows results of IV regressions of credit card usage on variables measuring long pay periods and the timing mismatch between income receipt and bill due dates. Long pay period is an indicator for whether a given day is a part of a 35-day versus 28-day pay period. 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. Timing mismatch is instrumented by Wednesday group assignment. Panel A shows the results in percentage points of credit card usage, while Panel B shows the same results normalized by the unconditional mean credit card usage in each category. The dependent variables represent the fraction of transactions in each category that are spent on credit cards, as opposed to debit cards or direct debits from checking accounts. The dependent variables include total spending across all consumption categories, and the four largest consumption categories excluding cash and check: *Retail*, *Utilities*, *Groceries*, and *Misc Bills*, which captures all bills that are not otherwise classified in other categories. Standard errors clustered by Wednesday group X year are shown in parentheses, and p-values are shown in brackets. There are 38.7 million household-day observations underlying all regressions.

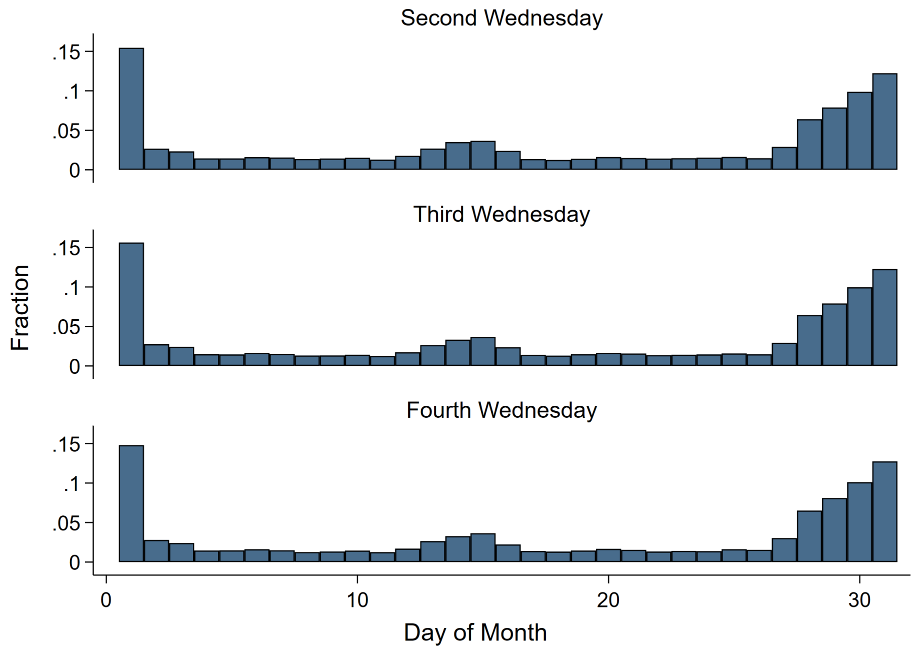
Figure A-1: Distribution of Long Pay Periods



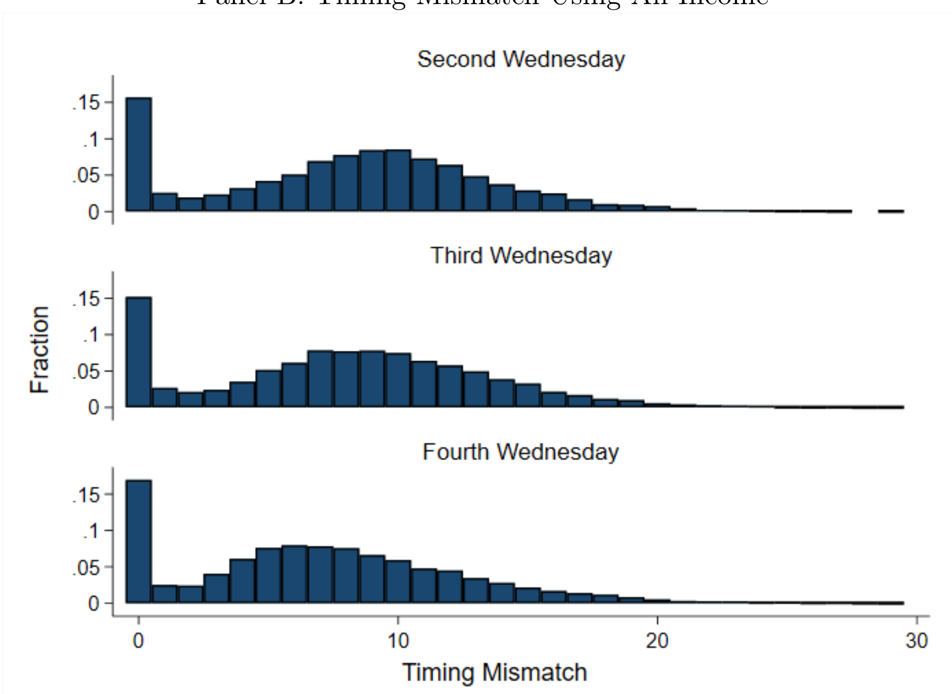
Note: This figure shows the distribution of 35-day (i.e. "long") pay periods during the years of our sample period. A month is marked as "long" if the pay periods starting in that calendar month have 35 instead of 28 days for Social Security beneficiaries paid on Wednesday.

Figure A-2: Non-Social Security Income and Timing Mismatch

Panel A: Payment Dates for Non-SSA Income



Panel B: Timing Mismatch Using All Income

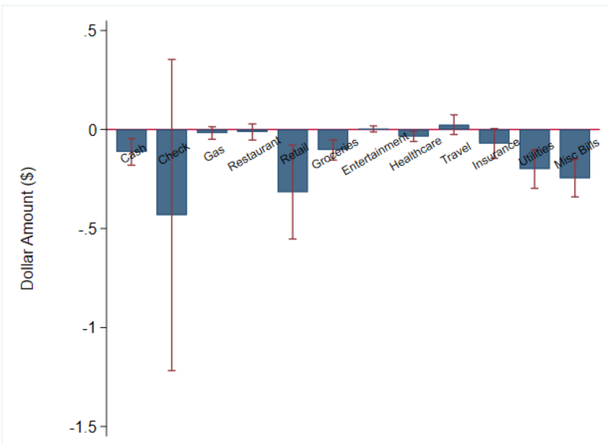


Note: Panel A shows the distribution of non-Social Security income dates by day of the month. Panel B presents the distribution of the household-level timing mismatch measure for each Wednesday group when using both Social Security and non-Social Security income.

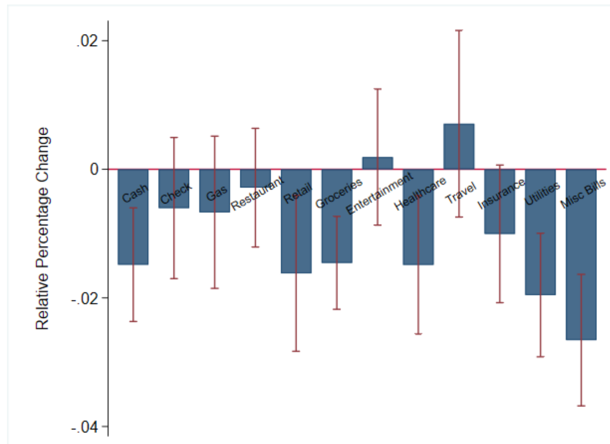
Figure A-3: The Effect of Income Timing on Spending By Category

Panel A: Long Pay Period Coefficients

(a) Dollar Amount ($)

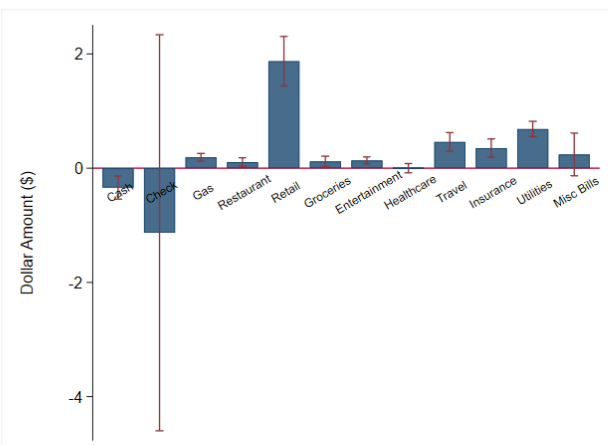


(b) Relative Percentage Change

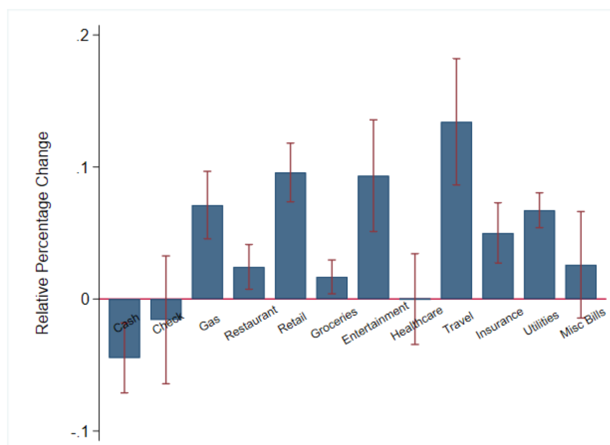


Panel B: Timing Mismatch Coefficients (weeks)

(c) Dollar Amount ($)



(d) Relative Percentage Change

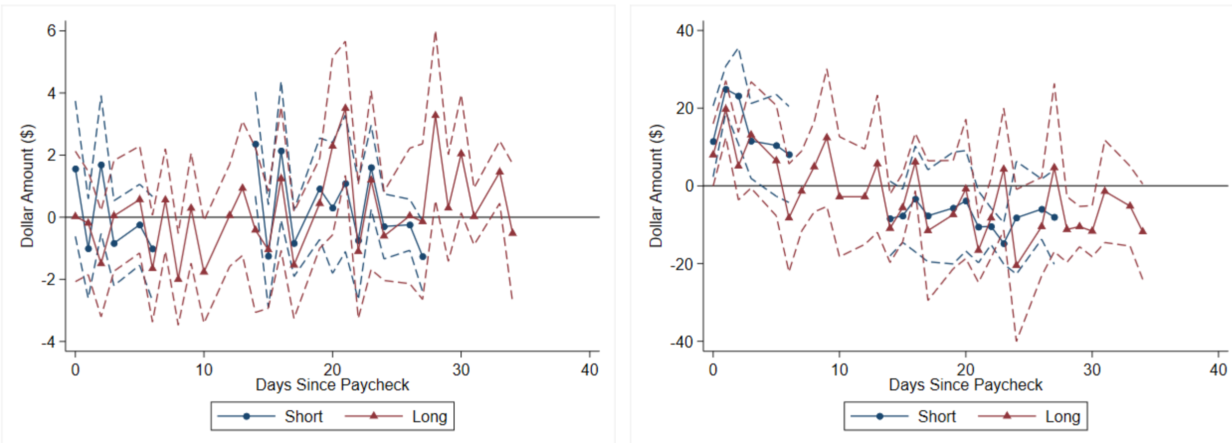


Note: The figure shows coefficient estimates and 95% confidence intervals corresponding to IV regressions of the daily amount of each spending category on an indicator for long pay periods (in Panel A) and an instrumented measure of timing mismatch (in Panel B). The regressions also include fixed effects for calendar year, calendar month, day of week, and day of month. The coefficient magnitudes are shown as absolute dollar amounts per day in subfigures a) and c), while they are expressed as a relative percentage of the mean in subfigures b) and d).

Figure A-4: Saving and Spending Over Short and Long Pay Periods

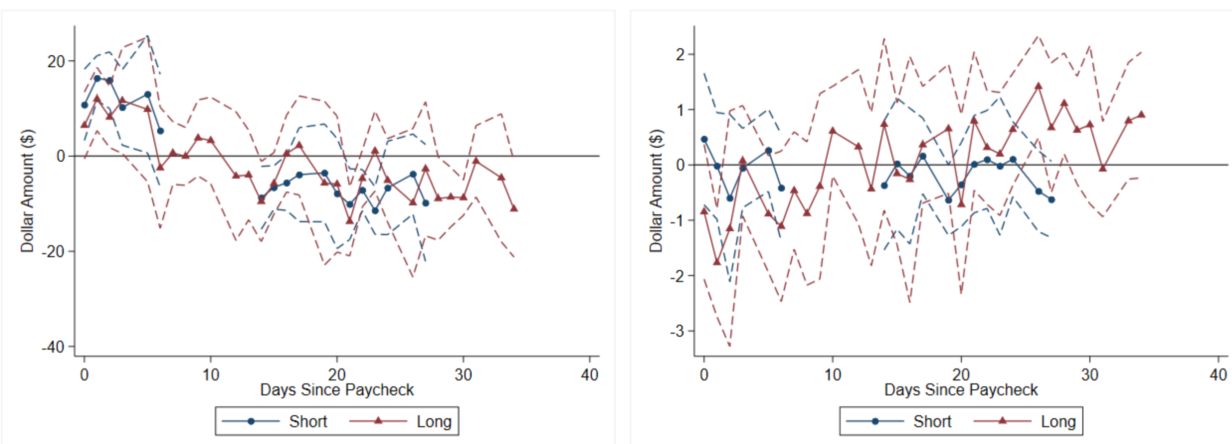
(a) Inflows From Savings

(b) Loan Outflows



(c) Consumption Spending

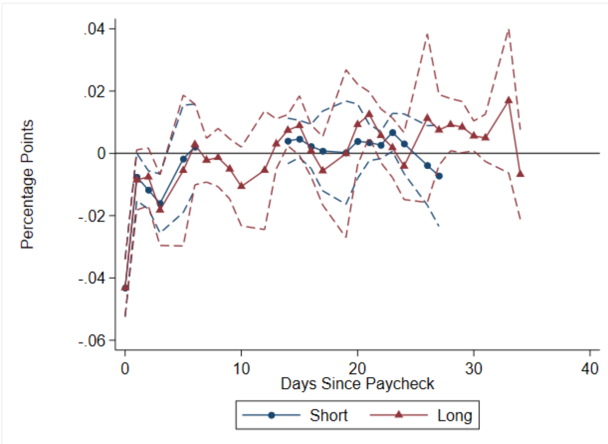
(d) Credit Card Spending



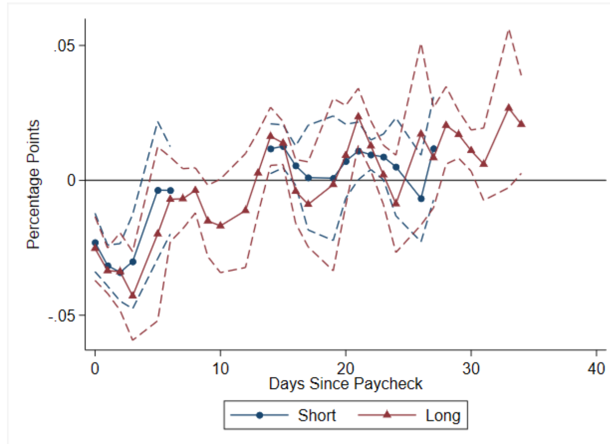
Note: The figure shows coefficient estimates and 95% confidence intervals corresponding to IV regressions of the daily net flows on saving and spending on indicators for the number of days since the last SSA paycheck, separately estimated for long and short pay periods. The regressions also include an instrumented measure of timing mismatch and fixed effects for calendar year, calendar month, day of week, and day of month. The coefficient magnitudes are shown as the average flows per day in dollars, where the average daily level for days 7-13 of short pay periods are normalized to 0.

Figure A-5: Credit Card Usage Over Short and Long Pay Periods

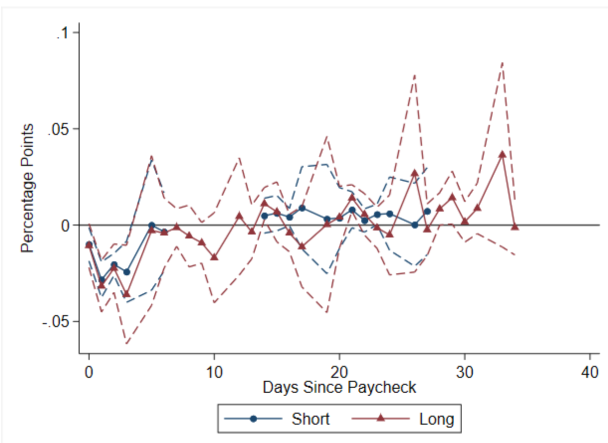
(a) Consumption Spending



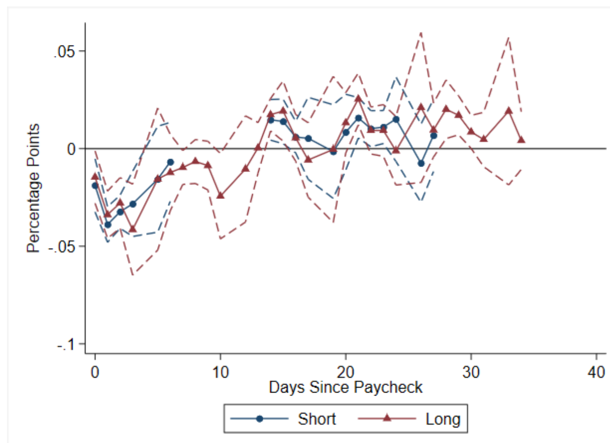
(b) Retail



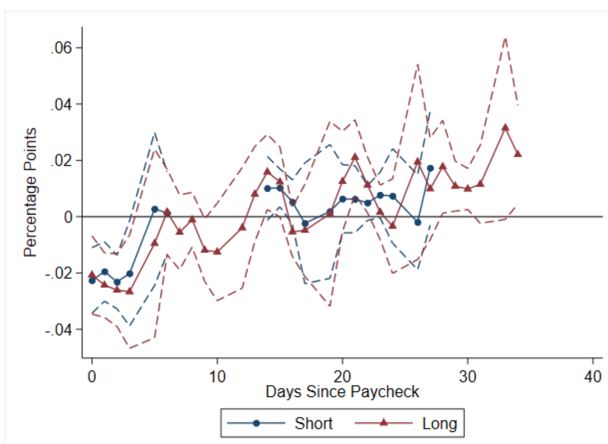
(c) Utilities



(d) Misc Bills



(e) Groceries



Note: The figure shows coefficient estimates and 95% confidence intervals corresponding to IV regressions of the daily usage of credit cards on indicators for the number of days since the last SSA paycheck, estimated separately for long and short pay periods. The regressions also include an instrumented measure of timing mismatch and fixed effects for calendar year, calendar month, day of week, and day of month. The coefficient magnitudes are shown as the average fraction of transactions in each category processed on linked credit cards, in percentage points (1 = 100%), where the daily levels of credit card usage for days 7-13 of short pay periods are normalized to 0.

Table A-1: Geographic Distribution of Account Aggregator Datase

State	% Households Residing			State	% Households Residing		
	Unfiltered Dataset	Soc Sec Sample	U.S. Census		Unfiltered Dataset	Soc Sec Sample	U.S. Census
Alabama	0.4%	0.7%	1.5%	Montana	0.1%	0.1%	0.3%
Alaska	0.2%	0.1%	0.2%	Nebraska	0.2%	0.1%	0.6%
Arizona	1.6%	4.2%	2.1%	Nevada	1.1%	1.7%	0.9%
Arkansas	0.3%	0.7%	0.9%	New Hampshire	0.2%	0.5%	0.4%
California	23.1%	16.6%	12.1%	New Jersey	2.6%	3.0%	2.8%
Colorado	0.8%	1.0%	1.6%	New Mexico	0.4%	1.0%	0.7%
Connecticut	1.2%	1.3%	1.2%	New York	22.2%	3.3%	6.3%
Delaware	0.1%	0.2%	0.3%	North Carolina	2.0%	3.7%	3.1%
District of Columbia	0.4%	0.1%	0.2%	North Dakota	0.1%	0.0%	0.2%
Florida	7.6%	13.9%	6.1%	Ohio	0.6%	0.7%	3.7%
Georgia	2.6%	4.5%	3.1%	Oklahoma	0.5%	0.8%	1.2%
Hawaii	0.3%	0.3%	0.4%	Oregon	0.6%	1.4%	1.2%
Idaho	0.1%	0.2%	0.5%	Pennsylvania	1.1%	1.0%	4.1%
Illinois	5.2%	1.6%	4.2%	Rhode Island	0.2%	0.4%	0.3%
Indiana	0.3%	0.4%	2.1%	South Carolina	0.9%	2.3%	1.5%
Iowa	0.1%	0.2%	1.0%	South Dakota	0.1%	0.0%	0.3%
Kansas	0.4%	0.6%	0.9%	Tennessee	0.8%	1.6%	2.1%
Kentucky	0.2%	0.3%	1.4%	Texas	10.1%	15.7%	8.1%
Louisiana	0.4%	0.5%	1.5%	Utah	0.2%	0.2%	0.9%
Maine	0.1%	0.3%	0.4%	Vermont	0.0%	0.1%	0.2%
Maryland	2.2%	2.6%	1.9%	Virginia	3.1%	3.5%	2.6%
Massachusetts	2.3%	2.5%	2.1%	Washington	1.1%	1.4%	2.2%
Michigan	0.7%	1.8%	3.2%	West Virginia	0.1%	0.1%	0.6%
Minnesota	0.3%	0.3%	1.7%	Wisconsin	0.2%	0.3%	1.8%
Mississippi	0.2%	0.2%	1.0%	Wyoming	0.0%	0.0%	0.2%
Missouri	0.7%	1.8%	1.9%				

Note: The table shows the geographic distribution of our sample relative to the 2010 U.S. Census. *Unfiltered dataset* is the dataset before restricting our sample to Social Security recipients. *Soc Sec Sample* is the Social Security subset of our dataset. *U.S. Census* is provided by the 2010 U.S. Census.