# Consumption Responses to Pay Frequency: Evidence from 'Extra' Paychecks[†]

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**Abstract**

Households often receive income and make consumption decisions at different frequencies. Misalignment between these two frequencies can generate variation in income that standard theory predicts should have little effect on spending. I test this prediction by exploiting variation in monthly income arising from the timing of biweekly pay. Biweekly workers typically receive two paychecks per month except for two months of the year, when they receive three. I find that spending increases by 18.4% following three-paycheck months and document that liquidity constraints likely cannot explain this result. I conclude by discussing several alternative explanations including heuristic thinking and mental accounting.

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# 1 Introduction

From investment decisions and home purchases to tax planning and grocery budgeting, households make complex financial plans over varying time horizons and at different frequencies. In making these plans, households must reconcile their cash inflows and outflows, aggregating both income and expenditures within the same, discrete "decision periods" (e.g., on a monthly or annual basis). Doing so can prove challenging, however, when the frequency at which households track their expenditures differs from the frequency at which they receive their income. For example, many large, recurring bills, such as mortgage or car payments, are due monthly, yet less than one-third of workers are paid on a monthly basis. When these two frequencies are *misaligned*, the amount of income a household receives may vary predictably across decision periods even though the actual flow of income remains constant. Under standard economic theory, this predictable variation in income should have little effect on spending behavior.

This paper provides empirical evidence that households adjust their spending in response to this type of misalignment. To show this, I exploit variation in monthly income arising from biweekly pay schedules. Biweekly-paid workers receive a paycheck every other week, or a total of 26 paychecks over the year. Because these 26 paychecks must be disbursed over 12 months, biweekly-paid workers typically receive two paychecks per month with the exception of two months out of the year, during which they receive three.[1] The timing of biweekly pay schedules thus gives rise to predictable variation in monthly income—an "extra" third paycheck received twice a year—while holding constant both total lifetime income and the environment in which that income is received. This pattern of income stands in contrast to that of semi-monthly or monthly pay schedules, under which workers receive the same amount of income each month.

A key distinguishing feature of this setting is that the third paychecks I study are not referred to as a bonus, special payment, or otherwise designated in any way that might induce biweekly workers to differentially respond to their receipt. Rather, the variation in income I examine (and thus, the extent to which there is an "extra" third paycheck) is simply an artifact of evaluating income on a monthly basis. Though a biweekly worker faces predictable variation in monthly income, they of course face no such variation in twice-weekly income. Thus, while it is not difficult to imagine biweekly households budgeting on a month-to-month basis, it is not a priori assumed whether a particular paycheck can be considered "extra." Any spending response to third paychecks must therefore arise either because households face liquidity constraints that bind at monthly frequencies or because

[1] On occasion, the calendar year is such that a biweekly worker will actually receive two paychecks per month with the exception of *three* months out of the year, during which they receive three. This occurs approximately once every eleven years and is accounted for in the analysis of this paper.

they choose to mentally bracket their income stream into monthly intervals.

To examine how households adjust their spending following "three paycheck months," I use panel data from the Consumer Expenditure Survey (CEX) over a twenty-year period from 1990 to 2010. I first identify households in the data whose heads are paid biweekly and determine the months during which they receive three paychecks. I then estimate the causal effect of these third paychecks using a difference-in-differences research design that compares spending responses following a given calendar month in years in which there are three paychecks distributed during that month to spending responses in years in which there are only two paychecks during that month. This empirical strategy takes advantage of a key feature of biweekly pay schedules: the set of months during which biweekly workers receive three paychecks differs from year to year. For example, a biweekly worker paid in the first week of January in 2008 would have received three paychecks in February and August of that year, while in the following year, that same worker would have received three paychecks in January and July instead. This variation allows me to control flexibly for seasonal patterns in spending that might arise even in the absence of any extra paychecks.

Using this identification strategy, I establish two main empirical results. First, I find that total household spending increases by approximately $262 (in 2010 dollars) on average in the month following a three-paycheck month and that this effect on spending does not persist in subsequent months. This effect represents a marginal propensity to spend of 0.157 out of the average paycheck and is a 9.2 percent increase in average monthly spending for biweekly households (or 18.4 percent after correcting for classification error).[2] Second, I find that this spending increase is due entirely to changes in durable spending, and specifically new vehicle purchases, with no corresponding response in non-durables. Conditional on purchasing a vehicle during the interview period, household spending on vehicles increases by roughly $2205 dollars on average following three paycheck months. These results are consistent with several other papers in the literature on responses to anticipated income receipt which also find large responses in durable spending (Souleles 1999; Adams et al. 2009; Parker et al. 2013; Aaronson et al. 2012). The results are robust to changes in sample composition as well as to variants of the main specification. These findings provide compelling evidence that, contrary to the predictions of standard theory, spending does in fact increase following months with three paychecks.

The key identification assumption underlying my empirical analysis is that changes in spending following a given calendar month in years in which there were three paychecks distributed during that month versus years in which there were only two would have evolved

[2]The estimated spending response to three paycheck months is subject to measurement error due to the inability to distinguish in the data which of two alternate schedules biweekly workers are paid by. Without correction, estimates of the spending response will be attenuated. Section 4.3 of the paper details the appropriate correction for this measurement error.

similarly were it not for the third paycheck. I provide three pieces of evidence in support of this assumption. First, I provide direct evidence documenting the absence of any discernible pre-trend leading up to the receipt of the third paycheck. Second, I estimate an alternative specification that compares changes in spending following three-paycheck months for biweekly-paid households to that of similar households who are paid monthly and therefore do not receive an "extra" paycheck. In contrast to biweekly workers, I find no corresponding effect for monthly-paid workers. Third, I conduct a series of placebo tests where I re-estimate the main specification using randomly generated schedules of third paychecks. These tests show that the probability of finding an effect as large as I do by chance is extremely small. Together, these three tests provide strong evidence in support of the validity of the parallel trends assumption.

The most natural explanation for why spending would respond to the receipt of a third paycheck is that households face binding liquidity constraints. To explore this possibility, I compare how the spending response I observe varies across households who are more or less likely to be constrained. To classify households as more or less constrained, I use four different proxy measures for liquidity: liquid assets, income, age, and committed consumption. This fourth measure is meant to capture the share of monthly income that is pre-committed to large, difficult-to-adjust expenditures such as rent or mortgage payments. Across all four proxy measures, I find no significant differences in the response to third paychecks. Together, these results suggest that liquidity constraints are unlikely to be the sole factor driving the spending responses I observe.[3]

Given the limited empirical support for the role of liquidity constraints, I briefly discuss several alternative explanations for the spending response to third paychecks motivated by behavioral models of non-standard preferences and beliefs, including time inconsistency, heuristic thinking, and mental accounting. While it is not possible for me to formally test these explanations, the spending responses I observe appear to be most consistent with models of heuristic thinking or mental accounting. Other recent studies on consumption responses to anticipated changes in income have similarly found evidence of spending behavior that may be driven at least in part by mental accounting (e.g., Baugh et al. 2021), which remains an important area for future research.

This research builds upon and contributes to the large literature using household-level micro-data to examine consumption responses to various types of anticipated income receipt. The breadth of the literature reflects both the general interest in understanding and cleanly identifying consumption responses to predictable or transitory changes in income, and the importance of estimating the causal effect of various fiscal policies that provide payments to

[3]This finding is consistent with several other papers that similarly find limited evidence for liquidity constraints to explain consumption responses to predictable income changes (Parker 1999; Souleles 1999; Shapiro and Slemrod 1995; Stephens, Jr. 2008; Stephens, Jr. and Unayama 2011; Baugh et al. 2021).

households. The sources of income receipt that are typically analyzed include both changes to permanent income (Wilcox 1989; Paxson 1993; Shapiro and Slemrod 1995; Shea 1995; Lusardi 1996; Parker 1999; Souleles 2002; Stephens, Jr. 2008; Aaronson et al. 2012; Ganong and Noel 2019) and predictable one-time payments such as tax refunds or stimulus payments (Souleles 1999; Browning and Collado 2001; Hsieh 2003; Johnson et al. 2006, 2009; Agarwal et al. 2007; Parker et al. 2013; Parker 2017; Kan et al. 2017; Keung 2018; Baker et al. 2021; Baugh et al. 2021; Parker et al. 2022). In contrast to much of this literature, the variation in income that I study arises merely as an artifact of evaluating income on a monthly basis and allows me to examine consumption responses to the timing of income flows using a source of anticipated income that is completely "designation-free."[4]

This paper also complements a related literature examining high-frequency responses to payments from a constant periodic income stream such as a paycheck or government transfer check (Stephens, Jr. 2003; Shapiro 2005; Huffman and Barenstein 2005; Stephens, Jr. 2006; Stephens, Jr. and Unayama 2011; Vellekoop 2018; Baugh and Wang 2021; Baugh and Correia 2022). While this paper also examines high-frequency responses, its focus is somewhat different. The papers in this literature are primarily concerned with the path of consumption over the course of a given pay period. Specifically, they look at whether households consumption smooth between anticipated payments and find that consumption significantly responds immediately following receipt before then declining. In contrast, I look at the path of consumption *across* pay periods and in response to periods with atypical income.

Finally, given the nature of the income variation that I study and the limited evidence I find in support of binding liquidity constraints, my findings also contribute to the large and growing literature on heuristics and biases (Tversky and Kahneman 1974; Laibson 1997; Barberis et al. 1998; Rabin and Schrag 1999; Loewenstein et al. 2003; Gabaix et al. 2006; Pope and Schweitzer 2011; Lacetera et al. 2012; Hastings and Shapiro 2013; Kőszegi and Szeidl 2013; Gathergood et al. 2019).[5] Most of the literature on household budgeting and mental accounting has focused on the categories to which expenditures or funds are allocated while giving relatively little attention to the *frequency* at which accounts are evaluated (Zhang and Sussman 2018). In contrast, I focus on the frequency of consumption decisions and specifically whether it differs from the frequency at which income arrives (i.e., whether there is misalignment). I show that when there is misalignment, adopting a heuristic such as constructing budgets based on typical monthly income can have important

[4]In a related paper, Ganong et al. (2020) examine the causal effect of "typical" income variation on consumption using employer-wide changes in monthly pay to instrument for changes in individual labor income. However, a significant portion of the variation in this instrument arises from fluctuations in income that workers may nonetheless naturally designate as "special," such as bonuses or commissions. In contrast, the variation I study arises only from the timing of regularly-occurring base pay.

[5]See Gilovich et al., eds (2002), DellaVigna (2009), and Beshears et al. (2018) for more examples.

implications for consumption.

The rest of the paper proceeds as follows. Section 2 introduces misalignment more formally. Section 3 provides a brief description of the Consumer Expenditure Survey data and is followed by a discussion in Section 4 of the empirical research design. Section 5 presents estimates of the household spending response to third paychecks. Section 6 considers several potential explanations for the main findings. Section 7 concludes.

# 2 Conceptual Framework

To formalize the notion of misalignment, this section presents a simple stylized model that modifies a standard consumption and savings framework to allow for the possibility that income and consumption decisions may differ in frequency. Using this model, I first show how misalignment can lead to predictable variation in current income. I then discuss the implications of this income variation for the path of consumption. To keep the model simple and focus the discussion on misalignment, I assume that capital markets are perfect and that individuals face no uncertainty.

## 2.1 Setup

Consider an infinitely-lived individual with discount factor $\delta$ and who chooses consumption, $c_t$, each period to maximize her expected total remaining lifetime utility

$$U_t(\{c_s\}_{s=t}^{\infty}) = \sum_{s=t}^{\infty} \delta^{s-t} u(c_s), \tag{1}$$

where $u(c)$ satisfies the conditions: $u_c \to \infty$ as $c \to 0$ and $u_c \to 0$ as $c \to \infty$. The individual makes her consumption decisions at a frequency defined by the period $t$. To help with intuition, it is easiest to think of time as a continuous measure that is divisible into periods of varying lengths. For example, if $t$ indexes months, then we can think of a period $t$ as being of "length" equal to one month and of consumption decisions as being made at a monthly frequency.

To allow for potential misalignment between consumption and income, I assume that the individual receives a periodic income flow equal to $y_\tau$, where $\tau$ may index a length of time different from that indexed by $t$. Thus total income received during a given consumption period $t$ is simply equal to $y_t = \sum_{\tau \in [t,t+1)} y_\tau$. When income is received at the same frequency as that of consumption decisions, this transformation reduces to the expression $y_t = y_\tau$.

I assume that the individual receives a fixed payment, $y_\tau = \bar{y}$, as income each $\tau$-period. This assumption reflects the fact that pay is often disbursed regularly and in fixed amounts.

Aggregating to the frequency of her consumption decisions, the individual's $t$-period income can be expressed as

$$y_t = \begin{cases} Y & \text{if } (t-1) \bmod n \neq 0 \\ (1+b)Y & \text{if } (t-1) \bmod n = 0 \end{cases}, \tag{2}$$

for some $n \geq 1$ and $b \geq 0$. As this expression shows, in most periods, the individual receives a fixed payment of $Y$, or "typical" income; however, every $n$ periods, she instead receives a payment of $(1+b)Y$, or "atypical" income. Under this framework, misalignment between the timing of consumption and the timing of income exists when $b$ is strictly greater than zero and $n$ is strictly greater than one. That is, $t$-period income varies when the timing of consumption and the timing of income are misaligned even though $\tau$-period income is constant.

In the context of this paper, an individual who is paid biweekly but makes her consumption decisions monthly faces misalignment between the timing of her consumption and the timing of her income. Specifically, her period $t$ income is given by equation (2), where each period $t$ is a month, typical income is equal to two paychecks ($Y = 2\bar{y}$), and atypical income is a result of the third paycheck ($bY = \bar{y}$) that is received every $n = 6$ months.[6] The same biweekly worker would face no misalignment if she were to instead make her consumption decisions at a biweekly frequency.

## 2.2 Predicting Consumption Responses in the Presence of Misalignment

We can next explore the implications of misalignment and any resulting income variation for the path of consumption. I start by characterizing lifetime wealth under the framework outlined above. Let an individual's cash-on-hand, $X_t$, be defined as the sum of her current income and assets and let $R$ be the gross interest rate she faces, which I assume to be constant over time. The present discounted value of the individual's remaining lifetime wealth at the beginning of time $t$ can be written as

$$\begin{aligned} W_t &= X_t + \sum_{i=1}^{\infty} \frac{y_{t+i}}{R^i} \\ &= X_t + \sum_{i=1}^{\infty} \frac{Y}{R^i} + \sum_{i=1}^{\infty} \frac{bY}{R^i} \cdot (\mathbb{1}_{\{(t-1+i) \bmod n = 0\}}), \end{aligned} \tag{3}$$

where the indicator $\mathbb{1}_{\{(t-1+i) \bmod n=0\}}$ evaluates to one in periods with atypical income and zero otherwise. Assuming the no-Ponzi condition, $\lim_{t\to\infty} \frac{X_{t+1}-y_{t+1}}{R^t} \geq 0$, is satisfied, the

[6]The frequency of arrival for third paychecks is not quite every six months, but $n = 6$ provides a good approximation and simplifies the exposition of the model substantially.

usual Euler equation characterizing optimality is given by

$$u_c(c_t) = \delta R u_c(c_{t+1}).$$

To complete the model, I assume individuals have CRRA preferences. We can then rewrite the above expression as

$$c_{t+1} = (\delta R)^{1/\rho} c_t. \tag{4}$$

Assuming some minimum impatience ($(\delta R)^{1/\rho} < 1$), there exists a well-defined solution where, in equilibrium, consumption is proportional to lifetime wealth for all periods $t$. Together with equation (4), this fact implies that optimal consumption is given by

$$c_t = (1 - (\delta R^{1-\rho})^{1/\rho}) W_t. \tag{5}$$

As is standard in such setups, consumption is independent of current income and depends only on lifetime wealth. Furthermore, lifetime wealth does not depend on the frequency of arrival for income or whether the timing of income and consumption are misaligned. To see this, note that

$$W_t = X_t + \sum_{i=1}^{\infty} \frac{y_{t+i}}{R^i} = X_t + \sum_{i=1}^{\infty} \frac{\sum_{\tau \in [t+i, t+i+1)} y_\tau}{R^i}.$$

Since individual income can always be aggregated to the same frequency as consumption decisions, the arrival frequency of income doesn't matter for lifetime wealth. And absent any borrowing constraints, an individual can simply borrow or save so as to undo any income variation that arises from misalignment. A standard consumption-savings framework therefore predicts that misalignment and the resulting variation in income should be irrelevant for the path of consumption. I test this prediction in the following empirical analysis.

## 3 Data

The empirical analysis in this paper uses data from the Consumer Expenditure Survey (CEX) interview sample between the years 1990 and 2010. The CEX is a nationally-representative, rotating panel survey conducted by the Bureau of Labor Statistics (BLS). The survey provides detailed information on household spending as well as income and household characteristics. Households in the survey are interviewed once every three months over five consecutive quarters. The first introductory interview gathers basic demographic information and details on the stock of major durables owned by the household. For each of the subsequent four interviews, households are asked to recall their expenditures over the past three months as well as the month in which each expenditure occurred. While households are interviewed on a quarterly basis, the design of the survey effectively provides monthly-level data on household expenditures.[7]

During the second and fifth interview, the CEX collects information on income and employment for each household member aged 14 or older. Crucially, the survey captures both the gross amount of his or her last paycheck as well as the length of the corresponding pay period. I use this information to identify a household member's pay frequency and in particular whether the member is paid biweekly.

### 3.1 Measuring Consumption Expenditures

The CEX captures expenditures in hundreds of categories, from power tools to child care. I aggregate these categories into five broad measures of consumption expenditure: food, strictly non-durable, non-durable, durable, and total expenditure. Changes in these five measures serve as the main outcomes of interest in the baseline empirical analysis.

Household expenditures on food include both food consumed at home and food consumed away from home. Food expenditure has been the focus of a number of papers exploring consumption behavior, in part because of the often limited information on non-food consumption in commonly-used longitudinal data sets.[8] However, restricting attention to food consumption alone has obvious limitations. For this reason, I also consider the broader category of non-durable expenditures as well as a related measure of "strictly non-durable" expenditures. Because the empirical analysis studies changes in expenditures over relatively short time-horizons, some expenditures commonly thought of as non-durable, such as apparel or reading materials, may be more appropriately classified as semi-durable. The strictly non-durable measure thus captures only the subset of non-durable expenditures that we might consider truly non-durable in the short run.[9] For the measure of durable goods, I include expenditures on major appliances, flooring, furniture, shelter, and vehicles. Finally, total expenditure is simply the sum of the durable and non-durable expenditure measures.

[7] Because I am interested in estimating spending responses in *months* following three paycheck months, I use the information provided by households on the month of expenditure to construct expenditure data on the household-monthly level rather than the household-quarterly level. As a result, time adjustment routines used by the BLS when constructing the CEX data have important consequences for estimation. These routines and their implications for my analysis are discussed in detail in Section 5.3.3 of the paper. Since households are asked to report expenditures over a three-month period, one additional concern is that the cognitive burden of remembering expenditures precisely may lead households to instead approximate their spending by reporting the same expenditure amounts for each month—effectively smoothing their reported expenditures across months. Such behavior would lead to measurement error in household expenditures and likely attenuate my findings.

[8] See Hall and Mishkin (1982), Zeldes (1989), Runkle (1991), and Shea (1995) for some examples of papers focusing on food consumption in testing the sensitivity of consumption to changes in current income.

[9] See Lusardi (1996) for additional discussion of strictly-non durable consumption.

### 3.2 Sample Restrictions and Descriptive Statistics

The sample restrictions I impose to arrive at the main analysis sample follow the existing literature closely and are discussed in detail in Appendix A. In addition to the standard restrictions from the literature, I impose two further restrictions that bear mentioning. The first restriction addresses an important assumption underlying the empirical analysis—the monthly variation in income arising from biweekly pay schedules is not just predictable but known. While it is not possible to directly measure awareness, we might be concerned that workers that are new to being paid biweekly are likely to be unaware of the third paychecks. Because the CEX does not include direct information on job tenure, as a next best approach, I restrict households to those with heads who report working full time (at least 50 weeks) over the past year and whose reported pay is unchanged between their second and fifth interview. This restriction selects for households whose heads are likely to have been employed at the same job during the past year and are therefore more likely to be aware of the presence and timing of the third paychecks.

The second sample restriction addresses an important limitation of the data. While the CEX provides information on the amount and corresponding pay period for a worker's last paycheck, it does not report the actual date on which he or she was paid. As a result, it is not possible to determine which of the two possible alternate biweekly pay schedules applies to a given biweekly-paid worker. To see why this data limitation is a concern, consider a biweekly worker that is paid during the first week of January 2008 and then every two weeks afterwards. This worker is paid on an alternate schedule to that of a biweekly worker that is paid during the *second* week of January and then every two week afterwards. The first biweekly worker would receive three paychecks in February and August of that year whereas the second biweekly worker would receive three paychecks in May and October. In other words, which schedule a biweekly worker follows determines the months of the year during which he or she will receive three paychecks instead of two. The possibility for members of the same household to be on alternate biweekly schedules introduces potential noise. I therefore exclude households where other employed members of the household are paid at the same frequency as the head of household from the sample.[10] In Section 4, I formally address how any remaining error due to this type of mis-classification in the final sample of biweekly-paid households will affect the interpretation of the results.

After sample restrictions, I am left with a full sample of 24,822 household-month observations for 7,776 households whose heads report being paid either weekly, biweekly, or monthly. The main sample used in the analysis consists of the 4,316 biweekly households from the full sample. Table 1 presents summary statistics for both the full sample of house-

[10]Relaxing this restriction leads to similar results.

holds (columns (1) and (2)) and the main sample of just biweekly households (columns (3) and (4)).[11] Average monthly expenditures for each of the five aggregate measures of consumption expenditure are reported both in levels and in changes. For both the full sample and the biweekly sample, durable and non-durable expenditures each represent about half of total monthly expenditures on average. Monthly changes in expenditure are quite small relative to the mean level of expenditure for each aggregate measure.

The final rows of the table summarize information on income and assets. The average paycheck amount in the full sample is $1,524 while the average annual before-tax earnings is $53,853. The differences in income and assets between the full sample and the biweekly sample are largely what would be expected. Households in the biweekly sample earn slightly more and hold slightly higher balances in their checking and savings accounts than the full sample of households but do not otherwise appear to significantly differ from the full sample across the key variables of interest. The lower income and assets for the full sample is primarily due to the inclusion of weekly households, who earn lower wages and have lower bank balances than biweekly- and monthly-paid workers. Figure 1 plots the distribution of household income by the three pay frequencies available in the full sample. As we would expect, the biweekly distribution is shifted slightly to the left of that of monthly workers, and the weekly distribution is shifted slightly to the left of that of biweekly workers.

# 4 Empirical Methodology

## 4.1 Identification Strategy

The main empirical analysis estimates how consumption expenditures respond to the monthly variation in income induced by biweekly pay schedules, or third paychecks. To do so, I exploit the fact that the calendar months in which there are three paychecks changes from year to year. For instance, a biweekly worker paid in the first week of January 2008 would receive three paychecks in February and August of that year. In 2009, that worker would instead receive three paychecks in January and July. Appendix Table B.1 lists the full schedule of months with three paychecks for each year between 1989 and 2010. As the table shows, each calendar month serves as a three paycheck month at least once during my sample period. I am thus able to identify responses to third paychecks by exploiting cross-year variation in when particular calendar months contain three paychecks.[12]

[11] Monetary values are in 2010 dollars in this and all subsequent tables.

[12] This empirical strategy uses variation within months but across households because the CEX only provides income and expenditure data for at most twelve consecutive months for any given household that is interviewed. Alternatively, I could compare responses within a household across months. This alternative approach, however, would not allow me to control for seasonal variation in consumption across months. Estimates using variation within households and across months are slightly larger than those controlling

Specifically, I estimate responses to third paychecks using a difference-in-differences research design that compares changes in consumption expenditures following a given calendar month for years in which there were three paychecks distributed during that month (treatment months) to changes in years for which there were only two paychecks during that month (control months). The key identification assumption underlying this approach is that, were it not for the third paycheck, changes in consumption expenditures following a given calendar month in years in which there were three paychecks distributed during that month versus years in which there were only two would have evolved similarly.

In Section 5 below, I provide two direct pieces of evidence in support of the validity of the parallel trends assumption. First, I show that there is no response in consumption expenditures leading up to the month in which there is a third paycheck and that the response only occurs once the third paycheck has been received. Second, I also validate my results using an alternative research design that compares the change in consumption following three paycheck months for biweekly-paid households to that of similar households whose heads were paid monthly. Because households who are paid monthly only receive a single paycheck in each month, this approach allows me to rule out the possibility that the set of three-paycheck months is somehow "special" in ways other than the fact that biweekly-paid household receive an extra paycheck during those months. This approach also addresses the concern that supply-side behaviors, such as store promotions or sales, happen to coincide with the timing of three paycheck months since it is unlikely that biweekly-paid households would be differentially exposed to these promotions.[13] The findings from this analysis are qualitatively similar to the baseline results, lending support to the claim that my estimates represent the true causal effect of the third paychecks on the spending pattern of biweekly-paid households.

## 4.2 Estimation

As described in Section 3, an important limitation of the CEX data is that it does not report the actual date on which a worker is paid. This data limitation has direct implications for the estimation strategy since there are two possible schedules according to which a biweekly worker can be paid. To fix notation, let $S_1$ be the set of three paycheck months under the first schedule and $S_2$ be the set of three paycheck months under the second schedule. Since the CEX does not allow me to distinguish which of the two possible schedules a given biweekly worker follows, I instead work with the full set of potential three paycheck months:

instead for seasonal variation across months.

[13] Adams et al. (2009) note that auto loan applications and sales spike precisely at the time of tax rebates, and that auto sales companies pay close attention to "tax rebate season." In discussions with a large auto company to assess whether they were likewise aware of third paychecks due to biweekly schedules, the company claimed no knowledge of these third paychecks, although they did respond to tax rebate season.

$S = S_1 \cup S_2$.

To measure how consumption expenditures change following these months, I estimate variants of the following specification:

$$\Delta c_{it} = \beta * \mathbf{1}_{\{t-1 \in S\}} + X'_{it}\alpha + \gamma_{m(t)} + \delta_{y(t)} + \epsilon_{it}, \tag{6}$$

where $\Delta c_{it}$ is the change in consumption expenditures from the prior month for household $i$ in month $t$, $X_{it}$ is a vector of household-specific and potentially time-varying taste-shifters, and $\gamma_{m(t)}$ and $\delta_{y(t)}$ are fixed effects for the calendar month (e.g., January) and year (e.g., 2008) to which month $t$ belongs. The dummy variable $\mathbf{1}_{\{t-1 \in S\}}$ is an indicator for whether the prior month is a three paycheck month under either schedule. I estimate responses in this way—for months *following*, not during three paycheck months—because third paychecks are paid out during the last week of a month. The coefficient of interest, $\beta$, measures the response in consumption expenditures to third paychecks, holding constant individual preference-driven changes in consumption, seasonal variation, and aggregate trends over time. To account for serial correlation and household-specific random shocks, I cluster the standard errors at the household level in all specifications.

### 4.3 Correcting for Classification Error

While in practice a biweekly worker only ever receives three paychecks during two months out of the year, the indicator, $\mathbf{1}_{\{t-1 \in S\}}$, is defined to assume four. Absent any correction, this mis-classification in biweekly schedules will lead to inconsistent estimates of $\beta$ that are biased towards zero. Intuitively, for a given household, only half of the months I identify as three-paycheck months will actually contain three paychecks. Under the assumption that non-three-paycheck months have no effect on consumption, this would imply that the estimate of $\beta$ from equation (6) is roughly half the size of the "true" effect of receiving a third paycheck.

To address this classification error, I formally derive and estimate a correction factor for $\beta$. Because I am unable to observe the fraction of biweekly households that follow one schedule versus the other, I estimate the correction factor allowing the true proportion of individuals on either schedule to vary between zero and one. I find that the estimated correction factor ranges from 1.9 to 2.1 depending on the share of households paid according to each schedule and is on average equal to 2.0.[14] We can therefore correct for the classification error by simply multiplying the estimate of $\beta$ from equation (6) by a factor of two. For simplicity, I present the uncorrected estimates in the results. Additional details on the

[14]The correction factor ranges as a result of the sample size. For an arbitrarily large sample, the correction factor is independent of the share of households under either schedule and converges to two.

classification correction are provided in Appendix B.

# 5 Consumption Responses to Third Paychecks

This section presents the main difference-in-differences estimates of the consumption response to third paychecks. I first present estimates for each of the five aggregate measures of consumption expenditure: food, strictly non-durable, non-durable, durable, and total expenditure. To give a sense of the path of consumption and to support the validity of the parallel trends assumption, I also present estimates from a more flexible difference-in-differences specification that allows the consumption response to vary by month relative to the three paycheck month. Finally, I estimate the response to third paychecks using disaggregated measures of consumption expenditure to determine which subcategories of expenditure may be driving the overall response.

## 5.1 The Response in Aggregate Consumption Expenditures

Table 2 presents estimates from the difference-in-differences specification given by equation (6) with the dollar change in household spending from month $t-1$ to month $t$ for each of the five aggregates measures of consumption expenditure as the outcome. Each column reports estimates from a separate regression that includes the indicator $\mathbf{1}_{\{t-1 \in S\}}$ for whether the prior month was a three paycheck month, fixed effects for the month and the year of observation, and a set of household-specific and potentially time-varying taste-shifters. The taste shifters include the age of the head of household and changes in the number of both children and adults in the household.

The first column of the table measures the average response in total expenditures to third paychecks. The reported coefficient estimate on the indicator $\mathbf{1}_{\{t-1 \in S\}}$ implies that biweekly households increase their total expenditures by \$262 on average in months following three paycheck months. This increase in total expenditures is statistically significant at the one-percent level and is economically meaningful, representing roughly 15.7 percent of the average biweekly paycheck and 9.2 percent of average monthly spending by biweekly households. Correcting for classification error, this estimate translates to \$523 or an 18.4 percent increase in total expenditures following three paycheck months. The remaining columns of the table report estimates of the response to third paychecks for durable, non-durable, strictly non-durable, and food expenditures. These estimates show that the overall response in total expenditures is driven almost entirely by changes in expenditures on durable goods. The estimated response in column (2) indicates that durable expenditures increases by \$257 following three paycheck months; in contrast, the estimated responses for non-durable expenditures in columns (3)–(5) are both small and statistically insignificant.

In the Appendix, I show that these estimates are robust to several alternative specifications.

To give a sense of the path of consumption, Figure 2 plots estimates using a more flexible difference-in-differences specification that includes the same controls as those given by equation (6), but that allows consumption responses to vary by month relative to the three paycheck month (denoted in the figure by $t = 0$). The purpose of this figure is two-fold. First, the figure allows us to examine whether there is short-run persistence in the response to third paychecks. Because third paychecks do not reflect actual changes in lifetime wealth, we might expect the response to decrease over time. Second, the figure allows us to assess the validity of the parallel trends assumption by showing the path of consumption leading up to the receipt of the third paycheck. Consistent with the estimate from column (2) of Table 2, the first panel of the figure shows an immediate increase in durable spending following a three paycheck month. However, this response does not appear to persist beyond the first month following receipt. Consistent with the estimate from column (4) of Table 2, the second panel shows no response in strictly non-durable expenditures following a three paycheck month.[15] For both durable and strictly non-durable expenditures, the figure shows no discernible pre-trend leading up to the receipt of the third paycheck. While I am unable to examine longer pre-trends, the lack of a trend during the two months leading up to the receipt of the third paycheck provides strong support for the validity of the parallel trends assumption underlying the difference-in-differences estimates.[16]

### 5.2 The Response in Durable Expenditures

The evidence presented in the previous section suggests that there are large increases in durable spending following three paycheck months. In this section, I examine household responses to third paychecks using disaggregated measures of durable spending to determine which subcategories of expenditure may be driving the overall response. I begin by considering expenditures on four subcategories of durable goods: vehicles (the value of the vehicle minus any trade-in allowance), furniture, flooring, and major appliances (e.g., refrigerators or stoves). The first column of Table 3 repeats the overall response in durable expenditures from column (2) of Table 2 for reference. The remaining columns of the table report estimates from the difference-in-differences specification given by equation (6) using the four subcategories of durable expenditures as the outcome. Disaggregating the overall response in durable expenditures indicates that the effect is concentrated primarily in spending on vehicles. There is a positive but economically insignificant response in spending on flooring. All other estimates are both statistically and economically insignificant.

[15] The higher precision in the estimated response for strictly non-durables than for durables reflects the higher variance in durable goods spending, which is often characterized by large and infrequent purchases.

[16] Because I am unable to observe which of the two possible pay schedules a biweekly worker follows, this specification can only include at most two leads and two lags to the three paycheck month.

Table 4 further disaggregates vehicle expenditures by whether the vehicle is a car (e.g., automobiles, trucks, and vans) or a motor vehicle (e.g., motorcycles, motor scooters, and mopeds) and by whether the vehicle is new or used. The first column of the table repeats the previously estimated response for vehicle expenditures from column (2) of Table 3. The difference-in-differences estimates in columns (2)–(5) for the four subcategories of vehicle expenditure indicate that purchases of new cars account for nearly 80 percent of the overall response in vehicle spending, with expenditures on new cars increasing by $201 on average following a three paycheck month. The remaining two columns of the table report estimates for the extensive margin probability of purchasing a car. These estimates are obtained from a difference-in-differences specification that includes the same controls and indicator $\mathbf{1}_{\{t-1\in S\}}$ given by equation (6), but that uses an indicator for whether a new car (column (6)) or used car (column (7)) was purchased as the outcome. To aid interpretation, I multiply this indicator by 100 so that all point estimates can be interpreted as percentage point effects. The specifications for these two columns are estimated using simple linear probability models and indicate a small but statistically significant increase of 0.4 percentage points, which corresponds to an increase of approximately 63 percent over the baseline probability of making a new car purchase in non-three paycheck months. Since the response in spending on motor vehicles is economically small and statistically insignificant, I focus on expenditures on cars from here on out.

While the preceding evidence suggests that spending on cars increases significantly following three paycheck months, the responses are estimated using the full purchase price of a car (minus any trade-in allowance) rather than actual out-of-pocket spending. Beginning in the second quarter of 1991, the BLS started collecting information on vehicle financing for the CEX. Using this information, I am able to estimate responses to third paychecks using several more detailed categories of spending on cars: down payment amounts, debt financing, and out-of-pocket expenditure (equal to the down payment if financed and the full purchase price if not).

Table 5 presents summary statistics on overall car expenditures (both new and used) as well as the three more detailed categories of spending on cars for the main sample of biweekly households. These average monthly expenditures are reported both unconditional (columns (1) and (2)) and conditional (columns (3) and (4)) on a biweekly household ever purchasing a car during their survey period. The overall monthly expenditure on cars in the sample is on average $299. However, these expenditures are made by a relatively small number of households: of the 4,316 biweekly households in the main sample, about 10 percent report purchasing a car during the 12-month period that they were surveyed.[17] Conditional on

[17] This fact is consistent with evidence from Misra and Surico (2014), who find that much of the durable spending response to the 2001 and 2008 tax rebates was driven by a relatively small group of households purchasing vehicles.

ever purchasing a car, the average overall expenditure on cars is \$2723, with approximately one-third of overall expenditure being out-of-pocket and the remaining two-thirds due to debt financing. Since households who ever purchase a car do not purchase one every month, it is also helpful to report summary statistics for actual car purchases. The average value of a car purchased by a biweekly household during the sample period is \$12,497, with again one-third being out-of-pocket and two-thirds due to financing. Conditional on financing part of the purchase, households put down approximately \$3531 or 21.2 percent of the purchase price of the car. By comparison, the average biweekly paycheck is approximately \$1,668.83. This means that the extra third paycheck is enough to cover nearly half of the average downpayment required, which for many households could plausibly affect the ability to purchase a vehicle.

Panel A of Table 6 presents the difference-in-differences estimates of the average response in overall car expenditures (column (1)) and the three more detailed categories of car spending (columns (2)–(4)) following a three paycheck month. The estimate reported in the first column of the table indicates that spending on cars, both new and used, increases by \$247 on average following a three paycheck month. Consistent with the descriptive statistics presented in Table 5, the response in out-of-pocket expenditures is approximately one-third of the overall response in car spending and indicates a statistically significant increase of \$83 following a three paycheck month, which suggests that response in overall car expenditures to three paychecks is not simply a result of debt financing. The estimated response in down payments is smaller and imprecisely estimated. Panel B of the table presents qualitatively similar estimates for the subset of biweekly households who report ever purchasing a car during their survey period.

Taken together, the results presented above suggest that biweekly workers do respond to the variation in income induced by misalignment. Consumption expenditures, and in particular spending on cars, increases significantly following a three paycheck month. These results are consistent with prior findings in the literature on consumption responses to anticipated income receipt that also report large responses in durable expenditures, and specifically, cars. (Souleles 1999; Adams et al. 2009; Parker et al. 2013; Aaronson et al. 2012).

## 5.3 Additional Robustness Checks

### *5.3.1 Comparing Against Monthly Households*

The identification assumption underlying the main difference-in-differences estimates is that, were it not for the third paycheck, changes in consumption expenditures following a given calendar month in years in which there were three paychecks distributed during that month versus years in which there were only two would have evolved similarly. An implication of this assumption is that there should be no corresponding response for workers paid at frequencies that do not induce income variation. As a further test of the parallel trends assumption, I add monthly-paid households to my estimation sample and estimate an alternative specification that compares the change in consumption following three paycheck months for biweekly-paid households to that of similar households that were paid monthly.[18] Specifically, I estimate a variant of the difference-in-differences specification given by equation equation (6) that adds an additional indicator for whether a household is paid monthly and the interaction of this indicator with the the indicator $\mathbf{1}_{\{t-1 \in S\}}$ for whether the prior month was a three paycheck month.

Table 7 presents estimates of the effect of third paychecks from this alternative specification for biweekly-paid and monthly-paid households using the five aggregate measures of consumption expenditure as the outcome. As seen in the first row of the table, the estimated response by biweekly-paid households are qualitatively similar to the baseline results reported in Table 2. In contrast, the estimated response for monthly-paid households reported in the second row of the table is statistically insignificant and, if anything, the opposite sign of that for biweekly households. The findings from this alternative approach lend support to the validity of the parallel trends assumption. They also help to rule out the concern that supply-side behaviors, such as store promotions or sales, happen to coincide with the timing of three paycheck months since it is unlikely that biweekly-paid households would be differentially exposed to these promotions.

#### *5.3.2 Placebo Tests*

The findings presented in the previous section suggest that it is unlikely that the set of three-paycheck months is somehow "special" in ways other than the fact that biweekly-paid households receive an extra paycheck during those months. Still, a potential concern remains that the difference-in-difference estimates I find are simply due to chance. To address this concern, I conduct a series of placebo tests for the estimate of the overall response in total expenditures following a three paycheck month. Each placebo estimate is generated using a false paycheck schedule under which a random subset of the months spanning the sample period from 1990 to 2010 are designated as three paycheck months. Specifically, to create a false paycheck schedule, I randomly select four separate months for each year in the sample

[18]The ideal comparison group would be households with heads who report being paid semi-monthly. Semi-monthly workers are paid twice a month, typically on the 15th and 30th of a month, and therefore receive a similar number of paychecks per year as biweekly workers but do not experience variation in the number of paychecks received each month. However, the number of households with heads who are semi-monthly is small (less than 4 percent of the households in my sample over the time span considered). For this reason, I use monthly-paid households as the comparison group for this analysis.

period to be three paycheck months. I select four months, not two, because the coefficient of interest, $\beta$, from the main specification given by equation (6) measures the response to third paychecks under either of the two possible biweekly pay schedules. Since the set of three paychecks months under the true paycheck schedule never follow consecutively, I also impose the restriction that none of the four randomly-selected months in a representative year can be consecutive. This exercise is repeated 1,000 times. For each of these 1,000 false paycheck schedules, I re-estimate the baseline regression from column (1) of Table 2 as if that schedule were the true schedule. Figure 3 plots the distribution of these 1,000 placebo coefficient estimates. The true estimate from column (1) of Table 2 is marked in the figure by the vertically dashed line. As the figure makes clear, the true estimate is far larger than almost all of the placebo estimates, and the distribution of placebo estimates is centered on zero. This suggests that overall response I estimate is unlikely to have been generated simply by chance.

#### *5.3.3 Time Adjustments in the CEX*

Households in the CEX are interviewed on a quarterly frequency. For each of these interviews, they are asked to recall their expenditures over the past three months (i.e., the reference period) as well as the month in which each expenditure occurred. While households in the CEX are interviewed on a quarterly basis, the design of the survey effectively provides monthly-level data on household expenditures. However, for a subset of expenditure categories (e.g., tobacco products or personal care services), households are asked to report their quarterly- or annual-level of expenditure rather than the more typical monthly-level of expenditure. The BLS uses pre-determined time adjustment routines when mapping expenditures in this subset of categories to their associated month of purchase.[19] Whether a given expenditure category is time-adjusted depends on both the type of expenditure and the information source for the expenditure.

In this section, I discuss three time adjustment routines in particular that may have important implications for the empirical analysis. Under the first time adjustment routine, the BLS divides reported quarterly expenditures by three and then maps this amount to each of the three months in the reference period. For example, if a household reported spending \$300 on tobacco products over the May to July period, the BLS would assign \$100 in tobacco expenditures to each of May, June, and July. Under the second routine, the BLS divides reported annual expenditures by twelve and then maps this amount to each of the three months in the reference period. Under the third routine, the BLS maps the entirety of reported expenditures for the category to a random month in the reference period. The first two time adjustment routines effectively smooth consumption expendi-

[19]See Hai et al. (2020) for additional description of the time adjustment routines used by the BLS.

tures across the months within a reference period, so any effect on my estimates would likely bias them toward zero. Since the month that expenditures are allocated to under the last time-adjustment routine is random and is therefore unlikely to coincide in timing with the set of three paycheck months, any effect under this routine would similarly bias my estimate toward zero. Nonetheless, as a robustness check, I re-estimate the main differences-in-differences specification given by equation (6) using the five aggregate measures of consumption expenditure as the outcome, but exclude from these measures the subset of expenditures that undergo one of the three time adjustment methods.[20] These estimates are reported in Table 8 and are similar in magnitude and significance to the estimates using the full set of expenditures from Table 2. This suggests that the time adjustment routines do not have a meaningful effect on the analysis.

# 6 Potential Explanations

The results in the previous section provide strong evidence that consumption expenditure, and in particular spending on cars, increases following three paycheck months. In this section, I explore potential explanations for these findings. First, I examine the role of liquidity constraints and find limited evidence that such constraints explain the response to third paychecks. I next discuss several additional explanations motivated by behavioral models of non-standard preferences and beliefs.

## 6.1 The Role of Liquidity Constraints

Liquidity constraints offer the most natural reason for why household spending would respond in this way and is the leading explanation for findings of consumption responses to predictable income changes in other contexts (see Jappelli and Pistaferri 2010 for an overview). In this subsection, I assess the role that liquidity constraints may play in driving my results by comparing how the consumption response to third paychecks varies across households who are more or less likely to be constrained.

To classify households as more or less constrained, I use four different proxy measures for liquidity: liquid assets (sum of household checking and savings account balances), income, age, and committed consumption. Liquid assets are perhaps the most direct way to proxy for liquidity, but account balances are collected only once over the survey period and are often missing in the data.[21] Any tests of the role of liquidity constraints using liquid assets

[20] I use the parsing file provided by the BLS which provides a list of the universal classification codes (UCCs) for expenditures that undergo time adjustment in addition to the type of time adjustment routine that is applied.

[21] For example, only one third of survey respondents in the main sample of biweekly-paid households report information on liquid assets.

are therefore likely to be significantly underpowered. For this reason, I also proxy for constraints using household (before-tax) income. While income may not reflect liquidity to the same extent as liquid assets, we would expect the two measures to be highly correlated. As a third proxy, I use the age of the head of the household and follow the literature in assuming younger households are more liquidity constrained than older households.[22]

These first three measures—liquid assets, income, and age—are standard proxies of liquidity used in the literature. To augment these standard measures, I also construct a fourth proxy for liquidity constraints that I refer to as "committed consumption." This measure is meant to capture the share of monthly wage income that is pre-committed to large, difficult-to-adjust expenditures such as rent or mortgage payments. In the presence of binding liquidity constraints, households with large committed consumption levels should be expected to exhibit greater sensitivity to cash-on-hand (Chetty and Szeidl 2007, 2016). To construct the measure of committed consumption, I aggregate monthly expenditures on mortgage payments, rent, car loans, and utilities for each household and then divide the total level by the monthly wage income for that household.[23] While this measure does not fully capture the true level of committed consumption, it is composed of expenditures we might reasonably think would be difficult to adjust. Relative to total income, which does not net out expenditures that are difficult to adjust, this measure may provide a more accurate measure of the liquidity available to households in the short run.

Table 9 presents estimates of the overall response in total expenditures following three paycheck months by whether a household is constrained or unconstrained under each of the four proxy measures. For each of the four proxies for liquidity, I classify households as either "constrained" or "unconstrained" using the median value of the measure. Specifically, I classify as constrained those households with below-median values of liquid assets, income, and age, and above-median share of monthly income that is committed. Since households that are more or less constrained may differ in the size of their paychecks, the table reports estimates using the dollar change in monthly income $\Delta Y_{it}$ as the explanatory variable of interest. This variable is simply the interaction of a household's biweekly paycheck amount with the indicator $\mathbf{1}_{\{t-1 \in S\}}$ from equation (6) for whether the prior month was a three paycheck month. The estimates from this specification can therefore be interpreted as the marginal propensity to spend out of the third paycheck.

The first column of Table 9 implies an overall marginal propensity to spend of 17 percent (or 34 percent after correcting for classification error) out of third paychecks. The remaining columns of the table report estimates from the same specification as column

[22] Using data from the Survey of Consumer Finances, Jappelli (1990) finds evidence that younger households are more likely to be liquidity constrained.

[23] Monthly wage income here is calculated as the wages for the head of household in a two paycheck month and is thus equal to two times the reported gross pay.

(1) but with the inclusion of the interaction of $\Delta Y_{it}$ with an indicator $\mathbf{1}_{\{Unconstrained\}}$ for whether a household is a relatively unconstrained household under the given proxy measure. The reported coefficient estimates on $\Delta Y_{it}$ in the first row for columns (2)-(5) present the response to third paychecks for constrained households while the reported coefficient estimates on $\Delta Y_{it} * \mathbf{1}_{\{Unconstrained\}}$ in the second row present the response for unconstrained households *relative* to that of constrained households. Across all four proxy measures, I find no significant differences in the response to third paychecks by a households likelihood of being constrained, and several estimates suggest differences between constrained and unconstrained households in the opposite direction of what we would expect.

Overall, these tests suggest that liquidity constraints are unlikely to be the sole factor driving the spending response I observe. An important caveat to these findings is that I am not able to directly measure liquidity and must instead use indirect proxy measures. To the extent that these proxies suffer from mis-classification error, estimates comparing constrained and unconstrained households may be biased against finding a differential response (Jappelli et al. 1998). In general, the overall evidence in the literature on the role of liquidity constraints is mixed. While these findings join several other papers in the literature that also find limited evidence for liquidity constraints to explain consumption responses to predictable income changes (Parker 1999; Souleles 1999; Shapiro and Slemrod 1995; Stephens, Jr. 2008; Stephens, Jr. and Unayama 2011; Baugh et al. 2021), recent evidence from Ganong et al. (2020) using more granular administrative data from consumer bank accounts does find that liquid assets drive welfare-relevant volatility in consumption in response to typical income shocks. While not statistically significant, the sign of the interaction terms on the age and committed consumption proxies in Table 9 do indicate smaller spending responses among those less likely to be constrained. This leaves open that possibility that some of the effect I document may be driven by liquidity constraints. This is especially true for the committed consumption proxy, which may provide a more accurate measure of short-run constraints. Nonetheless, given the general difficulty in measuring liquidity, it remains an open question whether the lack of consensus in the literature stems from measurement concerns or heterogeneity in the contexts where liquidity constraints play a role.

### 6.2 Alternative Behavioral Interpretations

The evidence presented in the previous subsection suggests a limited role for liquidity constraints to fully explain the finding that households increase their consumption expenditures following three paycheck months. One interesting aspect of the findings is that they appear primarily in spending on durables. This means that although third paychecks may significantly alter spending behavior, their effects on consumption may not be as large. Given this, the utility losses associated with failing to smooth expenditures across pay cycles may be small relative to the observed shifts in expenditure. Recent evidence suggests that the benefits of engaging in "near-rational" heuristic behaviors that relieve cognitive burdens may outweigh the costs from failing to perfectly smooth spending when the associated utility losses are small (Keung 2018). It may therefore be that part of the spending response I document is driven by behavioral factors rather than liquidity constraints. Motivated by this possibility, in this subsection, I consider several alternative explanations stemming from behavioral models of non-standard preferences and beliefs. While it is not possible to formally test these explanations without additional data, I briefly discuss their potential for rationalizing my findings.

*Budgeting Heuristics.* Determining the optimal consumption path requires that households make accurate predictions about their expected future income. One potential explanation for the empirical findings of this paper is that individuals naively extrapolate their current income when forming these expectations—effectively feeling wealthier following months with three paychecks. If individuals are overly optimistic about their wealth in this way, then we would expect to see increased spending in response to third paychecks. While there is no way for this paper to empirically test such a model of beliefs, one could imagine that individuals may be biased in this manner. The variation in monthly income generated by biweekly pay schedules introduces additional complexity to households financial decision-making.[24] As many empirical studies have shown, one way that individuals manage such complexities is by adopting rules-of-thumb or heuristics, and a commonly shared piece of financial advice is that households form monthly budgets based on their current cash flow.[25]

*Mental Accounting.* A second potential explanation for the increased spending following three paycheck months is that individuals are engaging in mental accounting behavior. Under a mental accounting framework, individuals no longer treat money as fungible and instead use a system of mental accounts to categorize and evaluate their income (Thaler 1985, 1999, 2008; see Zhang and Sussman 2018 for a review). One implication of engaging in such behavior is the potential for different marginal propensities to consume out of different sources of income, even if the income is anticipated (Shefrin and Thaler 1988; Thaler 1990).

[24]Empirical evidence has shown that individuals often have difficulty fully optimizing when facing such complexity. For example, Choi et al. (2011) find that individuals sub-optimally invest in their 401($k$) by contributing at a rate below the threshold matched by their employer, even when doing so is dominated by contributing at the match threshold. Abaluck and Gruber (2011) show that individuals choose Medicare Part D prescription drug plans that are strictly worse than other available plans.

[25]For example, studies have provided evidence of heuristic thinking in retirement savings decisions (Bernatzi and Thaler 2007); the market for used cars (Lacetera et al. 2012); and debt repayment (Gathergood et al. 2019). With respect to budgeting in particular, Argyle et al. (2020) provide evidence using data on auto-loan payments that suggests the use of monthly budgeting heuristics, and Zhang et al. (2022) provide survey evidence that the majority of individuals budget at a monthly frequency.

While third paychecks are not designated any differently from non-third paychecks, biweekly workers may nonetheless choose to treat their typical income of two paychecks per month as their "regular" income and to view third paychecks as a bonus distinct from regular income. Financial advice directed at biweekly-paid workers often reinforces this distinction. For example, Discover Financial Services, one of the largest credit card issuers in the United States, advises the following on a financial education page on budgeting:

> *"If you get paid every two weeks, you've probably noticed extra money coming your way certain months... You get two magical months like this a year: when you suddenly have a third paycheck and—the best part is—your monthly bills stay the same. Yes, it's appropriate to jump for joy—provided you have a plan for that extra income."*

If third paychecks are viewed as distinct from regular income, then it is plausible that individuals would earmark these paychecks specifically for other uses, such as the purchase of large durable goods. In the context of this paper, mental accounting behavior can thus explain the increased spending in response to third paychecks, even in the absence of any change in total lifetime income or binding liquidity constraints.

*Time Inconsistency with Sophistication.* The increase in spending on durable goods following three paycheck months is also consistent with a model in which individuals hold preferences that are present-biased and foresee having problems with self-control in the future. Because such individuals preferences are dynamically inconsistent, they may choose to invest in illiquid assets as a form of commitment against future overconsumption (Strotz 1956; Phelps and Pollak 1968; Laibson 1997; O'Donoghue and Rabin 1999). However, while this interpretation can explain increases in spending on durable goods following three paycheck months, it cannot explain why we observe increases in spending on debt-financed vehicles, the purchase of which commits a household to a stream of future installment payments that they may not be able to afford.

## 7 Conclusion

Many households receive income and make consumption decisions at different frequencies. This misalignment can result in predictable variation in the amount of income received per consumption decision period. Under standard assumptions, such variation should not matter for household spending patterns. I test this prediction by leveraging the variation in monthly income that arises from the timing of biweekly pay schedules. Biweekly workers typically receive two paychecks per month with the exception of two months out of the year, during which they receive three. In this paper, I document evidence that households adjust their spending in response to these third paychecks. Household spending is on average 18.4 percent higher following three paycheck months. Furthermore, I find that this spending response is due entirely to spending on durables, with no corresponding response in non-durables. To examine the role of liquidity, I compare spending responses for households who are more or less likely to be constrained. I document that liquidity constraints are unlikely to fully explain the spending response I observe.

These findings suggest that predictable variation in income can have large effects on household consumption patterns, even when that variation arises solely as a result of the way in which households choose to aggregate or "bracket" their income over time. I conclude by briefly discussing several alternative explanations for my results, which are motivated by behavioral models, including time inconsistency, heuristic thinking, and mental accounting. While the results in this paper are consistent with some of these alternative explanations, further research is needed to directly test and distinguish between them.

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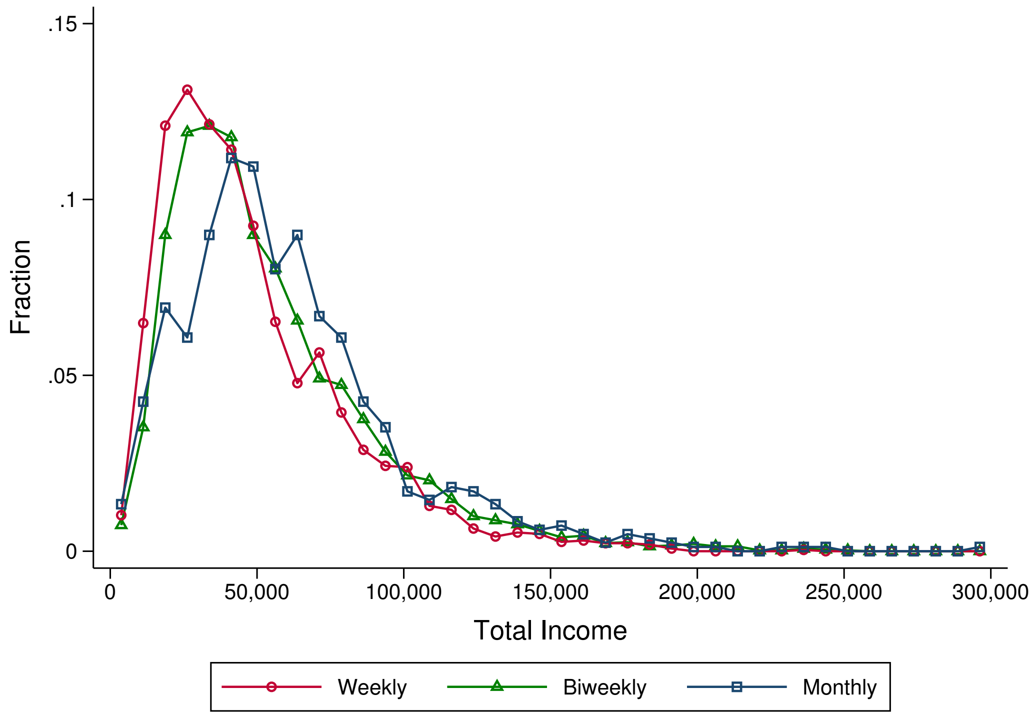
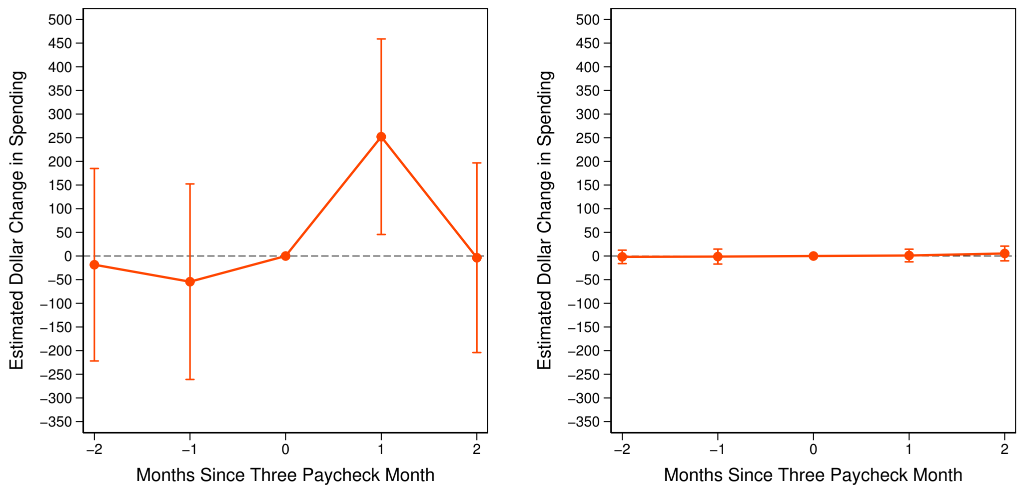
Fig. 1.—Total Income Distribution by Pay Frequency




Note.— This figure plots the distribution of total before-tax income (in real 2010 dollars) for households with heads who are paid either weekly, biweekly, or monthly for the full sample described in columns (1) and (2) of Table 1. A bin width of $7,500 is used for the figure.

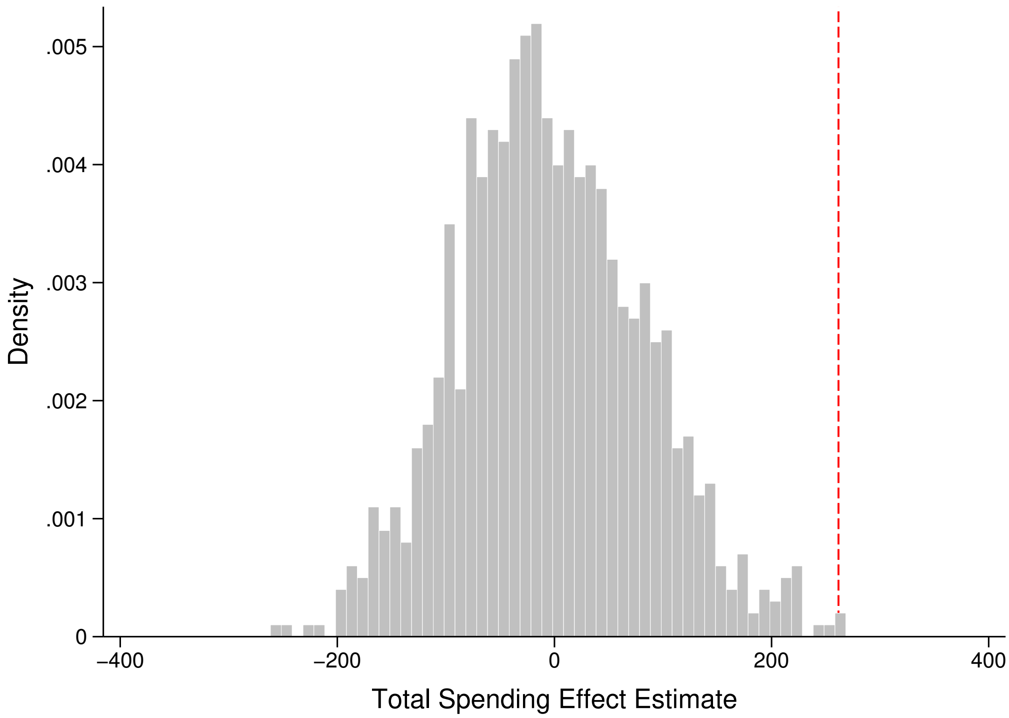
Fig. 2.—Timing of Durable and Strictly Non-Durable Spending




(a) Durable Spending (b) Strictly Non-Durable Spending

Note.— This figure plots estimates of the spending response for biweekly households derived from a flexible difference-in-differences regression that allows the response to vary by month relative to three paycheck months. Estimates are constructed by regressing the month-over-month dollar change in durable spending (Panel A) and strictly non-durable spending (Panel B) on a series of dummy variables indicating whether the month of expenditure falls in a given relative month as measured from months with three paychecks. Relative month zero (t=0) denotes a month with three paychecks and is the omitted category. Results are shown for the two months preceding and two months following three paycheck months. The 95% confidence intervals are based on standard errors that are clustered at the household level. The regression also includes month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls.

Fig. 3.—Distribution of Placebo Estimates of Spending Effect




Note.— This figure plots the distribution of 1,000 placebo estimates of the total spending effect following three paycheck months. The vertically dashed line shows the true estimate, which is taken from column (1) of Table 2. Each placebo estimate is generated using a false paycheck schedule under which a random subset of the months spanning the sample period from 1990 to 2010 are designated as three paycheck months. Since the set of three paychecks months under the true paycheck schedule never follow consecutively, each false paycheck schedule has the restriction that none of the randomly-selected months in a representative year can be consecutive. In addition to the main spending effect, the regressions used to generate the placebo estimates include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls.

TABLE 1
SUMMARY STATISTICS

| | Full Sample | | Biweekly Sample | |
|---|---|---|---|---|
| *Expenditure in Levels* ($): | | | | |
| Durables | 1,378.92 | (2,788.74) | 1,410.22 | (2,775.88) |
| Non-durables | 1,429.60 | (824.75) | 1,432.59 | (830.46) |
| Strictly Non-durables | 1,050.74 | (530.28) | 1,044.16 | (535.60) |
| Food | 513.99 | (320.06) | 510.69 | (331.38) |
| Total | 2,808.52 | (3,037.19) | 2,842.82 | (3,020.63) |
| *Changes in Expenditure* ($): | | | | |
| Durables | 16.93 | (3,802.95) | 12.86 | (3,773.67) |
| Non-durables | 26.16 | (456.38) | 26.26 | (458.07) |
| Strictly Non-durables | 8.06 | (251.42) | 7.18 | (257.17) |
| Food | 3.36 | (128.28) | 3.50 | (127.37) |
| Total | 43.09 | (3,846.42) | 39.12 | (3,815.04) |
| *Taste Shifters*: | | | | |
| Age | 39.80 | (10.42) | 39.89 | (10.38) |
| Δ Children | 0.00 | (0.08) | 0.00 | (0.08) |
| Δ Adults | 0.00 | (0.08) | 0.00 | (0.08) |
| *Income and Assets* ($): | | | | |
| Paycheck | 1,523.85 | (1,273.96) | 1,668.83 | (1,035.64) |
| Annual Income | 53,852.64 | (34,582.01) | 55,254.32 | (35,467.85) |
| Liquid Assets (N = 8,887) | 7,639.18 | (28,587.63) | 8,698.35 | (33,115.39) |
| Number of Observations | 24,822 | | 13,707 | |
| Number of Households | 7,776 | | 4,316 | |

NOTE.—This table presents descriptive statistics for both the full sample of weekly-, biweekly-, and monthly-paid households (columns (1) and (2)) and the main analysis sample of biweekly-paid households (columns (3) and (4)). All table entries represent sample means or, in parentheses, standard deviations. Observations are at the household-month level. Total expenditures are composed of durable and non-durable expenditures. Strictly non-durable expenditures are a subset of non-durable expenditures, and food expenditures are a subset of strictly non-durable expenditures. Age refers to the head of household only. Changes in the number of children include only children younger than 18. Paycheck amounts refer to the gross amount of the head of household's last pay. Annual income refers total before-tax income received by the households in the past year. Liquid assets are composed of savings and checking account balances and are available for 8,887 household-month observations out of the full sample of 24,822 observations. All dollar amounts are converted to real 2010 dollars.

TABLE 2
Spending Response to Extra Paychecks

| *Dependent variable:* $\Delta C_t$ | (1) Total | (2) Durable | (3) Non-durable | (4) Strictly Non-durable | (5) Food |
|---|---|---|---|---|---|
| $\mathbf{1}_{\{t-1 \in S\}}$ | 261.64*** | 256.96*** | 4.67 | –2.43 | –0.96 |
| | (95.24) | (93.98) | (9.76) | (5.51) | (2.61) |
| Demographic Controls | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y |
| N | 13,707 | 13,707 | 13,707 | 13,707 | 13,707 |

Note.—This table presents difference-in-differences estimates of the spending response for biweekly households following months with three paychecks. Each column reports estimates from a separate regression estimated at the household-month level where the dependent variable is the month-over-month dollar change in spending. Coefficients are reported for the indicator $\mathbb{1}_{\{t-1 \in S\}}$, which equals one if the previous month was a three paycheck month (i.e., if three paychecks of income are available in the present month $t$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in total spending, while columns (2) to (5) report coefficient estimates of the spending response for the following subcategories of total expenditure: durable, non-durable, strictly non-durable, and food. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE 3

Durable Spending Response to Extra Paychecks

| *Dependent variable:* $\Delta C_t$ | (1) Durable | (2) Vehicle Purchases | (3) Furniture | (4) Flooring | (5) Major Appliances |
|---|---|---|---|---|---|
| $\mathbf{1}_{\{t-1 \in S\}}$ | 256.96*** | 257.10*** | −5.15 | 3.54*** | −3.66 |
| | (93.98) | (91.98) | (10.11) | (1.27) | (4.62) |
| Demographic Controls | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y |
| N | 13,707 | 13,707 | 13,707 | 13,707 | 13,707 |

Note.—This table presents difference-in-differences estimates of the durable spending response for biweekly households following months with three paychecks. Each column reports estimates from a separate regression estimated at the household-month level where the dependent variable is the month-over-month dollar change in spending. Coefficients are reported for the indicator $\mathbb{1}_{\{t-1 \in S\}}$, which equals one if the previous month was a three paycheck month (i.e., if three paychecks of income are available in the present month $t$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in durable spending and is the same estimate reported in column (2) of Table 2. Columns (2) to (5) report coefficient estimates of the spending response for the following subcategories of durable expenditure: vehicles purchases, furniture, flooring, and major appliances. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

## TABLE 4
### Vehicle Spending Response to Extra Paychecks

| | *Dependent variable:* $\Delta C_t$ | | | | | *Probability of Purchase (%)* | |
|---|---|---|---|---|---|---|---|
| | (1) Vehicle Purchases | (2) New Cars | (3) Used Cars | (4) New Motor Vehicles | (5) Used Motor Vehicles | (6) New Cars | (7) Used Cars |
| $\mathbf{1}_{\{t-1 \in S\}}$ | 257.10*** | 201.40** | 45.41 | 8.09 | 2.21 | 0.38** | 0.20 |
| | (91.98) | (78.24) | (48.00) | (7.02) | (4.83) | (0.18) | (0.29) |
| Demographic Controls | Y | Y | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y | Y | Y |
| N | 13,707 | 13,707 | 13,707 | 13,707 | 13,707 | 13,707 | 13,707 |

Note.—This table presents difference-in-differences estimates of the vehicle spending response for biweekly households following months with three paychecks. Each column reports estimates from a separate regression estimated at the household-month level where the dependent variable is either the month-over-month dollar change in spending (columns (1) to (5)) or an indicator for whether a car was purchased (columns (6) and (7)). To aid interpretation, the indicator for whether a car was purchased is multiplied by 100 so that all point estimates can be interpreted as percentage point effects. Coefficients are reported for the indicator $\mathbb{1}_{\{t-1 \in S\}}$, which equals one if the previous month was a three paycheck month (i.e., if three paychecks of income are available in the present month $t$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in vehicle spending and is the same estimate reported in column (2) of Table 3. Columns (2) to (5) report coefficient estimates of the spending response for the following sub-categories of vehicle expenditure: new cars, used cars, new motor vehicles, and used motor vehicles. The final two columns of the table report coefficient estimates from a linear probability model of the likelihood that a new car (column (6)) or used car (column (7)) is purchased following months with three paychecks. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE 5
SUMMARY STATISTICS FOR CAR PURCHASES

| | Full Biweekly Sample | | Conditional on Ever Purchasing | |
|---|---|---|---|---|
| *Expenditure in Levels* ($): | | | | |
| New and Used Car Purchases | 299.13 | (2,529.08) | 2,723.41 | (7,187.10) |
| Out-of-pocket Expenditures | 83.36 | (1,112.94) | 757.53 | (3,281.87) |
| Financed Amount | 215.77 | (2,115.26) | 1,965.88 | (6,108.29) |
| Down Payment | 24.01 | (400.26) | 217.16 | (1,190.32) |
| *Changes in Expenditure* ($): | | | | |
| New and Used Car Purchases | −5.64 | (3,636.22) | −49.12 | (10,736.80) |
| Out-of-Pocket Expenditures | 3.43 | (1,635.86) | 29.86 | (4,830.22) |
| Financed Amount | −9.06 | (3,032.83) | −78.97 | (8,954.93) |
| Down Payment | −0.27 | (581.10) | −2.37 | (1,715.85) |
| Number of Observations | 13,707 | | 1,573 | |
| Number of Households | 4,316 | | 407 | |

NOTE.—This table present descriptive statistics on car expenditures (both new and used) for the main analysis sample of biweekly-paid households (columns (1) and (2)) and the subset of biweekly households who report having purchased a new or used car during the survey period (columns (3) and (4)). All table entries represent sample means or, in parentheses, standard deviations. Observations are at the household-month level. Car expenditures are reported both in levels and in month-over-month changes. New and used car purchases refers to the total dollar value of the car. Down payments includes car purchases made with zero down payment. All dollar amounts are converted to real 2010 dollars.

TABLE 6

Car Spending Response to Extra Paychecks by Financing Categories

| *Dependent variable:* $\Delta C_t$ | (1) Car Purchases | (2) Out-of-Pocket Expenditure | (3) Financing | (4) Down Payment |
|---|---|---|---|---|
| | *Panel A. Unconditional Estimates* | | | |
| $\mathbf{1}_{\{t-1\in S\}}$ | 246.81*** | 82.73* | 164.08** | 22.59 |
| | (91.57) | (47.76) | (72.76) | (15.28) |
| Demographic Controls | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y |
| N | 13,707 | 13,707 | 13,707 | 13,707 |
| | *Panel B. Conditional Estimates* | | | |
| $\mathbf{1}_{\{t-1\in S\}}$ | 2204.62*** | 733.27* | 1471.36** | 197.37 |
| | (808.61) | (414.67) | (653.03) | (128.90) |
| Demographic Controls | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y |
| N | 1,573 | 1,573 | 1,573 | 1,573 |

Note.—This table presents difference-in-differences estimates of the car spending response for biweekly households following months with three paychecks. Each column reports estimates from a separate regression run at the household-month level where the dependent variable is the month-over-month dollar change in spending. Coefficients are reported for the indicator $\mathbb{1}_{\{t-1\in S\}}$, which equals one if the previous month was a three paycheck month (i.e., if three paychecks of income are available in the present month $t$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in car spending (total dollar value of the car), while columns (2) to (4) report coefficient estimates of the spending response for the following subcategories of car expenditure: out-of-pocket expenditure, financing amounts, and down payments. Panel A. reports estimates using the full sample biweekly-paid households while Panel B. reports conditional estimates using only the subset of biweekly households who report having purchased a car. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE 7

Spending Response to Extra Paychecks by Pay Frequency

| *Dependent variable:* $\Delta C_t$ | (1) Total | (2) Durable | (3) Non-durable | (4) Strictly Non-durable | (5) Food |
|---|---|---|---|---|---|
| $\mathbf{1}_{\{t-1 \in S\}} * \mathbf{1}_{\{Biweekly\}}$ | 276.94*** | 268.86*** | 8.06 | –0.78 | –0.37 |
| | (94.15) | (92.98) | (9.68) | (5.49) | (2.62) |
| $\mathbf{1}_{\{t-1 \in S\}} * \mathbf{1}_{\{Monthly\}}$ | –326.52 | –324.70 | –1.73 | –8.20 | –6.31 |
| | (222.84) | (220.58) | (24.30) | (14.14) | (7.80) |
| Demographic Controls | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y |
| N | 16,266 | 16,266 | 16,266 | 16,266 | 16,266 |

Note.—This table presents estimates of the spending response for biweekly and monthly households following months with three paychecks. Each column reports estimates from a separate regression run at the household-month level where the dependent variable is the month-over-month dollar change in spending. Coefficients are reported for the interaction of an indicator $\mathbb{1}_{\{t-1 \in S\}}$, which equals one if the previous month was a three paycheck month, with an indicator for whether a household is paid either biweekly ($\mathbb{1}_{\{\text{Biweekly}\}}$) or monthly ($\mathbb{1}_{\{\text{Monthly}\}}$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in total spending, while columns (2) to (5) report coefficient estimates of the spending response for the following subcategories of total expenditure: durable, non-durable, strictly non-durable, and food. Reported estimates represent the total, not relative, spending response to extra paychecks for households at the given pay frequency. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE 8
Time-Unadjusted Spending Response to Extra Paychecks

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| *Dependent variable:* $\Delta C_t$ | Total | Durable | Non-durable | Strictly Non-durable | Food |
| $\mathbf{1}_{\{t-1\in S\}}$ | 250.79** | 252.89** | −2.10 | 0.00 | 2.74* |
| | (112.73) | (112.02) | (7.10) | (2.34) | (1.52) |
| Demographic Controls | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y |
| N | 8,466 | 8,466 | 8,466 | 8,466 | 8,466 |

Note.—This table presents difference-in-differences estimates of the spending response for biweekly households following months with three paychecks, where measures of spending exclude any expenditures that undergo time adjustment routines by the Bureau of Labor Statistics as detailed in Section 5.3.3. Each column reports estimates from a separate regression run at the household-month level where the dependent variable is the month-over-month dollar change in spending. Coefficients are reported for the indicator $\mathbb{1}_{\{t-1\in S\}}$, which equals one if the previous month was a three paycheck month (i.e., if three paychecks of income are available in the present month $t$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in total spending, while columns (2) to (5) report coefficient estimates of the spending response for the following subcategories of total expenditure: durable, non-durable, strictly non-durable, and food. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE 9
SPENDING RESPONSE TO EXTRA PAYCHECKS BY PROXIES FOR LIQUIDITY

| | | Proxy Measures | | | |
|---|---|---|---|---|---|
| *Dependent variable:* $\Delta C_t$ | Full Biweekly Sample | Liquid Assets | Before Tax Income | Age | Committed Consumption |
| $\Delta Y_t$ | 0.17*** | 0.01 | 0.07 | 0.22** | 0.23** |
| | (0.06) | (0.08) | (0.07) | (0.09) | (0.10) |
| $\Delta Y_t * \mathbf{1}_{\{Unconstrained\}}$ | | 0.08 | 0.13 | −0.09 | −0.08 |
| | | (0.12) | (0.10) | (0.12) | (0.12) |
| Demographic Controls | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y |
| N | 13,707 | 4,863 | 13,707 | 13,707 | 13,707 |

NOTE.—This table presents difference-in-differences estimates of the total spending response for biweekly households following months with three paychecks, by whether a household is relatively liquidity constrained or unconstrained. Each column reports estimates from a separate regression run at the household-month level, where the dependent variable is the month-over-month dollar change in spending. Coefficient estimates are reported for the change in income, $\Delta Y_t$, which equals the dollar amount of the head of household's last gross pay if the previous month was a three paycheck month and zero otherwise, and the interaction of that change with an indicator for whether a household is relatively unconstrained ($\mathbb{1}_{\{Unconstrained\}}$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in total spending for the full sample of biweekly households and is the same estimate reported in column (1) of Table C.1. Columns (2) to (5) report coefficient estimates of the total spending response by various proxy measures for liquidity, with estimates in the first row representing the response for households who are constrained (the omitted category) and estimates in the second row representing the relative difference in response for households that are unconstrained. Constrained households are defined as those with liquid asset holdings below the median (column (2)); total before-tax income below the median (column (3)); age for the household head below the median (column (4)); and committed consumption as a fraction of wages above the median (column (5)). Committed consumption is the sum of household expenditures on mortgage payments, rental payments, vehicle loan payments, and utilities payments for a given month. Monthly wages are based on typical wage income and is constructed using the amount of the head of household's last gross pay. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

# Appendix A Sample Restrictions

The empirical analysis in this paper uses data from the Consumer Expenditure Survey (CEX) interview sample between the years 1990 and 2010. This appendix describes the restrictions I impose to arrive at the main analysis sample. These restrictions follow the existing literature closely and reduce the potential influence on my estimates of measurement error or changes in circumstances unrelated to the presence of third paychecks. Appendix Table A.1 lists each sample restriction as well as the number of observations and households remaining after a given restriction is made. I describe these restrictions in more detail below.

Starting with an initial sample of 541,286 household-quarter observations for 184,893 consumer units, I first exclude any consumer units that are composed of multiple households. With this exclusion, consumer units and households can be thought of interchangeably. I next drop households where the head of household is either 1) employed in farming, forestry, or fishing, 2) self-employed or working without pay, or 3) living in student housing.[1] The next set of restrictions address potential concerns with income and expenditure reports. I drop any household that is flagged as having an "incomplete" or top-coded income report or an "invalid" checking or savings report. I further drop households if any component of the aggregate expenditure groups does not have an associated month and year of expenditure. I also exclude households who report ever receiving meals as pay and drop household-quarters with zero food expenditure reported for any month of the quarter.

I further exclude households with heads whose reported gross pay amount or period is either missing or flagged as "inconsistent." I also drop households whose gross pay is top-coded or whose income or gross pay is below the 1st percentile of the respective distribution. I next restrict households to those with heads who report working full time (atleast 50 weeks) over the past year and whose reported pay is unchanged between their second and fifth interview. Since I cannot directly observe job tenure in the data, this restriction selects for households whose heads are likely to have been employed at the same job during the past year and are therefore more likely to be aware of the presence and timing of the third paychecks. I drop households if the age of any household member changes by more than one year from quarter to quarter or if the household is missing information on family size, age of the head of household, or number of children (defined as members of the households younger than 18). I next restrict households to those with heads of households between 24 and 64 years of age who were paid weekly, biweekly, or monthly. Since I cannot observe which of two possible schedules a biweekly worker follows, the possibility for members of the same household to be on alternate biweekly schedules introduces potential noise. I

[1] The CEX defines as the "reference person" the first member mentioned by the respondent when asked the name of the person or one of the persons who owns or rents the home. The relationships of all other members within a consumer unit are defined in relation to the reference person.

thus exclude households where other employed members of the household are paid at the same frequency as the head of household from the sample. Finally, I exclude households who report unusually large changes in non-durable expenditure (log change greater than 2) between any two consecutive months. After sample restrictions, I am left with a full sample of 24,822 household-month observations for 7,776 households whose heads report being paid either weekly, biweekly, or monthly. The main sample used in the analysis consists of the 4,316 biweekly households from the full sample.

TABLE A.1
Sample Restriction Details

| Restriction | Observation Level | Observation Count | Hhld Count | Literature |
|---|---|---|---|---|
| Initial Sample | Hhld-Quarter | 541,286 | 184,893 | - |
| Multiple Households in Same Consumer Unit | Hhld-Quarter | 517,924 | 173,472 | Souleles 1999; Souleles 2002 |
| Employed in Farming, Forestry, or Fishing | Hhld-Quarter | 510,813 | 171,179 | Lusardi 1996; Souleles 1999; Souleles 2002 |
| Self-Employed or Not Working | Hhld-Quarter | 304,037 | 105,668 | Lusardi 1996 |
| Live in Student Housing | Hhld-Quarter | 302,632 | 104,734 | Souleles 1999; Souleles 2002; Hsieh 2003 |
| Missing Income Information | Hhld-Quarter | 232,308 | 82,169 | Lusardi 1996; Parker 1999; Stephens, Jr. 2008 |
| Top-coded Income Flag | Hhld-Quarter | 219,277 | 78,109 | Parker 1999; Stephens, Jr. 2008 |
| Invalid Checking or Savings Report | Hhld-Quarter | 176,248 | 64,742 | Lusardi 1996 |
| No Associated Date for Expenditure | Hhld-Quarter | 176,248 | 64,742 | Souleles 1999; Souleles 2002 |
| Zero Reported Food Expenditure | Hhld-Quarter | 175,961 | 64,675 | Souleles 1999; Souleles 2002; Hsieh 2003 |
| Received Meals as Pay | Hhld-Quarter | 164,503 | 60,840 | Souleles 1999; Souleles 2002 |
| Inconsistent/Missing Gross Pay Or Pay Period | Hhld-Quarter | 135,369 | 51,979 | - |
| Gross Pay or Income Less than 1st Pct. | Hhld-Quarter | 131,487 | 50,426 | - |
| Top-coded Gross Pay Flag | Hhld-Quarter | 128,281 | 49,295 | - |
| Gross Pay Changed Across Interviews | Hhld-Quarter | 33,599 | 22,330 | - |
| Pay Frequency Changed Across Interviews | Hhld-Quarter | 33,305 | 22,233 | - |
| Work $<$ 50 Weeks | Hhld-Quarter | 25,196 | 16,598 | - |
| Age Change $>$ 1 Year Across Quarters | Hhld-Quarter | 25,060 | 16,545 | Souleles 2002 |
| Missing Information on Family Size | Hhld-Quarter | 25,060 | 16,545 | Parker 1999; Hsieh 2003 |
| Not Paid Weekly, Biweekly, or Monthly | Hhld-Quarter | 23,158 | 15,304 | - |
| Other Household Members Paid Same Frequency | Hhld-Quarter | 16,199 | 10,931 | - |
| Age of Head Less Than 24 or Greater Than 64 | Hhld-Quarter | 14,639 | 9,784 | Souleles 1999; Souleles 2002 |
| Convert Observation Level† | Hhld-Month | 43,917 | 9,784 | - |
| Log Change in Non-Durables $>$ 2 | Hhld-Month | 32,658 | 7,776 | Lusardi 1996; Parker 1999 |
| Full Sample (Non-missing $\Delta C_t$) | Hhld-Month | 24,822 | 7,776 | - |

Note.—This table provides details on the sample restrictions taken in this paper. Table entries represent the number of observations and households remaining after dropping observations with the indicated characteristic. The last column lists examples of prior literature which impose similar sample restrictions. †Data in the Consumer Expenditure Survey is recorded at the household-quarter level. At each interview, households report details on the dollar amount, category, and month and year of purchase for their expenditures over the previous three months. Data is reshaped from the household-quarter level to the household-month level using the information on the timing of expenditure.

# Appendix B Classification Correction

The CEX asks individuals to report the period of time covered by their last gross pay. This allows me to identify the pay frequency of the heads of household and, in particular, whether the head is paid biweekly. In order to study spending responses following three paycheck months, I create a variable that indicates whether the previous month was a three paycheck month for a given biweekly worker's schedule. However, one limitation of the CEX is that I do not observe the actual date on which the last pay occurred. Because of this, I am unable to observe which of the two possible alternate schedules a given biweekly worker is paid by. Table B.1 lists the three paycheck months for the two alternate schedules from 1989 to 2010.[2] Each calendar month serves as a three paycheck month on one of the schedules at least once during the sample period.

Because I am unable to observe by which schedule a given biweekly worker is paid, I allow the worker to be on either schedule in the estimation approach outlined in Section 4. Specifically, the indicator, $\mathbf{1}_{\{t-1 \in S\}}$, from equation (6) is set equal to one if the previous month was a three paycheck month on *either* schedule. This indicator can be thought of as a noisy measure of which months are three paycheck months based on the biweekly worker's true schedule. For ease of exposition, let $x = \mathbf{1}_{\{t-1 \in S\}}$ denote this indicator, and let $X = \mathbf{1}_{\{t-1 \in S_j\}}$ be an indicator for whether the previous month is a three paycheck month based on the worker's true schedule $j \in \{1, 2\}$. Then we can write the following relation

$$x = X + u \tag{7}$$

where $u$ is an error term taking the value $u = 0$ when $X$ is measured without error and $u = 1$ when $X$ is mis-measured. Again for ease of exposition, let the change in consumption $\Delta C$ be denoted by $y$. Equation (6) can then be re-expressed as the following

$$\begin{aligned} y &= X\beta + Z\gamma + \epsilon \\ &= x\beta + Z\gamma + (\epsilon - u\beta) \end{aligned} \tag{8}$$

where $\gamma$ is a $[(k-1) \times 1]$ vector of parameters and $Z$ is an $[n \times (k-1)]$ matrix of the taste shifters and time dummies. The measurement error introduces a bias in the estimation of my parameter of interest, $\beta$. Given the binary nature of the indicator variable, the measurement error can be thought of as classification error in the biweekly worker schedules. Moreover, this classification error is non-classical in nature because the true value of the indicator variable is necessarily negatively correlated with the error. To see this, first note that

[2] The table includes three paycheck months in 1989 to account for the fact that households who were interviewed in the first quarter of 1990 may report expenditures in 1989.

whenever $x = 0$, the measurement error $u = 0$ since these are months that do not follow three paycheck months on either schedule. On the other hand, whenever $x = 1$, then either $u = 0$ if the month follows a three paycheck month on the worker's true schedule (i.e. $X = 1$) or $u = 1$ if the month follows a three paycheck month on the other schedule (i.e. $X = 0$). It therefore follows that $\text{Cov}(X, u) < 0$.

### B.1 Direction of bias

Classification error of this sort biases downwards the estimates of $\beta$ in naive OLS regressions. To show this, let $\tilde{X} = [x \quad Z]$ so that

$$y = \tilde{X}b + e. \tag{9}$$

I make the following three assumptions

$$\begin{aligned}&\text{(A1) } E(X'e) = 0 \text{ and } E(Z'e) = 0\\&\text{(A2) } E(Z'u) = 0\\&\text{(A3) } \text{Cov}(X, u) < 0.\end{aligned}$$

The first assumption (A1) is a standard assumption and states that the regressors of the true population regression are orthogonal to the error terms. The second assumption (A2) states that the classification error from mismeasurement of $X$ is orthogonal to the other regressors $Z$. The final assumption (A3) is that the classification error is negatively correlated with the true indicator, $X$. Following Aigner (1973) and Black et al. (2000), applying least squares to equation (9) gives the following estimators

$$\hat{b}^{OLS} = \begin{bmatrix} \hat{\beta} \\ \hat{\gamma} \end{bmatrix}^{OLS} = (\tilde{X}'\tilde{X})^{-1}\tilde{X}'y$$

where

$$\tilde{X}'\tilde{X} = \begin{bmatrix} x'x & x'Z \\ Z'x & Z'Z \end{bmatrix} \text{ and } \tilde{X}'y = \begin{bmatrix} x'y \\ Z'y \end{bmatrix}.$$

The sampling error $e$ is then given by

$$e = \begin{bmatrix} \hat{\beta} \\ \hat{\gamma} \end{bmatrix}^{OLS} - \begin{bmatrix} \beta \\ \gamma \end{bmatrix} = (\tilde{X}'\tilde{X})^{-1}\tilde{X}'\epsilon - (\tilde{X}'\tilde{X})^{-1}\tilde{X}'\mu\beta.$$

Given my first assumptions, A1, I can write

$$\text{plim}\hat{b}^{OLS} = \begin{bmatrix} \beta \\ \gamma \end{bmatrix} - \beta\Sigma_{\tilde{X}}^{-1}\text{Cov}(\tilde{X}, u) \tag{10}$$

where $\Sigma_{\tilde{X}} = \text{plim}(\frac{1}{n}\tilde{X}'\tilde{X})$.

To determine the direction of the bias, it is necessary to estimate both $\Sigma_{\tilde{X}}^{-1}$ and $\text{Cov}(\tilde{X}, u)$. From assumption A2, it holds that $\text{Cov}(Z, u) = 0$. Thus, for my key parameter of interest, $\beta$, equation (10) simplifies to

$$\text{plim}\hat{\beta}^{OLS} = \beta - \beta s_{11}\text{Cov}(x, u) \tag{11}$$

where $s_{11}$ is the first element in $\Sigma_{\tilde{X}}^{-1}$. The covariance in this expression depends on the joint distribution of $(x, u)$. To determine this, we need to know the probabilities of misclassification and the probability that the previous month is a three paycheck month. Let $\tilde{P} \equiv \text{Prob}(x = 1)$ and $\tilde{Q} = 1 - \tilde{P}$ denote the probabilities of the previous month being and not being a three paycheck month, respectively. Further, recall that months that are not three paycheck months on either schedule ($t-1 \notin S$) are correctly measured so that we can define $\eta \equiv \text{Prob}(u = 0|x = 0) = 1$. For months that are three paycheck months on either schedule ($t - 1 \in S$), the probability of misclassification depends on both the proportion of individuals on each schedule and the probability that a given observation follows a three paycheck month under Schedule 1 versus Schedule 2. Let $\lambda \in [0, 1]$ be the probability an individual is a Schedule 1 individual and $p \in [0, 1]$ be the probability a given observation follows a three paycheck month under Schedule 1 ($t-1 \in S_1$) rather than Schedule 2. Then the probability of misclassification for months that are three paycheck months under either schedule can be defined as $\nu \equiv \text{Prob}(u = 1|x = 1) = \lambda \cdot (1 - p) + (1 - \lambda) \cdot p$. The marginal distribution of $x$ is thus Bernoulli with parameter $\tilde{P}$. Similarly, the marginal distribution of $u$ is Bernoulli with parameter $\nu\tilde{P}$. Thus the covariance between $x$ and $u$ is given by

$$\begin{aligned}\text{Cov}(x, u) &= \nu\tilde{P} - \tilde{P}(\nu\tilde{P}) \\ &= \nu\tilde{P}\tilde{Q}\end{aligned}$$

Note that because $0 \leq \nu, \tilde{P}, \tilde{Q} \leq 1$, it must be that $0 \leq Cov(x, u) \leq 1$.

All that remains is to show that the first element, $s_{11}$, of $\Sigma_{\tilde{X}}^{-1}$ is positive. Because $\tilde{X}$ is full column rank, it follows that $\Sigma_{\tilde{X}}$ is positive-definite as is its inverse $\Sigma_{\tilde{X}}^{-1}$. The upper left determinants of positive definite matrices are positive, and so $s_{11} > 0$. Therefore, $\text{plim}\beta^{OLS} = \beta - \beta s_{11}\text{Cov}(x, u) \leq \beta$ and the estimate is inconsistent and downward biased.

## B.2 Relationship between bias and proportion of individuals on either schedule

Because I am unable to observe the proportion of individuals on either schedule, it is important to understand how the bias due to this measurement error varies with that proportion. Let $\beta^R = \dfrac{\beta}{\beta^{OLS}} = (1 - s_{11}\text{Cov}(x, u))^{-1}$ be the ratio of the true value to the estimated value of the parameter of interest. I establish two facts regarding this bias ratio and its relationship with the proportion of individuals, $\lambda$, on Schedule 1.

**Proposition B.1.**

*i. If $p = (1 - p)$, then the bias ratio is independent of $\lambda$.*

*ii. If $p \neq (1 - p)$, then the bias ratio is increasing in $\lambda$ for $p < \frac{1}{2}$ and decreasing in $\lambda$ for $p > \frac{1}{2}$.*

*Proof.* To see why these facts hold, recall that $\lambda$ only enters into the bias ratio through $\text{Cov}(x, u) = \nu\tilde{P}\tilde{Q} = [\lambda(1 - p) + (1 - \lambda)p]\tilde{P}\tilde{Q}$. When $p = (1 - p)$, this expression simplifies to $\text{Cov}(x, u) = \frac{1}{2}\tilde{P}\tilde{Q}$ which does not depend on $\lambda$. Hence, the bias ratio does not depend on $\lambda$. When $p \neq (1 - p)$, then

$$\begin{aligned} \frac{\partial\beta^R}{\partial\lambda} &= (1 - s_{11}\nu\tilde{P}\tilde{Q})^{-2}[s_{11}\tilde{P}\tilde{Q}(1 - 2p)] \\ &= (\beta^R)^2[s_{11}\tilde{P}\tilde{Q}(1 - 2p)]. \end{aligned} \tag{12}$$

Equation (12) is positive if $p < \frac{1}{2}$ and negative if $p > \frac{1}{2}$. □

The bias ratio is thus increasing in $\lambda$ for $p < \frac{1}{2}$ and decreasing in $\lambda$ for $p > \frac{1}{2}$. Note, however, that because the set of three paycheck months under Schedule 1 is the same size as the set of three paycheck months under Schedule 2 (i.e $|S_1| = |S_2|$), the value for $p$ converges in probability to $\frac{1}{2}$. Thus for an arbitrarily large sample size, the bias ratio is independent of the proportion of individuals on either schedule.

## B.3 Simulation

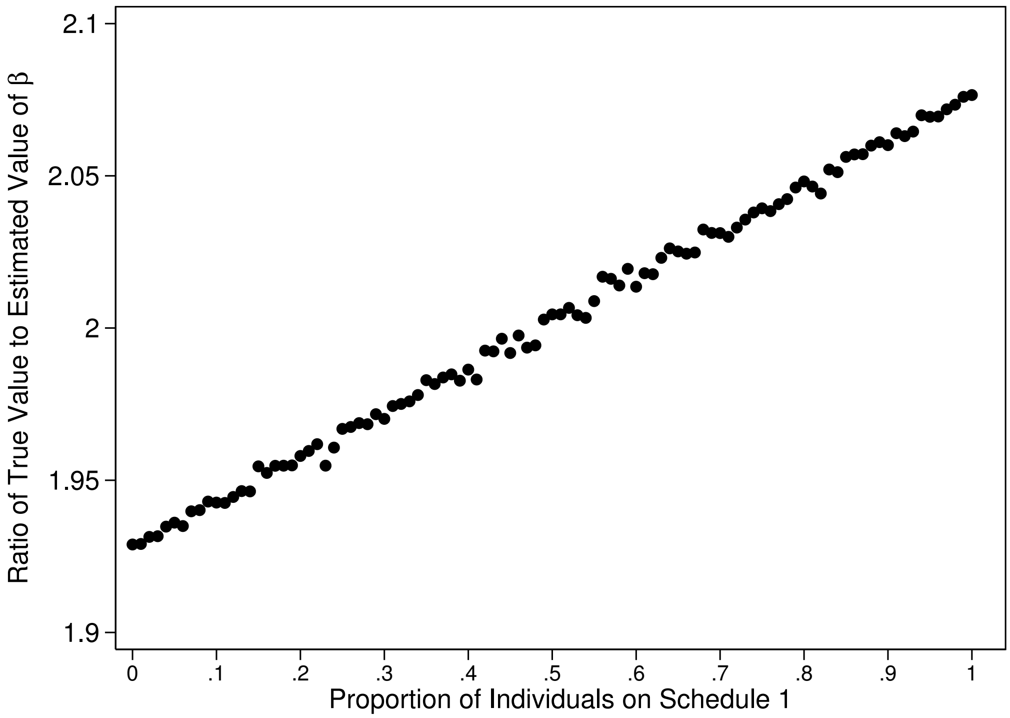
I run a series of simulations to gauge the magnitude of the bias and the extent to which it depends on the proportion of the sample that is on Schedule 1 as opposed to Schedule 2. To do this, I randomly assign a fraction, $\lambda \in [0, 1]$, of the sample observations to be Schedule 1 individuals and $1 - \lambda$ to be Schedule 2 individuals. I next create an indicator $\mathbf{1}_{\{t-1 \in S_j\}}$ for whether the previous month is a three paycheck month according to the worker's assigned schedule $j \in \{1, 2\}$. I further assume that I know the true data generating process for changes in consumption (i.e. the coefficients in equation (8) are known) which allows me to generate a "true" consumption path for each household. I then regress these true consumption changes on observed taste shifters, time dummies, and the mis-measured indicator $\mathbf{1}_{\{t-1 \in S\}}$ for whether a given month follows a worker's three paycheck month and compare the estimated coefficients with the values used to generate the consumption variable. To gauge the extent to which the bias introduced by classification error depends on $\lambda$, I run this simulation for values of $\lambda$ ranging from zero to one in increments of 0.01. Figure B.1 shows the ratio of the true value of $\beta$ to the estimated value $\hat{\beta}$ using the mismeasured indicator for different values of $\lambda$. The true value of $\beta$ is on average twice the size of the true value.

TABLE B.1
THE TIMING OF THREE PAYCHECK MONTHS FROM 1989-2010

| | Schedule 1 | | | Schedule 2 | | |
|---|---|---|---|---|---|---|
| Year | Month 1 | Month 2 | Month 3 | Month 1 | Month 2 | Month 3 |
| 1989 | Mar | Sept | . | Jun | Dec | . |
| 1990 | Mar | Aug | . | Jun | Nov | . |
| 1991 | Mar | Aug | . | May | Nov | . |
| 1992 | Jan | Jul | . | May | Oct | . |
| 1993 | Jan | Jul | Dec | Apr | Oct | . |
| 1994 | Jul | Dec | . | Apr | Sept | . |
| 1995 | Jun | Dec | . | Mar | Sept | . |
| 1996 | May | Nov | . | Mar | Aug | . |
| 1997 | May | Oct | . | Jan | Aug | . |
| 1998 | May | Oct | . | Jan | Jul | . |
| 1999 | Apr | Oct | . | Jan | Jul | Dec |
| 2000 | Mar | Sept | . | Jun | Dec | . |
| 2001 | Mar | Aug | . | Jun | Nov | . |
| 2002 | Mar | Aug | . | May | Nov | . |
| 2003 | Jan | Aug | . | May | Oct | . |
| 2004 | Jan | Jul | Dec | Apr | Oct | . |
| 2005 | Jul | Dec | . | Apr | Sept | . |
| 2006 | Jun | Dec | . | Mar | Sept | . |
| 2007 | Jun | Nov | . | Mar | Aug | . |
| 2008 | May | Oct | . | Feb | Aug | . |
| 2009 | May | Oct | . | Jan | Jul | . |
| 2010 | Apr | Oct | . | Jan | Jul | Dec |

NOTE.—This table lists all three paycheck months in a given year depending on which of the two possible pay schedules a biweekly worker may be paid by. In a handful of years, the calendar is such that biweekly workers receive two paychecks each month with the exception of *three* not two months, during which they receive three. The calendar includes three paychecks months for 1989 since some households interviewed in the first quarter of 1990 report expenditures from 1989. All calendar months serve as three paycheck months at least once over the sample period.

Fig. B.1.—Simulated Bias




Note.— This figure plots estimates of the bias ratio, $\beta^R$, by the proportion, $\lambda$, of individuals on Schedule 1 in increasing increments of 0.01. Using simulations, I estimate the bias ratio for each $\lambda$ by dividing the true value of $\beta$ by the estimated value of $\hat{\beta}$ using the mismeasured indicator from the main specification in equation (6). Additional details regarding the simulation exercise are in Appendix B.3.

# Appendix C Additional Robustness Checks

In this appendix, I present estimates from two variants to the main specification given by equation (6). The first alternative specification converts the estimates into a marginal propensity to spend. Specifically, I estimate the following specification

$$\Delta C_{it} = \beta * \Delta Y_{it} + \theta_{it}'\alpha + \gamma_t + \epsilon_{it} \tag{13}$$

where the dependent variable is the month-over-month dollar change in consumption expenditures but the key independent variable is now the change in income, $\Delta Y_{it}$, which equals the dollar amount of the head of household's last gross pay if the previous month was a three paycheck month and zero otherwise. While the indicator, $\mathbf{1}_{\{t-1 \in S\}}$, for whether a month follows a three paycheck month used in the main analysis lends itself to easier interpretation, it does not take full advantage of the variation available in the data set. Using the change in income allows me to leverage variation in not just the timing of third paychecks but also their *size*. Table C.1 presents estimates of the spending response from this alternate specification and shows a marginal propensity to spend of 0.17 out of third paychecks. Given an average paycheck amount of $1669 for biweekly households, this translates to roughly $284 in increased spending following three paycheck months, which is similar to the estimate from the main analysis in Table 2.

For the second alternative specification, I estimate the following equation

$$\Delta log(C_{it}) = \beta * \mathbf{1}_{\{t-1 \in S\}} + \theta_{it}'\alpha + \gamma_t + \epsilon_{it} \tag{14}$$

where the dependent variable is now the log month-over-month dollar change in consumption expenditures. Here, the parameter of interest, $\beta$, measures the percentage change in expenditure growth following a month with three paychecks. Table C.2 presents estimates from this specification for each of the five aggregate measures of consumption expenditure in the main analysis—food, strictly non-durable, non-durable, durable, and total expenditure—as the outcome. Consistent with the main findings, the estimates in Table C.2 indicate that there is a significant response in total spending following three paycheck months, though the estimate is slightly less significant than that of the main analysis.

TABLE C.1

SPENDING RESPONSE TO EXTRA PAYCHECKS USING LEVEL CHANGES IN INCOME

| *Dependent variable:* $\Delta C_t$ | (1) Total | (2) Durable | (3) Non-durable | (4) Strictly Non-durable | (5) Food |
|---|---|---|---|---|---|
| $\Delta Y_t$ | 0.17*** | 0.16*** | 0.01** | 0.00 | 0.00 |
| | (0.06) | (0.06) | (0.01) | (0.00) | (0.00) |
| Demographic Controls | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y |
| N | 13,707 | 13,707 | 13,707 | 13,707 | 13,707 |

NOTE.—This table presents difference-in-differences estimates of the total spending response for biweekly households following months with three paychecks. Each column reports estimates from a separate regression run at the household-month level, where the dependent variable is the month-over-month dollar change in spending. Coefficient estimates are reported for the change in income, $\Delta Y_t$, which equals the dollar amount of the head of household's last gross pay if the previous month was a three paycheck month and zero otherwise. All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in total spending, while columns (2) to (5) report coefficient estimates of the spending response for the following subcategories of total expenditure: durable, non-durable, strictly non-durable, and food. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.

TABLE C.2

Spending Response to Extra Paychecks Using Log Changes in Consumption

| *Dependent variable:* $\Delta log\ C_t$ | (1) Total | (2) Durable | (3) Non-durable | (4) Strictly Non-durable | (5) Food |
|---|---|---|---|---|---|
| $\mathbf{1}_{\{t-1 \in S\}}$ | 0.02** | 0.03 | 0.01 | 0.00 | 0.00 |
| | (0.01) | (0.02) | (0.01) | (0.00) | (0.00) |
| Demographic Controls | Y | Y | Y | Y | Y |
| Month and Year FEs | Y | Y | Y | Y | Y |
| N | 13,707 | 13,622 | 13,707 | 13,707 | 13,707 |

Note.—This table presents difference-in-differences estimates of the total spending response for biweekly households following months with three paychecks. Each column reports estimates from a separate regression estimated at the household-month level where the dependent variable is the log month-over-month dollar change in spending. Coefficients are reported for the indicator $\mathbb{1}_{\{t-1 \in S\}}$, which equals one if the previous month was a three paycheck month (i.e., if three paychecks of income are available in the present month $t$). All specifications include month and year fixed effects as well as the age of the head of household, changes in the number of children, and changes in the number of adults as controls. Column (1) reports the coefficient estimate for the overall response in total spending, while columns (2) to (5) report coefficient estimates of the spending response for the following subcategories of total expenditure: durable, non-durable, strictly non-durable, and food. Standard errors are reported in parentheses and are clustered at the household level. Significance levels 10%, 5%, and 1% are denoted by *, **, and ***, respectively.