Scarcity and cognitive function around payday: A conceptual and empirical analysis

Anandi Mani[1], Sendhil Mullainathan[2], Eldar Shafir[3] and Jiaying Zhao[4]

[1] Blavatnik School of Government, University of Oxford, [2] Booth School of Business, University of Chicago, [3] Dept. of Psychology & Woodrow Wilson School of Public & International Affairs, Princeton University, [4] Dept. of Psychology & Institute for Resources, Environment and Sustainability, University of British Columbia

Abstract
The ongoing demands around smoothing consumption with low and sporadic income flows in contexts of scarcity entail that minor changes in cash flows can have big psychological and behavioral effects. In this paper, we examine the behavioral and cognitive impact of routine periodic fluctuations in financial status of the poor around paydays. In particular, we draw a link between a range of documented behaviors and an increase in scarcity-induced cognitive load, closer to payday. Our results, along with those of others briefly reviewed, illustrate the outsized role in scarcity contexts of otherwise trivial changes in income flows and highlight the importance of carefully structured research designs in studying the myriad challenges in scarcity contexts.

1. Introduction

Being poor involves continually juggling expenses, anticipated and otherwise, against low and uncertain incomes. Managing one's expenses can feel like a veritable tightrope balancing act, where small missteps or mishaps can have big and bad consequences. Even those who earn annually a bit more than they spend, if on some days there's not enough cash to handle expenses, they must then resort to payday loans, pawn shops, and overdraft fees, with the accompanying high interest, shame and stress. Avoiding these menacing consequences demands unrelenting attention, constant monitoring and carefully calibrated spending.[1] As it turns out, a person focused on doing all that has less cognitive resources left for other things, from attending to peripheral matters, to self-control and long-term planning.

A growing body of work on the scarcity mindset suggests that coping with scarcity in these ways imposes cognitive load and directly impacts cognitive function, which in turn shapes behavior (Mullainathan & Shafir, 2013; Mani et al., 2013; see also Cannon et al., 2019; Hamilton et al., 2019). Budgetary concerns and preoccupations under scarcity consume cognitive resources, including attention and executive control, and as a result elicit a range of counter-productive behaviors such as attentional neglect, forgetting, impulsive spending, anxiety, and poor planning (Ong, Theseira, & Ng, 2019; Shah et al., 2012; Zhao & Tomm, 2018).

Scarcity mindsets are exacerbated by the juggling and firefighting pressures inherent to scarcity contexts. A scarcity mindset is not simply the outcome of scarce resources, but a function of the challenges and urgencies involved in managing everyday needs with limited resources. Small factors such as synchronicity between income and spending, predictability, default payments, rainy day funds, and so forth, can greatly reduce the persistent demands of managing life under scarcity. Well-timed income and expenses, especially when resources are scarce and there's little slack, demand less attention and effort than when income and expenses are misaligned. Mullainathan & Shafir (2013) propose the metaphor of cockpit design, where capable and motivated pilots are likely to perform -- and to survive -- better in well-designed as opposed to poorly-designed cockpits. Similarly, the poor are likely to fare better, and to experience less distraction and load, when their financial lives, rendered smoother and more predictable, are designed to yield fewer unexpected challenges or insurmountable demands.

In this paper, we focus on the question of cognitive function around payday, a recurring and well-identified moment directly pertinent to the smoothing of income and expenses. In this vein, several studies, some not even directly about financial outcomes, suggest that

[1] A recent first person-account by a single mother eking out a living cleaning homes in the U.S, captures this well: In her book, '*Maid*', Stephanie Land writes "I spent hours staring at my written budget, knowing almost to the day when I'd run out of savings… Living in poverty is a daily struggle for basic needs. The stress is all-consuming…" She captures cognitive challenges of managing with less money vividly when she says "Less money … meant standing in the middle of a grocery store aisle, a package of sponges in my hand, crunching numbers in my head to figure out if I could afford it." (*The Mental Load of being a poor mom*" – https://www.refinery29.com/en-us/2017/07/160057/the-mental-load-of-being-a-poor-mom).

consumption smoothing around paydays can lower cognitive load and improve outcomes. For example, a Cheque Day Study in Vancouver, BC was designed to test whether changing the frequency of payments could reduce drug use and related harm in homeless individuals. Preliminary data show that when monthly welfare checks are split in half and paid out twice a month, the quantity and frequency of drug use declines, compared to a control group with once-monthly payments (Richardson et al., 2019). In a similar study, low-income American workers whose EITC refunds were paid out periodically over time instead of in one lump sum reported increased economic security, decreased borrowing, higher capacity to afford child care and education or training, and lower financial stress (Belisle & Marzahl, 2015). In addition to frequency of payments, their timing can also facilitate smoothing. In a recent study, when EITC was paid out via monthly paychecks ahead of tax time, low-income Chicago workers exhibited lower financial stress and less depressive symptoms (Andrade et al., 2017). The advance periodic payment has several advantages over a lump sum payment, including the availability and predictability of funds throughout the year for both expected and unforeseen expenses, the enabling of bill payments without costly loans, and the ability to avoid some of the firefighting that comes with lack of liquidity in critical times. The Chicago pilot showed several positive impacts of the advance payments, including increased economic security, decreased borrowing behavior, greater capacity to pay for education, training, or childcare, and decreased financial stress (Holt, 2015). In fact, just delaying a portion (e.g., 20%) of the EITC refund for six months can promote savings and facilitate dealing with downstream income shocks (Halpern-Meekin et al., 2018).

Financial instability is costly, financially and psychologically, and can result in cognitive load, worry, fatigue, ultimately leading to deeper poverty traps. Nuanced features can facilitate or exacerbate financial juggling and psychological stress and thereby improve or impair behavior and well-being (Mullainathan & Shafir 2013; see also Gennetian & Shafir, 2015). In what follows, we explore this logic in greater detail, focusing on cognitive function and behavior change around payday. In particular, we describe the findings of a recent paper (by Carvalho, Meier and Wang, 2016), which examines some effects of the scarcity mindset around payday. We then delve more deeply into those authors' original data in order to explore and to demonstrate the potentially profound consequences of nuanced changes, such as exact distance from payday, on cognitive function in scarcity contexts.

In a concluding section we review several studies that have looked at the behavioral impact of small changes on the scarcity mindset, or that in retrospect can be interpreted through that lens. Our analyses, we suggest, provide an important lesson for scholars and policy makers. In a tightrope balancing act every little step matters. In the cockpit, each little lever can determine the success of a flight. In a scarcity context, each moment and its challenges can have a profound effect on cognitive resources and behavior. Greater sensitivity to the nuances of scarcity – not only how much the person makes, or spends, or owes, but precisely when and under what circumstances – can generate important findings, explain otherwise puzzling differences, and determine whether an intervention succeeds or fails.

2. Using Income Shocks to Measure Scarcity Effects

The mental burdens imposed by poverty are not necessarily confined to periods of sharp income fluctuations -- but the burdens are likely to be amplified around times of unpredictability and flux. Consequently, the timing of particular income shocks can be useful as a research design device, to measure scarcity effects. Indeed, the paper by Mani et al. (2013) does precisely this, by examining differences in farmers' cognitive function around the timing of their annual, and quite substantial, sugarcane harvest income.

It is noteworthy that for a particular income shock to be a suitable research design device for measuring scarcity effects, two features must be true: (i) the before-after differences in income levels due to the income shock must be significantly large, and (ii) the timing of this shock and the measurement of its impact must be sufficiently far, and thus separable, from other income and/or expenditure shocks the relevant sample of participants may have experienced. The rationale for (i) is simply that larger income shocks or those above a certain threshold level are more likely to trigger cognitive and behavioral impacts; The rationale for (ii) is that a shock being sufficiently far away from other shocks makes it possible to clearly detect its impact, without it being muddied by the possible effect of other events. Finally, as feature (iii), it is useful to ask whether there is uncertainty in the exact timing or precise magnitude of an income shock, since this kind of uncertainty could add additional psychological and juggling burden, over and above that from the actual change in income levels.

Viewing Pathologies of Poverty around Payday through a Scarcity Lens

In the context of the U.S., an income shock event that has been widely used to study the behaviors of the poor is the monthly payday which forms part of various welfare programs. There is a wide range of evidence showing various pathologies of poverty around the timing of paydays, with striking differences in behavior before payday as opposed to after. For instance, food stamps recipients tend to eat less well right before payday (see Gennetian et al., 2011), and crimes rates and episodes of violence tend to increase before payday in low-income neighborhoods (Carr & Packham, 2019; Foley, 2011). While both those effects could be due to lack of material resources alone, the evidence also shows that the poor tend to sleep less well before compared to after payday and to be less attentive parents before payday than after (Danziger & Lin, 2009; Gennetian, Darling, Aber, 2016; Shipler, 2005). Among Chicago public middle-school students, a 40 percent increase in school disciplinary events was observed at the end as compared to the beginning of the month for students whose families participate in The Supplemental Nutrition Assistance Program (SNAP), a much larger difference than that among fellow students not receiving SNAP benefits. (Gennetian, Seshadri, et al., 2016). Those and related behaviors seem consistent with the mental burdens of poverty, not just its material burdens. Could there be a direct causal link between the various scarcity-induced cognitive effects and the behavior changes shown by the poor around payday?

3. Measuring Scarcity Effects using Payday Income Shocks: A Case Study

A first step towards testing whether behavior change around paydays can be explained by the scarcity mindset would be to examine before-after differences in cognitive function among the poor, around a specific payday shock. Because small differences in context and timing of events can induce big differences in attention allocation and behavior, accurate measurement of cognitive function effects may be highly sensitive to specific features of the income shock under study.

To elaborate on this point in some detail, we use a recent paper by Carvalho, Meier and Wang (2016) (henceforth CMW) as a case study. This paper examined cognitive effects of poverty around payday, by randomly assigning low-income U.S. respondents to be surveyed either before or after their regular payday.[2] Perhaps surprisingly, in light of the literature on observed behavior changes around payday discussed above, CMW find no evidence of adverse cognitive effects of financial pressures before payday, as compared to after.

To better understand their result, we consider how the payday shocks that CMW chose for their studies fare on the three features described above.

On (i), i.e., the magnitude of the income shock, their chosen payday shocks fare well: Before- and after-payday group respondents in CMW's studies report significant differences in bank balances and expenditures. However, on (ii), i.e., the time distance of the shock and the measurement of its impact in relation to other income/expenditure shocks, CMW's choice of payday raises some concerns: Study respondents received up to four payments within the 1-month study window, of which one payment was chosen as the payday shock. Such high payment frequency suggests that other shocks may have occurred very close to the chosen payday. Finally, on (iii), regarding uncertainties in the timing or magnitude of the income shock, CMW specifically included only respondents who provided complete information on the frequency and dates of payments during the study period. The absence of uncertainty in timing and frequency of payments could be an additional factor contributing to CMW's null result on the cognitive impact of payday shocks. Based on an examination of their study design and data, we suggest that CMW's null effect may be attributable to a combination of design features on (ii) and (iii) above. We discuss each of these features next.

3.1 (Time) Distance of Payday Shock from other Income/Expenditure

[2] CMW apply this across-person design on two different study samples of U.S. respondents, both with annual household incomes less than $40,000. Study 1 was conducted with members of the RAND American Life Panel (ALP) between November 2012 and March 2013. Study 2 was conducted with members of the GfK Knowledge Panel (KP) between November and December of 2014. 45 percent of the Study 1 sample and 41 percent of the Study 2 sample had an annual family income below $20,000. Information on paydays is self-reported, with no distinction made across different sources of income, earned or unearned, private or public.

CMW report results from two studies with different samples, as described in footnote 2. Respondents in Study 1, had up to *four* paydays within the 1-month study window, out of which one was chosen by CMW as the payday shock. A high number of payments in close proximity *risks blurring the distinction between being in a before – versus after - payday situation*, which is crucial to measure the cognitive impact of poverty. In fact, consistent with this blurring that might come with a high number of payments, there are no significant before-after payday group differences in the level of financial anxiety that respondents in Study 1 report.

In Study 2 of CMW, respondents received up to 2 payments within the one-month study window. It is of course plausible that even for an individual who is paid bi-weekly or more frequently, the period leading up to the next payday may be one in which financial constraints are more severely felt. And this experienced scarcity could diminish cognitive function. However, in a setting where payday cycles are short, the power to detect such effects *will rely crucially on precisely surveying people in sufficiently tight time windows* before and after payday.

Respondents in the CMW studies were required to complete tests of cognitive function up to 7 days on either side of payday. A wide response time-window again blurs the distinction between being in the before-versus-after payday condition. To see this point more clearly, take the case of those whose payday cycle is every two weeks.[3] If such persons take the survey 7 days before payday, they are also taking it 7 days after (their previous) payday.

In what follows, we re-examine the effects of scarcity on cognitive function in the CMW data by taking into account how far from their payday respondents took the survey. We then also examine the effects of scarcity on cognitive function in the CMW data, using progressively tighter time windows around payday. For respondents with multiple paydays within the study period, we look at how CMW's original results may be sensitive to the precise assignment of payday in those cases. Finally, we discuss some design approaches to selection issues arising from unobservable attributes of respondents.

3.2 Payday Cycles and the Timing of Survey Completion

To set the stage for our analysis, we briefly describe some relevant details of the CMW approach. The authors first gathered baseline information about the dates and amounts of all payments expected by a sample of low-income US households within a one-month study window.[4] In case of multiple payments in this time period, CMW's rule for the choice of payday was as follows: "If the largest payment came two weeks or more after the previous payment, then payday was set as the date of this largest payment. Otherwise, the payday was set as the date that followed the longest interval without any

[3] In Study 2, 48.3% of the sample (1316 out of 2723 respondents) had 2 paydays within the 1-month study period, 663 before payday, and 653 after.

[4] Respondents with more than a specified number of payments within the 1-month study window were excluded from the study – five or more payments in Study 1, and three or more payments in study 2.

other payments. Participants whose payments were all less than 2 weeks apart were dropped from the study sample".

To create exogenous variation in the level of financial resources, CMW then randomly assigned respondents to receive an online follow-up survey either before or after their payday. As part of this follow-up survey, respondents carried out a number of different tasks, including some that measured their cognitive function. The follow-up surveys opened seven days before payday for the 'before' group and one day after payday for the 'after' group[5].

In order better to illustrate how research design details matter, we focus our analysis on study 2. The main reason for this is that participants in Study 1 had up to 4 payments in the 1-month study window,[6] and as the authors themselves point out, there are several reasons why the data from study 2 are more reliable.[7]

Participants in study 2 had at most two payments within the study time-window (footnote 12, CMW). Given the rule used for the choice of payday, either payment 1 or payment 2 or both came 14 days or more after the previous payment. (See first paragraph of this section for the rule). Participants in the online follow-up survey were free to choose when they actually took the survey within the available time window (including the option to complete it over multiple sessions).[8] As expected, not all participants completed the survey on the first day that it was available; hence there was substantial variation in how far from payday people completed the follow-up survey. Figure 1 reports how many respondents completed the survey within a specific time distance

[5] The actual number of days before payday that the survey became available was higher for some respondents. As the authors note in footnote 11: "In Study 2 we could not open surveys during weekends. Therefore, the Study 2 follow-up opened eight (nine) days before payday for participants assigned to the before-payday group whose payday fell on a Saturday (Sunday), and three (two) days after payday for participants assigned to the after-payday group whose payday fell on a Friday (Saturday). The payday fell on a Saturday or Sunday for 8 percent of the before-payday group (Observations = 109) and on a Friday or Saturday for 26 percent of the after-payday group (Observations = 355)."

[6] Consistent with the higher number of payments, there are no significant before-after payday differences in the level of financial anxiety that respondents in study 1 report. This is not the case in study 2.

[7] As CMW note (Para 4, page 277): "There are several reasons why the effects are estimated with greater precision in Study 2 than in Study 1. First, the compliance rate was substantially higher in Study 2: in Study 1, 30 percent of participants assigned to the before-payday group started the survey after payday, but in Study 2 approximately 2 percent of the before-payday group started the survey after payday. Second, we increased the sample size (i.e., the number of participants) by almost 150 percent. Third, there were more trials per participant (20 in Study 1 versus 48 in Study 2)."

[8] It is possible that unobserved characteristics drive both the choice of survey completion date and cognitive function or that scarcity may itself affect the choice of the survey completion date. We address these issues in Section 3.3 below.

before or after payday ("distance to payday;" DTP) [9][10]. As shown, the spread in this distribution is considerable. In the before-payday group, respondents completed the survey between 10 days before payday and up to 3 days after payday. In the after-payday group, respondents completed the survey any time between the day they got paid and up to 11 days later.

INSERT FIGURE 1 HERE

3.3 Measuring Cognitive Function: Effects of Distance to Payday

How may the wide range of survey completion dates affect the observed cognitive effects of scarcity? Presumably, the closer a before-payday respondent was to their next payday when completing the survey, the more stringent their financial situation was likely to have been, and hence the scarcity effects on their cognitive function. It is also possible that when the next payday is close at hand, there might be some reduction in financial anxiety on the days closest to payday, because of the income that is imminent. Thus, the average effect of scarcity within the before-payday group as a whole may mask considerable heterogeneity in scarcity effects across respondents who completed the survey in different time windows.

We therefore examine before-after payday group differences in cognitive function for samples in two ways. First, we simply account for how close to payday a respondent completed the follow-up survey. Second, we examine these same before-after payday group differences in cognitive function within progressively tighter time windows around payday.

We recognize that while respondents were randomly assigned to the before versus after payday survey groups, the day on which they took the survey was their own choice. Unobserved characteristics correlated with their choice of survey completion date but unrelated to scarcity could drive some (or all) of the before-after group differences in cognitive function that we examine. We discuss this issue of the endogeneity of respondents' distance to payday in subsection 3.4 below.

CMW use a Stroop test to measure cognitive function. They report regression results on

---

[9] The survey completion (time) distance to payday (DTP) ranges from -9.10 to 11.85. A value of -9.10 indicates that a respondent finished the survey on the 10th day before payday while a value of 11.85 indicates that the respondent finished the survey on the 11th day after payday.

[10] DTP was missing for 123 out of 2723 respondents (4.51% -- 62 before, 61 after payday). However, survey start time relative to payday was available for all respondents. 95% of before-group respondents and 94% of after-payday respondents completed the survey within the same day that they began. Hence the missing values for Survey completion time were imputed as follows: Survey completion time = Survey start time + Mean (Time taken to do survey, by Before/After Payday group).

two performance measures, the test score and the time taken[11]. To illustrate our point about nuanced research design, we focus on their Stroop test score measure. In Table 1 below, we simply re-run their regression for the test score, controlling for the distance of survey completion time to payday.

Column (1) in Table 1 replicates CMW's original result on the Stroop test score (Table 6, Column 4). Column (2) simply adds a control for time distance to payday (DTP), namely how long from payday respondents took the follow up survey[12]. As can be seen from a comparison of the two columns, CMW's original result on the Stroop test score is sensitive to when respondents take the survey: The coefficient on the treatment variable goes from 0.004 to -0.055; in other words, the before-payday (treatment) group does significantly worse on cognitive function relative to the after-payday group, once we account for survey completion time distance to payday (DTP). The coefficient on the DTP variable shows that respondents in the 'before' group have worse cognitive function closer to payday and that respondents in the 'after' group have poorer cognitive function farther away from payday. Column (3) uses an interaction specification to examine whether this DTP has a more significant adverse cognitive effect on the before-payday group; while this interaction effect is not statistically significant, the adverse impact noted in column (2), on the cognitive function of the before payday group, remains[13].

INSERT TABLE 1 HERE

Next, we examine the heterogeneity in the cognitive effects across respondents who completed the follow-up survey at different times. We re-run the same regressions as in Table 1, but for progressively more restricted samples covering tighter windows before payday[14]. In individual regressions, the before-payday sample goes from the full sample (i.e. survey completed up to 10 days before payday) to sub-samples that completed the survey up to 9 days before, up to 8 days before and so on, up until 1 day before payday

[11] The regression specification used is as follows: $Y_{in} = T_i + S_n + u_i$, where Y_{in} is the test outcome (score or time taken) of person *i* in Stroop task *n*, T_i is a treatment dummy that equals 1 if person *i* was assigned to the before-payday group and 0 if (s)he was assigned to the after-payday group, *S* represent Stroop task dummies (n=1 to 48) and u_i is a person-specific error term.

[12] This is the DTP variable that Figure 1 is based on, with values ranging from -9.10 to 11.85. Within the 'before' group respondents, larger (i.e. less negative) values indicate that respondents completed the survey *closer* to payday, while among the 'after' group respondents larger (more positive) values indicate that they completed the survey *further away* from payday.

[13] The results in Table 1 are robust to (i) defining DTP in terms of number of *days* (i.e. integer values as depicted in Figure 1), rather than the continuous variable available in CMW's data (ii) using imputed values based on the *median* survey completion time distance to payday (DTP) within the before and after payday groups (iii) dropping observations with missing values for this variable as well as (iv) dropping observations for surveys of before-payday respondents begun or completed after payday(non-compliers).

[14] The regression specification used is the same as in footnote 11, but for samples in different time windows.

(or later). The after-payday group includes the full 'after' sample in each regression[15].

INSERT FIGURE 2 HERE

Figure 2 reports the coefficients for the Stroop test score from each of these regressions, in progressively tighter time windows. Going from left to right, the individual bars show how the cognitive function gap between the before vs. after payday groups changes, as the survey-to-payday time gap in the before-group goes from 10 days ('All') to 1 day or less[16].

The first – left-most - bar in Figure 2 corresponds to the coefficient reported in CMW (Table 6, column 4), pooling across the full sample of respondents. There is no significant before-after payday difference in Stroop test scores overall. The second bar reports these results for respondents who completed the survey up to 9 days prior to payday. Jumping forward, the eighth bar shows the results for all before-payday respondents who took the test up to 3 days before payday. This sub-sample of respondents scored 4.8% lower on the Stroop test than the after-payday respondents.

Overall, Figure 2 illustrates how the Stroop score gap between the 'before' and 'after' group widens as payday approaches, with the 'before' group doing significantly worse starting around 4 days before payday. These results add useful day-by-day detail to those reported in Table 1, where we simply controlled for respondents' survey completion time distance to payday. The findings are consistent with scarcity effects anticipated in contexts of insufficient resources and lack of smoothing, i.e. reduced cognitive function, closer to a forthcoming payday.

We note that the results described above are sensitive to sample and specification details: In particular, the results are sensitive to the use of particular dependent and independent variables: Stroop time measures and survey start times (rather than completion times, to measure distance to payday) respectively. Similarly, the magnitude and significance of the results are lowered by the exclusion of respondents assigned to the before-payday group who completed the survey after payday, although the adverse before-payday impact persists. The analysis above uses the full sample provided by the authors, and adheres to an intent-to-treat specification for treatment assignment. Nevertheless, we recognize that the endogeneity of the survey response time can raise concerns because participants' timing could be correlated with other characteristics, observable or not. We return to a discussion of these issues in Section 3.5.

---

[15] An alternative approach would have been to cull before-after samples in *symmetric* time windows around payday. The specification used above is less restrictive in the comparison of before-after payday scarcity profiles than a symmetric specification. In the latter, respondents who experience the most scarcity in the before-payday group would be compared to those who experience the least scarcity in the after-payday group, as we tighten the time window. The results from the symmetric specification are similar to those reported in Figure 2, except in the 1-day time window around payday, when we have far fewer observations.

[16] The corresponding results for the time taken on the Stroop test as well as the underlying regressions for both performance measures are reported in Appendix Table I, panel A.

3.4 Payday Assignment given Multiple Paydays

There is another illuminating detail that is worth noting regarding the choice of a payday, in the case of participants with multiple paydays. As noted earlier, nearly half of the participants in CMW's Study 2 received two payments within the 4-week study period. For those in this group who had two unequal payments, CMW determined payday to be the one when the larger of the two payments was received, unless the time-gap from the previous payment was less than 14 days. (See paragraph 1 in Section 2 above).

However, there are several cases with two paydays with *equal* payments [17]. In these cases, CMW use the time gaps in payments - between the previous payment date outside the study window and date 1 (gap 1) as well as the time gap between payment dates 1 and 2 (gap 2). Their assignment rule for these cases is as follows: If gap 2 is larger than gap 1, payment date 2 should be assigned as the payday. In addition, they assigned date 2 as the payday in cases where gap 1 equals gap 2, or gap 1 is missing.

In these latter cases, however, with two equal payments *and* equal time gaps 1 and 2, it would be equally valid to assign *date 1* as the payday. Doing so changes the treatment status of 133 respondents from *before*-payday (with payday=date 2) to *after*-payday (with payday = date 1)[18]. We find that CMW's overall result is quite sensitive to this last assignment. We re-run the regressions underlying Figure 2 under this revised treatment assignment and report our results in Figure 3 below (see Appendix Table III for the regression table results underlying this figure).

INSERT FIGURE 3 HERE

Now, before-payday respondents do even worse than after-payday respondents closer to payday as compared to the results reported in Figure 2. The most noteworthy change is that cognitive function is now consistently worse in the before-payday group across *all* time-windows - and hence in the *full* sample.

---

[17] 557 respondents had two payments of equal amounts. Of these, 319 were assigned payday = date 2 and 238 were assigned payday = date 1.

[18] Of 319 respondents whose payday was set equal to payment date 2, 278 respondents would have their payday reset to date 1 based on our revised assignment. For these respondents, gap 1 (between the previous payday and payment date 1) either equals gap 2 (between payment dates 1 and 2) or is missing. 133 out of these 278 respondents were originally in the before-payday group and 145 in the after-payday group, by random assignment. Treatment status would only change for the first set of 133 respondents from being before-payday to after-payday (under date 1). The treatment status of the remaining 145 respondents would still be 'after payday' under payday = date 1 (albeit at greater time distance). (If observations with missing gap 1 values are assigned payday = date 2, then the numbers corresponding to 133 and 145 observations are 116 and 126 respectively. The results reported in Figure 3 and Appendix Table III remain very similar to the ones reported here.)

For all respondents whose payday was chosen as date 1, payday is correctly assigned where previous payment dates are available. There are 126 observations with missing previous payday data under payday=date 1.

3.5 Endogeneity of Survey Response Time

In order to establish a causal effect of scarcity on the performance gaps seen in Figures 2 and 3 above, participants' own choice of survey response timing should be uncorrelated with the outcome of interest, i.e. their cognitive function. This would have been true, for instance, had they completed the follow-up survey at a randomly assigned time distance from their payday. Of course, this cannot be done for the CMW study *ex-post*.

In the context of the CMW study, the best we can do ex-post is a balance check for the same set of observables that CMW used to verify their randomization, for sub-samples in progressively tighter time windows before payday[19]. This is not a solution to the concern over the endogeneity of the survey response timing, but it could at least help us detect any obvious selection issues in the before versus after payday samples. We carried out such a check for each of the daily samples, using the set of variables listed in Table 1, Appendix F of CMW (and reproduced here in Appendix Table II). CMW had used this set to verify that their randomization resulted in balanced before-versus-after payday groups. We report the results of our daily sample balance checks in Appendix Table II.

We compare CMW's original set of observables in samples going from 10 days before payday up to 1 day before payday against the full 'after' sample. Appendix Table II shows that some variables are not fully balanced across the before and after payday samples, in specific time windows. We therefore re-run the regressions underlying Figure 2 with controls included for these imbalanced variables. Our revised regression results with these controls are reported in Figure 4.

INSERT FIGURE 4 HERE

As seen in Figure 4, the overall pattern seen in Figure 2 does not change, notwithstanding some reduction in the size and statistical significance of the results. (The corresponding regression tables are reported in Appendix Table I, panel B). Thus, even after accounting for any imbalances on observable characteristics of before versus after payday samples in CMW's data, the evidence is consistent with scarcity effects resulting in reduced cognitive function before payday, relative to after.

To summarize, our reexamination of CMW's data suggests that the null effect they reported may well be due to insufficient sensitivity to time distance between the timing and measurement of the chosen payday shock and other income shocks within the study window. Given the endogeneity concerns over participants' choice of survey completion time, the results serve further to illustrate the importance of specific features of income shocks – the lapse of time between payday and other proximate income/expenditure shocks – in measuring scarcity effects.[20]

---

[19] See Table 1, Appendix F in CMW for a list of their variables.

[20] There is also a possibility that experiencing greater scarcity may itself cause individuals to delay completing the follow-up survey. If so, the coefficients in our regression may be biased to be larger closer to payday. However, this effect will still be driven by the scarcity mechanism of interest.

3.6 Uncertainty in Timing and/or Magnitude of Shocks:

A final feature of the CMW study noted earlier was the absence of uncertainty: CMW 'restricted the sample to participants who provided complete information about the number and dates of payment.' (footnote 13, CMW). We highlight this issue for two reasons. First, uncertainty is an important dimension in juggling the challenges of poverty. The absence of uncertainty in CMW's payday context renders their study design different from those used in previous studies, for instance Mani et al. (2013), where participants faced uncertainty in both the amount and timing of harvest payment(s). A recent study with Brazilian farmers by Lichand and Mani (2019) separately measured the impact of two key dimensions of poverty: low *levels* of income and *uncertainty* in income. They find that while low levels of income impede cognitive function among the poorest municipalities, uncertainty plays a bigger role in impeding cognitive function across a broad income spectrum of farmers facing drought risk. Both the higher income levels and the lack of uncertainty faced by the US participants in the CMW studies could have further contributed to the null effects the authors found.[21]

4. Nuanced behaviors under scarcity

The foregoing analyses have explored the potentially outsized role of minor differences in timing and uncertainty around payday. Viewed through the lens of a scarcity mindset, many 'puzzles' concerning the counter-productive behaviors of the poor can look quite different, and start making sense. Given the financial and mental tightrope act they must perform at all times, it becomes apparent how small nuances of context and timing, cash flows and program design that are seemingly inconsequential, can have large effects on the choices and outcomes of the poor. A few examples serve to illustrate this point more sharply.

Take the case of low fertilizer use in Africa, which may be a factor in low crop yields compared to places where use is higher (Morris et al., 2007). Duflo and colleagues (2011) document that fertilizer is available, affordable, effective, and even appreciated, but still not used. About 97 percent of Kenyan farmers surveyed said they intended to use fertilizer on their fields the following season, but only 37 percent actually ended up using fertilizer. One small tweak in service increased fertilizer use by 70 percent: home delivery of fertilizer (Duflo et al., 2011). Early home delivery amounts to a 10 percent discount on the market price of fertilizer, but it increases use by as much as a 50 percent subsidy would.

When the cognitive demands of poverty are high and attention to peripheral matters is diminished, keeping track of details can be a challenge. Banerjee, Duflo, and Glennerster

[21] It is also worth noting additional design features in Lichand and Mani's (2019) study that get around the issue of endogeneity in respondents' survey response time, relative to distance from payday: First, the study exploits *random* variation in the timing of paydays for conditional cash transfers to poor farmers in Brazil. Second, respondents are administered automated phone surveys, and do not choose when to take these surveys.

(2011) describe a program for fortifying flour with iron. A household had to tell the miller *just once* whether it wanted to have its flour enriched and the miller was supposed to act accordingly each subsequent time. Unfortunately, the participating millers flipped this around: they required the household to say whether they wanted iron added to their flour each time they brought grain to be milled. With this change, participation in the program plummeted, causing it to fail to achieve its objectives. This example suggests that the repeated need to instruct the miller constituted a significant demand on the already-burdened mind under poverty.

When pressing matters emerge, that's where attention is directed. When people in contexts of scarcity tunnel their attention on urgent matters, other things are neglected, just as an air traffic controller focusing on a potential collision course is prone to neglect other planes in the air. In moments such as these, subtle interventions that manage to enter a person's attentional tunnel can yield big results. In Udaipur, India, for example, an immunization program that managed to draw parents' attention by offering each parent who brought a child to be immunized a 2-pound bag of lentils—equivalent to about half a day's wages for an agricultural labourer—succeeded in greatly augmenting the fraction of children who were fully immunized in that part of rural India, from 18 to 29 percent (Banerjee et al., 2010).

In Malawi, offering a small incentive of around $0.15 (about 10 percent of the daily wage) more than doubled the fraction of people who picked up their HIV test result, from 34 percent to 70 percent (Thornton, 2008). Giving people a larger incentive—of up to $3—had an even larger effect, raising the rate further to over 90 percent. But the bulk of the jump—from 34 percent to over 70 percent— was achieved simply by moving from no incentive to a tiny incentive, suggesting that the motivation to change behavior was not so much a monetary one, as much as one that depended on drawing *attention* to the issue.

Given the limited resources and competing demands on the attention of the poor, the *timing* (and timeliness) of interventions can be a critical factor in these interventions' success. Consider another major challenge that the poor face: savings. In a series of experiments in Peru, Bolivia, and the Philippines, savings rates among the poor went up simply by providing them with timely reminders about their own saving goals (see Karlan et al., 2010). Regarding the challenge of fertilizer take-up discussed earlier, a special account that allowed farmers to lock up some of their money when they had it, *at the time of harvest payment*, in order to purchase fertilizer for the next season increased the use of fertilizer and other inputs, leading to higher crop sales (Brune et al., 2011). The observed increase in savings and fertilizer purchase decisions is not attributable merely to the availability of material resources; it is as much about well-timed opportunities that *align the allocation of attention* of those who are juggling scarce resources at those critical moments when resources are available and the right actions can be performed.

5. Concluding remarks

One of the fundamental lessons from the behavioral sciences in the last half century has been the large influence that context has over human behavior, along with our tendency to underappreciate the power of context. That lesson figures prominently in the recent advent of "nudging" interventions, where minor alterations in default deductions, for example, have been shown to dramatically alter how much people save for retirement (Madrian & Shea, 2001

By way of various analyses conducted throughout this paper, we explored some of the behavioral and cognitive repercussions of routine periodic fluctuations in financial status of the poor around paydays. In particular, we focused on scarcity-induced cognitive load as a function of seemingly minor changes in the size of financial shocks, time-distance to payday, and the role of uncertainty in the occurrence of shocks, which can greatly contribute to the juggling burden. Our analyses of an existing dataset that initially reached a very different set of conclusions highlight the importance of carefully structured research designs that incorporate these nuanced features when exploring the effects of consumption-smoothing challenges in scarcity contexts. Because the nuances of context matter, researchers are bound to gain new insight and increase the effectiveness of interventions as they become better versed in the particulars relevant to the situations under study. What might play an important role in participants' lives and mindsets? What detail are worth exploring with a finer lens? CMW's Study 2, for example, occurred between November 21st and December 18th, a pre-Christmas season known for unusually high stress among people of low income, who feel the pressure to buy family and friends gifts they cannot easily afford. It is possible that the before-versus-after payday differences in financial pressure, already so high then, were blunted during that season. A comparison with another time of year with lower baseline pressures might shed interesting light on that question. As research into the financial challenges and cognitive demands of scarcity progresses, we are bound to gain a better understanding of the contextual and behavioral aspects that matter most, as well as the best research designs and methodologies to explore them.

References

Andrade, F. C. D., Kramer, K. Z., Monk, J. K., Greenlee, A. J., & Mendenhall, R. (2017). Financial stress and depressive symptoms: The impact of an intervention of the Chicago Earned Income Tax Periodic Payment. Public health, 153, 99-102.

Banerjee, A., Duflo, E., & Glennerster, R. (2011). Is decentralized iron fortification a feasible option to fight anemia among the poorest?. In Explorations in the Economics of Aging (pp. 317-344). University of Chicago Press.

Banerjee, A. V., Duflo, E., Glennerster, R., & Kothari, D. (2010). Improving immunisation coverage in rural India: clustered randomised controlled evaluation of immunisation campaigns with and without incentives. Bmj, 340, c2220.

Bellisle, D., & Marzahl, D. (2015). restructuring the EITC: A Credit for the Modern Worker. *Center for Economic Progress Report*.

Brune, L., Giné, X., Goldberg, J., & Yang, D. (2011). Commitments to save: A field experiment in rural Malawi. The World Bank.

Cannon, C., Goldsmith, K., & Roux, C. (2019). A self-regulatory model of resource scarcity. *Journal of Consumer Psychology*, *29*(1), 104-127.

Carr, J. B., & Packham, A. (2019). Do income shocks affect domestic violence? Retrieved from: https://apackham.github.io/mywebsite/Carr_Packham_AbuseV4.5.pdf

Carvalho, L., Meier, S., & Wang, S. (2016). Poverty and economic decision-making: Evidence from changes in financial resources at payday. American Economic Review, 106(2): 260–284.

Danziger, S., & Lin, A. C. (Eds.). (2009). Coping with poverty: The social contexts of neighborhood, work, and family in the African-American community. University of Michigan Press.

Datta, S., & Mullainathan, S. (2014). Behavioral design: a new approach to development policy. Review of Income and Wealth, 60(1), 7-35.

Devereux, S. (2002). Can social safety nets reduce chronic poverty?. Development Policy Review, 20(5), 657-675.

Dinkelman, T., Lam, D., & Leibbrandt, M. (2008). Linking poverty and income shocks to risky sexual behaviour: evidence from a panel study of young adults in Cape Town. South African Journal of Economics, 76, S52-S74.

Duflo, E., Kremer, M., & Robinson, J. (2011). Nudging farmers to use fertilizer: Theory and experimental evidence from Kenya. American Economic Review, 101(6), 2350-90.

Foley, C. F. (2011). Welfare payments and crime. The Review of Economics and Statistics, 93(1), 97-112.

Gennetian, L., Darling, M., & Aber, J. L. (2016). Behavioral economics and developmental science: A new framework to support early childhood interventions. Journal of Applied Research on Children: Informing Policy for Children at Risk, 7(2), 2.

Gennetian, L. A., Sanbonmatsu, L., & Ludwig, J. (2011). An overview of moving to opportunity: A random assignment housing mobility study in five US cities. Neighborhood and life chances: How place matters in modern America, 163-178.

Gennetian, L. A., Seshadri, R., Hess, N. D., Winn, A. N., & Goerge, R. M. (2016). Supplemental Nutrition Assistance Program (SNAP) benefit cycles and student disciplinary infractions. *Social Service Review*, *90*(3), 403-433.

Gennetian, L. A., & Shafir, E. (2015). The persistence of poverty in the context of financial instability: A behavioral perspective. *Journal of Policy Analysis and Management*, *34*(4), 904-936.

Halpern-Meekin, S., Greene, S. S., Levin, E., & Edin, K. (2018). The rainy day earned income tax credit: A reform to boost financial security by helping low-wage workers build emergency savings. RSF: The Russell Sage Foundation Journal of the Social Sciences, 4(2), 161-176.

Hamilton, R., Thompson, D., Bone, S., Chaplin, L. N., Griskevicius, V., Goldsmith, K., ... & Piff, P. (2019). The effects of scarcity on consumer decision journeys. *Journal of the Academy of Marketing Science*, *47*(3), 532-550.

Haushofer, J., & Shapiro, J. (2016). The short-term impact of unconditional cash transfers to the poor: experimental evidence from Kenya. The Quarterly Journal of Economics, 131(4), 1973-2042.

Holt, S. (2015). Periodic payment of the earned income tax credit revisited. Washington, DC: Brookings Institution.

Karlan, D., Ratan, A. L., & Zinman, J. (2014). Savings by and for the Poor: A Research Review and Agenda. Review of Income and Wealth, 60(1), 36-78.

Lichand, G., & Mani, A. (2019). Cognitive droughts. Center for Competitive Advantage in the Global Economy, 485, 486.

Madrian, B. C., & Shea, D. F. (2001). The power of suggestion: Inertia in 401 (k) participation and savings behavior. *The Quarterly journal of economics*, *116*(4), 1149-1187.

Mani, A., Mullainathan, S., Shafir, E., & Zhao, J. (2013). Poverty impedes cognitive function. Science, 341, 976-980.

Morris, M., Kelly, V. A., Kopicki, R. J., & Byerlee, D. (2007). Fertilizer use in African agriculture: Lessons learned and good practice guidelines. The World Bank.

Mullainathan, S., & Shafir, E. (2013). Scarcity: Why having too little means so much. Macmillan.

Ong, Q., Theseira, W., & Ng, I. Y. (2019). Reducing debt improves psychological functioning and changes decision-making in the poor. *Proceedings of the National Academy of Sciences*, *116*(15), 7244-7249.

Richardson, L. et al. (2019). Cheque day study: The impact of alternative social assistance on drug related harm.

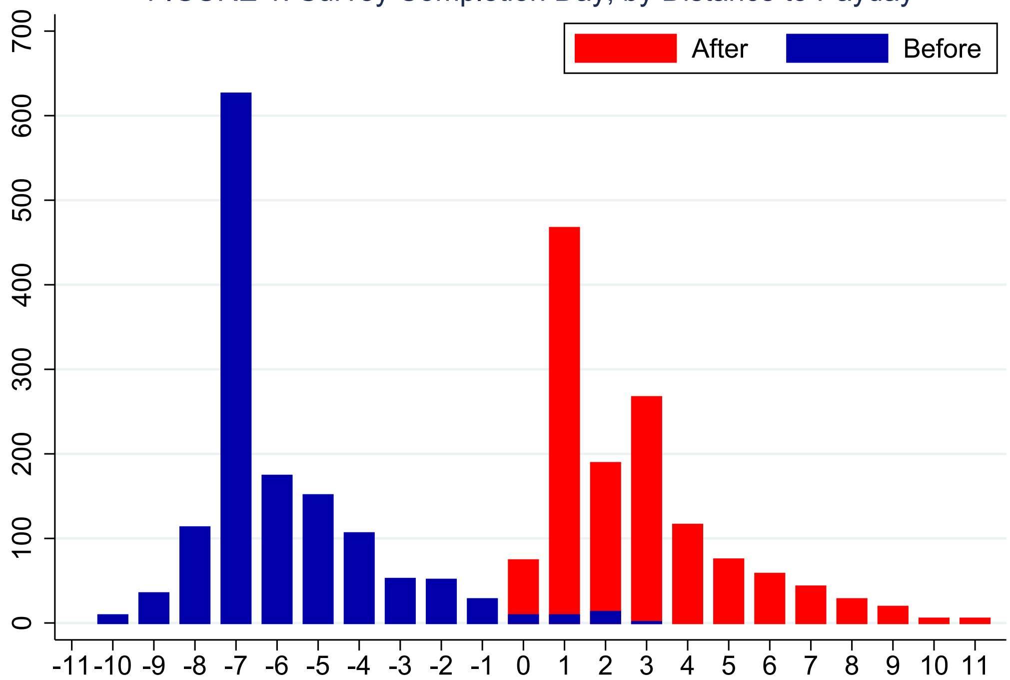
Shah, A. K., Mullainathan, S., & Shafir, E. (2012). Some consequences of having too little. Science, 338(6107), 682-685.

Shipler, D. K. (2005). The working poor: Invisible in America. Vintage.

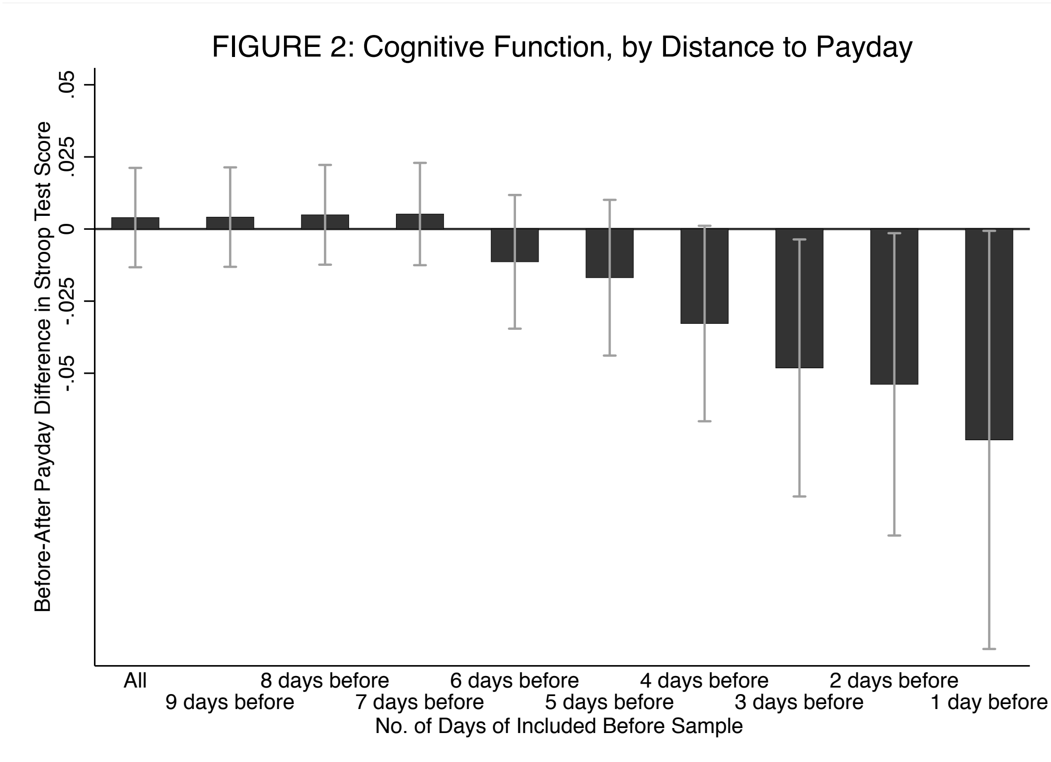
Thornton, R. L. (2008). The demand for, and impact of, learning HIV status. American Economic Review, 98(5), 1829-63.

Zhao, J. & Tomm, B. (2018). Psychological responses to scarcity. Oxford Research Encyclopedia of Psychology. New York: Oxford University Press.

FIGURE 1: Survey Completion Day, by Distance to Payday

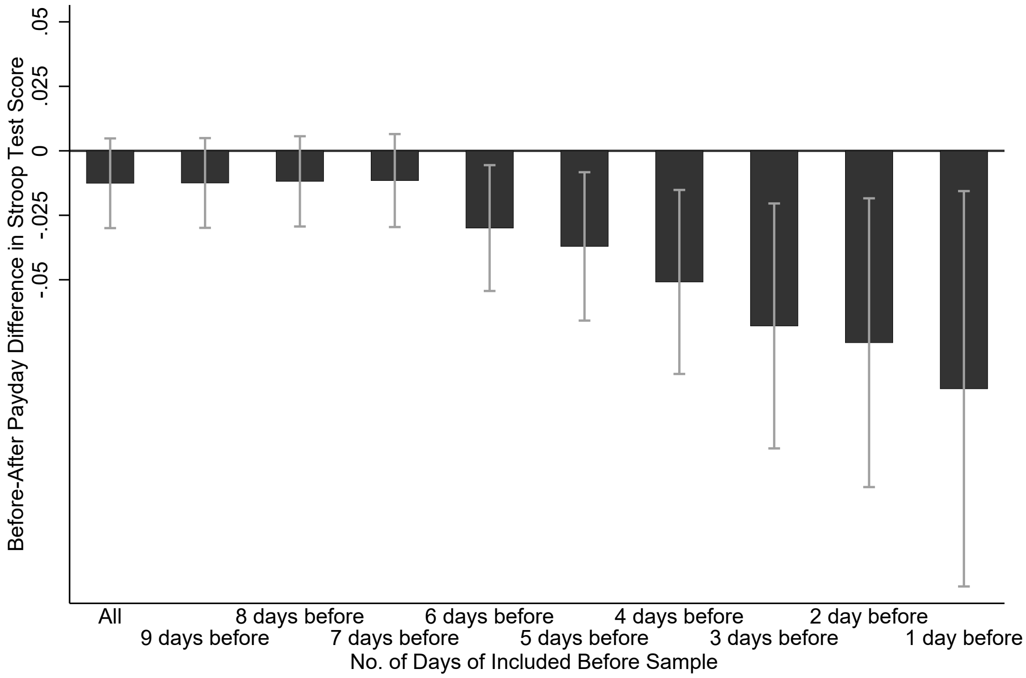


Note: Data from CMW, Study 2. Survey completion time distance to payday (DTP) was missing for 123 respondents (62 before, 61 after payday). Since survey start time relative to payday was available for all these respondents, these missing values for Survey completion time were imputed as = 'Survey start time + Mean(Time taken to do survey, by Before/After Payday group). See footnote 11 for additional details.



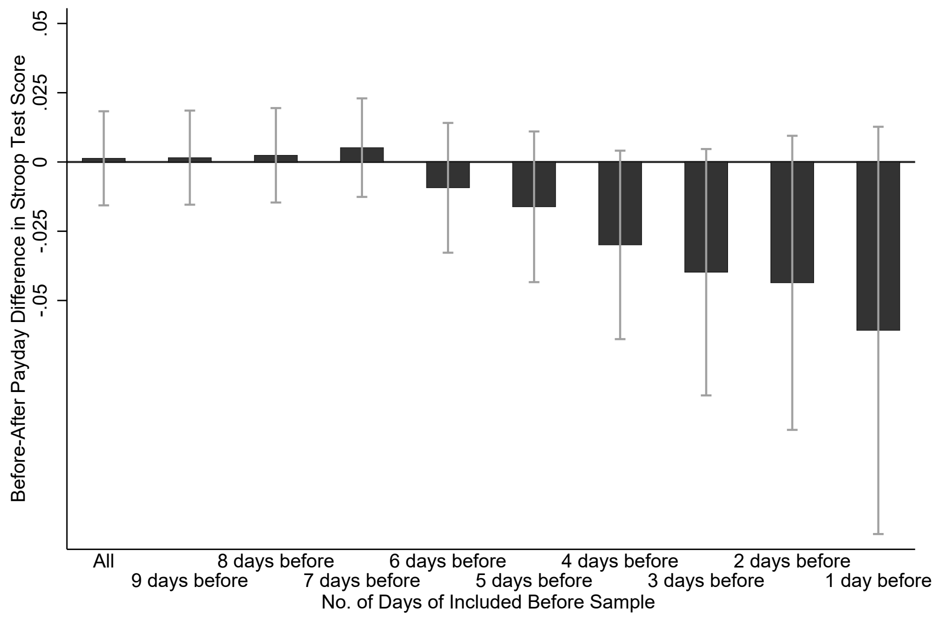
Notes: In the figure, the height of each black bar corresponds to the size of the coefficient from a regression that estimates the effect of being in the before-pay day group on Stroop test performance (percentage of correct answers out of 48); the bands show the 95% confidence intervals around this coefficient. See Appendix Table I, Panel A for the regression table corresponding to the above figure.

FIGURE 3: Cognitive Function, by Revised Distance to Payday



Notes: In Figure 3, the treatment status of 133 respondents with two equal payments in the study period is revised from 'before-payday' (payday = date 2) to 'after-payday' (payday = date 1). See Section 3.3 for details on this revision. In the figure, the height of each black bar is the size of coefficient from a regression that estimates the effect of being in the *revised* before (relative to the 'after') pay day group on Stroop test performance (percentage of correct answers out of 48); the bands show the 95% confidence intervals around this coefficient. See Appendix Table III for regression results underlying the above figure.

FIGURE 4: Cognitive Function, by Distance to Payday – with Controls



Notes: In the figure, the height of each black bar corresponds to the size of the coefficient from a regression that estimates the effect of being in the before-pay day group on Stroop test performance (percentage of correct answers out of 48) after controlling for variables unbalanced across the before vs. after payday samples; the bands show the 95% confidence intervals around this coefficient. See Appendix Table I, Panel B for the regression table corresponding to the above figure and Appendix Table II for the list of unbalanced observables included as controls, in individual time-windows.

Table 1: Cognitive Function (Stroop Score), By Distance To Payday

	Stroop Test Score		
	(1)	(2)	(3)
Before Payday Group - dummy variable	0.004	-0.055	-0.062
	[0.009]	[0.022]	[0.023]
Survey Completion -- Distance to payday (DTP)		-0.007	-0.004
		[0.002]	[0.003]
Before-Payday Group * DTP			-0.005
			[0.005]
Constant	0.799	0.821	0.813
	[0.009]	[0.011]	[0.013]
No. of Respondents	2723	2723	2723
No. of Observations	130038	130038	130038

Notes: Survey Completion-Distance to Payday is the time-gap between when an individual completed the follow-up survey and his/her payday. Stroop test score is the total score of an individual respondent across 48 different Stroop tasks. Standard errors are reported in square brackets.

FOR ONLINE PUBLICATION: APPENDIX TABLES

Table I: Cognitive function (Stroop Score), by Distance to payday

Panel A: No Controls	Stroop Test Score									
No. of Days Before-Payday included	1 day	2 days	3 days	4 days	5 days	6 days	7 days	8 days	9 days	10 da
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10
Before -Payday Group dummy	-0.073	-0.054	-0.048	-0.033	-0.017	-0.011	0.005	0.005	0.004	0.00
	[0.037]	[0.027]	[0.023]	[0.017]	[0.014]	[0.012]	[0.009]	[0.009]	[0.009]	[0.00
Constant	0.797	0.798	0.800	0.801	0.798	0.796	0.799	0.799	0.799	0.79
	[0.011]	[0.011]	[0.011]	[0.010]	[0.010]	[0.010]	[0.009]	[0.009]	[0.009]	[0.00
No. of Respondents in 'Before' Sample	60	111	163	269	420	594	1220	1333	1368	137
No. of Respondents in 'After' Sample	1346	1346	1346	1346	1346	1346	1346	1346	1346	134
No. of Observations	67147	69591	72038	77124	84343	92679	122508	127932	129606	1300

Panel B: Unbalanced Controls	Stroop Test Score									
No. of Days Before-Payday included	1 day	2 days	3 days	4 days	5 days	6 days	7 days	8 days	9 days	10 da
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10
Before -Payday Group dummy	-0.061	-0.044	-0.040	-0.030	-0.016	-0.009	0.005	0.002	0.002	0.00
	[0.037]	[0.027]	[0.023]	[0.017]	[0.014]	[0.012]	[0.009]	[0.009]	[0.009]	[0.00
Constant	0.788	0.836	0.724	0.838	0.787	0.754	0.799	0.814	0.814	0.81
	[0.015]	[0.017]	[0.025]	[0.016]	[0.017]	[0.017]	[0.009]	[0.009]	[0.009]	[0.00
No. of Respondents in 'Before' Sample	60	111	163	269	420	594	1220	1333	1368	137
No. of Respondents in 'After' Sample	1346	1346	1346	1346	1346	1346	1346	1346	1346	134
No. of Observations	67147	69591	72038	77124	84343	92679	122508	127932	129606	1300

Notes: Row 1 of each column is the coefficient from a regression of an individual respondent's Stroop score (on 48 different tasks) on a dummy variable = 1 if (s)he was assigned to the before-payday sample, rather than after. All regressions include the full After Payday sample and the before-payday sample within the *n*-day window prior to payday (*n*=1 to 10). Standard errors are reported in square brackets. See footnote 11 for the exact regression specification. Panel B includes controls for variables that were unbalanced across the included *n*-day before -payday sample versus the full after-payday sample, *n*=1 to 10. See Appendix Table II for the full list of these variables and those that were unbalanced. The set of variables we checked for are those used by CMW for their randomization balance check (Table 1 in Appendix F, CMW).

Table II: Randomization Balance Check, by Distance to payday

Control Variable	Mean After Days in Before Group	After-Before 1 day	p-value	After-Before 2 days	p-value	After-Before 3 days	p-value	After-Before 4 days	p-value	After-Before 5 days	p-value	After-Before 6 days	p-value	After-Bef 7
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Age	56.340	1.206	0.601	1.015	0.557	1.665	0.252	1.793	0.125	1.023	0.293	0.986	0.248	-0.218
Male	0.333	0.066	0.287	0.054	0.249	0.026	0.504	0.021	0.513	0.035	0.179	0.028	0.223	0.011
White	0.744	-0.006	0.913	0.032	0.460	0.075	0.040	0.041	0.163	0.017	0.476	0.001	0.953	-0.024
Black	0.117	0.000	1.000	0.009	0.787	0.006	0.815	0.005	0.811	0.014	0.421	0.012	0.432	0.020
Hispanic	0.088	-0.012	0.742	-0.047	0.095	-0.053	0.027	-0.028	0.154	-0.022	0.178	-0.008	0.557	0.010
Married	0.329	-0.171	0.006	-0.058	0.211	-0.027	0.494	-0.039	0.218	-0.021	0.429	-0.041	0.078	-0.025
Divorced	0.277	-0.023	0.699	-0.011	0.801	0.007	0.847	-0.002	0.955	-0.013	0.595	0.004	0.842	0.002
Widowed	0.137	0.053	0.236	-0.007	0.827	-0.017	0.561	0.014	0.538	0.018	0.352	0.015	0.353	-0.011
Household size	1.938	-0.578	0.000	-0.332	0.005	-0.283	0.005	-0.233	0.004	-0.114	0.092	-0.099	0.096	0.005
Working	0.243	0.043	0.447	0.018	0.675	-0.027	0.450	-0.028	0.324	-0.048	0.051	-0.047	0.030	-0.011
Unemployed	0.062	-0.038	0.233	-0.010	0.663	-0.030	0.138	-0.016	0.317	0.000	0.986	-0.001	0.958	0.007
Retired	0.396	0.079	0.219	0.081	0.094	0.102	0.012	0.110	0.001	0.079	0.003	0.069	0.004	0.003
Disabled	0.189	-0.095	0.069	-0.073	0.063	-0.051	0.123	-0.045	0.086	-0.028	0.207	-0.025	0.200	-0.010
College graduate	0.276	0.109	0.063	0.050	0.252	0.049	0.187	0.015	0.605	0.014	0.581	0.042	0.055	0.017
Some college	0.418	-0.048	0.458	0.004	0.937	0.020	0.634	0.028	0.396	-0.006	0.841	-0.030	0.226	0.005
High school graduate	0.244	-0.039	0.494	-0.026	0.544	-0.019	0.588	-0.020	0.499	0.004	0.869	0.002	0.925	-0.014
Annual household income less than 5k	0.051	0.001	0.986	-0.004	0.871	-0.017	0.359	-0.009	0.546	0.001	0.966	-0.003	0.758	0.001
Annual household income 5-10k	0.097	-0.053	0.183	-0.065	0.030	-0.068	0.007	-0.044	0.032	-0.031	0.068	-0.022	0.140	-0.010
Annual household income 10-15k	0.133	-0.034	0.455	-0.020	0.550	-0.014	0.615	-0.019	0.397	-0.031	0.107	-0.010	0.550	-0.019
Annual household income 15-20k	0.124	0.024	0.579	-0.011	0.735	0.001	0.960	0.001	0.949	0.022	0.231	0.020	0.216	0.021
Annual household income 20-25k	0.145	0.062	0.182	0.028	0.422	0.022	0.445	0.030	0.201	0.007	0.729	-0.008	0.634	-0.004
Annual household income 25-30k	0.144	0.061	0.186	0.063	0.065	0.058	0.042	0.029	0.212	0.023	0.240	0.013	0.454	0.003
Annual household income 30-35k	0.145	-0.088	0.060	-0.053	0.130	-0.033	0.263	-0.015	0.527	-0.010	0.618	-0.003	0.851	0.002
Annual household income 35-40k	0.161	0.028	0.565	0.062	0.083	0.051	0.091	0.027	0.259	0.021	0.307	0.015	0.410	0.006

Notes: This table reports the balance for each of a set of observables, between the before-payday sample included on day *n*(=1 to 10) versus the full after-payday sample. CMW used this set of variables to verify their random assignment of treatment status ex-post (Table 1, Appendix F, CMW). Variables unbalanced within specific time-windows are included as controls in the regression analysis reported in Figure 4 and Appendix Table I, Panel B.

Table III: Cognitive function (Stroop Score), by Distance to payday (Revised Treatment Status)

	Stroop Test Score									
No. of Days Before-Payday included	1 day	2 days	3 days	4 days	5 days	6 days	7 days	8 days	9 days	10 days
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Before -Payday Group dummy	-0.092	-0.074	-0.068	-0.051	-0.037	-0.030	-0.012	-0.012	-0.012	-0.013
	[0.039]	[0.029]	[0.024]	[0.018]	[0.015]	[0.012]	[0.009]	[0.009]	[0.009]	[0.009]
Constant	0.810	0.811	0.812	0.812	0.810	0.807	0.807	0.807	0.807	0.807
	[0.010]	[0.010]	[0.010]	[0.010]	[0.010]	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
No. of Respondents in 'Before' Sample	189	235	283	382	511	663	1230	1333	1368	1377
No. of Respondents in 'After' Sample	1346	1346	1346	1346	1346	1346	1346	1346	1346	1346
No. of Observations	73339	75543	77798	82548	88711	95991	122988	127932	129606	130038

Notes: In the above table, the treatment status of 145 observations was revised from 'before-payday' (payday = date 2) to 'after-payday' (payday = date 1). Each column is the coefficient from a regression of an individual respondent's Stroop score (on 48 different tasks) on a dummy variable = 1 if (s)he was assigned to the before-payday sample, rather than after. All regressions include the full After Payday sample and the before-payday sample within the *n*-day window prior to payday (*n*=1 to 10). Standard errors are reported in square brackets. See footnote 8 for the exact regression specification.