Scarcity and Cognitive Function around Payday: A Conceptual and Empirical Analysis

ANANDI MANI, SENDHIL MULLAINATHAN, ELDAR SHAFIR, AND JIAYING ZHAO

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 article, 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.

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 is 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 mind-set suggests that coping with scarcity in these ways imposes cognitive load and directly impacts cognitive function, which in turn shapes behavior (Mani et al. 2013; Mullainathan and Shafir 2013; see also Cannon, Goldsmith, and Roux 2019; Hamilton et al. 2019). Budgetary concerns and preoccupations under scarcity consume cognitive resources, including at-

tention and executive control and, as a result, elicit a range of counterproductive behaviors such as attentional neglect, forgetting, impulsive spending, anxiety, and poor planning (Shah et al. 2012; Zhao and Tomm 2018; Ong, Theseira, and Ng 2019).

Scarcity mind-sets are exacerbated by the juggling and firefighting pressures inherent to scarcity contexts. A scarcity mind-set 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 is little slack, demand less attention and effort than when income and expenses are misaligned. Mullainathan and 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

Anandi Mani (anandi.mani@bsg.ox.ac.uk) is a professor of behavioral economics and public policy, Blavatnik School of Government, University of Oxford, Oxford, UK. Sendhil Mullainathan (sendhil@chicagobooth.edu) is a Roman Family University Professor of Computational and Behavioral Science, Booth School of Business, University of Chicago, Chicago, IL, USA. Eldar Shafir (shafir@princeton.edu) is a Class of 1987 Professor of Behavioral Science and Public Policy, Department of Psychology and Woodrow Wilson School of Public and International Affairs, Princeton University, Princeton, NJ, USA. Jiaying Zhao (jiayingz@psych.ubc.ca) is an associate professor and Canada Research Chair, Department of Psychology and Institute for Resources, Environment and Sustainability, University of British Columbia, Vancouver BC, Canada.

1. A recent first person-account by a single mother eking out a living cleaning homes in the US captures this well: In her book, Stephanie Land (2019) 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" (Land 2015).

designed to yield fewer unexpected challenges or insurmountable demands.

In this article, 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 consumption smoothing around paydays can lower cognitive load and improve outcomes. For example, a Cheque Day Study in Vancouver, British Columbia, 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 Laing, and Mendell 2019). In a similar study, low-income American workers whose earned income tax credit (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 (Bellisle and 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 6 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, and 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 and Shafir 2013; see also Gennetian and 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 mind-set, 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.

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 feature (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 feature (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 United States, an income shock event that has been widely used to study the behaviors of the poor is the monthly payday that 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 (Foley 2011; Carr and Packham 2019). While both of 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 (Shipler 2005; Danziger and Lin 2009; Gennetian, Darling, and Aber 2016). Among Chicago public middle-school students, a 40% 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 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?

MEASURING SCARCITY EFFECTS USING PAYDAY INCOME SHOCKS: A CASE STUDY

A first step toward testing whether behavior change around paydays can be explained by the scarcity mind-set 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 et al. (2016) as a case study. This paper

examined cognitive effects of poverty around payday, by randomly assigning low-income US 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, Carvalho et al. (2016) 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 Carvalho et al. (2016) chose for their studies fare on the three features described above. On feature (i), that is, the magnitude of the income shock, their chosen payday shocks fare well: Before- and after-payday group respondents in Carvalho et al.'s (2016) studies report significant differences in bank balances and expenditures. However, on feature (ii), that is, the time distance of the shock and the measurement of its impact in relation to other income/expenditure shocks, Carvalho et al.'s (2016) 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 feature (iii), regarding uncertainties in the timing or magnitude of the income shock, Carvalho et al. (2016) 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 Carvalho et al.'s (2016) null result on the cognitive impact of payday shocks. Based on an examination of their study design and data, we suggest that Carvalho et al.'s (2016) null effect may be attributable to a combination of design features (ii) and (iii) above. We discuss each of these features next.

(Time) Distance of Payday Shock from Other Income/Expenditure

Carvalho et al. (2016) report results from two studies with different samples, as described in note 2. Respondents in

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study 1 had up to four paydays within the 1-month study window, of which one was chosen by Carvalho et al. (2016) 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 Carvalho et al. (2016), respondents received up to two payments within the 1-month study window. It is of course plausible that even for an individual who is paid biweekly 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 Carvalho et al. (2016) 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 2 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 reexamine the effects of scarcity on cognitive function in the Carvalho et al. (2016) data by taking into account how far from their payday that respondents took the survey. We then also examine the effects of scarcity on cognitive function in the Carvalho et al. (2016) data, using progressively tighter time windows around payday. For respondents with multiple paydays within the study period, we look at how Carvalho et al.'s (2016) 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.

Payday Cycles and the Timing of Survey Completion

To set the stage for our analysis, we briefly describe some relevant details of the Carvalho et al. (2016) 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 1-month study window.[4] In case of multiple payments in this time period, Carvalho et al.'s (2016) rule for the choice of payday was as follows: "If the largest payment came 2 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 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, Carvalho et al. (2016) 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 7 days before payday for the "before" group and 1 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 four payments in the 1-month study window, and as the authors themselves point out, there are several reasons why the data from study 2 are more reliable.[6]

Participants in study 2 had at most two payments within the study time window (note 12, Carvalho et al.

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3. In study 2, 48.3% of the sample (1,316 out of 2,723 respondents) had two paydays within the 1-month study period: 663 before payday and 653 after.

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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.

5. The actual number of days before payday that the survey became available was higher for some respondents. As the authors wrote in note 11: "In study 2 we could not open surveys during weekends. Therefore, the study 2 follow-up opened 8 (9) days before payday for participants assigned to the before-payday group whose payday fell on a Saturday (Sunday), and 3 (2) 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% of the before-payday group (observations = 109) and on a Friday or Saturday for 26% 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. Also, as Carvalho et al. (2016, 277) note: "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% of participants assigned to the before-payday group started the survey after payday, but in study 2 approximately 2% 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%. Third, there were more trials per participant (20 in study 1 vs 48 in study 2)."

2016). 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 para. 1 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).[7] 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 before or after payday ("distance to payday" [DTP]).[8] 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.

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
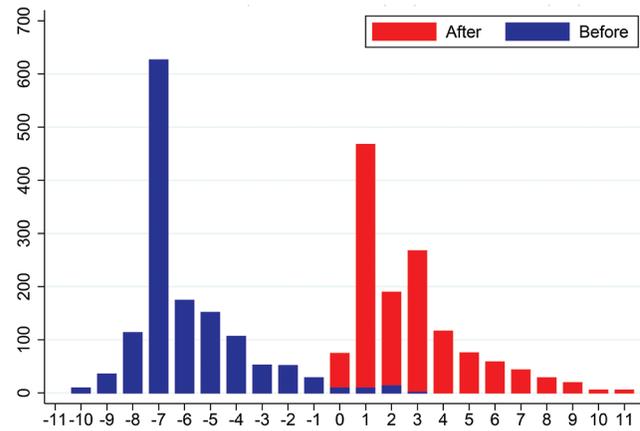
Figure 1. Survey completion day by distance to payday. Data are from Carvalho et al. (2016), study 2. Survey completion time distance to payday (DTP) was missing for 123 respondents (62 before, 61 after payday). Since survey start time relative to payday was available for all these respondents, these missing values for survey completion time were imputed as Survey start time + Mean (Time taken to do survey, by Before/After Payday group). See note 11 for additional details.

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 the section "Payday Assignment Given Multiple Paydays."

Carvalho et al. (2016) use a Stroop test to measure cognitive function. They report regression results on two performance measures, the test score and the time taken.[9] To

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7. 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 the section "Measuring Cognitive Function: Effects of Distance to Payday."

8. 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. In addition, DTP was missing for 123 out of 2,723 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).

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9. 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.

Table 1. Cognitive Function (Stroop Score), by Distance to Payday

	Stroop Test Score		
	(1)	(2)	(3)
Before payday group— dummy variable	.004	−.055	−.062
	[.009]	[.022]	[.023]
Survey completion— distance to payday		−.007	−.004
		[.002]	[.003]
Before-payday group × distance to payday			−.005
			[.005]
Constant	.799	.821	.813
	[.009]	[.011]	[.013]
No. of respondents	2,723	2,723	2,723
No. of observations	130,038	130,038	130,038

Note.—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.

illustrate our point about nuanced research design, we focus on their Stroop test score measure. In table 1, we simply rerun their regression for the test score, controlling for the distance of survey completion time to payday.

Column 1 in table 1 replicates Carvalho et al.'s (2016) original result on the Stroop test score (table 6, col. 4). Column 2 simply adds a control for time DTP, namely, how long from payday respondents took the follow-up survey.[10] As can be seen from a comparison of the two columns, Carvalho et al.'s (2016) 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 DTP. The coefficient on the DTP variable shows that respondents in the "before" group have worse cognitive func-

tion 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.[11]

Next we examine the heterogeneity in the cognitive effects across respondents who completed the follow-up survey at different times. We rerun the same regressions as in table 1 but for progressively more restricted samples covering tighter windows before payday.[12] In individual regressions, the before-payday sample goes from the full sample (i.e., survey completed up to 10 days before payday) to subsamples that completed the survey up to 9 days before, up to 8 days before and so on, up until 1 day before payday (or later). The after-payday group includes the full "after" sample in each regression.[13]

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 versus 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.[14]

The leftmost bar in figure 2 corresponds to the coefficient reported in Carvalho et al. (2016, table 6, col. 4), pooling

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10. This is the DTP variable that fig. 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.

11. The results in table 1 are robust to (i) defining DTP in terms of number of days (i.e., integer values as depicted in fig. 1), rather than the continuous variable available in Carvalho et al.'s (2016) 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 (noncompliers).

12. The regression specification used is the same as in note 11 but for samples in different time windows.

13. 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 fig. 2, except in the 1-day time window around payday, when we have far fewer observations.

14. The corresponding results for the time taken on the Stroop test as well as the underlying regressions for both performance measures are reported in app. table I, panel A.
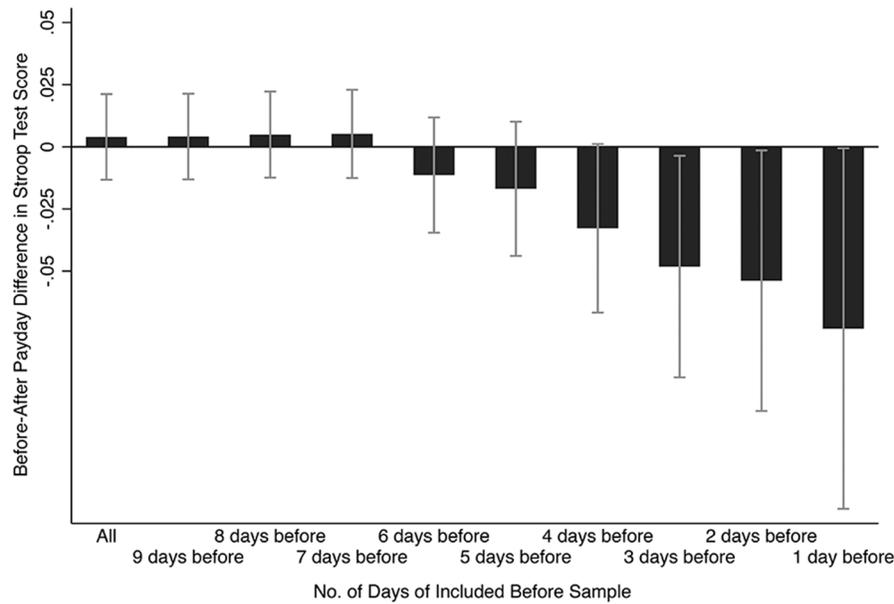
Figure 2. Cognitive function by distance to payday. Height of each black bar corresponds to the size of the coefficient from a regression that estimates the effect of being in the before-pay day group on Stroop test performance (percentage of correct answers out of 48); the bands show the 95% confidence intervals around this coefficient. See app. table I, panel A, for the regression table corresponding to the figure.

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 subsample 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, that is, 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 the section "Endogeneity of Survey Response Time."

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 Carvalho et al.'s (2016) study 2 received two payments within the 4-week study period. For those in this group who had two unequal payments, Carvalho et al. (2016) 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 para. 1 in the section "Using Income Shocks to Measure Scarcity Effects" above).

However, there are several cases with two paydays with equal payments.[15] In these cases, Carvalho et al. (2016) use

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15. In total, 557 respondents had two payments of equal amounts. Of these, 319 were assigned payday = date 2, and 238 were assigned payday = date 1.
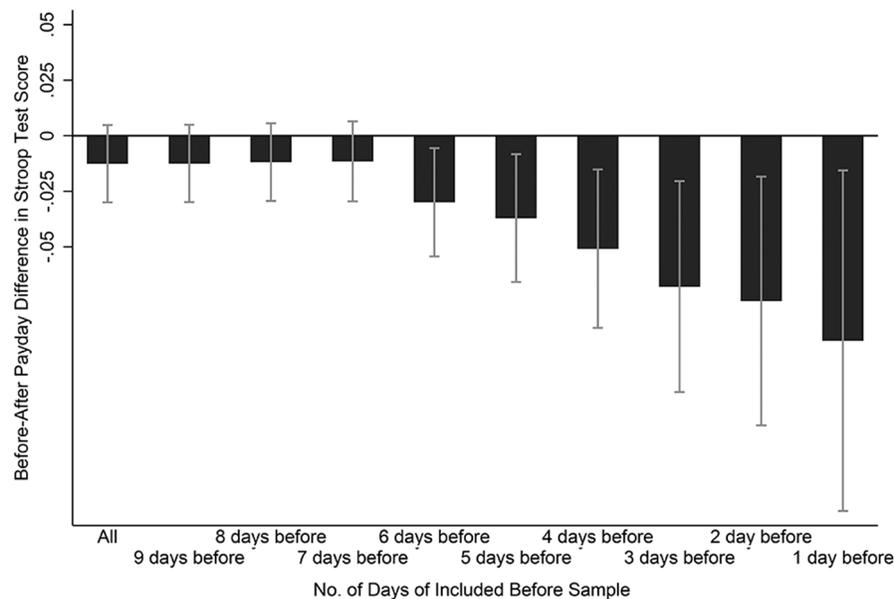
Figure 3. Cognitive function by revised distance to payday. Treatment status of 133 respondents with two equal payments in the study period is revised from "before-payday" (payday = date 2) to "after-payday" (payday = date 1). See the section "Measuring Cognitive Function: Effects of Distance to Payday" for details on this revision. In the figure, 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 app. table III for regression results underlying the above figure.

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).[16]

We find that Carvalho et al.'s (2016) overall result is quite sensitive to this last assignment. We rerun the regressions underlying figure 2 under this revised treatment assignment and report our results in figure 3 below (see app. table III for the regression table results underlying this figure; app. tables I–III are available online).

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.

Endogeneity of Survey Response Time
In order to establish a causal effect of scarcity on the performance gaps seen in figures 2 and 3, participants' own choice of survey response timing should be uncorrelated with the outcome of interest, that is, their cognitive function. This

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16. 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. And 133 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,

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respectively. The results reported in fig. 3 and app. 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.
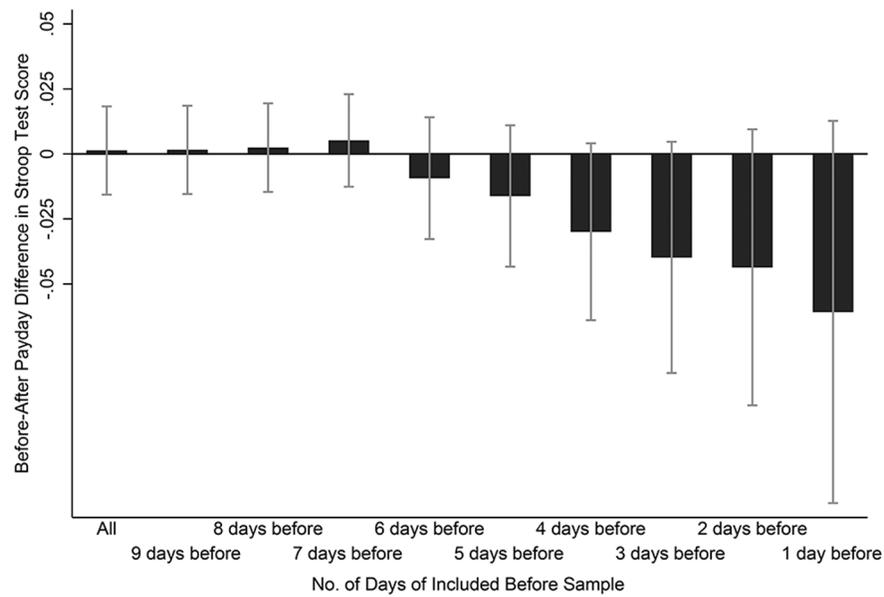
Figure 4. Cognitive function by distance to payday—with controls. Height of each black bar corresponds to the size of the coefficient from a regression that estimates the effect of being in the before-pay day group on Stroop test performance (percentage of correct answers out of 48) after controlling for variables unbalanced across the before vs after payday samples; the bands show the 95% confidence intervals around this coefficient. See app. table I, panel B, for the regression table corresponding to the above figure and app. table II for the list of unbalanced observables included as controls, in individual time windows.

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 Carvalho et al. (2016) study ex post.

In the context of the Carvalho et al. (2016) study, the best we can do ex post is a balance check for the same set of observables that Carvalho et al. (2016) used to verify their randomization, for subsamples in progressively tighter time windows before payday.[17] 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 Carvalho et al. (2016) (and reproduced here in app. table II). Carvalho et al. (2016) 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 Carvalho et al.'s (2016) 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 rerun the regressions underlying figure 2 with controls included for these imbalanced variables. Our revised regression results with these controls are reported in figure 4.

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 app. table I, panel B). Thus, even after accounting for any imbalances on observable characteristics of before versus after payday samples in Carvalho et al.'s (2016) data, the evidence is consistent with scarcity effects resulting in reduced cognitive function before payday, relative to after.

To summarize, our reexamination of Carvalho et al.'s (2016) 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

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17. See table 1, app. F, in Carvalho et al. (2016) for a list of their variables.

lapse of time between payday and other proximate income/expenditure shocks—in measuring scarcity effects.[18]

Uncertainty in Timing and/or Magnitude of Shocks

A final feature of the Carvalho et al. (2016) study noted earlier was the absence of uncertainty: "restricted the sample to participants who provided complete information about the number and dates of payment" (note 13, Carvalho et al. (2016). We highlight this issue for two reasons. First, uncertainty is an important dimension in juggling the challenges of poverty. The absence of uncertainty in Carvalho et al.'s (2016) 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 Carvalho et al. (2016) studies could have further contributed to the null effects that the authors found.[19]

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 counterproductive 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 de-

sign 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.[20]

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% of Kenyan farmers surveyed said that they intended to use fertilizer on their fields the following season, but only 37% actually ended up using fertilizer. One small tweak in service increased fertilizer use by 70%: home delivery of fertilizer (Duflo et al. 2011). Early home delivery amounts to a 10% discount on the market price of fertilizer, but it increases use by as much as a 50% 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 (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% (Banerjee et al. 2010).

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18. 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.

19. 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.

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20. We refer the reader to Datta and Mullainathan (2014) for an extended discussion of these issues.

In Malawi, offering a small incentive of around $0.15 (about 10% of the daily wage) more than doubled the fraction of people who picked up their HIV test result, from 34% to 70% (Thornton 2008). Giving people a larger incentive—of up to $3—had an even larger effect, raising the rate further to over 90%. But the bulk of the jump—from 34% to over 70%—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. 2014). Regarding the challenge of fertilizer use 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.

CONCLUSION

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 and Shea 2001

By way of various analyses conducted throughout this article, 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 data set 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 mind-sets? What details are worth exploring with a finer lens? Carvalho et al.'s (2016) study 2, for example, occurred between November 21 and December 18, a pre-Christmas season known for unusually high stress among people of low income, who feel the pressure to buy family and friends gifts that 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.

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