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Poverty and Economic Decision-Making: Evidence from Changes in Financial Resources at Payday[†]

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We study the effect of financial resources on decision-making. Low-income US households are randomly assigned to receive an online survey before or after payday. The survey collects measures of cognitive function and administers risk and intertemporal choice tasks. The study design generates variation in cash, checking and savings balances, and expenditures. Before-payday participants behave as if they are more present-biased when making intertemporal choices about monetary rewards but not when making intertemporal choices about nonmonetary real-effort tasks. Nor do we find before-after differences in risk-taking, the quality of decision-making, the performance in cognitive function tasks, or in heuristic judgments. (*JEL* C83, D14, D81, D91, I32)

The poor often behave differently from the nonpoor. For example, they are more likely to make use of expensive payday loans and check-cashing services, to play lotteries, and to repeatedly borrow at high interest rates.[1] The debate about the reasons for such differences has a long and contentious history in the social sciences. The two opposing views are that the poor rationally adapt and make optimal decisions for their economic environment or that a "culture of poverty" shapes their preferences and makes them more prone to mistakes.[2] Among economists, this debate has been manifest in lingering questions of whether the poor are more impatient, more risk averse, and have lower self-control, all of which could trap them in

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[1] Rhine, Greene, and Toussaint-Comeau (2006); Ananth, Karlan, and Mullainathan (2007); Haisley, Mostafa, and Loewenstein (2008); Bertrand and Morse (2011); Dobbie and Skiba (2013).

[2] For example, Schultz (1964) and Lewis (1966). See Bertrand, Mullainathan, and Shafir (2004) and Duflo (2006) for more recent perspectives.

a cycle of poverty.[3] A third view emerges from the work of Mullainathan, Shafir, and co-authors.[4] They argue that scarcity, defined as "having less than you feel you need" (Mullainathan and Shafir 2013, p. 4), impedes cognitive functioning, which in turn may lead to decision-making errors and myopic behavior.[5]

There are major challenges in isolating the causal effects of economic circumstances on decision-making empirically. There may not only be a reverse causality bias—that is, the economic decisions one makes determine one's economic circumstances—but also be unobserved individual characteristics, such as cognitive ability, confounding the relationship between economic circumstances and decision-making. Further complicating identification of the effects of poverty on time preferences is the possibility that poverty may affect credit constraints and arbitrage opportunities, which in turn could influence intertemporal choices (e.g., Frederick, Loewenstein, and O'Donoghue 2002).

Previous work already has documented that expenditures and the caloric intake of some households increase sharply at payday (e.g., Stephens 2003, 2006; Huffman and Barenstein 2005; Shapiro 2005; Mastrobuoni and Weinberg 2009). This paper uses changes in financial resources at payday to empirically investigate whether financial resources have a *causal* effect on economic decision-making.

To exploit the sharp change in financial resources at payday, we designed and administered online surveys in which 3,821 participants with annual household income below $40,000 were randomly assigned to a group that was surveyed shortly *before* payday—henceforth, the before-payday group—or a group surveyed shortly *after* payday—henceforth, the after-payday group. Then we collected measures of cognitive function and administered incentivized risk choice and (monetary and nonmonetary) intertemporal choice tasks. Our goal was to investigate whether the before-payday group would behave differently from the after-payday group.

As in previous related experimental studies (e.g., Spears 2011; Mani et al. 2013), the variation in financial resources that we use to identify our effects is temporary, anticipated, and perhaps equally important, is anticipated to be temporary.[6] The participants we surveyed before payday knew when their next payment would arrive and when more money would come to them. Thus, our study speaks to the effects of sharp but short-lived variations in financial resources. It is this particular impoverishment before payday that we allude to when we refer to "poverty." It is still an open question whether our findings generalize to similar effects for a permanent shift in permanent income.

Our results contribute to at least two important strands of literature: first, they provide some insights on the causal effects of poverty on time and risk preferences. We find that the before-payday group behaved as if they were more present-biased when making intertemporal choices about monetary rewards. Conceptually,

[3] Lawrance (1991); Banerjee and Mullainathan (2010); Tanaka, Camerer, and Nguyen (2010); Spears (2011); Gloede, Menkhoff, and Waibel (2015); Bernheim, Ray, and Yeltekin (2015); Carvalho (2013); Haushofer, Schunk, and Fehr (2013).

[4] Shah, Mullainathan, and Shafir (2012); Mullainathan and Shafir (2013); Mani et al. (2013).

[5] A number of studies document an association between cognitive ability and economic choices (e.g., Burks et al. 2009; Dohmen et al. 2010; Benjamin, Brown, and Shapiro 2013).

[6] Mani et al. (2013) exploit the discontinuity in financial resources at harvest for Indian sugarcane farmers. They interviewed farmers pre-harvest in July and August and interviewed them again post-harvest in September and October.

what appears to be the effect of poverty on present-biased preferences could be alternatively attributed to differences in (lack of) attention to the future (Karlan et al. forthcoming) or to liquidity constraints (Dean and Sautmann 2015; Ambrus et al. 2015; Epper 2015). Our results *suggest* the latter: that liquidity constraints explain why the before-payday group behaved as if they were more present-biased. Our evidence also shows that the before-payday and after-payday groups make similar risk choices, suggesting that economic circumstances do not affect the willingness to take risks.

Second, our findings contribute to the debate on poverty and decision-making (e.g., Spears 2011; Mullainathan and Shafir 2013; Haushofer and Fehr 2014), but do not support the hypothesis that financial strain per se impedes cognitive function and worsens the quality of decision-making. We find that participants surveyed before and after payday performed similarly on a number of cognitive function tasks. Furthermore, we find no difference in the likelihood of heuristic judgment, and no significant difference between the two groups in the quality of the decision-making as measured by the consistency of intertemporal and risk choices with rationality.

The paper is structured as follows. Section I discusses the study design; Section II presents the results, and is followed by a concluding discussion.

I. Study Design

We collected data using 2 ongoing Internet panels with respondents aged 18 and over living in the United States. Study 1 was conducted with members of the RAND American Life Panel (ALP) between November 2012 and March 2013.[7] Study 2 was conducted with members of the GfK KnowledgePanel (KP) between November and December of 2014.[8] As we discuss in more detail below, we ran Study 2 to investigate if some results from Study 1 could be replicated in a different Internet panel and with a larger sample size.

A key feature of these two Internet panels is that they contain a large number of low-to-moderate income members, which allowed us to restrict our study samples to respondents with an annual household income of $40,000 or less.[9] Forty-five percent of the Study 1 sample and 41 percent of the Study 2 sample had an annual family income below $20,000. Other results indicate that both samples had participants with low socioeconomic status: 45 percent in Study 1 and 50 percent in Study 2 had zero or negative nonhousing wealth; one-fifth reported being disabled; and fewer than 40 percent were working. Finally, because of a shortage of money, 51 percent of the Study 1 sample and 40 percent of the Study 2 sample had experienced (at least) 1 of the following in the 12 months before the studies: could not pay electricity, gas, or phone bills; could not pay for car registration or insurance; pawned or sold something; went without meals; were unable to heat home; sought assistance from welfare or community organizations; sought assistance from friends

[7] https://mmicdata.rand.org/alp/.

[8] http://www.gfk.com/us/Solutions/consumer-panels/Pages/GfK-KnowledgePanel.aspx.

[9] In both panels, respondents without Internet access at the time of recruitment are provided computers and an Internet access subscription, thus permitting the recruitment of poor households without Internet access that may have not been able to participate otherwise.

or family; or took a payday loan. (See online Appendix C for more details about the socioeconomic status of the sample.)

Both Study 1 and Study 2 consisted of one baseline and one follow-up survey. The baseline surveys collected information that was used to determine participants' paydays. The opening dates of the follow-up surveys, which were specific to each study participant, depended on the participant's payday and her random assignment.[10] Specifically, the follow-up surveys opened seven days before payday for participants assigned to the before-payday group and one day after payday for participants assigned to the after-payday group.[11] Participants were sent e-mails informing them when the survey was available. The follow-up surveys measured various aspects of decision-making for the two randomly assigned groups.

A. *The Baseline Survey and Study Sample*

The baseline surveys collected data on the dates and amounts of all payments that the participant (and his/her spouse) expected to receive during a reference period: January 2013 for Study 1 and from November 21, 2014 to December 20, 2014 for Study 2.[12] (See online Appendix A for screenshots of the baseline survey.) The study then focused on subjects who provided complete information about the number and dates of payments.[13] (See online Appendix D for more details about the payments.)

These data were then used to identify the payday of each participant. 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 other payments. Participants whose payments were all less than 2 weeks apart were dropped from the study sample. (See online Appendix E, which gives details about sample restrictions and survey nonresponse, for the flow of participants through the study.)

The baseline survey also collected information used to identify subgroups of participants whose financial circumstances we would expect to change more sharply at payday, namely (i) participants who had experienced financial hardship (e.g., could not pay bills); (ii) participants who reported living from paycheck to paycheck; (iii) participants who were forced to reduce their food consumption because they ran out of money (only in Study 2); and (iv) participants who could not, or would have to do something drastic to, raise $2,000 in a week for an emergency (only in Study 2).

[10] Spears (2012) used a similar design: recipients of South Africa's old age pension were randomly assigned to be surveyed before or after receiving the monthly pension payment as a means of studying cognitive limits and intertemporal choices.

[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).

[12] To test the survey design, we conducted a pilot in May of 2010 with about 200 respondents; we randomly assigned whether a participant was surveyed before or after payday.

[13] In Study 1 we dropped from the sample participants who reported that they expected five or more payments (from all sources). In Study 2 we were more restrictive; we dropped from the sample participants who expected to receive payments in three or more different dates during the reference period. The rationale for dropping these participants is that their income should be spread out sufficiently over time, making it easier for them to smooth consumption. In both studies we restricted the sample to participants who provided complete information about the number and dates of payments.

In addition, we look at participants who received only one payment per month, who would likely have a harder time in smoothing consumption, as well as participants with an annual household income of $20,000 or less (Mani et al. 2013 find effects of scarcity on the cognitive function of shoppers at a New Jersey mall, all of whom had an annual household income of $20,000 or more).

B. *Randomization and Treatment Compliance*

The study participants then were randomly assigned to the before-payday group or the after-payday group using a stratified sampling and re-randomization procedure (see online Appendix F for more details).[14] The randomization was successful in making assignment to the before-payday group orthogonal to observable baseline characteristics (see online Appendix F).

The study design generated variation in the time participants started and completed the survey. In Study 1, the median respondent assigned to the *before*-payday group started the survey 2.4 days *before* payday and completed it 1.5 days *before* payday. The median respondent assigned to the *after*-payday group started the survey 4.4 days *after* payday and completed it 5 days *after* payday.[15] In Study 2, the median respondent assigned to the *before*-payday group started and completed the survey 6.3 days *before* payday. The median respondent assigned to the *after*-payday group started the survey 2.6 days *after* payday and completed it 2.7 days *after* payday. The differences between the two groups all were statistically significant at 1 percent.

Note that although the study design allowed us to manipulate when the follow-up survey was made available to a participant, we could not control when the participant started the survey. Thus, we expected there to be imperfect compliance, in the sense that some of the participants assigned to the before-payday group could effectively start (or finish) the follow-up survey after payday. In practice, about 70 percent of the Study 1 participants assigned to the before-payday group started the survey before payday, while 63 percent completed the survey before payday.[16]

Study 2 was designed with several procedures in place to achieve a higher compliance rate than in Study 1.[17] Approximately 98 percent of the participants assigned to

[14] In both Study 1 and Study 2 we stratified on how strongly participants agreed with the statement "I live from paycheck to paycheck" and on whether they anticipated receiving only one payment during the reference period because we planned to check whether the effects would be any different for those participants whose economic circumstances could be expected to change more sharply at payday. In Study 1 we also stratified on whether the respondent had a college education and on whether the survey would open before December 31, 2012 if the respondent were assigned to the before-payday group. In Study 2 we additionally stratified on whether participants had an annual household income of $20,000 or less, on how hard it would be for them to raise $2,000 in a week for an emergency, and on how strongly they agreed with the statement "Money starts to run out before the next payment arrives and we are forced to cut the size of meals, skip meals, or eat more low cost foods to make ends meet."

[15] Because participants were not required to complete the survey in one sitting, the time between when they started the survey and when they completed it may have been much longer than the time it would take to effectively complete the survey without interruption.

[16] Results are similar if the sample is restricted to participants who started the follow-up survey within 7 days of its opening (results available upon request).

[17] The mechanisms were: (i) the baseline survey stayed in the field for only seven days in order to recruit participants who would be more likely to also answer the follow-up survey within seven days; (ii) we provided (delayed) monetary incentives for answering the follow-up survey within seven days; and (iii) the follow-up survey remained in the field for only ten days. Importantly, participants were informed that the "compliance incentives" would be paid on February 3, 2015, more than a month after the final close date of the follow-up survey. In addition, we

the before-payday group started and completed the Study 2 follow-up survey before payday.

In our analysis, we estimate intention-to-treat (ITT) effects, exploiting the random assignment to the before-payday group as a source of exogenous variation in starting the survey before payday. However, the ITT estimates are biased toward zero because of the imperfect compliance. To address this issue, Table 7 presents 2SLS estimates of the causal relationship between economic circumstances and decision-making: we use the random assignment to the before-payday group to instrument for economic circumstances.

C. *The Follow-Up Surveys*

The follow-up surveys collected measures of economic decision-making, cognitive function, and financial circumstances. We discuss them here briefly; for more details and screenshots of the follow-up survey, see online Appendix B.

Economic Decision-Making.—Two intertemporal choice tasks—one with monetary rewards and one with nonmonetary rewards—and a risk choice task were administered in Study 1. In the monetary intertemporal choice task, a variant of Andreoni and Sprenger's (2012) convex time budget (CTB), participants were asked to allocate an experimental budget of \$500 into two payments with pre-specified dates, the second of which included interest. Participants had to make 12 of these choices in which the experimental interest rate varied (0 percent, 0.5 percent, 1 percent, or 3 percent), as did the mailing date of the first payment (either today or 4 weeks from now) and the time delay between the 2 payments (4 weeks or 8 weeks). Approximately 1 percent of the participants were randomly selected to be paid based on 1 of their 12 choices.

Study 1 participants also were asked to make intertemporal choices regarding real effort (similar to Augenblick, Niederle, and Sprenger 2015) in order to address concerns about the use of monetary rewards in measuring time discounting (e.g., Frederick, Loewenstein, and O'Donoghue 2002). Specifically, participants had to choose between completing a shorter survey within 5 days or a longer (30 minute) survey within 35 days. They were asked to make five such choices, with the length of the earlier survey gradually increasing (from 15 to 18, 21, 24, and 27 minutes). Five similar choices followed, in which the deadlines were shifted from 5 to 90 days (shorter) and 35 to 120 days (longer). Approximately 1 percent of the participants were randomly selected to have one of their ten choices implemented (i.e., "implementation surveys" were sent to those selected participants).[18]

To analyze their willingness to take risks, the Study 1 participants were presented a risk choice task designed by Eckel and Grossman (2002). Here, participants were

randomized the "compliance incentive" to be either \$2 or \$8, and half of participants were given the opportunity to make a pledge (at the end of the baseline survey) that they would answer the follow-up survey within seven days. We planned to use these manipulations as instruments for selection into compliance. The compliance rate turned out to be so high (~98 percent) that we have not had to use them.

[18] If they completed the survey before the deadline, they received a \$50 Amazon gift card and \$20 was added to the quarterly check they regularly received for answering surveys. The dates of these payments were fixed and thus did not depend on when respondents finished the implementation surveys (as long as they were completed before the deadline).

asked to choose one of six lotteries, each with a 50-50 chance of paying a lower or a higher reward. The six (higher/lower) pairings were (\$28/\$28), (\$36/\$24), (\$44/\$20), (\$52/\$16), (\$60/\$12), and (\$72/\$0). Approximately 10 percent of participants were randomly selected to actually be paid according to their choices.[19]

In Study 2, we measured the willingness to take risks using the risk choice task from Choi et al. (2014).[20] Here participants were asked to invest an experimental endowment in two securities whose payouts depend on the outcome of a coin toss. In practice, the participants were asked to choose a point along a budget constraint, where the y-axis corresponds to the payoff if the coin comes up heads and the x-axis to the payoff if the coin comes up tails. Each participant was shown 25 budget lines where we varied the experimental endowment and the relative price of the assets. The within-subject variation in choices across the budget lines provided us with measures of quality of decision-making, which is explained in more details in Section IIB3. Ten percent of participants were randomly selected to be paid based on one of their 25 choices.

The two intertemporal choice tasks administered in Study 1 provide additional measures of the quality of decision-making. In the task with monetary rewards, the assumptions of additive separability and monotonicity predict that the later payment should increase with the experimental interest rate (Giné et al. 2014). In the task with nonmonetary rewards, where we used a multiple price list, we could investigate whether participants have at most one switching point (Burks et al. 2009).

Cognitive Function.—To measure cognitive function, we used the Flanker task, a working memory task, and the cognitive reflection test (CRT) in Study 1 and the numerical Stroop task in Study 2. In the Flanker task, a well-established inhibitory control task that is part of the NIH toolbox (Zelazo et al. 2013), subjects are supposed to focus on a central stimulus while trying to ignore distracting stimuli (Ericksen and Ericksen 1974). In the working memory task, participants are asked to recall a sequence of colors; the length of the sequence gradually increases if the participant can successfully repeat a given sequence. The CRT measures one's ability to suppress an intuitive and spontaneous incorrect answer and instead to give the deliberative and reflective correct answer (Frederick 2005).[21] In addition to these tests of cognitive function, we have (for Study 1 only) other measures of participants' cognitive abilities, including fluid and crystallized intelligence, which were collected in previous ALP surveys. Table I2 in online Appendix I shows that our measures of cognitive function are strongly correlated with these other measures of cognitive ability.

In Study 2, we administered a web version of the numerical Stroop task used in Mani et al. (2013) to measure cognitive control. In the numerical Stroop participants are presented with a number, e.g., 888, where a digit is repeated a number of times.

[19] Two additional tasks in Study 1 measured loss aversion, as in Fehr and Goette (2007), and simplicity seeking, as in Iyengar and Kamenica (2010). The latter task was incentivized; the former was not.

[20] Because of budget constraints this task was administered to 45 percent of the Study 2 sample.

[21] We also included in Study 1 two items to measure the use of heuristics. One question from Toplak, West, and Stanovich (2011) captures whether the respondent believes in the gambler's fallacy: that is, the incorrect expectation that after one particular realization of a random variable the next realization of this same random variable will be different. Sensitivity to framing was measured using the "disease problem" proposed by Tversky and Kahneman (1981).

The participant must identify the number of times the digit is repeated, i.e., three, rather than name the digit itself. Mani et al. (2013) conducted 72 trials with some repeats; of those, we selected a subset with 48 trials by excluding repeats.

Financial Circumstances.—Both follow-up surveys included questions on cash holdings, checking and savings accounts balances, and expenditures, which allow us to check if the study design generated variation in financial circumstances.

II. Results

Section IIA shows that the study design generated substantial differences in the financial resources of the before-payday and after-payday groups. We then examine whether these differences in financial resources were accompanied by differences in economic choices (Section IIB) and in cognitive functions (Section IIC).

A. *Financial Circumstances*

Table 1 presents OLS and median regressions, where a measure of financial circumstances—either cash holdings, checking and savings balances, or total expenditures in the last seven days—is regressed on an indicator variable for being randomly assigned to the before-payday group and a constant. The coefficient on the constant gives the mean or median for the after-payday group.

The results in Table 1 indicate that the before-payday group had fewer financial resources than the after-payday group: the before-payday group's median cash holdings was 22 percent (Study 1) and 14 percent (Study 2) lower than that of the after-payday group, and they typically had 31 percent (Study 1) and 33 percent (Study 2) less in their checking and savings accounts. The median expenditures of the before-payday group were also 20 percent (Study 1) and 33 percent (Study 2) lower than those of the after-payday group.[22]

These findings are consistent with well-documented results that total expenditures and food expenditures increase sharply at payday (e.g., Stephens 2003, 2006).[23] In Study 1, we find that median grocery expenditures were 11 percent lower before payday than after payday (we did not collect data on grocery expenditures in Study 2).[24]

Previous work also has documented that caloric intake decreases over the pay cycle (e.g., Shapiro 2005; Mastrobuoni and Weinberg 2009), which cannot be

[22] The before-after differences in cash-on-hand (i.e., cash + checking and savings) can be compared to the dollar amount of the payments. The median amount of the payment expected to be received at payday was $800 (Study 1) and $1,054 (Study 2). The median amount of all payments expected to be received during the reference period was $1,379 (Study 1) and $1,500 (Study 2).

[23] Hastings and Washington (2010) find that food prices are higher in the beginning of the month, which puts into question the hypothesis that households make one trip to the grocery store and then store food to be consumed over the month.

[24] We administered questions about purchases of durables to 45 percent of the Study 2 sample. Fewer than 10 percent of those surveyed bought one of the durables listed. The before-after payday difference in purchase of durables is too small to explain the before-after difference in total expenditures that we find. If anything, the results show that the before-after difference in total expenditures is larger when we exclude participants who purchased durable goods. See Tables G5 amd G6 in online Appendix G.

Table 1—Cash, Checking and Savings Balances, and Total Expenditures

	Cash		Checking and savings		Total expenditures	
	Study 1	Study 2	Study 1	Study 2	Study 1	Study 2
OLS						
{Before payday}	−$114	−$40	−$1,947	−$6,346	−$553	−$703
	[52]**	[72]	[1,859]	[4,732]	[328]*	[363]*
Constant	$217	$286	$6,626	$15,683	$1,156	$1,435
	[49]***	[53]***	[1,495]***	[4,652]***	[326]***	[356]***
Median regression						
{Before payday}	−$10	−$7	−$230	−$500	−$100	−$200
	[4]**	[4]*	[100]**	[142]***	[36]***	[28]***
Constant	$45	$50	$730	$1,500	$500	$600
	[3]***	[3]***	[72]***	[101]***	[25]***	[20]***
p-value Wilcoxon test equality of distributions						
	0.02	0.00	0.04	0.00	0.01	0.00
Observations	1,054	2,497	851	2,290	1,056	2,496

Notes: This table reports results from OLS and quantile regressions (quantile 0.5) of the dependent variables shown in the column headings on an indicator variable identifying participants assigned to the before-payday group and a constant. Robust standard errors in brackets. The last panel shows the *p*-value of a Wilcoxon rank-sum test. The checking and savings results exclude respondents who did not have a checking or savings account. Indicator variables are in curly brackets.

explained by bills coinciding with payday.[25] These studies used extensive food diaries to measure caloric intake accurately. Unfortunately, we could not afford to use food diaries, so instead, in Study 2, we measured food consumption by asking participants about the number of portions they had eaten in the previous 24 hours of the following 9 items: fresh fruits, fried potatoes, fresh vegetables, soda, fast food, desserts, any type of meat, any type of seafood, and alcohol. The point estimates indicate that the before-payday group consumed less of six of these items—fresh fruits, fresh vegetables, desserts, meat, seafood, and alcohol—than the after-payday group; however none of the differences are statistically significant.[26]

While all of the individuals in our two samples are relatively poor, we can focus on particular subgroups whose financial circumstances we would expect to change more sharply at payday (see Section IA). Similar to Mastrobuoni and Weinberg (2009), in Table 2 we document that for these subgroups, median expenditures are substantially lower before payday than after payday. For example, median expenditures were 50 percent lower before payday than after payday for the caloric crunch and the liquidity-constrained subgroups.

In sum, at the time of the follow-up surveys, the financial circumstances of the two groups were substantially different. In what follows, we investigate whether having fewer financial resources affected the decision-making and behavior of the before-payday group. First we present the results for the overall sample. In Figure 2

[25] Although Gelman et al. (2014) find that the excess sensitivity of spending is partly explained by the coincident timing of regular income and regular spending for their sample as a whole, they also show that, for individuals with low liquidity, there is substantial excessive sensitivity of nonrecurring spending.

[26] Mani et al. (2013, p. 979) find that "pre-harvest farmers were not eating less" than the post-harvest farmers, and report that "the Stroop results persist even in regressions in which food consumption is included as a control variable."

TABLE 2—EXPENDITURES FOR MORE FINANCIALLY STRAINED SUBGROUPS

	Median regression Total expenditures last 7 days	
	Study 1	Study 2
One payment		
{Before payday}	−\$200***	−\$300***
Constant	\$500***	\$650***
Observations	423	1,285
Financial hardship		
{Before payday}	−\$200***	−\$300***
Constant	\$600***	\$700***
Observations	547	994
I live from paycheck to paycheck		
{Before payday}	−\$150***	−\$290***
Constant	\$550***	\$690***
Observations	557	1,191
Annual household income ≤ \$20,000		
{Before payday}	−\$100*	−\$248***
Constant	\$400***	\$548***
Observations	470	1,011
Caloric crunch		
{Before payday}	—	−\$345***
Constant		\$695***
Observations		1,159
Could not raise \$2,000 for emergency		
{Before payday}	—	−\$345***
Constant		\$695***
Observations		1,240

Notes: This table reports estimated coefficients from quantile regressions (quantile 0.5) of total expenditures on an indicator for the before-payday group and a constant. The 6 measures of financial strain are: (i) receiving 1 payment per month; (ii) having experienced a financial hardship in the previous 12 months; (iii) agreeing or strongly agreeing with the statement "I live from paycheck to paycheck"; (iv) having an annual household income of \$20,000 or less; (v) being forced to reduce consumption at the end of pay cycle; or (vi) not being able to, or having to do something drastic to, raise \$2,000 in one week for an emergency. Standard errors are not reported (available upon request). Indicator variables are in curly brackets.

and online Appendix H we show that these results also hold if the sample is restricted to "more strained" subgroups of participants, those whose financial circumstances have been shown to change more sharply at payday.

B. *Economic Decision-Making*

This section investigates whether the before-payday and after-payday groups make different intertemporal (B1) and risk choices (B2). Subsection B3 examines the quality of those decisions (B3) in terms of their consistency with rationality.

B1 *Intertemporal Choices*.—Economists have long debated whether the poor have higher discount rates (e.g., Lawrance 1991; Carvalho 2013; Haushofer, Schunk, and Fehr 2013; Meier and Sprenger 2015). Because the poor are more likely to be liquidity constrained, it is particularly challenging to test this hypothesis (Pender 1996). If an individual cannot borrow against future income, then her marginal utility of $1 today may be higher than her marginal utility of $1 in the future, which could be confounded with a high discount rate (e.g., Frederick, Loewenstein, and O'Donoghue 2002; Stahl 2013; Dean and Sautmann 2015). Some recent models also predict that the positive expectation of future income (in our case the payment expected to be received at payday) or liquidity constraints can lead to intertemporal choices that may look like present-biased choices (Ambrus et al. 2015; Epper 2015).

Table 3 presents the results from the two intertemporal choice tasks. First we look at the CTB task, where participants had to make intertemporal choices about money, as presented in the column "Monetary" of Table 3.

The results show that the before-payday group behaved as if they were more present-biased when making intertemporal choices about monetary rewards. The before-payday group increased the amount of the "sooner check" by $10.60 in response to the change in the sooner date from four weeks from today to today. This difference is statistically significant at the 1 percent level.

In contrast, there is no evidence that the before-payday group had a higher (exponential) discount rate or a higher intertemporal elasticity of substitution than the after-payday group. These differences are not statistically significant and not economically meaningful. In online Appendix J, we show the distribution of choices and we use the CTB framework to estimate utility-function parameters that better quantify the differences in behavior across the two groups. We are able to rule out that the before-after (absolute) difference in the utility curvature parameter was greater than 0.003.[27] Discount rates are less precisely estimated, but we can still rule out the before-payday group having an annual discount rate 1.05 percentage points higher than the after-payday group.[28] The finding that the before-payday group was as responsive to changes in the experimental interest rate as the after-payday group does not support the view that scarcity leads the poor to pay less attention or to neglect the future.

Although the result that the before-payday group behaved as if it were more present-biased is consistent with the interpretation that scarcity reduces self-control, such behavior can also be explained by before-after differences in liquidity constraints (Dean and Sautmann 2015; Epper 2015). Consistent with these theories, our results show that individuals who did not have a credit card—and presumably were more likely to be liquidity constrained—behaved as if they were more present-biased before payday than after payday; this relationship is not observed for individuals with a credit card.[29]

Another way to disentangle the effects of economic circumstances on time preferences from liquidity constraints is to look at nonmonetary intertemporal choices.

[27] The CTB framework assumes constant relative risk aversion (CRRA) preferences, where $u(c) = c^{\alpha}/\alpha$ and α is the utility curvature parameter. However, this calculation is sensitive to the particular assumptions about background consumption.

[28] The after-payday group is estimated to have an annual discount rate of 9.4 percent.

[29] Table I1 in online Appendix I shows that the coefficient on {Before payday} $\times$ {Immediate Rewards} is significant for those who do not have a credit card but it is half as large and not significant for those with a credit card. The difference across the two groups is not statistically significant.

Table 3—Intertemporal Choices

	Monetary $ amount sooner reward	Nonmonetary monthly discount rate
{Before payday} × {Immediate rewards/task}	10.6	−0.03
	[3.83]***	[0.025]
{Before payday} × Interest rate	2.7	—
	[3.24]	
{Before payday} × Delay time	−1.4	—
	[1.06]	
{Before payday}	−6.3	0.02
	[9.80]	[0.027]
{Immediate rewards/task}	−5.3	0.09
	[2.76]*	[0.018]***
Interest rate	−47.3	—
	[2.34]***	
Delay time	−0.7	—
	0.72	
Constant	304.3	0.31
	[6.83]***	[0.019]***
Observations	12,720	2,050
Choices	12,720	10,250
Subjects	1,060	1,025

Notes: Monetary column reports results from an OLS regression where the dependent variable is the dollar amount of the sooner payment. Immediate rewards is an indicator variable that is 1 if the mailing date of the sooner payment is today. Delay time is the interval between the sooner and later payments. The sample is restricted to the 1,060 subjects who made all 12 choices in the task with monetary rewards. Nonmonetary column reports estimates from an interval regression where the dependent variable is the interval measure of the individual discount rate (IDR). Two IDRs are estimated for each subject; 1 for each time frame. Immediate task is an indicator variable for the "5 days (sooner) × 35 days (later)" time frame. Standard errors clustered at the individual level. The sample is restricted to the 1,025 subjects who made all 10 choices in the nonmonetary intertemporal task. Indicator variables are in curly brackets.

Augenblick, Niederle, and Sprenger (2015) argue that intertemporal choices about real effort are better suited than intertemporal choices about monetary rewards to capturing dynamic time-inconsistent preferences, because the latter are subject to several confounds. In the nonmonetary column of Table 3 we present subjects' intertemporal choices between a shorter survey earlier and a longer survey later. We estimate an interval regression where the dependent variable is a measure of individual discount rate (as in Meier and Sprenger 2015).[30]

We find that the two groups made similar intertemporal choices about a costly real-effort task: choosing between answering a shorter survey earlier and a longer survey later. In line with Augenblick, Niederle, and Sprenger (2015), both groups' behavior was *consistent* with present bias: the implied monthly discount rate was

[30]Let x be the duration of the longest sooner survey the subject chose over the later survey. The (lower bound, upper bound) of the discount rate intervals were: (15/30,18/30) for $x = 15$; (18/30,21/30) for $x = 18$; (21/30,24/30) for $x = 21$; (24/30,27/30) for $x = 24$, and (27/30,1) for $x = 27$. The interval was (0,15/30) for those who always chose the later survey.

9 percentage points higher when the shorter-sooner survey had to be completed within 5 days (as opposed to 90 days).

However, there was no evidence of differential present bias between the before- and after-payday group in the task when participants had to make intertemporal choices about real effort.[31] Although one should be cautious in comparing intertemporal choices about monetary rewards to intertemporal choices about real effort (because discount rates may be domain-specific: see, e.g., Reuben, Sapienza, and Zingales 2010; Ubfal 2016), this result *suggests* that liquidity constraints may explain why the before-payday group behaved as if it was more present-biased in the monetary intertemporal choice task.

B2 *Risk Choices*.—Next we use data from two different risk choice tasks conducted in Study 1 and Study 2, respectively, to investigate whether risk choices are affected by financial circumstances. The change in financial resources at payday could affect the willingness to take risks in two ways. First, liquidity constraints could increase the marginal utility of consumption and reduce the willingness to take risks. Second, scarcity could have a direct effect on risk preferences per se (e.g., Tanaka 2010; Gloede, Menkhoff, and Waibel 2015). These effects could partly offset each other if scarcity *reduced* risk aversion.[32]

As Table 4 shows, the before-payday and after-payday groups made similar risk choices in Study 1 and Study 2.[33] The before-payday group behaved as if they were less risk averse, but the differences are small and not statistically significant. Both groups also made similar choices in two additional risk-related choice tasks: there were no before-after differences in either the loss aversion or the simplicity-seeking experimental tasks (see online Appendix G1). Overall, these results indicate that financial circumstances do not affect risk choices.

B3 *Quality of Decision-Making*.—The intertemporal and risk choice tasks also permit investigating if there are differences in the quality of the decision-making of the before-payday and after-payday groups. In the task with monetary rewards (i.e., CTB), the assumptions of additive separability and monotonicity allow for a strong prediction: the amount allocated to the later payment should increase with the experimental interest rate. Following Giné et al. (2014), we measure consistency as the fraction of times in which subjects increased (or kept constant) the later reward in response to an increase in the experimental interest rate.[34] In the task with

[31] Even though one could worry that the before-payday and after-payday groups may have had different time constraints, we find no evidence to support that hypothesis. For example, there is no statistically significant difference in how much time the two groups took to complete the follow-up survey or in how likely they were to start or complete the follow-up survey. The result of no different present bias also holds if the sample is restricted to participants who not were working at the time of the follow-up survey.

[32] Moreover, liquidity constraints should not influence individuals' risk choices if subjects were "narrowly bracketing" when making their risk choices (see, e.g., Tversky and Kahneman 1981, Rabin and Weizsacker 2009).

[33] The CRRA parameter intervals are: $(-\infty,0)$ for those who chose (70/2); (0,0.50) for (60/12); (0.50,0.71) for (52/16); (0.71,1.16) for (44/20); (1.16,3.46) for (36/24); and $(3.46,+\infty)$ for (28,28).

[34] Following Giné et al. (2014), we divided the 12 decisions of each subject into 9 pairs, where each element of the pair was the amount allocated to the more delayed payment. The first element was the amount allocated under interest rate r_1 and the second element was the amount allocated under interest rate r_2, where r_2 was the next highest interest after r_1 (so for example $r_1 = 0\%$ and $r_2 = 0.5\%$). For each subject there were 9 pairs, 3 for each time frame. The pair was identified as consistent if the later reward under r_2 was greater or equal to the later reward under r_1.

TABLE 4—RISK CHOICES

	Study 1 CRRA parameter	Study 2 % allocated to cheapest asset
{Before payday}	−0.10	0.00
	[0.152]	[0.007]
Constant	1.66	0.61
	[0.110]***	[0.005]***
Observations	1,064	1,119

Notes: The first column reports estimates from an interval regression where the dependent variable is the interval measure of the coefficient of relative risk aversion. The last column reports results from an OLS regression where the dependent variable is the fraction allocated to the cheapest asset (Choi et al. 2014 propose this as a nonparametric measure of risk attitudes that does not require assumptions about the parametric form of the underlying utility function). Robust standard errors in brackets. Indicator variables are in curly brackets.

nonmonetary rewards, we follow Burks et al. (2009) and define subjects as being consistent if they had at most one switching point (for each time frame). Our outcomes of interest are the measures of consistency in each task.

Following a series of recent studies (Choi et al. 2007a, b, 2014), we use Study 2's risk choices to measure the quality of decision-making, assessing the consistency of these choices with economic rationality. The first measure, Afriat's (1967) critical cost efficiency index (CCEI), captures violations of the General Axiom of Revealed Preference (GARP). Consistency with GARP is a necessary but not sufficient condition for high quality decision-making (consistency with GARP requires that preferences are consistent over alternatives but any preference ordering is acceptable). The second measure captures violations of GARP and violations of first order stochastic dominance (FOSD), the failure to recognize that some allocations yield payoff distributions with unambiguously lower returns. See Choi et al. (2014) for more details on how these measures are constructed.

Table 5 shows that there are no statistically significant differences in the consistency of intertemporal choices or risk choices with rationality. In the task with monetary rewards, the before-payday group was 2 percentage points less likely to be consistent than the after-payday group (which had an 84 percent consistency rate), but this difference was not statistically significant. Similarly, the before-after payday difference in CCEI scores was small and not statistically significant.[35]

In terms of heuristics, there are no differences in sensitivity of the two groups to framing, or to the likelihood of succumbing to the gambler's fallacy (Study 1). These results are shown in online Appendix G1.

In sum, we do not find any significant differences in the quality of economic decision-making before and after payday.[36]

[35] Nor do we find a difference in consistency in the loss aversion task (Study 1). The mean for the after-payday group was 0.811 and the before-after difference was −0.004 with a p-value of 0.86.

[36] Choi et al. (2014) find that the quality of decision-making measured by CCEI scores strongly correlates with (log) wealth, which they interpret as evidence that decision-making ability determines economic circumstances. Our results are not inconsistent with the Choi et al. (2014) results (in fact, we also find a correlation between CCEI and (the inverse hyperbolic sine) of assets) because we investigate whether the relationship runs in the opposite direction, that is, whether economic circumstances *causally* affect decision-making.

Table 5—Quality of Decision-Making

	Consistency intertemporal choices (Study 1)		Consistency risk choices (Study 2)	
	{Increased later $ reward in response to interest raise}	(Nonmonetary) {At most one switching point}	GARP CCEI score	GARP + FOSD CCEI score
{Before payday}	−0.02	−0.02	0.00	−0.01
	[0.013]	[0.018]	[0.009]	[0.013]
Constant	0.84	0.89	0.85	0.73
	[0.013]***	[0.013]***	[0.007]***	[0.009]***
Observations	9,540	2,050	1,119	1,119
Subjects	1,060	1,025	1,119	1,119

Notes: The first two columns report the results from OLS regressions where the dependent variable is a measure of consistency in intertemporal choices. In the first column, which shows consistency in intertemporal choices about monetary rewards, the dependent variable is an indicator variable for whether the subject increased (or kept constant) the later reward in response to an increase in the experimental interest rate (Giné et al. 2014). In the second column, which shows consistency in intertemporal choices about real effort, the dependent variable is 1 if the participant had at most one switching point for each time frame (Burks et al. 2009). The first column includes dummies for each pair of choices (see footnote 34) while the second column includes a time-frame-specific dummy. The last two columns report results from OLS regressions where the dependent variable is a measure of consistency in risk choices. In the third column, which shows violations of the GARP, the dependent variable is Afriat's critical cost efficiency index (CCEI). The last column examines a unified measure of GARP violations and violations of stochastic dominance by combining the actual data from Study 2's risk choice task and the mirror image of these data (see Choi et al. 2014 for more details). In the first two columns, the standard errors are clustered at the individual level. In the last two, robust standard errors are estimated. Indicator variables are in curly brackets.

C. *Cognition*

To investigate whether economic circumstances affect cognition, we first examine whether the before-payday and after-payday groups perform differently on cognitive function tasks. Then, we discuss the magnitude and standard errors of our estimates and compare them to Mani et al. (2013). Next, we look at whether the difference in economic circumstances is accompanied by a difference in the perception of scarcity. Finally, we study the before-after payday differences in cognitive function when the sample is restricted to the more financially strained subgroups.

Cognitive Function.—Table 6, which presents results from four different tasks/tests used to measure cognitive function, shows that the before-payday and after-payday groups performed similarly. On the Flanker task (Study 1), participants assigned to the before-payday group were 2 percent slower in their response time on average than the after-payday group, but they were also 1 percentage point more likely to respond correctly. On the numerical Stroop task (Study 2), the before-payday participants were 1 percent faster on average and they were no less likely to respond correctly. None of these differences were statistically significant at the 10 percent level. In addition, the before-payday group performed slightly better in the working memory task (Study 1) and in the cognitive reflection test (Study 1); again, these differences were not statistically significant.

In sum, there are no before-after differences in cognitive function when we look at the overall sample.

Magnitude and Standard Errors of Cognitive Function Results.—This section discusses the magnitude and precision of the estimated effects on cognition and

TABLE 6—COGNITIVE FUNCTION

	Study 1 flanker		Study 2 numerical Stroop		Study 1	
					Working memory	Cognitive reflection
	ln(*Time*)	{Correct}	ln(Time)	{Correct}	Mem. span	% correct
{Before payday}	0.02	0.01	−0.01	0.00	0.02	0.01
	[0.028]	[0.010]	[0.011]	[0.009]	[0.239]	[0.014]
Constant	8.06	0.86	7.79	0.80	4.69	0.11
	[0.030]***	[0.012]***	[0.010]***	[0.009]***	[0.164]***	[0.010]***
Observations	20,557	20,557	130,038	130,038	1,038	1,045
Trials	20,557	20,557	130,038	130,038	—	—
Subjects	1,076	1,076	2,723	2,723	1,038	1,045

Notes: See Section IC for a description of the Flanker, numerical Stroop, and working memory tasks, and the cognitive reflection test. This table reports results from OLS regressions of the dependent variables shown in the column headings on an indicator variable for the before-payday group and a constant (the regressions in the first four columns also include trial-specific dummies). Response times in the Flanker and numerical Stroop tasks were measured in milliseconds. Memory span is the length of the longest list of colors the participant was able to reproduce. In the first four columns the standard errors are clustered at the individual level. In the last two, robust standard errors are estimated. Indicator variables are in curly brackets.

compares them to the results of Mani et al. (2013). Here we focus on the effect of economic circumstances on (the log of) response time in cognitive control tasks, an outcome measured in all three studies: Study 1, Study 2, and Mani et al. (2013). As described in Section IC, we administered the Flanker task in Study 1 and we intentionally administered the same cognitive control task administered by Mani et al. (2013) in Study 2: the numerical Stroop task.

The first three columns of Table 7 show intention-to-treat (ITT) estimates. The response time of the before-payday group was 1.70 percent higher on average than the response time of the after-payday group in Study 1; in Study 2 the response time of the before-payday group was 0.33 percent *lower* on average than the response time of the after-payday group. Using Mani et al.'s (2013) data, we estimate that the response time of sugarcane farmers was 19 percent higher on average before harvest than after harvest.[37] Although Mani et al.'s (2013) ITT estimate lies outside our 95 percent confidence intervals, the ITT estimates may not be directly comparable because the size of the economic shock may be different across the three studies.

Before we present the evidence on the size of economic shocks, it is worth asking how much larger the difference in economic circumstances before and after harvest would have to be, relative to the difference in economic circumstances before and after payday, for us to reconcile the ITT estimates of Study 1 and Study 2 with Mani et al.'s (2013) ITT estimate. For example, to reconcile Study 1's ITT point estimate with Mani et al.'s (2013), the before-versus-after-harvest economic shock would need to be more than 11 times larger than Study 1's before-versus-after-payday economic shock. Because Study 2's and Mani et al.'s (2013) ITT estimates have opposite signs,

[37] We thank Mani et al. for generously sharing their data with us. The estimates in the third column of Table 7 are different from the estimates in Mani et al.'s (2013) Table 1 because we use the log of response time as our dependent variable while they use the response of time in levels. The sample size is also different because the response time was equal to zero for one pre-harvest observation (the corresponding post-harvest observation for this participant was also dropped).

Table 7—ITT, First Stage, and 2SLS

	ITT ln(Time)			First stage IHS(Expenditures)		2SLS ln(Time)	
	Study 1	Study 2	Mani et al.	Study 1	Study 2	Study 1	Study 2
{Before payday}	0.02	0.00		−0.23	−0.48		
	[0.029]	[0.011]		[0.096]**	[0.053]***		
{Before harvest}			0.19				
			[0.036]***				
IHS(Expenditures)						−0.07	0.00
						[0.129]	[0.023]
Constant	8.06	7.78	7.49	6.59	6.97	8.54	7.77
	[0.031]***	[0.011]***	[0.011]***	[0.065]***	[0.034]***	[0.832]***	[0.156]***
Observations	20,206	119,684	902	1,056	2,496	20,206	119,684
Trials	20,206	119,684	—	—	—	20,206	119,684
Subjects	1,056	2,496	451	1,056	2,496	1,056	2,496

Notes: This table compares estimates from Study 1 and Study 2 to the estimates obtained using Mani et al.'s (2013) data. The first three columns show intent-to-treat (ITT) estimates. The two middle columns estimate the before versus after payday difference in (the inverse hyperbolic sine of) total expenditures. Finally, the last two columns show 2SLS estimates where the before payday indicator is used to instrument for total expenditures. Standard errors clustered at the individual level (robust standard errors are estimated in the two middle columns). Indicator variables are in curly brackets.

it is not possible to reconcile these estimates. We can also compare the upper bounds of Study 1's and Study 2's 95 percent confidence intervals to Mani et al.'s (2013) ITT point estimate. This comparison suggests that the before-versus-after-harvest difference in economic circumstances would have to be 2.6 times (Study 1) or 9 times (Study 2) larger than the before-versus-after-payday difference in economic circumstances for Mani et al.'s (2013) point estimate to lie within our 95 percent confidence intervals.[38]

Next we look at expenditures to take into account the differences in economic circumstances before and after payday or harvest. Columns 4 and 5 of Table 7 estimate the size of the economic shocks in Study 1 and Study 2: on average, the before-payday group spent 23 percent less than the after-payday group in Study 1 and 48 percent less in Study 2. Mani et al. (2013) did not collect data on expenditures for their main sample, but they did collect expenditure data for their pilot study with the same design.[39] Despite the limitations of these data (i.e., small sample size and the usual challenges of measuring expenditures of poor households), they provide the best estimate we have of the difference in economic circumstances before and after harvest. According to these data, total expenditures were 10 percent lower on average before harvest than after harvest. The before-after difference in total expenditures is driven by expenditures for goods and services other than food: expenditures for other goods and services were on average 30 percent lower before harvest than after harvest.[40]

[38] There are substantial differences across the economic environments in Mani et al. (2013) and in Study 1 and Study 2, such that more empirical research is needed on the plausibility of these magnitude disparities.

[39] The pilot was conducted with 188 farmers in the districts of Thanjavur, Thiruvarur, Perambalur, and Pudokattai in Tamil Nadu. We thank Mani et al. for generously sharing the results from the pilot.

[40] Total expenditures were calculated as the sum of three types of expenditures: (i) food; (ii) other goods and services; and (iii) events (e.g., weddings, festivals, funerals, etc.). The average expenditure for these categories

To convert the estimated effects of the three studies to a common unit for comparison purposes, we need to re-scale the ITT estimates by the size of the economic shocks. For Study 1 and Study 2, we estimate 2SLS models: we use the random assignment to the before-payday group to instrument for expenditures. For the sugarcane farmers in India, we divide the ITT estimates by 30 percent (we do not have the microdata to estimate 2SLS). This exercise also addresses the issue of imperfect compliance, that is, that a fraction of participants who were assigned to the before-payday group started the survey after payday.

The last two columns of Table 7 show the 2SLS point estimates: In Study 1 a 10 percent increase in expenditures is associated with a 0.74 percent reduction in response time; in Study 2 a 10 percent increase in expenditures is associated with a 0.01 percent *increase* in response time. The re-scaled estimates for sugarcane farmers in India (using a before-versus-after-harvest difference of 30 percent) suggest that a 10 percent increase in expenditures is associated with a 6.36 percent reduction in response time.

In terms of the precision of the estimates, the 95 percent confidence interval of a 10 percent increase in expenditures range from a 3.26 percent reduction to a 1.78 percent increase in the response time in Study 1. In Study 2 the 95 percent confidence interval (of a 10 percent increase in expenditures) range from a 0.44 percent reduction to a 0.46 percent increase in the response time. Thus we can rule out the effect that we estimate using Mani et al.'s (2013) data.

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).

A power analysis of Study 2 indicates that with a sample size of 2,700 participants and a 98 percent compliance rate (i.e., the fraction of the before-payday group who completed the follow-up survey before payday) we can detect a *before-versus-after-payday difference* of 0.11 of a standard deviation in a two-sided test with power 0.8 and a significance level of 5 percent. If we standardize the response time in Mani et al.'s (2013) data, we estimate a *before-versus-after-harvest difference* of 1.12 of a standard deviation. That is, in Study 2 we were powered to detect an effect that was one-tenth of the effect found by Mani et al. (2013).

Finally, when we standardize the response time in Study 2, our ITT estimates yield a *before-versus-after-payday difference* of 0.004 of a standard deviation (SD) with a 95 percent confidence interval ranging from a reduction of 0.05 of a SD to an increase of 0.05 of a SD. In other words, to reconcile Study 2's estimates with Mani et al.'s (2013) estimates, the differences in economic circumstances before and after harvest would have to be more than 20 times larger than the differences in economic circumstances before and after payday.

before harvest was (i) 2,664 rupees (Rs); (ii) 2,297 Rs; and (iii) 2,520 Rs. After harvest the average expenditures were: (i) 2,592 Rs; (ii) 3,289 Rs; and (iii) 2,457 Rs.

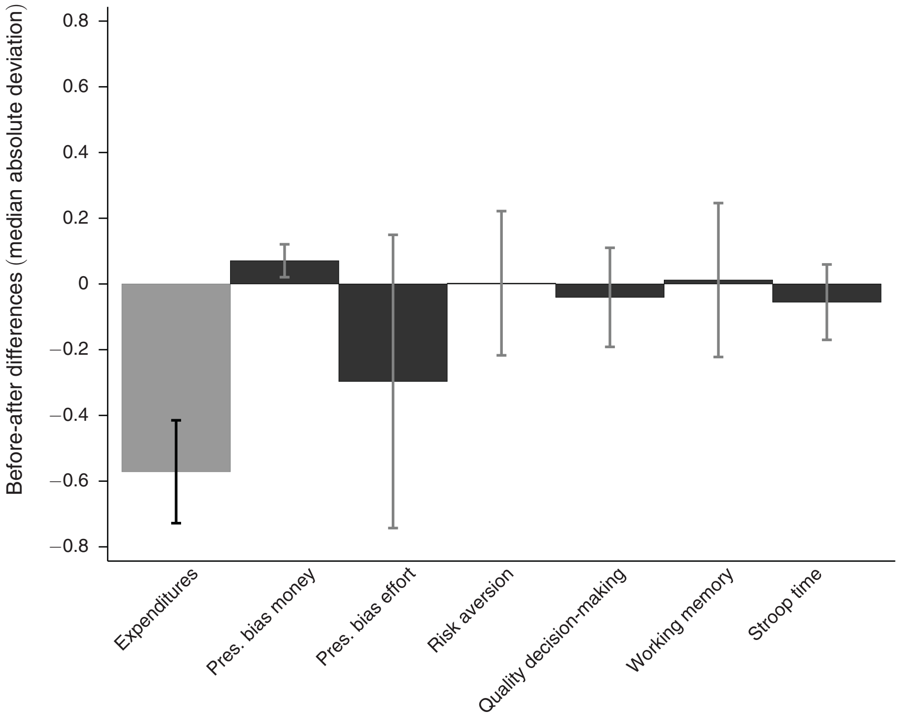

FIGURE 1. SUMMARY OF MAIN RESULTS

Notes: The bars show before-after differences for the following outcomes: expenditures (Study 2), present bias in intertemporal choices about money (Study 1), present bias in intertemporal choices about real effort (Study 1), risk aversion (Study 2), quality of decision-making measured in terms of violations of GARP and monotonicity with respect to FOSD (Study 2), memory span in working memory task (Study 1), and log of response time in numerical Stroop task (Study 2). The outcomes are scaled to comparable units by subtracting the median for the after-payday group and dividing by the median absolute deviation for the after-payday group. The height of the bar corresponds to the coefficient on the before-payday indicator variable in a regression of the outcome on the before-payday indicator variable and a constant. All regressions are OLS regressions with the exception of expenditures (median regression) and present bias effort (interval regression). In Stroop time, trial-specific dummies are included. The bands show 95 percent confidence intervals. Subjects = 2,496 (expenditures); 1,060 (present bias money); 1,025 (present bias effort); 1,119 (risk aversion); 1,119 (quality of decision-making); 1,038 (working memory); and 2,723 (Stroop time).

Summary of Main Results.—Figure 1 summarizes the main results (all outcomes are scaled to comparable units; see footnote of Figure 1 for more details). It shows that the before-payday group spent significantly less than the after-payday group. The before-payday group also behaved as if they were more present-biased when making intertemporal choices about monetary rewards, but not when they were making intertemporal choices about real effort. There are also no differences in the willingness to take risks, quality of decision-making, or in cognitive functioning.

Subjective Perceptions of Financial Strain.—Mullainathan and Shafir (2013, p. 4) posit that it is the feeling of scarcity, i.e., the feeling of "having less than you feel you need," that explains why poverty preoccupies the mind and therefore impedes cognitive function. While we show that there are differences in economic circumstances before and after payday, one could wonder whether these differences in economic circumstances translate into differences in the feeling of scarcity.

Four questions in Study 1 could be interpreted as proxies for the subjective perception of scarcity (note, however, that these questions were not originally designed and included in Study 1 to measure perception of scarcity).[41] Table G8 in online Appendix G shows that for three of these measures the results have the opposite sign from what we would expect: the before-payday group reports better subjective economic circumstances than the after-payday group. Even though none of these results is statistically significant, they are nevertheless intriguing.

Therefore, in Study 2 we wanted to investigate whether the before-payday group truly felt no more preoccupied, or whether the questions in Study 1 just were not well suited to detecting before-after differences. In Study 2 we thus used questions that were specifically designed to measure the subjective perception of scarcity. There are no such standard measures, so we designed five questions to get at the idea of being preoccupied by scarcity and which make the temporal dimension more salient (by providing the last 24 hours as the relevant reference period).[42] We believe that these new questions provide more accurate measures of the subjective perception of scarcity than the questions in Study 1.

The responses to these questions in Study 2 *suggest* that the before-payday group experienced more of a feeling of scarcity than the after-payday group. All five point estimates go in this direction and two of them are statistically significant. The five estimates are jointly significant at 10 percent. In other words, these findings indicate that there is no difference in the cognitive function of the after-payday and before-payday, even though the latter was more preoccupied by scarcity.

While our results are a step toward understanding how material scarcity might (or might not) translate into particular thoughts or mental preoccupations with not having enough, they also point toward nuances that need to be explored further. Some of the questions we used might be more suited for reliably eliciting a subject's perceptions than others. More rigorous tests of potential survey questions with different populations are needed before we can converge on a standard set to get at perceptions of scarcity.

More Financially Strained Subgroups.—This section focuses on those subgroups whose financial circumstances change more sharply at payday (as shown in Table 2). Figure 2 shows before-after differences in the average (log) response time in the numerical Stroop (Study 2) for the overall sample and for the more financially strained subgroups.[43] The first bar shows the before-after difference for the

[41] The four questions were: (i) How hard do you think it will be to cover expenses you expect to have in the next 5 days with the money you have now?; (ii) Suppose you had only one week to raise $2,000 for an emergency. Which of the best describes how hard it would be for you to get the money: I could easily raise the money; I could raise the money, but it would involve some sacrifices; I would have to do something drastic to raise the money?; (iii) How satisfied are you with the current financial situation of your household?; and (iv) How stressed do you feel about your personal finances?

[42] The questions were: "In the last 24 hours, how often: (i) … were you troubled about coping with ordinary bills?; (ii) … did you worry about having enough money to make ends meet?; (iii) … did you think about future expenses, some of which may be unexpected?; and (iv) … were you preoccupied with thoughts about your personal finances?" In addition, we asked "We are interested in understanding if people's concerns about having enough money to make ends meet change over the month. Relative to other days this month, how concerned were you in the last 24 hours about having less money than you need to make ends meet?"

[43] We focus on Study 2 because it has a high compliance rate and a larger sample size, which makes it better suited for the subgroup analysis.

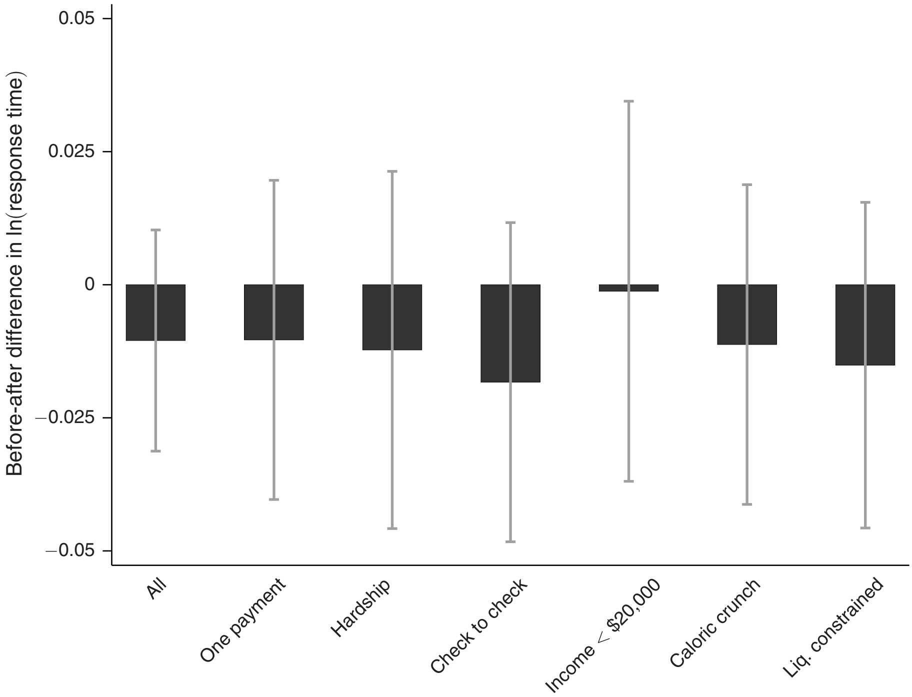

Figure 2. Cognitive Function for More Financially Strained Subgroups

Notes: The different bars present results from regressions estimated separately for different subsamples: the first bar for the overall sample and the others for the more financially strained subgroups. The height of the bar corresponds to the coefficient on the before-payday indicator variable in a regression of the (log) response time in the numerical Stroop (Study 2) on the before-payday indicator variable and trial-specific dummies. The bands show 95 percent confidence intervals. Subjects = 2,723 (all); 1,407 (one payment); 1,095 (hardship); 1,303 (paycheck to paycheck); 1,115 (annual household income of $20,000 or less); 1,263 (caloric crunch); and 1,365 (liquidity constrained).

overall subsample; the other bars show the results for the more financially strained subgroups: (i) respondents who received one payment only per month; (ii) respondents who had experienced financial hardship; (iii) respondents who reported living from paycheck to paycheck; (iv) respondents with an annual household income of $20,000 or less; (v) respondents who were forced to reduce food consumption because they ran out of money; and (vi) respondents who could not, or would have to do something drastic to, raise $2,000 in a week for an emergency.

If scarcity does impede cognitive function, then the before-after difference in response time should be positive (i.e., the before-payday group should be slower than the after-payday group). Moreover, we would expect the before-after difference in response time to be *more positive* for the more financially strained subgroups than for the overall sample.

Figure 2 shows that—even among the more financially strained subgroups—there is no evidence that scarcity impedes cognitive function. To mention just two subgroups: we do not find a before-after difference in response time even for participants with an annual household income of $20,000 or less. This is an interesting subgroup, because Mani et al. (2013) did find effects of scarcity on cognitive function for a US population making *more* than $20,000 per year. Nor is there a before-after difference in response time for the subgroup who could not, or would have to do something drastic to, raise \$2,000 in a week for an emergency (in online Appendix Table H9 we show that this result also holds for other proxies of credit constraints). In general, there is no indication that the before-after difference in response time is *more positive* for the subgroups than for the overall sample. Indeed, as the confidence bars illustrate, it is not possible to distinguish the effects for the subgroups from the effects for the overall sample.

In online Appendix H we show that this result also holds for other outcomes.

III. Conclusion

In this paper, we use the sharp change in financial resources at payday for a low-SES population to examine the causal effects of financial resources on decision-making. While previous work has documented associations between income/wealth and decision-making quality and preferences (e.g., Choi et al. 2014; Meier and Sprenger 2010), our study design allows us to establish a causal link between financial circumstances and economic decision-making. Thus, ours is the first study we know of that provides experimental evidence on whether financial resources affect the economic decision-making of poor US families.

Our results indicate that scarce resources indeed can affect one's willingness to delay gratification: the before-payday participants behaved as if they were more present-biased when making choices about monetary rewards. However, any present-biased behavior was the same before and after payday when the participants had to choose a costly real-effort task. Taken together, these results suggest that the observed difference in the monetary intertemporal choices is most likely due to liquidity constraints, not to poverty reducing one's self-control. Our findings are consistent with the emerging theoretical literature on intertemporal choices under liquidity constraints and expectation of future income (Ambrus et al. 2015; Epper 2015). In addition, we do not find differences between the before-payday and after-payday groups in their willingness to take risks.

Our results also do not support the hypothesis that financial strain by itself worsens the quality of decision-making. Even though there are substantial differences in financial resources before and after payday for our samples of poor US households, we find no evidence that the quality of their decision-making, or being prone to heuristic judgments differs across the before-payday and after-payday groups. Nor do we find before-after differences in key aspects of cognitive functions, such as inhibitory control or working memory. Our results hold even for the more financially strained subgroups, whose financial circumstances change more sharply at payday.

In conjunction with the previous literature, our findings suggest that more research needs to be done to understand the effects on cognitive functions and economic decision-making of the interplay between long-term socioeconomic status and short-term financial circumstances. We have shown that short-term variation in financial resources does not deterministically lead to cognitive deficits and decision-making mistakes, in contrast to what previous studies suggest. Future research should investigate whether our findings generalize to individuals with different patterns of resource variation—for example, more permanent shocks to their permanent income or less certainty about future income streams. Taken together with others in the literature, our study also suggests the need for further research to clarify the link between objective scarcity and perceived scarcity.

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