Poverty and Economic Decision-Making: Evidence from Changes in Financial Resources at Payday*

LEANDRO S. CARVALHO, STEPHAN MEIER, AND STEPHANIE W. WANG

We study the effect of financial resources on decision-making. Low-income U.S. 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 non-monetary real-effort tasks. Nor do we find before-after differences in risk-taking, the quality of intertemporal decision-making, the performance in cognitive function tasks, or in heuristic judgments.

*Meier: Columbia University, Graduate School of Business, Uris Hall, 3022 Broadway, New York, NY 10027 (email: sm3087@columbia.edu). This paper benefited from comments from Dan Benjamin, Jeffrey V. Butler, Andrew Caplin, Dana Goldman, Ori Heffetz, Sendhil Mullainathan, Eldar Shafir, Jesse Shapiro, Charlie Sprenger, and participants in seminars and conferences where it was presented. Carvalho thanks the RAND Roybal Center for Finacial Decisionmaking and the Resource Center for Minority Aging Research (RCMAR) at the University of Southern California's Schaeffer Center, Meier thanks the Columbia Business School, and Wang thanks the University of Pittsburgh Central Research Development Fund for generous research support.

The poor often behave differently from the non-poor. 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 underlying such differences has a long and contentious history in the social sciences; the two opposing views are that either 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, which could trap them into 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 pg. 4), impedes cognitive functioning, which in turn may lead to decision-making errors and myopic behavior.[5]

Empirically, there are major challenges in isolating the causal effects of economic circumstances on decision-making. There may not be only a reverse causality bias – that is, the economic decisions one makes determine one's economic circumstances – but also be unobserved individual characteristics, such as innate cognitive ability, confounding the relationship between economic circumstances and decision-making. Identification of the effects of poverty on time preferences is further complicated by the possibility that poverty may affect credit constraints and arbitrage opportunities, which in turn could influence intertemporal choices (e.g., Frederick et al. 2002).

[1] Rhine et al. 2006; Ananth et al. 2007; Haisley et al. 2008; Bertrand and Morse 2011; Dobbie and Skiba 2013.
[2] E.g., Schultz 1964 and Lewis 1965. See Bertrand et al. 2004 and Duflo 2006 for more recent perspectives.
[3] Lawrance 1991; Banerjee and Mullainathan 2010; Tanaka et al. 2010; Spears 2011; Gloede et al. 2012; Bernheim et al. 2013; Carvalho 2013; Haushofer et al. 2013.
[4] Shah et al. 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 et al. 2013).

This paper uses changes in financial resources at payday to empirically investigate whether financial resources have a *causal* effect on economic decision-making. Previous work has documented that the expenditures and the caloric intake of some households increase sharply at payday (e.g., Stephens 2003, 2006; Huffman and Barenstein 2004; Shapiro 2005).

To exploit the sharp change in financial resources at payday, we designed and administered online surveys: 1,191 participants with annual household income below $40,000 were randomly assigned to a group surveyed shortly *before* payday (hereafter, the before-payday group) or shortly *after* payday (hereafter, the after-payday group). We then collected measures of cognitive function and administered incentivized risk choice and incentivized (monetary and non-monetary) intertemporal choice tasks. Our goal was to investigate whether the before-payday group would behave differently from the after-payday group.

Like the 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. The participants surveyed before payday knew when their next payment would arrive and when more money would come in. 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 how 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, the results provide insights on the effects of poverty on time and risk preferences, suggesting that there are no such relationships. Although we find that the before-payday group behaved as if they were more present-biased when making intertemporal choices about monetary rewards, this behavior is most likely explained by differences in liquidity constraints, since the two groups made

similar intertemporal choices about *non*-monetary rewards. They also made 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), 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 differences across the two groups in the quality of the decision-making, as measured in terms of the consistency of the intertemporal choices.

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

I. Study Design

We collected data using the RAND-USC American Life Panel (ALP), an ongoing internet panel with respondents aged 18 and over living in the United States. About twice a month, respondents receive an email with a request to visit the ALP site and complete questionnaires. Respondents without Internet access at the time of recruitment are provided laptops and an internet access subscription, partly mitigating any concerns about selection bias due to internet access. The panel recently has been expanded to include more than 2,000 members of vulnerable populations drawn from zip codes with a large fraction of low-income, low-education, or minority populations.

We restricted our study sample to panel members with an annual household income of $40,000 or less. More than one third of the sample had an annual

family income below $15,000 and 45 percent had less than $20,000.[6] Other results indicate that the study sample had low socioeconomic status (hereafter, SES): more than 45% had zero or negative non-housing wealth and 60% had zero or negative housing wealth.[7] Other characteristics associated with poverty were: 18% of our sample were unemployed, one-fifth reported being disabled, and only 37% were working at the time of the survey. Approximately 17% did not have a checking or savings account (compared to a 7% national average, FINRA 2013), and more than 45% did not have a credit card (compared to a 29% national average, FINRA 2013). Finally, because of a shortage of money, more than half of them had experienced (at least) one of the following in the 12 months before the study: 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 or family; or took a payday loan. (See Appendix C for more details about the socioeconomic status of the sample.)

The study consisted of one baseline and one follow-up survey. The baseline survey collected information that was used to determine participants' paydays. The opening dates of the follow-up survey, which were specific to each study participant, depended on the participant's payday and her random assignment.[8] Specifically, the follow-up survey 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.[9] Participants were sent emails informing them when the survey was available. The follow-up survey measured various aspects of decision-making for the two randomly assigned groups.

[6] These figures were calculated using the sample of 1,098 participants who started the follow-up survey.
[7] See footnote of table C2 in Appendix C for more details about the wealth measures.
[8] Spears (2012) used a similar design in which recipients of South Africa's old age pension were randomly assigned to be surveyed before or after receiving the monthly pension payment to study cognitive limits and intertemporal choices.
[9] For a participant whose payday fell on January 15, the survey would be available at January 8 at 12:00:01am if she were assigned to the before-payday group and at January 16 at 12:00:01am if she were assigned to the after-payday group.

A. The Baseline Survey and Study Sample

The baseline survey collected data on the dates and amounts of all payments that the participant (and his/her spouse) expected to receive during January 2013.[10] (See 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, and on those who anticipated receiving fewer than five payments from a single income source in January 2013.[11] (See Appendix D for more details about the payments.)

These data were then used to identify the payday of each participant payday. If the largest payment came two weeks or more after the previous payment, the payday was set as the date of this largest payment. Otherwise, the payday was set as the date that followed an interval of 14 days or more without any other payments. Two hundred and eight participants whose payments were all less than 2 weeks apart were dropped from the study sample. (See Appendix E, which gives details about sample restrictions and survey nonresponse, for the flow of participants through the study.)

B. Randomization and Treatment Compliance

The remaining 1,191 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 Appendix F for more details). We stratified on whether at baseline participants strongly agreed with the statement "I live from paycheck to paycheck" and on whether they anticipated receiving only one payment in January 2013, because we planned to check whether the effects would

[10] 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.

[11] The rationale for dropping respondents who anticipated five payments or more from a single income source is that their income should be spread out sufficiently over time, making it easier for them to smooth consumption.

be any different for those participants whose economic circumstances could be expected to change more sharply at payday.[12] The randomization was successful in making assignment to the before-payday group orthogonal to observable baseline characteristics (see Appendix F).

The study design generated variation in the time at which participants started and finished the survey. 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.[13] The median respondent assigned to the *after*-payday group started the survey 4.4 days *after* payday and completed it 5 days *after* payday. The differences across the two groups were all statistically significant at 1%.

Note that although the study design allowed us to manipulate when the survey was made available to a participant, we did not have control over when the participant started (or finished) the survey. Consequently, it was expected that there would 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% of the participants assigned to the before-payday group started the survey before payday, while 63% completed the survey before payday.[14] By construction, all participants assigned to the after-payday group started the survey at least one day after payday (see the first table in Appendix J for more detailed results). In our analysis, we estimate intent-to-treat effects, exploiting the random assignment to the before-payday group as a source of exogenous variation in starting the survey before payday.[15]

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[12] We also stratified on whether the respondent had some college education and on whether the survey would open before 12/31/2012 if the respondent were assigned to the before-payday group.

[13] Because participants were not required to complete the survey in just one sitting, the time interval between when they started the survey and when they completed it may have been much longer than the time it would effectively take to complete the survey without interruption. The median participant took 51 minutes to complete the survey.

[14] Results are similar if the sample is restricted to participants who started the survey within 7 days of its opening. See Appendix I.

[15] To take into account the imperfect compliance, one could scale up the estimates by instrumenting for starting the survey before payday using the assignment into the before-payday. But it is an open question as to how many days after payday participants re-enter a period of low financial resources. For this reason we opt for presenting intent-to-treat estimates.

C. The Follow-up Survey

The follow-up survey collected measures of (1) economic decision-making, (2) cognitive function, and (3) financial circumstances. We discuss them here briefly; for more details and screenshots of the follow-up survey, see Appendix B.

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

Another task required participants to make intertemporal choices regarding real effort (similar to Augenblick et al. 2013) in order to address concerns about the use of monetary rewards in measuring time discounting (e.g., Frederick et al. 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 sooner 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 one percent of the participants were randomly selected to

have one of their 10 choices implemented (i.e., "implementation surveys" were sent to those selected participants).[16]

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

Two additional tasks 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.

(2) Cognitive Function. To measure cognitive function, we used the Flanker task, a working memory task, and the Cognitive Reflection Test (CRT). 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 to instead give the deliberative and reflective correct answer (Frederick 2005). In addition to these tests of cognitive function, we have other measures of participants' cognitive abilities, including both fluid and crystallized intelligence, which were collected in previous ALP surveys. The final table in Appendix I shows that our measures of cognitive function are strongly correlated with these other measures of cognitive ability.

[16] 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).

We also included two items to measure the use of heuristics. One question from Toplak et al. (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).

(3) Financial circumstances. The follow-up survey included questions on cash holdings, checking and savings accounts balances, and expenditures, which permit checking if the study design generated variation in financial circumstances.

II. Results

Section IIA below 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 intertemporal choices (Section IIB), risk choices (Section IIC), consistency of intertemporal choices (Section IID), and cognitive functions (Section IIE).

A. Cash Holdings, Checking and Savings Balances, and Expenditures

Table 1 indicates that the before-payday group had fewer financial resources than the after-payday group: on average, the before-payday group had $114 less in cash (52% of the after-payday group mean) and $1,947 less in their checking and savings accounts (29% of the after-payday group mean). The before-payday group also reported having spent $553 less in the previous seven days (48% of the after-payday group mean), which is consistent with well-documented results that expenditures increase sharply at payday (e.g., Stephens 2003, 2006).

Table 1: Cash, Checking and Savings Accounts, and Total Expenditures

	Cash		*Checking and Savings*		*Total Expenditures*	
	OLS	Quantile 0.5	OLS	Quantile 0.5	OLS	Quantile 0.5
{Before Payday}	-$114	-$10	-$1,947	-$230	-$553	-$100
	[52]**	[4]**	[1,859]	[100]**	[328]*	[36]***
Constant	$217	$45	$6,626	$730	$1,156	$500
	[49]***	[3]***	[1,495]***	[72]***	[326]***	[25]***

Notes: This table reports results from OLS and quantile regressions (quantile 0.5) of the dependent variables shown in the columns headings on an indicator variable identifying participants assigned to the before-payday group and a constant. Standard errors in brackets. The checking and savings results exclude respondents who did not have a checking or savings account. N = 1,054 (cash), 851 (checking and savings), 1,056 (expenditures).

We also find that the median grocery expenditures were 11% lower for the before-payday group (not shown in the table). Previous work had documented that caloric intake decreases over the pay cycle (Shapiro 2005), which cannot be explained by bills coinciding with payday.

Similarly to Mastrobuoni and Weinberg (2009), we document in Table 2 that the increase in expenditures after payday is sharper for those who would likely have a harder time in smoothing consumption. In particular, for "more strained" subgroups, median expenditures are lower before payday than after payday. The results are much weaker for the "less strained" subgroups though: the before-after difference in median expenditures is also negative, but the magnitudes are much smaller, and in three of four cases they are not statistically different from zero. Although the sample sizes are small, we can reject the null hypothesis that the *negative* effect for the more strained subgroup is smaller (in absolute terms) than the *negative* effect for the less strained subgroup in three of the four cases.

In sum, the financial circumstances of the two groups at the time of the follow-up survey were substantially different, and having fewer financial resources may have affected the decision-making and behavior of the before-payday group. We present below the results for the overall sample. Appendix H shows that these results also hold if the sample is restricted to "more strained"

subgroups of participants whose financial circumstances one would expect to change more sharply at payday or to those who made less than $20,000 per year.

Table 2: Expenditures for More Financially Strained and Less Strained Subgroups

	Quantile Regression (Quantile 0.5)		
	Total Expenditures Last 7 Days		P-value Test
	(1)	(2)	(1) ≥ (2)
	One Payment (423)	*Multiple Payments* (633)	
{Before Payday}	-$200***	-$60	0.03**
Constant	$500***	$460***	
	Financial Hardship (547)	*No Hardship* (508)	
{Before Payday}	-$200***	-$50	0.01**
Constant	$600***	$400***	
	Wealth Distribution		
	Bottom Half (509)	*Upper Half* (518)	
{Before Payday}	-$109**	-$92*	0.42
Constant	$500***	$460***	
	"I Live from Paycheck to Paycheck"		
	Strongly Agree (557)	*Others* (499)	
{Before Payday}	-$150***	-$50	0.07*
Constant	$550***	$400***	

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. Separate results for subgroups more and less financially strained are shown respectively in columns (1) and (2). The four measures of financial strain are: A) receiving one payment over the month (versus multiple payments); B) having gone through a financial hardship in the previous 12 months: could not pay electricity, gas, or phone bills, car registration or insurance; pawned or sold something; went without meals; were unable to heat home; sought assistance from welfare or community organizations, friends or family; or took a payday loan; C) being in the lower half of the wealth distribution; and 4) having strongly agreed with the statement "I live from paycheck to paycheck."The last column reports the p-value of a one-sided hypothesis test that the negative effect for the more strained subgroup is smaller (in absolute terms) than the negative effect for the less strained subgroup. Standard errors are not reported in the table (available upon request). Number of observations in parentheses.

B. Intertemporal Choices

Economists have long debated whether the poor have higher discount rates (e.g., Lawrance 1991, Carvalho 2013; Haushofer et al. 2013). 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, 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 et al. 2002; Dean and Sautmann 2013).

Table 3: Intertemporal Choices about Monetary Rewards

	$ Amount of Sooner Reward	
	Coefficient	Standard Error
{Before Payday} * {Immediate Rewards}	10.6	3.83***
{Before Payday} * Interest Rate	2.7	3.24
{Before Payday} * Delay Time	-1.4	1.06
{Before Payday}	-6.3	9.80
{Immediate Rewards}	-5.3	2.75*
Experimental Interest Rate	-47.3	2.33***
Delay Time	-0.7	0.72
Constant	304.3	6.83***

Notes: This table 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. "Delat Time" is the time 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. $N = 12{,}720$.

Table 3 shows 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 in the future to today. This difference is statistically significant at the 1% level.

There are no other statistically significant differences across the two groups and we can rule out economically meaningful differences. In Appendix K we use the CTB framework to estimate utility-function parameters that better quantify the differences in behavior across the two groups. We can rule out that the before-after (absolute) difference in the utility curvature parameter was greater than 0.003.[17] The discount rates are estimated with less precision, but we still can rule out that the before-payday group had an annual discount rate 1.05 percentage points higher than the after-payday group.[18]

Although the result that the before-payday group behaved as if it was more present-biased is consistent with the interpretation that scarcity reduces self-control, it is also possible that such behavior could be explained by before-after differences in liquidity constraints. Augenblick et al. (2013) argue that intertemporal choices about real effort are better suited than intertemporal choices about monetary rewards to capture dynamic time inconsistent preferences, because the latter are subject to several confounds. In Table 4 we analyze subjects' intertemporal choices between a shorter survey sooner 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 2010).[19]

We find that the two groups behaved similarly in making intertemporal choices about a costly real-effort task, namely choosing between answering a shorter survey sooner and a longer survey later. Both groups displayed behavior that was *consistent* with present-bias: the implied monthly discount rate was 9 percentage points higher when the shorter-sooner survey had to be completed within five days (as opposed to 90 days).

[17] The CTB framework assumes constant relative risk aversion (CRRA) risk preferences, where $u(c) = c^{\alpha}/\alpha$ and α is the utility curvature parameter. This figure is however sensitive to the particular assumptions about background consumption.
[18] The after-payday group is estimated to have an annual discount rate of 9.4%.
[19] 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.

Table 4: Intertemporal Choices about Real Effort

	Monthly Discount Rate
{Before Payday} * {Immediate Task}	-0.03
	[0.025]
{Before Payday}	0.02
	[0.027]
{Immediate Task}	0.09
(5-day deadline for short-sooner survey)	[0.018]***
Constant	0.31
	[0.019]***

Notes: This table 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; one for each time frame. "Immediate Task" is an indicator variable for the "5 days (sooner) x 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 non-monetary intertemporal task. $N = 2{,}050$.

However, there is no evidence of differential present-bias in this task. Although one should be cautious in comparing intertemporal choices about monetary rewards to intertemporal choices about real effort, 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. 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 such hypothesis.[20]

C. Risk Choices

As Table 5 shows, the before-payday and after-payday groups make similar risk choices.[21] The before-payday group behaves as if they are less risk averse, but the differences are small and not statistically significant.[22] The before-payday

[20] 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.
[21] 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).
[22] The p-value of a Wilcoxon rank-sum test that the risk choices of two groups were from the same distribution was 0.42.

and after-payday groups also make 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 Appendix G).

Table 5: Risk Choices and Consistency in Intertemporal Choices

	Risk Choice Task	Consistency in Intertemporal Choices	
	CRRA Parameter	*% of Times Responded to Increase in Interest by Increasing $ Later Reward*	(Non-Monetary) *1 if at Most One Switching Point*
{Before Payday}	-0.10	-0.02	-0.01
	[0.152]	[0.013]	[0.023]
Constant	1.66	0.84	0.84
	[0.110]***	[0.009]***	[0.016]***
N	1,064	1,060	1,025

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 two columns report results from OLS regressions where the dependent variable is a measure of consistency in intertemporal choices. In the second column, which investigates consistency in intertemporal choices about monetary rewards, the dependent variable is the fraction of times in which the subject increased (or kept constant) the later reward in response to an increase in the experimental interest rate (Gine et al. 2013). In the last column, which investigates 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). Robust standard errors in brackets.

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. On the other hand, scarcity could have a direct effect on risk preferences *per se* (e.g., Tanaka et al. 2010; Gloede et al. 2012). These effects could partly offset each other if scarcity *reduced* risk aversion.[23]

[23] 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).

D. Consistency of Intertemporal Choices

To investigate whether economic circumstances affect the quality of decision-making, we first look at consistency in the intertemporal choices and then examine performance in the cognitive function tasks.

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 Gine et al. (2013), 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.[24] In the task with non-monetary rewards, we follow Burks et al. (2009). We 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.

As the last two columns of Table 5 show there are no statistically significant differences in the consistency of intertemporal choices. In the task with monetary rewards, the before-payday group was two percentage points less likely to be consistent than the after-payday group (who had an 84% consistency rate), but this difference was not statistically significant. The before-after difference in the non-monetary intertemporal task also was not statistically significant.[25]

In terms of heuristics, there are no differences in sensitivity of the two groups to framing, or how likely they were to succumb to the gambler's fallacy. These results are shown in Appendix G.

[24] Following Gine et al. (2013), we divided the twelve 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.

[25] Nor do we find a difference in the consistency in the loss aversion task. 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.

E. Cognitive Function

As shown in Table 6, the before-payday and after-payday groups performed similarly in the tasks and tests used to measure cognitive function. On the Flanker task, participants assigned to the before-payday group were on average 2 percent slower in their response time than the after-payday group, but they were also 1 percentage point more likely to respond correctly. None of these differences were statistically significant at the 10% level. The before-payday group performed slightly better in the working memory task and in the Cognitive Reflection Test (Frederick 2005), but, again, these differences were not statistically significant.[26]

Table 6: Cognitive Function

	Inhibitory Control (Flanker)		*Working Memory*	*Cog. Reflection*
	Ln(Time)	% Correct	Memory Span	% Correct
{Before Payday}	0.02	0.01	0.02	0.01
	[0.028]	[0.010]	[0.239]	[0.014]
Constant	7.16	0.92	4.69	0.11
	[0.029]***	[0.010]***	[0.164]***	[0.010]***
N	20,557	20,557	1,038	1,045

Notes: See pages 8 and 9 for a description of the Flanker 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 two columns also include trial-specific dummies). Response time in the Flanker task was measured in milliseconds. Memory span is the length of the longest list of colors the participant was able to reproduce. In the first two columns the standard errors are clustered at the individual level. In the last two robust standard errors are estimated.

These results contrast with the findings of Mani et al. (2013) who compared the performance of sugar cane farmers in India in related cognitive function tasks during the pre-harvest season, when they supposedly have fewer resources, and during the post-harvest season. They found for example that farmers spent 10.8%

[26] Appendix H shows that these results hold if the sample is restricted to participants who made less than $20,000 per year. Mani et al. (2013) found effects of scarcity on cognitive function for a U.S. population making *more* than $20,000 per year.

more time on the Stroop task before the harvest than after the harvest, and that farmers were 1.03 percentage points less likely to respond correctly before the harvest than after the harvest. A useful exercise is to compare our observed before-after differences in performance on the Flanker task to the pre-post harvest differences in performance on the Stroop task that Mani et al. (2013) observed. Our confidence intervals imply that in the Flanker task*, in the worst case scenario,* the before-payday group was 7.2% slower and 1.23 percentage points less likely to respond correctly than the after-payday group. Thus, we can rule the effect that Mani et al. (2013) observed on the Stroop time. However, the lower bound of our 95% confidence interval is comparable to their point estimates of the harvest effect on the fraction of correct responses.[27]

The difference between our results and Mani et al. (2013) suggest that we need to refine our understanding of how limited resources affect cognitive functioning. We showed that short-term variation in financial resources does not deterministically lead to cognitive deficits and decision-making mistakes in contrast with what previous studies found. As suggested by Mullainathan and Shafir (2013), scarcity per se may not be enough: "[t]he feeling of scarcity is distinct from its physical reality. Physical limits, of course, play a role…[b]ut so does our subjective perception…." (pg. 11). While it is challenging to conceptualize and measure this subjective perception of scarcity, preliminary results from our own study suggest that the mapping from scarce resources to the subjective perception of scarcity is not trivial (see Table J2 in Appendix J). While it is beyond the scope of this paper, future research should a) focus on how to measure subjective perception of scarcity reliably and b) investigate whether such perception must be present for financial scarcity to affect cognitive functioning.

[27] One important caveat: because Mani et al. (2013) do not provide estimates of the difference in income between the pre-harvest and post-harvest seasons, it is not possible to properly scale their effects to account for pre-post harvest differences in income which they rely on that may be greater (or smaller) than the before-after payday differences in income we rely on. Similarly, these calculations are based on intent-to-treat estimates that do not correct for imperfect compliance.

III. Discussion and 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. Thus, ours is the first study we know of that provides experimental evidence on whether financial resources affect the economic decision-making of poor U.S. families.

Our results indicate that scarce resources indeed can affect one's willingness to delay gratification: 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 choice most likely is due to liquidity constraints, not to poverty reducing one's self-control.

Nor do our results 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, we find no evidence that the willingness to take risks, the quality of decision-making (measured in terms of the consistency of the intertemporal choices), or being prone to heuristic judgments (gambler's fallacy and framing) differs across the before-payday and after-payday groups. Furthermore, we do not find before-after differences in key aspects of cognitive functions, such as inhibitory control or working memory.

In conjunction with the previous literature, our findings suggest that more 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 with what previous studies suggest. Future

research should investigate whether our findings generalize to individuals who have more permanent shocks to their permanent income.

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