Economic Decision-making in Poverty Depletes Behavioral Control

by

Dean Spears, Princeton University

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Abstract

Economic theory and common sense suggest that time preference can cause or perpetuate poverty. Might poverty also or instead cause impatient or impulsive behavior? This paper reports a randomized lab experiment and a partially randomized field experiment, both in India, and analysis of the American Time Use Survey. In all three studies, poverty is associated with diminished behavioral control. The primary contribution is to isolate the direction of causality from poverty to behavior; three theoretical mechanisms from psychology cannot be definitively separated. One supported explanation is that poverty, by making economic decision-making more difficult for the poor, depletes cognitive control.

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1 Introduction

Irving Fisher (1930), detailing his *Theory of Interest*, explains that "a small income, other things being equal, tends to produce a high rate of impatience." This is both "rational" — immediate survival is necessary to enjoy any future income or utility at all — and "irrational" — "the effect of poverty is often to relax foresight and self-control and to tempt us to 'trust to luck' for the future."

Subsequent economists, however, have seen time preferences as causally prior properties of persons, and important determinants of who accumulates wealth and who remains poor. Deaton (1990) observes that allowing heterogeneity in discount rates in a theory of consumption under borrowing constraints "divides the population into two groups, one of which lives a little better than hand to mouth but never has more than enough to meet emergencies, while the other, as a group, saves and steadily accumulates assents." For consumers whose impatience exceeds the rate of return to investing, remaining poor is optimal. Similarly, Lawrance (1991) proposes different rates of time preference as "one possible explanation for observed heterogeneity in savings behavior across socioeconomic classes," estimating that the poor are less patient from the fact that their consumption grows less quickly.

While time preference influences wealth directly through savings, it could also have indirect effects by shaping investments in education (*cf* Card, 1995) or health (*eg* Fuchs, 1982). The behavioral economics of time-inconsistency has further focused on implications of heterogeneity in discounting, present bias, and sophistication (O'Donoghue and Rabin, 1999). Thus, Ashraf et al. (2006) argue that, absent certain institutions, hyperbolic discounters are especially unlikely to save.

Yet, recent findings and theories in both psychology and economics suggest revisiting Fisher's suggestion. Indeed, poor people — like rich people — do often act impatiently. But, if there is an association between poverty and low behavioral control, could it partially reflect a causal effect of being poor on behavior, rather than the other way around? If so,

what might be the mechanism?

After reviewing three theoretical mechanisms proposed or inspired by the cognitive or social psychological literature, this paper will report three empirical studies. Primarily, these studies collectively identify a causal effect of poverty on behavior. Organized around this objective, the studies cannot definitively separate the three potential mechanisms. The best-supported explanation may be that, in these three cases, poverty appears to have made economic decision-making more consuming of cognitive control for poorer people than for richer people. Poverty causes difficult decisions, which deplete behavioral control.

Section 2 presents a randomized lab experiment in the field. By experimentally assigning participants to "wealth" and "poverty" in the lab, and manipulating whether participants made economic decisions, it identifies a causal effect of making economic decisions with small budgets. Section 3 reports a partially randomized field experiment. Participants, whose wealth was observed, made a real purchasing decision either before or after a task that measured their control. Choosing first was depleting only for the poorer participants, and this interactive effect was greatest for participants with the least cognitive resources. Section 4 describes patterns of secondary eating in the American Time Use Survey. Unlike other types of activity, shopping is associated with more secondary eating for poorer people, but not for richer.

1.1 Poverty and behavior

This paper is far from the first to suggest that poverty interacts perniciously with psychological limits and biases that are common to rich and poor people. Lewis (1959), studying Mexican slum dwellers, famously argued that poor people develop a "culture of poverty": a set of values that is adaptive to their poverty, but ultimately limiting.[1] Banerjee (2000) detailed theoretically that poverty might change behavior either by making the poor desperate,

[1]Other authors, outside of economics, suggesting that poverty could deter people from pursuing their own interests or escaping poverty include Orwell (1937), Scott (1977), and Karelis (2007).

or by leaving them vulnerable. Bertrand et al. (2004), Duflo (2006), and Hall (2008) all have proposed interactions between poverty and "behavioral" decision-making. Mullainathan and Shafir (2010), whose recent studies are most complementary to those in this paper, demonstrate greater depleting effects on math performance by New Jersey mall shoppers of an expensive hypothetical car repair decision than an inexpensive one, with the greatest effects on less wealthy shoppers.

Poverty may have many effects on behavior, many of which could be unrelated to behavioral control.[2] What this paper adds to this literature is, primarily, an experimental demonstration of a causal effect of poverty on behavioral control, induced by actual economic decision-making in the lab and in the field. I use "behavioral control" to include what psychologists and others write about as "willpower," "self-control," "self-regulation," or "executive" control or function: the pursuit of intentional behavioral goals, potentially despite automatic alternative behaviors or impulses.

Three theorized mechanisms from the psychological literature could be individually or jointly responsible for this effect of poverty. The three theoretical mechanisms are similar and complementary. Each proposes a limited mental resource that poverty occupies or consumes, leaving less capacity to guide or regulate behavior.

Figure 1 summaries the theories, which are detailed in the Appendix. The first mechanism, highlighting scarcity, proposes that attention is limited, and is directed to whatever domain is scarce. When poverty is the key form of scarcity facing a person, it can over-occupy her attention, resulting in worse performance.

The other two theories propose, more narrowly, that poverty is depleting because it changes the consequences of decision-making. The theory of ego depletion proposes that willpower is limited, and is consumed by resisting temptation or inhibiting behavior. Applied

[2]In economics, for example, Case (2001) finds effects of the South African pension on stress; Ray (2006) considers poverty's interactions with aspirations; Banerjee and Mullainathan (2010) propose that the sophisticatedly tempted poor will not save money that they know will be wasted in the future; Spears (2010b) finds large impacts of deliberation costs on the rural Indian poor.

to poverty by Ozdenoren et al. (forthcoming), this theory predicts that because poor people cannot afford to indulge, decisions require them to resist temptation, and therefore deplete their willpower.

Finally, the third theory suggests that it is cognitive control — the cognitive process, associated with working memory, that directs attention and inhibits automatic behaviors to pursue executive goals — that is the key limiting constraint. Because, in poverty, the same economic decision represents a more conflicting trade-off among more important priorities, economic decision-making is more difficult. These difficult decisions deplete cognitive control. For example, Wang et al. (2010) find that, relative to participants making easier decisions, participants making more difficult decisions involving conflicting trade-offs are more likely to subsequently choose unhealthy snacks and entertaining, rather than educational, movies.

The three studies below strike different balances among internal and external validity. Primarily, they separate a causal effect of poverty from the countervailing effects of time preference. Additionally, although not definitively, they collectively may distinguish among the theories in figure 1. Does poverty change behavior? If so, does it do so even when poor people do not have to make decisions? If poverty's effect depends on decision-making, is there only an impact when the decision-maker resists temptation or uses willpower, or is there an effect any time poverty has made an economic decision difficult? Does the effect depend on cognitive resources? Answering these questions may help indicate which mechanism or mechanisms may be at work in the cases studied here, but the focus will be on the direction of causality.

2 Lab experiment in the field

In July 2010, with the assistance of two research assistants, I conducted a lab experiment in Banswara, a small city in rural southern Rajasthan, in India. The experiment randomly assigned "wealth" and "poverty" in the experiment's context. This isolated an effect of

poverty, ruling out reverse causality or other confounds.

2.1 Procedure

The experiment had three stages. First, participants played a "store game" that required some of them to make an economic decision. Second, participants' behavioral control was measured on two tasks. The experiment is designed to estimate the effect of different versions of the store game on performance in the behavioral tasks. Third, participants were asked a set of economic and demographic survey questions. The experiment was conducted in Vagri, a language similar to Hindi. The research assistants did not know my hypotheses.

2.1.1 Store game: depletion

In the store game, participants were told to imagine that they are in a store with three items: a 500ml bottle of cooking oil, a tiffin (a metal food storage container), and a bundle of synthetic rope. Participants were randomly assigned to receive either one or two of these items — thus, to be relatively "poor" or "rich," although these terms were not used in the experiment. They were independently randomly assigned to either be allowed and required to choose which item or items they would receive, or to simply be told. Thus, each was randomly assigned to one of four conditions:

$$\{(rich, choice), (poor, choice), (rich, no\ \ choice), (poor, no\ \ choice)\}.$$

In the no-choice condition, goods were given in the same distribution as they were chosen by participants in the choice condition. In both conditions, it was made clear to participants that they did not have to pay for the items, either out of pocket or out of their participation payment. Randomization into receiving one or two items was done manually: the participant pulled a card with one or two dots out of a bucket. This was done to ensure that the unequal distribution of prizes would seem fair, but happened before the participant was told what

the randomization would determine to prevent anticipatory utility. Assignment to choice conditions was done randomly in advance with a computer, and participants were not told that having choice was a randomized experimental condition.

While not crucial to the experiment's primary purpose of isolating a causal effect of poverty, the oil and tiffin were used in the experiment because their interpretation might clarify the mechanism of poverty's effect. In this population, the cooking oil likely represented temptation: it had a slightly lower market price, but could be eaten to add good-tasting calories to food today. The tiffin, which offered no immediate benefit, would be an investment good, especially since almost all of the participants traveled to Banswara for work from a home village. The rope, while valuable and chosen by a few participants, had no special interpretation. If participants had the hypothesized preferences, "rich" participants could afford what they wanted and did not face a difficult economic trade-off, while "poor" participants had to choose between temptation and investment. Moreover, participants who choose the oil may be interpretable as not having used willpower to resist temptation (although they may have attempted and failed to resist).

2.1.2 Handgrip and Stroop task: behavioral control

After playing the store game, participants' performance was measured on two tasks: first squeezing a handgrip and then a Stroop-like task, which will be described below. The handgrip was commercially-purchased exercise equipment, consisting of two padded bars connected with a spring (see figure 2). Participants were asked to squeeze the handgrip as long as they could, and were stopped after three minutes if still squeezing. Squeezing time ranged from a minimum of 22 seconds to a maximum of 180, with a mean of 103. Prior research has often used handgrips to measure control.[3] For example, Muraven et al. (1998)

[3]According to Muraven et al. (1998) "squeezing a handgrip is a well-established measure of self-regulatory ability," because "prior research has concluded that maintaining a grip is almost entirely a measure of self-control and has very little to do with overall bodily strength" (777). Even if this is false, participants are randomly assigned to treatments.

find that after being asked to control their emotions during an upsetting movie, participants did not squeeze a similar handgrip as long as control-group participants did who merely watched the movie.

In the Stroop-like task, participants were shown cards on which a single-digit number was repeated several times. They were asked to say then number of times the number was shown, not the number itself. For example, if the card shows "5 5," the answer is "two" not "five." A research assistant first discussed two example cards with each participant and then flipped one at a time through eight cards in a fixed order. Participants' accuracy was recorded; scores ranged from 0 to 8 with a median of 6.

The canonical Stroop (1935) task involves naming the color of the ink that a word is printed in, not the color that the word names. This is difficult because it requires overriding the response of reading the color word, which is more automatic. For example, Richeson and Shelton (2003) show that experimental participants who have practiced self-regulation in an interracial interaction perform worse in a subsequent color-naming Stroop task. They find that performance is worsened only for those participants for whom the initial task would be depleting: in their experiment, people with high racial prejudice scores.

In this population of Vagri-speaking day laborers, reading words would not be automatic, as intended in the Stroop task, because many are illiterate. Flowers et al. (1979) modified the Stroop test to use numbers. Reading numbers is more automatic than counting even for illiterate people, due to their familiarity with money, so this Stroop-like task measures behavioral control. For example, Mullainathan and Shafir (2010) measure the difference in Tamil sugar cane farmers' performance on a numerical Stroop task before and after their harvest.

2.1.3 Participants

The experiment's 57 participants were adult men who were recruited in the early morning from an outdoor meeting-point that serves as an informal market for casual day labor.

Participants were hired to participate in the study as their work for the day and were paid 100 rupees, in addition to tea and snacks and the items they received in the experimental game. Participants waited in a large room with a monitor until called individually and in a random order to a smaller room for the experiment. Each participant was required to leave the study site after the experiment.

The experiment was conducted over two consecutive days. On the second day, participants were recruited from a meeting point and a bus stand located in a different part of the city from the first day's recruitment site. Each participant had his picture taken at the end of the experiment to ensure that he did not participate again the next day. No participant, during debriefing, reported having heard of this study before coming to the experiment. The research assistants and I believe that no participant had any information about the particular games, decisions, and tasks in the experiment.

2.2 Econometric strategy and validity

Does economic decision-making deplete cognitive resources of the poor and worsen subsequent behavioral control? The answer requires an estimate of the interaction between poverty and choice:

$$\bar{z}_i = \beta_0 + \beta_1 \text{poor}_i + \beta_2 \text{choice}_i + \beta_3 \text{poor}_i \times \text{choice}_i + \varepsilon_i, \tag{1}$$

where *poor* and *choice* are dummy indicators for experimental assignment and $\bar{z}_i$ is the mean of the z-score of participant i's performance in the two measures, squeezing time and Stroop accuracy.

Does poverty change behavior? The causal interpretation of the coefficients derives from the random assignment of experimental treatments. In particular, participants' budgets were randomly assigned, ruling out that choices determined their wealth at the lab store. Table 1 reports summary statistics for survey questions and verifies that, in this finite sample, randomization did not produce any statistically observable differences.

If economic decisions in poverty deplete resources used for behavioral control, then $\hat{\beta}_3$ should be negative. On the other hand, if scarcity itself drives any effect of poverty on depletion, the negative effect should be found in $\hat{\beta}_2 < 0$, not $\hat{\beta}_3$: no choice is necessary for poverty to worsen performance through this mechanism. Alternatively, $\hat{\beta}_2$ can be interpreted as controlling for experimenter demand, if participants who receive more are more willing to perform experimental tasks.

2.3 Results

Table 2 presents the results. Being randomly assigned to face a difficult economic decision with a small budget caused worse performance: $\hat{\beta}_3 < 0$. The table presents robust standard errors, but with such a small sample, nonparametric randomization inference can be used, randomly re-assigning outcomes to experimental groups. This procedure produces one- and two-sided p-values of 0.023 and 0.047 for the estimate of the coefficient on the interaction.[4]

Columns 2 and 4 include controls for age, whether married, ever school, and whether the participant correctly reported the day of the week. These controls are unnecessary in a randomized experiment and potentially biasing in a finite sample (Freedman, 2008), but are included as a robustness check. The lack of a direct effect of being assigned to receive two goods rather than one is evidence that the performance depletion is not due to an experimenter demand effect or reciprocity in which participants are more eager to please the experimenter after receiving a greater gift.

Through what mechanism might experimental poverty have had this effect? There was no direct effect of prizes being scarce but out of the participant's control: $\hat{\beta}_1$ cannot be distinguished from zero. Scarcity caused worse performance only when tests followed an economic decision.

To be depleting, must the decision use willpower? If control were depleted only through

[4]Results using only handgrip or Stroop performance, rather than their mean, are similar, but not statistically significant in this small sample

the use of willpower, and if the cooking oil were a tempting good, then there would be no interaction when restricting the sample to participants who chose or were assigned the oil if none of these participants used willpower to resist temptation. However, as columns 3 and 4 demonstrate, if anything the effect was larger for this group, although the effect is not statistically significantly different from the effect for the entire sample. While this suggests that limited cognitive control, not limited willpower specifically, was the mechanism, it cannot be ruled out that some participants who chose the oil may have first used willpower trying to resist temptation and then succumbed, or that multiple mechanisms were active.

3 Field experiment

In July and August 2010, the same two research assistants conducted a field experiment in rural villages of Banswara district, in Rajasthan, India. Participants made real spending decisions. Each day both surveyors traveled to two villages, one richer and one poorer. The surveyors offered participants a product for sale either before or after asking them to squeeze a handgrip and recorded economic and demographic information about participants. Decision-making proved depleting only for poorer participants. This interactive effect is greatest for participants with the least cognitive resources.

3.1 Procedure

The experiment was conducted in a 15-minute one-on-one interview in Vagri, during an unscheduled visit to the participant's home. The experiment had three components: an economic decision, squeezing a handgrip, and a set of survey questions that included a measure of cognitive resources (the Stroop task was not used). The order of the decision and the performance task was randomized. Half of each surveyor's participants in each village made the decision first, before squeezing the handgrip, and the other half squeezed the handgrip first, before learning about the decision.

In the economic decision, surveyors offered participants the opportunity to purchase a package of two 120 gram bars of handwashing and body soap for 10 rupees. The brand, Lifebuoy, is a brand marketed for health and the price was a 60 percent discount off of the retail price, so participants may have been tempted to take advantage of the special offer. Surveyors explained that they received the soap from a college for this project. They emphasized that participants could buy the soap if they wanted to, or not; that the decision was the participant's; and that the participant could take as long as necessary to decide. Most only deliberated for a few seconds. Forty-three percent of participants bought the soap, suggesting the soap was priced such that neither buying nor rejecting was an obvious response.

The handgrip task was the same as in the lab experiment. Participants were asked to squeeze a handgrip as long as they could, and were stopped after three minutes. Because half of the participants squeezed the handgrip before they were aware of the soap offer, the data can be used to estimate any direct effect of wealth on handgrip ability.

After demographic and economic survey questions, participants were given a working memory test. The surveyor read the participant a list of five simple words, asked a set of irrelevant survey questions, and then asked the participant to repeat as many of the words as he could remember. The mean participant remembered less than two words. Spears (2010a) found that a similar test predicted consumption behavior among South African pension recipients.

The two surveyors, both male, conducted 216 valid interviews with adult males from age 18 to 65. Interviews were conducted with the participant alone, and the surveyors were trained to discontinue the experiment if it could not be done alone, in order to promote anonymity and isolate individual decision-making, not social preferences. No more than one participant was interviewed from any household. Surveyors were instructed not to interview anybody who they suspected may have seen, overheard, or heard about the experiment before. Randomization of the order of experimental tasks was done by preparing two otherwise

identical versions of the survey form which were arranged into packets for each surveyor, for each village. Therefore, random assignment was stratified within village-surveyor combinations. Forms were sealed in opaque envelopes and surveyors were instructed never to look at the next form until a participant had consented to the interview.

3.2 Econometric strategy and validity

Does economic decision-making deplete performance for the poor but not for the rich? Again, the econometric question is whether the effect of poverty interacts with having made an economic choice:

$$\text{squeeze}_i = \beta_0 + \beta_1 \text{soap first}_{ij} + \beta_2 \text{poor}_{ij} + \beta_3 \text{soap first}_{ij} \times \text{poor}_{ij} + \alpha_j + X_{ij}\theta + \varepsilon_{ij}, \quad (2)$$

where *squeeze* is time squeezed in seconds, *soap first* is an indicator for making the economic decision before squeezing the handgrip, and *poor* represents one of the measures of poverty that will be used. Village fixed effects α_j and demographic and economic controls X_{ij} will be used in some specifications. Participants are indexed i and villages j. As before, I hypothesize that the interaction β_3 is negative. Three indicators of poverty will be used: being in the bottom half of the distribution of the asset count in this sample, the surveyor's assessment (before economic survey questions) of whether the participant's clothes are either clean or torn, and the first principal component of all socioeconomic questions (ownership of each asset as a dummy, clothes, native and mother's village).

Soap first is randomly assigned, so it is unlikely to be correlated with many other measures. Table 3 reports summary statistics by experimental group and finds no statistically or economically significant imbalance.

Wealth and poverty are not, of course, randomly assigned, and may be endogenously related to handgrip squeezing. Results will be shown with and without controls for age, age^2, household size, whether married, ever school, the measure of short term memory, an indicator

for already having soap in the house, an indicator for somebody in the household being sick in the past week, and fixed effects. The causal interpretation depends on the assumption that the interaction between poverty and assignment to decide first is independent of residual correlates of handgrip squeezing, conditional on these covariates.

The surveyors were instructed to travel to two villages together each day, one richer and one poorer, according to a schedule set in advance. The schedule was made by selecting the richest and poorest nearby villages, according to Indian census data. The assignment of richer and poorer villages to the morning or afternoon was randomly counterbalanced across days. This process ensured economic diversity in the sample and prevented wealth from being correlated with time of the interview.

If poverty influences performance by depleting cognitive resources in particular then the interactive effect should be least for participants with the most cognitive ability: their resources would be less likely to become consumed. This will be tested by estimating the full triple interaction of poverty and decision-making with the score on the working memory test. Working memory is closely related to cognitive control (Shamosh et al., 2008) and may be the resource used to maintain executive goals. Experimentally occupying participants' working memory results in more impulsive behavior (Getz et al., 2009), such as choosing chocolate cake over fruit salad (Shiv and Fedorikhin, 1999).

I hypothesize that the coefficient on the triple interaction should be positive: the negative effect of decision-making on performance for poor participants should be absent (less negative) for those with more cognitive abilities. While the working memory test came at the end of the experiment (to avoid influencing the main result) and may have been influenced by it, by the time participants took the working memory test they had completed both the handgrip task and the economic decision, so there would be no mechanical correlation between their interaction and the working memory score.

3.3 Results

Table 4 presents the main result of the field experiment. Before being offered soap, poorer and richer participants squeeze, on average, the same length of time. Deciding whether or not to buy the soap had no effect on handgrip behavior for richer participants, but caused poorer participants to squeeze for an average of 40 seconds less time, out of a mean of 108 seconds. Nonparametric randomization inference that re-randomized within surveyor-cluster combinations found a p-value of 0.001 for the first column of panel 1.

Various indicators of poverty find the same result. This result is robust to omitting participants who do not squeeze the handgrip at all (panel B) or to using Tobit estimates (panel C; squeezing time could not be below zero or above 180 seconds) and to including covariates. Indicating poverty with a different cutpoint in the asset count (the bottom third of participants, rather than the median) produces similar results ($\hat{\beta}_3 = -34$s; $t = -2.31$).

3.3.1 Depletability causing poverty?

In the lab experiment, a causal effect of "poverty" was demonstrated with random assignment. The relationship between depletion, depletability, and real-world poverty could be complex. Even if the poor do not have lower levels of, for example, willpower than the rich, might they have become poor or failed to escape poverty because their equal-sized stocks of regulatory resources are more readily depleted?

In the field experiment, poverty was not randomly assigned. But depletability causing poverty is plausible only for participants who have experienced economic mobility. Finding the same result in a subsample restricted to participants unlikely to have been sorted into poverty due to their depetability makes this reverse causality less plausible. One imperfect way to isolate the effect of poverty may be to focus on participants who match the *a priori* designation of their village as rich or poor from census data; another is to focus on those who still live in the village where their mothers lived when they were born, in a society where geographic and economic mobility are related.

Figure 3 presents results for those participants who report still living in their native village. Again, in both of these low-mobility sub-samples, decision-making had no effect on the rich, but reduced squeezing time for the poor (match census: $n = 131$, $\hat{\beta}_3 = -45$s, $t = -2.66$; mother lived in same village when participant born: $n = 95$, $\hat{\beta}_3 = -60$s, $t = -2.76$). This suggests that it is not the case that these results are explained by those who are most easily depleted by economic decisions being more likely to become poor.

3.3.2 Theories of poverty

Having demonstrated an effect of poverty, a secondary question is whether these data are more consistent with some of the three mechanisms than others.

The clearest evidence for a role for cognitive resources in in the triple interaction with working memory, reported in table 5. The key coefficient is the triple interaction, which shows the increase in $\hat{\beta}_3$ in equation 2 associated with each additional word remembered on the working memory test. Interpreting these results require summing the coefficients: for example, in the first specification, requiring a poor participant to make the economic decision would decrease squeezing time by an average of 54 seconds if he remembered no words on the test, but only by an average of 40 seconds if he remembered one word, with a similarly declining effect as the working memory score increases.

Economic decision-making worsened subsequent performance for the poor, but to a greater degree for those with lower cognitive resources than those with higher cognitive resources. As the table shows, this effect is robust to respecifications and nonparametric randomization inference finds one- and two-sided p values of 0.016 and 0.050 for the triple interaction.

As in the lab experiment, there was no direct effect of scarcity on performance. The coefficient on poverty is statistically significant in only one of 18 regressions, where it is positive. The effect was concentrated among those who made a decision.

Also like in the lab experiment, splitting the sample into the 86 participants who did and

109 participants who did not buy the soap could test whether the effect was concentrated on those who resisted temptation, if the discounted, name-brand soap can be interpreted as a tempting offer. Again, it was not: if anything, the interactive effect was about ten seconds greater in absolute value for participants who bought the soap, although this triple interaction is not significant ($t = -0.50$). Here, the effect was an effect of decision-making, with no evidence of an effect of scarcity overall or resisting temptation specifically.

4 Secondary eating while shopping

The first two studies show effects of poverty on behavioral control, but not as exhibited in a behavior with important implications: do handgrips and Stroop games matter? Moreover, the experimental studies demonstrate depletion resulting from a particular decision: perhaps other decisions are difficult and depleting for the rich? The third study addresses both of these concerns by studying a cross-section of Americans making whatever spending decisions they do at their level of wealth.

The American Time Use Survey (ATUS) provides representative data on what Americans do during the 24 hours in a day (*cf.* Hamermesh et al., 2005). It records each respondent's primary activity at every moment of one day. In particular, it records when participants are shopping, making economic decisions. This data is matched to household economic and demographic data from the Current Population Survey (CPS).

In 2008, an eating and health module also recorded whether participants were secondarily eating during each event. Secondary eating is "eating while doing other activities such as driving or watching TV" (Bureau of Labor Statistics, 2010). Secondary eating may sometimes reflect a failure of cognitive control: it is by definition not fully attended to, and may not reflect the deliberate pursuit of health goals.[5] "Mindless eating" without

[5]Hamermesh (2010), who terms secondary eating "grazing," argues from price theory that secondary eating will increase as earnings do (an increase in the opportunity cost of primary eating) and finds some evidence for this in the ATUS.

"consumption monitoring" facilitates overeating (Wansink and Sobal, 2007). For example, in an experiment conducted by Wansink et al. (2005), treatment group participants were unable to visually monitor their consumption because a hidden mechanism secretly refilled their soup bowls. These participants ate 73 percent more soup than control participants with normal, finite soup bowls, but did not believe they had eaten more or claim to feel more sated. In the ATUS data, a one-hour increase in daily time spent secondarily eating is linearly associated with a 0.09-point increase in BMI (two-sided $p = 0.085$).

Shopping and making purchases require economic decision-making. If this decision-making is particularly depleting for poorer people, and if secondary eating is a mindless behavior often in conflict with Americans' health goals, then this economic decision-making should especially encourage secondary eating among the poor. In the ATUS, shopping is accompanied by secondary eating among poorer people more often than among richer people.

4.1 Data

This section uses the 2008 wave of the ATUS. The ATUS is sponsored by the Bureau of Labor Statistics and conducted by the U.S. Census Bureau. It randomly selects households that have recently participated in the CPS, and then uniformly randomly selects an adult participant from within the household. Therefore, time use data can be matched with respondent data from the ATUS and household data from the CPS.

Each respondent details the previous day to an interviewer in a phone interview. Days are recorded from 4:00 am until 4:00 am on the day of the interview. Interviewers are trained to facilitate recall by working forwards and backwards and to record verbatim descriptions of activities. These activities are then classified according to a three-tier taxonomy; for example "household activities" include care for "lawn, garden, and houseplants," which includes maintaining "ponds, pools, and hot tubs." The median respondents reported 19 events in their days, 14 at the 25^{th} percentile and 25 at the 75^{th}.

The eating and health module was sponsored by the U.S. Department of Agriculture's

Economic Research Service and the National Institutes of Healths National Cancer Institute. It asked about subjective health, health indicators such as weight, and food sources and preparation. In particular, it asked whether the respondent was secondarily eating during each event in the daily diaries.

Of 6,923 respondents in the sample, household economic data is available for 6,711 and personal earnings data (including values of zero) is available for 4,134. Using the categories pre-coded in the CPS data, 13 percent of respondents lived in households with income less than 130 percent of the poverty line, and 23 percent lived in household with less than 185 percent; I will refer to the former group as "very poor" and the latter group as "poor."

Eight percent of all events involved secondary eating, compared with 4.6 percent of shopping events that are not grocery shopping. Among activities, secondary eating is most common at work, followed by during socializing or leisure. Of all events, 3.5 percent are shopping, and the average shopping event lasts twenty minutes. Richer people shop slightly more often, but not statistically significantly ($p = 0.36$).

4.2 Econometric strategy

Relative to other event types, is shopping accompanied by secondary eating more often for poorer participants than for richer participants? I estimate the linear probability regression

$$\begin{aligned} secondary_{it} \;=\; & \beta_0 + \beta_1 shopping_{i,t} \times richer_i + \beta_2 shopping_{i,t} + \beta_3 richer_i + \\ & \theta X_i + \vartheta Y_{i,t} + \alpha_1 hour_t + \alpha_2 day_t + \varepsilon_{i,t}, \end{aligned} \tag{3}$$

where i indexes respondents and t indexes events in i's day. *Secondary* is an indicator of secondary eating by i during event t; *shopping* indicates whether t was a shopping event for i, and *richer* is either an indicator that the participant's household is not poor in the CPS or her weekly earnings. In most specifications, I include only non-grocery shopping in *shopping* to prevent a confounding effect of food availability, but results will be shown to be robust to

including all shopping. Events are categorized by the hour of their midpoint (for example, an event that starts at 11:30 am and lasts until 1:30 pm is categorized under 12pm); α_1 includes fixed effects for these 24 categories. With α_2, day of the week fixed effects are included.

Because poverty varies at the respondent level, not the event level, respondent fixed effects cannot be used. Standard errors are clustered by respondent. All estimates are weighted according to the recommended sampling weights. The main results use individual events as t, the units of observation, but to protect against a mechanical effect of the number of events in the day, I also include results with a constructed balanced-panel data set where each hour is an observation and indicators report whether an activity occurred at all during that hour.

Identification in this experiment depends on the assumption that the interaction of poverty and shopping is independent of other correlates of secondary eating, conditional on fixed effects and other controls. In addition to ordinary omitted variable bias, these controls are intended to rule out mechanical correlations due to relationships among events in a day. Respondent controls X_i and event controls $Y_{i,t}$ are added separately. Event controls are the duration of the event (as a quartic polynomial), indicators for being at home or at work, and an indicator for being with a child. Respondent demographic and economic controls are sex, age, weight, BMI, and number of children, as well as indicators for being employed, being out of the labor force, and being employed full-time. Respondent controls also include details of how the respondent spent that day: the number of events reported (quadratically); total time spent alone, with friends, and with family; time spent in primary eating (quadratically); an indicator for having cooked that day and time spent cooking.

4.3 Results

Figure 4 summarizes the mean results without covariates: although around 8 percent of non-shopping events involve secondary eating for the rich and poor alike, during shopping the poor are more than a third more likely (about 6 percentage points, rather than about 4.5) to be secondarily eating than the rich.

Table 6 confirms that this interaction is similar and statistically significant even after including a range of controls. The estimate that the association between shopping and secondary eating is about two percentage points greater for poor people is robust to various respecifications.[6]

Beginning in column three, a similar interaction between poverty and housework is included as a placebo. It is not statistically distinguishable from zero and does not change the estimates for shopping. Additionally, measuring economic well-being with personal earnings produces a similar interaction: for participants with mean earnings, shopping is associated with a 1.2 percentage point increase in secondary eating, an association that becomes negative for participants with weekly earnings more than $81 above the mean.

The right-side panel excludes events when the respondent is sleeping or primarily eating, as these cannot involve secondary eating, so they cannot show a difference across the rich and poor. Effects are similar, even greater in magnitude for the main specification. Including grocery shopping among shopping or restricting the indicator of poverty to the poorest produces comparable estimates.

Table 7 reports a set of placebo regressions. The final specification from column 5 of table 6 is repeated with events during which the theory does not predict an interactive effect: leisure time, watching tv, doing housework, being at work, and the lag of the shopping variable. None of these event types statistically significantly reproduces the negative interaction with shopping. The positive coefficient on work may reflect different types of jobs, or a spurious result of running many regressions.

Table 8 estimates the same specification with hours, rather than events, as the units of observation. For each hour in each respondent's day, I constructed indicators of whether the respondent went shopping during any part of that hour and whether the respondent did any secondary eating during that hour, as well as similar indicators for the covariates.

[6]While not reported in the table, estimating the logit of secondary eating in equation 3 produces a similar marginal effect of 2.8 percentage points for $\hat{\beta}_1$, with a two-sided p-value of 0.062.

Richer respondents are more likely than poorer respondents to be secondarily eating while not shopping, but less likely while shopping.

As a final robustness check, the right-hand panel includes results for secondary drinking. Secondary drinking is more difficult to interpret because it includes both, for example, soft drink consumption — which may be inconsistent with health goals — and coffee consumption, which may promote goals and occurs often and continuously during events of long duration. Secondary drinking accompanies 16 percent of events. Nevertheless, a negative coefficient may be expected if secondary drinking is done mindlessly or impulsively. The results show that during shopping the frequency of secondary drinking increases for poorer people, but does not change or slightly decreases for richer people.

4.4 Interpretation

These results are consistent with the prediction that economic decision-making will cause depleted behavioral control specifically among the poor, but it cannot be ruled out that they are driven by an omitted correlation. For example, poor people may go to different stores, shop differently, or have different health goals.

Unlike in the field experiment, no measure of working memory is available to isolate a specifically cognitive mechanism. However, in general and as before, poor people are not unconditionally more likely to be secondarily eating, suggesting that any effect is not caused by scarcity alone. Moreover, there is no direct evidence that respondents were resisting temptation while shopping, only that they were making purchasing choices. These findings are consistent with the theory of limited cognitive control, but a combination of the three cannot be ruled out.

5 Conclusions and discussion

Economic decision-making had negative effects on controlled behavior when participants were poorer. This may be because for poorer participants, decisions required more difficult trade-offs, and were more depleting of cognitive resources.

Random assignment of experimental "poverty" in the lab experiment and regression-controlled and subsample analysis in the field experiment and survey data underscore that, in these data, poverty causes depleted performance, rather than the other way around. Results show little specific support for theories of a particular role for depletable willpower or a generic effect of scarcity. In the field experiment, heterogeneous effects according to working memory are consistent with a theory of poverty's effect on cognitive control. However, all three of these mechanisms could be active and important, especially in other populations or contexts.

Certainly rich people, too, sometimes face difficult economic decisions; these may sometimes be depleting. However, the decisions studied in the experiments had behavioral effects even at tiny financial magnitudes, small enough that the poor must face them routinely. Moreover, the time-use data found that shopping's depleting effect was limited to the poor when rich and poor respondents made whatever purchases they made in a representative day. Although a richer person's budget may enable her to face a difficult choice between, perhaps, two vacations, she also has the option of not making this choice at all. If, as the lab experiment suggests, even routine food decisions are costly and difficult for the very poor, then their depleting effect is more inescapable.

These studies add to the growing evidence for a cognitive dimension of what is typically considered time preference. Additionally, they could be important for policy. Gilens (1999) summarizes his research on American political attitudes: "In large measure, Americans hate welfare because they view it as a program that rewards the undeserving poor" — here, the lazy, impulsive, myopic poor. This view that poverty is caused by bad decisions and bad

behavior is commonly held and politically influential, but may be moderated by evidence of the potential complexity of the causal ties between poverty and behavioral control.

A Theories of poverty and behavioral control

A.1 Scarcity & limited attention

Mullainathan and Shafir (2010) propose that poverty is psychologically important because it is a form of scarcity. Scarcity, they suggest, causes people to experience stress and to focus their attention on the domains where resources seem most scarce. Because attention is limited, people attend to what is scarce to the exclusion of other potentially important decisions. Importantly, in this model poverty is merely one form of scarcity; limits to, for example, a busy person's time or a dieter's meals would produce similar psychological results.[7]

Mullainathan and Shafir report interviews with Indian sugar cane farmers before and after their harvests: before, outcomes are uncertain and resources are scarce; after, some uncertainty is resolved and resources are more plentiful if farmers were credit constrained. After the harvest, farmers exhibit less stress and perform better on the Stroop test, which requires participants to override an impulsive, but wrong, answer with a deliberative response.

A.2 Ego-depletion & limited willpower

"Ego-depletion" is Baumeister et al.'s (1998) name for their theory that self-control is produced with a limited willpower stock that is temporarily used up when people regulate their emotions or resist temptation. Thus, because "exerting self-control may consume self-control strength, reducing the amount of strength available for subsequent self-control efforts," Mu-

[7]This theory is related to, but not identical to, Banerjee and Mullainathan's (2008) model of agents who can allocate a unit of attention to home or work. Because poor people are unable to afford security at home, they are distracted from being productive at work whether or not a problem ultimately arises at home.

raven and Baumeister (2000) suggest that "self-control operates like a muscle."

While ego-depletion was not originally intended as a theory of poverty, the need for self-control may arise particularly often for the poor. Spending money and spending willpower can be substitutes. Many offers of tempting purchases that are easily affordable for richer people require a poorer person to use willpower and save her money instead.[8] If willpower is limited, and if a poorer person can afford less indulgence, then poverty will deplete self-control when the poor face expensive temptation.

Ozdenoren et al. (forthcoming) develop an economic model of the optimal response to temptation given finite willpower. Even a poorer person with the same amount of willpower as a richer person must resist temptation more often. Therefore their model predicts that "behavioral differences between rich and poor people sometimes attributed to differences in self-control skills may reflect wealth differences and nothing more."

A.3 Difficult choices & limited cognitive control

Cognitive resources play an important role in economic behavior because they facilitate economic deliberation and global decision-making. Burks et al. (2008) find that in addition to choosing larger, later payments in the lab, truck drivers with better performance on cognitive tests are more likely to keep their job long enough to avoid incurring a costly debt for training. In a field experiment among pension recipients in Cape Town, consumption declines less steeply across the pension month among participants who show more cognitive ability on a working-memory test (Spears, 2010a). Lab experiments that manipulate cognitive resources by depleting them find similar results. Together, these results suggest that behaviors commonly attributed to attitudes such as "impatience" may actually reflect cognitive regulation of behavior.

[8]For example, Banerjee and Mullainathan (2010) explore implications of agents' sophistication about their "declining temptation" — the idea "that the fraction of the marginal dollar that is spent on temptation goods decreases with overall consumption," where temptation goods are goods in a multi-period/multi-self model that only generate utility for the self of the period when they are consumed. They justify this assumption partially with the observation that temptations such as tasty foods are satiable.

A third mechanism by which poverty could influence subsequent decision-making is by taxing cognitive control. Cognitive control facilitates "the ability to select a weaker, task-relevant response. . . in the face of competition from an otherwise stronger, but task-irrelevant one" (Miller and Cohen, 2001). Cognitive control responds to conflict in mental processing (Botvinivk et al., 2001), is used to make decisions (McGuire and Botvinick, 2010), and may employ working memory to direct attention, inhibit impulses, override automatic processes, and maintain goals. Cognitive control is limited (Monsell, 2003), and may be "depletable" in the sense that monitoring processes can be occupied (Robinson et al., 2010).

Experimental evidence confirms that difficult choices are cognitively costly. Vohs et al. (2008) report experiments in which, after making choices, participants showed less stamina and persistence and more procrastination than a control group that did not choose. This is unsurprising given evidence of deliberation costs, especially costs of economic decision-making. Tversky and Shafir (1992) find that people avoid making difficult trade-offs, deferring choice when options are in conflict, such that no option dominates another. Kool et al. (forthcoming) demonstrate that participants choose actions to avoid cognitive demand, but that this inclination responds to incentives. In a field experiment in rural India, Spears (2010b) finds that price sensitivity depends on the interaction between price and decision costs. The results are consistent with a model predicting that, faced with a given offer, poorer people will pay deliberation costs more often and be more likely to forego valuable opportunities.

Limits to cognitive control matter to poor people because poverty raises the stakes of many economic decisions. For poorer people, the same economic decision may represent a more difficult trade-off between more valuable alternatives with less margin for error. Such decisions would demand more costly deliberation — including, but not only, when emotions must be regulated. If cognitive resources are limited, this would leave less remaining cognitive control for other decisions or behaviors.

To propose that cognitive control is limited is to agree with the other two models that

attention and behavioral regulation are limited, because these are features of cognitive control. Emphasizing limits to this broader resource highlights that resisting temptation and difficult decision-making both can be depleting. Moreover, choosing under poverty could impair performance across the goal-seeking tasks for which cognitive control is important.

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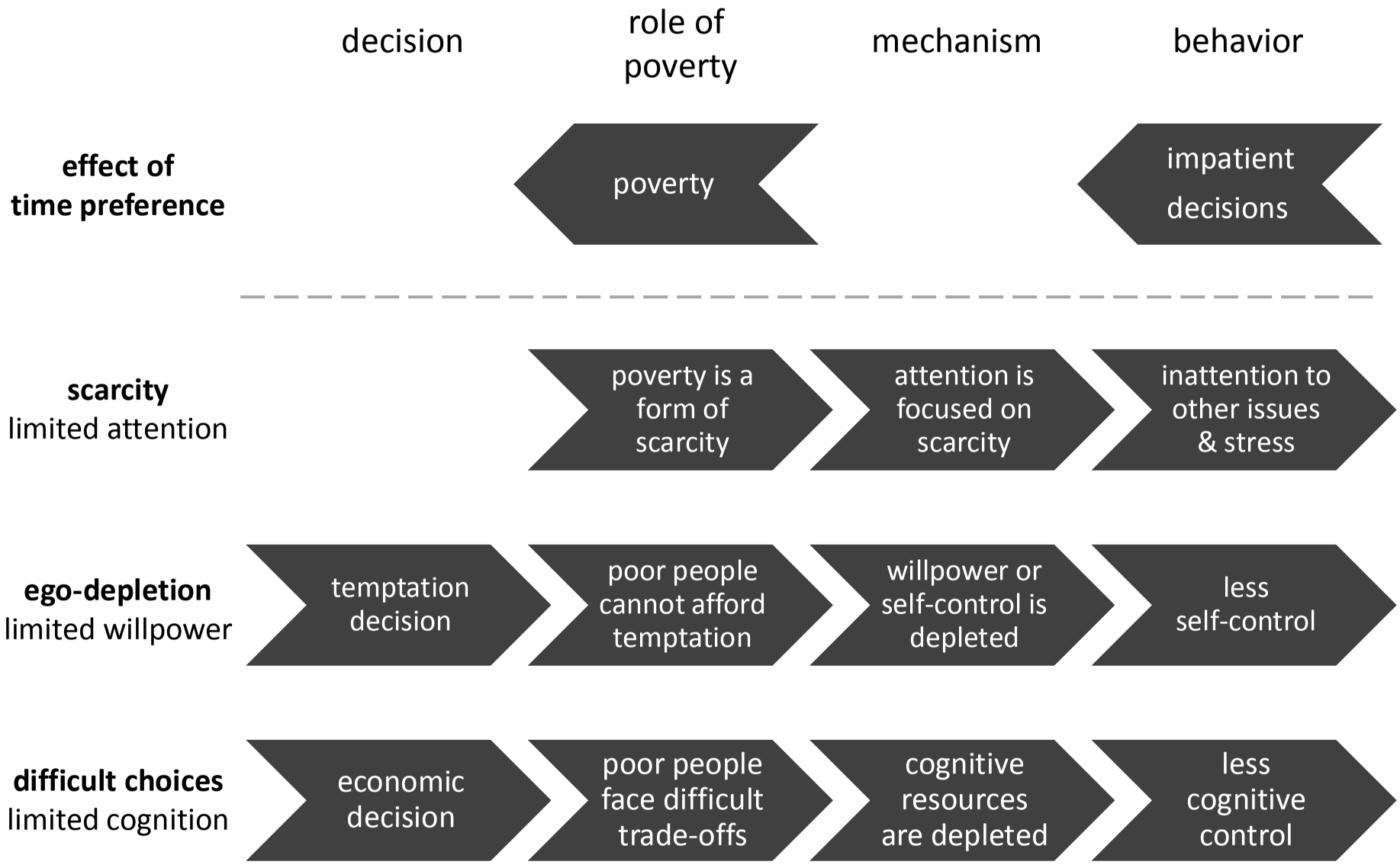

Figure 1: Theories of poverty and decision-making

Figure 2: Two handgrips, similar to the ones used in the experiments

Table 1: Lab experiment: Summary statistics by experimental group

		no choice		choice		
		rich	poor	rich	poor	$F_{3,53}$
age	26.65	27.54	24.00	26.73	28.00	0.64
married	0.95	0.92	0.92	0.93	1.00	0.40
school	0.70	0.69	0.62	0.80	0.69	0.37
knows day of week	0.61	0.69	0.54	0.47	0.75	1.08
n	57	13	13	15	16	

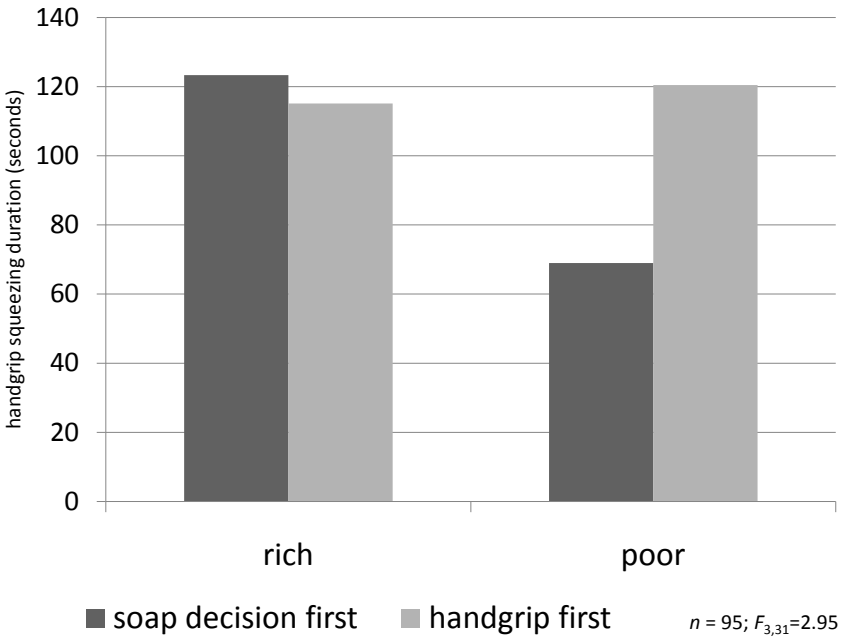

Figure 3: Field experiment: Results for participants who live in natal villages

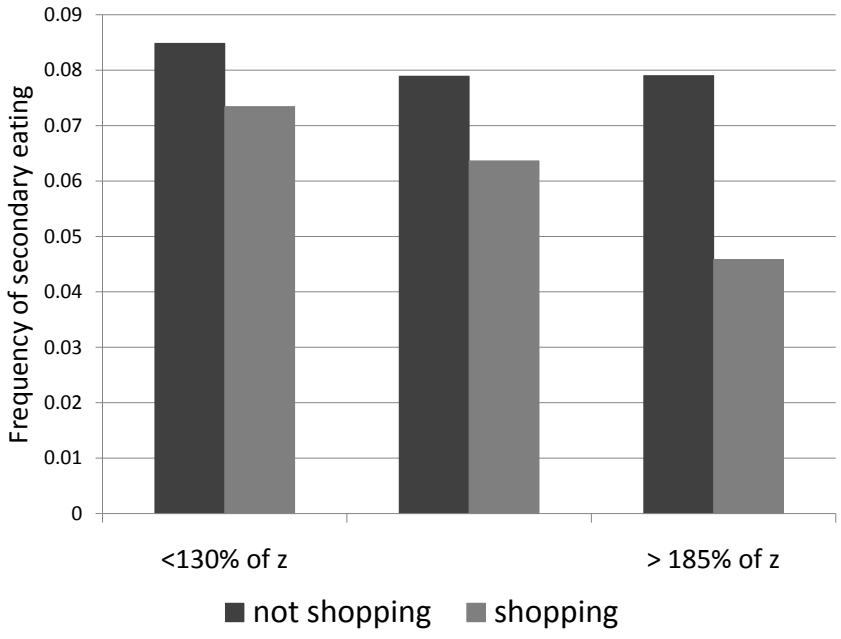

Figure 4: Time use: Secondary eating by poverty status

Table 2: Lab experiment: Performance z-scores by experimental treatment

	(1)	(2)	(3)	(4)
	full sample		chose or given oil	
poor	0.0627	0.0835	0.541	0.794
	(0.253)	(0.260)	(0.463)	(0.362)
choice	0.532	0.565	0.519	0.577
	(0.213)	(0.209)	(0.225)	(0.226)
poor × choice	-0.726	-0.736	-1.402	-1.645
	(0.342)	(0.370)	(0.550)	(0.451)
covariates		✓		✓
c	-0.125	0.385	-0.125	0.452
	(0.164)	(0.348)	(0.167)	(0.341)
n	57	57	36	36

Robust standard errors in parentheses. Covariates are age, whether married, ever school, and whether the participant correctly reported the day of the week. The dependent variable is the mean of the respondent's standardized z score of handgrip and Stroop performance.

Table 3: Field experiment: Summary statistics by randomized group

	$\bar{x}$	handgrip first	decision first	t
age	38.3	38.4	38.3	0.07
household size	5.72	5.64	5.81	0.58
asset count	4.82	4.65	5.00	1.08
soap in house	0.74	0.70	0.78	1.40
member sick in last week	0.51	0.48	0.54	0.89
bought soap	0.43	0.41	0.44	0.37
memory test	1.54	1.41	1.67	1.44
order within cluster	4.40	4.25	4.55	1.01
match rich/pooor village	0.61	0.62	0.59	0.63
n	216	110	106	

Table 4: Poverty-mediated effects of economic decision-making on handgrip behavior

Panel A: Seconds squeezed handgrip, OLS

	low assets		clothes dirty & torn		poverty index	
soap first	8.317	1.386	-12.36	-16.77**	-13.83**	-17.04**
	(8.001)	(8.911)	(8.116)	(7.357)	(6.497)	(6.737)
poverty	9.103	19.07	-11.15	0.374	-2.056	0.203
	(10.95)	(11.48)	(15.36)	(15.87)	(2.416)	(3.650)
interaction	-47.14***	-43.56***	-41.76*	-39.98*	-6.652**	-6.780*
	(13.45)	(12.09)	(22.14)	(22.62)	(3.041)	(3.495)
covariates		✓		✓		✓
c	111.2***	11.13	118.1***	17.47	118.0***	26.33
	(6.688)	(47.35)	(5.779)	(48.68)	(4.730)	(42.01)
n	216	211	216	211	216	211

Panel B: Seconds squeezed handgrip, conditional on squeezing, OLS

	low assets		clothes dirty & torn		poverty index	
soap first	6.388	1.556	-1.001	-5.860	-3.047	-8.281
	(7.564)	(8.581)	(7.278)	(7.300)	(6.168)	(6.594)
poverty	6.592	10.05	-4.010	-2.130	0.331	-0.689
	(9.056)	(10.15)	(12.93)	(13.92)	(2.163)	(2.984)
interaction	-21.69*	-22.44*	-37.96*	-39.31*	-3.207	-3.426
	(11.87)	(12.03)	(18.99)	(21.95)	(2.928)	(3.274)
covariates		✓		✓		✓
c	118.0***	44.23	122.2***	46.79	121.2***	51.93
	(5.732)	(40.27)	(5.553)	(41.60)	(4.794)	(37.91)
n	195	190	195	190	195	190

Panel C: Seconds squeezed handgrip, Tobit

	low assets		clothes dirty & torn		poverty index	
soap first	7.108	-0.928	-17.68*	-22.96***	-19.74**	-23.45***
	(11.17)	(10.63)	(9.207)	(8.411)	(8.177)	(7.747)
poverty	11.30	23.70*	-10.90	3.680	-2.256	0.628
	(11.75)	(13.31)	(17.18)	(16.33)	(2.908)	(4.094)
interaction	-57.50***	-53.56***	-52.45*	-50.79*	-8.249**	-8.634**
	(17.14)	(15.66)	(27.11)	(25.89)	(3.936)	(3.940)
covariates		✓		✓		✓
c	113.4***	9.871	121.4***	15.84	121.6***	27.08
	(8.435)	(50.64)	(6.201)	(50.21)	(5.547)	(49.61)
n	216	211	216	211	216	211

Clustered standard errors in parentheses (33 village-surveyor combinations). * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Covariates are age, age^2, household size, whether married, ever school, a measure of short term memory, an indicator for already having soap in the house, an indicator for somebody in the household being sick in the past week, and a fixed effect. In panels A and C, not squeezing is counted as zero seconds.

Table 5: Squeezing in seconds: Interactive effect depends on cognitive resources

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A: OLS			Panel B: sqz > 0			Panel C: Tobit		
decision first	25.58	33.80	25.78	8.950	12.09	6.510	28.51	38.10*	27.47
	(21.16)	(21.74)	(22.82)	(17.58)	(18.96)	(19.95)	(19.89)	(20.94)	(22.24)
poor	41.01**	37.65**	30.09*	27.60	22.02	15.64	43.95*	38.42	27.95
	(17.40)	(18.14)	(17.69)	(18.58)	(16.66)	(16.84)	(22.44)	(24.13)	(24.23)
decision × poor	-79.29***	-87.68***	-77.69***	-27.87	-37.92	-34.36	-94.32***	-103.6***	-91.32***
	(25.97)	(25.89)	(26.60)	(23.60)	(24.00)	(24.57)	(24.13)	(24.38)	(24.27)
cognition	17.19***	24.31***	15.79***	10.13	15.77***	9.925*	19.10**	27.17***	16.07**
	(5.975)	(5.354)	(5.950)	(6.155)	(5.172)	(5.695)	(7.444)	(6.845)	(7.806)
poor × cognition	-16.41*	-16.73**	-15.07**	-11.20	-12.89*	-11.40	-14.91	-14.41*	-12.07
	(9.059)	(7.862)	(7.549)	(8.645)	(7.480)	(7.422)	(9.631)	(8.658)	(8.587)
decision × cognition	-11.02	-16.13*	-13.35	-3.029	-5.731	-4.022	-12.96	-18.90**	-15.18
	(8.336)	(8.360)	(8.711)	(8.181)	(7.643)	(7.945)	(8.459)	(9.041)	(9.509)
decision × poor	*24.80***	*35.25****	*35.07****	*6.179*	*17.35*†	*18.44**	*26.81***	*38.27****	*38.19****
× cognition	*(12.33)*	*(11.62)*	*(11.44)*	*(10.79)*	*(11.08)*	*(11.11)*	*(11.61)*	*(12.49)*	*(12.08)*
age			-1.317***			-0.924***			-1.693***
			(0.357)			(0.311)			(0.402)
cluster FE		✓	✓		✓	✓		✓	✓
c	78.52***	55.80**	126.8***	98.00***	84.39***	134.3***	76.66***	53.51**	145.7***
	(14.58)	(27.48)	(31.54)	(14.51)	(26.67)	(29.33)	(17.97)	(21.42)	(33.57)
n	214	214	212	193	193	191	214	214	212

Clustered standard errors in parentheses (33 village-surveyor combinations). "Cognition" is score 0-5 on a working memory test. "sqz > 0" is OLS conditional on squeezing a positive amount of time. In panels A and C, not squeezing is counted as zero seconds.

* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; † $p = 0.12$.

Table 6: Secondary eating: linear probability during an event

	Full sample				Subsample of events excluding sleeping and primary eating			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
shopping:	no groceries	no groceries	no groceries	no groceries	no groceries	inc. groceries	no groceries	no groceries
richer:	not poor	not poor	not poor	earnings	not poor	not poor	not v. poor	earnings
shopping × richer	-0.0228**	-0.0226**	-0.0234**	-0.0151*	-0.0251**	-0.0162*	-0.0192†	-0.0118††
	(0.0113)	(0.0112)	(0.0112)	(0.00782)	(0.0108)	(0.00844)	(0.0124)	(0.00775)
shopping	0.0314***	0.0313***	0.0318***	0.0122**	0.00645	-0.00828	0.00340	-0.0152***
	(0.0102)	(0.0101)	(0.0101)	(0.00550)	(0.00972)	(0.00747)	(0.0116)	(0.00544)
richer	0.00737*	0.00848**	0.00928**	0.00182	0.00760	0.00747	0.00585	-0.000127
	(0.00396)	(0.00422)	(0.00429)	(0.00434)	(0.00554)	(0.00559)	(0.00676)	(0.00546)
housework × richer			-0.0138	-0.00357	-0.00693	-0.00686	-0.0108	-0.00552
			(0.0101)	(0.00813)	(0.0104)	(0.0104)	(0.0135)	(0.00803)
housework			0.00448	-0.00816	-0.0189**	-0.0191**	-0.0148	-0.0257***
			(0.00906)	(0.00547)	(0.00935)	(0.00937)	(0.0127)	(0.00566)
time & day FEs	✓	✓	✓	✓	✓	✓	✓	✓
event controls	✓	✓	✓	✓	✓	✓	✓	✓
respondent controls		✓	✓	✓	✓	✓	✓	✓
c	0.0641***	0.0956***	0.0949***	0.0888***	0.120***	0.120***	0.120***	0.112***
	(0.00986)	(0.0141)	(0.0141)	(0.0177)	(0.0170)	(0.0170)	(0.0176)	(0.0217)
n	138,309	138,309	138,309	85,093	111,360	111,360	111,360	68,858
respondents	6,711	6,711	6,711	4,134	6,711	6,711	6,711	4,134
R^2	0.0907	0.0925	0.0925	0.104	0.155	0.156	0.155	0.166

Standard errors clustered by respondent in parentheses. Earnings are demeaned and in $100 per week. Poverty reflects household CPS categories.

Two-sided p-values: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; † $p = 0.12$; †† $p = 0.13$.

Table 7: Secondary eating: shopping versus other event types

	(1)	(2)	(3)	(4)	(5)	(6)
event:	shopping	lag shopping	leisure	tv	housework	at work
interaction	-0.0251**	-0.0132	-0.00510	-0.00247	-0.00599	0.0417**
	(0.0108)	(0.0149)	(0.00836)	(0.0118)	(0.0104)	(0.0189)
event	0.00645	0.0570***	0.0629***	0.0573***	-0.0194**	-0.00735
	(0.00972)	(0.0135)	(0.00775)	(0.0107)	(0.00936)	(0.0160)
hh not poor	0.00760	0.00675	0.00916+	0.00828	0.00672	0.00387
	(0.00554)	(0.00555)	(0.00554)	(0.00556)	(0.00560)	(0.00553)
c	0.120***	0.121***	0.0997***	0.109***	0.121***	0.123***
	(0.0170)	(0.0170)	(0.0174)	(0.0172)	(0.0170)	(0.0169)
n	111,360	110,890	111,360	111,360	111,360	111,360

Standard errors clustered by respondent in parentheses. Poverty reflects household CPS categories.
Two-sided p-values: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

Table 8: Secondary eating and drinking: linear probability during an hour

	Secondary eating				Secondary drinking			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full sample		Not sleeping		Full sample		Not sleeping	
interaction	-0.0453*	-0.0359†	-0.0540**	-0.0440*	-0.0639**	-0.0555**	-0.0550**	-0.0482*
	(0.0255)	(0.0250)	(0.0266)	(0.0259)	(0.0265)	(0.0254)	(0.0266)	(0.0256)
shopping	-0.0272	0.0416*	-0.0493**	0.0168	0.0317	0.0787***	-0.00434	0.0342
	(0.0228)	(0.0224)	(0.0236)	(0.0231)	(0.0239)	(0.0229)	(0.0236)	(0.0226)
not poor	0.0280***	0.0362***	0.0287***	0.0457***	0.0212**	0.0217**	0.0200	0.0266**
	(0.00693)	(0.00666)	(0.0102)	(0.00965)	(0.00981)	(0.0102)	(0.0131)	(0.0134)
time & day FEs	✓	✓	✓	✓	✓	✓	✓	✓
hour controls		✓		✓		✓		✓
respondent controls		✓		✓		✓		✓
c	-0.0352***	0.236***	0.138***	0.506***	-0.0190	0.102***	0.258***	0.414***
	(0.0126)	(0.0236)	(0.0284)	(0.0413)	(0.0175)	(0.0360)	(0.0358)	(0.0558)
n (hours)	140,928	140,928	91,999	91,999	140,928	140,928	91,999	91,999
respondents	6,711	6,711	6,585	6,585	6,711	6,711	6,585	6,585

Standard errors clustered by respondent in parentheses. Poverty reflects household CPS categories.

Two-sided p-values: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; † $p = 0.15$.