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Economic Decision-Making in Poverty Depletes Behavioral Control

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Economic Decision-Making in Poverty Depletes Behavioral Control*

Dean Spears

Abstract

Economic theory and conventional wisdom 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 of this empirical paper is to isolate the direction of causality from poverty to behavior. Three similar possible theoretical mechanisms, found in the psychology and behavioral economics literatures, cannot be definitively separated. One supported theoretical explanation is that poverty, by making economic decision-making more difficult, depletes cognitive control.

KEYWORDS: poverty, decision-making, self-control, willpower, behavioral economics, lab and field experiments, India, time use, ATUS

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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, generally have seen time preferences as relatively fixed properties of persons: causally prior to behavior and important determinants of who accumulates wealth and who remains poor.[1] 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 assets." 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 (*e.g.* 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, Karlan, and Yin (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 poverty on behavior, rather than the other way around? If so, what might be the mechanism?

This paper reports results of three new empirical studies. The paper's contribution is to identify, collectively among the studies, a causal effect of

[1] A notable theoretical exception is Becker and Mulligan's (1997) model of optimal choice of "future-oriented capital."

poverty on behavior.[2] To our knowledge, this is the first paper in this literature in which participants make real economic decisions in a randomized experiment. Although causality is well-identified, the results are potentially consistent with three theoretical mechanisms from the psychology and behavioral economics literature. These are reviewed in the next subsection, and explored in depth in an appendix. The best-supported explanation may be that, in these three studies, poverty appears to have made economic decision-making more consuming of cognitive control for poorer people than for richer people: poverty makes even routine economic decisions more difficult.

Section 2 presents a randomized lab experiment in the field, where participants visited an experimental "store." 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 behavioral 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 an empirical paper, tailored to identify a causal effect of poverty on behavior, as theorized by many prior observers. So, 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 ulti-

[2] An important literature in psychology has also provided some evidence that poverty can diminish behavioral control by studying children (*e.g.* Brody, Flor, and Gibson, 1999, Evans, Gonnella, Marcynyszyn, Gentile, and Salpekar, 2005, Evans and English, 2002). While it remains true that children in poor households differ on average from children in non-poor households in many ways, including family background, these studies of children do rule out the confounding *economic* mechanism that the children's own impatience caused their poverty.

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mately limiting.[3] Banerjee (2000) detailed theoretically that poverty might change behavior either by making the poor desperate, or by leaving them vulnerable. Bertrand, Mullainathan, and Shafir (2004), Duflo (2006), and Hall (2008) all have recently proposed interactions between poverty and "behavioral" decision-making. Mullainathan and Shafir (2010), whose recent, prior studies are most related 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.[4] What this paper adds to this literature is, primarily, an experimental demonstration of a causal effect of poverty on behavioral control, in particular. This paper will use "behavioral control" to include what psychologists and others write about as "willpower," "patience," "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, well-established within 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 remaining capacity to guide or regulate behavior. These theories are not all new psychology, but they were not, in general, originally developed to be applied to poverty. To a psychologist familiar with the original theories, some of the studies in this paper might seem like replications: they are special applications to economics that show that well-known processes can also be implicated by poverty.

[3]Other authors, outside of economics, suggesting ways that poverty could deter people from pursuing their own interests or escaping poverty include Orwell (1937), Scott (1977), Gaventa (1982) and Karelis (2007).

[4]In economics, for example, Bernheim, Ray, and Şevin Yeltekin (1999, 2011) model mechanism design under time inconsistency, demonstrating that credit-constrained poor people may be unable to implement personal rules that discipline their patience; 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 those poor people who are sophisticated about their time-inconsistent temptation will not save money that they know will be wasted in the future; Spears (2011a) finds that, given reference dependent preferences, low expectations among the poor deter labor supply in Cape Town; and Spears (2011b) finds that poverty interacts with aversion to compound lotteries to deter aspirations in El Salvador.

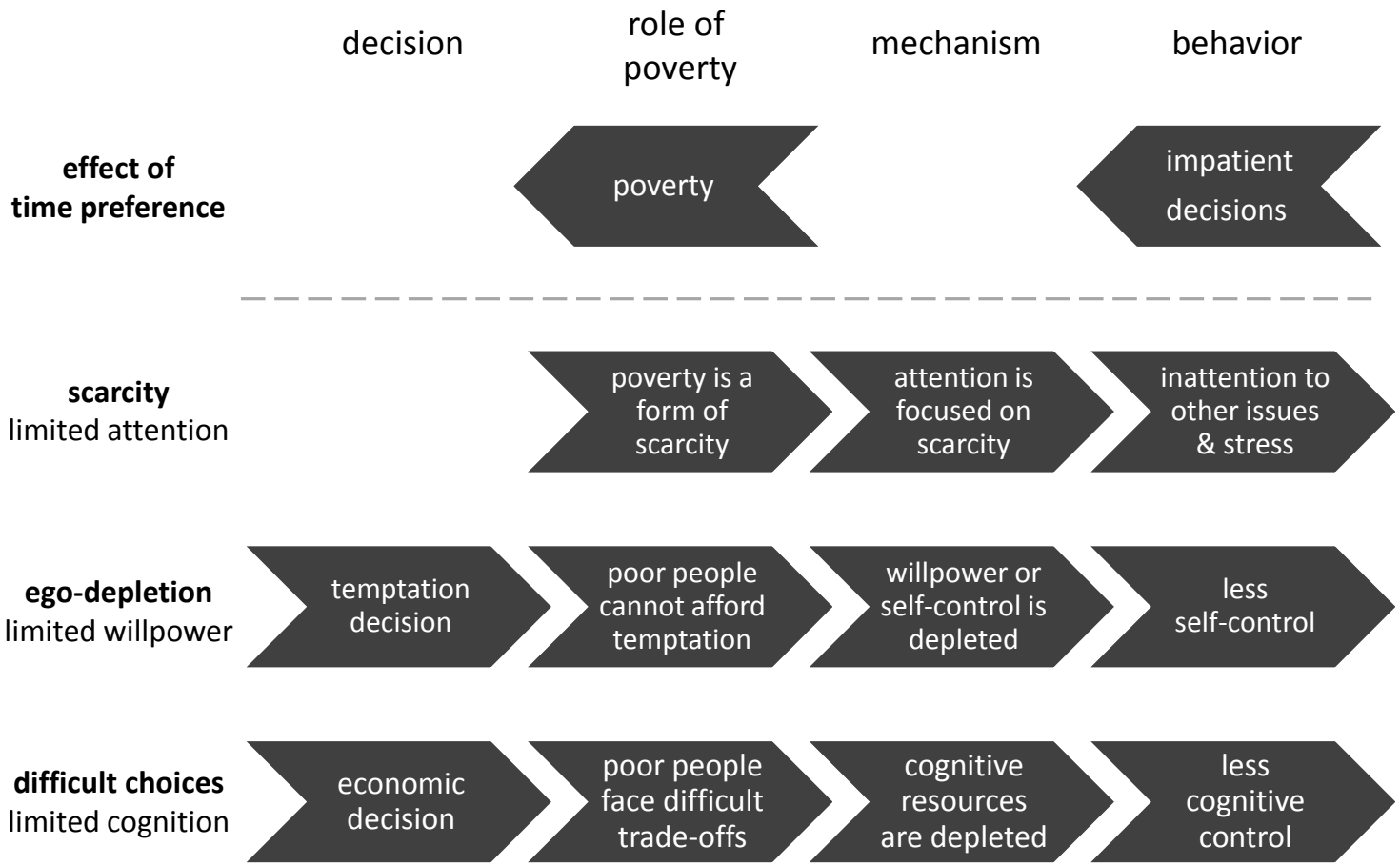

Figure 1: Theories of poverty and decision-making

Figure 1 presents the theoretical mechanisms, which are detailed in the appendix, along with a more extensive literature review. The first highlights a general effect of scarcity as a context for many economic decisions. As the figure shows, the second and third propose consequences of making particular economic decisions while poor. In these views, poverty is depleting because it changes the consequences of decision-making.

The first mechanism, recently proposed by Mullainathan and Shafir, highlights scarcity: because attention is limited, it is directed to whatever domain is scarce. They suggest that poverty, which they interpret narrowly as money scarcity, would over-occupy a person's attention, reducing performance in important decisions or behaviors unrelated to money or wealth and potentially causing over-anxiety in financial decisions.

The second, the theory of ego depletion — associated with social psychologists Roy Baumeister, Mark Muraven, Dianne Tice, and Kathleen Vohs — proposes that willpower is limited, and is consumed by resisting temptation or inhibiting behavior. For example, Baumeister, Bratslavsky, Muraven, and Tice (1998) found that lab experiment participants who first forewent chocolate chip cookies to eat radishes then subsequently persisted for less time at an impossible puzzle task than members of a control group who were not required to use self-control to resist the cookies. Applied to economics and poverty by Ozdenoren, Salant, and Silverman (forthcoming), this theory predicts that because poor people cannot afford to indulge, tempting economic decisions require them to resist, and therefore deplete their willpower.

Finally, the third theory suggests that it is cognitive control that is the key limiting constraint. Cognitive control is the cognitive process, associated with working memory, that directs attention and inhibits automatic behaviors to pursue executive goals. Because the same economic decision can represent a more conflicting trade-off among more important priorities for a poorer person, economic decision-making is more difficult in poverty than otherwise, as further observed by Mullainathan and Shafir (2010). Similarly, Spears (2009) predicted theoretically and found evidence in a field experiment that poorer people must pay deliberation costs more often than richer people — because smaller sums of money represent bigger financial stakes for the poor than the rich — with consequences for whether they make important but inexpensive investments in health.

Poverty's difficult decisions could deplete cognitive control. For example, in experiments unrelated to poverty, Wang, Novemsky, Dhar, and Baumeister (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. Because cognitive control is used both to process mental conflicts and to resist impulses in the pursuit of goals, poorer people who have had to make economic decisions could subsequently exhibit less behavioral control.

The three studies below primarily separate an effect of poverty from the countervailing effects of time preference. Additionally, they collectively may help distinguish among the theories in figure 1. However, completely distinguishing among the mechanisms may not be possible for several reasons. First, the theories are not mutually exclusive; all three mechanisms could simultaneously impact the poor. Second, they all similarly propose that poverty is particularly depleting of limited mental resources. Finally, they belong to different traditions within psychology – for example, limited willpower is from social psychology, and limited cognitive control from cognitive psychology – and therefore may describe similar phenomena according to different paradigms.[5]

The clearest distinction may be that the theories of limited willpower and cognition both describe effects of making decisions while being poor; in contrast, the theory of limited attention describes an effect of scarcity, potentially influencing behavioral control even without prior decision-making. Evidence that depletion only follows decision-making could, therefore, be evidence for the willpower or cognitive mechanisms. Further, showing that economic decision-making causes depletion even when the decision does not involve resisting temptation would be evidence for the cognitive mechanism, rather than the ego depletion mechanism. Finally, finding that the effect of poverty on behavioral control depends on a person's stock of one of the hypothesized resources – attention, willpower, or cognitive control – would suggest that depletion of that resource is an important mechanism. The studies below may be suggestive among these distinctions, but their most important implication is that some combination of these three similar mechanisms matters for the behavioral control of the poor.

2 Lab experiment in the field

In July 2010, we conducted a lab experiment in Banswara, a small city in rural southern Rajasthan, in India. The participants were locally recruited casual laborers who had been hired to participate in this research as a day's work. The

[5] As an example of the difficulty in separating these theories from different sub-disciplines of psychology in these applied experiments, a recent working paper by Kool and Botvinick (2011) proposes a limited-cognition account for experiments documenting "ego" depletion of self-control.

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experiment randomly assigned "wealth" and "poverty" in the experiment's context. This isolated an effect of poverty, ruling out reverse causality.

2.1 Procedure

The experiment had three stages. First, participants played a "store game" that required some of them to make an economic decision, out of a randomly assigned larger or smaller budget. Second, participants' behavioral control was measured on two tasks. The experiment was designed to estimate the effect of different experimental treatments within 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 experiment involved two research assistants; they did not know our hypotheses.

2.1.1 Store game: depletion

In the store game, participants completed a simple economic transaction under one of four experimental conditions. They were told to imagine that the experimental room was a store. In the lab were the three items available at the store: 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 for free — thus, to be relatively "poor" with a smaller budget at the store, or "rich" with a larger budget at the store, although these terms were not used in the experiment. In particular, richer participants could afford both of the typically more desirable items (the oil and the tiffin), while the poorer participants had to choose between them. Participants were also independently randomly assigned to either be allowed and required to choose which item or items they would receive, or to simply be told which they would receive. 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 physically: the participant pulled a card with one or two dots out of a bucket. This

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

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 or not was a randomized experimental condition.

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. Handgrip squeezing time is a common dependent variable in ego depletion experiments in social psychology (*e.g.* Muraven, Tice, and Baumeister, 1998); the Stroop task is more commonly used as a measure of executive control in cognitive psychology (*e.g.* Mullainathan and Shafir, 2010). Ultimately, the experiment will find very similar results for both, suggesting that they measure related resources used to produce behavioral control.

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

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has often used handgrips to measure control.[6] For example, Muraven et al. (1998) 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, Warner, and Polansky (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 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 the items they received in the experimental game. Participants waited in a large room with

[6] 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.

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 bus stand located in a different part of the city from the first day's recruitment site; most were just arriving in Banswara to begin a period of temporary labor migration. 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 believe that no participant had any information about the particular games, decisions, and tasks in the experiment.

2.2 Econometric strategy and validity

Did 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 $\hat{\beta}_1 < 0$, not $\hat{\beta}_3$: no choice is necessary for poverty to worsen performance through this mechanism. Alternatively, $\hat{\beta}_1$ can be interpreted as controlling for experimenter demand, if participants who receive more are more willing to perform experimental tasks.

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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	

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.

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.[7]

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.

Isolating the direction of causality was the goal of this study. Still, we can tentatively ask: 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. This suggests that one of the two theories that specified a particular role for decision-making – the theories of limited willpower and limited cognitive control – may have been responsible.

Between these two, did participants only demonstrate subsequent behavioral depletion when they had used willpower to resist temptation? The data can only suggest an answer. The important distinction between rich and poor experimental conditions is that "rich" participants could afford two items, while "poor" participants could only afford one; it does not directly matter which items participants might prefer or why. However, 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.

[7]Results using only handgrip or Stroop performance, rather than their mean, as the dependent variable are similar: the standarized coefficient estimates of the interaction are -0.77 ($p = 0.15$) for the math questions alone and -0.69 ($p = 0.20$) for the handgrip. The standard errors are larger because the single measures are more noisy than the averages; the variance of the mean is 46 percent of the variances of the two individual measures (both constructed to be 1).

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In this population, the cooking oil likely represented temptation: it had a slightly lower market price than the tiffin, but could be eaten to add good-tasting calories to food today (the participants were generally migrant workers, who would not be going home to eat with their families that day). 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. Therefore, participants who chose the oil may not have used willpower to resist temptation; if this interpretation is correct, then they would not have been affected by the "ego-depletion" mechanism. All of these interpretations, however, are secondary to the primary goal of distinguishing the effects of large and small budget sets.

If control were depleted only through 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 decision-making itself, 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 16 rural villages of Banswara district, in Rajasthan, India. Participants made real spending decisions. Each day both surveyors traveled to two new villages, one richer and one poorer according to census indicators. The surveyors offered participants a product for sale either before or after asking them to squeeze a handgrip, and then recorded economic and demographic information about participants. Decision-making proved depleting only for poorer participants. This interactive effect was greatest for participants with the least cognitive resources; this suggests that the third mechanism, limited cognitive control, may be at least partially responsible.

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 (to simplify the field interview and because the results were similar in the lab experiment, 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. As described below and in the appendix, working memory is considered to be a crucial resource for cognitive control. 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. Because of the intervening questions, this is an example of a complex working memory span task, a highly predictive measure in which "subjects remember a short stimulus list for later recall, and must simultaneously engage in a secondary 'processing' task" (Chein, Moore, and Conway, 2011). The mean participant remembered less than two words. Spears (2010) found that a similar test predicted apparent patience in consumption behavior among South African pension recipients.

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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.

The surveyors went to a different village each morning and afternoon, never returned to the same village, and spent no more than two or three hours in a village. The mean village had 8.3 interviews, and the median 7. The villages were pre-selected based on Indian census data and were geographically dispersed. It is unlikely that any participant had heard of the experiment before his 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:

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

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, we 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 an index made of the first principal component of all socioeconomic questions.

Soap first is randomly assigned, so it is unlikely to be correlated with many other measures. Table 3 reports summary statistics by experimental

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	

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 village 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 sets of area 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, then the interactive effect should be least for participants with the most cognitive ability: their resources would be less likely to become consumed. This is 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, DeYoung, Green, Reis, Johnson, Conway, Engle, Braver, and Gray, 2008) and may be the resource used to maintain executive goals. Experimentally occupying participants' working memory results in more impulsive behavior (Getz, Tomlin, Nystrom, Cohen, and Conway, 2009), such

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as choosing chocolate cake over fruit salad (Shiv and Fedorikhin, 1999). If poverty influences behavior by depleting limited cognitive resources, then 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.

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 cells found a p-value of 0.001 for the interaction in the first column of panel A.

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 ($\hat{\beta}_3 = -34$ sec; $t = -2.31$).

One possible threat to identification would be if participants did not deliberate at the appropriate time. While it is unlikely that any participant had heard of the experiment before his interview, a participant who had already thought about the decision might not have to deliberate during the interview. However, restricting the sample only to the half of participants interviewed first in each village finds similar results, although less precisely estimated due to the smaller sample: for example, the estimates of the interactions $\hat{\beta}_3$ from the first columns of panels A, B, and C become -33, -16, and -41 seconds, respectively ($t = -1.84, -1.18,$ and -1.89). No tests for structural break — triple-interacting the model with an indicator for being the first interview in a village, or one of the first two, or one of the first three, or so on — find a differential effect of earlier interviews at any break point.[8] Because earlier interviews in a village are no different, participants appear to have been appropriately surprised by the experiment.

[8]In particular, during the first or first and second interviews in each village, no participant could have previously heard about the experiment. Null hypotheses that the interactive effect is not different in the first or first and second interviews fail to be rejected with p values of 0.75 and 0.22, respectively.

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

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

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

Two-sided p-values: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$. Clustered standard errors in parentheses (33 village-surveyor combinations). 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 village fixed effects. In panels A and C, not squeezing is counted as zero seconds.

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 more complex. For example, poorer people might not receive early-life investments in health and nutrition that would increase later-life cognitive resources (Currie, 2009). More importantly for assessing causality in this paper, might people have become poor because they were more deplet*able*? Even if the poor do not have lower initial stocks 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, so no appeal to randomization can rule out that poorer people exhibited more depletion because their depletability made them poor. But it is only possible that individual differences in behavioral control have sorted people into relative wealth and poverty if participants have experienced economic mobility. Finding the same result in a low-mobility subsample, restricted to participants unlikely to have been sorted into poverty due to their depletability, makes this confounding interpretation 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 importantly 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: $n = 131$, $\hat{\beta}_3 = -45$ sec, $t = -2.66$; mother lived in same village when participant born: $n = 95$, $\hat{\beta}_3 = -60$ sec, $t = -2.76$). These estimates are not very different from the effects found in the full sample. 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. However, we emphasize that this sub-sample analysis can only point towards causality, and cannot itself definitively rule out a reverse effect of depletability.

3.3.2 Theories of poverty

Having demonstrated an effect of poverty, a secondary question is whether these data can help distinguish among potential mechanisms. The clearest evidence for a role for cognitive resources is in the triple interaction with working

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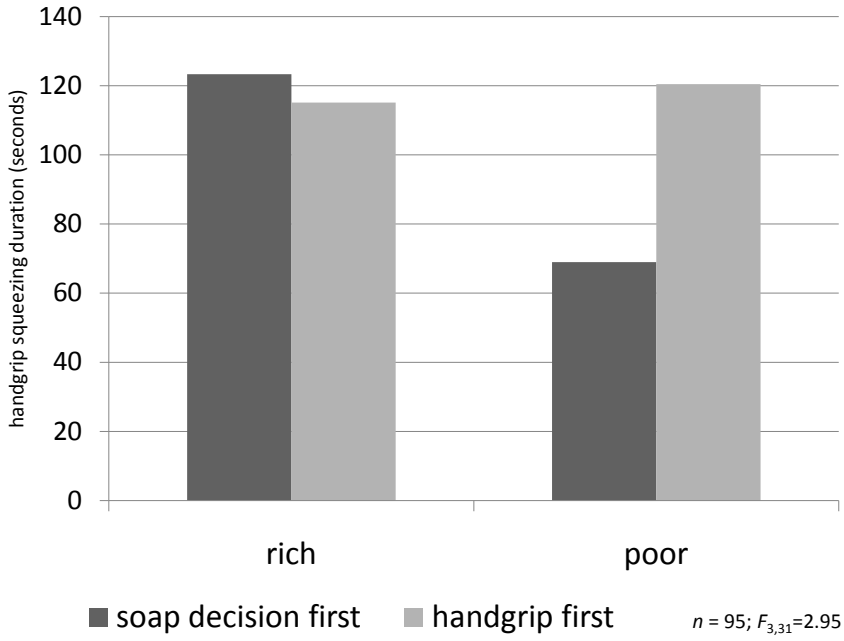

Figure 3: Field experiment: Results for participants who live in natal villages

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 requires 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.

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

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

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

Clustered standard errors in parentheses (33 village-surveyor combinations). "Cognition" is a 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. Two sided p-values: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; † $p = 0.12$.

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Because the working memory test happened at the end of the interview, one might worry that it was influenced by the experimental treatment. However, by the time of the working memory test all participants had completed both the handgrip task and the soap decision, with a further intervening set of survey questions before the test. The experimental treatment only changed the relative *timing* of the handgrip task to immediately before or after the soap decision, not the total exposure to depletion. Moreover, working memory scores are uncorrelated with the experimental treatment and with its interaction with poverty, whether estimated with OLS ($\chi^2_{2\text{ dof}} = 3.04$, $p = 0.22$) or as an ordered logit ($\chi^2_{2\text{ dof}} = 2.88$, $p = 0.24$). These findings are consistent with considerable evidence from psychology of the predictive importance of differences across individuals in working memory (*cf.* Conway, Jarrold, Kane, Miyake, and Towse, 2008).

As in the lab experiment, there was no direct effect of poverty 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.

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, Frazis, and Stewart, 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 behavioral control: it is by definition not fully attended to, and may not reflect the

deliberate pursuit of health goals.[9] "Mindless eating" without "consumption monitoring" facilitates overeating (Wansink and Sobal, 2007). For example, in an experiment conducted by Wansink, Painter, and North (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-group 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

[9] 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.

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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; we 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? We 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, we 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, we 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 study 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 or 7 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.[10]

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.

[10]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.

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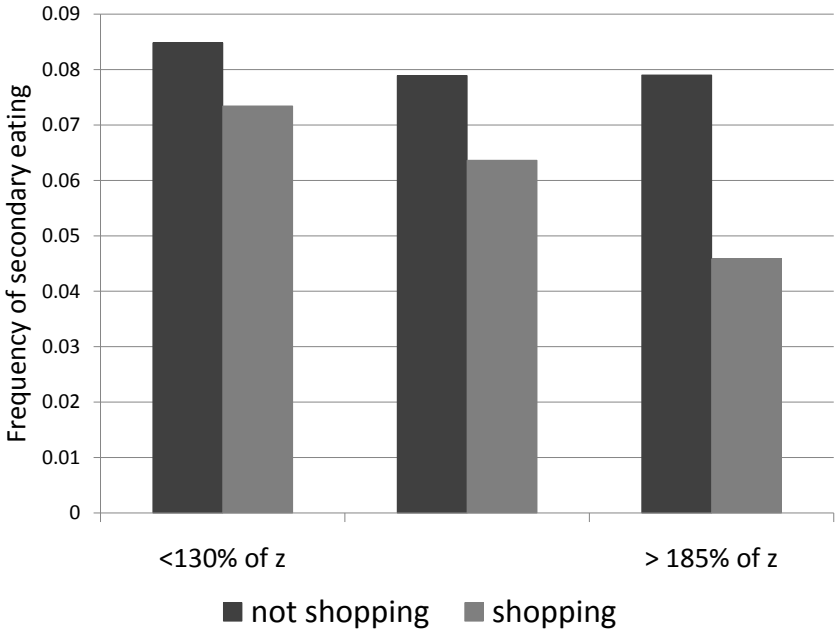

Figure 4: Time use: Secondary eating by poverty status

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 (for example, if food is more readily available, or work is more cognitively depleting for richer people), 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, we 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. 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.

These results are consistent with the prediction that economic decision-making will cause depleted behavioral control specifically among the poor, but it cannot be definitively 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. No information is available in the ATUS about where participants are shopping. Unlike in the field experiment, no measure of working memory is available to isolate a specifically cognitive mechanism.

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Table 6: Secondary eating: linear probability during an event

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

Standard errors clustered by respondent in parentheses. The restricted sample of events excludes sleeping and primary eating. Columns marked "no groc." exclude grocery shopping from shopping; columns marked "inc. groc." include grocery shopping. Operationalizations of being "richer" are indicators for being not poor and not very poor and a continuous measure of earnings. Earnings are demeaned and in $100 per week. Poverty reflects household CPS categories. Two-sided p-values: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; † $p = 0.12$.

Table 7: Secondary eating: shopping versus other event types

	(1)	(2)	(3)	(4)	(5)	(6)
event:	shopping	lag shopping	leisure	tv	housework	at work
event × not poor	-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)
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: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

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

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

Standard errors clustered by respondent in parentheses. Poverty reflects household CPS categories. Two-sided p-values: * $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$; † $p = 0.15$.

5 Conclusions and discussion

Economic decision-making diminished behavioral control when participants were poorer. In these data, poverty appears to cause depleted performance, rather than the other way around. Isolating this direction of causality was the main objective of this paper.

Having done this, which theoretical mechanism seems most likely to have mattered in these studies? The three mechanisms are not definitively separated by this evidence. In these studies, poverty had an effect particularly when participants made economic decisions, as opposed to poverty having a diffuse, background effect without economic decision-making. Moreover, the depleting effect of economic decision-making on the poor was greatest, on average, among those with the least working memory. These findings are consistent with a theory of poverty's depletion of limited 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 or tempting 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 even when rich and poor respondents made whatever purchases they ordinarily made. Although a richer person's budget may enable her to face a difficult choice between two luxury items, this was not quantitatively important enough to appear in a statistically representative day. Further, the richer consumer has the option of not making this choice at all. If, as the lab experiment suggests, even routine decisions about everyday goods are costly and difficult for the very poor, then their depleting effect is more inescapable.

These studies provide no direct evidence about the duration of depletion, or whether it may accumulate over many depleting circumstances. However, recent experiments by Gailliot, Baumeister, DeWall, Maner, Plant, Tice, Brewer, and Schmeichel (2007) suggest that increasing blood glucose levels may accelerate recovery of behavioral control after depletion. Unfortunately such a salve may be less accessible to, for example, the undernourished Indian poor.

These studies add to the growing evidence for a cognitive dimension of what is ordinarily modeled as time *preference* (*e.g.* Spears, 2010). 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" —

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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 Appendix: Theories of poverty & behavior

When Shantideva, an 8th century Indian Buddhist, vowed to follow a path of service and enlightenment, he made a particular commitment to fulfill the material needs of the poor, who would never otherwise have the mental resources to concentrate on their own enlightenment without distraction. While probably not the first scholarly observation of poverty's psychological effects on behavior, Shantideva's is the first that we are aware of. This appendix is a brief review of others since then, with extra weight on the economics literature. Anand and Lea (2011) offer a recent review that is more focused on the psychology literature about poverty.

A.1 Scarcity, limited attention, & stress

In an important antecedent to this paper, 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,[11] and their performance suffers due to stress. While outside the scope of this paper, in their 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.

Mullainathan and Shafir report interviews with Indian sugar cane farmers. They interview the same 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. In the second interview, 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.

[11] 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.

The closest antecedent in empirical economics to Mullainathan and Shafir's work may be Ariely, Gneezy, Loewenestein, and Mazar's (2009) experiments on "large stakes" and "choking under pressure," with relative poverty as a contextual independent variable. Arguing that "very high incentives result in a decrease in performance,"[12] and that whether an incentive is high is relative, they conducted experiments in Tamil Nadu, India. Because the participants were poor, the maximum reward in their experimental game (400 rupees, or about eight dollars at market exchange rates), was 80 percent of the local per capita monthly expenditure. In creativity, memory, and motor coordination games, Indian participants offered these high incentives exhibited worse performance. Ariely et al. find a similar effect on MIT students, but only at much larger monetary stakes; of course, relative poverty in this comparison is not exogenously assigned.

Somewhat similarly, Case (2001) found less depression (a correlate of stress) on average among adults in poor South African households that had an older member who was receiving the state old age pension, relative to households with an older adult who was not eligible for the pension. This finding builds on Adler, Boyce, Chesney, Cohen, Folkman, Khan, and Syme's (1994) suggestion that the well-documented positive correlation between health and socioeconomic status could be partially explained by psychosocial factors, including depression, stress, and hostility (*e.g.* Rojas, 2011, Santiago, Wadsworth, and Stump, 2011). Haushofer (2011) has argued from neuroeconomic evidence that, among other effects "poverty raises levels of the stress hormone cortisol;" the result of this is to "impair executive function and exacerbate behavioral biases in economic choice, and thus contribute to the perpetuation of poverty." Evans et al. (2005) find that chaos in the lives of poor elementary school students in New York is associated with exhibiting more psychological distress and less self-regulation, as rated by their teachers.

Theoretically, Karelis (2007) offers a philosopher's account of the effects of poverty on behavior, emphasizing the cumulatively overwhelming effects of deprivation. Acting to relieve one problem is valuable if you only have one problem, Karelis offers, but is not worth the effort if you have many problems: soothing the pain of one bee sting is unnoticeable among ten others. As a result, marginal utility may be *increasing* among the poor, resulting in apparently impatient behavior.

[12]This is an application of psychologists' "Yerkes-Dodson law," Ariely et al. explain, holding that "there is an optimal level of arousal for executing tasks, and that departures from this level in either direction can lead to a decrement in performance."

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Bernheim et al. (1999, 2011) offer a result where poverty has similar consequences through a very different theoretical mechanism. They consider the game theoretic personal rules that a person with time inconsistent impatience could implement: can I threaten myself with future misbehavior bad enough to discipline myself today? They argue that poorer people who are credit constrained may be unable to regulate their own impatience: their limited stock of assets bounds the grimness of the trigger with which they can threaten themselves. As they observe, this can explain the documented demand of the poor for external commitment mechanisms (*e.g.* Ashraf et al., 2006).

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," Muraven 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.[13] 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, depletable willpower. A poorer person with even 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."

[13]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 period in which they are consumed. They justify this assumption partially with the observation that temptations such as tasty foods are satiable.

A.3 Difficult choices & limited cognitive control

> "...without choice, without knowledge for choice if he had it, without power of choice if he had knowledge..."
> *Let Us Now Praise Famous Men*, Agee (1941)

George Orwell (1937) traveled to the north of England during the Great Depression to learn about the lives of the poor. His detailed and moving account emphasizes the difficult decisions the poor have to make about what not to buy. Orwell recounts one family's meager budget, observes what it omits, and remarks that "any money spent on these would mean reduction on some other item;" elsewhere in the book he details the "psychological adjustment" inherent in the dilemmas of the poor, necessary to avoid "continued agonies of despair." In her history of poor and pregnant women in nineteenth-century Paris, Fuchs (1992) explains how "women made choices, albeit without adequate information, without many options, and without much planning." Some of the poor women she describes resorted to infanticide or child abandonment: "infanticide may have been an act of desperate self-defense in a male-dominated society where many women had low status, low self-esteem, and few options, and where they tended to be economically vulnerable."[14] Such poverty is not only history: Collins, Morduch, Rutherford, and Ruthven (2009) collected detailed "financial diaries" from poor households in Bangladesh, India, and South Africa, documenting the complexity and difficulty of the intertemporal financial decisions that the poor must make to manage their small and irregular incomes.

Cognitive resources play an important role in economic behavior because they facilitate economic deliberation and global decision-making. Burks, Carpenter, Götte, and Rustichini (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, 2010). 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 (Benjamin, Brown, and Shapiro, 2006).

[14] Again, poverty's terrible set of options has a psychological cost: "Women may have been telling the truth when they said that they did not remember the delivery and what happened afterward."

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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, Braver, Barch, Carter, and Cohen, 2001), is used to make decisions (McGuire and Botvinick, 2010), and may employ working memory to direct attention,[15] inhibit impulses, override automatic processes, and maintain goals. Cognitive control is limited (Monsell, 2003).

Experimental evidence confirms that difficult choices are cognitively costly. Vohs, Baumeister, Schmeichel, Twenge, Nelson, and Tice (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.

Limits to cognitive control matter to poor people because poverty raises the stakes of many economic decisions, as observed by Mullainathan and Shafir (2010). 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 deliberation. Spears (2009) documented relatively poorer women in the rural Indian district of Kutch being more likely than relatively richer women to undertake mentally costly deliberation about spending small sums of money: deliberation was worth the effort, given their higher marginal disutility of wasting money. Such deliberation could be depleting, Wang et al. (2010) show — including, but not only, when emotions or temptation must be regulated. If cognitive resources are limited, this would leave less remaining cognitive control for other decisions or behaviors.

One potential resolution among these three mechanisms may be that 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 systems. For the purpose of applying psychological research to better understand poverty, emphasizing limits to cognitive control — which is a broader resource — highlights that our abilities to resist temptation and to make difficult decisions both face limits.

[15] Although an example of a general effect of poverty, rather than an effect of making a particular decision while poor, Evans and Schamberg (2009) importantly document that childhood poverty is inversely related to working memory in young adulthood, and that this association is mediated by childhood chronic stress.

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