

## Alcohol and Self-Control: A Field Experiment in India<sup>†</sup>

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*This paper studies alcohol consumption among low-income workers in India. In a 3-week field experiment, the majority of 229 cycle-rickshaw drivers were willing to forgo substantial monetary payments in order to set incentives for themselves to remain sober, thus exhibiting demand for commitment to sobriety. Randomly receiving sobriety incentives significantly reduced daytime drinking while leaving overall drinking unchanged. I find no evidence of higher daytime sobriety significantly changing labor supply, productivity, or earnings. In contrast, increasing sobriety raised savings by 50 percent, an effect that does not appear to be solely explained by changes in income net of alcohol expenditures. (JEL C93, D14, I12, J22, J24, J31, O12)*

Heavy alcohol consumption among male low-income workers is common in India and other developing countries. Excessive drinking can have severe consequences for individuals and their families, yet our understanding of such effects is limited. In particular, acute alcohol intoxication is thought to affect myopia and self-control, such that alcohol consumption could interfere with a variety of forward-looking decisions and behaviors. By affecting productivity, labor supply, savings decisions, and human capital investments, alcohol could reduce earnings and wealth accumulation and thus deepen poverty. However, though theoretically possible, we do not know whether such effects are present or economically meaningful in reality.

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Moreover, little knowledge exists about policy options to alleviate the potential negative impacts of alcohol.

Since alcohol consumption has long been associated with self-control problems, commitment devices could help improve outcomes. A hallmark prediction of economic models of sophisticated agents with self-control problems is demand for commitment devices which allow individuals to curb their future self-control problems by increasing the relative price of undesirable choices. Previous papers have considered the impact of commitment devices in a number of domains, including saving, smoking, and intertemporal effort provision. Existing evidence shows that the availability of commitment devices does indeed help, at least in some of the cases (Ashraf, Karlan, and Yin 2006; Kaur, Kremer, and Mullainathan 2015). However, few real-world examples of successful commitment devices exist, and empirical evidence of positive willingness to pay for such devices is scarce, calling into question the underlying models and the efficacy of commitment devices (Laibson 2015, 2018).

Against this background, this paper considers alcohol consumption among cycle-rickshaw drivers in Chennai, India, a population for whom drinking is likely a serious problem. In a 3-week field experiment with 229 men, I offered financial incentives for sobriety to a random subset of individuals, while a second group received unconditional payments of similar magnitude. The remaining individuals were offered the choice between sobriety incentives and unconditional payments. The randomized nature of the experiment allows me to investigate the impact of increased sobriety on labor market outcomes and savings behavior. To measure the impact of acute intoxication on intertemporal choices, all subjects were provided with a high-return savings opportunity. For a cross-randomized subset of study participants, the savings account was a commitment savings account, i.e., individuals could not withdraw their savings until the end of their participation in the study.

Individuals' choices between sobriety incentives and unconditional payments reveal substantial willingness to pay and thus demand for commitment to increase their sobriety. In three sets of weekly decisions that each elicited preferences for sobriety incentives in the subsequent week, over one-half of the study participants chose the incentives when they were weakly dominated by the unconditional payment option. Even more striking, over one-third of study participants preferred incentives for sobriety over unconditional payments, even when the unconditional payments were strictly higher than the maximum possible amount that subjects could earn with the sobriety incentives. These men were willing to sacrifice study payments of about 10 percent of daily income even in the best-case scenario of visiting the study office sober every day.

This finding provides clear evidence for a desire for sobriety by making future drinking more costly, in contrast to the predictions of the Becker and Murphy (1988) rational addiction model, but in line with Gruber and Kőszegi (2001). The observed demand for commitment indicates a greater awareness of and willingness to overcome self-control problems than found in most other settings, such as smoking, exercising, saving, and real-effort choices (Giné, Karlan, and Zinman 2010; Royer, Stehr, and Sydnor 2015; Ashraf, Karlan, and Yin 2006; Augenblick, Niederle, and Sprenger 2015). Since demand for commitment implies sophistication regarding an underlying self-control problem, the evidence also contrasts with recent evidence

documenting near-complete naïveté regarding present bias (Augenblick and Rabin forthcoming).

The financial incentives significantly increased individuals' sobriety during their daily study office visits. Sobriety incentives decreased daytime drinking as measured by a 33 percent (or 13 percentage point) increase in the fraction of individuals who visited the study office sober and equivalent reductions in breathalyzer scores and self-reported drinking. However, overall alcohol consumption and expenditures remained nearly unchanged. This finding implies that individuals largely shifted their drinking to later times of the day rather than reducing their overall drinking as a response to the incentives. In contrast to existing evidence of persistent impacts of short-run incentives for health-related behaviors (Prendergast et al. 2006, Dupas 2014), financial incentives do not appear to be effective at persistently reducing drinking in this context.

The increase in daytime sobriety due to the incentives provides a "first stage" to estimate the impact of sobriety on labor market outcomes and savings behavior. Perhaps surprisingly, I do not find evidence of significant changes in labor supply, productivity, or earnings, though I cannot reject treatment effects of about 10 to 15 percent for these outcomes. In contrast, offering sobriety incentives increased individuals' daily savings at the study office by over 50 percent compared to a control group that received similar average study payments independent of their alcohol consumption. Two potential channels contribute to this increase in savings: changes in income net of alcohol expenditures and changes in decision making for given net income. Given the lack of significant changes in earnings and alcohol expenditures, the sobriety incentives increased net incomes only slightly. It therefore appears that increased sobriety altered individuals' savings behavior for given net income.

The relationship between the effects of sobriety incentives and commitment savings provides further evidence of this hypothesis. I find that sobriety incentives and the commitment savings feature were substitutes in terms of their effect on savings. While commitment savings and sobriety incentives each individually increased subjects' savings, there was no additional effect of the savings commitment feature on savings by individuals who were offered sobriety incentives, and vice versa. This finding suggests that alcohol causes self-control problems, in line with psychology research on "alcohol myopia." Steele and Josephs (1990) argue that alcohol has particularly strong effects in situations of "inhibition conflict," i.e., with two competing motivations, one of which is simple, present, or salient, while the other is complicated, in the future, or remote. One interpretation of this theory is that alcohol causes present bias. The findings from my field experiment support this interpretation in the context of savings decisions and demonstrate that alcohol-induced myopia can have economically meaningful consequences.<sup>1</sup>

<sup>1</sup> While there is considerable evidence that alcohol myopia affects a range of social behaviors such as aggression and altruism, studies on alcohol myopia did not consider savings decisions or intertemporal choice (Giancola et al. 2010). However, many cross-sectional studies, including several on alcohol, found a correlation between impulsive "delayed reward discounting" (DRD) and addictive behavior, without establishing existence or direction of causality (MacKillopp et al. 2011). Experimental lab studies consistently found acute alcohol intoxication reducing inhibitory control in computer tasks (Perry and Carroll 2008), but studies on the effects of alcohol on impulsive DRD found mixed evidence (Richards et al. 1999; Ortner, MacDonald, and Olmstead 2003). My study differs from previous experimental studies in a number of ways. In particular, (i) the duration of the experiment was significantly longer (over three weeks versus one day), (ii) sample characteristics were markedly different, (iii) stakes were

This paper contributes to the growing literature on saving decisions among the poor. Several recent studies emphasize the importance of technologies for committing to savings and show that the availability and design of savings accounts are important determinants of savings behavior among the poor (Dupas and Robinson 2013; Karlan, Ratan, and Zinman 2014). This paper shows that helping individuals to overcome underlying self-control problems regarding specific goods can be a substitute for commitment devices for overall consumption-saving decisions. Finally, the paper suggests second-best policies aimed at reducing the costs of inebriation by shifting critical decisions away from drinking times could be welfare-improving even if they do not change overall drinking levels.

The remainder of this paper is organized as follows. Section I provides an overview of the study background, including alcohol consumption patterns in Chennai and in developing countries more generally. Section II describes the experimental design, characterizes the study sample, and discusses randomization checks. Section III considers the extent to which self-control problems contribute to the demand for alcohol by investigating rickshaw drivers' demand for incentives. Section IV then describes the impact of increased sobriety on savings, and Section V investigates the interaction between sobriety and commitment savings. Section VI concludes.

## I. Alcohol in Chennai, India, and Developing Countries

There is scarce information regarding drinking patterns in developing countries, especially among the poor. As a first step toward a systematic understanding of the prevalence of drinking among male manual laborers in developing countries, I conducted a short survey with 1,227 men from 10 low-income professions in Chennai in August and September 2014. Surveyors approached individuals from these groups during regular work hours and offered them a small compensation for answering a short questionnaire about their alcohol consumption, including a breathalyzer test. According to these surveys, the overall prevalence of alcohol consumption among low-income men is high (upper panel of online Appendix Figure A.1);<sup>2</sup> 76.1 percent of individuals reported drinking alcohol on the previous day, ranging across professions from 37 percent (porters) to as high as 98 percent (sewage workers).

On days when individuals consume alcohol, they drink considerable quantities of alcohol (lower panel of online Appendix Figure A.1). Conditional on drinking alcohol on the previous day, men of the different professions reported drinking

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higher (relative to income), and (iv) the main outcome was the amount saved after three weeks. In the perhaps most closely related field study, Ben-David and Bos (2017) provide complementary evidence on the impact of alcohol availability on credit-market behavior using variation in liquor store opening hours in Sweden.

<sup>2</sup>The prevalence of alcohol consumption among women in Chennai and in India overall is substantially lower. It is consistently estimated to be below 5 percent in India, with higher estimates for North-Eastern states and lower estimates for Tamil Nadu (where Chennai is located) and other South Indian states (Benegal 2005). In the most recent National Family Health Survey (Round 3 2005/6), the (reported) prevalence of female alcohol consumption was 2.2 percent (IIPS and Macro International 2008). It is highest in the lowest wealth (6.2 percent) and education (4.3 percent) quintiles.

average amounts ranging from 3.8 to 6.5 standard drinks on the same day.<sup>3</sup> Since alcohol is an expensive good, the resulting income shares spent on alcohol are enormous (upper panel of online Appendix Figure A.2). On average, individuals reported spending between 9.2 and 43.0 percent of their daily incomes of Rs 300 (\$5) to Rs 500 (\$8) on alcohol. These numbers are particularly remarkable since many low-income men in Chennai are the sole income earners of their families. Finally, 25.2 percent of individuals were inebriated or drunk during these surveys, which all took place during the day and during many of the individuals' regular work hours (lower panel of online Appendix Figure A.2).

The substantial level of alcohol consumption found among male low-income workers in Chennai raises the question of how these numbers compare to other estimates for Chennai, for India, and for developing countries overall. Limited data availability of alcohol consumption and especially breathalyzer scores, as well as data inconsistencies make answering this question difficult (Gupta et al. 2003). However, there is reason to believe that the estimates for Chennai are not unusual compared to other parts of India or other developing countries. The WHO Global Status Report on Alcohol and Health provides country-by-country estimates of alcohol prevalence and consumption levels (WHO 2014). According to this report, male drinkers in India, about one-quarter of the total male population, drink about 5 standard drinks per day on average, only slightly less than the average of the physical quantities shown in online Appendix Figure A.1.

## II. Experimental Design and Balance Checks

The experiment took place between April and September 2014. Two hundred twenty-nine cycle-rickshaw drivers working in central Chennai were asked to visit a nearby study office every day for three weeks each. During these daily visits, individuals completed a breathalyzer test and a short survey on labor supply, earnings, and expenditure patterns of the previous day, and alcohol consumption both on the previous day and on the current day before coming to the study office. To study the impact of increased sobriety on savings behavior, all subjects were given the opportunity to save money at the study office.

Participants were randomly assigned to various treatment groups with the following considerations. First, to create exogenous variation in sobriety, a randomly-selected subsample of study participants was offered financial incentives to visit the study office sober, while the remaining individuals were paid for coming to the study office regardless of their alcohol consumption. Second, to measure individuals' demand for sobriety incentives and thus to identify self-control problems regarding alcohol, a randomly-selected subset of individuals was given the choice between incentives for sobriety and unconditional payments. Third, to examine the interaction between sobriety incentives and commitment savings, a cross-randomized subset of individuals was provided with a commitment savings account, i.e., a savings account that

<sup>3</sup>I follow the US definition of a standard drink as described in WHO (2001). According to this definition, a standard drink contains 14 grams of pure ethanol. A small bottle of beer (330 ml at 5 percent alcohol), a glass of wine (140 ml at 12 percent alcohol), or a shot of hard liquor (40 ml at 40 percent alcohol) each contains about one standard drink.

did not allow them to withdraw their savings until the end of their participation in the study.

### *A. Recruitment and Screening*

The study population consisted of male cycle-rickshaw drivers aged 25 to 60 in Chennai, India.<sup>4</sup> Individuals enrolled in the study went through a three-stage recruitment and screening process. Due to capacity constraints, enrollment was conducted on a rolling basis such that there were typically between 30 and 60 participants enrolled in the study at any given point in time.

*Field Recruitment and Screening.*—Field surveyors approached potential participants near the study office during work hours and asked interested individuals to answer a few questions to determine their eligibility to participate in “a paid study in Chennai.”<sup>5</sup> Individuals were eligible to proceed to the next stage if they met the following screening criteria: (i) males between 25 and 60 years old, inclusive, (ii) fluency in Tamil, the local language, (iii) had worked at least 5 days per week on average as a rickshaw puller during the previous month, (iv) had lived in Chennai for at least 6 months, (v) reported no plans to leave Chennai during the ensuing 6 weeks, and (vi) self-reported an average daily consumption of 0.7 to 2.0 “quarters” of hard liquor (equivalent to 3.0 to 8.7 standard drinks) per day.<sup>6</sup> If an individual satisfied all field-screening criteria, he was invited to visit the study office to learn more about the study and to complete a more thorough screening survey to determine his eligibility.

*Office Screening.*—The primary goal of the more detailed office screening procedure was to reduce the risks associated with the study, in particular risks related to alcohol withdrawal symptoms. The criteria used in this procedure included screening for previous and current medical conditions such as seizures, liver diseases, previous withdrawal experiences, and intake of several sedative medications and medications for diabetes and hypertension. This thorough medical screening procedure was strictly necessary since reducing one’s alcohol consumption can lead to serious withdrawal symptoms, particularly subsequent to extended periods of heavy drinking. If not adequately treated, individuals can develop delirium tremens (DTs), a severe and potentially even lethal medical condition (Wetterling et al. 1994, Schuckit et al. 1995).

<sup>4</sup>The study population included both passenger cycle-rickshaw drivers as in Schofield (2014) and cargo cycle-rickshaw drivers. In an earlier study in the same area, Schofield (2014) exclusively enrolled passenger-rickshaw drivers with a body-mass index (BMI) below 20. To avoid overlap between the two samples, my study only enrolled passenger cycle-rickshaw drivers with a BMI above 20. There was no BMI-related restriction for cargo cycle-rickshaw drivers.

<sup>5</sup>The main goals of this screening process were: (i) to ensure a homogeneous sample, (ii) to facilitate efficient communication, (iii) to limit attrition from the study due to reasons unrelated to alcohol consumption.

<sup>6</sup>“Quarters” refer to small bottles of 180 ml each. Nearly 100 percent of drinkers among cycle-rickshaw drivers (and most other low-income populations in Chennai) consume exclusively hard liquor, specifically rum or brandy. The drinks that individuals consume contain over 40 percent alcohol by volume (80 proof), and they maximize the quantity of alcohol per rupee. One quarter of hard liquor is equivalent to approximately 4.35 standard drinks. The lower bound on the number of quarters was chosen to ensure a potential treatment effect of the incentives on alcohol consumption. The upper bound on the number of quarters was chosen to lower the risk of serious withdrawal symptoms.



*Lead-In Period.*—Overall attrition and, in particular, differential attrition are first-order threats to the validity of any randomized-controlled trial. In my study, attrition was of particular concern since the study required participants to visit the study office every day for three weeks with varying payment structures across treatment groups. Moreover, in early-stage piloting, a significant fraction of individuals visited the study office on the first day, which provided high remuneration to compensate for the time-consuming enrollment procedures, but then dropped out of the study relatively quickly. To avoid this outcome in the study and to limit attrition more generally, participants were required to attend on three consecutive study days (the “lead-in period”) before being fully enrolled in the study and informed about their treatment status. They were allowed to repeat the lead-in period once if they missed one or more of the three consecutive days during their first attempt.

*Selection.*—At each stage, between 60 and 73 percent of individuals were able and willing to proceed to the subsequent stage (online Appendix Table A.1). As a result, 29 percent of the initially approached individuals made it to the randomized phase of the study. Among individuals approached on the street to conduct the field screening survey, 60 percent were eligible and decided to visit the study office to complete the office screening survey. Twenty-seven percent were either not willing to participate in the survey when first approached (15 percent), or were not interested in learning more about the study after participating in the survey and found to be eligible (12 percent). The majority among the remaining individuals participated in the survey but did not meet the drinking criteria outlined above, primarily because they were abstinent from alcohol or reported drinking less than 3 standard drinks per day on average. During the next stage, the office screening survey, 73 percent of individuals were found eligible. About one-half of the ineligible individuals were not able to participate due to medical reasons. Finally, 66 percent of individuals passed the lead-in period. Importantly, leaving the study at this stage is not related to alcohol consumption as measured by individuals’ sobriety during their first visit to the study office.

### B. Timeline and Treatment Groups

Figure 1 provides an overview of the study timeline. All participants completed five phases of the study. During the first four phases, consisting of 20 study days in total, individuals were asked to visit the study office every day, excluding Sundays, at a time of their choosing between 6 PM and 10 PM. The office was located in the vicinity of their usual area of work to limit the time required for the visit. During Phase 1, the first 4 days of the study, all individuals were paid Rs 90 (\$1.50) for visiting the study office, regardless of their blood alcohol content (BAC). This period served to gather baseline data in the absence of incentives and to screen individuals for willingness to visit the study office regularly. On day 4, individuals were randomly allocated to one of the following 3 experimental conditions for the subsequent 15 days.<sup>7</sup>

<sup>7</sup>In addition to receiving (potential) monetary incentives for sobriety, individuals in the Incentive and Choice Groups were also asked to forecast their sobriety if they were to receive incentives. Individuals were then informed of the weekly monetary payments implied by the different choices based on these predictions.

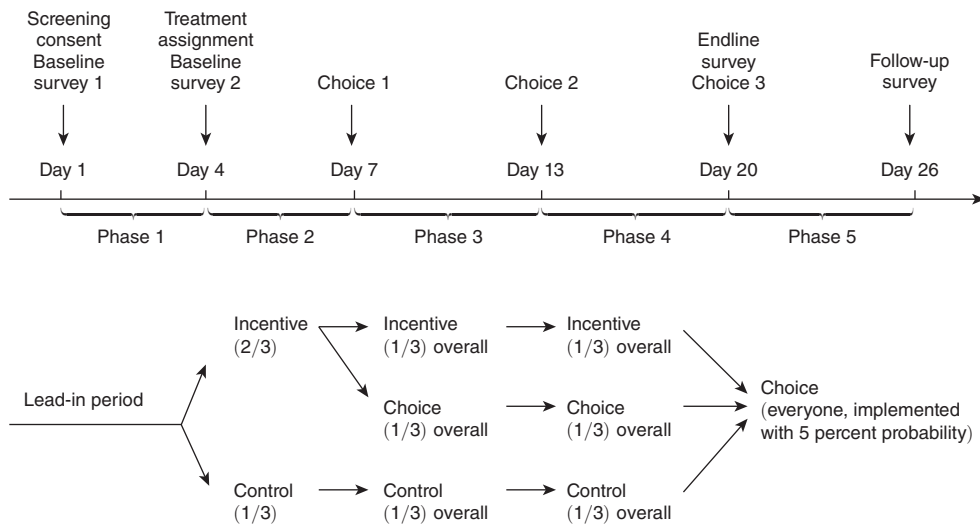


FIGURE 1. EXPERIMENTAL DESIGN

*Notes:* This figure gives an overview of the experimental design and the timeline of the study.

- On day 1, individuals responded to a screening survey. Interested individuals then gave informed consent upon learning more about the study. Regardless of the consent decision regarding participation in the full study, all individuals were asked to complete a baseline survey, for which separate consent was elicited.
- On day 4, individuals who passed the lead-in period (Phase 1) completed a second baseline survey, and were then informed of their treatment status. On this day, individuals were fully informed about their payment structure and the decisions to be made over the course of the study as described in the main text below.
- After facing the same payment schedule as the Incentive Group in Phase 2, the Choice Group was asked to choose whether they wanted to continue receiving these incentives, or whether they preferred payments that did not depend on their breathalyzer scores. Choices were made on days 7 and 13, each for the subsequent week.
- On day 20, all individuals were asked to participate in an endline survey. No incentives for sobriety were given on this day. All individuals were then given the same choices between conditional and unconditional payments as individuals in the Choice Group on days 7 and 13. To ensure incentive compatibility, these choices were then implemented for a small subset (5 percent) of study participants. One week after their last day in the study, individuals were visited for a follow-up survey including a breathalyzer test.

- (i) **Control Group:** The Control Group was paid Rs 90 (\$1.50) per visit regardless of BAC on days 5 through 19. These participants simply continued with the payment schedule from Phase 1.
- (ii) **Incentive Group:** The Incentive Group was given incentives to remain sober on days 5 through 19. These payments consisted of Rs 60 (\$1) for visiting the study office and an additional Rs 60 if the individual was sober as measured by a score of 0 on the breathalyzer test. Hence, the payment was Rs 60 if they arrived at the office with a positive BAC and Rs 120 if they arrived sober. Given the reported daily labor income of about Rs 300 (\$5) in the sample, individuals in the Incentive Group received relatively high-powered incentives for sobriety.
- (iii) **Choice Group:** The Choice Group was designed to elicit individuals' demand for sobriety incentives and simultaneously contribute to the estimation of the impact of increased sobriety. To familiarize individuals with the incentives, the



Choice Group was given the same incentives as the Incentive Group in Phase 2 (days 5 to 7). Then, right before the start of Phase 3 (day 7) and Phase 4 (day 13), they were asked to choose for the subsequent 6 study days whether they preferred to continue receiving incentives or to receive unconditional payments ranging from Rs 90 (\$1.50) to Rs 150 (\$2.50), as described below.

*Eliciting Willingness to Pay for Incentives.*—On days 7 and 13 of the study, surveyors elicited individuals' preferences for each of the 3 choices shown in the short table below. Each of these choices consisted of a trade-off between two options. The first option, Option A, was the same for all choices. The payment structure in this option was the same as in the Incentive Group, i.e., a payment of Rs 60 (\$1) for arriving with a positive BAC, and Rs 120 (\$2) for arriving sober. In contrast, Option B varied across the 3 choices, with unconditional amounts of Rs 90, Rs 120, and Rs 150. To gather as much information as possible while ensuring incentive compatibility, surveyors elicited preferences for all three choices before one of these choices was randomly selected to be implemented. To maintain similar average study payments across treatment groups, Choice 1 was implemented in 90 percent of choice instances (independent over time) so that particularly high payments were only paid out to a small number of individuals in the Choice Group.<sup>8</sup>

Choice	Option A		Option B
	BAC > 0	BAC = 0	Regardless of BAC
1.	Rs 60	Rs 120	Rs 90
2.	Rs 60	Rs 120	Rs 120
3.	Rs 60	Rs 120	Rs 150

I designed these choices with two main objectives in mind: first, to elicit individuals' demand for commitment to sobriety and, hence, potential self-control problems regarding alcohol consumption; second, to allow the Choice Group to be part of the evaluation of the impact of incentives for sobriety. In addition, given low literacy and numeracy levels in the study sample, the design seeks to minimize the complexity of decisions while achieving the other two objectives. More specifically, Option A was the same across choices, and individuals were given 3 study days to familiarize themselves with these incentives during Phase 2. Accordingly, in all 3 choices, subjects knew Option A from previous office visits, and Option B was simply a fixed payment regardless of BAC as already experienced in Phase 1. To address potential concerns regarding anchoring effects, we randomized the order of choices. One-half of the participants made their choices in the order as outlined above, and the remaining individuals completed their choices in the opposite order.

<sup>8</sup>Before making their choices, study participants were instructed to take all choices seriously since each choice had a positive probability of being implemented. Individuals were not informed regarding the specific probabilities of implementing each of the choices. One potential concern regarding the procedure to elicit demand for commitment in this study is that subjects' choices may have been affected by the fact that none of the choices was implemented with certainty. Such effects would be a particular concern for this study if they increased the demand for commitment. However, the existing evidence suggests that introducing uncertainty into intertemporal choices *reduces* present bias as measured by the "immediacy effect" (Keren and Roelofsma 1995, Weber and Chapman 2005).

*Demand for Commitment.*—The choice of the conditional payment (Option A) in Choice 1 is not necessarily evidence of demand for commitment. An individual who did not prefer to change his drinking patterns may have chosen Option A if he expected to visit the study office sober at least 50 percent of the time and, therefore, to receive higher average study payments than he would from choosing Option B. In contrast, study payments in Option B weakly dominated those in Option A for Choice 2. Therefore, choosing Option A in Choice 2 is evidence of demand for commitment to increase sobriety, which reveals underlying self-control problems. Furthermore, study payments in Option B *strictly* dominated those in Option A for Choice 3. Choosing Option A in Choice 3 implied sacrificing Rs 30 (\$0.50) in study payments per day even during sober visits to the study office, a nontrivial amount given reported labor income of about Rs 300 (\$5) per day.

*Endline.*—On day 20 of the study, all participants were asked to come to the study office once again for an endline visit at any time of the day. No incentives for sobriety were provided on this day. During this visit, surveyors conducted the endline survey with individuals and participants received the money they had saved as well as their matching contribution, as described below. Moreover, *all* study participants were given the same set of three choices, described above. This allows me to test whether exposure to incentives for sobriety affected subsequent demand for incentives. Surveyors again elicited preferences for all three choices and then randomly selected one of them to be implemented to ensure incentive compatibility. However, the choices from day 20 were only implemented for a randomly selected 5 percent of individuals for budgetary and logistical reasons. These selected individuals were invited to visit the study office for six additional days. For the remaining study participants, the endline visit was the last scheduled visit to the study office.

*Follow-Up Visit.*—To measure potential effects of the intervention beyond the incentivized period, surveyors attempted to visit each study participant about one week after their last scheduled office visit. Surveyors announced this visit during the informed-consent procedures and reminded participants of this visit on day 20 of the study. However, surveyors did not inform participants regarding the exact day of this follow-up visit. During this visit, individuals were breathalyzed and surveyed once again on the main outcomes of interest. The compensation for this visit did not depend on the individuals' breathalyzer scores.

### *C. Outcomes of Interest, Savings Treatments, and Lottery*

The main outcomes of interest in this study are (i) alcohol consumption and expenditures, (ii) savings behavior, and (iii) labor-market participation and earnings. Each of these outcomes is described below.

*Alcohol Consumption.*—Surveyors collected daily data during each study office visit by measuring individuals' blood alcohol content (BAC) and via self-reports regarding drinking times, quantities consumed, and amounts spent on alcohol. BAC was measured via breathalyzer tests using devices with a US Department of

Transportation level of precision.<sup>9</sup> During each visit, after the breathalyzer test (in an attempt to maximize truthfulness of answers), study participants were asked about their alcohol consumption on the same day prior to visiting the study office and about their overall alcohol consumption on the previous day.

*Savings Behavior.*—To study individuals' savings behavior, all individuals were given the opportunity to save money in an individual savings box at the study office. During each office visit, study participants could save up to Rs 200, using either payments received from the study or money from other sources. Two features of the savings opportunity were cross-randomized to the sobriety-incentive treatment groups:

- (i) **Matching Contribution Rate:** Individuals were offered a matching contribution (“savings bonus”) as an incentive to save. During their endline visit, subjects were paid out their savings plus a matching contribution. This matching contribution was randomized with equal probability to be either 10 percent or 20 percent of the amount saved by the end of the study.<sup>10</sup> Hence, even in a setting with high daily interest rates, saving money at the study office was a relatively high-return investment for many study participants.
- (ii) **Commitment Savings:** One-half of the study participants were randomly selected to have their savings account include a commitment feature. Instead of being able to withdraw money during any of their daily visits between 6 PM and 10 PM, they were only allowed to withdraw money at the end of their participation in the study.<sup>11</sup> Notably, the savings option for the remaining individuals also entailed a weak commitment feature. While individuals could withdraw as much as they desired on any given office visit, they were only able to withdraw money in the evenings, i.e., between 6 PM and 10 PM.<sup>12</sup>

I designed the savings treatments with the goal of studying the impact of increased sobriety on savings behavior and, more generally, on intertemporal choices and investments in high-return opportunities. The cross-randomized commitment savings feature permits studying the relationship between sobriety and self-control in savings decisions. Study participants were informed of their matching contribution upon receiving their lockbox, i.e., on the first day of their participation in the study. The commitment-savings feature was introduced to the relevant subsample on day 5

<sup>9</sup>As in Burghart, Glimcher, and Lazzaro (2013), this study uses breathalyzer model AlcoHawk PT500 (Q3 Innovations LLC). For more information on the measurement of BAC via breathalyzers, see O'Daire (2009).

<sup>10</sup>Individuals found the matching contribution easier to understand than a daily interest rate on savings during early-stage piloting work. The implied daily interest rate from saving an additional rupee increased for each participant over the course of his participation in the study. However, anecdotal evidence suggests that few individuals were aware of this feature.

<sup>11</sup>For ethical reasons, all individuals had the option to leave the study and withdraw all of their money on any day of the study.

<sup>12</sup>The design of the matching contribution also entails a commitment feature given that individuals only received it if they kept their savings at the study office until the last day of the study.

of the study, so as to avoid potential differential attrition occurring before individuals were informed of their sobriety incentive treatment status.

*Lottery.*—In addition to the payments described above, study participants were given the opportunity to earn additional study payments via a lottery on days 10 through 18 of the study. Surveyors implemented the lottery as follows. If the participant arrived at the study office on a day on which he was assigned to participate in the lottery, he was given the opportunity to spin a “wheel of fortune,” which gave him the chance to win a voucher for Rs 30 or Rs 60, each with a small probability. This voucher was valid only on the participant’s subsequent study day, i.e., if the participant came back on the following study day and showed the voucher, he received the equivalent cash amount at the beginning of his visit. The lottery allows me to estimate the impact of study payments on savings at the study office.

*Labor-Market Outcomes.*—These variables include earnings, labor supply, and productivity using individuals’ self-reports during the baseline survey, the daily surveys, and the endline survey. Reported earnings are a combination of income from rickshaw work and other sources such as load work. Labor supply is a combination of the number of days worked per week and the number of hours worked per day. Productivity is calculated as income per hour worked.

*Other Expenditure Patterns.*—To measure potential treatment effects on individuals’ expenditure patterns, study participants were asked to report (i) amounts given to their wives and other family members, (ii) expenditures on food, and (iii) expenditures on “temptation goods,” including tea, coffee, and tobacco.

#### *D. Sample Characteristics and Randomization Checks*

Study participants’ key background characteristics and randomization checks are summarized in the online Appendix. Appendix Tables A.2 through A.4 give an overview of basic demographics, alcohol-, work-, and savings-related variables at the beginning of the study. As to be expected with a large number of comparisons, some characteristics are imbalanced across treatment groups. Six out of 102 coefficients are statistically significantly different at the 10 percent level, and another four coefficients are significantly different at the 5 percent level. Most notably, individuals in the Control Group reported lower savings at baseline than in the Incentive and Choice Groups, a statistically significant difference when comparing the Control Group to the Incentive and Choice Groups combined. As illustrated in online Appendix Figure A.3, this difference is driven entirely by six individuals who reported very high baseline savings, among them one individual in the Choice Group who reported having Rs 1 million in cash savings at his home.

The differences in savings reported at baseline do not explain the treatment effects shown below. First, there were only small and statistically-insignificant differences in savings at the study office across treatment groups in the unincentivized Phase 1 (last row of online Appendix Table A.4). Second, controlling for Phase 1 savings and baseline survey variables, including total savings, does not substantially alter the regression results. If anything, the estimated effect of sobriety incentives on

savings becomes larger. Third, there is no apparent relationship between reported savings in the baseline survey and savings at the study office.<sup>13</sup>

### III. Demand for Commitment to Sobriety

A key prediction of economic models of sophisticated agents with self-control problems is demand for commitment devices (Laibson 1997, Gül and Pesendorfer 2001, Bernheim and Rangel 2004, Fudenberg and Levine 2006). A growing literature demonstrates demand for commitment in a number of domains ranging from smoking to exercising and real-efforts tasks, as summarized in Table 1. While there is considerable evidence of individuals engaging in commitment contracts when they are *potentially* costly, there is limited evidence that individuals are willing to pay significant amounts for commitment beyond the potential costs of failing to achieve the behavior they are committing to.

The few studies that did elicit willingness to pay (WTP) for commitment found relatively low average willingness to pay among individuals (Chow 2011; Milkman, Minson, and Volpp 2014; Beshears et al. 2015; Houser et al. 2018).<sup>14</sup> For instance, in a study on real-effort allocation over time, over one-half of the individuals were willing to restrict their future choice set when the price of this option is zero, but this demand for commitment dropped to 9 percent when the price of the commitment device was increased to \$0.25 (Augenblick, Niederle, and Sprenger 2015). The lack of evidence of positive willingness to pay for commitment calls into question the viability of market-based commitment devices as a way to help individuals overcome their self-control problems (Laibson 2015, 2018).

In contrast to the existing evidence, study participants in Chennai exhibited significant demand for commitment to sobriety, even at the cost of giving up considerable payments. During each choice session, individuals chose their incentive structure for the subsequent six study days.<sup>15</sup> One-third to one-half of study participants chose sobriety incentives over unconditional payments, even when this choice entailed a potential or certain reduction in study payments (panel A of Figure 2 and online Appendix Table A.6). In each week, about one-half of the individuals chose sobriety incentives over receiving Rs 120 unconditionally. Strikingly, about one-third of individuals in the Choice Group preferred sobriety incentives over receiving Rs 150

<sup>13</sup> Among the six individuals with total savings above Rs 200,000 in the baseline survey, four were in the Choice Group, and two were in the Incentive Group. Only two of them, both in the Choice Group, saved more than the average study participant over the course of the study. However, their influence on the results below was negligible, in particular because these individuals already saved high amounts in the unincentivized Phase 1, which the regressions control for.

<sup>14</sup> A notable exception is the recent evidence in Casaburi and Macchiavello (2019) who find that a vast majority of a sample of Kenyan dairy farmers were willing to accept 15 percent lower output prices in exchange for lowering the frequency of their output payments.

<sup>15</sup> Attrition and inconsistencies of decisions during the choice session pose relatively minor concerns for the analysis (online Appendix Table A.5). In the Choice Group, less than 7 percent of individuals missed their choices in any given week, and, in each week, less than 7 percent of individuals stated inconsistent preferences. Furthermore, over 88 percent of all study participants completed the endline choices with consistent choices. This fraction varies only slightly across treatment groups (90.1 in the Incentive Group and 88.0 in the Choice Group versus 86.7 in the Control Group). In an attempt to be conservative regarding the demand for commitment in Figure 2 and online Appendix Table A.6, an individual was counted as not choosing incentives in any given choice if he did not attend the respective choice session or if he attended, but made inconsistent choices. The regressions in Table 2 are conditional on attendance. The analysis is robust to alternative specifications.

TABLE 1—DEMAND FOR COMMITMENT IN EXISTING STUDIES

Domain Authors (year)—study population (country)	Demand for commitment		Price
	Weakly dominated (potentially costly)	Strictly dominated (for sure costly)	Cost of commitment (for people who commit)
<i>Savings</i>			
Ashraf, Karlan, and Yin (2006): bank clients (Philippines)	28	—	—
Dupas and Robinson (2013): ROSCA members (Kenya)	65	—	—
Beshears et al. (2015): representative panel (USA)	68	72 to 79	1 percent of return
Karlan and Linden (2016): students (Uganda)	44	—	—
Brune et al. (2016): farmers (Malawi)	21	—	—
John (forthcoming): low-income individuals (Philippines)	27 to 42	—	—
Casaburi and Macchiavello (2019): dairy farmers (Kenya)	91	86	15 percent of output price
<i>Work and effort tasks</i>			
Ariely and Wertenbroch (2002): students (USA)	73	—	—
Bisin and Hyndman (2014): students (USA)	31 to 62	—	—
Kaur, Kremer, and Mullainathan (2015): data-entry workers (India)	36	—	—
Augenblick, Niederle, and Sprenger (2015): students (USA)	59	9	\$0.25
Bonein and Denant-Boèmont (2015): students (France)	42	—	—
Toussaert (2018): students (USA)	45	21	\$0.22
Exley and Naecker (2017): students (USA)	41 to 65	—	—
Houser et al. (2018): students (USA)	48	24	\$1
<i>Health-related behaviors</i>			
Giné, Karlan, and Zinman (2010): smokers (Philippines)	11	—	—
Milkman, Minson, and Volpp (2014): gym members (USA)	—	61 percent with WTP > 0 (BDM)	—
Schwartz et al. (2014): grocery shoppers (USA)	36	—	—
Royer, Stehr, and Sydnor (2015): gym members (USA)	12	—	—
Sadoff, Samek, and Sprenger (2015): grocery shoppers (USA)	33	—	—
Alan and Ertaç (2015): chocolate-eating children (Turkey)	69	—	—
Halpern et al. (2015): smoking CVS employees (USA)	14	—	—
Bhattacharya, Garber, and Goldhaber-Fiebert (2015): StickK users	23	—	—
Halpern et al. (2015): smoking CVS employees (USA)	14	—	—
Bai et al. (2017): high-blood-pressure patients (India)	14	—	—
Toussaert (2019): NYU faculty and staff (USA)	48 to 65	—	—
<i>Gaming</i>			
Chow and Acland (2011): game players (USA)	35	—	—
Chow (2011): students (USA)	79	10 percent with WTP > 0 (BDM)	—

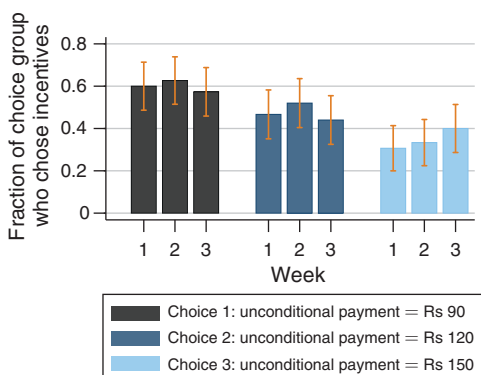
*Notes:* This table summarizes the existing evidence of demand for commitment in academic studies. Column 1 shows the percentage of individuals demanding commitment when using the commitment device is *potentially* costly, i.e. in cases without an explicitly positive price for the commitment contract beyond the potential costs of failing to achieve the behavior individuals are committing to and/or reduced flexibility due to commitment. Column 2 shows the percentage of individuals exhibiting positive willingness to pay for the commitment device, i.e., demand for commitment when engaging in the commitment device requires foregoing or paying financial or other rewards. Column 3 shows the corresponding costs, i.e., the explicit price of commitment in these cases. For excellent and more detailed summaries of the literature, see Bryan, Karlan, and Nelson (2010) and Cohen et al. (2016).

regardless of their breathalyzer scores. Setting aside potential impacts of the incentives on attendance, choosing to forgo Rs 150 implied reductions of Rs 30 (\$0.50) in study payments at the minimum (on days when the individual visits the study office sober) and Rs 90 (\$1.50) at the maximum (on days when the individual visits the study with a positive breathalyzer score), representing between 10 and 30 percent of reported daily labor earnings.

The high demand for incentives does not appear to be the result of misunderstandings. During each choice session, surveyors spent considerable time and effort ensuring participants' sound understanding of the choices faced. In particular, surveyors



Panel A. Demand for incentives over time



Panel B. Demand for incentives across treatment groups

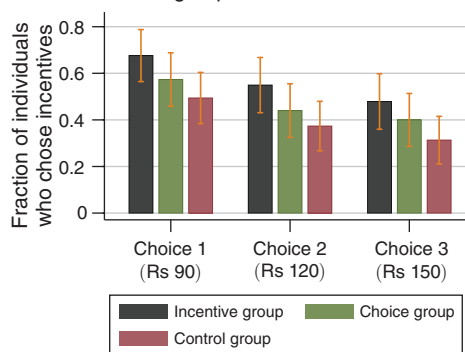


FIGURE 2. CHOICES ACROSS TREATMENT GROUPS AND OVER TIME

Notes: This figure depicts the fraction of individuals who preferred incentives for sobriety over unconditional payments.

- All choices were made for the subsequent six study days. Under incentives for sobriety, if an individual visited the study office, he received Rs 60 (\$1) if his breathalyzer score was positive, and Rs 120 (\$2) if his breathalyzer score was 0.
- Unconditional payments are Rs 90 (Choice 1), Rs 120 (Choice 2), and Rs 150 (Choice 3). Hence, an individual exhibited demand for commitment to sobriety if he chose incentives in Choices 2 and/or 3. During each of the choice sessions, individuals made all three choices before one of them was randomly selected to be implemented.
- If an individual did not complete the set of choices, or if he chose inconsistently, the observation is counted as *not* preferring incentives. During a given choice session, an individual chose inconsistently if he chose Option B for the unconditional amount  $Y_1$ , but Option A for the unconditional amount  $Y_2$  with  $Y_2 > Y_1$ .
- Panel A of the figure shows how the fraction of individuals in the Choice Group who chose incentives evolved over time (On days 7, 13, and 20 of the study). Panel B of the figure depicts the fraction of individuals who chose incentives on day 20 in the three treatment groups. Error bars show 95 percent confidence intervals.

clarified potential losses in study payments as a consequence of their choices. Comprehension questions and further clarifications as needed then solidified comprehension before participants engaged in their choices. Moreover, if participants were making simple “trembling hand” mistakes during their first choice session, one would expect subsequent demand for incentives to decrease over time as participants learned the (potentially negative) consequences of their choices. Instead, if anything, the fraction of individuals choosing sobriety incentives increased slightly over time. In addition, while somewhat overconfident on average, individuals’ beliefs regarding their future sobriety under incentives were fairly accurate on average, in particular in the second half of the study (online Appendix Figure A.4).

Moreover, the demand for incentives exhibits reassuring patterns (Table 2, panel A). First, sobriety during the choice strongly and consistently predicts demand for incentives.<sup>16</sup> Second, individuals’ beliefs regarding the frequency of future sober study office visits strongly predict demand for incentives. Third, the difference in sobriety between Phase 2 (when some individuals were receiving incentives) and

<sup>16</sup>This relationship could reflect the fact that acute alcohol intoxication directly influenced individuals’ choices, but it might also simply reflect the fact that incentives worked better for individuals who visited the study office sober (since they were already incentivized when making their choices).

TABLE 2—DEMAND FOR SOBRIETY INCENTIVES

	Chose incentives					
	Rs 90		Rs 120		Rs 150	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Choices in the choice group (weeks 1, 2, and 3)</i>						
BAC during choice	-1.41 (0.36)		-1.15 (0.34)		-0.74 (0.31)	
Incentives increased sobriety		0.05 (0.06)		0.07 (0.08)		0.15 (0.08)
Expected number of sober days		0.09 (0.01)		0.06 (0.02)		0.04 (0.01)
Observations	211	211	211	211	211	211
$R^2$	0.70	0.75	0.59	0.61	0.45	0.47
Control group mean in week 1	0.60	0.60	0.47	0.47	0.31	0.31
<i>Panel B. Choice by all participants (week 3)</i>						
BAC during choice	-1.69 (0.34)	-1.70 (0.33)	-1.11 (0.34)	-1.13 (0.34)	-1.12 (0.33)	-1.12 (0.32)
Incentives	0.12 (0.07)		0.14 (0.08)		0.14 (0.08)	
Choice	0.10 (0.08)		0.11 (0.08)		0.14 (0.08)	
Pooled alcohol treatment		0.11 (0.06)		0.13 (0.07)		0.14 (0.07)
Observations	215	215	215	215	215	215
$R^2$	0.69	0.69	0.55	0.55	0.50	0.50
Control group mean in week 3	0.49	0.49	0.37	0.37	0.31	0.31

Notes: This table considers correlates of the demand for sobriety incentives.

- In all columns, the outcome variable is whether the individual chose incentives over unconditional payments. The unconditional amounts are Rs 90, Rs 120, and Rs 150, respectively.
- *BAC during choice* refers to the subjects' blood alcohol content measured during the visit to the study office when he was choosing between the incentives and unconditional amounts. Before making these choices, individuals were asked on how many days they expected to visit the study office sober if they were to receive incentives for sobriety during the subsequent six days. *Expected number of sober days* refers to subjects' answer to this question. *Incentives increased sobriety* indicates whether the individual visited the study office sober more often in the preceding (potentially incentivized) phase of the study compared to the unincentivized Phase 2.
- All regressions control for surveyor fixed effects and order of choice fixed effects. Standard errors are in parentheses, and they are clustered by individual in panel A.

Phase 1 (the pre-incentive period) positively predicts demand for incentives, in particular for the Rs 150 choice. This relationship is reassuring since individuals should have chosen costly incentives only when they expected them to help increase their sobriety, which in turn was informed by their own experience in the study.

Given the lack of evidence of significant willingness to pay for commitment in other settings, a natural question is which factors contributed to the relatively high demand for commitment in this setting. While it is difficult to provide a definite answer to this question, several factors may have been important in this context. First, study participants had significant experience with alcohol consumption and the potentially resulting self-control problems. The average study participant had been drinking alcohol for over a decade and many of them had been drinking (almost) daily. This significant experience may have curbed naïveté regarding their self-control problems, a factor that is often implicated in suppressing individuals' demand for commitment (Laibson 2015).

Second, individuals perceived the costs associated with their drinking as significant. Many individuals expressed a strong desire to reduce their drinking in surveys and informal conversations. These men had spent substantial income shares on daily alcohol consumption for many years before participating in the study. Compared to these expenses, the forgone study payments due to the commitment choices may have appeared relatively small to individuals, especially if they implied a positive (perceived) chance of reducing subsequent alcohol consumption in the longer run. Third, by the design of the study, individuals had experience with the incentives when making their choices, similarly to study participants in Kaur, Kremer, and Mullainathan (2015). This exposure may have impacted the demand for incentives and commitment. Fourth, commitment contracts were implicitly defined via choices of different structures of study payments. That is, individuals were not explicitly asked whether they were willing to give up money that they earned in the labor market on their own. As a result, they may have perceived their choices as decisions between various gains rather than considering potential losses in study payments due to commitment choices.

A remaining concern is that social desirability bias may have also contributed to individuals' demand for commitment. While it is impossible to rule out such effects altogether, several reasons may mitigate concerns regarding potential social desirability bias. First, the stakes involved in individuals' choices were considerable, which is reassuring given recent evidence suggesting that demand effects are less likely to occur with high stakes (de Quidt, Haushofer, and Roth 2018). Second, demand for commitment in the Choice Group was elicited three times over the course of two weeks. As a result, individuals had opportunities to learn about the costs of potentially suboptimal choices induced by demand effects as well as about the lack of negative consequences of their choices and/or visiting the study office inebriated beyond potentially lower study payments as part of the incentive scheme. Third, almost all studies eliciting demand for commitment are subject to similar concerns. However, demand for commitment in most of these studies is low, suggesting that there are reasons other than social desirability contributing to the high demand for commitment in my setting.

The structure of the experiment makes it possible to consider whether exposure to sobriety incentives in the past affected the demand for the incentives. For all three choices, the Incentive Group was more likely to choose incentives than was the Control Group (panel B of Figure 2). The fraction of individuals choosing incentives in the Choice Groups (on day 20) was in between the corresponding fractions in the Incentive and Control Groups. The corresponding regressions show differences between the fraction choosing incentives in the Incentive and Control Groups for all 3 choices, though only columns 3 through 6 show statistically significant differences (Table 2, panel B). Again, higher sobriety during the time of choosing predicted a higher probability of choosing incentives.

#### IV. The Impacts of Increased Sobriety

Day drinking among cycle-rickshaw drivers in Chennai is a common phenomenon. About one-half of the study participants in the Control Group reported drinking during the day (panel B of Figure 4). Consistent with these self-reports, breathalyzer

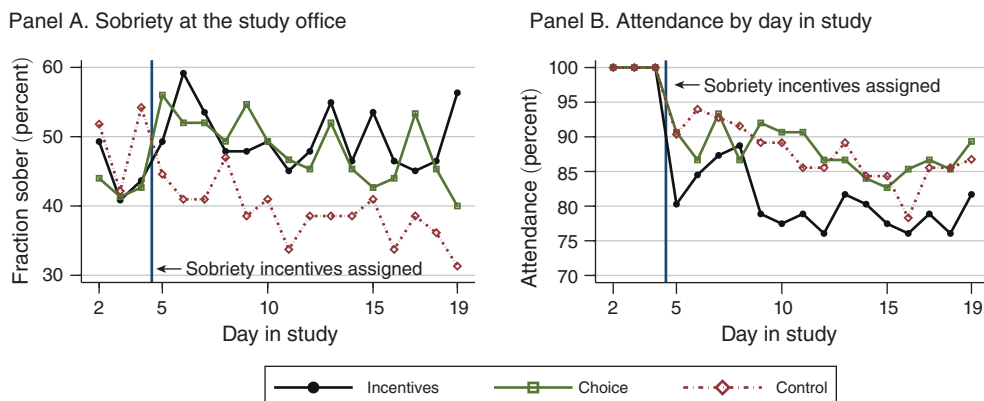


FIGURE 3. THE IMPACT OF INCENTIVES ON SOBRIETY AND ATTENDANCE

Notes: This figure shows sobriety and attendance by study day for the three sobriety incentive treatment groups.

- Panel A shows the fraction of individuals who visited the study office sober. The indicator variable *Sober at the study office* takes on the value 1 for a study participant on any given day of the study if he (i) visited the study office on this day, and (ii) his breathalyzer test was exactly zero. On any given day, the variable takes on the value 0 for individuals who visited the office, but had a positive breathalyzer test score and for individuals who did not visit the study office on that day.
- Panel B shows the fraction of individuals who visited the study office. Since only individuals who came to the study office on days 2 through 4 were fully enrolled in the study, by construction, attendance is 100 percent on days 2 through 4.

scores during study office visits indicated significant inebriation levels. The average blood alcohol content (BAC) in the Control Group was 0.09 percent, exceeding the legal driving limit in most US states (0.08 percent). Against this background, I designed the above-described experiment to investigate whether financial incentives can alter these drinking patterns and to estimate the impact of increased sobriety on labor market outcomes and savings behavior.

#### A. The Impact of Incentives on Daytime Drinking

Financial incentives significantly reduced daytime drinking, as indicated by three measures. My main measure to assess the impact of incentives on daytime drinking is the fraction of individuals who arrived sober at the study office among *all* enrolled participants. That is, anyone who did not visit the study office on a particular day was counted as “not sober at the study office,” along with individuals who arrived at the study office with a positive BAC. Since attendance in the Incentive Group was lower than in the Control Group, as shown in panel B of Figure 3, this measure is preferable to other measures of sobriety as it is less vulnerable to attrition concerns (see also the discussion in Section IVE). This main measure of sobriety is complemented by average breathalyzer scores and the self-reported number of drinks before arriving at the study office, both conditional on attendance.

Financial incentives significantly increased the fraction of individuals who arrived at the study office sober (panel A of Figure 3). In the pre-incentive period, about one-half of the individuals in each of the three groups visited the study office sober. This fraction gradually declined in the Control Group to about 35 percent by

the end of the study.<sup>17</sup> In contrast, with the start of the incentivized period, sobriety in the Incentive and Choice Groups increased by about 10 to 15 percentage points. Subsequent sobriety at the study office also declined in these two groups, but the difference to the Control Group remained roughly constant. Remarkably, the point estimates for the two treatments on the different sobriety measures are nearly identical, despite the fact that only about two-thirds of individuals in the Choice Group chose to receive sobriety incentives.<sup>18</sup> This pattern is unsurprising during the first seven days of the study, as all individuals in the two groups faced identical incentives then. However, sobriety levels in these two groups tracked each other even after about one-third of the individuals in the Choice Group chose not to continue receiving incentives. Given the standard errors of the estimates, I cannot rule out that the treatment effects are significantly different across the two groups. Nevertheless, taken at face value, the similarity of drinking patterns in the Choice and Incentive Groups suggests sophistication regarding the impact of the incentives on individuals' sobriety. That is, individuals who chose incentives had larger-than-average treatment effects on sobriety. Conversely, individuals who chose unconditional payments had smaller-than-average treatment effects on sobriety.<sup>19</sup>

The corresponding regressions confirm the visual results (panel A of Table 3). Individuals in the Incentive and Choice Groups were approximately 13 percentage points more likely to visit the study office sober than individuals in the Control Group. Conditional on visiting the study office, the average BAC in the Incentive Group was 2 to 3 percentage points lower. Moreover, both treatments reduced the reported number of drinks imbibed before visiting the study office by about one standard drink from a base of just under three standard drinks. These treatment effects each correspond to a one-quarter to one-third change relative to the Control Group.

### B. *The Impact of Incentives on Overall Drinking*

The impacts of sobriety incentives on overall alcohol consumption were considerably smaller than the impacts on daytime drinking, implying that subjects who responded to the incentives mostly shifted their alcohol consumption to later times of the day rather than reducing their overall consumption or not drinking at all. Panel A of Figure 4 shows the evolution of reported number of drinks before coming to the study office and overall. While the incentives reduced drinking before study office visits significantly, the impact on overall drinking was considerably smaller.<sup>20</sup> Consistent with these results, the incentives caused a shift in the distribution of the timing of individuals' reported first drink of the day (panel B of

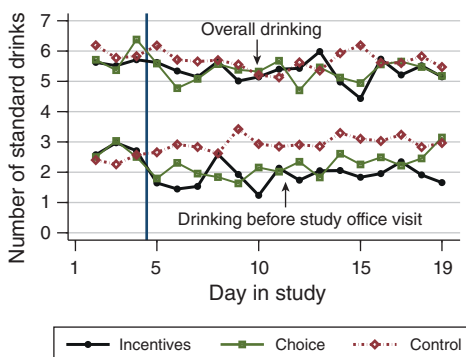
<sup>17</sup>The decline in sobriety in the Control Group over the course of the study is in part explained by the lower overall attendance in *all* treatment groups. In addition, individuals may have felt more comfortable visiting the study office inebriated or drunk at later stages of the study.

<sup>18</sup>Whether an individual in the Choice Group received incentives was determined by the choice that was randomly selected to be implemented for this individual. Choice 1 was implemented 90 percent of the time, such that the fraction of individuals in the Choice Group who actually received incentives closely tracked the fraction of individuals who chose incentives over Rs 90.

<sup>19</sup>These discussions assume that self-imposed and externally imposed incentives were equally effective, which may not have been the case. For instance, external incentives may have decreased intrinsic motivation to stay sober (Bénabou and Tirole 2003).

<sup>20</sup>Notably, overall drinking in the Control Group was already slightly higher on days 2 to 4, i.e., before the incentives to remain sober were assigned.

Panel A. Drinking before study office visit and overall



Panel B. Time of first drink

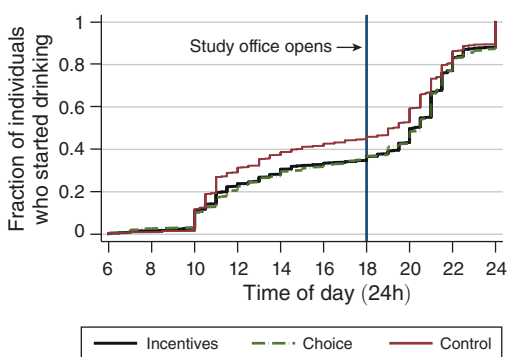


FIGURE 4. THE IMPACT OF INCENTIVES ON DAY DRINKING AND OVERALL DRINKING

Notes: This figure shows the impact of the two sobriety treatments on drinking patterns.

- Panel A shows the self-reported number of standard drinks consumed before the study office visit and the overall number of standard drinks consumed per day.
- Panel B shows the CDF of individuals' reported time of their first drink on any given day.

Figure 4). About 10 percent of the individuals in the Incentive and Choice Groups delayed the time of their first drink from between 10 AM and noon to the evening. Importantly, however, individuals in the Incentive and Choice Groups did not arrive at the study office earlier than individuals in the Choice Group. In fact, on average, individuals in the Incentive Group visited the study office a few minutes later (online Appendix Table A.7).

These visual impressions are confirmed by the regression results (panel B of Table 3). First, both treatments reduced reported overall alcohol consumption by about 0.3 standard drinks per day, about one-third of the effect on the reported number of drinks before coming to the study office as described above. None of these estimates are statistically significant. Second, the reduction at the extensive margin of drinking was small at best. The point estimate for the pooled treatment effect suggests a 2 percentage point increase in reported abstinence from drinking altogether on any given day, but none of the estimates are statistically significant. Third, the treatment effect on reported overall alcohol expenditures is a reduction of about Rs 9 per day, with a point estimate of Rs 8.8 for the pooled treatment effect.

### C. The Impact of Increased Sobriety on Labor Market Outcomes

Alcohol consumption has long been hypothesized to interfere with individuals' ability to earn income, yet well-identified causal evidence is scarce (Cook and Moore 2000).<sup>21</sup> While positive, I estimate the effect of sobriety incentives on earnings to be

<sup>21</sup> Irving Fisher (1926) was among the first to investigate the relationship between alcohol and productivity. Based on small-sample experiments by Miles (1924) that showed negative effects of alcohol on typewriting efficiency, Fisher (1926) argued that drinking alcohol slowed down the "human machine." He also argued that industrial efficiency was one of the main reasons behind the introduction of alcohol prohibition in the United States. While many studies since Fisher (1926) have considered the relationship between alcohol consumption, income, and productivity (for an



TABLE 3—THE EFFECT OF INCENTIVES ON SOBRIETY

	Sober		BAC		Number of drinks	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A. Drinking before study office visits</i>						
Incentives	0.12 (0.04)		−0.03 (0.01)		−1.07 (0.26)	
Choice	0.14 (0.04)		−0.02 (0.01)		−0.86 (0.24)	
Pooled alcohol treatment		0.13 (0.04)		−0.02 (0.01)		−0.96 (0.22)
Observations	3,435	3,435	2,932	2,932	2,929	2,929
R <sup>2</sup>	0.31	0.31	0.46	0.46	0.32	0.32
Control group mean	0.39	0.39	0.09	0.09	2.96	2.96
	Number of drinks		No drink		Rs/day	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel B. Overall drinking</i>						
Incentives	−0.30 (0.26)		0.02 (0.03)		−8.06 (5.14)	
Choice	−0.30 (0.26)		0.02 (0.03)		−9.35 (4.63)	
Pooled alcohol treatment		−0.30 (0.22)		0.02 (0.03)		−8.76 (4.36)
Observations	2,932	2,932	2,930	2,930	2,932	2,932
R <sup>2</sup>	0.18	0.18	0.07	0.07	0.18	0.18
Control group mean	5.65	5.65	0.10	0.10	90.89	90.89

*Notes:* This table considers the effect of the two sobriety incentive treatments on drinking patterns before and during study office visits (panel A) as well as on overall drinking (panel B). All regressions use data from day 5 through day 19 of the study (i.e., the treatment period).

- Panel A: The outcome variable in columns 1 and 2 indicates whether an individual on a given day visited the study office *and* had a zero breathalyzer score on this day, and 0 otherwise. Individuals who did not visit the study office on any given day are counted as “not sober at the study office.” Columns 3 and 4 consider individuals’ measured blood alcohol content from a breathalyzer test. Columns 5 and 6 consider the reported number of drinks *before* visiting the study office on any given day.
- Panel B: Columns 1 and 2 consider the overall number of drinks on any given day. Columns 3 and 4 consider abstinence, i.e., instances of no drinking at all on any given day. Columns 5 and 6 show reported expenditures on alcohol consumption (Rs/day).
- All regressions include Phase 1 Controls and Baseline Survey Controls. Phase 1 Controls are the fraction of sober days, mean BAC during study office visits, the mean reported number of standard drinks consumed before coming to the study office and overall, and reported overall alcohol expenditures (all in Phase 1). Baseline Survey Control variables are baseline survey variables shown in online Appendix Tables A.2 through A.4. Standard errors are in parentheses, clustered by individual.

relatively small and statistically insignificant, with a point estimate for the pooled treatment effect of Rs 7.2 per day (columns 1 and 2 of Table 4). Similarly, the estimates on labor supply are relatively small and not statistically significant. In fact, surprisingly, the estimates of the treatment effect on labor supply at the extensive margin (i.e., whether an individual worked at all on any given day) are negative (columns 3 and 4). In contrast, the point estimates on hours worked overall are positive

overview, see Science Group of the European Alcohol and Health Forum 2011), there is a dearth of well-identified studies of the causal effect of alcohol on earnings and productivity, especially in developing countries.

TABLE 4—THE EFFECT OF SOBRIETY INCENTIVES ON LABOR MARKET OUTCOMES

	Earnings		Did any work		Hours worked	
	(1)	(2)	(3)	(4)	(5)	(6)
Incentives	7.35 (16.34)		−0.05 (0.03)		0.03 (0.35)	
Choice	6.97 (19.09)		−0.02 (0.03)		0.06 (0.34)	
Pooled alcohol treatment		7.15 (15.41)		−0.04 (0.03)		0.05 (0.31)
Observations	3,085	3,085	3,085	3,085	3,083	3,083
$R^2$	0.34	0.34	0.09	0.09	0.18	0.18
Control group mean	287.71	287.71	0.89	0.89	6.83	6.83

Notes: This table shows the impact of the two sobriety incentive treatments on labor market outcomes.

- All regressions use data from day 5 (the first day of sobriety incentives) through day 19 (the last day of sobriety incentives) of the study.
- The outcome variables are (i) reported earnings (Rs/day; columns 1 and 2), (ii) whether an individual worked on a particular day (columns 3 and 4), and (iii) the (unconditional) number of hours worked per day (columns 5 and 6). If an individual did not work on any given day, this day is counted as zero hours worked.
- The data used in the regressions are from retrospective surveys on the consecutive study days, during which individuals were asked about earnings and hours worked on the previous day. In addition, individuals were asked about the same outcomes two or three days previously if they missed a day or two and on Mondays.
- Standard errors are in parentheses, clustered by individual. All regressions include Phase 1 and Baseline Survey Controls as in Table 3.

in most specifications, though, again, none of them is statistically significant (columns 5 and 6).

Importantly, these estimates do not imply that alcohol does not have profound effects on labor-market outcomes for at least three reasons. First, the estimates in Table 4 are relatively imprecise, such that I cannot rule out meaningful effects of daytime drinking on labor-market behavior. While large in relative terms, the impact of the incentives on daytime drinking was only moderate in absolute terms (13 percentage points). A more powerful intervention to reduce daytime drinking might well cause larger effects. Second, the impact of reduced drinking in the medium or long run might be much larger than the short-run effects considered in this paper. For instance, a cycle-rickshaw driver may need to build up a reputation as a reliable, sober driver to be able to receive more trips. Third, in my setting, the potentially negative impact of alcohol on productivity and labor supply due to reduced physical or cognitive function may have been mitigated by the analgesic effects of alcohol, which may not be the case in other settings.

#### D. The Impact of Increased Sobriety on Savings Behavior

Inebriation levels during study office visits were negatively correlated with daily amounts saved during the same office visits, both across Control Group participants and within the same individuals over time (Appendix Figure A.8). The results from this experiment suggest that this correlation reflects a causal impact. Both sobriety incentive treatments increased savings at the study office (panel A of Figure 5). Until day 4, when individuals learned their incentive-treatment status, average amounts saved were nearly identical across treatment groups. After the start of the incentivized period, individuals in the Incentive and Choice Groups saved 46 percent and

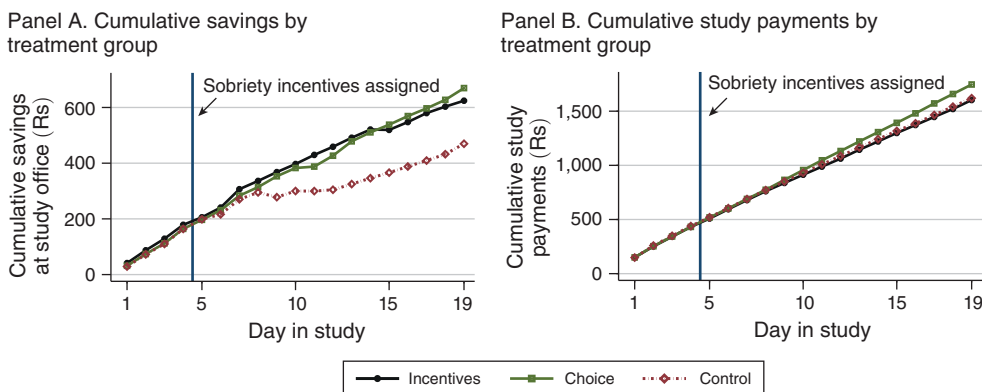


FIGURE 5. CUMULATIVE SAVINGS AND STUDY PAYMENTS BY DAY IN STUDY

Notes: This figure depicts subjects' cumulative savings at the study office (panel A) and cumulative study payments (panel B) by alcohol incentive treatment group.

65 percent more until the end of the study (Rs 446 and Rs 505 in the Incentive and Choice Groups, respectively, compared to Rs 306 in the Control Group). The difference in savings across treatment groups did not emerge immediately after the beginning of the incentivized period, but accumulated mainly between days 8 and 15.

The corresponding regression results in Table 5 confirm the visual evidence. Individuals in both the Incentive and Choice Groups saved more at the study office, though only the coefficients for the Choice Group are statistically significant. My preferred estimate, the pooled estimate controlling for study payments, shows an impact of Rs 11.64 (column 4), which corresponds to an increase of 57 percent compared to Control Group savings of Rs 20.42. Remarkably, this intention-to-treat (ITT) estimate of the impact of sobriety incentives on savings is of the same order of magnitude as increasing the matching contribution on savings from 10 to 20 percent, or introducing a commitment feature to the savings option.<sup>22</sup>

The estimated treatment effect is larger for the Choice Group than for the Incentive Group, though I cannot reject that the coefficients for the two treatment groups are equal. Differential study payments across treatment groups may have been responsible for these differences or for any differences in savings across treatment groups more generally. Indeed, the Choice Group received slightly higher study payments (Rs 7 per day) compared to the Control Group (panel B of Figure 5). However, the Incentive Group received slightly lower study payments than the Control Group, which implies that differences in average study payments cannot explain the higher savings in this group. Accordingly, controlling for study payments does not substantially alter the estimated treatment effects. For instance, controlling for study payments reduces the estimate for the pooled treatment effect from Rs 14.08 to Rs 11.64 per day (columns 3 and 4 of Table 5).

<sup>22</sup> As discussed above, individuals in the *No commitment savings* group were also given a weak commitment feature since they were only able to withdraw money during their study visits between 6 PM and 10 PM. Therefore, the estimate for *Commitment savings* is likely an underestimate of the potential impact of commitment on savings.

TABLE 5—THE EFFECT OF SOBRIETY INCENTIVES ON SAVINGS AT THE STUDY OFFICE

	Amount saved at study office					+	–
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Incentives	11.28 (6.22)	10.69 (6.02)					
Choice	16.62 (5.58)	12.54 (5.48)					
Pooled alcohol treatment			14.08 (4.98)	11.64 (4.86)	26.38 (8.37)	17.68 (6.38)	8.70 (5.12)
High matching contribution	9.34 (4.66)	10.56 (4.42)	9.27 (4.64)	10.54 (4.42)	16.81 (7.67)	9.28 (6.49)	7.53 (4.15)
Commitment savings	5.31 (4.92)	4.69 (4.78)	5.27 (4.92)	4.68 (4.77)	13.68 (7.08)	4.02 (5.70)	9.66 (3.22)
Savings at study office in Phase 1 (Rs/day)	0.60 (0.08)	0.55 (0.08)	0.60 (0.08)	0.55 (0.08)	0.59 (0.08)	0.73 (0.06)	–0.13 (0.04)
Daily study payment (Rs)		0.32 (0.05)		0.32 (0.05)			
Pooled alcohol treatment × commitment save					–14.03 (10.41)	–8.44 (7.93)	–5.59 (4.85)
Pooled alcohol treatment × high match					–11.69 (11.00)	–3.30 (8.51)	–8.39 (5.28)
Observations	3,435	3,435	3,435	3,435	3,435	3,435	3,435
$R^2$	0.13	0.14	0.12	0.14	0.13	0.43	0.02
Control group mean	20.42	20.42	20.42	20.42	20.42	28.61	–8.19

Notes: This table shows the impact of the two sobriety incentive treatments on participants' daily amount saved at the study office (Rs/day).

- All regressions use data from day 5 (the first day of sobriety incentives) through day 19 (the last day of sobriety incentives) of the study. The outcome variable is the daily (net) amount saved at the study office. If an individual did not visit the study office on any given day of the study, the amount saved is set to zero on this day. Similarly, the daily study payment is zero for those observations.
- *High matching contribution* indicates whether an individual was offered a 20 percent matching contribution on their savings as opposed to 10 percent. *Commitment savings* indicates whether the individual was not allowed to withdraw their savings until the last day of the study.
- Columns 1 and 2 show regressions for the two sobriety incentive treatments separately. Columns 3 and 4 show pooled regressions for the Incentive and Choice Groups.
- Column 5 shows the same regressions including interactions between the pooled alcohol treatment and the two savings treatment conditions. Columns 6 and 7 show the same regressions for deposits (i.e., weakly positive savings) and withdrawals (i.e., weakly negative savings), respectively. If an individual did not make any deposit or withdrawal on a given day, the withdrawal or deposit amount is set to 0 for this variable on that day.
- Standard errors are in parentheses, clustered by individual. All regressions control for Phase 1 and Baseline Survey Controls as described above, as well as for Phase 1 savings.

### E. The Role of Differential Attendance

Given the attendance patterns shown in panel B of Figure 3 (see also online Appendix Table A.8), it is important to understand whether the results above can be explained by differential attendance. Overall attendance across all treatment groups and days of the study was high, 88.4 percent overall and 85.4 percent post-treatment assignment.<sup>23</sup> However, compared to the Choice and Control Groups, individuals in the Incentive Group were about 7 percentage points less likely to visit the study

<sup>23</sup> By construction, attendance in the lead-in period (Phase 1) was 100 percent.

office post Phase 1. This attendance gap emerged with the start of sobriety incentives and remained relatively constant thereafter. One potential explanation for this difference is that some individuals in the Incentive Group were not able or willing to remain sober until their study office visit on some days, and, hence, faced reduced incentives to visit the study office on those days. This explanation is consistent with the fact that there was no attendance gap between the Choice and Control Groups because individuals for whom sobriety incentives were not effective or preferable were able to select out of them.

The treatment effects on sobriety and saving do not appear to be explained by the attendance patterns. Differential attendance only occurred in the Incentive Group, i.e., the Choice Group was as likely to visit the study office as the Control Group, which makes an explanation based on attendance alone implausible. Moreover, importantly, the two main outcome measures for sobriety and saving are chosen to yield conservative estimates of treatment effects. The main measure of sobriety in the study is the fraction of individuals who arrived sober at the study office among *all* individuals in the respective treatment groups. This measure is conservative given the lower attendance in the Incentive Group compared to the other treatment groups. To make this point more formally, panel A of online Appendix Table A.9 shows estimates of Lee (2009) bounds as well as Imbens and Manski (2004) 95 percent confidence intervals for each of the treatments individually and for the combined sobriety treatments. If anything, the estimated impacts of the incentives on sobriety are larger than in the baseline specification shown in Table 3.

The main measure of individuals' savings was constructed to be conservative as well. In particular, the daily amount saved at the study office was set to 0 whenever an individual did not visit the study office. This measure of savings behavior is conservative given that even individuals with high levels of inebriation saved positive amounts on average (online Appendix Figure A.8). Panel B of online Appendix Table A.9 shows the corresponding Lee (2009) bounds. While all point estimates are positive, the confidence interval for the Incentive treatment includes 0. Since differential attendance is not an issue for the Choice Group, the bounds and confidence interval are considerably tighter and strictly positive for the Choice treatment. However, given the wide bounds for the Incentive Choice Group, the bounds for the pooled treatment groups are fairly wide as well and the confidence interval includes 0.<sup>24</sup>

#### *F. Household Resources and Other Expenditures*

The increase in savings due to the incentives treatments does not appear to have crowded out other resources available to individuals' families (columns 1 through 6 of online Appendix Table A.11). Such effects would be concerning since rickshaw drivers often give resources to their wives as an implicit way to save money. However, though imprecisely estimated, I find suggestive evidence that sobriety incentives increased money given to wives by about Rs 10.5 (column 2). In contrast, resources spent on other family expenses decreased by about Rs 7.3 (column 4) such

<sup>24</sup>In a related robustness check, online Appendix Table A.10 shows that the regression results from Table 5 are largely unchanged by winsorizing the data.

that reported overall resources spent on family expenses increased by about Rs 3.2 (column 6), again not a statistically significant estimate.

There is no systematic evidence of the incentives affecting expenditures on other goods (columns 7 through 12 of online Appendix Table A.11). Expenses on food outside of the household as well as on coffee and tea remained nearly unchanged (columns 8 and 10). Of particular interest are expenditures on tobacco products, as they are often thought of as complements to alcohol (Room 2004). However, there is no evidence of consistent impacts on these expenses (column 12). The lack of impacts is perhaps not particularly surprising in light of the fact that reported expenditures on tobacco and paan products are low to begin with.<sup>25</sup> Moreover, the incentives reduced overall alcohol expenditures only moderately, therefore limiting the scope of effects through complementarities in consumption.

## V. Mechanisms

The observed impacts of the sobriety incentives on individuals' savings patterns raise the question of whether these impacts reflect changes in individuals' savings decisions for *given* resources. An alternative or complementary channel could be increased income net of alcohol expenditures. This section provides two pieces of evidence to support the hypothesis that increased sobriety altered individuals' decisions beyond such mechanical effects.

### A. Accounting for Mechanical Effects

Assessing the contribution of increased resources requires knowledge of the marginal propensity to save (MPS) out of additional resources. As described in Section IIC, the lottery was designed to provide an estimate of the MPS by inducing random variation in study payments. There are important differences between the lottery payments and other increased resources due to reduced alcohol expenditures or increased earnings and study payments. For instance, the lottery payments were one-time payments by construction, while increased study payments or earnings were more permanent in nature. Moreover, individuals may have been overconfident about their future earnings due to expected increases in sobriety.<sup>26</sup> With such caveats in mind, using the randomized lottery payments, I estimate a marginal propensity to save of 0.16 to 0.19 in the Control Group, and 0.30 to 0.40 in the pooled alcohol treatment groups (online Appendix Table A.12).

Combining the estimates from the previous sections, it is now possible to assess the share of the increase in savings explained by mechanical effects, under the fairly strong assumption that the MPS estimate from the lottery is an accurate approximation of individuals' marginal propensity to save out of increased earnings net of alcohol expenditures. The starting point in this decomposition is the estimate

<sup>25</sup> Paan is a mixture of ingredients including betel leaf, areca nut, and often tobacco. Chewing paan is popular in many parts of India.

<sup>26</sup> There are some additional concerns regarding the validity of the lottery as a way to estimate the MPS since some individuals early in the study won the lottery unusually often. However, excluding these individuals from the estimation in fact *reduces* the estimated MPS and therefore the contribution of mechanical effects to the savings results described above.



of Rs 11.64 for the pooled sobriety incentive treatment effect from Table 5 (which controls for study payments). From this estimate, I use the control group's MPS to subtract mechanical effects of (i) the contribution of reduced alcohol expenditures (Rs 1.49), and (ii) the contribution of increased earnings (Rs 1.22).<sup>27</sup> This calculation leaves an unexplained treatment effect of Rs 8.93. This amount corresponds to over two-thirds of the overall treatment effect, or one-third of savings in the Control Group, suggesting that increased sobriety indeed impacted savings behavior beyond mechanical effects on incomes net of alcohol expenditures.

### B. Interactions between Commitment Devices

The structure of the experiment allows for an additional test of the hypothesis that increasing sobriety mitigates self-control problems. If self-control problems prevent individuals from saving as much as they would like to, and if commitment savings products help sophisticated individuals overcome these problems, then commitment savings should have a larger effect for individuals with more severe self-control problems. Hence, if alcohol intoxication reduces self-control, then increasing sobriety should lower the effect of commitment savings.<sup>28</sup>

A simple model of a present-biased consumer as in Laibson (1997) formalizes this intuition in the online Appendix. A specific case (iso-elastic utility) demonstrates two features of this model. First, the impact of commitment savings is an inverse-U-shaped function of present bias for sophisticated individuals. The impact of commitment savings devices on savings is lowest for individuals without present bias ( $\beta \approx 1$ ) and for the most present-biased individuals ( $\beta \approx 0$ ). Thus, for individuals with the greatest need to overcome self-control problems, commitment savings devices in the form in which they are often offered may only be moderately helpful (if at all).<sup>29</sup> Second, in the iso-elastic case and for the empirically relevant parameter range of  $\beta > 0.5$ , an increase in  $\beta$  lowers the impact of commitment savings on savings. While this result may not generalize beyond the iso-elastic case, a decrease in the impact of commitment savings due to increased sobriety can be viewed as evidence for increased self-control due to increased sobriety.

Since the sobriety incentives and the enhanced commitment-savings feature were cross-randomized, I can investigate whether the two interventions are substitutes or complements in their impact on savings. Figure 6 provides evidence that they are substitutes. The figure depicts cumulative savings by the pooled sobriety treatment and the cross-randomized savings conditions.<sup>30</sup> In panel A, individuals are divided

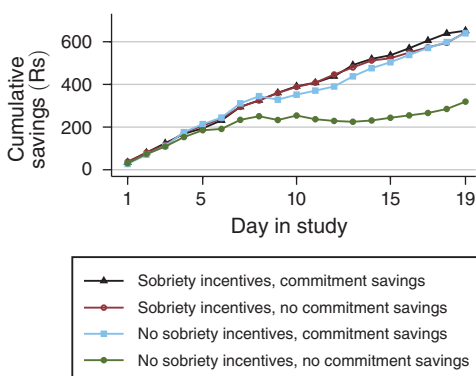
<sup>27</sup> I use the control group MPS (0.17, column 6 of online Appendix Table A.12) since the calculations are meant to understand how much of the estimated treatment effects can be explained by mechanical effects under the null hypothesis that alcohol has no effect on intertemporal choice.

<sup>28</sup> This simple intuition overlooks an additional, opposing effect. While commitment savings products may help individuals overcome self-control problems in future savings decisions by preventing them from withdrawing their savings prematurely, the immediate decision to save always requires incurring instantaneous costs. A sophisticated individual with severe self-control problems may not save (much) even if a commitment savings product is offered, simply because he does not put much weight on future consumption. In the extreme case of no self-control, the individual will not save regardless of the availability of a commitment option.

<sup>29</sup> Interventions designed along the lines of Save More Tomorrow (Thaler and Benartzi 2004) overcome this problem, since they allow individuals to commit to saving more without reducing current consumption.

<sup>30</sup> The two sobriety treatments are pooled solely for expositional purposes. The equivalent graphs without pooling the sobriety treatment groups show only very minor differences in savings behavior between the Incentive and

Panel A. Interaction of sobriety incentives and commitment savings



Panel B. Interaction of sobriety incentives and matching contribution

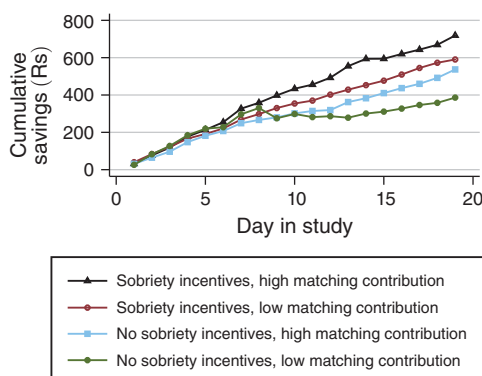


FIGURE 6. INTERACTION BETWEEN SOBRIETY INCENTIVES AND SAVINGS TREATMENTS

Notes: This figure shows the interaction between the cross-randomized sobriety incentives and savings treatments. Panel A shows cumulative savings for four different groups, who were offered:

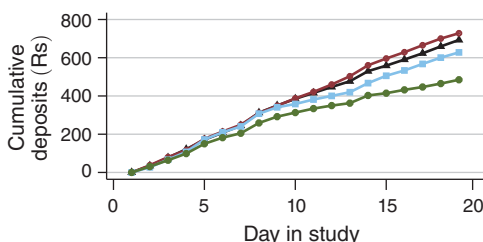
- (i) neither sobriety incentives nor commitment savings (green line with solid circles),
- (ii) no sobriety incentives, but commitment savings (blue line with squares),
- (iii) sobriety incentives, but not commitment savings (red line with solid circles), and
- (iv) both sobriety incentives and commitment savings (black line with triangles).

Panel B shows the equivalent graph for the interaction between receiving sobriety incentives and a high matching contribution (20 percent instead of 10 percent on the amount saved by day 20).

into four groups according to whether they were offered sobriety incentives (pooling the Incentive and Choice Groups) and whether their savings option included the cross-randomized commitment-savings feature. Cumulative savings for the four groups were nearly identical through the pre-incentive period until day 4, and throughout the study, three of the four lines in the graph remain nearly indistinguishable. However, the group that received neither commitment savings nor the alcohol treatment saved distinctly less than each of the remaining groups subsequently. While both incentives for sobriety and the commitment-savings feature had a large impact on savings on their own, being assigned to both of these treatments did not further increase savings.

The corresponding regression estimates are in line with the visual impression (column 5 of Table 5). The estimated interaction effect between sobriety incentives and commitment savings is negative and about the same size as the estimated impact of sobriety incentives and commitment savings on their own. However, standard errors are large and the coefficient is by no means statistically significant at conventional levels ( $p = 0.18$ ). The differences across treatment groups were due to differences in both deposits and withdrawals (columns 6 and 7 of Table 5 and Figure 7). Compared to the group with neither incentives for sobriety nor commitment savings, sobriety incentives and commitment savings each on their

Panel A. Sobriety versus commitment savings: cumulative deposits



Panel B. Sobriety versus commitment savings: cumulative withdrawals

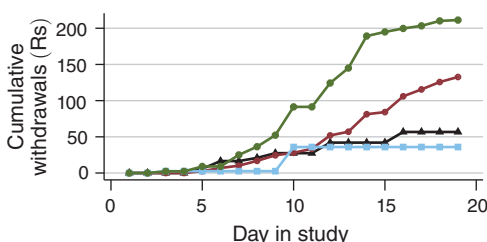


FIGURE 7. DEPOSITS AND WITHDRAWALS

Note: This figure splits up the results shown in panel A of Figure 6 into cumulative deposits (panel A) and cumulative withdrawals (panel B).

own increased deposits, and reduced withdrawals. However, the coefficients are similarly imprecise.<sup>31</sup>

These results suggest that increasing sobriety reduced self-control problems in savings decisions. An alternative interpretation could be that alcohol is a key temptation good for this population such that reducing alcohol consumption mitigates the need for commitment savings. However, given that the intervention only moderately reduced overall alcohol consumption and expenditures, this channel is unlikely. A second competing explanation could be that there was an upper bound on how much individuals were able to or wanted to save. However, this explanation is inconsistent with the fact that about one-half of the negative interaction is explained by withdrawals rather than by deposits only. Moreover, average daily savings were well below the savings limit of Rs 200 per day and individuals saved Rs 200 only about 6 percent of the time. Over the course of the study, all individuals received relatively large study payments in addition to their usual earnings outside of the study, which appear to have been largely unaffected by the study. Accordingly, the majority of individuals would have been able to increase their savings if they had preferred to do so.

## VI. Conclusion

Heavy alcohol consumption is common among low-income workers in India. Many of these men drink alcohol every day, and day drinking is common in some professions. This paper shows that financial incentives for sobriety can significantly reduce day drinking among cycle-rickshaw drivers, but the sobriety incentives do

<sup>31</sup> Notably, the estimated interaction effect between sobriety incentives and the matching contribution treatment is negative as well, though smaller in magnitude. The negative interaction between the sobriety incentive treatment and the high matching contribution could be explained by the commitment feature of the matching contribution (as discussed in Section IIC).

not meaningfully affect overall drinking. Perhaps surprisingly, I do not find evidence of decreased daytime drinking translating into increased labor supply, productivity, or earnings. However, individuals who were randomized to receive sobriety incentives took more advantage of a high-return savings opportunity. These increases in savings appear to be not merely due to mechanical effects of increased income net of alcohol expenditures, but rather due to changes in savings decisions for given resources.

The findings above raise the question of why so many study participants exhibited demand for commitment despite the fact that incentives only caused a moderate reduction in overall drinking. Several not mutually exclusive explanations are possible. First, as documented above, sobriety incentives caused several small benefits which may well add up to Rs 30. Moreover, the ITT estimates reflect average effects of incentives, which mask potentially important heterogeneity in impacts. On average, though not statistically significant, sobriety incentives increased reported earnings and reduced reported alcohol expenditures by about Rs 10 each. In addition, sobriety incentives increased savings significantly and may have also affected other decisions. Moreover, individuals may have valued daytime sobriety on its own despite potentially increased disutility of work due to exacerbated physical pain.

Second, partial naïveté may have contributed to the demand for commitment. Underestimating the extent of their self-control problems due to partial or full naïveté as in O'Donoghue and Rabin (1999) may lower the demand for (costly) commitment by decreasing the perceived benefits of commitment (Laibson 2015). However, partial naïveté can also increase the demand for commitment by causing individuals to overestimate the effectiveness of commitment devices in overcoming their self-control problems.<sup>32</sup> In this context, some individuals may have underestimated their self-control problems or overestimated the usefulness of the incentives for sobriety in reducing their daytime or overall drinking.

While it is impossible to rule out partial naïveté as a contributor to the demand for incentives, there are two reasons to believe that individuals' incentive choices were fairly sophisticated. First, individuals' beliefs regarding their future sobriety (when incentivized) during study office visits were fairly accurate on average. Second, as also discussed above, the ITT estimates of the impacts on sobriety are very similar for the Incentive and Choice Groups. Accordingly, the local average treatment effect for those who took up the incentives voluntarily in the Choice Group exceeded the average treatment effect in the overall sample, which in turn implies some sophistication on behalf of study participants regarding the impact of the incentives.

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<sup>32</sup>For a more detailed discussion and an application in the savings domain, see John (forthcoming).

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