A Laboratory Test of the Resource Curse Hypothesis

Andreas Leibbrandt* and John Lynham**

** Monash University, Department of Economics, 3800 Clayton, Australia, andreas.leibbrandt@monash.edu*

*** University of Hawai'i, Department of Economics, 96822 Honolulu, USA, lynham@hawaii.edu*

Abstract: Conventional wisdom suggests that resource wealth should boost economic growth. Yet there is conflicting evidence about whether natural resources are indeed a blessing or a curse. We make use of specially designed economic experiments to investigate how resource abundance affects economic behavior in the absence or presence of regulatory institutions. We observe that in the absence of regulatory institutions, groups with access to large resource pools use resources less efficiently than groups with access to small resource pools. However, if regulatory institutions are present, we show that groups with access to large resource pools perform better than groups with access to small resource pools. Our findings also reveal that resource users are more willing to regulate access to abundant than to small resource pools. These findings provide causal evidence for the resource curse hypothesis and identify the causes for the pitfalls and potentials of resource wealth.

1. Introduction

Are societies with abundant resources cursed? The resource curse thesis (Auty, 1993) refers to the paradox that some societies with abundant natural resources have worse economic outcomes than those that lack natural resources.[1] Usually, this 'paradox of plenty' is attributed to the crowding-out of activities that improve economic outcomes.[2] Explanations following the crowding-out/in logic are that natural resource wealth crowds-out positive externalities like entrepreneurial activity (Torvik, 2002) and human capital development (Gylfason et al., 1999) or crowds-in anti-growth activities such as rent-seeking (Auty, 2001), violent conflict (Collier and Hoeffler, 2005) and corruption (Vicente, 2010). There is considerable disagreement about the relevance of resource abundance for economic outcomes: some studies conclude that there is indeed a resource curse (Ross, 2001; Sachs and Warner, 1995; Sachs and Warner, 2001) whereas others question its existence altogether (Brunnschweiler and Bulte, 2008; Alexeev and Conrad, 2009).[3] Moreover, there are studies that suggest the institutional environment crucially determines whether large resource pools are a blessing or a curse (Mehlum et al., 2006; Robinson et al., 2006; Boschini et al., 2007) and it also seems possible that the level of resource endowment may determine the institutional environment (Ross, 2001).

One main unsettled question is whether *(i)* the abundance of resources itself, *(ii)* other variables such as the institutional environment, or *(iii)* the interaction between resource wealth

[1] For example, over the period 1965-1998, all lower- and middle-income countries experienced average per capita GNP increases of 2.2% per annum but OPEC countries as a whole experienced per capita GNP *decreases* of 1.3% per annum over the same period. See Gylfason (2001).

[2] Sachs and Warner (2001, p. 833) write: "Most current explanations for the curse have a crowding-out logic. Natural resources crowd-out activity *x*. Activity *x* drives growth." And later (p. 835): "It seems fair to say that some variant of these crowding-out stories are the most likely explanations for the curse of natural resources".

[3] One of the reasons why studies on the resource curse disagree is that they use different measures for resource abundance and economic development outcomes. Sachs and Warner, for example, use exports of natural resources as a % of GDP as a measure for resource abundance, which has been criticized because it captures resource dependence rather than abundance.

and other variables *cause* inferior economic outcomes (Norman, 2009). To provide a rigorous test of the existence of the resource curse and whether it can be prevented, this study uses randomized experimental methods. The main advantage of this approach is the possibility to observe how a single exogenous change in the level of resource abundance affects economic behavior at both the individual and group level. While the decision environment is quite simplified, it still captures the crowding-out potential of resource abundance and the important trade-offs between individual and group benefits that characterize the inefficient exploitation of many natural resources in the field. The main disadvantage is that our findings may be difficult to extrapolate to complex natural field settings, although there is evidence that these kinds of experiments can significantly predict individual resource exploitation decisions in the field (Fehr and Leibbrandt, 2011).

In our experiment, individuals are randomly assigned to societies (groups of three) and simultaneously decide about the extent to which they want to exploit a non-renewable resource (a common pool of money). The experiment lasts until the resource pool of the group is depleted, but maximally for five time periods. If the group's claims do not exceed the capacity of the resource pool then, at the end of each period, a fraction of the resource pool that has not been exploited is transferred to a public good account, which produces positive externalities (an interest rate accrues). The accrued interest is equally distributed among the group members at the end of the experiment. There are four treatments in our experiment in which we vary resource wealth ($20 or $100) and whether individuals have the institutional capacity to limit access to the resource pool (no voting vs. voting over resource protection).

We find sharp treatment differences and a significant resource wealth × institution interaction. If resource wealth is high, individuals request on average *82% more* at the start of the

experiment than when the resource wealth is low. However, if individuals have the option to establish an institution that limits exploitation, individuals exploit on average *50% less* at the start of the experiment if the resource wealth is high than when it is low. Moreover, individuals in the low resource wealth treatment are 3.2 times more likely to vote *against* any resource protection as compared to individuals in the high resource wealth treatment. These treatment dependent behaviors lead to pronounced differences in growth rates. For example, giving subjects the option to establish an institution that limits exploitation increases growth by a factor of 26.8 if the resource wealth is high but only by a factor of 4.4 if the resource wealth is low.

Our experimental set-up provides a *complementary* approach to existing empirical studies on the resource curse which typically rely on cross-country comparisons (Sachs and Warner, 1995, 2001; Brunnschweiler and Bulte, 2008; Alexeev and Conrad, 2009), case studies (Sala-i-Martin and Subramanian, 2003; Wright and Czelusta, 2004; Angrist and Kugler, 2008, Vicente, 2010) or panels (Manzano and Rigobon, 2007; Murshed, 2004; Collier and Goderis, 2007).[4] Results based on non-experimental data are difficult to interpret because their units of observation differ on many, possibly unobservable dimensions, have unique histories, and all or some of these differences may crowd-in unproductive activities. A particular challenge such non-experimental studies face is to understand the combined role of institutions and resources. For example, within the current literature, it remains unclear whether resource abundance affects the institutional environment or whether the institutional environment determines whether resource abundance is a curse. By experimentally randomizing resource abundance and institutions we are able to investigate the causal impact of resource abundance on the institutional environment.

[4] For recent overviews see Wick and Bulte (2009) and van der Ploeg (2011).

Our study may also be of interest for the rich theoretical work exploring mechanisms through which resource abundance influences growth. In line with the design of our resource depletion game, a number of political economy theories emphasize the way that resource booms can encourage rent seeking (Tornell and Lane, 1999; Torvik, 2002; Mehlum et al, 2006; Hodler, 2006, van der Ploeg and Rohner, 2012). For example, in Torvik (2002), a greater amount of natural resources increases the number of entrepreneurs engaged in rent seeking and reduces the number of entrepreneurs running productive firms: more natural resources can thus lead to lower welfare. In Mehlum et al (2006), entrepreneurs can either "grab" rents from natural resources or they can invest them in production. If institutions are weak, all resources are grabbed but if resources are strong then all the resources are invested in production and the spoils are divided equally among all entrepreneurs. In Hodler (2006), natural resources cause fighting activities between rivaling groups; fighting reduces productive activities. We hope that our study fills an intellectual gap between the abstract, causal approach of this theory literature and the real-world, observational approach of the empirical literature.

Finally, our study also contributes to the experimental literature investigating whether cooperation decreases when stakes are increased (Forsythe et al, 1994; Hoffman et al, 1996; Slonim and Roth, 1998; Camerer and Hogarth, 1999; Cameron, 1999; Clark and Sefton, 2001; Cherry et al, 2002; Parco et al, 2002; Rapoport et al, 2003; Carpenter et al, 2005; Johansson-Stenman et al, 2005: Andersen et al, forthcoming), an important topic as out-of-lab cooperation, conserving non-renewable resources in the field, for example, frequently involves very high stakes. In contrast to most of these studies, which report no or only minor stake effects, we find sharp decreases in cooperation levels when stakes are increased. Important differences between these and our study are that we investigate cooperation under different stakes when actors make

simultaneous decisions in groups (N>2) about the extent to which they exhaust a non-renewable resource.[5] In addition, we are not aware of any other experiment that interacts stake size with institutional choice.

2. Experimental Design

We designed a game such that it captures the central feature of most political economy explanations for the resource curse: resource booms attract individuals away from activities that produce positive growth externalities. These explanations follow the crowding-out/in logic and are based on the idea that large resource pools undermine cooperation among society members (i.e., crowd-in conflict/corruption/rent-seeking), and thus harm the functioning of the society. While there are several proposed explanations for the resource curse (Sachs and Warner, 2001), we chose to focus on the crowding-in variant for at least four reasons. First, there is recent evidence that there are significant correlations between cooperation/conflict/corruption measures and economic outcomes (Ross, 2001; Collier and Hoeffler, 2005; Vicente, 2010). Second, there is also evidence for links between the institutional environment and economic outcomes (Bohn and Deacon, 2000; Acemoglu et al, 2001; Mehlum et al, 2006; Robinson et al, 2006; Boschini et al, 2007), which suggests a crucial role of cooperation/conflict/corruption as these behaviors are likely to be related to the institutional environment (Svensson, 2005; Mocan, 2008). Third, the crowding-in variant seems to be less contested than many other variants such as the Dutch Disease (Corden and Neary, 1982; Sachs and Warner, 2001; Mehlum et al, 2006). Fourth, cooperation/conflict can be accurately and objectively identified in a behavioral experiment.

[5] To the best of our knowledge, there are only two studies investigating cooperation in groups for different stake sizes (Marwell and Ames, 1980 and Kocher et al, 2008). Both do not find significant stake size effects in their public goods games. One important difference between their and our decision setting is that subjects can take money away from a group account in our setting, which closer mirrors the real-world resource curse and employs a frame that suppresses warm-glow (Andreoni, 1995).

We call our game the *resource depletion game*. In this game, individuals are randomly assigned to groups of three and simultaneously decide about the extent to which they want to exploit a common pool of money (the resource pool). Each group member has the capacity to deplete the resource pool and the experiment lasts until the resource pool of the group is depleted (but maximally for five time periods). If the group's claims on the non-renewable resource exceed its capacity in a given period then the pool is divided in proportion to the individual requests. If the group's claims do not exceed its capacity then after each period a fraction of the resource pool that has not been exploited (up to 20% of the initial resource endowment) is transferred to a public good account where a one-time (i.e. not compounding) interest rate of 50% accrues.[6] This last feature can be rationalized in different ways, it captures: *i)* investments in public goods/human capital/entrepreneurship/formal sectors etc. that generate positive externalities for society as a whole, *ii)* avoided opportunity cost when resource users refrain from fighting over the resource, or *iii)* an increase in value of a non-renewable resource over time (e.g. because of increasing scarcity). The money invested in the public good and the accrued interest are equally distributed among the group members at the end of the experiment. For example, if \$20 is transferred to the public good account in the first period, then this is increased by 50% to become \$30 and, at the end of the final period, divided equally among the group so that each member receives \$10.

To mirror differences in resource wealth, we assign groups either to a small (\$20; *S-treatment*) or a large resource pool (\$100; *L-treatment*). If resource users are selfish they will

[6] For example, suppose the initial resource pool is \$100. At the end of period 1, \$$R_1$ remains in the pool. If R_1 is greater than 20% of \$100 then \$20 is permanently transferred to the public good account and the group proceeds to period 2 with \$(R_1-20) in the resource pool. If R_1 is less than or equal to 20% of \$100 then R_1 is transferred to the public good account and the game ends. At the end of period 2, \$$R_2$ remains in the pool. If R_2 is greater than 20% of \$100 then \$20 is permanently transferred to the public good account and the group proceeds to period 3 with \$(R_2-20) in the resource pool. If R_2 is less than or equal to 20% of \$100 then R_2 is transferred to the public good account and the game ends. This pattern continues until the end of period 5 since R_5 must always be less than or equal to \$20. Thus, the game can last, at most, for five periods but may end sooner.

immediately deplete the resource pool independently of its size and thus not invest any money in the public good. For simplicity and to obtain a pronounced social dilemma, the game was modelled such that the optimal decision for self-interested individuals is to deplete the resource whereas for society the optimal decision is zero exploitation; i.e., none of the group members extracts any positive amount from the resource pool. We investigate the existence of a resource curse by observing whether exploitation levels are higher among groups assigned large resource pools and, as a consequence, their resources are depleted faster and used in a less efficient manner.

Thus, the incentive structure in the resource depletion game is similar but simpler than the incentive structure in the standard common pool resource game (Ostrom et al, 1992; Ostrom et al, 1994). An important difference to the common pool resource game and negatively framed public goods games (Brewer and Kramer, 1986; Andreoni, 1995) is that the duration of the game depends on the resource users' choices in the resource depletion game whereas it is fixed in the common pool resource game. These features of our game arguably capture more closely the decision resource users face in the field when harvesting non-renewable natural resources (such as diamonds, gold, or oil - natural resources typically associated with the resource curse) because: *i)* there is one resource that can be depleted and *ii)* the resource does not grow or renew over time.

Our experiment has two additional treatments (*VS* and *VL*) where we introduce an institution that can limit access to the resource pool.[7] These two treatments take into account that groups in the field may have the institutional capacity to reconcile their opportunistic interests with the efficient use of the resource pool. We implemented the possibility of establishing a

[7] For experimental studies on voting in different cooperation contexts see Walker et al. (2000) and Tyran and Feld (2006).

regulatory institution through a voting mechanism. Before individuals decided on their exploitation of the resource, they voted over the limitation of access to either the small ($20; VS-treatment) or large ($100; VL-treatment) resource pool. The choices available to subjects were 100% limitation (the resource is completely protected from individual removals), 80%, 60%, 40%, 20% and 0% (no protection – as in treatments S and L). The voting for any of the available choices was always costless. We decided that the majority decision was enforced such that the second lowest voted percentage level was chosen as the restriction level; i.e., the median vote. For example, if group member A chose 100%, B 40%, and C 20%, then 40% of the resource pool was protected from extraction in this period. Before the individuals made their exploitation decision, they were informed about the outcome of the voting decision, i.e. the extent to which access to the resource was limited in a given period. The voting mechanism was chosen in order to give individuals the possibility to implement a strong institution with the help of a majority rule decision as simply and quickly as possible. Every subject in the voting treatments was required before the start of the experiment to answer additional control questions to test that they understood how the mechanism would be implemented.

Because limitation of access to the resource pool for all group members is in each of the group member's own self-interest, one would expect that they use their power to establish a strong institution that limits access to the resource pool completely. However, if there is a resource curse even when such regulatory institutions are available, we should still observe higher exploitation levels in groups with large resource pools and find that these pools are depleted sooner and used in a less efficient manner. If resource abundance can still crowd-in conflict/corruption/selfishness, it should be harder for groups with large resource pools to agree on restrictions.

258 subjects participated in this experiment in the four treatments (S: N=87, L: N=78, VS: N=48, VL: N=45). 40% of subjects were female and 71% were undergraduates (29% were graduate or non-traditional students). The experiments were conducted with the experimental software Z-tree (Fischbacher, 2007). Each of the 258 subjects participated in only one of the treatments. The experiments lasted for maximally one hour including payment. The average payoff was $27.40 including a show-up fee of $5, the minimal payoff was $5, and the maximal payoff was $105. The instructions were neutrally framed, for example, the resource pool was referred to as an 'open group account'. The experimental instructions for all treatments are in the appendix.

3. Experimental Results

Individual Resource Exploitation

Consistent with the resource curse hypothesis, we find that there are large treatment differences when resource exploitation cannot be regulated. The two histograms at the top of Figure 1 illustrate the differences between the S-treatment and L-treatment and show that individual resource exploitation is significantly larger if the resource pool is large. In the S-treatment we observe that individuals request on average *only 37.4%* of the resource pool in the first period. 44.8% refrain from exploitation completely and only 26.4% completely deplete the small resource pool. In contrast, in the L-treatment we observe that individuals request on average *as much as 68%* of the resource pool in the first period (Mann-Whitney U-test, Z=4.326, P<0.0001, two sided, N=165), that only 19.2% refrain from exploitation completely (Fisher's Exact test, P<0.001, two sided, N=165), and that more than double the proportion of subjects

decide to completely exploit the large resource pool in the first period (57.7%; Fisher's Exact test, $P<0.001$, two sided, $N=165$). Appendix Figure A illustrates individual resource exploitation in all periods in treatments S and L.

{INSERT FIGURE 1 ABOUT HERE}

There are also significant treatment differences when resource exploitation can be regulated – but in the opposite direction. The two histograms at the bottom of Figure 1 illustrate the differences between the VS-treatment and the VL-treatment where resource users can vote for limiting access to the resource pool when the resource pool is small or large. The histograms suggest that individual resource exploitation is more constrained when the resource pool is large. In the VS-treatment we observe that individuals exploit on average 18.1% of the resource pool in period 1. In contrast, in the VL-treatment we observe that individuals request on average only 9.2% of the resource pool, which is statistically significantly less (Mann-Whitney *U*-test, $Z = 1.997$, $P=0.0458$, two sided, $N=93$). Appendix Figure B illustrates the frequencies of individual resource exploitation across all five periods in the treatments VS and VL. As compared to Appendix Figure A where individuals could not restrain exploitation, we observe a completely different pattern here: the mode is zero exploitation in all periods for both treatments.

Table 1 provides econometric support for the observable differences in the previous figures and also shows whether the availability of a regulatory institution significantly interacts with the resource pool wealth. Models 1 and 2 regress individual resource exploitation on treatment, and treatment interactions. Model 1 uses only data from the first period in a OLS regression whereas model 2 uses data from all periods and controls for period effects in a random effects GLS regression. The omitted category (i.e., the constant in the regression model) is

individual exploitation in the S-treatment. The coefficients in models 1-4 represent the *absolute* change in *%* of individual resource exploitation; i.e., a coefficient of *x* for variable *y* means that individual exploitation is *x%* of the resource pool +/- the coefficient of the constant.

Models 1 and 2 show that institution × resource wealth interactions are highly significant ($p<0.001$) and have large coefficients. The interaction coefficients of –39.48 and –38.62 represent the additional reduction in resource exploitation when moving from L to VL as compared to when moving from S to VS, highlighting that our voting institution has a much stronger impact on resource exploitation when resources are large. More precisely, when there is a restricting institution available, average individual resource exploitation in period 1 decreases by 19.3% for the small resource pool but by 58.8% for the large resource pool.

{INSERT TABLE 1 ABOUT HERE}

Group Outcomes

Moving from individual to group outcomes, Table 2 shows the likelihood of complete resource depletion over time in our four treatments. We observe that resource depletion clearly differs across treatments. It is quickest in L (no group survives past period 2) and slowest in VL (2/3 of the groups make it to the last period). Model 1 of Table 3 uses an OLS model with the period until which a group lasted as the dependent variable and shows that all treatment differences in survival are significant at $p<0.021$. The coefficients show that the voting institution enabled groups with small resources to stay alive for 1.82 periods longer than those without. The institution × resource wealth interaction shows that groups which have the option to restrict access to the larger resource pool stay alive for a further 1.37 periods.

{INSERT TABLES 2 & 3 ABOUT HERE}

We now turn our attention to growth. The aforementioned differences in individual resource exploitation lead to significantly different growth rates. Groups with access to small resource pools (*N*=29) achieve much higher economic growth (7.2%), calculated as the percentage growth of the initial endowment of wealth, than groups with access to large resource pools (*N*=26, 1.5%; Mann-Whitney *U*-test, *P*=0.038, two sided, *N*=55) and face a lower risk that their resources are depleted in an earlier period (Fisher's Exact test, *P*=0.063, two sided, *N*=55). No group in the L-treatment achieves a growth rate beyond 15% whereas more than 20% in the S-treatment have growth rates of at least 17.5%.

In the voting treatments, we observe that groups with access to small resource pools (*N*=16) achieve a lower asset growth (31.8%) than groups with access to large resource pools (*N*=15, 40.2%; Mann-Whitney *U*-test*, Z=1.146, P*=0.252, two sided, *N*=31) and face a lower risk that their resources are depleted earlier (Fisher's Exact test, *P*=0.344, two sided, *N*=31). Only half of the groups achieve growth rates larger than 30% when the resource pool is small in comparison to 80% of the groups when the resource pool is large. Only 6.7% of the groups in VL deplete the resource in the first period compared to 25% of the groups in VS. Two-thirds of the groups reach the final period in VL but only half in VS. Table 3, model 2 uses an OLS model with growth rates as the dependent variable. We observe that all treatment differences in growth rates are significant at the 5%-level. The institution × resource wealth interaction shows that the growth rate is 14 percentage points larger when moving from L to VL than when moving from S to SL.

Willingness to Restrict Access to Resource Exploitation: Voting Behavior

The previous sections on individual resource exploitation and group outcomes report strong institution × resource wealth interaction effects. In this section, we provide evidence that these interactions are driven by two factors: *(i)* the differential willingness to restrict access to resources and *(ii)* the crowding-in of rent-seeking in VS. We explain each of these factors in turn. Figure 2 illustrates the number of individual votes for resource access restriction in treatments VS and VL in all periods. In the VS-treatment where the resource pool is small, we observe that 23.2% (20.8% in period 1) of the votes are against any resource access restriction while 57.7% (52.1% in period 1) of the votes are in favour of complete resource access restriction. In contrast in the VL-treatment where the resource pool is large, we observe that only 7.3% (8.9% in period 1) of the votes are against any resource access restriction while 82.8% (73.3% in period 1) of the votes are in favour of complete resource access restriction. Thus, individuals use the voting institution to better protect resources when they are large (Mann-Whitney *U*-test, $Z=5.409$, $P<0.0001$, two sided, $N=360$ for all periods; $Z=2.259$, $P=0.0239$, two-sided, $N=93$ for period 1 only). The treatment differences over periods are also statistically significant using a random effects model controlling for period effects and with standard errors clustered at the individual level (p=0.002).

{INSERT FIGURE 2 ABOUT HERE}

The different voting behaviors in VS and VL result in different protection levels and different levels of disagreement over the optimal protection levels. Only 56.25% of the groups in VS resources enjoy complete resource protection in the first period, compared to 80% of the groups in VL. While in VS only 68.45% of the individuals voted for the protection level that was actually implemented, this figure is substantially higher in VL (83.85%). Did being out-voted have an impact on individual exploitation decisions? We find no indication that individuals who

voted for a lower or higher restriction level than implemented exploit more or less in VS and VL than individuals whose vote reflected the voting outcome (four *t*-tests, $P>0.246$, two-sided) suggesting that the median voting outcome has no negative impact on overruled individuals – regardless of the size of the resource pool.

Individual Resource Exploitation Conditional on Access

The second factor explaining the interaction effect is that there are no differences in individual extraction for *unprotected* resources regardless of resource wealth, suggesting that the voting institution crowds-in extraction when resources are small. To start, in the first period in VL subjects extract 68.1% of the unprotected resources, which is very similar to VS where 65.4% is extracted (Mann-Whitney *U*-test, $Z=0.024$, $P=0.981$, two sided, $N=30$). For all periods the percentages are 63.6% in VL and 60.1% in VS (Mann-Whitney *U*-test, $Z=0.380$, $P=0.705$, two sided, $N=69$). Thus, while the possibility to restrict resource access does not change extraction levels for the protected resources when the resource pool is large (Mann-Whitney *U*-test, $Z=0.237$, $P=0.813$, two sided, $N=114$), it crowds in high extraction levels in VS as compared to S (Mann-Whitney *U*-test, $Z=4.066$, $P<0.0001$, two sided, $N=180$).

Models 3 and 4 of Table 1 provide more evidence for the treatment specific extraction of unprotected resources. The models use the percentage of unprotected resources extracted by an individual (conditional on the resource pool being accessible) as the dependent variable. Model 3 regresses individual extraction of unprotected resources in period 1 on treatments and treatment interactions and model 4 uses a random effects GLS model with clustered standard errors to regress individual extraction of unprotected resources in all periods on treatments and treatment

interactions. We observe that resource exploitation is clearly larger in VS as compared to S (by 24.5 to 28 percentage points) suggesting that the regulatory institution crowds out the voluntary willingness to refrain from exploitation if the resource pool is small.[9] In addition, we observe that the institution × resource wealth interaction is significantly negative (by -27.4 to -27.9 percentage points), cancelling out the institutional effect. This provides further evidence that the voting institution did not further crowd-in any additional extraction in VL. This may be because altruistic behaviour is already crowded out in the high stakes settings, as our earlier results suggested.

4. Discussion

By studying the exploitation of non-renewable resource pools in specifically designed behavioural experiments we are able to provide internally valid evidence for the existence of the resource curse. If groups cannot form regulatory institutions, we find that large resource pools are more heavily exploited compared to small resource pools leading to faster resource depletion and less asset growth. However, if groups have the possibility to form strong regulatory institutions, we observe that large resource pools are better protected than small resource pools, resulting in less extraction and longer lasting resources. The sharp interaction effect between institution and resource wealth is driven by a more pronounced willingness to protect resources if they are large and the crowding-in of resource exploitation if there are regulating institutions when resources are small. As for the ultimate mechanisms underlying these data patterns, it seems plausible that many resource users are willing to cooperate to reduce resource exploitation

[9] A plausible mechanism for this crowding-out of altruistic behavior is the erosion of a social norm by the introduction of a regulatory institution (Gneezy and Rustichini, 2000), akin to the responsibility alleviation effect (Charness, 2000). However, our simple experimental design does not allow us to rule out other explanations, such as the voting served as a signal to individuals with restraint that they have been randomly assigned partners who wish to exploit the resource, although the data does not support this hypothesis.

but are susceptible to self-control problems pushing them towards selfish behavior when stakes increase – a problem that can be resolved if they have access to a regulation mechanism (Rachlin, 2004).

The findings in our behavioural experiment are partly consistent with the existing cross-country evidence on the relevance of institutions (Mehlum et al., 2006; Robinson et al., 2006; Boschini et al., 2007). We corroborate their findings that there is a resource curse in the absence of good institutions and show in addition that good institutions are the cause for abundant resources to be a blessing. Thus, this study fills a gap that exists between contradicting, inconclusive empirical evidence on the one side and conclusive theoretical explanations on the other. Due to obvious issues of external validity, our results do not provide striking policy recommendations for nation states. But they do represent a significant contribution to our understanding of the internal validity of the resource curse hypothesis. Groups in weak institutional environments are cursed by large resources but this curse can be lifted by the introduction of well-enforced, democratically chosen rules.

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Figures 1-2

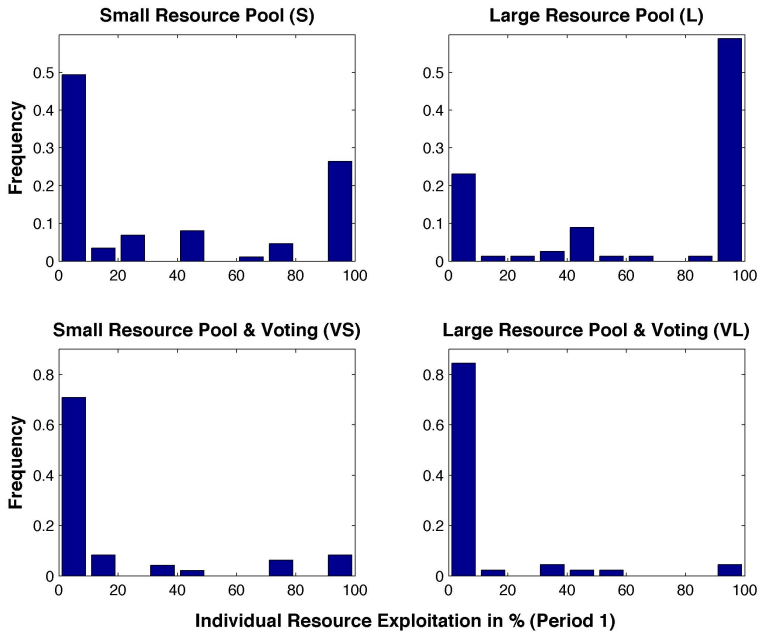


Figure 1: Individual resource exploitation depending on the size of resource pool and on whether individuals could restrict resource pool exploitation. Top left (right) shows exploitation for the small (large) resource pool. Bottom left (right) shows exploitation after voting for regulatory institutions of the small (large) resource pool.

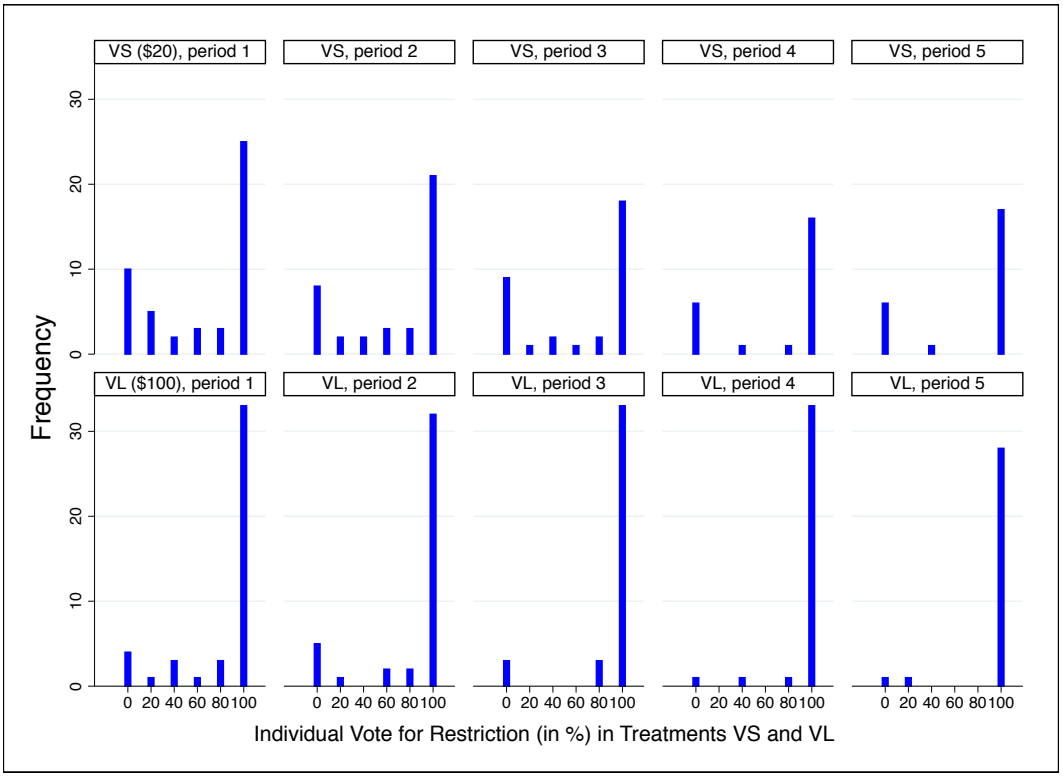


Figure 2. This figure shows the number of individual votes for restriction of access to the resource pool in each of the five periods. The top panel shows the patterns for the VS-treatment where the resource pool was $20 and the bottom panel shows the patterns for the VL-treatment where the resource pool was $100.

Tables 1-3

	(1)	(2)	(3)	(4)
models	exploitation in period 1	exploitation in all periods	conditional exploitation in period 1	conditional exploitation in all periods
Institution	-19.289***	-20.036***	28.003***	24.499***
(VS-treatment)	(6.594)	(6.098)	(10.137)	(8.961)
Resource Wealth	30.573***	30.131***	30.573***	28.972***
(L-treatment)	(6.623)	(6.579)	(6.640)	(6.472)
Institution × Resource Wealth	-39.476***	-38.625***	-27.934*	-27.426**
(VL-treatment)	(8.898)	(8.348)	(16.774)	(13.983)
Period		-0.569**		0.702
		(0.221)		(2.264)
Constant	37.414***	37.427***	37.414***	37.748***
(S-treatment)	(4.600)	(4.482)	(4.611)	(5.398)
Random effects?	no	yes	no	yes
R-sqr	0.255	0.339	0.114	0.133
N	258	588	195	294

Notes: *p<0.1, **p<0.05, ***p<0.001. Robust standard errors in parentheses. Standard errors are clustered on individual level in models 2 and 4.

Table 1. Explaining Individual Exploitation.

Treatment	Period 1	Period 2	Period 3	Period 4
S	65.5%	86.2%	93.1%	93.1%
L	92.3%	100%	-	-
VS	25%	31.3%	50%	50%
VL	6.7%	13.3%	20%	33.3%

Table 2. This table shows the likelihood in % that the resource pool is completely exhausted in a given period depending on the treatment. Treatment S = $20 resource pool, no voting; L = $100, no voting; VS = $20, voting; VL = $100, voting.

models	(1) survival	(2) growth
Institution	1.817***	0.246***
(VS-treatment)	(0.481)	(0.055)
Resource Wealth	-0.544**	-0.057**
(L-treatment)	(0.215)	(0.025)
Institution × Resource Wealth	1.373**	0.140**
(VL-treatment)	(0.584)	(0.070)
Constant	0.621***	0.072***
(S-treatment)	(0.208)	(0.024)
R-sqr	0.553	0.578
N	86	86

Notes: *p<0.1, **p<0.05, ***p<0.001. Robust standard errors in parentheses. Observations in group level. Survival defines in which period group resources are exhausted.

Table 3. Survival and Growth in Groups (*OLS*)

Appendix

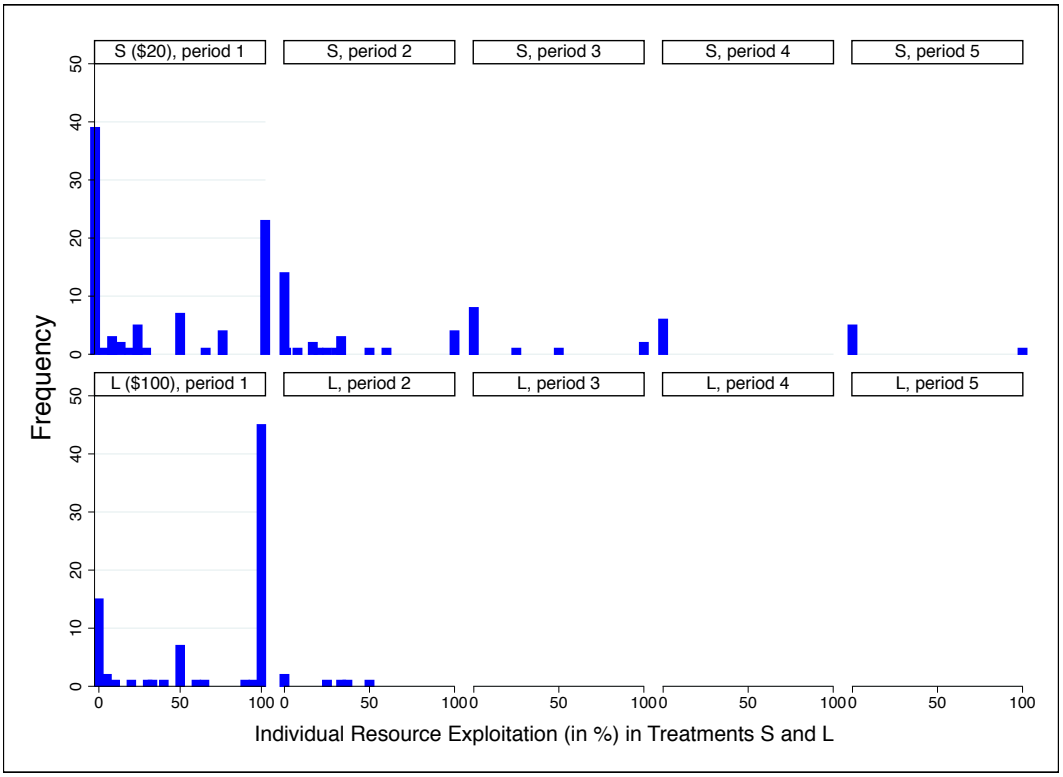


Figure A. This figure shows the level of individual exploitation of the available resource pool in each of the five periods in %. The top panel shows the patterns for the S-treatment where the resource pool was $20 and the bottom panel shows the patterns for the L-treatment where the resource pool was $100.

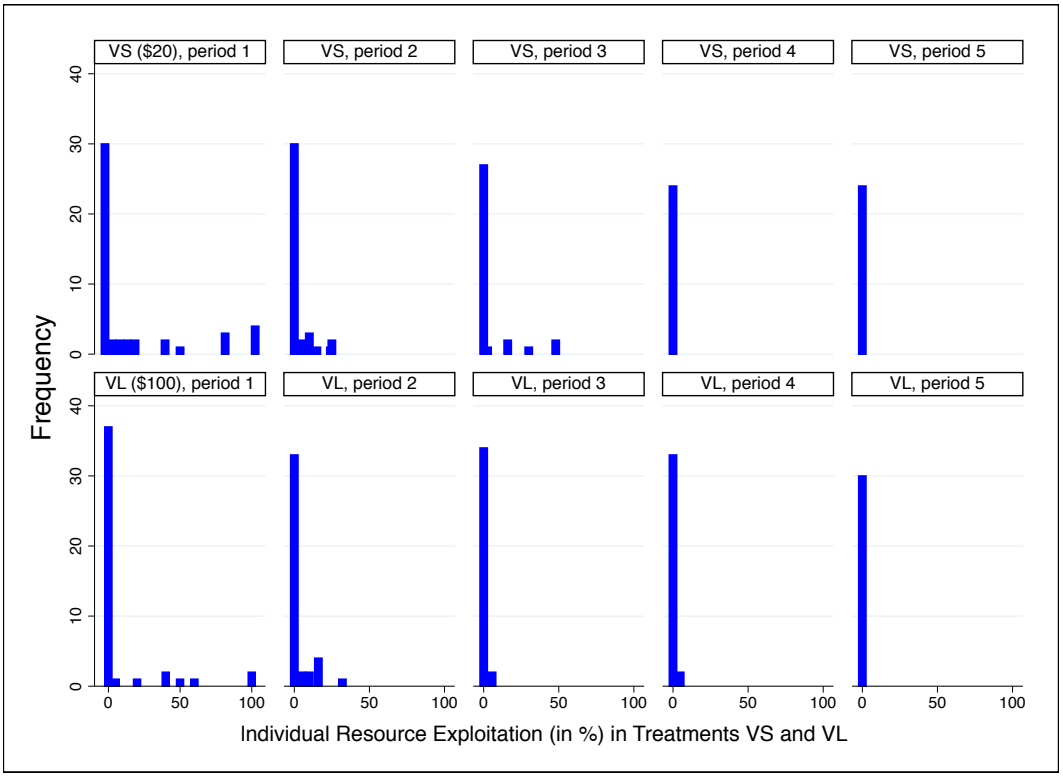


Figure B. This figure shows the level of individual exploitation of the resource pool in each of the five periods in % in the treatments where subjects voted for restriction of access to the resource pool. The top panel shows the patterns for the VS-treatment where the resource pool was $20 and the bottom panel shows the patterns for the VL-treatment where the resource pool was $100.

Experiment Instructions

1.1. Instructions for the S-treatment

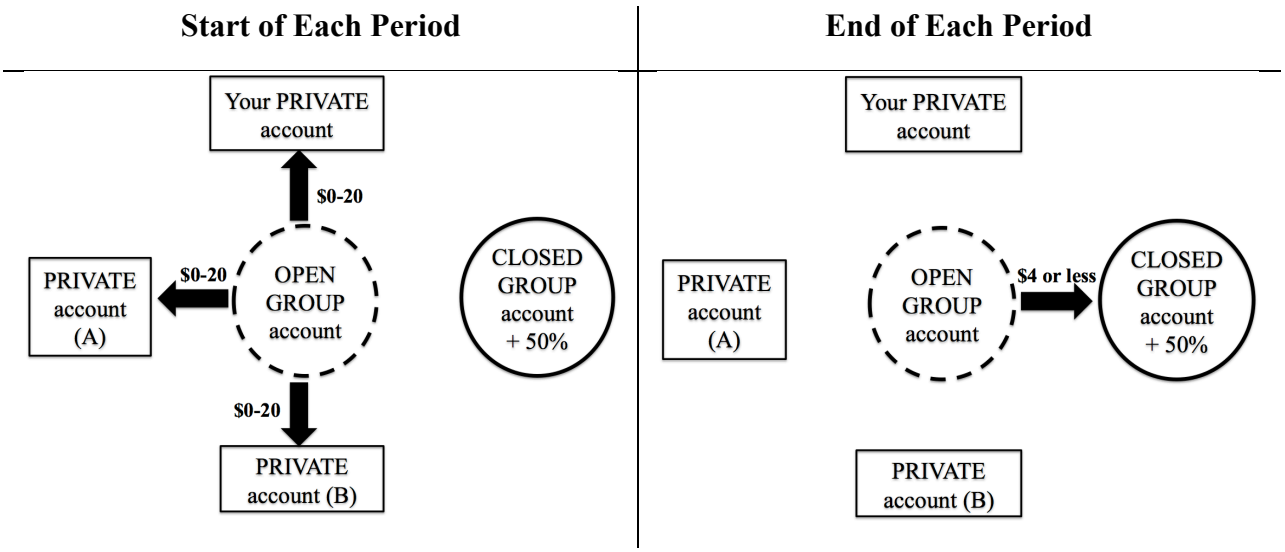
Introduction

Welcome! You are about to take part in an experiment in the economics of decision making. You are guaranteed at least $5 for participating in today's experiment. In addition, you may receive additional earnings as the result of the outcomes in this session. The additional earnings you earn will depend on your and other participants' decisions, so please follow the instructions carefully. Today's session will take about thirty minutes. At the end of the session you will be paid in private and in cash. Please do not communicate with other participants during this session. If you have a question, feel free to raise your hand.

Details

At the beginning of this decision making experiment you will be matched with two other people, randomly selected from the people in this room, to form a group of three. You will remain in this group of three people for the duration of the whole decision making experiment. Note that you will not learn who the people are in your group, neither during nor after today's session. Likewise, the people in your group will not learn about your identity.

There are 3 different accounts in this decision making experiment: an OPEN GROUP account, your PRIVATE account, and a CLOSED GROUP account. The figure below illustrates these three accounts and the decision making experiment. At the beginning there is $20 in the OPEN GROUP account, $5 in your PRIVATE account, and $0 in the CLOSED GROUP account.



In each period of this decision making experiment you and the two people in your group will decide how many dollars to move from the OPEN GROUP account to your PRIVATE account. Once dollars are removed from the OPEN GROUP account they cannot be returned. Any dollars that you move from the OPEN GROUP account to your PRIVATE account are yours to keep. At the end of each period, $4 is transferred from the OPEN GROUP account to the CLOSED GROUP account. Note that you and the group members cannot access the dollars in the CLOSED GROUP account during the experiment. However, at the end of the experiment, all the money in the CLOSED GROUP account will increase by 50 percent and will then be equally distributed between the members of your group. Depending on how many dollars are removed from the OPEN GROUP account, three things can happen:

(1) If at the end of a period there is more than $4 left in the OPEN GROUP account, $4 will be transferred automatically to the CLOSED GROUP account *and* the experiment will continue for an additional period in which you and the members in your group will again decide how many dollars to remove from the remaining dollars in the OPEN GROUP account.

(2) If at the end of a period there is $4 **or less** left in the OPEN GROUP account, all remaining dollars will be transferred automatically to the CLOSED GROUP account *and* the experiment ends.

(3) If at the end of the period the total claims on the OPEN GROUP account exceed the number of dollars remaining then the experiment ends and a simple tie-breaking rule will be used: the money in the OPEN GROUP account will be divided in proportion to the individual requests.

Thus, the decision making experiment will last until the OPEN GROUP account is empty at the end of a period. As soon as the OPEN group account is empty the experiment is over. You will then have to wait for all of the other groups to finish the experiment. Once every group has finished, you will be paid in private. Because an amount of up to $4 is automatically transferred from the OPEN GROUP account to the CLOSED GROUP account at the end of a period, the decision making experiment can last for 5 periods.

Your Payment

At the end of this decision making experiment you will receive the $5 already in your PRIVATE account, any money you added to your PRIVATE account during the experiment plus 1/3 of the dollars in the CLOSED GROUP account **including** the accrued 50 percent interest in this account.

Examples

Below are some examples to help you understand how the experiment will work.

a. Suppose that in *every* period, nobody in your group decides to remove any money from the OPEN GROUP account. Then, at the end of every period, $4 will be transferred from the OPEN GROUP account to the CLOSED GROUP account. The experiment will last for 5 periods and there will be $20 in the CLOSED GROUP account. The money in the CLOSED GROUP account will be increased by 50% to make $30. This $30 will then be divided equally among the members of your group so each group member will receive $10 from the CLOSED GROUP account. Therefore, you and your two group members will each receive the $5 that was already in your PRIVATE account plus the $10 from your share in the CLOSED GROUP account = $15. Altogether, your group earns a total of $45.

b. In the 1st period, you decide to remove $20 from the OPEN GROUP account, your group member (A) $20 and (B) $20. That is, your group decided to remove $20 + $20 + $20 = $60 from the OPEN GROUP account, which exceeds the money remaining in this account. Therefore, no dollars can be transferred to the CLOSED GROUP account and the experiment ends after the 1st period. The simple tie-breaking rule is applied: each group member will receive ($20/$60) × $20 = $6.67 plus the $5 already in their PRIVATE accounts = $11.67. Altogether, your group earns a total of $35.

c. In the 1st period, you and your two group members each decide to remove $1 from the OPEN GROUP account. Thus, after the 1st period there is $20 – (3 × $1) = $17 left in the OPEN GROUP account and $4 is transferred to the CLOSED GROUP account leaving $13 for the 2nd period in the OPEN GROUP account. In the 2nd period, each group member again decides to remove $1. After the 2nd period there is $13 – (3 × $1) = $10 left in the OPEN GROUP account. Thus, $4 is transferred to the CLOSED GROUP account leaving $10 - $4 = $6 for the 3rd period in the OPEN GROUP account. In the 3rd period, you and group member (A) each decide to remove $1. Group member (B) decides to remove $0. After the 3rd period there is $6 – (2 × $1) = $4 left in the OPEN GROUP account. Thus, $4 is transferred to the CLOSED GROUP account and since there is no money left in the OPEN GROUP account the experiment ends after the 3rd period. There is now $12 in the CLOSED GROUP account and this will be increased by 50% to make $18. This $18 will then be divided equally among the members of your group so each group member will receive $6 from the CLOSED GROUP account. For payment, you will receive the $5 already in your private account plus $1 + $1 + $1 = $3 from the additions to your PRIVATE account plus the $6 from your share in the CLOSED GROUP account = $5 + $3 + $6 = $14. Since group member (A) made the same decisions as you, they will also earn $14. You will later be asked to calculate the earnings for group member (B). Altogether, your group earns a total of $41.

Please raise your hand if you have any questions and I will come to your place and answer them in private. Please also fill out the following control questions. The decision making experiment will start as soon as all participants have answered the control questions correctly.

Control Questions

1. The experiment *always* lasts for 5 periods. ___no ___yes

2. I can remove dollars from the CLOSED GROUP account. ____no ____yes

3. At the end of a period, how many dollars are transferred from the OPEN GROUP account to the CLOSED GROUP account if there is (i) $10, (ii) $2 left in the OPEN GROUP account?
(i)____ (ii)_____

4. How many dollars does group member (B) earn in example (c.) above? _____

5. If anyone in your group removes $20 in the 1st period, what is the TOTAL amount of money that your group will earn? _____

6. What is the maximum TOTAL amount of money that your group can earn? _____

7. How much should your group members remove from the OPEN GROUP account every period to achieve this maximum? _____

8. For you to make the most money individually, how much should (i) you extract in the first period? (ii) (A) and (B) extract in the first period? (i)____ (ii)_____

1.2. Instruction for the L-treatment

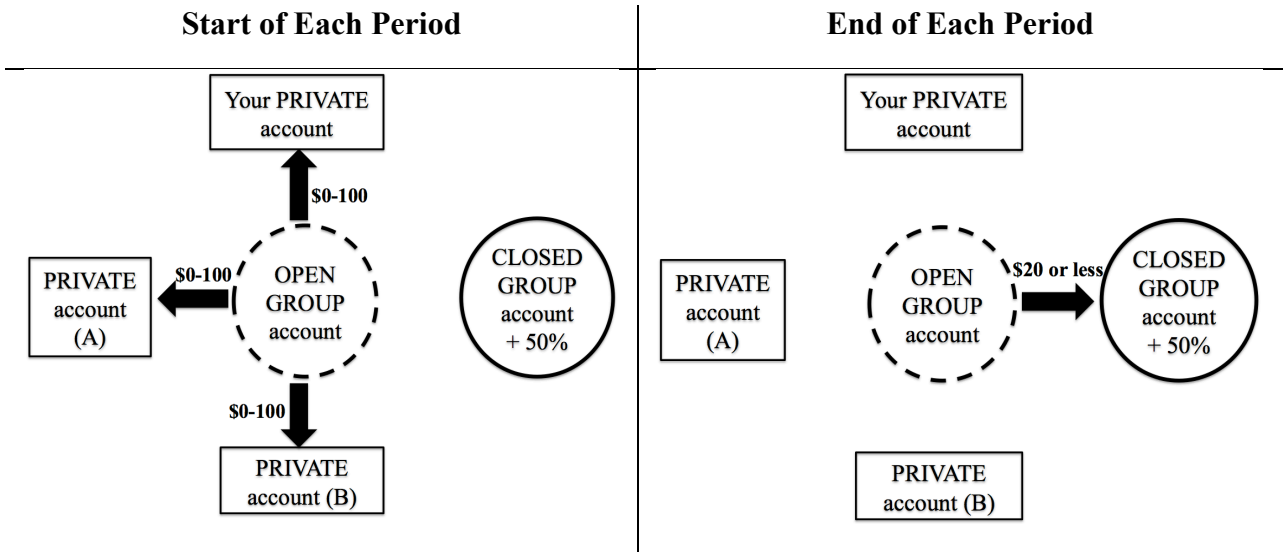
Introduction

Welcome! You are about to take part in an experiment in the economics of decision making. You are guaranteed at least $5 for participating in today's experiment. In addition, you may receive additional earnings as the result of the outcomes in this session. The additional earnings you earn will depend on your and other participants' decisions, so please follow the instructions carefully. Today's session will take about thirty minutes. At the end of the session you will be paid in private and in cash. Please do not communicate with other participants during this session. If you have a question, feel free to raise your hand.

Details

At the beginning of this decision making experiment you will be matched with two other people, randomly selected from the people in this room, to form a group of three. You will remain in this group of three people for the duration of the whole decision making experiment. Note that you will not learn who the people are in your group, neither during nor after today's session. Likewise, the people in your group will not learn about your identity.

There are 3 different accounts in this decision making experiment: an OPEN GROUP account, your PRIVATE account, and a CLOSED GROUP account. The figure below illustrates these three accounts and the decision making experiment. At the beginning there is $100 in the OPEN GROUP account, $5 in your PRIVATE account, and $0 in the CLOSED GROUP account.



In each period of this decision making experiment you and the two people in your group will decide how many dollars to move from the OPEN GROUP account to your PRIVATE account. Once dollars are removed from the OPEN GROUP account they cannot be returned. Any dollars that you move from the OPEN GROUP account to your PRIVATE account are yours to keep. At the end of each period, $20 is transferred from the OPEN GROUP account to the CLOSED GROUP account. Note that you and the group members cannot access the dollars in the CLOSED GROUP account during the experiment. However, at the end of the experiment, all the money in the CLOSED GROUP account will increase by 50 percent and will then be equally distributed between the members of your group. Depending on how many dollars are removed from the OPEN GROUP account, three things can happen:

(1) If at the end of a period there is more than $20 left in the OPEN GROUP account, $20 will be transferred automatically to the CLOSED GROUP account *and* the experiment will continue for an additional period in which you and the members in your group will again decide how many dollars to remove from the remaining dollars in the OPEN GROUP account.

(2) If at the end of a period there is $20 **or less** left in the OPEN GROUP account, all remaining dollars will be transferred automatically to the CLOSED GROUP account *and* the experiment ends.

(3) If at the end of the period the total claims on the OPEN GROUP account exceed the number of dollars remaining then the experiment ends and a simple tie-breaking rule will be used: the money in the OPEN GROUP account will be divided in proportion to the individual requests.

Thus, the decision making experiment will last until the OPEN GROUP account is empty at the end of a period. As soon as the OPEN group account is empty the experiment is over. You will then have to wait for all of the other groups to finish the experiment. Once every group has finished, you will be paid in private. Because an amount of up to $20 is automatically transferred from the OPEN GROUP account to the CLOSED GROUP account at the end of a period, the decision making experiment can last for 5 periods.

Your Payment

At the end of this decision making experiment you will receive the $5 already in your PRIVATE account, any money you added to your PRIVATE account during the experiment plus 1/3 of the dollars in the CLOSED GROUP account **including** the accrued 50 percent interest in this account.

Examples

Below are some examples to help you understand how the experiment will work.

d. Suppose that in *every* period, nobody in your group decides to remove any money from the OPEN GROUP account. Then, at the end of every period, $20 will be transferred from the OPEN GROUP account to the CLOSED GROUP account. The experiment will last for 5 periods and there will be $100 in the CLOSED GROUP account. The money in the CLOSED GROUP account will be increased by 50% to make $150. This $150 will then be divided equally among the members of your group so each group member will receive $50 from the CLOSED GROUP account. Therefore, you and your two group members will each receive the $5 that was already in your PRIVATE account plus the $50 from your share in the CLOSED GROUP account = $55. Altogether, your group earns a total of $165.

e. In the 1st period, you decide to remove $100 from the OPEN GROUP account, your group member (A) $100 and (B) $100. That is, your group decided to remove $100 + $100 + $100 = $300 from the OPEN GROUP account, which exceeds the money remaining in this account. Therefore, no dollars can be transferred to the CLOSED GROUP account and the experiment ends after the 1st period. The simple tie-breaking rule is applied: each group member will receive ($100/$300) × $100 = $33.33 plus the $5 already in their PRIVATE accounts = $38.33. Altogether, your group earns a total of $115.

f. In the 1st period, you and your two group members each decide to remove $5 from the OPEN GROUP account. Thus, after the 1st period there is $100 – (3 × $5) = $85 left in the OPEN GROUP account and $20 is transferred to the CLOSED GROUP account leaving $65 for the 2nd period in the OPEN GROUP account. In the 2nd period, each group member again decides to remove $5. After the 2nd period there is $65 – (3 × $5) = $50 left in the OPEN GROUP account. Thus, $20 is transferred to the CLOSED GROUP account leaving $50 - $20 = $30 for the 3rd period in the OPEN GROUP account. In the 3rd period, you and group member (A) each decide to remove $5. Group member (B) decides to remove $0. After the 3rd period there is $30 – (2 × $5) = $20 left in the OPEN GROUP account. Thus, $20 is transferred to the CLOSED GROUP account and since there is no money left in the OPEN GROUP account the experiment ends after the 3rd period. There is now $60 in the CLOSED GROUP account and this will be increased by 50% to make $90. This $90 will then be divided equally among the members of your group so each group member will receive $30 from the CLOSED GROUP account. For payment, you will receive the $5 already in your PRIVATE account plus $5 + $5 + $5 = $15 from the additions to your PRIVATE account plus the $30 from your share in the CLOSED GROUP account = $5 + $15 + $30 = $50. Since group member (A) made the same decisions as you, they will also earn $50. You will later be asked to calculate the earnings for group member (B). Altogether, your group earns a total of $145.

Please raise your hand if you have any questions. Please also fill out the following control questions. The decision making experiment will start as soon as all participants have answered the control questions correctly.

Control Questions

1. The experiment *always* lasts for 5 periods. ___no ___yes

2. I can remove dollars from the CLOSED GROUP account. ____no ____yes

3. At the end of a period, how many dollars are transferred from the OPEN GROUP account to the CLOSED GROUP account if there is (i) $50, (ii) $10 left in the OPEN GROUP account?
(i)____ (ii)_____

4. How many dollars does group member (B) earn in example (c.) above? _____

5. If anyone in your group removes $100 in the 1st period, what is the TOTAL amount of money that your group will earn?_____

6. What is the maximum TOTAL amount of money that your group can earn? _____

7. How much should your group members remove from the OPEN GROUP account every period to achieve this maximum? _____

8. For you to make the most money individually, how much should (i) you extract in the first period? (ii) (A) and (B) extract in the first period? (i)____ (ii)_____

1.3. Instructions for the VS-treatment

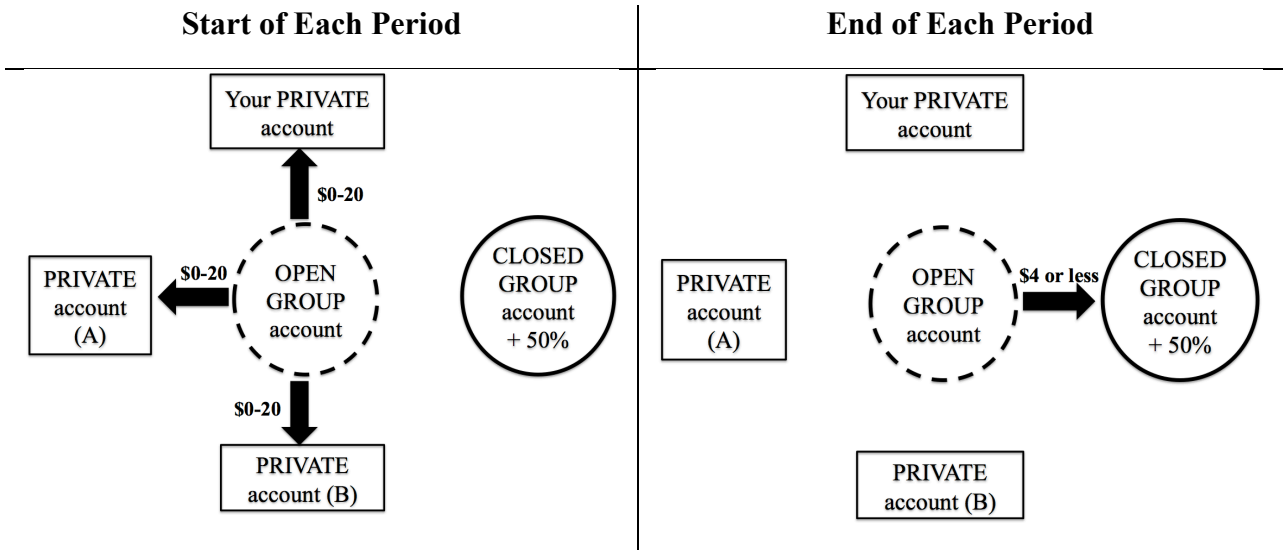
Introduction

Welcome! You are about to take part in an experiment in the economics of decision making. You are guaranteed at least $5 for participating in today's experiment. In addition, you may receive additional earnings as the result of the outcomes in this session. The additional earnings you earn will depend on your and other participants' decisions, so please follow the instructions carefully. Today's session will take about 30 minutes. At the end of the session you will be paid in private and in cash. Please do not communicate with other participants during this session. If you have a question, feel free to raise your hand.

Details

At the beginning of this decision making experiment you will be matched with two other people, randomly selected from the people in this room, to form a group of three. You will remain in this group of three people for the duration of the whole decision making experiment. Note that you will not learn who the people are in your group, neither during nor after today's session. Likewise, the people in your group will not learn about your identity.

There are 3 different accounts in this decision making experiment: an OPEN GROUP account, your PRIVATE account, and a CLOSED GROUP account. The figure below illustrates these three accounts and the decision making experiment. At the beginning there is $20 in the OPEN GROUP account, $5 in your PRIVATE account, and $0 in the CLOSED GROUP account.



In each period of this decision making experiment you and the two people in your group will decide how many dollars to move from the OPEN GROUP account to your PRIVATE account. Once dollars are removed from the OPEN GROUP account they cannot be returned. Any dollars that you move from the OPEN GROUP account to your PRIVATE account are yours to keep. At the end of each period, $4 is transferred from the OPEN GROUP account to the CLOSED GROUP account. Note that you and the group members cannot access the dollars in the CLOSED GROUP account during the experiment. However, at the end of the experiment, all the money in the CLOSED GROUP account will increase by 50 percent and will then be equally distributed between the members of your group. Depending on how many dollars are removed from the OPEN GROUP account, three things can happen:

(1) If at the end of a period there is more than $4 left in the OPEN GROUP account, $4 will be transferred automatically to the CLOSED GROUP account *and* the experiment will continue for an additional period in which you and the members in your group will again decide how many dollars to remove from the remaining dollars in the OPEN GROUP account.

(2) If at the end of a period there is $4 **or less** left in the OPEN GROUP account, all remaining dollars will be transferred automatically to the CLOSED GROUP account *and* the experiment ends.

(3) If at the end of the period the total claims on the OPEN GROUP account exceed the number of dollars remaining then the experiment ends and a simple tie-breaking rule will be used: the money in the OPEN GROUP account will be divided in proportion to the individual requests.

Thus, the decision making experiment will last until the OPEN GROUP account is empty at the end of a period. As soon as the OPEN group account is empty the experiment is over. You will then have to wait for all of the other groups to finish the experiment. Once every group has finished, you will be paid in private. Because an amount of up to $4 is automatically transferred from the OPEN GROUP account to the CLOSED GROUP account at the end of a period, the decision making experiment can last for 5 periods.

Examples

Below are some examples to help you understand how the experiment will work.

g. Suppose that in *every* period, nobody in your group decides to remove any money from the OPEN GROUP account. Then, at the end of every period, $4 will be transferred from the OPEN GROUP account to the CLOSED GROUP account. The experiment will last for 5 periods and

there will be $20 in the CLOSED GROUP account. The money in the CLOSED GROUP account will be increased by 50% to make $30. This $30 will then be divided equally among the members of your group so each group member will receive $10 from the CLOSED GROUP account. Therefore, you and your two group members will each receive the $5 that was already in your PRIVATE account plus the $10 from your share in the CLOSED GROUP account = $15. Altogether, your group earns a total of $45.

h. In the 1st period, you decide to remove $20 from the OPEN GROUP account, your group member (A) $20 and (B) $20. That is, your group decided to remove $20 + $20 + $20 = $60 from the OPEN GROUP account, which exceeds the money remaining in this account. Therefore, no dollars can be transferred to the CLOSED GROUP account and the experiment ends after the 1st period. The simple tie-breaking rule is applied: each group member will receive ($20/$60) × $20 = $6.67 plus the $5 already in their PRIVATE accounts = $11.67. Altogether, your group earns a total of $35.

i. In the 1st period, you and your two group members each decide to remove $1 from the OPEN GROUP account. Thus, after the 1st period there is $20 – (3 × $1) = $17 left in the OPEN GROUP account and $4 is transferred to the CLOSED GROUP account leaving $13 for the 2nd period in the OPEN GROUP account. In the 2nd period, each group member again decides to remove $1. After the 2nd period there is $13 – (3 × $1) = $10 left in the OPEN GROUP account. Thus, $4 is transferred to the CLOSED GROUP account leaving $10 - $4 = $6 for the 3rd period in the OPEN GROUP account. In the 3rd period, you and group member (A) each decide to remove $1. Group member (B) decides to remove $0. After the 3rd period there is $6 – (2 × $1) = $4 left in the OPEN GROUP account. Thus, $4 is transferred to the CLOSED GROUP account and since there is no money left in the OPEN GROUP account the experiment ends after the 3rd period. There is now $12 in the CLOSED GROUP account and this will be increased by 50% to make $18. This $18 will then be divided equally among the members of your group so each group member will receive $6 from the CLOSED GROUP account. For payment, you will receive the $5 already in your PRIVATE account plus $1 + $1 + $1 = $3 from the additions to your PRIVATE account plus the $6 from your share in the CLOSED GROUP account = $5 + $3 + $6 = $14. Since group member (A) made the same decisions as you, they will also earn $14. You will later be asked to calculate the earnings for group member (B). Altogether, your group earns a total of $41.

Voting

At the start of each period before you decide how many dollars you will remove, you and your two group members will vote over the extent to which you want to limit possible removals from the OPEN GROUP account. Removals from the OPEN GROUP account will be limited to the extent to which at least two people in your group agree. There are different levels of limitations possible: 100%, 80%, 60%, 40%, 20%, and 0%. For example, a limitation level of 40% in the 1st period in which there is $20 in the OPEN GROUP account means that $8 *cannot* be removed from the OPEN GROUP account in this period.

If you vote to limit possible removals, it is assumed that you would agree to a limitation that is below the limitation you voted for. For example, if you vote for 60%, one of your group members for 40%, and the other for 0%, then the mechanism will implement for this period a limitation of 40%, meaning that 40% of the dollars in the OPEN GROUP account cannot be removed. If you vote for 60%, and the other two group members for 20%, then the mechanism will implement for this period a limitation of 20%, meaning that 20% of the dollars in the OPEN GROUP account cannot be removed. If you vote for 60%, and the other two group members for 0%, then the mechanism will implement for this period a limitation of 0%, meaning that 0% of the dollars in the OPEN GROUP account cannot be removed, i.e. all of it can be removed.

Your Payment

At the end of this decision making experiment you will receive the $5 already in your PRIVATE account, any money you added to your PRIVATE account during the experiment plus 1/3 of the dollars in the CLOSED GROUP account **including** the accrued 50 percent interest in this account.

Please raise your hand if you have any questions. Please also fill out the following control questions. The decision making experiment will start as soon as all participants have answered the control questions correctly.

Control Questions

1. The experiment *always* lasts for 5 periods. ___no ___yes

2. I can remove dollars from the CLOSED GROUP account. ____no ____yes

3. At the end of a period, how many dollars are transferred from the OPEN GROUP account to the CLOSED GROUP account if there is (i) $10, (ii) $2 left in the OPEN GROUP account?
(i)____ (ii)_____

4. How many dollars does group member (B) earn in example (c.) above? _____

5. You vote for a limitation of 0%, (A) for 60%, and (B) for 40%, which limitation will be implemented? ____%

6. If there is $20 in the OPEN GROUP account and a limitation of 20% is implemented, how many dollars can the group members maximally transfer to their PRIVATE accounts? _____

7. If anyone in your group removes $20 in the 1st period, what is the TOTAL amount of money that your group will earn?_____

8. What is the maximum TOTAL amount of money that your group can earn? _____

9. How much should your group members remove from the OPEN GROUP account every period to achieve this maximum? _____

10. For you to make the most money individually, how much should (i) you extract in the first period? (ii) (A) and (B) extract in the first period? (i)____ (ii)_____

1.4. Instructions for the VL-treatment

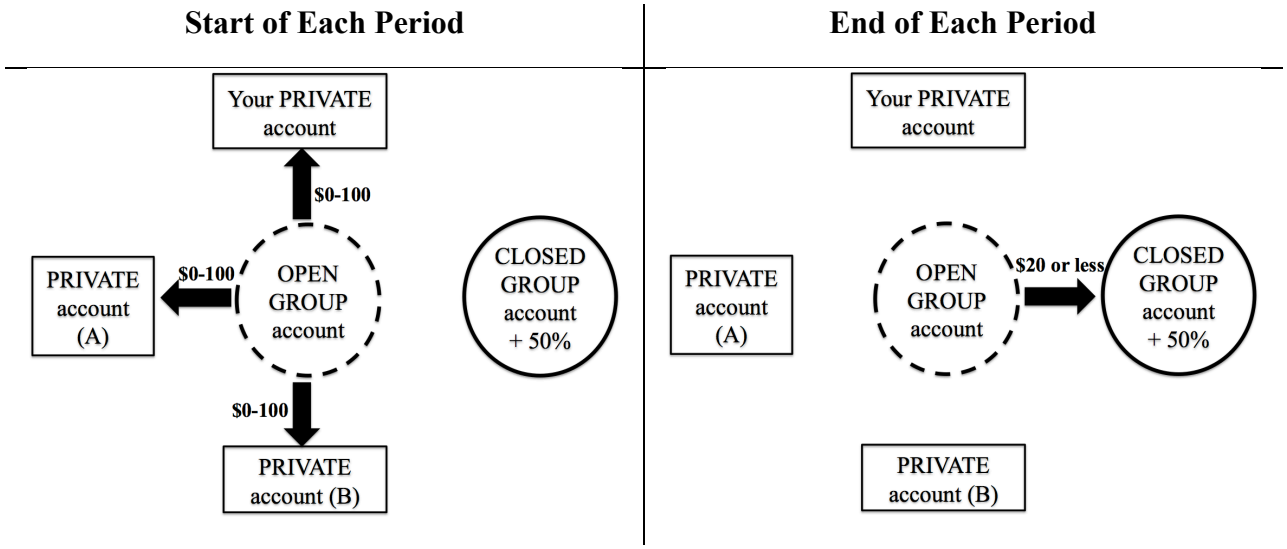
Introduction

Welcome! You are about to take part in an experiment in the economics of decision making. You are guaranteed at least $5 for participating in today's experiment. In addition, you may receive additional earnings as the result of the outcomes in this session. The additional earnings you earn will depend on your and other participants' decisions, so please follow the instructions carefully. Today's session will take about 30 minutes. At the end of the session you will be paid in private and in cash. Please do not communicate with other participants during this session. If you have a question, feel free to raise your hand.

Details

At the beginning of this decision making experiment you will be matched with two other people, randomly selected from the people in this room, to form a group of three. You will remain in this group of three people for the duration of the whole decision making experiment. Note that you will not learn who the people are in your group, neither during nor after today's session. Likewise, the people in your group will not learn about your identity.

There are 3 different accounts in this decision making experiment: an OPEN GROUP account, your PRIVATE account, and a CLOSED GROUP account. The figure below illustrates these three accounts and the decision making experiment. At the beginning there is $100 in the OPEN GROUP account, $5 in your PRIVATE account, and $0 in the CLOSED GROUP account.



In each period of this decision making experiment you and the two people in your group will decide how many dollars to move from the OPEN GROUP account to your PRIVATE account. Once dollars are removed from the OPEN GROUP account they cannot be returned. Any dollars that you move from the OPEN GROUP account to your PRIVATE account are yours to keep. At the end of each period, $20 is transferred from the OPEN GROUP account to the CLOSED GROUP account. Note that you and the group members cannot access the dollars in the CLOSED GROUP account during the experiment. However, at the end of the experiment, all the money in the CLOSED GROUP account will increase by 50 percent and will then be equally distributed between the members of your group. Depending on how many dollars are removed from the OPEN GROUP account, three things can happen:

(1) If at the end of a period there is more than $20 left in the OPEN GROUP account, $20 will be transferred automatically to the CLOSED GROUP account *and* the experiment will continue for an additional period in which you and the members in your group will again decide how many dollars to remove from the remaining dollars in the OPEN GROUP account.

(2) If at the end of a period there is $20 **or less** left in the OPEN GROUP account, all remaining dollars will be transferred automatically to the CLOSED GROUP account *and* the experiment ends.

(3) If at the end of the period the total claims on the OPEN GROUP account exceed the number of dollars remaining then the experiment ends and a simple tie-breaking rule will be used: the money in the OPEN GROUP account will be divided in proportion to the individual requests.

Thus, the decision making experiment will last until the OPEN GROUP account is empty at the end of a period. As soon as the OPEN group account is empty the experiment is over. You will then have to wait for all of the other groups to finish the experiment. Once every group has finished, you will be paid in private. Because an amount of up to $20 is automatically transferred from the OPEN GROUP account to the CLOSED GROUP account at the end of a period, the decision making experiment can last for 5 periods.

Examples

Below are some examples to help you understand how the experiment will work.

j. Suppose that in *every* period, nobody in your group decides to remove any money from the OPEN GROUP account. Then, at the end of every period, $20 will be transferred from the OPEN GROUP account to the CLOSED GROUP account. The experiment will last for 5 periods and there will be $100 in the CLOSED GROUP account. The money in the CLOSED GROUP account will be increased by 50% to make $150. This $150 will then be divided equally among the members of your group so each group member will receive $50 from the CLOSED GROUP account. Therefore, you and your two group members will each receive the $5 that was already in your PRIVATE account plus the $50 from your share in the CLOSED GROUP account = $55. Altogether, your group earns a total of $165.

k. In the 1st period, you decide to remove $100 from the OPEN GROUP account, your group member (A) $100 and (B) $100. That is, your group decided to remove $100 + $100 + $100 = $300 from the OPEN GROUP account, which exceeds the money remaining in this account. Therefore, no dollars can be transferred to the CLOSED GROUP account and the experiment ends after the 1st period. The simple tie-breaking rule is applied: each group member will receive ($100/$300) × $100 = $33.33 plus the $5 already in their PRIVATE accounts = $38.33. Altogether, your group earns a total of $115.

l. In the 1st period, you and your two group members each decide to remove $5 from the OPEN GROUP account. Thus, after the 1st period there is $100 – (3 × $5) = $85 left in the OPEN GROUP account and $20 is transferred to the CLOSED GROUP account leaving $65 for the 2nd period in the OPEN GROUP account. In the 2nd period, each group member again decides to remove $5. After the 2nd period there is $65 – (3 × $5) = $50 left in the OPEN GROUP account. Thus, $20 is transferred to the CLOSED GROUP account leaving $50 - $20 = $30 for the 3rd period in the OPEN GROUP account. In the 3rd period, you and group member (A) each decide to remove $5. Group member (B) decides to remove $0. After the 3rd period there is $30 – (2 × $5) = $20 left in the OPEN GROUP account. Thus, $20 is transferred to the CLOSED GROUP account and since there is no money left in the OPEN GROUP account the experiment ends after the 3rd period. There is now $60 in the CLOSED GROUP account and this will be increased by 50% to make $90. This $90 will then be divided equally among the members of your group so each group member will receive $30 from the CLOSED GROUP account. For payment, you will receive the $5 already in your PRIVATE account plus $5 + $5 + $5 = $15 from the additions to your PRIVATE account plus the $30 from your share in the CLOSED GROUP account = $5 + $15 + $30 = $50. Since group member (A) made the same decisions as you, they will also earn $50. You will later be asked to calculate the earnings for group member (B). Altogether, your group earns a total of $145.

Voting

At the start of each period before you decide how many dollars you will remove, you and your two group members will vote over the extent to which you want to limit possible removals from the OPEN GROUP account. Removals from the OPEN GROUP account will be limited to the extent to which at least two people in your group agree. There are different levels of limitations

possible: 100%, 80%, 60%, 40%, 20%, and 0%. For example, a limitation level of 40% in the 1st period in which there is $100 in the OPEN GROUP account means that $40 *cannot* be removed from the OPEN GROUP account in this period.

If you vote to limit possible removals, it is assumed that you would agree to a limitation that is below the limitation you voted for. For example, if you vote for 60%, one of your group members for 40%, and the other for 0%, then the mechanism will implement for this period a limitation of 40%, meaning that 40% of the dollars in the OPEN GROUP account cannot be removed. If you vote for 60%, and the other two group members for 20%, then the mechanism will implement for this period a limitation of 20%, meaning that 20% of the dollars in the OPEN GROUP account cannot be removed. If you vote for 60%, and the other two group members for 0%, then the mechanism will implement for this period a limitation of 0%, meaning that 0% of the dollars in the OPEN GROUP account cannot be removed, i.e. all of it can be removed.

Your Payment

At the end of this decision making experiment you will receive the $5 already in your PRIVATE account, any money you added to your PRIVATE account during the experiment plus 1/3 of the dollars in the CLOSED GROUP account **including** the accrued 50 percent interest in this account.

Please raise your hand if you have any questions. Please also fill out the following control questions. The decision making experiment will start as soon as all participants have answered the control questions correctly.

Control Questions

1. The experiment *always* lasts for 5 periods. ___no ___yes

2. I can remove dollars from the CLOSED GROUP account. ____no ____yes

3. At the end of a period, how many dollars are transferred from the OPEN GROUP account to the CLOSED GROUP account if there is (i) $50, (ii) $10 left in the OPEN GROUP account?
(i)____ (ii)_____

4. How many dollars does group member (B) earn in example (c.) above? _____

5. You vote for a limitation of 0%, (A) for 60%, and (B) for 40%, which limitation will be implemented? ____%

6. If there is $100 in the OPEN GROUP account and a limitation of 20% is implemented, how many dollars can the group members maximally transfer to their PRIVATE accounts? _____

7. If anyone in your group removes $100 in the 1st period, what is the TOTAL amount of money that your group will earn?_____

8. What is the maximum TOTAL amount of money that your group can earn? _____

9. How much should your group members remove from the OPEN GROUP account every period to achieve this maximum? _____

10. For you to make the most money individually, how much should (i) you extract in the first period? (ii) (A) and (B) extract in the first period? (i)____ (ii)_____

Conditional Cash Transfers and Civil Conflict: Experimental Evidence from the Philippines*

Benjamin Crost † Joseph H. Felter‡ Patrick B. Johnston§

January 21, 2013

Abstract

Conditional cash transfer (CCT) programs have become a popular tool of poverty reduction that is increasingly used in conflict-affected areas. However, there is limited evidence so far on how CCT programs affect conflict, and theoretical predictions are ambiguous. We exploit an experiment that randomly assigned eligibility for a CCT program at the village level to estimate the effect of conditional cash transfers on the intensity of civil conflict in the Philippines. We find that cash transfers caused a substantial decrease in conflict incidents in treatment villages relative to control villages.

*The authors thank Eli Berman, Christian Deloria, Radha K. Iyengar, Jacob N. Shapiro, and seminar participants at the NBER Economics of National Security meeting for comments on an earlier version. Felter and Johnston acknowledge support from AFOSR Award No. FA9550-09-1-0314. Any opinions, findings, conclusions, and recommendations expressed in this publication are the authors' and do not necessarily reflect AFOSR's views.

†Assistant Professor, Department of Economics, University of Colorado Denver, Campus Box 181, Denver, CO 80217-3364. *Email:* `Benjamin.Crost@ucdenver.edu`.

‡Senior Research Scholar, Center for International Security and Cooperation, Stanford University, 616 Serra St., Stanford, CA 94305-6165. *Email:* `joseph.felter@stanford.edu`.

§Associate Political Scientist, RAND Corporation, 1200 S. Hayes St., Arlington, VA 22202-5050. *Email:* `Patrick_Johnston@RAND.org`.

1 Introduction

A large and growing body of research shows that civil conflict has a wide range of negative effects on the welfare of affected populations. In addition to direct casualties, conflict causes lower economic growth (Abadie and Gardeazabal, 2003; Lopez and Wodon, 2005) and reduced education attainment rates (Leon, forthcoming), as well as adverse health outcomes, like low birth weight (Ghobarah et al., 2003; Camacho, 2005; Mansour and Rees, forthcoming). Conflict-affected countries have substantially lower rates of poverty reduction and make slower progress toward the Millennium Development Goals than peaceful countries (World Bank, 2012). In light of these findings, international donors such as the World Bank are advocating for increases in development aid to conflict-affected countries, partly in the hope that aid will help reduce conflict (World Bank, 2012).

However, evidence on the effect of aid on conflict is mixed. Berman et al. (2011) find that small-scale aid and reconstruction spending disbursed by the US Army in Iraq led to a decrease in violence against US forces and civilians. On the other hand, Crost and Johnston (2010) find that infrastructure spending disbursed in the form of community-driven development (CDD) projects increased conflict in the Philippines. Similarly, Nunn and Qian (2012) find that US food aid increased conflict in recipient countries. This mixed evidence suggests that aid can either increase or decrease conflict, depending on the way in which it is disbursed.

One of the most popular ways of disbursing development aid is in the form of conditional cash-transfer (CCT) programs. These programs distribute cash transfers to poor households that meet a number of conditions, such as child vaccinations and school attendance. Over the past decade CCT programs have become one of the most important tools for delivering development aid and a large literature documents their positive impacts on the well-being

of the poor. However, despite their popularity and the large literature on their impacts, there is so far no empirical evidence on how CCT programs affect civil conflict. The issue is clearly both timely and important. CCT programs are currently operating in numerous conflict-affected countries including Colombia, India, Indonesia and the Philippines. Some commentators have even proposed that a CCT program may help build peace in Afghanistan (Kenny, 2011).

This paper advances the literature by estimating the effect of a large CCT program, the Philippines' *Pantawid Pamilyang Pilipino Program* (4Ps), on the intensity of civil conflict. The 4Ps program distributes cash transfers to approximately one million of the poorest households in the Philippines. To estimate 4Ps' impact on conflict, we exploit a randomized experiment conducted by the World Bank in 2009. In this experiment, 130 villages in 8 municipalities of the Philippines were randomly divided into a treatment group in which the 4Ps program was introduced in 2009, and a control group in which it was delayed until 2010. Using unique village-level conflict data from the Armed Forces of the Philippines, we estimate the causal effect of the program by comparing the intensity of violence in treatment and control villages in 2009. We find that the program causes a significant decrease in conflict-related incidents in treatment villages. To our knowledge, this is the first direct experimental evidence that CCT programs can reduce civil conflict.

The conflict-reducing effect of cash-transfers is consistent with previous findings that positive economic shocks reduce civil conflict (Miguel et al., 2004; Dube and Vargas, 2013). There are two potential mechanisms through which this effect might operate. First, CCT programs may increase popular support for the government by "winning hearts and minds". As a result, the population is more likely to supply intelligence on insurgents to the government, enabling the government to apprehend insurgents and reducing insurgent attack rates Berman et al. (2011). This mechanism is supported by the finding of (Manacorda et al., 2011) that CCT programs can increase popular support for incumbent governments. Second, CCT programs

may increase the opportunity cost of joining an insurgency. This could be either because the transfers boost the local economy and create higher incomes from peaceful activities, or because the conditions imposed on program participants make it difficult to receive transfers while being active in the insurgency. Either way, an increase in the opportunity cost of joining an insurgency would likely reduce conflict by making insurgent recruiting more difficult.

While we cannot say with certainty which mechanism explains our experimental results, they suggest that the effect of CCTs is different from those of other types of aid interventions like community-driven development programs and food aid, which have been found to increase conflict Crost and Johnston (2010); Nunn and Qian (2012). We discuss some possible reasons for this difference in the conclusion to this paper. Going forward, our results suggests a ripe opportunity for future research on how and when various means of targeting and delivering aid can reduce rather than exacerbate the risk violent conflict.

2 Institutional Background

2.1 The 4Ps Program

This paper studies the *Pantawid Pamilyang Pilipino Program* (4Ps), a conditional cash-transfer program implemented by the Philippine government's Department of Social Welfare and Development and partly funded through loans from the World Bank and the Asian Development Bank. Since its inception in 2007, the program has financed transfers to approximately one million households in 782 cities and municipalities in 81 provinces in all 17 regions in the Philippines[1] and is currently the country's flagship antipoverty program.

[1] These statistics were current as of January 2011. See Arulpragasam et al. 2011, p. 1.

4Ps emulates the model of other successful CCT programs, such as Mexico's *Oportunidades* and Brazil's *Bolsa Familia*. Like its predecessors, is intended to reduce poverty and promote human capital investment by providing grants to poor households on the condition that they satisfy basic health and education requirements. In order to receive transfers, recipient households are required to ensure their children attend school and get numerous vaccinations and deworming treatments. Pregnant women are required to get regular pre- and post-natal health check-ups.

Households are eligible for transfers through the program if their per capita income is below the regional poverty line and they have children aged 0-14. Per capita incomes are estimated by a Proxy-Means Test (PMT) based on the following indicators: household consumption; education of household members; occupation; housing conditions; access to basic services; ownership of assets; tenure status of housing; and regional dummy variables. [2] Finally, the lists of households identified by the PMT are validated through spot-checks and community assemblies. (Usui, 2011). The program was initially targeted to municipalities with a poverty incidence greater than 50%, so that a large share of the population was eligible for the cash transfers. For instance, approximately 52% of all households were eligible for transfers in the villages that made up the experimental sample (Redaelli, 2009).

4Ps transfers amount to a substantial amount of income for recipients. The maximum transfer amount corresponds to 23 percent of the national poverty line; households above the poverty line are ineligible for the program. Families with three or more eligible children receive the maximum annual grant of PHP 15,000, as long as they meet the program's conditions; the minimum annual grant is PHP 8,000, to families with only one child. At current exchange rates, 4Ps transfers range from roughly $200–$370. The 4Ps transfer size is comparable to CCT programs in Latin America. In Mexico's *Oportunidades*, the transfer size

[2] The PMT's formula is not disclosed publicly, in order to minimize the chances of strategic reporting of census data. Moreover, instead of asking directly about the income and expenditure of households in collecting local census data, the PMT instead estimates them with household-level socioeconomic indicators.

is approximately 21 percent of total annual household expenditures; in Colombia's *Familias en Accion*, it represents about 15 percent of the minimum wage; and in Nicaragua's *Red de Proteccion* Social, it is about 17 percent of annual household expenditures (Fernandez and Olfindo, 2011, p. 6).

The relatively large size of the transfer created a strong incentive to comply with the program conditions. In the villages covered in "Set 1" of the program, from which the experimental sample was drawn, 87 percent of eligible households complied with the program's conditions and received transfers (Fernandez and Olfindo, 2011, p. 8-9).

2.2 Civil Conflict in the Philippines

The Philippines is home to multiple long-running insurgencies with distinct motives and characteristics. The country's largest and most active insurgent organization during the 2001-2009 period of study was the New People's Army (NPA). The NPA's strength averaged approximately 7000 fighters over this period, and the group was active in 63 of the country's 73 provinces. [3] Over 60 percent of the operational incidents reported by units of the Armed Forces of the Philippines' (AFP) in the field involved elements of the NPA. In the villages that took part in the 4Ps experiment, the NPA was involved in 72.1 percent of the reported incidents. The country's second-largest insurgent movement is the Moro Islamic Liberation Front (MILF), an Islamist separatist movement active in the southwestern provinces on the island of Mindanao. Between 2001 and 2009, the MILF was involved in 11 percent of security incidents reported nationwide and 9.6 percent of incidents in the villages under study. The remaining incidents involved insurgent splinter groups and criminal groups that the AFP refers to as Lawless Elements, who were involved in just under 19 percent of nationwide

[3] Estimates based on information maintained by the Armed Forces of the Philippines Deputy Chief of Staff for Intelligence (J2).

incidents and 18.3 percent of incidents in the villages under study. Finally, the al-Qaeda-affiliated Abu Sayyaf Group (ASG) were involved in 5 percent of the incidents reported by the military nationwide, but in none of the incidents in the villages under study.[4]

3 Empirical Strategy

The randomized experiment we exploit was conducted by the World Bank in 2009. In the experiment, 130 villages were randomly divided into 65 treatment villages, in which the 4Ps program was introduced in 2009 and 65 control villages, in which the program's start was delayed until 2010. The details of the experiment are described in (Redaelli, 2009).

The experimental sampling followed a three-step procedure. First, four eligible provinces—Lanao del Norte, Mountain Province, Negros Oriental, and Occidental Mindoro—were selected from a pool of eight provinces that were scheduled to begin receiving the 4Ps program in 2009. These provinces were non-randomly selected on the basis of geography to ensure that the evaluation would cover areas in each of the country's three major island groups, Luzon, Visayas, and Mindanao (Redaelli, 2009, p. 20). Second, two eligible municipalities were randomly selected from each province to participate in the evaluation. Finally, half of the villages within each of these eight municipalities were randomly assigned to the treatment group and the other half to the control group, leading to a sample of 65 treatment villages and 65 control villages. Table 1 contains information on the treatment assignment of villages in each of the 8 participating municipalities. Overall, the experimental villages contain 47,627 households, out of which 24,651 were eligible for the 4Ps program (Redaelli, 2009).

[4]The Abu Sayyaf Group operates mainly in remote areas of Basilan and Sulu provinces, which did not take part in the experimental evaluation since 4Ps was already operating in both provinces by late 2008.

Our empirical strategy estimates the causal effect of the 4Ps program by comparing the number of conflict incidents on treatment and control villages. Our baseline estimates come from the following regression:

$$Y_i = \beta_0 + \beta_1 Treat_i + \beta_3 X_i + \epsilon_i \tag{1}$$

where Y_i is the number of conflict incidents village i experienced in 2009, and $Treat_i$ is an indicator variable for villages assigned to the treatment group. The model further controls for a set of observed village characteristics X_i. The causal effect of the 4Ps program is captured by the parameter β_1, associated with the treatment indicator.

To improve the precision of our estimates we also use a difference-in-differences estimator using data for the period 2001-2009:

$$Y_{it} = \beta_0 + \beta_1 Treat_i \times Y2009_t + \beta_2 Treat_i + \beta_3 X_i + \epsilon_i \tag{2}$$

In this equation, $Y2009_t$ denotes an indicator for observations taken in 2009, the year in which the program was active in the treatment villages but not the control villages. The causal effect of the 4Ps program is estimated by the parameter β_1, associated with the interaction of the treatment indicator and the indicator for the treatment year, 2009. The parameter β_2, associated with the uninteracted treatment indicator, estimates the pre-treatment difference in conflict between the treatment villages and the control villages. To account for possible serial correlation in the error-term, we cluster the standard errors of this regression at the village level.

4 Results

4.1 Data, Summary Statistics and Balance Tests

We use three different sources of data for our empirical analysis. Data on conflict incidents was compiled from unclassified portions of the reports submitted by units of the Armed Forces of the Philippines deployed to conduct counterinsurgency and other internal security operations in the field. The database includes information on every operational incident reported by the AFP during the period of observation of 2001–2009. In total, it contains information on almost 26,000 unique incidents.[5] The dependent variable is an annual count of conflict incidents per village. Incident counts are a useful proxy of the intensity of conflict and have been used by previous studies such as Berman et al. (2011), Beath et al. (2011) and Dube and Vargas (2013). The location of each incident was recorded using Global Positioning System technology and matched to the village in which it occurred. Data on the treatment assignment of villages comes from 4Ps program data, which is maintained by the Philippine Department of Social Welfare and Development (DSWD). Data on village characteristics comes from the Philippines' 2000 National Census.

Table 2 presents summary statistics and balance tests for village-level control variables. The control variables consist of the villages population as well as indicators for the presence of paved streets, electricity, a communal water system and at least one store. All variables are from the 2000 National Census of the Philippines, except for the conflict incidents variable, which is the annual average over the pre-treatment period 2001-08.

The first two columns show means for treatment and control villages separately. Column 5 shows p-values of t-tests for differences in. The results show that treatment villages had

[5]Felter (2005) provides a comprehensive overview of the AFP data. Replication data will be made available through the Empirical Studies of Conflict (ESOC) Project.

slightly more conflict incidents in the pre-treatment period and slightly worse infrastructure than control villages, as they are less likely to have paved streets, electricity and stores, and more likely to have a communal water system.[6] However, these differences are not statistically significant at conventional significance levels, which increases our confidence that the randomization was successful.

Figure 2 shows graphical evidence of the effect of the 4Ps program on conflict. The top panel compares the trends in the average number of incidents experienced by treatment and control villages over the period of observation, 2001–2009, while the bottom panel plots the differences between the groups. The figure shows that treatment and control group had relatively steady and almost identical levels of conflict in the early pre-treatment period, 2001–2006. In 2007–2008, both groups experienced an upward trend, which was slightly steeper for the treatment group. In 2009, when the program was implemented in treatment villages, conflict in these villages dropped sharply; in control villages, by contrast, conflict continued on the same upward trend that it had followed during the previous years. To test whether the difference in conflict levels in the late pre-treatment period constitutes evidence for a failure of randomization, we conduct a robustness test for its statistical significance, which we report together with the main results in the next subsection.

The summary statistics show that the average number of conflict incidents per village in the study area is relatively low. In the pre-treatment period 2001–2008, villages experienced on average approximately 0.1 conflict incidents per year. While this seems like a small number of incidents it does not necessarily indicate a low intensity of conflict. For comparison, Beath et al. (2011) report that the villages in their experimental study of aid and conflict in Afghanistan experienced on average only 0.02 conflict incidents within 1 km of the village in the entire period of observation, 2004–2007 (and 0.2 incidents within 10km of the village).

[6] Communal water systems are more likely to be present in poorer villages, while richer villages are likely to have piped water access to individual household.

The average number of incidents per village in our study area is therefore higher than in the more peaceful regions of Afghanistan before the surge in US troop levels. However, a low level of violence does not mean that a conflict is economically insignificant. In addition to the lives and resources lost to violence, the mere presence of insurgents distorts economic incentives, by increasing entrepreneurial risks and/or imposing an implicit tax from extortion and bribes paid to insurgents for protection.

4.2 The Causal Effect of Cash Transfers on Conflict: Experimental Evidence

As explained in Section 3, we identify the causal effect of 4Ps on conflict using data from a randomized control trial of 130 villages in eight randomly-selected municipalities in four provinces that took place in 2009. Since the dependent variable is a count of the number of incidents, we use Negative Binomial models in addition to the standard OLS models for our estimations. We use Negative Binomial instead of Poisson models because the incidents variable exhibits overdispersion. We find similar results, however, in Poisson models (available on request).

Table 3 displays the results of Equation 1 in section 3. To make interpretation easier, we report marginal effects instead of coefficients for the Negative Binomial models (note, however, that the asterisks in Table 3 denote significance of the underlying coefficient). The estimated effect of the 4Ps program is the coefficient associated with the treatment indicator. The results show that the effect of the 4Ps program is negative, large, statistically significant and robust to the inclusion of control variables (Column 2) and municipality fixed effects (Column 3). Table 4 shows the results of the difference-in-differences estimator described in Equation 2. The estimated effect of the 4Ps program is the coefficient associated with the

interaction of the treatment indicator and the indicator for the treatment year, 2009. The results confirm those of Table 3.

The point estimates of both estimators suggest that the program reduced conflict by approximately 0.2 incidents per village per year. If this effect could be extrapolated to all of the approximately 14,000 villages covered by the program, it would add up to a total reduction of approximately 2800 incidents per year (of course the program's actual effect may well be smaller, since not all areas are affected by conflict to the same extent, so that the program's effect may be heterogeneous).

4.3 Tests for Pre-Treatment Differences

The parameter associated with the treatment indicator in Table 4, captures the baseline difference in conflict between treatment and control group over the entire period of observation except for the program year of 2009. This difference is small and not statistically significant, which suggests that the randomization was successful, so that treatment and control villages do not differ in unobserved variables that affect conflict. However, the steeper increase in incidents in treatment groups in 2007 and 2008 raises the possibility that treatment and control villages may have experienced unobserved shocks in the late pre-treatment period, so that they may have differed in unobserved variables immediately before the start of the experiment.

To test this, Table 5 presents estimates of the difference in conflict between treatment and control villages in the pre-treatment year 2008. The results show that, while the number of incidents was higher in treatment villages in 2008, the difference was not statistically significant. We therefore conclude that there is no evidence for a failure of randomization that resulted in unobserved differences between treatment and control villages before the

start of the experiment.

5 Conclusion

This paper presents an experimental evaluation of the effect of a large conditional cash transfer (CCT) program—the Philippines' *4Ps*—on the intensity of violence in civil conflict. In the last decade CCT programs have become one of the most important tools for delivering development aid and a large literature documents their positive impacts on the well-being of the poor. CCT programs are currently operating in numerous conflict-affected countries including Colombia, India, Indonesia and the Philippines. Some commentators have even proposed that a CCT program may help build peace in Afghanistan (Kenny, 2011). However, our the present study constitutes the first direct empirical evidence on how CCT programs affect civil conflict.

Our experimental results suggest that the 4Ps program caused a substantial reduction in the number of conflict incidents in the program area. This conflict-reducing effect is consistent with previous findings that positive economic shocks reduce civil conflict (Miguel et al., 2004; Dube and Vargas, 2013). There are two potential mechanisms through which CCT programs might reduce conflict. First, the transfer payments may increase popular support for the government by "winning hearts and minds". As a result, the population is more likely to supply intelligence on insurgents to the government, enabling the government to apprehend insurgents and reducing insurgent attack rates Berman et al. (2011). This mechanism is supported by the finding of (Manacorda et al., 2011) that a CCTs program can increase popular support for incumbent governments. Second, CCT programs may increase the opportunity cost of joining an insurgency. This could be either because the transfers boost the local economy and create higher incomes from peaceful activities, or because the conditions

imposed on program participants make it difficult to receive transfers while being active in the insurgency.[7] Either way, an increase in the opportunity cost of joining an insurgency would likely reduce conflict by making insurgent recruiting more difficult.

While we cannot say with certainty which mechanism explains our experimental results, they clearly suggest that the effect of CCTs is different from those of other types of aid interventions like CDDs and food aid, which have been found to increase conflict Crost and Johnston (2010); Nunn and Qian (2012). Of particular interest is the comparison with the results of Crost and Johnston (2010), who found that a CDD program, called KALAHI-CIDSS, increased conflict in the Philippines. The KALAHI-CIDSS program took place at a similar time (2003-2009) and in similar geographic regions as the 4Ps experiment studied in the current paper (which took place in 2009). Furthermore, both programs were implemented by the same agency, the Philippine government's Department of Social Welfare and Development. It is therefore unlikely that the opposite effects of these two programs are entirely due to institutional differences or differences in the local intensity or characteristics of the conflict.

Crost and Johnston (2010) cite two possible explanations for their finding that the KALAHI-CIDSS program increased conflict in the Philippines. First, if successful aid programs increase popular support for the government as suggested by the "hearts-and-minds" hypothesis, insurgents will have an incentive to sabotage the programs to prevent this from happening, which might exacerbate conflict at least in the short run.[8] Second, aid programs can increase conflict by increasing the amount of resources that the conflicting parties fight over (Hirshleifer, 1989; Grossman, 1991; Skaperdas, 1992).

There are several reasons why CCTs would be less likely to increase conflict through these

[7] Program participants have to attend monthly meetings in their village in order to remain eligible for the cash transfers, while joining an insurgency usually entails leaving one's village for extended periods of time.

[8] See Powell (2012) for a theoretical discussion of how shifts in power can cause conflict.

mechanisms than CDD programs or food aid. For one, community-driven development programs disburse aid through small infrastructure projects through a participatory democratic process. As a result, they create highly visible targets—the infrastructure itself as well as the community meetings needed to carry out the project – which insurgents can attack in their efforts to derail the program. By contrast, conditional cash-transfer programs such as 4Ps target households directly and disburse aid in cash primarily through electronic transfers to beneficiaries' bank accounts. This gives insurgents fewer high-profile targets and makes it more difficult to derail the program. In support of this hypothesis, there is anecdotal evidence, reported by Crost and Johnston (2010) that insurgents were able to derail implementation of the KALAHI-CIDSS program in a number of areas, but no analogous evidence for the 4Ps program. A similar reason might explain the different effects of cash-transfers and food aid, which needs to be physically transported to its destination and therefore also creates visible targets and incentives for looting them.

While we cannot say precisely which features of conditional cash-transfer programs explain their conflict-reducing effect, our findings provide evidence that the way in which aid is disbursed determines its impact on civil conflict. Going forward, this suggests a ripe opportunity for additional study of how and when various means of targeting and delivering aid can reduce rather than exacerbate the risk violent conflict.

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Figures and Tables

Figure 1: Map of 4Ps Study Areas

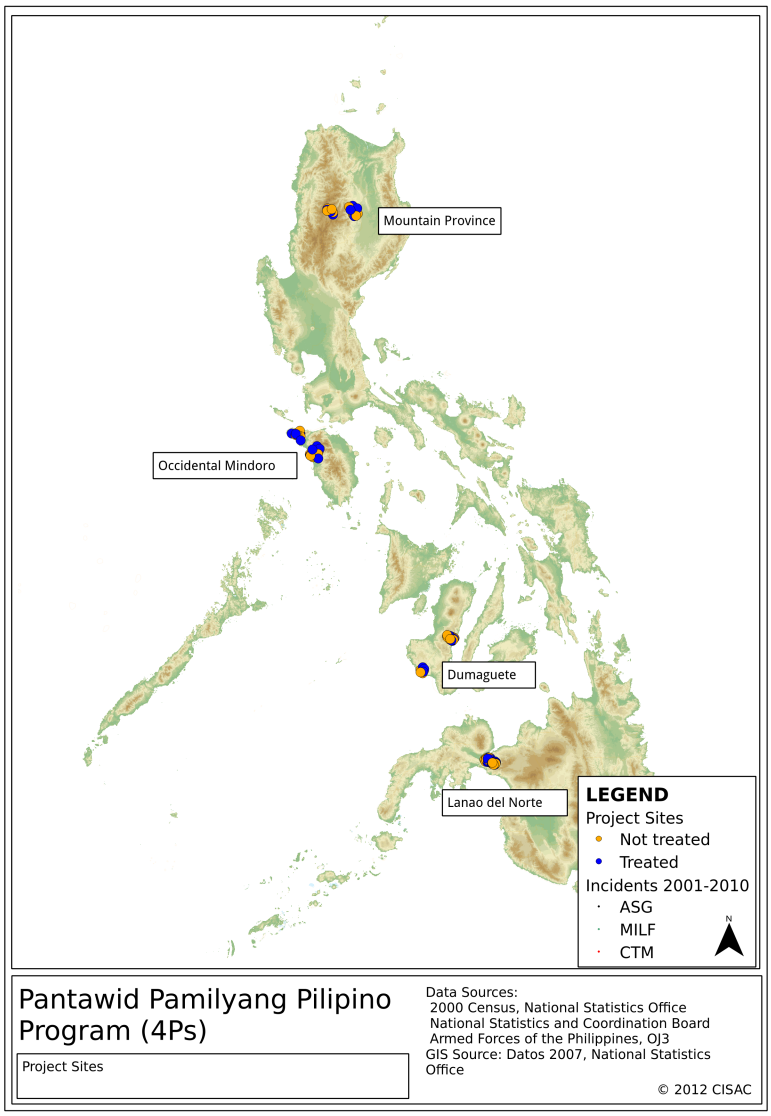

Figure 2: Time Trends of Conflict in Treatment and Control Villages

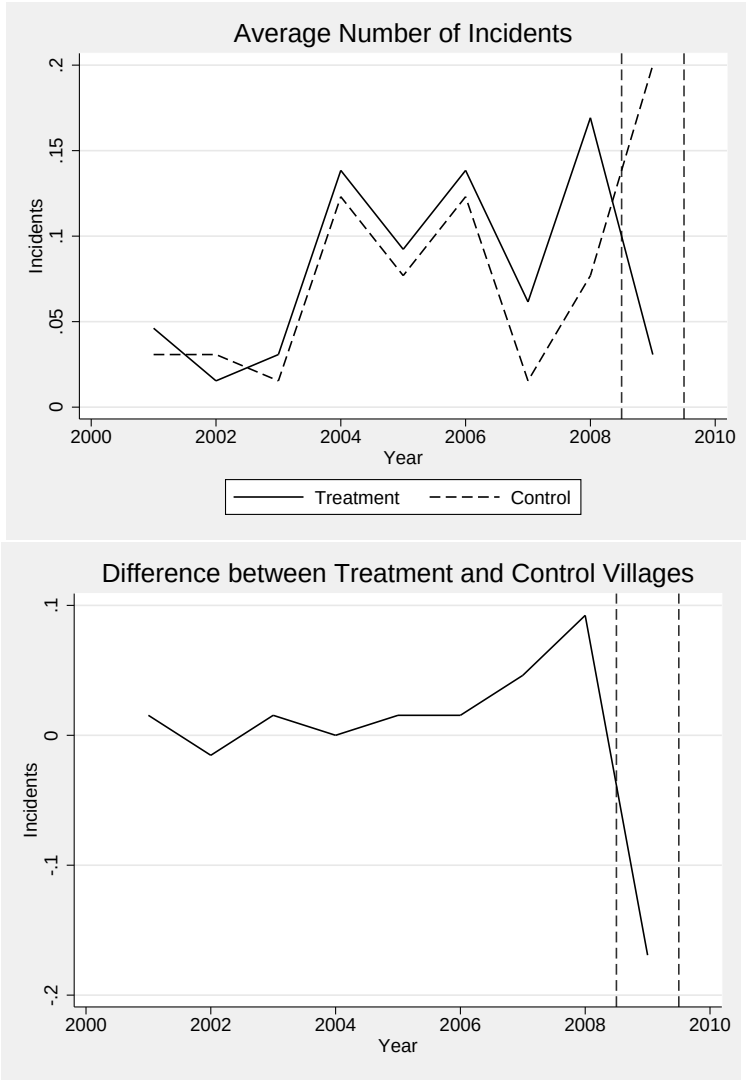

Table 1: 4Ps Experimental Sample

Region	**Province**	**Municipality**	**Treatments**	**Controls**
CAR	Mountain Province	Paracelis	4	5
CAR	Mountain Province	Sadanga	4	4
Region IV-B	Occidental Mindoro	Paluan	6	6
Region IV-B	Occidental Mindoro	Santa Cruz	5	6
Region VII	Negros Oriental	Jimalalud	15	13
Region VII	Negros Oriental	Basay	5	5
Region X	Lanao del Norte	Lala	13	14
Region X	Lanao del Norte	Salvador	13	12

Table 2: Summary Statistics and Balance Tests

Variable	Treatment	Control	Difference	P-Value
Conflict Incidents	.087	.063	.023	.52
Population	1475	1419	55	.81
Paved Streets	.215	.323	-.108	.17
Highway Access	.477	.508	-.031	.73
Communal Water System	.169	.154	.015	.81
Electricity	0.55	0.66	-0.108	0.21
Store	0.785	0.800	-0.015	.83
Health Clinic	0.492	0.462	0.031	.73
Observations	65	65	130	130

Summary statistics and balance tests of conflict incidents and village level control variables. The conflict incidents variable is the annual average over the pre-treatment period 2001-2008. All other variables are from the 2000 National Census of the Philippines.

Table 3: The Causal Effect of the 4Ps Program on Civil Conflict: Experimental Estimates

	Dependent Variable: Number of Incidents in 2009					
	Negative Binomial			OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-0.216**	-0.317**	-0.285***	-0.169*	-0.188*	-0.210**
	(0.153)	(0.286)	(0.132)	(0.093)	(0.095)	(0.095)
Population (1000)		0.0044	-0.0022		0.026	0.063
		(0.099)	(0.074)		(0.044)	(0.048)
Paved Streets		-2.33	-2.65		-0.228*	-0.426***
		(225.26)	(1419.2)		(0.128)	(0.148)
Highway Access		-0.337	-0.036		-0.186*	-0.029
		(0.348)	(0.113)		(0.112)	(0.139)
Electricity		0.0003	-0.054		0.066	0.060
		(0.130)	(0.100)		(0.134)	(0.129)
Communal Water System		-0.082	0.057		-0.108	-0.060
		(0.172)	(0.156)		(0.137)	(0.187)
Health Clinic		0.049	-0.176		0.104	0.067
		(0.133)	(0.150)		(0.112)	(0.124)
Store		-0.215	-0.361		-0.066	-0.083
		(0.275)	(0.204)		(0.134)	(0.164)
Constant				0.200***	0.305	
				(0.066)	(0.139)**	
Municipality Fixed Effects	No	No	Yes	No	No	Yes
Observations	130	130	130	130	130	130

For negative binomial regressions the reported values are marginal effects. *, ** *** denote statistical significance of the *underlying coefficient* at the 10%, 5% and 1% levels.

Table 4: The Causal Effect of the 4Ps Program on Civil Conflict: Difference-in-Differences Estimates

	Dependent Variable: Number of Incidents			
	Negative Binomial		OLS	
	(1)	(2)	(3)	(4)
Treatment × Year 2009	-0.175**	-0.195**	-0.194**	-0.204**
	(0.087)	(0.057)	(0.098)	(0.098)
Treatment	0.028	0.046	0.025	0.031
	(0.040)	(0.029)	(0.036)	(0.032)
Municipality-by-Year Fixed Effects	No	Yes	No	Yes
Observations	1170	1170	1170	1170
Municipalities	130	130	130	130

For negative binomial regressions the reported values are marginal effects. *, ** *** denote statistical significance of the *underlying coefficient* at the 10%, 5% and 1% levels.

Table 5: Robustness Test for Failure of Randomization: Pre-Treatment Difference in Conflict

	Dependent Variable: Number of Incidents in 2008					
	Negative Binomial			OLS		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.065	0.042	0.066	0.092	0.079	0.085
	(0.057)	(0.078)	(0.067)	(0.058)	(0.075)	(0.075)
Population (1000)		-0.0019	-0.0028		-0.0009	-0.0030
		(0.0064)	(0.0064)		(0.0042)	(0.0045)
Paved Streets		-0.058	-0.138		-0.031	-0.16
		(0.073)	(0.113)		(0.064)	(0.10)
Highway Access		0.045	0.062		0.024	-0.035
		(0.045)	(0.046)		(0.034)	(0.30)
Electricity		-0.042	-0.044		-0.030	-0.018
		(0.078)	(0.042)		(0.0034)	(0.035)
Communal Water System		0.034	-0.036		0.025	-0.035
		(0.032)	(0.034)		(0.029)	(0.030)
Store		0.013	0.0041		0.017	0.0042
		(0.036)	(0.037)		(0.032)	(0.032)
Health Clinic		0.034	0.042		0.026	0.047
		(0.033)	(0.036)		(0.028)	(0.029)*
Constant				0.076***	0.074	
				(0.014)	(0.067)	
Municipality Fixed Effects	No	No	Yes	No	No	Yes
Observations	130	130	130	130	130	130

For negative binomial regressions the reported values are marginal effects. *, ** *** denote statistical significance of the *underlying coefficient* at the 10%, 5% and 1% levels.

Self-Control in Teams:

Evidence from a Field Experiment of Commitment Contracts and Team Incentives for Smoking Cessation*

Job Market Paper

Justin S. White†

October 31, 2012

Abstract

The degree to which peer pressure promotes self-control in team-based health interventions remains largely untested. Moreover, peer pressure and cash incentives have rarely been mobilized in tandem. To this end, we conducted a randomized controlled trial in rural Thailand to test a novel intervention that combines commitment contracts for smoking cessation with team incentives that activate peer pressure. We find that, relative to the control group, the intervention increased biochemically verified smoking abstinence by 20-25% points (90-136%) at 6 months (3 months post-intervention). Moreover, the intervention cost about $300 per marginal quitter, less than half that of common smoking cessation aids in Thailand. We find evidence that teammates had a large causal effect on each other's outcomes. The team effects are non-linear with respect to participants' *ex ante* quit predictions: the success of less confident smokers increases with a teammate's degree of self-confidence whereas the success of more confident smokers does not change. Optimal team formation consists of heterogeneous teams in which more confident smokers are paired with less confident smokers.

*I thank William Dow, Stefano DellaVigna, David Levine, Rita Hamad, and seminar and conference audiences at the ASHEcon biennial meeting, the Annual Health Economics Conference, the PAA annual meeting, the APHA annual meeting, UC Berkeley, Mahidol, and Chulalongkorn for helpful feedback. All errors are my own. Parichart Sukanthamala provided excellent research assistance. Suthat Rungruanghiranya, Tawima Sirirassamee, and Chaturon Tangsangwornthamma offered valuable advice while in the field. The study was funded by grants from the U.S. National Institutes of Health (NIA P30-AG012839, NIA T32-AG000246, NICHD R21-HD056581). Ethics review committees at Srinakharinwirot University and UC Berkeley approved the study. The trial is registered at `ClinicalTrials.gov`, number NCT01311115.

†University of California, Berkeley; `jswhite@berkeley.edu`; `http://jswhite.weebly.com/`

1 Introduction

Tobacco-attributable mortality is projected to reach 8.3 million people per year within the next two decades, accounting for one in 10 deaths worldwide (Mathers and Loncar, 2006). More than 80% of this mortality is projected to occur in low-income and middle-income countries. Treatment for tobacco dependence is currently not widely available in low-resource settings in the developed and developing world. A principal barrier is the relatively high cost of smoking cessation aids. In this study, we apply recent findings from the behavioral economics literature to design a novel intervention that uses social and monetary incentives for delivering smoking cessation assistance to smokers in low-resource communities.[1]

We fielded a randomized controlled trial in 42 villages in central Thailand to study the effects on smoking abstinence of combining commitment contracts and team incentives. Typically, commitment contracts are binding financial pre-commitments in which the return of an individual's money depends on the person achieving a specified goal. We place individuals in teams of two and offer team incentives conditioned on the outcomes of both team members in order to induce peer pressure, a strong force for regulating motivation and self-control (Asch, 1951; Cialdini, 2007). Both the contracts and team incentives aim to motivate individuals to maintain self-control: commitment contracts rely on financially backed agreements and team incentives harness a powerful mix of social incentives on top of the cash incentives. While commitment contracts have been shown to be modestly successful at enhancing long-run smoking abstinence (Giné, Karlan and Zinman, 2010), neither team-based social support interventions nor simply paying people to quit have consistently done so (Cahill and Perera, 2011; Park et al., 2012; May et al., 2006).[2] Our study tests if peer pressure, long recognized as a contributor to risky health behaviors, can be activated along with financial motivation and social support to foster positive health behavior.[3]

Peer support groups and other forms of team-based interventions have been a common approach to health behavior change, as witnessed by the popularity of organizations such as Weight Watchers and Alcoholics Anonymous. Advocates of team-based approaches often highlight the ability of teams to provide members with knowledge, motivation, and emotional support. However, team-based interventions can also be harmful under certain circumstances. In particular, if a person fails to achieve a goal, his or her teammates may

[1] Our focus is in closing the smoking treatment gap in rural areas, but our intervention is flexible enough to be offered in a variety of settings, including in clinics and worksites.

[2] Some literatures would refer to our team concept as a buddy, partner, or peer group intervention. We believe that "team intervention" best captures the spirit of our trial, and adopt that terminology throughout.

[3] Our two-arm feasibility trial was not designed to disentangle all of the causal pathways mobilized by the intervention, although we are planning a larger evaluation to do so.

become disenchanted, performing worse than if they had acted alone. This discouragement effect could account for the lack of success of some team interventions for smoking cessation (Park et al., 2012). There is a need for rigorous research that documents the effects of health interventions involving small teams. In our study, we exploit random team assignment to credibly identify the team effects. We try to understand the degree to which teams yield positive or negative spillover effects for our study participants. Specifically, we test a theoretical prediction from Battaglini, Bénabou and Tirole (2005) that teams have non-linear effects, such that being paired with a person who has a high self-assessed probability of quitting has a positive influence on a teammate's success and being paired with a person who has a low probability has a negative influence on a teammate's success.

We randomly assigned 201 participants in a 1:2 ratio to a control group that received two rounds of smoking cessation counseling or to a treatment group that received the counseling plus a three-part *team commitment contract*: 1) a savings account with a minimum balance of $1.67, weekly deposit collection of additional voluntary deposits, and a project-matched contribution of $5-10, all of which were forfeited if the person failed to abstain from smoking, 2) a $40 cash bonus if the person and an assigned teammate both abstained, and 3) weekly text message reminders for 10 weeks after enrollment. Participants could pre-select a teammate or choose to be randomly assigned a teammate from the same village and gender at enrollment.[4] The team bonus is equivalent to roughly four days of household income (Thailand National Statistics Office, 2008). All incentives were contingent on 7-day smoking abstinence assessed biochemically at 3 months. We also biochemically verified smoking abstinence at 6 months and collected self-reported smoking status at 14 months.[5]

We designed our team commitment intervention with several theoretical constructs in mind. First, smokers may suffer from *present bias*, a systematic over-valuation of the present relative to future time periods. Present bias may result in self-control problems whereby a smoker abandons a quit attempt because the craving and withdrawal costs loom large relative to the longer-run health and financial consequences. We offer a commitment contract to smokers, because binding pre-commitments are an established mechanism for motivating present-biased individuals to display willpower (Bryan, Karlan and Nelson, 2010). Second, *peer pressure* can provide a way for individuals to overcome pre-commitment problems (Babcock and Hartman, 2011), although the pathway has rarely been exploited. We offer team incentives to supplement a financial pre-commitment with a social commitment to smoking abstinence.[6] Third, individuals often fail to follow through on their plans because

[4] We adopted this design feature to compare the effects of teams with arbitrarily assigned and naturally occurring social ties. We also believed it would increase study take-up.

[5] We lost one participant to follow-up at 3 months and no participants at 6 months, aside from one death.

[6] The presence of the cash bonus means that the intervention is not a pure commitment device, which

limited attention distracts them from the goals they set (Karlan et al., 2011; Cadena and Schoar, 2011; Cadena et al., 2011). We provide participants with weekly text message reminders, which have been shown to assist individuals with limited attention (Karlan et al., 2011; Free et al., 2011). The weekly visits from deposit collectors also serve as a reminder. Finally, though not a primary motivation, our data also allow us to test for the presence of *projection bias*, namely the degree to which smokers fully appreciate the value of being smoke-free. Behavioral economists find that projection-biased individuals under-predict how their preferences will change in the future, leading to an aversion to depart from the status quo (Loewenstein, O'Donoghue and Rabin, 2003).

Our study makes three main contributions. First, we characterize the extent to which smokers succumb to two behavioral biases that can hamper their ability to quit smoking: present bias and projection bias.[7] Evidence on smokers' behavioral biases is limited, though frequently used to justify policy interventions.[8] Second, we test a unique variant of a theory-driven intervention designed to overcome these behavioral biases. We test the intervention in a low-resource setting where conventional cessation services are not readily available. Further, we compare the incremental cost-effectiveness of our intervention to two common smoking cessation aids in Thailand, in order to determine the viability of team commitment as an alternative to current approaches. Finally, we examine the effects that teammates have on each other's outcomes. We quantify the causal effect on a person quitting of a teammate quitting, and we test a theoretical prediction regarding the non-linear nature of these team effects with respect to baseline quit predictions.[9] We further investigate the preferred rule that a social planner might use to assign teams, in line with recent attempts to find optimal policies for sorting individuals into teams (Graham, Imbens and Ridder, 2009; Bhattacharya, 2009).

a standard economic agent would not take. Rather, smokers may enter our study motivated solely by the potential financial gain. The upside is that the payments may also attract naïve present-biased agents who are not aware of their self-control problems and thus would not enter a pure agreement but who might benefit from pre-commitment.

[7] We test whether participants hold overoptimistic beliefs regarding their future self-control (naïveté regarding their present bias) and under-predict how much they will value being smoke-free (projection bias).

[8] Some studies assume the existence of these biases (Gruber and Köszegi, 2001), and others infer their presence based on smokers' use of pre-commitments (Wertenbroch, 1998; Gruber and Mullainathan, 2005). For an example of these concepts being applied to a recent policy discussion , see a 2010 paper from the U.K.'s Cabinet Office Behavioural Insights Team: `http://v.gd/healthnudge`. Accessed March 12, 2011.

[9] There is some empirical support for non-linear team effects, including Babcock et al. (2011) and Bandiera, Barankay and Rasul (2010).

2 Background

In this section, we describe two systematic errors in decision making that impede smokers' ability to quit smoking: projection bias and present bias. We then describe how the team commitment intervention may mitigate the impact of these biases on smokers' quit attempts.

Many people mispredict what their preferences will be in the future (Loewenstein and Schkade, 1999). In particular, individuals, especially those in a state of heightened emotion, may project their current preferences on predictions of future utility, recognizing that their preferences will evolve but under-predicting the magnitude of the change (Loewenstein, O'Donoghue and Rabin, 2003). This so-called projection bias might lead smokers who are in an addicted state to under-appreciate what life would be like if smoke-free. Only a small literature has examined projection bias in field settings (Read and van Leeuwen, 1998; Conlin, O'Donoghue and Vogelsang, 2007; Acland and Levy, 2011; Simonsohn, 2010; Busse et al., 2012). Levy (2010) provides the only field evidence for smokers, concluding that U.S. smokers under-estimate their change in smoking tastes by 40–50%. We try to gather some of the first experimental evidence from a field setting of whether smokers fully value the benefits of quitting. Projection bias, if present, would suggest the need for interventions that alter smokers' predictions of the gains of quitting.

The hallmark of a self-control problem, also known as *present bias*, is that a person systematically deviates from a plan considered optimal when formulated in the previous period. Present bias can impede a person's ability to fulfill his or her *ex ante* preferences and can diminish a person's long-run welfare (O'Donoghue and Rabin, 1999). O'Donoghue and Rabin (1999) distinguish between *sophisticated* agents who fully recognize their present bias, and *naïve* agents who are oblivious. Many studies find that agents are partially naïve, realizing they are present-biased but remaining overoptimistic about the degree to which they will remain so in the future (DellaVigna, 2009). Studies have linked present bias to health-related behaviors such as smoking (Levy, 2010) and exercise (DellaVigna and Malmendier, 2006).

Theory and evidence from behavioral economics suggest that present-biased individuals benefit from commitment contracts, whereby individuals pre-commit to incurring a penalty, often monetary in nature, for failure to achieve a goal (Bryan, Karlan and Nelson, 2010). Pre-commitment motivates a person to follow through on a goal in order to avoid the penalty for failure. A weakness of commitment contracts is that partially naïve individuals might be willing to sign up, but their tendency to delay costly investments may prevent them from putting enough at stake to motivate themselves ("under-commitment"). For example, our study is modeled after the CARES trial, which finds that 66% of smokers who took up a

basic commitment contract for smoking cessation failed to quit (Giné, Karlan and Zinman, 2010).[10] In this study, we test a novel approach for strengthening commitment with the aim of increasing goal attainment: we supplement a commitment contract for smoking cessation with social and monetary incentives.

Monetary payments have been used to promote a variety of personal health behaviors. Many studies find improved outcomes (e.g., Charness and Gneezy, 2009; Volpp et al., 2008), although monetary reinforcement of health behaviors has not been uniformly successful. A systematic review on competitions and cash incentives for smoking cessation concludes that, although incentives raise short-term quit rates, these gains prove fleeting (Cahill and Perera, 2011). Incentives often attract smokers who are financially motivated but unmotivated to stay abstinent, increasing relapse beyond the reward schedule.[11] Although not powerful enough to promote long-term quitting, in the short run cash incentives may help projection-biased agents who under-value the benefits of quitting.

The social effects of peer pressure have been documented across a range of settings (Falk and Ichino, 2006; Mas and Moretti, 2009; Karlan, 2007; Gerber, Green and Larimer, 2008). Team incentives, which condition awards on team production, may trigger peer pressure by inducing a variety of responses: a sense of responsibility; feelings of guilt, shame, and embarrassment; fear of social sanctions; a desire to be liked or respected; and closer teammate monitoring. The literature on team compensation finds that these incentives can improve productivity (Hamilton, Nickerson and Owan, 2003; Jones and Kato, 1995; Knez and Simester, 2001).[12,13] For example, Babcock et al. (2011) conclude that team incentives for gym attendance are as effective as equal-sized individual incentives, despite necessarily having a smaller expected payoff.[14] Only a handful of studies examine the use of peer pressure as a commitment mechanism for present-biased individuals (Dupas and Robinson, 2011; Gugerty, 2007; Kast, Meier and Pomeranz, 2010; Kullgren, Troxel, Loewenstein, Asch,

[10] The CARES trial, run by Giné, Karlan and Zinman (2010) in the Philippines, finds that 11% of those offered the contracts signed up, and contract users deposited, on average, 20% of one month's income over 6 months. On an intention-to-treat basis, the contracts raised the 12-month quit rate by 3.5% points from an 8.9% base.

[11] Crowding out of intrinsic motivation is an oft-cited reason for recidivism (Deci, Koestner and Ryan, 1999; Fehr and Falk, 2002), although the field evidence for crowd-out is weak (Cameron, Banko and Pierce, 2001).

[12] Group incentive schemes may also lead to free-riding (Olson, 1965). Shirking is not a concern in our setting, where payoffs depend on joint binary outcomes.

[13] Our work relates to voluminous literatures in health and education on peer effects and the influence and relationships among social network ties. (e.g., Sacerdote, 2001, 2011; Carrell, Hoekstra and West, 2011; Smith and Christakis, 2008; Leahey et al., 2010). In some cases, the underlying pathways may relate to peer pressure.

[14] Babcock et al. (2011) tracked a small number of college students for one month only (two weeks post-enrollment) and did not report outcomes after payments ended. We build on this promising design to test team incentives in a realistic field setting designed to have (and test) longer-term effects.

Norton, Wesby, Tao, Zhu and Volpp, 2012). These studies conclude that social commitment and peer monitoring can help members of informal savings groups to save money.[15] Our study adds to this nascent literature by clarifying the role peer pressure can play in adhering to health-promoting behavior.

Despite the potency of peer pressure and monetary incentives for influencing health behavior, researchers have rarely mobilized the two forces in tandem. A combination of team incentives and commitment contracts differs from contingent cash payments and basic commitment contracts in three key respects. First, participants must deposit money up front, selecting for motivated individuals who are most likely to benefit from the incentives, potentially improving the incentives' (cost-)effectiveness. Second, theory predicts that basic commitment contracts attract sophisticated agents who are aware of their self-control problem (Bryan, Karlan and Nelson, 2010), whereas the cash from team incentives may also draw in partially or fully naïve agents.[16] Moreover, team incentives may be especially helpful for (partially) naïve agents who are prone to under-commit. Third, team incentives add social incentives to the monetary incentives.

3 Model

In this section, we present an overview of our theoretical model. A technical elaboration of the model is provided in Appendix A. Our social learning model of self-control in teams is adapted from the work of Battaglini, Bénabou and Tirole (2005). It yields predictions about how certain behavioral biases affect smokers and in turn how smokers afflicted with these biases will influence each other when placed in two-person teams analogous to our intervention.

A key feature of the model is that present-biased agents learn about their own likelihood of exerting self-control by observing the actions of a teammate. Social learning operates in our setting through two channels. First, teammates' actions directly enter each others' payoffs via the team bonus. A person's motivation and choice of effort will depend on her self-assessed probability of earning the team bonus, which in turn depends on how likely she deems her teammate to show self-restraint.[17] Second, a person may gain (or lose) self-confidence after observing the successes (or failures) of a teammate. This occurs because agents may

[15] Kast, Meier and Pomeranz (2010) conclude that inducing peer pressure by sending text messages about the participant's success to a non-participating friend is no more effective than sending reminders to the participant. Our team incentives represent a stronger form of peer pressure. We also test the effects of our intervention above and beyond verbal commitment (see Section 4.2).

[16] The incentives could also attract time-consistent ("rational") smokers, although we find that a substantial share of participants hold overoptimistic beliefs about their ability to display self-control.

[17] We assume in this section and in Appendix A that the agent is female, and her teammate is male.

possess two traits: imperfect self-knowledge and imperfect recall of past actions.[18] Imperfect self-knowledge leads a person to try to intuit her ability to show self-control by examining her own past actions. She fears creating behavioral precedents, whereby a lapse today increases the likelihood of impulsivity in the future, leading to a concern for self-reputation (Bénabou and Tirole, 2004). However, imperfect recall of past actions means that a self-evaluation of one's history is not reliable. Consequently, a person turns to others to glean information about her own ability to show self-control. The model focuses on a teammate's effects on individuals with weak self-control ("weak types"), for whom good news or bad news from a teammate can be decisive, as opposed to strong-willed agents ("strong types") who resist temptation regardless of teammate type.

Battaglini, Bénabou and Tirole (2005) show that teams can produce positive or negative spillover effects for weak types. Although the positive aspects of teamwork are often touted, it is important to recognize that in theory team-based interventions could also be harmful. Encouraging reports of a teammate's self-control increase one's own chances of exerting self-control (a "good news equilibrium") and discouraging reports about a teammate's self-control decrease one's own chances of exerting self-control (a "bad news equilibrium"). At times, we refer to the positive spillovers from good news as an *encouragement effect* and the negative spillovers from bad news as a *discouragement effect*. According to the model, two factors determine the equilibrium state: 1) beliefs about a teammate's self-control and 2) informativeness of a teammate's actions. Beliefs matter, as stated above, because of teammates' correlated payoffs and a person's reputational concerns. Informativeness is based on the similarity of teammates, both in terms of how similar they perceive each other's self-control to be and the strength of their social ties. As the "correlation" between teammates strengthens, Battaglini, Bénabou and Tirole (2005) shows that self-restraint and welfare improve in the good news equilibrium and deteriorate in the bad news equilibrium.

3.1 Comparative Statics

Table 1 summarizes the comparative statics that follow directly from the model. For ease of interpretation, we include the notation for each parameter as defined in Appendix A. The key testable prediction is that team effects are non-linear with respect to the "correlation"

[18] The cognitive psychology literature has long studied imperfect self-knowledge and people's poor insight in their own cognitive processes (Bem, 1967; Nisbett and Wilson, 1977; Ross, 1977). Recall of cravings, pain, and discomfort tend to be systematically biased (Loewenstein, 1996; Loewenstein and Schkade, 1999; Kahneman et al., 1997). In addition, people selectively "forget" past lapses, often attributing successes to personal factors and failures to situational factors (Miller and Ross, 1975; Bradley, 1978). This can manifest itself as overconfidence in one's skills and abilities (Svenson, 1981). Several studies find that individuals are overoptimistic about their ability to exercise self-control, which is compatible with partial naïveté with respect to present bias (DellaVigna, 2009).

between a person and her teammate's confidence in showing self-control (θ). The model also suggests that the probability of showing self-restraint increases with: a person's self-confidence (ρ^1), a teammate's self-confidence (ρ^2), and the degree of self-control (β). Self-restraint decreases with the degree of projection bias (α). Several additional predictions are less model-specific. For example, self-restraint increases with the long-run payoff of abstaining from smoking ($V = V(H, m)$), where the benefits include both the health gains (H) and monetary rewards (m) contingent on abstaining. Self-restraint also increases with the cost of lapsing ($d = d(k, s)$), notably the amount of deposits committed to the person's savings account (k).

Team commitment contracts manipulate several model parameters. First, team commitment increases the cost of a lapse (d) through an increase in the social and monetary costs of failing to quit smoking. A person has control over the financial stake in quitting through the amount deposited in the commitment savings account. A weak type will become more likely to resist temptation as the account balance increases (as seen from Equations 8 and 9).

Second, the strength of social ties between teammates enters the model in two ways. On the one hand, a stronger partnership increases the social cost of failure as part of d, which is predicted to increase the likelihood of perseverance. On the other hand, stronger social ties will increase the informativeness of a teammate's actions (θ). In such a case, a stronger tie will accentuate the team effects, whether positive or negative. *Ex ante* a stonger dyadic relationship will make the pairing of two strong types stronger (via both channels), and will make the pairing of two weak types weaker as long as the informativeness of observing a close friend outweighs the social cost of letting down that friend.

Third, the team bonus enhances the returns to quitting (V). This feature is predicted to increase the probability of quitting, relative to a control group. Incentivizing the quit attempt is especially helpful for projection-biased smokers in an addicted state, who under-predict the extent to which they will enjoy being smoke-free. Team incentives also increase the degree to which a teammate's self-confidence matters for one's own effort choice (θ) by introducing correlated payoffs. As θ increases, the non-monotonic nature of the team effects are reinforced, strengthening the encouragement and discouragement effects. In the latter case, team commitment contracts may exacerbate self-control problems, particularly among pairs in which both members deliver bad news (i.e., in which both have low self-confidence).

4 Experimental Design

4.1 Study Site

We recruited smokers from villages in six subdistricts in central Thailand.[19] Each village has about 500 residents, and most people from the same village know each other. Median household income in the area is roughly $10 per day (Thailand National Statistics Office, 2008). Even though the study area lies within 100 miles of Bangkok, the local economy is predominantly agrarian. The area includes a mix of majority-Buddhist and majority-Muslim communities, and, for many residents, community life is oriented around religious activities and celebrations held at the local Buddhist temple or mosque. Four of the subdistricts lie within the catchment area of the region's major academic medical center, where the study team was based.

Thailand was an early adopter of tobacco control regulations in the region, starting in the early 1990s. Regulations include pictorial warning labels on cigarette packs, relatively high excise tax rates, bans on the display of tobacco at the point of sale, and comprehensive advertising bans. Thanks in part to these policies, daily smoking prevalence among men fell from 56% in 1991 to 37% in 2006 (Levy et al., 2008). The female smoking prevalence has remained under 5%. Roughly 41% of Thai men are daily smokers, compared to 36% of urban men (World Health Organization, 2009).[20] As many as half of Thai smokers use hand-rolled tobacco that can cost as little as $0.10 per pack-equivalent, as opposed to manufactured cigarettes that cost roughly $2 per pack (Hammond et al., 2008). Consumption of hand-rolled tobacco is concentrated in rural areas, such as the study communities.

Demand for quitting is relatively high in Thailand. Half of smokers reported a quit attempt in the prior year, nearly 90% of which did not involve a smoking cessation aid or professional support (World Health Organization, 2009). Smoking cessation programs in Thailand have expanded in recent years but are still limited to a handful of hospitals and community pharmacies, most of which are located in urban areas, yet quit rates rose as high as 10% in 2007 (White and Ross, 2012). Thailand's early adoption of tobacco control policies, high demand for quitting, and low use of professional services for smoking cessation make it an excellent setting for testing innovative approaches to promote quitting.

[19] The subdistricts, which span three districts in Nakhon Nayok province, are: Bueng San, Chumpon, Khao Phoem, Klong Yai, Ongkharak, and Pak Phli.

[20] Globally, the smoking prevalence in rural areas is also higher in rural areas than in urban areas (25.8% versus 24.3%), according to the 2003 World Health Survey. Statistic available at: `http://www.who.int/gho/urban_health/risk_factors/tobacco_text/en/index.html`. Accessed on February 12, 2012.

4.2 Study Design

Figure 1 shows the experimental design. Prior to recruitment, 253 community health workers (CHWs) were paid to undertake a census of smokers in their village, in order to target recruitment efforts and to measure trial participation.[21] CHWs reported a total of 2,055 smokers from 42 villages. Research staff held informational meetings within each study village, and CHWs also recruited smokers to enter the trial. All current smokers aged 20 and older who resided in a study community were eligible to enroll. Smoking status at enrollment was based on self-report and verified with eyewitness reports by CHWs. During enrollment meetings held from December 2010 to March 2011, 215 smokers from 30 of 42 eligible villages enrolled in the trial. In 12 villages, CHWs did not recruit any participants.[22] The meetings were held in public spaces within each village, in order to minimize the time and travel costs associated with the intervention. Anecdotally, the on-site enrollment substantially boosted participation. Prior to randomization, participants completed a screening questionnaire and provided written informed consent. All 215 enrollees signed a form agreeing to take up the intervention (i.e., to pay the minimum required deposit) if assigned to the treatment group. Participants were told during the consenting process that they would be invited to return for urine testing at 3 months and 6 months, although specific testing dates were not announced until the week of the follow-up.

The study followed a two-step stratified randomization procedure: 1) assignment to a two-person team and 2) random allocation to the treatment and control group. In the first step, participants were able to select a teammate prior to enrollment ("pre-selected" pairs) or to be randomly assigned to a teammate at enrollment. Randomly formed teams were stratified by village and sex. For village-sex strata with an odd number of at least three non-pre-selected enrollees, the "extra" person was retained in the sample ($n = 13$), and faced the same treatment allocation probabilities as those randomly assigned a teammate and those in a pre-selected pair. We dropped 14 individuals from the sample, 12 of whom belonged to a village-sex strata with one person and thus had no probability of being assigned a teammate (e.g., the lone female recruit from a given village) and two of whom arrived late to the enrollment meeting. The final sample included 201 participants, 188 of whom were assigned to a dyadic team.

In the second step, teams were randomly allocated to the control group or treatment

[21] In Thailand, CHWs have an assigned *kum* of roughly 10-15 households, in which they conduct a variety of health promotion activities. We asked CHWs to survey and recruit smokers living in their *kum*. A CHW is a position of respect within the community and tends to be held by civic-minded individuals, mostly women.

[22] Slack demand in these villages resulted from a lack of interest or effort from CHWs in some cases and a lack of interest from smokers in others.

group in a 1:2 ratio.[23] Note that control group members were also assigned a teammate, either one they pre-selected or a "synthetic teammate" whose identity was never revealed and used only for analysis. Pre-selected teams assigned to the control group were not given any instructions regarding whether to interact with their teammate.[24] At each enrollment meeting, a programmer implemented the random team and allocation sequences using computer-generated random numbers, concealing the random allocation sequence from other field staff and participants. The field coordinator received assignments from the programmer and then informed participants of their allocation.

While the randomization procedure took place, a smoking cessation counselor provided a group counseling session to all participants. At the end of each session, each participant signed and retained a certificate stating "I promise to quit smoking within 3 months to improve my health and that of my family." Thus, the intervention tests the effect of team commitment contracts above and beyond a verbal commitment. The field coordinator then announced treatment status assignment, and the control group was dismissed. The control group had no intervention-related activities following enrollment, aside from a second round of counseling at 3 months. Treated participants learned their teammate's identity, met briefly with their teammate to discuss plans (e.g., proposed frequency of contact and preferred nature of their interactions), provided a baseline deposit, and then were dismissed.

In addition to the control group's offerings, the treatment group received three components, the combination of which we call team commitment. First, each treated individual opened a commitment savings account with the project at enrollment. The account had a minimum opening balance of $1.67 (50 Thai baht). For 10 weeks after enrollment, a CHW visited the participant weekly to collect additional, voluntary contributions to the account. A triple-entry receipt system (with copies for the participant, CHW, and field coordinator) was used to track deposits, and the project collected deposits and a copy of the receipts from CHWs biweekly. The project added a $5 starter contribution to each treated participant's account and an extra $5 (THB 150) if the person reached an account balance of $5. The deadline for reaching this second match was randomized, such that each treated team was randomly assigned in a 1:1 ratio to have a deadline of 1 month or 3 months after enrollment.[25] The participant had the deposits and matching contribution

[23] We wanted to increase the number of teams receiving the intervention in our pilot study and to improve power for sub-analyses involving treated teams only.

[24] Presumably, some of these teammates provided each other with social support during the quit attempt, although the project made no effort to encourage or discourage these social interactions.

[25] The time-limited match manipulates the timing of the deadline while holding constant the incentive package. The early deadline is designed to stimulate depositing and thereby to nudge smokers toward setting an earlier quit date than they otherwise would, because they would have more to lose by procrastinating. Participants assigned to the later date are predicted to delay making deposits in order to wait and see if

refunded only if the person had quit smoking as assessed at 3 months. Second, if the person and his or her teammate both abstained from smoking at 3 months, each received a cash bonus of $40 (THB 1200), about 16% of median monthly household income.[26] Third, the project sent weekly text messages to boost the frequency and intensity of deposits and to increase the strength and salience of teammate monitoring and support.

Participants returned to the same meeting site 3 months after enrollment. At that time, all participants received cessation counseling. Treated participants also received financial rewards if they had quit, as described above. Quitting is defined as the 7-day point prevalence of biochemically verified abstinence. In other words, "quitters" had to self-report abstaining from smoking for at least 7 days and to pass a urine test.[27] Participants were tested for smoking abstinence 3 months and 6 months after enrollment using a NicCheck™ urine test for nicotine and cotinine, a metabollite of nicotine.[28] The color-coded test strips give results in 15 minutes. According to the manufacturer, the test has both a sensitivity and specificity of 97% and a detection period of 3-4 days for a smoker of 5-10 cigarettes per day and 5-6 days for a smoker of 20-30 cigarettes per day.[29] Participants and field staff were not informed of the detection period. The assessor of the urine test was blinded to treatment allocation. Urine containers were labeled with a unique identification number assigned to each participant. Anyone who disputed the test results could request a second test, although field staff encountered very few disputes. For all participants who did not attend either the 3-month or 6-month meeting, the field coordinator contacted the person by phone or else through a CHW to ascertain the person's self-reported smoking status. All individuals who reported having quit were visited at home to verify their status by urine test.[30] Shortly after the 3-month meeting, the field coordinator conducted a series of semi-structured qualitative interviews with participants ($n = 15$) and deposit collectors ($n = 14$) to enrich the research team's understanding of the intervention's impact.

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they can quit. However, the smaller financial commitment feeds back to delay the quit date. After three months, they are predicted to be less likely to have quit and less likely to have triggered the second match. The design is in the spirit of Duflo, Kremer and Robinson (2011), who show that time-limited deadlines help to counter procrastination.

[26] By comparison, Volpp et al. (2009) offered some of the largest cash incentives for quitting to date: roughly 27% of household income (our calculations). Note that the expected value of a team bonus is lower than an individual bonus of equal size after accounting for the teammate's probability of failure.

[27] We independently verified the self-reports against eyewitness reports from community health workers. With the exception of one or two participants, these reports concorded.

[28] Participants went one at a time into public bathroom facilities to provide urine samples. Research staff monitored participants to ensure that they did not carry any containers into the bathroom. The same research staff were used at enrollment and follow-up, allowing them to verify the identity of the participant with near certainty. Some CHWs were also on-hand at follow-up.

[29] The detection period is based on a phone conversation with Don Mossman, founder of NicCheck™.

[30] None of these participants passed the urine test. One subject declined to report his smoking status at 3 months. We count him as a continuing smoker in our intention-to-treat analysis.

At 6 months—that is, 3 months after all incentives were awarded—field staff biochemically assessed abstinence. The 6-month visit dates were announced less than a week in advance, reducing the ability of smokers to abstain right before the tests. Brief surveys were administered at the 3-month and 6-month follow-up meetings. Scheduled urine testing at 12 months was replaced by telephone follow-up at 13-16 months (denoted hereafter as 14 months) due to severe flooding in the study area in fall 2011. We paid an inconvenience fee of $3 per follow-up meeting attended to the control group at 3 and 6 months and to the treatment group at 6 months. Importantly, at both the 6-month and 14-month follow-ups, there are no differential incentives between the control group and treatment group to game the urine test or to misreport smoking status. Any difference in abstinence rates at those time points can reasonably be attributed to the intervention.

4.3 Empirical Strategy

4.3.1 Take-up of the Intervention

We measure trial take-up as the subset of smokers living in the study area who consented to enter the trial. The total number of smokers in the area is drawn from the census conducted by community health workers prior to recruitment. Each consenting individual agreed in writing to deposit at least $1.67 if assigned to the treatment group.

4.3.2 Treatment Effects on Smoking Abstinence

We estimate the intention-to-treat effect that our team commitment intervention has on smoking abstinence. The outcome $QUIT_{ijt} \in \{0, 1\}$ depends on a latent variable $QUIT^*_{ijt}$ of the propensity for individual i in pair j at month $t \in \{3, 6, 14\}$ to abstain from smoking.[31] The latent variable model is:

$$QUIT^*_{ijt} = \alpha_0 + \alpha_1 TREAT_j + \mathbf{X}_{ij}\alpha_{\mathbf{2}} + \epsilon_{ijt} \tag{1}$$

where $TREAT_j$ is an indicator variable for assignment to the intervention group; $\mathbf{X}_{ij}$ is a vector of baseline socio-demographic, smoking, and trial characteristics listed in Table 2; and ϵ_{1j} is a stochastic error term. The average treatment effect of the team commitment intervention, relative to the control group, is α_1. We run Equation 1 for biochemically verified 7-day smoking abstinence at 3 months and 6 months and for self-reported 7-day smoking abstinence at 14 months. We take the verified, 6-month results as our best measure of the

[31] Throughout, we present the linear form of our models, although estimation uses logit models unless otherwise specified.

intervention's impact on longer-run behavior change. For this regression and all others, we cluster standard errors at the team level, unless otherwise specified.

4.3.3 Cost-Effectiveness

Cost per marginal quitter refers to additional quitting in the intervention group compared to the control group. We calculated the cost per marginal quitter for the team commitment intervention and for two of the most common smoking cessation aids in Thailand: nicotine gum and varenicline, a physician-prescribed medication.[32] We also compare these figures to the cost per quitter for a basic commitment contract from the CARES trial, reported in Giné, Karlan and Zinman (2010). All costs are denominated in U.S. dollars, adjusted for differences in purchasing power parity (PPP) ($1 = THB 17.09).[33]

The costing for our intervention uses a programmatic perspective. Cost items include incentives (team bonus and matching contributions), personnel (full-time field coordinator, nurses who served as smoking cessation counselors, and deposit collectors), urine testing supplies, office supplies, text messages to participants and project-related phone calls, transportation of field staff, and forfeited deposits from continuing smokers, and excludes the subjects' own costs of quitting and survey costs. We also include a scenario of the feasible incremental cost per quitter if we had made three minor changes that should not alter the intervention's effectiveness, namely paying the deposit collectors piece rate rather than a fixed amount, hiring the field coordinator for a full-time equivalent of 2 months instead of 3 months, and buying the urine test strips locally. The estimated costs for the pharmacological interventions are based on each product's costs, as marketed and sold in Thailand at the time of the trial. We assume a 12-week course of each pharmacological aid, in order to fit the standard of care in Thailand.

Effectiveness is reported as the average treatment effect from logistic regressions. The exception is for the basic commitment contract, for which we use the treatment-on-the-treated effect reported in Giné, Karlan and Zinman (2009, 2010). We use two effectiveness measures for each pharmacological approach, one derived from available local studies and one from multi-country meta-analyses.

Additional details on inputs into our calculations are provided in Table 4.

[32] Varenicline is marketed in the U.S. under the brand name of Chantix™ and in Thailand as Champix™. Several brands of nicotine gum are available in Thailand; we estimated the costs for Nicomild™, one of if not the lowest-priced manufacturers in the country.

[33] The PPP exchange rate for 2010 is taken from the World Bank's World DataBank, accessed on May 26, 2012, and available at: `http://databank.worldbank.org/ddp/home.do`. All costs from Thailand are roughly half as large if we instead use the currency exchange rate ($1 ≈ THB 30).

4.3.4 Predictions about Quitting

We use participants' self-predictions about quitting to test for the presence of naïveté with respect to their present bias and projection bias with respect to the benefits of quitting. Participants predicted the probability that they would not be smoking in 3 months, elicited at baseline, 3 months, and 6 months. We used a visual scale labeled from $0 - 100\%$ to elicit the predictions, and participants had to report beliefs in 10% increments. At baseline, treated participants also gave social predictions of the probability each participant from their village would not be smoking in 3 months. For members $i \in 1, 2$ of dyadic teams $j = 1, \ldots, J$, let ρ^1_{1j} be the index person's self-prediction, ρ^2_{1j} be the person's prediction for her teammate, and ρ^2_{2j} be the teammate's self-prediction.

As a first step, we plot the distributions of predictions about the index person, as reported by the index person and others. We disaggregate the social predictions into those made by friends versus acquaintances to rule out that any observed differences are driven by access to differential information about the index person and her ability to quit. Next, we track how the self-predictions evolve over time and how they compare to subsequent quit behavior. The time path informs whether participants revise any overly optimistic beliefs once the participants gain experience with the costs of quitting. If smokers hold rational expectations, post-intervention beliefs will correspond to later observed behavior, in expectation, whereas divergence between predictions and behavior is indicative of partial naïveté regarding present bias. We also implement the difference-in-differences test of projection bias described in Section A.3. The intervention exogenously leads smokers from the treatment group to be more likely to exit an addicted state and, consequently, to perceive more accurately the benefits of being smoke-free. The double-difference of quit predictions (by pre- vs. post-intervention and treatment vs. control group) is weakly positive for projection-biased agents. Importantly, this setup sweeps out any time-invariant or group-invariant factors and is robust to any degree of present bias.

4.3.5 Team Effects

We start by testing the effect on smoking abstinence of the strength of social ties with one's teammate. According to our theoretical model, the sign of the effect is ambiguous. Next, we test if a teammate's quit status has a causal impact on one's own quit status. We then test for the non-linearity of these team effects as our model predicts. Finally, we calculate the quit rate under different assignment rules for matching individuals into teams. For these analyses, we focus attention on team effects at the intervention's completion (3-month end point) and omit the time index in the equations for notational simplicity.

We use several measures of the strength of teammates' social ties, including whether a teammate is pre-selected, the geographic distance between teammates' houses, the nature of their pre-trial relationship (acquaintance, close friend, or relative), the frequency of social contact prior to the trial, and whether prior to team assignment the index person listed her teammate as her closest, top two closest, or top five closest friends, among those participants enrolled in the trial. We restrict the sample to randomly formed teams for each of these analyses, except for the test of the effect of pre-selecting a teammate.

We posit that teammates have a causal influence on each other's quit behavior. A major challenge in the estimation is the joint determination of teammates' behavior, leading to potential simultaneity bias and omitted variables bias (e.g., correlated shocks). To infer the causal effect of a teammate's quit status, we use the mean quit predictions of all others from the same village (from all teams $k \neq j$) for that teammate $\bar{\rho}^2_{ik}$ as an excluded instrument for the teammate's subsequent quit status at follow-up. The exclusion restriction is met among those randomly matched with a teammate. We specify our model below as a two-stage least squares (2SLS) estimator, although we also run a bivariate probit model that some research suggests is a more robust procedure (Bhattacharya, Goldman and McCaffrey, 2006). The reduced form effect of a teammate's quit predictions on the index person's quit status is:

$$QUIT^*_{1j} = \alpha_0 + \alpha_1 \rho^1_{1j} + \alpha_3 \bar{\rho}^2_{ik} + \mathbf{X}_{ij}\alpha_{\mathbf{4}} + v^2_{1j} \tag{2}$$

The first and second stages for the two-stage setup are:

$$\begin{aligned} QUIT^*_{2j} &= \beta_0 + \beta_1 \rho^1_{1j} + \beta_2 \bar{\rho}^2_{ik} + \mathbf{X}_{ij}\beta_{\mathbf{3}} + v^1_{1j} \\ QUIT^*_{1j} &= \zeta_0 + \zeta_1 \rho^1_{1j} + \zeta_3 \widehat{QUIT}^*_{2j} + \mathbf{X}_{ij}\zeta_{\mathbf{4}} + v^2_{1j} \end{aligned} \tag{3}$$

where v^1_{1j} and v^2_{1j} are the first- and second-stage error terms and $\widehat{QUIT}^*_{2j}$ is the fitted value of a teammate's quit status. The coefficient ζ_3 is the causal effect of teammate's quit status on the index person's quit status. Our bivariate probit specification allows for correlation between v^1_{1j} and v^2_{1j}. We bootstrap the standard errors on the bivariate probit estimates using 1,000 replications, as boostrapping helps account for the overly narrow confidence intervals produced by the estimation procedure (Chiburis, Das and Lokshin, 2012).

Next, we test whether a person's own quit beliefs at baseline help to predict subsequent quit behavior:

$$QUIT^*_{1j} = \alpha_0 + \alpha_1 \rho^1_{1j} + \mathbf{X}_{ij}\alpha_{\mathbf{2}} + \epsilon_{ij} \tag{4}$$

For consistency with subsequent estimations, we restrict the sample to members of randomly formed teams in the treatment group.

We then examine the effect on quitting of a teammate's quit predictions for himself ρ^2_{2j}. Although the index person's self-predictions ρ^1_{1j} may be endogenous, the effect of a teammate's self-predictions is cleanly identified among the subset of randomly formed teams:

$$QUIT^*_{1j} = \alpha_0 + \alpha_1\rho^1_{1j} + \alpha_2\rho^2_{2j} + \alpha_3(\rho^1_{1j} \times \rho^2_{2j}) + \mathbf{X}_{ij}\alpha_{\mathbf{4}} + \epsilon_{ij} \tag{5}$$

In an alternate specification, we consider the quit predictions for a teammate from the viewpoint of the index person ρ^2_{1j}. We also test specifications that substitute into Equation 4 the mean quit predictions of all others for the index person $\bar{\rho}^1_{ik}$, the teammate's prediction for the index person ρ^1_{2j}, and the degree of overconfidence of the index person, as represented by the difference between her self-prediction and the mean predictions of all others for the index person $(\rho^1_{1j} - \bar{\rho}^1_{ik})$.

Based on the theoretical model and the empirical literature (e.g., Bandiera, Barankay and Rasul, 2010; Babcock et al. 2011), we expect that the team effects may be non-linear. To test the potential non-linearities induced by teammates' quit predictions, we first dichotomize baseline self-predictions at the median (between predictions of 70% and 80%): $\tilde{\rho} \in \{\underline{\rho}, \bar{\rho}\}$, where $\underline{\rho}$ is a Low type and $\bar{\rho}$ is a High type. Let $r_{ijm} = \mathbb{1}\{\tilde{\rho}^1_{1j} \times \tilde{\rho}^2_{2j}\} = \{r_{ij1}, r_{ij2}, r_{ij3}, r_{ij4}\}$, corresponding to pair types {(Low, Low), (Low, High), (High, Low), (High, High)}, where the first item in parentheses denotes Agent 1's type and the second Agent 2's type. Then, we run the model:

$$QUIT^*_{1j} = \theta_0 + \theta_1 r_{1j2} + \theta_2 r_{1j3} + \theta_3 r_{1j4} + \mathbf{X}_{ij}\theta_{\mathbf{4}} + \epsilon_{1j} \tag{6}$$

In this equation, a negative coefficient on r_{1j2} implies that less confident individuals are diferentially affected by a teammate's type and a post-estimation test of $\theta_2 < \theta_3$ would support the presence of differential effects for more confident individuals. To further assess the consequences of different pairing regimes, we use the fitted values from a regression with $(\tilde{\rho}^1_{1j} \times \tilde{\rho}^2_{2j})$ to predict the overall quit probability under two scenarios: 1) if all participants had been assigned to a teammate of the same type, i.e., (Low, Low) and (High, High) and 2) if all teams were of the opposite type, i.e., (Low, High) and (High, Low).

5 Results

5.1 Intervention Take-Up and Sample Characteristics

According to the household census, 2,055 smokers lived in the 42 study communities. However, only 84.9% of community health workers returned data collection forms (98.7%

in the 30 villages where at least one smoker enrolled in the trial). The household census and village-level population data imply an adult smoking prevalence in the study area of 29.0% for males and 2.0% for females. The trial enrolled 215 smokers from 30 villages, a participation rate of 10.5% among census takers, nearly identical to the percentage reported in the Philippines CARES trial. Unlike the CARES trial, take-up of our trial is not strictly a measure of demand for commitment, as our participants may have enrolled in order to qualify for monetary incentives. Take-up may be interpreted as a measure of demand for the team commitment intervention. We can adjust for the incomplete census reporting to estimate an alternate measure of trial take-up. Assuming random non-reporting (= 2,055/0.849), trial take-up is 8.9%, although this likely understates participation, as smokers not counted in the census were not likely invited to enroll in the trial. Among the 30 villages where at least one smoker enrolled in the trial, the participation rate is 13.3%. Among smokers who reported pre-trial plans to quit, the participation rate is 39.1%.

Table 2 shows baseline characteristics of participants and non-participants living in the study area. Participants are mostly men, mostly middle-aged, long-time smokers of three decades on average, and a majority use hand-rolled tobacco. The major difference between the groups is that less than 20% of non-participants expressed an interest in quitting, whereas more than 80% of participants did. This indicates that the intervention attracted a group of fairly motivated smokers, as expected. Baseline socio-demographic and smoking characteristics between the treatment and control groups were similar.[34] One notable exception is that, by chance, more pre-selected teams were assigned to the control group. Due to this imbalance and also to the endogeneity of pre-selecting a teammate, most of the analyses described below are restricted to randomly formed teams.

5.2 Treatment Effects on Smoking Abstinence

Smoking abstinence at 6 months was biochemically assessed for 93.9% of the treatment group ($n = 123$) and 87.0% of the control group ($n = 60$), a statistically insignificant difference. Non-responders were contacted by phone or else visited by a CHW to collect their self-reported smoking status. All reported themselves to be current smokers (or else the project visited them to verify their status), and eyewitness reports from CHWs confirmed that all had been seen smoking during the prior week. Figure 2 shows the unadjusted and regression-adjusted fitted quit probability by treatment status. At the intervention's end, 3 months after enrollment, 46.2% of the treatment group ($n = 61$) and 14.5% of the

[34] We do not control directly for each person's tobacco expenditures at baseline, but by controlling for tobacco type and cigarette consumption, we functionally do so, because tobacco prices vary little across geographic areas in Thailand (White and Ross, 2012).

control group ($n = 10$) had quit. The share of contract users who quit at the end of the intervention period was significantly greater than the 34.1% in the Philippines CARES trial ($t(131) = 2.78, p < 0.003$). At the primary end point of 6 months, 44.3% of the treatment group ($n = 58$) and 18.8% of the control group ($n = 13$) had quit. During the 3 months after incentives ended, nine treated participants (14.8%) relapsed. Thirteen treated participants (21.3%) relapsed between 3 and 14 months.

Analyses of intervention effects on quitting are performed on participants who had complete baseline data (Table 3; full results in Table C.1). Controlling for baseline factors, the intervention increased quitting by 28.1% points at 3 months and by 20.1% points at 6 months. The intervention's effects persisted to 14 months (42.0% quit), based on unconfirmed self-reports, although the share of control group members reporting having quit increased (24.6%), such that the average treatment effect of 13.2% points is marginally significant ($p = 0.051$).

The effectiveness of our behavioral intervention is on par with pharmacotherapy. Meta-analyses find that the risk ratios of smoking abstinence at 6 months or more for varenicline and nicotine replacement therapy, compared to placebo or a control group, are 2.27 (95% CI 2.02–2.55) and 1.58 (95% CI 1.55–1.66) (Stead et al., 2008; Cahill et al., 2012), whereas team commitment had a risk ratio of 2.35 (95% CI 1.39–3.98) at 6 months.

We cannot fully explore the causal pathways that contribute to the large treatment effect. That said, the text message reminders do not appear to have driven our entire results. In a sub-analysis, we find that treated participants who received any text message reminders ($n = 50$) were only marginally significantly more likely to quit at 3 months than treated participants who did not, most of whom had no phone (data not shown). Also, when we drop these 50 participants from the sample, the average treatment effect of the intervention remains the same magnitude.

5.3 Cost-Effectiveness

Figure 3 shows a forest plot of the incremental cost-effectiveness results. The team commitment intervention cost $281 per additional quitter (95% CI 187–562). With three simple logistical changes listed in Table 4, the intervention could feasibly be conducted for $195 per additional quitter (95% CI 130–390).[35] In comparison, the individual commitment contracts fielded in the Philippines CARES trial cost $700 per additional quitter (Giné et al.,

[35] We also calculate the cost per marginal quitter using self-reported smoking abstinence at 14 months, which fits more closely with the duration of the CARES trial but less so with the estimates for the pharmacological approaches. The actual team commitment intervention would cost $412 (95% CI 223–2,690), and the feasible intervention would cost $286 (95% CI 155–1,869).

2010), with an exceptionally large confidence interval because the treatment-on-the-treated effect used to generate the estimate comes from instrumental variables estimation. To the extent that the point estimates between trials differ, albeit insignificantly, the cost differences may result from our trial's reliance on CHWs, rather than professional staff, and a 3-month deposit period instead of 6 months. The cost per additional quitter for a 12-week course of nicotine gum in Thailand is $2,260 (95% CI 1,301–8,586) using effectiveness data from Thailand (Rungruanghiranya et al., 2008), and $1,780 (95% CI 1,414–2,401) using effectiveness data from a multi-country meta-analysis (Stead et al., 2008). The analogous estimates for a 12-week course of varenicline in Thailand are $790 (95% CI 524–1,607) using effectiveness data from Asian smokers (Wang et al., 2009) and $2,073 (95% CI 1,357–4,388) using effectiveness data from a multi-country meta-analysis (Cahill, Stead and Lancaster, 2012).

5.4 Predictions about Quitting

Participants showed far more confidence in their own ability to quit smoking than others had in them (Figure 4a). The distribution of participants' self-predictions is highly right skewed, such that a full 38% of participants expected to quit in 3 months with 100% certainty. In contrast, friends displayed considerably more pessimism toward the index person. The distribution of friends' predictions is bimodal, with peaks around 50% and 75% and without the heaping at probability 1. Acquaintances, who have less informative priors regarding the index person's abilities, give social predictions that follow a relatively normal distribution. A full 73% of participants are overconfident relative to the mean predictions of others (friends and acquaintances) for the index person, with the mean index person overshooting by 15% points and the modal person by 20% points (Figure 4b).

In Figure 5, we directly compare a person's self-predictions to her subsequent quit behavior. Under a standard economic model, an individual's prediction of future utility and consumption will match her realized utility and consumption in expectations. Predictions and realizations of smoking consumption diverge greatly in our sample. On average, participants held beliefs at baseline that were more than two times too optimistic. Whereas the mean participant gave herself a 79% chance of quitting prior to the intervention, only 35% of participants actually succeeded. The social predictions from Figure 4a, in particular those from friends, better reflect subsequent quit behavior, although they too are overly optimistic.[36]

Participants revised their predictions downwards following the intervention. Presumably,

[36] Research on social predictions is limited. Dunning et al. (1990) also finds that people are overconfident about their teammates' abilities.

participants better understood the nature of their time preferences and the cost function they were facing. Controlling for baseline characteristics, revisions between the baseline and 3-month predictions are modest, amounting to only 6.1% points (Column 1, Table 5). This adjustment accounts for roughly 14.1% of the 43.4%-point misprediction at baseline. In other words, participants' beliefs grew more realistic, but continued to be severely overoptimistic. Such failure to correct mistaken beliefs is highly suggestive that many participants are at least partially naïve regarding their lack of self-control.[37] Moreover, even after two rounds of mostly failed quit attempts, participants continue to cling to overoptimistic beliefs when elicited after 6 months. That learning about self-control is so limited in our environment highlights the degree to which naïve beliefs can persist over long periods of time. It also reinforces the notion that the revision between the first and second predictions resulted from information learned during the intervention period.

We use the self-predictions to implement Acland and Levy's double-differences test of projection bias. The goal is to determine whether participants project their current beliefs on their predictions of their future tastes. Intuitively, a confirmatory finding implies that smokers expect that quitting smoking would be less enjoyable than it actually is. Such mispredictions could stand in the way of smokers initiating meaningful quit attempts. We observe that, post-intervention, treated participants revise their predictions upwards by 7.9% points, compared to the control group (Column 3, Table 5). This marginally significant estimate is consistent with projection bias, in which continuing smokers fail to value fully the benefits of being smoke-free. The magnitude of the revisions we observe amount to about 40% of our average treatment effect. We can also compare the by-group differences at 3 months and at 6 months (Column 4). The interaction effect is larger for the 3-month predictions, although we cannot reject that the two estimates are equal. The revision at 3 months of 9.5% points translates into a revision of about 47% of the average treatment effect. Ours is the first test of projection-biased smokers using experimental data, of which we are aware, although Levy (2010) uses quasi-experimental methods to estimate that smokers underestimate their change in tastes by 40–50%. Thus, our estimates compare favorably to his, and we conclude that on average smokers show signs of projection bias.

[37] We have other corroborative evidence of naïveté. Our sample consists largely of long-time smokers who have incurred multiple (median of two) costly failed quit attempts in the past. At baseline, 57% of smokers identified "habit or physical addiction" or temptation from "people around you were smoking" as a primary reason for past failure. The latter is distinct from "desire to be social". "Stress" accounted for most other responses, and could also include a time-inconsistent dimension.

5.5 Team Effects

Of those in the treatment group, 27.3% earned the team bonus. Team outcomes were not evenly dispersed between the treatment and control groups. In the control group, 3.6% of individuals were in (pre-selected or synthetic) teams in which both members quit at 6 months, 32.1% in teams in which one quit and one smoked, and 64.3% in teams in which both failed to quit. In contrast, the breakdown for the treatment group is significantly different: 26.2%, 36.9%, and 36.9%, respectively ($\chi^2(2) = 17.1, p < 0.001$).

We investigated the effect of the strength of teammates' social ties on quitting at 3 months (Table 6). Of our seven measures of social tie strength, only two were significant. Participants paired with their closest or one of their five closest friends in the trial were 21.3% and 22.8% points more likely to quit smoking at 3 months. Yet, pre-selected teams did not out-perform randomly formed teams, and the sign of the coefficient is negative. In the regression-adjusted model for the full sample (Figure C.1), preselecting a teammate reduces the likelihood of quitting by a highly significant 22% points. Perhaps close friends are better able to ignore the social costs of failing to quit, under the belief that their friendship can withstand the disappointment. Alternatively, close friends may enable each other to smoke, for example, sharing a cigarette during social gatherings.

We estimate the causal effect of a teammate's quit status at 3 months on the index person's contemporaneous quit status (Table 7).[38] In the reduced form equation, the coefficient of interest implies that a 10%-point increase in others' mean predictions for one's teammate leads to about a 6%-point increase in the index person's abstinence. An F-test of the excluded instrument in the first stage of the two-stage procedure indicates that it is moderately strong ($F(1, 58) = 11.6$). The corresponding test for a probit model is: $\chi^2(2) = 8.7$. The second-stage estimates imply that a teammate who quits smoking increases the index person's likelihood of quitting by 53.6% in the OLS model and 39.2% in the bivariate probit model. Both coefficients are statistically significant, although the former is only marginally significant. The estimated coefficients are extremely large relative to the roughly 20% average treatment effect. In contrast, the naïve estimator in Column 7 gives a smaller, insignificant coefficient. The downward bias in the naïve estimator is somewhat puzzling and goes against our priors.

Next, we characterize the nature of the team effects using participants' quit predictions. Table 8 displays the relationships between baseline quit beliefs and subsequent smoking behavior. All models are restricted to treated teams in which pairs were randomly assigned and control for our full set of baseline characteristics. In sharp contrast with our theoretical

[38] We also interacted the excluded instrument with our measure for the strength of baseline social ties, but did not detect any significant interaction effects, possibly due to a lack of statistical power.

model, a person's self-predictions have no predictive power for her quit status 3 months later (Column 1). Yet, a teammate's self-prediction leads to a significant increase in the index person's likelihood of quitting. Increasing the teammate's prediction by 10% points corresponds to a a 4.5%-point increase in the index person's quit probability (Column 2). In the context of our theoretical model, we might interpret this relationship as a person's will being fortified after observing her teammate's self-confidence. If a teammate displays self-assuredness, then the index person might consider herself to have a greater likelihood of earning the team incentives, leading to increased effort and motivation on the part of the index person. As all other participants increase their evaluation of the index person's chances of quitting, she becomes much more likely to quit—in roughly a 1:1 correspondence (Column 4). A teammate's predictions for the index person likewise relate to the person's quit status at 3 months (Column 7). In contrast, the index person's prediction for her teammate is not related to the index person's own quit probability (Column 8).

We interact the dichotomized self-predictions of the index person and her teammate (Column 9) and plot the fitted probabilities (Figure 6a) from a regression-adjusted model in order to test for non-linear team effects.[39] Indeed, the team effects are non-monotonic in teammate's self-confidence. A team of (Low, High) type is 45.8% points more likely to quit smoking, compared to a (Low, Low) dyad, meaning that a person's quit probability increases dramatically when paired with a self-confident teammate. This differential effect could be interpreted as an encouragement effect from the perspective of an index person paired with a High type or as a discouragement effect from the perspective of an index person paired with a Low type. Given that Low types in the control group have a similar average quit probability as the (Low, Low) pairings, we consider this as suggestive but not conclusive evidence that the differential is driven by an encouragement effect for (Low, High) types. In contrast, High types are not significantly affected by a teammate's type. The theoretical model poses two possible explanations: the pattern may imply no encouragement or discouragement effects, or High types may be analogous to "strong" types from the theoretical model, i.e., individuals who would have quit regardless of teammate assignment. That these smokers have had a 30-year smoking tenure dotted with multiple quit attempts, on average, suggests that the individuals more closely resemble weak types from the model.

Among the intervention's actual team pairings, the fitted probability of quitting from Equation 6 is 48.3%. We also predict the quit probability under the scenario that all participants had been randomly paired with a teammate of the same type—(Low,

[39] Figure C.1 provides a side-by-side comparison of the unadjusted model and a model controlling for all covariates. The patterns are qualitatively similar across models. While a teammate's self-prediction is exogenous to the index person, the index person's self-predictions are endogenous. As such, we prefer the adjusted model, which controls for certain potential confounders.

Low) and (High, High) dyads—and under the scenario that all pairings had been of opposite type—(Low, High) and (High, Low). The predicted probabilities are shown in Figure C.1. Same-type pairings are predicted to yield a quit rate of 40.4% and opposite-type pairings are predicted to yield a quit rate of 53.8%, and these differences are statistically significant. Matching more confident individuals with less confident individuals leads to an encouragement effect for the less confident individuals without incurring any large discouragement penalty for the more confident individuals. We also tested these scenarios using others' mean predictions for the index person and for the teammate (not shown). The results were similar but far noisier, and the differential effect is no longer significant. Thus, self-predictions are the clearest contributor to the heterogeneous team effects.

During the qualitative interviews, some participants attributed their success to the team aspect of the intervention. One participant said, "I like [team] competition because I would procrastinate if I had to quit all by myself. I would wait and never think that I will actually do it today. This time was like many other times that I told myself and failed. I succeeded this time because I said that it must be today." Other participants credited the bonus with strengthening the social interactions with the teammate: "I thought about the bonus all the time because I knew that I could definitely quit....This also made me talk to my teammate more because both of us would get the bonus if we succeeded. We tempted each other using this bonus." Other participants were more ambivalent: "My partner and I rarely talked. It would be better if my teammate was someone who is closer to me because I'd dare talk to him more.... But this could also affect me if I couldn't quit but my teammate could, and I knew I'd dragged my teammate down. He wouldn't get the bonus because of me."

6 Discussion

We find that trial participants displayed signs of two key behavioral biases: naïveté about present bias and projection bias about the benefits of quitting. Projection bias led smokers to under-value smoking cessation while at the same time naïveté led smokers to be wildly overoptimistic about their chances of quitting successfully. On average, smokers under-predicted the benefits of being smoke-free by 40–50% and over-predicted their ability to quit by more than two-fold. Smokers maintain these mistaken beliefs for at least 6 months, highlighting the persistence of these errors and the need for interventions that can correct them. These results add to a limited empirical literature on the presence of these biases for smoking.

Our team commitment intervention was designed to counter present bias by strengthening participants' financial and social stake in quitting. The intervention substantially increased

the likelihood of biochemically verified smoking abstinence 3 months after the intervention ended and 6 months after enrollment. The provision of cash incentives for quitting smoking has not consistently increased long-term smoking abstinence (Cahill and Perera, 2011). We show that cash incentives contingent on team production may be effective in combination with commitment contracts. Relative to basic commitment contracts tested in the Philippines (Giné, Karlan and Zinman, 2010), team commitment contracts reduced the failure rate of users, highlighting the potential of stronger commitment through team incentives to promote quitting. However, about half of our contract users still failed to quit, suggesting that our intervention did not fully resolve the problems of under-commitment and lack of self-control faced by our study participants.

In our intervention, teammates had strong effects on each other's outcomes. The bivariate probit estimation points to a causal effect of a teammate quitting of 39% points. Team-based interventions that aim to enhance social support have not consistently increased smoking abstinence (May et al., 2006; Park et al., 2009). We also find that the text message reminders cannot fully explain the magnitude of our average treatment effect. Thus, we posit that some other aspect of the team incentives, such as peer pressure, is responsible for the strong team effects. A larger, more complex evaluation is needed to test this hypothesis and to discern the relative contribution of the intervention's potential pathways to smoking abstinence.

Our analyses indicate that the team effects are nonlinear with respect to baseline predictions for quitting, as our model predicted. Certain other findings did not adhere to the model predictions. For example, smoking abstinence did not increase with a person's self-confidence in quitting. The non-linear team effects imply that the preferred rule entails sorting individuals into heterogeneous teams based on baseline assessments of one's own quit probability.[40] Optimal rules for assortative matching is an exciting new area of research, although the task warrants caution; empirically driven assignment rules can lead to unanticipated outcomes (Carrell, Sacerdote and West, 2012). Future research should attempt to replicate our findings.

Few studies have assessed smoking cessation interventions in population-based settings in the developing world, and even fewer have assessed strategies targeted to rural populations, despite the large share of rural deaths attributable to tobacco use. Our intervention translated into a decrease in smoking rates of 2-5% points in the study area.[41] A change of

[40] Alcoholics Anonymous pairs new members with a sponsor who has been abstinent long-term. Many self-help groups have similar programs. It is unclear the extent to which a signal of strong willpower from someone like the sponsor can influence the behavior of other members.

[41] The decline is 2% if we conservatively assume all control group members would have quit in the absence of the intervention. The decline is 5% if we assume that no one would have quit in the absence of the intervention.

such magnitude could potentially lead to a multiplier effect if quitting spreads through social networks as some researchers assert (Cutler and Glaeser, 2010; Christakis and Fowler, 2008). We also find low relapse rates among participants. Coordinated quit attempts of friends within the same community may reduce recidivism, potentially by changing the norms of tobacco use within a smoker's social network.

The incremental cost-effectiveness analysis indicates that our intervention performed favorably relative to the smoking treatments most used in Thailand and relative to other economic evaluations of smoking cessation therapies (Ruger and Lazar, 2012). We have not calculated the cost per lives saved nor the cost per disability-adjusted life year (DALY) averted, but given the available estimates of DALYs averted from nicotine replacement therapy and other tobacco control interventions (Ransom et al., 2000), the team commitment intervention likely meets the World Health Organization's (WHO) standard for "very cost-effective" in Thailand, defined as being less than gross domestic product ($8,600, PPP-adjusted, in 2011).[42] The health gains from our intervention are large if existing estimates of the benefits from smoking cessation transfer to the Thai context. Smoking cessation among men aged 55 (the closest average age to our study population) extended life expectancy by nearly 5 years in the U.S. (Taylor Jr. et al., 2002). Life expectancy at birth in Thailand was 70 in 2009, according to official WHO estimates, compared to 78.1 in the U.S.

Our study has several limitations. First, external validity is a concern for a small trial fielded in 42 communities. Smoking prevalence in our communities matches national estimates for rural areas, and our communities are diverse, including Buddhist and Muslim areas; however, the communities were sampled out of convenience, not to represent a broader geographic area. More generally, one might worry that Thailand's high demand for quitting and comprehensive tobacco control regulations make it a special case, although smoking patterns in other developing countries are likely to follow suit as a result of tobacco control reforms already underway. Second, the two-arm trial cannot disentangle the causal pathways by which the intervention worked. The next step will involve a larger evaluation that seeks to clarify the potential mechanisms underlying team commitment's success (e.g., financial commitment vs. peer pressure vs. regular reminders) and to investigate the nature of the team effects. Third, the predictions on which much of our analysis relied were not elicited in an incentive-compatible manner, leaving open the possibility that participants reported predictions that are somehow systematically biased. Many studies find that incentivized and unincentivized predictions are similar (Delavande, Giné and McKenzie, 2011), although we

[42] This estimate comes from the World Bank's World DataBank (2012). Available at: `http://databank.worldbank.org/ddp/home.do`. Accessed on May 26, 2012.

are unable to confirm that subjects reported their true beliefs. Finally, our small sample size precluded us from taking a more granular look at certain extensions of our theoretical model, including the types of pairings that inhibit and promote goal attainment.

Our study shows that a simple intervention enhanced the likelihood of smoking cessation in rural communities. Team commitment contracts may offer a viable, cost-effective alternative to current smoking cessation approaches in low-resource settings. Meanwhile, the findings raise exciting new possibilities for mobilizing peer pressure to effect positive health behavior change.

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Figure 1: Experimental design

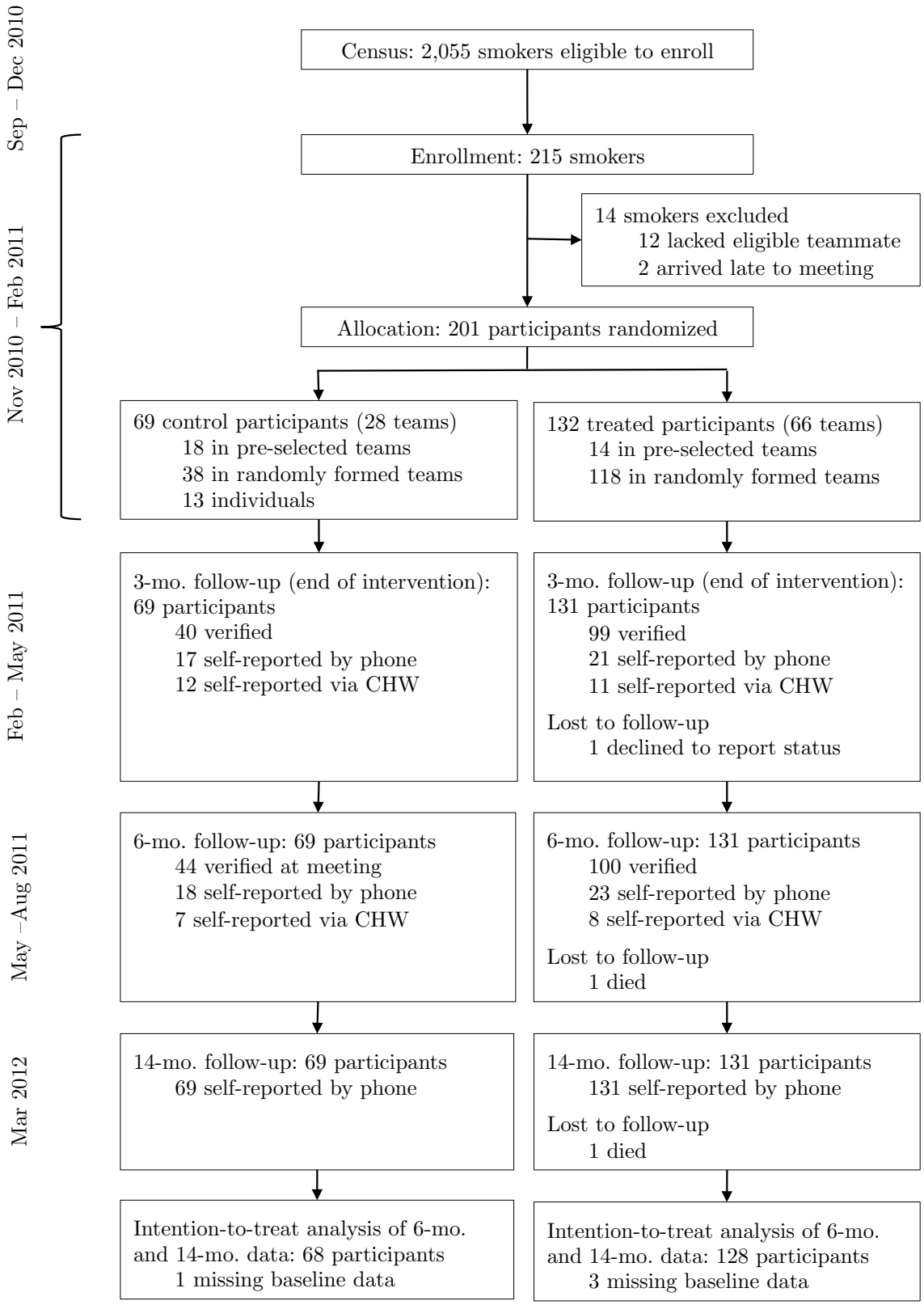
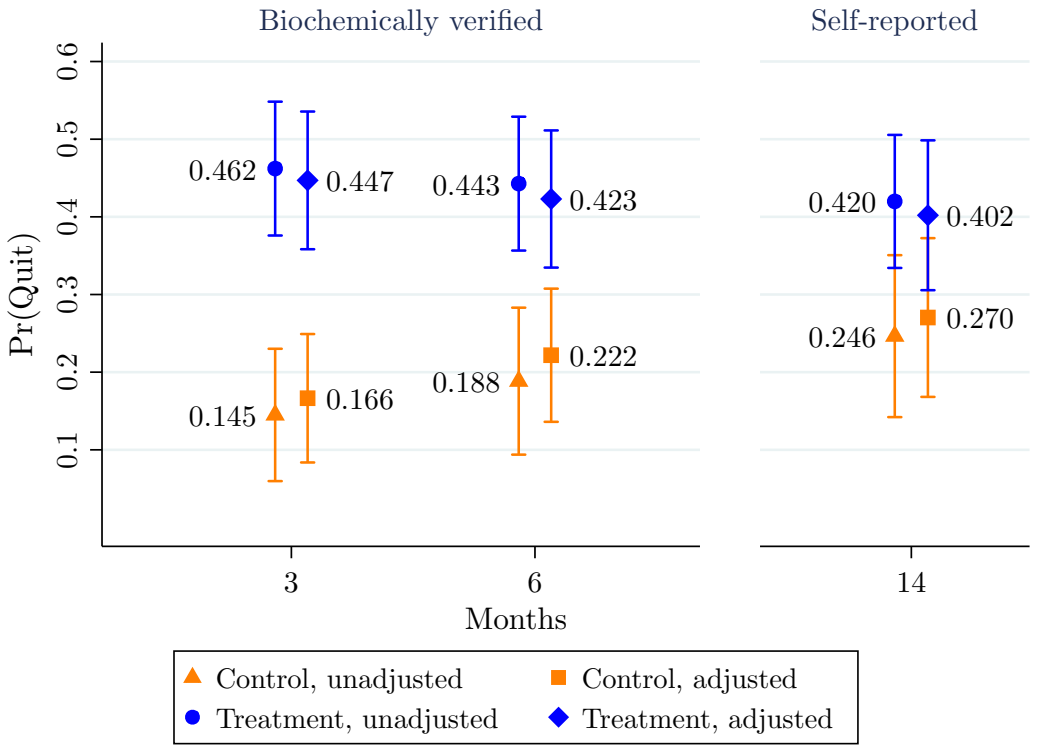
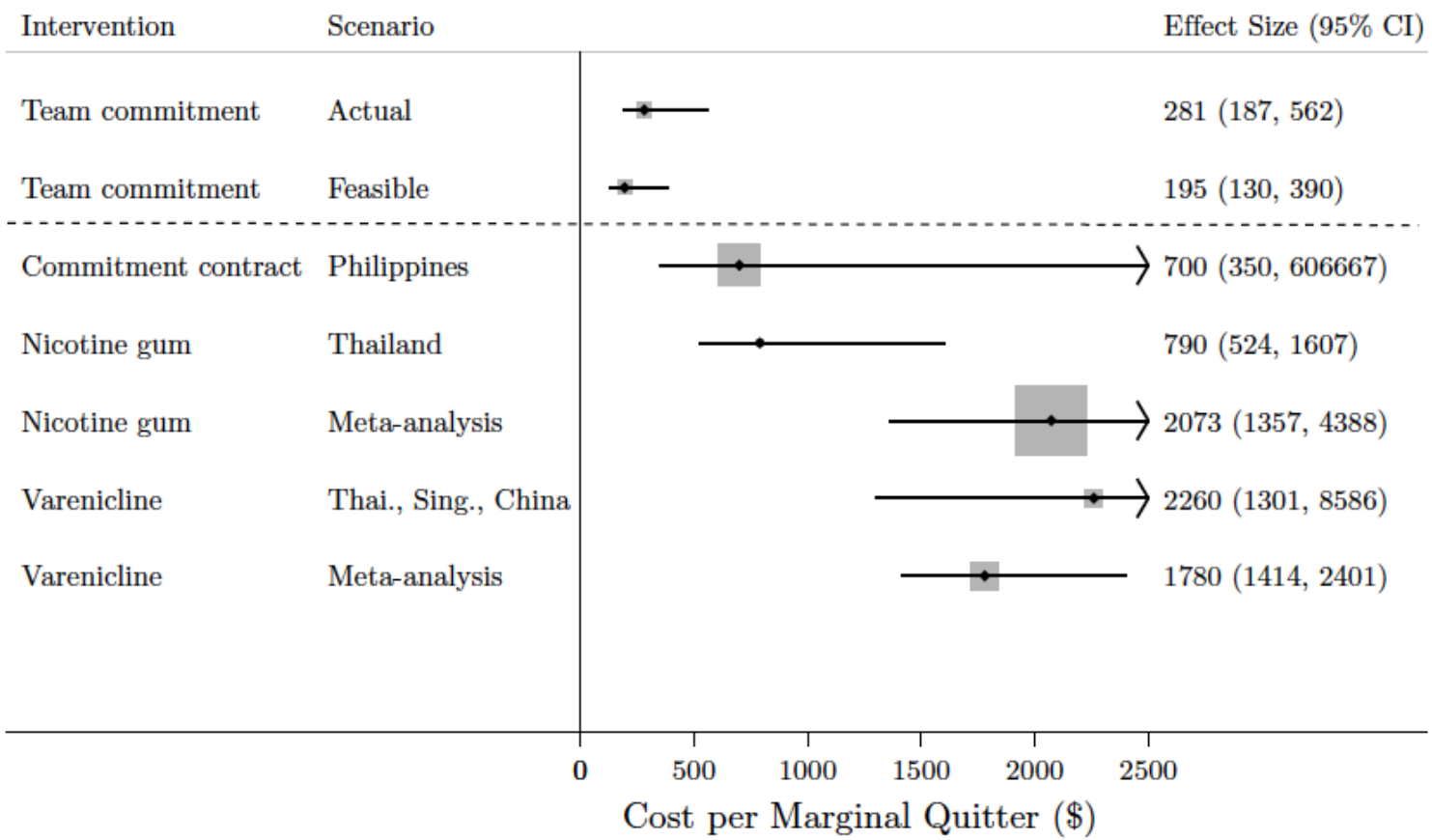


Figure 2: Predicted probability of smoking abstinence, by month and treatment status



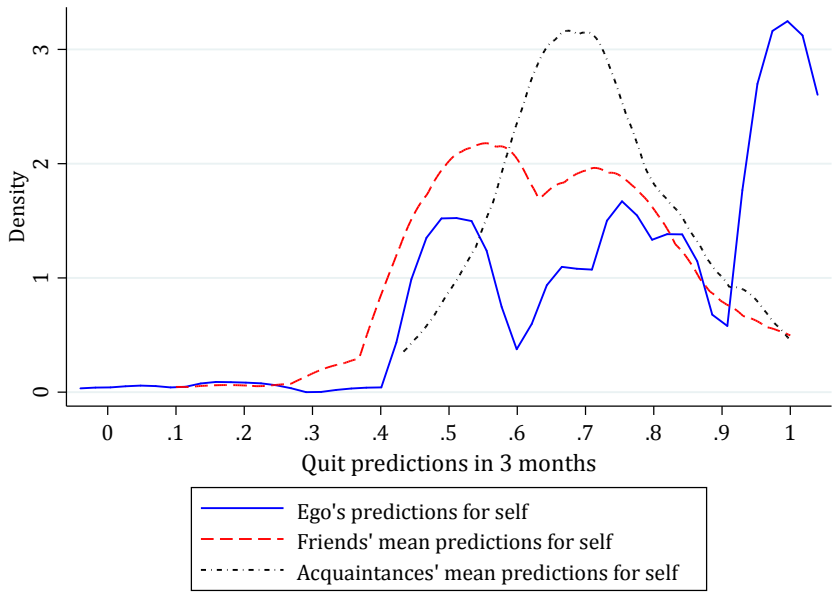
Note: Adjusted probabilities are derived from the logit models in Table 3. Error bars represent a 95% confidence interval, based on standard errors clustered at the team level.

Figure 3: Cost per marginal quitter, by type of intervention

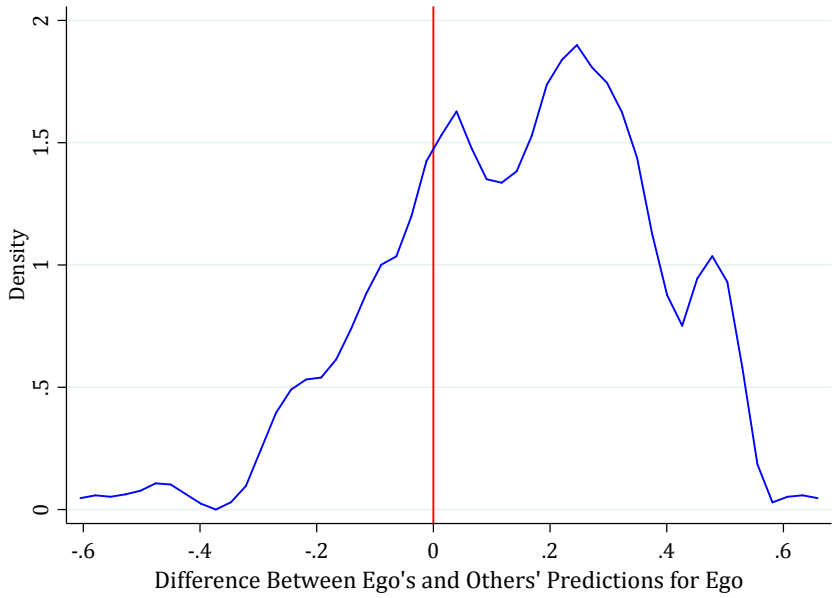


Note: Cost per marginal quitter refers to additional quitting in the intervention group compared to the control group. Our team commitment intervention is displayed above the dotted line. The effect size is based on the average marginal effects from logistic regressions (or the treatment-on-the-treated effect, in the case of the Philippines intervention). Markers are weighted by sample size. See Table 4 for details on calculations and data sources.

Figure 4: Distribution of baseline predictions about quitting



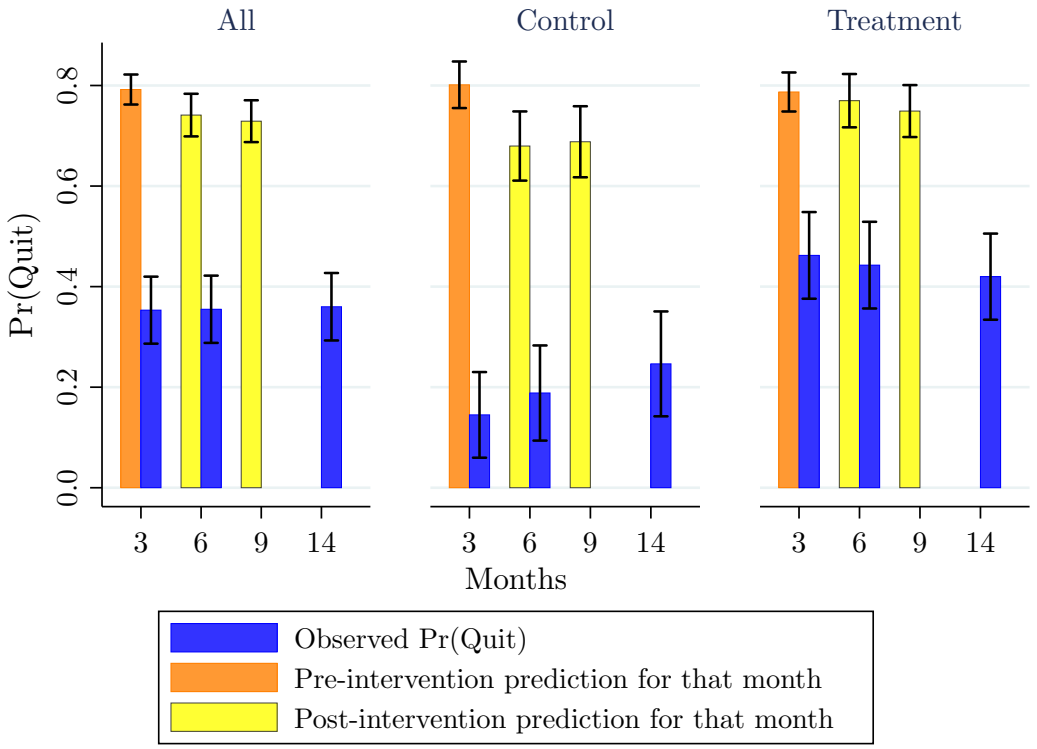
(a) Ego's, friends', and acquaintances' predictions for ego



(b) Difference between ego's self-predictions and others' mean predictions for ego

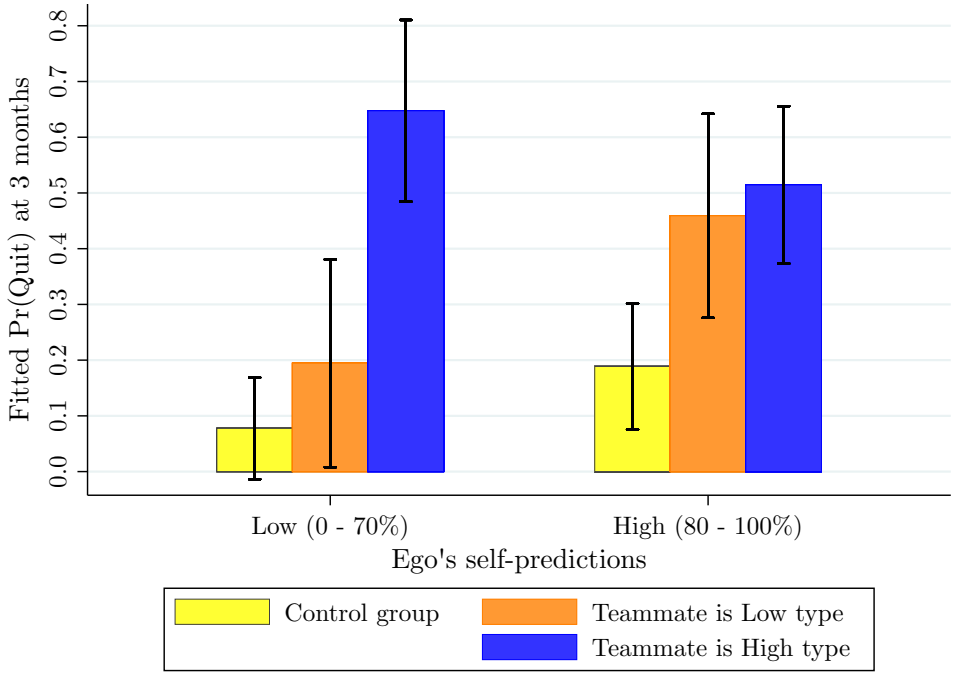
Note: Baseline predictions of the probability that the person will not be smoking in 3 months. "Ego" refers to the index person. Friends refer to the five closest social ties from the same village who enrolled in the trial. The distributions are kernel densities from an Epanechnikov function with optimal bandwidth.

Figure 5: Predicted vs. observed smoking abstinence, by treatment status and month

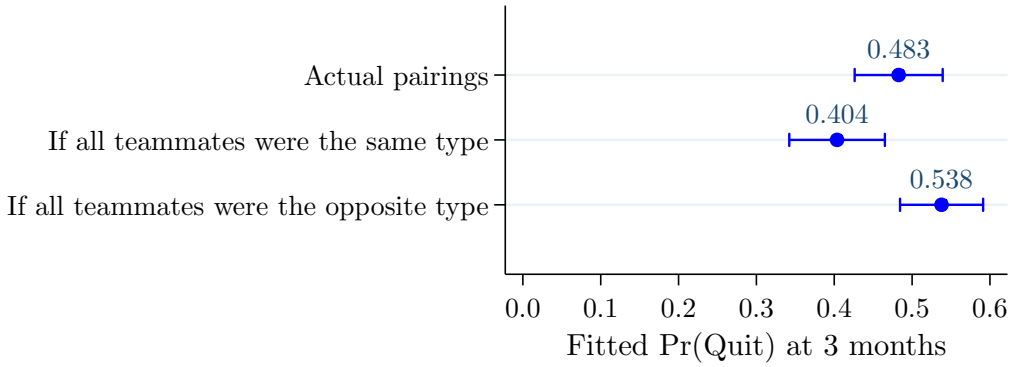


Note: Error bars represent a 95% confidence interval. Predictions of the probability that a person will not be smoking in 3 months were elicited at baseline, 3 months, and 6 months. This figure plots predictions at their target month (e.g., at 3 months for baseline predictions). The horizontal axis is not drawn to scale.

Figure 6: Own and teammate's self-predictions and actual quitting at 3 months
(Randomly formed teams in the treatment group)



(a) Effect of own and teammate's self-predictions on fitted Pr(Quit) at 3 months



(b) Average fitted Pr(Quit), by scenario

Note: Sample restricted to randomly formed, treated teams ($n = 116$). Self-predictions for quitting are dichotomized at the median into low ($0 - 70\%$) and high ($80 - 100\%$). Fitted probabilities are based on a logit model of quitting at 3 months, controlling for all baseline covariates listed in Table 2, subdistrict, and smoking cessation counselor and quadratic terms for age, income, and cigarettes smoked per day. Error bars represent the 95% confidence interval, clustering standard errors at the team level. Same-type pairings are teams in which both teammates are low types or both are high types, whereas opposite-type pairings are teams in which one teammate is low type and one is high type.

Table 1: Comparative statics from theoretical model

#	Parameter	Description	Shift in Pr(Self-restraint) if parameter increases
1	ρ^1	A person's self-confidence	+
2	ρ^2	A teammate's self-confidence	+
3	θ	"Correlation" in teammate's type	
		With "good news" (both confident)	+
		With "bad news" (both not confident)	−
4	α	Degree of projection	−
5	β	Degree of self-control	+
6	V	Long-run payoff from abstaining	+
7	H	Health gains from abstaining	+
8	m	Monetary rewards from abstaining	+
9	c	Cost of abstaining	−
10	d	Cost of a lapse	+
11	k	Amount of deposits	+
12	s	Social costs of failure	+

Note: A "+" shift indicates an increase in the probability of self-restraint, and a "−" shift indicates a decrease in the probability of self-restraint. Indenting of the description indicates that the subcategory is a function of the category in which it falls.

Table 2: Balance of baseline characteristics

	Non-participants	Trial participants: All	Trial participants: Control group	Trial participants: Treatment group	t-test of (1) vs. (2) (p-value)	t-test of (3) vs. (4) (p-value)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Socio-demographic characteristics						
Male	0.926	0.872	0.868	0.875	0.001	0.884
	(0.262)	(0.334)	(0.341)	(0.332)		
Age	45.21	51.06	51.07	51.05	< 0.001	0.993
	(15.06)	(13.86)	(14.04)	(13.82)		
Monthly household income,		3.838	3.513	4.011		0.506
in $100s		(4.971)	2.809)	(5.805)		
Education						
0-3 years		0.469	0.485	0.461		0.747
		(0.500)	(0.503)	(0.500)		
4-6 years		0.260	0.324	0.227		0.142
		(0.440)	(0.471)	(0.420)		
7+ years		0.270	0.191	0.313		0.069
		(0.445)	(0.396)	(0.465)		
Currently married		0.791	0.794	0.789		0.934
		(0.408)	(0.407)	(0.410)		
Buddhist vs. Muslim		0.689	0.691	0.688		0.958
		(0.464)	(0.465)	(0.465)		
Works in agriculture		0.633	0.603	0.648		0.532
		(0.483)	(0.493)	(0.479)		
Self-rated health is good to		0.296	0.324	0.281		0.539
excellent vs. fair to poor		(0.458)	(0.471)	(0.451)		
Panel B. Smoking characteristics						
Average cigs. smoked per day	13.86	12.79	14.24	12.02	0.077	0.132
	(7.41)	(9.79)	(11.15)	(8.93)		
Type of tobacco used						
Manufactured cigs. only	0.301	0.301	0.294	0.305	0.634	0.878
	(0.459)	(0.460)	(0.459)	(0.462)		
Handrolled cigs. only	0.585	0.480	0.485	0.477	0.010	0.908
	(0.459)	(0.460)	(0.459)	(0.462)		
Both handrolled and	0.114	0.219	0.221	0.219	< 0.001	0.977
manufactured cigs.	(0.317)	(0.415)	(0.418)	(0.415)		
Number of past quit attempts		2.676	2.824	2.598		0.582
		(2.728)	(2.938)	(2.617)		
Number of years since	20.49	31.31	31.93	30.98	< 0.001	0.674
initiated smoking	(13.28)	(14.87)	(14.47)	(15.12)		
Prediction of Pr(Quit) in		0.796	0.799	0.795		0.918
3 months		(0.208)	(0.193)	(0.217)		

Continued on next page

Table 2 – *Continued from previous page*

	Non-participants	Trial participants: All	Trial participants: Control group	Trial participants: Treatment group	t-test of (1) vs. (2) (p-value)	t-test of (3) vs. (4) (p-value)
	(1)	(2)	(3)	(4)	(5)	(6)
Planning to quit smoking within 6 months vs. not	0.196	0.821	0.853	0.805	< 0.001	0.404
	(0.397)	(0.384)	(0.357)	(0.398)		
Belief that quitting is very important to me vs. not		0.765	0.735	0.781		0.472
		(0.425)	(0.444)	(0.415)		
Number of other adult smokers in the household		0.658	0.632	0.672		0.800
		(1.033)	(1.196)	(0.940)		
All of person's 5 best friends are smokers vs. not		0.515	0.574	0.484		0.237
		(0.501)	(0.498)	(0.502)		
Panel C. Trial characteristics						
Preselected teammate vs. randomly assigned		0.158	0.265	0.102		0.003
		(0.366)	(0.444)	(0.303)		
Number of observations	1145	196	128	68		

Note: Mean and standard deviation (in parentheses) of each variable are reported. Only a subset of variables were collected in the census for non-participants, i.e., those smokers living in the study area who did not enroll in the trial.

Table 3: Average treatment effects at 3, 6, and 14 months

	Biochemically verified		Self-reported
	Abstinence at 3 months	Abstinence at 6 months	Abstinence at 14 months
	(1)	(2)	(3)
Treatment	0.281***	0.201***	0.132*
	(0.058)	(0.056)	(0.068)
Control variables	Yes	Yes	Yes
Number of participants	197	196	196
Number of teams	120	120	120
Mean of dependent variable	0.147	0.191	0.250
Pseudo-R^2	0.29	0.32	0.21

Note: Average marginal effects are calculated from logit models, controlling for all baseline variables listed in Table 2, as well as subdistrict, cessation counselor, and quadratic terms for age, income, and cigarettes smoked per day. Robust standard errors, clustered at the team level, are given in parentheses. Smoking abstinence is defined as the 7-day point prevalence. Statistical significance: * 0.10 ** 0.05 *** 0.01.

Table 4: Assumptions for analysis of cost per marginal quitter of smoking cessation interventions

	Cost per recipient in Thailand		Effectiveness				
Intervention	Cost ($)	Notes/sources	Scenario	Quits in control	Quits in interv'n	Effect size (95% CI)	Notes/sources
Team commitment (actual)	71	Includes team bonus, matching contributions, forfeited deposits, personnel (field coordinator, counselors, deposit collectors), urine test supplies, transport, text messages, and office supplies.	Thailand	13/69 (18.8%)	58/131 (44.3%)	0.254 (0.127–0.381)	Urine test-confirmed 7-day abstinence at 6 months. Unadjusted effect clustered by team.
Team commitment (feasible)	50	Same as above, except pay deposit collectors piece rate, hire field coordinator full-time for 2 months instead of 3, buy test strips locally.	Thailand	13/69 (18.8%)	58/131 (44.3%)	0.254 (0.127–0.381)	Urine test-confirmed 7-day abstinence at 6 months. Unadjusted effect clustered by team.
Basic commitment contract	218	Author's calculation based on reported cost per quitter and point prevalence of abstinence (Giné et al., 2010).	Philippines	55/616 (8.9%)	Account users: 29/83 (34.9%)	0.312 (0.00036–0.623)	Urine test-confirmed point prevalence at 12 months. Effect size is treatment-on-the-treated effect (Giné et al., 2009, 2010).
Nicotine gum	365	12-week course of Nicomild, a low-, cost provider of nicotine gum in Thailand. Unit price of $1.50 (THB 45) per 9-piece pack, as reported on Nicomild Web site.	Thailand	2/21 (9.5%)	10/20 (50.0%)	0.373 (0.184–0.563)	CO-confirmed point prevalence at 24 weeks (Rungruang. et al., 2008).
			Multi-nat'l meta-analysis	37/436 (8.5%)	101/454 (22.2%)	0.142 (0.067–0.217)	Point prevalence for nicotine gum at 6+ mos., 7 studies (Stead et al., 2008). SEs clustered by study.
Varenicline	835	12-week course of varenicline, marketed in Thailand as Champix Unit price of $2 (THB 60) per day, as reported by the Clear Skies smoking cessation clinic located in the study area.	Thailand, China, Singapore	63/165 (38.2%)	42/168 (25.0%)	0.130 (0.034–0.227)	CO-confirmed continuous abstinence at 24 weeks (Wang et al., 2009).
			Multi-nat'l meta-analysis	331/2754 (12.0%)	954/3412 (28.0%)	0.166 (0.123–0.209)	Continuous abstinence at 24+ weeks, 14 studies (Cahill et al., 2012). SEs clustered by study. Point prevalence not available.

Note: Costs are adjusted for the purchasing power parity exchange rate of THB 17.09 to $1 in 2010, based on data from the World Bank. Effect sizes are reported as average marginal effects based on logistic regressions, except as noted for the basic commitment contract.

Table 5: Change in quit predictions over time

	(1)	(2)	(3)	(4)
Post-intervention time dummy	-0.066***		-0.118***	
	(0.021)		(0.034)	
Time dummies				
0 months (ref)				
3 months		-0.061**		-0.125***
		(0.024)		(0.037)
6 months		-0.070***		-0.112***
		(0.024)		(0.039)
		[0.662]		[0.722]
Treatment	0.029	0.029	-0.020	-0.020
	(0.027)	(0.027)	(0.032)	(0.032)
Post × Treatment			0.079*	
			(0.043)	
3 months × Treatment				0.095**
				(0.048)
6 months × Treatment				0.064
				(0.049)
				[0.483]
Constant	1.106***	1.106***	1.140***	1.139***
	(0.189)	(0.190)	(0.187)	(0.188)
Number of person-months	540	540	540	540
Number of participants	197	197	197	197
R^2	0.26	0.26	0.26	0.27

Note: Coefficients are derived from OLS models of self-predictions of the probability a person will not be smoking in 3 months, controlling for all covariates listed in Table 2, subdistrict, cessation counselor, and quadratic terms for age, income, and cigarettes smoked per day. Predictions of the probability that a person will not be smoking in 3 months were elicited at baseline, 3 months, and 6 months. Robust standard errors, clustered at the individual level, are in parentheses. The p-value from a post-estimation Wald test of equality between the 3-month and 6-month coefficients is in brackets. Statistical significance: * 0.10 ** 0.05 *** 0.01.

Table 6: Effect of social ties of teammates on quit status at 3 months

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pre-selected teammate	-0.120						
	(0.151)						
Distance between teammates' houses (km)		0.005					
		(0.029)					
Pre-trial relationship with teammate							
Acquaintances/strangers (ref)							
Close friends			-0.056				
			(0.127)				
Relatives			-0.072				
			(0.130)				
Teammates talked at least weekly pre-trial				0.128			
				(0.095)			
Teammates is closest friend in trial					0.213**		
					(0.109)		
Teammates is one of 2 closest friends in trial						0.167	
						(0.105)	
Teammates is one of 5 closest friends in trial							0.228**
							(0.105)
Number of participants	132	116	108	104	118	118	118
Number of teams	66	58	54	58	59	59	59
Log likelihood	-90.8	-80.3	-74.6	-71.1	-79.6	-80.1	-78.7

Note: Coefficients are expressed as average marginal effects, based on logit models, using robust standard errors clustered at the team level. Models 2 to 8 restrict the sample to randomly assigned treated teams. Statistical significance: * 0.10 ** 0.05 *** 0.01.

Table 7: Effect of teammate's quit status on ego's quit status at 3 months
(Randomly formed teams in the treatment group)

	Ego's quit status (Reduced form)		Teammate's quit status (First stage)		Ego's quit status (Second stage)		
	OLS	Probit	OLS	Probit	2SLS	Bivariate Probit	Probit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Teammate's quit status					0.536*	0.392***	0.177
					(0.279)	(0.123)	(0.118)
Mean predictions of others for teammate	0.628*	0.584*	1.172***	1.206***			
	(0.327)	(0.314)	(0.344)	(0.336)			
Ego's self-predictions	0.470**	0.467**	0.292	0.303	0.314	0.251	0.399**
	(0.185)	(0.186)	(0.209)	(0.196)	(0.234)	(0.206)	(0.193)
Constant	-0.297		-0.584		0.017		
	(0.359)		(0.362)		(0.292)		
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of participants	117	117	117	117	117	117	117
Number of teams	59	59	59	59	59	59	59
(Pseudo-)R^2	0.20	0.16	0.24	0.20	0.09		0.33
F statistic of instrument			11.6	8.7			

Note: The sample is restricted to *randomly formed* treated teams. All coefficients are reported as average marginal effects, along with robust standard errors clustered at the team level. All models control for sex, age, income, cigarettes per day, and type of tobacco. The two-stage least squares (2SLS) and bivariate probit models in Columns 5 and 6 instrument for teammate's quit status at 3 months using all participants' mean quit predictions for the teammate at baseline, excluding the predictions of the index person and the teammate him/herself. Model 6 includes bootstrapped standard errors. Model 7 is the naïve estimator. Statistical significance: * 0.10 ** 0.05 *** 0.01.

Table 8: Predicted and observed quitting at 3 months
(Randomly formed teams in the treatment group)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Ego's self-predictions	0.158	0.174	0.165	-0.109	-0.083	0.833**	-0.018	0.235	
	(0.218)	(0.214)	(0.214)	(0.231)	(0.229)	(0.324)	(0.254)	(0.255)	
Teammate's self-predictions		0.449**	0.337**		0.491***		0.316*		
		(0.182)	(0.170)		(0.175)		(0.175)		
Ego's predictions × Teammate's predictions			-1.947						
			(1.821)						
Others' mean predictions for ego				0.942***	1.066***				
				(0.287)	(0.286)				
Overconfidence (= Ego − Others' predictions)						-0.942***			
						(0.287)			
Teammate's predictions for ego							0.358**		
							(0.181)		
Ego's predictions for teammate								0.329	
								(0.248)	
Team type, based on self-predictions									
Ego low, teammate low (ref)									
Ego low, teammate high									0.458***
									(0.119)
Ego high, teammate low									0.263*
									(0.121)
Ego high, teammate high									0.318***
									(0.110)
Number of participants	116	116	116	112	112	112	113	102	116
Number of teams	59	59	59	59	59	59	65	59	59
Pseudo-R^2	0.25	0.29	0.28	0.32	0.36	0.32	0.35	0.28	0.32
Log likelihood	-59.9	-56.8	-57.8	-53.1	-49.6	-53.1	-50.8	-51.0	-54.7

Note: The sample is restricted to randomly formed, treated teams. Coefficients are expressed as average marginal effects, calculated from logit models of quitting at 3 months, controlling for all covariates listed in Table 2, subdistrict, cessation counselor, and quadratic terms for age, income, and cigarettes smoked per day. Robust standard errors, clustered at the team level, are in parentheses. Model 3 includes an interaction between ego's and teammate's self-predictions. Team type in Model 9 is based on each teammate's self-predictions, dichotomized as low (0-70%) and high (80-100%). Statistical significance: * 0.10 ** 0.05 *** 0.01.

Appendix A Elaboration of the Theoretical Model

A.1 Model Setup

We follow the general setup of Battaglini, Bénabou and Tirole (2005), hereafter BBT (Figure A.1). We also embed peer pressure, financial commitment, and a projection bias parameter in the BBT model in order to expand the set of model predictions. Imagine a two-period game, $t = 1, 2$, each with two subperiods.[43] A present self and a future self decide consumption of an addictive good at t_1 and t_2, respectively. In the first subperiod, the agent decides whether or not to exert self-control over the behavior, say smoking. Choosing to smoke (i.e., no willpower, NW) delivers an immediate payoff a, whereas exercising willpower (W) delivers no immediate payoff. In the second subperiod, the decision maker lapses (R) or abstains from smoking (A). Abstaining has an immediate psychic and physical cost $c > 0$ from nicotine cravings and withdrawal symptoms, and delivers a delayed benefit $V = V(H, m)$ that is a function of the health gains (H) and monetary rewards (m) contingent on quitting. During an unassisted quit attempt, giving up in the second subperiod entails no cost ($d = 0$), whereas $d > 0$ in the presence of social sanctions (s) or forfeited deposits from a commitment contract (k), both of which are discounted to the present. A lapse yields a delayed benefit b such that $a < b < V$. We follow BBT and assume that $b > a$, implying that some restraint has value as a signal to oneself and to others about the degree of self-control one possesses. Self-signaling restraint can induce a future self to show additional restraint.

The model incorporates behavioral parameters for present bias and projection bias. A hyperbolic discounting parameter $\beta \in [0, 1]$ captures the agent's present bias.[44] For a time-consistent smoker, $\beta = 1$. The present-biased smoker places undue emphasis on satisfying an immediate urge in the first subperiod relative to *ex ante* preferences and similarly discounts the future benefits of quitting too heavily in the second subperiod because the cravings and withdrawal are particularly salient ($\beta < 1$).[45] Following Loewenstein, O'Donoghue and Rabin (2003), we also add to the model a projection bias parameter $\alpha \in [0, 1]$ that represents the degree to which agents project their current preferences on predictions of future utility. In so doing, projection-biased smokers ($\alpha < 1$) under-value the benefits V they will reap from abstaining, down-weighting them by a factor of $1 - \alpha$.[46] For a smoker without projection bias, $\alpha = 1$.

Two main features of the BBT model are: 1) state-contingent present bias and 2) imperfect self-knowledge about one's degree of present bias. Degree of self-control is represented as $\beta \in \{\beta_L, \beta_H\}$, where β_L implies weak self-control and β_H strong self-control.[47]

[43] The dynamic setup enables agents to generate concerns for self-reputation and thus gives rise to informational externalities from teammates.

[44] Building on the work of Strotz (1955), Pollak (1968), and others, the β-δ model generates preference reversals by embedding in the standard utility function an additional discount factor β on utility earned in future time periods (Laibson, 1997). Hyperbolic discounting is also an empirical regularity (Ainslie, 1992).

[45] In principle, the self-control parameter could differ in each subperiod (Bénabou and Tirole, 2004). Because our main concern is the choice at the decision node between A and R we assume without loss of generality that β is fixed over time.

[46] Predicted future benefits are a weighted sum of current and future tastes: $\hat{V}_1 = \alpha V_0 + (1 - \alpha)V_1$. We normalize $V_0 = 0$.

[47] Bénabou and Tirole (2004) and Duflo, Kremer and Robinson (2011) follow a similar approach.

Smokers do not know their type at the start of Period 1; rather, they have common priors ρ and $1-\rho$ on β_H and β_L. These beliefs may be interpreted in several ways. They correspond roughly to predicted self-control, $\hat{\beta}$, in the β-δ model (O'Donoghue and Rabin, 1999). As $\hat{\beta} \to \beta$, an agent is more aware of her time-inconsistency. We later use the time path of these predictions to discern a person's degree of naïveté with respect to present bias. More generally, the priors may be interpreted as *self-efficacy beliefs* about quitting smoking. Self-efficacy refers to self-confidence in one's abilities to undertake a set of actions (Bandura, 1998).[48]

We first consider equilibrium in the absence of external incentives ($d = 0$) and in Section 3.1 discuss the implications for our intervention when $d > 0$. In Period 1, abstaining is a dominant strategy for a strong-willed person (β_H), whereas a weak type (β_L) prefers not to exercise self-control in the absence of reputational concerns (i.e., if current behavior will not influence future decisions):

$$(1-\alpha)V - \frac{c}{\beta_L} < b - d < (1-\alpha)V - \frac{c}{\beta_H} \tag{7}$$

The exposition below concentrates on the decisions of weak-willed agents, whose choices depend on self-reputation and social spillovers. The maximum value of self-reputation is the discounted difference between choosing no self-control (NW) and choosing self-control but lapsing (Bénabou and Tirole, 2004), as seen in Equation 8. A weak type resists temptation (chooses A) in Period 1 if:

$$(1-\alpha)V - \frac{c}{\beta_L} + \delta(b-a) > b - d \tag{8}$$

In other words, the person shows restraint when the benefits from abstaining, including from self-signaling, eclipse the craving costs.

At the start of Period 2, the smoker displays self-control only if sufficiently confident that her future self will resist temptation. Otherwise, the craving costs are not worth enduring. Let ρ' denote the person's updated prior in Period 2. *Ex post* the weak type, who is tempted to light up, chooses W if:

$$\rho'[(1-\alpha)V - c] + (1-\rho')(b-d) > \frac{a}{\beta_L} \tag{9}$$

Equation 9 implies a threshold condition for the level of self-confidence needed to choose W:

Alternatively, BBT specify that agents differ in the severity of their cravings and withdrawal, such that $c \in \{c_L, c_H\}$. We adopt the former approach, given that commitment contracts are hypothesized to relate to short-term time preferences. In contrast, pharmacological aids, such as nicotine replacement therapy, act by reducing craving costs c.

[48] Self-efficacy is a more appropriate construct in this context than is self-esteem, which implies a person's overall sense of self-worth. Self-efficacy beliefs regulate motivation for completing a task by determining the goals people set for themselves and the strength of commitment and effort exerted to attain those goals (Bandura, 1998). We use self-confidence synonymously with self-efficacy.

$\rho' > \rho^\star$, where $\rho^\star$ is defined as:

$$\rho^*[(1-\alpha)V - c] + (1-\rho^*)(b-d) \equiv \frac{a}{\beta_L} \tag{10}$$

At the point of indifference between W and NW, the payoff from lighting up is balanced by the expected utility from attempting to exert self-control.

A.2 Equilibrium Self-Restraint

BBT characterize the equilibrium strategy for the subgame where the decision node between A and R has been reached in Period 1, using a perfect Bayesian equilibrium as the solution concept.[49] The outcome of this subgame determines the success of any quit attempt.

BBT adopt a single-agent benchmark for assessing equilibrium behavior. Let $x_s(\rho)$ represent the strategy of a single agent. In equilibrium, a strong-willed smoker always abstains in Period 1 (Equation 7). A weak-willed smoker abstains with probability 1 only if $\rho \geq \rho^*$. For lower levels of self-confidence such that $\rho < \rho^*$, "the weak type's probability of pooling [with the strong type] must be low enough that observing [perseverance] is sufficiently good news to raise Self 2's posterior from ρ to ρ^*, where he is willing to randomize between W and NW" (Battaglini, Bénabou and Tirole, 2005). BBT call this condition the *informativeness constraint*, $\Pr_{x,\rho}(\beta = \beta_H|A) = \rho^*$. It uniquely defines the equilibrium strategy for the weak single agent as an increasing function $x_s(\rho)$, shown in Figure A.2. The probability of abstaining in Period 1 increases with self-confidence, starting at the origin and reaching one at $\rho = \rho^*$.

Turning to the two-agent case, the equilibrium outcome depends on expectations for a teammate's self-control and the similarity in degree of self-control between teammates. Agents rely on observing the smoking decisions and display of self-control from teammates in order to learn about their own ability to quit. The extent to which a person learns from others depends on how relevant she views the display of self-control of those around her. A setting with homogeneous pairings provides the key testable predictions for our study.[50] Let members $i \in \{1, 2\}$ of dyad j have the same confidence level in their own self-control, $\rho^1 = \rho^2 = \rho$, and undertake the same strategy, $x^1 = x^2 = x$. Let $\theta \in [0, 1]$ be the degree of informativeness of a teammate's self-control, where $\theta = 0$ implies that a teammate's self-control is independent of the index person's beliefs and $\theta = 1$ implies that the teammate's self-control fully determines the index person's beliefs. BBT define θ as part

[49] PBE is appropriate for cases in which an agent is one of several types (e.g., strong-willed and weak-willed) and information about type is incomplete.

[50] BBT extend the model to the case of heterogenous pairs and find qualitatively similar results, with somewhat richer predictions that we are under-powered to test. A person's *ex ante* welfare is hump-shaped with respect to her teammate's probability of exercising self-restraint in Period 2. A person maximizes *ex ante* welfare when paired with a teammate who has a slightly worse self-control problem than one's own, making his successes more encouraging and his failures less discouraging.

of the conditional probabilities of being a strong or weak type:

$$\pi_{HH} \equiv \Pr(\beta' = \beta_H | \beta = \beta_H) = \rho + \theta(1-\rho) \tag{11}$$
$$\pi_{LL} \equiv \Pr(\beta' = \beta_L | \beta = \beta_L) = \theta\rho + (1-\rho)$$

We can denote $\mu_{AR}(x;\rho,\theta)$ as the posterior probability that Agent 1 is a strong type, given that she chose A in the first period but her teammate, Agent 2, chose R and that weak types play A with probability x. Let $\mu_{AA}(x;\rho,\theta)$ be the posterior that both played A in the first period. The event AA is the "good news" state where the agent observes her teammate displaying self-control, and the event AR is the "bad news" state where the agent observes her teammate succumbing to cravings. BBT show that in equilibrium, the following equation holds:

$$x_{AR}(\rho;\theta) \leq x \leq x_{AA}(\rho;\theta), \tag{12}$$

where

$$x_{AA}(\rho;\theta) \equiv \max\{x \in [0,1] | \mu_{AA}(\rho;\theta) \geq \rho^*\}, \tag{13}$$
$$x_{AR}(\rho;\theta) \equiv \min\{x \in [0,1] | \mu_{AR}(\rho;\theta) \leq \rho^*\}$$

Equation 12 says that a person whose teammate lapses has a weakly lower probability of self-restraint than a person whose teammate abstains. This condition defines two curves in Figure A.2, a shift up of the single-agent curve in the good news state to $x_{AA}(\rho;\theta)$ and a shift down of the single-agent curve in the bad news state to $x_{AR}(\rho;\theta)$. Intuitively, bad news (teammate plays R) reduces a person's reputational gain from playing A, a *discouragement effect* that lowers the person's probability of abstaining. Good news (teammate plays A) does the reverse, leading to an *encouragement effect* that increases a person's probability of abstaining. Both equilibria exist for an intermediate range of values $x_I(\rho;\theta)$, characterized in equilibrium as a downward-sloping curve. As θ increases, x_{AR} pivots down and x_{AA} pivots up. In other words, as a teammate's actions become more informative, the probability of self-restraint improves with good news and deteriorates with bad news.

BBT formalize the equilibrium self-restraint as follows:

Proposition 1. *The set of equilibria is fully characterized by two threshold functions* $\rho_1(\theta) : [0,1] \to [0,\rho^*]$ *and* $\rho_2(\theta) : [0,1] \to [0,\rho^*/(1-\theta)]$ *such that:*

(i) *For* $\rho < \rho_1(\theta)$ *there is a unique equilibrium of the "bad news" type:* $x = x_{AR}(\rho : \theta)$.

(ii) *For* $\rho > \rho_2(\theta)$ *there is a unique equilibrium of the "good news" type:* $x = x_{RR}(\rho : \theta)$.

(iii) *For* $\rho \in [\rho_1(\theta), \rho_2(\theta)]$ *there are three equilibria:* $x_{AR}(\rho : \theta), x_I(\rho : \theta)$, *and* $x_{AA}(\rho : \theta)$.

Moreover, for any $\theta > 0, \rho_1(\theta) < \rho_2(\theta)$, *but as correlation converges to zero, so does the measure of the set of initial conditions for which there is a multiplicity of equilbiria:* $\lim_{\theta\to\infty} |\rho_2(\theta) - \rho_1(\theta)| = 0$

A.3 Projection Bias

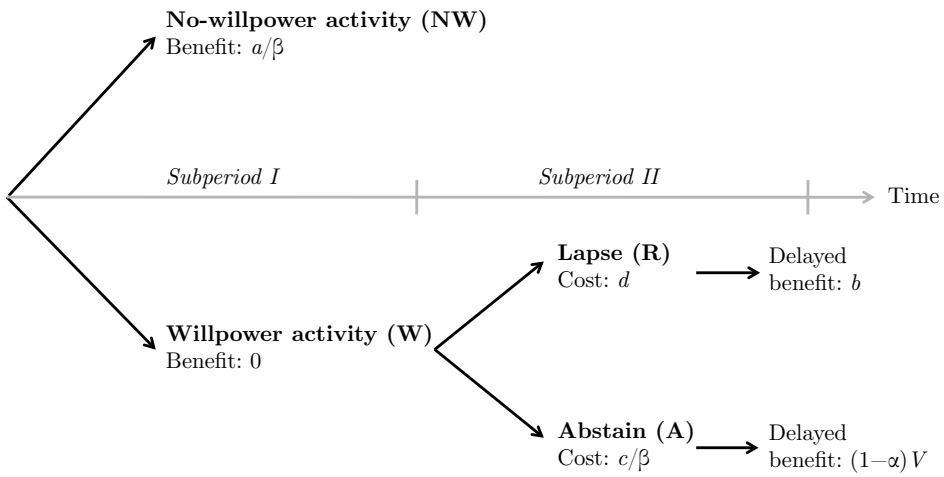
The theoretical model assumes that the returns to quitting are subject to projection bias regarding the benefits of being smoke-free. Our framework provides an opportunity to test

this assertion using a difference-in-differences test developed by Acland and Levy (2011). Let $g = \{0, 1\}$, where 0 corresponds to no intervention and 1 corresponds to team commitment. Further, let $\omega_{t,g}(x; \rho, \alpha, \theta)$ be a weak agent's valuation at time $t \in \{pre, post\}$ of the net expected gains of choosing A. It follows that, if Self 1 plays strategy x and Self 2 plays a pure strategy following AA and RR, then a projection-biased agent's *ex ante* and *ex post* valuations are:

$$\omega_{pre,g}(x; \rho, \alpha, \theta) = (1 - \alpha)V - b - \frac{c}{\beta_L} + \delta[(1 - \theta)\rho + (1 - (1 - \theta)x](b - a) \tag{14}$$
$$\omega_{post,g}(x; \rho, \theta) = V - b - \frac{c}{\beta_L} + \delta[(1 - \theta)\rho + (1 - (1 - \theta)\rho)x](b - a).$$

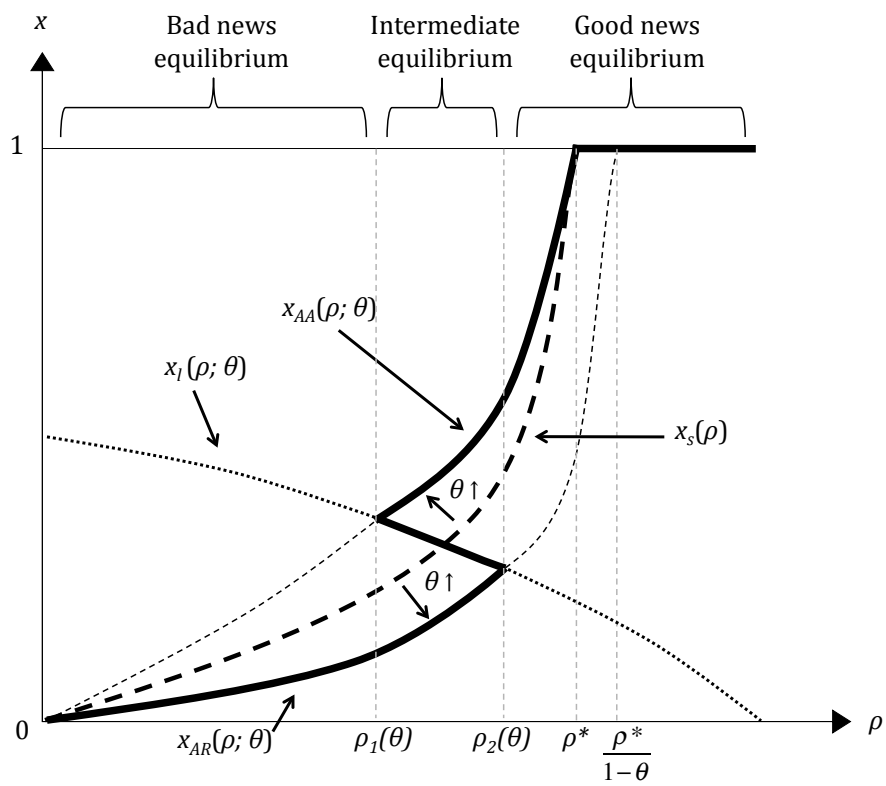
Ex ante a smoker in an addicted state discounts the benefits of being in a smoke-free state by $(1 - \alpha)$ (first equation above), whereas once the benefits V are realized *ex post*, participants value them fully (second equation above). We take advantage of the fact that the intervention exogenously increases the likelihood that treated participants will exit the addicted state relative to control participants, and thus the treatment group will be more likely to accurately perceive the benefits of being smoke-free. In other words, we hypothesize that the difference-in-difference in predictions of the gains to quitting, $(\omega_{post,1} - \omega_{pre,1}) - (\omega_{post,0} - \omega_{pre,0})$, is weakly positive for projection-based agents (Acland and Levy, 2011).

Figure A.1: Decision tree of payoffs for any given period $t = 1, 2$



Note: Adapted from Battaglini, Bénabou and Tirole (2005). Key alterations include the addition of a projection bias parameter and the cost of a lapse d.

Figure A.2: Equilibrium self-restraint in a homogeneous pair



Note: Adapted from Battaglini, Bénabou and Tirole (2005). The upward-sloping dashed line (▬ ▬) denotes the single-agent case; the solid line (▬) denotes the two-agent case.

Appendix B Usage of the Contracts

B.1 Methods

We track the balance of participants' commitment savings accounts in aggregate and by week. During each weekly visit, community health workers recorded the amount deposited, the person's self-reported smoking status, whether the person had talked to her teammate that week, and whether the participant believed that her teammate had smoked that week.

We analyze the relationship between teammates' deposit behavior. In particular, we examine the relationship between their final account balances, the decision to make a deposit based on whether the teammate deposited that week or the week before, and the decision to make a deposit based on whether the participant believed that the teammate had smoked that week. We also look at the effects of randomizing the deadline for the second matching contribution as 1 month or 3 months (no deadline). (See Footnote 25 for the rationale behind the manipulation.)

We run two sets of regressions for the deposit analysis. The first set, run at the person level, looks at the relationship between aggregate deposit patterns (e.g., total number of deposits and total account balance) and smoking abstinence at 3 months. The second set of regressions, run at the person-week level, are based on weekly deposit behavior. For the regressions of weekly smoking abstinence, we regress two outcomes—smoking abstinence that week and the decision to make a deposit that week—on various deposit characteristics. We run three specifications for the regressions of smoking abstinence that week: 1) week dummies and the full set of control variables, 2) week dummies, the controls, and lagged smoking status reported the week before, and 3) week dummies and individual fixed effects. For the regressions of the decision to make a deposit that week, we run regressions with week dummies and controls and regressions with week dummies and individual fixed effects.

B.2 Results

All but two smokers made at least the minimum required deposit in the commitment savings account at enrollment, and 86% deposited more than the required amount, indicating that most participants used their account. Figure B.1a shows the distribution of total deposits at the end of the 10-week deposit period. In total, the median balance was about \$7, roughly 3% of median monthly income and far less than reported in the Philippines CARES study. One possible explanation, which we are unable to test, is that the presence of the team bonus crowded out a person's incentive to deposit. If a person requires a certain stake in the quit attempt in order to succeed, the cash bonus may substitute for the need to commit financially. The substitutability and complementarity of commitment contracts and cash incentives has not been addressed in the literature. Another contributor may be that participants felt less need to deposit because personal tobacco expenditures are low in our setting due to the common use of cheap hand-rolled tobacco. Also, our 3-month deposit period is half the duration of the one in the CARES trial.

The distribution of deposits shifts right for those who quit smoking at the end of the intervention period (Figure B.1a). The median quitter deposited \$10, far greater than \$5.67 for the median continuing smoker ($p < 0.01$). Figure B.1b displays the average weekly

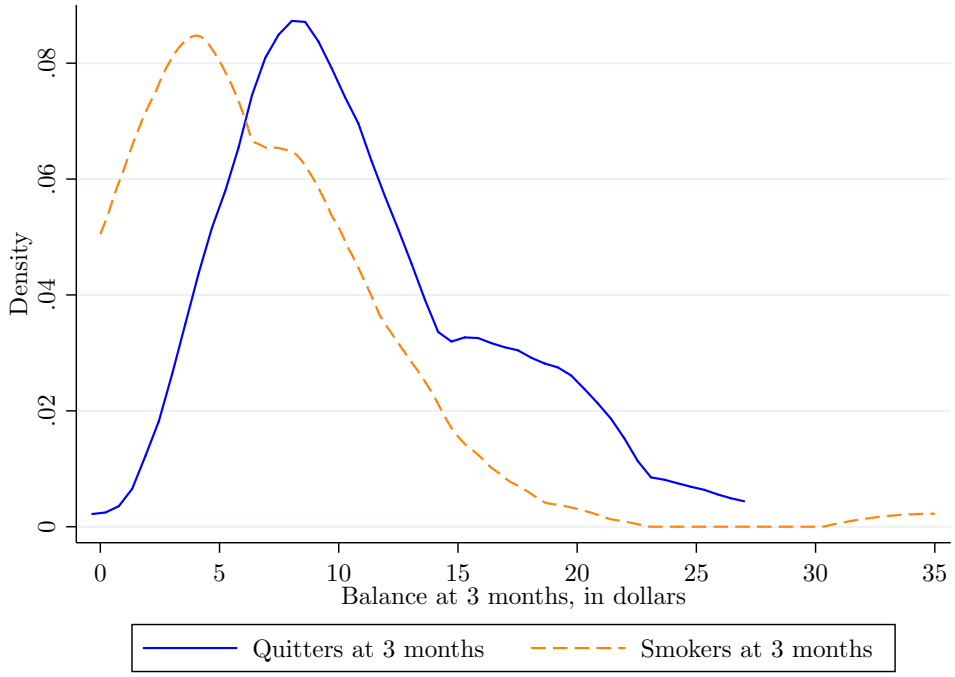
deposits, by 3-month smoking status. Week 0 denotes the week of enrollment. Even at Week 1, a large gap exists between eventual quitters and smokers, and the difference persists throughout the deposit period. A positive relationship between depositing and quitting is consistent with the theoretical model, although causality may be bi-directional. Larger deposits may increase the chance of quitting through greater commitment, or quitting may increase the probability of depositing by reducing uncertainty.

Participants expressed diverging views on the influence of making deposits on their behavior. Some participants stated during the qualitative interviews that the financial commitment was critical to their success: "Depositing money totally changed my thoughts. It always urged me every time when the village health volunteer visited and collected the money." Similarly, another said: "When the project first gave advice about quitting smoking, I did not think much about the money. But when I kept making deposits, I wanted to quit even more." Other participants were skeptical about the role of depositing *per se* on their behavior: "Sometimes I forgot for a while that I needed to quit smoking, but when the collector came, that reminded me that I had to quit smoking. Depositing money did not actually urge me that much. In general, it was just to remind myself."

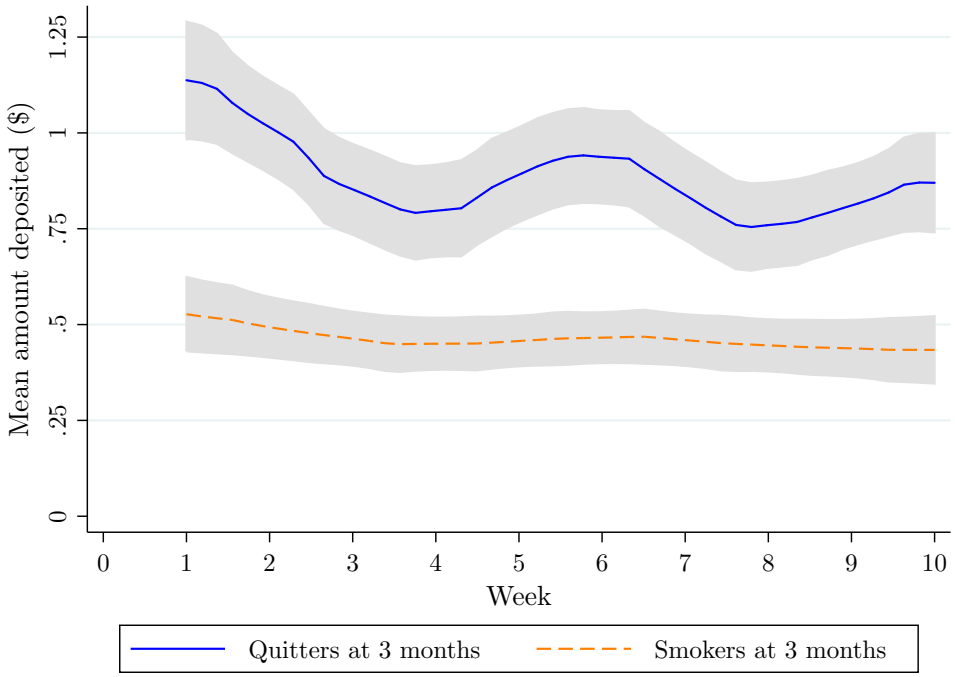
The experimental manipulation involving the time-limited matching contribution was designed to nudge participants toward depositing earlier, increasing their financial commitment and accelerating their quit date. The time-limited group of participants had similar mean deposits and greater modal deposits than the no-deadline group (Figure B.2), even though the latter had two extra months to reach the match trigger. The time-limited group was also 16% points more likely to deposit during the first month of the intervention (Table B.2, Column 4). Both of these fit with the intended design of the manipulation. However, we find that the time-limited group was no more likely to quit smoking at 3 months (Column 1).

Participants' depositing behavior appears to respond to their teammates' actions. If a teammate made a deposit that week, the index person was far more likely to make a deposit (Columns 4 and 5) and more likely to abstain from smoking that week (Columns 1 to 3). If a person believed her teammate had not smoked that week, the person was 7-20% more likely to abstain from smoking that week (Columns 1 to 3).

Figure B.1: Deposits by 3-month smoking status



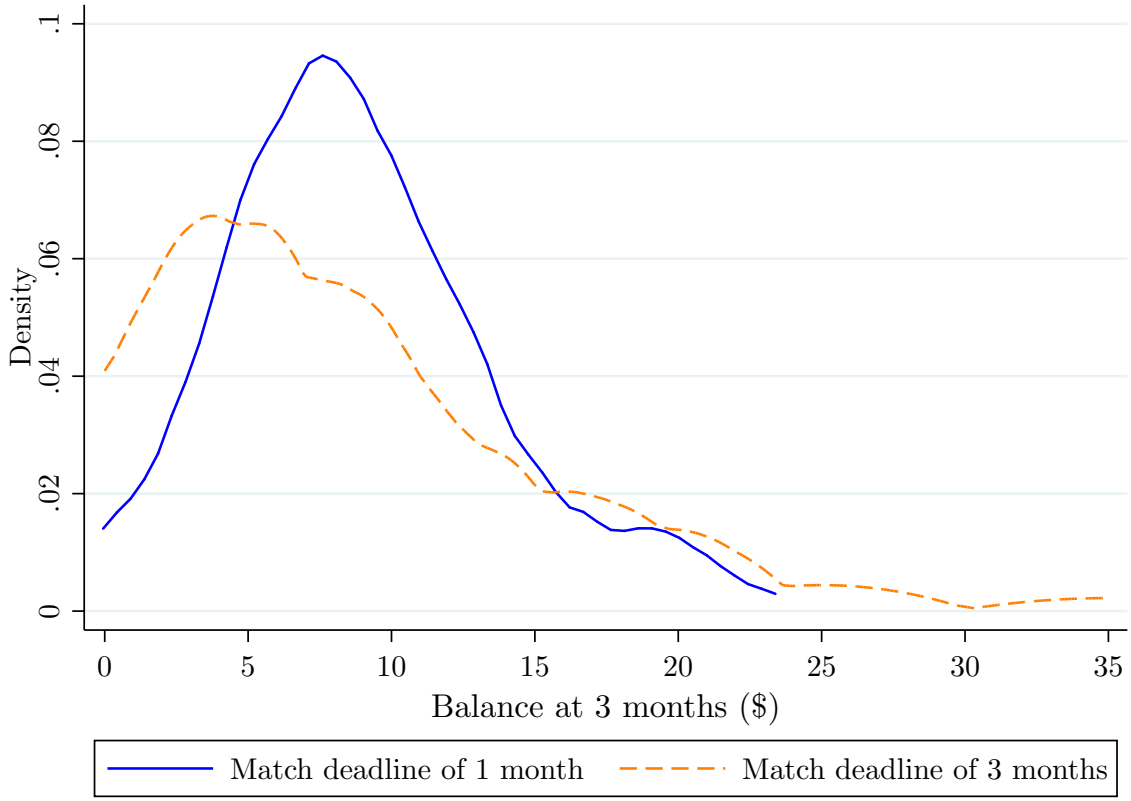
(a) Balance at 3 months



(b) Mean amount deposited per week

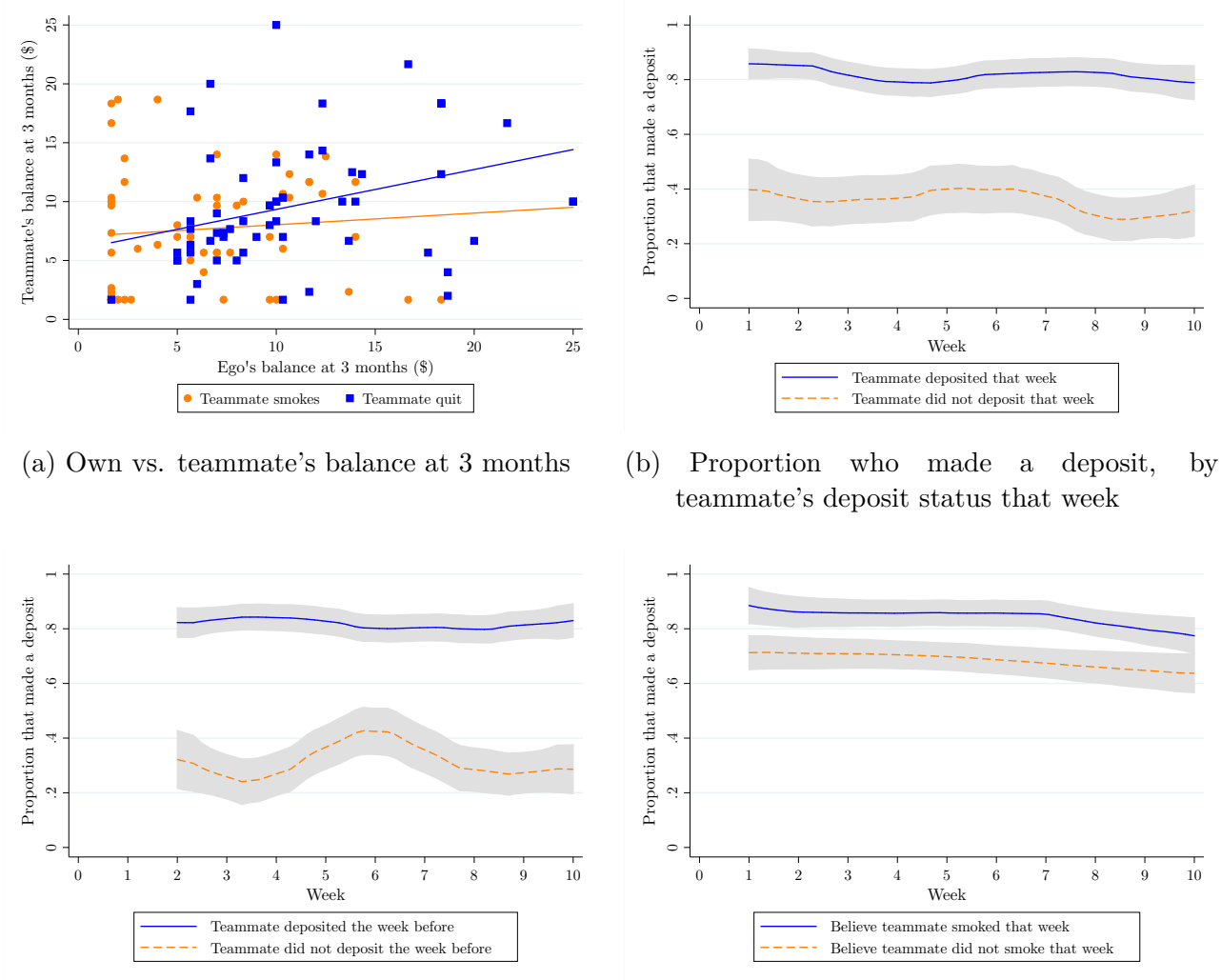
Note: Panel (a) includes kernel densities from an Epanechnikov function and optimal bandwidth of 2.00. Panel (b) is based on a kernel-weighted local polynomial regression using an Epanechnikov kernel and optimal bandwidth of 0.75. The gray bands represent a 95% confidence interval.

Figure B.2: Distribution of account balances at 3 months, by deadline for matching contribution



Note: Based on a kernel density with an Epanechnikov kernel function and optimal bandwidth of 1.72.

Figure B.3: Association between teammates' deposit patterns



(a) Own vs. teammate's balance at 3 months

(b) Proportion who made a deposit, by teammate's deposit status that week

(c) Proportion who made a deposit, by teammate's deposit status the week before

(d) Proportion who made a deposit, by own reports about teammate's smoking status that week

Note: Panels (b) to (d) are based on a kernel-weighted local polynomial regression using an Epanechnikov kernel. Gray bands represent a 95% confidence interval. Panel (d) excludes individuals who are unsure of their teammate's smoking status.

Table B.1: Usage of deposit accounts

	Number of accounts	25th pctile.	Mean	Median	75th pctile.	Standard deviation
Panel A. Balance and deposits						
Opening balance*	132	1.67	1.96	1.67	1.67	1.20
Smokers at 3 months	71	1.67	1.82	1.67	1.67	0.88
Quitters at 3 months	61	1.67	2.12	1.67	1.67	1.49
Total number of deposits	132	4	7.58	10	11	3.98
Smokers at 3 months	71	1	6.41	9	11	4.38
Quitters at 3 months	61	7	8.93	11	11	2.97
Balance at 3 months	132	5.00	8.59	7.33	11.17	5.84
Smokers at 3 months	71	1.67	6.48	5.67	9.67	5.48
Quitters at 3 months	61	7.00	11.05	10.00	13.83	5.31
Panel B. Team bonus						
Proportion who won the team bonus	132	0	0.27	0	1	0.45
Smokers at 3 months	71	0	0	0	0	0
Quitters at 3 months	61	0	0.59	1	1	0.50
Panel C. Matching contribution						
Proportion assigned to 1-month deadline for the match†	132	0	0.52	1	1	0.50
Proportion who qualified for the match‡	132	0	0.55	1	1	0.45
Smokers at 3 months	71	0	0.42	0	1	0.50
Assigned to 1-month match	30	0	0.30	0	1	0.47
Assigned to 3-month match	41	0	0.51	1	1	0.51
Quitters at 3 months	61	0	0.69	1	1	0.47
Assigned to 1-month match	38	0	0.53	1	1	0.51
Assigned to 3-month match	23	0	0.96	1	1	0.21

Note: \$1 $\approx$ 30 Thai baht.

* The minimum opening balance was \$1.67 (50 baht).

† Each treated team was randomly assigned to have a deadline for reaching a \$5 (150 baht) balance of 1 month or 3 months after enrollment.

‡ To qualify for the matching contribution, the participant had to reach a balance of \$5.

Table B.2: Multivariate analysis of depositing

	Ego's quit status			Ego made a deposit	
	(1)	(2)	(3)	(4)	(5)
Panel A. Total deposits					
Number of deposits	0.018*				
	(0.011)				
	[129]				
Account balance at 3 months	0.030***				
	(0.008)				
	[129]				
Assigned to 1-month deadline for the match vs. 3 months	0.095				
	(0.106)				
	[129]				
Qualified for the match (balance of $5+)	0.246***				
	(0.080)				
	[129]				
Panel B. Deposits by week					
Amount deposited that week	0.182***	0.050***	0.016		
	(0.034)	(0.015)	(0.021)		
	[1128]	[916]	[1128]		
Amount deposited the week before	0.113***	0.033***	-0.014		
	(0.029)	(0.010)	(0.016)		
	[1128]	[916]	[1128]		
Made a deposit that week vs. not	0.253***	0.067***	0.093**		
	(0.055)	(0.022)	(0.040)		
	[1128]	[916]	[1128]		
Made a deposit the week before	0.166***	0.057**	-0.013		
	(0.055)	(0.022)	(0.048)		
	[1128]	[916]	[1128]		
(1-month deadline) × **1**(Weeks 1 to 4) †	-0.037	0.011	0.016	0.0162***	
	(0.066)	(0.018)	(0.021)	(0.054)	
	[1128]	[916]	[1128]	[1419]	
Teammate made a deposit that week vs. not	0.111**	0.029*	0.033	0.283***	0.177***
	(0.050)	(0.017)	(0.029)	(0.045)	(0.041)
	[1128]	[916]	[1128]	[1419]	[1419]
Teammate made a deposit the week before vs. not	0.118**	0.021	-0.018	0.264***	0.062*
	(0.049)	(0.020)	(0.036)	(0.046)	(0.035)
	[1128]	[916]	[1128]	[1419]	[1419]

Continued on next page

Table B.2 – *Continued from previous page*

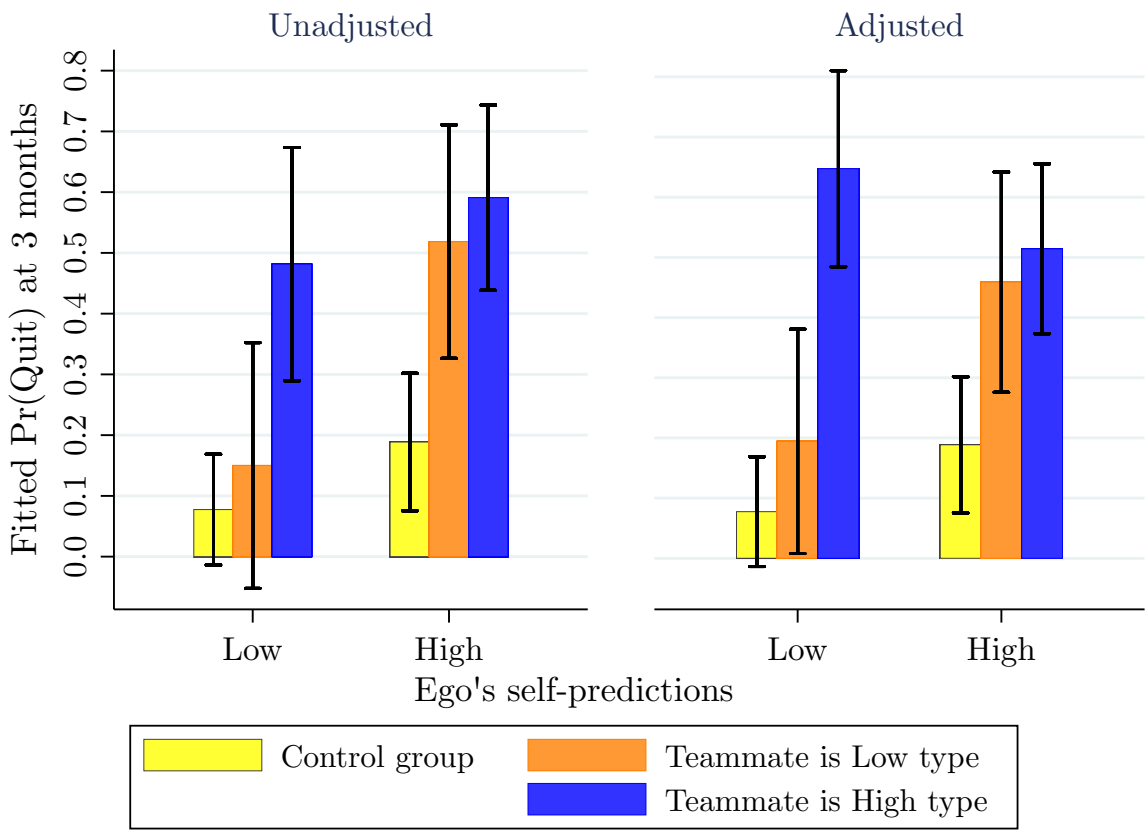
	Ego's quit status			Ego made a deposit	
	(1)	(2)	(3)	(4)	(5)
Ego believes teammate did not smoke that week	0.227***	0.075***	0.195**	0.056	-0.001
	(0.054)	(0.028)	(0.076)	(0.046)	(0.029)
	[999]	[824]	[999]	[1001]	[1001]
Ego believes teammate did not smoke the week before	0.150***	-0.014	0.103*	0.053	-0.006
	(0.050)	(0.018)	(0.055)	(0.048)	(0.033)
	[828]	[826]	[828]	[892]	[892]
Week dummies (Panel B only)	Yes	Yes	Yes	Yes	Yes
Control variables	Yes	Yes		Yes	
Quit status in prior week		Yes			
Individual fixed effects			Yes		Yes

† The first term is the assigned deadline. The second term is an indicator for Weeks 1 to 4.

Note: Each coefficient is drawn from a separate regression. In Panel A, observations are at the person level, and quitting refers to smoking abstinence at 3 months. In Panel B, observations are at the person-week level and quitting refers to abstaining from smoking as reported that week. Models 1, 2, and 4 report average marginal effects calculated from logit models, including our full set of controls. Models 3 and 5 are linear probability models with individual and week fixed effects. Robust SEs, clustered at the team level, are in parentheses. The number of observations from each regression is in brackets. Statistical significance: * 0.10 ** 0.05 *** 0.01.

Appendix C Additional Figures and Tables

Figure C.1: Own and teammate's self-predictions and actual quitting at 3 months
(Randomly formed teams in the treatment group)



Note: Sample restricted to formed assigned, treated teams ($n = 116$). Fitted probabilities are based on a logit model of quitting at 3 months. The adjusted model controls for all covariates listed in Table 2, subdistrict, and smoking cessation counselor and quadratic terms for age, income, and cigarettes smoked per day. Error bars represent the 95% confidence interval, clustering standard errors at the team level.

Table C.1: Average treatment effects at 3, 6, and 14 months
(Full output)

	Biochemically verified		Self-reported
	Abstinence at 3 months	Abstinence at 6 months	Abstinence at 14 months
	(1)	(2)	(3)
Treatment	0.281***	0.201***	0.132*
	(0.058)	(0.056)	(0.068)
Male	0.009	-0.033	0.025
	(0.110)	(0.096)	(0.115)
Age	0.007	0.012**	0.013**
	(0.005)	(0.005)	(0.005)
Monthly household income, in $100s	-0.014	-0.005	0.004
	(0.014)	(0.011)	(0.019)
Education			
0-3 years (ref)			
4-6 years	-0.137	-0.086	0.032
	(0.085)	(0.099)	(0.093)
7+ years	-0.020	0.016	0.032
	(0.098)	(0.096)	(0.095)
Currently married	0.097	0.037	0.072
	(0.070)	(0.073)	(0.081)
Buddhist vs. Muslim	0.097	0.070	0.124
	(0.115)	(0.117)	(0.120)
Works in agriculture	-0.114	-0.067	-0.057
	(0.073)	(0.075)	(0.076)
Self-rated health is good to excellent	-0.019	-0.034	-0.096
	(0.068)	(0.065)	(0.066)
Average cigarettes smoked per day	-0.016***	-0.015***	-0.007*
	(0.004)	(0.004)	(0.004)
Type of tobacco used			
Manufactured cigarettes only (ref)			
Handrolled cigarettes only	-0.016	0.025	-0.026
	(0.073)	(0.077)	(0.086)
Both	0.058	0.127	0.129
	(0.093)	(0.089)	(0.098)
Number of past quit attempts	0.007	-0.005	0.012
	(0.011)	(0.013)	(0.012)
Number of years since initiation	-0.007	-0.011*	-0.007

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Table C.1 – *Continued from previous page*

	Biochemically verified		Self-reported
	Abstinence at 3 months	Abstinence at 6 months	Abstinence at 14 months
	(1)	(2)	(3)
	(0.005)	(0.006)	(0.005)
Prediction of Pr(Quit) in 3 months	0.020	0.025	0.022
	(0.015)	(0.016)	(0.017)
Planning to quit within 6 months	-0.033	0.006	-0.013
	(0.091)	(0.095)	(0.099)
Belief that quitting is very important	0.158*	0.111	0.105
	(0.084)	(0.082)	(0.076)
Number of other adult smokers in HH	0.062**	0.050*	0.045
	(0.030)	(0.026)	(0.030)
All of 5 best friends are smokers	-0.015	0.010	-0.006
	(0.061)	(0.065)	(0.073)
Preselected teammate	-0.220***	-0.270***	-0.143
	(0.080)	(0.067)	(0.118)
Number of participants	197	196	196
Number of teams	120	120	120
Mean of dependent variable	0.147	0.191	0.250
Pseudo-R^2	0.29	0.32	0.21
Log likelihood	-85.8	-87.4	-99.4

Note: Average marginal effects are calculated from logit models, controlling for all variables listed, as well as subdistrict and cessation counselor. The models include quadratic terms for age, income, and cigarettes smoked per day. Robust standard errors, clustered at the team level, are given in parentheses. Smoking abstinence is defined as the 7-day point prevalence. Statistical significance: * 0.10 ** 0.05 *** 0.01.

The Effects of Savings on Risk-Taking and Intertemporal Choice Behavior: Evidence from a Randomized Experiment *

Leandro Carvalho
RAND Corporation

Silvia Prina
Case Western Reserve University

Justin Sydnor
University of Wisconsin

February 2013

Abstract

We investigate whether saving affects risk-taking and intertemporal choices. A field experiment in Nepal randomized access to savings accounts among a population who mostly had never had one before, generating random variation in savings behavior. A year later we administered lottery-choice and intertemporal choice tasks. Our reduced-form results show the treatment group is less risk averse and more willing to delay rewards than the control. Combining the randomized variation with a structural model, we estimate the treatment has an annual discount rate 2 percentage points lower and an intertemporal elasticity of substitution 7% higher. We provide suggestive evidence that the results are driven by changes in preferences rather than wealth effects.

* This research would not have been possible without the outstanding work of Yashodhara Rana who served as our project coordinator. Carvalho thanks the Russell Sage Foundation and the RAND Roybal Center for Finacial Decisionmaking, Prina thanks IPA-Yale University Microsavings and Payments Innovation Initiative and the Weatherhead School of Management, and Sydnor thanks the Wisconsin School of Business for generous research support.

1. Introduction

Individual attitudes toward risk and intertemporal choices are fundamental to savings decisions. But it is also possible that the act of saving and accumulating assets may change these attitudes. Do individuals become more willing to accept financial risks as they accumulate savings? Do those who save become more willing to tradeoff lower consumption in the near term for higher consumption in the future? Answering these questions is important for understanding the overall effects of institutions, policies and programs that are inductive to saving. For example, market failures or institutions that prevent the poor from saving help may give rise to poverty traps if limited opportunities for saving shape one's attitudes toward risk and intertemporal choices. Similarly, if saving feeds back to preferences, historical episodes that further saving could push economies in different directions beyond just the effects of capital accumulation.

It is generally challenging, however, to assess whether increased savings behavior changes attitudes toward risk and intertemporal choices, because whether one saves in the first place is largely determined by one's risk and time preferences. One mechanism by which savings could affect decisions in these domains is through the effect of wealth on the marginal utility of consumption. Despite a rich history of this topic in economics, only a limited number of studies have been able to investigate these potential wealth effects using instruments that generate plausibly exogenous variation in wealth (e.g., Brunnermeir and Nagel, 2008; Paravisini, Rappoport, and Ravina, 2012), and the findings are mixed. More broadly, there is a long history in economics and psychology suggesting that forward-looking behaviors like saving, and access to institutions that enable those activities, could fundamentally alter preferences (e.g., Becker and Mulligan, 1997; Bowles, 1998; Frederick, Loewenstein and O'Donoghue, 2002; Shah, Mullainathan, and Shafir, 2012). There has been little empirical work, however, that can shed light on whether savings behavior alters time and risk preferences.

In this study we exploit a unique field experiment to investigate whether attitudes toward risk and intertemporal choices are affected by the act of saving. Prina (2012) reports the results of the field experiment in Nepal, which randomized 1,236 poor villagers into either a control group or a treatment group that gained access to formal savings accounts. The savings account represented for most of our sample the first access to a formal savings product. More than 80% of the treatment group opened an account, which had neither maintenance nor withdrawal fees and had no minimum balance requirement. They used it actively, making on average 42 deposits and 3 withdrawals over a one-year-period. The experiment generated exogenous variation in access to a savings account and in savings behavior, which, according to the hypotheses discussed above, may have changed the treatment group's attitudes toward risk and their intertemporal choices.

One year after the introduction of the savings accounts we administered to both the control and treatment groups a) an incentivized lottery-choice task typically used to measure risk preferences, b) a hypothetical intertemporal-choice task typical of those used to measure time preferences, and c) an incentivized experimental task based on the Convex Time Budget (CTB) method proposed by Andreoni and Sprenger (forthcoming). In the lottery-choice task subjects were asked to choose their preferred lottery (whose outcome would depend on a coin flip) among a set of options with different levels of risk and expected value. In the first intertemporal-choice task participants were asked to make hypothetical choices between a smaller, sooner monetary reward and a larger, more delayed monetary reward. The adapted CTB task allows us to investigate how treatment and control change their intertemporal allocations in response to changes in the time frame and in the experimental interest rate at which they can exchange sooner experimental rewards for later experimental rewards—see Gine, Goldberg, Silverman and Yang (forthcoming) for an alternative field adaptation of the CTB.

Our reduced-form results show the treatment group is less risk averse and more willing to accept delayed rewards than the control. We find that the treatment group was significantly more likely to choose risk-neutral or risk-loving options than the control group in the experimental lottery-choice task. In the hypothetical intertemporal-choice task the treatment group was significantly more likely than control to choose higher but delayed payments over a range of delay times and delay rewards. In the CTB task overall the treatment group allocated more money to the future than the control, although this difference is not statistically significant. The treatment group was also more responsive than the control group to an increase in the experimental interest rate, implying that within the CTB allocations the treatment group had a higher intertemporal elasticity of substitution. Finally, there is mixed evidence on which group is more responsive to an increase in the length of delay between the sooner and later rewards.

One of the attractive features of Andreoni and Sprenger's (forthcoming) CTB framework is that, if one is willing to make structural assumptions about the utility function (e.g., CRRA utility), it is possible to estimate preference parameters that separately identify present bias, the exponential discount rate and the intertemporal elasticity of substitution (i.e., the curvature of the utility function) for the control and treatment groups. In our baseline specification we estimate that the control group has an annualized discount rate of approximately 26% (annual inflation in Nepal tends to be above 10%).

Our estimates show the treatment group has an annualized discount rate 2 percentage points lower but this difference is not statistically significant. We also estimate that the treatment has an intertemporal elasticity of substitution that is approximately 7% higher than that of the control group, though again this result is not statistically significant. Finally we find no evidence of present bias for either group and

estimate the present-bias coefficient to be precisely 1 for each group. This result is consistent with Augenblick, Niederle and Sprenger (2012) that document that tasks involving choices over monetary rewards may be less suited to capture present bias than tasks involving choices over real-effort-tasks.

An interesting question emerges as to whether the differences in risk attitudes and intertemporal choices we observe between the savings group and the control group are driven primarily by the higher levels of accumulated wealth for the savings group or by a more fundamental change in preferences. That question is particularly relevant for understanding our reduced form effects related to the marginal utility of consumption – namely, our finding of lower risk aversion in the lottery-choice task and greater responsiveness to the interest rate in the CTB task for the treatment group. However, there is a fundamental challenge, both practically and at a deeper conceptual level, to distinguish whether any observed changes in the marginal utility of consumption are driven by wealth differences with a stable preference structure or by changes in preferences. The distinction between these two potential channels hinges crucially on assumptions about both the degree to which individuals incorporate background wealth/consumption when making isolated decisions (i.e., the extent of "narrow bracketing") and about the nature of the utility function.

Nonetheless, we see value in providing suggestive evidence about the potential mechanisms here, because the distinction between wealth effects and preference changes may matter for thinking about the implications of our findings. If the effects of savings are driven primarily by wealth effects, then these effects might also arise from other exogenous shocks to wealth, such as windfalls, inheritance, and fluctuations in asset markets. On the other hand, fundamental preference changes would likely arise primarily due to the *act of saving* and the way that behavior changes thought processes about risk and utility at different times. In that case, other processes that generate exogenous shocks to wealth may not generate the same sort of dynamics we observe here. We discuss these different mechanisms in detail in Section 4 of the paper. A number of observations suggest that the subjects are not fully integrating their background consumption and assets when making decisions in our experimental choice tasks. We also find results in our structural estimation that suggest the narrow bracketing assumption may more sensibly fit the data than models with asset integration. Taken together we feel these patterns are suggestive that exposure to savings accounts may have led to some degree of fundamental preference changes for the treatment group.

Our study contributes to a number of different streams of literature. Our paper joins a growing literature exploring the determinants of time and risk preferences. Becker and Mulligan (1997) develop a a model of endogenous preference formation in which individuals can choose behaviors that affect how they discount the future. They argue, for instance, that financial instruments such as piggy banks may

make individuals more forward-looking by diverting attention toward the future. A number of recent empirical studies (e.g., Guiso, Sapienza, and Zingales 2004, 2008; Nagel and Malmendier, 2011, Shah, Mullainathan, and Shafir, 2012) have looked at whether life experiences affect preferences and beliefs related to time and risk preferences. There is also a small literature that looks at whether time and risk preferences have biological origins (e.g., Sapienza, Zingales, Maestripieri, 2009; Garbarino, Slonim and Sydnor, 2011). Work in psychology has found that differences in time preferences are associated with the ability to envision future situations and that practice at delaying gratification (such as savings behavior) may increase one's ability to exert self-control (Baumeister and Heatherton 1996, Taylor et al. 1998, Strathman et al. 1994, Muraven and Baumeister 2000 and Nenkov et al. 2008). There are also substantial literatures, mostly in finance, that have explored whether variations in wealth affect attitudes toward risk (e.g., Brunnermeir and Nagel, 2008; Paravisini, Rappoport, and Ravina, 2012) and intertemporal choices (Lawrance 1991, Atkeson and Ogaki 1996, Ogaki and Atkeson 1997). Finally, this study adds to a growing literature in development economics that studies how access to financial products shapes the lives of the poor (Aportela, 1999; Banerjee et al., 2011; Bruhn and Love, 2009; Burgess and Pande, 2005; Dupas and Robinson, forthcoming; Kaboski and Townsend, 2005; Karlan and Zinman, 2010a and 2010b). Our study takes a new angle on this by exploring whether access to financial products might have spillovers more generally into how future-oriented and risk averse a person is.

The paper is organized as follows. Section 2 describes the background of the savings experiment conducted by Prina (2012) and outlines the design of our experiment choice tasks. Section 3 presents the reduced form results for each of the 3 different choice tasks. In Section 4 we outline the theoretical framework for our structural estimation, extending the work of Andreoni and Sprenger (forthcoming) to account for the discrete-choice nature of our version of the CTB task. This section contains structural estimates based on the CTB task under a range of assumptions about background consumption and discusses the distinction between the wealth-accumulation and preference-change mechanisms for our results. We conclude the paper in Section 5.

2. Background and Experimental Design

2.1 The Savings Accounts Field Experiment

Formal financial access in Nepal is very limited. Only 20% of Nepalese households have a bank account, according to the nationally representative "Access to Financial Services Survey," conducted in 2006 by the World Bank (Ferrari, Jaffrin, and Shrestha 2007). Not surprisingly, access is concentrated in urban areas and among the wealthy. Thus, most households typically save informally, storing cash at

home, saving in the form of durable goods and livestock, or participating to Rotating Savings and Credit Associations (ROSCAs).

In the randomized field experiment run by Prina (2012), GONESA bank gave access to savings accounts to a random subsample of poor households in 17 slums surrounding Pokhara, Nepal's second largest city. Before the introduction of the savings accounts, a household baseline survey was conducted during May 2010 in the 17 slums to census households with a female head ages 18-55.[1] The baseline survey collected information on household composition, education, income, income shocks, monetary and non-monetary asset ownership, borrowing, and expenditures on durables and non-durables. In total, 1,236 households were surveyed at baseline.

Separate public lotteries were held in each slum to randomly assign the 1,236 female household heads to treatment and control groups. Of those 1,236 women, 626 were randomly assigned to the treatment group, and were offered the option to open a savings account at the local bank-branch office. The women assigned to the control group were not given this option.

The accounts have all the characteristics of any formal savings account. The enrollment procedure is simple and account holders are provided with an easy-to-use passbook savings account. The bank does not charge any opening, maintenance, or withdrawal fees and pays a 6% nominal yearly interest (inflation was 12% in 2009, 8% in 2010 and 5% in 2011), similar to the average alternative available in the Nepalese market (Nepal Rastra Bank, 2011). In addition, the savings account does not have a minimum balance requirement.[2] Customers can make transactions at the local bank-branch offices in the slums, which are open twice a week for three hours, or at the bank's main office, located in downtown Pokhara, during regular business hours. After completion of the baseline survey, GONESA bank progressively began operating in the slums between the last two weeks of May and the first week of June 2010.

2.2 Data

In our analysis, we use data from three household surveys: the baseline survey discussed above and two follow-up surveys conducted in June 2011 and September of 2011. The first follow-up survey, which was conducted one year after the beginning of the intervention, included the hypothetical intertemporal-choice task in which participants were asked to make choices between a smaller, sooner monetary reward and a larger, more delayed monetary reward. It also repeated the modules that were part of the baseline

[1]Female household head is defined here as the female member taking care of the household. Based on this definition, 99% of the households living in the 17 slums were surveyed by the enumerators.
[2]The money deposited in the savings account is fully liquid for withdrawal; the savings account is fully flexible and operates without any commitment to save a given amount or to save for a specific purpose.

survey and collected additional information on household expenditures.[3] In the second follow-up survey, which went into the field three months after the first follow-up survey, we administered the lottery-choice and the CTB tasks.

2.3 Risk Aversion and the Lottery-Choice Task

In the lottery task, subjects were asked to choose among five lotteries, which differed on how much they paid depending on whether a coin landed on heads or on tails.[4] The lottery-choice task is similar to that used by Eckel and Grossman (2002) and Garbarino, Slonim, and Sydnor (2011). Each lottery had a 50-50 chance, based on a coin flip, of paying either a lower or higher reward. The five (lower; higher) pairings were (20; 20), (15; 30), (10; 40), (5; 50) and (0; 55). The choices in the lottery task allow one to rank subjects according to their risk aversion: subjects that are more risk averse will choose the lotteries with lower expected value and lower variance. The least risky lottery option involved a sure payout of 20 Rupees, while the most risky option (0; 55) was a mean-preserving spread of the second-most risky, and as such should only be chosen by risk-loving individuals. Given the low level of literacy of our sample, we opted for a visual presentation of the options. Each option was represented with pictures of Rupees bills corresponding to the amount of money that would be paid if the coin landed on heads or tails (see Appendix Figure 1 for a reproduction of the images shown to subjects).

2.4 Hypothetical Intertemporal Choice Task

In the first follow-up survey, we measured willingness to delay gratification by asking individuals to make hypothetical choices between a smaller sooner monetary reward and a larger later monetary reward (Tversky and Kahneman 1986, Benzion, Rapoport, and Yagil 1989, Shelley 1993). Study participants were initially asked to choose between receiving 200 rupees today or 250 rupees in 1 month. Those who chose 200 rupees today (over 250 rupees in 1 month) were then asked to make a second choice between 200 rupees today or 330 rupees in 1 month. Those who chose 250 rupees in 1 month (over 200 rupeees today) were asked to make a second choice between 200 rupees today or 220 rupees in 1 month. The choices in this intertemporal choice task allow one to rank subjects according to their willingness to delay gratification: subjects that are more impatient will be less willing to wait to receive a larger reward.

---

[3]Of the 1,236 households interviewed at baseline, 91% (i.e., 1,118) were found and surveyed in the first follow-up survey. Attrition for completing the first follow-up survey is not correlated with observables.

[4]Subjects did the lottery choice task after making their decisions in the four CTB games, but prior to learning which of the four CTB games they would be paid for. Immediately after making the choice in the lottery choice task, a coin was flipped and the subject received a voucher for the amount of money corresponding to her option choice and the coin flip. The voucher was redeemable starting that day at GONESA bank headquarters. To ensure that the risk game did not influence the participants' choices in the CTB game, subjects were informed about this game and the potential money from this game only after making their allocation decisions.

We asked a second set of questions varying the time frame (two months or in three months) to investigate hyperbolic discounting. These survey questions are presented in Appendix Figure 1 and Appendix Figure 2.

2.5 Incentivized Intertemporal Choice Task

We adapted an experimental procedure developed by Andreoni and Sprenger (forthcoming**)** called the "Convex Time Budget" method (henceforth, CTB) to the context of our sample of mostly uneducated villagers. In the CTB, subjects are given an experimental budget and must decide how much of this money they would like to receive at a sooner specified date and how much they would like to receive at a later specified date. The amount they choose to receive later is paid with an experimental interest rate, as a reward for delaying gratification. In practice, subjects are solving a two-period intertemporal allocation problem by choosing an allocation along the intertemporal budget constraint determined by the experimental budget and the experimental interest rate. Andreoni and Sprenger (forthcoming) used a computer display that allowed for a quasi-continuous choice set. In our study we use an even simpler version of this CTB choice task.[5]

In our adaptation of the task, participants were asked to make choices between 3 options. The 3 options corresponded to 3 (non-corner) allocations along an intertemporal budget constraint with an experimental endowment of 200 Nepalese Rupees (Rs) and an implicit experimental interest rate of either 10% or 20%. Subjects were asked to make four of these choices (henceforth, games), in which we varied the time frame and the experimental interest rate.

Table 1 lists the parameters of each one of the four games and the three possible allocations in each game. In game 1, the interest rate was 10%, the sooner date was "today" and the later date was "in 1 month", such that the time delay (i.e., the time interval between the sooner and later dates) was one month. Game 2 had the same interest rate and time delay than game 1, but the sooner date in game 2 was "in 1 month" (consequently, the later date was "in 2 months" in game 2). Games 2 and 3 had the same time frame, but the interest rate was 10% in game 2 and 20% in game 3. Finally, the interest rate was 20%

[5]Giné, Goldberg, Silverman and Yang (2012) also adapted the CTB method into an experiment in the field involving tobacco farmers in Malawi. In their experiment, participants had a higher level of education then our sample (4.5 years of schooling versus 2 years in our sample). Thus, the level of sophistication of their experiment is higher. In particular, in their experiment, each participant was presented with a small bowl containing 20 tokens and two empty dishes, a "sooner dish" and a "later dish." Individuals were explained that each token allocated to the "sooner dish" would pay them an amount tomorrow while each token allocated to the "future dish" would pay them a larger amount in 30 days. Participants were then asked to allocate the 20 tokens between the present and future dishes. The value of the token placed in the future dish determined the implicit interest rate for waiting. The idea of the experiment is that—for a given interest rate—an individual that is more forward-looking will put more tokens in the future dish than an individual that is more present-oriented.

in games 3 and 4, but the time delay was 1 month in game 3 and 5 months in game 4 (in both the sooner date was "in 1 month"). One of the four games was randomly selected for payment.[6]

Limiting the decision in each game to a choice between three options greatly simplified the decisions subjects had to make and allowed for a visual presentation of the options with pictures of Rupee bills (see Appendix Figures 4-7 for a reproduction of the images shown to study participants). As with the lottery-choice task, the visual presentation of the options was crucial given the low level of literacy and the little familiarity with interest rates of our sample.[7] In addition, the enumerators were instructed to follow a protocol to carefully explain the task to participants and to have subjects practice before making their choices.[8]

One interesting feature of the CTB method is that we can investigate whether treatment and control groups respond differently to changes in the experimental interest rate or in the time frame. Moreover, as we explain in greater detail in section 4, the variations in the time frame and the interest rate enable one to investigate (under some structural assumptions) whether the treatment and control have different preference parameters, namely the present bias, the exponential discount rate and the intertemporal elasticity of substitution.

2.6 Sample Characteristics and Balance Check

Table 2 shows summary statistics of baseline characteristics, separately for treatment and control groups. The women participating in the savings experiment are very poor. They have on average two years of schooling, and live in households whose weekly household income averaged (at baseline) 1,600 Nepalese Rupees (~$20) and with household assets amounting to 50,000 Rupees (~$625). Households have on average 4.5 members with 2 children. Household members earn income from multiple sources: working as an agricultural or construction worker, collecting sand and stones, selling agricultural products, raising livestock and poultry, having a small shop, working as driver, receiving remittances, rents and pensions, among others (not shown in the table).

Only 15% of households had a bank account before the introduction of the program. Given the lack of access to formal savings products, it is not surprising that most households typically save via microfinance institutions, savings and credit cooperatives, and Rotating Savings and Credit Associations

[6]The selection of which game the subject was paid for was determined using the roll of a four-sided die. Payments were made using vouchers that the participant could redeem at GONESA's main office, with which they are familiar. At the end of the experiment, all money was paid with vouchers. Each voucher contained the soonest date the money could be redeemed. Each participant received two vouchers from this choice task, one for her "sooner" payment and one for her "later payment."
[7]The sample has on average two years of schooling (Prina 2012).
[8]The protocol of the experiment can be found in the Appendix.

(ROSCAs).[9] They also save by either investing in durable goods or livestock or by storing cash at home.[10] Additionally, households seem to rely on financial transactions with informal partners, such as friends, moneylenders, and shopkeepers, rather than with formal institutions, like banks—88% of them had at least one outstanding loan (most loans are taken from ROSCAs (45%), MFIs (40%), and family, friends, or neighbors).[11] This is consistent with previous literature showing that the poor have a portfolio of transactions and relationships (Banerjee, Duflo, Glennerster, and Kinnan, 2010; Collins et al. 2009; Dupas and Robinson 2010). Finally, monetary assets account for 40% of total assets while non-monetary assets, such as consumer durables, livestock and poultry, account for the remaining 60%.

Table 2 shows that the control and treatment groups are balanced along all background characteristics (Prina 2012).

2.7 Usage of the Savings Accounts and Savings Accumulation

The experiment generated exogenous variation in access to savings accounts and in savings behavior. At baseline roughly 15% of the control and treatment groups had a bank account. A year later 82% of the treatment group had a savings account at the GONESA bank (the percentage of control households with a bank account remained at 15%). Administrative bank data show 78% of the treatment used the savings account actively, making at least two deposits within the first year of being offered the account.[12] Over this one-year period account holders made on average 45 transactions: 3 withdrawals and 42 deposits (or 0.8 deposits per week). The average deposit was of 124 rupees, roughly 8% of the average weekly household income at baseline. The average weekly balance steadily increased reaching, a year after the start of the intervention, Rs. 2,362 for the average account holder (roughly 1.5 times the average weekly household income at baseline).

Access to the savings account increased monetary assets by more than 50% (Prina 2012). Total assets, which include monetary and non-monetary assets (consumer durables and livestock), grew by 16%—suggesting the increase in monetary assets did not crowd out savings in non-monetary assets. Prina (2012) also documents households reduced the amount of cash savings, but households do not seem to reallocate assets away from other types of savings institutions, formal or informal. Hence, it is possible households

[9] A ROSCA is a savings group formed by individuals who decide to make regular cyclical contributions to a fund in order to build together a pool of money, which then rotates among group members, being given as a lump sum to one member in each cycle.
[10] Households typically had about one week worth of household income stored at home.
[11] This is in line with the statistics from the nationally representative survey conducted in 2006 by the World Bank. The survey shows that over two-thirds of Nepalese households had an outstanding loan from a formal or informal institution (Ferrari et al. 2007)
[12] This is a high take-up rate, compared to the results of similar studies (Dupas and Robinson 2010; Ashraf et al. 2006).

might perceive the savings account as a valuable addition to the set of financial institutions they use, but not necessarily as a substitute.[13]

3. Reduced Form Results

We begin our discussion of the results by looking at the reduced form treatment-control differences in their choices in the experimental tasks.

3.1. Lottery Choices

Figure 1 presents the distribution over the five possible choices in the lottery-choice task, separately for the control and treatment groups. The bars are indexed by the *lower* x *higher* amounts subjects would be paid if a coin landed on *heads* x *tails*. For example, the first bar from left to right shows the fraction of subjects who chose the risk-free option that paid 20 rupees irrespective of the coin toss. Similarly, the second bar from left to right shows the fraction of subjects who chose the lottery that paid 30 rupees if the coin landed on heads and 15 rupees if it landed on tails. Thus, the bars further to the right correspond to lotteries with higher expected value and higher variance.

Figure 1 shows the treatment group is more willing to choose riskier lotteries. The distribution of the treatment group is shifted to right relative to the distribution of control, that is, the treatment group is more likely than the control group to choose options with higher expected value and higher variance.

Table 3 reproduces the results presented graphically in Figure 1. Columns (1) and (3) show the fraction of subjects who chose each option, separately for treatment (1) and control (3). Columns (2) and (4) report the standard deviations for treatment and control, respectively. Column (5) reports the treatment-control difference in means. Finally, column (6) shows the p-value of a two-sided hypothesis test that the mean of the control and the treatment groups are the same. Tables 4, 5 and 6 use a similar structure to the one of Table 3.

The results in Table 3 confirm the treatment group is less risk averse than the control group: The treatment group is 4 percentage points less likely to choose the risk-free option that paid 20 rupees irrespective of the coin toss. This result is statistically significant at 5%.

3.2. Hypothetical Intertemporal Binary Choices

[13]For example, savings accounts and ROSCAs differ greatly across several characteristics. The social component of ROSCA participation, with its structure of regular contributions made publicly to a common fund, helps individuals to commit themselves to save (Gugerty 2007). This feature is not present in a formal savings account such as the one offered. Also, ROSCAs are usually set up to enable the group members to buy durable goods and are unsuitable devices to save for anticipated expenses that are incurred by several members at the same time (e.g., school expenses at the beginning of the school year), because only one member of a ROSCA can get the pot in each cycle.

Figure 2 presents the distribution over the four possible choices in the hypothetical intertemporal choice task in which subjects had to choose between 300 rupees in 1 month and a larger amount in 2 months. The bars are indexed by the delayed amount subjects would require to be willing to wait. For example, the second bar from left to right shows the fraction of subjects who were willing to wait for 495 rupees—that is, they preferred receiving 495 rupees in 2 months over 300 rupees in 1 month. Similarly, the third and fourth bars from left to right show the fraction of subjects who were willing to wait for 375 rupees and 330 rupees, respectively. Finally, the first bar shows the fraction of subjects who would demand more than 495 rupees to be willing to wait—that is, they preferred 300 rupees in 1 month over 495 rupees in 2 months. Thus, the bars further to the right correspond to participants who are more willing to delay gratification. Figure 3 presents the distribution over the four possible choices when subjects had to choose between 200 rupees today and a larger amount in 1 month.

Figures 2 and 3 show the treatment group was more willing than the control group to accept delayed payments in the hypothetical intertemporal choice task. In both figures the mass of distribution of the treatment group is shifted to the right relative to the distribution of the control group. Table 4 confirms these results.

The treatment group is roughly 6 percentage points more likely than the control group to be willing to give up 300 Rs in 1 month in exchange for 375 Rs in 2 months. This difference is statistically significant at 5%.

3.3. Incentivized CTB Choices

Figure 4 shows for each game the distribution of choices in the CTB experimental task, separately for the control and treatment groups. Four sets of two bars are presented. Each set corresponds to one of the four games; the left bar in each set corresponds to the distribution of choices among the control group while the right bar corresponds to the distribution of choices among the treatment group. Each bar contains two parts: a blue part that is above the x-axis and a red part that is below the x-axis. The blue part corresponds to the fraction of participants who were the most willing to delay gratification, choosing to delay the maximum amount of 150 rupees (50 rupees sooner). The red part corresponds to the fraction of participants who were the least willing to delay gratification, delaying the minimum amount of 50 rupees (150 rupees sooner). Thus, an increase in the willingness to delay gratification corresponds to an increase in the blue bar and a reduction in the red bar.

The differences in choices across games reflect changes in the parameters of the intertemporal choice across the games. In game 1 the experimental interest rate was 10%, the sooner date was “today” and the later date was “in 1 month.” The sooner date was changed from “today” to “in 1 month” between games 1

and 2 while the time interval between the sooner and later dates and the experimental interest rate were held constant. Thus, present biased individuals would be supposedly more willing to delay gratification in game 2 than in game 1. Games 2 and 3 had the same time frame (sooner date = in 1 month; later date = in 2 months), but the interest rate was increased from 10% in game 2 to 20% in game 3. Individuals with a higher intertemporal elasticity of substitution would be the ones to reallocate more money to the later date in response to a change in the interest rate. Finally, the time delay was increased from one month in game 3 to five months in game 4. While the sooner date was the same in games 3 and 4 ("in 1 month"), the later date was "in 2 months" in game 3 and "in 6 months" in game 4 (the interest rate was held constant at 20% between games 3 and 4). Individuals with a higher discount rate would be the ones to reallocate more resources to the sooner date in response to an increase in the time delay.

The comparison of choices across games suggests that participants understood the experimental task. For example, subjects re-allocate significantly more money to the later date when the experimental interest rate is increased from game 2 to game 3. Subjects also reallocate more money to the sooner date when the delay time is increased from game 3 to game 4. Interestingly, we see no evidence of present bias. The choices in games 1 and 2 are very similar, even though the sooner date is "today" in game 1 and "in 1 month" in game 2. Andreoni and Sprenger (forthcoming) also found no evidence of present bias when they conducted the CTB task with undergraduate students. The results of Augenblick, Niederle and Sprenger (2012) suggest that tasks involving choices over monetary rewards may be less suited to capture present bias than tasks involving choices over real-effort-tasks. We turn now to the treatment-control differences.

Figure 4 suggests the treatment is more willing to delay gratification. The treatment group is more likely to delay the maximum amount possible of 150 rupees and less likely to delay the minimum amount possible of 50 rupees (with the exception of game 2). Table 5 reproduces the results presented graphically in Figure 4.

Table 5 shows the treatment was 3.5 percentage points more likely than the control to delay in game 1 the maximum amount possible of 150 rupees. In game 3 the treatment was roughly 5 percentage points more likely to delay the maximum amount possible. This difference is statistically significant at 10%. The treatment group is also 2 and 4 percentage points less likely to delay the smallest amount possible in games 3 and 4, respectively.

However the differences are modest and the standard errors are large such that—even though they mostly have the expected signs—they are not statistically significant. Take for example the fraction of subjects choosing to delay the maximum amount of 150 rupees in all four games: The treatment group is 2 percentage points more likely to always delay the maximum amount. As a basis of comparison, the fraction of control subjects choosing to delay the maximum amount increases roughly 12 percentage

points when the experimental interest rate is increased from 10% to 20%. Thus, the treatment-control difference corresponds to one-sixth of the effect of doubling the interest rate (the result is remarkably similar if one considers the figures in Panel B instead).

We investigate next whether treatment and control groups respond differently to changes in the parameters of the experimental task, which may give us further insight into why the treatment group may be more willing to delay gratification. For this purpose, we compare how the allocations of treatment and control groups change between: i) games 1 and game 2 (change in the sooner date); ii) games 2 and 3 (change in the experimental interest rate); and iii) games 3 and 4 (change in time delay). The results are shown in Table 6. For example, they show that the increase in the fraction of subjects choosing to delay the maximum amount is larger among the control group (and the reduction in the fraction of subjects choosing to delay the minimum amount is larger among the control), which is consistent with the control group being more present biased than the treatment group. These differences are not however statistically significant.

Interestingly, the treatment group is more responsive than the control group to an increase in the experimental interest rate. When the experimental interest rate increases from 10% to 20%, there is a 17 percentage points increase in the fraction of treatment choosing to delay the maximum amount and a 12 percentage points increase among the control. Similarly, the increase in experimental interest rates leads to a 11 percentage points decrease in the fraction of the treatment choosing to delay the minimum amount and a 5 percentage points reduction among the control. This difference is statistically significant at 10%.

Finally, the evidence on which group is more responsive to the increase in the time delay is mixed. As expected, for both groups the increase in the time delay increases the fraction of participants choosing to delay the minimum amount of 50 and decreases the fraction of participants choosing to delay the maximum amount of 150. The increase in the fraction of participants choosing to delay the minimum amount is smaller among the treatment group, which is consistent with the treatment group being less responsive to the increase in the delay time. However, the decrease in the fraction of participants choosing to delay the maximum amount is larger among the treatment, which would suggest the control group is less responsive. Anyhow, these differences are not statistically significant.

The reduced-form results show that the treatment group is more responsive to an increase in the experimental interest rate, which suggests that the treatment group may be more willing to delay gratification because it has a higher intertemporal elasticity of substitution. This hypothesis is also consistent with the evidence that the treatment is more likely to choose riskier lotteries in the lottery choice task. In a model with constant-relative-risk-aversion (CRRA) risk preferences, a higher intertemporal elasticity of substitution would correspond to a less concave and more risk-neutral utility function.

4. Structural Results

In Section 3 we documented that treatment and control make different choices in the experimental tasks, remaining agnostic about what may underlie these differences in behavior. In this section we look at the hypothesis that the differential behavior may be driven by differences in preferences: *How different would the preferences of the control and treatment groups have to be to generate the experimental tasks choices we observe in the data*? If one is willing to make structural assumptions about the utility function, the randomized variation can be combined with a structural model to estimate the preference parameters of the control and treatment groups. For this purpose we use data from the CTB task, which allows us to jointly estimate the present bias, the exponential discount rate and the intertemporal elasticity of substitution under a single unified framework.

4.1 Model

We follow Andreoni and Sprenger (forthcoming) in modeling the intertemporal choice an agent with time separable utility and quasi-hyperbolic time preferences faces in the experimental task. In a given game g the agent must choose between receiving 150, 100 or 50 rupees sooner. The later reward, LR_g, is given by:

$$LR_g = \left(200 - SR_g\right) * R_g, \tag{1}$$

where SR_g is the sooner reward, and R_g is the gross experimental interest rate in game g. Assuming the agent has constant-relative-risk-aversion (CRRA) risk preferences, the utility of a given allocation is given by:

$$\mathrm{U}\left(SR_g, LR_g\right) = \left[\left(SR_g + \omega_1\right)^{1-\frac{1}{\theta}} + \beta^{\tau_g}\delta^{k_g}\left(LR_g + \omega_2\right)^{1-\frac{1}{\theta}}\right] / \left[1 - \frac{1}{\theta}\right], \tag{2}$$

where the preference parameters are: θ, the intertemporal elasticity of substitution; β, the present bias; and δ, the monthly discount factor. The parameters of the game g intertemporal choice are: τ_g, an indicator variable that is 1 if the sooner date in game g is today (and 0 otherwise); k_g, the time delay (in months) between the sooner and later dates; and R_g is the gross experimental interest rate. The parameter ω_1 is the background consumption in the period in which the agent receives the sooner reward and ω_2 is the background consumption in the period in which the agent receives the later reward. Andersen et al (2008) define background consumption as "*the optimized consumption stream based on wealth and income that is [perfectly] anticipated before allowing for the effects of the money offered in the*

experimental tasks."[14] It is easy to show that the agent chooses to receive 150 sooner if condition (3) holds and chooses 50 sooner if condition (4) holds:

$$ln\frac{(150+\omega_1)^{1-\frac{1}{\theta}}-(100+\omega_1)^{1-\frac{1}{\theta}}}{\left(100R_g+\omega_2\right)^{1-\frac{1}{\theta}}-\left(50R_g+\omega_2\right)^{1-\frac{1}{\theta}}}>Y_g^*, \tag{3}$$

$$ln\frac{(100+\omega_1)^{1-\frac{1}{\theta}}-(50+\omega_1)^{1-\frac{1}{\theta}}}{\left(150R_g+\omega_2\right)^{1-\frac{1}{\theta}}-\left(100R_g+\omega_2\right)^{1-\frac{1}{\theta}}}<Y_g^*, \tag{4}$$

where $Y_g^* = \tau_g \ln\beta + k_g \ln\delta$ is (the log of) the effective discount factor in game g. If neither condition (3) nor condition (4) holds, then the agent chooses to receive 100 sooner.[15]

In taking the model to the data, we assume an addictive error structure:

$$Y_{i,g}^* = \tau_g \ln\beta + k_g \ln\delta + \varepsilon_{i,g}, \tag{5}$$

where $\varepsilon_{i,g}$ is an error term that is specific to individual i and game g and is normally distributed with mean zero and variance σ^2—i.e., $\varepsilon_{i,g} \sim N(0,\sigma^2)$. Under these assumptions, the likelihood of individual i's choice in game g is given by:[16]

$$\mathcal{L}_{i,g} = \begin{cases} 1-\Phi\left(\frac{1}{\sigma}ln\frac{(100+\omega_{i,1})^{1-\frac{1}{\theta}}-(50+\omega_{i,1})^{1-\frac{1}{\theta}}}{(150R_g+\omega_{i,2})^{1-\frac{1}{\theta}}-(100R_g+\omega_{i,2})^{1-\frac{1}{\theta}}}-\frac{\ln\beta}{\sigma}\tau_g-\frac{\ln\delta}{\sigma}k_g\right) & \text{if } SR_{i,g}=50, \\ \Phi\left(\frac{1}{\sigma}ln\frac{(100+\omega_{i,1})^{1-\frac{1}{\theta}}-(50+\omega_{i,1})^{1-\frac{1}{\theta}}}{(150R_g+\omega_{i,2})^{1-\frac{1}{\theta}}-(100R_g+\omega_{i,2})^{1-\frac{1}{\theta}}}-\frac{\ln\beta}{\sigma}\tau_g-\frac{\ln\delta}{\sigma}k_g\right)- & \\ -\Phi\left(\frac{1}{\sigma}ln\frac{(150+\omega_{i,1})^{1-\frac{1}{\theta}}-(100+\omega_{i,1})^{1-\frac{1}{\theta}}}{(100R_g+\omega_{i,2})^{1-\frac{1}{\theta}}-(50R_g+\omega_{i,2})^{1-\frac{1}{\theta}}}-\frac{\ln\beta}{\sigma}\tau_g-\frac{\ln\delta}{\sigma}k_g\right) & \text{if } SR_{i,g}=100, \\ \Phi\left(\frac{1}{\sigma}ln\frac{(150+\omega_{i,1})^{1-\frac{1}{\theta}}-(100+\omega_{i,1})^{1-\frac{1}{\theta}}}{(100R_g+\omega_{i,2})^{1-\frac{1}{\theta}}-(50R_g+\omega_{i,2})^{1-\frac{1}{\theta}}}-\frac{\ln\beta}{\sigma}\tau_g-\frac{\ln\delta}{\sigma}k_g\right) & \text{if } SR_{i,g}=150, \end{cases} \tag{6}$$

where $\omega_{i,1}$ and $\omega_{i,2}$ allow for individual-level variation in background consumption. Using (6) we estimate the variance of the error term σ^2 and separate preference parameters (δ, β, θ) for the control and treatment groups through maximum likelihood. The variance of the error term is assumed to be the same for the two groups.

[14]Notice there is an assumption, which is the standard in the literature, that the agent chooses the optimal background consumption without taking the experimental rewards into account, such that the agent does not re-optimize if there is any reallocation of the experimental rewards.
[15]It is trivial to show that conditions (3) and (4) cannot jointly hold.
[16]Andreoni, Kahn and Sprenger (2012) adopt an alternative approach and use interval-censored Tobit to estimate the preference parameters when the Convex Time Budget task involves a choice between few options.

4.2 Structural Estimates and Variations in Background Consumption

In this subsection we present our structural estimates. An important issue in estimating preference parameters from experimental choice data is how to treat background consumption or wealth in the utility model. One approach is to assume that choices in the experiment are based solely on the outcomes of the experiment and that individuals "narrow bracket" by ignoring background assets when making their experimental choices. In our setting narrow bracketing is equivalent to assuming that $\omega_1 = \omega_2 = 0$ in equation (6). Another approach is to assume that individuals fully integrate their experimental choices with their background consumption. These different approaches matter a great deal for estimates of preference parameters, especially for the level of relative risk aversion (e.g., Rabin 2000, Rabin and Thaler 2001, Schechter 2007).

As we discussed in section 2.7, gaining access to the savings account has enabled the treatment group to accumulate more wealth than the control group. This implies that—*if individuals are integrating their background consumption and wealth with their experimental rewards*—they may behave differently in the experimental tasks even if they have the same preferences. If, however, subjects are narrow bracketing, then the differences in behavior between the groups will map into different estimates of the preference parameters (δ, θ, β). Here we provide an extensive discussion of these different approaches and provide structural estimates under different assumptions about the integration of background consumption.

4.2.1 Evidence of Narrow Bracketing

There are a number of reasons to believe that subjects were likely narrow bracketing when engaging in our CTB and lottery-choice tasks. One piece of evidence in that direction is that subjects failed to take advantage of a simple arbitrage opportunity. The experimental interest rate was much higher than the prevailing market interest rates and higher than the rate of interest the treatment group earned on their savings accounts. The CTB payment amounts were also fairly modest compared to the level of total financial assets of these households. As such, if subjects understood the situation and fully incorporated background consumption, they should have overwhelmingly allocated as much as possible to the later consumption in the CTB. Regardless of their desired consumption path with the CTB earnings, they could have achieved a better outcome by allocating all money in the CTB to the future and adjusting their background saving to achieve the consumption pattern they desired. However, a substantial fraction of participants make less-than-perfectly-patient choices in the CTB, even among those from the treatment group with substantial savings, which indicates they were not perfectly integrating. We do not find that lack of integration particularly surprising, because these households are not very financially sophisticated

and similar arguments have been raised when considering experimental subjects in a wide range of studies on risk aversion and intertemporal choice.

Another piece of evidence that suggests subjects were not fully integrating their choices in the CTB and lottery-choice tasks with their background consumption comes from analyzing the choices of subjects who were administered these tasks on different dates. The tasks happened to be administered around the *Dashain*, Nepal's most important national holiday. Households incur major expenses associated with the *Dashain* festivities, which in 2011 happened between October 3rd and October 12th.[17] We expect then that the *Dashain* would cause large variations in levels of background consumption and cause potential liquidity constraints for these households. As such, if subjects were directly integrating their background consumption in the CTB, we would expect to see differences in CTB allocations between subjects who played the CTB or the lottery-choice task closer or farther from the *Dashain*.

Figure 5 shows the relationship between household savings and the date at which the experimental tasks were administered. It plots the average savings among participants surveyed at a given day. The diameter of the circle reflects the mass of participants who were surveyed at the given day. The section of the graph between October 3rd and October 12th has no data and corresponds to the *Dashain*, when no interviews were conducted. The figure shows that there is a strong negative relationship between savings and the proximity to the *Dashain*: In roughly 30 days the average savings reduced approximately from 60,000 rupees all the way to 5,000 rupees. If individuals were integrating, one would expect that individuals would be less willing to delay gratification and less willing to take risks as it got closer to the holiday and they became increasingly liquidity constrained. However, the data do not support this hypothesis. Figure 6 plots the fraction of participants who chose in game 1 to receive the largest sooner reward of 150 rupees, which they could redeem on the same day, against the interview date. There is no evidence that individuals were less willing to delay gratification as it closer to the holiday. Figure 7 is consistent with Figure 6. There is no evidence that individuals were more likely to choose the risk-free option in the lottery-choice task as it got closer to the holidays.

4.2.2 Structural Estimates Assuming Narrow Bracketing

Table 7 presents the results from the structural estimation. Panel A shows the estimates of the preference parameters and panel B reports the p-value of hypothesis tests of treatment-control differences in the preference parameters. The estimates in the first column assume narrow bracketing, which is equivalent to assuming that $\omega_1 = \omega_2 = 0$ in equation (6).

[17] A household would spend money among other things buying new clothes and animals like buffaloes, ducks and goats to be slaughtered as sacrifices for the goddesses.

The results indicate that the treatment group has an annual discount rate 2 percentage points lower than the control and an intertemporal elasticity of substitution 7% higher. The annual discount rate of the control group is 26% while the annual discount rate of the treatment group is 24%; the difference is not statistically significant. These discount rates are somewhat reasonable given that annual inflation in Nepal tends to be above 10%. Consistent with the reduced form results, the results show no present bias as β is estimated to be equal to 1.

4.2.3 Differences in Background Consumption

Although we presented suggestive evidence in 4.2.1 that individuals are not fully integrating their background consumption, it is important to investigate how treatment-control differences in background consumption could potentially affect our results. The CRRA utility implies that differences in the *levels* of background consumption would have only second order effects on discount rates, as the results we will discuss in section 4.2.4 confirm. However, differences in the *profiles* of background consumption could lead to different CTB choices, even holding fixed preference parameters. Equation (2) in Section 4.1 highlights that the profile of background consumption affects the marginal utility of the experimental reward: The marginal utility of the sooner reward (relative to the marginal utility of the later reward) is decreasing in sooner background consumption ω_1 and increasing in later background consumption ω_2. Thus, even if control and treatment had the same preference parameters, differential profiles of background consumption would have led to different choices in the CTB task. In particular, the group with the flatter profile – with slower background consumption growth – would be more likely to choose the delayed payment options.

While it is clear that the treatment group has higher levels of background financial assets than the control group, differences in the profile of background consumption of control and treatment groups may depend on whether the sooner period for the CTB falls during relatively lean times or not. During lean times, the treatment group can use their buffer wealth to help smooth consumption and would likely have a flatter consumption profile, while the control group would have an upward-sloping profile. During normal times, we might expect the treatment group to have a slightly steeper profile. Because the savings account allows the treatment to save at a higher interest rate than the control group, one would expect the treatment to take advantage of the opportunity by reducing current background consumption in exchange for higher background consumption in the future.[18] In the next sub-section we investigate how these different patterns could affect the results.

[18]Prina (2012) finds no treatment-control difference in income one year after the introduction of the savings accounts.

Before turning to the structural estimation, it is important to stress that overall we have reason to believe that – if there were any treatment-control differences in background consumption around the time when subjects were administered the CTB – those differences should be relatively small. Table 8 shows summary statistics for expenditure data collected in the first follow-up survey, one year after the introduction of the savings accounts. It reports means and standard deviations and the p-value of hypothesis tests of whether 1) the means were the same across the two groups and 2) the variance within the treatment group was the same or higher than the variance within the control group. The first set of columns shows summary statistics for indicator variables of whether the household had purchased a particular consumption item. The second set of columns reports results for expenditure data including 0 expenditures for those who reported not having purchased the consumption item. The third set excludes this latter group. The comparison of means shows no treatment-control differences except for expenditures with textbooks, school uniforms and school supplies. There is also no strong support for the hypothesis that – because they cannot smooth income shocks – the variance of consumption is higher within the control group. We rarely reject the null hypothesis that the variance within the treatment group is as high or higher than the variance within the control group, including for the sum of all expenditures. Hence, while the savings accounts created an important resource for the treatment group, it would be misleading to assume they led to massively different consumption profiles for the different groups.

4.2.4 Structural Estimates Assuming Integration

Columns 2 through 7 of Table 7 show structural estimation results for different assumptions about background consumption. In Column 2 we assume that all members of each group have background consumption equal to 3,000 Rupees, which is close to the average typical weekly income reported by these households, and that the background consumption is constant over time. Assuming that households integrate this level of consumption only changes the size of the estimated intertemporal elasticity of substitution parameter. All of the patterns, and in particular the discount rates, are unchanged: The treatment group has a discount rate 2 percentage points lower and an intertemporal elasticity of substitution 7% higher than the control, but these differences are not statistically significant.

In Column 3 we assume different *levels* of background consumption for the control (3,000 Rupees) and the treatment (4,000 Rupees), but maintain the assumption that those levels of consumption are constant over time. Again the level of background consumption has no effect on estimated discount rates. However, due to the tight link between utility curvature and consumption levels in the CRRA model, assuming higher levels of background consumption for the treatment group leads to estimates of the IES that are actually higher for the control group. More modest differences in background consumption can lead to estimates of identical θ parameters for the two groups. As such, we conclude from this exercise

that, if the treatment group has modestly higher levels of background consumption, the patterns in our data could be consistent with lower discount rates for the treatment group but identical intertemporal elasticities of consumption for the two groups. In columns 4 and 5 of Table 7 we consider cases in which the two groups have different profiles of background consumption.

In Column 4 we simulate a scenario in which the sooner experimental reward would have been received at a leaner time, in which case the background consumption of the control group would be expected to grow (and the marginal utility of the sooner reward to increase) between the sooner and the later CTB dates. The treatment, however, can use the buffer wealth they have accumulated to smooth background consumption over time. This assumption lines up fairly well with the timing of the *Dashain* festival. Here we set background consumption for control at 2,980 in the sooner period and 3,000 in the later period, while treatment is held at 3,000 in both periods. This assumption implies that the treatment has an annual discount rate 5 percentage points *higher* than the control. There is no underlying explanation for why access to savings would make the treatment group substantially *less patient* than the control group.

In Column 5 we consider the alternative hypothesis that the treatment chooses a steeper profile of background consumption because they can save at a higher interest rate than the control. The estimates imply the treatment had an annual discount rate 9 percentage points lower than the control (statistically significant at 5%), but that the two groups had comparable intertemporal elasticities of substitution. We have also conducted exercises as in columns 4 and 5 but with much steeper gradients, such as a 10% effect, in which case we find extreme results with enormous differences in discount rates and in the intertemporal elasticities of substitution (results not shown in Table 7, but available from authors upon request).

In columns 6 and 7 we estimate the model allowing for individual-level heterogeneity in background consumption. In column 6 we use as our measure of background consumption in each period the individual's self-reported level of "typical weekly income" given during the first follow-up survey conducted about three months before our elicitation tasks. Allowing for this heterogeneity gives very similar results to those presented in Columns 1 and 2. We find slightly higher IES for the treatment group and a 3 percentage point lower discount rate for the treatment group, though again the standard errors on the structural estimates are large and neither difference is statistically significant. Finally in the last column we couple the individual-level heterogeneity with a potential shock to income for the control group in the earlier period. At the time subjects took part in the elicitation tasks, they were asked to report their income level in the previous week. For many subjects that level of income was significantly below the level of typical income they report in the endline survey (median difference for control = -

2,033 and for treatment = -2,000). The savings accounts available to the treatment group might allow them to smooth consumption better than the control. We capture this effect by setting sooner-period background consumption for the control group equal to a mixture of 25% income reported right before the CTB and 75% typical weekly income. This generates a heterogeneous consumption shock for the control. For both the control in the later period and for the treatment in both periods we use typical weekly income as the measure of background consumption. This level of shock for the control group results in estimates of the discount rate that are identical between control and treatment. However, in this specification the control group has a significantly higher intertemporal elasticity of substitution. That result is hard to reconcile with the more risk-averse choices the control group makes in the lottery choice task.

The results in Columns 4, 5 and 7 highlight that the structural estimates are very sensitive to assumptions about the slope of background consumption. In general we find that even rather modest differences in background consumption profiles between the groups would generate very different behavior in the CTB under the assumptions that individuals integrate background wealth and the two groups have identical preferences. Since the differences in behavior in the CTB between groups are not extreme and since the preference-differences needed to explain those relatively similar choices seem implausible, such as the access to savings accounts having *increased* discount rates, we think these results largely suggest that individuals either were not integrating their background consumption when making the CTB choices or that the two groups did have very similar background consumption gradients over time. In either case, under this interpretation, these results would lend more support for a "preference change" rather than "wealth effects" explanation for the mechanism behind the different choice patterns we observe for the two groups.

5. Conclusion

In this paper we exploited a field experiment that randomized access to savings accounts to investigate whether attitudes toward risk and intertemporal choices are affected by the act of saving. Because the majority of the study population had never had a savings account before, the experiment generated random variation in savings behavior.

A year later we administered a lottery-choice and intertemporal choice tasks. In the lottery-choice task subjects were asked to choose their preferred lottery (whose outcome would depend on a coin flip) among a set of options with different levels of risk and expected value. In a hypothetical intertemporal-choice task participants were asked to make choices between a smaller, sooner monetary reward and a

larger, more delayed monetary reward. Finally, we conducted an incentivized intertemporal-choice task based on the Convex Time Budget (CTB) method (Andreoni and Sprenger forthcoming).

Our reduced-form results show that the treatment group is less risk averse and more willing to accept delayed rewards than the control. We find that the treatment group was significantly more likely to choose risk-neutral or risk-loving options than the control group in the experimental lottery-choice task. In the hypothetical intertemporal-choice task the treatment group was significantly more likely than control to choose higher but delayed payments over a range of delay times and delay rewards. In the CTB task overall the treatment group allocated more money to the future than the control, although this difference is not statistically significant. The treatment group was also more responsive than the control group to an increase in the experimental interest rate, implying that within the CTB allocations the treatment group had a higher intertemporal elasticity of substitution.

Combining the randomized variation with a structural model, we estimate the preference parameters of the control and treatment. Our estimates show that the treatment group has an annualized discount rate 2 percentage points lower but this difference is not statistically significant. We also estimate that the treatment has an intertemporal elasticity of substitution that is approximately 7% higher than that of the control group, though again that result is not statistically significant. We find no evidence of present bias for either group and estimate the present-bias coefficient to be precisely 1 for each group.

Finally, we provided suggestive evidence that the subjects were not fully integrating their background consumption and assets when making decisions in our experimental choice tasks, which indicates that the differences in choices we observe are due to changes in preferences rather than wealth effects.

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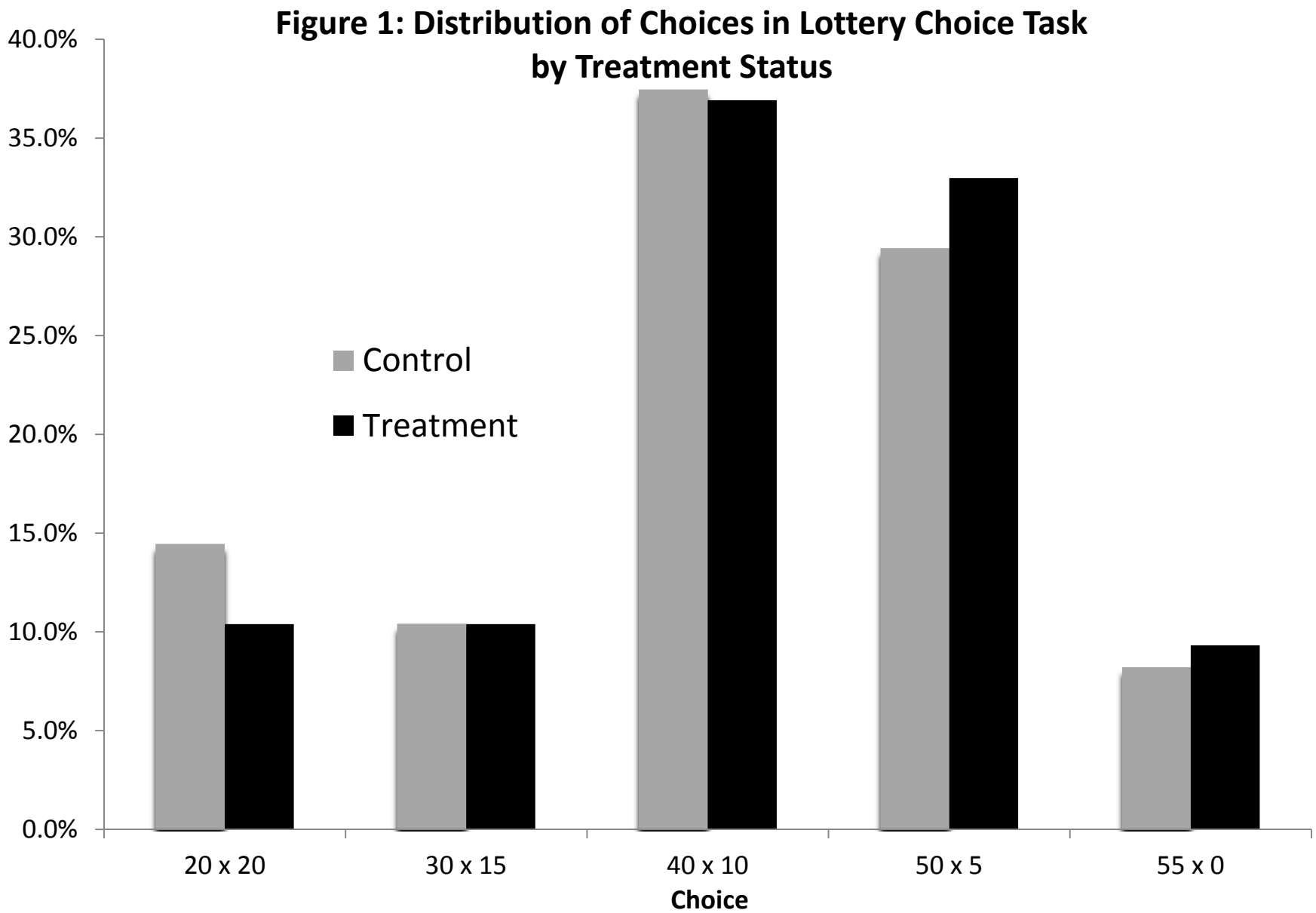
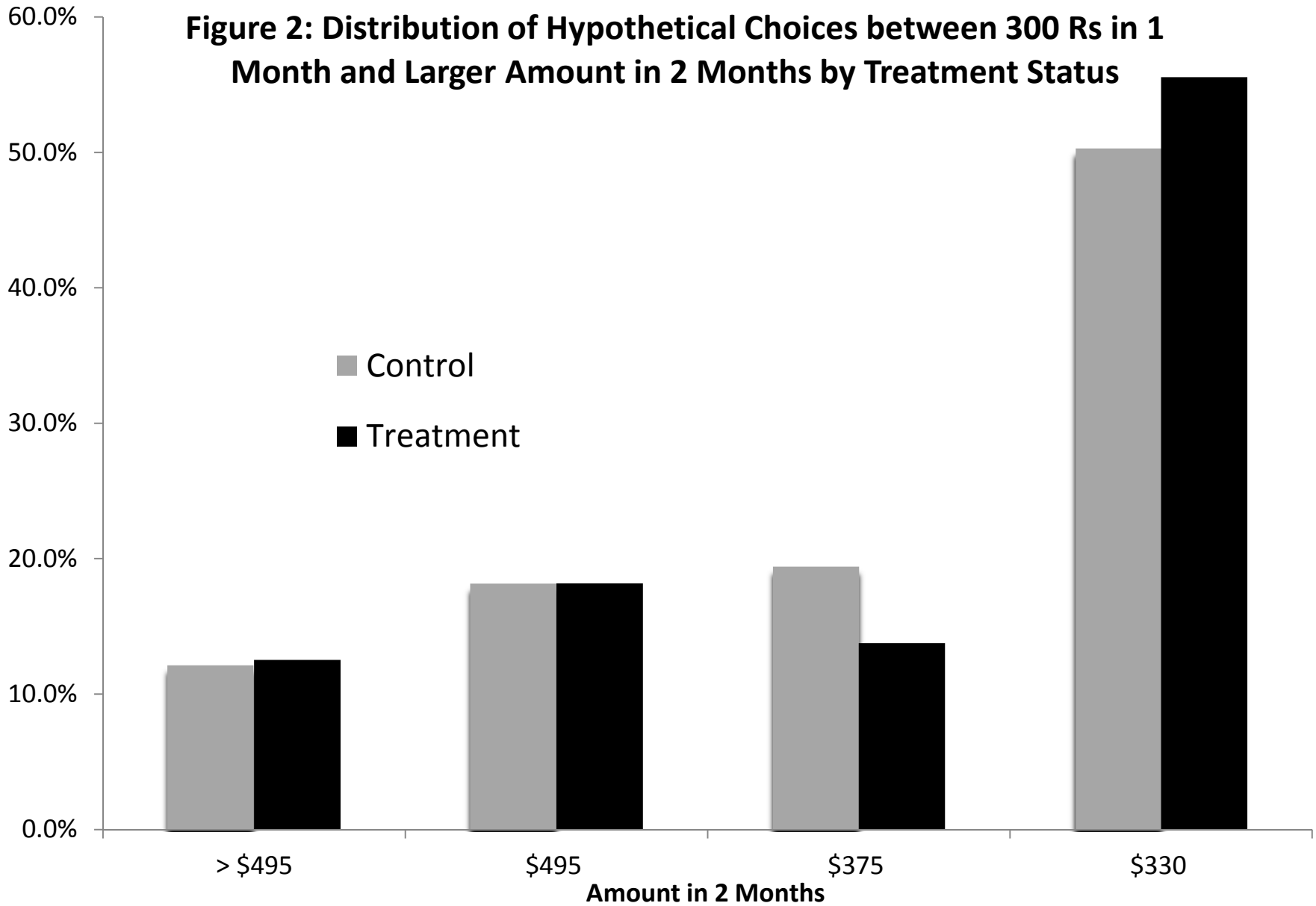
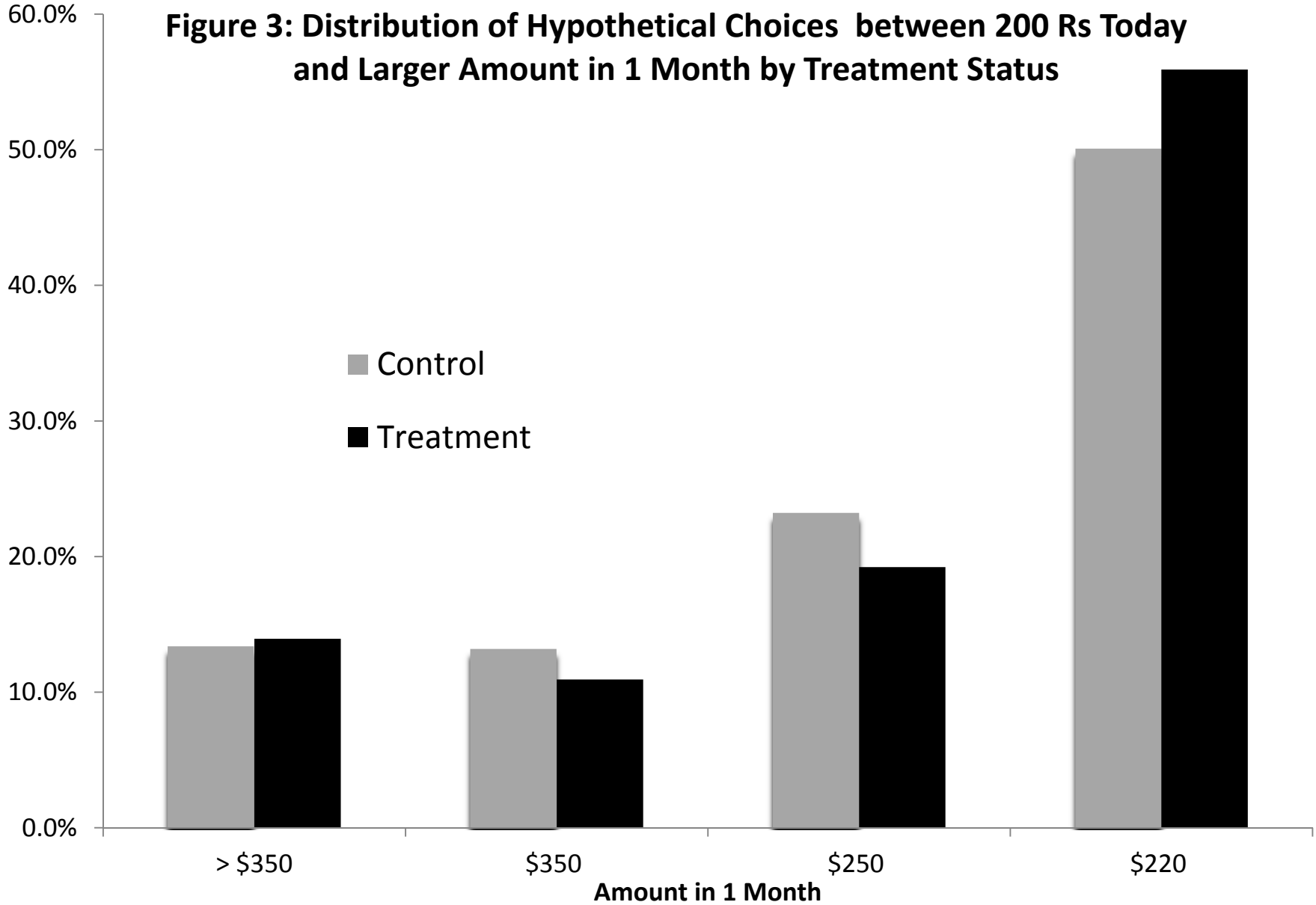


Note: The figure shows the distribution of choices in the lottery choice task by treatment status. The two values shown below each bar correspond to the amounts subjects would get if the coin landed on heads or if it landed on tails.



Note: The figure shows the distribution of choices in a task in which subjects had to make hypothetical choices between 300 Rs in 1 month and a larger amount in 2 months. The horizontal axis shows the amount that was required for subjects to be willing to delay 300 Rs.



Note: The figure shows the distribution of choices in a task in which subjects had to make hypothetical choices between 200 Rs today and a larger amount in 1 month. The horizontal axis shows the amount that was required for subjects to be willing to delay the 200 amount.

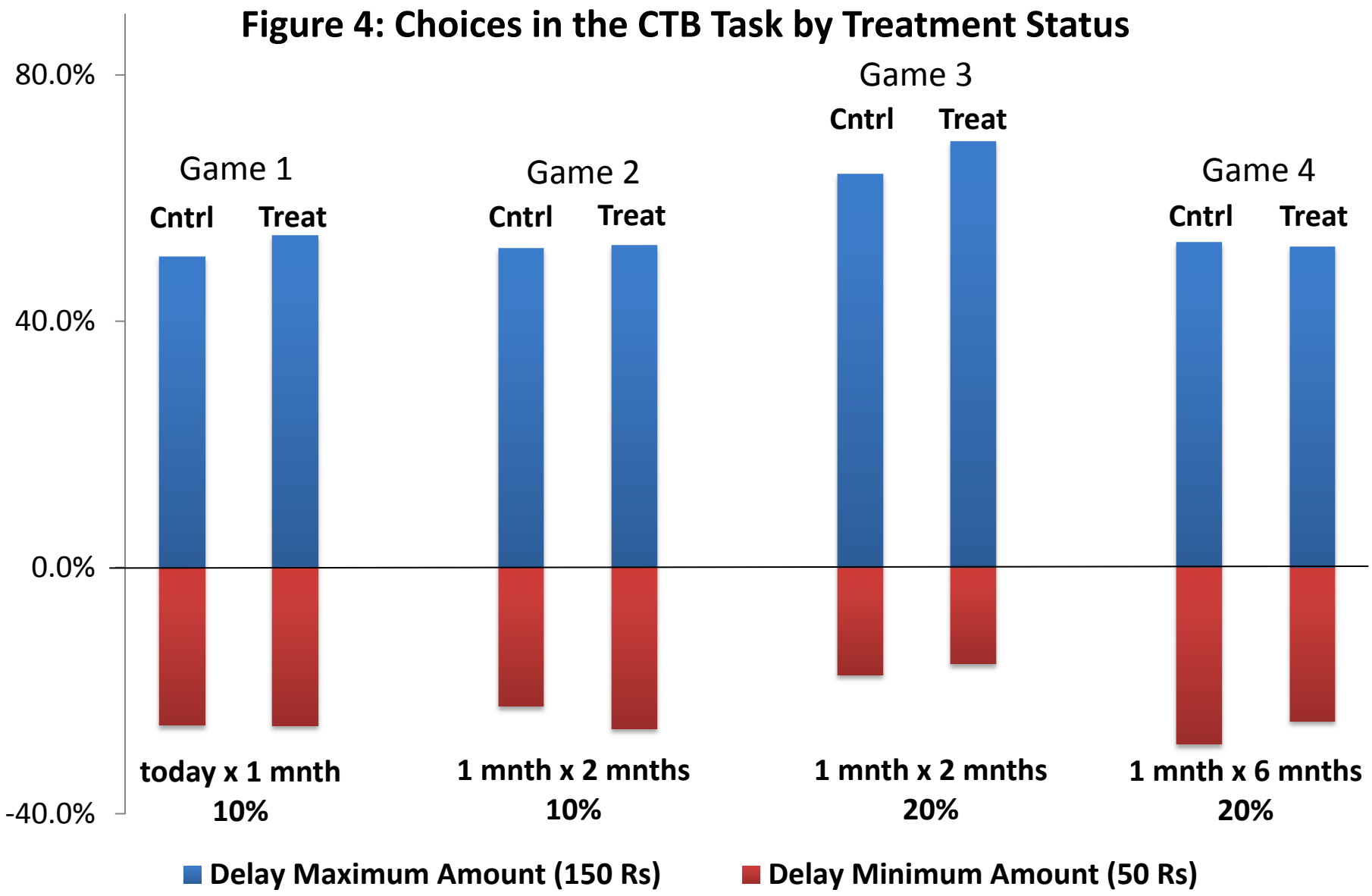
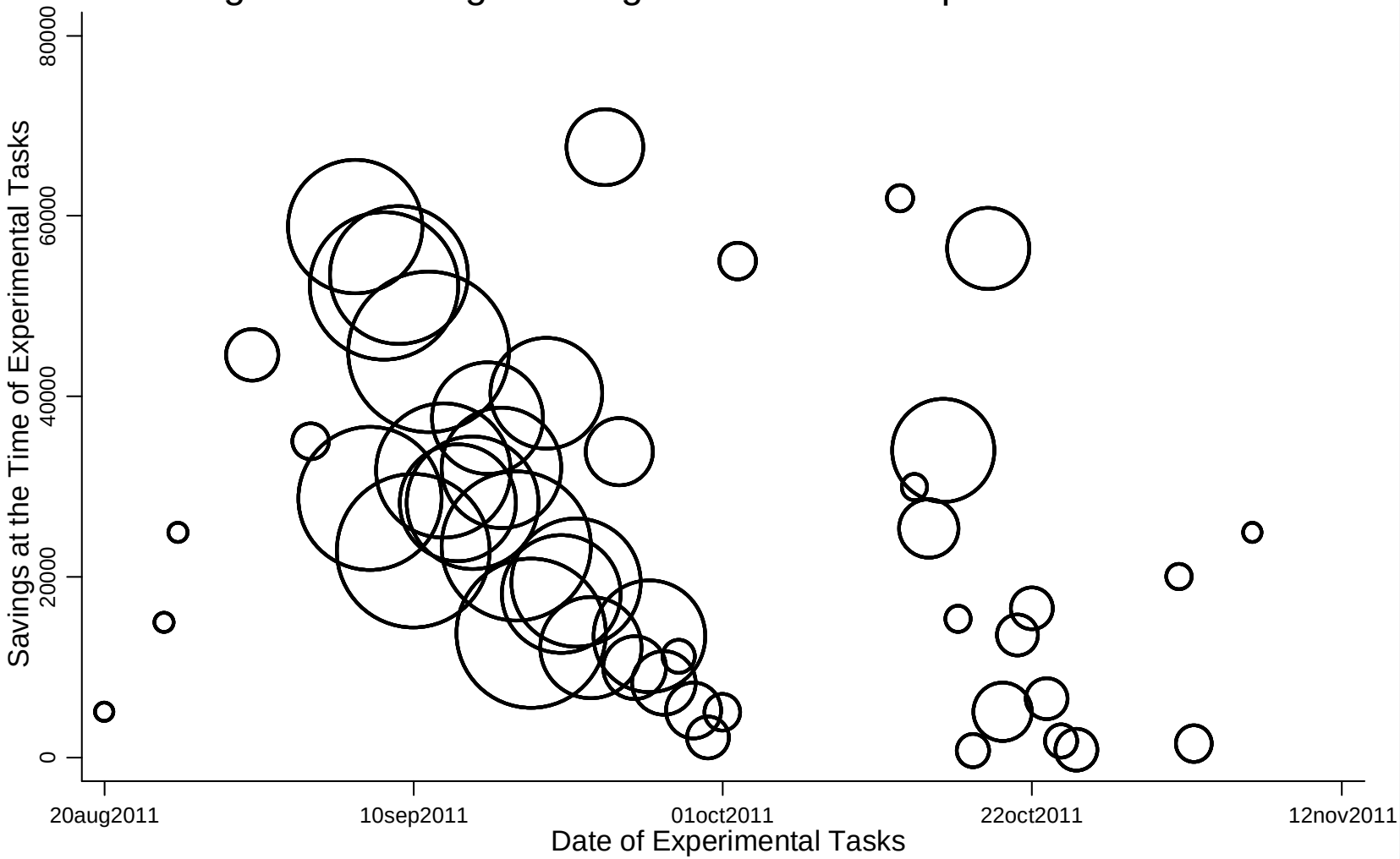


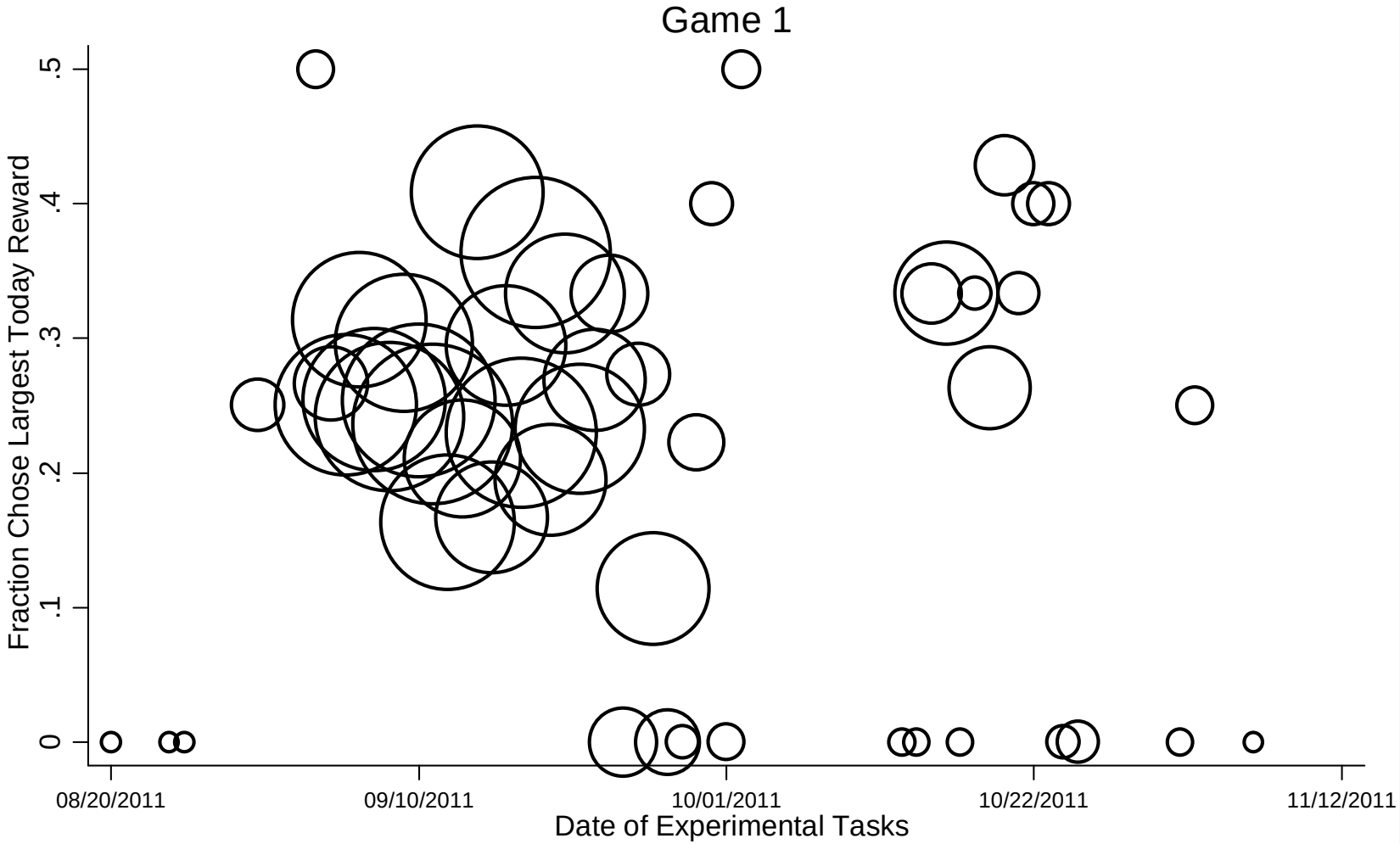
Figure 4: Choices in the CTB Task by Treatment Status
80.0%
40.0%
0.0%
-40.0%
Game 1
Cntrl
Treat
Game 2
Cntrl
Treat
Game 3
Cntrl
Treat
Game 4
Cntrl
Treat
today x 1 mnth
10%
1 mnth x 2 mnths
10%
1 mnth x 2 mnths
20%
1 mnth x 6 mnths
20%
Delay Maximum Amount (150 Rs)
Delay Minimum Amount (50 Rs)

Figure 5: Average Savings and Date of Experimental Tasks



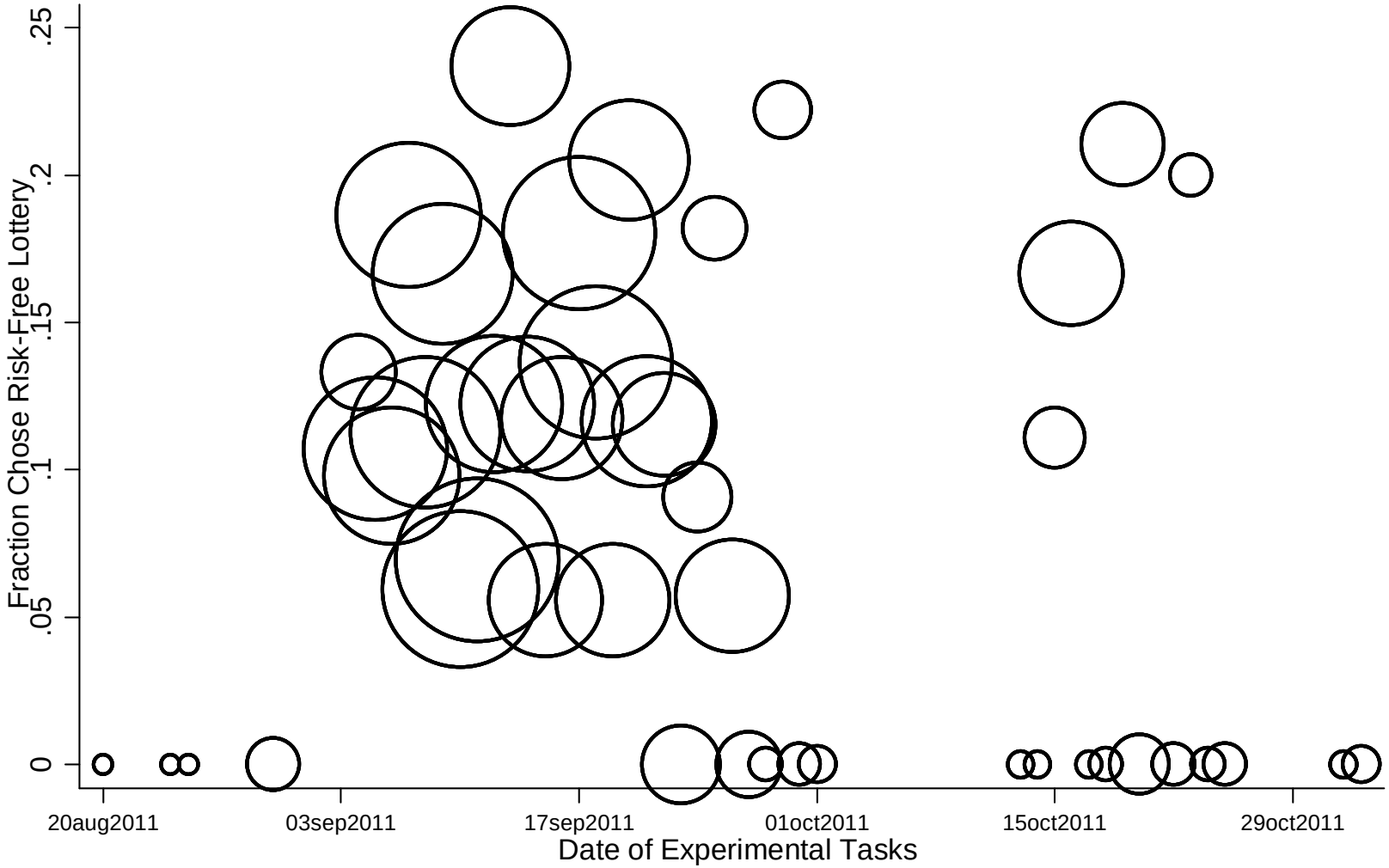
Note: The figure shows average savings (at the time of the experiment tasks) of participants who were administered the tasks at a given day. The balls' circumferences correspond to the mass of participants surveyed at the given day.

Figure 6: Largest Today Reward and Date of Experimental Tasks



Note: The figure shows the fraction of participants who were administered the experimental tasks at a given day that chose the largest today reward of 150 rupees. The balls' circumferences correspond to the mass of participants surveyed at the given day.

Figure 7: Risk-Free Lottery and Date of Experimental Tasks



Note: The figure shows the fraction of participants who were administered the experimental tasks at a given day that chose the risk-free lottery (which paid 20 rupees irrespective of the coin toss). The balls' circumferences correspond to the mass of participants surveyed at the given day.

Table 1: Choices for Adapted Convex Time Budget (CTB) Task

Game	Interest Rate	Sooner date	Later date	Allocation A		Allocation B		Allocation C	
				sooner	later	sooner	later	sooner	later
1	10%	today	1 month	Rs. 150	Rs. 55	Rs. 100	Rs. 110	Rs. 50	Rs. 165
2	10%	1 month	2 months	Rs. 150	Rs. 55	Rs. 100	Rs. 110	Rs. 50	Rs. 165
3	20%	1 month	2 months	Rs. 150	Rs. 60	Rs. 100	Rs. 120	Rs. 50	Rs. 180
4	20%	1 month	6 months	Rs. 150	Rs. 60	Rs. 100	Rs. 120	Rs. 50	Rs. 180

Note: The table shows the parameters of the intertemporal choice task. Each row corresponds to a different choice ("game") participants had to make between three different allocations (A, B, and C). The allocations differed in how much they paid at a sooner and a later dates. The sooner and later dates and the (monthly) interest rate varied across games.

Table 2: Descriptive Statistics by Treatment Status

	Treatment		Control		Difference in Means	Hypothesis Test
	(1) Means	(2) SD	(3) Means	(4) SD	(5) (1) - (3)	(6) P-value
Characteristics of the Female Head of Household)						
Age	36.7	11.40	36.5	11.70	25.3	0.82
Years of education	2.8	3.07	2.7	2.90	-0.3	0.50
Proportion married/living with partner	89%	0.29	88%	0.30	0.6	0.44
Household Characteristics						
Household size	4.5	1.69	4.5	1.65	2.8	0.72
Number of children	2.2	1.30	2.1	1.29	0.9	0.68
Total income last week *(in 1,000 Nepalese Rupees)*	1.7	5.8	1.6	5.1	-4.2	0.82
Proportion of households entrepreneurs	17%	0.38	16%	0.37	-21%	0.67
Proportion of households owning the house	82%	0.38	82%	0.39	44%	0.83
Proportion owning the land on which the house is built	77%	0.42	76%	0.43	35%	0.55
Experienced a negative income shock	43%	0.50	41%	0.49	-7%	0.43
Assets *(in 1,000 Nepalese Rupees)*						
Total Assets	47.0	59.9	42.3	49.6	-12.9	0.14
Total Monetary Assets	16.8	47.9	13.0	35.9	-31.1	0.11
Proportion of households with money in a bank	17%	0.38	15%	0.36	-21%	0.33
Total money in bank accounts	6.9	36.9	4.3	23.5	-30.1	0.14
Proportion of households with money in a ROSCA	18%	0.39	18%	0.38	-21%	0.79
Total money in ROSCA	3.2	17.0	2.1	8.5	-13.9	0.16
Proportion of households with money in an MFI	51%	0.50	53%	0.50	1%	0.51
Total money in MFIs	3.6	12.8	3.8	18.9	-9.2	0.91
Total amount of cash at home	2.2	5.5	1.9	4.2	-3.3	0.28
Total Non-Monetary Assets	30.2	28.7	29.4	28.6	1.4	0.62
Non-monetary assets from consumer durables	25.5	24.3	24.8	24.9	1.2	0.62
Non-monetary assets from livestock	4.7	12.8	4.6	12.3	-8.1	0.88
Liabilities						
Total amount owed by the household *(in 1,000 Nepalese Rupees)*	46.9	98.5	52.0	267.7	-51.7	0.66
Proportion of households with outstanding loans	90%	0.30	88%	0.33	60%	0.25

Note: The table reports the means and standard deviation of variables, separately by treatment status. The last column reports the t-statistic of two-way tests of the equality of the means across the two groups. All monetary values are reported in 1,000 Nepalese Rupees. Marital status has been modified so that missing values are replaced by the village averages.

Table 3: Treatment Effects on Risky Choices

	Choices Payment conditional on coin toss		Treatment (1) Means	 (2) SD	Control (3) Means	 (4) SD	Difference in Means (5) (1) - (3)	Hypothesis Test (6) P-value
	Heads	*Tails*						
#1	20	20	10.4%	0.013	14.4%	0.015	-4.1%	0.04 **
#2	30	15	10.4%	0.013	10.4%	0.013	0.0%	0.99
#3	40	10	36.9%	0.020	37.5%	0.021	-0.6%	0.85
#4	50	5	33.0%	0.020	29.4%	0.020	3.5%	0.20
#5	55	0	9.3%	0.012	8.2%	0.012	1.1%	0.52

Note : The table reports the distribution of choices in a lottery-choice task in which subjects chose one among five lotteries that paid different amounts depending on a coin toss. The first set of columns show the contingent payments of each lottery. Columns (1) and (3) show the fraction of respondents who chose each option, separately for treatment (1) and control (3). Columns (2) and (4) report the standard deviations. Columns (5) reports the treatment-control difference in means. Column (6) shows the p-value of a two-sided hypothesis test that the means are the same for the two groups.

Table 4: Treatment Effects on Hypothetical Intertemporal Choices

Choices	Treatment (1) Means	Treatment (2) SD	Control (3) Means	Control (4) SD	Difference in Means (5) (1) - (3)	Hypothesis Test (6) P-value
Panel A: Choice between 300 Rs in 1 Month (sooner) and Larger Amount in 2 Months (later)						
Prefers 300 sooner over 495 later	12.5%	0.014	12.2%	0.014	0.4%	0.85
Prefers 495 later over 300 sooner	18.2%	0.016	18.2%	0.016	0.0%	0.99
Prefers 375 later over 300 sooner	13.8%	0.015	19.4%	0.017	-5.7%	0.01 **
Prefers 330 later over 300 sooner	55.6%	0.021	50.3%	0.021	5.3%	0.08 *
Panel B: Choice between 200 Rs Today (sooner) and Larger Amount in 1 Month (later)						
Prefers 200 sooner over 350 later	13.9%	0.015	13.4%	0.015	0.5%	0.81
Prefers 350 later over 200 sooner	10.9%	0.013	13.3%	0.015	-2.3%	0.24
Prefers 250 later over 200 sooner	19.2%	0.017	23.2%	0.018	-4.0%	0.10
Prefers 220 later over 200 sooner	55.9%	0.021	50.1%	0.021	5.8%	0.05 *

Note : The table reports the distribution of choices in two hypothetical intertemporal choice tasks. Panel A reports the choices in a task in which subjects chose between receiving 300 rupees in 1 month and a larger amount in 2 months. Panel B reports the choices in a task in which subjects chose between receiving 200 rupees today and a larger amount in 1 month. The choices in this intertemporal choice tasks allow one to rank subjects according to their willingness to delay gratification. For example, in Panel A subjects who chose 300 in 1 month over 495 in 2 months were the least willing to accept a delayed payment while those who chose 330 in 2 months over 300 in 1 month were the most willing to accept a delayed payment. Columns (1) and (3) show the fraction of respondents who chose each option, separately for treatment (1) and control (3). Columns (2) and (4) report the standard deviations. Columns (5) reports the treatment-control difference in means. Column (6) shows the p-value of a two-sided hypothesis test that the means are the same for the two groups.

Table 5: Treatment Effects on Convex Time Budget (CTB) Choices

Game	Treatment (1) Means	Treatment (2) SD	Control (3) Means	Control (4) SD	Difference in Means (5) (1) - (3)	Hypothesis Test (6) P-value
Panel A: Fraction Choosing to Delay Maximum Amount Possible (Sooner Reward = 50)						
Game 1	53.9%	0.021	50.5%	0.021	3.5%	0.25
Game 2	52.3%	0.021	51.9%	0.021	0.4%	0.89
Game 3	69.2%	0.020	64.0%	0.021	5.2%	0.07 *
Game 4	52.2%	0.021	52.8%	0.021	-0.7%	0.82
All Games	24.7%	0.018	22.7%	0.018	2.1%	0.42
Panel B: Fraction Choosing to Delay Minimum Amount Possible (Sooner Reward = 150)						
Game 1	25.6%	0.019	25.6%	0.019	0.0%	0.99
Game 2	26.2%	0.019	22.5%	0.018	3.7%	0.15
Game 3	15.6%	0.015	17.4%	0.016	-1.8%	0.43
Game 4	24.9%	0.018	28.7%	0.019	-3.8%	0.16
All Games	3.4%	0.008	4.2%	0.009	-0.8%	0.49

Note : The table reports the distribution of choices in the adapted Convex Time Budget (CTB) task. Panel A reports the fraction of subjects who were the most willing to accept a delay payment; they chose a sooner reward of 50 rupees and delayed the maximum amount possible. Panel B reports the fraction of subjects who were the least willing to accept a delay payment; they chose a sooner reward of 150 rupees and delayed the minimum amount possible. Columns (1) and (3) show the fraction of respondents who chose each option, separately for treatment (1) and control (3). Columns (2) and (4) report the standard deviations. Columns (5) reports the treatment-control difference in means. Column (6) shows the p-value of a two-sided hypothesis test that the means are the same for the two groups.

Table 6: Do Treatment and Control Respond Differently to Changes in the Parameters of the Convex Time Budget (CTB) Task?

Changes in the Parameters of the Intertemporal Choice	Treatment (1) Means	Treatment (2) SD	Control (3) Means	Control (4) SD	Difference in Means (5) (1) - (3)	Hypothesis Test (6) P-value
Panel A: Increase in Fraction Choosing to Delay Maximum Amount Possible (Sooner Reward = 50)						
Changing sooner date from today to a month later	-1.6%	0.026	1.5%	0.026	-3.1%	0.40
Increase in interest rate from 10% to 20%	16.9%	0.023	12.1%	0.026	4.8%	0.17
Increase in time delay from 1 month to 5 months	-17.0%	0.026	-11.2%	0.026	-5.9%	0.11
Panel B: Increase in Fraction Choosing to Delay Minimum Amount Possible (Sooner Reward = 150)						
Changing sooner date from today to a month later	0.5%	0.024	-3.1%	0.022	3.7%	0.25
Increase in interest rate from 10% to 20%	-10.6%	0.021	-5.1%	0.021	-5.5%	0.06 *
Increase in time delay from 1 month to 5 months	9.3%	0.022	11.3%	0.022	-2.0%	0.52

Note: The table investigates whether treatment and control groups respond differently to changes in the parameters of the intertemporal choice task, namely the sooner date, the experimental interest rate, and the time interval between the sooner and later dates. Panel A reports the increase in the fraction of subjects most willing to accept a delay payment across two subsequent games. Panel B reports the increase in the fraction of subjects the least willing to accept a delay payment across two subsequent games. From game 1 to game 2, the sooner date was changed from "today" to "in 1 month." From game 2 to game 3 the experimental interest rate was increased from 10% to 20%. Finally, from game 3 to game 4 the time delay between the sooner and later payments was increased from 1 month to 5 months. Columns (1) and (3) show the means, separately for treatment (1) and control (3). Columns (2) and (4) report the standard deviations. Columns (5) reports the treatment-control difference in means. Column (6) shows the p-value of a two-sided hypothesis test that the means are the same for the two groups.

Table 7: Maximum Likelihood Estimation of Preference Parameters

	Background Consumption						
Control group:	**ω_1= 0**	**ω_1= 3,000**	**ω_1= 3,000**	**ω_1= 2,980**	**ω_1= 3,000**	**ω_1= typical[1]**	**ω_1= "shock"[2]**
	ω_2= 0	**ω_2= 3,000**	**ω_2= 3,000**	**ω_2= 3,000**	**ω_2= 3,000**	**ω_2= typical**	**ω_2= typical**
Treatment group:	**ω_1= 0**	**ω_1= 3,000**	**ω_1= 4,000**	**ω_1= 3,000**	**ω_1= 3,000**	**ω_1= typical**	**ω_1= typical**
	ω_2= 0	**ω_2= 3,000**	**ω_2= 4,000**	**ω_2= 3,000**	**ω_2= 3,020**	**ω_2= typical**	**ω_2= typical**
Panel A: Parameter Estimates							
Annual Discount Rate Control $((1/\delta^{12}) - 1)$	26%	26%	26%	21%	28%	28%	26%
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.04]	[0.04]
Annual Discount Rate Treatment $((1/\delta^{12}) - 1)$	24%	24%	24%	26%	19%	25%	26%
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
Intertemporal Elasticity of Substitution Control (θ)	8.73	0.29	0.29	0.32	0.31	0.39	3.04
	[0.57]	[0.02]	[0.02]	[0.02]	[0.02]	[0.05]	[0.23]
Intertemporal Elasticity of Substitution Treatment (θ)	9.35	0.31	0.23	0.33	0.33	0.41	0.42
	[0.65]	[0.02]	[0.02]	[0.02]	[0.02]	[0.04]	[0.04]
Present Bias Control (β)	1.00	1.00	1.00	1.01	1.00	1.00	1.01
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Present Bias Treatment (β)	1.01	1.01	1.01	1.00	1.02	1.01	1.01
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Standard Deviation of Error (σ)	0.20	0.20	0.20	0.18	0.18	0.20	0.20
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Panel B: Hypothesis Tests (P-Values)							
Test Difference in Annual Discount Rates	0.66	0.67	0.67	0.17	0.04	0.52	0.93
Test Difference in Present Bias	0.57	0.57	0.57	0.51	0.09	0.63	0.58
Test Difference in Intertemporal Elasticity of Substitution	0.34	0.35	0.003	0.53	0.23	0.70	0.00
Joint Test Differences in Preference Parameters	0.67	0.67	0.03	0.53	0.02	0.80	0.00
Observations	4,420	4,420	4,420	4,420	4,420	4,268	4,268

Note: Standard errors clustered at the individual level in brackets. Each column reports estimates from a Maximum Likelihood Estimation predicting choice of sooner rewards for each game in the CTB, taking into account the 3 discrete choices available to subjects. The columns differ in the values of background consumption in each period assumed in the model. Column 1 is the "narrow bracketing" case and assumes zero background consumption incorporated in the CTB choices. Columns 2 and 3 assume different levels of static background consumption that are applied to everyone in that treatment or control group in both periods. Columns 4 and 5 assume upward slope of consumption for control and treatment respectively. Columns 5 and 6 allow for individual heterogeneity in background consumption. In Column 5 we use as the measure of background consumption the level of income the household reported earning in a typical week at baseline. This level is held constant in both periods. In Column 6 we allow the background consumption for the earlier period in the CTB for the control group to be a 75/25 mixture between typical weekly income at endline and the reported weekly income the week before the CTB was played. For the treatment group in Column 5 the background consumption is typical weekly income at endline in both periods.

[1]This measure is based on subjects' survey responses at endline (approximately 1 month prior to our choice-task measurements) stating their household weekly income in a typical week.

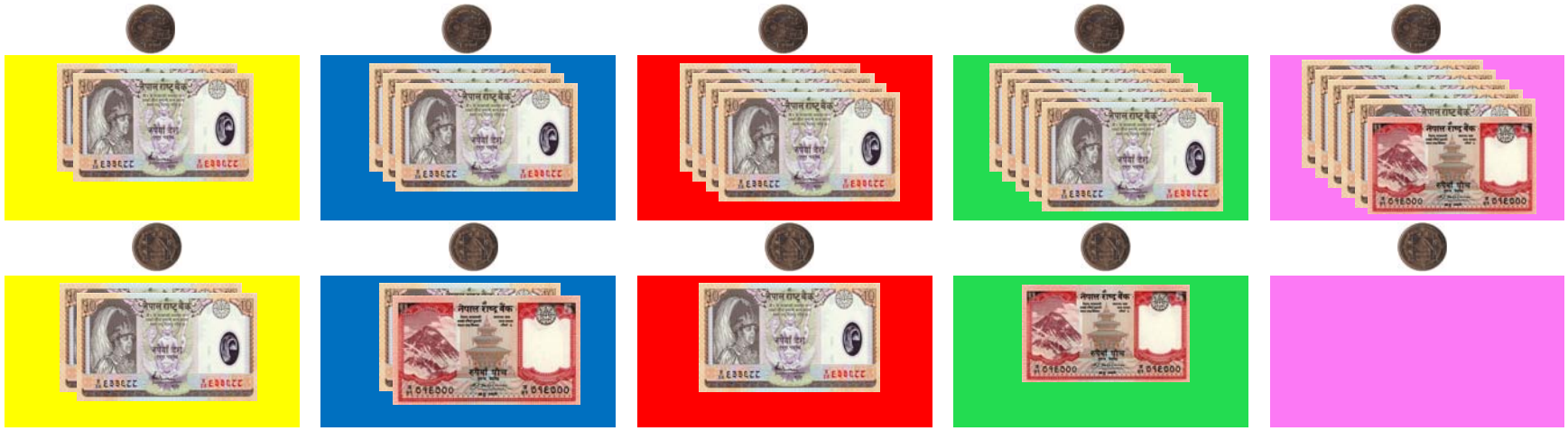
[2] This measure is a mixture of typical weekly income measured at endline and the subjects' reported weekly income in the week prior to the CTB elicitation task. Observed weekly income immediately prior to CTB was on average significantly lower than reported typical income. As such, we construct a measure of background consumption incorporating this negative shock to income for the control group using a weighted average of typical weekly income and observed income in the week prior to CTB, with 75% weight put on typical income.

Table 8: Summary Statistics of Expenditure Data
(First Follow-up Survey; one year after program implementation)

	Bought this item in last …. (30 /7) days?						**Expenditures in Rupees** (including 0's)						**Expenditures in Rupees** (excluding 0's)					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
	Means		Test	Standard Deviation		Test	Means		Test	Standard Deviation		Test	Means		Test	Standard Deviation		Test
	Treatment	Control	(1) = (2)	Treatment	Control	(4) ≥ (5)	Treatment	Control	(7) = (8)	Treatment	Control	(10) ≥ (11)	Treatment	Control	(13) = (14)	Treatment	Control	(16) ≥ (17)
Last 30 days																		
Women's clothing	18%	22%	0.18	0.387	0.411	0.08	279	218	0.52	2,170	590	1.00	1,523	1,013	0.27	4,896	903	1.00
Men's clothing	10%	10%	0.93	0.299	0.301	0.43	126	110	0.67	783	482	1.00	1,279	1,093	0.58	2,191	1,120	1.00
Children's clothing	23%	20%	0.25	0.423	0.404	0.87	219	235	0.76	595	1,057	0.00	942	1,149	0.31	918	2,108	0.00
Women's foowear	20%	18%	0.52	0.398	0.387	0.76	59	50	0.42	168	202	0.00	298	273	0.60	268	404	0.00
Men's footwear	8%	9%	0.86	0.279	0.283	0.36	43	39	0.76	220	188	1.00	509	450	0.59	583	472	0.92
Children's footwear	28%	28%	0.96	0.449	0.448	0.51	130	147	0.49	327	452	0.00	467	528	0.38	477	733	0.00
Medicines and traditional remedies	23%	18%	0.02	0.423	0.382	0.99	766	724	0.87	3,832	4,915	0.00	3,289	4,090	0.52	7,422	11,124	0.00
Health services	16%	18%	0.33	0.364	0.384	0.11	805	1,136	0.49	6,082	9,586	0.00	5,131	6,354	0.66	14,679	22,015	0.00
School fees	43%	39%	0.22	0.495	0.489	0.62	1,031	875	0.25	2,509	1,947	1.00	2,405	2,230	0.54	3,378	2,580	1.00
Textbooks	43%	32%	0.00	0.495	0.468	0.91	692	552	0.06	1,231	1,233	0.48	1,628	1,710	0.59	1,429	1,655	0.02
School uniforms	30%	22%	0.00	0.459	0.414	0.99	362	270	0.03	742	678	0.98	1,199	1,233	0.76	908	955	0.27
School supplies	57%	51%	0.05	0.495	0.500	0.41	351	280	0.02	546	469	1.00	614	547	0.15	600	533	0.98
Personal care ítems	94%	94%	0.79	0.234	0.241	0.24	277	279	0.88	229	284	0.00	294	298	0.82	225	284	0.00
House cleaning articles	23%	22%	0.68	0.418	0.411	0.66	22	18	0.41	103	53	1.00	99	85	0.48	199	87	1.00
Repair and household maintenance	4%	5%	0.68	0.202	0.213	0.10	1,249	1,118	0.82	10,871	7,732	1.00	29,513	23,565	0.57	45,145	27,531	0.99
Festivals	7%	7%	0.75	0.259	0.251	0.77	79	92	0.71	436	700	0.00	1,099	1,366	0.53	1,244	2,376	0.00
Bus and taxi fares	63%	64%	0.76	0.484	0.481	0.55	311	367	0.16	579	727	0.00	495	576	0.16	665	842	0.00
Dowry or bride price	7%	9%	0.13	0.250	0.288	0.00	96	293	0.28	481	4,307	0.00	1,435	3,212	0.44	1,253	14,053	0.00
Funeral expenses	1%	1%	0.44	0.118	0.095	1.00	229	5	0.06	2,769	64	1.00	16,256	530	0.08	17,949	455	1.00
Marriage, birth and other cerimonies	5%	3%	0.08	0.213	0.163	1.00	56	14	0.04	483	111	1.00	1,184	506	0.19	1,921	466	1.00
Last 7 days																		
Cigarettes and tobacco	56%	57%	0.77	0.497	0.495	0.52	84	74	0.17	135	101	1.00	149	129	0.06	150	104	1.00
Alcohol	40%	40%	0.93	0.489	0.490	0.49	156	132	0.28	475	204	1.00	395	332	0.20	691	195	1.00
Gambling	2%	2%	0.88	0.132	0.127	0.80	4	7	0.37	37	73	0.00	222	427	0.17	183	404	0.01
Goat and Lamb	14%	11%	0.09	0.350	0.313	1.00	61	57	0.69	187	203	0.03	429	517	0.12	295	374	0.02
Chicken and Poultry	65%	60%	0.08	0.478	0.491	0.27	204	183	0.18	274	248	0.99	316	308	0.71	284	256	0.98
Bufallo and Beef	29%	29%	0.92	0.454	0.453	0.53	85	80	0.64	191	157	1.00	296	284	0.63	255	173	1.00
Pork	7%	8%	0.78	0.262	0.269	0.27	20	20	0.94	84	75	0.99	273	253	0.52	162	118	0.98
Fish	4%	3%	0.21	0.193	0.158	1.00	10	9	0.80	59	60	0.44	252	346	0.11	175	153	0.69
TOTAL Last 30 Days Items							7,183	6,822	0.68	14,531	14,718	0.38						
TOTAL Cigarettes + Alcohol + Gambling							244	213	0.23	528	280	1.00						
TOTAL Food Items							381	349	0.16	389	368	0.91						
TOTAL All Items							9,845	9,068	0.39	15,113	14,665	0.76						

Note : The table provides summary statistics of the expenditure data collected in the first follow-up survey, one year after the intervention and one year before the experimental tasks were conducted. To compute total expenditures that include the "last 30 days" items and the "last 7 days" items, we multiplied the latter by 30/7. One dollar is approximately equal to 80 rupees.

Appendix: Frames Used in the Lottery-Choice Task



Appendix Figure 1: The five choices in the lottery-choice task

Appendix Figure 2: Hypothetical Choice Between 200 Rs Today and Larger Amount in 1 Month

Imagine a reputable NGO is going to give you some money. You can choose between getting this money sooner or later. No matter what your choice is, you can trust that the NGO will give you this money for sure. If you choose to get it later, you have to wait to get the money but you get more money for sure. Which of these two options would you choose?

Receive 200 Rs today for sure OR Receive 250 Rs in 1 month for sure

What if instead the choice were between these two options, which would you choose?

Receive 200 Rs today for sure OR Receive 330 Rs in 1 month for sure

What if instead the choice were between these two options, which would you choose?

Receive 200 Rs today for sure OR Receive 220 Rs in 1 month for sure

Appendix Figure 3: Hypothetical Choice Between 300 Rs in 1 Month and Larger Amount in 2 Months

Now I would like you to imagine that the same reputable NGO is going to give you a different payment of money. You could get this money in 2 months from today or 3 months from today for sure. If you decide to wait longer, you will receive more money. Which of these two options would you choose?

Receive 300 Rs in 1 month for sure OR Receive 375 Rs in 2 months for sure

What if instead the choice were between these two options, which would you choose?

Receive 300 Rs in 1 month for sure OR Receive 495 Rs in 2 months for sure

What if instead the choice were between these two options, which would you choose?

Receive 300 Rs in 1 month for sure OR Receive 330 Rs in 2 months for sure

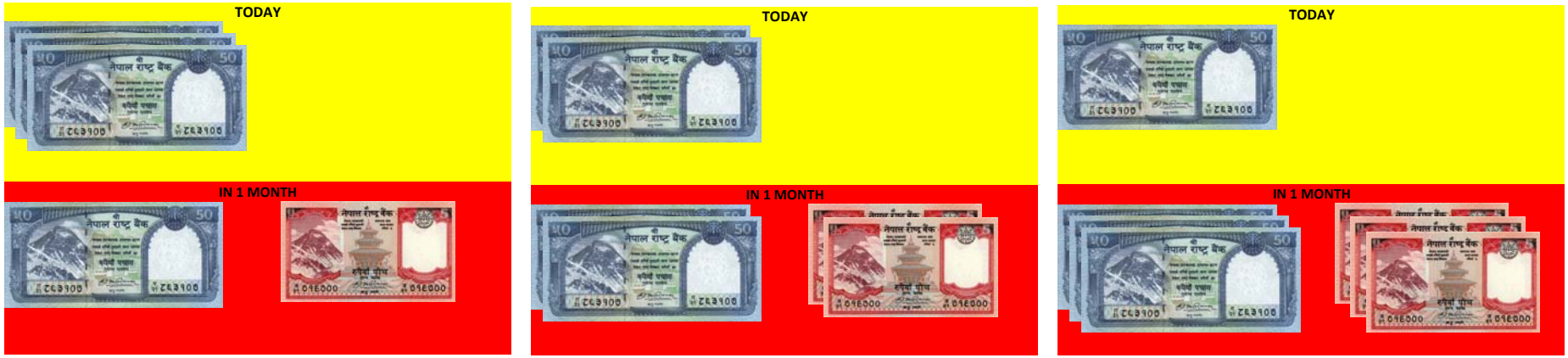
Hypothetical Intertemporal Choice Task

BLOCK M: Time Preferences (from the first follow-up questionnaire)

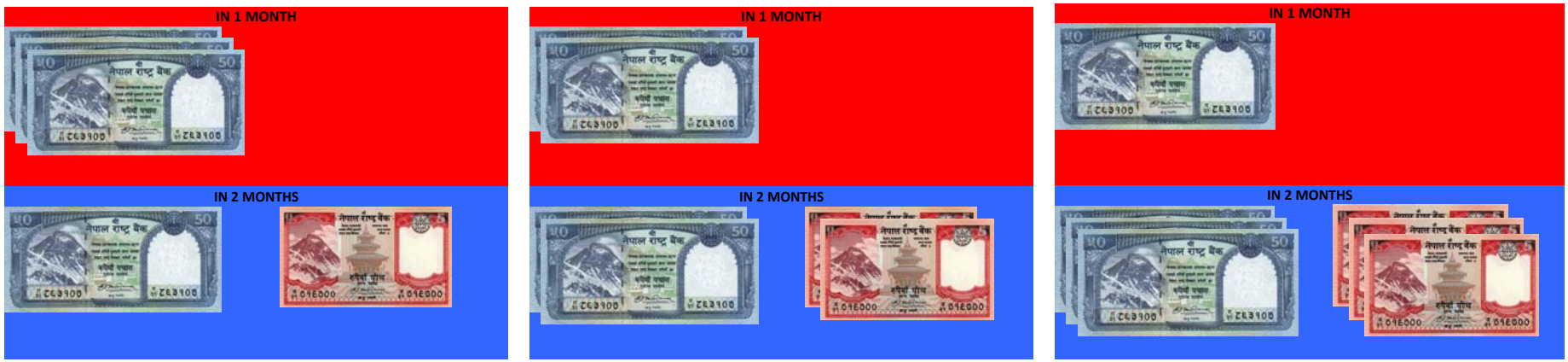
I am no going to ask you some hypothetical questions.
There is no right or wrong answer. I just want to know what you think.

M1	**Imagine a reputable NGO is going to give you some money. You can choose between getting that money sooner or later. No matter what you choice is, you can trust that the NGO will give you this money for sure. If you choose to get it later, you have to wait to get the money but you get more money for sure. Which of these two options would you choose?**	Receive 200 Rs today for sure (next).....1 Receive 250 Rs in one month for sure (skip to M3) 2
M2	**What if instead the choice were between these two options, which would you choose?**	Receive 200 Rs today for sure (skip to M4).....1 Receive 330 Rs in one month for sure (skip to M4) …….....2
M3	**What if instead the choice were between these two options, which would you choose?**	Receive 200 Rs today for sure (next).....1 Receive 220 Rs in one month for sure (next) ……..……..2
M4	**Now I would like you to imagine that the same reputable NGO is going to give you a different payment of money. You could get this money either 2 months from today or 3 months from today for sure. If you decide to wait longer, you will receive more money. Which of these two options would you choose?**	Receive 300 Rs in two months for sure (next).....1 Receive 375 Rs in three month for sure (skip to M6) ……2
M5	**What if instead the choice were between these two options, which would you choose?**	Receive 300 Rs in two months for sure (skip to M7) .....1 Receive 495 Rs in three month for sure (skip to M7) ……..….2
M6	**What if instead the choice were between these two options, which would you choose?**	Receive 300 Rs in two months for sure (next) ……...1 Receive 330 Rs in three month for sure (next) ……..……2
M7	**Do you care more about the present or the future?**	The present .....1 The future …..…….....2

Appendix: Frames Used in Adapted Convex Time Budget (CTB) Task

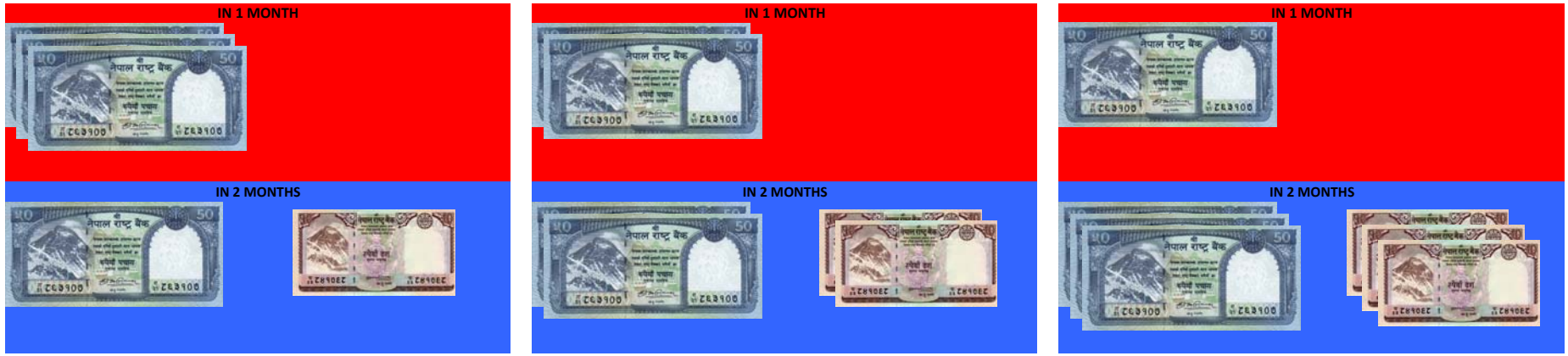


Appendix Figure 4: CTB choice task, game 1 (allocations A, B, and C)

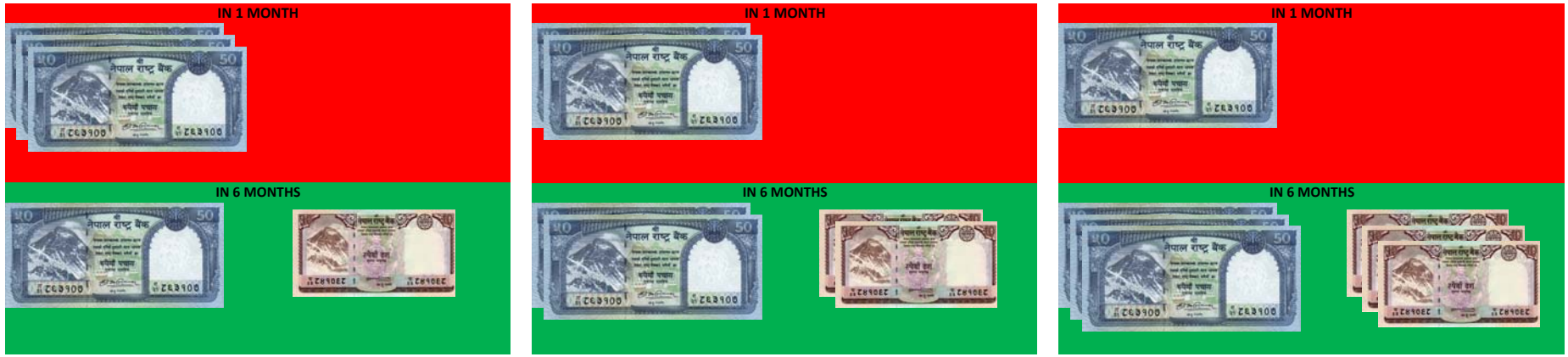


Appendix Figure 5: CTB choice task, game 2 (allocations A, B, and C)

Appendix: Frames Used in Adapted Convex Time Budget (CTB) Task



Appendix Figure 6: CTB choice task, game 3 (allocations A, B, and C)



Appendix Figure 7: CTB choice task, game 4 (allocations A, B, and C)

Instructions for Adapted Convex Time Budget Task

[Before starting to play the game, make sure IN THE CHECKLIST ALL ITEMS ARE CHECKED.

Also make sure that sets 1-2-3-4 lay on top of each other, with set 1 on top. The sets are the following:

- **Set 1 which displays "today – in 1 month" with low reward for waiting**
- **Set 2 which displays "in 1 month – in 2 months" with low reward for waiting**
- **Set 3 which displays "in 1 month – in 2 months" with high reward for waiting**
- **Set 4 which displays "in 1 month – in 6 months" with high reward for waiting**

Before meeting with a new respondent make sure that sets are in the correct order.]

[Opening Instructions]

Good morning, my name is _____

Today we are going to play a game. For participating in this game you will receive some money for sure. You are going to be paid with vouchers that you can redeem at GONESA's main office.

There is no right or wrong answer in this game. We will first practice together, then we will play for real. I will tell you when we will start playing for real.

[Practice #1: Making the Respondent Familiar with the Game's Material]

[1. Take out the Example Frame displaying the 150 today and 165 in 1 month option
Say:]

In this game you will have to choose among 3 different options.

Let me first show you an example of what these options look like.

An option pays money in two dates: some money today and some money in 1 month.

In this game when could you get some money?
(Correct answer: *today and in 1 month*)

The amount of money below the yellow label shows how much money you get paid today. The amount of money below the red label shows how much money you get paid in 1 month.

When do you get paid the amount of money shown below the yellow label?
(Correct answer: *today.*)
When do you get paid the amount of money shown below the red label?
(Correct answer: *in 1 month.*)

The option shown here as an example pays 150 Rs today and 165 Rs in 1 month.

I'll show you now the options you can choose from.

[1. Take out
Set 1 displaying the 3 possible choices for "today – in 1 month"
The example index card
Say:]

In this game you have 3 options and you have to choose one.

How many options do you have in this game?
(Correct answer: 3)

You have to choose among the following options:
- *250 Rs today and 55 Rs in 1 month*
- *150 Rs today and 165 Rs in 1 month*
- *50 Rs today and 275 Rs in 1 month*

So if you choose to wait 1 month to get 100 Rs you will get 10 Rupees more, but you will have to wait 1 month.
And if you choose to wait 1 month to get 200 Rs you will get 20 Rupees more, but you will have to wait 1 month.

If you choose to wait 1 month to get 100 Rs how many Rs more will you get?
(Correct answer: *10 Rupees*)
[If the respondent does not answer correctly, repeat the phrases above and ask again.]

If you choose to wait 1 month to get 200 Rs how many Rs more will you get?
(Correct answer: *20 Rupees*)
[If the respondent does not answer correctly, repeat the phrases above and ask again.]

Now let's make sure that you know what your options are.

If you choose option 1 how many Rs will you get today?
(Correct answer: *250 Rupees*)
If you choose option 1 how many Rs will you get in 1 month?
(Correct answer: *55 Rupees*)

If you choose option 2 how many Rs will you get today?
(Correct answer: *150 Rupees*)
If you choose option 2 how many Rs will you get in 1 month?
(Correct answer: *165 Rupees*)

If you choose option 3 how many Rs will you get today?
(Correct answer: *50 Rupees*)
If you choose option 3 how many Rs will you get in 1 month?
(Correct answer: *275 Rupees*)

[If the respondent does not answer correctly, explain the game again.]
Ok. Now, make a choice pointing at the option you prefer among the 3.

[Let the respondent choose. Point at the option she chose, then ask:]
According to your choice, how much money would you get today?
According to your choice, how much money would you get in 1 month?
[Point at the amount of money they would get today according to her choice, then ask:]
According to you choice, when would get ____ Rupees?
[Point at the amount of money they would get in 1 month according to her choice, then ask:]
According to you choice, when would get ____ Rupees?

[If the respondent does not answer correctly, explain the game again.
If the respondent answers correctly, write down her choice in the example index card.]

I will write down your answer to this practice decision. See this card? I will write your answer on this. Here **[Point at the top part of the index card]** *I will write ____ Rupees today and here* **[Point at the bottom part of the index card]** *I will write ____ Rupees in 1 month.*

[Explaining Which Choice They Get Paid For]

[(1) Record the answer from Practice #1 in the questionnaire (in the line "Practice #1")
2) Keep displaying set 1 showing the 3 possible choices for "today - in 1 month" with high interest rate.
3) Put away:
- the example index card
4) Take out all 4 index cards to show to the respondent that there are numbers written on the back of each card.]

We will play the game for real in a moment.
We will play the game 4 times. In each game you will have to choose between 3 options. One of the 4 games will be selected to be paid and you will be paid the option you chose in the selected game. Now I will explain to you how we will determine which choice you are paid for.

When you make the real decisions, we will record your answers on cards like these.

[(1) Turn over all index cards to show to the respondent that there are numbers written on the back of each card.
2) Take out the dice.]

Then, we will use this dice to decide which card is selected. See the numbers on the back of these cards? We will roll this dice, and then whichever card has the number that comes up on the dice is the card that we will use for your vouchers.

[Give the respondent the dice, and let her roll.
Point at the number on the selected card and say:]

Since the number on the dice is ____, that matches this card.

[Turn the selected card over and say:]

This was just for practice, but if it had been the real decision, since this card won, you would get paid ____ Rupees (time in the top part of the selected card) and ____ Rupees (time in the bottom part of the selected card). You would get two vouchers.

[Show her 2 example vouchers and say:]

You would receive one voucher that you could redeem starting (time in the top part of the selected card) for ____ Rupees and a second voucher that you could redeem starting (time in bottom part of the selected card) for ____ Rupees.

The important thing to remember when playing the game is that any of the choices you make could end up being the one you get paid for. So it is important to always make careful decisions and think about which option you really prefer.

Actual Game

[1) Put away all index cards and the dice
2) Keep displaying Set 1
3) Take out index card #1
4) Say:]

****

GAME 1

Let's play the real game for real now for the first time.
As before you have 3 options and you have to choose one.

In this game you can get paid some money today and some money in 1 month.

In this game when could you get some money?
(Correct answer: *today AND in 1 month*)

The amount of money below the yellow label shows how much money you get paid today. The amount of money below the red label shows how much money you get paid in 1 month.

When do you get paid the amount of money shown below the yellow label?
(Correct answer: *today.*)
When do you get paid the amount of money shown below the red label?
(Correct answer: *in 1 month.*)

You have to choose among the following options:
- *250 Rs today and 55 Rs in 1 month*
- *150 Rs today and 165 Rs in 1 month*
- *50 Rs today and 275 Rs in 1 month*

So if you choose to wait 1 month to get 100 Rs you will get 10 Rupees more, but you will have to wait 1 month.
And if you choose to wait 1 month to get 200 Rs you will get 20 Rupees more, but you will have to wait 1 month.

Ok. Now, make a choice pointing at the option you prefer among the 3. When you have decided, I will write down your answers on this card. Remember that later, we will roll a dice, and this card could end up being the one that wins and you get paid for. So please think very carefully about the money you want today and the money you want in 1 month.

[Let the respondent choose then point at the amount of money she would get today, according to her choice, then ask:]
According to your choice, how much money would you get today?
[Point at the amount of money she would get today, according to her choice, then ask:]
According to your choice, how much money would you get in 1 month?

The way you have chosen, you could get ____ Rupees today, and ____ Rupees in 1 month. Do you like this choice, or do you want to try again?

[1) Let the respondent think as much as she wants and let her ask questions.
2) Once she is satisfied, write the Rupees amount on index card #1
3) Record the answer from Game #1 in the questionnaire (in the line "Game #1")
4) Put index card #1 on the right hand side of the respondent with the card number in display.
5) Put away Set 1 so that Set 2 shows.
6) Then, take out index card #2
7) Say:]

****

GAME 2

Now let's play the game for real for a second time.
As before you have 3 options and you have to choose one.
However, now you can get paid some money in 1 month and some money in 2 months.

In this game when could you get some money?
(Correct answer: *in 1 month AND in 2 months*)

The amount of money below the red label shows how much money you get paid in 1 month.
The amount of money below the blue label shows how much money you get paid in 2 months.

When do you get paid the amount of money shown below the red label?
(Correct answer: *in 1 month.*)
When do you get paid the amount of money shown below the blue label?
(Correct answer: *in 2 months.*)

You have to choose among the following options:
- *250 Rs in 1 month and 55 Rs in 2 months*
- *150 Rs in 1 month and 165 Rs in 2 months*
- *50 Rs in 1 month and 275 Rs in 2 months*

So if you choose to wait 2 months instead of 1 month to get 100 Rs you will get 10 Rupees more, but you will have to wait 2 months instead of 1 month.
And if you choose to wait 2 months instead of 1 month to get 200 Rs you will get 20 Rupees more, but you will have to wait 2 months instead of 1 month.

Ok. Now, make a choice pointing at the option you prefer among the 3. When you have decided, I will write down your answers on this card. Remember that later, we will roll a dice, and this card could end up being the one that wins and you get paid for. So please think very carefully about the money you want in 1 month and the money you want in 2 months.

[Point at the amount of money they would get today according to her choice, then ask:]
According to your choice, how much money would you get in in 1 month?
[Point at the amount of money they would get today according to her choice, then ask:]
According to your choice, how much money would you get in 2 months?

The way you have chosen, you could get ____ Rupees in 1 month, and ____ Rupees in 2 months.
Do you like this choice, or do you want to try again?

[1) Let the respondent think as much as she wants and let her ask questions.
2) Once she is satisfied, write the Rupees amount on index card #2
3) Record the answer from Game #2 in the questionnaire (in the line "Game #2")
4) Put index card #2 on the right hand side of the respondent with the card number in display.
5) Put away Set 2 so that Set 3 shows.
6) Then, say:]

[Practice #2: The Respondent Practices Playing the Game once again]

[1) Take away:
- Set 2 displaying the 3 possible choices for "in 1 month – in 2 months" with low interest rate
2) Take out:
- Set 3 displaying the 3 possible choices for "in 1 month – in 2 months" with high interest rate]

Now let's practice playing the game one more time, before you get to play again. The difference is that now you get even more money if you decide to wait.

Now if you choose to wait 2 months instead of 1 month to get 100 Rs you will get 40 Rupees more, but you will have to wait 2 months instead of 1 month.
And if you choose to wait 2 months instead of 1 month to get 200 Rs you will get 80 Rupees more, but you will have to wait 2 months instead of 1 month.

If you choose to wait 2 months to get 100 Rs how many Rs more will you get?
(Correct answer: *40 Rupees*)
[If the respondent does not answer correctly, repeat the phrases above and ask again.]

If you choose to wait 2 months to get 200 Rs how many Rs more will you get?
(Correct answer: *80 Rupees*)
[If the respondent does not answer correctly, repeat the phrases above and ask again.]

As before, the amount of money below the red label shows how much money you get paid in 1 month. The amount of money below the blue label shows how much money you get paid in 2 months.

You have to choose among the following options:
- *250 Rs in 1 month and 70 Rs in 2 months*
- *150 Rs in 1 month and 210 Rs in 2 months*
- *50 Rs in 1 month and 350 Rs in 2 months*

Ok. Now, make a choice pointing at the option you prefer among the 3.

[Let the respondent choose then say:]
According to your choice, how much money would you get in 1 month?
According to your choice, how much money would you get in 2 months?

[Point at the amount of money they would get in 1 month according to her choice, then ask:]
According to you choice, when would get ____ Rupees?

[Point at the amount of money they would get in 2 months according to her choice, then ask:]
According to you choice, when would get ____ Rupees?

[1) Record the answer from Practice #2 in the questionnaire (in the line "Practice #2")
2) KEEP DISPLAYING set 3 showing the 3 possible choices for "in 1 month – in 2 months" with high interest rate.]

****

GAME 3

Now let's play the game for real for a third time.
As before you have 3 options and you have to choose one.
In this game you can get paid some money in 1 month and some money in 2 months.

In this game when could you get some money?
(Correct answer: *in 1 month AND in 2 months*)

The amount of money below the red label shows how much money you get paid in 1 month.
The amount of money below the blue label shows how much money you get paid in 2 months.

When do you get paid the amount of money shown below the red label?
(Correct answer: *in 1 month.*)
When do you get paid the amount of money shown below the blue label?
(Correct answer: *in 2 months.*)

You have to choose among the following options:
- *250 Rs in 1 month and 70 Rs in 2 months*
- *150 Rs in 1 month and 210 Rs in 2 months*
- *50 Rs in 1 month and 350 Rs in 2 months*

So if you choose to wait 2 months instead of 1 month to get 100 Rs you will get 40 Rupees more, but you will have to wait 2 months instead of 1 month.
And if you choose to wait 2 months instead of 1 month to get 200 Rs you will get 80 Rupees more, but you will have to wait 2 months instead of 1 month.

Ok. Now, make a choice pointing at the option you prefer among the 3. When you have decided, I will write down your answers on this card. Remember that later, we will roll a dice, and this card could end up being the one that wins and you get paid for. So please think very carefully about the money you want in 1 month and the money you want in 2 months.

[Point at the amount of money they would get today according to her choice, then ask:]
According to your choice, how much money would you get in in 1 month?
[Point at the amount of money they would get today according to her choice, then ask:]
According to your choice, how much money would you get in 2 months?

The way you have chosen, you could get ____ Rupees in 1 month, and ____ Rupees in 2 months.
Do you like this choice, or do you want to try again?

[1) Let the respondent think as much as she wants and let her ask questions.
2) Once she is satisfied, write the Rupees amount on index card #3
3) Record the answer from Game #3 in the questionnaire (in the line "Game #3")
4) Put index card #3 on the right hand side of the respondent with the card number in display.
5) Put away Set 3 so that Set 4 shows.
6) Then, take out index card #4
7) Say:]

****

GAME 4

Now let's play the game for real for a fourth time.
As before you have 3 options and you have to pick one.
However, now you can get paid some money in 1 month and some money in 6 months.

In this game when could you get some money?
(Correct answer: *in 1 month AND in 6 months*)

Notice that now you have to wait even more if you want to get paid more!!!
Now to get more Rs you have to wait 6 months, not 2 months!

Now, how many months do you have to wait to get more Rupees?
(Correct answer: *6 months*)
[If the respondent does not answer correctly, repeat the phrase above and ask again.]

The amount of money below the red label shows how much money you get paid in 1 month.
The amount of money below the green label shows how much money you get paid in 6 months.

When do you get paid the amount of money shown below the red label?
(Correct answer: *in 1 month.*)
When do you get paid the amount of money shown below the green label?
(Correct answer: *in 6 months.*)

You have to choose among the following options:
- *250 Rs in 1 month and 70 Rs in 6 months*
- *150 Rs in 1 month and 210 Rs in 6 months*
- *50 Rs in 1 month and 350 Rs in 6 months*

So if you choose to wait 6 months instead of 2 months to get 100 Rs you will get 40 Rupees more, but you will have to wait 6 months instead of 2 months.
And if you choose to wait 6 months instead of 2 months to get 200 Rs you will get 80 Rupees more, but you will have to wait 6 months instead of 2 months.

If you choose to wait 6 months to get 100 Rs how many Rs more will you get?
(Correct answer: *40 Rupees*)
[If the respondent does not answer correctly, repeat the phrases above and ask again.]

If you choose to wait 6 months to get 200 Rs how many Rs more will you get?
(Correct answer: *80 Rupees*)
[If the respondent does not answer correctly, repeat the phrases above and ask again.]

Ok. Now, make a choice pointing at the option you prefer among the 3. When you have decided, I will write down your answers on this card. Remember that later, we will roll a dice, and this card could end up being the one that wins and you get paid for. So please think very carefully about the money you want in 1 month and the money you want in 6 months.

[Point at the amount of money they would get today according to her choice, then ask:]
According to your choice, how much money would you get in in 1 month?
[Point at the amount of money they would get today according to her choice, then ask:]
According to your choice, how much money would you get in 6 months?

The way you have chosen, you could get ____ Rupees in 1 month, and ____ Rupees in 6 months.
Do you like this choice, or do you want to try again?

[1) Let the respondent think as much as she wants and let her ask questions.
2) Once she is satisfied, write the Rupees amount on index card #4
3) Record the answer from Game #4 in the questionnaire (in the line "Game #4")
4) Put index card #4 on the right hand side of the respondent with the card number in display.
5) Put away all the material except the 4 index cards and the questionnaire.
6) Say:]

Before we roll the dice to determine which of the choices you just made will be the one you get paid, we would like to ask you a few questions

FILL IN QUESTIONS A1-A6 BEFORE GOING AHEAD WITH THE PROTOCOL

I would like to ask some detailed questions about your savings.
Please let me remind you that any information that you will provide will be kept strictly confidential. This means that no one inside or outside your community will know about it.

A1	**How much savings do you and your household have right now? Please include cash at home, savings in a bank, in a savings organization, in a DHUKUTI, etc.** (Prompt answer)	Rupees [ ][ ][ ][ ][ ][ ] If none, write 0 If the respondent does not know write 99. If the respondent does not want to answer write 88.

Now I would like to ask some detailed questions about your income.
Please let me remind you that any information that you will provide will be kept strictly confidential. This means that no one inside or outside your community will know about it.

Control Variable: **NO. OF SOURCE OF INCOME(only for data entry purpose):** _____

	A2	**A3**
List the different sources of household cash income starting with the most relevant.	**Source of household cash income.** (Use the codes listed below)	**Please give me your best estimate of your household cash income <u>LAST WEEK.</u>** Amount in Rupees
1st source of income		
2nd source of income		
3rd source of income		
4th source of income		
5th source of income		

<u>CODE for G1 (Source of household income):</u>

1. Income from sales of agr.production
2. Income from agricultural labor
3. Income from livestock and poultry
4. Income from sand and stone collection
5. Income from constr. and masonry
6. Driver
7. Bus fare collector
8. Helper
9. Income from a small shop
10. Garnment and wool spinning
11. Jewelry income
12. Government job (full time)
13. Teacher
14. Pension
15. Rent
16. Remittances
17. Other_____
18. Alcohol making
19. Private Job (full time)
20. Partime/temporary job not listed in the previous sources of income

Now I would like to ask some detailed questions about your loans.
Please let me remind you that any information that you will provide will be kept strictly confidential. This means that no one in the community or outside the community will know about it.

A4	**How much money do you and your household currently owe?** (Prompt answer)	Rupees [ ][ ][ ][ ][ ][ ] If the respondent does not know write 99. If the respondent does not want to answer write 88. (If household don't have any loan, Write "0" and skip to BLOCK D, but please make sure the household did not borrow any money asking all the options mentioned in H2 below)

I will now ask about all the purchases made for your household in the LAST WEEK, regardless of which person made them.
INSTRUCTIONS: Write the answer or the code corresponding to the answer given by the respondent in the appropriate space below.

	A5	**A6**
	How many days did your household eat .....? DNK … 99. DNA …88.	**How many days in a typical week during the next month do you think your household will eat .....?** DNK … 99. DNA …88
Item	**Days**	**Days**
1. Goat/Lamb		
2. Chicken/Poultry		
3. Buffalo/Beef		
4. Pork		
5. Fish		

Instructions for Lottery-Choice Task

[1) Before starting to play the Head-Tail game, make sure you have put next to the respondent, but not visible, the 4 index cards.
2) Take out:
- The Head Tail Example Frame displaying *40 Rs if the* coin lands on *heads and 10 Rs if the* coin lands on *tails.*
- The coin for the Head-Tail Game
Say:]

Before we roll the dice to decide which card is selected, we will play one last game.
For participating in this game you will receive some money for sure. You are going to be paid with a third voucher that you can redeem at GONESA's main office starting in 1 month.
There is no right or wrong answer in this game. Let me explain you first how we play this game.
How much money you win in this game will depend on what comes up when you toss this coin here. **[Hand the subject the coin and let them look at it]**
In this game you have to choose among five different options.

Here is an example of how an option in this game looks like.
[Point at the Head Tail Example Frame]
An option pays one amount if the coin lands on "heads" and a different amount if the coin lands on "tails".

The amount of money above the coin landing heads up shows how much money you get paid if the coin lands on heads. The amount of money above the coin landing tails up shows how much money you get paid if the coin lands on tails.

When do you get paid the amount of money shown above the coin landing heads?
[Point at the coin landing heads]
(Correct answer: If the coin lands on *heads.*)
How much do you get paid if the coin lands heads?
(Correct answer: *40.*)

When do you get paid the amount of money shown above the coin landing tails up?
[Point at the coin landing tails]
(Correct answer: If the coin lands on *tails.*)
How much do you get paid if the coin lands tails?
(Correct answer: *10.*)

The option shown here as an example pays 40 Rs if the coin lands on *heads and 10 Rs if the* coin lands on *tails.*

This was only an example. Let me show you now the options you can choose from.

[1) Record the answer from this Practice in the questionnaire (in the line "Practice Head Tail").
2) Put away the Head Tail Example Frame.
3) Take out the Head-Tail set made of 5 laminated colored papers representing the 5 possible choices.
4) Point to the first option and say:]

You have to choose among the following options:

- *20 Rs if the* coin lands on *heads and 20 Rs if the* coin lands on *tails*
- *30 Rs if the* coin lands on *heads and 15 Rs if the* coin lands on *tails*
- *40 Rs if the* coin lands on *heads and 10 Rs if the* coin lands on *tails*
- *50 Rs if the* coin lands on *heads and 5 Rs if the* coin lands on *tails*
- *55 Rs if the* coin lands on *heads and 0 Rs if the* coin lands on *tails*

If you choose one of the options where you get more money when the coin lands on heads, then you get less money if the coin lands instead on tails.
Now let's make sure that you know what your options are.

If you choose yellow how many Rs will you get if the coin lands on *heads?*
(Correct answer: *20 Rupees*)
If you choose yellow how many Rs will you get if the coin lands on *tails?*
(Correct answer: *20 Rupees*)

If you choose blue how many Rs will you get if the coin lands on *heads?*
(Correct answer: *30 Rupees*)
If you choose blue how many Rs will you get if the coin lands on *tails?*
(Correct answer: *15 Rupees*)

If you choose red how many Rs will you get if the coin lands on *heads?*
(Correct answer: *40 Rupees*)
If you choose red how many Rs will you get if the coin lands on *tails?*
(Correct answer: *10 Rupees*)

If you choose green how many Rs will you get if the coin lands on *heads?*
(Correct answer: *50 Rupees*)
If you choose green how many Rs will you get if the coin lands on *tails?*
(Correct answer: *5 Rupees*)

If you choose pink how many Rs will you get if the coin lands on *heads?*
(Correct answer: *55 Rupees*)
If you choose pink how many Rs will you get if the coin lands on *tails?*
(Correct answer: *0 Rupees*)

[If the respondent does not answer correctly, explain the game again.]
Ok. Now, make a choice pointing at the option you prefer among the 5.

[Let the respondent choose.
*You have selected the [**say the color]** option.*
Point at the option she chose, then ask:]
According to your choice, how much money will you get if the coin lands on *heads?*
According to your choice, how much money will you get if the coin lands on *tails?*
[If the respondent does not answer correctly, explain the game again.

[1) Allow the participant to change her choice if she wants and then repeat the question.
2) Continue until she is sure of her choice.
3) Record the answer from the Head-Tail Game in the questionnaire (in the line "Head-Tail Game").]

Thank you for being patient with this game. Now, toss the coin to see how much you will get paid.

[1. Give the coin to the respondent to toss.
2. Show her how much she gets.]

> *Since the coin landed on _____, this is the money you will get.*
> *I will now fill out the vouchers for this amount.*

[1. Fill out the voucher and let the respondent clearly see that you are writing out the voucher to match her choice.
2. Record the voucher amount in the ledger.
3. Sign the voucher
4. Give the voucher to the respondent.]

Choosing an allocation (Card selection) for Game 1

[1. Put away the questionnaire
2. Take the dice
3. Put all 4 index cards in front of the respondent and display their numbers.
4. Say:]

> *Thank you for being patient with all of these decisions and questions. Now, roll the dice to find out which of your choices, for the game we played at the beginning, you will be paid for.*

[1. Give the dice to the respondent to roll.
2. Show her which card wins.]

> *Since you rolled a _____, this is the card that matches.*
> *I will now fill out the vouchers for this card.*

[1. Let the respondent hold the index card that was chosen.
2. Fill out the vouchers and let the respondent clearly see that you are writing out the vouchers to match her choice.
3. Record the voucher amounts in the ledger.
4. Sign the vouchers
5. Give the vouchers to the respondent
6. Get the index card back and erase the content.]

Enumerator (left) explains to subject (right) how much she will be paid if she chooses option A in game 2: 150 rupees in 1 month (shown in the red rectangle) and 55 rupees in 2 months (shown in the blue rectangle). The options were introduced one by one.

Enumerator (right) writing down the choice of subject (left) in the lottery choice task. The 5 options – in 5 different colors – were shown side by side. The upper rectangles showed the payments if a coin landed on tails. The lower rectangles showed the payment if it landed on tails.