Poverty Negates the Impact of Social Norms on Cheating

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Abstract

Cheating such as corruption and tax evasion are prevalent in the developing world; therefore, many interventions have been undertaken to reduce cheating in developing countries. Although some field evidence shows that poverty is correlated with cheating, the *causal* effect of poverty on cheating in the field and the effectiveness of interventions on financially constrained people remain an open question. We present results from a lab-in-the-field experiment with low-income rice farmers in Thailand ($N = 568$), in which we, first, investigate the *causal* effect of poverty on cheating and, second, test whether poverty affects the effectiveness of a social-norm intervention to reduce cheating. We show that poverty itself does not affect willingness to cheat. However, although a social-norm-reminder intervention reduced cheating when the population was richer (after harvest), it had no effect when the population was poorer (before harvest). Our results suggest that the timing of interventions to change behavior might matter.

JEL Classification: C91, C93, D82, D91

Keywords: cheating; lying; poverty; social norms; interventions; lab-in-the-field experiment

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1. Introduction

There has long been debate in economics on whether poverty fosters or dampens prosocial behavior. Empirically, the question remains open. One strand of empirical literature using field data showed that poor or lower-social-class individuals are more generous, charitable, trusting, willing to help, and less likely to cheat (Piff et al., 2010; Piff, et al., 2012). Other scholars found that, in contrast, poor or lower-social-class individuals are less trusting, less trustworthy, less intrinsically motivated, more likely to behave antisocially, and less likely to enforce sharing (Bartos, 2016; Glaeser et al., 2000; Haushofer, 2013; Haushofer and Fehr, 2014; Jiang and Lim, 2018; Prediger et al., 2013; Prediger et al., 2014; Shalvi, 2016). Finally, some scholars found no effect of poverty or lower social class on cheating, giving, or cooperating (Aksoy and Palma, 2019; Andreoni et al., 2017; Bartos, 2016; Gächter and Schulz, 2016; Prediger et al., 2013).

The mixed empirical evidence may be due to factors such as differences in experimental design and procedure as well as different ways of looking at poverty (relative vs. absolute).[1] Importantly, a large fraction of the studies are *correlational* (Andreoni et al., 2017; Gächter and Schulz, 2016; Glaeser et al., 2000; Haushofer, 2013; Haushofer and Fehr, 2014; Jiang and Lim, 2016; Piff et al., 2010; Piff et al., 2012; Shalvi, 2016), and only a few studies used an exogenous income shock to establish a causal link between poverty and prosocial/antisocial behavior (Aksoy and Palma, 2019; Bartos, 2016; Prediger et al., 2013; Prediger et al., 2014; see Sharma et al., 2014, for a lab study). In other words, most of the field studies cannot show a *causal* effect of poverty on prosocial behavior because they cannot exclude reverse causality, that is, that prosocial preferences predict income and not the other way around, or cannot exclude omitted variable bias, that is, a third unobservable factor affecting both income and prosocial behavior.

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[1] Some of the papers mentioned above (e.g., Piff et al., 2012) looked at relative poverty; others (e.g., Bartos, 2016) also considered absolute poverty.

Our investigation focused on providing a *causal* link between poverty and behavior in a field setting. In particular, we investigated how poverty affects cheating behavior. Many everyday interactions are based on asymmetric information (i.e., I know something that you do not know), which tempts some people to cheat for monetary benefit such as soliciting bribes or evading taxes. The economic and social consequences of cheating are substantial (Mauro, 1995; Olken and Pande, 2012; Pranab, 1997); thus, understanding the factors that influence cheating decisions is important. Although there is robust experimental evidence about the effect of the cost of lying, gender, culture, or probability of detection on cheating in the lab (Abeler et al., 2019; Fischbacher and Föllmi-Heusi, 2013; Gächter and Schulz, 2016; Gneezy et al., 2013; Gneezy et al., 2018; Kajackaite and Gneezy, 2017; Mazar et al., 2008), there is limited evidence about whether poverty causally affects the decision to cheat in the field. There is also no evidence about whether poverty affects the effectiveness of interventions to reduce cheating—despite the considerable funds spent creating and implementing such interventions. In a number of studies, poverty (or scarcity of another kind) was shown to reduce cognitive capacity and cognitive performance. The main evidence showing that scarcity reduces cognitive capacity is the experimental evidence that cognitive function scores are lower in a scarcity period than in an abundance period for the *same* people (Mani et al., 2013; Mullainathan and Shafir, 2013). Therefore, poverty can easily reduce the effectiveness of interventions requiring cognitive capacity.

The goal of this study was therefore twofold. First, we were interested in the *causal* effect of poverty on cheating behavior in the field, in general. For this purpose we recruited a unique population of Thai rice farmers, let them play a simple cheating game (Gneezy et al., 2018) and exploited the differences in financial constraints they face before harvest (when they are poor)

and after harvest (when they are richer; similar to Bartos, 2016; Mani et al., 2013). Second, we explored the effectiveness of an intervention to reduce cheating when our participants face different levels of financial constraints. For this, we used a popular instrument—a social-norm reminder. We believe that it is important not only to analyze the effect of poverty on the general tendency of ethical and prosocial behavior but also to consider how interventions can achieve desirable behavior when the target population faces (or does not face) poverty.

Whether poverty causes more cheating is ultimately an empirical question, as there are arguments for why being financially constrained can increase or not change the willingness to cheat. On one hand, clearly, being financially constrained increases the need for cash now and also increases the marginal utility of money (Carvalho et al., 2016). As a result, the immediate need for money can lead to more cheating than when one is less financially constrained. On the other hand, there is vast experimental lab evidence that cheating can be insensitive to many parameters such as stakes (Abeler et al., 2019; Kajackaite and Gneezy, 2017). If experimental participants do not react to changes in stakes while cheating, then they might not react to changes in income.

In terms of the effectiveness of a social-norm-reminder intervention, the social-norm reminder has proved to work well and is a powerful tool in many contexts (Frey and Meier, 2004; Goldstein et al., 2008; Schultz et al., 2007; Thaler and Sunstein, 2008), including tax compliance (e.g., Hallsworth et al., 2017 [conducted in the UK]; Del Carpio, 2014 [conducted in Peru]). We therefore expect the norm-reminder intervention to work well in our sample, especially when people are not financially constrained.[2] However, poverty might influence the

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[2] Despite their popularity among policymakers and many success stories, there are some cases where social-norm interventions were unsuccessful in changing behavior such as cheating or tax compliance (e.g., Castro & Scartascini, 2015 [Argentinian sample]; Fellner et al., 2013 [Austrian sample]). In these studies, social-norm reminders had no effect overall but worked depending on beliefs about evasion behavior of others (Fellner et al., 2013) or depending

intervention's effectiveness. Previous experimental evidence showed that scarcity reduces one's cognitive capacity because it captures one's attention (Mani et al., 2013; Mullainathan and Shafir, 2013; Shah et al., 2012). In other words, scarcity leads to "tunneling": being (financially) constrained consumes many cognitive resources itself and leaves one with fewer cognitive resources for other domains (because of the human cognitive system having limited capacity; Baddeley and Hitch, 1974; Miller, 1956; Neisser, 1976). Following on this experimental evidence, we argue that in our experiment individuals in poverty may lack the cognitive bandwidth to digest the norm reminder and may be less likely to react to it. The reasoning is simple: the more cognitive capacity is used for managing poverty, the fewer cognitive resources can be used for managing the social-norm reminder. In addition, even if our experimental individuals in poverty have enough cognitive bandwidth to internalize the norm reminder, they have fewer financial possibilities to adopt the desired behavior than richer participants because they need the cash more. In other words, poor participants have less space for "maneuvering" than richer participants. Put together, either due to the "tunneling" or limited financial possibility to follow the norm or both, we expect norm interventions to work worse when our participants face poverty.

We conducted a large-scale lab-in-the-field experiment with rice farmers in rural Thailand to test the hypotheses described above. We used a simple lying game and social norm reminders in a lab-in-the-field setting to measure cheating behavior and effectiveness of social-norm intervention to reduce cheating before and after harvest. We found that poverty itself does

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on past compliance behaviors (Castro & Scartascini, 2015). However, because Thailand is a highly collectivist country according to the Hofstede model of national culture (Hofstede, 1984; see www.hofstede-insights.com for current information on national culture of over 100 countries) and because people in collectivist societies tend to conform more to social norms than people in individualistic cultures (Bond & Smith, 1996; Cialdini et al., 1999), we expect the social-norm reminder to work better in our sample than in the individualistic Austrian and Argentinian samples used in the examples above.

not affect willingness to cheat – that is, the farmers cheated to a similar extent before and after harvest. When reminded of the social norm, the participants did not cheat less when they were poorer (before harvest), but the social-norm-reminder intervention reduced cheating when the participants were richer (after harvest). This result suggests that the timing of interventions to change behavior might matter.

The remainder of the paper is organized as follows: In section 2, we describe the unique financial situation of our participants, the experimental design and procedure. Section 3 provides the experimental results. In section 4, we discuss the results and provide policy implications.

2. Experimental design and procedure

In the experiment, we use a 2x2 between-subjects design with four treatments, varying the financial situation of the participants (before harvest vs. after harvest) and the norm reminder (baseline cheating game vs. norm-reminder game). In this section, we first demonstrate the financial differences between the participants before and after harvest and later describe the experimental treatments and procedure.

2.1 Participants and their financial situation before and after harvest

For our experiment, we recruited 568 rice farmers from 48 villages in rural Thailand. These rice farmers have generally a low income (93% are eligible to apply for a government subsidy for grocery; see Appendix A for more sociodemographic characteristics of our sample). Rice is important for our participants. They use it for subsistence consumption; it constitutes their main source of nutrition. Also, most (65% of all participants) sell their rice on the market, a large and important additional source of income. The income from other activities throughout the year is not high, and the investment in rice production is substantial (18% of total annual expenditures).

Importantly for our study, rice cultivation happens just once a year, at the beginning of the rainy season, because of water availability. As a result, the farmers have difficulties in smoothening their consumption over the year. Therefore, they are relatively poor before harvest but relatively rich after harvest. This exogenous income generation caused by the harvest allows us to investigate a *causal* effect of poverty on cheating and is the reason why we chose the Thai rice farmers as the subject pool for our experiment.

The experiment took place in 48 villages in Ubon Ratchathani, northeast Thailand. 283 farmers participated in our experiment before harvest (September 2017) and another 285 farmers participated after harvest (December 2017).[3] The before- and after-harvest farmers are from the same subdistricts but different villages (24 villages before harvest and another 24 villages after harvest) and were randomly assigned to treatments and experimental sessions. The before- and after-harvest farmers do not differ in sociodemographic characteristics such as age, gender, education, or family size (see Appendix A), which shows that our randomization worked.

The data collected in a post-experimental questionnaire showed that the rice farmers are indeed much poorer before harvest than after harvest—confirming our empirical strategy (see Table 1). On average, the before-harvest participants reported 73% lower household income in the relevant month than the after-harvest participants (mean income $M_{before} = ฿11,533$, $SD_{before} = ฿14,379$ vs. $M_{after} = ฿42,442$, $SD_{after} = ฿59,925$; $t(557) = 8.38$, $p < 0.001$). It follows that the effective income—computed by dividing household income by the square root of household size—is also 73% lower before harvest than after harvest ($M_{before} = ฿5,467$, $SD_{before} = ฿6,894$ vs. $M_{after} = ฿20,217$, $SD_{after} = ฿29,427$; $t(557) = 8.15$, $p < 0.001$). Furthermore,

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[3] Since there were four experimental conditions in the experiment, we had around 140 independent observations per cell. This sample size is higher than the sample sizes used in the remaining literature using similar random-draw games. We set the sample size based on our financial limitations. We predefined the stopping rule of collecting the data—we decided to not run any further sessions after visiting the 48 set villages.

the reported household debt is 28% higher before harvest (M_{before} = ฿192,086,

SD_{before} = ฿228,264 vs. M_{after} = ฿138,338, SD_{after} = ฿164,093; $t(557)$ = 3.20, $p < 0.01$), and the

before-harvest participants are less likely to have savings of any kind, including livestock (76%

vs. 95%; test of proportion, $Z = 6.45$, $p < 0.001$).

Table 1. Before- and after-harvest differences in financial situation

	HH Income	HH Expenditures	Amount of HH Debt standing	Individual Savings (dummy)
OLS {After-harvest}	฿31,241 [3,647]***	฿7,111 [1,003]***	-฿52,698 [16,711]***	0.19 [0.03]***
Constant	-฿7,296 [5,454]	฿3,196 [1,743]*	฿132,436 [22,987]***	0.76 [0.03]***
Median regression {Post-harvest}	฿14,000 [1,755]***	฿5,407 [737]***	-฿33,333 [14,258]**	-
Constant	฿1,800 [2,484]	฿4,213 [1,044]***	฿50,000 [20,182]**	-
p-value in Wilcoxon test for equality of distributions				
	<0.001	<0.001	<0.01	<0.001
Observations	559	559	559	559

Notes: This table reports results from OLS and quantile regressions (quantile 0.5) of the dependent variables shown in the column headings on an indicator variable identifying that the participant is assigned to the after-harvest group. We control for household size. Robust standard errors in brackets. The last panel shows the p-value of a Wilcoxon rank-sum test. The household expenditures include expenditures on agricultural activities.
* p<0.10, ** p<0.05, *** p<0.01.

Also, other factors such as nutrition, stress, or subjective well-being might be different

before and after harvest and thus affect behavior in experiments. However, before- and after-

harvest farmers reported similar stress (M_{before} = 6.09, SD_{before} = 2.41 vs. M_{after} = 5.80,

$SD_{after} = 2.70$; $t(557) = 1.32$, $p = 0.19$) and subjective well-being levels ($M_{before} = 8.27$,

$SD_{before} = 2.04$ vs. $M_{after} = 8.23$, $SD_{after} = 0.12$; $t(557) = 0.24$, $p = 0.81$).[4] Regarding nutrition,

only one participant (in the before-harvest group) indicated the months when we conducted our

experiment as hardest in terms of food. Thus, we concluded that nutrition is good both before

and after harvest. The regressions presented in Table 2 confirm these results. That is, we found

clear evidence that only financial situation but not the other factors such as subjective well-

being, stress, or nutrition are significantly different before and after harvest. Naturally, there

might also be other factors that differ before and after harvest and which we did not control for.

Although we cannot be sure of this, we used answers to an extensive post-experiment

questionnaire to discover as much about our participants as possible (see Appendix B.2) and

found that the only variables that we controlled for and that differed before and after harvest are

those related to financial situation.

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[4] On first sight, it is rather surprising that perceived happiness and stress do not differ before and after harvest. Our speculative interpretation is that the mainly Buddhist Thai farmers see "stress" or "happiness" as something bigger than being financially constrained / not constrained (see Ekman et al., 2005, for a discussion on Buddhism and happiness). In addition, a large body of literature showed a weak relationship between financial situation and subjective well-being, a proxy for happiness (e.g., Diener et al., 1999; Frey and Stutzer, 2002). Overall, our participants reported low stress levels and high happiness levels both before (means of stress and happiness amount to 6.09 [out of 14] and 8.27 [out of 10], respectively) and after harvest (means of stress and happiness amount to 5.80 [out of 14] and 8.23 [out of 10], respectively). Importantly, we followed on Mani et al. (2013) and Mullainathan and Shafir (2013) when distinguishing between scarcity "reducing cognitive capacity" and scarcity "*causing stress.*" Of course, one might feel *stressed* when facing poverty (which does not show in our post-questionnaire data but, for example, is the case in Mani et al.'s [2013] data). However, according to the vast scarcity literature, being biologically stressed is not necessary to having a taxed cognitive capacity. Scarcity will preoccupy one and consume her/his cognitive capacity, which in turn will have negative effects on one's cognitive functioning; biological stress can accompany it, but not necessarily.

Table 2. Before- and after-harvest differences in stress level, subjective wellbeing and nutrition

	Stress level	Subj well-being	Current month is hardest for food (dummy)
OLS {After-harvest}	-0.29 [0.22]	-0.04 [0.17]	-0.004 [0.004]
Constant	6.09 [0.14]***	8.27 [0.12]***	0.004 [0.004]
Median regression {After harvest}	0 [0.27]	0 [0.36]	-
Constant	6 [0.19]***	9 [0.25]***	-
p-value Wilcoxon test equality of distribution	0.37	0.46	0.32
Observations	559	559	559

Notes: This table reports results from OLS and quantile regressions (quantile 0.5) of the dependent variables shown in the column headings on an indicator variable identifying participants assigned to the after-harvest groups and a constant. Robust standard errors in brackets. The last panel shows the p-value of a Wilcoxon rank-sum test. Stress level is ranged from 1 to 14. Subjective well-being is ranged from 1 to 10.
* p<0.10, ** p<0.05, *** p<0.01.

2.2 Experimental treatments

We used a simple game to measure cheating behavior of the rice farmers (Fischbacher and Föllmi-Heusi, 2013; Gneezy et al., 2018). In the first experiment—the baseline cheating game—we give each farmer a sealed envelope with 10 folded pieces of paper bearing the numbers from 1 to 10. We asked the farmers to blindly take out one piece of paper, observe the number, put it back into the envelope, seal the envelope, and then report the number on a reporting sheet. The payoff was 10 Thai Baht times the number reported. This created an incentive to cheat for monetary benefit. As researchers, we did not know exactly which individuals cheated and by

how much, but we could infer the approximate level of cheating by comparing the expected theoretical distribution of reports (i.e., uniform distribution of the numbers between 1 and 10) with the actual reported distribution of numbers. That is, in the absence of cheating, we should observe that every number between 1 and 10 occurs approximately 10% of the time and that the average reported number is not statistically significantly different from 5.5 (the sum of 1 through 10, divided by 10). By contrast, if reported numbers were higher than expected it would indicate that participants cheated.

In the second experiment—the norm-reminder game—we tested for the effectiveness of a social-norm-reminder intervention to reduce cheating. In this experiment, before playing the cheating game, participants were informed that most rice farmers in their province find cheating for one's own benefit unacceptable. The exact wording of the prompt was "We ran a survey on farmers in this province and the majority finds cheating for one's own benefit unacceptable." We in fact asked participants, at the end of the first experimental sessions of the baseline cheating game, to rate how acceptable cheating for one's own benefit is, on a scale from 1 to 10. Most participants found such cheating very unacceptable.

We conducted both the baseline cheating game and the norm-reminder game before and after harvest and did so with different participants, which leads us to a 2x2 between-subject factorial design.

2.3 Procedure

We conducted paper-and-pencil experiments in all sessions (see Appendix B.1 for the exact instructions and Appendix C for the procedure). In this paper, we use data from the cheating

experiment,[5] for which we used the following protocol. First, participants drew a random seat

number (from 1 to 12) and took the seat correspondingly. After they signed the consent form, we

explained the experiments to them. They were informed that there would be four decision

activities and that we would explain the rules for each game at the beginning of that particular

game. After we described the game through a written script and presentation materials,

participants had to answer test questions correctly. Only then did we proceed with the

experiment.

The entire experimental session took on average 74 minutes, from which the Cheating

experiment took only 7 minutes. The post-experimental questionnaire section lasted an additional

96 minutes. It took about 20 minutes to interview each person, but because there were only three

or four assistants in each session, most participants had to wait to be interviewed. The average

experimental earnings were 279 Baht (8.45 USD, equivalent to the purchasing power of 22.63

USD), with average earnings of 64 Baht in the Cheating experiment (1.94 USD, equivalent to the

purchasing power of 5.19 USD).[6] In addition, participants received 100 Baht for showing up and

100 Baht for the interview. The additional payment for the interview was announced after the

experiment had ended.

3. Results

Panel A in Figure 1 shows the reported average number, and Figure 2, the distribution of the

reported numbers in the two games, before and after harvest. In the baseline cheating game,

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[5] Participants did three experiments prior to the Cheating experiment: Prisoner's Dilemma, Prisoner's Dilemma with third-party punishment, and the Dictator game. All decisions were made in private. The order of the experiments remained the same for all participants in all sessions. No feedback about experimental earnings from each game was provided during the experiment. We checked whether farmers' earnings and decisions in the previous games had an effect on their cheating behavior and found no significant effect using regression analyses.
[6] The exchange rate of 1 USD was 33 Thai Baht on experimental days. However, the purchasing power parity (PPP) conversion factor (GDP) was 12.33 Thai Baht per 1 USD in 2015 (World Bank, 2018).

farmers cheated statistically significantly. They reported, on average, 6.62. Importantly, the extent of cheating was the same before and after harvest ($M_{base\_before} = 6.61$, $SD_{base\_before} = 2.65$ vs. $M_{base\_after} = 6.64$, $SD_{base\_after} = 2.75$; $t(282) = -0.10$, $p = 0.92$). Before harvest, in expectation participants overreported by 20.18% (mean of reported number $M_{base\_before} = 6.61$; [chi]2 (9, N = 140) = 32.57, $p < 0.001$) and after harvest by 20.73% ($M_{base\_after} = 6.64$; [chi]2 (9, N = 144) = 35.31, $p < 0.001$). That is, in our setting, poverty itself does not change the willingness to cheat.

In the norm-reminder game, before harvest, participants overreported by 14.73% ($M_{norm\_before} = 6.31$; [chi]2 (9, N = 139) = 23.66, $p = 0.005$), which is not significantly less than in the baseline cheating game ($M_{base\_before} = 6.61$, $SD_{base\_before} = 2.65$ vs. $M_{norm\_before} = 6.31$, $SD_{norm\_before} = 2.94$; $t(277) = 0.89$, $p = 0.38$). In contrast, the norm-reminder tool was effective after the harvest: it reduced cheating significantly relative to the baseline cheating game ($M_{base\_after} = 6.64$, $SD_{base\_after} = 2.75$ vs. $M_{norm\_after} = 5.96$, $SD_{norm\_after} = 2.85$; $t(278) = 2.02$, $p = 0.04$) and yielded a reporting level that is not different from chance ($M_{norm\_after} = 5.96$; [chi]2 (9, N = 136) = 14.59, $p = 0.10$); that is, in expectation, people do not cheat in this case.

Figure 1. Average reported numbers in baseline cheating game and norm-reminder game
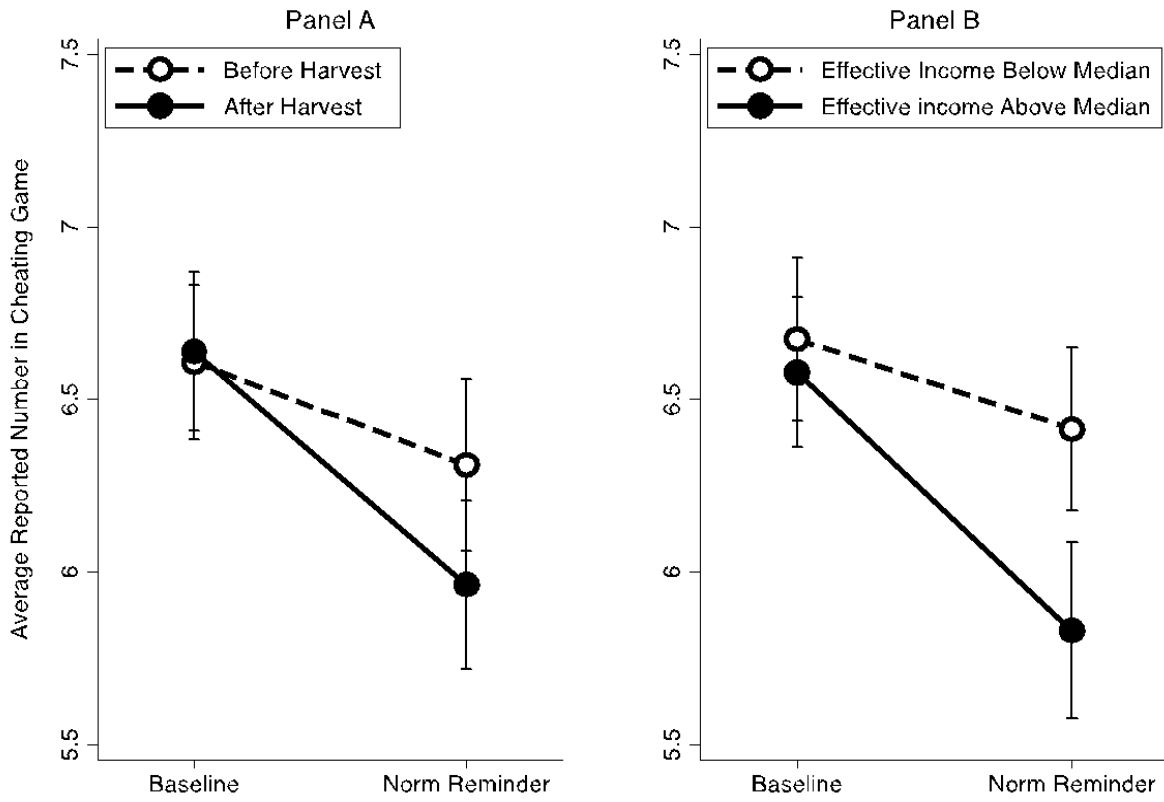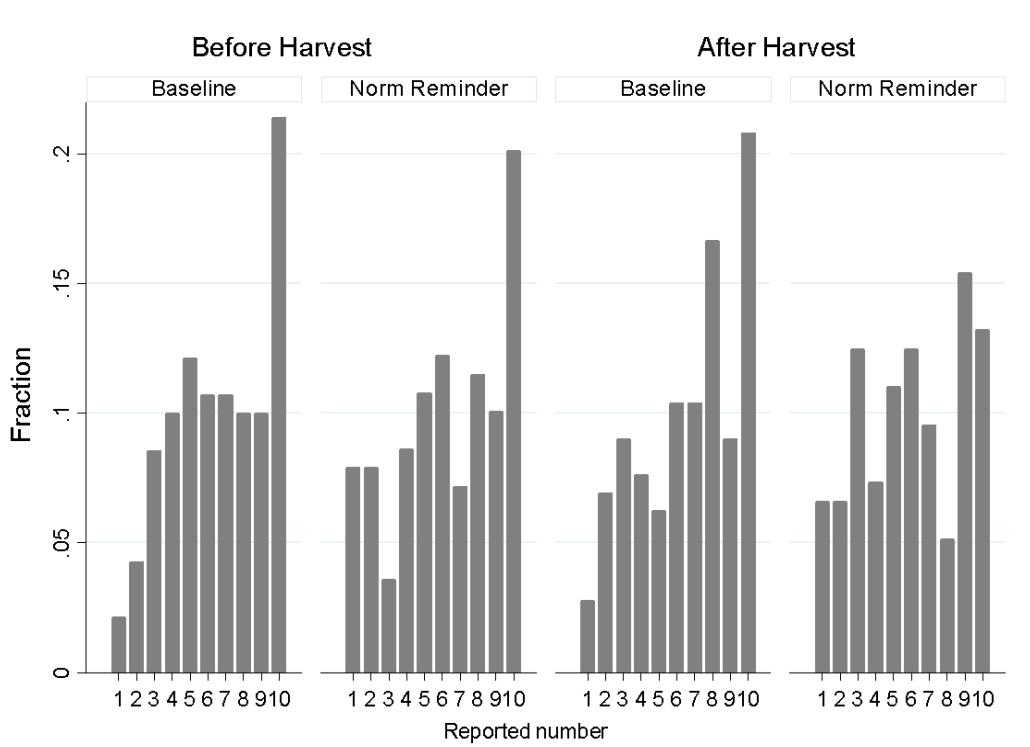
Figure 2. Distribution of reported numbers in baseline cheating game and norm-reminder game before and after harvest

Another way to analyze the effect of poverty on cheating is to look at the correlational relationship between poverty and cheating behavior. Instead of comparing the behavior before and after harvest, we pooled the before- and after-harvest groups and divided our subject pool by their effective income through a median split, calling the farmers above the median "higher income" and those below the median "lower income." We found correlational income effects that are very similar to the causal harvest effects on cheating (see Panel B in Figure 1). In the baseline cheating game, lower- and higher-income farmers cheated to a similar extent (M_{base_lower} = 6.67, SD_{base_lower} = 2.76 vs. M_{base_higher} = 6.58, SD_{base_higher} = 2.65; $t(282) = 0.30$, $p = 0.76$). In the norm-reminder game, lower-income participants did not react to the norm reminder

($M_{\text{base\_lower}} = 6.67$, $SD_{\text{base\_lower}} = 2.76$ vs. $M_{\text{norm\_lower}} = 6.41$, $SD_{\text{norm\_lower}} = 2.86$; $t(278) = 0.77$, $p = 0.44$), whereas the norm reminder was effective for higher-income individuals ($M_{\text{base\_higher}} = 6.58$, $SD_{\text{base\_higher}} = 2.65$ vs. $M_{\text{norm\_higher}} = 5.83$, $SD_{\text{norm\_higher}} = 2.92$; $t(277) = 2.24$, $p = 0.03$). Note, however, that these results are only correlational.

4. Discussion

Our study analyzed how poverty affects the tendency to cheat in a field setting. Our experiment revealed two main results with a set of implications.

First, poverty itself does not change humans' inclination to cheat. From a purely economic perspective, it is somewhat surprising that cheating is not higher before harvest than after harvest. Some previous evidence suggested that financial circumstances are *correlated* with unethical behavior (Gächter and Schulz, 2016). We showed that income itself is not significant enough to *causally* affect one's propensity to cheat. That is, people in poverty need cash immediately, but their need does not make them neglect the moral disutility associated with cheating. This result is in line with results from an experiment independently conducted at the same time as our experiment: Aksoy and Palma (2019) measured cheating behavior before and during harvest with Guatemalan coffee farmers and found that cheating for one's own benefit was the same before and during harvest.[7] That the baseline result is replicated at two ends of the world makes us even more confident in this result. Differently from ours and Aksoy and Palma's (2019) experimental design, in which the changes in financial situation are naturally occurring (and not made salient), Sharma et al. (2014) used an exogenous financial deprivation manipulation before a cheating game. The financial deprivation was either subjective (answering questions on financial deprivation beforehand) or objective (losing money in pre-stages of the

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[7] We were unaware of each other's experiments and conducted our experiments in the exact same weeks.

cheating game). The authors found that the feeling of being financially disadvantaged in a Western student sample and a U.S. MTurk sample leads to significantly higher cheating levels. Put together, the evidence from our experiment and the experiment by Aksoy and Palma (2019) suggests that financial constraints *themselves* might not lead to increases in cheating behavior. However, when the financial disadvantage is made more *salient* (as in Sharma et al., 2014), it might lead to higher cheating levels.

Second, poverty renders the moral reminder ineffective. The explanation, which is in line with the result, is that scarcity reduces one's cognitive resources and leads to "tunneling." In other words, because of scarcity, financially restricted farmers might have not paid attention to the norm reminder that we provided them because they were preoccupied with the financial constraints they were facing. A potential additional explanation for the result is economic: whereas rich farmers can afford to internalize the norm and follow the desired behavior, poor farmers have fewer financial possibilities for changing their behavior according to the norm.

Interventions that were shown to be highly effective and to work when people are richer and less constrained might not work when people are financially constrained. This makes it hard to change the norms in poor regions and neighborhoods. However, our results suggest that, first, timing of the interventions might matter. Most households experience some more-or-less predictable cycles of financial constraints due to pay cycles or seasonality in income streams. Interventions to change behavior need to take those cycles into account and time their implementation in periods of fewer financial constraints. Second, a social-norm reminder was ineffective when people were poor in our study, but it is just one of the potential interventions to change cheating behavior. This intervention requires attention and cognitive resources to be effective—resources that financially constrained individuals may lack. When choosing a

particular type of intervention, one needs to remember that poverty reduces people's cognitive capacity and might affect interventions as a result. Note that we tested just one intervention and that our intervention requires *slack*—cognitive resources—whereas some other interventions might instead create slack. Simple interventions that change the choice architecture (Johnson et al., 2012) and that require fewer cognitive resources, such as punishment for cheating (Khalmetski et al., 2017), might be more effective. Also, other simple interventions that have proved to work well in other contexts could possibly be more effective than a social-norm reminder, such as letting people sign an oath (as is Mazar et al., 2008) or changing the perceived likelihood of being caught and punished (as in Castro and Scartascini, 2015; Hallsworth et al., 2015).

Our study, however, also has some limitations that should inform future work. Given our setup, the study cannot say much about which aspect of poverty is important for interventions and cheating. In particular, regarding our preferred mechanism of financial constraints affecting cognitive resources, future studies should investigate the effect of relative, absolute poverty and/or income volatility on cognitive resources. It will matter greatly for policymakers to understand which aspect of poverty lowers the efficacy of interventions.

Finally, in our study, we measure cheating in one specific game. This allows us to study behavior in a widely used and easy-to-study paradigm. Future studies should investigate how harvest/income and social-norm reminders (or different interventions) affect different tasks and situations—perhaps those naturally occurring, such as tax evasion or corruption.

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Appendices

Appendix A: Socio-demographic/economic characteristics of participants

Table A1. Socio-demographic/economic characteristics of participants

Characteristics	Before harvest group	After harvest group	Tests	P-value
Age (years)	49.28	50.40	t-test	p = 0.21
Female (%)	70.97	73.57	proportions test	p = 0.49
Education (years in school)	6.56	6.27	t-test	p = 0.24
No. of household members	4.58	4.50	t-test	p = 0.61
No. of working household members	2.32	2.44	t-test	p = 0.20
Multi-generational household (%)[8]	86.02	82.86	proportions test	p = 0.35
Married (%)	94.96	95.04	proportions test	p = 0.69
No. of children of the participants	2.47	2.45	t-test	p = 0.85
In debt (%)	90.32	89.29	proportions test	p = 0.69
The income boost (Thai Baht)	7204	7999	t-test	p = 0.53

Note: The table shows that before- and after-harvest groups have similar characteristics, i.e., they do not differ in age, gender, education, number of children, proportion of people being in debt and the income boost generated by harvest.

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[8] Normally, in an agricultural family everyone who (still) can work will work, at least in their farms. So, only children or old people who are already retired do not work. From this, it follows that a household which contains non-working members is very likely multi-generational. Therefore, for the dummy of a multi-generational household, we calculated the difference between number of household members and number of working household members. The dummy takes a value of "1", if the difference is positive and "0" if there is no difference.

Appendix B: Instructions of the experiment and questionnaire

B.1 Instructions of the experiment

Instructions
(Used to explain the experiment to the participants verbally)

Note: Phrases/sentences in (*blanket)* are notes for the instructors and are not read out loud.

General information

Hello everyone. Thank you everyone for joining our activity today. Welcome. This project studies decision-making under uncertainty of rice farmers in Ubon Ratchathani. This research is collaboration between Mahidol University, Thailand and Columbia University, USA and is funded by Columbia University.

(Earn real money)
Today we are going to complete 4 activities and conduct a questionnaire. You have already earned 100 Baht for showing-up today. In addition, you will earn more money from the four activities. Your earnings depend on your decisions and decisions of others. You will receive your payoff in cash directly after today's activity. All activities will take around 3 hours.

(Anonymity)
We will identify your decisions only with your cubicle number. The experimenter outside will record your decisions by your cubicle number. He/she does not know who you are, your name or your appearance,. So now nobody will be able to link your decisions with your name and identity.

(Consent form)
It seems like everyone would like to start, doesn't it? Before we start, please sign this consent form. The consent form contains the information stated earlier. In addition, it is stated that …
- In the data analysis, we will not link your decisions with your identity but only with your cubicle number
- Your decisions will be kept confidentially
- You participate in these activities voluntarily
- You can withdraw from the study any time. In that case, you will be paid only 100 Baht for showing up
- If you have any questions, please contact Suparee Boonmanunt, Mahidol University. Email: suparee.boo@mahdiol.edu. Tel: 024415000 ext 2213.

(More …)
There is another team conducting the same activities with farmers from another village. Please do not communicate with others during these activities for your own sake. If you have any questions during the activities, please raise your hand and we will come to you. If you violate the rules, we will ask you to leave the experiment without any additional payment. Any questions so far?

Activities 1-3

…

Activity 4 (Cheating experiment)

We have finished three activities already. Well done! Thank you so much. Now you are going to participate in the next activity. You will be able to earn additional money.

What you do
- *(Show an example envelope and 10 pieces of paper as well as demonstrate how to draw a piece and put the piece back in the envelope according to the following steps)*
- We will give you an envelope with 10 pieces of paper in it.
- The numbers on the paper pieces are 1, 2, 3, 4, 5, 6, 7, 8, 9 and 10.
- They are placed in a random order.
- We will ask you to take out one piece of paper in private.
- Once you take a piece of paper out of the envelope, you will see a number.
- Then we ask you to put the piece of paper back in the envelope.
- We ask you to report the number that you saw on an answer sheet.

Payment
- The number you report determines how much money you will be paid.
- You will be paid ten times the number you report.
- *(Distribute this table)*

If you write	You receive		If you write	You receive
1	10 Baht		6	60 Baht
2	20 Baht		7	70 Baht
3	30 Baht		8	80 Baht
4	40 Baht		9	90 Baht
5	50 Baht		10	100 Baht

Understand?

Do you have any questions so far? Let's do some test questions to see whether you understand this activity. *(Distribute test questions)*

Test questions

1. If you report "1", how much will you receive? ……………. Baht
2. If you report "5", how much will you receive? ……………. Baht
3. If you report "10", how much will you receive? ………….. Baht

(Collect test questions)

Norm treatment *(in the afternoon session only!)*

We ran a survey on farmers in this province and the majority finds cheating for one's own benefit unacceptable.

We are about to start *(Distribute the materials)*

Now you have …
- The envelope with 10 pieces of paper,
- The answer sheet,
- The table that you can look up how much you will receive when you report each number 1 – 10.

How can you report a number you saw?
<u>*(Show the answer sheet)*</u>
- The answer sheet looks like this.
- Please write down the number you see here.

Date ……/ 9 (or 12) / 2017 session: ◯ Morning ◯ Afternoon

Seat no.: ……………

Answer sheet for Activity 4

Please write down the number you saw …………………

Let's start!

- Please take one piece of paper out of the envelope in private.
- Please see what number you get.
- Please put the piece of paper back in the envelope.
- Please report the number you saw on the answer sheet.
  (Wait until everyone finished)
- Please write down your cubicle number on the upper right corner.
- If you are done, please close the folder.
- Please wait quietly until everyone is done.

All decision activities are done.

We have finished all four decision activities. You are doing great. Now please let us interview you one-by-one, while the researchers outside are preparing the payment for you. For answering this questionnaire, you will get 100 Baht in addition. Please wait until the interviewer comes to you. When the interview is done, please wait until you are called out to get your payment. We will inform you about the results of all activities in private as well. Thank you very much for your participation and attention.

B.2 Post-experiment questionnaire

Questionnaire

Date: September / December ……, 2017   ▢ 1.Morning   ▢ 2.Afternoon

ID number ....   ▢ Team 1   ▢ Team 2

Interviewer   ....

Checker   ....

Please make a tick in the ☐ that you agree with

1. All things considered, how satisfied are you with your life as a whole these days?

☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐

1   2   3   4   5   6   7   8   9   10

very unsatisfied   very satisfied

2. Think about the first activity, you have to choose between, to keep or send your endowment to your random match. As you were making the decision, what did you think your match will do?

☐ 0. Keep   ☐ 1. Send

3. Please circle the picture that best describes your current relationship with an average person from your village.
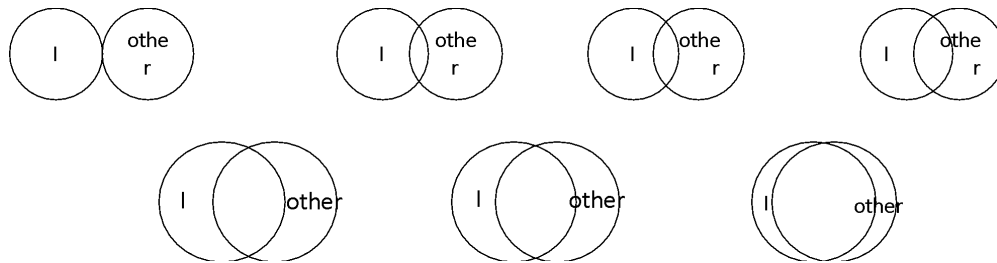
4. Please circle the picture that best describes your current relationship with an average person from another village.
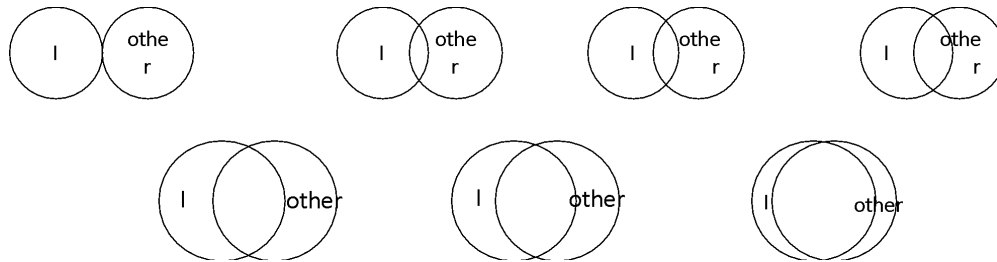
5. All things considered, would you say that most people can be trusted or that you need to be very careful in dealing with people? *(Choose one answer)*

    ▨ 0. Need to be very careful   ▨ 1. Most people can be trusted

6. Do you think that other people only have the best intentions?

☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐

0   1   2   3   4   5   6   7   8   9   10

do not agree at all   agree perfectly

7. How willing would you be to give to good causes without expecting anything in return?

☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐

0   1   2   3   4   5   6   7   8   9   10

completely unwilling   very willing

8. Are you a person who is generally willing to take risks, or do you try to avoid taking risks?

☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐

0   1   2   3   4   5   6   7   8   9   10

completely unwilling   very willing

9. Are you generally an impatient person, or someone who always shows great patience?

Please use a scale from 0 to 10, where a 0 means you are "very impatient" and a 10 means you are "very patient".

☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐

0   1   2   3   4   5   6   7   8   9   10

completely unwilling   very willing

10. Are you a person who is generally willing to give up something that is beneficial for you today in order to benefit more from that in the future or are you not willing to do so?

☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐   ☐

0	1	2	3	4	5	6	7	8	9	10

completely unwilling   very willing

11. How satisfied are you with the financial situation of your household?

☐	☐	☐	☐	☐	☐	☐	☐	☐	☐
1	2	3	4	5	6	7	8	9	10

very unsatisfied   very satisfied

12. Does this year's rice production meet your expectation? *(only after-harvest sessions)*

☐ 1.Lower than expected   ☐ 2. As much as expected   ☐ 3. Higher than expected

13. To what purposes do you use your rice production (choose all that apply)? *(only after-harvest sessions)*

☐ 1. To sell   ☐ 2. To my own consumption

☐ 3. To give to my family members (who do not live together)

☐ 4. To pay debt / rent   ☐ 5. Others, specify ……………..

14. How many Rai do you plant rice? … Rai. Owned ... Rai, rented … Rai, public… Rai.

15. Expenditure for rice farming

Expenditure	Month of expenditure	Baht (Total) in that month
1) Plowing the field		
2) Rice seeds (if any)		
3) Sowing of rice seeds (if any)		
4) Transplanting of rice (if any)		
5) Herbicide/Pesticide (if any)		
6) Chemical fertilizer (if any)		
7) Organic fertilizer (if any)		
8) Rent (if any)		
9) Harvesting cost		
10) Others, specify ……………..		
Total		

16. How many Rai do you plant ...….? ... Rai. Owned ... Rai, rented … Rai, public … Rai.

17. Expenditure for …………….. farming

Expenditure	Month of expenditure	Baht (Total) in that month
1) Plowing the field		
2) Young plants (if any)		
3) Planning (if any)		
4) Herbicide/Pesticide (if any)		
5) Chemical fertilizer (if any)		
6) Organic fertilizer (if any)		
7) Rent (if any)		
8) Harvesting		
9) Others, specify ………………		
Total		

18. How many Rai do you plant ...…? ... Rai. Owned ... Rai, rented … Rai, public … Rai.

19. Expenditure for …………….. farming

Expenditure	Month of expenditure	Baht (Total) in that month
1) Plowing the field		
2) Young plants (if any)		
3) Planning (if any)		
4) Herbicide/Pesticide (if any)		
5) Chemical fertilizer (if any)		
6) Organic fertilizer (if any)		
7) Rent (if any)		
8) Harvesting		
9) Others, specify ………………		
Total		

20. Monthly "basic" household expenditure

Expenditure	Baht/Month
1) Rent/Montage	
2) Water	
3) Electricity	
4) Telephone/internet/mobile phone	
5) Food	
6) Transportation	
7) Consumption goods (e.g. soap, shampoo, detergent)	

8) School fee for family members	
9) Cigarette/tobacco & alcohol	
10) Parties/celebrations	
11) Merit/donations	
12) Cloths/cosmetics/decorations	
13) Installment for vehicles or electronic machines	
14) Lottery/Gambling	
15) Others, specify ……………………………………	
Total	

21. Household income around the year

Item	Month of the income	Amount in that particular month	Is it an estimation?
1. Rice			
2. Plant 1 ……………			
3. Plant 2 ……………			
4. Livestock 1 ………..			
5. Livestock 2 …………..			
6. Regular work ………..			
7. Wage labor			
8. Transfer from family members (who work outside the village)			
9. Government transfers for old people			
10. Government transfers for poor people			
11. Other ………………			
12. Other ………………			

22. Which months are usually the best in terms of net income for you? (the first three months)

1. ……………….….   2. ………………………..   3. …………………………

☐ No difference

23. Which months are usually the most difficult in terms of net income for you? (the first three months)

1. ……………….….   2. ………………………..   3. …………………………

☐ No difference

24. Which months are usually the best in terms of food for you? (the first three months)

1. …………………….….   2. ………………………...   3. …………………………

☐ No difference

25. Which months are usually the most difficult in terms of food for you? (the first three months)

1. …………………….….   2. ………………………...   3. …………………………

☐ No difference

26. Does any adult in the household currently have a loan?

☐ 0. No (Go to 30)   ☐ 1. Yes

27 How much does your household borrow from your friends and relatives? …. Baht.

28 How much loan does your household have? ……………. Baht.

29. In a year, in which months do you get a loan, pay interest and repay the loan? Do you have difficulties paying them in those particular months? (Tick in the table below)

Month	Loan receipt?	Interest paid?	Loan repaid?	Difficulties with payment?
April				
May				
June				
July				
August				
September				
October				
November				
December				
January				
February				
March				

30. In the last month has the household been negatively affected by any of the following problems?

    ☐ 1. Unusually high level of **crop pests and diseases**

☐ 2. Unusually high level of **livestock diseases**

☐ 3. Death or illness of **main earner** in the household (please choose): Loss of income / Medical expenses (please specify)

☐ 4. Death of illness of **other earners** in the household (please choose): Loss of income / Medical expenses (please specify)

☐ 5. Loss of productive or consumption assets / livestock / crops due to **flood**

☐ 6. Loss of productive or consumption assets / livestock / crops due to **theft**

☐ 7. Loss of productive or consumption assets / livestock / crops due to **fire**

31. Do you currently have any savings?

☐ 0. No (Go to 33)　　　　　　　　☐ 1. Yes

32. Where do you save?

☐1. At home　　　　　　☐2. Bank　　　　　　☐3. Savings groups/cooperatives

☐4. Insurance company　　☐5. Gold　　　　　　☐6. Livestock (cows / buffalos)

☐7. ROSCAs　　　　　　☐8. Savings programs for retirement

☐9. Cremation group　　　☐10. Others, specify ………………．

33. Does your family live in your own house on your own land?

☐ 0. No　　　　　　　　　☐ 1. Yes, we live in our own house in our own land

34. Have you lived in this village since you were born?

☐　0. No　　　　　　　　☐ 1. Yes

35. How acceptable is it to cheat for one's own private benefit?

☐　　☐　　☐　　☐　　☐　　☐　　☐　　☐　　☐　　☐

1　　2　　3　　4　　5　　6　　7　　8　　9　　10

very unacceptable　　　　　　　　　　　　　　　　very acceptable

36. In the last month, how often have you felt that you were unable to control the important things in your life?

☐　　　　☐　　　　☐　　　　☐　　　　☐

1	2	3	4	5
Never	Almost never	Sometimes	Fairly often	Very often

37. In the last month, how often have you felt confident about your ability to handle your personal problems?

☐	☐	☐	☐	☐
1	2	3	4	5
Never	Almost never	Sometimes	Fairly often	Very often

38. In the last month, how often have you felt that things were going your way?

☐	☐	☐	☐	☐
1	2	3	4	5
Never	Almost never	Sometimes	Fairly often	Very often

39. In the last month, how often have you felt difficulties were piling up so high that you could not overcome them?

☐	☐	☐	☐	☐
1	2	3	4	5
Never	Almost never	Sometimes	Fairly often	Very often

40. Gender: ☐ 0. Male   ☐ 1. Female

41. Age: ……………… years old

42. Marriage status: ☐ 0. Single   ☐ 1. Married ☐ 2. Divorced / Widow

43. Children: …………………..

44. How many members are there in your family? ………………………

45. Amount of members who are working ……………………

46. Your level of education

	1.No School		2.Primary school (level)…………
	3.Secondary school (level) …………..		4.Diploma (level) …………..
	5.Bachelor degree (level) …………..		6.Higher than Bachelor degree

47. Main occupation (select one choice)

☐ 1.Rice farmer ☐ 2.Farmer, specify ………………

☐ 3.Wageworker ☐ 4.Government officer

☐ 5.Company employee ☐ 6.Business owner

☐ 7.Others, specify ……………………

48. Supplemental occupation (select all that apply)

☐ 1.Rice farmer ☐ 2.Farmer, specify ………………

☐ 3.Wageworker ☐ 4.Livestocks, specify …………

☐ 5.Government officer ☐ 6.Company employer

☐ 7.Public health volunteer ☐ 8.Others, specify ……………

49. Are you a community leader, e.g. village headman, assistant headman, savings group committee, informal leader, etc.?

☐ 0. No ☐ 1 Yes

50. Are you a member of any of these following groups? *(Only after-harvest sessions)*

☐ 0.No

☐ 1.Savings group / Cooperatives ☐ 2.Cremation group

☐ 3.Female occupational group ☐ 4.Organic farming group

☐ 5.Volunteering group in development project ☐ 6.Scouts

☐ 7.Resource conservation group ☐ 8.Others, specify ……………

51. Could we contact you in case we have further questions?

☐ 0. No ☐ 1 Yes, mobile number …………………………….

Thank you very much for participating in this survey

Appendix C: Detailed experimental procedure

Set up

We conducted a cheating experiment with 568 rice farmers who grow rice once a year. We used a between-subject experimental design with 283 farmers before harvest and 285 farmers after harvest. We conducted 24 pre-harvest sessions in 24 villages with one session in each village (on 25-30th of September, 2017) and 24 post-harvest sessions in other 24 villages (on 8-14th of December, 2017). See Figure S1 for the location of the different villages.

We recruited 12 farmers in each village through brochures. Interested farmers had to register for the experiment with a community leader suggested by the Community Development Department. If more than 12 farmers registered, we randomly chose 12 farmers who could participate in the study. Since some registered farmers did not show up, there were 3 sessions with fewer than 12 participants: 10 participants in one before-harvest and one after-harvest session and 9 participants in one before-harvest session.

Procedure

We conducted four one-shot pen-and-paper experiments in all sessions. Prior to the Cheating experiment, farmers played Prisoner's Dilemma, Prisoner's Dilemma with third party punishment and the Dictator game. All decisions were made in private. The order of the experiments remained the same for all participants in all 48 sessions. No feedback about experimental earnings from each game was provided in during the experiment. In this study, we focus only on decisions in the Cheating experiment.[9] To be on the safe side, we checked whether farmer's earnings and decisions in the previous games have an effect on their cheating behavior. We find no significant effect of the previous decisions or earnings on the cheating behavior in regressions.

The protocol was as follows. First, participants drew a random seat number (from 1 to 12) and took the seat correspondingly. After they signed the consent form, we explained the

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[9] There were four experimental conditions in the Cheating experiment, which leads us to around 140 independent observations a condition. This sample size is higher than the sample sizes used in the remaining literature using similar random draw games.

experiments to them. They were informed that there would be four decision activities and we would explain the rules of each game at the beginning of that particular game. After describing the game through a written script and presentation materials, subjects had to answer test questions correctly. Only then we proceeded with the experiment.

Plastic cubicles and folders for putting answer sheets inside were used to keep decisions private. Furthermore, in each session there was one experimenter who instructed the experiments and another one who sat outside and typed farmers' decisions in computer after each decision. The experimenter inside could see participants faces but not their decisions, while the experimenter outside could see participants decisions but not their faces. With this procedure we could not identify participants' answers. After the experiment, participants answered a questionnaire face-to-face with an interviewer, who was unaware of any of their decisions in the experiments. Finally, we paid farmers directly at the end of each session in cash.

Pilots

Prior to the experiments, we conducted two pilots in order to test the protocol, instructions and other instruments and to train our research assistants to conduct the experiments. We ran the first pilot with 24 Thai undergraduate students of Mahidol University divided in two sessions (on August 2nd, 2017). Later, on August 20th, 2017, we ran a second pilot with 24 actual rice farmers in another district of Ubon Ratchathani, also divided in two sessions.