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Does poverty negate the impact of social norms on cheating?

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ABSTRACT

Cheating such as corruption and tax evasion is 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 for 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 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.

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, 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, 2016; Prediger et al., 2013, 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, 2012; Shalvi, 2016), and only a few studies used an exogenous income shock to establish a causal

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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.

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link between poverty and prosocial/antisocial behavior (Aksoy and Palma, 2019; Bartos, 2016; Prediger et al., 2013, 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 because they cannot exclude omitted variable bias, that is, a third unobservable factor affecting both income and prosocial behavior.

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, 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 the willingness to cheat or leave it unchanged. On one hand, clearly, being financially constrained increases the immediate need for money as well as its marginal utility (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 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 sufficient cognitive bandwidth to internalize the norm reminder, they have fewer financial possibilities for adopting 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, because of either "tunneling" or a limited financial possibility for following 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 between-subjects experimental design to

[2] Despite their popularity among policymakers and the many success stories, there are some cases where social-norm interventions were unsuccessful in changing behavior such as cheating or tax noncompliance (e.g., Castro and 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 on past compliance behaviors (Castro and 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 and 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.

measure cheating behavior and the effectiveness of a social-norm intervention to reduce cheating before and after harvest. We found that poverty itself does not affect willingness to cheat—that is, participants cheated to a similar extent before and after harvest. When reminded of the social norm, they did not cheat less when they were poorer (before harvest), but the social-norm-reminder intervention reduced cheating when they were richer (after harvest). This result suggests that the timing of interventions to change behavior might matter. However, note that even though interaction-effects analysis shows that norms work more than two times better after harvest than before harvest, which we perceive to be *economically* significant, the interaction effect is not *statistically* significant.

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 2×2 *between-subjects* design with four treatments, varying the financial situation of the participants (before harvest vs. after harvest) and the existence of a social-norm reminder (baseline cheating game [no norm-reminder included] vs. norm-reminder game). In this section, we first demonstrate the financial differences between the participants before and after harvest and then 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.[3] These rice farmers have generally a low income (93% are eligible to apply for a government subsidy for groceries; see Table A.1 in Appendix A in Supplementary Material 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 why we chose the Thai rice farmers as the participant 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).[4] The before- and after-harvest farmers are from the same subdistricts but different villages (24 villages before harvest and another 24 villages after harvest). The villages were randomly assigned to treatments and experimental sessions, with each village participating in only one experimental session (and one treatment).[5] The before- and after-harvest farmers do not differ in sociodemographic characteristics such as age, gender, education, or family size (see Table A.1 in Supplementary Material), 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 vs. M_{after} = ฿42,442, $p < 0.001$, t-test; all tests in the paper are two-sided, if not noted otherwise). 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 vs. M_{after} = ฿20,217, $p < 0.001$, t-test). Also, household expenditures of before-harvest participants were 38% lower than those of after-harvest participants (M_{before} = ฿11,269 vs. M_{after} = ฿18,238, $p < 0.01$, t-test). Furthermore, the reported household debt is 28% higher before harvest (M_{before} = ฿192,086 vs. M_{after} = ฿138,338, $p < 0.01$, t-test), and the before-harvest participants are less likely to have savings of any kind, including livestock (76% vs. 95%, $p < 0.001$, test of proportion).

In Table 1, we present ordinary least squares (OLS) regression results, in which we use the following specification:

$$Y_i = \alpha + \gamma_0 Harvest_i + \gamma_1 No.\ of\ HH\ members_i + e_i,$$

where the dependent variable Y_i is either household (HH) Income, HH Expenditure, Amount of HH Debt standing or Savings (this is a dummy, which is equal to 1 if the household has any savings, and 0 otherwise). The indicator variable $Harvest_i$ is equal to 1 if the individual is in an after-harvest group and 0 if the individual is in a before-harvest group. All standard errors are clustered at the village level. In line with the previous analysis, we find that after the harvest, households have a significantly higher income and expenditures, have a lower amount of debt standing, and are more likely to have savings.

[3] We excluded nine farmers because they did not cultivate and harvest rice that year. This yielded a final sample of 559 farmers.

[4] Since there were four experimental conditions in the experiment, we obtained around 140 independent observations per cell. This sample is larger than those in other literature using similar random-draw games. We determined the sample size based on our financial limitations, and decided before conducting the study that we would end data collection upon completing our visits to the 48 villages selected.

[5] Assignment to a before- or after-harvest condition and to a baseline cheating game or a norm-reminder game was random.

Table 1
Before- and after-harvest differences in household (HH) financial situation.

	HH Income	HH Expenditures	Amount of HH Debt Standing	Savings (dummy)
{After-harvest}	฿31,241 (5,719)***	฿7,111 (1,377)***	-฿52,698 (24,169)**	0.19 (0.04)***
No. of HH members	฿4,111 (1,116)***	฿1,762 (427)***	฿13,022 (4,420)***	-
Constant	-฿7,296 (5,023)	฿3,196 (1,649)*	฿132,436 (28,975)***	0.76 (0.04)***
p-value Wilcoxon test equality of distribution	<0.001	<0.001	<0.01	<0.001
Observations	559	559	559	559

Notes: This table reports results from OLS regressions of the dependent variables shown in the column headings on an indicator variable identifying participants assigned to the after-harvest groups and a constant controlling for HH size when variables are at the HH level (the first three). Standard errors, clustered at the village level, are in parentheses. The fourth row shows the p-value of a Wilcoxon rank-sum test. HH expenditures include expenditures on agricultural activities. ฿= Thai Bhat. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Other factors such as nutrition, stress, or subjective well-being might also be different before and after harvest and thus affect behavior in experiments. However, before- and after-harvest farmers reported similar levels of stress ($M_{\text{before}} = 6.09$ vs. $M_{\text{after}} = 5.80$, $p = 0.19$, t-test) and subjective well-being ($M_{\text{before}} = 8.27$ vs. $M_{\text{after}} = 8.23$, $p = 0.81$, t-test).[6] Regarding nutrition, only one participant (in the before-harvest group) indicated the months when we conducted our experiment as being hardest in terms of food. Thus, we concluded that nutrition is good both before and after harvest.

The regression results in Table 2 confirm these results. In Table 2, we use the following regression specification:

$$Y_i = \alpha + \gamma_0 Harvest_i + e_i,$$

where the dependent variable Y_i is either Stress level, Subjective well-being, or Current-month-is-hardest-in-terms-of-food (a dummy equal to 1 if the current month is the hardest for the participant in terms of food). The indicator variable $Harvest_i$ is equal to 1 if the individual is in an after-harvest group and 0 if they are in a before-harvest group. All standard errors are clustered at the village level. In line with the previous analysis, we find that all three dependent variables are not different before and after harvest.

That is, we found clear evidence that only financial situation, and not the other factors such as subjective well-being, stress, or nutrition, is 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 in Supplementary Material) and found that the only variables that we controlled for and that differed before and after harvest are those related to financial situation.

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 participant a sealed envelope containing 10 folded pieces of paper bearing the numbers from 1 to 10. We asked participants to blindly take out one piece of paper, observe the number, return it to the envelope, seal the envelope, and then report the observed 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 or 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, this would indicate that participants cheated.

6 At 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). Overall, our participants reported low stress levels and high happiness levels both before (means of stress and happiness are 6.09 [out of 14] and 8.27 [out of 10], respectively) and after harvest (means of stress and happiness are 5.80 [out of 14] and 8.23 [out of 10], respectively). Importantly, we followed 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 data from Mani et al., 2013). However, according to the vast scarcity literature, experiencing biological stress is not necessary to experiencing a taxed cognitive capacity. Scarcity will preoccupy a person and consume their cognitive capacity, which in turn will have negative effects on their cognitive functioning; biological stress can accompany it, but not necessarily.

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

	Stress Level	Subjective Well-Being	Current Month Is Hardest for Food (dummy)
{After-harvest}	−0.29 [0.24]	−0.04 [0.21]	−0.004 [0.004]
Constant	6.09 [0.17]***	8.27 [0.17]***	0.004 [0.004]
p-value Wilcoxon test equality of distribution	0.37	0.46	0.32
Observations	559	559	559

Notes: This table reports results from OLS regressions of the dependent variables shown in the column headings on an indicator variable identifying participants assigned to the after-harvest groups and a constant. Standard errors, clustered at the village level, are in brackets. The third row shows the p-value of a Wilcoxon rank-sum test. Stress level ranges from 1 to 14. Subjective well-being ranges from 1 to 10. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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.[7]

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

Table 3 presents the treatments and the number of independent observations in each treatment.

Table 3
Treatments.

2×2 Between-Subjects Design	Before harvest (N)	After harvest (N)
Baseline cheating game	140	144
Norm-reminder game	139	136

2.3. Procedure

We conducted paper-and-pencil experiments in all sessions (see Appendix B.1 in Supplementary Material for the exact instructions and Appendix C in Supplementary Material for the detailed experimental procedure). In this paper, we use data from the cheating experiment,[8] for which we used the following protocol. First, participants drew a random seat number (from 1 to 12) and took the corresponding seat. 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, of 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).[9] In addition,

[7] We used the first two experimental sessions to determine the social-norm reminder. These sessions were baseline cheating-game sessions with 24 participants all together. On average, the participants rated that lying is acceptable at a level of 1.63 (with 1 being very unacceptable and 10 very acceptable), and 79.17% participants chose "very unacceptable" (marking 1) as their answer. We ask participants how acceptable lying is, at the end of each experimental session. The average rate of acceptance amounts to 1.70 over all participants, with 79.43% of participants choosing "very unacceptable" as their answer. That is, lying for one's own benefit is perceived as being very unacceptable in our sample.

[8] Participants performed 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 cheating behavior and found no significant effect using regression analyses.

[9] 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).

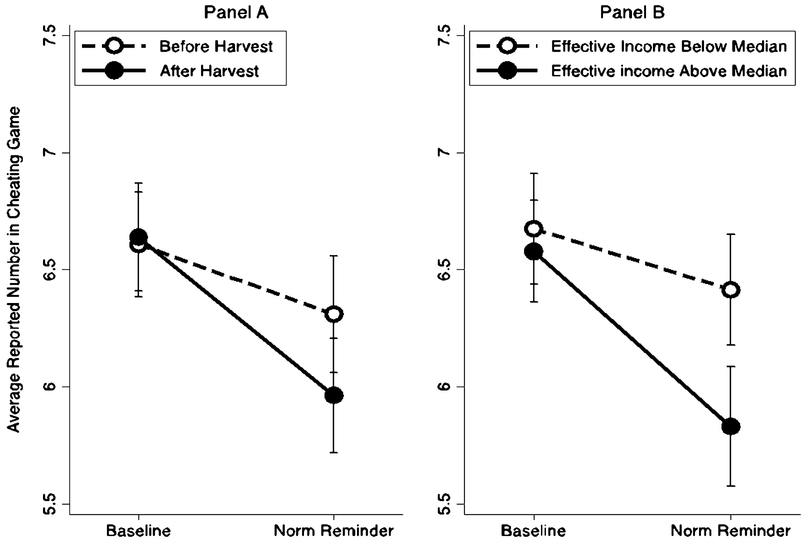

Fig. 1. Average reported numbers in the baseline cheating game and the norm-reminder game.

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 Fig. 1 shows the average reported number, and Fig. 2 shows the distribution of the reported numbers in the two games, before and after harvest. In the baseline cheating game, farmers cheated statistically significantly. They reported, on average, 6.62. Importantly, the extent of cheating was the same before and after harvest ($M_{\text{base\_before}}$ = 6.61vs. $M_{\text{base\_after}}$ = 6.64, p = 0.92, t-test). Before harvest, in expectation, participants overreported by 20.18% (cheating was statistically significant, p < 0.001, Chi-square-test) and after harvest by 20.73% (cheating was statistically significant, p < 0.001, Chi-square test). That is, in our setting, poverty itself does not change willingness to cheat.

In the norm-reminder game, before harvest, participants overreported by 14.73% (cheating was statistically significant, p = 0.005, Chi-square-test). On average, participants reported 6.31, which is not significantly less than in the baseline cheating game ($M_{\text{base\_before}}$ = 6.61 vs. $M_{\text{norm\_before}}$ = 6.31, p = 0.38, t-test). In contrast, the norm-reminder tool was effective after the harvest: it reduced cheating significantly relative to the baseline cheating game ($M_{\text{base\_after}}$ = 6.64 vs. $M_{\text{norm\_after}}$ = 5.96, p = 0.04, t-test) and yielded a reporting level that is not different from chance (p = 0.10, Chi-square-test); that is, in expectation, people do not cheat in this treatment.[10]

In the next step, we present OLS regressions results (see Table 4). We run four OLS regressions including a regression with the interaction term *After harvest* × *Norm* intervention of the following form:

$$Report_i = \alpha + \gamma_0 Harvest_i + \gamma_1 Harvest_i \times Norm_i + \delta Norm_i + \beta x_i + e_i,$$

where the dependent variable $Reported_i$ is Reported Number (1–10) by individual i. The indicator variable $Harvest_i$ is equal to 1 if the individual is in an after-harvest group and 0 if the individual is in a before-harvest group. The indicator variable $Norm_i$ is equal to 1 for the norm-reminder game and 0 for the baseline cheating game. Sociodemographic controls x like gender, age, education level, family status, and economic status are included in regression specifications (3) and (4). All standard errors are clustered at the village level.

In general, the regressions support the previous analysis. The first regression specification (Column 1) shows that overall cheating is not different before and after harvest (i.e., the coefficient on *After Harvest* is small and not significant). Furthermore, we find that when we look at the whole sample (before harvest and after harvest), the norm intervention lowers cheating (the coefficient on *Norm intervention* is negative and marginally significant). In the second regression specification, we test for the interaction between harvest and norm intervention (Column 2). We find that the intervention is more than twice as effective after harvest than before harvest (while the coefficient on *Norm intervention* amounts to −0.30, the interaction effect of *After harvest* × *Norm* amounts to −0.38). Whereas the doubled effect size shows that norm intervention is

[10] In addition, note that in all the treatments, only high numbers are overreported significantly. We find that, in expectation, eights are overreported in base_after, nines are overreported in norm_after, and tens are overreported in base_before, norm_before, and base_after (p < 0.05, one-sided binomial tests), whereas no other number is overreported significantly in any treatment. The finding that cheating happens at high numbers is in line with the rest of the cheating literature using random-draw games (for a meta-study, see Abeler et al., 2019). For instance, excluding the most extreme reporting of tens from the data set would make the average overreporting in the four treatments indistinguishable.

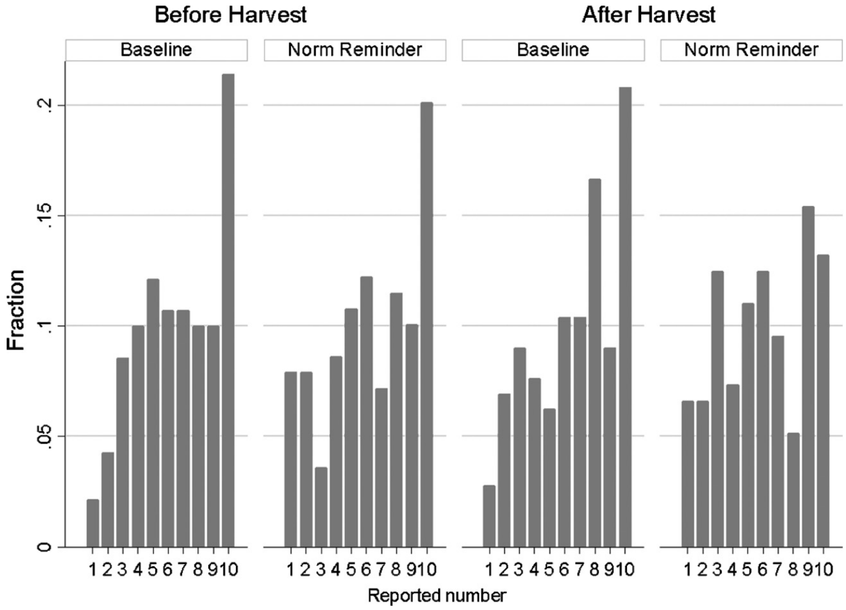

Fig. 2. Distribution of reported numbers in baseline cheating game and norm-reminder game before and after harvest.

economically significantly more effective after harvest, the interaction is not statistically significant on conventional levels. Note also that we lack sufficient power to detect a significant interaction effect. As stated earlier, we set the sample size based on our financial limitations, which led us to visit 48 villages and a total of 559 observations. An ex-post power calculation shows that after harvest—when we expected to see a large treatment effect of norms—given the sample size of 280, the means, and standard deviations, the power we have amounts to 52.58%. Because we have relatively low power even in the after-harvest group, in which we expect a large norms' effect, we are clearly underpowered for detecting significant interaction effects of norms between the before- and after-harvest groups.

Overall, we find that (1) in the baseline cheating game, cheating behavior is not different before and after harvest; (2) a social-norm reminder significantly reduces cheating after harvest (when the population is richer) but not before harvest (when the population is poorer); and (3) the interaction of harvest and social-norm reminder suggests that norms work better after harvest than before harvest, but the interaction effect is not statistically significant.

Finally, another way to analyze the effect of poverty on cheating is to examine 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 participant 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 Fig. 1). In the baseline cheating game, lower- and higher-income farmers cheated to a similar extent ($M_{\text{base\_lower}} = 6.67$ vs. $M_{\text{base\_higher}} = 6.58$, $p = 0.76$, t-test). In the norm-reminder game, lower-income participants did not react to the norm reminder ($M_{\text{base\_lower}} = 6.67$ vs. $M_{\text{norm\_lower}} = 6.41$, $p = 0.44$, t-test), whereas the norm reminder was effective for higher-income individuals ($M_{\text{base\_higher}} = 6.58$ vs. $M_{\text{norm\_higher}} = 5.83$, $p = 0.03$, t-test). 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 those 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.[11] That the baseline result is replicated at two ends of the world makes us even more confident

[11] We were unaware of each other's experiments and conducted them during the exact same weeks.

Table 4
Results of OLS regressions on cheating behavior.

	Reported Number (1–10)			
Dependent Variable	(1)	(2)	(3)	(4)
After Harvest	−0.15	0.03	−0.13	0.01
	(0.27)	(0.37)	(0.28)	(0.38)
Norm Intervention	−0.49*	−0.30	−0.48*	−0.33
	(0.27)	(0.41)	(0.28)	(0.43)
After Harvest × Norm	-	−0.38	-	−0.29
		(0.54)		(0.56)
Demographic Controls				
Female			−0.23	−0.23
			(0.28)	(0.29)
Age			−0.01	−0.01
			(0.02)	(0.02)
Education (years in school)			0.05	0.05
			(0.06)	(0.06)
Married			−0.02	−0.03
			(0.34)	(0.34)
No. of children			−0.03	−0.04
			(0.12)	(0.12)
Household annual income			<−0.001	<−0.001
			(<0.001)	(<0.001)
Savings (dummy)			0.11	0.12
			(0.29)	(0.29)
Constant	6.70***	6.61***	7.01***	6.93***
	(0.25)	(0.29)	(1.20)	(1.19)
No. of participants	559	559	559	559
R^2	0.0083	0.0094	0.0156	0.0162

Notes: Standard errors, clustered at the village level, are in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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 cheating game). They found that the feeling of being financially disadvantaged in a Western student sample and a U.S. MTurk sample led to significantly higher cheating levels. When combined, 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 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 in order 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 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.

Declaration of competing interest

The authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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Appendix. Supplementary material

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.geb.2020.09.009.

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