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Essays on Field Experiments and Impact Evaluation

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Summary of the thesis

This thesis comprises the four self-contained papers summarized below.

Paper 1: *Improving Welfare Through Climate-Friendly Agriculture: The Case of the System of Rice Intensification (SRI).*

This paper investigates the adoption and impact of a novel rice farming technology known as SRI, the system of rice intensification. SRI is a low-tech and climate-friendly farming system and involving the following principles: raising rice seedlings in a carefully managed, garden-like nursery; single widely spaced transplants; early and regular weeding; carefully controlled water management; and application of compost to the extent possible (Uphoff, 2002). Instead of traditional rice field flooding, SRI implies keeping the fields only moist. This requires less water and reduces methane emission (Khosa et al., 2011). Hence, SRI is a potential adaptation and mitigation strategy against climate change. However, adoption and sustained use depend on its profitability compared with the traditional methods.

Rich survey data is used to investigate the economic impact of SRI on the welfare of smallholder farmers in rural Tanzania. Previous studies have documented a positive impact of the technology on crop yield (Stoop et al., 2002). However, some studies argue that SRI yield gains come with an increased labor demand due to the labor-intensive nature of some of its components (Barrett et al., 2004; Noltze et al., 2012). We argue that if the increased labor cost is large enough to outweigh the yield gains, the celebrated yield impact of the technology could be misleading. We thus extend the literature by assessing the impact of the technology on net household income, accounting for the increased labor cost. In addition, contrary to previous literature, we provide the first evidence of the impact of SRI among rain-dependent farmers, who are likely more vulnerable to climate change than those under an irrigation scheme. The results suggest that SRI indeed improves yield even in rain-dependent areas, but its profitability (i.e., net farm income) hinges on the actual market price farmers face. SRI becomes profitable only when the rice variety sells at the same market price as that of traditional varieties, but results in loss when SRI rice sells at a lower price. We argue that the effort of promoting adoption of such climate-friendly agricultural practices requires complementary institutional reform and support in order to ensure their profitability to smallholder farmers.

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Paper 2: *Selling now or later, to process or not? The role of risk and time preferences in rice farmers' decisions*

The interest in using lab or field experiments in economics to understand behavior unobservable in actual settings has increased in recent years. An important research question in this strand of literature is to what extent behavioral parameters elicited through lab or field experiments can explain actual economic behavior (see, e.g., List and Levitt, 2007; Falk and Heckman, 2009). A few studies have attempted to experimentally measure uncertainty and time preference attitudes and test whether they explain various aspects of human behavior, such as savings, smoking, alcohol consumption, and occupational choice (see, e.g., Sutter et al., 2013; Maier and Sprenger, 2010; Tanaka et al., 2010). In development economics, recent contributions show the importance of risk and/or time preferences when it comes to technology and product adoption (Duflo et al., 2009; Giné and Yang, 2009; Liu, 2012; Liu and Huang, 2013).

In this paper, we carry out experiments to measure risk, ambiguity, and time preferences among Tanzanian rice farmers and use the results to explain actual field behavior. In particular, we look into previously unexplored post-harvest decisions of farmers, i.e., whether to sell paddy (unprocessed) or processed rice and whether to sell the harvest immediately or store it for future sale. Processing and storing rice implies processing costs, price uncertainties, and a delay in income. Our results show that estimated risk and time preferences predict farmers' field behavior. Impatient farmers are less likely to store paddy, and risk-averse farmers are less likely both to process and to store paddy for future sales. These results imply that there is scope for improving rice farmers' welfare substantially by addressing the uncertainties and problems associated with rice processing and storage.

Paper 3: *Credit, LPG Stove Adoption and Charcoal Consumption: Evidence from a Randomized Controlled Trial*

Many households in urban Africa continue to use charcoal for cooking even when income increases. Tanzania (TZ) experienced rapid economic growth from 2001 to 2007, yet the number of households using charcoal as their main source of cooking energy in Dar es Salaam increased from 47 percent to more than 70 percent over the same period (World Bank, 2009). This contradicts the expectations of the energy ladder hypothesis, which predicts declining reliance on biomass fuel as income rises. Biomass fuels like charcoal have adverse impacts on forests, biodiversity, health of household members, and the climate (Köhlin et al. 2011; Hanna et al, 2012). What does it take to make households switch to cleaner energy sources such as electricity or liquefied petroleum gas, LPG?

One factor discouraging households from switching to clean energy sources is the high startup cost of modern cooking appliances (Miller and Mobarak, 2013; Lewis and Patanayak, 2012). As a short-run solution, most previous studies have focused on assessing the uptake and impact of improved biomass fuel stoves, which actually promote the use of the same fuels but with higher efficiency (Hanna et al., 2012; Miller and Mobarak, 2013; Burwen and Levine, 2012; Gebreegziabher et al., 2014). Our study extends this growing body of literature by evaluating the impact of a relatively more modern and cleaner cook stove, the LPG stove, on charcoal consumption among poor urban households. Our intervention encourages a total fuel switch rather than a mere reduction of biomass fuel. We design and implement a novel randomized controlled trial to measure the uptake and impact of the LPG stove and whether it matters if the stove is acquired on credit or through a subsidy. We find a high level of stove uptake (70 percent) when liquidity constraints are relaxed through either credit or a subsidy. In addition, a number of covariates (e.g., ownership of saving account, number of years using the charcoal stove, whether the residential building is privately owned, and distance from the nearest charcoal vendor) are found to influence the adoption decision. We show that relative to households in the control group, adoption of LPG stoves reduced charcoal use by 47.5% in the treated group. However, subsidies for stove purchases resulted in a much larger reduction in charcoal use (54 percent) than providing access to credit (41 percent). We highlight the importance of relaxing households' financial constraints and improving access to credit in order to encourage urban households to switch to clean energy sources and save the remaining forest resources of Africa.

Paper 4: *Why (field) experiments on unethical behavior are important: Comparing stated and revealed behavior*

Unethical or dishonest behavior in the form of lying, cheating, and pursuing one's own self-interest instead of following a focal social convention or norm is widespread. The literature shows that humans engage in unethical acts in order to maximize expected utility where the focus is on monetary rewards (Becker, 1968), but they also refrain from profitable acts of cheating in many cases (Ariely, 2012). If there is a clear tension between being honest and maximizing one's individual monetary return, there seems to be a general tendency to follow the norm and forgo profit. However, there is considerable individual heterogeneity, and circumstances, framing, the monetary consequences of the trade-off, beliefs about the norm, peer behavior, and many other aspects matter as well (Ariely, 2012; Gneezy, 2005; Rosenbaum et al., 2014).

Understanding unethical behavior is essential to many phenomena in the real world. The vast majority of existing studies have relied on stated behavior in surveys, and some have been based on incentivized experiments in the laboratory. The problem with naturally occurring data in this context is that dishonest behavior often cannot be observed or can only be observed partially, creating all sorts of problems with the interpretation of data. Randomized controlled trials in the field offer a potential remedy, but so far they have been used very sparsely when it comes to studying dishonest behavior. Among the few recent exceptions in economics are Shu et al. (2012), Azar et al. (2013), and Pruckner and Sausgruber (2013).

In this paper, we carry out a field experiment in a unique setting. A survey administered more than one year before the field experiment allows us to compare stated unethical behavior with revealed behavior in the same situation. Our results indicate a strong discrepancy between stated and revealed behavior. This suggests that, given a natural setting, people may actually behave differently from what they would otherwise "brand" themselves to be. This calls for using caution when interpreting stated behavioral measures in research on unethical behavior.

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Paper 1

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Improving Welfare Through Climate-Friendly Agriculture: The Case of the System of Rice Intensification

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Abstract We use rich survey data to investigate the economic impact of a climate-friendly rice farming method known as the system of rice intensification (SRI) on the welfare of rain-dependent small-holder farmers in Tanzania. SRI reduces water consumption by half, which makes it a promising farming system in the adaptation to climate change in moisture-constrained areas, and it does not require flooding of rice fields, resulting in reduced methane emissions. Endogenous switching regression results suggest that SRI indeed improves yield in rain-dependent areas, but its profitability hinges on the actual market price farmers face. SRI becomes profitable only when the rice variety sells at the same market price as that of traditional varieties, but results in loss when SRI rice sells at a lower price. We argue that the effort of promoting adoption of such types of climate-friendly agricultural practices requires complementary institutional reform and support in order to ensure their profitability to small-holder farmers.

Keywords Adaptation to climate change · Endogenous switching regression · Impact evaluation · System of rice intensification · Tanzania

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1 Introduction

There is strong scientific evidence that our planet is warming and that this is resulting in climate change, which is predicted to impact society and ecosystems in several ways. Climate change is expected to result in extreme weather events, changing precipitation, sea-level rise, high risk of extinction of marine species, and declining agricultural yield in many regions of the world (IPCC 2014). Given its high dependence on climatic variables, agriculture will be affected more adversely than other sectors. One of the most vulnerable regions to climate change is Sub-Saharan Africa, whose agricultural sector provides livelihood for over 70 % of the population and which is known for its low productivity. However, due to lack of political will by governments, the process of reaching a deal to mitigate climate change through reduction of greenhouse gas emissions has been challenging (UNFCCC 2014). As a result, many governments, climate activists, and others have emphasized the urgent need for adaptation to climate change in a variety of ways. This paper investigates the potential adaptation role of a climate-friendly rice farming practice known as the system of rice intensification (SRI) in improving yield in a rain-dependent farming setup.

SRI is a low-tech but climate-friendly farming system developed outside research and development-intensive agricultural institutions by a Jesuit priest in Madagascar in the 1980s. It involves raising rice seedlings in a carefully managed, garden-like nursery; single widely-spaced transplants; early and regular weeding; carefully controlled water management; and application of compost to the extent possible (Stoop et al. 2002; Uphoff 2002). SRI has been shown to increase yield by more than 100 % and reduce water demand by about 50 % (Stoop et al. 2002; Uphoff 2002), making it a potentially effective farming technique in the adaptation to climate change in moisture-constrained areas in the future. Furthermore, the traditional method of growing rice involves flooding of rice fields with water and this has been documented to cause anaerobic decomposition of organic matter in the soil, which results in the release of methane, the second major greenhouse gas (USEPA 2006; Khosa et al. 2011). Given its low use of water without flooding of rice fields, SRI has been documented to reduce methane emission by 22–64 % (Gathorne-Hardy et al. 2013; Suryavanshi et al. 2013; Choi et al. 2014), making it a useful agricultural practice to mitigate climate change. Studies (Gathorne-Hardy et al. 2013; Suryavanshi et al. 2013) also show that the global warming potential of traditional rice fields is reduced by 20–30 % trough application of SRI.[1] Not surprisingly, these claims have generated substantial discussion among agricultural scientists (Glover 2011).

The few early studies undertaken by economists seem to confirm that yields do increase, yet SRI is labor demanding and the overall effect of its adoption on net income may be insignificant. Using data from small-holder farmers in Timor Leste, Noltze et al. (2013) show that SRI improves yield and income but when compared with conventional rice grown under favorable conditions, it is not beneficial. Takahashi and Barrett (2014) also used data from Indonesia and document that the farming practice results in significant yield increases. However, these authors argue that given SRI involves increased use of family labor, it reduces allocation of family labor to non-farm activities and consequently does not result in income gains. This increased labor requirement of SRI has been documented to partly explain the low adoption rate and in some cases disadoption of the technology in developing countries (Barrett et al. 2004; Noltze et al. 2012; Moser and Barret 2003).

[1] See "http://sri.cals.cornell.edu" for comprehensive information on the productivity and climate impacts of SRI.

In the present paper, we build on these earlier studies and investigate the impact of SRI on yield and total household income in a rain-fed small-holder farming set-up. The contributions are twofold: First, while previous studies have investigated the impact of SRI in a set-up where rice farming takes place with irrigation, we examine the impact in a rain-fed farming set-up in rural Tanzania, a Sub-Saharan African country that has been documented to be highly vulnerable to climate change (Kreft and Eckstein 2014). If SRI is proven to provide more yield than the conventional rice farming practice in a moisture-constrained rain-fed farming set-up, it can play a significant role in the adaptation to climate change by small-holder farmers in developing countries. Second, Takahashi and Barrett (2014) argue that although SRI improves yield, it may not have a significant impact on income because of its relatively higher labor requirement. These authors however did not have detailed data on hired labor, leading to potential underestimation of the labor cost of the technology. We collected detailed data on both family and hired labor and compute the net economic benefits of SRI using credible imputation techniques following (Jacoby 1993). As a result, we are able to investigate the impact of the technology on important household outcomes like yield, farm income and overall household income at different price levels of paddy.

Endogenous switching regression results, which take care of selection into SRI suggest that the practice is indeed yield enhancing in a rain-fed set-up. Adopting this climate-friendly farming practice on average offers 58 % higher yield per acre. This provides strong evidence that the method promotes yield while reducing water consumption and methane emissions. However, SRI farmers on average have higher costs/acre due to increased demand for both family and hired labor. The most pivotal effect of the technology on our sample of Tanzanian farmers is the differential impact on revenue, which is determined by the market price of paddy. While the traditional rice cultivated by non-SRI farmers and the rice breed cultivated by SRI farmers (known as SARO 5) are treated as homogenous goods in the metropolitan areas of the country, SRI farmers in the study area received a substantially lower price per kg for SARO 5 in the local market just after harvest. Our estimates indicate that when using the low SARO 5 paddy price (46 % lower than the price paid for the traditional paddy), SRI farmers earn significantly less profit than non-SRI farmers. However, for a uniform price across all rice varieties, a situation that prevailed in the market a few months later, adopting SRI becomes a relatively more profitable decision despite increased labor costs. The key policy implication that emerges from our analysis concerns the importance of addressing distortion and uncertainty of market price of rice and alleviating storage problems.

This paper is structured as follows: Sect. 2 introduces the context and survey area. Section 3 presents the empirical framework and estimation strategy. The data and descriptive statistics of key variables are presented in Sect. 4. Section 5 discusses regression results from an endogenous switching regression model. Finally, Sect. 6 concludes the paper.

2 Context and Study Area

Agriculture is estimated to account for about 10–12 % of total greenhouse gas emissions globally (IPCC 2014). The amount of greenhouse gases emitted from agriculture, forestry, and fisheries almost doubled over the past fifty years (FAO 2014). Rice is an agricultural crop that contributes significantly to greenhouse gas emissions. Worldwide, rice is estimated to grow on more than 140 million hectares of land, and 90 % of rice land is estimated to be flooded during growing (Wassmann et al. 2009). Scientific evidence shows that flooding of rice fields causes anaerobic decomposition of organic matter in the soil and thus emissions of methane, the second major greenhouse gas (USEPA 2006; Khosa et al. 2011).

SRI was invented in 1983 by Father Henri de Laulanie, a French Jesuit priest in Madagascar (Stoop et al. 2002). It originally constituted a standard set of principles to be applied jointly, which include: (1) raising seedlings in a carefully managed, garden-like nursery; (2) early transplanting of 8–15 days old seedlings; (3) single, widely spaced transplants; (4) early and regular weeding; (5) carefully controlled water management; and (6) application of compost to the extent possible (Stoop et al. 2002; Uphoff 2002). However, it was later recommended that these principles should not be regarded as a "standard package" but rather as a suite of flexible principles to be adapted to local conditions (Uphoff 2002; Glover 2011). For example, it is advised that, when necessary, including the use of other better inputs such as high yield varieties, mechanical weeders, and fertilizer into these practices will maximize the gain from SRI (CIIFAD 2012).

Our survey took place in the Kilombero district in the Morogoro region of Tanzania. Approximately 80 % of Tanzania's population live in rural areas and agriculture comprises more than 25 % of the country's GDP (CIA 2014). Agricultural production is dominated by production of cassava, maize, and rice by small-holder farmers. In terms of cereal production, rice is the second most important cereal and is cultivated by 95 % of farmers in the survey region (NBS 2015). Rice harvest is therefore central to the welfare of the country's population.

SRI was first introduced in Tanzania in 2009 by a rice-producing company called AGRICA through its subsidiary firm Kilombero Plantation Ltd (KPL), which begun cultivating rice in the Kilombero region. The program was initially introduced to farmers from three villages (Lukolongo, Mngeta, and Mkangawalo) and later expanded to cover nine villages in the Kilombero district. Initial adopters received training on an SRI plantation using demonstration plots (0.25 acres large), on which KPL financed all the extra costs associated with the training. After observing the outcome from the demonstration plots, the adopting farmers applied the technology on their own plots in the following cropping season.

New farmers have joined the program in each subsequent year since 2009. For example, in the year 2010/11, 250 more farmers from six villages joined the program and 10 more demonstration plots were established in all villages, each serving 25 farmers. In the agricultural year 2011/12, 1350 more farmers joined the program. In November 2011, NAFAKA (USAID Feed the Future Project) joined AGRICA for a rapid expansion of the project, and together they planned to scale up the project to cover up to 5000 households by 2016. With extra support from the African Enterprise Challenge Fund, KPL has scaled up SRI to about 6500 farm households already in 2014. More expansion is envisaged with significant amounts of resources devoted to it but with little knowledge on the "true" impact of the technology among its users, which is important in order to justify such expansion and sustainability of the technology.

The SRI in Kilombero has been introduced among the rain-dependent farmers resulting in a number of modifications to fit such agro-ecological conditions. The SRI principles in Kilombero include: (1) sorting of the rice seeds to select good versus bad seeds, (2) direct planting of two seeds per hole in upland areas, (3) widely spaced seeds/seedlings on a 25 cm $\times$ 25 cm grid square pattern, (4) mechanical weeding using simple mechanical weeders, (5) use of chemical fertilizer, and (6) use of an improved seed variety known as SARO 5. Such modifications of SRI are accepted and very common in other parts of the world as pointed out by Uphoff (2002) and Glover (2011).

Thus, one cannot obviously rely on the documented impact of SRI in irrigated agricultural systems to justify its expansion in rain-fed set-ups. A rigorous and independent impact evaluation study is important to ascertain its true impact. This is in line with Noltze et al. (2013), who point out that the impact of SRI may depend crucially on the reference system or context. Results from the current study will provide the first evidence on the impact of SRI on yield and welfare in small-holder rain-dependent agriculture. Such evidence should be very useful to policy makers and others who aim at intensifying adoption of the technology in other areas within the country and the continent at large to improve food security and adaptation to climate change.

3 Empirical Strategy

To investigate the impact of SRI on yield and the welfare of rain-dependent small-holder farmers, we need to address the potential problem of selection bias. Selection bias originates from the fact that we do not know what the outcome for a household participating in the program (treated household) would have been had it not participated. If treatment were assigned randomly, the outcome of untreated households would serve as a good estimate of the counterfactual. However, if households participating in the program have characteristics that differ from those of the untreated, it is very likely that a comparison of the outcome between the two groups (treated and untreated) will give biased results. As participation in the SRI program was not decided randomly as it would be the case in randomised control trials (RCT), one can expect biased results if a simple OLS is applied to estimate the impact of SRI on the outcome variables of interest.

The other credible strategy to identify the impact of SRI on welfare of smallholder farmers would be to use a difference-in-differences estimation technique on data collected from both the treatment and control groups before and after the SRI intervention. Such a method is applicable when the technology is distributed exogenously to a group of farmers in a series of interventions over time. Unfortunately our data is observational data collected after the technology has been adopted by a group of farmers. As a result, we are not able to use this method.

The next suitable method to account for selection bias is the endogenous switching regression model (Maddala 1983). Using conditional expectations, i.e., the hypothetical case of the outcome for SRI farmers had they not participated; it is possible to compute an estimate of the impact of SRI participation. It is thus possible to compare this expected outcome with actual outcome to infer a selection bias-corrected estimate of the impact of SRI. We adopt this method to estimate the impact of SRI on yield and the welfare of rain-dependent small-holder farmers in rural Tanzania because it takes both observed and unobserved (e.g., motivation and attitude of farmers) factors into account when estimating the impact of the program.[2]

A switching regression is performed in two stages. In the first stage, selection into the program is specified with a binary model, and the equations for the outcome of interest, in this case rice yield per acre and farm profit, are modeled for both SRI participants and non-participants conditional on selection. A rational farmer is assumed to decide to participate in SRI when the expected utility derived from participation in SRI (S_1^*) is greater than the utility received from not participating (S_0^*). However, given that one does not observe expected utility but only participation in SRI, the participation decision (S) is treated as dichotomous: $S = 1$ if $S_1^* > S_0^*$ and $S = 0$ otherwise. One could thus use a latent variable framework to model the decision to participate in SRI as follows:

$$S^* = Z\alpha + \varepsilon, \tag{1}$$

[2] One other alternative method to estimate the impact of a program on outcome variables of interest using cross-sectional observational data is the propensity score matching (PSM). However, this method assumes that selection into a program is based on observable characteristics only (Heckman et al. 1997), which we do not expect to be the case in rural Tanzania. As a result, we do not use it in this paper.

where Z represents an $n \times m$ matrix of explanatory variables (farm and household characteristics), α is an $m \times 1$ vector of model parameters to be estimated, and ε is an $n \times 1$ vector of normally distributed mean zero random error terms.

In the second stage, separate outcome equations for each outcome variable of interest are specified for SRI participants and non-participants.

$$y_1 = X_1\beta_1 + \epsilon_1 \quad \textit{if}\; S = 1 \tag{2}$$

$$y_0 = X_0\beta_0 + \epsilon_0 \quad \textit{if}\; S = 0, \tag{3}$$

where y_j $(j = 1, 0)$ is an $n \times 1$ vector of outcome variables per acre; y_1 and y_0 indicate the outcome variables (yield, profit and total household income) for SRI and non-SRI farm households, respectively. X_j represents an $n \times K$ matrix of explanatory variables and β_j is a $k \times 1$ vector of parameters to be estimated. If unobserved farmer characteristics, such as farmers ability and motivation determine both the decision to take part in SRI and the outcome variables, the error term in the selection equation, i.e., (1) would be correlated with the error terms in (2) and (3).

The error terms ε, ϵ_1 and ϵ_0 are assumed to follow a tri-variate normal distribution with zero mean and a non-singular covariance matrix specified as:

$$cov(\varepsilon, \epsilon_1, \epsilon_0) = \begin{pmatrix} \sigma^2_{\epsilon_0} & \sigma_{\epsilon_1\epsilon_0} & \sigma_{\epsilon_0\varepsilon} \\ \sigma_{\epsilon_1\epsilon_0} & \sigma^2_{\epsilon_1} & \sigma_{\epsilon_1\varepsilon} \\ \sigma_{\epsilon_0\varepsilon} & \sigma_{\epsilon_1\varepsilon} & \sigma^2_{\varepsilon} \end{pmatrix}, \tag{4}$$

where σ^2_{ε}, is the variance of equation (1), i.e., the selection equation, which is assumed to be 1 as the vector of parameters in ε are estimable only up to a scale factor. $\sigma^2_{\epsilon_1}$, and $\sigma^2_{\epsilon_0}$ are the variances of the error terms ϵ_1 and ϵ_0 in Eqs. (2) and (3) respectively, and $\sigma_{\epsilon_1\varepsilon}$ and $\sigma_{\epsilon_0\varepsilon}$ represent the covariance between ε and ϵ_1, and ϵ_0, respectively. The covariance between ϵ_0 and ϵ_1 is not defined as the outcome variables of interest are never observed simultaneously.

If there is selection bias, conditional on participation in SRI, the expected values of the error terms in Eqs. (2) and (3) will be different from zero:

$$E(\epsilon_1|S = 1) = E(\epsilon_1|\varepsilon > -\alpha Z) = \sigma_{\epsilon_1\varepsilon}\frac{\phi(Z\alpha)}{\Phi(Z\alpha)} = \sigma_{\epsilon_1\varepsilon}\lambda_1 \tag{5}$$

$$E(\epsilon_0|S = 0) = E(\epsilon_0|\varepsilon \le -\alpha Z) = \sigma_{\epsilon_0\varepsilon}\frac{-\phi(Z'\alpha)}{1 - \Phi(Z'\alpha)} = \sigma_{\epsilon_0\varepsilon}\lambda_0 \tag{6}$$

where ϕ and Φ are the probability density and the cumulative distribution function of the standard normal distribution, respectively. Following Maddala (1983), one can make the substitution $\lambda_1 = \phi(Z\alpha)/\Phi(Z\alpha)$, $\lambda_0 = -\phi(Z\alpha)/1 - \Phi(Z\alpha)$ and write the outcome equations for participants and non-participants of SRI as:

$$y_1 = X_1\beta_1 + \sigma_{\epsilon_1\varepsilon}\lambda_1 + u_1 \quad \textit{if} \quad S = 1 \tag{7}$$

$$y_0 = X_0\beta_0 + \sigma_{\epsilon_0\varepsilon}\lambda_0 + u_0 \quad \textit{if} \quad S = 0. \tag{8}$$

As the terms $\sigma_{\epsilon_j\varepsilon}\lambda_j$ are omitted from Eqs. (2) and (3), ordinary least square (OLS) estimation would result in biased and inconsistent estimates of the β parameters in the two equations. In addition, the error terms u_j would be heteroskedastic and as a result, OLS would give inefficient parameter estimators for the βs in (7) and (8). An efficient method to estimate endogenous switching regression models is full information maximum likelihood estimation (FIML). If one could find at least one variable in Z that is excluded from X, the parameters of interest can be estimated consistently using the FIML method which works in a simultaneous equation framework.

In this study, our main interest is to estimate the treatment effect of participation in SRI, i.e., how participation in the SRI program affects rice yield per acre, farm profit and total household income. The endogenous switching regression method can be used to compare expected yield and farm profit with the counterfactual hypothetical case that farm households did not adopt SRI. One could derive the conditional expectations and counterfactual hypothetical cases as follows:

$$E(Y_1|S=1) = X_1\beta_1 + \sigma_{\epsilon_1\varepsilon}\lambda_1 \tag{9}$$

$$E(Y_0|S=0) = X_0\beta_0 + \sigma_{\epsilon_0\varepsilon}\lambda_0 \tag{10}$$

$$E(Y_0|S=1) = X_1\beta_0 + \sigma_{\epsilon_0\varepsilon}\lambda_1 \tag{11}$$

$$E(Y_1|S=0) = X_0\beta_1 + \sigma_{\epsilon_1\varepsilon}\lambda_0. \tag{12}$$

Following Heckman et al. (2001), one can compute the average treatment effect on the treated (ATT) (the change in the outcome variable of interest due to participation in SRI) from Eqs. (9) and (11) as follows:

$$ATT = E(Y_1|S=1) - E(Y_0|S=1) = X_1(\beta_1 - \beta_0) + (\epsilon_1\varepsilon - \epsilon_0\varepsilon)\lambda_1. \tag{13}$$

Similarly, we can compute the effect of the treatment on the untreated (ATU) for the farm households that actually did not participate in SRI as the difference between Eqs. (12) and (10) as:

$$ATU = E(Y_1|S=0) - E(Y_0|S=0) = X_0(\beta_1 - \beta_0) + (\epsilon_1\varepsilon - \epsilon_0\varepsilon)\lambda_0. \tag{14}$$

4 Data and Descriptive Statistics

The data used in this study were collected in a survey conducted in the Kilombero district, located in the Morogoro region, Tanzania. The survey was conducted on 334 randomly selected rice farming households from eight villages in the Kilombero district for the farming season ending in June 2013. We collected information on all farming inputs applied from plot preparation to post-harvesting, alongside the output and marketing information. Out of the sampled farm households, 194 had adopted and applied SRI on at least one of their plots, while 140 had not. For each sampled household that operated multiple plots, one of the plots was randomly selected and detailed plot specific information was then collected for that particular plot. In addition to farming related data, we conducted real field experiments to elicit risk, ambiguity and time preference parameters of household heads, who make production and other important decisions in the household.

4.1 Farmer, Household, and Plot Variables

Table 1 outlines descriptive statistics of the variables for both SRI adopter and non-adopter farm households and statistical test results for differences in means. For convenience, we classified these variables into four categories: farmer characteristics, household characteristics, plot-specific characteristics, and plot-level application of SRI components. In all groups of variables, we observe statistically significant differences in mean values between SRI adopter and non-adopters for several variables. Specifically, SRI farmers are relatively older, belong to more social groups, and lived for longer years in the village than non-SRI farmers. SRI farm households also have larger households, more wealth, better access and larger visits by agricultural extension agents.

Table 1 Selected farmer, household and plot characteristics by SRI status

Variable	SRI		Non-SRI		Diff
	Mean	SD	Mean	SD	
Farmer characteristics					
Risk preference	0.584	0.276	0.561	0.269	0.023
Ambiguity preference	0.000	0.289	0.004	0.299	−0.004
Male	0.887	0.318	0.936	0.246	−0.049
Age (years)	44.356	12.215	40.793	11.143	3.563***
Education (years of schooling)	7.057	1.767	7.014	2.218	0.042
Literate (dummy = 1 if can read and write)	0.974	0.159	0.950	0.219	0.024
Married (dummy = 1 if married)	0.871	0.336	0.850	0.358	0.021
Experience in growing rice (years)	15.588	10.016	13.921	9.186	1.666
Social network (number of social groups)	0.944	0.182	0.776	0.399	0.168***
Number of years lived in the village	15.192	10.332	12.711	9.117	2.480**
Household characteristics					
Household size	4.892	1.825	4.421	1.843	0.470**
Wealth (assets values in 000 TZS)	832.545	1937.125	433,822	682.530	398.723**
Agriculture (whether farming is the main source of income)	0.974	0.159	0.943	0.233	0.031
Extension services (dummy, 1 = yes)	0.619	0.487	0.164	0.372	0.454***
Extension frequency (number of visits in a month)	1.459	1.564	0.264	0.685	1.194***
Plot-specific characteristics					
Plot size (acre)	0.981	0.719	2.789	3.229	−1.809***
Very fertile plot (dummy, 1 = yes)	0.412	0.494	0.407	0.493	0.005
Sloppy plot (dummy, 1 = yes)	0.119	0.324	0.157	0.365	−0.039
Distance of plot from homestead (min)	3.747	4.108	4.602	4.720	−0.855*
Distance of homestead from input market (min)	102.258	228.873	67.150	159.542	35.108
Plot-level application of SRI					
Quantity of seed (kg)	15.643	15.755	26.291	16.193	−10.648***
Household labor (man-days per acre)	63.326	84.183	32.931	31.122	30.395***
Household labor per adult equivalent (adjusted man-days per acre)	58.211	73.304	30.683	29.222	27.528***

Table 1 continued

Variable	SRI		Non-SRI		Diff
	Mean	SD	Mean	SD	
Hired labor (man-days per acre)	21.184	34.877	0.000	0.000	21.184***
Chemical fertilizer (dummy = 1 if fertilizer was applied on plot)	0.866	0.342	0.086	0.281	0.780***
Sort seed (dummy = 1 if seeds were sorted before planting)	0.918	0.276	0.450	0.499	0.468***
SARO 5 (dummy = 1 if SARO 5 seed variety was applied on plot)	0.969	0.174	0.121	0.328	0.848***
Square grid (dummy = 1 if planting was done on square grids)	0.856	0.352	0.071	0.258	0.784***
Observations	194		140		

At the plot level, SRI is practiced on relatively smaller plots (1 acre) compared with conventional methods (3 acres), and on plots located relatively closer to the homestead. This is likely due to the relatively higher production costs of the SRI technology (due to increased labor demand and purchase of supplementary inputs) and obviously to the perceived need for closer care and monitoring.

Table 1 also presents the extent of adoption of the different SRI components by adopters and non-adopters. Each component is applied by more than 85 % but never 100 % of the adopters. This may suggest the possibility of partial adoption of the package by a small fraction of the sample, and the phenomenon is not new in the SRI technology literature (e.g., see Takahashi and Barrett 2014; Noltze et al. 2012). We thus classify farmers as "SRI farmers" if they adopt at least four of the six components of the technology. We observe that SRI farm households apply larger quantity of seed on their plot than non-SRI farmers.[3]

The SRI has been documented to require more labor than the conventional rice planting method. Table 1 summarizes the labor requirement by SRI adoption status. Consistent with previous literature, SRI requires significantly more labor per acre than conventional rice cultivation methods. SRI farm households devote more of their working days to the farming process and even have to hire external labor to complement the household workforce. The difference is very large, possibly explaining the reason for small total acreage cultivated with SRI despite the potential yield gains. On average, one SRI acre requires a total of 63 man-days per season (21 of which come from hired labor) compared with only 33 man-days per conventionally farmed acre.

4.2 Outcome Variables

Table 2 presents outcome variables of interest by SRI adoption status. We consider four outcome variables: yield/acre, farm profit/acre, non-farm income (consisting of off-farm income and remittances), and total household income. Yield is calculated as the amount of

[3] In the results section, we introduce a different definition of SRI and perform some robustness checks.

Table 2 Outcome variables by SRI status

Variable	SRI		Non-SRI		Diff
	Mean	SD	Mean	SD	
Yield (tonnes/acre)	2.69	4.52	1.06	0.65	1.63***
Village-level average price of paddy per kg (in TZS)	343.81	46.37	638.90	90.51	−295.09**
Profit1: profit/acre at actual price faced by SRI farmers (in 000 TZS)	392.33	1147.05	594.05	430.21	−201.72**
Profit2: profit/acre at similar village-level prices (in 000 TZS)	883.12	1934.32	463.62	310.92	419.5**
Off-farm total annual income (in 000 TZS)	693.50	1031.09	657.74	1508.29	35.76
Total annual remittances (in 000 TZS)	4.23	5.56	3.75	5.43	0.48
Total income (Profit1)	1085.83	1567.98	1254.51	1620.36	−168.678
Total income (Profit2)	1576.62	2236.49	1124.08	1576.75	452.541**
Observations	194		140		

paddy harvested (in tons) per farmed acre. Remittance constitutes total amount of money in Tanzanian shillings (TZS)[4] received by the household from a relative living either abroad or in other regions of the country in the past farming season. Total household net income constitutes the sum of farm profit and non-farm income earned from either self or wage employment and remittances, all computed for the same farming season. Farm profit is calculated as the difference between total revenue from harvested rice paddy and total production costs incurred during the farming season.

To compute the farm revenue, we collected information on per unit market prices of unprocessed paddy and multiplied it by total harvest to obtain the total revenue per acre. Notably, around the survey month (September 2013, immediately after the harvesting season), we observe a significant difference in farm gate price between SRI rice and that from traditional methods, with the former about 344 TSH/kg while the later being about 639 TSH/kg. This shows that the price paid for the SRI paddy is lower by about 46 % from that of the traditional rice paddy. A follow-up survey in the same villages in February 2014 revealed that the unit prices of the two rice varieties converged. Surprisingly, across all the months, we do not observe similar price differences between the two varieties in larger urban markets, especially the Dar es Salaam region, the largest city in the country. There does not seem to exist any distinction between SRI and conventionally grown rice varieties in the final market since they are often mixed prior to selling and sold as one type of crop.

Given what we observe in the final market, it seems clear that the price difference we captured in our survey is likely to be just a spurious difference caused by some kind of information asymmetry and market imperfection. SRI farmers specifically mentioned that middlemen in the area force them to sell their paddy from the SARO 5 breed at a lower price because the rice does not taste as the traditional rice variety and consumers in urban areas pay less for it. Takahashi and Barrett (2014) find similar differences in their setting and

[4] At the time of the survey, 1 USD = 1600 TZS.

decide to ignore the difference and use the same price for both varieties. We, however, take this price difference into account to shed some light on its potential implications on welfare of small-holder farmers. Thus we compute two different types of revenue for rice farmers. In the first case (revenue1), we use the actual prices faced by the farmers, assuming that the observed price difference is genuine. In the second case (revenue2), we compute rice revenue based on the village-level mean prices regardless of rice variety, assuming that the observed difference is purely spurious. To allow comparison of our results, the calculations of total incomes take into account these price differences.

We then calculate production costs accounting for all inputs used in the 2012/2013 farming season, from farm preparations to harvest. Inputs for which we collected data include seeds, fertilizer, herbicides, pesticides, hired labor, and unpaid family labor. Given the unpaid nature of household labor, it is not trivial to assign value to such labor input. One approach could be to use observed market wages to reflect the opportunity cost of the unpaid family labor, as recommended in Rosenzweig (1980) and applied in Takahashi and Barrett (2014). However, this requires the strong assumption that labor markets are very competitive such that the value of the marginal product of labor for a self-employed farmer equals that of the market wages. However, labor markets in developing countries, especially in rural areas, are far from perfect and choosing to work on family farms may reflect a difference in the value of marginal product of labor on household farm to that of the market wage rates (Jacoby 1993; Barrett et al. 2008; Chavas et al. 2005). In order to avoid such measurement error, we employ an alternative approach—the shadow wage approach—suggested by Jacoby (1993).

To this end, we first estimate the Cobb–Douglas production function where two types of labor (hired and household labor) enter as two distinct production inputs together with seeds and fertilizer.[5] We then estimate the marginal product of household labor for each farming unit as the product between the estimated coefficient of household labor and yield-labor ratio for each household. Shadow wage for the household unpaid labor is then given as the value of the marginal product of labor in the household, considering the total man-days worked on the plot by all household members across the farming period. Total labor cost per acre is then calculated as the sum of total shadow wages of the household and total market wages paid out by the household to hired workers per farm acre. In doing so, we computed family labor in adult equivalent units utilizing the scales used by the World Bank for Tanzania.[6]

The sum of production costs thus constitutes the cost of all purchased inputs (including hired labor) and the total shadow wage for household labor adjusted for adult equivalent units. Farm profits are then calculated as the difference between total revenue and total production cost. We thus have two different profits (profit1 and profit2) depending on whether revenue1 or revenue2 is in use. Total household income was computed as the sum of profit, off-farm income and remittances received in the same farming season.

According to Table 2, the average yield of an SRI plot is about 2.69 tons/acre, which is statistically significantly higher than that of non-SRI plots, which is only 1.06 tons/acre. This implies that SRI farmers on average enjoy about 154 % more yield/acre than non-SRI farmers. Whether this gain in yield translates into higher profits in the face of increased production costs is what we explore in the next section. Preliminary assessment of the descriptive statistics on profit/acre suggests two different results depending on the profit variable used. While profit1 (computed with 46 % lower price for the SRI paddy variety) suggests that SRI farmers generate a lower average profit than their non-SRI counterparts, profit2 (which assumes the same price for the two paddy varieties) gives the opposite outcome. Preliminarily, the table

5 Results are available from the authors upon request.

6 See NBS (2008) for details on the adult equivalent units.

reveals that SRI farmers earn more from off-farm sources and remittances. However, the mean differences in these variables between SRI and non-SRI farmers are not statistically significant. As expected, SRI farmers enjoy significantly larger total household income under similar market prices for paddy. It is important to note that these descriptive statistics represent simple mean comparisons and thus do not take into account selection bias.

5 Results

Table 3 presents results from the endogenous switching regression model estimated with the full information maximum likelihood with standard errors clustered at the *village* level.[7] The first set of columns report the selection equation (Eq. 1) on adopting SRI or not. The second and third sets of columns present the outcome equation (the log of yield/acre) under the SRI (Eq. 2) and non-SRI (Eq. 3) regimes, respectively. We use the number of years farmers had lived in the village and social networks (measured by number of group memberships) as the identifying instruments as these variables are expected to affect participation in SRI but not the outcome variables of interest directly. We follow Di Falco et al. (2011) to check for the admissibility of these instruments by undertaking a simple falsification test: if the identifying instrument is valid, it will affect adoption of SRI but it will not affect the outcome variable of interest among farm households that did not adopt SRI.[8] Table 5 presented in Appendix shows that both the number of years farmers had lived in the village and social networks are valid selection instruments. They jointly and statistically significantly affect the decision to adopt SRI or not adopt (Model 1, $\chi^2 = 27.53$; $p = 0.00$) but not the log of yield per acre by the farm households that did not adopt SRI (Model 2, $F\text{-}stat = 0.63$; $p = 0.54$).

Given the large size of the tables and that this study focuses on several outcome variables, in this section we only present and discuss the first stage results for the yield outcome presented in Table 3. We focus more on the discussion of the estimated impact of SRI on all the outcome variables, which is the primary objective of our study. The first-stage results for all other outcome variables are available from the authors upon request, and their interpretation follows the same analogy as those for the yield outcome variable.

Results from the selection equation presented in column 1 of Table 3 show that male farmers are less likely to participate in SRI while literate farmers are more likely to participate in SRI. Richer farm households and those with better access to extension services also have a higher likelihood of participating in SRI, as shown by the statistically significant coefficients of log of wealth and extension services variables. Table 3 on the other hand shows that plot size and the quantity of seed applied have negative relation with adoption of SRI. This most probably reflects the productivity potential of the SARO 5 rice variety and the SRI method, i.e., its ability to give higher yield with lower quantity of seed on relatively smaller plots. The endogenous switching regression results also show that SRI farm households allocate more labor to their plots than non-SRI farm households. This is expected given the relatively higher labor requirement of this technology. Finally, having access to larger social network has a positive and statistically significant effect on SRI participation. The strong role of social networks we find here is consistent with earlier studies (e.g., Bandiera and Rasul 2006; Conley and Udry 2010) documenting the role of information through social networks in diffusing

[7] We estimated our regressions in STATA using the "movestay" command developed by Lokshin and Sajaia (2004).

[8] We thank an anonymous reviewer for suggesting this test.

Table 3 Endogenous switching regression results for yield

Variables	[1]		[2]		[3]	
	SRI adoption		Yield: SRI		Yield: non-SRI	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Farmer characteristics						
Risk preference	−0.041	0.616	0.111	0.118	−0.189	0.118
Ambiguity preference	−0.653	0.578	0.084	0.087	−0.071	0.149
Male	−0.717***	0.302	−0.078	0.093	−0.058	0.165
Age (years)	0.018	0.031	0.033***	0.009	0.004	0.027
Age squared/100	−0.020	0.040	−0.034***	0.011	−0.007	0.029
Education (years of schooling)	−0.060	0.090	0.035*	0.021	−0.006	0.027
Literate (dummy = 1 if can read and write)	1.547**	0.669	−0.066	0.247	−0.127	0.136
Married (dummy = 1 if married)	0.140	0.374	0.263***	0.088	0.059	0.116
Experience in growing rice (years)	0.003	0.010	−0.006	0.004	0.000	0.001
Household characteristics						
Household size	0.065	0.056	−0.022	0.014	−0.011	0.029
Wealth (Assets values in 000 Tshs)	0.359**	0.161	0.041	0.035	0.003	0.053
Agriculture (whether farming is the main source of income)	0.787	0.663	−0.344	0.229	0.065	0.109
Extension services (dummy, 1 = yes)	0.820*	0.486	−0.026	0.088	−0.040	0.122
Extension frequency (number of visits in a month)	0.237	0.243	0.037	0.037	0.027	0.074
Plot-specific characteristics						
Plot size (acres)	−1.076***	0.140	−0.035	0.079	−0.001	0.017
Very fertile plot (dummy, 1 = yes)	0.382*	0.222	−0.032	0.063	−0.019	0.043
Sloppy plot (dummy, 1 = yes)	0.149	0.413	−0.160**	0.074	0.099***	0.038
Distance of plot from homestead (min)	0.006	0.017	−0.008	0.009	−0.002	0.004
Distance of homestead from input market (min)	0.001	0.001	0.000	0.000	0.000*	0.000
Total labor (man-days adjusted per adult equivalent unit)	0.010***	0.003	0.001	0.001	−0.001	0.002
Quantity of seed (kg)	−0.059***	0.014	0.008***	0.003	0.002	0.004
Social network (number of group memberships)	1.321***	0.260	–	–	–	–

Table 3 continued

Variables	[1]		[2]		[3]	
	SRI adoption		Yield: SRI		Yield: non-SRI	
	Coeff.	SE	Coeff.	SE	Coeff.	SE
Number of years lived in the village	0.012	0.018	–	–	–	–
Intercept	−6.026**	2.667	−0.127	0.316	0.818	0.898
ln σ_i	–	–	−0.750	0.169	−1.282	0.413
$\sigma_{i\varepsilon}$	–	–	−0.352	0.260	−0.765	1.251
Wald test for independent equations χ^2	–	–	–	–	2.94*	
Observations	334	–	194	–	140	

Standard errors clustered at the village level
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

productivity-enhancing modern agricultural technologies. We don't find the effect of number of years lived in village to be statistically insignificant.

The estimates for the outcome equations (the log of yield) are presented in columns 2 and 3 of Table 3. The statistically significant estimates of the covariance matrix and the Wald test result, which rejects the null hypothesis that the three equations are jointly independent (Lokshin and Sajaia 2004), favor the endogenous switching regression. The results suggest notable differences in correlation of several variables with the yield between adopters and non-adopters. For example, while applying larger quantity of seed is associated with higher yield among SRI farmers, it does not have a statistically significant effect on yield by non-SRI farm households. In addition, we find that other differences such as age, education, marital status, and slope of plots have different impacts on the two regimes. Such differences probably highlight that the two groups of farmers are potentially different in several characteristics, which implies that there is selection into the SRI program. In view of this, applying the endogenous switching regression framework, which takes selection based on observable and unobservable characteristics between the two groups of farmers into account, would be the appropriate estimation approach than pooling the data into a single equation.

5.1 The Economic Returns to SRI Participation

The key objective of this paper is to estimate the treatment effect of participation in SRI for small-holder farmers operating in a rain-fed set-up. Table 4 presents the average treatment effect on the treated (ATT) and the average effect of the treatment on the untreated (ATU). We investigate the impact of SRI participation on four key outcome variables: yield measured as the amount of paddy harvested in tons/acre, on-farm profit measured as the difference between rice revenue and production cost, non-farm income representing off-farm income and remittances, and total farm household income measured as the sum of crop profit and non-farm income.

The estimation results for the ATT are presented in column [1] of Table 4. As can be seen, SRI generates significantly higher yields to adopters compared with what they would have harvested under a non-adoption scenario, i.e., if they did not adopt the technology. SRI on average results in a 58 % increase in yield as shown by the difference in the logarithm

Table 4 Average treatment effects of SRI on farm household economic outcomes

Outcome variable	[1]			[2]			[3]
	SRI-farmers			Non-SRI farmers			Transitional heterogeneity (TH)
	With SRI	Without SRI	ATT	With SRI	Without SRI	ATU	ATT − ATU
A. Yield (in logs)	1.092	0.512	0.580***	1.140	0.684	0.456***	0.124***
	(0.019)	(0.011)	(0.025)	(0.019)	(0.009)	(0.021)	(0.0026)
B. On-farm profit							
Profit1	392.410	599.14	−206.72***	26.380	594.470	−568.090***	361.370***
	(36.583)	(14.554)	(35.300)	(87.500)	(10.348)	(86.570)	(6.892)
Profit2	883.200	455.51	427.702***	455.369	463.917	−8.548	436.250***
	(61.499)	(9.600)	(60.586)	(124.030)	(7.280)	(123.018)	(10.206)
C. Non-farm income							
Off-farm total annual income	688.778	693.567	−4.789	−71.900	663.220	−735.11***	730.321***
	(40.797)	(40.327)	(51.828)	(80.690)	(43.330)	(87.320)	(7.646)
Remittances	4.280	4.750	−0.470	9.690	3.720	5.970	−6.44***
	(0.326)	(0.172)	(0.308)	(5.300)	(0.222)	(5.230)	(0.376)
D. Total income (B + C)							
Total income (Profit1)	1085.692	1367.667	−281.975***	1176.576	1252.441	−75.865	−206.110***
	(58.104)	(44.104)	(61.718)	(165.1163)	(48.896)	(163.719)	(12.854)
Total income (Profit2)	1576.438	1247.975	328.464***	1567.862	1121.622	446.241*	−117.777***
	(78.042)	(42.699)	(76.648)	(279.259)	(48.155)	(274.641)	(20.745)

of yield/acre with and without the technology in the SRI group. This is positive news to small-holder rice farmers who operate in rain-fed and moisture-constrained areas. The finding reveals the potential of the SRI practice as an effective response to climate change. Moreover, the yield gain is obtained with a reduction in water use and without having to flood the rice field, resulting in reduced emissions of methane, an important greenhouse gas.

Panel B of Table 4 reveals however that the impact of SRI on farm profit depends on the type of farm gate price that farmers face. If farmers face a lower price for the SRI paddy (or if we assume that the price differences are genuine), SRI adoption results in a significantly lower profit than the conventional method. Estimated results show that the ATT for SRI farmers is a loss (profit 1) of 206,720 Tanzanian shillings (TZS) compared with the non-adoption scenario. However, if one considers a situation where both rice varieties are sold at uniform prices, the gain in yield reported in panel A translates into a gain in farm profit even in the face of increased labor cost. SRI adopter farm households in this case enjoy TZS 427,000 more farm profit/acre compared with the scenario of not adopting. This is a key finding which adds to the technology adoption literature in developing countries that uninsured ex-post risk (such as unpredictable output price) may discourage farmers from adopting new productivity-enhancing modern agricultural technologies (Alem et al. 2010; Dercon and Christiaensen 2011; Alem and Broussard 2013).

Turning to the other outcome variables, the estimation results show that SRI has a negative effect on off-farm income and remittances although the effects are not statistically significant. Such insignificant differences are likely due to the fact that not so many household members in this setting have access to off-farm sources of incomes due to lack of employment opportunities. For example, on average, only one member per household in our data set is reported to have earned off-farm income (either from wage or self-employment) during the surveyed farming season. However, with the assumption of similar prices between SRI and conventional rice varieties (profit2), the technology is found to have a significant positive impact on the overall total household income of the adopters. On average, adopters increase their total household net income by approximately 328,400 TZS in each season.

Column [2] of Table 4 reports the impact of SRI on non-adopter farmers, i.e., what the impact on them would have been had they adopted SRI. This is an important policy question for governments, NGOs, international organizations, etc. interested in promoting adoption of productivity-enhancing technologies, such as SRI. The interesting question in this respect is, would the non-adopter farm households do equally well had they been given the opportunity to adopt the technology? The results in column [2] suggest that on average, the non-adopter farmers would gain similar benefits in most of the outcome variables, albeit with different magnitudes. Adoption of SRI by non-adopters would result in a positive but relatively smaller yield gain (i.e., 45.6 vs. 58 % by adopters). Non-adopters would also lose relatively more in terms of profit1 but would have a relatively larger gain in net total household income under profit2 compared to adopters of SRI. The final column (column 3) of Table 4 shows the transitional heterogeneity (TH = ATT − ATU). The results show that SRI farm households would have produced significantly more paddy/acre and would have enjoyed larger on-farm profits than those farm households that did not adopt SRI in the counterfactual case. This points out that there are some important sources of heterogeneity that makes SRI farm households produce more than non-SRI farmers, an important issue to take care of while estimating the impact of the SRI technology.

The differences in impact of the program on the outcome variables of interest between adopters and non-adopters are consistent with existing literature (e.g., Di Falco et al. 2011; Noltze et al. 2013; Carter and Milon 2005) and are mainly explained by the possible heterogeneity between the two groups. One important finding here is that, qualitatively, SRI is likely to benefit both types of farmers—adopters and non-adopters—providing support to the scaling up efforts. The apparent gain differences between the groups can be narrowed by addressing their main drivers, such as degree of social networks. Such networks are likely to have made SRI adopters exposed to better information and learning opportunities, something that currently puts them in a better position than non-adopters.

In this paper, we classified farmers as "SRI farmers" if they adopted at least four of the six components of the technology. Such a definition might be arbitrary. In order to address any concern that may arise due to such a definition, we considered different criteria and redefined SRI status. We specifically categorized farm households as SRI adopters if they follow three of the SRI practices: (i) sort seed, (ii) use square greed, and (iii) engage in mechanical weeding. These are the standard principles that are common in all SRI practices applied elsewhere in the world. This redefinition gave rise to a sample of 152 adopters only. We then estimated the ATT and ATU of the SRI technology. The results presented in Table 6 in Appendix confirm that our results remained qualitatively the same albeit with slight differences in magnitude. SRI on average results in an almost 60 % increase in yield as shown by the difference in the logarithm of yield/acre with and without the technology in the SRI group. The results in column [2] also suggest that on average, the non-adopter farmers would gain 59 % in yield/acre had they adopted SRI, a yield gain similar in magnitude with adopters of SRI.

6 Conclusions

The SRI has been documented to reduce water demand by about half (Stoop et al. 2002; Uphoff 2002), which makes it a potentially effective farming practice to implement in response to climate change in low-rainfall areas. SRI also results in a substantial reduction in the amount of methane emitted to the atmosphere as it does not involve flooding of rice fields (USEPA 2006; Khosa et al. 2011). This paper applies an endogenous switching regression model on detailed farm-level data on rain-fed farmers in rural Tanzania to investigate the impact of SRI on yield, farm profit, and overall household income. The endogenous switching regression technique enables us to control for unobserved farmer characteristics likely to affect both participation in SRI and the outcome variables of interest. The contributions of the paper are therefore in providing new evidence on the impact of the farming practice on yield in a moisture-constrained set-up and in revealing its full welfare impact by accounting for the full cost of the technology from detailed labor data.

Endogenous switching regression results confirm the importance of controlling for self-selection based on unobserved farmer characteristics. These unobservables capture important characteristics, such as motivation and attitude of farmers. But even after controlling for self-selection bias, the results show that participation in SRI increases yield significantly. On average, participation in SRI increases yield per acre by about 58 %. This is an important finding highlighting the significant potential of the technology in becoming an effective farming practice to implement in response to climate change in areas with erratic rainfall. Interestingly, our results also show that the impact of SRI on yield is larger for the farm households that actually did adopt than for the farm households that did not adopt SRI in the counterfactual case that they adopted. However, we find that the profitability of SRI hinges on the farm gate price that farmers face. If the price of SRI rice is significantly lower than that of traditionally grown rice, SRI results in a loss to farmers. However, if the rice varieties in the market have similar prices, the SRI technology results in a significantly higher profit compared with traditional methods. We do not find any statistical impacts of SRI on off-farm income and remittances, but if SRI rice is priced the same as traditionally grown rice, the yield gains are translated into gains in total household income.

The significant impact on yield documented in this study also suggests the potential of the technology in improving food security of poor rural communities in Sub-Saharan Africa. In Tanzania, like many other Sub-Saharan countries, rice is a major staple food next to cassava and maize. As a result, improving rice productivity is among the government's most important agricultural policy objectives. In this respect, the present study provides useful information to promote the practice of SRI in other rice-producing areas of the country. To this end, the role of social networks, which have been documented to play a significant role in the diffusion of SRI and other productivity-enhancing agricultural technologies (Bandiera and Rasul 2006; Conley and Udry 2010), should be given due consideration.

Finally, adoption and continued use of such climate-friendly and productivity-enhancing agricultural technologies to a great extent depend on their profitability. We documented that the price of the rice variety from SRI was about 46 % lower than that of the traditional rice at the farm gate in the surveyed area. However, we did not observe any difference in the price of the two rice varieties in larger urban markets, especially in the Dar es Salaam region, the largest city in the country. Actually, there does not seem to be any distinction between SRI and traditional rice varieties in the final markets since they are often treated as homogenous goods. Qualitative discussions with the farmers indicate that middle men take advantage of the information asymmetry in the rice market and force farmers to sell SRI paddy and rice at a lower price. This calls for the attention of the government to tackle such types of information asymmetry and price uncertainty, and ensure that farmers are offered the right price for their products.

Additional lessons can be learned from future research in relation to the exact impact of the SRI on yield and other household outcomes. Although our study sheds light on the possible impact of the technology in a rain-fed setup, we acknowledge the possible limitation of our data and identification strategy in disentangling the impact of SRI on welfare of farm households. The version of SRI practiced in the study area involves application of chemical fertiliser and an improved seed variety (SARO 5), while most traditional rice farmers in the area apply very little fertiliser on their plots and use traditional rice varieties such as the one called "Zambia". In view of this, some of the impact of the SRI technology may have been pronounced by the effect of improved seeds and chemical fertiliser. Future research based on a more detailed data set on the different versions of SRI applied in the area or in other Sub-Saharan African countries with similar set-ups can shed light on these and other aspects of the technology.

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Appendix

See Tables 5 and 6.

Table 5 Parameter estimates—test on validity of instruments

Variable	[1]		[2]	
	Adopted SRI		Yield: non-SRI	
	Coeff.	SE	Coeff.	SE
Social network (number of group memberships)	1.202***	0.245	0.022	0.058
Number of years lived in the village	0.016**	0.007	−0.002	0.002
Intercept	−1.068***	0.256	0.696***	0.058
Wald test on instruments	$\chi^2 = 27.530$***		F-stat = 0.63	
Observations	334		140	

Regression in column [1]: a binary probit model (pseudo $R^2 = 0.070$; regression in column [2]: ordinary least square ($R^2 = 0.007$). Standard errors clustered at the village level in parentheses
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 6 Average treatment effects of SRI on farm household yield

Outcome variable	[1]			[2]			[3]
	SRI-farmers			Non-SRI farmers			Transitional heterogeneity (TH)
	With SRI	Without SRI	ATT	With SRI	Without SRI	ATU	ATT − ATU
A. Yield (in logs)	1.103	0.504	0.599***	1.273	0.685	0.588***	0.011 ***
	(0.025)	(0.014)	(0.032)	(0.024)	(0.009)	(0.026)	(0.003)
Observations	152			140			

Average treatment effects computed based on a new definition of SRI status

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Paper 2

Selling now or later, to process or not?

The role of risk and time preferences in rice farmers' decisions

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Abstract:

In this study, we carry out experiments to measure risk, ambiguity, and time preferences among Tanzanian rice farmers and use the results to explain actual field behavior. In particular, we look into previously unexplored post-harvest decisions of farmers, i.e., whether to sell paddy (unprocessed) or processed rice and whether to sell the harvest immediately or store it for future sale. Processing and storing rice implies higher expected revenues but also processing costs, price uncertainties, and a delay in income. Our results show that estimated risk and time preferences predict farmers' field behavior. Impatient farmers are less likely to store paddy, and risk-averse farmers are less likely both to process and to store paddy for future sales. These results imply that there is scope for improving rice farmers' welfare substantially by addressing the uncertainties and problems associated with rice processing and storage.

JEL Classification: C25 C91 C93 D84 Q13

Key words: Ambiguity, Rice marketing, Risk preferences, Time preferences, Artefactual field experiment

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1. INTRODUCTION

The interest in using lab or field experiments in economics to understand behavior unobservable in actual settings has increased in recent years. An important research question in this strand of literature is to what extent behavioral parameters elicited through lab or field experiments can explain actual economic behavior (List and Levitt, 2007; Falk and Heckman, 2009). A few studies have attempted to experimentally measure uncertainty and time preference attitudes and test whether they explain various aspects of human behavior, such as savings, smoking, alcohol consumption, and occupational choice (Sutter et al., 2013; Maier and Sprenger, 2010; Tanaka et al., 2010). In development economics, recent contributions show the importance of risk and/or time preferences when it comes to technology and product adoption (Duflo et al., 2009; Giné and Yang, 2009; Liu, 2013; Liu and Huang, 2013).

It is plausible to argue that, just as risk and time preferences may influence non-farmers' choices concerning savings, profitable investments, and a healthy lifestyle (Sutter et al., 2013; Berge et al., 2014) as well as farmers' adoption of improved technologies (Liu, 2012; Liu and Huang, 2013; Duflo et al., 2009), they may also limit farmers' choices when it comes to post-harvest decisions. A few studies have attempted to explore other determinants of these decisions. Fafchamps and Hill (2005) analyze the choice between selling the yield at the farm gates and transporting it to the markets for an expected higher price among cotton farmers in Uganda and find liquidity constraints to be an important factor, but give little attention to risk and time preferences. Fu et al. (1988) study the attitudes of U.S. peanut farmers to various marketing alternatives and find that farmers who are content with status quo price levels have a lower probability of adopting an alternative with a better expected outcome. McLeay and Zwart (1998) analyze farmers' choice of cash sales versus forward contracts in New Zealand and find that some farmers may opt for forward contracts to avoid market price volatility, implying that risk preferences and transaction costs are likely to influence the marketing decisions. Yet, Tanaka et al. (2010) hold that risk aversion and impatience may be an important reason for why some people remain poor.

In the present paper, we experimentally elicit time, risk, and ambiguity preferences in a group of Tanzanian rice farmers.[2] These parameters are then used to analyze the choices made by the farmers in terms of whether to process or not process their paddy before selling and

[2] Risk aversion refers to aversion to risky outcomes with known probabilities, while ambiguity aversion relates to the outcomes with unknown distribution. Ambiguity could be relevant because in several aspects uncertainty may involve an outcome with a vague or unknown probability distribution (Vieider et al., 2015; Akay et al.,2012; Sutter et al., 2013)

whether to store paddy for future sale. To our knowledge, no previous study has explored the joint influence of these parameters on farmers' actual post-harvest decisions in developing countries. Instead, previous studies have either investigated the behavior of non-farming economic agents (and mostly in developed countries) or have only studied the role of a single attitude (i.e., either uncertainty or time preference but not both) on farmers' behavior, raising concerns about estimation bias (Sutter et al., 2013). In addition, the studies that have explored the role of one of these attitudes on farmers' decisions have generally focused on pre-harvest decisions, such as technology adoption (e.g., Feder, 1980; Liu, 2012; Liu and Huang, 2013).

Kilombero rice farmers in the Morogoro region of Tanzania – the region focused on in this study – usually plant their rice in January and harvest in June-August. Like most farmers in developing countries, one major post-harvest decision concerns whether to sell paddy or process it and sell a value-added crop, i.e., white rice, and likely earn more. Another choice to make is whether to sell immediately after the harvest or store paddy and likely get a higher price for either paddy or processed rice a couple of months later (Burke, 2014; Bellemare et al., 2013; Saha and Stroud, 1998). Despite the potential price gain from processing or storing – which we show may increase income by up to 48 percent – usually only a small fraction of farmers decide to process or store the harvest for future sales.

Processing paddy implies various costs, but most importantly it implies a risk as the outcome may vary in terms of fractions of high and low quality rice (i.e., the outcome is uncertain). If the outcome is really poor, with a large share of low-quality rice, farmers may in fact incur a loss compared with selling it as paddy. However, a large share of high-quality rice is a more likely outcome, implying a substantial rise in income compared with selling it as paddy. The uncertainty in outcome can largely be attributed to the moisture level of the paddy at the time of processing, as poor farmers cannot objectively assess this property due to lack of access to moisture meters. Similarly, despite the expected price increase a few months after harvest, farmers face uncertainty as prices may also be influenced by unpredictable factors unrelated to local supply and demand (Saha and Stroud, 1998). In addition, storing paddy for future sales implies delayed earnings, and thus impatience may also influence farmers' choices.

We follow the experimental design in Sutter et al. (2013).[3] Their design is simple and neat, making it ideal in a developing country context, where a majority of smallholder farmers have

[3] Akay et al. (2012) use the same design to measure only risk and ambiguity attitudes in Ethiopia. However, they do not estimate time preference parameters or link the uncertainty parameter to any field behavior.

low levels of education. We estimate attitudes to risk and ambiguity based on certainty equivalents, and measure attitudes to delay (impatience) based on the future equivalents of the fixed payoff at the earlier point in time. As a payment vehicle, we utilize a mobile phone banking system that is widespread in Tanzania to minimize the risk and transaction costs associated with the delivery of future/delayed rewards (for time preference experiments) to the respondents. The system is also convenient to use and is well-trusted by respondents.

Our experimental results suggest that, on average, Tanzanian rice farmers are risk averse, slightly ambiguity averse, and impatient. Our results are in line with both Sutter et al. (2013), who elicit these parameters for children and adolescents in Austria, and related studies reviewed in Fredrick et al. (2002). When we link the experimental parameters to field behavior, we find that farmers who store harvest for future sales are generally less risk averse and less impatient, and their households are more likely to be male headed than other households. The choice to process the paddy is more likely among less risk averse, younger, and more educated farmers, as well as among farmers with a shorter distance to the mill and a larger total harvest.

The rest of the paper is organized as follows. Section 2 presents the general and specific design of the experiments, and the empirical strategy is presented in Section 3. Section 4 provides a general overview of the data and descriptive statistics. In Section 5, we present experimental results and discuss the main estimation results, analyzing the impact of experimental behavior on farmers' marketing decisions. Section 6 concludes the paper.

2. DESCRIPTION OF EXPERIMENTS

General design issues

We carried out artefactual field experiments (Harrison and List, 2004), i.e., with non-student subjects, to elicit risk, ambiguity, and time preferences. The experiments were conducted with 337 randomly selected rice farmers from eight villages in the Morogoro region, a large rice-producing area in Tanzania. Each subject completed several experimental games and one survey. We conducted both the experiments and the survey with the head of the sampled households, and all selected farmers agreed to participate in the survey and the experiments.[4]

[4] In total, three subjects did not complete both parts (i.e., the survey and experiments) and were therefore dropped from our observations.

In order to test the survey and the experiments instruments, a pilot study with 30 farmers not used in the final sample was carried out a few weeks in advance. All respondents faced the same decision tasks, instructions, and payoffs. The subjects were informed that they would earn money from the experiments and that the exact amount earned would depend on their choices. Each session including the survey and the experiments lasted approximately 3.5 hours per respondent.

Prior to each experimental session, the author and the local village leader gave a brief introduction of the research team. The participants were informed that the research group came from the University of Dar es Salaam and was in the area purely for research reasons. The subjects were also told that the games they were about to play resembled decisions farmers make in their daily economic lives. Following the introduction, the experimental instructions were clearly spelled out by the author. The subjects were encouraged to ask questions whenever they felt that something was unclear, and the answers to their questions were given in private, but they were not told how to respond in the experiment (Tanaka et al., 2010; Barham et al., 2012). The instructions were followed by a practice game to help subjects understand the basic logic of the games. All subjects were given the exact same instructions and decision tasks.

We controlled for two potential order effects. First, half of the subjects started with the experiments and then answered survey questions, while the other half did the opposite. Second, half of the sample began with the time preference game while the other half began with the uncertainty game. We used the same format for the risk and ambiguity experiments.

All subjects received a show-up fee of TZS 3,000.[5] Also, the subjects were informed that some of their earnings would be paid in the near future (in 2 weeks, 4 weeks, 24 weeks, or 26 weeks). All the future money would be delivered to the respondents through their mobile phone banking accounts on the given date. The average total earnings from the experiments and the show-up fee equaled TZS 10,900 (approximately 7 USD), which is equivalent to, on average, three days of agricultural work at minimum wage in Tanzania.

During the pilot, respondents were asked to choose how they wanted their future payments to be made. All respondents requested that the money be transferred directly to their mobile phone accounts as opposed to receiving it through their village leader or large locally based

[5] 1 USD ≈TZS 1,600 in September, 2013 when the experiments were carried out.

and reputable farming company. One possible explanation for this could be that subjects trusted the researcher (who would later do the payment directly)more than their local leaders. Yet another plausible explanation is that a direct transfer of money to their mobile phone accounts minimizes any transaction inefficiencies associated with the use of a third party. Thus, to ensure trust regarding the future payments in the time preference experiments, all respondents were guaranteed in front of their local leaders that the future money would be sent directly to their mobile phone accounts. Actually, early surveyed farmers who opted for a two-week delay received their money (through the same system) even before we left the field grounds.

Measuring uncertainty and time preferences in experiments

Economists have developed a variety of experimental methods to elicit individual risk, ambiguity, and time preferences. The choice of which one to utilize largely depends on the question one wants to answer and, importantly, the characteristics of the target population (Charness et al., 2013). The multiple price list (MPL) method with monetary rewards is one popular alternative. While appreciated for its simplicity to administer, especially to respondents with low cognitive ability, this method is criticized for overestimating discount rates in time preference experiments (Frederick et al., 2002; Andersen et al., 2008; Andreoni and Sprenger, 2012). This is largely attributed to the experimenters' linear utility assumption, which leads to upward-biased discount rates if the utility function is concave.

One solution to this problem is to jointly estimate subjects' risk and time preferences using the MPL method (Anderson et al., 2008; Sutter et al., 2013) combined with choosing the safest method of delivering the future rewards (as perceived by the respondents). In this study, we follow Sutter et al. (2013) in jointly eliciting risk, ambiguity, and time preferences from the same subject pool. Their designs are simple versions of the standard choice list tasks (i.e., the MPL method), modified to consider the comprehension ability of children and adolescents who are still in primary or middle school. In contrast to the standard MPL method based on Holt and Laury (2002), where subjects compare gambles with changing probability distributions, in this design subjects only compare one (fixed) gamble with monotonically increasing sure amounts, making them relatively easier to follow and understand. This design also worked well with farmers in Ethiopia who were largely comparable to our subjects (Akay et al., 2012).

In this design, subjects were repeatedly asked to choose between a fixed gamble (or a constant immediate payoff in the time preference game) and an increasingly attractive sure (or future) payoff. The point at which a subject switches from preferring the former option to the latter carries information about his or her risk (or intertemporal) preferences.

An alternative solution to the problem would be to use a hybrid approach to estimate both time preference and risk preference using the convex time budget approach by Andreoni and Sprenger (2012). However, this design (originally applied among university students at UC San Diego) seems relatively complex, potentially making it hard to understand and to correctly make choices for subjects with low levels of education (Yang and Carlson, 2012).

Another potential problem with MPL is multiple switching. We enforced monotonic switching by asking the subjects to choose the point in each series at which they wanted to switch from option A to option B (Harrison et al., 2005; Tanaka et al., 2010).

Risk and ambiguity experiments

In each experiment, subjects completed a series of 20 ordered choices between playing a lottery with a 50% chance of winning (risk) and taking a sure amount. If they chose the lottery, they could either win a constant amount of money (TZS 5,000) by betting on the color of a ball to be blindly drawn from a bag (i.e., bag A for the risky prospect and bag B for the ambiguity prospect) or end up empty-handed. The subject could opt for the sure amount of money at any point in the series of choices. The gamble amount TZS 5,000 was kept constant, while the sure amount increased monotonically from TZS 250 to TZS 5,000.

Each respondent made 40 numbered choices, 20 in the risk and 20 in the ambiguity game, and at the end one of the choices was randomly selected to be played with real money (Table B1 in the Appendix presents the choice sets used).

When a lottery was played for real money, the subject was presented with a container holding 40 balls, of which 20 were blue and 20 were orange. In the risk experiment, subjects picked 10 blue balls and 10 orange balls from the container and put them in bag A. The subject saw and counted the chosen balls and therefore knew the color distribution of the balls in bag A. In the ambiguity game, the subject filled bag B with 20 balls from the same container without seeing the chosen colors. Hence, in the ambiguity experiments subjects knew only the total number of balls but not the distribution.[6]

With regard to the payment, all earnings from this session and the show-up fee were paid in private and in cash immediately after the respondent had completed both the experiments and the survey questions.

Calculating the risk/ambiguity parameters

Using a model-free approach, we calculate the *certainty equivalents* from raw switching points to measure the risk and ambiguity attitudes (Sutter et al., 2013). Certainty equivalents for the prospects are calculated as midpoints between the two sure payoffs where the subject switches from choosing the lottery to the sure payoff. Thus, we cannot define the certainty equivalents of subjects who always chose the lottery. At the other end of the spectrum, for subjects who always chose the sure amount, the certainty equivalents were calculated as the midpoint between zero and the sure amount in the first row, i.e., TZS 250.

Denoting the certainty equivalent for risky prospects CE_r and the lottery prize L (TZS 5,000 in our case), we measure the risk attitude θ using the following formula:

$$\theta = 1 - \frac{CE_r}{L}, \text{ where } 0 \leq \theta \leq 1. \qquad (1)$$

$\theta < 0.5$ indicates risk-loving behavior and $\theta > 0.5$ indicates risk-aversion behavior. $\theta = 0.5$ implies risk neutrality.

Denoting the certainty equivalent for an ambiguous prospect CE_a, we calculate the ambiguity aversion parameter Ω as follows:

$$\Omega = \frac{CE_r - CE_a}{CE_r + CE_a}, \text{ where } -1 \leq \Omega \leq 1. \qquad (2)$$

A value of -1 suggests extreme ambiguity loving, while zero and 1 indicate ambiguity neutrality and extreme ambiguity aversion, respectively. The larger the absolute difference between the two certainty equivalents, the stronger the ambiguity attitude.

[6] The chance of winning is still 50%, but this is not obvious to the subjects, hence the label ambiguity.

Time preference experiment

We elicit attitudes to delay (time preference) by letting subjects choose between sure payoffs at two different points in time. We use choice lists where the early payoff remains fixed and the later payoff is increased monotonically. To test for the presence of a time frame effect (i.e., hyperbolic discounting, which implies that the discount rates decrease as a function of time delay) and delay/speed-up asymmetry and magnitude effects (i.e., a situation where larger sums of money suffer from less proportional discounting than smaller ones), subjects were presented with five experiment sets designed to reflect different delay periods (two weeks and six months), different periods of prompt payment (today and two weeks from today), and different magnitudes of sooner rewards (TZS 4,000 and TZS 6,000), respectively (Frederick et al., 2002). To control for the order effect, subjects completed these five choice lists in random order.

Table B2 in the Appendix presents the choice lists used in the experiments. Sheets 1 and 2 are similar in terms of payments and length of delay period (i.e., two weeks); they differ in terms of the prompt payment period, with sheet 2 having an upfront delay of two weeks. The same applies to sheets 3 and 4, albeit with a longer delay period (i.e., six months). Comparisons between parameters estimated from sheets 1 and 2, and between sheets 3 and 4, allow us to test for the present bias/hyperbolic discounting. In addition, sheets 1 and 5 are similar in all respects except the amount of payments, allowing us to test for the stake/magnitude effect.

In total, the subjects made 50 choices in the five randomly presented choice sets. At the end of the experiment, we randomly selected one of the sheets and then randomly chose one choice set from the selected sheet to be played for a real payment.

The main problem with estimating the time preference parameter using real pay-off experiments involves the equivalence between different time points in terms of transaction costs, uncertainty of delivery of the payment, and inflation (Frederick et al., 2002; Andersen et al., 2008; Sutter et al., 2013). We already explained how we minimize the transaction costs and uncertainty issue in the general design section. To minimize the confounding effect of inflation, we used relatively short delay periods (e.g., 2 weeks, 4 weeks, and 24 weeks), since Tanzania has relatively high inflation rates (on average 10 percent 2005–2013). This design also helps us mimic the maximum period between the harvest and selling of the processed paddy if the farmer opts to sell the processed rice and forgoes the earlier money from selling paddy immediately after harvest. In addition, the time frame enabled us to pay most subjects before we left the field, strengthening their trust in any future experiments.

Calculating the time preference parameters

We calculate the future equivalents of the fixed payoff at the earlier point in time as the midpoint between the two later payoffs where the respondent chooses to switch from the earlier to the later payment. Considering for example sheet 1 in Table B2 of the Appendix, if a subject chooses to switch from the sooner amount (i.e., option A) to the later amount (i.e., option B) in row 6, then her future equivalent will be calculated as the midpoint between TZS 4,800 and TZS 5,000 (i.e., TZS 4,900). The larger the future equivalent, the more impatient the individual is. The main limitation of this approach lies in the difficulty of calculating the future equivalents for non-switching respondents.

3. EMPIRICAL STRATEGY

When modeling the studied post-harvest decisions, one important challenge to consider is the large number of zeros in the dependent variable. We argue that the majority of rice farmers sell their rice as unprocessed paddy rather than processed rice (or sell immediately instead of storing for future sale), implying zero value of the dependent variable for a good fraction of our sample. Too many zeros in the dependent variable (i.e., zero-inflated) may cause a series of econometric problems when estimating the effect of the control variables of interest. For example, it violates the normal distribution assumption of the classical regression models (and there is no transformation of the variable that will spread out the zeros and achieve normality), posing econometric challenges to hypothesis testing and prediction as the standard t- and F-tests are no longer valid (Gujarati, 2004).

One traditional approach to model such data has been to use censored or sample selection models such as Tobit and Heckman models (Greene, 2012). These models are based on the latent variable or potential outcome framework, where the observed zeros are the outcome of assigning zero value to any potential outcome that is below or equal to zero. The model therefore implicitly assumes a single underlying distribution of the data. However, these models may not be appropriate in cases like ours where the zeros are actual and observable (i.e., genuine zeros) rather than the outcome of censoring (Madden, 2008; Neelon, 2013).

In the present paper, we employ a two-part model to estimate the impact of uncertainty and time preferences on rice farmers' post-harvest decisions. The two-part model allows us to jointly model the participation and intensity decisions. The motive behind the two-part model is that the participation decision differs from the quantity or intensity decision in a fundamental way (Humphreys, 2013). Under this framework, the data is viewed as arising from two distinct stochastic processes, the first governing the occurrence of zeros and the second determining the observed values given a non-zero response. The model is estimated by using a logit or probit model for the probability of observing a non-zero value of the dependent variable, i.e., the binary part of the data, along with ordinary least squares (OLS) or generalized linear models (GLM) for the sub-sample with positive observations, i.e., the continuous part of the data (Buntin, 2003; Madden, 2008). In contrast to the Tobit and Heckman models, the two-part model is not motivated by the latent variable framework but rather by the conditional mean assumption that:

$$E(Y_i|Y_i > 0, X_i) = X_i\beta. \quad (3)$$

The dependent variable, Y_i, is usually log-transformed before the OLS to address the skewness problem. This results in the Bernoulli log-normal two-part model, given by:

$$f(Y_i) = (1 - \tau_i)1_{(Y_i=0)} + \tau_i LN(Y_i; \mu_i, \sigma^2)1_{(Y_i>0)}, \quad (4)$$

where $\tau_i = \Pr(Y > 0)$ and $LN(Y_i; \mu_i, \sigma^2)$ denote the log-normal density evaluated at Y, and μ and σ^2 denote the mean and variance of $\ln(Y|Y > 0)$, implying that :

$g(\tau_i) = g[\Pr(Y_i > 0)] = X_i'\beta_1$ and

$$\mu_i = E[\ln(Y_i)\,|Y_i > 0] = X_i'\beta_2, i = 1, \dots, n. \quad (5)$$

If the link function (τ_i) meets the assumption for a probit model, the coefficient β_1 measures the change in the log odds of a positive response per one unit change in a given independent variable X_i, controlling for other covariates. Likewise, with the log-normal transformation, β_2 measures the effect of the control variables on the mean $\ln(Y_i)|Y_i > 0$. To calculate the marginal effects, one needs to convert back from the log scale.

We estimate the two-part model, where the dependent variable is either the fraction of the harvest that is sold as processed or the fraction of the harvest that is stored for future sales. In this way, we take care of the differences in absolute amounts that are probably attributed to differences in yields rather than to commitment to the processing/storage decisions. For farmers who sell everything as processed, the value of this fraction will be equal to one, while for those who process only part of the harvest, it will be less than one. The same applies to the storage variable. We then transform the variable into a logarithm, after adding 1 (i.e., log of Y+1), recognizing the presence of zeros in our variables.

We control for both uncertainty and time preference variables, jointly estimated from the artefactual field experiments. By doing this, we remove the confounding effect of the risk preference behavior on the estimated time preferences. We also control for other important variables, including social networks, transaction costs, access to storage facilities, general trust in others, total harvests, whether the household has adopted a new yield-enhancing rice-farming technology introduced in the area (known as system of rice intensification, or SRI), access to the milling/processing machines, and social and economic characteristics.

4. DATA AND DESCRIPTIVE STATISTICS

We conducted both the survey and experiments with a total of 337 randomly selected heads of rice-farming household. The subjects were from eight villages in four different wards of the Morogoro region, which is one of the largest rice-producing regions in Tanzania. However, at the time of the survey and the experiments, 69 farmers were at the end of the harvesting process and had not yet made their selling decisions. Since we cannot say anything a priori about their marketing choices by the time they harvest, we decided to drop this sub-sample from our analysis.[7]

Table 1 provides the descriptive statistics of all key variables for the remaining part of the sample. We find that rice farmers in the area are on average risk averse (with a risk aversion measure of 0.56), ambiguity neutral (0.0), and slightly impatient (with the average future

[7] One potential problem resulting from this decision is sample selection bias, if the dropped households differ systematically from the retained sub-sample. However, we find no systematic differences in either the experimental variables or the control variables between the two groups (see Table A1 in the Appendix).

equivalent of TZS 4,740 being statistically larger than the sooner reward of TZS 4,000). As for the social and economic variables, the data shows that on average 91 percent of the sampled households are male headed. The household head is on average 42.2 years old and has 7.1 years of schooling,[8] and the average household consists of approximately five individuals. In addition, the average household spends TZS 4,405 on daily basic needs (e.g., food, fuels, water, and transportation) per day. We find that 61 percent of our sample had adopted a new rice-farming technology commonly known as SRI and that the area is dominated by Christians, with only 14 percent of our sample being Muslims.

Looking at the outcome variables, as expected, only 24 percent of our sample reported to process some rice before selling it. The remaining share reported selling everything as paddy. Of those who never processed the harvest, 45 percent stated outcome uncertainty as the main reason for their choice and 17 percent stated immediate need for cash as major reason. Similarly, despite the claims that storing and selling the yield a few months after harvest generates higher income (e.g., Burke, 2014; Bellemare et al., 2013; Saha and Stroud, 1998), only 35 percent of our sample chose to do so. We also asked whether the subjects usually preserved the stored grains (e.g., by using pesticides and rat traps), and only 33 percent responded in the affirmative.

The average total harvest in the survey agricultural year was 4.51 tons per household. Selling prices vary for both paddy and processed rice, but on average paddy earned TZS 313/kg at the time of the survey. The corresponding figures for processed high- and low-quality rice after adjusting for weight losses and processing costs were TZS 432/kg and TZS 253/kg, respectively. Processing also implies transportation costs and delayed income. Given a high share of high-quality rice, processing implies increased income. However, processing may also imply a risk, and in case processing results in a high share of low-quality rice, farmers may even incur a loss compared with selling their harvest as paddy.

[8] Complete primary schooling in Tanzania takes 7 years.

Table 1: Descriptive statistics of key variables by rice marketing category

Variable	Mean	SD
Risk aversion [0,1], 0.5=risk neutral	0.56	0.271
Ambiguity aversion [-1,1], 0=ambiguity neutral	0.00	0.296
Impatience-future equivalent ('000 TZS)	4.74	0.425
Age	42.15	11.691
Male dummy (1=yes)	0.91	0.285
Household size	4.57	1.883
Years of schooling	7.13	2.024
Household daily expenditure on basic needs ('000 TZS)	4.39	2.025
Asset wealth ('000 TZS)	634.79	1511.225
Whether household uses SRI technology	0.61	0.490
Muslim religion dummy (1=yes)	0.14	0.352
Whether one generally trusts other people (1=yes)	0.07	0.262
Whether household stores some rice for future sale (1=yes)	0.35	0.477
Whether household processes some rice before selling (1=yes)	0.24	0.426
Whether household preserves (e.g., spraying, rat traps) once stored (1=Yes)	0.33	0.471
Whether rice quality uncertainty is the reason for not processing before selling (1=Yes)*	0.45	0.499
Whether immediate need for money is the reason for not processing (1=Yes)*	0.17	0.393
Whether processing cost is the reason for not processing (1=Yes)*	0.26	0.446
Total harvest (tones)	4.51	4.500
Cost of milling (TZS per kg)	44.41	16.697
Price_high quality rice (TZS per kg)	706.22	185.982
Price_low quality rice (TZS per kg)	452.35	125.638
price_paddy (TZS Per kg)	313.47	104.956
Price difference (high versus low quality rice)	253.87	163.726
Distance to the milling machine (km)	1.59	2.717
Social network (number of social groups connected to)	1.87	1.231
Observations	270	

*Share of only those who did not process before selling (206 observations)

5. MAIN RESULTS

5.1. Determinants of Experimental Behavior

Risk and ambiguity behaviors

On average, we find that rice farmers are risk averse with risk aversion parameters of 0.562 but approximately ambiguity neutral with a parameter of 0.009. See Table 2 for the correlates of the risk and ambiguity preference behaviors.

Table 2: OLS regression analysis for risk and ambiguity attitudes

VARIABLES	(1) risk_5000	(2) ambiguity
Male dummy	-0.138**	-0.024
	(0.063)	(0.055)
Years of schooling of the respondent	0.004	0.010
	(0.008)	(0.007)
Whether respondent is Muslim	0.016	0.006
	(0.049)	(0.033)
Number of children in respondent's family	-0.001	0.005
	(0.005)	(0.005)
Age of respondent	0.001	0.001
	(0.001)	(0.001)
Household size	-0.004	0.014
	(0.010)	(0.011)
Mean daily household expenditure on food (in Log)	0.005	-0.023
	(0.037)	(0.040)
Wealth (measured as log of total asset value)	0.009	-0.032*
	(0.014)	(0.018)
Order effect: experiments preceded survey	0.032	-0.080*
	(0.040)	(0.041)
Order effect: uncertainty games preceded time preference games	0.007	0.011
	(0.046)	(0.048)
Constant	0.487	0.435
	(0.329)	(0.371)
Village fixed effects	YES	YES
Observations	270	270
R-squared	0.069	0.070

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The regression results show that males are relatively less risk averse than females, yet the difference is not different from zero when it comes to ambiguity behavior. The gender difference in risk preferences is consistent with previous findings (Sutter et al., 2013; Yesuf and Bluffstone, 2009). We also find that, while household wealth is uncorrelated with risk aversion, it is negatively correlated with the ambiguity measure at a 10 percent level of significance. The wealthier farmers are relatively less ambiguity averse, other factors held constant.

Given that a large share of households who never process reported fear/risk of ending up with a bad outcome (i.e., broken rice) as the major reason for their choice, an opportunity exists to investigate whether such stated risk aversion is consistent with the revealed behavior from the experiment. Table 3 below suggests a strong positive correlation between stated responses on fear of bad outcome and experimental risk aversion. Households that are more risk averse are the most likely to report the uncertainty factor as their main reason for selling everything as paddy. The correlation gets stronger if we only consider a sub-sample of households who never process before selling (column 1 versus column 2). The results are robust even after controlling for other covariates of risk preferences (column 3). These results strengthen the internal validity of our experimental design and are consistent with Vieder et al. (2015). However, we do not find any correlation between experimental risk aversion and stated need for immediate money (delay aversion).

Table 3: Correlation between experimental risk aversion and stated reason for non-processing

VARIABLES	(1) Experimental risk (whole sample)	(2) Experimental risk (non-processing hhlds only)	(3) Experimental risk (non-processing hhld only with other controls)
Reported uncertainty aversion	0.288***	0.390***	0.401***
	(0.029)	(0.030)	(0.030)
Reported delay aversion	-0.036	0.039	0.048
	(0.044)	(0.046)	(0.046)
Male			-0.042
			(0.046)
education			0.014**
			(0.007)
muslims			0.102***
			(0.034)
siblings			-0.003
			(0.004)
age			0.002
			(0.001)
hhsize			0.013
			(0.009)
Constant	0.457***	0.382***	0.188**
	(0.021)	(0.024)	(0.094)
Observations	270	206	206
R-squared	0.277	0.487	0.521

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Impatience behavior

Future equivalents and the implied mean annual discount rates capturing impatience behavior of farmers in rural Tanzania are presented in Table 4 below. The results show that future equivalents are statistically higher than the early payment for a majority of our sample, suggesting that these farmers are on average impatient. A Wilcoxon signed-rank test rejects the null hypothesis that the distribution for future equivalents and earlier payment is the same at the 1 percent level. This is consistent regardless of delay period, with or without the presence of delay, and stake size. We also calculate the implicit discount rate using the future equivalents and obtain significantly high mean and median discount rates; see Panel B of Table 4. Consistent with previous literature (e.g., Sutter et al., 2013), we find that discount rates are considerably higher with shorter delays (two weeks; columns 1 and 2 in the table) compared with longer delays (24 weeks; columns 3 and 4 in the table). We also find weak evidence of present bias with short delay periods (columns 1 and 2 in the table), which totally disappears with longer delays (columns 3 and 4 in the table).

Table 4 Future equivalents and mean annual discount rates (%)[9]

	Delay				
	2 weeks	2 weeks with upfront delay	24 weeks	24 weeks with upfront delay	24 weeks, TZS 6,000
A: Future equivalent (TZS)	4740	4783	4790	4824	6871
B: Annual discount rates (%)	431	454	38	40	345

One challenge we encountered in the time preference experiments is that of non-switching respondents, which makes it difficult to estimate their future equivalent. In a well-functioning capital market, the non-switching could suggest that laboratory interest rates are relatively lower than outside market rates, such that it is profitable for rational individuals to take the money sooner and lend it to the outside market (Cubit and Read, 2007; Coller and Williams, 1999). However, this seems unlikely as capital markets hardly exist in the setting of the study. Another possible explanation could be that due to generally low levels of education in our setting, some of the non-switchers probably did not understand the game instructions. We show in Table A2 in the Appendix that only the education variable correlates with the probability of non-switching. Subjects with more education are more likely to have switched than those with less education. This is robust regardless of the length of the delay period, the stake effect, and whether there is an upfront delay.

In such non-switching behavior, some researchers attempt to recover consistent preferences by using some imputation methods, while others drop such subjects as most of such

[9] Using continuous discounting, the discount rates are calculated based on the following formula:
$r = \ln\left(future\,\frac{equivalent}{early\ payoff}\right) * (\frac{52}{number\ of\ week\ for\ the\ delay})$.

inconsistencies are due to either mistakes or misunderstanding of the instructions (Sutter et al., 2013). We follow the latter group, agreeing that no reliably consistent preferences can be recovered from such choice lists. We therefore opt to drop the 44 subjects who did not switch at all in the subsequent sections of the analysis.

We then estimate the correlates of the impatient behaviors and present the results in Table 5. We find that risk aversion is negatively (and statistically significantly) correlated with the future equivalents values. This relationship is robust to all forms of the design (i.e., fe1-fe5), controlling for length of delay period, with and without the presence of upfront delay, and stake size. This affirms the argument in the literature that time preference parameters are likely to be confounded by risk preference behavior, supporting the need to control for both variables in the model. We also find that the future equivalent values are indeed affected by whether uncertainty or time preference experiments are administered first (i.e., order effect). More exactly, subjects who played the uncertainty games first were more likely to demonstrate a higher level of impatience (i.e., higher future equivalents) in the time preference games, and vice versa. However, just as in the uncertainty experiments, the survey vs. experiment order does not matter statistically.

Table 5: OLS regression analysis for time preference attitudes (future equivalents)

VARIABLES	(1) fe1	(2) fe2	(3) fe3	(4) fe4	(5) fe5
risk_5000	-478.965***	-495.749***	-532.287***	-512.093***	-548.910***
	(141.121)	(141.287)	(143.790)	(148.128)	(163.881)
Ambiguity	-15.439	-70.531	25.421	45.926	65.237
	(101.701)	(112.178)	(106.688)	(115.404)	(135.722)
Expenditure	0.012	0.008	0.006	0.020	-0.004
	(0.014)	(0.014)	(0.014)	(0.015)	(0.017)
Hhsize	-26.093	-22.892	-8.135	-20.369	-22.831
	(16.401)	(17.074)	(17.643)	(18.373)	(19.631)
Age	-1.468	-3.222	-1.802	0.046	-2.817
	(2.601)	(2.937)	(2.875)	(2.925)	(2.790)
Male	197.591**	123.473	74.279	134.642	175.661
	(93.438)	(84.132)	(111.189)	(104.666)	(109.625)
Education	-0.750	-13.847	17.217	-10.016	2.112
	(13.103)	(15.213)	(18.893)	(17.472)	(15.685)
Muslims	-6.062	95.260	55.384	91.179	111.164
	(73.373)	(81.539)	(85.860)	(93.698)	(101.481)
Siblings	3.610	2.799	-2.291	-12.310	-1.512
	(9.567)	(8.362)	(8.703)	(10.488)	(9.420)
Trust	2.263	-15.112	-26.205	-38.431	-25.057
	(29.914)	(34.087)	(34.506)	(34.198)	(36.615)
Lwealth	10.243	12.690	-10.199	0.670	19.212
	(21.513)	(28.798)	(25.620)	(27.012)	(32.543)
Order effect: experiments preceded the survey	-14.179	29.550	-59.392	-160.570**	-49.872
	(60.489)	(65.545)	(68.667)	(65.440)	(73.328)
Order effect: uncertainty preceded the time preference experiments	103.285*	139.972**	82.479	128.351*	154.309**
	(54.408)	(60.159)	(59.628)	(65.395)	(66.029)
Constant	4,758.986***	5,008.080***	5,154.589***	5,189.448***	6,980.936***
	(338.010)	(387.352)	(355.265)	(350.741)	(454.863)
Observations	226	219	196	195	226
R-squared	0.148	0.145	0.140	0.176	0.153

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5.2. The role of risk and time preferences in rice farmers' decisions

Table 6 below presents the estimation results from the two-part model. Panel A shows the estimation results for the processing decision and Panel B the result for the storage decision. The first column of each panel presents the binary probit results for the participation decision (i.e., the choice to process/store some fraction of the crop) and the second column shows the effect of the variables on the log of the actual fraction sold as processed (fraction stored), given a decision to process (to store) (i.e., the OLS results).

The regression results in Panel A, column 1 show that risk preference behavior indeed has a significant influence on farmers' decision about which rice product to sell. The coefficient of the risk aversion measure is negative and statistically significant at the 5 percent level. Consistent with our hypothesis, more risk-averse farmers are less likely than their counterparts to sell processed rice. However, we find that neither ambiguity nor the future equivalent (time preference) affects the decision regarding which rice product to sell. We note that the main explanation for the time preference results could be the very short time farmers take to dry their paddy before processing and the low level of waiting in line at the mill, something that we learned later in the project. On average, farmers use only 0.6 days to sun-dry the paddy before it is ready for processing and 97 percent of the respondents have reasonably close access to the milling machines, saving them time by not having to wait in line. We did not foresee such results a priori, but it should be kept in mind that they may differ in a setting with different solar intensity during the harvest and less access to milling machines.

We also find that all other social and economic characteristics have the expected signs of influence on our dependent variable. For example, younger farmers and relatively more educated ones are more likely to sell processed rice than their counterparts. This is plausible as education enables people to assess uncertainties and make more informed decisions. Households' economic status (measured in wealth) and total amount of rice harvested have a positive effect on the decision to process the paddy before selling. We also find that increasing distance from the homestead to the nearest milling machine (a proxy measure of transaction cost) reduces the probability of selling processed paddy, other factors held constant.

The second column in Panel A presents the results for the second part of the model, the determinants of the actual fraction of the harvest sold as processed (converted to natural logarithms), given a decision to process. Although we did not anticipate these results a priori, we find that none of our main variables influence the processing intensity. For example, the risk measures do not influence intensity, which implies that when a farmer has decided to process the harvest, the magnitude of his or her risk aversion does not affect the amount to be processed. These findings suggest that the gains from selling processed rice are fundamentally threatened by the risk of getting a bad outcome from processing, but once the farmer is optimistic about the outcome (i.e., a risk lover in this setting), he or she processes everything set aside for selling, which makes risk preferences not influence the second stage of the decision. There could be several reasons for such intensity choices. For example, once the farmer has decided to process, it could be of interest to process everything and sell at once to minimize the transaction costs associated with crop marketing (e.g., convenience, transport costs, and processing). This is even more relevant in rural settings where bulk buyers (i.e., the middle men) show up at the village market only a few times a month and also considering that these farmers usually do not have so much to sell. Figure 1A in the Appendix shows that nearly 70 percent of those who chose to sell part of their crop as processed actually processed 100 percent of their sales. Consequently, we find that none of the other independent variables (except trust) are statistically significant in explaining the intensity decision.

These findings have important implications. Addressing the causes of outcome uncertainty due to processing (i.e. determinant of good or bad quality rice) could make farmers to choose a more profitable option, i.e., to sell processed rice rather than paddy. Potential source of such uncertainty is documented to be mainly the wrong level of moisture content in the paddy at the time of processing. Not knowing about and/or not having access to a moisture meter (a modern device for testing moisture levels in cereals) makes the decision to process paddy more of a gamble. Increasing farmers' access to and training on the meters could change their behavior and hence their welfare, all else being equal.

If rice farmers can sell their crop as processed and manage to get good quality rice with certainty, they can increase their net income by 48 percent for each kilogram sold. While 100 kg of paddy would give a farmer an average of TZS 31,300, the 71.4 kg of rice from the same bag of paddy would earn him or her an average of TZS 50,408, resulting in a gross gain of TZS 19,108. Accounting for the average milling cost of TZS 4,441 for the same bag leaves the farmer with a net gain of TZS 14,667 per 100 kg of paddy sold. This gain is more than what the same farming households need to spend on their daily basic needs for three days, implying that, by failing to process one ton of harvest before sale, a household loses what it would have spent on an entire month of household needs. This suggests that policies facilitating processing prior to selling would significantly improve the welfare of rice farmers.

Moving to Panel B of Table 6, we find that both risk and time preferences explain the farmers' decision to store crops for future sales. More risk-averse and impatient farmers are more likely to sell their harvest right away instead of storing it for expected future higher income. The risk results support the existing body of literature that has documented the erratic nature of intertemporal price trends in developing countries (Burke, 2014; Bellemare et al., 2013; Saha and Stroud, 1998). The revenue gain from the intertemporal arbitrage is uncertain and influenced by several factors, including the price of substitutes, availability of imports, and expectations regarding future harvests. For example, a year before our study, the Tanzanian government relaxed the import quotas for rice, which resulted in a significant fall in the prices of local varieties at a time when farmers expected an increase (see Figure 2A in the Appendix).[10] Farmers' responses to such uncertainties tend to differ, and we provide evidence that risk-averse farmers are likely to be more cautious. When it comes to the storage decision, patient farmers accept the cash delay associated with storing at harvest to a larger extent in exchange for higher expected income, which is reflected by the time preference results.

In addition, we find that while male-headed and SRI-adopting households are more likely than their counterparts to store for arbitrage reasons, the opposite is true for larger families and households that incur preservation costs. Not all farmers usually incur extra preservation costs given a storage decision. We find that those who reported to have incurred such costs at least in the previous farming season were less likely to store during the current season, ceteris paribus.

[10] Rice harvesting usually begins June-September. Prices are expected to gradually increase from, say, November to August before a new harvest period begins. However, a reverse trend was observed in the country due to large imports of cheap foreign rice varieties, confirming the uncertainty nature of the expected gains from storing harvest.

Table 6: Determinants of paddy processing and storage (two-part regression results)

	Panel A: Processing		Panel B: Storage for sale	
	(1)	(2)	(3)	(4)
VARIABLES	probit	regress	probit	regress
Risk aversion	-0.0832**	0.0429	-0.180***	0.00561
	(0.0424)	(0.0272)	(0.0588)	(0.00628)
ambiguity aversion	-0.197	0.201	-0.265	-0.0161
	(0.346)	(0.152)	(0.466)	(0.0477)
Impatience (future equivalents)	-0.000550	-0.00565	-0.119***	0.000427
	(0.0197)	(0.00931)	(0.0296)	(0.00396)
Milling cost (TZS per kg of paddy)	0.00929	0.00323	0.0131	-0.00149*
	(0.00680)	(0.00384)	(0.0122)	(0.000891)
Distance to the milling machine (log of km)	-0.413**	0.217	-0.134	0.00716
	(0.203)	(0.145)	(0.226)	(0.0221)
Total harvest (log)	0.400**	-0.0604	-0.187	-0.0133
	(0.175)	(0.0508)	(0.223)	(0.0267)
Male dummy	-0.151	0.131	1.025**	-0.0656
	(0.439)	(0.227)	(0.467)	(0.0547)
Age (years)	-0.0342***	-0.00253	0.0215	0.00118
	(0.0106)	(0.00397)	(0.0161)	(0.00126)
Education (years of schooling)	0.213***	-0.0235	-0.0582	0.00376
	(0.0605)	(0.0312)	(0.0605)	(0.00735)
Household size	0.0847	-0.0311	-0.172**	0.00400
	(0.0601)	(0.0388)	(0.0852)	(0.00911)
Asset wealth (log)	0.116	0.0393	0.0931	0.0222*
	(0.0871)	(0.0367)	(0.122)	(0.0116)
SRI adopters (dummy)	-0.557**	-0.0130	0.540*	-0.0485
	(0.231)	(0.0783)	(0.298)	(0.0331)
Social network (number of social groups)	0.598	-0.00576	-0.434	0.0372
	(0.430)	(0.137)	(0.544)	(0.0599)
General trust in others (dummy)	0.281	-0.335**	-0.530	0.00126
	(0.372)	(0.155)	(0.599)	(0.0681)
Mean daily expenditure (log)	0.127	0.0900	0.105	0.0188
	(0.214)	(0.0897)	(0.295)	(0.0320)
Log distance to village warehouse (km)	0.0652	-0.0416	0.127	0.0149
	(0.0894)	(0.0353)	(0.143)	(0.0120)
Satisfied with inhouse storage_dummy			-0.211	0.0338
			(0.300)	(0.0334)
Whether preservation during storage (dummy)			-0.714**	-0.00783
			(0.353)	(0.0339)
Village_dummies	YES	YES	YES	YES
Order effects	YES	YES	YES	YES
Constant	-6.754**	0.0951	10.17***	-0.0203
	(2.855)	(1.101)	(3.566)	(0.378)
Pseudo R-2	0.2087		0.3105	
Observations	226	226	226	226

Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

6. CONCLUSION

In this paper we carry out artefactual field experiments with rice farmers in the Morogoro region in Tanzania to elicit ambiguity aversion, risk aversion, and time preferences. We use the design of Sutter et al. (2013) and our results are in line with previous experiments, indicating that the design is useful also in an environment with subjects having limited education. Our experimental measures suggest that rice farmers are on average slightly impatient and risk and ambiguity averse. We linked the estimated parameters to the real world decisions of the rice farmers to process/not process some of their paddy and to store/not store some of their harvest in order to obtain an expected higher price in the future. The contributions of the paper are therefore in jointly controlling for risk aversion, ambiguity aversion, and time preference parameters to model actual post-harvest decisions of farmers concerning paddy processing and storing of yield. These important decisions of smallholder farmers have not received sufficient attention in previous studies.

Morogoro rice farmers seem to be involved in suboptimal marketing behavior to a large extent, as is common among poor farmers in developing countries. A large fraction of our sample sell their harvest as unprocessed paddy, despite the fact that their income from processing and selling it as rice could be expected to be substantially higher. Similarly, many farmers refrain from storing paddy for later sale at an expected higher price.

When we link the experimental parameters to field behavior, we find that storing harvest for future sales is more likely among those who are more risk neutral and more patient, as well as among male-headed households. As for the processing decision, we show that farmers who are more risk neutral, younger, and more educated are more likely to process their paddy. Shorter distance to the milling machines and larger total harvest also imply an increased likelihood of processing harvest, and larger households are less likely to store rice for future sales. Most notable for our sample is the high predictive power of our experimentally elicited risk preferences for field behavior, with regard to both processing and storing. Previous studies have often found low predictive power of risk preferences (e.g., Dohmen et al., 2011; Sutter et al., 2013). Although we experienced some problems when assessing time preferences, our impatience parameter is significant at the 1 percent level of significance in explaining farmers’ choice to store paddy for future sale.

Overall, our results support the claim by Tanaka et al. (2010) that risk aversion and impatience partly explain why some people remain poor. The rice farmers in Kilombero, Morogoro, would certainly benefit from better credit facilities and insurance possibilities. Similarly, improved infrastructure in terms of better roads and increased access to motorized vehicles would extend their potential market range.

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APPENDIX

Table A1: Probit estimation results: Determinants of the rice selling status at the time of the survey

VARIABLES	sold
Risk (experimental measure of risk aversion)	-0.533
	(0.482)
Ambiguity (experimental measure of ambiguity aversion)	-0.00420
	(0.299)
Future equivalent	-6.11e-05
	(0.000223)
cost_milling (Rice milling cost per kg in TZS)	0.00122
	(0.00645)
Totalharvest (Total rice harvested by the household from all its plots)	4.29e-05
	(3.76e-05)
Usesri (Dummy variable for whether household has adopted SRI technology)	0.206
	(0.208)
Age (Age of the head of household in years)	-0.0122
	(0.00876)
Education (Years of schooling of household head)	0.105**
	(0.0458)
Wealth (measured as log of asset values)	-0.0888
	(0.0980)
price_paddy	-0.000461
	(0.000795)
Male dummy	0.284
	(0.324)
Social network dummy (total number of social and farming groups the household is connected to)	0.102
	(0.0897)
Village fixed effects	YES
Constant	1.333
	(1.608)
Pseudo R2	0.1377
Observations	271

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2: Probit estimation results: Determinants of the non-switching behavior in the time preference experiments

	(fe1)	(fe2)	(fe3)	(fe4)	(fe5)
VARIABLES	**switch1**	**switch2**	**switch3**	**switch4**	**switch5**
Years of education	0.150***	0.189***	0.139***	0.0905**	0.151***
	(0.0489)	(0.0546)	(0.0482)	(0.0459)	(0.0505)
Age of the respondent	-0.00845	-0.00244	-0.00684	-0.000711	-0.0113
	(0.00881)	(0.00814)	(0.00746)	(0.00742)	(0.00817)
Risk aversion	-0.176	-0.866**	-0.180	0.121	-0.270
	(0.412)	(0.401)	(0.353)	(0.351)	(0.405)
Ambiguity aversion	0.0351	0.383	-0.142	0.117	0.276
	(0.356)	(0.357)	(0.329)	(0.327)	(0.345)
Male dummy	-0.181	-0.674	-0.750*	-0.383	-0.578
	(0.379)	(0.446)	(0.385)	(0.323)	(0.437)
Daily expenditure	-4.63e-05	-3.79e-05	2.91e-06	3.52e-05	4.42e-05
	(4.73e-05)	(4.73e-05)	(4.48e-05)	(4.59e-05)	(4.75e-05)
Asset wealth	0.102	0.193**	0.0299	-0.00927	0.0201
	(0.0798)	(0.0852)	(0.0698)	(0.0753)	(0.0768)
Degree of trust in others	-0.0316	-0.122	-0.0690	-0.0721	-0.125
	(0.113)	(0.110)	(0.0952)	(0.0982)	(0.111)
Household size	0.158***	0.106*	0.120**	0.139***	0.117**
	(0.0609)	(0.0566)	(0.0490)	(0.0483)	(0.0547)
Muslim dummy	0.00658	-0.268	0.0693	0.169	0.0399
	(0.261)	(0.246)	(0.244)	(0.249)	(0.270)
Constant	-1.116	-1.558	-0.0680	-0.240	0.432
	(1.118)	(1.135)	(0.952)	(1.007)	(1.084)
Observations	270	270	270	270	270

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B1: Risk/ambiguity aversion decision sheet

A: Risk prospects

In each of the choice sets numbered 1-20 below, please indicate whether you would like to take a sure but smaller amount or gamble and draw a ball from a container (i.e., a lottery). The container holds 20 balls, of which ten are white and ten are red. If you get a white ball you win TZS 5,000. Please complete each row numbered 1-20 by marking one of the two boxes.

B: Ambiguity prospects

In each of the choice sets numbered 1-20 below, please indicate whether you would like to take a sure but smaller amount or gamble and draw a ball from a container (i.e., a lottery). The container holds 20 balls, of which some are white and some are red. You pick a color, and if you get that color you win TZS 5,000. Please complete each row numbered 1-20 by marking one of the two boxes.

	Gamble			Sure reward in TZS	
1.	Draw from container A	☐	Or	Get 250	☐
2.	Draw from container A	☐	Or	Get 500	☐
3	Draw from container A	☐	Or	Get 750	☐
4	Draw from container A	☐	Or	Get 1,000	☐
5	Draw from container A	☐	Or	Get 1,250	☐
6	Draw from container A	☐	Or	Get 1,500	☐
7	Draw from container A	☐	Or	Get 1,750	☐
8	Draw from container A	☐	Or	Get 2,000	☐
9	Draw from container A	☐	Or	Get 2,250	☐
10	Draw from container A	☐	Or	Get 2,500	☐
11.	Draw from container A	☐	Or	Get 2,750	☐
12.	Draw from container A	☐	Or	Get 3,000	☐
13	Draw from container A	☐	Or	Get 3,250	☐
14	Draw from container A	☐	Or	Get 3,500	☐
15	Draw from container A	☐	Or	Get 3,750	☐
16	Draw from container A	☐	Or	Get 4,000	☐
17	Draw from container A	☐	Or	Get 4,250	☐
18	Draw from container A	☐	Or	Get 4,500	☐
19	Draw from container A	☐	Or	Get 4,750	☐
20	Draw from container A	☐	Or	Get 5,000	☐

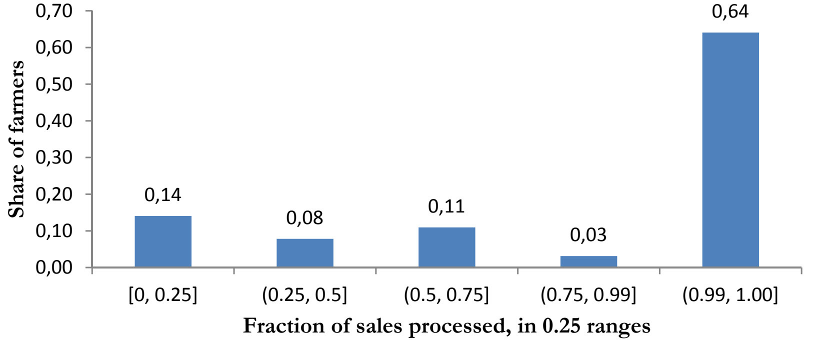
Table B2: Time preference decision sheets

In each of the choice sets numbered 1-10 below, please indicate whether you would like the smaller sure payment **today** (or two weeks from today) or the larger payment for sure in two weeks (or 4 weeks/six months) from today. Please complete each row numbered 1-10 by marking the preferred option.

S/N	Reference period	Future period	Current amount (A)	Future amount(B)	Mark a preferred option A or B
Sheet 1					
1	Today	2 weeks	4,000	4,000	A or B
2	Today	2 weeks	4,000	4,200	A or B
3	Today	2 weeks	4,000	4,400	A or B
4	Today	2 weeks	4,000	4,600	A or B
5	Today	2 weeks	4,000	4,800	A or B
6	Today	2 weeks	4,000	5,000	A or B
7	Today	2 weeks	4,000	5,200	A or B
8	Today	2 weeks	4,000	5,400	A or B
9	Today	2 weeks	4,000	5,600	A or B
10	Today	2 weeks	4,000	6,000	A or B
Sheet 2					
1	2 weeks	4 weeks	4,000	4,000	A or B
2	2 weeks	4 weeks	4,000	4,200	A or B
3	2 weeks	4 weeks	4,000	4,400	A or B
4	2 weeks	4 weeks	4,000	4,600	A or B
5	2 weeks	4 weeks	4,000	4,800	A or B
6	2 weeks	4 weeks	4,000	5,000	A or B
7	2 weeks	4 weeks	4,000	5,200	A or B
8	2 weeks	4 weeks	4,000	5,400	A or B
9	2 weeks	4 weeks	4,000	5,600	A or B
10	2 weeks	4 weeks	4,000	6,000	A or B
Sheet 3					
1	Today	24 weeks	**4,000**	4,000	A or B
2	Today	24 weeks	4,000	4,200	A or B
3	Today	24 weeks	4,000	4,400	A or B
4	Today	24 weeks	4,000	4,600	A or B
5	Today	24 weeks	4,000	4,800	A or B
6	Today	24 weeks	4,000	5,000	A or B
7	Today	24 weeks	4,000	5,200	A or B
8	Today	24 weeks	4,000	5,400	A or B
9	Today	24 weeks	4,000	5,600	A or B
10	Today	24 weeks	4,000	6,000	A or B
	Sheet 4				
1	2 weeks	26 weeks	**4,000**	4,000	A or B
2	2 weeks	26 weeks	4,000	4,200	A or B
3	2 weeks	26 weeks	4,000	4,400	A or B

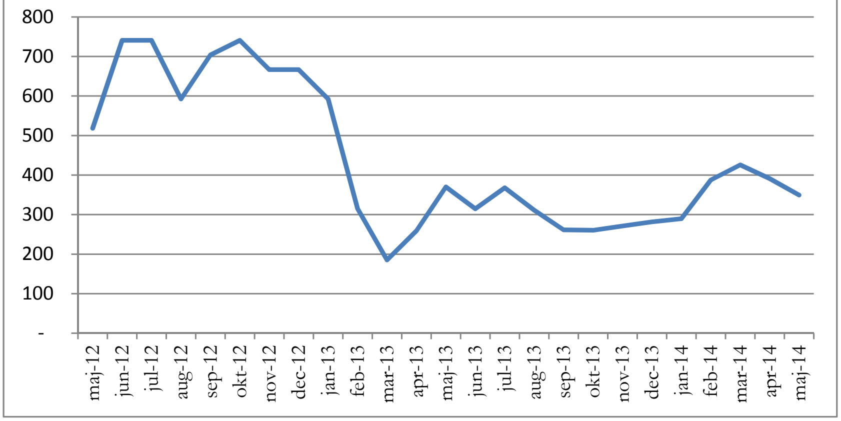
4	2 weeks	26 weeks	4,000	4,600	A or B
5	2 weeks	26 weeks	4,000	4,800	A or B
6	2 weeks	26 weeks	4,000	5,000	A or B
7	2 weeks	26 weeks	4,000	5,200	A or B
8	2 weeks	26 weeks	4,000	5,400	A or B
9	2 weeks	26 weeks	4,000	5,600	A or B
10	2 weeks	26 weeks	4,000	6,000	A or B
Sheet 5					
1	Today	2 weeks	6,000	6,000	A or B
2	Today	2 weeks	6,000	6,250	A or B
3	Today	2 weeks	6,000	6,500	A or B
4	Today	2 weeks	6,000	6,750	A or B
5	Today	2 weeks	6,000	7,000	A or B
6	Today	2 weeks	6,000	7,250	A or B
7	Today	2 weeks	6,000	7,500	A or B
8	Today	2 weeks	6,000	7,750	A or B
9	Today	2 weeks	6,000	8,000	A or B
10	Today	2 weeks	6,000	8,250	A or B

Figure 1A: Distribution of farmers who sold processed rice by fraction of sales processed



Source: Author's own calculations

Figure 2A: Paddy monthly prices in 2012-2013(TZS/kg)



Source: KPL, 2013

Picture 1: Demonstrating drawing 20 balls for a risk lottery during the pilot experimental session

Picture2: An example of an experimental session

Paper 3

Credit, LPG Stove Adoption and Charcoal Consumption: Evidence from a Randomised Controlled Trial*

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Abstract

The high start-up cost of modern cooking appliances has been shown to be the key factor that hinders transition of households from biomass energy to clean energy in developing countries. We designed a randomised controlled trial to identify the impact of relaxing households' liquidity constraints on LPG stove adoption and charcoal use in urban Tanzania. In collaboration with a local micro-finance institution, we randomly assigned households into a subsidy treatment and a credit treatment, which included different repayment arrangements. We show that, relative to households in the control group, adoption of LPG stoves reduced charcoal use by 47.5% in the treated group. However, providing subsidies for stove purchases resulted in a much larger reduction in charcoal use (54%) than did providing access to credit (41%). We highlight the importance of relaxing households' financial constraints and improving access to credit to encourage urban households to switch to clean energy sources and save the remaining forest resources of Africa.

JEL Classification: G21, G31, O10, O13, Q23, Q51.

Keywords: Charcoal, LPG Stoves, Deforestation, Liquidity Constraint, Credit

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1 Introduction

Charcoal is the main source of cooking energy for households in urban areas of many Sub-Saharan (SSA) countries (Campbell et al., 2007; Mercer et al., 2011). In the urban parts of Tanzania - the country on which we focus in this paper - the proportion of households that use charcoal to meet their main cooking needs increased from 47 percent in 2001 to 71 percent in 2007, and Dar es Salaam city alone consumes 500,000 tonnes of charcoal, half of the total annual charcoal consumption of the country (World Bank, 2009). On the other hand, many SSA countries have been experiencing economic growth which resulted in increased income and living standard in urban areas (AfDB, 2014). The fact that charcoal consumption has been increasing with increasing income is contrary to the predictions of the "energy ladder hypothesis", which has been the key theory in explaining energy transition in developing and emerging countries (Heltberg, 2005; Masera et al. 2000). This theory postulates that households consume biomass fuels such as fuelwood and charcoal at lower levels of income and switch to modern fuels such as kerosene, natural gas, and electricity as their income increases. In this paper, we use a novel randomised controlled trial (RCT) to shed light on the key factors that induce households in urban areas of Africa to shift from charcoal to Liquified Petrolium Gas (LPG).

Using biomass fuels such as charcoal has serious environmental, health, and climatic implications. The use of charcoal for cooking in urban areas and firewood in rural areas of SSA has been a prime cause of deforestation and forest degradation (Campbell et al., 2007; Brown and Bird, 2008; Mercer et al., 2011), clearly resulting in loss of irreplaceble biodiversity and degradation of local ecosystems (Allen and Barnes, 1985; Geist and Lambin, 2002; Hofstad et al., 2009; Köhlin et al., 2011). Biomass fuelwood use is also associated with indoor air pollution, which claims 3.3% of the global burden of disease, especially that of women and children and causes about 2 million premature deaths per year (WHO, 2009). Recent studies also documented that biomass fuel, often burned in inefficient cookstoves, contributes to climate change through its emission of harmful greenhouse gases, including black carbon and carbon dioxide (Sagar and Kartha, 2007; Kandlikar, et al. 2009; Grieshop et al., 2011).

Transition to cleaner fuels is conditional on adoption of appropriate cooking appliances, which can have significant financial implications for poor households, who will forgo consumption of other items to acquire them (Edward and Langpap, 2005; Lewis and Pattanayak, 2012). Using carefully executed randomised controlled trials, a few studies (Smith-Sivertsen, 2009; Hanna et al. 2012; Miller and Mobarak, 2013) have investigated the factors that promote adoption of improved biomass cookstoves and their impact on indoor air quality, health, and fuelwood consumption in rural areas of developing countries. These studies identify social networks, availability of continuous technical support, cultural factors, and good designs that meet households' expectations as important factors that promote the adoption and continued use of improved biomass cookstoves. The few existing studies focusing on adoption of modern (clean) cookstoves use observational data (e.g., Edward and Langpap, 2005; Alem et al. 2014) and identify the high start-up cost as the key factor that hinders households from switching to appliances that use clean energy, such as LPG stoves.

The key question is then whether helping urban households relax liquidity constraints can induce them switch to modern cookstoves, or whether dependence on charcoal for cooking is driven by cultural factors that cannot be altered by public policy in the short-run. In this paper, we provide the first rigorous evidence on the causal effects of relaxing households' liquidity constraints on adoption of LPG stoves and on charcoal consumption. We collaborated with Tanzania's largest micro-finance institution (WAT-SACCO) and randomly allocated households in Dar es Salaam, the largest city in the country, into a "purchase through subsidy" treatment and "purchase on credit" treatment, which constituted three types of credit schemes (payback daily, payback weekly and payback monthly) repayable in six months. To the best of our knowledge, this paper is the first to provide rigorous evidence on the impact of relaxing liquidity constraint on adoption of high-cost cooking appliances and on charcoal consumption.

We take advantage of our randomised design to estimate the impact of adoption of LPG stoves through subsidy and the three types of credit schemes on charcoal consumption. Our results indicate that LPG stove adoption overall resulted in a significant reduction in total charcoal use by the treatment group. Specifically, average treatment effects on the treated (ATT) estimates indicate that households in treated communities consumed 47 percent less charcoal compared to the control group four months after the program was rolled out. This amounted to a reduction in charcoal consumption from 19 kg/week at the baseline to 13 kg/week during the follow-up. However, we find a much larger (54%) reduction in charcoal consumption by households who adopted LPG stoves through subsidy compared to those who acquired them through credit (41%).

Africa's tropical forests have significant carbon sequestration capacity but are at greater risk than those in other parts of the world, disappearing three times faster than the world average (Mercer et al. 2011).[1] Our findings have significant implications for policies that aim at promoting transition of households to clean energy sources, and saving the remaining forest resources of the continent. Given that reducing the startup cost of LGP stoves has significant impact on their adoption and consequently on charcoal use, governments, international donor agencies and other stakeholders should consider channeling resources to improve affordability of LPG stoves to the poor.

The rest of the paper is structured as follows. The next section describes the study area, experimental set-up, and timeline. Section 3 presents descriptive statistics of key variables for both the treatment and control groups. Section 4 presents experimental results on the impact of LPG stove adoption on charcoal consumption, the key outcome variable of interest. Finally, section 5 concludes the paper.

2 Experimental Design

2.1 Study Area

Our study was conducted in Kinondoni and Temeke, two of the three districts of Dar es Salaam, the largest city of Tanzania. These two districts are located at the two extreme ends of the city,

[1] The study by Mercer et al. (2011) actually documents that 30 million ha of Africa's forest, an area equivalent to the size of Finland, was deforested during 2000-2010, 80% of which was for energy consumption.

separated in between by Ilala, a third district. Ilala, which we used for the pilot, is the smallest district both in terms of geographical size and population.[2] Dar es Salaam is the most populous region in Tanzania (with nearly 5 million people) and over 70% of its population uses charcoal as their main source of cooking fuel (NBS, 2012). The heavy reliance on charcoal is evident from the open charcoal markets spread throughout the city. Approximately 1 million tonnes of charcoal is consumed for cooking in Tanzania annually and Dar es Salaam alone consumes half of this amount (World Bank, 2009).

Tanzania has recently discovered huge reserves of natural gas, which is expected to play a significant role in the country's economy by transforming the energy sector and boosting the gross domestic product.[3] Since 2010, several offshore natural gas discoveries have been made by the BG Group in partnership with Ophir Energy, and Statoil in partnership with Exxon Mobil, reaching around 30 trillion cubic feet of recoverable natural gas reserve. With more discoveries envisaged, a pipeline has been constructed to transport natural gas from Mnazi Bay (the central point of discovery) to Dar es Salaam. These discoveries are expected to significantly reduce the cost of gas and electric energy and create the incentive for households to switch away from charcoal to meet cooking energy needs. However, this transition could be significantly constrained by the relatively high startup cost of modern cooking appliances, especially for poor households. Findings from the baseline survey, which we present in the next sections support this skepticism. Almost all households we surveyed (99 percent) stated a high level of awareness about LPG stoves and their benefits but felt constrained not to adopt, mainly because of the high initial cost.[4]

Our study is conducted at an important time to provide useful and policy relevant evidence on the constraints that households face in adopting modern cookstoves and switching away from charcoal, as well as the roles public policy can play in tackling these constraints.. Given the similarities of many Sub-Saharan African countries with Tanzania in terms of access to energy, the findings from this study will also have significant relevance to other African countries.

2.2 Sample Selection and Design

In order to conduct our experiment, we chose two wards from each of Temeke and Ilala districts, from a total of 34 and 30 wards respectively. We chose Sandali and Azimio wards from Temeke district, and Manzese and Mwananyamala wards from Kinondoni. The selected wards are the residences of a majority of the low income urban households in Dar es Salaam and share similar socioeconomic characteristics but are located at a distance from each other. The wards benefited reasonably equally from the Community Infrastructure Upgrading Program (CIUP) implemented by the Dar es Salaam city council between 2005-2010. The program involved improving the quality of roads, footpaths, drainage, sanitation, solid waste, street lighting, public toilets and drinking water (URT, 2004; URT, 2010).

We approached ward secretaries - government officials responsible for administrating wards

[2]See figure A1 in the appendix for map of Dar es Salaam.

[3]http://allafrica.com/stories/201504030134.html.

[4]Currently, less than 4 percent of households in urban Tanzania own modern cooking stoves such as electric or gas stoves (NBS 2012).

under districts - to provide us with the list of all sub-wards, the lowest administrative units in urban areas (also known as streets), ranked by the average economic status of resident households. We then selected the top four streets by their rankings in terms of economic status from each ward to participate in our study, which gave us a total of 16 streets. The key argument for selecting households this way is the fact that re-filling LPG gas once the startup gas runs out requires a bulk purchase (as opposed to low cost daily purchase for charcoal, which is common in the city) and thus the targeted population should be able to afford such costs. Finally, we asked the 16 sub-ward leaders to prepare a roster of eligible households in their streets, from which we randomly selected a total of 722 households to participate in the baseline survey. Eligibility criteria required that the selected households never owned/used an LPG stove and used charcoal (but not kerosene) as their main source of cooking energy.[5]

In order to minimise contamination (spill-over effects from treatment groups to the control group), we assigned treatments at street (sub-ward) level. The sampled streets are scattered across the districts and are reasonably large by geographical size and demographics, with an average of about 3000 households in each sampled street. Street-level randomization also makes implementation of the program relatively easier as it seems fair from households' point of view, and is politically acceptable to the ward leaders. It is therefore important to note that our randomization is done at street-level but the outcome variables of interest are measured at household-level.

We are interested in answering three key research questions: first, we want to identify the impact of LPG stoves (regardless of their mode of acquisition) on charcoal consumption, the key outcome variable of interest; second, we are interested in exploring whether the impact on charcoal consumption is different depending on the mode of acquisition (subsidy or credit); and third, we want to assess the degree of stove use and satisfaction with the stoves by households under the two treatments. We thus randomly assigned five streets into the credit treatment, four streets into the subsidy treatment and kept the remaining 8 streets as the control group. As a result, 216 households were potentially assigned to the credit treatment, 209 to the subsidy treatment and 299 to the control group.

2.3 Timeline and Implementation

We obtained a research permit for this project from the office of Dar es Salaam Regional and Districts Administrative Secretaries, and implemented a fact-finding survey of 40 urban households during October-November 2014. The aim of this survey was to document both qualitative and quantitative background information about knowledge, adoption and usage (and non-usage) of both LPG and charcoal stoves in all districts, important information that we later use to design our interventions. We designed a short questionnaire and conducted a few focus group discussion sessions that allowed us to obtain informative responses. At this stage, we also included a set of questions on households' maximum willingness to pay for an LPG stove package and whether they would like to have the stove package on subsidy or on credit and pay for it bit by bit over a certain period. We found encouraging responses from households regarding knowledge and willingness to

[5]The proportion of households that use kerosene gas in Dar es Salaam is only about 7.8% (NBS 2012).

adopt LPG stoves, either on credit or through subsidy programs. We also found that high start-up cost seemed to be the main factor that hindered households from acquiring the stove.

We conducted a comprehensive baseline survey during March-April 2015, covering all 722 sampled households in the 16 sub-wards. In the baseline we included questions on demographic and other socioeconomic characteristics, cooking habits, stove use, and awareness and willingness to pay for LPG stoves. This was important information given that the cost of acquiring the stove package is reasonably high and it is natural that some households may not be willing to buy it either on credit or through a subsidy. In addition to household-level information, we collected community-level information such as distance to the nearest charcoal market, access to roads, etc.

In early May 2015, we conducted a pre-intervention survey to check whether the households who were assigned to the treatment group were willing to buy the LPG stove. During this time, we informed the treatment group that their household was one of the households randomly selected to receive an LPG stove through a subsidy or credit and that the stoves were planned to be delivered approximately 1-2 weeks after the pre-intervention survey. The households were then asked whether they would like to be a part of the program. Only 296 households of the 425 households who were randomly chosen to participate in the program agreed to purchase the stoves, and the remaining 129 households (30%) declined to participate. We later check whether such refusals to uptake the stoves are likely to bias our sample.

We implemented the LPG stove program in collaboration with a Saving and Credit Cooperative (SACCO) named "Women Advancement Trust" (WAT) which helped us with handling the delivery of the stoves and collection of repayment instalments for the credit treatment households. WAT-SACCO is one of the fast-growing saving and credit cooperatives that are working to provide access to micro-finance for the urban poor. So far, WAT has gained a good reputation and credibility in disbursement and handling of different types of loans, including micro-credit to finance the purchase of home appliances.[6] In order to make the loan credible and minimize the default rate, we followed all procedures for getting such loans as per the rules of the SACCO , but with a few modifications to suit to the objectives of this study. For example, we did not require households to present any physical asset other than the stove itself as collateral. In addition, all credit treatment households were required to pay TZS 20,000 (i.e., 10% of total loan) upfront as their initial re-payment on the day of stove delivery. In addition, they were required to provide a letter of guarantee from their local government offices, which in Tanzanian context is credible.

The intervention was implemented in late May 2015. All households selected for the treatments were invited for training before they were handed the LPG stove in its full package. The training included instructions on how to safely use, clean, maintain and re-fill the LPG stoves once the startup gas runs out.[7] Households under the credit treatment were provided extra instructions regarding their specific credit scheme, including how to fill in the application forms, the required documents, how the payments will be collected, etc. All participants were allowed to ask as many questions as they wished and answers were given by the survey team. To minimize associated transaction costs and inconvenience, we required households receiving the stoves on credit to transfer

[6]See "http://watsaccos.co.tz" for more information about WAT-SACCO.

[7]See figure A2 in the appendix for pictures taken during training and home visits.

the repayment instalments to a given mobile phone account managed by WAT using their mobile phone banking system. The transfers were set to be done during the working hours of either each working day of the week, every Monday or every 30th day of the month, depending on the treatment type. The complete loan repayment period was set to be six months after delivery of the stove, with repayment rates of either TZS 33,350 per month, TZS 8,350 per week or TZS 1,200 per day, depending on the treatment type. We did not charge any interest on the loans but required beneficiary households to cover minor transaction fees charged by mobile phone companies during loan repayment.

We then conducted a midline follow-up survey at the end of September 2015 - approximately four months after the stoves were distributed - to collect information on key outcome variables of interest, including charcoal consumption, LPG stove use, compliance with treatment, and satisfaction with the stoves.[8]

3 Data and Descriptive Statistics

Table 1 presents descriptive statistics of key household socioeconomic characteristics, cooking pattern, charcoal use and stated demand for LPG stoves at the baseline. Panel A shows that the average age of the household head is 48 years, the majority of whom (67%) are male, and the average education is 7.1 years of schooling, which is slightly higher than the standard primary school level in Tanzania (7 years). About half of the sample households live in privately owned households, but only 41% have access to a separate private kitchen, the remainder either cooking in their corridors or sharing a kitchen with other households. Consistent with our expectation, the majority of our sample households are low-income urban dwellers with average reported mean annual income of TZS 309,000 (about USD 172).[9] We notice, however that the reported average daily expenditure on basic consumption items is TZS 9,600, which on annual basis is nearly eleven times larger than the reported income. This overwhelming difference provides additional evidence that, compared to consumption expenditure, income in developing countries is significantly underreported (Deaton, 1997; Deaton and Grosh, 2000). In our subsequent analysis, we rely on consumption expenditure to capture economic status of households.

Table 1 about here

There is a large dependence on charcoal to meet cooking energy needs by households in urban Tanzania (Panel B). The average household cooked using charcoal for about 24 years and consumes 18.7 kg of charcoal per week, which costs about 11,000 TZS. We use insights from a recent study to shed light on the devastating consequences of charcoal use in Tanzania. Luoga et al, (2000) show that it requires one hectare of the Miombo woodland forest of Tanzania to produce approximately 3 tonnes of charcoal. Using rough computation, it is easy to show that our sample of households deplete an equivalent of 0.6 ha of forest every week. When it comes to the intra-household decision

[8]We initially planned to conduct the mid-line survey six months after the stoves were distributed. However, the 2015 Tanzania National Election was scheduled in October 2015. In order to avoid interferences in our survey due to election related activities, we instead decided to conduct the mid-line survey in September 2015, four months after intervention.

[9]At the time of the baseline survey, 1 USD = 1800 TZS.

on the choice of cook stoves, only 47 percent reported that the head is the main decision maker about the type of stoves to be used by the household. This suggests that on average spouses (wives) have fairly strong intra-household bargaining power when it comes to acquisition of kitchen appliances. The type of meals cooked by the household could influence the amount and type of fuel used due to the cooking time and taste of food. During the fact finding survey, a few respondents argued that, while rice tastes better when cooked on a charcoal stove, it takes significantly longer to boil beans (the main ingredient for the complementary sauce) on the stove. Our baseline data suggests that nearly half of the sample cook rice and beans very often, with about 19 meals cooked per week.

Low adoption of LPG stoves in Dar es Salaam seems to be mainly driven by liquidity constraints. Panel C of Table 1 reports that 99 percent of the sample households knew about LPG stoves and 80 percent know someone within their close network who uses the stove.. However, 93 percent of the the sample households reported the high startup cost of the stove package as the main constraint to their adoption, while 70 percent indicated the cost of refilling LPG gas as a challenge. Difference in taste of food cooked using LPG stoves does not seem to be an important reason for not owning LPG stoves for almost the entire sample. Only 2 percent reported it as the main reason for not owning an LPG stove. This could be partly because none of the households in our sample used an LPG stove previously so they did not experience the taste of food cooked using the stove. This number may change during the endline survey when households are asked the same question after they had experienced cooking using the LPG stove. When asked if they wish to have an LPG stove in the future, in case their economic status improves, a staggering 96% of our sample households replied "yes" but their current average willingness to pay for the stove package is only TZS 63,420, which is much lower than the market price (200,000 TZS) of the stove package in Dar es Salaam.

Randomisation of treatment should insure that on average treatment and control groups have similar baseline characteristics. In order to check this, in Table 2, we present means of several key characteristics of households in both groups, as well as test results for the null hypothesis that the difference in means is statistically significantly not different from zero. For nearly all the variables presented, the difference in means is not statistically different from zero. The sole exception is that there is a statistically significant difference in the means of the variable "owning a saving account" between the credit treatment and the control group. Although this is unfortunate, we don't think it will bias our results because the proportion of households who own a saving account in the control group is about 9 percentage points higher than in the credit treatment group.

Table 2 about here

In order to investigate whether the decision by some of the treatment households not to buy the LPG stoves resulted in a systematic difference between the treatment and control groups, we performed a simple mean comparison test for all relevant baseline characteristics. Results reported in Table 3 indicate that none of the baseline variables seem to be statistically different between the treatment and control groups. Consequently, the decision not to buy by some of the potential treatment group households is less likely to create bias in our sample.

Table 3 about here

4 Results

4.1 Charcoal Consumption

Given the randomised nature of our design, we can identify the impact of adoption of LPG stoves on charcoal consumption from the single mean differences between treatment and control groups in an OLS regression. In order to minimise measurement error, during both surveys households were asked to keep a record of the quantity of charcoal used during the most recent week in the local units. We visited four charcoal markets in each ward and constructed average conversion factors to standard units by measuring each available local unit using a digital scale. We then converted all local units reported by households into standard units using these conversion factors.

We begin with results from the simple mean comparison of weekly charcoal consumption between the treatment and control groups during the baseline and follow-up, as reported in Table 6. Panel A presents the results for the quantity of charcoal consumed. While the two groups reported the same consumption of charcoal per week during the baseline (19 kg), treated households consumed 6 kg less in a week compared to the control households during the follow-up survey. This translates into a large reduction in charcoal use which is statistically significant at the one percent level. In panel B, we present the monetary value of the reduction in charcoal due to adoption of LPG stoves. The results reveal that adoption of LPG stoves reduced the amount of weekly charcoal expenditure for the treatment group by about 3,800 (USD 2.1) compared to the control group.

Table 4 about here

Table 5 provides formal empirical estimation of average treatment effects on the treated (ATT) from an OLS model. Column (1) presents the results for the impact of adoption of LPG, regardless of the treatment type. In column (2), we extend the analysis by controlling for the type of treatment (subsidy and credit). This is very important from a public policy point of view given the ongoing debate about the idea that people tend to value and use goods less when they receive them at a lower price (e.g., Hoffman et al, 2008; Hoffman, 2009; Cohen & Dupas, 2010). Consistent with the observation in the mean comparison presented in the previous table, column 1 of Table 5 suggests that LPG adoption reduced charcoal consumption by about 47.5 percent per week compared to the treatment group. When we assess the impact by the treatment type, results in column 2 suggest a relatively larger impact (54 percent) for the stoves adopted through a subsidy compared to the control group than those purchased on credit (41 percent). The results remain robust even after controlling for other covariates (column 3). We argue that the difference in the impact of LPG stoves acquired between the credit and subsidy treatments could be explained by several factors. The main reason could be the fact that we conducted our midline followup survey four months after the interventions and before households who bought the stoves on credit have paid back the full amount of the LPG loan. It is therefore plausible to expect that the credit households are still hesitant to use the stove relative to those who received the stoves through a subsidy and who actually have full ownership. This could be more pronounced by the fact that the stoves themselves are collateral for the credit.

Table 5 about here

4.2 Satisfaction with LPG Stoves

In addition to identifying the impact of LPG stove adoption on charcoal use, it would be interesting to investigate how often adopter households use the stoves and whether the intensity of use differs across treatments. One could anticipate that provision of LPG stoves would encourage households to switch from charcoal to LPG. However, existing empirical evidence (e.g., Masera et al. 2000; Heltberg, 2005) suggests that households may continue to use the charcoal stove in combination with the LPG stove, a phenomenon known as "fuel stacking". During the follow-up survey, almost 25 percent of the treated households (i.e., 74 households) reported not to have used the LPG stove over the past one week.[10] In Table 6, we explore if stove use and intensity are correlated with the type of treatment assigned to households. Results suggest that the number of times the stove is put to use is not correlated with the treatment category. These results are robust to controlling for other covariates. However, we find education (years of schooling) to be positively correlated with the intensity of LPG stove use.

Table 6 about here

We finally explore the extent to which households who received LPG stoves are satisfied with the different attributes. Figure 1 shows the distribution of responses to the satisfaction questions. Results suggest that the majority of households are satisfied with all features of the stove, including stove quality (80 percent), stove functioning (79 percent), gas cost (77 percent), food taste (73 percent) and cooking convenience (80 percent). These results indicate that the type of LPG stoves we distributed have a high acceptance rate by the sample of treated households in urban Tanzania.

Figure 1 about here

In order to explore the correlates of reported levels of satisfaction with the different attributes of LPG stoves, we run simple OLS models of satisfaction and report the regression results in Table 7. Two variables appear to be consistently important correlates of satisfaction with LPG stoves. These are household size and years of schooling. Households headed by educated individuals tend to be satisfied with all aspects of the LPG stoves. Larger households tend to be satisfied with all aspect of the stove except with functioning. We do not, however, find any evidence suggesting satisfaction with stove attributes is correlated with the type of treatment, as indicated by the coefficient of the credit treatment variable, which is statistically insignificant.

Table 7 about here

5 Conclusions

Charcoal, largely consumed by households in urban areas, has been documented to be one of the main causes of deforestation and forest degradation in Africa. Forest clearing for charcoal production results in loss of invaluable biodiversity and destruction of local ecosystems. One important factor that hinders transition of households from biomass energy to clean energy sources is the high start-up cost of modern cooking appliances. In order to test this hypothesis, we collaborated with one of Tanzania's largest micro-finance institutions, WAT-SACOS, and implemented an LPG gas

[10] See Table A3 in the appendix for the distribution of reasons for not using the stoves.

stove program in a randomised controlled trial setup. The program involved provision of a durable and high-quality two-burner LPG stove package through a subsidy and on credit, which included different repayment arrangements. To the best of our knowledge, this is the first study to provide rigorous evidence on the causal effects of relaxing households' financial constraints on on adoption of modern cooking appliances that have a high-start up cost and on charcoal consumption..

The LPG stoves we offered had a high uptake rate by urban households in Tanzania, with 70 percent adoption by those who were randomly assigned to the treatments. Our results indicate that, overall, adoption of LPG stoves reduced charcoal consumption by about 47.5 percent per week compared to the control group. When we assess the impact by the treatment type, estimates suggest that, compared to the control group, those who adopted the stoves through a subsidised price reduced charcoal consumption by 54 percent while those who adopted the stoves on credit reduced charcoal consumption by 41 percent. These results are robust to controlling for other household covariates. This finding is consistent with the reported use frequency by households, with those who obtained the stoves through subsidy using them more often than those who obtained them on credit. The difference in stove use and impact on charcoal consumption was most likely driven by the fact that the follow-up survey took place a couple of months before the full credit amount had been paid out by households, who probably did not feel complete ownership of the LPG stoves.

Millions of hectares of Africa's forests are destroyed for production of charcoal and firewood each year. Given the documented high carbon sequestration potential of Tanzania's forests, targeting reduction of charcoal production is likely to provide substantial external benefits to society at large. The findings from our study provide useful insights on how to reduce charcoal consumption in urban areas of Africa. Both the descriptive statistics and results from our randomised controlled trial demonstrate that the high start-up cost of modern cooking appliances such as LPG stoves is the main factor that prohibits households from switching to modern and environmentally-friendly energy. In view of this, simple policy interventions such as reducing the import duty on LPG stoves could increase adoption and use of LPG stoves and consequently reduce charcoal consumption. This is the main message of our study, which could be useful to policymakers, donors, and other stakeholders who are interested in saving the remaining forest resources of Africa.

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Figure 1: Satisfaction with different features of LPG stoves

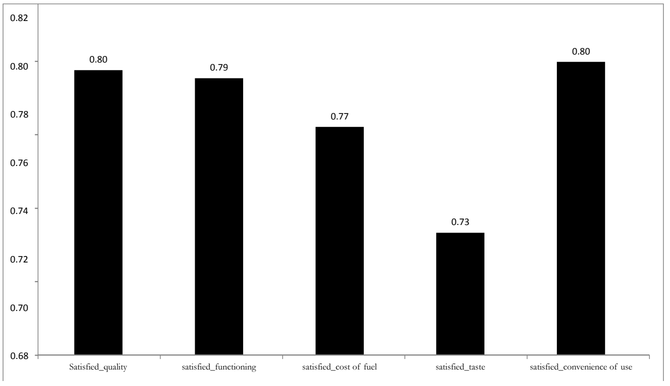

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Table 1: Descriptive Statistics at Baseline

	Mean	SD
PanelA: Socioeconomic Characteristics		
Age	48,004	13,351
Male	0,670	0,470
Household size	5,768	2,222
Annual income (TZS)	309931,000	256702,700
Years of schooling	7,165	3,076
Muslim (dummy, 1= yes)	0,793	0,405
Has access to main grid electricity in the house (dummy, 1= yes)	0,750	0,433
Average household daily expenditure	9661,586	18043,120
Access to separate kitchen room (dummy, 1= yes)	0,406	0,491
Residential house is privately owned (dummy, 1= yes)	0,505	0,500
At least one member owns a saving account (dummy, 1= yes)	0,373	0,484
Panel B: Cooking Pattern and Charcoal Use		
Number of years using charcoal stove	23,748	11,662
Head decides on acquisition of stove (dummy, 1= yes)	0,469	0,499
Distance to nearest charcoal market (in minutes)	4,349	4,224
Number of meals cooked last week	18,885	3,560
Number of meals cooked last week using charcoal	16,073	4,698
Rice, main staple for the household (dummy, 1= yes)	0,477	0,500
Beans, main sauce (dummy, 1= yes)	0,551	0,498
Amount of charcoal used last week (in Kg.)	18,719	10,049
Total expenditure on charcoal last week (in TZS)	10948,030	6107,990
Panel C: Demand for LPG stoves		
Household knows about LPG stoves (dummy, 1= yes)	0,985	0,123
Knows someone using LPG stove (dummy, 1= yes)	0,803	0,398
High start up cost is main reason for not owning LPG (dummy, 1= yes)	0,934	0,249
High cost of refilling is main reason for not owning LPG (dummy, 1= yes)	0,701	0,458
Difference in taste of food cooked is main reason for not owning LPG (dummy, 1= yes)	0,024	0,152
Household wishes to own LPG stove in the future (dummy, 1= yes)	0,961	0,193
Maximum willingness to pay for an LPG stove package (TZS)	63419,670	38548,520
Can afford gas refilling cost (dummy, 1= yes)	0,882	0,323
Walking distance to the nearest LPG gas dealer (in minutes)	17,757	14,102
Observations	722	

Table 2: Descriptive Statistics by Treatment Type

	[1 - Credit]		[2 - Subsidy]		[3 - Control]		[Diff. 1 Vs 3]	[Diff. 2 Vs 3]
PanelA: Socioeconomic Characteristics								
Age	47,769	12,718	47,048	11,974	47,451	12,802	0,317	-0,403
Male	0,694	0,462	0,660	0,475	0,717	0,451	-0,023	-0,057
Household size	5,644	2,039	5,799	2,236	5,997	2,258	-0,353	-0,198
Annual income in TZS (log)	12,486	0,729	12,427	0,738	12,496	0,695	-0,010	-0,069
Years of schooling	7,602	3,261	7,565	3,022	7,404	2,711	0,198	0,161
Access to main grid electricity (dummy, 1= yes)	0,745	0,437	0,809	0,394	0,811	0,392	-0,066	-0,003
Average household daily expenditure	8877,315	5968,327	10545,450	13892,930	9168,350	6438,137	-291,035	1377,100
Separate kitchen (dummy, 1= yes)	0,421	0,495	0,368	0,484	0,421	0,495	0,000	-0,052
Residential house privately owned (dummy, 1= yes)	0,472	0,500	0,483	0,501	0,515	0,501	-0,043	-0,032
Saving account (dummy, 1= yes)	0,366	0,483	0,469	0,500	0,458	0,499	-0,092**	0,011
Panel B: Cooking Pattern and Charcoal Use								
Number of years using charcoal stove	23,736	11,169	22,737	10,862	22,987	10,814	0,750	-0,250
Head decides on acquisition of stove (dummy, 1= yes)	0,472	0,500	0,435	0,497	0,421	0,495	0,051	0,015
Distance to nearest charcoal market (in minutes)	4,512	4,125	4,696	3,902	4,236	4,752	0,276	0,460
Number of meals cooked last week	19,222	3,105	19,364	3,344	19,121	3,492	0,101	0,242
Number of meals cooked last week using charcoal	16,759	4,136	16,292	4,892	16,364	4,654	0,396	-0,072
Rice, main staple for the household (dummy, 1= yes)	0,537	0,500	0,502	0,501	0,488	0,501	0,049	0,014
Beans, main sauce (dummy, 1= yes)	0,560	0,498	0,488	0,501	0,522	0,500	0,038	-0,034
Amount of charcoal used last week (in Kg.)	19,088	8,942	18,482	9,043	19,734	11,735	-0,646	-1,252
Total expenditure on charcoal last week (in TZS)	11137,440	5191,617	10804,530	4921,571	11498,890	7474,942	-361,450	-694,360
Panel C: Demand for LPG stoves								
Household knows about LPG stoves (dummy, 1= yes)	0,981	0,135	0,990	0,098	0,983	0,129	-0,002	0,007
Knows someone using LPG stove (dummy, 1= yes)	0,819	0,386	0,804	0,398	0,791	0,407	0,028	0,013
High start-up cost of LPG (dummy, 1= yes)	0,949	0,220	0,914	0,281	0,936	0,245	0,013	-0,022
High cost of refilling (dummy, 1= yes)	0,704	0,458	0,699	0,460	0,700	0,459	0,003	-0,002
Difference in taste of food (dummy, 1= yes)	0,037	0,189	0,014	0,119	0,020	0,141	0,017	-0,006
Max. willingness to pay for an LPG stove (TZS)	64199,070	37458,220	67263,160	36888,770	60148,150	40281,420	4050,920	7115,010
Can afford gas refilling cost (dummy, 1= yes)	0,889	0,315	0,904	0,295	0,862	0,346	0,027	0,042
Distance to the nearest LPG gas dealer (in minutes)	18,951	15,113	16,145	12,919	18,022	14,081	0,930	-1,876
Observations	216		209		297			

Notes: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 3: Descriptive Statistics by Treatment Type: Accounting for Uptake

	[Treatment]		[Control]		[Diff.]
	Coeff.	SE	Coeff.	SE	Coeff.
Panel A: Socioeconomic Characteristics Age	47,236	12,183	47,505	12,812	-0,269
Male	0,661	0,474	0,721	0,449	-0,060
Household size	5,712	2,173	5,997	2,258	-0,284
Annual income in TZS (log)	12,494	0,733	12,495	0,696	0,000
Years of schooling	7,642	3,127	7,404	2,711	0,238
Average household daily expenditure	10321,920	12560,680	9168,350	6438,137	1153,570
Separate kitchen (dummy, 1= yes)	0,411	0,493	0,421	0,495	-0,010
Residential house privately owned (dummy, 1= yes)	0,466	0,500	0,515	0,501	-0,049
Saving account (dummy, 1= yes)	0,462	0,499	0,458	0,499	0,004
Panel B: Cooking Pattern and Charcoal Use					
Number of years using charcoal stove	24,007	11,516	22,987	10,814	1,020
Head decides on aquisition of stove (dummy, 1= yes)	0,466	0,500	0,421	0,495	0,045
Distance to nearest charcoal market (in minutes)	4,945	4,060	4,236	4,752	0,710
Number of meals cooked last week	19,301	3,303	19,121	3,492	0,180
Number of meals cooked using charcoal	16,356	4,596	16,364	4,654	-0,007
Rice, main staple for the household (dummy, 1= yes)	0,497	0,501	0,488	0,501	0,008
Beans, main sauce (dummy, 1= yes)	0,476	0,500	0,522	0,500	-0,046
Amount of charcoal used last week (in Kg.)	19,193	8,781	19,734	11,735	-0,541
Total expenditure on charcoal last week (in TZS)	11072,760	4821,808	11498,890	7474,942	-426,130
Panel C: Demand for LPG stoves					
Household knows about LPG stoves (dummy, 1= yes)	0,983	0,130	0,983	0,129	0,000
Knows someone using LPG stove (dummy, 1= yes)	0,818	0,386	0,791	0,407	0,027
High start-up cost of LPG (dummy, 1= yes)	0,921	0,270	0,936	0,245	-0,015
High cost of refilling (dummy, 1= yes)	0,719	0,450	0,700	0,459	0,019
Difference in taste of food (dummy, 1= yes)	0,031	0,173	0,020	0,141	0,011
Distance to the nearest gas dealer	17,836	14,423	18,022	14,081	-0,186
Observations	296		297		

Notes: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 4: Charcoal Consumption at Baseline and Follow-up, Treatment and Control Group

	[Baseline]		[Follow-up]		[Diff.]		
	Mean	SD	Mean	SD	Mean	SD	Obs.
Panel A: Amount of Charcoal in KG.							
Treated	19,24	9,18	13,52	17,33	-5,72	1,14	296
Control	19,40	11,70	19,71	10,02	-0,31	0,87	314
Diff	-0,16	0,85	-6,19 ***	1,14			
Panel B: Value of Charcoal in TZS							
Treated	11112,86	5163,02	8354,46	6541,76	2758,41	483,57	296
Control	11279,09	7455,48	12125,99	7191,85	-846,90	584,59	314
Diff	-166	522	-3772***	558			

Notes: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 5: Impact of LPG Stoves on Charcoal Consumption

	[1]	[2]	[3]
	[Charcoal/Week - kg (log)]	[Charcoal/Week - kg (log)]	[Charcoal/Week - kg (log)]
Treatment	-0.475***		
	(0.0881)		
Credit Treatment		-0.414***	-0.384***
		(0.0938)	(0.0783)
Subsidy Treatment		-0.541***	-0.527***
		(0.134)	(0.126)
Intercept	2.899***	2.899***	2.784***
	(0.0369)	(0.0369)	(0.248)
Controls	No	No	Yes
Observations	593	593	593
R-squared	0.091	0.094	0.122

Notes: $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 6: LPG Stove Use: OLS Regression Results

	LPG Use	LPG Use
Credit Treatment	-1.126	-1.159
	(1.205)	(1.105)
Age		0.0268
		(0.0486)
Household size		0.315
		(0.306)
Years of schooling		0.445*
		(0.219)
Male		-0.837
		(0.686)
Separate kitchen (dummy, 1= yes)		-1.135
		(0.991)
Residential house privately owned (dummy, 1= yes)		-1.526
		(1.072)
Number of years using charcoal stove		-0.0414
		(0.0375)
Head decides on acquisition of stove (dummy, 1= yes)		1.295
		(1.195)
Number of meals cooked last week		-0.0388
		(0.176)
Intercept	12.03***	8.446*
	(0.669)	(3.952)
Observations	296	296
R-squared	0.005	0.059

Notes: Standard errors clustered at the street level, $***p < 0.01, **p < 0.05, *p < 0.1$.

Table 7: Satisfaction With LPG Stoves: Probit Regression Results

	Quality	Functioning	Food Taste	Cost	Convenience
Credit Treatment	-0.273	-0.309	-0.142	-0.237	-0.250
	(0.248)	(0.228)	(0.212)	(0.212)	(0.217)
Age	-0.00510	-0.000919	0.000793	0.00441	-0.00177
	(0.00755)	(0.00751)	(0.00967)	(0.00683)	(0.00808)
Household size	0.0795*	0.0448	0.0839*	0.0416	0.0878***
	(0.0408)	(0.0379)	(0.0476)	(0.0368)	(0.0290)
Years of schooling	0.0756**	0.0800**	0.0998***	0.0718*	0.0789**
	(0.0327)	(0.0318)	(0.0327)	(0.0416)	(0.0322)
Male	-0.366*	-0.374	-0.267	-0.407*	-0.435*
	(0.205)	(0.256)	(0.262)	(0.245)	(0.247)
Separate kitchen	-0.0582	-0.0119	-0.00106	0.107	0.0527
	(0.222)	(0.234)	(0.187)	(0.229)	(0.221)
Residential house privately owned	-0.266	-0.243	-0.196	-0.286	-0.223
	(0.183)	(0.220)	(0.186)	(0.189)	(0.190)
Number of years using charcoal stove	0.00779	-0.000186	-0.00815	-0.000188	-0.00395
	(0.00818)	(0.00807)	(0.00803)	(0.00808)	(0.00809)
Head decides on acquisition of stove	0.0683	0.0123	0.119	-0.0531	0.0111
	(0.286)	(0.281)	(0.228)	(0.261)	(0.265)
Number of meals cooked last week	-0.00608	-0.00579	0.000139	-0.00231	0.0111
	(0.0267)	(0.0300)	(0.0248)	(0.0339)	(0.0315)
Intercept	0.542	0.689	-0.138	0.351	0.264
	(0.671)	(0.716)	(0.638)	(0.833)	(0.823)
Observations	296	296	296	296	296
Pseudo R^2 - squared	0.065	0.061	0.066	0.049	0.065

Notes: Robustness standard errors in parentheses, $***p < 0.01, **p < 0.05, *p < 0.1$.

Table A1 Reasons for not using LPG Stoves

	No.	%
Gas run out	5	0,07
Stove/parts mulfunction	3	0,04
Type of food cooked	2	0,03
Not confident on how to operate the stove	4	0,05
Non of the above	60	0,81
Observations	74	1,00

Figure 1: Map of Dar Es Salaam City Council Showing Municipalities

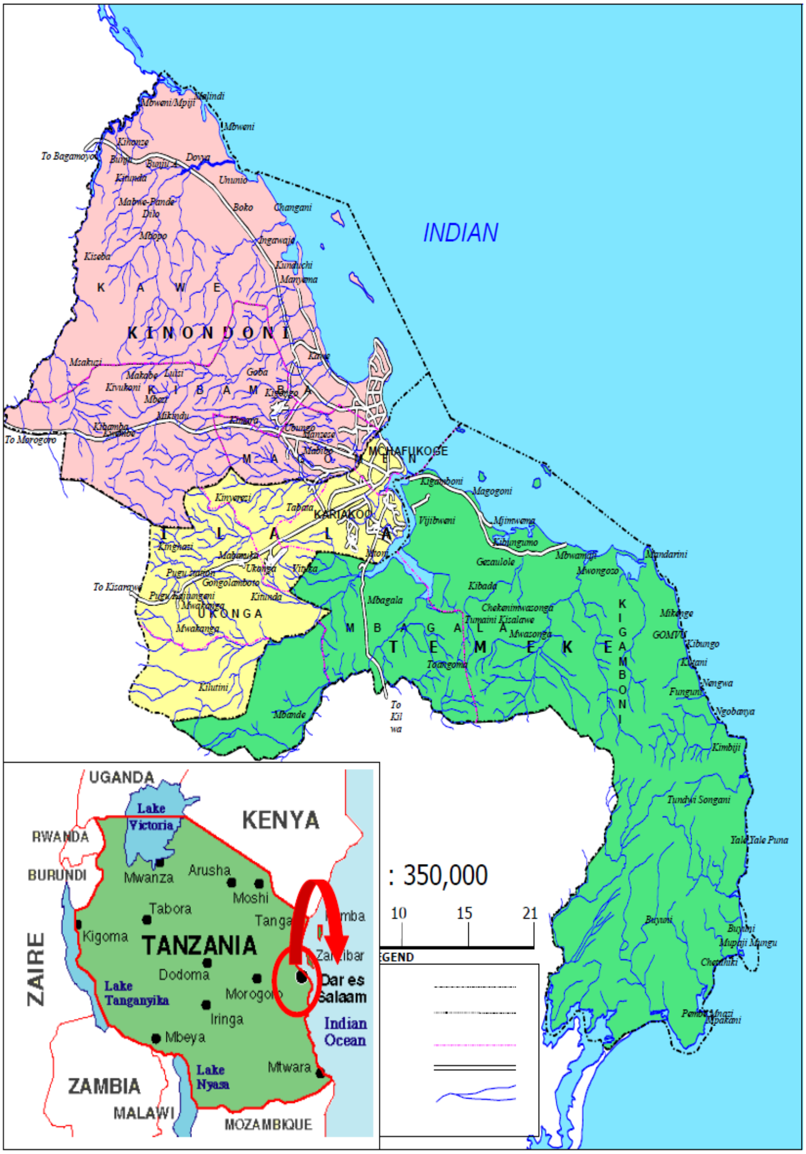

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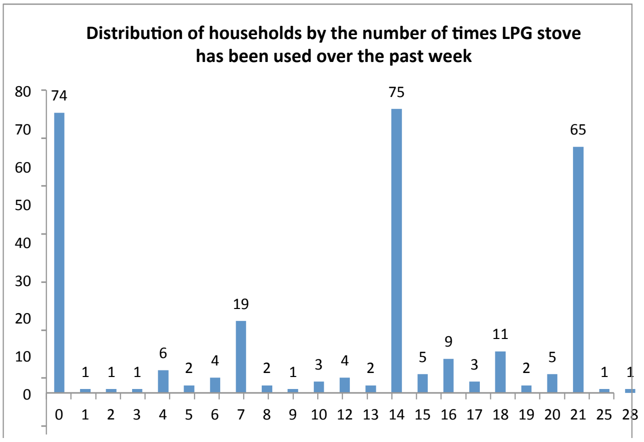
Figure A2.
I). Subjects attending a training session.

II). Picture taken during home visits at follow-up survey.

Figure A3



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Paper 4

Why (field) experiments on unethical behavior are important: Comparing stated and revealed behavior*

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Abstract

Understanding unethical behavior is essential to many phenomena in the real world. The vast majority of existing studies have relied on stated behavior in surveys and some on incentivized experiments in the laboratory. In this paper, we carry out a field experiment in a unique setting. A survey more than one year before the field experiment allows us to compare stated unethical behavior with revealed behavior in the same situation. Our results indicate a strong discrepancy between stated and revealed behavior. This suggests that, given a natural setting, people may actually behave inconsistently with the way in which they otherwise "brand" themselves. Our findings raise cautions about the interpretation of stated behavioral measures commonly used in research on unethical behavior.

JEL Classification: C93, D01, D03

Keywords: Honesty; kindness; guilt; field experiment; behavioral economics

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1. Introduction

Unethical or dishonest behavior in the form of lying, cheating and pursuing one's own self-interest instead of following a focal social convention or norm is widespread. The literature shows that humans engage in unethical acts in order to maximize expected utility where the focus is on monetary rewards (Becker, 1968), but they also refrain from profitable acts of cheating in many cases (Ariely, 2012). If there is a clear tension between being honest and maximizing one's individual monetary return, there seems to be a general tendency to follow the norm and forgo profit. However, there is considerable individual heterogeneity, and circumstances, framing, the monetary consequences of the trade-off, beliefs about the norm, peer behavior, and many other aspects matter (e.g., Ariely, 2012; Gneezy, 2005; Rosenbaum et al., 2014).

A quickly growing literature in economics uses laboratory experiments to study the trade-off between honest and dishonest behavior and its many determinants. While laboratory experiments on dishonest behavior are very powerful in studying incentive effects in decision making, by their very nature, they sometimes appear artificial to participants. Throwing dice or tossing coins, for instance, and using the results of the throws to determine payoffs with a monetary incentive to lie (see Houser et al., 2012; Fischbacher and Föllmi-Heusi, 2013), is not a task that decision makers are requested to perform in the real world. Similarly, individual beliefs on the potential of monitoring by the experimenter, even if excluded by the design, could affect the validity of the results outside the laboratory. Therefore, it seems desirable to complement laboratory experiments with field data on behavior in typical decision making situations in which people can be honest or dishonest. The problem with naturally occurring data in this context is that dishonest behavior often cannot be observed or can only be observed partially, creating all sorts of problems with the interpretation of data. Randomized controlled trials (RCTs) in the field are a potential remedy, but so far they have been very scarce when it comes to studying dishonest behavior. Among the few recent exceptions in economics are Shu et al. (2012), Azar et al. (2013), and Pruckner and Sausgruber (2013).

The objective of this study is twofold. First, it adds to the scarce existing evidence on unethical or dishonest behavior in the field, based on a RCT. We use a very natural setup to assess honesty and moral/ethical behavior in the field, and participants do not know that they are part of an experiment. Second, we have the unique opportunity to link revealed behavior in our RCT with stated behavior in a moral dilemma based on a survey conducted more than one year before the field experiment. This link can be made on the individual level, and it allows us to assess the correlation between stated behavior and revealed behavior in a moral context. As far as we know, we are the first to do so, and our specific setup (the stated question embedded in a much larger survey and the long time between eliciting stated behavior and revealed behavior) makes it very unlikely that participants draw a connection between the survey and the experiment, excluding concerns regarding a potential preference for consistent behavior across the two elicitation methods.

Understanding this association between stated and revealed behavior is important. If answers to appropriate survey questions are highly correlated with revealed behavior, the former would be clearly more desirable than the latter: non-incentivized surveys are usually cheaper and less intrusive, and they usually imply less of ethical concerns. However, if the correlation is weak (see e.g., Bertrand and Mullainathan, 2001), it seems warranted to incur the various costs of field experiments to gain more knowledge on moral behavior.

There is a related discussion on the external validity of laboratory experiments in the field (e.g., Levitt and List, 2007; Falk and Heckman, 2009). Our focus here is not on the stability of behavior when going from the laboratory to the field (lab-field), but on the comparison between two situations in the field (field-field) with different incentives.

As mentioned, the economics literature using RCTs to study unethical behavior in the field is limited.[1] Haan and Kooreman (2002) study the likelihood of paying for candy bars by company employees in an honor system and find that a large proportion of employees do not pay and that their average payments decline over time. Levitt (2006) uses a similar setup to investigate honesty in paying for bagels and donuts by corporate employees. He observes that average payment for bagels declines with a rise in bagel price and increases with the fraction of uneaten donuts and bagels.

More recently, Shu et al. (2012) analyze moral pledges when signing forms and show that signing a form with a proof of honest intent at the beginning rather than at the end of the form increases honest reporting. Pruckner and Sausgruber (2013) look at stealing of newspapers

[1] Obviously, the set of existing papers depends on the definition of unethical and ethical behavior. Here, we do not discuss studies that look at charitable giving and allocation decisions, because all existing field experiments are concerned with donations (giving money) rather than, as in our case, dishonesty in keeping somebody else's money.

from dispatch units on the street. They also show that reminders regarding morality increase the level of honesty, while recaps of the legal norm have no significant effect on behavior. Azar et al. (2013) implement a field experiment in a restaurant that, in spirit, is close to our setup, even though our focus is very different. They let restaurant waiters intentionally return too much change to customers and show that those who receive 10 extra Shekels (about USD 3) are less likely to return them to the waiter than those who receive 40 extra Shekels (about USD 12).[2] In Abeler et al. (2014), the dice-throw paradigm (see Fischbacher and Föllmi-Heusi, 2013) is used to study lying in a representative sample using telephone interviews. They observe surprisingly little evidence for lying among the respondents on the telephone, but the unusual request to throw dice and report the result to a stranger on the phone might have contributed to this result, despite the monetary incentives to report high numbers.[3] Hanna and Wang (2014) provide an interesting result that is based on a laboratory experiment, but where laboratory behavior is linked to real-world decision making. They use the dice-throwing task to predict whether dishonest students are more likely to indicate that they want to work in the public sector, which is indeed the case. They also find that cheating in the dice-throwing task among public workers is associated with more corrupt behavior of these officials. In a somewhat similar vein, Franzen and Pointner (2014) look at the correlation between dictator giving behavior in the laboratory and honesty in returning an intentionally misdirected letter containing money. Indeed, dictator giving is associated with more honesty.

The association between stated and revealed behavior is obviously an important issue in any research that relies on surveys. One example is the long and ongoing debate regarding the usefulness of stated preference methods in general and contingent valuation studies in particular for assessing non-use values when valuing the environment (Diamond and Hausman, 1994; Hanemann, 1994; Hausman, 2012; Kling et al., 2012). The findings of the current study might thus be relevant beyond the realm of moral decision-making.

Our empirical results show that there is only a weak association between stated and revealed behavior. Relying on stated behavior, when drawing policy conclusions, is thus far from

[2] In a somewhat less controlled fashion, Reader's Digest magazine "lost" twelve wallets in each of 16 cities around the world and checked how many of them were actually returned to the owner (http://www.rd.com/slideshows/most-honest-cities-lost-wallet-test/view-all/).

[3] Bucciol et al. (2013) look at free-riding in public transportation in Italy. In their sample, 43% of passengers do not pay. However, the motivation to cheat in situations that involve a company or a public organization might be different than in a bilateral interaction among equal individuals.

optimal. Interestingly, people deviate from their stated behavior in two directions – they behave honestly even though they stated that they would be dishonest and they behave dishonestly even though they stated that they would be honest – but the bulk of our observations go in the expected direction of stated honesty and revealed dishonesty. Our survey allowed the response “not sure what to do,” which was chosen by less than 10% of the respondents, but the subjects in this group turned out to be by far the most honest in the RCT.

Our RCT implements three different treatments which show that the actual levels of honesty or dishonesty are easily malleable, depending on framing and on incentives. The specific effects depend on the situation that decision makers face, but we think that the general result of malleability, which also has been shown in laboratory experiments and in the few existing field experiments in economics, carries over to the field. Socio-economic background variables and individual uncertainty preferences are weak predictors of ethical or unethical behavior in our data. However, this might have to do with our sample of participants that may not be representative outside the given context.

The rest of this paper is organized as follows. Section 2 describes the details of our field setup and our empirical identification. In Section 3, the design of our RCT is discussed, and Section 4 provides our empirical results. Finally, section 5 discusses our findings and concludes the paper.

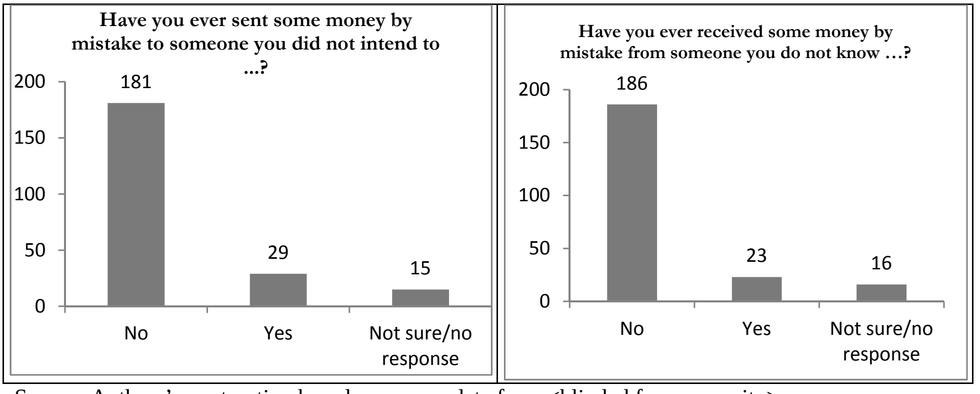
2. Measuring honesty in the field

Both our stated and revealed behavior will be related to unintended or potentially misdirected monetary transfers using mobile phone banking in Tanzania. Our variable of interest is the individual inclination to return an unintended transfer to the sender, despite the fact that the sender cannot enforce such a return. Hence the return is voluntary, but we will use different one-time messages to induce returning the money. We will call returning the money ethical behavior and keeping the money (despite a request to return it) unethical, although this classification might not be fully appropriate in every case.

Because we draw heavily on the mobile phone banking system in Tanzania, we first describe how it works. The use of mobile phone banking in Tanzania has grown rapidly since its first introduction in 2008. According to the central Bank of Tanzania, by December 2013, the country had more than 30 million registered mobile phone bank accounts, of which more than 11 million were recorded as active (BoT, 2014). A recent survey by InterMedia (2013) asserts that 65% of households owned at least one account as of January, 2013. Importantly, within only one month (December, 2013), mobile money transfer businesses in the country performed transactions worth more than Tanzanian Shillings (TZS) 3 trillion (equaling USD 1.8 billion) (Di Castri and Gidvani, 2014).

Because of the large customer base and number of transactions in a day, unintended transfers are common. For one reason or another, one can transfer the money to person A instead of person B. The sender may then immediately learn about the mistake when (s)he receives a text message from the service provider confirming the transaction to a “wrong” number (or name). When this happens, the sender will have to ask the receiver to send back the money. The receiver then decides on whether to send the money back or just keep it. In contrast to bank transfers, a mobile phone transfer cannot simply be recalled due to difficulty in enforcing return—a fact that both the sender and the receiver are well aware of.

Figure 1: Past experience with unintended mobile-phone based money transfers



Source: Authors’ construction based on survey data from <blinded for anonymity>.

Figure 1 presents the data from two survey questions[4], asking whether respondents have either sent or received money by mistake through their mobile phone accounts within the past year. It is obvious that incorrect transfers are common. More than 50 out of 225 respondents have either sent money unintentionally or received money from a source that had sent it unintentionally, and three had experienced both.

While not returning money in such circumstances is a crime in countries such as the UK and the US, the legal obligation is vague in most countries, including Tanzania. The situation is even worse in the mobile phone banking system. For example, Airtel Telephone Company states in its terms of service that any amount of "*airtel-money*" transferred erroneously by the customer shall remain the sole responsibility of the customer and the company will have no liability whatsoever regarding the transaction.[5]

We argue in our identifying assumption that whether or not the receiver sends back the money largely depends on how honest he or she is. One potential concern with this argument is whether what we capture by the decision to return or not is indeed a measure of dishonesty or whether people are just risk averse, i.e., worried that they may be caught and legally charged for theft should they decide to keep the money. In response, it is important to note that the mobile bank system in Tanzania was introduced prior to the establishment of a regulatory framework by the central bank to govern its operations (Di Castri and Gidvani, 2014). Upon the initial proposal by Vodacom Telephone Company, the central bank issued a so-called "letter of no objection" to the company and voiced its intention that a legal framework would be formulated later. To date, there exists no legal framework governing mobile banking transactions, and the receiver can hardly be charged with legal offenses in such circumstances. Also, as mentioned, mobile phone companies articulate that any amount transferred erroneously by the customer shall remain the sole responsibility of the customer. It is thus generally perceived that the probability of being caught and legally prosecuted is close to zero.

A related concern, even in the absence of formal punishment, is that the sender could privately look for the receiver and punish that person informally. However, in the mobile

[4] Data are based on a multi-purpose survey, aimed at assessing the adoption and impact of a climate-friendly agricultural technology, called the System of Rice Intensification (SRI). Details are provided in Section 3.

[5] See http://www.airtel.in/personal/money/terms-of-use and http://africa.airtel.com/wps/wcm/connect/africarevamp/Tanzania/AirtelMoney/faq/

phone framework in Tanzania, the search costs to the sender are too high to pursue such a motive. To be precise, it is difficult to obtain information on the whereabouts of the receiver, although mobile phone accounts are registered. This is common knowledge to both parties. Given the setup, it is not surprising that a good share of mistakenly transferred money never gets returned to the sender. Further, in case risk or ambiguity preferences do matter, our data also allow us to directly control for their influence.

3. Participants, experimental design, and hypotheses

3.1 Participants

We conducted our field experiment with 225 randomly selected heads of farming households from rural Tanzania (specifically from eight villages of the Morogoro region) in November 2014. Our participants took part in a large, multi-purpose survey, involving a monetarily incentivized elicitation of risk, ambiguity and time preferences in September 2013. The survey involved 338 randomly selected households. During the survey, each participant was asked whether s/he owns an active mobile phone account that is registered for mobile phone banking. About 90 percent (302 participants) provided an affirmative answer, and the remaining household heads indicated that they usually use either relatives' or neighbors' phones to make calls or carry out such transactions. The mobile phone numbers used by all participants were recorded in the survey, regardless of who owned the phone. This high share of access to the system motivated the use of mobile phone banking to deliver delayed payments for time preference elicitation in 2013.

In order to study unethical behavior, we ran the experiment, including only those respondents who reported privately owning a mobile phone account, while dropping those who used another person's account. Three days prior to the start of the experiment, we conducted a pre-test of the recorded numbers to check whether they were still active and registered for mobile phone banking. We did it twice on that day (November, 3rd, 2014), calling from different phone numbers which were not used in the actual experiment. From this pre-test, 226 phone numbers were found to be still active and registered for mobile phone banking.[6] We then randomly assigned 225 farmers into three treatment groups.

3.2 Experimental design

To conduct our experiment, we bought and registered nine different mobile phone sim-cards, three from each of the three major mobile phone companies providing service in the area (Vodacom, Airtel, and Tigo). In order to avoid any potential bias due to the sex of the sender, all numbers were registered with male names, given that 93% of the participating household heads in the experiment are male. None of the sim-cards had been used during the payment for the preference elicitation in 2013. We then topped up the accounts with money and sent the money to the farmer. Each received exactly the same amount of money, TZS 20,000 (equivalent to about USD 12)[7]. Once the money is sent, the network system sends back a confirmation message that a specified sum of money has been sent to the owner of a particular mobile phone. We then immediately sent a text message to the receiver asking them to return the money (less TZS 500 covering the transaction cost). Our three treatments differ in the message sent (see Table 1). We used sim-cards registered under the same service provider as that of a given participant/recipient. This enabled us to confirm the names of our participants on their accounts before we made the transfer; hence, we could be sure that we were sending the money to the right person. Delivery status of the sent messages provided an extra confirmation of the transfer.

Our three treatments vary the message to the receiver of the transfer. The CONTROL treatment implements a friendly but rather neutral message to the receiver. It reads “Hi, I have just transferred TZS 20,000 to your m-pesa (or tigo-pesa, etc) account. I was not supposed to send it to you. Could you kindly transfer back TZS 19,500 and use TZS 500 for the transfer fee? Thank you very much.” Our KINDNESS treatment intends to invoke reciprocity by giving a “gift” to the recipient. The message is very similar (see Table 1), but we invite the recipient to keep TZS 5,000 (plus the fee of TZS 500) and only return TZS 14,500 to the

[6] During the experiment and even two days later, the money and text message could not be delivered to one of the 226 subjects. We dropped the observation, leaving us with 225 observations.

[7] The average household expenditure on daily basic needs in our study area was TZS 4,500 at the time of the survey. Hence, the transferred amount corresponds to more than four days of expenditures for an average household.

sender. We chose 25% as a share to be retained, because it seemed a good compromise between being large enough to really matter for the recipient and still small enough to make it worthwhile for the sender to make the offer (see also Fehr and Gächter, 2000). The third treatment, GUILT, aims to induce guilt in the recipient. We alter the message by stating that the money was intended for one of the largest orphanage centers in Tanzania, the *Msimbazi Orphanage Centre,* to help poor children. The total amount of money returned by the subjects in this treatment later on was indeed donated to the Msimbazi Orphanage Centre in Dar es Salaam, Tanzania.

Table 1: Treatments of the field experiments: English translation of the sent messages

Treatment	Message sent
CONTROL	*Hi, I have just transferred TZS. 20,000 to your m-pesa (or tigo-pesa, etc) account. I was not supposed to send it to you. Could you kindly transfer back TZS. 19,500 and use TZS.500 for the transfer fee? Thank you very much.*
KINDNESS	*Hi, I have just transferred TZS. 20,000 to your m-pesa (or tigo-pesa, etc) account. I was not supposed to send it to you. Could you kindly transfer at least 14,500 back to me and you may keep TZS 5000 as my token of appreciation? Thank you very much.*
GUILT	*Hi, I have just transferred TZS. 20,000 to your m-pesa (or tigo-pesa, etc) account. I was not supposed to send it to you but rather to the head of Msimbazi Orphanage Centre to help those poor orphan children. Could you kindly transfer back TZS. 19,500 and use TZS.500 for the transfer fee? Thank you very much.*

The experiment was conducted within 20 hours divided into two days starting from the afternoon of November 6th to the morning of November 7th, 2014. In order to minimize spillover of the information across participants within the same village, we made sure, to the extent possible, that all the participants within the same village were sent the money on the same day and at the same hour[8]. Although mistaken transactions are common, behavior of experimental participants could be biased if they knew in advance of another person who

[8] We tested whether the day on which the experiment was conducted matters for the probability of returning the money. The results presented in Table 6 below suggest statistically zero correlation.

received the same amount of money and a similar message. Participants were never contacted again, and telephone numbers were deleted after a couple of weeks.

In the survey in September 2013, we asked all participants a hypothetical question on what they would do in case they received TZS 100,000 (about USD 59 at the time of the survey) by mistake on their account. The answer to this question is used in the comparison between stated and revealed behavior. The survey also allows us to control for background variables such as income, education, religious attitudes, and of course uncertainty preferences.

We decided to actually send "only" TZS 20,000 instead of TZS 100,000, after realizing from the results in the survey that TZS 20,000 is already equivalent to about four days of household expenditure. The larger amount could have created too much attention in the villages with people talking about it, and it also would have induced a much more severe moral dilemma, which we wanted to avoid for research ethics reasons.

3.3 Predictions

In line with the existing literature, we expect some returns in CONTROL. With respect to the KINDNESS treatment, it is unclear *ex ante* what behavioral response the gift is likely to induce. It could increase the propensity to return the money (see, for instance, Falk, 2007; Kube et al., 2012), unless the monetary gift crowds out an intrinsic inclination to return (i.e., crowds out the potential warm glow from returning the money; see, e.g., Mellström and Johannesson, 2010, with respect to blood donations). Return levels could even be lower than in CONTROL due to self-image concerns, but our working hypothesis is that recipients return more in KINDNESS than in CONTROL.

Some recent laboratory experiments conducted in developed countries suggest that people's revealed level of honesty may be affected when they learn that a third party is likely to be affected by their behavior, either positively or negatively (e.g., Gino and Pierce, 2009; Wiltermuth, 2011; Gino et al., 2013). Other studies have revealed that induced guilt can significantly affect how a person may behave toward others (e.g., Cunningham et al, 1980; Rebega et al, 2013). Hence, we expect return rates to be higher in GUILT than in CONTROL, assuming that the treatment indeed induced guilt among the recipients. We remain agnostic with regard to the relationship between the return rates in KINDNESS and GUILT.

<u>Hypothesis 1:</u> In contrast to selfish predictions, there is a positive return rate in CONTROL.

<u>Hypothesis 2:</u> A "gift" in the form of an offer to share money in KINDNESS increases the return rate significantly over the one in CONTROL.

<u>Hypothesis 3:</u> Inducing guilt in the recipient by stating a good cause for which the money was intended increases the return rate in GUILT significantly over the one in CONTROL.

Now we turn to our central research question concerning the relationship between stated and revealed behavior. It is difficult to predict the relationship between stated and revealed behavior when it comes to moral issues. We are not aware of a previous study that addresses this aspect in a comparable way as we do. Assuming that surveys can elicit truthful answers even in sensitive contexts, we expect that there is a correlation between stated and revealed behavior on the individual level, but that the correlation is far from perfect.

<u>Hypothesis 4:</u> Stated and revealed behaviors in all three treatments are correlated on the individual level.

3.4 Research ethics concerns

Laboratory and field experiments on cheating and moral behavior create a trade-off from the perspective of research ethics. They naturally put decision makers into situations that involve a moral conflict. It is particularly this conflict that one wants to study, and we argue that the moral conflict is significant, but not as severe as to generate psychological disconfort in our case. Note that any resource allocation experiment (such as the dictator game) involves a similar conflict.

In our case, we link data from a questionnaire with decisions after the receipt of the money. To address our main research question (summarized in Hypothesis 4), it is necessary to make this link on the individual level and without the consent of participants. The latter is the case in many field experiments in economics. However, the potential psychological effect of the study on participants seems acceptable because it is expected to be very limited. All safeguards regarding anonymity (removal of mobile phone numbers and names from the data after the completion of the experiment) have been implemented.[9]

All experiments on unethical behavior inside and outside the laboratory also involve an intentionally misleading signal regarding the real intention of the experimenters. The dice-throw paradigm signals that researchers are interested in dice throws, but they are actually interested in cheating. Losing one's wallet to study return behavior signals that the loss was by mistake, but actually it was intentional. The same is true for our transfers (and the messages). Notice that the money stated to be for the orphanage and returned by the recipients in treatment GUILT was actually transferred to the orphanage after the experiment to avoid deception.

4. Experimental results

We organize the results from our study along the following lines. In Section 4.1, we look at an overview of descriptive variables on the return levels in order to address Hypotheses 1-3. In Section 4.2, we empirically link stated behavior in the survey with revealed behavior in the field experiment. Finally, in Section 4.3, we take another look at the determinants of unethical behavior.

4.1 Descriptive overview

We have 13 out 225 cases in which we are not sure whether the person received the money. For the descriptive overview, we proceed on the assumption that those 13 people indeed got the transfer. Later on, we will drop them for robustness checks, but we will always indicate clearly when we do this.

[9] The Economics Department at the University of Gothenburg has no Institutional Review Board (IRB), and there is no requirement to ask for IRB approval for experiments. Nonetheless, we tried to implement all provisions that an IRB would have asked for at other European universities. The University of Dar es Salaam's Research Board (a body similar to IRBs) approved the experiment.

Our random allocation to the three treatments seems to have been successful. Out of 24 comparisons, only one – household daily spending – was significantly different between the Control and Guilt groups (see Table A.1 in the Appendix, where means and standard deviations for relevant variables are also displayed).

When we look at return levels, we note that most people who returned money returned the requested amount. In CONTROL, a total of 18 participants returned money, of whom 10 returned the requested amount of TZS 19,500, five returned TZS 19,000 and three returned TZS 20,000. In KINDNESS, we observe more variation, with returns between TZS 14,500 and 20,000 among those who returned money. For the GUILT treatment, 24 participants returned TZS 19,500, four returned TZS 20,000 and the remaining 47 nothing. Given the lack of variation in individual return levels when a positive amount is returned, deliberately induced by our setup, we consider return a binary variable. Both KINDNESS and GUILT result in higher return rates and total amount returned compared to CONTROL. Return rates were highest from KINDNESS treatment, but given that many of those returning in KINDNESS kept the 'gift' of TZS 5,000 the total amount returned was the highest from the GUILT treatment.

Table 2 provides an overview of the averages in the three treatments and the results of two-sided significance tests. In general we find that 34.7 % of our sample returned some money to the sender. Only 24% of the participants in CONTROL returned the requested amount (or a very similar amount). Consistent with Hypothesis 1, this is clearly above zero – it is more than just behavioral noise – although the great majority did not return the transfer.

Result 1: Consistent with Hypothesis 1, some recipients in the CONTROL returned a positive amount of money to the sender although a majority does not return.

KINDNESS induces a higher return rate, consistent with Hypothesis 2. The rate almost doubles to 42.7% compared to CONTROL, and the difference between the two treatments is highly significant. Also consistent with Hypothesis 2, offering a "gift" induced reciprocity. In addition, the mean amount returned is greater in KINDESS, although the difference is not significant at conventional levels. The mean returned amount is TZS 6,661 in KINDNESS, compared to a mean returned amount of TZS 4,667 in CONTROL. Despite the lack of significance, the benefit of the "gift" obviously outweighs its costs on average.

Result 2: KINDNESS induces significantly higher return rates than CONTROL and the sender is better off on average in KINDNESS. This is despite the lower returned absolute amounts as a consequence of the "gift,".

Table 2: Distribution of return rate by treatment

Treatment	Number of observations	Returned	Probability of returning	p-value of FE-test	Returned amount	p-value of MWU-test
CONTROL	75	18	24,0%	-	4,667	-
KINDNESS	75	32	42,7%	0.02**	6,661	0.23
GUILT	75	28	37,3%	0.11	7,567	0.03**
All	225	78	34,7%			

Note: The p-values refer to two-sided Fischer exact (FE) and Mann-Whitney-U (MWU) tests for the difference in proportions and in means (medians) between the control group and the treatment groups.
** significant at the 1% level.

Treatment GUILT lies in between. It induces a return rate of 37.3%, which is higher than the one in CONTROL, as proposed in Hypothesis 3, but the difference between GUILT and CONTROL misses conventional levels of significance. On the other hand, GUILT creates the highest average amount returned among the three treatments, at TZS 7,567, which is significantly higher than in CONTROL and is consistent with Hypothesis 3.

Result 3: GUILT induces higher return rates than CONTROL. It shows the highest average returned amounts of all three treatments.

The general impression is that both treatments work in the same direction. From the perspective of the return rate, treatment KINDNESS works best, whereas from the perspective of the average amount returned, treatment GUILT works better, because the cost of the gift cannot be compensated by the higher return rate in KINDNESS, when compared to GUILT.

4.2 Do people act as they brand themselves? Stated versus revealed honesty

When asked in the survey, only 45% of the sample responded that they would return the entire amount. Table 3 provides a comparison of the fractions from the stated honesty and from the revealed honesty. We pool data from all treatments for Table 3, but qualitatively there is not much difference. However, we assume that sending back TZS 14,500 in the KINDNESS treatment means sending back the entire amount. At first sight, it seems that the stated levels of honesty correspond at least somewhat with the actual behavior. Those who said that they would return the entire amount actually did that more often than those who said that they would send some money back, but the difference is small. Interestingly, almost one-third (29.6%) of the 24% who said they would keep the entire amount actually sent the entire requested amount back. The highest actual return rate is shown by those who indicated in the survey that they were not sure what they would do. It is interesting that this is the group that is most honest when it comes to actual behavior, but the absolute number of people is relatively small (20 respondents), and therefore we do not want to over-interpret the result. On average, 67.1% stated that they would return at least some of the money, but the actual fraction is 35.1%. Hence, we find that on average 2/3 of our participants claim to be honest, but only 1/3 actually behave in an honest way.

Table 3: Share of those who returned the money (i.e., revealed honesty), by type of survey promise (i.e., stated honesty)

	Stated honesty: Distribution	**Fraction that returned requested amount**
Send the entire amount back	45.3%	37.3%
Send some of money back	21.8%	28.6%
Not sure what to do	8.9%	55.0%
Keep the entire amount	24.0%	29.6%
Whole sample	**67.1%**	**35.1%**

Table 4 combines the data from the survey (in columns) with the data from the field experiment (in rows). For instance, 38 people who said that they would keep the entire amount actually kept the entire amount; 16 of those sent some or the entire amount back. Table 4 excludes the category "not sure what to do," because it does not exist for revealed behavior. One can see that there are deviations from the stated behavior in both directions – i.e., toward being actually more honest than stated ("positive surprise") and toward being less honest than stated ("negative surprise") – but the second case is clearly more frequent. If we also exclude the category "send some money back," which is the one that is most difficult to assess in comparisons between stated and revealed behavior in our setup, we have 156 observations. Of those, 16 (10%) surprised positively and 65 (42%) surprised negatively. The remaining 75 (48%) behave as indicated in the survey.

Table 4: Transition matrix from stated honesty to revealed honesty (excluding the survey category "Not sure what to do")

<table>
<tr><th>Stated
Revealed</th><th>Keep the entire amount</th><th>Send some of money back</th><th>Send the entire amount back</th></tr>
<tr><td>Keep the entire amount</td><td>38</td><td>(14)</td><td rowspan="2">65</td></tr>
<tr><td>Send some money back</td><td rowspan="2">16</td><td>35</td></tr>
<tr><td>Send the entire amount back</td><td>(14)</td><td>37</td></tr>
<tr><td>Number of observations</td><td>54</td><td>49</td><td>102</td></tr>
</table>

The real test of the concept is however whether the stated behavior has predictive power for the actual behavior. In Table 5, we run a set of regressions to address this issue. There are several ways to look at the data. We look at the entire sample (225 observations), without excluding those for whom we are not entirely sure whether they received the money (13 observations)[10]. The sensitivity analysis for the smaller sample with 212 observations is provided in the appendix. We use a binary variable as the dependent variable, because all other options seem inferior for several reasons. First, the returned amount is a strangely distributed variable. Second, the vast majority of participants either returned the entire requested amount or nothing. The few exceptions who returned more or less than the requested amount do not change the picture. Third, a probit regression is easier to interpret than other models such as hurdle models. To make sure that the results are robust – in particular, with respect to the interaction of dummy variables – we provide OLS estimates in the appendix.

Table 5 displays several significant results, indicating a connection between stated and revealed behavior. The important aspect to notice is that the omitted group comprises those respondents who stated in the survey that they did not know what they would do upon receiving the transfer. People who answered that they would "send some money back" or "keep the entire amount" are significantly less likely to actually return money. However, there is clearly no significant difference in the likelihood of returning money between the three

[10] This group includes those for whom, although the system sent the money to their account, for some reason we did not receive the delivery notification status of our treatment message. Because we closed our phone numbers just a few days after the experiment, we couldn't observe the actual day of the message delivery

main categories “send the entire amount back”, “send some money back”, and “keep the entire amount.” These results are robust to the inclusion of treatment dummies and when we control for further background variables such as socio-economic variables as well as risk and ambiguity attitudes.

Table 5. Estimation results from probit models (marginal effects) – revealed honesty

Dependent variable **Dummy: Money Returned**	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**
STATED: Send the entire amount back	0.03 (0.06)	-	-	-0.17 (0.11)	-	-0.16 (0.11)	-
STATED: Send some of money back	-	-0.08 (0.07)	-	-0.23** (0.10)	-	-0.22** (0.10)	-
STATED: Keep the entire amount	-	-	-0.07 (0.07)	-0.22** (0.10)	-	-0.20* (0.10)	-
STATED AMOUNT index	-	-	-	-	0.03 (0.08)	-	0.02 (0.08)
Treatment KINDNESS	-	-	-	-	-	0.18** (0.08)	0.19** (0.08)
Treatment GUILT	-	-	-	-	-	0.15* (0.08)	0.14* (0.08)
Number of observations	225	225	225	225	225	225	225

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

We also tried to create an index for stated honesty by arbitrarily assigning the values 0 to “keep the entire amount”, 0.5 to “send some money back”, 1 to “send the entire amount back”, and 0.25 to “not sure what to do”. The index is not significant in any specification, and its coefficient remains far from significant levels even if one excludes the 20 observations for the category “not sure what to do”, because their assignment to a value of 0.25 seems most arbitrary.

In general, results do not change, although in some cases the significance of coefficients for main effects vanish, once we interact the treatment dummies with the answer categories from the survey. Overall, it seems that the stated behavior has little predictive power for what people actually do in our experiment, when they face the same situation in real life. To what extent is this conclusion in line with the finding in Table 4, which shows that 48% behave as stated? On average, there is some consistency, but this consistency does not necessarily lead to strong predictive power. One issue to bear in mind is the handling of the category “send some money back" (and to a lesser extent the category "not sure what to to"). Depending on how these categories are handled, the consistency level goes either up or down. The above-mentioned 48% are more or less the upper boundary, and any other definition or assignment would reduce average consistency levels.

Result 4: The predictive power of stated behavior for actual behavior is weak in our experiment.

One possibility is to extend Tables 4 and 5 by regressing background and treatment variables on a dummy capturing consistency of answers regarding stated and revealed behavior. We have done that in different specifications. However, it appears that our sample is not large enough to show a clear relationship between any of the background variables – be it socio-economic or preference-based – and consistency. This is not surprising. One would expect that any potential relationship is more subtle and would only show up in a much larger sample.[11]

4.3 Determinants of honesty

Finally, we want to address the determinants of honesty. In this sub-section, we look at observable characteristics that predict whether people return money, and we disregard their stated behavior. Table 6 gives the results of probit models with a binary variable for returned money as the dependent variable. Again, the sensitivity analyses, using OLS and the restricted sample, are provided in the appendix.

Table 6 indicates that age and years of schooling are positively associated with being honest. The relationship between age and ethical behavior seems to be inversely U-shaped, indicated by the negative sign of the squared term. These results do not change when we control for income or household expenditures. Household expenditures and income are never significant in the regression. We also tried to control for religious activity as a determinant, but it was always far from significant.

[11] Results are available on request.

Finally, our claim that uncertainty attitudes would not play a role in the decision to return money is confirmed in the data. The two coefficients for risk and ambiguity attitudes are far from being significant in Model [3] in Table 6.

Table 6. Estimation results from probit models (marginal effects) – revealed honesty: determinants.

Dependent variable Dummy: Money Returned	**[1]**	**[2]**	**[3]**
Age	0.000 (0.002)	0.034* (0.019)	0.035* (0.200)
Age squared	-	-0.0004* (0.0002)	-0.0004* (0.0002)
Years of schooling	-	0.021* (0.012)	0.022* (0.012)
Risk aversion	-	-	-0.003 (0.128)
Ambiguity aversion	-	-	-0.161 (0.127)
First day of experiment (dummy)	-	-	0.053 (0.065)
Treatment KINDNESS	0.200** (0.081)	0.205** (0.082)	0.200** (0.082)
Treatment GUILT	0.143* (0.082)	0.143* (0.082)	0.136* (0.083)
Number of observations	225	225	225

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

Result 5: Age and years of schooling have a significant influence on the revealed level of honesty in our sample.

5. Discussion and Conclusion

We have used a unique setting to link stated honesty to revealed honesty on the individual level in a naturally occurring situation in the field that creates a moral dilemma between being honest and accepting a monetary gain for being dishonest. Our main result indicates a strong discrepancy between stated and revealed behavior or, in other words, a weak association between words and actions.

First of all, our findings indicate that hypothetical surveys on ethical and unethical behaviors poorly reflect the actions taken in the same real-world situations. As a consequence, it seems important to use experiments, and in particular field experiments, to study the determinants of unethical behavior and to assess the effects of potential interventions to support and maintain ethical behavior. Our experiment is not the only one showing that revealed ethical and unethical behavior is quite malleable in response to the circumstances and the incentives, but it is, to our knowledge, the first one that is able to fully control for the comparison between stated and revealed behavior. Interestingly, we observe deviations between stated and revealed behavior in both directions, toward being more honest than self-stated and toward being less honest than stated. This indicates that there is a certain expected noise in behavior that might also be attributed to the time that passed between the survey and the experiment, but importantly there is also a clear bias. The case of stating honesty and actually behaving dishonestly is more than four times as frequent in relative terms than the opposite case. Hence, simple random variation in behavior over time cannot explain our results.

We have also ruled out explanations based on the fear of formal and informal punishment that might be differently salient in the abstract survey situation and the real-world decision. First, we argue that our institutional setup is unlikely to give rise to such fears. Second, if participants nonetheless perceived the situation as risky, participants should become more honest in the actual decision than in the hypothetical survey response. Third, controlling for risk and ambiguity attitudes as potential determinants of behavior when receiving money, which should matter if participants perceived the situation as risky or uncertain, does not change our results, and the measures for uncertainty in the regressions are far from being significant in any specification.

While we do not want to over-state the result, because it is based on slightly less than 10% of our sample, we note that the group of participants who responded “Not sure what to do” in the survey was actually by far the most honest group when it came to actual behavior. If this particular finding is robust, which has to be established in future work, one can imagine question techniques in surveys that might be helpful in increasing the predictability of survey answers. Surveys will always be important tools in the social sciences, but one has to be careful not to over-interpret their predictive power, in particular when it comes to moral behavior.

With regard to the mechanisms implemented in our treatments, guilt works as expected, and giving a gift to induce people to return the money does not crowd out the intrinsic inclination to return the transfer. Both have a positive effect on the propensity to return the transfer. On balance, the specific implementation of the treatment matters. More research in this area is needed to establish general and robust patterns of human behavior. Our main contribution to this discussion is that simple surveys are often not enough to establish knowledge about patterns of ethical and unethical behavior in the field.

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Appendix

TableA.1: Means (standard deviations) of main pre-treatment variables

Variable	Full sample	CONTROL	KINDNESS	GUILT	CONTROL vs. KINDNESS[a]	CONTROL vs. GUILT[a]
Age	43.41	43.79	42.25	44.20	0.39	0.70
	(11.427)	(11.038)	(11.859)	(11.430)		
Years of schooling	8.49	8.37	8.53	8.56	0.99	0.72
	(2.727)	(2.735)	(2.952)	(2.511)		
Male dummy	0.93	0.96	0.93	0.89	0.47	0.12
	(0.258)	(0.197)	(0.251)	(0.311)		
Household (HH) size	4.62	4.75	4.53	4.59	0.48	0.57
	(1.819)	(1.846)	(1.833)	(1.794)		
HH daily spending	4.51	4.29	4.20	5.04	0.60	0.01**
(in TZS 1,000)	(2.098)	(1.986)	(2.119)	(2.111)		
HH annual off-farm	509.68	449.57	402.80	676.66	0.56	0.28
income (in TZS 1,000)	(1070.9)	(942.792)	(691.56)	(1435.7)		
Experience: money sent	0.13	0.12	0.13	0.13	0.81	0.81
erroneously (dummy)	(0.336)	(0.327)	(0.342)	(0.342)		
Experience: money rec'd	0.10	0.09	0.11	0.11	0.79	0.79
erroneously (dummy)	(0.304)	(0.293)	(0.311)	(0.311)		
Alcoholic beverage daily	0.15	0.15	0.15	0.16	1.00	0.82
(dummy)	(0.359)	(0.356)	(0.356)	(0.369)		
Religious (dummy)	0.17	0.12	0.19	0.20	0.26	0.18
	(0.375)	(0.327)	(0.392)	(0.403)		
Risk attitude [0,1];	0.56	0.52	0.57	0.58	0.30	0.19
0.5=risk neutral	(0.270)	(0.283)	(0.255)	(0.273)		
Ambiguity attitude [-1,1];	0.00	0.03	-0.01	-0.01	0.13	0.76
0=ambiguity neutral	(0.282)	(0.339)	(0.253)	(0.249)		
Observations	225	75	75	75	75	75

[a] P-values for two-sided Mann-Whitney-U-test.
** significant on the 1%-level.

Table 5a. Estimation results from probit model (marginal effects) – revealed honesty (212 observations)

Dependent variable Dummy: Money Returned	[1]	[2]	[3]	[4]	[5]	[6]	[7]
STATED: Send the entire amount back	0.02 (0.07)	-	-	-0.20* (0.11)	-	-0.20* (0.11)	-
STATED: Send some of money back	-	-0.09 (0.088)	-	- 0.26** (0.10)	-	- 0.26** (0.10)	-
STATED: Keep the entire amount	-	-	- 0.045 (0.08)	-0.23** (0.10)	-	-0.21* (0.11)	-
STATED AMOUNT index	-	-	-	-	0.01 (0.08)	-	-0.01 (0.08)
Treatment KINDNESS	-	-	-	-	-	0.19** (0.09)	0.20** (0.08)
Treatment GUILT	-	-	-	-	-	0.18** (0.09)	0.17** (0.08)
Number of observations	212	212	212	212	212	212	212

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

Table 5b. Estimation results from OLS model– revealed honesty (full sample)

Dependent variable Dummy: Money Returned	[1]	[2]	[3]	[4]	[5]	[6]	[7]
STATED: Send the entire amount back	0.03 (0.06)	-	-	-0.19 (0.12)	-	-0.18 (0.12)	-
STATED: Send some of money back	-	-0.08 (0.08)	-	-0.26** (0.13)	-	-0.25* (0.13)	-
STATED: Keep the entire amount	-	-	-0.07 (0.07)	-0.25** (0.12)	-	-0.22* (0.12)	-
STATED AMOUNT index	-	-	-	-	0.03 (0.08)	-	0.01 (0.08)
Treatment KINDNESS	-	-	-	-	-	0.17** (0.08)	0.19** (0.08)
Treatment GUILT	-	-	-	-	-	0.14* (0.08)	0.13* (0.08)
Number of observations	225	225	225	225	225	225	225

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

Table 5c. Estimation results from OLS model– revealed honesty (212 observations)

Dependent variable Dummy: Money Returned	**[1]**	**[2]**	**[3]**	**[4]**	**[5]**	**[6]**	**[7]**
STATED: Send the entire amount back	0.02 (0.07)	-	-	-0.22* (0.12)	-	-0.21* (0.12)	-
STATED: Send some of money back	-	-0.09 (0.09)	-	- 0.30** (0.13)	-	- 0.29** (0.13)	-
STATED: Keep the entire amount	-	-	- 0.05 (0.08)	-0.27** (0.13)	-	-0.24* (0.13)	-
STATED AMOUNT index	-	-	-	-	0.01 (0.08)	-	-0.01 (0.08)
Treatment KINDNESS	-	-	-	-	-	0.17** (0.08)	0.19** (0.08)
Treatment GUILT	-	-	-	-	-	0.16** (0.08)	0.16** (0.08)
Number of observations	212	212	212	212	212	212	212

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

Table 6a. Estimation results from probit models (marginal effects) – revealed honesty: determinants (212 observations)

Dependent variable Dummy: Money Returned	**[1]**	**[2]**	**[3]**
Age	0.001 (0.003)	0.035* (0.021)	0.036* (0.207)
Age squared	-	-0.0004* (0.0002)	-0.0004* (0.0002)
Years of schooling	-	0.021 (0.013)	0.022* (0.013)
Risk aversion	-	-	-0.012 (0.132)
Ambiguity aversion	-	-	-0.210 (0.134)
Treatment KINDNESS	0.203** (0.084)	0.204** (0.085)	0.203** (0.086)
Treatment GUILT	0.169** (0.085)	0.163* (0.085)	0.162* (0.086)
Number of observations	212	212	212

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

Table 6b. Estimation results from OLS models– revealed honesty: determinants (full sample)

Dependent variable Dummy: Money Returned	**[1]**	**[2]**	**[3]**
Age	0.001 (0.003)	0.031* (0.018)	0.032* (0.018)
Age squared	-	-0.0003* (0.0002)	-0.0003* (0.0002)
Years of schooling	-	0.019* (0.012)	0.020* (0.012)
Risk aversion	-	-	-0.010 (0.126)
Ambiguity aversion	-	-	-0.147 (0.120)
Treatment KINDNESS	0.188** (0.077)	0.194** (0.077)	0.190** (0.077)
Treatment GUILT	0.133* (0.077)	0.132* (0.077)	0.126 (0.077)
Number of observations	225	225	225

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

Table 6c. Estimation results from OLS models – revealed honesty: determinants (212 observations)

Dependent variable Dummy: Money Returned	**[1]**	**[2]**	**[3]**
Age	0.001 (0.003)	0.032* (0.019)	0.032* (0.019)
Age squared	-	-0.0003 (0.0002)	-0.0003* (0.0002)
Years of schooling	-	0.019 (0.012)	0.020* (0.012)
Risk aversion	-	-	-0.013 (0.129)
Ambiguity aversion	-	-	-0.184 (0.123)
Treatment KINDNESS	0.191** (0.080)	0.191** (0.080)	0.186** (0.080)
Treatment GUILT	0.157* (0.080)	0.150* (0.080)	0.144* (0.080)
Number of observations	212	212	212

Note: *** denotes significance at the 1% level, ** at the 5% level and * at the 10% level. Standard errors in parentheses.

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