Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies

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Abstract

This paper measures the economic impacts of social pressures to share income with kin and neighbors in rural Kenyan villages. We conduct a lab experiment in which we randomly vary the observability of investment returns to test whether subjects reduce their income in order to keep it hidden. We find that women adopt an investment strategy that conceals the size of their initial endowment in the experiment, though that strategy reduces their expected earnings. This effect is largest among women with relatives attending the experiment, who invest 22 percent less when income is observable. At the village level, the extent to which experimental subjects engage in income hiding within the experiment is negatively associated with the probability of skilled employment and the value of household assets.

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"Whoever has a more mobile occupation, and less respect for tradition, tries to cover his tracks. In Dodoma, I once ran into a street vendor hawking oranges who used to bring these fruits to my house in Dar es Salaam. I was happy to see him, and asked him what he was doing here, five hundred kilometers from the capital. He had had to flee from his cousins, he explained. He had shared his meager profits with them for a long time, but finally had had enough, and ran. 'I will have a few cents for a while,' he said happily. 'Until they find me again!' "

— Kapuscinski (2002)

1 Introduction

Risk is a pervasive aspect of the lives of individuals in many developing economies, and informal risk-pooling arrangements which help households cope with shocks can have substantial welfare impacts when credit and insurance markets are incomplete. A substantial body of empirical evidence documents the existence of mutual insurance arrangements throughout the world, demonstrating that informal mechanisms typically fail to completely insure households against idiosyncratic shocks (cf. Townsend 1994, Coate and Ravallion 1993, Fafchamps and Lund 2003). Much of the literature focuses on mutual insurance arrangements which are efficient given constraints, characterizing the conditions under which self-interested households will enter risk-pooling schemes voluntarily *ex ante* and the participation constraints which keep households from defecting *ex post*.[1] However, the expectation of future transfers is only one of many reasons households offer assistance to those worse off: altruism, guilt, and social pressure to share income may also play a role (Scott 1976, Foster and Rosenzweig 2001). In fact, several recent studies suggest that individuals living in poor, rural communities often feel obligated to make transfers to relatives and neighbors, and that successful families who do not make sufficient transfers to others can face harsh social sanctions (Platteau 2000, Hoff and Sen 2006, Comola and Fafchamps 2010). For example, Barr and Stein (2008) argue that Zimbabwean villagers punish households who are becoming better off than their neighbors by refusing to attend the funerals of members of those families. When social pressures to

[1] See Ligon, Thomas, and Worrall (2002), Albarran and Attanasio (2003), and Kinnan (2010) for examples. Foster and Rosenzweig (2001), which explores the impact of altruism on the set of self-enforcing insurance arrangements, is an important exception.

assist kin, and the sanctions against those who violate sharing norms, are strong enough, they can reduce incentives to make profitable investments and drive savings into lower-return technologies which are less observable to family members. Baland, Guirkinger, and Mali (2007) provide evidence of this type of behavior in Cameroon, where members of credit cooperatives take out loans to signal that they are liquidity constrained — even when they also hold substantial savings — in order to avoid sharing accumulated wealth with relatives.

In this paper, we report the results of an experiment designed to measure the economic impacts of social pressures to share income with relatives and neighbors in rural villages in Sub-Saharan Africa. We use a controlled laboratory environment to explore behaviors which are difficult or impossible to document using survey data: the willingness to forgo profitable investment opportunities so as to keep one's income secret from relatives and neighbors. We conduct economic experiments in 26 rural, agricultural communities in western Kenya. Within the experiment, subjects receive an endowment which they divide between a risk-free savings account and a risky, but profitable investment. The size of the endowment varied across subjects, though the distribution of endowment sizes is common knowledge, creating an incentives for those receiving the larger amount to invest in a manner which keeps the size of their budget hidden. While the amount saved is always private information, we randomly vary whether the amount invested in the risky security can be observed by other subjects. The implication is that subjects who received a larger endowment may seek to hide this information by investing no more than the amount of the smaller endowment. Within the experiment, we also offer a subset of subjects the option of paying to keep their investment returns secret, allowing us to measure the willingness-to-pay to hide income directly.

We find convincing evidence that women are willing to reduce their expected income to avoid making investment returns observable to their kin. Women receiving the large endowment, who may wish to hide this fact from their family, are 25.4 percent (9.6 percentage points) more likely to invest an amount no larger than the smaller endowment when returns are observable; this is equivalent to a 5.4 percentage point decline in investment level. We find no similar tendency to hide income among men; this is consistent with recent work by Dupas and Robinson (2009), who find evidence that female daily wage earners in western Kenya are more savings constrained than

men in similar occupations. The effect we observe among women appears to be driven primarily by the behavior of female subjects with relatives attending the experiment, who would be able to observe income from the experiment directly. Estimates suggest that these women invest 22.9 percent less when investment income is observable than when it is hidden; they are 41.8 percentage points more likely to invest no more than the amount of the small endowment, suggesting that their strategy is designed to keep the size of their endowment hidden. Impacts are unlikely to be driven by in-laws providing information to husbands, since estimates are similar among the sample of women who have never been married.

Among subjects given the opportunity to pay a randomly-assigned price to keep income hidden, 30 percent of those able to afford the cost of hiding income choose to do so. These subjects pay and average of 15 percent of their gross payout from the experiment. At the village level, the tendency to hide income within the experiment is negatively associated with business ownership, employment, and asset accumulation, suggesting that pressures to share may be an obstacle to development.

The rest of this paper is organized as follows. Section 2 describes our experimental design and procedures. Section 4 presents our main empirical results. Section 5 concludes.

2 Experimental Design and Procedures

2.1 Structure of the Experiment

The experiment was designed to introduce exogenous variation in the observability of investment returns. Within the experiment, each participant was given an initial endowment, either 80 or 180 Kenyan shillings.[2] Each subject divided her endowment between a zero-risk, zero-interest savings account and an investment which was risky but potentially profitable. The participant received five times the amount that she chose to invest in the risky prospect with probability one half, and lost the amount invested otherwise. A coin was flipped to determine whether each risky investment

[2]The endowments were equivalent to 1.04 and 2.34 U.S. dollars, respectively, at the time of the experiment. Among our subjects, the median daily wage for individuals in full-time, unskilled employment is 100 shillings, and the median daily wage for subjects in full-time work in the skilled sector is 225 shillings.

was successful. Thus, the main decision subjects faced was how much of their endowment to invest in the risky security and how much to allocate to the secure, zero-profit alternative.

Within the experiment, players were randomly assigned to one of six treatments. First, players were allocated either the smaller endowment of 80 shillings or the larger endowment of 180 shillings. Endowment sizes were always private information — experimenters never identified which subjects received the larger endowment. However, the distribution of endowments was common knowledge, so all subjects were aware that half the participants received an extra hundred shillings. Those who received this larger amount could choose whether to invest in a manner which kept their endowment size hidden, for example, by investing 80 shillings or less.

Every player was also assigned to either the **private** treatment, the **public–mandatory** treatment, or the **public–price** treatment. Participants assigned to the **private** treatment were able to keep their investment income secret: none of the decisions they made in the experiment were ever disclosed to other participants. In contrast, those assigned to the **public–mandatory** treatment were required to make an announcement revealing how much they had invested in the risky security, and whether their investment was successful, to all of the other participants at the end of the experiment.[3] Finally, those assigned to the **public–price** condition were obliged to make the public announcement revealing investment returns, but were given the option of paying a price, p, to avoid making the announcement. Prices ranged from ten to 60 shillings, and were randomly assigned to subjects in the public–price treatment. Subjects were informed what price they faced *before* making their investment decisions, but decided whether to pay the price *after* investment returns were realized. Hence, subjects in the public–price treatment were not always able to afford to buy out: those who invested and lost a large fraction of their endowment did not always have enough experimental income left over to pay the exit price, p.[4]

Random assignment to treatment generated exogenous variation in the observability of investment returns and created costly opportunities to hide income. Assignment to the public treatments meant that outcomes were verifiable, and might therefore facilitate risk-pooling and, consequently,

[3]Subjects were informed up front that they were allowed to delegate the task of making the public announcement to a member of the research team if they wished to avoid the public speaking aspect of the announcement process.

[4]Subjects were never allowed to use money from outside the experiment to pay to avoid the public announcement.

risk-taking. On the other hand, if subjects face social pressure to share with community members, they may reduce their level of investment when returns are visible. In particular, the experiment creates two mechanisms through which subjects could pay a price to hide income from others. First, subjects in the public–price treatment could pay the randomly-chosen price, p, to conceal their investment decision. Second, subjects receiving the larger endowment could keep that income secret by investing no more than 80 shillings.

2.2 Experimental Procedures

Experiments were conducted in 26 rural, predominantly agricultural communities in western Kenya.[5] One day prior to each experimental session, the survey team conducted a door-to-door recruitment campaign, visiting as many households within the village as possible.[6] All households within each village were invited to send members to participate in the experimental economic game session the following day. 80.4 percent of households contacted prior to the sessions chose to participate.[7] Experimental sessions were conducted in empty classrooms at local primary schools. Sessions included an average of 83 subjects; no session included fewer than 65 or more than 100 participants. Each session lasted approximately three hours.

Within each session, participants were stratified by gender and education level (an indicator for having done any post-primary education). There were six experimental treatments:

$$\{\text{small budget}, \text{large budget}\} \times \{\text{private}, \text{public–mandatory}, \text{public–price}\}.$$

Within each stratum, players were randomly assigned to each of the six treatments with equal

[5] Communities were selected to be at least five kilometers apart from one another, to prevent overlap in subject populations, and to avoid areas where IPA–Kenya had ongoing projects.

[6] When the recruitment team was unable to contact and survey a household prior to the experiment, village elders were asked to invite them to attend the subsequent experimental session.

[7] In Appendix Table A13, we examine the correlates of choosing to send a household member to the experimental session. Participants are broadly representative of the overall population, though larger households are more likely to send a member to the experiment. Women are more likely to attend if they are from poorer households or if they have relatively higher math skills. Among men, both the set of age category indicators and the set of education category indicators are jointly significant. Subjects in more isolated communities — as measured by Euclidean distance from a paved road — are more likely to participate, though this cannot bias our results since assignment to treatment occurs within villages.

probability. Players assigned to the payout treatments were subsequently assigned a random "exit price" from the set of multiples of ten between ten and 60. Subjects were also assigned individual ID numbers, which were worn throughout the experiment and used to record individual decisions.

Experimental sessions were structured as follows. After a brief introduction, enumerators read the instructions and answered participant questions, illustrating the decisions that a subject might face with a series of wall posters. Subjects were then called outside one at a time, by ID number, to make their investment decisions. Since some participants had limited literacy skills, decisions were recorded by members of the research team. To ensure that earnings not announced publicly remained private information, each enumerator sat at a desk in an otherwise empty section of the schoolyard. Enumerators began by asking a series of questions designed to test whether a subject understood the experiment. Subjects were then informed whether they had received the large or small endowment and whether they were assigned to the private, public-mandatory, or public-price treatment. Those who were assigned to the public-price treatment were also told what price they would need to pay if they wished to avoid the public announcement. Subjects then made their investment decisions: each was handed a number of ten shilling coins equivalent to her endowment; the participant divided these coins between a "savings" cup and a "business" cup. After recording a subject's investment decision, the enumerator would give the subject a one shilling coin to flip.[8] The outcome of the coin flip determined whether the money placed in the business cup was multiplied by five or removed from the subject's final payout. Subjects assigned to the public-price treatment were then asked whether they wanted to pay the fee to avoid announcing their investment results. If they had enough money left to pay the fee, and they chose to do so, it was deducted from their payout. After all decisions had been recorded, public announcements were made. At the end of the session, subjects were called outside one at a time to receive their payouts. Each subject received her payout in private, and was allowed to leave directly after receiving her money. Figure 1 summarizes the progression of activities within the experiment.

[8]To limit the possibility of influencing the outcome of the coin flip, each subject placed the coin into a sealed, opaque container which she shook vigorously before opening it to reveal the outcome of the coin toss.

2.3 Experimental Subjects

Sessions were conducted in Kenya's Western Province, in three adjoining districts: Bunyala, Samia, and Butula.[9] All three districts are predominantly smallholder farming communities, though Samia and Bunyala also have ports on Lake Victoria. Summary statistics on experimental subjects are presented in Table 1. 60 percent of subjects were female. Respondents ranged in age from 18 to 88. 9.4 percent of subjects had no formal schooling, while 12.2 percent had finished secondary school. The median participant was a 34 year old married woman with seven years of education, living in a six-person household with her husband and her four children. The median participant's household owns one bicycle, one cell phone, four chickens, and two mosquito nets, but does not own a television, any cattle or goats, or a motor vehicle. 23.0 percent of respondents live in households with at least one employed household member; most (64.6 percent) employed subjects do agricultural work or other unskilled labor. The median monthly wage among participants with regular employment was 1500 Kenyan shillings, or just under one dollar a day (assuming twenty work days per month). 35.0 percent of subjects operate their own business enterprise; among these businesses, only 11.6 percent have one or more employees. 16.7 percent of participants have bank savings accounts,[10] and 52.8 percent are members of rotating savings and credit associations (ROSCAs).[11]

Most experimental subjects in our sample live amongst their kin, and are embedded in inter-household transfer networks. The median participant has two close relatives (parents, parents-in-law, adult children, grandparents, siblings, aunts, and uncles) living outside her household but in the same village, plus another five more distant relatives. 43.8 percent of subjects had received a transfer in the last three months, while 89.8 percent report making a transfer to another household

[9] Kenya's recent redistricting carved these three former administrative divisions of Busia District off as new districts of their own. One of the districts, Butula, was declared a new district during the course of this project.

[10] Dupas and Robinson (2009) found that less than 3 percent of the daily wage earners sampled in Bumala, Kenya, had savings accounts. While Bumala is just a few kilometers from the region where the present study took place, their data were collected over two years before our household survey. The daily wage earners (primarily market vendors and bicycle taxi drivers) included in their study may also be somewhat worse off than our subjects.

[11] Gugerty (2007) surveys ROSCA participants in Busia and Teso Districts in western Kenya; she argues that the social component of ROSCA participation helps individuals overcome savings constraints. Anderson and Baland (2002) show income-earning women living in Nairobi slums use ROSCAs to protect their savings from their husbands. Dupas and Robinson (2009) show that female daily income earners make more productive investments when given access to even a costly savings account.

over the same period. The median household making a transfer had given 363 shillings, while the median household receiving a transfer had been given 600 shillings. In the three months prior to being surveyed, 42.4 percent of subjects' households had been asked for a gift or loan, and 89.7 percent of households had contributed money to a "harambee," a local fundraising drive.[12]

Summary statistics on the distribution of participant characteristics across experimental treatments are reported in Table 2.[13] The randomization was successful, generating minimal differences in observables across treatments. Of 23 variables reported, only the number of distant family members living in one's village differs significantly across treatments. This variation is clearly driven by outliers: the maximum number (within a treatment) of distant relatives reported to live in ones village ranges from 96 to 199. A quantile regression of the median number of distant family members on the set of treatment dummies does not find significant differences across experimental treatments (results not shown).

3 Theoretical Framework

In this section, we outline a simple theoretical model of individual decisions within the experiment. In all experimental treatments, individual i divides her budget of $m_i \in \{m_s, m_l\}$ between the business cup (a risky security) and the savings cup (a risk-free but zero return savings technology). When income is private, i seeks to maximize

$$\begin{aligned} E\left[u_i(b_i, s_i|m_i)\right] &= \frac{1}{2}u_i\left(s_i + c_{i0}\right) + \frac{1}{2}u_i\left(s_i + 5b_i + c_{i0}\right) \\ &= \frac{1}{2}u_i\left(m_i - b_i + c_{i0}\right) + \frac{1}{2}u_i\left(m_i + 4b_i + c_{i0}\right), \end{aligned} \tag{1}$$

where b_i is the amount invested in the risky security, $s_i = m_i - b_i$ is the amount saved, and c_{i0} is background consumption or permanent income. Following Ashraf (2009) and Goldberg (2010), we assume income is observable whenever an individual is known to have received it with probability

[12] A *harambee*, pronounced *hah-rahm-bay* is a self-help effort in which community members contribute money or resources to assist a particular person in need. They may be for sending a child to school, paying for a wedding, or any number of other purposes. The concept existed within a number of different tribal groups in Kenya, but was made into a national rallying cry by Kenya's first president, Jomo Kenyatta (Ngau 1987).

[13] We show that the randomization of the price of avoiding making an announcement was successful, though the randomization was not stratified, in Table 8.

one; income is unobservable whenever a person can plausibly deny having received it. In the private information treatments, a subject can claim to have invested her income and lost it, limiting the potential for social pressure to share payouts. Thus, individual i's optimal interior solution in the private treatment, b_i^{pri}, solves

$$u_i'\left(m_i - b_i^{pri} + c_{i0}\right) = 4u_i'\left(m_i + 4b_i^{pri} + c_{i0}\right). \tag{2}$$

In what follows, we assume that individual preferences can be represented by a utility function of the CRRA form with parameter $\rho_i \geq 0$ and that background consumption, c_{i0}, is equal to zero. The scale invariance property of the CRRA utility function is consistent with aggregate data from the private information treatments: subjects allocate an average of 51.8 percent of their budgets to the business cup when they receive the smaller endowment, and an average of 51.9 percent of their budgets to the business cup when they receive the larger endowment.[14]

Given these two assumptions, the amount invested in the business cup by individual i in the private information treatment is given by

$$b_i^{pri} = \left(\frac{4^{1/\rho_i} - 1}{4^{1/\rho_i} + 4}\right) m_i. \tag{3}$$

Thus, when individual risk preferences can be represented by a utility function of the CRRA form, the proportion of the budget invested depends on ρ_i, but not on the size of the budget. When ρ_i is close to zero, an individual will invest almost all of her budget in the risky prospect; the proportion of m_i invested decreases with ρ_i.

To see how individual i's optimization problem changes when she is obliged to stand up and announce her investment income, we assume that the public announcement impacts utility through two channels. First, some individuals prefer to avoid public attention, while others may enjoy such attention. Let $\kappa_i \in \Re$ denote the utility cost of making *any* public announcement; we assume this is

[14] A Mann-Whitney test fails to reject the null hypothesis (p-value 0.865) that the fraction of the budget invested does not depend on the budget size. There is, however, suggestive evidence that women invest a slightly larger fraction of their budgets when they receive the larger endowment (53.5 versus 50.7 percent invested) while men invest slightly less (49.3 versus 53.6 percent). Since these differences are quite small in magnitude, we focus on the CRRA case.

generally negative but may be positive for some individuals, and that this utility cost is additively separable from consumption utility. Individual i's total utility is given by

$$U_i = u_i(c_i) - \kappa_i. \tag{4}$$

Second, individuals may face social pressure to share observable income. Following Ashraf (2009) and Goldberg (2010), we assume that individual i is obliged to transfer a proportion, τ_i, of observable income to members of her social network — for example, her spouse or her relatives — and that income is observable when an individual is known to have it with probability one, whether it is announced or not. For example, if each villager receives a grant of one thousand shillings from an aid agency, then that income is observable even if it is not distributed publicly. Within the experiment, the implication is that an individual assigned to the public treatment who receives the smaller endowment, m_s, cannot hide any of her income, since every subject is known to have received at least m_s and she is forced to announce whether her investment succeeded. In contrast, an individual receiving $m_l = m_s + d$ can choose to invest $b \leq m_s$, thereby making d shillings of income unobservable.

Consider the decision problem facing a subject assigned to the public treatment receiving the smaller endowment, m_s. She chooses $b \leq m_s$ such that

$$b^* = \text{argmax}_{b_i \leq m_s} \frac{1}{2}\frac{[(1-\tau_i)(m_s - b_i)]^{1-\rho_i}}{1-\rho_i} + \frac{1}{2}\frac{[(1-\tau_i)(m_s + 4b_i)]^{1-\rho_i}}{1-\rho_i} - \kappa_i \tag{5}$$

Since the $1 - \tau_i$ and the κ_i terms drops out of the first-order condition characterizing the optimal interior solution, individuals receiving the smaller endowment make the same allocation decisions regardless of whether investment returns are public or private. Proposition 1 characterizes individual behavior and welfare for subjects receiving the smaller endowment in the public treatment.

Proposition 1. *If individual i is receives the smaller endowment, m_s, then her optimal investment in the business cup in the public treatment is equal to her optimal investment when she is assigned*

to the private treatment:

$$b_i^{pub}(m_s) = b_i^{pri}(m_s) = \left(\frac{4^{1/\rho_i} - 1}{4^{1/\rho_i} + 4}\right) m_s.$$

For $\tau_i > 0$ and $\kappa_i \leq 0$, expected utility is lower in the public treatment than in the private treatment, and is decreasing in τ_i.

Subjects who receive the larger endowment have the option of investing an amount less than or equal to the smaller endowment, thereby creating "plausible deniability" and making themselves indistinguishable from those who received the smaller endowment. The discrete change in observable income means that both utility and marginal utility are discontinuous at $b_i = m_s$. An individual who invests $b_i > m_s$ in the public treatment will choose

$$\begin{aligned} b_i^* &= \text{argmax}_{b_i > m_s} \frac{1}{2}\frac{\left[(1-\tau_i)(m_s - b_i + d)\right]^{1-\rho_i}}{1-\rho_i} + \frac{1}{2}\frac{\left[(1-\tau_i)(m_s + 4b_i + d)\right]^{1-\rho_i}}{1-\rho_i} - \kappa_i \\ &= \left(\frac{4^{1/\rho_i} - 1}{4^{1/\rho_i} + 4}\right)(m_s + d), \end{aligned} \tag{6}$$

the same budget share she would have invested in the private information treatment. However, since utility is lower in the presence of social pressure to share observable income, such individuals may prefer to set $b_i = m_s$. Proposition 2 characterizes this threshold.

Proposition 2. *There exists a threshold risk aversion parameter, $\underline{\rho}$, such that if $\rho_i \leq \underline{\rho}$, then:*

1. *i's optimal investment in the private, large endowment treatment, $b_i^{pri}(m_l)$, exceeds m_s;*

2. *i's optimal investment in the public, large endowment treatment is*

$$b_i^{pub}(m_l) \in \left\{m_s, b_i^{pri}(m_l)\right\}; \tag{7}$$

3. *there exists $\bar{\tau}(\rho_i) \in (0,1)$ such that $b_i^{pub}(m_l) = m_s$ if and only if $\tau_i \geq \bar{\tau}(\rho_i)$.*

Proposition 2 demonstrates that individuals with sufficiently low levels of risk aversion, who would invest more than m_s in the large endowment, private information treatment, will either do the same in the public treatment or will invest exactly m_s, the highest level of investment which

keeps the size of one's endowment hidden. Whether the latter strategy is optimal depends on the extent of social pressure one anticipates.

In contrast, an individual who invests $b < m_s$ will choose

$$\begin{aligned} b^* &= \text{argmax}_{b<m_s} \frac{1}{2}\frac{\left[(1-\tau_i)(m_s-b)+d\right]^{1-\rho_i}}{1-\rho_i} + \frac{1}{2}\frac{\left[(1-\tau_i)(m_s+4b)+d\right]^{1-\rho_i}}{1-\rho_i} - \kappa_i \\ &= \left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right) m_s + \frac{\left(4^{1/\rho_i}-1\right)d}{(1-\tau_i)\left(4^{1/\rho_i}+4\right)}. \end{aligned} \tag{8}$$

Interestingly, since $0 < 1-\tau < 1$,

$$\left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right)(m_s+d) < \left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right) m_s + \frac{\left(4^{1/\rho_i}-1\right)d}{(1-\tau_i)\left(4^{1/\rho_i}+4\right)}; \tag{9}$$

the implication is that sufficiently risk averse individuals, who would invest less than m_s in the private treatment, will invest *more* when investment returns are observable than when they are private. Proposition 3 characterizes this pattern of behavior.

Proposition 3. *For all $\tau \in (0,1)$, there exists a threshold risk aversion parameter, $\bar{\rho}(\tau)$ such that:*

1. *$\underline{\rho} < \bar{\rho}(\tau)$;*
2. *i's optimal investment in the public, large endowment treatment, $b_i^{pub}(m_l)$, satisfies*

$$b_i^{pub}(m_l) \in \left(b_i^{pri}(m_l), m_s\right) \tag{10}$$

   if and only if $\rho_i > \bar{\rho}(\tau_i)$;

3. *otherwise, $b_i^{pub} = m_s$ if $\underline{\rho} < \rho_i \leq \bar{\rho}(\tau_i)$.*

4 Results

4.1 Individual Investment Decisions

Summary statistics on outcomes in the experiment are presented in Table 3. Panel A describes the key experimental outcomes of interest: the amount invested (i.e. put in the business cup rather than

the savings cup), which is subsequently announced in the public treatments, and the willingness to pay to avoid making a public announcement. On average, participants chose to invest just over half their endowment in the risky prospect. There is no evidence that subjects invest a larger fraction of their endowment when they receive the larger budget of 180 shillings.

Among those with the larger endowment, the amount invested is slightly lower, on average, in the public treatments than in the private treatment. When allotted 180 shillings, participants could avoid publicly revealing that their endowment exceeded that of others by investing 80 shillings or less; the frequency of this choice is tabulated in the third row of Table 3, and is higher in the public treatments than in the private treatment.

In the public–price treatment, the price of avoiding the public announcement ranged from ten to 60 shillings, yielding a mean exit price of 35 shillings. This price was, on average, 61.5 percent of participants' gross payout in the low endowment condition, and 22.2 percent in the high endowment condition. 21 percent of participants in the small endowment condition pay the price to avoid announcing, as do 34 percent in the large endowment condition. The mean accepted price is 20.1 percent of the gross payout in the low endowment condition and 12.5 percent in the high endowment condition.

Average final payouts are tabulated in the last row of Panel A. Payouts are higher in the private treatments than in the analogous public treatments. On average, subjects took home 240.6 shillings, which was equivalent to 3.04 US dollars at the time of the experiment.

In Panel B of Table 3, we break down mean investment amount by gender, education level, and whether a subject's relatives attended the experiment. There is no evidence of differences in behavior between the public and private treatments among subjects who received the smaller endowment. This is unsurprising, since these subjects cannot obscure the size of their budget by altering their investment strategy. However, the data clearly suggest that women, more educated subjects, and those with relatives present invest less in the public treatments than in the private condition when they receive the larger endowment.

We begin our regression analysis by considering the main effects of the experimental treatments on investment level, as measured by the amount of money allocated to the business cup. We

estimate the OLS regression specification

$$Investment_{ivt} = \alpha + \beta Public_i + \gamma Large_i + \delta Public \times Large_i + \eta_v + X_i'\zeta + \varepsilon_{ivt} \qquad (11)$$

where $Investment_{ivt}$ is the amount invested in the risky security by subject i in village v who was assigned to experimental treatment t, $Public_i$ is an indicator for assignment to one of the public treatments, $Large_i$ is an indicator for receiving the larger endowment, $Public \times Large_i$ is an interaction between assignment to the public treatments and receiving the large endowment, η_v is a village fixed effect, and X_i' is a vector of individual controls including dummies for age and education categories, gender, marital status, household size, and the log value of household assets. Results are reported in Table 4. Odd-numbered columns report regression results without controls; even-numbered columns include them. Not surprisingly, receiving the larger endowment has a substantial positive impact on the amount invested.[15] In Columns 5 through 8, we split the sample by gender. Women appear to respond more to receiving the larger endowment: the coefficient on *Large* suggests that men invest 45.8 shillings more, on average, when their endowment is 180 shillings instead of 80 shillings, while women receiving the large endowment invest 55.8 shillings more in the risky security. Being married and having greater household assets predict increased investment within the experiment, though neither is consistently significant in all specifications.

When the $Public \times Large$ interaction is omitted, the coefficient on the *Public* dummy is negative, but not significant (Columns 1 and 2). Similarly, the overall effect of assignment to the public treatments is negative (and larger) but insignificant among those receiving the large endowment (Columns 3 and 4). The interaction between assignment to a public treatment and receiving the large budget is negative and significant among women (Columns 7 and 8), but positive and insignificant among men (Columns 5 and 6). The coefficient estimates suggests that women receiving the large budget invest 6.5 percent less when returns are observable than when they are hidden.

The results suggest thus far suggest that neither women nor men reduce their investment level when returns are observable unless their is an opportunity to hide income: the coefficient on the

[15] We focus on the amount invested, rather than the fraction of the budget invested, to obviate assumptions about the nature of utility functions.

Public is positive and insignificant once the interaction with the large budget indicator is included, demonstrating that it is only women receiving the 180 shilling endowment who appear to invest less in the public treatments. This suggests that women may be hiding their larger endowments by investing 80 shillings or less. To test this hypothesis, we estimate probit regressions where

$$\Pr\left[Investment \leq 80\right] = \Phi\left(\alpha + \beta Public_i + X_i'\zeta + \epsilon_{ivt}\right) \tag{12}$$

among the sample randomly assigned to the large endowment treatments, as well as a linear probability model (LPM) where the outcome is an indicator for investing 80 shillings or less (Table 5). In both specifications, women are significantly more likely to invest no more than 80 shillings in the public treatments: both the probit and LPM estimates indicate that women are ten percentage points more likely to invest 80 shillings or less when investment returns are observable than when they are private information. The coefficient on *Public* in the sample of men is not significant in any specification, in line with our previous results.

Our hypothesis is that women face social pressure to share income, and this creates an incentive to hide investment returns when possible, even if it is costly to do so. If this hypothesis is true, then the extent of income hiding should be associated with factors predicting the level of social pressure an individual is likely to face after the experiment. We focus on close kin as the group most likely to pressure individuals into sharing income.[16] We test this by creating an indicator for whether a subject's close kin attended the experiment, and interacting these with assignment to the public treatment.[17] We restrict the sample to those in the large endowment treatments, who have the opportunity to hide the size of their endowment by reducing they amount they invest. We estimate the OLS regression

$$Investment_{ivt} = \alpha + \beta Public_i + \lambda KinPresent_i + \phi Public \times KinPresent_i + \eta_v + X_i'\zeta + \varepsilon_{ivt}. \tag{13}$$

Among women, the interaction between having kin attending the experiment and assignment to

[16] Hoff and Sen (2006) highlight the role played by kin networks in extracting surplus from successful relatives, while **?** provide evidence that individuals seek to hide income from family members.

[17] We define close kin as parents, grandparents, siblings, grown children, and aunts and uncles.

the public treatment is consistently negative and significant(Table 6, Panel A). The coefficient estimate is extremely large, suggesting that women with relatives present invest 21.4 shillings less when investment returns are observable than when they are hidden, which is equivalent to more than a twenty percent reduction investment relative to the private information treatment. After including the kin variables in the regression, the point estimate for the coefficient on *Public* remains negative, but is no longer statistically significant, suggesting that much of the "income hiding" is being done by women with relatives at the game who would observe investment returns directly.

An alternative hypothesis is that kin are significant because they pass information about wives' incomes to their husbands.[18] We present two pieces of evidence which suggest that this is not case. First, among the small set of never-married and previously married women who have relatives attending the game, we observe substantially higher levels of investment in the private treatment than in the public treatments. These women invest an average of 95.7 shillings in the private treatment versus 84.0 shillings in the public treatments. The difference in investment levels across treatments is not statistically significant in this sub-sample, but that is due to the fact that the vast majority of female subjects are married: only twelve unmarried women in the large endowment treatments have close relatives attending the experiment. We also estimate Equation 13 including an additional control for whether one's spouse attended the experiment, and an interaction between that and the *Public* dummy. In Column 7 of Table 6, we omit the kin variables to focus on the impact of having one's spouse in attendance. The coefficient on having the $Public \times SpousePresent$ interaction is negative but not significant, and is substantially smaller in magnitude than the analogous kin variable. In Column 8, we include both the kin and spouse attendance indicators, plus the interactions between these and assignment to the public treatments. The coefficients on all four variables are similar in magnitude and significance to the estimated coefficients when only one kin or spouse variables are included. Specifically, the interaction between having close relatives attending the experiment and assignment to the public treatment remains negative and statistically significant after controlling for attendance by one's husband. As in the simpler specification, the estimated coefficient suggests more than a twenty percent reduction in the amount invested when

[18] Robinson (2008) and Ashraf (2009) document limited commitment and observability effects *within* the household, while Anderson and Baland (2002) argue that Kenyan women use ROSCAs to hide savings from their husbands.

returns are observable.

Among men, the coefficient on $Public \times KinPresent$ is negative, but not statistically significant. Interestingly, the $Public \times SpousePresent$ is large, negative, and marginally significant in one of two specifications. For both men and women, the all coefficients on the interactions between assignment to the public treatments and having kin or a spouse at the experiment are negative.

We test whether the patterns of investment reduction among women with kin attending the experiment are consistent with income hiding by re-estimating Equation 13 with the indicator for investing 80 shillings or less as the dependent variable (Table 6, Panel B). It is apparent that the reduction in investment by women with kin present is consistent with income hiding: the coefficient on the $Public \times KinPresent_i$ variable suggests that the probability of investing no more than the small endowment amount is 35.3 percentage points higher in the public treatments than in the private information condition.[19] Among men, having one's kin or spouse present is not significantly associated with income hiding in any specification, though the point estimates on the spouse variable suggests a large change in the probability of investing 80 shillings or less.

4.2 Income Hiding across Villages

Next, we examine the association between the level of income hiding within a community and village-level outcomes. For each community, we create a measure of income hiding among men and among women within the experiment. The "public-private investment difference" is the difference between the amount invested by those in the private, large endowment treatment and the amount invested in the public, large endowment treatments within a specific gender in a single village. For instance, if all the women assigned to the private, large endowment treatment in village v invested 110 shillings, and all the women in the public treatments invested 105 shillings, the public-private investment difference for women in village v would be five. A positive difference indicates that subjects receiving the large endowment invest less in the public treatment than in the private

[19] Another way to arrive at the same conclusion would be estimate a linear probability model predicting the likelihood of investing 80 shillings or less in the restricted sample of women with relatives attending the experiment. In this sample, the variable of interest is the indicator for random assignment to a Public treatment. The coefficient is positive and significant (p-value < 0.001, results not shown). The magnitude indicates that assignment to the public treatment increases the likelihood of investing 80 shillings or less by 45.5 percentage points.

treatment.[20]

Figure 3 plots the relationship between the public-private investment difference and

In Table 7, we explore the relationship between village outcomes and the propensity to hide income. We estimate OLS regressions of the form

$$Y_v = \alpha + \beta Hiding_{gv} + \gamma_g + X_v'\zeta + \varepsilon_v \tag{14}$$

where Y_v is the first principal component of a set of village income indicators (discussed below), $Hiding_{gv}$ is the three of income hiding by gender g in village v, γ_g is a gender ("hiding by women") fixed effect, and X_v is a vector of village level controls. We construct our measure of village income by taking the first principal component of the following: the proportion of men (attending the experiment) who are employed in a skilled or professional sector job, the proportion of men who own a business with at least one employee, and the average of the log of household assets. Village level controls, which are included in even-numbered columns in Table 7, are the average level of educational attainment by subjects, the average number of close kin living in the village, ethnolinguistic fractionalization, the average number of community groups to which subjects belong, and the Euclidean distance to the nearest paved road, in kilometers.

Given the small sample size, not all coefficients of interest are statistically significant. However, the broad patterns are highly suggestive. When hiding by both men and women are included in a single regression, generating a sample of $n = 52$, the coefficient on income hiding is negative and significant with and without additional controls. When we separate the sample by gender (Columns 3–6), we see that income hiding by both men and women is negatively related with village level outcomes, though the coefficients are not significant in all specifications. This suggests that the willingness to pay to keep investment returns secret which we observe in the experiment is negatively associated with village-level development, and is consistent with the hypothesis that

[20] An alternative would be to measure whether subjects receiving the large endowment are less likely to invest more than 80 shillings when investment returns are observable. We define the "public-private hiding difference" as the difference between the proportion investing 80 shillings or less in the public treatments and the proportion in the private treatments and the "public-private hiding ratio" as the ratio of these two proportions. Results are similar when either of these measures is used, though the latter is not defined for three village-gender observations where subjects in the private treatment never invest 80 shillings or less.

pressure to share income — which we detect using the experiment — discourages investment and labor supply. However, in the non-experimental setting, we cannot be sure of the direction of causality: pressures to share income, which drives income hiding in the experiment, may simply be stronger in poorer communities.

4.3 The Willingness-to-Pay to Hide Income

Our final set of empirical results relate to the willingness-to-pay to avoid the public announcement. As discussed in Section 2, one third of subjects were randomly assigned to the public–price treatment. These subjects were offered the option of "buying out" of announcing their investment income to the other subjects. Subjects assigned to the price treatments were randomly assigned an exit price from the set: 10, 20, 30, 40, 50, and 60 shillings. Table 8 reports summary statistics in the price treatments, broken down by exit price. Though the randomization was not stratified, it was largely successful: the treatment dummies for prices 20 through 60 are only jointly significant for two of the 22 variables reported, and differences in fertilizer use are only significant at the 90 percent level.

Of 627 subjects who are able to afford to buy out of the public announcement, 30.3 percent chose to do so, paying an average price of 29.3 shillings — equivalent to 15.3 percent of earnings for those buying out. This suggests that the willingness-to-pay to keep income hidden is substantial. Figure 2 plots the proportion of subjects paying to avoid the public announcement, broken down by endowment size and exit price. Within each endowment size, subjects are clearly more likely to buy out at lower prices, though the impact of price appears more pronounced among those assigned to the low endowment condition.

Table 9 reports OLS regressions of the probability of paying to avoid the public announcement on exogenous factors: the price of buying out, receiving the large endowment, having the coin land on heads, and interaction terms. Subjects with more observable income are clearly more likely to pay to avoid the public announcement: the coefficient on $Heads$ is consistently positive and significant, suggesting that subjects with successful investments are 17 percentage points more likely to pay to avoid making the public announcement. It is also clear that subjects are less

inclined to buy out at higher exit prices. In both cases, results are similar when the sample is restricted to those able to afford the exit price.

Analysis of factors associated with willingness-to-pay is complicated by the fact that, while prices are randomly assigned, gross payouts (savings plus investment returns) are not. Even among those randomly assigned the smaller endowment who saw their investment income disappear, 26.3 percent of men and 14.1 percent of women choose to pay to avoid the public announcement, suggesting that a desire to hide income may not be the only reason subjects pay the exit price. Controlling for potential wealth effects is complicated by the fact that gross payouts are not independent of randomly-assigned exit prices or individual characteristics such as risk preferences. Table 10 reports the results of regressions of amount invested within the experiment on the price of exit, restricting the sample to those randomly assigned to the public–price treatments. Random assignment to a higher price of avoiding the public announcement is associated with significantly lower levels of investment. In contrast to our previous results, this appears is true for both men (Columns 5 and 6) and women (Columns 7 and 8). Finally, 16.8 percent of subjects in the low endowment group and 1.4 percent of subjects in the high endowment group are unable to afford to pay the exit price because their investments failed, and these excluded individuals are not randomly selected from the subject pool.

Thus, results relating individual characteristics to the propensity to pay to avoid the public announcement should be viewed with caution, but we report them nonetheless. Table 11 reports the results of OLS regressions of an indicator for buying out on kin variables, broken down by gender and budget size. For both men and women, the interaction between *Heads* and having either one's kin or one's spouse is consistently positive, but not significant. In Table 12, the sample is restricted to subjects with successful investments. Again, coefficients are not statistically significant, but point estimates suggest that women with kin or a spouse present are more likely to buy out than other women with observable investment income.

5 Conclusions

We report the results of a novel economic experiment designed to measure the impact of social pressures to share on investment incentives in Kenyan villages. Participants who know that the outcome of their investments will be made public choose decisions that are less profitable in expectation. Results are strongest for those who have relatives present at the experiment. When we offer some participants the opportunity to pay a fee to avoid making an announcement, they do so at substantial cost: 15 percent of their gross payout, on average. This suggests that the effective informal tax rate in the village is even higher.

We hypothesize that the behavior observed in this experiment is a sign that village sharing norms distort investment incentives towards less visible, but potentially less profitable, investments, and may consequently slow economic growth. The negative correlations we observe between the extent of "hiding" at the village level and the level of prosperity in the village, measured several different ways, are in agreement with this interpretation. However, such results should be interpreted with caution, since the direction of causality is unclear. Moreover, the efficiency impacts of social pressure to share income will clearly depend on the range of income-hiding technologies available.

Studies of mutual insurance typically assumed that transfer arrangements are on the efficient frontier, though the analogous assumption has been questioned in intrahousehold bargaining contexts. Our work suggests that relationships with close kin outside the household may be similar to within-household interactions, and that social sanctions which encourage cooperation and sharing may also have disincentive effects.

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Figure 1: Structure of Experiment

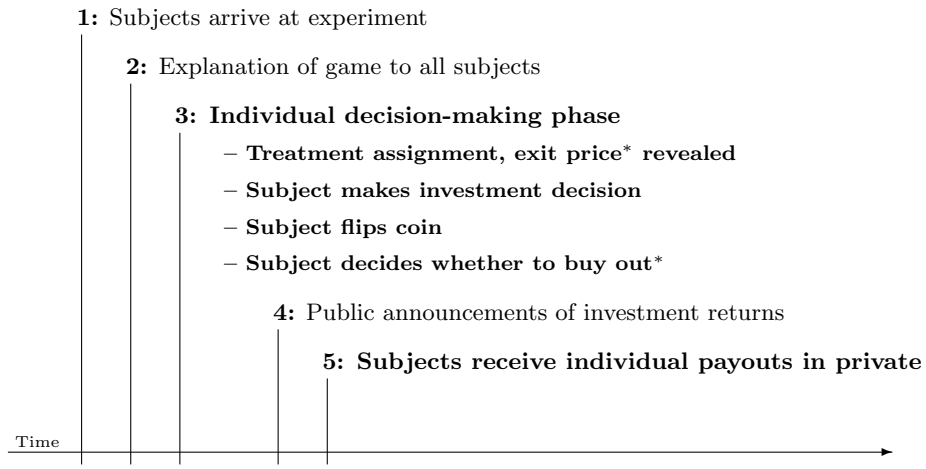

Figure 2: Proportion of Subjects Paying to Avoid Announcing

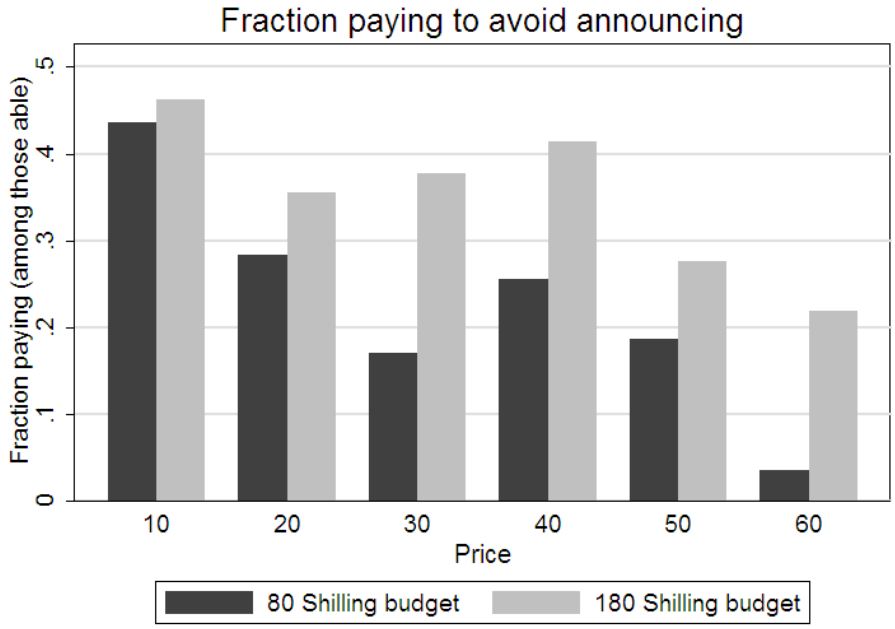

Figure 3: Relationship between Income Hiding in Experiment and Village Outcomes

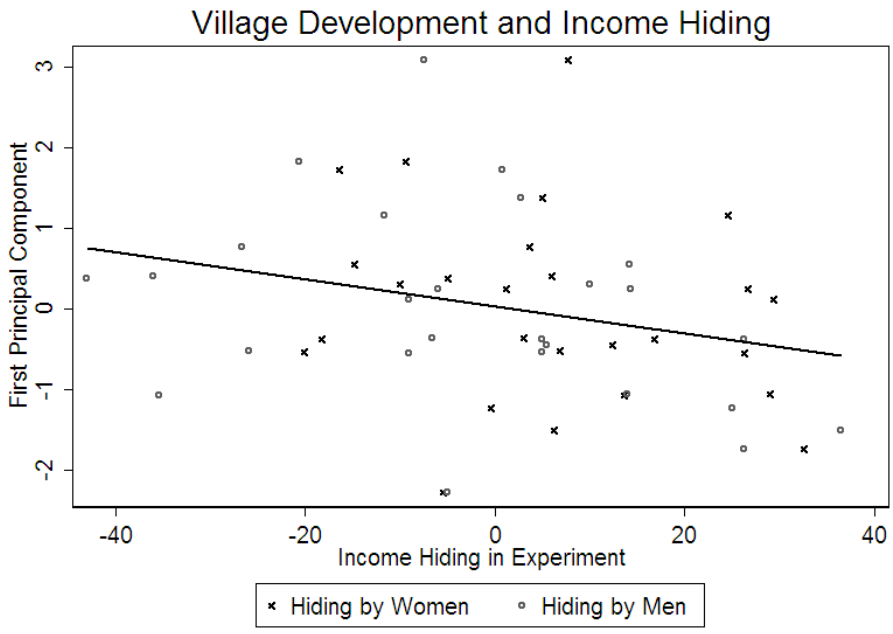

Table 1: Summary Statistics on Experimental Subjects

Variable	Mean	S.D.	Median	Min	Max	N
Female	0.61	0.49	1	0	1	2146
Years of schooling	6.74	3.37	7	0	16	2146
Age	36.82	14.27	34	18	88	2128
Currently married	0.76	0.42	1	0	1	2146
Ever married	0.88	0.32	1	0	1	2146
No. chicken owned by HH	6.41	7.19	4	0	40	2146
No. cattle owned by HH	1.20	2.08	0	0	36	2145
No. bicycles owned by HH	0.83	0.76	1	0	6	2146
No. phones owned by HH	0.73	0.82	1	0	6	2146
No. televisions owned by HH	0.14	0.39	0	0	3	2146
No. mosquito nets owned by HH	2.13	1.45	2	0	11	2146
Value of HH assets (in Kenyan shillings)	35624.98	49717.77	27125	1000	1722600	2146
HH size	6.18	2.82	6	1	26	2146
Close relatives in village (outside of HH)	2.36	2.57	2	0	19	2146
Distant relatives in village	10.41	16.12	5	0	199	2146
Close relatives attending experiment	0.19	0.39	0	0	1	2146
HH farms	0.99	0.12	1	0	1	2146
HH uses fertilizer on crops	0.46	0.50	0	0	1	2115
Has regular employment	0.09	0.28	0	0	1	2146
Monthly wages (if employed)	2976.17	4661.84	1500	100	33000	179
Any HH member employed	0.23	0.42	0	0	1	2146
Self-employed	0.35	0.48	0	0	1	2146
Has bank savings account	0.17	0.37	0	0	1	2143
Member of ROSCA	0.53	0.50	1	0	1	2143
Community groups	2.76	1.87	3	0	10	2146
Belongs to Luhya ethnic group	0.80	0.40	1	0	1	2146
Belongs to Teso ethnic group	0.08	0.28	0	0	1	2146
Belongs to Luo ethnic group	0.11	0.31	0	0	1	2146
Christian	0.98	0.14	1	0	1	2146
Attended church last week	0.67	0.47	1	0	1	2146

Table 2: Summary Statistics by Experimental Treatment

Treatment:	Private	Public	Payment	Private	Public	Payment	
Budget Size:	Small	Small	Small	Large	Large	Large	F-Stat
Proportion female	0.61	0.60	0.61	0.61	0.61	0.60	0.05
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Years of schooling	6.53	6.87	6.84	6.58	6.71	6.94	0.87
	(0.18)	(0.17)	(0.17)	(0.19)	(0.18)	(0.18)	
Any secondary school	0.40	0.42	0.44	0.44	0.42	0.41	0.28
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Age	37.57	37.65	36.14	36.44	36.94	36.06	0.86
	(0.76)	(0.77)	(0.76)	(0.75)	(0.76)	(0.75)	
Currently married	0.79	0.78	0.76	0.75	0.77	0.74	0.86
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Ever married	0.90	0.91	0.89	0.86	0.89	0.86	1.37
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
HH size	6.42	5.92	6.20	6.03	6.24	6.27	1.46
	(0.15)	(0.14)	(0.16)	(0.15)	(0.14)	(0.15)	
Value of HH assets	35834.28	36954.05	31719.71	39776.12	36134.78	33015.94	1.18
	(2083.41)	(1763.41)	(1347.37)	(4969.81)	(2167.95)	(1591.75)	
Close relatives in village	2.44	2.34	2.53	2.33	2.41	2.13	0.96
	(0.13)	(0.15)	(0.14)	(0.14)	(0.13)	(0.12)	
Distant relatives in village	11.56	8.21	11.18	9.02	11.37	11.24	2.89**
	(0.91)	(0.61)	(0.78)	(0.78)	(1.07)	(0.89)	
HH farms	0.98	0.98	0.99	0.98	0.99	0.99	0.35
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
HH uses fertilizer on crops	0.44	0.45	0.48	0.45	0.44	0.50	0.93
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Any HH member employed	0.22	0.24	0.23	0.21	0.25	0.24	0.51
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Has bank savings account	0.18	0.18	0.16	0.17	0.14	0.17	0.53
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Member of ROSCA	0.53	0.54	0.55	0.52	0.53	0.50	0.43
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	
Transfers to HHs in village	573.05	493.70	638.93	411.47	492.31	484.36	0.80
	(118.49)	(65.18)	(134.58)	(43.84)	(61.95)	(69.47)	
Transfers from HHs in village	330.75	215.80	162.09	141.07	188.27	130.79	1.10
	(126.27)	(62.21)	(56.85)	(28.92)	(67.82)	(21.06)	
Community groups	2.87	2.70	2.79	2.66	2.88	2.66	1.08
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	
Belongs to Luhya ethnic group	0.79	0.79	0.79	0.82	0.82	0.80	0.63
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	
Belongs to Teso ethnic group	0.09	0.09	0.09	0.08	0.06	0.08	0.47
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	
Belongs to Luo ethnic group	0.12	0.12	0.11	0.09	0.10	0.11	0.59
	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	
Christian	0.98	0.98	0.97	0.97	0.99	0.99	0.98
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	
Attended church last week	0.70	0.66	0.70	0.62	0.65	0.68	1.52
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	

Standard errors in parentheses. Within each village, randomized assignment to treatment was stratified by gender and education level (an indicator for going beyond primary school).

Table 3: Outcomes in Experiment by Treatment

Treatment:	Private	Public	Payment	Private	Public	Payment
Budget Size:	Small	Small	Small	Large	Large	Large
Panel A: Outcomes in Experiment						
Business investment	41.44	42.59	42.00	93.35	91.98	90.26
	(0.82)	(0.86)	(0.81)	(1.90)	(1.88)	(1.84)
Fraction invested	0.52	0.53	0.53	0.52	0.51	0.50
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Investing 80 shillings or less	.	.	.	0.42	0.46	0.48
				(0.03)	(0.03)	(0.03)
Mean exit price (Kenyan shillings)	.	.	34.84	.	.	35.07
			(0.91)			(0.91)
Mean exit price (% gross)	.	.	61.48	.	.	22.15
			(4.47)			(1.43)
Proportion buying out	.	.	0.21	.	.	0.34
			(0.02)			(0.03)
Mean accepted exit price (% gross)	.	.	20.12	.	.	12.45
			(2.24)			(1.28)
Proportion heads	0.54	0.47	0.52	0.57	0.50	0.55
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Average payout (Kenyan shillings)	153.73	139.16	141.07	355.01	321.54	339.27
	(6.02)	(6.11)	(5.99)	(13.57)	(13.46)	(13.51)
Panel B: Business Investment within Experiment, by Demographic Group						
Men	42.90	44.50	42.28	88.71	93.07	89.42
	(1.41)	(1.29)	(1.18)	(3.26)	(3.17)	(3.08)
Women	40.49	41.31	41.82	96.30	91.28	90.82
	(1.00)	(1.15)	(1.10)	(2.30)	(2.33)	(2.29)
Primary school only	41.40	42.30	42.89	90.94	93.21	91.53
	(0.98)	(1.13)	(1.14)	(2.50)	(2.43)	(2.44)
Some secondary school	41.49	42.99	40.86	96.47	90.27	88.46
	(1.44)	(1.34)	(1.13)	(2.91)	(2.98)	(2.80)
No kin attending experiment	41.44	42.59	41.93	91.60	93.03	89.42
	(0.90)	(0.96)	(0.93)	(2.10)	(1.98)	(2.05)
Kin attending experiment	41.41	42.61	42.25	101.41	87.50	93.62
	(2.05)	(1.98)	(1.66)	(4.39)	(5.21)	(4.19)

Standard errors in parentheses. Within each village, randomized assignment to treatment was stratified by gender and education level (an indicator for going beyond primary school).

Table 4: OLS Regressions of Amount Invested by Experimental Treatment

Sample:	All	All	All	All	Men	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public treatment	-0.651	-0.755	0.871	0.834	0.542	0.31	1.067	1.099
	(1.251)	(1.249)	(1.013)	(1.045)	(1.658)	(1.759)	(1.274)	(1.362)
Large budget	49.876***	49.776***	51.916***	51.912***	45.808***	45.940***	55.810***	55.576***
	(1.185)	(1.186)	(2.068)	(2.064)	(3.550)	(3.462)	(2.501)	(2.493)
Public × large budget	.	.	-3.085	-3.230	2.012	1.979	-6.310**	-6.335**
			(2.523)	(2.510)	(4.274)	(4.205)	(3.091)	(3.070)
Female	.	0.409	.	0.393	.	.	.	.
		(1.335)		(1.336)				
Natural log of HH assets	.	1.086	.	1.060	.	2.675**	.	0.146
		(0.814)		(0.814)		(1.213)		(1.089)
Married	.	2.894*	.	2.912**	.	4.657	.	3.428*
		(1.480)		(1.479)		(3.313)		(1.752)
HH size	.	0.047	.	0.063	.	0.586	.	-0.209
		(0.238)		(0.238)		(0.373)		(0.309)
Constant	42.441***	30.622***	41.436***	29.742***	42.897***	13.544	40.491***	38.522***
	(0.954)	(8.250)	(0.821)	(8.222)	(1.406)	(12.333)	(0.996)	(10.692)
Village FEs	No	Yes	No	Yes	No	Yes	No	Yes
Age Category FEs	No	Yes	No	Yes	No	Yes	No	Yes
Education Category FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	2145	2145	2145	2145	847	847	1298	1298
R^2	0.456	0.472	0.457	0.473	0.408	0.455	0.492	0.515

Robust standard errors in parentheses. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level.

Table 5: Regressions of Indicator for Investing 80 Shillings or Less

Specification:	Probit Model				Linear Probability Model			
Sample:	Men	Men	Women	Women	Men	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public treatment	-0.063	-0.06	0.246**	0.288***	-0.025	-0.022	0.096**	0.105**
	(0.13)	(0.138)	(0.106)	(0.11)	(0.052)	(0.051)	(0.041)	(0.042)
Natural log of HH assets	.	-0.141	.	0.145*	.	-0.05	.	0.053*
		(0.088)		(0.075)		(0.032)		(0.028)
Married	.	-0.174	.	-0.357***	.	-0.062	.	-0.129**
		(0.201)		(0.135)		(0.076)		(0.051)
HH size	.	-0.001	.	0.007	.	-0.0006	.	0.003
		(0.025)		(0.022)		(0.009)		(0.008)
Village FEs	No	Yes	No	Yes	No	Yes	No	Yes
Age Category FEs	No	Yes	No	Yes	No	Yes	No	Yes
Education Category FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	417	417	644	644	417	417	644	644
R^2	.	.	.	.	0.0006	0.141	0.008	0.085
Pseudo R^2	0.0004	0.112	0.006	0.065	.	.	.	.

Robust standard errors in parentheses. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level. Sample restricted to subjects receiving larger endowment.

Table 6: OLS Regressions of Investment Amount when Kin Are Present

Sample:	Men	Men	Men	Men	Women	Women	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent Variable = Amount Invested								
Public treatment	4.598	4.443	4.426	6.339	-3.177	-3.313	-4.849	-2.706
	(4.498)	(4.530)	(3.919)	(4.607)	(3.042)	(3.046)	(2.991)	(3.166)
Close kin attended game	14.603**	12.545	.	11.228	10.581*	12.283*	.	12.165*
	(7.230)	(7.628)		(7.654)	(6.295)	(6.314)		(6.349)
Close kin at game × public	-9.995	-9.463	.	-8.247	-21.236***	-21.284***	.	-20.942**
	(8.635)	(8.664)		(8.687)	(8.203)	(8.172)		(8.236)
No. of close kin in village	.	0.779	.	0.808	.	-0.857	.	-0.881
		(0.731)		(0.732)		(0.735)		(0.737)
Spouse at game	.	.	25.607	23.774	.	.	3.182	2.880
			(17.466)	(17.662)			(7.649)	(7.447)
Spouse at game × public	.	.	-34.301*	-32.169	.	.	-8.090	-7.410
			(19.582)	(19.721)			(10.174)	(10.072)
Observations	417	417	417	417	644	644	644	644
R^2	0.142	0.145	0.142	0.154	0.098	0.099	0.09	0.1
Panel B: Dependent Variable = Indicator for Investing 80 Shillings or Less								
Public treatment	-0.016	-0.012	-0.044	-0.034	0.065	0.066	0.102**	0.064
	(0.062)	(0.063)	(0.054)	(0.064)	(0.045)	(0.045)	(0.044)	(0.047)
Close kin attended game	-0.088	-0.036	.	-0.028	-0.223**	-0.236***	.	-0.236***
	(0.094)	(0.099)		(0.1)	(0.089)	(0.091)		(0.091)
Close kin at game × public	-0.004	-0.018	.	-0.02	0.353***	0.353***	.	0.352***
	(0.114)	(0.114)		(0.115)	(0.121)	(0.12)		(0.121)
No. of close kin in village	.	-0.02*	.	-0.02*	.	0.007	.	0.007
		(0.011)		(0.011)		(0.012)		(0.012)
Spouse at game	.	.	-0.104	-0.1	.	.	-0.029	-0.018
			(0.18)	(0.184)			(0.118)	(0.115)
Spouse at game × public	.	.	0.288	0.281	.	.	0.038	0.021
			(0.204)	(0.205)			(0.148)	(0.145)
Observations	417	417	417	417	644	644	644	644
R^2	0.147	0.156	0.149	0.163	0.095	0.096	0.085	0.096

Robust standard errors in parentheses. Indicators for age and education categories, marital status, HH size, the log value of HH assets, and a constant are included as controls in all specifications. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level. Sample restricted to subjects receiving larger endowment.

Table 7: OLS Regressions of Village-Level Outcomes on Income Hiding in Experiment

	(1)	(2)	(3)	(4)	(5)	(6)
Public-private investment difference	-0.018**	-0.016***	-0.019**	-0.014	-0.016	-0.021**
	(0.007)	(0.006)	(0.009)	(0.008)	(0.013)	(0.01)
Hiding by women	0.141	0.129	.	.	.	.
	(0.327)	(0.236)				
Mean years of schooling	.	-0.385**	.	-0.388	.	-0.38
		(0.193)		(0.297)		(0.282)
Mean close kin in village	.	1.684***	.	1.664***	.	1.714***
		(0.262)		(0.373)		(0.437)
ELF	.	3.151***	.	3.039***	.	3.341***
		(0.722)		(1.046)		(1.156)
Mean community groups	.	0.727***	.	0.662	.	0.812**
		(0.271)		(0.44)		(0.38)
Distance to paved road	.	0.016	.	0.021	.	0.009
		(0.024)		(0.037)		(0.033)
Observations	52	52	26	26	26	26
R^2	0.073	0.569	0.105	0.563	0.042	0.585

Robust standard errors clustered at village level. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level.

Table 8: Summary Statistics by Price of Exit

Price of Exit:	10	20	30	40	50	60	F-Stat
Proportion female	0.61	0.61	0.49	0.61	0.64	0.65	1.55
	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	
Years schooling	7.37	6.87	6.77	6.44	6.91	7.00	0.94
	(0.28)	(0.28)	(0.30)	(0.34)	(0.31)	(0.33)	
Age	35.19	35.89	37.28	37.89	36.96	33.13	1.69
	(1.25)	(1.26)	(1.36)	(1.39)	(1.27)	(1.26)	
Currently married	0.72	0.74	0.73	0.80	0.78	0.73	0.65
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	
Ever married	0.85	0.90	0.85	0.94	0.87	0.82	1.66
	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	
HH size	6.44	6.10	6.35	6.08	6.44	5.98	0.56
	(0.29)	(0.28)	(0.25)	(0.26)	(0.27)	(0.25)	
Value of HH assets	32264.68	32437.60	31288.64	29460.53	35533.60	32900.00	0.63
	(2234.69)	(2382.03)	(2073.85)	(2115.26)	(3285.77)	(2829.08)	
Close relatives in village	2.15	2.25	2.34	2.50	2.30	2.48	0.33
	(0.19)	(0.22)	(0.23)	(0.26)	(0.25)	(0.24)	
Distant relatives in village	10.23	10.58	13.41	9.97	9.55	13.93	1.68
	(1.26)	(1.31)	(1.75)	(1.14)	(1.32)	(1.81)	
HH farms	0.99	0.98	0.99	0.98	1.00	0.99	0.47
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	
HH uses fertilizer on crops	0.49	0.51	0.52	0.45	0.39	0.59	2.21*
	(0.05)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)	
HH member employed	0.29	0.17	0.26	0.23	0.17	0.31	2.44**
	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	
Saves at bank	0.18	0.16	0.14	0.12	0.18	0.21	0.78
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	
Participates in ROSCA	0.52	0.57	0.46	0.43	0.57	0.56	1.61
	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	
Transfers to HHs in village	734.72	533.96	464.64	468.00	612.04	558.32	0.29
	(329.87)	(153.90)	(125.67)	(121.39)	(138.17)	(187.20)	
Transfers from HHs in village	91.28	242.64	221.32	75.70	98.38	144.77	0.94
	(34.45)	(133.18)	(96.37)	(25.17)	(29.45)	(32.00)	
No. of community groups	2.82	2.76	2.47	2.46	3.05	2.76	1.76
	(0.18)	(0.16)	(0.19)	(0.16)	(0.16)	(0.17)	
Belongs to Luhya ethnic group	0.76	0.78	0.85	0.73	0.83	0.84	1.68
	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.04)	
Belongs to Teso ethnic group	0.10	0.10	0.06	0.12	0.10	0.03	1.63
	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	
Belongs to Luo ethnic group	0.14	0.10	0.09	0.15	0.06	0.13	1.46
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	
Christian	0.97	0.98	0.99	0.97	0.98	0.97	0.24
	(0.02)	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	
Attended church last week	0.63	0.66	0.72	0.71	0.74	0.67	0.81
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	
Proportion buying out	0.45	0.31	0.26	0.32	0.19	0.12	7.41***
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	

Robust standard errors in parentheses.

Table 9: OLS Regressions of Paying to Avoid Announcing

Sample:	Entire sample			Only those able to pay			Heads only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price of exit	-0.006***	-0.007***	-0.007***	-0.005***	-0.006***	-0.008***	-0.005***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Large budget	0.144***	0.043	0.034	0.123***	0.05	0.015	0.183***	0.14
	(0.033)	(0.083)	(0.101)	(0.036)	(0.086)	(0.103)	(0.049)	(0.12)
Price of exit × large budget	.	0.003	0.002	.	0.002	0.003	.	0.001
		(0.002)	(0.002)		(0.002)	(0.003)		(0.003)
Coin flip lands heads	.	.	0.17***	.	.	0.174***	.	.
			(0.041)			(0.052)		
Heads × large budget	.	.	0.052	.	.	0.05	.	.
			(0.126)			(0.13)		
Heads × price × large budget	.	.	0.0008	.	.	0.0005	.	.
			(0.003)			(0.003)		
Observations	690	690	689	627	627	627	368	368
R^2	0.102	0.105	0.161	0.081	0.085	0.135	0.135	0.136

Robust standard errors in parentheses. Indicators for age and education categories, marital status, HH size, the log value of HH assets, and a constant are included as controls in all specifications. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level.

Table 10: OLS Regressions of Business Investment on the Price of Exit

Sample:	Entire sample				Men only		Women only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price of exit	-0.194***	-0.192***	0.025	0.034	-0.029	0.015	0.055	0.065
	(0.063)	(0.065)	(0.047)	(0.048)	(0.066)	(0.076)	(0.063)	(0.066)
Large budget	48.306***	48.344***	63.592***	64.122***	62.079***	64.958***	64.615***	63.839***
	(1.999)	(2.039)	(4.968)	(5.015)	(8.412)	(8.540)	(6.049)	(6.335)
Price of exit × large budget	.	.	-0.437***	-0.45***	-0.44**	-0.519**	-0.437***	-0.424***
			(0.124)	(0.125)	(0.208)	(0.219)	(0.152)	(0.156)
Female	.	1.038	.	1.222	.	.	.	.
		(2.311)		(2.276)				
Natural log of HH assets	.	1.016	.	1.250	.	1.498	.	1.531
		(1.404)		(1.402)		(2.086)		(2.015)
Married	.	5.013**	.	4.472**	.	8.739	.	3.102
		(2.289)		(2.267)		(6.768)		(2.681)
HH size	.	0.125	.	0.19	.	1.053	.	-0.581
		(0.449)		(0.441)		(0.723)		(0.586)
Age Category FEs	No	Yes	No	Yes	No	Yes	No	Yes
Education Category FEs	No	Yes	No	Yes	No	Yes	No	Yes
Observations	690	690	690	690	274	274	416	416
R^2	0.464	0.475	0.475	0.486	0.45	0.493	0.493	0.507

Robust standard errors in parentheses. Indicators for age and education categories, marital status, HH size, the log value of HH assets, and a constant are included as controls in all specifications. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 11: Buying Out When Kin Are Present

Dependent Variable: Pays to Avoid Announcement

Sample:	Women	Women	Women	Men	Men	Men
Specification:	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Coin flip lands heads	0.213***	0.212***	0.234***	0.143**	-0.021	0.281***
	(0.044)	(0.058)	(0.065)	(0.067)	(0.086)	(0.099)
Kin at game	-0.015	-0.059	0.105	-0.069	-0.147	-0.018
	(0.085)	(0.065)	(0.235)	(0.071)	(0.096)	(0.106)
Heads × kin at game	0.092	0.123	-0.005	0.112	0.198	0.008
	(0.126)	(0.146)	(0.268)	(0.113)	(0.148)	(0.18)
Observations	416	209	207	273	136	137
R^2	0.207	0.243	0.244	0.218	0.282	0.244
Coin flip lands heads	0.217***	0.246***	0.215***	0.162***	0.006	0.274***
	(0.046)	(0.06)	(0.068)	(0.059)	(0.079)	(0.092)
Spouse attended game	-0.152	-0.27***	-0.128	-0.031	-0.048	-0.01
	(0.094)	(0.095)	(0.19)	(0.119)	(0.113)	(0.265)
Heads × spouse at game	0.264	0.248	0.302	0.01	0.216	-0.257
	(0.209)	(0.262)	(0.325)	(0.151)	(0.175)	(0.322)
Observations	383	191	192	259	130	129
R^2	0.218	0.276	0.255	0.202	0.287	0.241

Robust standard errors in parentheses. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level. Age and educational category fixed effects, HH size, HH assets, marital status, and budget×price indicators included in all specifications.

Table 12: Buying Out When Kin Are Present

Dependent Variable: Pays to Avoid Announcement

Sample:	Women	Women	Women	Men	Men	Men
Specification:	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Kin at game	0.121	.	.	-0.0006	.	.
	(0.093)			(0.097)		
Spouse attended game	.	0.158	.	.	-0.04	.
		(0.19)			(0.106)	
Spouse or kin at game	.	.	0.152*	.	.	-0.047
			(0.089)			(0.094)
Observations	220	205	220	148	137	148
R^2	0.224	0.238	0.229	0.266	0.251	0.268

Robust standard errors in parentheses. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level. Age and educational category fixed effects, HH size, HH assets, marital status, and budget$\times$price indicators included in all specifications.

A Appendix

A.1 Proofs

Proof of Proposition 1. *The optimal investment level in the private, small endowment treatment can be derived from the first order condition as follows:*

$$\begin{aligned}
\frac{1}{2}\left(m_s - b_i^{pri}\right)^{-\rho_i} &= \frac{4}{2}\left(m_s + 4b_i^{pri}\right)^{-\rho_i} \\
\Leftrightarrow \left(m_s + 4b_i^{pri}\right) &= 4^{1/\rho_i}\left(m_s - b_i^{pri}\right) \\
\Leftrightarrow 4b_i^{pri} + 4^{1/\rho_i} b_i^{pri} &= 4^{1/\rho_i} m_s - m_s \\
\Leftrightarrow b_i^{pri} &= \left(\frac{4^{1/\rho_i} - 1}{4^{1/\rho_i} + 4}\right) m_s
\end{aligned}$$

The first order condition characterizing the optimal investment level in the public, small endowment treatment can be derived from the following first order condition:

$$\frac{1}{2}\left[(1-\tau_i)\left(m_s - b_i^{pub}\right)\right]^{-\rho_i} = \frac{4}{2}\left[(1-\tau_i)\left(m_s + 4b_i^{pub}\right)\right]^{-\rho_i} \tag{15}$$

In this case, it is apparent that the $1-\tau_i$ term drops out immediately, making the FOC identical to the one above. Thus, the solutions to the two FOCs must be the same: $b_i^{pri}(m_s) = b_i^{pub}(m_s)$.

Next, consider expected utility given the utility-maximizing investment choice. Expected utility in the private, small endowment treatment is

$$\begin{aligned}
E\left[u_i\left(b_i^{pri}|m_s,\rho_i\right)\right] &= \frac{1}{2}\left[\frac{\left(m_s - b_i^{pri}\right)^{1-\rho_i}}{1-\rho_i} + \frac{\left(m_s + 4b_i^{pri}\right)^{1-\rho_i}}{1-\rho_i}\right] \\
&= \frac{1}{2(1-\rho_i)}\left[\left(m_s - \frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\cdot m_s\right)^{1-\rho_i} + \left(m_s + 4\cdot\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\cdot m_s\right)^{1-\rho_i}\right] \\
&= \frac{m_s^{1-\rho_i}}{2(1-\rho_i)}\left[\left(\frac{4^{1/\rho_i}+4-4^{1/\rho_i}+1}{4^{1/\rho_i}+4}\right)^{1-\rho_i} + \left(\frac{4^{1/\rho_i}+4+4\cdot 4^{1/\rho_i}-4}{4^{1/\rho_i}+4}\right)^{1-\rho_i}\right] \\
&= \frac{m_s^{1-\rho_i}}{2(1-\rho_i)}\left[\left(\frac{5}{4^{1/\rho_i}+4}\right)^{1-\rho_i} + \left(\frac{5\cdot 4^{1/\rho_i}}{4^{1/\rho_i}+4}\right)^{1-\rho_i}\right] \\
&= \frac{1+4^{\frac{1-\rho_i}{\rho_i}}}{2(1-\rho_i)}\left(\frac{5m_s}{4^{1/\rho_i}+4}\right)^{1-\rho_i}
\end{aligned}$$

Similarly, expected utility in the public, small endowment treatment is

$$\begin{aligned}
E\left[u_i\left(b_i^{pub}|m_s,\rho_i,\tau_i\right)\right] &= \frac{1}{2}\left[\frac{(1-\tau_i)^{1-\rho_i}\left(m_s - b_i^{pub}\right)^{1-\rho_i}}{1-\rho_i} + \frac{(1-\tau_i)^{1-\rho_i}\left(m_s + 4b_i^{pub}\right)^{1-\rho_i}}{1-\rho_i}\right] \\
&= \frac{1+4^{\frac{1-\rho_i}{\rho_i}}}{2(1-\rho_i)}\left(\frac{5m_s}{4^{1/\rho_i}+4}\right)^{1-\rho_i}(1-\tau_i)^{1-\rho_i}
\end{aligned}$$

To simplify notation, let

$$\Gamma_i = \frac{1+4^{\frac{1-\rho_i}{\rho_i}}}{2}\left(\frac{5m_s}{4^{1/\rho_i}+4}\right)^{1-\rho_i}(1-\tau_i)^{1-\rho_i}.$$

Note that $\Gamma_i \geq 0$ for all $\rho_i > 0$. For $\rho \in (0,1)$,

$$\begin{aligned} 1 > (1-\tau_i)^{1-\rho_i} &\Rightarrow 1-\rho_i > (1-\tau_i)^{1-\rho_i}(1-\rho_i) \\ &\Rightarrow (1-\rho_i)\Gamma_i > (1-\tau_i)^{1-\rho_i}(1-\rho_i)\Gamma_i \end{aligned}$$

so expected utility is higher in the private (small endowment) treatment than in the public (small endowment) treatment. For $\rho > 1$,

$$\begin{aligned} 1 < (1-\tau_i)^{1-\rho_i} &\Rightarrow 1-\rho_i > (1-\tau_i)^{1-\rho_i}(1-\rho_i) \\ &\Rightarrow (1-\rho_i)\Gamma_i > (1-\tau_i)^{1-\rho_i}(1-\rho_i)\Gamma_i \end{aligned}$$

since $1-\rho_i < 0$. Again, expected utility is higher in the private (small endowment) treatment than in the public (small endowment) treatment.

Finally, consider the derivative with respect to τ_i of maximized expected utility in the public, small endowment treatment.

$$\begin{aligned} \frac{\partial}{\partial \tau_i} E\left[u_i\left(b_i^{pub} | m_s, \rho_i, \tau_i\right)\right] &= -\frac{\Gamma_i}{1-\rho_i}(1-\rho_i)(1-\tau_i)^{-\rho_i} \\ &= -\Gamma_i (1-\tau_i)^{-\rho_i} \end{aligned}$$

The derivative is clearly negative, since Γ_i and $1-\tau_i$ are always positive.
□

Proof of Proposition 2. *To arrive at Claim 1, observe that*

$$\begin{aligned} \frac{\partial}{\partial \rho_i} b_i^{pri}(m_l) &= \left(\frac{4^{1/\rho_i}\ln 4\left(-\rho_i^{-2}\right)\left(4^{1/\rho_i}+4\right) - 4^{1/\rho_i}\ln 4\left(-\rho_i^{-2}\right)\left(4^{1/\rho_i}-1\right)}{(4^{1/\rho_i}+4)^2}\right) m_l \\ &= \left(\frac{-4\cdot 4^{1/\rho_i}\ln 4\left(\rho_i^{-2}\right) - 4^{1/\rho_i}\ln 4\left(\rho_i^{-2}\right)}{(4^{1/\rho_i}+4)^2}\right) m_l \\ &= \left(\frac{-5\cdot 4^{1/\rho_i}\ln 4\left(\rho_i^{-2}\right)}{(4^{1/\rho_i}+4)^2}\right) m_l \end{aligned}$$

which is clearly negative since ρ_i and m_l are positive numbers. Moreover, we can explicitly define $\underline{\rho}$ as follows:

$$\begin{aligned} b_i^{pri}(m_l) = \left(\frac{4^{1/\underline{\rho}}-1}{4^{1/\underline{\rho}}+4}\right)(m_s+d) &= m_s \\ \Leftrightarrow \left(4^{1/\underline{\rho}}-1\right)(m_s+d) &= \left(4^{1/\underline{\rho}}+4\right)m_s \\ \Leftrightarrow -m_s + 4^{1/\underline{\rho}}d - d &= 4m_s \\ \Leftrightarrow \frac{1}{\underline{\rho}}\ln 4 + \ln d &= \ln(5m_s+d) \\ \Leftrightarrow \underline{\rho} &= \frac{\ln 4}{\ln(5m_s+d) - \ln d} \end{aligned}$$

Individuals with $\rho_i < \underline{\rho}$ invest more than m_s in the private, large endowment treatment.

To see that such individuals will either invest b_i^{pri} or m_s in the public, large endowment treatment, note that the interior solution for an optimal investment below *m_s which obscures the size of one's endowment is*

$$\left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right)m_s + \frac{\left(4^{1/\rho_i}-1\right)d}{(1-\tau_i)\left(4^{1/\rho_i}+4\right)}.$$

Since this is greater than b_i^{pri}, such an interior solution below m_s is clearly not possible for individuals with $\rho_i < \underline{\rho}$, for whom $b_i^{pri} > m_s$.

Finally, to arrive at Claim 3, observe that if i sets her investment level at b_i^{pri}, her expected utility is

$$\begin{aligned}
E\left[u_i\left(b_i^{pri}|m_l,\rho_i,\tau_i\right)\right] &= \frac{1}{2}\frac{\left[(1-\tau_i)\left(m_l - b_i^{pri}\right)\right]^{1-\rho_i}}{1-\rho_i} + \frac{1}{2}\frac{\left[(1-\tau_i)\left(m_l + 4b_i^{pri}\right)\right]^{1-\rho_i}}{1-\rho_i} - \kappa_i \\
&= \frac{1}{2}\frac{(1-\tau_i)^{1-\rho_i}}{1-\rho_i}\left\{\left[m_l - \left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right)(m_l)\right]^{1-\rho_i} + \left[m_l + 4\left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right)(m_l)\right]^{1-\rho_i}\right\} - \kappa_i \\
&= \frac{1+4^{\frac{1-\rho_i}{\rho_i}}}{2(1-\rho_i)}\left[\frac{5m_l(1-\tau_i)}{4^{1/\rho_i}+4}\right]^{1-\rho_i} - \kappa_i
\end{aligned}$$

If i invests exactly m_s, on the other hand, her expected utility is

$$\begin{aligned}
E\left[u_i\left(b_i = m_s|m_l,\rho_i,\tau_i\right)\right] &= \frac{1}{2}\frac{\left[(1-\tau_i)(m_s-m_s)+d\right]^{1-\rho}}{1-\rho} + \frac{1}{2}\frac{\left[(1-\tau_i)(m_s+4m_s)+d\right]^{1-\rho}}{1-\rho} - \kappa_i \\
&= \frac{1}{2}\frac{d^{1-\rho}}{1-\rho} + \frac{1}{2}\frac{\left[(1-\tau_i)5m_s+d\right]^{1-\rho}}{1-\rho} - \kappa_i
\end{aligned}$$

Hence, i prefers to invest exactly m_s in the public, large endowment treatment whenever

$$\begin{aligned}
&\frac{1}{2}\frac{d^{1-\rho}}{1-\rho} + \frac{1}{2}\frac{\left[(1-\tau_i)5m_s+d\right]^{1-\rho}}{1-\rho} - \kappa_i \geq \frac{1+4^{\frac{1-\rho_i}{\rho_i}}}{2(1-\rho_i)}\left[\frac{5m_l(1-\tau_i)}{4^{1/\rho_i}+4}\right]^{1-\rho_i} - \kappa_i \\
\Leftrightarrow\ &\frac{d^{1-\rho}}{1-\rho} + \frac{\left[(1-\tau_i)5m_s+d\right]^{1-\rho}}{1-\rho} \geq \frac{1+4^{\frac{1-\rho_i}{\rho_i}}}{(1-\rho_i)}\left[\frac{(1-\tau_i)5(m_s+d)}{4^{1/\rho_i}+4}\right]^{1-\rho_i}
\end{aligned}$$

By definition, the RHS is higher when τ_i is zero, since $b_i^{pri}(m_l)$ is the optimal investment level in the private, large endowment treatment, when the investment level $b_i - m_s$ is available. On the other hand, the RHS approaches zero as τ_i approaches one, so setting b_i to m_s must be optimal for sufficiently high values of τ_i.

To be completed.

Proof of Proposition 3. *To be completed.*

A.2 Experimental Instructions

Translated from Swahili. Original Swahili instructions available upon request.

Read to participants at start of experimental session:

In this game, you will be given money which you will divide between two cups: a savings cup and a business cup. The money that you put in the business cup can be used to generate more money, as in a business, but it can also be lost. At the end of the game, we'll ask some of you to stand up and report your investment decisions and outcomes to the rest of the room.

Now we will explain the game to you step by step. First, we will tell you how much money you have to use in the game. The amount of money that we give you at the start of the game is how much you get to divide between the two cups. Each of you will receive at least 80 shillings, but a few of you will receive more. Before we came, we put all of your numbers into a bag and we pulled out one third of them. Demonstrate. We did this without looking - like this - so we didn't know which numbers we would pull out. The people with the seat numbers we pulled out will be given 180 shillings; everyone else will be given 80 shillings. So, everyone receives at least 80 shillings, but one third of you will receive more.

The money that you are paid is yours, and you will decide how to divide it between the two cups — the savings cup and the business cup. The money that you put in the business cup can be used to generate more money — like a business — but that money can also be lost. Your investment will either succeed or fail. If it succeeds, you will be paid five times the amount you put in the business cup; if it fails, you will lose the money you put into the business cup. So, if the "business" succeeds, you get back more than you put in the business cup. If the "business" fails, you lose all the money that you put in the business cup. Money that you put in the savings cup just sits there until the end of the game: you'll get to take all of the money in the savings cup.

How do we determine what happens to the money that you put in the business cup? After you divide your money between the two cups, we will ask you to shake a coin in a bottle - like this. Whether your coin lands with heads or tails facing up will determine what happens to the money in the business cup — the money will either be multiplied by five, or it will be lost. Both possibilities are equally likely, and you don't know in advance which one is going to happen. If your coin lands with heads facing up, you are paid five times the amount you put into the business cup. If your coin lands facing down, you lose all the money you put in the business cup.

So, if you put 10 shillings into the business cup, how much will you get at the end of the game? You'll shake a coin in a bottle to determine how much. If the coin lands with heads facing up, you'll get five times 10 shillings — that's 50 shillings. However, if the coin lands with tails facing up, you'll lose the 10 shillings you put into the business cup. Either way, you'll still get to take the money that you put in the savings cup.

You can put as much or as little as you want into the business cup. If you like, you can put everything in the savings cup, and nothing in the business cup. Or if you like, you can put everything in the business cup, and nothing in the savings cup. The decision is yours. For each amount that you might put in the business cup, this poster tells you what can happen to your money. For each amount that you might put in the business cup, you can see — here — how much money you'll receive if the coin lands with heads facing up, and you can see that you will lose your investment if the coin lands with tails facing up.

Are there any questions so far? Let's go through a couple of examples. First, imagine that you start with 80 shillings, and you decide to put 70 shillings into the business cup, and the remaining 10 shillings into the savings cup. What happens next? We will let you shake the coin in the bottle. If the coin lands with heads facing up, then you receive 5 times the 70 shillings in the business cup — that's 350 shillings — plus the 10 shillings in the savings cup. That's a total of 360 shillings. However, if the coin lands with tails facing up, then you will lose everything you put in the business cup, and you will only receive the 10 shillings you put in the savings cup — so, you take home 10 shillings at the end of the game.

Now, imagine that you start with 180 shillings, and you decide to put 90 shillings into the business cup, and the remaining 10 shillings into the savings cup. If the coin lands with tails facing up, you lose the 90 shillings in the business cup, and you will get only the 90 shillings in the savings cup. However, if the coin

lands with heads facing up you'll take home the 90 shillings in the savings cup and 5 times the 90 shillings in the business cup. That's 90 shillings, together with 450 shillings, or in total, 540 shillings.

Are there any questions so far? After everyone makes their decisions, we'll ask about half of you to stand up and announce to the room how much money you put into the business cup and whether the coin landed with heads or tails facing up. However, you will not be required to announce how much you put into the savings cup. Only half of you will be asked to make an announcement. Whether we ask you to announce your decisions to the room has nothing to do with how much money you receive, or your actions in the game. When you come outside, we'll tell you whether you will have to announce your investment before you make any decisions.

For example, X is a participant in this game.[21] We would like him/her to announce the amount of money he/she put in the business cup. X, are you ready? How much money did you put in the business cup? *X responds: 20 Shillings.* The coin landed with which side facing up? *X responds: Heads.* Thank you X, please sit. How much money did this participant put in the business cup? *Audience responds: 20.* And the coin landed with which side facing up? *Audience responds: Heads.* Therefore, he/she received how much money from the business? *Audience responds: 100.* Think: how much money did he/she put in the savings cup? In fact, we can't know. This is his/her secret. It is possible that he/she started with 80 shillings, and he/she put 60 shillings in the savings cup; it is also possible that he/she started with 180 shillings, and he/she put 160 shillings in the savings cup. We can't know. Still, you are not required to announce what you put in the savings cup.

For another example, Y is a participant in this game. Y, are you ready? How much money did you put in the business cup? *Y responds: 150 Shillings.* The coin landed with which side facing up? *Y responds: Heads.* Thank you Y, please sit. How much money did this participant put in the business cup? *Audience responds: 150.* And the coin landed with which side facing up? *Audience responds: Heads.* Therefore, he/she received how much money from the business? *Audience responds: 750 Shillings.* Think: how much money did he/she put in the savings cup? 30 shillings. Why? It's clear that he started with 180 shillings, because he put 150 shillings in the business cup, so we can be sure he put 30 shillings in the savings cup. He couldn't have put 150 shillings in the business cup if he had started with only 80 shillings.

For the last example, Z is a participant in this game. Z, are you ready? How much money did you put in the business cup? *Z responds: 60 Shillings.* The coin landed with which side facing up? *Z responds: Tails.* Thank you Z, please sit. How much money did this participant put in the business cup? *Audience responds: 60.* And the coin landed with which side facing up? *Audience responds: Tails.* Therefore, he/she received how much money from the business? *Audience responds: 0.* Think: how much money did he/she put in the savings cup? In fact, again, we can't know.

The announcement is like having a small shop. This shop has been well stocked with many goods. Is it clear that you have put a lot of money into this shop? *Audience responds: Yes.* If this business succeeds, will it be easy to see whether it has many customers? *Audience responds: Yes.* Do we know how much money you have in a bank account? *Audience responds: No.* Therefore, this is the reason we are asking you to announce the amount of money you have put into the business, and whether it succeeded, but we aren't asking you to announce how much money you put in the savings cup.

Some of the people who we ask to announce their decisions will also be given the opportunity to avoid having to make an announcement to the room. We'll give those few people a chance to pay a fee to avoid announcing their decisions to the rest of the room. Before you make your decisions, we'll tell you whether you will be given the chance to pay a fee and avoid announcing your decisions to the room. The fee will be between ten shillings and sixty shillings — we'll tell you before you make your decisions.

Are there any questions so far? In short: there are two amounts of money a person can receive to use in this game. You will be given 80 shillings, or 180 shillings. You'll decide how you want to divide that money between a business cup and a savings cup. The money that you put in the business cup can be used to generate more money — like a business — but that money can also be lost. Let's remind ourselves: how

[21] The real first names of the research assistants playing the roles of the three example subjects were used during experimental sessions.

much money could you put in the business cup? Zero, ten, twenty, thirty, forty, up to all the money you have been given to use in the game. You'll shake a coin in a bottle to determine the outcome. If it lands with heads facing up, you'll get five times what you put in the business cup; if the coin lands with tails facing up, you'll lose the money that you put in the business cup. But remember, you will get all the money you put in the savings cup. After everyone has made their business decisions, some of you will be asked to stand up and describe your choices to everyone in the room. Even if we ask you to announce your decisions to everyone else here, we may also give you the opportunity to "buy out" of having to make an announcement.

Are there any questions? Now we've finished explaining the instructions for the game, so we'll call you outside one at a time to make your decisions. When you come outside, you'll sit down at a desk with one of us. We will record all of your choices, and you will find out how much money you win in the game. We ask that you refrain from talking throughout the game, even after you've made your decisions. Are we understood? We really want the individual decision of each person here, and not the decision of your neighbor. Anyone who is found to be having conversations will be removed from the game, and will not be paid.

Read to individual subjects not assigned to the public-price treatments:

Statements in italics are instructions to research assistants, and were not read aloud.

First, I will tell you how much money you have to use in the game; then, you will decide how to divide it between your savings cup and your business cup. To make sure that you understand the game, I'm going to ask you a couple of questions. Do you understand that there are two possible amounts of money you might receive in this game? What are the two amounts? Do you understand what will happen to the money that you put in the business cup? What will happen? *Make sure that the respondent understands the structure of the game.* Do you have any question before we begin?

You are part of the group receiving 80 shillings (180) to use in the game, but you know that others are receiving 180 (80) shillings, right? You will not (will) have to announce your decisions to the rest of the participants at the end of the game. *Repeat previous sentence.* Got it? You have to decide how much you want to put into the savings cup and how much you want to put into the business cup. *Hand the respondent the coins.* After you divide the money, I'll let you shake a coin inside this bottle to determine what happens to the money in the business cup. *Wait while respondent makes his/her decision, and then record decision.*

OK, I'll let you shake a coin inside this bottle to determine what happens to the money that you put in the business cup. *Demonstrate, then ask the respondent to shake the coin. Record outcome.* Thanks. Now I'll ask you to wait while everyone else makes their decisions.

Read to individual subjects assigned to the public-price treatments:

Statements in italics are instructions to research assistants, and were not read aloud.

First, I will tell you how much money you have to use in the game; then, you will decide how to divide it between your savings cup and your business cup. To make sure that you understand the game, I'm going to ask you a couple of questions. Do you understand that there are two possible amounts of money you might receive in this game? What are the two amounts? Do you understand what will happen to the money that you put in the business cup? What will happen? *Make sure that the respondent understands the structure of the game.* Do you have any question before we begin?

You are part of the group receiving 80 shillings (180) to use in the game, but you know that others are receiving 180 (80) shillings, right? You have been chosen to announce your decisions to the rest of the participants at the end of the game, but you will be given the opportunity to pay a fee to avoid doing so. Do you understand? The fee will be ____. You have to decide how much you want to put into the savings cup and how much you want to put into the business cup. So it will be like this: first, you will decide how much money to put into the business cup and savings cup; then, you will shake the coin in the bottle to decide what will happen to the money in the business cup; and then if you have enough money to pay the fee to avoid announcing, you will be able to decide whether to pay or announce — if not, you will have to announce. Do you understand? *Hand the respondent the coins.* After you divide the money, I'll let you shake a coin inside this bottle to determine what happens to the money in the business cup. *Wait while respondent makes his/her decision, and then record decision.*

OK, I'll let you shake a coin inside this bottle to determine what happens to the money that you put in the business cup. *Demonstrate, then ask the respondent to shake the coin. Record outcome.*

If the respondent has enough money left to pay the fee: Now, will you pay the fee, or announce? *Record choice.* Thanks. Now I'll ask you to wait while everyone else makes their decisions.

A.3 Additional Tables and Figures

Figure A4: Proportion of Subjects Paying to Avoid Announcing

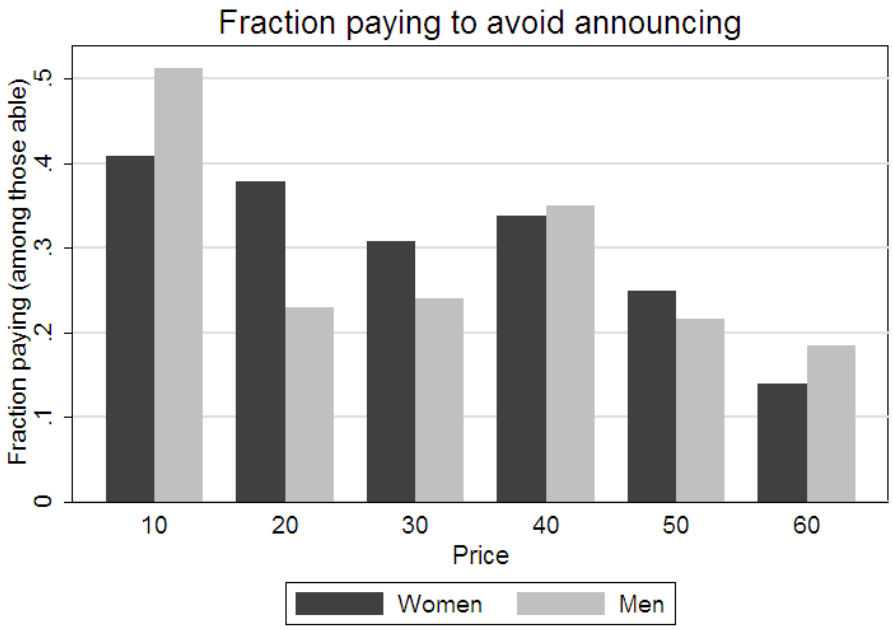

Table A13: OLS Regressions of Participation in Experiment

Sample:	All	All	Men	Men	Women	Women
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.034	-0.034	.	.	.	.
	(0.024)	(0.024)				
Natural log of HH assets	-0.027*	-0.027**	-0.001	0.004	-0.044**	-0.041**
	(0.014)	(0.014)	(0.02)	(0.02)	(0.019)	(0.018)
Any HH member employed	0.005	-0.002	0.027	0.02	0.0004	-0.003
	(0.022)	(0.022)	(0.036)	(0.035)	(0.028)	(0.028)
Married	-0.008	-0.009	0.021	0.023	-0.007	-0.008
	(0.025)	(0.025)	(0.054)	(0.057)	(0.032)	(0.033)
HH size	0.01***	0.01***	0.01*	0.011**	0.011**	0.011**
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
No. of close kin in village	0.001	0.002	0.007	0.01	-0.007	-0.007
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)
No. of distant kin in village	-0.0005	-0.0005	-0.001	-0.001	0.0002	0.0003
	(0.0007)	(0.0007)	(0.001)	(0.001)	(0.0009)	(0.0009)
Distance to paved road	0.012***	.	0.008**	.	0.016***	.
	(0.002)		(0.003)		(0.003)	
Belongs to Teso ethnic group	-0.056	-0.061	-0.053	-0.037	-0.062	-0.063
	(0.038)	(0.04)	(0.065)	(0.073)	(0.047)	(0.049)
Belongs to Luo ethnic group	-0.052	-0.047	-0.08	-0.126	-0.03	-0.025
	(0.033)	(0.035)	(0.069)	(0.106)	(0.038)	(0.039)
Other ethnic minority	-0.066	-0.032	0.033	0.111	-0.133	-0.108
	(0.158)	(0.161)	(0.069)	(0.094)	(0.213)	(0.211)
Ethnolinguistic fractionalization	-0.071	.	-0.115	.	-0.045	.
	(0.053)		(0.099)		(0.064)	
No. of community groups	0.002	0.0008	-0.011	-0.012	0.01	0.005
	(0.007)	(0.007)	(0.01)	(0.01)	(0.009)	(0.009)
HH received gift or loan in last 3 mos.	0.025	0.025	0.035	0.027	0.019	0.01
	(0.02)	(0.019)	(0.033)	(0.033)	(0.024)	(0.024)
HH gave gift or loan in last 3 mos.	0.048	0.055*	0.052	0.056	0.05	0.052
	(0.033)	(0.033)	(0.062)	(0.062)	(0.04)	(0.04)
Has bank savings account	-0.014	-0.013	-0.005	0.015	-0.036	-0.032
	(0.033)	(0.033)	(0.042)	(0.043)	(0.052)	(0.054)
Participates in ROSCA	0.025	0.021	0.046	0.034	0.012	0.019
	(0.023)	(0.023)	(0.037)	(0.037)	(0.029)	(0.03)
Received loan from MFI	-0.09	-0.092	-0.105	-0.154*	-0.049	-0.047
	(0.061)	(0.06)	(0.086)	(0.086)	(0.087)	(0.085)
Received loan from moneylender	-0.002	-0.008	-0.05	-0.008	0.039	0.034
	(0.048)	(0.047)	(0.083)	(0.074)	(0.059)	(0.06)
No. of correct math responses	0.037***	0.039***	0.028	0.03	0.044***	0.042***
	(0.011)	(0.011)	(0.02)	(0.02)	(0.013)	(0.014)
Willing to take risks	0.028	0.034*	0.016	0.024	0.034	0.041*
	(0.019)	(0.02)	(0.033)	(0.035)	(0.024)	(0.025)
Most people can be trusted	0.023	0.013	0.049	0.065	0.005	-0.004
	(0.028)	(0.028)	(0.042)	(0.042)	(0.037)	(0.037)
Most people try to be fair	-0.016	-0.016	-0.016	-0.021	-0.014	-0.013
	(0.019)	(0.019)	(0.033)	(0.034)	(0.024)	(0.024)
Constant	0.844***	0.881***	0.636***	0.54**	0.922***	0.984***
	(0.143)	(0.142)	(0.214)	(0.211)	(0.184)	(0.181)
Village FEs	Yes	No	Yes	No	Yes	No
Age variables p-value	0.274	0.289	0.017	0.036	0.544	0.675
Education variables p-value	0.383	0.42	0.035	0.037	0.224	0.461
Observations	1603	1603	526	526	1077	1077
R^2	0.072	0.09	0.079	0.141	0.087	0.112

Robust standard errors in parentheses. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level.