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. Parameter estimates suggest that women anticipate that observable income will be "taxed" at a rate above four percent; this effective tax rate nearly doubles when kin can observe income directly. Though this paper provides experimental evidence from a single African country — Kenya — observational studies suggest that similar kin pressures may be prevalent in many rural areas throughout Sub-Saharan Africa.

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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, Alger and Weibull 2010). 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, Di Falco and Bulte 2011, Comola and Fafchamps 2011, Dupas and Robinson forthcoming–b). 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 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 (2011) 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. Anecdotal evidence suggests that pressures to share income with kin are common in many parts of the world. However, Collier and Garg (1999)

[1] See Ligon, Thomas, and Worrall (2002), Albarran and Attanasio (2003), and Kinnan (2011) for examples. Foster and Rosenzweig (2001), which explores the impact of altruism on the set of self-enforcing insurance arrangements, is an important exception.

argue that such pressures are most intense in Africa, and are common to many ethnic groups across the continent; they write that "African kin groups are distinctive both by their ubiquity and by the strength of their claims upon members."

In this paper, we report the results of an experiment designed to measure social pressure 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 to keep income secret. We conduct economic experiments in 26 rural, agricultural communities in western Kenya. Within the experiment, subjects receive an endowment which they divided between a risk-free savings account and a risky but profitable investment. The size of the endowment varies across subjects, and the distribution of endowment sizes is common knowledge. 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, creating an incentive for those receiving the large endowment to invest no more than the amount of the small endowment, thereby keeping their endowment size hidden. We also offer a subset of subjects the option of paying to keep their investment returns secret, allowing us to directly measure the willingness-to-pay to hide income.

In light of evidence that women and men have different risk preferences (cf. Croson and Gneezy 2009) and that women in poor communities may have more trouble accumulating savings (cf. Dupas and Robinson forthcoming–a), we stratify our experiment by gender, and report results for men and women separately.[2] We find convincing evidence that women are willing to reduce their expected income to avoid making investment returns observable. Women receiving the large endowment, who may wish to hide this fact from their relatives and neighbors, are 25.4 percent (9.6 percentage points) more likely to invest an amount no larger than the small 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. The effect we observe among women appears to be driven primarily by the behavior of those with relatives attending the experiment, who would

[2]Dupas and Robinson (forthcoming–a) find evidence that female daily wage earners in western Kenya are more savings constrained than men in similar occupations. In a similar vein, De Mel, McKenzie, and Woodruff (2008), De Mel, McKenzie, and Woodruff (2009), and Fafchamps, McKenzie, Quinn, and Woodruff (2011) find lower returns to capital for microenterprises operated by women than for those operated by men.

be able to observe their payoffs directly. Estimates suggest that these women invest 22.1 percent less when investment income is observable than when it is hidden; they are 38.9 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. Consistent with a simple model of decisions in the experiment when subjects face pressure to share income and risk preferences are heterogeneous, we find that women receiving the large endowment are more likely to invest exactly the amount of the small endowment when investment returns are observable, and that this tendency is also more pronounced among women with relatives present. Impacts are unlikely to be driven by in-laws providing information to husbands: there is no direct impact of having one's husband present, and choice patterns are similar in the sub-sample of unmarried women.

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 an average of 15 percent of their gross payout from the experiment. Though women are no more likely than men to pay to avoid the public announcement, their willingness to do so is strongly related to the amount of observable income they receive; this is not true for men. At the village level, women's tendency to hide income within the experiment is negatively associated with durable asset accumulation by households, skilled and formal sector employment, and the probability of using fertilizer on crops, suggesting that social pressure to share may hinder growth and development.

After presenting our reduced form results, we estimate the magnitude of the "kin tax" parameter via maximum simulated likelihood in a mixed logit framework. In our setting, this extension is important because the size of the treatment effect of observability depends on individual risk preferences. Decisions in our private information treatments, in which investment returns are unobservable, suggest substantial heterogeneity in risk aversion across subjects.[3] After controlling for unobserved heterogeneity in risk preferences, we find a statistically significant tax for women, averaging four percent in general. For women whose kin attend the experimental session, consistent with the reduced form pattern, the tax appears to be twice as large. Simulations suggest that this

[3]See Hey and Orme (1994); Choi, Fisman, Gale, and Kariv (2007); Andersen, Harrison, Lau, and Rutström (2008); Choi, Kariv, Müller, and Silverman (2011), and Von Gaudecker, van Soest, and Wengström (2011) for further experimental evidence on risk preference heterogeneity.

level of social pressure may lead to dramatic reduction in the likelihood of starting a business.

The main aim of this paper is to document the importance of social pressure in interhousehold transfer relationships within kin networks in poor communities. Though the title is meant to be tongue-in-cheek, it is intended to highlight the fact that, in many village economies, the line separating the household from the rest of the extended family can be quite blurry (cf. Randall, Coast, and Leone 2011), and something akin to (potentially inefficient) household bargaining may occur between households within the same kin network. This paper is a first step toward exploring the dynamics of social pressure within these complex relationships empirically.

We make several contributions to the existing literature. First, we introduce a novel lab experiment designed to measure social pressure to share income in field settings. The experiment is simple to understand, but provides subjects with a rich menu of investment options and multiple mechanisms for hiding their income. Our design allows us to effectively rule out several alternative explanations: behavior is not consistent with models of attention aversion or a desire to avoid social sanctions against risk-taking, and the lack of a direct effect of having one's spouse present suggests that kin networks are not just passing information on to husbands. Second, treatment assignments within the experiment were randomized within villages, allowing us to explore the association between community outcomes and income hiding in the lab. Third, we link decisions in the experiment to a model of individual investment choices when risk preferences are heterogeneous, and estimate this model via maximum simulated likelihood. This allows us to recover an estimate of the "kin tax" parameter, and to simulate the magnitude of its impact on microentrepreneurship.

The rest of this paper is organized as follows: Section 2 describes our experimental design and procedures; Section 3 presents a simple theoretical framework for interpreting our results; Section 4 presents our main reduced-form empirical results; Section 5 presents our structural framework and estimates; and Section 6 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.[4] Each subject divided her endowment between a zero-risk, zero-interest savings account and an investment which was risky but profitable in expectation. The subject 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 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 large endowment. However, the distribution of endowments was common knowledge, so all subjects were aware that half the participants received an extra 100 shillings.

Every player was also assigned to either the **private** treatment or one of two public information treatments, the **public** treatment or the **price** treatment. Participants assigned to the **private** treatment were able to keep their investment income secret: the decisions they made in the experiment were never disclosed to other participants. In contrast, those assigned to the **public** 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.[5] The amounts that subjects invested in the zero-interest savings technology were never revealed, so those who received the larger amount could choose whether to invest

[4]Experimental sessions were conducted between August 3 and October 1, 2009. Over that period, the value of the US dollar relative to the Kenyan shilling fluctuated between 74.5 and 77.25 shillings to the dollar. We report dollar amounts using the exchange rate 75.9 shillings to the dollar, which is the average over that period. The two endowments were equivalent to 1.05 and 2.35 U.S. dollars, respectively. Among our subjects, the median monthly wage for individuals in full-time, unskilled employment is 2000 shillings (26.35 U.S. dollars), and the median monthly wage for subjects in full-time work in the skilled sector is 4750 shillings (62.58 U.S. dollars).

[5]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.

80 shillings or less so as to obscure their endowment size. Finally, those assigned to the **price** treatment were obliged to make the public announcement revealing their investment returns unless they preferred to pay a price, p, to avoid making the announcement. Prices ranged from 10 to 60 shillings, and were randomly assigned to subjects in the 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 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.[6]

Random assignment to treatment generated exogenous variation in the observability of investment returns and created costly opportunities to hide income. Assignment to the public information treatments meant that outcomes were verifiable, and might therefore facilitate risk-pooling and, consequently, risk-taking. On the other hand, if subjects face social pressure to share income with neighbors and kin, they might be willing to pay for obscurity when returns are visible.[7] In particular, the experiment creates two mechanisms through which subjects could incur a cost to hide income from others. First, those receiving the larger endowment could keep their endowment size secret by investing no more than 80 shillings. Second, subjects in the price treatment could pay the randomly assigned price, p, to conceal their income entirely.

2.2 Experimental Procedures

Experiments were conducted in 26 rural, predominantly agricultural communities in western Kenya.[8] 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.[9] All households within each village were invited to send members to participate in the experimental economic game session the

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

[7]Our design is related to those of Barr and Genicot (2008), Attanasio, Barr, Cardenas, Genicot, and Meghir (2012), and Ligon and Schechter (forthcoming), who also use experiments which capture the desire to avoid social sanctions outside the lab.

[8]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.

[9]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.

following day. 80.4 percent of households contacted prior to the sessions chose to participate.[10] 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 subjects. 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, corresponding to the three information conditions (private, public, price) interacted with the two endowment sizes. Within each stratum, players were randomly assigned to each of the six treatments with equal probability.[11] Players assigned to the price treatments were subsequently assigned a random price from the set of multiples of ten between 10 and 60.[12]

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.[13] 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 make sure that subjects 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, or price treatment. Those who were assigned to the 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 subject was handed a number of 10 shilling coins equivalent to her endowment; the participant

[10]In Table of Appendix III (online), we examine the correlates of choosing to send a household member to the experimental session. Participants were broadly representative of the overall population, though as expected larger households were more likely to send a member to the experiment. Women from poorer households and those with relatively higher math skills were more likely to attend. Men were more likely to attend if they attended church the previous week. Subjects in more isolated communities — as measured by distance from a paved road — were more likely to participate, though this cannot bias our results since assignment to treatment occurs within villages.

[11]The first experimental session did not include the two price treatments, so players were assigned to each of the other four treatments with equal probability.

[12]Note that each session included subjects assigned to all three information conditions, so it was impossible to infer which individuals chose to pay to avoid the public announcement, since these subjects could not be distinguished from those randomly assigned to the private treatments.

[13]Detailed instructions are included in Appendix II (online).

divided these coins between a "savings" cup and a "business" cup.[14] After recording a subject's investment decision, the enumerator would give the subject a one shilling coin to flip.[15] 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 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 payoff. 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 immediately after receiving her money. Figure 1 summarizes the progression of activities within the experiment.

2.3 Experimental Subjects

Sessions were conducted in Kenya's Western Province, in three adjoining districts: Bunyala, Samia, and Butula.[16] 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. 61 percent of subjects were female. Respondents ranged in age from eighteen 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 full-time employment was $2,050$ Kenyan shillings, or 1.35 USD per day (assuming twenty work

[14]The swahili words for "savings" and "business" are, respectively, "akiba" and "biashara." The savings cup was marked with a large letter "A" and the business cup was marked with a large letter "B." The two cups made of plastic and were identical in size and color.

[15]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.

[16]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.

days per month). 35.0 percent of subjects operate their own business enterprise; among these businesses, only 11.6 percent have any employees. 16.7 percent of participants have bank savings accounts,[17] and 52.8 percent are members of rotating savings and credit associations (ROSCAs).[18]

Most experimental subjects in our sample live amongst their kin, and are embedded in interhousehold transfer networks. The median participant has seven relatives living outside her household but in the same village: two close relatives (parents, parents-in-law, adult children, grandparents, siblings, aunts, and uncles) and 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 over the same period.[19] 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.[20]

Table 1 reports tests of balance across our six experimental treatments.[21] The randomization was successful, generating minimal differences in observables across treatments. Of 68 tests (34 for each gender) reported, we observe only five significant differences in observables across treatments.

[17]Dupas and Robinson (forthcoming–a) 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.

[18]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 (forthcoming–a) show that female daily income earners make more productive investments when given access to even a costly savings account.

[19]There are several reasons for the asymmetry between the probability of making a transfer and the probability or receiving a transfer. First, households comprising only children and/or the ill or handicapped could not participate (because only those adults who could be physically present for the session and were able to hear the oral instructions could take part), but are likely to be net recipients of transfers. Another reason is that most households in any village will make a small transfer toward the funeral expenses of their neighbors; since funerals are relatively rare events, we would expect to see an asymmetry between the likelihood of giving and receiving in the short-term. Finally, transfer data are quite noisy, and households are more likely to report transfers made than transfers received (Comola and Fafchamps 2011).

[20]A *harambee* 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).

[21]We show that the randomization of the price of avoiding making an announcement was successful, though the randomization was not stratified, in Table 2 of Appendix III (online).

Though the number of distant family members living in one's village differs significantly across treatments in the sample of men, this variation is driven by outliers: the maximum number (within a treatment) of distant relatives reported to live in one's village ranges from 72 to 154. 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). Among women attending the experiment, there is a significant difference in the number of televisions owned across treatments, and a marginally significant difference in the number of cows, but no difference in the total value of durable household assets.

3 Theoretical Framework

In this section, we outline a simple theoretical model of individual decisions within the experiment. Subjects faced a finite set of investment options — nine in the small endowment treatments and nineteen in the large endowment treatments. However, we present decisions in a continuous, non-stochastic framework in order to derive simple comparative statics that yield testable predictions in reduced-form regressions. In Section 5, we present a discrete version of the model that aligns precisely with the choices subjects faced, and that allows us to recover the structural parameters of interest through maximum likelihood estimation.

3.1 Individual Decisions in Private Information Treatments

In all 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 interest 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 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

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

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.[22]

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.

[22]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 restrict attention to the CRRA case.

3.2 Individual Decisions in Public Treatments

Next, we consider how individual i's optimization problem changes when she is obliged to stand up and announce her investment income. After making the public announcement, 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.[23] For example, if it is common knowledge that each villager receives a grant of a certain size, then that income is observable even if it is not distributed publicly. Within the experiment, the implication is that a subject 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 assigned to the private treatment can plausibly claim to have invested and lost all or most of her endowment, and a subject 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 $b_{i,s}^{pub} \leq m_s$ such that

$$b_{i,s}^{pub} = \text{argmax}_{b_{i,s} \leq m_s} \; \frac{1}{2} \frac{\left[(1-\tau_i)\,(m_s - b_{i,s})\right]^{1-\rho_i}}{1-\rho_i} + \frac{1}{2} \frac{\left[(1-\tau_i)\,(m_s + 4b_{i,s})\right]^{1-\rho_i}}{1-\rho_i} \tag{4}$$

Since the $1 - \tau_i$ term drops out of the first-order condition characterizing the optimal interior solution, individuals receiving the small 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 small endowment in the public treatment.

Proposition 1. *If individual i receives the small endowment, m_s, then her optimal investment in*

[23] See De Mel, McKenzie, and Woodruff (2009) for a closely related modeling approach.

the business cup in the public treatment is equal to her optimal investment in the private treatment:

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

Expected utility is lower in the public treatment than in the private treatment when $\tau_i > 0$. Expected utility is decreasing in τ_i for all $\tau_i \in [0, 1]$.

The proof follows directly from the solution to i's expected utility maximization problem. Since $b_{i,s}^{pub} = b_{i,s}^{pri}$, whenever $\tau_i > 0$, expected utility must be lower in the public treatment because consumption is lower than in the private treatment in both outcome states.

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 small endowment. The discrete change in observable income means that both utility and marginal utility are discontinuous at $b = m_s$. Recall that $d = m_l - m_s$. An individual who invests $b > m_s$ in the public treatment will choose

$$\begin{aligned} b_{i,l}^{*} &= \text{argmax}_{b_{i,l} > m_s} \frac{1}{2} \frac{\left[\left(1-\tau_i\right)\left(m_s - b_{i,l} + d\right)\right]^{1-\rho_i}}{1-\rho_i} + \frac{1}{2} \frac{\left[\left(1-\tau_i\right)\left(m_s + 4b_{i,l} + d\right)\right]^{1-\rho_i}}{1-\rho_i} \\ &= \left(\frac{4^{1/\rho_i} - 1}{4^{1/\rho_i} + 4}\right)\left(m_s + d\right), \end{aligned} \tag{5}$$

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

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, $b_{i,l}^{pri}$, exceeds m_s;*

2. *i's optimal investment in the public, large endowment treatment takes one of two values:*

$$b_{i,l}^{pub} \in \left\{m_s, b_{i,l}^{pri}\right\}; \tag{6}$$

3. *and, finally, there exists $\underline{\tau}\left(\rho_i\right) \in (0, 1)$ such that $b_{i,l}^{pub} = m_s$ if and only if $\tau_i \geq \underline{\tau}\left(\rho_i\right)$.*

Proposition 2 demonstrates that individuals with sufficiently low levels of risk aversion, who would invest more than m_s in the private, large endowment treatment, will either do the same in the public treatment or will invest exactly m_s, the highest level of investment which keeps endowment size hidden. Whether the latter strategy is optimal depends on the level of social pressure to share one's winnings.

In contrast, an individual who invests $b \leq m_s$ in the public, large endowment treatment will choose

$$\begin{aligned} b^*_{i,l} &= \text{argmax}_{b_{i,l}<m_s} \; \frac{1}{2}\frac{\left[(1-\tau_i)\left(m_s - b_{i,l}\right)+d\right]^{1-\rho_i}}{1-\rho_i} + \frac{1}{2}\frac{\left[(1-\tau_i)\left(m_s + 4b_{i,l}\right)+d\right]^{1-\rho_i}}{1-\rho_i} - \kappa_i \\ &= \left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right)\left(m_s + \frac{d}{1-\tau_i}\right). \end{aligned} \tag{7}$$

Since $0 < 1-\tau < 1$,

$$\left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right)\left(m_s + d\right) < \left(\frac{4^{1/\rho_i}-1}{4^{1/\rho_i}+4}\right)\left(m_s + \frac{d}{1-\tau_i}\right); \tag{8}$$

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 $\rho_i > \underline{\rho}$, there exists a threshold social pressure parameter, $\bar{\tau}(\rho_i)$ such that:*

1. *i's optimal investment in the public, large endowment treatment, b^{pub}_{i,m_l}, falls in the interval:*

$$b^{pub}_{i,l} \in \left(b^{pri}_{i,l}, m_s\right) \tag{9}$$

   if and only if $\tau_i < \bar{\tau}(\rho_i)$;

2. *otherwise, for all $\rho_i > \underline{\rho}$ and $\tau_i \geq \bar{\tau}(\rho_i)$, $b^{pub}_{i,l} = m_s$.*

Propositions 2 and 3 partition (ρ_i, τ_i) space into four cases which are depicted in Figure 2. Whether individuals receiving the large endowment invest above or below m_s in the private treatment depends on risk aversion. The more risk averse individuals invest below m_s in the private treatment (Regions A and B in Figure 2), while those who are less risk averse invest above m_s in the private treatment (Regions C and D in Figure 2). Regions A and B are separated from Regions C and D at $\rho = \underline{\rho}$. The formula for $\underline{\rho}$ is given in Appendix Equation 42; $\underline{\rho}$ equals $\ln(4)/\ln(5) \approx 0.861$ for the endowment sizes used in this experiment.

As described in Proposition 2, less risk averse types facing significant social pressure to share income (Region C) invest less in the public, large endowment treatment than they do in the private, large endowment treatment: they reduce their investment to exactly m_s. The behavior of less risk averse types who face little social pressure (Region D) is not affected by assignment to the public treatment.[24] However, more risk averse types facing any nonzero social pressure (Regions A and B) increase investment when returns are announced publicly. This is because the first-order condition characterizing optimal investment changes when only a portion of income is taxed, as shown in Inequality 8 and described in Proposition 3. How much risk averse types increase investment depends on the levels of social pressure and risk aversion: those facing lower social pressure (Region A) invest more when investment returns are announced publicly, but still less than m_s; those facing higher social pressure (Region B) shift up to exactly m_s in the public treatment.[25] Figure 3 provides additional intuition, depicting expected utility in the public and private treatments as a function of the level of investment.

3.3 Individual Decisions in Price Treatments

We now consider individual decisions in the price treatments, where subjects can choose to pay a randomly chosen price, $p > 0$, to avoid making the public announcement. Observe that subject i receiving gross payout $x_i > 0$ in the small endowment treatment prefers to pay p whenever

$$\frac{(x_i - p)^{1-\rho_i}}{1-\rho_i} \geq \frac{(1-\tau_i)^{1-\rho_i} x^{1-\rho_i}}{1-\rho_i}, \tag{10}$$

[24]The boundary between Regions C and D is given by the relationship between $\underline{\tau}$ and ρ. Along this boundary, the utility-maximizing investment above m_s, given in Equation 5, yields the same utility as investing exactly m_s (thereby hiding d). This boundary does not appear analytically tractable, but can be calculated numerically.

[25]Along the boundary between A and B, $\bar{\tau}$ can be expressed analytically by solving for $\bar{\tau}$ as a function of ρ (for $\rho > \underline{\rho}$) for which the interior solution maximizing utility conditional on investing at most m_s, shown in Equation 7, is to invest exactly m_s. The formula for $\bar{\tau}$ is given in Appendix Equation 80.

or $p \leq \tau_i x_i$. Similarly, a subject in the large endowment treatment prefers to pay p to avoid the public announcement whenever

$$\frac{(x_i - p)^{1-\rho_i}}{1-\rho_i} \geq \frac{[(1-\tau_i)x + \tau_i d \cdot \mathbb{1}\{b_i \leq m_s\}]^{1-\rho_i}}{1-\rho_i}, \tag{11}$$

or $p \leq \tau_i x_i - \tau_i d \cdot \mathbb{1}\{b_i \leq m_s\}$. It is apparent that, for any fixed $0 \leq \bar{p} < x$ that makes i exactly indifferent between paying $\bar{p}$ and making the public announcement, i strictly prefers to pay p for all $p < \bar{p}$ and strictly prefers not to for all $p > \bar{p}$. Similarly, for any fixed $\bar{x}$ which makes i precisely indifferent, she strictly prefers to pay p whenever her payout is above $\bar{x}$ and strictly prefers to make the public announcement whenever her gross payout is below $\bar{x}$. Hence, we need only consider three strategies for subjects in the price treatments: always paying p to avoid the public announcement, never paying p, and paying p when the investment is successful, but not when it is unsuccessful.[26]

When p is sufficiently high, never paying p is the optimal strategy, and individual investment decisions will be identical to those in the public treatments. For example, consider a subject in the small endowment treatment facing a τ of 0.10 who invests half her endowment in the business cup. If her investment is successful, her gross payout is 240 shillings, so she would be indifferent between announcing her investment return and paying a price of 24 shillings to avoid the announcement. For higher values of p, her investment decisions will be the same as in the public treatments.

When $\tau_i > 0$ and p is sufficiently low, always paying p to avoid the public announcement is optimal. In this case, the optimal investment level is given by

$$b_i^{always} = \left(\frac{4^{1/\rho} - 1}{4^{1/\rho} + 4}\right)(m_i - p). \tag{12}$$

Hence, investment levels are equivalent to those in the private information treatments, but with a smaller budget of $m_i - p$, accounting for the fact that subjects always plan to pay p to avoid making the announcement. Following directly from Equation (12), we can see that for those subjects who plan to always pay to avoid making the public announcement, optimal investment

[26] In other words, it will never be optimal for a subject to pay p when her investment is unsuccessful but not when it is successful. In Section 5, we refer to these strategies as always, never, and heads, respectively.

levels are decreasing in p.

For intermediate values of p and $\tau > 0$, it is optimal to follow a strategy of paying to avoid the announcement only when the investment succeeds. Predictions in this setting are less clear-cut, and we defer consideration of this case to Section 5.

3.4 Extensions to the Model

In this section, we explore the possibility that individual behavior in the public information treatments might be influenced by factors other than social pressure to share income. We consider two such alternative explanations. First, we discuss the hypothesis that subjects might be willing to pay to avoid the public announcement because they are averse to attention or public scrutiny. We refer to this as "attention aversion." We then explore the possibility that risky behaviors — including investing in the business cup — might be socially sanctioned.

3.4.1 Attention Aversion

A straightforward model of participants' desire not to draw attention to themselves — without regard to any effective taxation — is to incorporate an additive cost parameter, κ, into the utility function:

$$u_i^{attention}(x) = \begin{cases} u_i(x) & \text{if no announcement is made} \\ u_i(x) - \kappa & \text{if public announcement occurs} \end{cases} \tag{13}$$

This parameter, κ, does not affect the first order condition characterizing the optimal allocation decision. It will therefore have no impact on the likelihood of investing exactly 80 shillings in either the public treatment or the price treatment (when the price of giving is high). In light of this, our reduced-form results on investment decisions are evidence of social pressure, and cannot be driven by the desire to avoid public attention. However, within the price treatment, $\kappa > 0$ would influence the willingness to pay to avoid making the public announcement. We return to this point in Section 5, when we explicitly estimate the κ parameter to test whether it is a significant factor driving decisions in the price treatments.

3.4.2 Social Sanctions Against Risk-Taking

Another alternative hypothesis is that differences in investment choices between the private and public information treatments stem from the desire to avoid potential social sanctions against risk-taking — for example, if investing in the business cup were perceived as similar to gambling. We made it clear to participants that there was no way for them to realize an actual loss during the experiment, and used the "business cup" framing to push this point more subtly. However, we can also examine the data for patterns that would suggest that participants were concerned about hiding their risky investments from others.

We consider two possible strategies for modeling social sanctions against risk-taking. First, if investing any positive amount were seen as gambling and subjects wished to avoid being observed taking such risks, we should see a larger fraction of participants choosing to invest zero in the public information treatments than in the private treatments. Alternatively, if the size of the sanction against gambling were increasing in the amount invested in the business cup, this would add a downward sloping cost to the expected utility function, thereby reducing the optimal investment amount, b^*. This would lead to an overall reduction in investment levels in the public information treatments relative to the private treatments, and a particularly strong move away from large investment amounts if the function were convex; however, this would not lead to an increase in the proportion of subjects investing exactly the amount of the small endowment.

4 Results

In this section, we characterize individual choices within the experiment and estimate the impacts of observability on investment decisions and final payoffs. We report results relating to investment decisions in Section 4.1, and describe decisions regarding whether to pay to avoid the public announcement in Section 4.2.

Summary statistics on outcomes in the experiment are presented in Table 2. Averaging across all treatments, subjects earned approximately 240 Kenyan shillings (3.16 US dollars) in the experiment, which is equivalent to eight percent of mean monthly wages among subjects reporting paid employment. Thus, stakes were large, but not life-altering. There is substantial variation in payoffs across across treatments: subjects in the public, small endowment treatment received the lowest payoffs, averaging 139 Kenyan shillings (1.83 US dollars); those in the private, large endowment treatment earned the most, on average 355 Kenyan shillings (4.68 US dollars).

4.1 Individual Investment Decisions

On average, subjects chose to invest just over half their endowment in the business cup (Table 2). The fraction of the endowment invested is similar in the large and small endowment treatments. Among those receiving the larger endowment, the amount invested is slightly lower in the two public information treatments (the public and price treatments) than in the private treatment. When allotted 180 shillings, subjects could avoid revealing that their endowment exceeded that of others by investing 80 shillings or less; the probability of doing so is higher in the public information treatments than in the private treatment.

We present histograms of investment decisions in Figure 4. Dark bars represent the distribution of investment decisions in the private treatments; lighter bars represent investment decisions in the two public information treatments. As predicted by the model, decisions are similar in the public and private small endowment treatments. However, women receiving the large endowment are more likely to invest exactly 80 shillings in the public information treatments than in the private, large endowment treatment. A Kolmogorov-Smirnov test rejects the hypothesis that the two distributions are equal (p-value 0.068). There is no difference in behavior across treatments among men receiving the large endowment.

We now examine the impact of observability on investment decisions in a regression framework, estimating the effects of assignment to one of the public information treatments on the amount of money invested in the business cup. We estimate the OLS regression:

$$Investment_{itv} = \alpha + \beta_1 Public_i + \beta_2 Large_i + \beta_3 Public \times Large_i + \eta_v + X_i'\zeta + \varepsilon_{itv} \tag{14}$$

where $Investment_{itv}$ is the amount invested in the business cup by subject i assigned to treatment

t in village v, $Public_i$ is an indicator for assignment to either the public or the price treatment, $Large_i$ is an indicator for receiving the large endowment, $Public \times Large_i$ is an interaction between $Public_i$ and $Large_i$, η_v is a village fixed effect, and X_i' is a vector of individual controls including dummies for age and education categories, marital status, household size, and the log value of household durable assets.[27] Standard errors are clustered at the village level in all specifications.

Results, disaggregated by gender, are reported in Table 3. Odd-numbered columns report regression results without controls; even-numbered columns include them. The $Public \times Large_i$ interaction is negative and significant among women (Columns 1 and 2), but positive and insignificant among men (Columns 3 and 4). The coefficient estimates indicate that women receiving the large budget invest approximately 6.3 shillings, or 6.5 percent, less when returns are observable than when they are hidden. Adding village fixed effects and additional controls has almost no impact on estimated coefficients and significance levels.

Next, we test whether the observed changes in investment levels are consistent with women attempting to obscure the size of their endowments. We test the hypothesis, predicted by the model, that subjects are more likely to invest exactly 80 shillings when investment returns are observable by estimating probit regressions of the form

$$\Pr\left[Investment = 80\right] = \Phi\left(\alpha + \beta Public_i + X_i'\zeta\right) \tag{15}$$

among the sample of subjects randomly assigned to the large endowment treatments, as well as a linear probability model (LPM) where the dependent variable is an indicator for investing exactly 80 shillings (Table 4, Panel A). In all specifications, women are significantly more likely to invest exactly 80 shillings in the public information treatments. For example, the LPM estimates indicate that women are 6.2 to 7.0 percentage points more likely to invest 80 shillings when investment returns are observable. The coefficient on *Public* in the sample of men is not significant in any specification, in line with our previous results.

[27] As discussed in Section 3, decisions in the price treatments will be identical to decisions in the public treatments when p is sufficiently high, so we pool data from both public information treatments to maximize power. Coefficient estimates are similar when data from the price treatments is omitted.

Moving beyond the precise predictions of the model presented in Section 3, we also estimate similar specifications using indicators for investing either 70 or 80 shillings (Table 4, Panel B) and for investing any amount less than or equal to 80 shillings (Table 4, Panel C). Since only 2.3 percent of women and 6.0 percent of men in the private, small endowment treatment actually invest 80 shillings, subjects might feel that investing 70 shillings better obscures the size of their endowment.[28] In more general terms, any investment level less than or equal to 80 shillings obscures the size of the subject's endowment, and may be seen as consistent with a desire to hide a portion of the large endowment from others. In all specifications, the *Public* variable is positive and significant in the sample of women, but insignificant in the sample of men. Women receiving the large endowment are between 7.0 and 7.6 percentage points more likely to invest 70 or 80 shillings and between 9.6 and 10.5 percentage points more likely to invest 80 shillings or less than those in the private, large endowment treatment.

4.1.1 Treatment Effect Heterogeneity

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. Under this hypothesis, 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 outside the household as the group most likely to pressure individuals into sharing income.[29] We test this by interacting the *Public* variable with indicators for whether or not a subject had any close kin attending the experiment, thereby disaggregating the impact of the public information treatments.[30] We estimate

[28] Investing less than, but not exactly 80 shillings may reflect level-k reasoning, along the lines first discussed by Nagel (1995), Stahl and Wilson (1994), and Stahl and Wilson (1995).

[29] Hoff and Sen (2006) highlight the role played by kin networks in extracting surplus from successful relatives, while Baland, Guirkinger, and Mali (2011) provide evidence that individuals seek to hide income from family members.

[30] We define close kin as parents, grandparents, siblings, grown children, and aunts and uncles. Note that we were unable to stratify treatment assignment by kin presence because the variable was constructed after experimental sessions took place using a name-matching algorithm.

the OLS regression equation

$$\begin{aligned} Y_{itv} = \alpha &+ \beta_1 KinPresent_i + \beta_2 Public \times KinPresent_i \\ &+ \beta_3 Public \times NoKinPresent_i + \eta_v + X_i'\zeta + \varepsilon_{itv} \end{aligned} \tag{16}$$

where Y_{itv} is one of three outcomes of interest: investment level, an indicator for investing exactly 80 shillings, or an indicator for investing no more than 80 shillings.[31] We restrict the sample to subjects assigned to the large endowment treatments, who could obscure the size of their endowment by investing 80 shillings or less. We include village fixed effects (η_v) and controls for age and education categories, HH size, marital status, and household durable asset holdings in all specifications.

Results are reported in (Table 5, Panel A). Women with close kin attending the experiment invest less in the public information treatments and are more likely to invest exactly 80 shillings and no more than 80 shillings. The coefficient estimates are extremely large. Women with relatives present invest 24.5 shillings less (p-value 0.0175) when investment returns are observable than when they are hidden, which is equivalent to more than a twenty percent reduction in investment relative to the private information treatment. These women are also 14.9 percentage points more likely to invest exactly 80 shillings (p-value 0.0491) and 41.8 percentage points more likely to invest 80 shillings or less (p-value 0.0012). In contrast, point estimates are smaller and not statistically significant for women with no kin attending the experiment. As expected, we also find no statistically significant results for men.

An alternative hypothesis is that kin are significant because they pass information about wives' incomes to their husbands.[32] This is of particular concern because 76.5 percent of women in our sample are married, and the two main ethnic groups in the area — the Luhya and the Luo — are both patrilocal (Brabin 1984, Luke and Munshi 2006), so married women typically live near their in-laws rather than their blood relatives. We present two pieces of evidence which suggest that kin are not simply conveying information to husbands.

[31] We estimate linear probability models for the two binary outcomes. Probit results are similar in magnitude and significance, and are omitted to save space.

[32] 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.

First, we estimate Equation 16 replacing the kin variables with indicators for whether or not a subject's spouse attended the experiment (Table 5, Panel B). If kin were only important because they shared information with husbands, we would expect the direct effect of a husband's presence to be at least as large as the impact of having kin at the experiment. However, we find that the interaction between $Public$ and the indicator for having a spouse present is not significantly associated with the amount invested or the probability of investing either exactly 80 shillings or 80 shillings or less. Moreover, the $Public \times NoSpousePresent$ variable is significantly associated with the probability of investing exactly 80 shillings and 80 shillings or less, suggesting that the main impact of the public treatment is on women whose husbands were not present.

We also examine investment decisions among the small sample of women receiving the large endowment who are not married and have close kin attending the session. These women invest an average of 95.7 shillings in the private treatment versus 84.0 shillings in the public information treatments, and they are 51.4 percentage points more likely to invest 80 shillings or less in the public treatments. These differences are not statistically significant in this twelve subject sub-sample, but it nonetheless appears that women with kin present are concerned about observability even in cases where the kin cannot possibly pass information on to husbands.

4.1.2 Income Hiding across Villages

Next, we explore the association between the level of income hiding, aggregated within a community, and village-level outcomes. If pressure to share income does, in fact, act like a tax on investment returns, we would expect to see a correlation between the level of social pressure within a community and a range of development indicators. We explore this by constructing a village-level measure of income hiding motivated by the theoretical framework presented in Section 3. Among those receiving the large endowment, the model suggests that greater pressure to share income will lead to an increase in the likelihood of investing exactly 80 shillings when returns are observable. In light of this, we create a village-level measure of the extent of income hiding: the difference between the proportion of subjects investing exactly 80 shillings in the two public information, large endowment treatments and the proportion investing exactly 80 shillings in the private, large endowment treatment.[33] For each village in our sample, we create a measure of income hiding among women, and a separate measure of income hiding among men.

Across all 26 communities, the average level of income hiding among women is 7.27 percent; the 95 percent confidence interval for the mean level of income hiding across villages is $[-0.002, 0.147]$. Unsurprisingly, the average level of income hiding among men is substantially lower, only 1.14 percent; the 95 percent confidence interval, $[-0.085, 0.108]$, suggests that the variation in the measure of income hiding among men may be largely attributable to noise. In light of this and our earlier results, we focus on the associations between income hiding among women and village-level outcomes.

Figure 5 plots the relationship between our measure of income hiding among women and four village-level outcome variables: the log value of durable household assets, the fraction of subjects in either formal or skilled employment, the average level of wages from paid work, and the mean share of farm households that used fertilizer in the last year.[34] All estimated relationships are negative, suggesting that more income hiding is associated with lower levels of household wealth and regular employment.

We explore these relationships in a regression framework in Table 6. In Column 1, we estimate the relationship between log household durable assets and income hiding among women. In spite of the small sample size, the estimated relationship is marginally significant (p-value 0.070). In Column 2, we include village-level controls for distance from the village to the nearest paved road, the mean education level of subjects from that community, the mean number of close relatives in the village, and the mean number of community groups in which subjects participate. After adding these controls, the estimated association between income hiding and household assets becomes slightly larger and more significant (p-value 0.0252).

In the remainder of the table, we replicate these specifications using the other three outcome variables of interest; we find similar significant, negative relationship in all specifications. Higher

[33] All results presented in this section are nearly identical if we use the difference between the proportion investing exactly 80 shillings in the public, large endowment treatment (only) and the proportion investing exactly 80 shillings in the private, large endowment treatment, omitting subjects who participated in the price treatment.

[34] We include the last outcome in light of evidence presented in Duflo, Kremer, and Robinson (2011) and Giné, Goldberg, Silverman, and Yang (2012) that farmers in Kenya and Malawi, respectively, often plan to use fertilizer, but are unable to save the money to purchase it in the lean season which occurs after planting but before the harvest.

levels of income hiding within the experiment are associated with lower rates of skilled and formal employment and lower wages, and are also associated with substantial reductions in the probability of using fertilizer.

Such cross-community results do not merit a causal interpretation. Nonetheless, these findings are of interest for two reasons. First, results are consistent with the view that pressure to share income may act as a drag on savings and investment, and may therefore slow development. An alternative, though not mutually exclusive, explanation is that in the poorest areas, enforced sharing norms remain an essential tool for handling risk, and thus the pressure to share is the most acute. In either case, the evidence suggests that social pressure to share income is greatest in the worst off areas, and should be viewed as an important social factor in models of poor, rural village economies. More generally, these results go some way toward addressing the concern that individual choices in lab experiments may not be meaningfully correlated with decisions and outcomes outside the lab (cf. Levitt and List 2007); in this case, they clearly are.

4.2 The Willingness-to-Pay to Hide Income

We now examine the willingness-to-pay to avoid the public announcement using data from the price treatments. Subjects assigned to the price treatments were offered the option of paying a randomly chosen price — 10, 20, 30, 40, 50, or 60 shillings — to avoid announcing their investment income to the other subjects.[35] Of 690 subjects assigned to the price treatments, 627 subjects could afford to pay p to avoid the public announcement, and 190 (30.3 percent of those able to pay) chose to do so. Subjects who chose to buy out paid an average price of 29.3 shillings — equivalent to an average of 15.3 percent of their gross earnings. Figure 6 plots the proportion of subjects paying to avoid the public announcement, broken down by gender, endowment size, and exit price. Subjects are generally more likely to buy out at lower prices, though this is not as evident among men receiving the large endowment.

Table 7 reports OLS regressions of the probability of paying to avoid the public announcement

[35] Though this randomization was not stratified, it was largely successful. Table 2 in Appendix III (online) reports the results of a balance check exercise in which subject characteristics were regressed on dummies for prices 20 through 60. The price variables are jointly significant at the five percent level in only four of the 68 F-tests.

on exogenous factors: the randomly assigned price and indicators for receiving the large endowment and having the coin land with heads facing up. Women 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 approximately 23 percentage points more likely to pay to avoid announcing their investment return. Men, in contrast, are not significantly more likely to pay to avoid the announcement when their investments are successful. As suggested by the figures, both men and women are less inclined to buy out at higher exit prices: the coefficient on the price variable is negative and significant in all specifications.

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. As the model suggests, the relationship between pressure to share income, individual risk preferences, and the unconditional probability of paying to avoid the public announcement is complicated; and we observe whether a subject pays the exit price after a particular realization of the coin flip, but not whether she planned to never buy out, buy out if her investment was successful, or always buy out. In light of this, we defer consideration of the explicit willingness to pay to Section 5.

Table 8 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 price treatments. The model predicts that for subjects planning to pay to avoid the public announcement whether or not their investment was successful, the amount invested will be a decreasing function of the price avoiding the public announcement. Consistent with this prediction, we find that 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 women (Columns 1 and 2) and men (Columns 3 and 4).

5 Estimating the Social Pressure Parameter

In this section, we estimate the magnitude of the social pressure parameter, τ, while controlling for unobserved heterogeneity in individual risk aversion. As discussed above, the treatment effects reported in Section 4 demonstrate that among women τ is not equal to zero: making investment returns observable impacts individual choices. However, the magnitude of the reduced form impact does not identify τ because the treatment effect depends on an individual's level of risk aversion. Fortunately, random assignment should insure that the distribution of individual risk parameters in each of the six treatments is representative of the population distribution. We can therefore estimate the parameters of that distribution using data from the private treatments, and estimate τ while controlling for heterogeneity in risk aversion in a structural framework.

We model decisions in a discrete choice setting which mirrors our implementation of the portfolio choice problem in the experiment: each subject decides how many ten shilling coins to allocate to the business cup, and allocates the remainder to the savings cup. Subjects in the small endowment treatments have nine options $(0, 10, 20, \ldots, 80)$, while those assigned to the large endowment treatments have nineteen $(0, 10, 20, \ldots, 180)$.

In what follows, we index experimental treatments with t, individual subjects with i, and investment options with j and, when necessary, k. We assume that subject i's expected utility of investing b_j is given by:

$$EU_{ij} = EV_{ij} + \varepsilon_{ij} \tag{17}$$

where EV_{ij} denotes the explicitly-modeled expected utility of investing b_j (what Train (2003) terms "representative utility") and ε_{ij} is an i.i.d. type 1 extreme value distributed preference shock.[36] Behavior in the private treatments suggests that individuals have heterogeneous risk preferences. As in Section 3, we assume these can be represented by a CRRA utility function, $v_i(\cdot)$, parameterized by ρ_i, and that individual ρ_i parameters are normally distributed with mean μ_ρ and variance σ_ρ^2. The probability that subject i chooses to invest b_j can then be written in the form of a mixed logit model:

$$P_{ij} = \int \left(\frac{e^{EV_{ij}/\sigma_\varepsilon}}{\sum_{k=1,\ldots,J_t} e^{EV_{ik}/\sigma_\varepsilon}} \right) f(\rho)\, d\rho. \tag{18}$$

where σ_ε^2 is proportional to the variance of $\varepsilon_{ij} - \varepsilon_{ik}$.[37] Let y_{ij} be an indicator function equal to one

[36] Loomes (2005) refers to such error terms as "Fechner errors." See Hey and Orme (1994) and Von Gaudecker, van Soest, and Wengström (2011) for examples of their use in modeling stochastic choices in individual decision-making experiments.

[37] When $V_{ij} = X'\beta$, β and σ_ε are not separately identified; σ_ε is identified in our framework because EV_{ij} is a

if subject i chooses to invest amount b_j. The log-likelihood function for treatment t can be written as:

$$LL_t = \sum_{i \in I_t} \sum_{j \in J_t} y_{ij} \ln \left[\int \left(\frac{e^{EV_{ij}/\sigma_\varepsilon}}{\sum_{k=1,\ldots,J_t} e^{EV_{ik}/\sigma_\varepsilon}} \right) f(\rho) \, d\rho \right]. \tag{19}$$

The log-likelihoods can then be summed across treatments. We simulate the log-likelihood by taking one thousand random draws from the distribution of ρ_i for each individual, and maximize the simulated log-likelihood numerically.[38]

5.1 Heterogeneity in Individual Risk Preferences

We express the CRRA utility function as

$$v(x|\rho_i) = \frac{1}{\eta_i} x^{1-\rho_i}, \tag{20}$$

where $\eta_i = 900^{1-\rho_i} - 10^{1-\rho_i}$, the difference between the utility of the highest possible payout in the experiment and the utility of the lowest strictly positive payout.[39] Since VNM expected utility functions represent preference orderings over lotteries and are robust to positive, affine transformations, this utility function represents the same preferences as the standard CRRA formulation,

$$u(x|\rho_i) = \frac{x^{1-\rho_i}}{1-\rho_i}. \tag{21}$$

However, in a mixed logit framework, the probability of choosing investment option b_j depends on the magnitude of the difference between EV_{ij} and the utilities associated with other options, and not just the position of b_j in the preference ordering. Hence, the scale of EV_{ij} is directly related to the likelihood of choosing an investment option, b_k, that is less-preferred in the sense that

non-linear function of parameters.

[38] Our implementation follows the procedures outlined in Train (2003). We implement this using the MATLAB command `fminunc` using the default Broyden-Fletcher-Goldfarb-Shanno (BFGS) updating procedure. Standard errors are calculated using the inverse Hessian.

[39] See Goeree, Holt, and Palfrey (2003) for a similar approach. Using the lowest possible payout, zero, is not feasible because $u(x) \to -\infty$ as $x \to 0$ for subjects with $\rho \geq 1$. Results are almost identical when $10^{1-\rho}$ is replaced with $1^{1-\rho}$ or $0.01^{1-\rho}$.

$EV_{ik} < EV_{ij}$.[40] The standard normalization of the CRRA utility function leads to very different scalings of the utility function across the range of feasible ρ_i values. As a result, for a fixed value of σ_ε, it forces individuals with low values of ρ_i to make choices that are close to deterministic, while individuals with high enough ρ_i parameters make choices which approach a uniform distribution.

For example, consider investment decisions in the private treatments. Using our scaling, the expected utility of investing b_j is given by:

$$EU_{ij} = \underbrace{\frac{1}{2\eta_i}(m_i - b_j)^{1-\rho_i} + \frac{1}{2\eta_i}(m_i + 4b_j)^{1-\rho_i}}_{EV_{ij}} + \varepsilon_{ij}, \tag{22}$$

while η_i would be replaced with $(1 - \rho_i)$ if we instead used the scaling in Equation 21. When the conventional scaling is used, as in Equation 21, investing any amount between 0 and 70 shillings in the private, small endowment treatment leads to EV_{ij} values between 26 and 38 for an agent with $\rho_i = 0.35$, but EV_{ij} values between -0.37 and -0.20 for an agent with $\rho_i = 1.5$. The range of EV values is substantially smaller for the more risk averse agent. As a consequence, when $\sigma_\varepsilon = 0.3$ the agent with $\rho_i = 0.35$ would choose the EV-maximizing amount, 70 shillings, more than 85 percent of the time, but the agent with $\rho_i = 1.5$ would choose the EV-maximizing investment of 20 shillings less than 14 percent of the time, and would choose all of the options less than 70 shillings with probabilities between 0.11 and 0.14.

Our proposed "utility range" (UR) scaling of the CRRA utility function addresses this issue. In the example considered above, UR scaling implies that, given $\sigma_\varepsilon = 0.3$, a subject with $\rho_i = 0.35$ would choose the EV-maximizing amount, 70 shillings, with probability 0.125, while the subject with $\rho_i = 1.5$ would choose the EV-maximizing investment of 20 shillings with probability 0.150. If the noise parameter, σ_ε, were reduced to 0.01, the less risk averse subject would chose the EV-maximizing amount with probability 0.419, while the more risk averse subject would chose the EV-maximizing amount with probability 0.441.

Though simple to implement, UR scaling generates results which are similar to those generated

[40] We acknowledge the slight abuse of the term "less-preferred" in this context since, by construction, the chosen option is always the most-preferred once the unobserved preference shock has been taken into account.

by the "contextual utility" model of Wilcox (2008), in which utility is scaled by the difference in the utilities faced by an individual decision maker within a specific choice problem, and when the expected utilities are replaced with their certainty equivalents as in Von Gaudecker, van Soest, and Wengström (2011). We explore the relationship between the form of scaling used and the estimated parameters μ_ρ and σ_ρ in Table 9. We report the parameter estimates using UR scaling in Column 1, parameters estimated using the utility function defined in Equation 21 in Column 2, parameters estimated using the certainty equivalent in place of EV_{ij} in Column 3, and parameters estimated using the contextual utility model in Column 4. We include data from both large and small endowment private treatments; the expected utility expression in both treatments is given in Equation 22. The contextual utility model in Column 4 uses different scalings for the large and small endowment treatment; and the certainty equivalent model in Column 3 raises EV_{ij} to the $1/(1-\rho_i)$ power to convert utility back into monetary terms.

UR scaling generates parameter estimates for μ_ρ and σ_ρ which are nearly identical to those produced using either the certainty equivalent or the contextual utility procedures. The estimated μ_ρ is between 0.756 and 0.762 in all three models, while the estimated σ_ρ ranges from 0.199 to 0.205 (Table 9).[41] Estimated levels of risk aversion are higher than those typically reported for undergraduate subjects (cf. Holt and Laury 2002, Goeree, Holt, and Palfrey 2003) and slightly higher than typical estimates of risk aversion in non-student populations (cf. Andersen, Harrison, Lau, and Rutström 2008, Harrison, Humphrey, and Verschoor 2010, Tanaka, Camerer, and Nguyen 2010).[42] As the table demonstrates, though the utility range scaling, certainty equivalent, and contextual utility models all lead to comparable parameter estimates, using the standard CRRA utility function, in which $x^{1-\rho_i}$ is divided by $1-\rho_i$, leads to slightly different parameter estimates (Table 9, Column 2).

In Figure 7, we compare actual investment decisions in the experiments to the predictions of the model given the estimated μ_ρ and σ_ρ parameters (using UR scaling). The top panels present

[41] We also estimate σ_ε, but omit it from the table to save space. As expected given the different utility scalings, the models generate different estimates of σ_ε.

[42] Andersen, Harrison, Lau, and Rutström (2008) report mean CRRA parameters of 0.74 in a representative sample of the Danish population. Harrison, Humphrey, and Verschoor (2010) estimate a mean CRRA parameter of 0.536 in a sample of subjects drawn from Ethiopia, India, and Uganda. Tanaka, Camerer, and Nguyen (2010) estimate the average CRRA parameter to be between 0.59 and 0.63 among their Vietnamese subjects.

in-sample predictions: decisions in the private, small endowment treatment, which were included in the data set used for estimation. The bottom panels present actual and simulated investment decisions in the public, small endowment treatment, which was not included in the estimation sample. In both cases, the model appears to predict the distribution of actual investment decisions successfully.

5.2 Individual Decisions when Investment Returns Are Observable

While individual i's investment decision in either of the private treatments depends only on the size of i's endowment and her level of risk aversion, choices in the public treatments also depend on τ_i, and choices in the price treatments depend on both τ_i and p. In the small endowment, public treatment, the expected utility of investing b_j is given by:

$$EU_{ij} = \underbrace{\frac{1}{2\eta_i}[(1-\tau_i)(m_s - b_j)]^{1-\rho_i} + \frac{1}{2\eta_i}[(1-\tau_i)(m_s + 4b_j)]^{1-\rho_i}}_{EV_{ij}} + \varepsilon_{ij}, \tag{23}$$

where, again, $\eta_i = 900^{1-\rho_i} - 10^{1-\rho_i}$. Given this expression for EV_{ij}, the log-likelihood function is defined by Equations 18 and 19. Note that, referring to Equation 22, we could re-write this as

$$EU_{ij} = (1-\tau_i)^{1-\rho_i} EV_{ij}^{private} + \varepsilon_{ij}. \tag{24}$$

As discussed earlier, the EV-maximizing investment choice is identical in the small endowment public and private treatments. However, scaling down the EV values by $(1-\tau_i)^{1-\rho}$ should make choices in the public treatment slightly noisier, and this is exactly what we find: the standard deviation the amount invested is 15.8 in the private, small endowment treatment, and it is 16.6 in the analogous public treatment.

In the public, large endowment treatment, subjects have the option of investing 80 shillings (m_s) or less, thereby making 100 shillings ($m_l - m_s$) unobservable to other participants. We indicate this potentially hidden quantity as:

$$H_{ij} = (m_l - m_s) \cdot \mathbb{1}\{b_j \leq m_s\} \tag{25}$$

where $\mathbb{1}\{\cdot\}$ is the indicator function. Thus, the expected utility of investing b_j is given by:

$$EU_{ij} = \underbrace{\frac{1}{2\eta_i}[(1-\tau_i)(m_l - b_j) + \tau_i H_{ij}]^{1-\rho_i} + \frac{1}{2\eta_i}[(1-\tau_i)(m_l + 4b_j) + \tau_i H_{ij}]^{1-\rho_i}}_{EV_{ij}} + \varepsilon_{ij} \tag{26}$$

Again, given the EV_{ij} values, the log-likelihood function is defined by Equations 18 and 19.

In the price treatments, subjects first decide how much to invest, then learn their investment return, and finally decide whether to pay the price, p, to avoid the public announcement (if they have enough money to do so after investment outcomes are realized). The probability of any observed sequence of choices — investing b_j, and then deciding whether to pay p to avoid the announcement — is the product of the probability of making the observed investment decision and the conditional probability of making the observed buyout decision.

As discussed in Section 3, there are three possible strategies which may be optimal for subjects in the price treatment: never paying p, always paying p, and paying p only when the investment is successful. We refer to these strategies as *never*, *always*, and *heads*, respectively. For some values of b_j and p, *always* may not be a viable strategy — for example, participant i cannot plan to pay p if she invests her entire endowment and that investment fails.

The expected utility of the *never* strategy is defined by Equation 23 for the small endowment treatment and by Equation 26 for the large endowment treatment. The representative utilities of the *always* and *heads* strategies are defined as follows:

$$EV_{ij}^{always} = \frac{1}{2\eta_i}(m_i - b_j - p)^{1-\rho_i} + \frac{1}{2\eta_i}(m_i + 4b_j - p)^{1-\rho_i} \tag{27}$$

$$EV_{ij}^{heads} = \begin{cases} \frac{1}{2\eta_i}[(1-\tau_i)(m_s - b_j)]^{1-\rho_i} + \frac{1}{2\eta_i}(m_s + 4b_j - p)^{1-\rho_i} & \text{if } m_i = m_s \\ \frac{1}{2\eta_i}[(1-\tau_i)(m_l - b_j) + \tau_i H_{ij}]^{1-\rho_i} + \frac{1}{2\eta_i}(m_l + 4b_j - p)^{1-\rho_i} & \text{if } m_i = m_l \end{cases} \tag{28}$$

We assume subjects make investment decisions without knowing the realization of the preference shock which will influence their buyout decisions. Hence, subject i in a price treatment chooses investment amount b_j to maximize:

$$EU_{ij} = \max\left\{EV_{ij}^{never}, EV_{ij}^{heads}, EV_{ij}^{always}\right\} + \varepsilon_{ij}. \tag{29}$$

Whenever $m_i - b_j < p$, EV_{ij}^{always} is omitted from Equation 29.

After the investment return is realized, if i has a gross payout greater than p, she decides whether to pay p to avoid the public announcement. Given a realized gross payout of x_i, the utility of paying to avoid the announcement is:

$$\frac{1}{\eta_i}\left(x_i - p\right)^{1-\rho_i} + \zeta_{i0} \tag{30}$$

This includes ζ_{i0}, participant i's idiosyncratic utility from avoiding announcement given this payout. Meanwhile, the utility of making the announcement is:

$$\frac{1}{\eta_i}\left((1-\tau_i)x_i + \tau_i H_{ij}\right)^{1-\rho_i} + \zeta_{i1} \tag{31}$$

where, analogously, ζ_{i1} is idiosyncratic utility associated with announcing. We assume that ζ_{i0} and ζ_{i1} are draws from a type 1 extreme value distribution, and that they are independent of each other and of the ε_{ij} terms. Let e_i equal one if subject i chooses to pay p and zero otherwise. Now define

$$\Delta U_i^{exit} = \underbrace{(-1)^{e_i}\left(\frac{1}{\eta_i}\left(x_i - p\right)^{1-\rho_i} - \frac{1}{\eta_i}\left((1-\tau_i)x_i + \tau_i H_{ij}\right)^{1-\rho_i}\right)}_{\Delta V_i^{exit}} + (-1)^{e_i}(\zeta_{i0} - \zeta_{i1}) \tag{32}$$

so that ΔV_i^{exit} is the difference between the utility of the action that was not chosen (paying p to avoid the public announcement or not) and the utility of taking the chosen action. The probability of i's observed sequence of actions is then given by:

$$P_{ij}P_i^{exit} = \int \left(\frac{e^{EV_{ij}/\sigma_\varepsilon}}{\sum_{k=1,\ldots,J} e^{EV_{ik}/\sigma_\varepsilon}} \cdot \frac{1}{1+e^{\Delta V_i^{exit}/\gamma}} \right) f(\rho)\, d\rho, \tag{33}$$

where γ is proportional to the variance of the difference between ζ_{i0} and ζ_{i1}, the Gumbel-distributed noise parameters specific to the buy out decision.

Before proceeding to our estimates of the kin tax parameter, it is worth reviewing the sources of identification in our model. Because subjects choose from a rich menu of investment options, the distribution of investment decisions in the two private treatments allow us to identify μ_ρ, σ_ρ, and σ_ε (as demonstrated in Table 9). The kin tax parameter, τ, is identified by the differences in the distribution of investment decisions between the private, large endowment treatment and the two public information, large endowment treatments; the relationship between the willingness to pay to avoid the public announcement and *observable* income provides a second source of identification. Finally, the variance of the logit error term in the buy out decisions, γ, is identified by the excess willingness to pay to avoid the public announcement at very low gross payouts and high exit prices; our model attributes this to the stochastic component of individual decisions.

5.3 Parameter Estimates

Summing the log likelihoods in all treatments as shown in Equation 19 — with EV_{ij} expressions from Equations 22, 23, 26, and 29, and the conditional probability in Equation 33 — we can estimate all parameters of the model using the full dataset. We do so separately for men and women, as the experimental treatment randomization was stratified on gender to permit separate analysis. Parameter estimates are reported in Table 10. In Column 1, we report estimates from a simplified likelihood function which only uses data from investment decisions. In Column 2, we report parameter estimates based on all the decisions within the experiment.

For women, the estimated τ ranges from 0.0432 to 0.0450, and is significantly different from zero at the 99 percent confidence level. Among men, the point estimate is smaller, ranging from 0.0234 to 0.0267, and is not statistically significant. In Figure 8, we compare the predictions of the model to the actual frequency with which subjects pay to avoid the public announcement. We disaggregate the data according to the implied tax rate facing subjects: the randomly-assigned exit price they faced divided by their gross payout. In a deterministic choice model, subjects will pay to avoid the public announcement whenever $\tau_i > p_i/x_i$, where x_i is their gross payout (or $\tau_i > p_i/[x_i - (m_l - m_s)]$ for subjects assigned to the large endowment treatment who invested less than m_s in the business cup). However, as the figure demonstrates, the stochastic component plays a large role in individual decisions within our experiment, particularly when the difference between the expected utility of paying p and the expected utility of sharing income with others is small — though the estimated τ parameter is less than 0.05, the model predicts the observed rates of paying to avoid the public announcement even when the price is more than five percent of the gross payout. For women, the estimated parameters fit the data well, though the model over-predicts the likelihood of paying p at implied tax rates between 20 and 30 percent.

As discussed in Section 3, one alternative explanation for the relatively high willingness to pay to avoid the public announcement is that subjects are attention averse, and wish to avoid being asked to stand up in front of their neighbors, irrespective of whether their income from the experiment will be revealed. We explore this by incorporating an additional parameter, κ, into the model, which is simply subtracted from all EV_{ij} expressions any time a public announcement is being made. Intuitively, κ is identified from excess willingness to pay to avoid the announcement in the price treatments, as compared to the implied willingness to pay identified from the increased tendency to invest exactly 80 shillings in the public treatments, and by the imperfect correlation between total income and observable income, since subjects seeking to avoid social pressure would only be willing to pay to hide observable income from neighbors and relatives. As shown in Column 3, κ is precisely estimated and very close to zero in the sample of women, and we cannot reject the null that κ is zero for both women and men.

Our ability to explore potential heterogeneity in τ is limited by the relatively small samples in different demographic categories, particularly in the price treatment, where subjects faced different randomly-assigned prices and decisions depended on both prices and (not randomly-assigned) gross payouts. However, in light of our reduced form results, we take an initial step in this direction by allowing τ to depend on whether a subject's close relatives were present at the experiment. In Table 11, we report parameter estimates when we replace the single τ parameter with two separate parameters, $\tau_{no\ kin\ present}$ and $\tau_{kin\ present}$. Among women, $\tau_{no\ kin\ present}$ is slightly smaller than in the full sample, at 0.043, but remains statistically significant. The estimated value of $\tau_{kin\ present}$ is 0.080 — substantially higher, and also statistically significant. Among men, as in the full sample, $\tau_{no\ kin\ present}$ remans about 2.7 percent, while the estimated $\tau_{kin\ present}$ is slightly below zero and imprecisely estimated. Thus, consistent with our reduced form results, we find evidence that women with kin present at the experiment are more exposed to social pressure to share income.

5.4 Simulations

5.4.1 Alternative laboratory experiments

The reduced form results depend on the specific parameters of the investment scenario that we presented to the participants, while the structural parameter estimates shown in Tables 10 and 11 do not. To illustrate this, we briefly show how the reduced form results would have differed with the same population, but using slightly different experimental parameters.

To simulate the experiment for women, we use the parameters in Column 2 of Table 10: $\mu_\rho = 0.7488$, $\sigma_\rho = 0.1992$, $\sigma_\varepsilon = 0.0125$, $\tau = 0.045$, and $\gamma = 0.0588$. With these values, we can simulate behavior under a variety of settings. For a simple demonstration, we consider the same laboratory experiment but with different investment returns. In the actual lab setting, as described in Section 2, the investment amount was multiplied by five when successful, and zero when unsuccessful. We simulate the experiment for a range of values of the successful investment return on the interval [2,12], and show the predictions for one of the reduced form regressions — Table 4, Panel C, Columns 3 and 4. For subjects receiving the large endowment treatment, we compare behavior in the private treatment to choices in the two public information treatments, and measure the change in the probability of investing no more than the small endowment (80 shillings).

For the value 5.0, highlighted in Figure 9, the predicted change in the fraction of participants investing 80 shillings or less is 8 percentage points. This is close to the point estimates of 9.6 and 10.5 percentage points shown in Columns 3 and 4 of Table 4, Panel C. Figure 9 shows, however, that we might have seen even larger reduced form effects if successful investments had been multiplied by four rather than five, while we would have seen smaller effects if successful investments had been multiplied by three or ten, for example.

The intuition behind the figure is straightforward: the largest reduced form effect is found when much of the population invests just over 80 shillings in the private treatment. Effects diminish as investment returns go up because the investment becomes so profitable that most participants invest well above 80 shillings, and it is no longer worth sacrificing one's expected return to avoid paying τ. On the other side, effects diminish as investment returns decline because few participants invest over 80 shillings in the private treatment when the risky investment has a low expected return.

Similar figures can be constructed for other reduced form results in the paper, or by varying other features of the laboratory experiment, such as the endowment sizes, the probability of successful investment, or the range of exit prices. The key insight is that while the reduced form results would have varied across a range of closely related laboratory scenarios, the underlying structural parameters remain the same. It is these parameters that allow us to model behavior both in the lab, and beyond it.

5.5 Impacts of social pressure on entrepreneurship

Our results thus far indicate that women in the experiment behave as if they expect to be pressed to share four percent of their cash income with others, and substantially more if their close kin can observe their income directly. A 4–8 percent "kin tax" may have large disincentive effects if, for example, relatives observe wages or micro-enterprise revenues, but are not able to easily separate profits from total income by accounting for labor time and indirect costs, both of which may be unobserved.[43] We explore this possibility by simulating a simple model of individual entrepreneurship adapted from the theoretical framework in Banerjee, Duflo, Glennerster, and Kinnan (2010). In their model, individuals receive income y_i in each of two periods. y_i can be seen as an individual's income from subsistence agriculture. In the first period, each person decides

[43] This would be consistent with evidence that many micro-entrepreneurs are unable to calculate their own profits, and do not correctly deduct time and indirect costs (cf. Karlan, Knight, and Udry 2012).

whether to invest in a microenterprise which yields return

$$A\left(K_i - \underline{K}\right) \tag{34}$$

where K_i is the amount that i invests in her microenterprise. To keep the model as simple as possible, we assume that individuals can neither save nor borrow. Thus, i chooses K_i to maximize

$$\frac{1}{\eta_i}\left(y_i - K_i\right)^{1-\rho_i} + \delta\frac{1}{\eta_i}\left(y_i + (1-\tau)A\left(K_i - \underline{K}\right)\right)^{1-\rho_i}. \tag{35}$$

Thus, we assume that individuals are pressed to share a proportion of their business income, but are able to avoid sharing their subsistence income — for example, because it may never be converted into cash.

We simulate a village economy comprising even numbers of poor and non-poor individuals. Poor individuals receive an income of 800 shillings in every period, while the non-poor receive an income of 1500 shillings. Numbers are roughly equivalent to the 30^{th} and 70^{th} percentiles of the rural consumption distribution in Uganda, as reported in Uganda Bureau of Statistics (2006).[44] We discretize the decision problem by assuming that individuals can invest any multiple of 100 shillings in a microenterprise. We do not allow individual consumption to drop to zero in any period, and we set $\underline{K}$ equal to 100 shillings, so that the fixed costs of starting an enterprise are quite low. We assume that individuals have heterogeneous risk preferences, and that individual CRRA parameters are normally distributed according to the mean and variance reported in Column 2 of Table 10.

We explore values of A ranging from 1.5 to 2.5. For each A value, we take $10,000$ draws from the distribution of CRRA parameters and calculate the fraction of women who invest a positive amount in a microenterprise and the average amount invested. We report results for three values of τ: when τ equals zero (i.e. in the absence of social pressure to share income, when τ is equal to 4.5 percent (as in our pooled data), and when τ is equal to 8 percent (our estimated value of τ when relatives are able to observe income streams directly). Results from the simulations are

[44] We were unable to find analogous statistics for rural Kenya. Beegle, De Weerdt, and Dercon (2011) report similar figures for rural Tanzania.

presented in Figure 10.

In the absence of kin pressure, only 1 percent of women become entrepreneurs when A is equal to 1.5, while 98 percent of women become entrepreneurs when A is equal to 2.5. Averaging across all values of A between 1.5 and 2.5, our simulations suggest that 59.9 percent of women would become entrepreneurs in the absence of kin pressure; but that number drops to 44.2 percent when τ is equal to 8 percent. Similarly, our simulations suggest that women would invest an average of 341 shillings in their enterprises in the absence of kin pressure, but this drops to 247 shillings if τ is equal to 8 percent. Thus, an 8 percent kin tax leads to more than a 27 percent decline in overall investment.

Thus, the investment impacts of relatively small kin taxes can be quite large. However, as the figures suggest, the simulated impact of social pressure depends on the value of A, the return on business investment. At the highest value of A we consider, 2.5, moving from a τ of zero to a τ of 8 percent reduces the fraction of women starting businesses from 0.98 to 0.97, and reduces average business investment by only 7 percent (from 598 shillings to 555 shillings). Intuitively, when entrepreneurship is profitable enough, social pressure has little effect since the return to starting a business is large even after sharing a portion of one's revenue with kin. On the other hand, at the value of A which maximizes the impact of social pressure to share, 1.94, even the relatively lower τ of 4.5 percent reduces the percent of women starting businesses from 64 to 40, and reduces average business investment by more than a third (from 349 to 232 shillings). At this level of A, a τ of 8 percent would have even more dramatic impacts, reducing the share of women starting businesses to only 26 percent, and reducing the average investment level by 56.7 percent. Thus, a relatively moderate level of social pressure to share can have large disincentive effects and, potentially, consequences for growth. However, the overall impacts will depend on the range of investments available to individuals, and their relative levels of observability.

6 Conclusions

We report the results of a novel economic experiment designed to measure social pressure to share income in Kenyan villages. Our approach is stratified to ensure balance, randomized within villages, and is conducted on a large sample. The design permits both reduced-form estimates to find results in line with comparative static predictions, and structural estimates to identify parameters of interest in the presence of heterogeneity.

Women who know that the outcome of their investments will be made public make investments 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: 30 percent of those able to pay to avoid the public announcement choose to do so; these subjects sacrifice 15 percent of their gross payout, on average. Structural estimates of the average "kin tax" are significantly different from zero for women, estimated at roughly 4.3 percent for those whose relatives did not attend the experiment, but at 8.0 percent for those with relatives present. Our model of stochastic choices in the experiment fits the data well, explaining both investment and exit decisions. We see no evidence that this behavior can be explained by household bargaining with a spouse, aversion to public announcements, or aversion to risk-taking in general.

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 income 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, and correlation between observability and profitability. There is still much to be learned about the range of observable and unobservable investment opportunities available to poor households, the set of technologies for hiding or protecting income from pressure to share, and the mechanics of that pressure. Nonetheless, simulating a simple model of entrepreneurship using our estimated structural parameters suggests that the levels of pressure to share documented in this paper may lead to large reductions in the probability of starting a business and the overall level of entrepreneurial investment. Whether the levels of social pressure we observe are large or small will depend on the ways in which social pressure is exerted outside of the lab: for example, whether individuals are pressed to share all cash income (for example, business revenues) or only their profits after compensating themselves for their indirect costs or labor time.

Studies of mutual insurance typically assume that transfer arrangements are on the efficient frontier, though the analogous assumption has been called into question 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 important disincentive effects.

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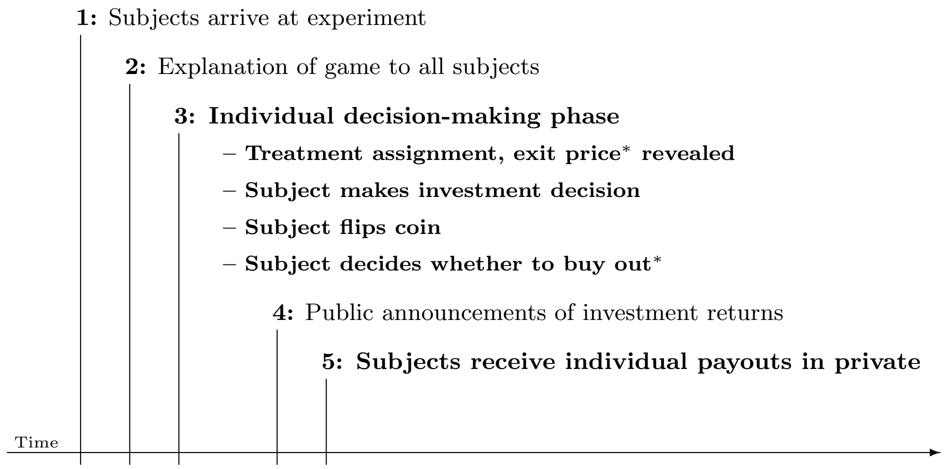
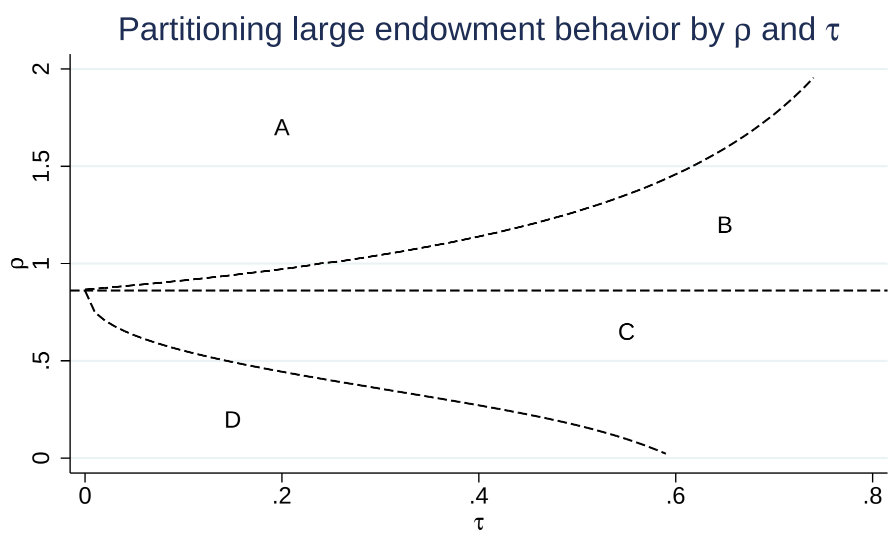
Figure 1: Structure of Experiment



Activities in plain text took place in primary school classrooms, with all subjects seated together. Activities in bold text took place in one-on-one interactions between individual subjects and enumerators; during these interactions, subjects and members of the research team were seated at desks in private locations in the schoolyard.

Figure 2: Behavior in Large Endowment, Public Treatment



Region A: High ρ, low τ − invest below 80 in both public and private
Region B: High ρ, high τ − invest below 80 in private, but exactly 80 in public
Region C: Low ρ, high τ − invest above 80 in private, but exactly 80 in public
Region D: Low ρ, low τ − invest above 80 in both public and private
Note: values above calculated using large endowment=180 and small endowment=80.

Figure 3: Expected Utility in Large Endowment, Public Treatment

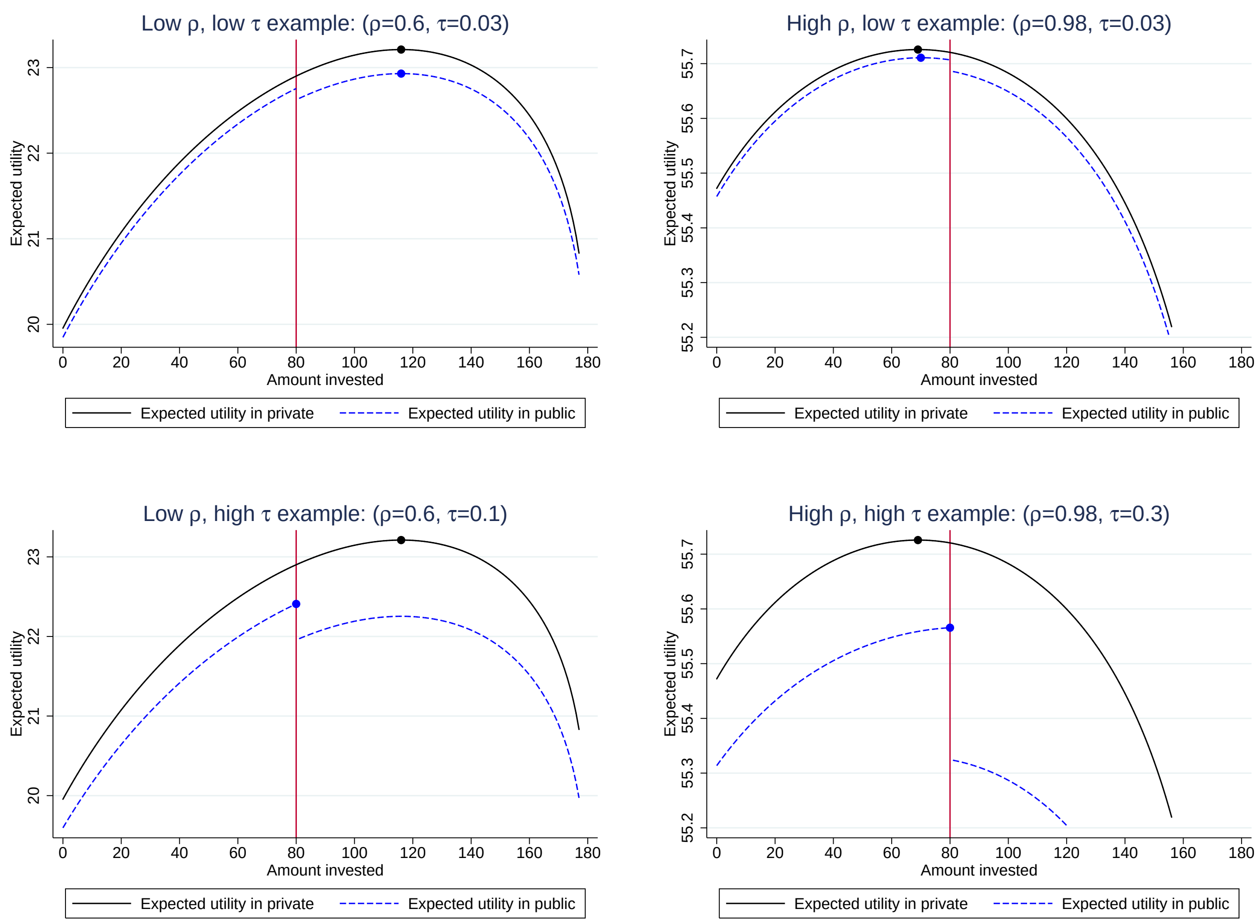


Figure 4: Histograms of Investment in the Business Cup by Treatment

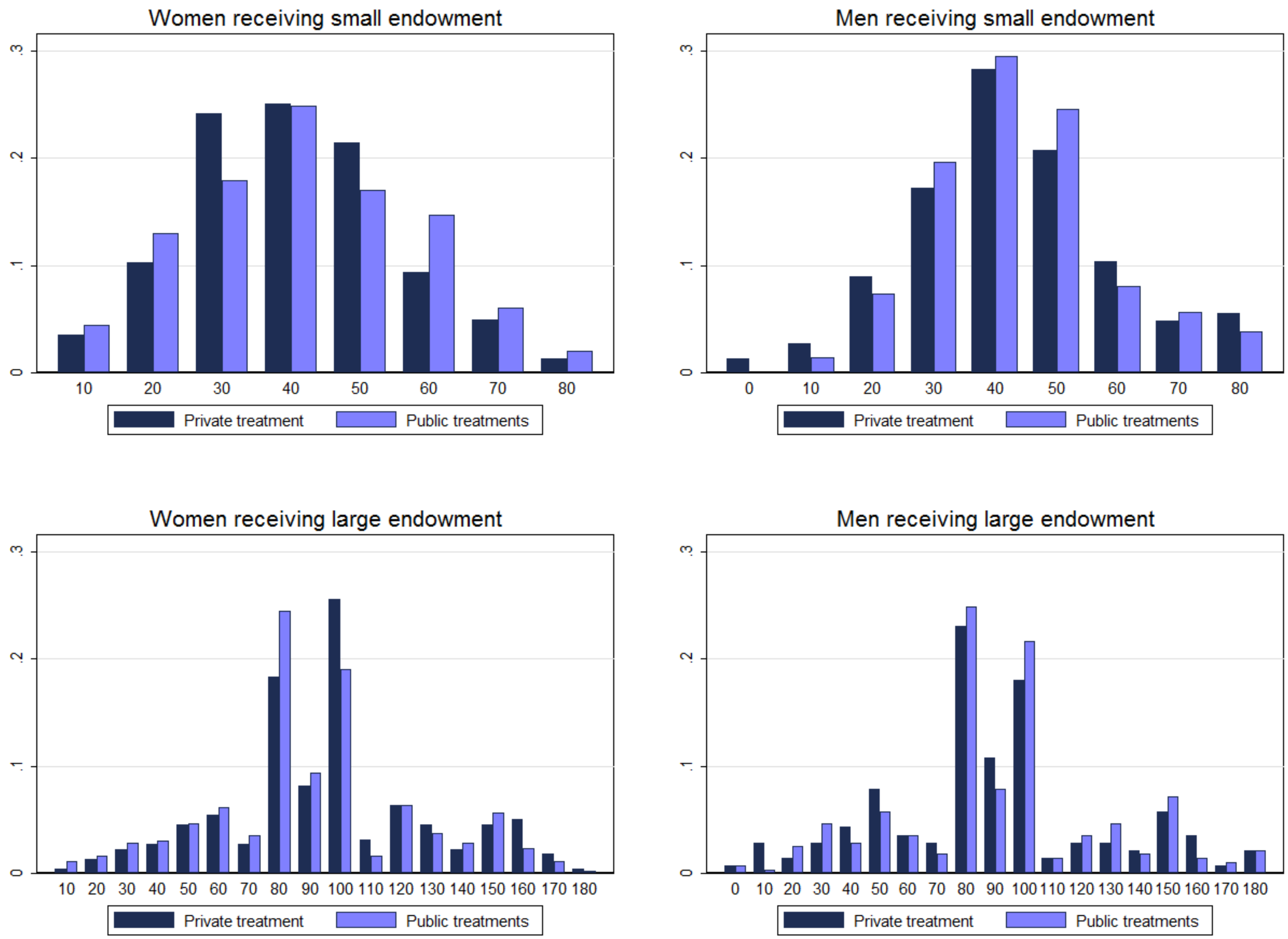


Figure 5: Income Hiding and Village-Level Outcomes

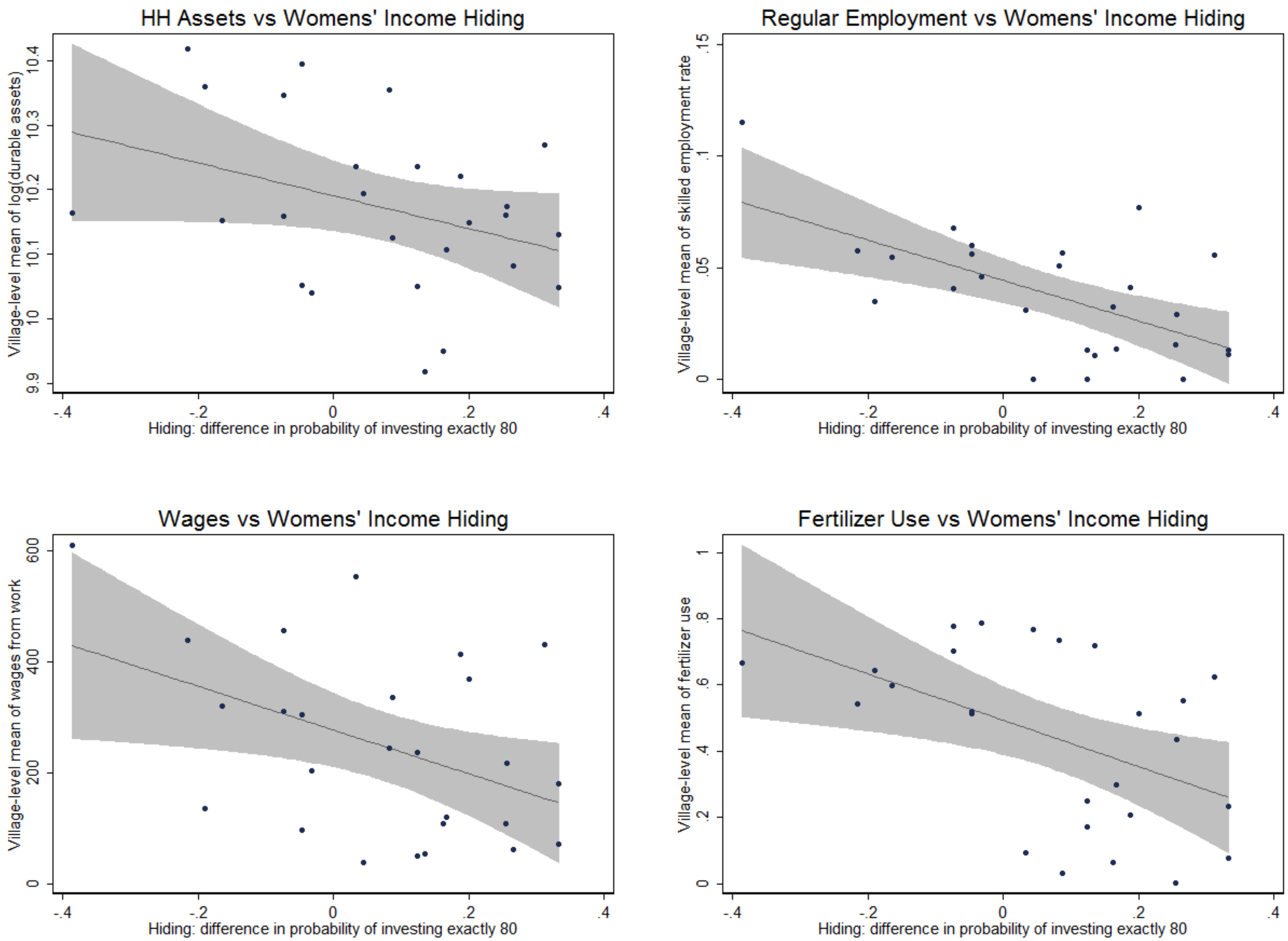
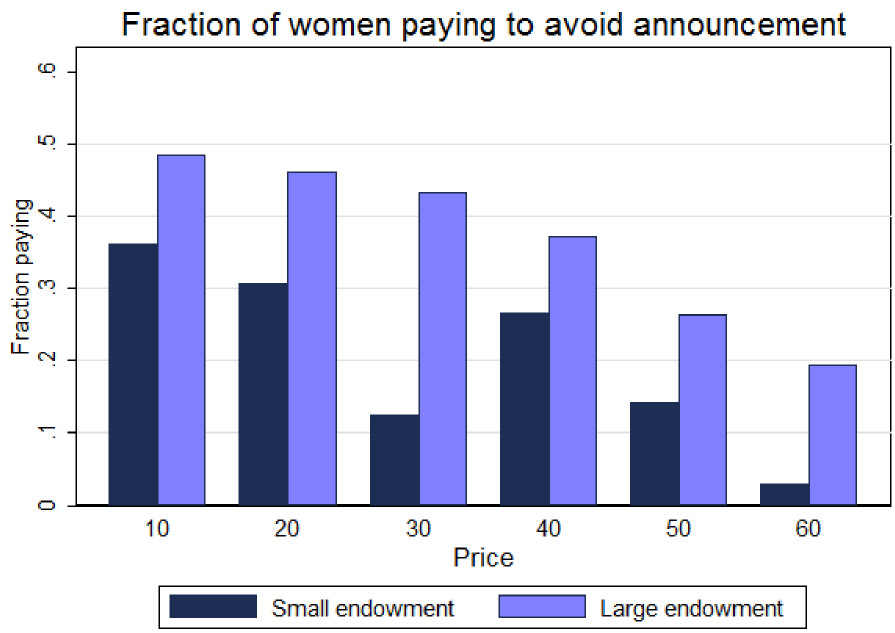


Figure 6: Fraction of Subjects Paying to Avoid Announcing



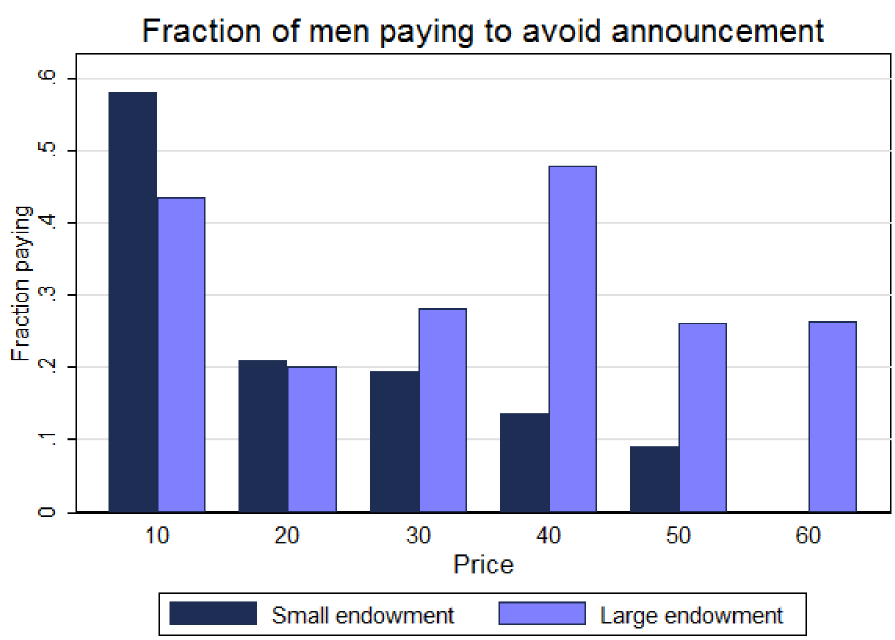


Figure 7: Actual vs. Predicted Investment Decisions

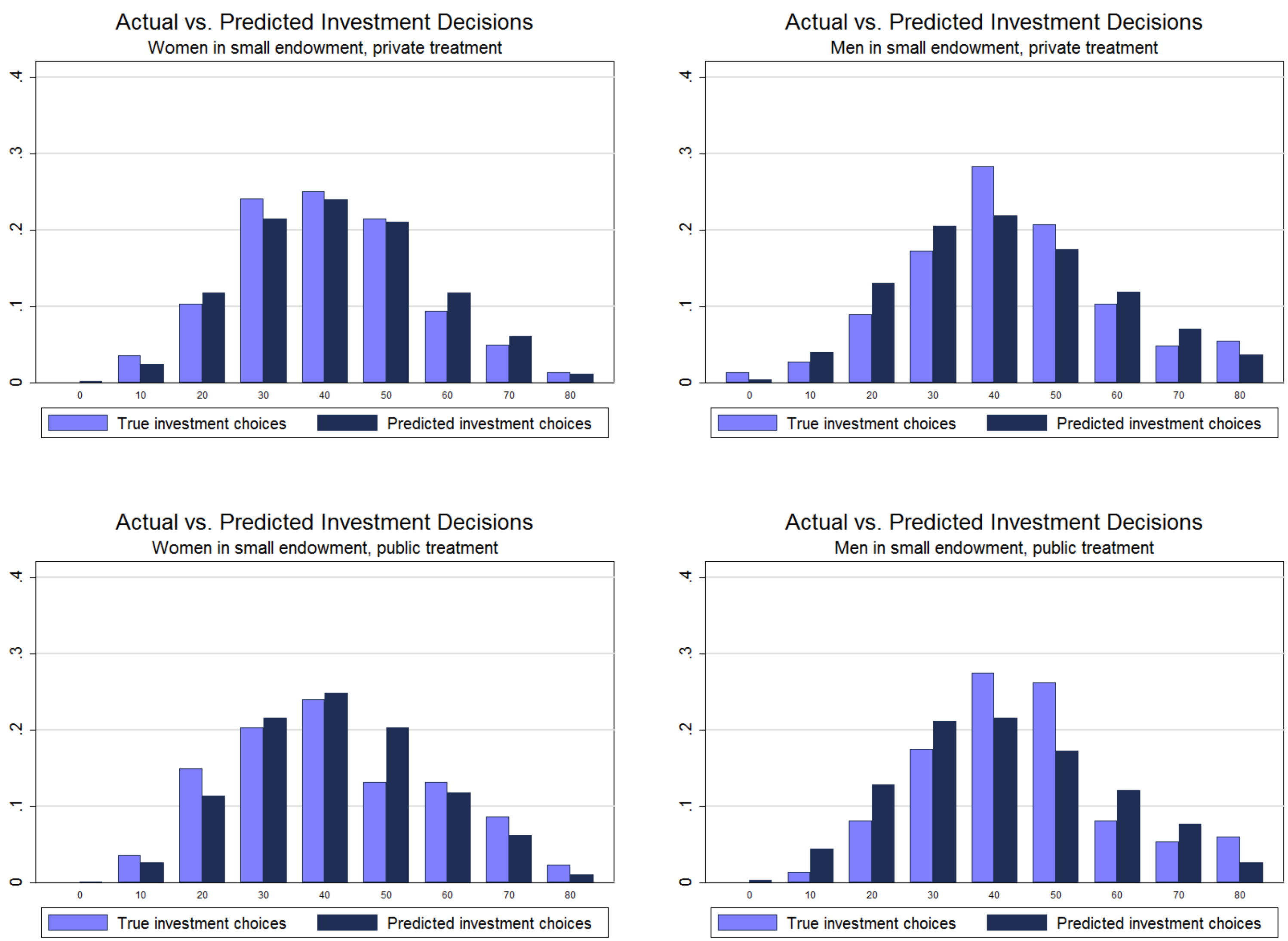
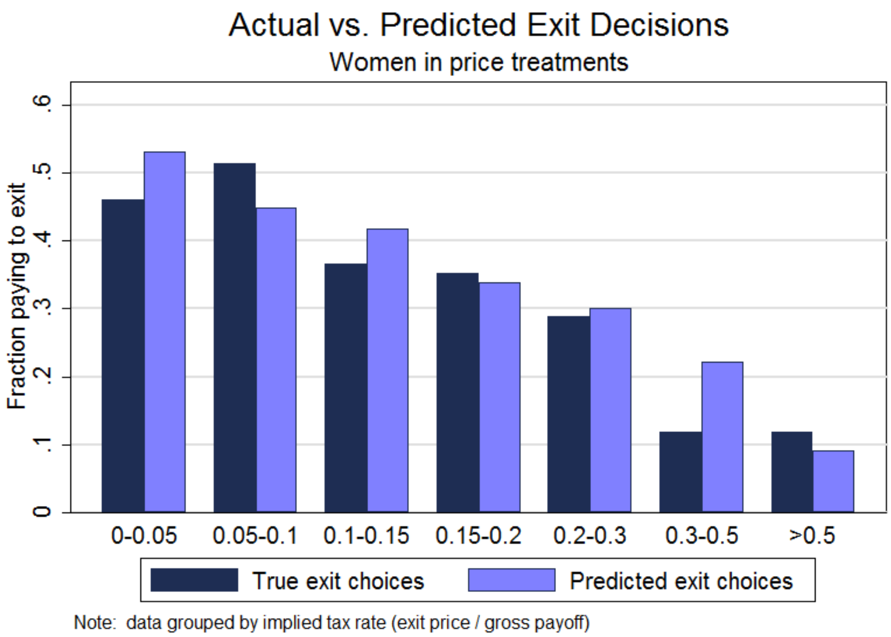


Figure 8: Actual vs. Predicted Exit Decisions



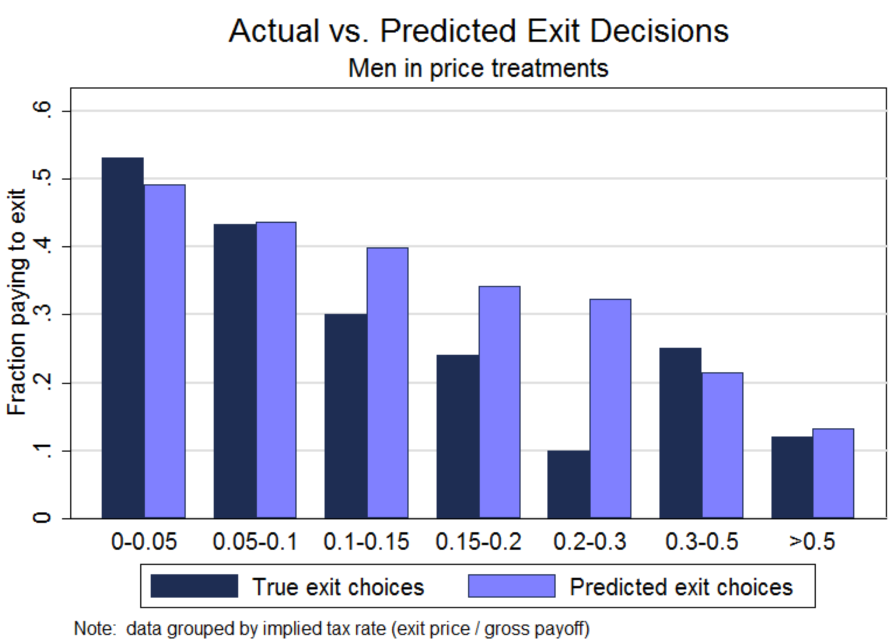
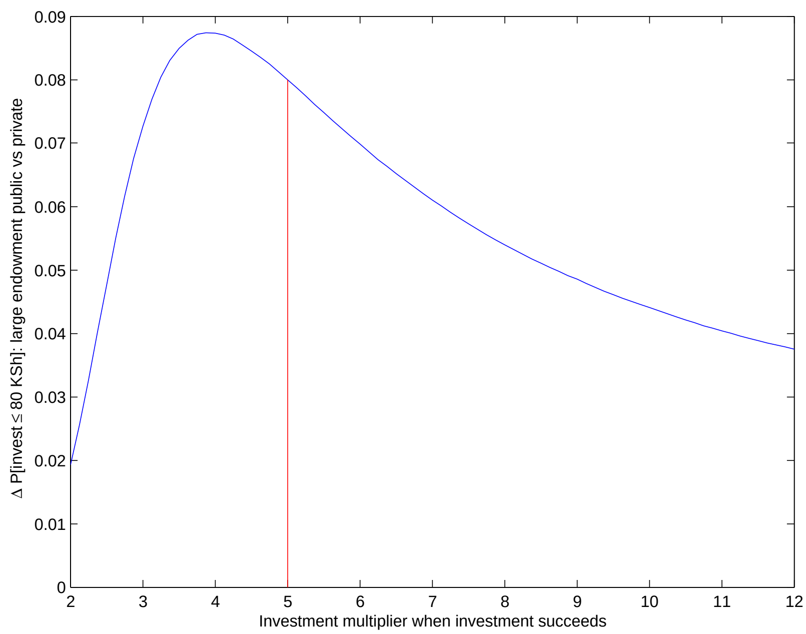
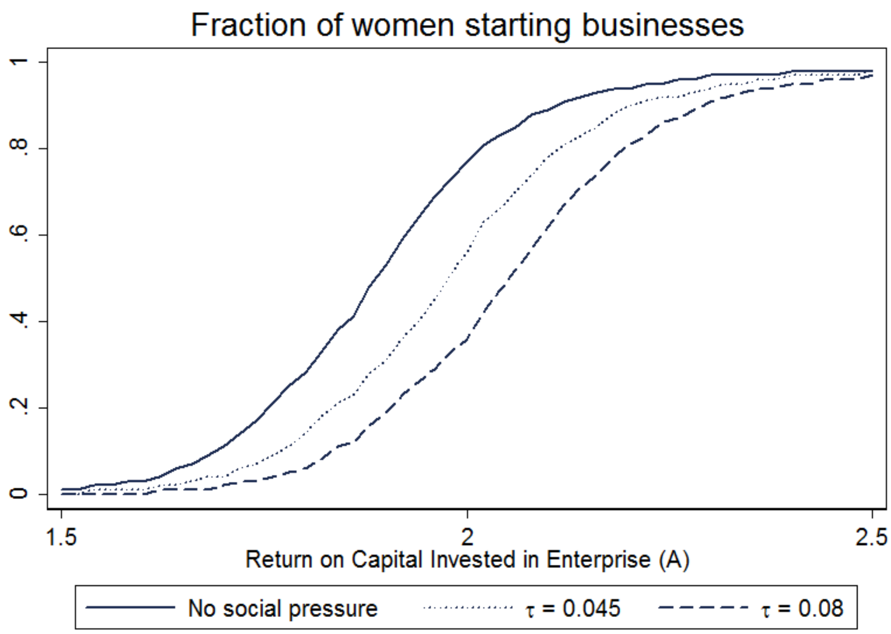


Figure 9: Simulated Alternative Laboratory Experiments



Results from 10 million simulations of players' behavior in alternative laboratory experiments in which a successful investment sees its value multiplied by a value between 2.0 and 12.0. The value 5.0, used in the actual experiment we perform, is highlighted. Parameters used in these simulations are taken from Column 2 of Table 10 for women.

Figure 10: Simulated Entrepreneurship Decisions



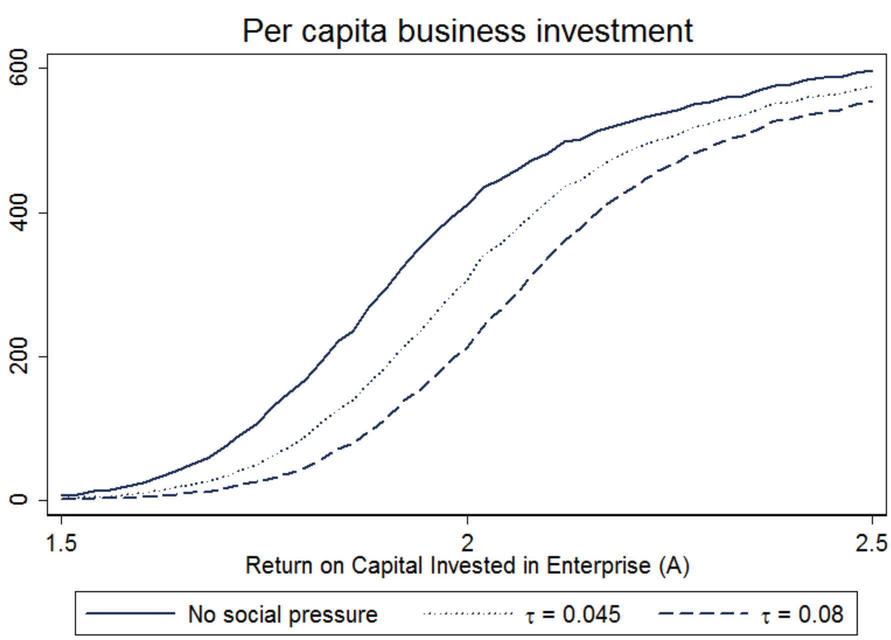


Table 1: Summary Statistics on Experimental Subjects

							— Balance —	
Variable	Mean	S.D.	Median	Min	Max	N	Women	Men
Female	0.61	0.49	1	0	1	2145	.	.
Years of schooling	6.74	3.36	7	0	16	2145	0.18	0.98
Age	36.82	14.28	34	18	88	2127	0.35	0.13
Currently married	0.77	0.42	1	0	1	2145	0.73	0.18
Ever married	0.88	0.32	1	0	1	2145	0.85	0.16
HH size	6.18	2.82	6	1	26	2145	0.14	0.40
Close relatives in village (outside of HH)	2.36	2.57	2	0	19	2145	0.15	0.10
Distant relatives in village	10.41	16.12	5	0	199	2145	0.20	0.01^{***}
Close relatives attending experiment	0.19	0.39	0	0	1	2145	0.64	0.73
No. chicken owned by HH	6.42	7.19	4	0	40	2145	0.65	0.85
No. cattle owned by HH	1.20	2.08	0	0	36	2144	0.08^{*}	0.38
No. bicycles owned by HH	0.83	0.76	1	0	6	2145	0.24	0.81
No. phones owned by HH	0.73	0.82	1	0	6	2145	0.84	0.93
No. televisions owned by HH	0.14	0.39	0	0	3	2145	0.01^{**}	0.92
Value of durable HH assets (in US dollars)	469.31	655.19	357.05	13.18	22695.65	2145	1.66	0.91
HH farms	0.99	0.12	1	0	1	2145	0.81	0.31
HH uses fertilizer on crops	0.46	0.50	0	0	1	2114	0.20	0.98
Has regular employment	0.08	0.28	0	0	1	2145	0.24	0.75
Monthly wages if employed (in US dollars)	39.28	61.59	19.76	1.32	434.78	178	0.72	2.79^{**}
Any HH member employed	0.23	0.42	0	0	1	2145	0.72	0.74
Self-employed	0.35	0.48	0	0	1	2145	0.27	0.79
Has bank savings account	0.17	0.37	0	0	1	2142	0.10^{*}	0.21
Member of ROSCA	0.53	0.50	1	0	1	2142	0.28	0.22
Received loan from MFI	0.04	0.20	0	0	1	2141	0.51	0.49
HH gave transfer in last 3 months	0.90	0.31	1	0	1	2145	0.44	0.88
Transfers to HHs in village (in US dollars)	6.79	21.96	1.98	0.00	480.90	2145	0.68	0.38
HH received transfer in last 3 months	0.41	0.49	0	0	1	2145	0.13	0.13
Transfers from HHs in village (in US dollars)	2.58	17.62	0.00	0.00	527.80	2145	1.11	0.37
Community groups	2.76	1.87	3	0	10	2145	0.30	0.13
Belongs to Luhya ethnic group	0.80	0.40	1	0	1	2145	0.36	0.99
Belongs to Luo ethnic group	0.11	0.31	0	0	1	2145	0.39	1.00
Belongs to Teso ethnic group	0.08	0.28	0	0	1	2145	0.66	0.89
Christian	0.98	0.14	1	0	1	2145	0.38	0.48
Attended church last week	0.67	0.47	1	0	1	2145	0.15	0.78
Number of correct math responses	2.13	1.01	2	0	3	1998	0.43	0.95

Balance columns report p-values from F test of the joint significance of treatment dummies in a regression in which the variable listed in the first column is used as the dependent variable. Regressions are estimated separately by gender. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 2: Summary Statistics on Outcomes in Experiment by Treatment

Information Condition:	PRIVATE	PUBLIC	PRICE	PRIVATE	PUBLIC	PRICE	ALL
Budget Size:	SMALL	SMALL	SMALL	LARGE	LARGE	LARGE	ALL
Amount invested in business cup	41.44	42.59	42.00	93.35	91.98	90.26	66.68
	(0.82)	(0.86)	(0.81)	(1.90)	(1.88)	(1.84)	(0.80)
Fraction of budget invested	0.52	0.53	0.53	0.52	0.51	0.50	0.52
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
Average payout (in Kenyan shillings)	153.73	139.16	141.07	355.01	321.54	339.27	240.61
	(6.02)	(6.11)	(5.99)	(13.57)	(13.46)	(13.51)	(4.73)
Average payout (in US dollars)	2.03	1.83	1.86	4.68	4.24	4.47	3.17
	(0.08)	(0.08)	(0.08)	(0.18)	(0.18)	(0.18)	(0.06)
Proportion investing exactly m_s	.	.	.	0.20	0.23	0.27	.
				(0.02)	(0.02)	(0.02)	
Proportion investing m_s or less	.	.	.	0.42	0.46	0.48	.
				(0.03)	(0.03)	(0.03)	
Mean exit price (in Kenyan shillings)	.	.	34.84	.	.	35.07	34.96
			(0.91)			(0.91)	(0.64)
Proportion buying out	.	.	0.21	.	.	0.34	0.28
			(0.02)			(0.03)	(0.02)
Mean accepted exit price (in Kenyan shillings)	.	.	25.21	.	.	31.68	29.26
			(1.74)			(1.50)	(1.16)
N	369	370	345	358	358	345	2145

Standard errors in parentheses.

Table 3: OLS Regressions of Amount Invested by Experimental Treatment

Sample:	WOMEN (1)	WOMEN (2)	MEN (3)	MEN (4)
Public treatments	1.067	1.099	0.542	0.31
	(1.038)	(0.983)	(1.560)	(1.400)
Large budget	55.810***	55.576***	45.808***	45.940***
	(2.487)	(2.594)	(3.430)	(3.669)
Public × large budget	-6.310*	-6.335**	2.012	1.979
	(3.233)	(3.215)	(4.075)	(4.242)
Constant	40.491***	38.522***	42.897***	13.544
	(0.972)	(11.177)	(1.290)	(12.735)
Village FEs	No	Yes	No	Yes
Additional Controls	No	Yes	No	Yes
Observations	1298	1298	847	847
R^2	0.492	0.515	0.408	0.455

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 4: Regressions of Investment Outcomes for Subjects in Large Endowment Treatments

Sample:	WOMEN WITH LARGE ENDOWMENT				MEN WITH LARGE ENDOWMENT			
Specification:	PROBIT	PROBIT	OLS	OLS	PROBIT	PROBIT	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Dependent Variable = Indicator for Investing Exactly 80 Shillings								
Public treatments	0.214*	0.246*	0.062*	0.07*	0.058	0.051	0.018	0.015
	(0.129)	(0.136)	(0.037)	(0.038)	(0.144)	(0.153)	(0.045)	(0.045)
Panel B: Dependent Variable = Indicator for Investing 70 or 80 Shillings								
Public treatments	0.223*	0.254**	0.07**	0.076**	0.022	0.025	0.007	0.007
	(0.115)	(0.124)	(0.035)	(0.037)	(0.136)	(0.146)	(0.044)	(0.047)
Panel C: Dependent Variable = Indicator for Investing 80 Shillings or Less								
Public treatments	0.246**	0.288**	0.096**	0.105**	-0.063	-0.06	-0.025	-0.022
	(0.107)	(0.122)	(0.041)	(0.044)	(0.133)	(0.159)	(0.053)	(0.059)
Village FEs	No	Yes	No	Yes	No	Yes	No	Yes
Additional Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	417	417	417	417	644	644	644	644

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. Sample restricted to subjects receiving larger endowment. See Figure 4 for histograms of investment amounts.

Table 5: OLS Regressions of Investment Amount when Kin Are Present

Sample:	Women with Large Endowment			Men with Large Endowment		
Dependent Variable:	Investment	Exactly 80	80 or Less	Investment	Exactly 80	80 or Less
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Treatment Effects for Subjects with and without Kin Present						
Close kin attended game	10.658*	-0.075	-0.223***	14.613**	0.004	-0.092
	(6.114)	(0.067)	(0.077)	(6.978)	(0.093)	(0.111)
Close kin at game × public	-24.448**	0.149**	0.418***	-5.406	-0.057	-0.017
	(9.610)	(0.072)	(0.114)	(8.180)	(0.087)	(0.095)
No close kin at game × public	-3.214	0.059	0.065	4.609	0.05	-0.021
	(3.276)	(0.041)	(0.048)	(5.042)	(0.065)	(0.075)
Spouse at game	-1.698	-0.029	-0.005	-0.258	0.01	0.108
	(4.530)	(0.061)	(0.078)	(6.986)	(0.092)	(0.076)
Observations	644	644	644	417	417	417
R^2	0.098	0.08	0.095	0.142	0.096	0.15
Panel B: Treatment Effects for Subjects with and without a Spouse Present						
Spouse at game	3.374	0.023	-0.028	24.951**	0.049	-0.096
	(7.411)	(0.078)	(0.118)	(12.658)	(0.118)	(0.127)
Spouse at game × public	-13.020	-0.007	0.139	-29.038*	-0.023	0.234
	(9.493)	(0.099)	(0.154)	(15.676)	(0.12)	(0.159)
No spouse at game × public	-4.869	0.077**	0.102**	3.950	0.021	-0.039
	(3.263)	(0.039)	(0.042)	(3.797)	(0.049)	(0.06)
Close kin attended game	-2.640	-0.019	-0.003	7.314*	-0.07	-0.085*
	(4.015)	(0.053)	(0.066)	(4.118)	(0.047)	(0.05)
Observations	644	644	644	417	417	417
R^2	0.091	0.079	0.085	0.149	0.093	0.154

Robust standard errors clustered at village level. 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 6: OLS Regressions of Village-Level Outcomes on Income Hiding by Women in Experiment

Dependent Variable:	LN HH ASSETS		HAS REGULAR JOB		WAGES FROM WORK		FERTILIZER USE	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Income hiding (exactly 80)	-0.254*	-0.312**	-0.09***	-0.096***	-394.458**	-457.222***	-0.7***	-0.504**
	(0.134)	(0.129)	(0.024)	(0.024)	(164.124)	(136.549)	(0.254)	(0.244)
Distance to paved road	.	-0.003	.	0.0006	.	3.252	.	0.003
		(0.005)		(0.0009)		(5.105)		(0.009)
Mean(education)	.	-0.01	.	0.005	.	79.035**	.	0.085
		(0.037)		(0.007)		(38.989)		(0.07)
Mean(close kin in village)	.	0.114**	.	0.016*	.	118.374**	.	-0.253***
		(0.051)		(0.009)		(53.973)		(0.097)
Mean(community groups)	.	0.056	.	0.008	.	28.711	.	0.164
		(0.053)		(0.01)		(56.265)		(0.101)
Observations	26	26	26	26	26	26	26	26
R^2	0.13	0.394	0.368	0.546	0.194	0.58	0.24	0.472

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. LN HH ASSETS is the average of the log value (in Kenyan shillings) of durable assets owned by households. HAS REGULAR JOB is the fraction of participants with formal, skilled, and/or professional employment. WAGES FROM WORK is the average of wages received from paid work over the last month; wages are set to zero for subjects with no paid employment. FERTILIZER USE denotes the fraction of households engaged in agricultural that used fertilizer over the previous twelve month period.

Table 7: OLS Regressions of Paying to Avoid Announcing

Sample:	Women in price treatments				Men in price treatments			
	All subjects		Able to pay		All subjects		Able to pay	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price of exit	-0.006***	-0.006***	-0.005***	-0.007***	-0.005***	-0.009***	-0.004***	-0.01***
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)	(0.002)	(0.003)
Large budget	0.155***	0.16	0.136***	0.154	0.119**	-0.241	0.093	-0.28*
	(0.039)	(0.146)	(0.04)	(0.148)	(0.057)	(0.154)	(0.063)	(0.162)
Price × large budget	.	-0.0003	.	0.0001	.	0.007**	.	0.008**
		(0.003)		(0.004)		(0.003)		(0.004)
Coin flip lands heads	.	0.225***	.	0.23***	.	0.07	.	0.053
		(0.057)		(0.068)		(0.059)		(0.077)
Heads × large budget	.	-0.012	.	-0.023	.	0.236***	.	0.251***
		(0.093)		(0.1)		(0.073)		(0.084)
Constant	0.42***	0.317***	0.424***	0.327***	0.368***	0.464***	0.376***	0.503***
	(0.055)	(0.084)	(0.056)	(0.086)	(0.076)	(0.106)	(0.077)	(0.113)
Observations	416	416	380	380	274	273	247	247
R^2	0.08	0.138	0.055	0.107	0.053	0.139	0.031	0.112

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. See Figure 6 for the fraction of subjects buying out, broken down by price.

Table 8: OLS Regressions of Business Investment on the Price of Exit

Sample:	Women only		Men only	
	(1)	(2)	(3)	(4)
Price of exit	0.055	0.065	-0.029	0.015
	(0.066)	(0.06)	(0.058)	(0.085)
Large budget	64.615***	63.839***	62.079***	64.958***
	(6.689)	(6.724)	(9.602)	(9.191)
Price × large budget	-0.437***	-0.424**	-0.44**	-0.519**
	(0.169)	(0.17)	(0.222)	(0.222)
Includes Controls	No	Yes	No	Yes
Observations	416	416	274	274
R^2	0.493	0.507	0.45	0.493

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. Sample restricted to subjects assigned to price treatments. Controls are: indicators for age and education categories, marital status, HH size, the log value of HH assets, and a constant.

Table 9: Comparing Estimated Distributions of CRRA Parameters

Scaling:	UR	$1-\rho$	CE	CU
	(1)	(2)	(3)	(4)
Panel B: Women in Private Treatments				
μ_ρ	0.7562	0.7972	0.7589	0.7617
	(0.0163)	(0.0150)	(0.0158)	(0.0163)
σ_ρ	0.1994	0.2355	0.2011	0.2046
	(0.0170)	(0.0115)	(0.0154)	(0.0167)
Panel B: Men in Private Treatments				
μ_ρ	0.7747	0.8168	0.7836	0.7762
	(0.0233)	(0.0215)	(0.0234)	(0.0232)
σ_ρ	0.2657	0.2811	0.2681	0.2647
	(0.0225)	(0.0126)	(0.0221)	(0.0217)

Standard errors in parentheses. Estimates generated using data from private treatments only. CE estimation is done by replacing expected utilities with certainty equivalents in the likelihood function. CU is identical to (1) except that subjects in the small endowment treatment have their utilities scaled by $400^{1-\rho} - 10^{1-\rho}$.

Table 10: Parameter Estimates

	(1)	(2)	(3)
Panel A: Women in All Treatments			
μ_ρ	0.7498***	0.7488***	0.7504***
	(0.0108)	(0.0107)	(0.0114)
σ_ρ	0.2000***	0.1992***	0.2006***
	(0.0116)	(0.0115)	(0.0118)
σ_ϵ	0.0125***	0.0125***	0.0124***
	(0.0011)	(0.0011)	(0.0011)
τ	0.0432***	0.0450***	0.0419***
	(0.0124)	(0.0113)	(0.0097)
γ		0.0588***	0.0577***
		(0.0088)	(0.0114)
κ			0.0022
			(0.0118)
Panel B: Men in All Treatments			
μ_ρ	0.7555***	0.7557***	0.7557***
	(0.0131)	(0.0132)	(0.0134)
σ_ρ	0.2385***	0.2391***	0.2395***
	(0.0125)	(0.0125)	(0.0125)
σ_ϵ	0.0101***	0.0102***	0.0102***
	(0.0012)	(0.0012)	(0.0012)
τ	0.0267*	0.0234*	0.0242*
	(0.0139)	(0.0134)	(0.0146)
γ		0.0623***	0.0917***
		(0.0122)	(0.0334)
κ			-0.0325
			(0.0292)

Standard errors in parentheses.

Table 11: Parameter Estimates Allowing for Heterogeneity in τ

Sample:	WOMEN	MEN
	(1)	(2)
μ_ρ	0.750***	0.760***
	(0.011)	(0.013)
σ_ρ	0.199***	0.241***
	(0.011)	(0.012)
σ_ϵ	0.013***	0.010***
	(0.001)	(0.001)
$\tau_{no\ kin\ present}$	0.043***	0.027*
	(0.012)	(0.015)
$\tau_{kin\ present}$	0.080**	-0.011
	(0.032)	(0.022)
γ	0.058***	0.062***
	(0.009)	(0.012)

Standard errors in parentheses.

A Appendix

To simplify notation, we omit i subscripts when there is no possibility of ambiguity.

A.1 Proofs

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

$$\frac{1}{2}\left(m_s - b_s^{pri}\right)^{-\rho} = \frac{4}{2}\left(m_s + 4b_s^{pri}\right)^{-\rho}$$

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

$$\frac{1}{2}\left[(1-\tau)\left(m_s - b_s^{pub}\right)\right]^{-\rho} = \frac{4}{2}\left[(1-\tau)\left(m_s + 4b_s^{pub}\right)\right]^{-\rho}. \tag{36}$$

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

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

$$\begin{aligned}
E\left[u_i\left(b_s^{pri}|\rho\right)\right] &= \frac{1}{2}\left[\frac{\left(m_s - b_s^{pri}\right)^{1-\rho}}{1-\rho} + \frac{\left(m_s + 4b_s^{pri}\right)^{1-\rho}}{1-\rho}\right] \\
&= \frac{1}{2(1-\rho)}\left[\left(m_s - \frac{4^{1/\rho}-1}{4^{1/\rho}+4}\cdot m_s\right)^{1-\rho} + \left(m_s + 4\cdot\frac{4^{1/\rho}-1}{4^{1/\rho}+4}\cdot m_s\right)^{1-\rho}\right] \\
&= \frac{m_s^{1-\rho}}{2(1-\rho)}\left[\left(\frac{4^{1/\rho}+4-4^{1/\rho}+1}{4^{1/\rho}+4}\right)^{1-\rho} + \left(\frac{4^{1/\rho}+4+4\cdot 4^{1/\rho}-4}{4^{1/\rho}+4}\right)^{1-\rho}\right] \\
&= \frac{m_s^{1-\rho}}{2(1-\rho)}\left[\left(\frac{5}{4^{1/\rho}+4}\right)^{1-\rho} + \left(\frac{5\cdot 4^{1/\rho}}{4^{1/\rho}+4}\right)^{1-\rho}\right] \\
&= \frac{1+4^{\frac{1-\rho}{\rho}}}{2(1-\rho)}\left(\frac{5m_s}{4^{1/\rho}+4}\right)^{1-\rho} \\
&= \frac{(5m_s)^{1-\rho}\left(4^{1/\rho}+4\right)^{\rho}}{8(1-\rho)}.
\end{aligned}$$

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

$$\begin{aligned}
E\left[u_i\left(b_s^{pub}|m_s,\rho,\tau\right)\right] &= \frac{1}{2}\left[\frac{(1-\tau)^{1-\rho}\left(m_s - b_s^{pub}\right)^{1-\rho}}{1-\rho} + \frac{(1-\tau)^{1-\rho}\left(m_s + 4b_s^{pub}\right)^{1-\rho}}{1-\rho}\right] \\
&= \frac{(5m_s)^{1-\rho}\left(4^{1/\rho}+4\right)^{\rho}}{8(1-\rho)}(1-\tau)^{1-\rho}.
\end{aligned}$$

To simplify notation, let

$$\Gamma = (5m_s)^{1-\rho}\left(4^{1/\rho}+4\right)^{\rho}\left(\frac{1}{8}\right).$$

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

$$\begin{aligned} 1 > (1-\tau)^{1-\rho} &\Rightarrow 1-\rho > (1-\tau)^{1-\rho}(1-\rho) \\ &\Rightarrow (1-\rho)\Gamma > (1-\tau)^{1-\rho}(1-\rho)\Gamma. \end{aligned}$$

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

$$\begin{aligned} 1 < (1-\tau)^{1-\rho} &\Rightarrow 1-\rho > (1-\tau)^{1-\rho}(1-\rho) \\ &\Rightarrow (1-\rho)\Gamma > (1-\tau)^{1-\rho}(1-\rho)\Gamma. \end{aligned}$$

since $1-\rho < 0$. 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 of maximized expected utility in the public, small endowment treatment:

$$\begin{aligned} \frac{\partial}{\partial\tau} E\left[u_i\left(b_s^{pub}|m_s,\rho,\tau\right)\right] &= \frac{\partial}{\partial\tau}\frac{\Gamma}{1-\rho}(1-\tau)^{1-\rho} \\ &= -\frac{\Gamma}{1-\rho}(1-\rho)(1-\tau)^{-\rho} \\ &= -\Gamma(1-\tau)^{-\rho}. \end{aligned}$$

The derivative is clearly negative, since Γ and $1-\tau$ are always positive.

□

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

$$\frac{\partial}{\partial\rho} b_l^{pri} = \left(\frac{\left(4^{1/\rho}+4\right)4^{1/\rho}\ln 4\left(-\rho^{-2}\right) - \left(4^{1/\rho}-1\right)4^{1/\rho}\ln 4\left(-\rho^{-2}\right)}{(4^{1/\rho}+4)^2}\right) m_l \tag{37}$$

$$= \left(\frac{-4\cdot 4^{1/\rho}\ln 4\left(\rho^{-2}\right) - 4^{1/\rho}\ln 4\left(\rho^{-2}\right)}{(4^{1/\rho}+4)^2}\right) m_l \tag{38}$$

$$= \left(\frac{-5\cdot 4^{1/\rho}\ln 4}{\rho^2(4^{1/\rho}+4)^2}\right) m_l. \tag{39}$$

This is clearly negative since ρ and m_l are positive numbers. Thus, the optimal investment in the private, large endowment treatment is decreasing in ρ. As is apparent from inspection of Equation (3), b_l^{pri} approaches m_l as ρ goes to zero, and b_l^{pri} approaches zero as ρ goes to infinity. Thus, signing the derivative demonstrates that $\underline{\rho}$ exists. We can explicitly define $\underline{\rho}$ as follows:

$$b_l^{pri} = \left(\frac{4^{1/\underline{\rho}}-1}{4^{1/\underline{\rho}}+4}\right) m_l = m_s \tag{40}$$

$$\Leftrightarrow \left(4^{1/\underline{\rho}}-1\right)(m_s+d) = \left(4^{1/\underline{\rho}}+4\right) m_s \tag{41}$$

$$\Leftrightarrow \underline{\rho} = \frac{\ln 4}{\ln(5m_s+d)-\ln d}. \tag{42}$$

Since b_l^{pri} is decreasing in ρ, individuals with $\rho < \underline{\rho}$ invest more than m_s in the private, large endowment treatment.

To see that such individuals will either invest b_l^{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*

$$\left(\frac{4^{1/\rho}-1}{4^{1/\rho}+4}\right) m_s + \frac{\left(4^{1/\rho}-1\right) d}{(1-\tau)\left(4^{1/\rho}+4\right)}. \tag{43}$$

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

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

$$E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right] = \frac{1}{2}\left(\frac{((1-\tau)(m_l-b_l^{pri}))^{1-\rho}}{1-\rho} + \frac{((1-\tau)(m_l+4b_l^{pri}))^{1-\rho}}{1-\rho}\right) \tag{44}$$

$$= \frac{(1-\tau)^{1-\rho}}{2(1-\rho)}\left((m_l-b_l^{pri})^{1-\rho} + (m_l+4b_l^{pri})^{1-\rho}\right). \tag{45}$$

Inspection of Equation (45) above reveals that:

$$\lim_{\tau\to 1} E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right] = \begin{cases} 0 & \text{if } \rho < 1 \\ -\infty & \text{if } \rho > 1 \end{cases} \tag{46}$$

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

$$E\left[u_i\left(m_s|\rho,\tau\right)\right] = \frac{1}{2}\left(\frac{d^{1-\rho}}{1-\rho} + \frac{[(1-\tau)(5m_s)+d]^{1-\rho}}{1-\rho}\right) \tag{47}$$

Inspection of Equation (47) above reveals that:

$$\lim_{\tau\to 1} E\left[u_i\left(m_s|\rho,\tau\right)\right] = \frac{d^{1-\rho}}{1-\rho} > \begin{cases} 0 & \text{if } \rho < 1 \\ -\infty & \text{if } \rho > 1 \end{cases} \tag{48}$$

Thus, combining Inequality (46) with Equation (48), we see that

$$\lim_{\tau\to 1} E\left[u_i\left(m_s|\rho,\tau\right)\right] > \lim_{\tau\to 1} E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right] \tag{49}$$

But by definition,

$$E\left[u_i\left(m_s|\rho,\tau\right)\right]\big|_{\tau=0} < E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right]\Big|_{\tau=0} \tag{50}$$

Thus, by the intermediate value theorem, there must exist at least one $\underline{\tau} \in (0,1)$ such that:

$$E\left[u_i\left(m_s|\rho,\tau\right)\right]\big|_{\underline{\tau}} = E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right]\Big|_{\underline{\tau}} \tag{51}$$

To show that there is ***exactly*** *one such $\underline{\tau}$, we will demonstrate that*

$$\frac{\partial E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right]}{\partial\tau} < \frac{\partial E\left[u_i\left(m_s|\rho,\tau\right)\right]}{\partial\tau} \tag{52}$$

Inequality (52) above implies that

$$E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right] - E\left[u_i\left(m_s|\rho,\tau\right)\right] \tag{53}$$

is monotonically decreasing in τ, and when combined with the intermediate value theorem, this means that $\underline{\tau}$ is unique, and that $b_l^{pub} = m_s$ for all $\tau \geq \underline{\tau}$.

In order to show Inequality (52), we rely on the fact that $\rho < \underline{\rho}$, which implies

$$b_l^{pri} = \left(\frac{4^{1/\rho} - 1}{4^{1/\rho} + 4}\right) m_l > m_s \tag{54}$$

Taking the derivatives of the expected utility equations (45 and 47) with respect to τ:

$$\frac{\partial}{\partial \tau} E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right] = \frac{\partial}{\partial \tau}\frac{(1-\tau)^{1-\rho}}{2(1-\rho)}\left((m_l - b_l^{pri})^{1-\rho} + (m_l + 4b_l^{pri})^{1-\rho}\right) \tag{55}$$

$$= -\frac{(1-\tau)^{-\rho}}{2}\left((m_l - b_l^{pri})^{1-\rho} + (m_l + 4b_l^{pri})^{1-\rho}\right) \tag{56}$$

$$\frac{\partial}{\partial \tau} E\left[u_i\left(m_s|\rho,\tau\right)\right] = \frac{\partial}{\partial \tau}\frac{1}{2}\left(\frac{d^{1-\rho}}{1-\rho} + \frac{[(1-\tau)(5m_s) + d]^{1-\rho}}{1-\rho}\right) \tag{57}$$

$$= \frac{\partial}{\partial \tau}\frac{1}{2}\left(\frac{(m_l - m_s)^{1-\rho}}{1-\rho} + \frac{[(1-\tau)(5m_s) + m_l - m_s]^{1-\rho}}{1-\rho}\right) \tag{58}$$

$$= -\frac{5m_s}{2}\left[(1-\tau)(5m_s) + m_l - m_s\right]^{-\rho} \tag{59}$$

For $\rho > 0$ and $x > 0$, $x^{-\rho}$ is decreasing in x, and thus $-x^{-\rho}$ is increasing in x. Thus, because $[(1-\tau)(5m_s) + (1-\tau)(m_l - m_s)] < [(1-\tau)(5m_s) + m_l - m_s]$, we have:

$$\frac{\partial}{\partial \tau} E\left[u_i\left(m_s|\rho,\tau\right)\right] = -\frac{5m_s}{2}\left[(1-\tau)(5m_s) + m_l - m_s\right]^{-\rho} \tag{60}$$

$$> -\frac{5m_s}{2}\left[(1-\tau)(5m_s) + (1-\tau)(m_l - m_s)\right]^{-\rho} \tag{61}$$

$$= -\frac{5m_s(1-\tau)^{-\rho}}{2}[m_l + 4m_s]^{-\rho} \tag{62}$$

Then, because $m_l > m_s$, we have:

$$\frac{\partial}{\partial \tau} E\left[u_i\left(m_s|\rho,\tau\right)\right] > -\frac{(m_l + 4m_s)(1-\tau)^{-\rho}}{2}\left([m_l + 4m_s]^{-\rho}\right) \tag{63}$$

$$= -\frac{(1-\tau)^{-\rho}}{2}(m_l + 4m_s)^{1-\rho} \tag{64}$$

Note that the bound on $E\left[u_i\left(m_s|\rho,\tau\right)\right]$ in Equation (64) resembles the expression in Equation (56) above. All that remains is to show that the sum of the two terms in Equation (56) is always larger in magnitude than the analogous term in Equation (64).

For $\rho \in (0,1)$, $x^{1-\rho}$ is increasing in x. Since $\rho < \underline{\rho}$ implies that $b_l^{pri} > m_s$, we know that

$$(m_l + 4m_s)^{1-\rho} < (m_l + 4b_l^{pri})^{1-\rho} \tag{65}$$

and

$$-\frac{(1-\tau)^{-\rho}}{2}(m_l + 4m_s)^{1-\rho} > -\frac{(1-\tau)^{-\rho}}{2}\left(m_l + 4b_l^{pri}\right)^{1-\rho}. \tag{66}$$

Then, since $(m_l - b_l^{pri})^{1-\rho}$ is positive, it is clear that

$$\begin{aligned}
\frac{\partial}{\partial \tau} E\left[u_i\left(m_s|\rho,\tau\right)\right] &> -\frac{(1-\tau)^{-\rho}}{2}\left(m_l + 4b_l^{pri}\right)^{1-\rho} && (67)\\
&> -\frac{(1-\tau)^{-\rho}}{2}\left((m_l - b_l^{pri})^{1-\rho} + (m_l + 4b_l^{pri})^{1-\rho}\right) && (68)\\
&= \frac{\partial}{\partial \tau} E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right] && (69)
\end{aligned}$$

whenever $\rho < \underline{\rho}$ and $\rho \in (0,1)$. Alternatively, if $\rho > 1$, $x^{1-\rho}$ is decreasing in x, so

$$\begin{aligned}
m_l + 4m_s &> m_l - b_l^{pri} && (70)\\
\Rightarrow (m_l + 4m_s)^{1-\rho} &< (m_l - b_l^{pri})^{1-\rho} && (71)\\
\Rightarrow -\frac{(1-\tau)^{-\rho}}{2}\left(m_l + 4m_s\right)^{1-\rho} &> -\frac{(1-\tau)^{-\rho}}{2}\left(m_l - b_l^{pri}\right)^{1-\rho} && (72)
\end{aligned}$$

Hence,

$$\begin{aligned}
E\left[u_i\left(m_s|\rho,\tau\right)\right] &> -\frac{(1-\tau)^{-\rho}}{2}\left(m_l + 4m_s\right)^{1-\rho} && (73)\\
&> -\frac{(1-\tau)^{-\rho}}{2}\left(m_l - b_l^{pri}\right)^{1-\rho} && (74)\\
&> E\left[u_i\left(b_l^{pri}|\rho,\tau\right)\right] && (75)
\end{aligned}$$

□

Proof of Proposition 3. *Recall Equation (7), which states that an individual who invests $b < m_s$ will choose:*

$$b^* = \left(\frac{4^{1/\rho}-1}{4^{1/\rho}+4}\right) m_s + \frac{\left(4^{1/\rho}-1\right)d}{(1-\tau)\left(4^{1/\rho}+4\right)}. \tag{76}$$

Note that

$$\begin{aligned}
\left(\frac{4^{1/\rho}-1}{4^{1/\rho}+4}\right) m_s + \frac{\left(4^{1/\rho}-1\right)d}{(1-\tau)\left(4^{1/\rho}+4\right)} &\le m_s && (77)\\
\Leftrightarrow \left(4^{1/\rho}-1\right) d &\le 5\left(1-\tau\right) m_s && (78)\\
\Leftrightarrow \tau &\le 1 - \left(4^{1/\rho}-1\right)\frac{d}{5m_s}. && (79)
\end{aligned}$$

Inspection reveals that the

$$\bar{\tau}\left(\rho\right) = 1 - \left(4^{1/\rho}-1\right)\frac{d}{5m_s} \tag{80}$$

is increasing in ρ and approaches one as ρ approaches infinity. Moreover, solving the equation $\bar{\tau}(\rho) = 0$ reveals that $\bar{\tau}(\rho)$ is exactly zero when $\rho = \underline{\rho}$. Thus, for $\rho > \underline{\rho}$, there exists a $\bar{\tau}(\rho) \in (0,1)$ such that

$$\left(\frac{4^{1/\rho}-1}{4^{1/\rho}+4}\right) m_s + \frac{\left(4^{1/\rho}-1\right)d}{(1-\tau)\left(4^{1/\rho}+4\right)} < m_s \tag{81}$$

whenever $\tau < \bar{\tau}(\rho)$. Individuals with $\rho > \underline{\rho}$ and $\tau < \bar{\tau}(\rho)$ who are assigned to the public, large endowment treatment will choose the optimal b_l^{pub} characterized by Equation (81), thereby guaranteeing that $b_l^{pri} < b_l^{pub} < m_s$.

Otherwise, if $\rho > \underline{\rho}$ *but* $\tau \geq \bar{\tau}(\rho)$, $b_l^{pri} < m_s$, *but*

$$\left(\frac{4^{1/\rho}-1}{4^{1/\rho}+4}\right) m_s + \frac{\left(4^{1/\rho}-1\right) d}{(1-\tau)\left(4^{1/\rho}+4\right)} \geq m_s. \tag{82}$$

In this case,

$$\lim_{b \to m_s^-} \frac{\partial}{\partial b} E\left[u_i\left(b|\rho,\tau\right)\right] > 0 \text{ but} \lim_{b \to m_s^+} \frac{\partial}{\partial b} E\left[u_i\left(b|\rho,\tau\right)\right] < 0. \tag{83}$$

As b *increases beyond* m_s, *the amount that individuals are pressured into sharing increases discontinuously, so consumption and utility decrease discontinuously. Hence, these individuals will set* $b_l^{pub} = m_s$.

□