Online Appendix:
Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economics

Not for print publication

A. Proofs

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

A.1 Proof of Proposition 1

Outline of the Proof. We wish to show that the probability of investing 80 shillings or less is higher in the public, large endowment treatment than in the private, large endowment treatment. The probability of investing 80 shillings or less (i.e. of the the event $b \leq 80$ occurring) in either the private or the public large endowment treatment is:

$$\Pr(b \leq 80) = \frac{\sum_{b=0}^{b=80} e^{[EV^t(b)]/\sigma_\varepsilon}}{\sum_{b=0}^{b=80} e^{[EV^t(b)]/\sigma_\varepsilon} + \sum_{b=90}^{b=180} e^{[EV^t(b)]/\sigma_\varepsilon}} \tag{1}$$

where $EV^t(b)$ indicates the CRRA expected utility of investing $b \geq 0$ in treatment t. Using the specific values of m_l and m_s for the experiment, we have:

$$EV^{PRIVATE \times LARGE}(b) = \frac{1}{2(1-\rho)} \left[(180+4b)^{1-\rho} + (180-b)^{1-\rho} \right]$$

In the public, large endowment treatment, the CRRA expected utility is:

$$EV^{PUBLIC \times LARGE}(b) = \frac{1}{2(1-\rho)} \left(\left[(1-\tau)(180+4b) + \tau 100 \mathbb{1}\{b \leq 80\} \right]^{1-\rho} + \left[(1-\tau)(180-b) + \tau 100 \mathbb{1}\{b \leq 80\} \right]^{1-\rho} \right)$$

Note that for all b, $EV^{PRIVATE \times LARGE}(b) = EV^{PUBLIC \times LARGE}(b)$ in the special case where $\tau = 0$. Thus, to prove the proposition, we can use the investment probability formulas from the public, large endowment treatment to show that, as τ moves from zero to a positive number, the probability of choosing to invest an amount of at most 80 increases. This also allows us to simplify our notation by using EV

as a shorthand for $EV^{PUBLIC \times LARGE}$ throughout the remainder of the proof, since we consider only expressions for the CRRA expected utilities in the public, large endowment treatment.

To easily distinguish values of b that are less than or equal to 80 from higher values, we partition the set of investment levels $B = \{0, 10, \ldots, 180\}$ as follows: $B = A \cup Z$ where $A = \{0, 10, \ldots, 80\}$ and $Z = \{90, 100, \ldots, 180\}$. This allows us to rewrite the probability of investing 80 shillings or less as

$$\Pr(b \leq 80) = \frac{\sum_{a \in A} e^{[EV(a)]/\sigma_\varepsilon}}{\sum_{a \in A} e^{[EV(a)]/\sigma_\varepsilon} + \sum_{z \in Z} e^{[EV(z)]/\sigma_\varepsilon}}. \tag{2}$$

Given the logit functional form of the probability, any one of the $[EV(a)]/\sigma_\varepsilon$ values can be subtracted from all of the exponents in the expression without changing the value of the expression — this is equivalent to multiplying the entire expression by $\left(1/e^{[EV(a)]/\sigma_\varepsilon}\right) / \left(1/e^{[EV(a)]/\sigma_\varepsilon}\right)$. Thus, for any fixed $\tilde{a} \in A$, we can write

$$\Pr(b \leq 80) = \frac{\sum_{a \in A} e^{[EV(a) - EV(\tilde{a})]/\sigma_\varepsilon}}{\sum_{a \in A} e^{[EV(a) - EV(\tilde{a})]/\sigma_\varepsilon} + \sum_{z \in Z} e^{[EV(z) - EV(\tilde{a})]/\sigma_\varepsilon}}. \tag{3}$$

The proof will build upon two lemmas. In the first, we show that for any $a \in A$ and $z \in Z$,

$$[EV(z)] - [EV(\tilde{a})]$$

is decreasing in τ. Thus, as τ moves from zero (in the private treatment) to a positive number (in the public treatment), every rescaled $e^{[EV(z) - EV(\tilde{a})]/\sigma_\varepsilon}$ term decreases. In the second lemma, we demonstrate that $\tilde{a}$ can be chosen so that, as τ moves from zero to a positive number, every $e^{[EV(a) - EV(\tilde{a})]/\sigma_\varepsilon}$ term (weakly) increases. Taken together, these lemmas imply that

$$\sum_{a=0}^{a=80} e^{[EV(a) - EV(\tilde{a})]/\sigma_\varepsilon}$$

is higher and that

$$\sum_{z=90}^{z=180} e^{[EV(z) - EV(\tilde{a})]/\sigma_\varepsilon}$$

is lower in the public, large endowment treatment than in the private, large endowment treatment; hence, the probability of investing 80 shillings or less given by Equation 3 is higher in the public treatments. We now proceed to state and prove the two lemmas before formally proving the proposition.

Lemma 1. *For any $a \in \{0, 10, \ldots, 80\}$ and $z \in \{90, 100, \ldots, 180\}$, the expression*

$$\begin{aligned} EV(z) - EV(a) = & \frac{1}{2(1-\rho)} \left([(1-\tau)(180+4z)]^{1-\rho} + [(1-\tau)(180-z)]^{1-\rho} \right) \\ & - \frac{1}{2(1-\rho)} \left([(1-\tau)(180+4a) + 100\tau]^{1-\rho} + [(1-\tau)(180-a) + 100\tau]^{1-\rho} \right) \end{aligned}$$

is decreasing in τ.

Proof of Lemma 1. The proof of the lemma proceeds by taking the derivative of the expression and successively showing higher upper bounds until one of them is clearly less than zero. We begin by rewriting the expression of interest as:

$$\begin{aligned} EV(z) - EV(a) = & \frac{1}{2(1-\rho)} \Big((1-\tau)^{1-\rho} \left[(180+4z)^{1-\rho} + (180-z)^{1-\rho} \right] \\ & - [100 + (1-\tau)(80+4a)]^{1-\rho} + [100 + (1-\tau)(80-a)]^{1-\rho} \Big) \end{aligned}$$

Taking the derivative with respect to τ allows us to define:

$$\frac{\partial}{\partial\tau}\big[EV(z)-EV(a)\big] = -\frac{(1-\tau)^{-\rho}}{2}\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right] + \frac{1}{2}\underbrace{\left[(80+4a)[100+(1-\tau)(80+4a)]^{-\rho}\right]}_{=p}+\frac{1}{2}\underbrace{\left[(80-a)[100+(1-\tau)(80-a)]^{-\rho}\right]}_{=q} \tag{4}$$

We will now proceed by characterizing expressions B, C, and D and a positive constant k such that $k\frac{\partial}{\partial\tau}[EV(z)-EV(a)] \leq B$, $B < C$, $C \leq D$, and $D < 0$.

In order to establish a larger B (such that $k\frac{\partial}{\partial\tau}[EV(z)-EV(a)] \leq B$), we note that the terms labeled p and q in Equation 4 both have the form $X \cdot Y^{-\rho}$. These are weakly decreasing in Y for $X \geq 0$ (since $\rho \geq 0$), so if we substitute $W < Y$, the new terms will be larger than the originals: $X \cdot Y^{-\rho} < X \cdot W^{-\rho}$. We therefore substitute $(1-\tau)100$ for 100. For the p term, we have:

$$\begin{aligned}(80+4a)(100+(1-\tau)[80+4a])^{-\rho} &\leq (80+4a)\left[(1-\tau)100+(1-\tau)(80+4a)\right]^{-\rho}\\ &= (80+4a)(1-\tau)^{-\rho}[180+4a]^{-\rho}\end{aligned}$$

For the q term, we have:

$$\begin{aligned}(80-a)\left[100+(1-\tau)(80-a)\right]^{-\rho} &\leq (80-a)\left[(1-\tau)100+(1-\tau)(80-a)\right]^{-\rho}\\ &= (80-a)(1-\tau)^{-\rho}(180-a)^{-\rho}\end{aligned}$$

Combining these back into equation 4 and multiplying through by $(1-\tau)^{\rho}$ gives us:

$$k\frac{\partial}{\partial\tau}[EV(z)-EV(a)] \leq B = -\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right]+(80+4a)(180+4a)^{-\rho}+(80-a)(180-a)^{-\rho} \tag{5}$$

where $k = (1-\tau)^{\rho}/2$.

We now establish an even higher bound, C. In each of the right two terms, 80 can be rewritten as the sum of 180 and -100, breaking the two terms into four:

$$\begin{aligned}B =& -\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right]\\ &+\underbrace{(180+4a)(180+4a)^{-\rho}-100(180+4a)^{-\rho}}_{=(80+4a)(180+4a)^{-\rho}}+\underbrace{(180-a)(180-a)^{-\rho}-100(180-a)^{-\rho}}_{=(80-a)(180-a)^{-\rho}}\\ =& -\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right]+\left[(180+4a)^{1-\rho}+(180-a)^{1-\rho}\right]-100\left[\frac{1}{(180+4a)^{\rho}}+\frac{1}{(180-a)^{\rho}}\right]\end{aligned} \tag{6}$$

The rightmost term may then be re-written as:

$$-100\left[\frac{1}{(180+4a)^{\rho}}+\frac{1}{(180-a)^{\rho}}\right] = -100\left[\frac{1}{(180+4a)(180+4a)^{\rho-1}}+\frac{1}{(180-a)(180-a)^{\rho-1}}\right] \tag{7}$$

This term is negative, so an upper bound for the term will be its smallest possible magnitude. Inspecting the two denominators, we see the terms $(180+4a)^{\rho-1}$ and $(180-a)^{\rho-1}$. We can increase one of the denominators by replacing the smaller of these with the larger. Which is larger depends on ρ. If $\rho < 1$, then $\rho-1$ is negative, so the function $x^{\rho-1}$ is decreasing in x, and $(180-a)^{\rho-1}$ is larger; alternatively, for $\rho > 1$, $\rho-1$ is positive, so $x^{\rho-1}$ is increasing in x, and $(180+4a)^{\rho-1}$ is larger.[1] The proof works either way. We first illustrate with the $\rho < 1$ case: $(180-a)^{\rho-1}$ is larger than $(180+4a)^{\rho-1}$, so, to find an upper bound (on a negative expression), we increase one of the denominators, substituting $(180-a)^{\rho-1}$

[1] The special case of $\rho = 1$ will be treated separately below.

for $(180+4a)^{\rho-1}$:

$$-100\left[\frac{1}{(180+4a)^{\rho}}+\frac{1}{(180-a)^{\rho}}\right]=-100\left[\frac{1}{(180+4a)(180+4a)^{\rho-1}}+\frac{1}{(180-a)(180-a)^{\rho-1}}\right]$$
$$<-100\left[\frac{1}{(180+4a)(180-a)^{\rho-1}}+\frac{1}{(180-a)(180-a)^{\rho-1}}\right]$$

Which may be rewritten:

$$-100\left[\frac{1}{(180+4a)^{\rho}}+\frac{1}{(180-a)^{\rho}}\right]<-(180-a)^{1-\rho}\cdot 100\cdot\left[\frac{1}{(180+4a)}+\frac{1}{(180-a)}\right] \tag{8}$$

Substituting into 6, we now have:

$$\begin{aligned}B&<C\\&=-\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right]+\left[(180+4a)^{1-\rho}+(180-a)^{1-\rho}\right]\\&\quad-(180-a)^{1-\rho}\cdot(100)\cdot\left[\frac{1}{(180+4a)}+\frac{1}{(180-a)}\right]\end{aligned} \tag{9}$$

The term on the last line is still negative, and thus it is bounded above by its smallest magnitude. The sum of the two fractions on the right is decreasing at $a=0$, but increases to ∞ as $a\to 180$. We can minimize the sum by finding the interior solution: taking the first order condition and solving the resulting quadratic equation, we find that the minimum occurs at $a=30$. Substituting into part of inequality 8 gives us:

$$\begin{aligned}(100)\cdot\left(\frac{1}{180+4a}+\frac{1}{180-a}\right)&<\frac{100}{180+4\cdot 30}+\frac{100}{180-30}\\&=\frac{10}{18+12}+\frac{10}{18-3}\\&=\frac{10}{30}+\frac{10}{15}\\&=\frac{1}{3}+\frac{2}{3}\\&=1\end{aligned} \tag{10}$$

Substituting this back into inequality 9, we have:

$$\begin{aligned}C\le D&=-\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right]+\left[(180+4a)^{1-\rho}+(180-a)^{1-\rho}\right]-(180-a)^{1-\rho}\cdot 1\\&=-\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right]+(180+4a)^{1-\rho}\end{aligned} \tag{11}$$

However, we know that $a<z$, so:

$$180-z<180+4a<180+4z \tag{12}$$

and thus, because $x^{1-\rho}$ is continuous and monotonic, the larger of the two negative terms on the left must be greater in magnitude than the positive term on the right. Thus, $D<0$ when $\rho<1$.

The case where $\rho>1$ follows a nearly identical proof. After equation 7, we instead substitute $(180+4a)^{\rho-1}$ for $(180-a)^{\rho-1}$, allowing us to write:

$$-100\left[\frac{1}{(180+4a)^{\rho}}+\frac{1}{(180-a)^{\rho}}\right]<-(180+4a)^{1-\rho}\cdot 100\cdot\left[\frac{1}{(180+4a)}+\frac{1}{(180-a)}\right] \tag{13}$$

This yields a different version of equation 11:

$$C<D=-\left[(180+4z)^{1-\rho}+(180-z)^{1-\rho}\right]+(180-a)^{1-\rho} \tag{14}$$

However, again, we know that $a < z$, so:

$$180 - z < 180 - a < 180 + 4z \tag{15}$$

and thus we can once again be certain that the larger of the two negative terms in equation 14 is greater in magnitude than the positive term. Thus, it is also the case that $D < 0$ when $\rho > 1$.

Thus, for both the $\rho > 1$ or $\rho < 1$ cases, we have found a positive constant k such that $k\frac{\partial}{\partial\tau}\left[EV(z) - EV(a)\right] \leq B$, $B < C$, $C \leq D$, and $D < 0$.

It only remains to consider the $\rho = 1$ case. When $\rho = 1$, $EV(z) - EV(a)$ can be written as:

$$\frac{1}{2}\Big(2\ln(1-\tau) + \ln(180+4z) + \ln(180-z) - \ln\left[100 + (1-\tau)(80+4a)\right] - \ln\left[100 + (1-\tau)(80-a)\right]\Big) \tag{16}$$

Taking the derivative with respect to τ yields:

$$\frac{1}{2}\left[-\frac{2}{1-\tau} + \frac{80+4a}{100+(1-\tau)(80+4a)} + \frac{80-a}{100+(1-\tau)(80-a)}\right] \tag{17}$$

This may be bounded above — with weak inequality for $\tau = 0$, and with strict inequality for positive τ — by decreasing the denominators of the positive terms, thereby increasing the total. To do this, we substitute in $\tau 100$ for 100, yielding:

$$\frac{\partial}{\partial\tau}\left[EV(z) - EV(a)\right] \leq \frac{1}{2}\left[-\frac{2}{1-\tau} + \frac{80+4a}{(1-\tau)100+(1-\tau)(80+4a)} + \frac{80-a}{(1-\tau)100+(1-\tau)(80-a)}\right] \tag{18}$$

Then pulling out the common factor:

$$\frac{\partial}{\partial\tau}\left[EV(z) - EV(a)\right] \leq \frac{1}{2(1-\tau)}\left(-2 + \frac{80+4a}{180+4a} + \frac{80-a}{180-a}\right) \tag{19}$$

Each of the two fractions in the bracketed expression is clearly less than 1, so their sum is clearly less than 2. Thus, with strict inequality:

$$\frac{\partial}{\partial\tau}\left[EV(z) - EV(a)\right] < 0$$

when $\rho = 1$. Thus, we have now demonstrated that the lemma holds in the $\rho < 1$, $\rho = 1$, and $\rho > 1$ cases. □

Lemma 2. *For any $\bar{\tau} > 0$, there exists $\bar{a}(\bar{\tau}) \in A = \{0, 10, ..., 80\}$ such that*

$$\left[EV(a) - EV\left(\bar{a}(\bar{\tau})\right)\Big|\tau > 0\right] \geq \left[EV(a) - EV\left(\bar{a}(\bar{\tau})\right)\Big|\tau = 0\right] \tag{20}$$

for all $a \in \{0, 10, ..., 80\}$.

Proof of Lemma 2. We begin by rewriting the expression of interest as:

$$\left[EV(a)|\tau > 0\right] - \left[EV(a)|\tau = 0\right] \geq \left[EV\left(\bar{a}(\bar{\tau})\right)|\tau > 0\right] - \left[EV\left(\bar{a}(\bar{\tau})\right)|\tau = 0\right] \tag{21}$$

Define:

$$\bar{a}(\bar{\tau}) = \underset{a\in\{0,10,\ldots,80\}}{\arg\min}\left(\left[EV\left(\bar{a}(\bar{\tau})\right)|\tau > 0\right] - \left[EV\left(\bar{a}(\bar{\tau})\right)|\tau = 0\right]\right) \tag{22}$$

By construction, inequality 21 must be satisfied by this $\bar{a}(\bar{\tau})$, proving Lemma 2. □

Proof of Proposition 1. We wish to show that

$$\Pr(b \leq 80) = \frac{\sum_{a \in A} e^{[EV(a) - EV(\tilde{a})]/\sigma_\varepsilon}}{\sum_{a \in A} e^{[EV(a) - EV(\tilde{a})]/\sigma_\varepsilon} + \sum_{z \in Z} e^{[EV(z) - EV(\tilde{a})]/\sigma_\varepsilon}}. \tag{23}$$

increases as we move from the private, large endowment treatment ($\tau = 0$) to the public, large endowment treatment ($\tau \in (0, 1)$). Because σ_ε is a positive constant and e^x is increasing in x, Lemma 1 implies that every

$$e^{[EV(z) - EV(\tilde{a})]/\sigma_\varepsilon}$$

term is lower when $\tau = 0$ than when $\tau > 0$. Similarly, Lemma 2 implies that every

$$e^{[EV(a) - EV(\tilde{a})]/\sigma_\varepsilon}$$

is weakly higher when $\tau = 0$ than when $\tau > 0$. Thus, because

$$\frac{X}{X + Y}$$

is increasing in X and decreasing in Y, the probability if investing no more than 80 shillings must be weakly higher in the public, large endowment treatment than in the private, large endowment treatment. □

B. Experimental Instructions

Translated from Swahili. Original Swahili instructions available upon request.

Read to participants at start of experimental session:

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

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

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

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

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

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

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

Now, imagine that you start with 180 shillings, and you decide to put 90 shillings into the business cup, and the remaining 90 shillings into the savings cup. If the coin lands with tails facing up, you lose the 90 shillings in the business cup, and you will get only the 90 shillings in the savings cup. However, if the coin lands with heads facing up you'll take home the 90 shillings in the savings cup and 5 times the 90 shillings in the business cup. That's 90 shillings, together with 450 shillings, or in total, 540 shillings.

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

For example, X is a participant in this game.[2] We would like him/her to announce the amount of money he/she put in the business cup. X, are you ready? How much money did you put in the business cup? *X responds: 20 Shillings.* The coin landed with which side facing up? *X responds: Heads.* Thank you X, please sit. How much money did this participant put in the business cup? *Audience responds: 20.* And the coin landed with which side facing up? *Audience responds: Heads.* Therefore, he/she received how much money from the business? *Audience responds: 100.* Think: how much money did he/she put in the savings cup? In fact, we can't know. This is his/her secret. It is possible that he/she started with 80 shillings, and he/she put 60 shillings in the savings cup; it is also possible that he/she started with 180 shillings, and he/she put 160 shillings in the savings cup. We can't know. Still, you are not required to announce what you put in the savings cup.

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

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

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

Some of the people who we ask to announce their decisions will also be given the opportunity to avoid having to make an announcement to the room. We'll give those few people a chance to pay a fee to avoid announcing their decisions to the rest of the room. Before you make your decisions, we'll tell you whether you will be given the chance to pay a fee and avoid announcing your decisions to the room. The fee will be between 10 shillings and 60 shillings — we'll tell you before you make your decisions.

Are there any questions so far? In short: there are two amounts of money a person can receive to use in this game. You will be given 80 shillings, or 180 shillings. You'll decide how you want to divide that money between a business cup and a savings cup. The money that you put in the business cup can be used to generate more money — like a business — but that money can also be lost. Let's remind ourselves: how much money could you put in the business cup? Zero, ten, twenty, thirty, forty, up to all the money you have been given to use in the game. You'll shake a coin in a bottle to determine the outcome. If it lands with heads facing up, you'll get five times what you put in the business cup; if the coin lands with tails facing up, you'll lose the money that you put in the business cup. But remember, you will get all the money you put in the savings cup. After everyone has made their business decisions, some of you will be

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

asked to stand up and describe your choices to everyone in the room. Even if we ask you to announce your decisions to everyone else here, we may also give you the opportunity to "buy out" of having to make an announcement.

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

Read to individual subjects not assigned to the Price Treatments:

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

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

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

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

Read to individual subjects assigned to the Price Treatments:

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

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

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

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

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

C. Additional Summary Statistics and Balance Checks

Table 1: Random Assignment of Experimental Treatments: Additional Balance Checks

Treatments:		All	Small Endowment		Large Endowment	
Subjects:		All	Women	Men	Women	Men
	Mean	(1)	(2)	(3)	(4)	(5)
Years of schooling	6.74	0.51	0.23	0.96	0.13	0.86
Age	36.82	0.51	0.14	0.90	0.76	0.32
Currently married	0.77	0.50	0.42	0.81	0.60	0.99
Spouse attended the experiment	0.08	0.03^{**}	0.02^{**}	0.22	0.23	0.69
Ever married	0.88	0.26	0.58	0.94	0.58	0.55
HH size	6.18	0.21	0.05^{**}	0.16	0.48	0.47
Close relatives in village (outside of HH)	2.36	0.38	0.08^{*}	0.87	0.32	0.02^{**}
Any close relatives attended the experiment	0.19	0.88	0.26	0.74	0.75	0.41
Distant relatives in village	10.41	0.00^{***}	0.02^{**}	0.01^{***}	0.73	0.09^{*}
No. chicken owned by HH	6.42	0.75	0.63	0.43	0.34	0.87
No. cattle owned by HH	1.20	0.03^{**}	0.03^{**}	0.93	0.30	0.11
No. bicycles owned by HH	0.83	0.21	0.07^{*}	0.73	0.37	0.77
No. phones owned by HH	0.73	1.00	0.52	0.68	0.87	0.94
No. televisions owned by HH	0.14	0.13	0.01^{**}	0.59	0.16	0.81
Value of durable HH assets (in US dollars)	469.31	0.11	0.01^{***}	0.88	0.41	0.37
HH farms	0.99	0.85	0.31	0.22	0.88	0.17
HH uses fertilizer on crops	0.46	0.47	0.20	0.86	0.17	0.83
Has regular employment	0.08	0.54	0.08^{*}	0.53	0.62	0.51
Monthly wages if employed (in US dollars)	39.28	0.13	0.22	0.14	0.27	0.06^{*}
Any HH member employed	0.23	0.76	0.69	0.47	0.37	0.75
Self-employed	0.35	0.45	0.09^{*}	0.51	0.54	0.91
Has bank account	0.17	0.72	0.03^{**}	0.43	0.20	0.07^{*}
Member of ROSCA	0.53	0.83	0.14	0.06^{*}	0.98	0.54
HH gave transfer in last 3 months	0.90	0.58	0.24	0.93	0.51	0.69
Transfers to HHs in village (in US dollars)	6.79	0.49	0.87	0.57	0.88	0.61
HH received transfer in last 3 months	0.41	0.03^{**}	0.29	0.06^{*}	0.29	0.27
Transfers from HHs in village (in US dollars)	2.58	0.49	0.12	0.96	0.79	0.69
Community groups	2.76	0.37	0.56	0.03^{**}	0.31	0.49
Belongs to Luhya ethnic group	0.80	0.66	0.95	0.88	0.40	0.88
Local minority ethnic group	0.20	0.67	0.93	0.87	0.37	0.88
Christian	0.98	0.46	0.44	0.67	0.48	0.22
Observations		2145	654	430	644	417

Numbered columns report p-values from tests of the joint significance of price dummies in a regression in which the variable listed in the first column is used as the dependent variable. $***$ indicates significance at the 99 percent level; $**$ indicates significance at the 95 percent level; and $*$ indicates significance at the 90 percent level.

Table 2: Random Assignment of Exit Prices: Balance Check

Sample:	All Subjects	Women	Men
Female	0.19	.	.
Years of schooling	0.43	0.08*	0.81
Age	0.11	0.02**	0.55
Currently married	0.63	0.38	0.12
Spouse attended the experiment	0.74	0.27	0.09*
Ever married	0.06*	0.31	0.11
HH size	0.71	0.56	0.48
Close relatives in village (outside of HH)	0.87	0.34	0.77
Any close relatives attended the experiment	0.92	0.13	0.39
Distant relatives in village	0.23	0.16	0.92
Close relatives attending experiment	0.92	0.13	0.39
No. chicken owned by HH	0.51	0.64	0.56
No. cattle owned by HH	0.66	0.92	0.07*
No. bicycles owned by HH	0.65	0.42	0.83
No. phones owned by HH	0.54	0.09*	0.88
No. televisions owned by HH	0.91	0.28	0.62
Value of durable HH assets (in US dollars)	0.72	0.30	0.91
HH farms	0.22	0.31	0.32
HH uses fertilizer on crops	0.05**	0.22	0.08*
Has regular employment	0.39	0.79	0.52
Monthly wages if employed (in US dollars)	0.42	0.20	0.41
Any HH member employed	0.03**	0.05*	0.46
Self-employed	0.53	0.73	0.52
Has bank account	0.54	0.52	0.19
Member of ROSCA	0.15	0.04**	0.16
HH gave transfer in last 3 months	0.87	0.67	0.23
Transfers to HHs in village (in US dollars)	0.94	0.91	0.95
HH received transfer in last 3 months	0.05*	0.05**	0.40
Transfers from HHs in village (in US dollars)	0.35	0.13	0.58
Community groups	0.11	0.01**	0.51
Belongs to Luhya ethnic group	0.15	0.00***	0.00***
Local minority ethnic group	0.13	0.00***	0.00***
Christian	0.81	0.22	0.06*
Observations	690	416	274

Table reports p-values from tests of the joint significance of price dummies in a regression in which the variable listed in the first column is used as the dependent variable. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level.

Table 3: Summary Statistics on Outcomes in Experiment, by Treatment

Panel A: All Subjects

Information Condition: *Budget Size:*	Price Small	Price Large	All All
Price of avoiding public announcement (in Kenyan shillings)	34.84	35.07	34.96
	(0.91)	(0.91)	(0.64)
Paid to avoid public announcement?	0.21	0.34	0.28
	(0.02)	(0.03)	(0.02)
Paid to avoid announcement (conditional on successful investment)?	0.29	0.45	0.37
	(0.03)	(0.04)	(0.03)
Paid to avoid announcement (conditional on failed investment)?	0.12	0.21	0.17
	(0.03)	(0.03)	(0.02)
Amount paid to avoid public announcement (in Kenyan shillings)	25.21	31.68	29.26
	(1.74)	(1.50)	(1.16)
Price / observable income (conditional on choosing to pay)	0.19	0.12	0.15
	(0.02)	(0.01)	(0.01)
Price / observable income (conditional on choosing not to pay)	0.28	0.24	0.26
	(0.02)	(0.02)	(0.01)
Observations	345	345	690

Standard errors in parentheses. One US dollar was equivalent to 75.9 Kenyan shillings at the time of the experiment.

Panel B: Women Only

Information Condition: *Budget Size:*	Price Small	Price Large	All All
Price of avoiding public announcement (in Kenyan shillings)	35.22	35.80	35.50
	(1.21)	(1.19)	(0.85)
Paid to avoid public announcement?	0.21	0.36	0.29
	(0.03)	(0.03)	(0.02)
Paid to avoid announcement (conditional on successful investment)?	0.32	0.45	0.39
	(0.05)	(0.05)	(0.03)
Paid to avoid announcement (conditional on failed investment)?	0.10	0.25	0.17
	(0.03)	(0.05)	(0.03)
Amount paid to avoid public announcement (in Kenyan shillings)	26.82	30.93	29.41
	(2.32)	(1.86)	(1.46)
Price / observable income (conditional on choosing to pay)	0.18	0.13	0.15
	(0.02)	(0.02)	(0.01)
Price / observable income (conditional on choosing not to pay)	0.30	0.22	0.27
	(0.02)	(0.02)	(0.02)
Observations	209	207	416

Standard errors in parentheses. One US dollar was equivalent to 75.9 Kenyan shillings at the time of the experiment.

Panel C: Men Only

Information Condition: *Budget Size:*	Price Small	Price Large	All All
Price of avoiding public announcement (in Kenyan shillings)	34.26	33.99	34.12
	(1.38)	(1.42)	(0.99)
Paid to avoid public announcement?	0.20	0.32	0.26
	(0.03)	(0.04)	(0.03)
Paid to avoid announcement (conditional on successful investment)?	0.24	0.46	0.35
	(0.05)	(0.06)	(0.04)
Paid to avoid announcement (conditional on failed investment)?	0.15	0.16	0.15
	(0.05)	(0.05)	(0.03)
Amount paid to avoid public announcement (in Kenyan shillings)	22.59	32.95	29.01

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	(2.54)	(2.53)	(1.93)
Price / observable income (conditional on choosing to pay)	0.20	0.10	0.14
	(0.04)	(0.02)	(0.02)
Price / observable income (conditional on choosing not to pay)	0.25	0.25	0.25
	(0.02)	(0.03)	(0.02)
Observations	136	138	274

Standard errors in parentheses. One US dollar was equivalent to 75.9 Kenyan shillings at the time of the experiment.

D. The Endogeneity of Subject Characteristics: Gender

In this section, we examine the possibility that gender may be proxying for some other household or individual characteristic that actually explains the differential impacts of observability that we document in the paper. We proceed in three steps. We begin by presenting our main regression results (on investment probabilities and the willingness-to-pay to avoid making the public announcement) for the pooled sample of men and women. Our most robust result is that women are more likely to invest no more than 80 shillings in the public and price (large endowment) treatments than in the private treatment; in the pooled specifications, we show that this represents a statistically significant difference in the treatment effect of observability on women relative to men. We then ask whether there is evidence of differential selection into the experiment by gender. Our data suggest that a number of individual and household characteristics predict participation in the experiment, but we find no evidence of differential selection by gender. Finally, we argue that, if gender is simply proxying for another characteristic, it must be the case that such a characteristic is (a) associated with gender in the cross-section and (b) associated with the observed treatment effect of observability in a way (directionally) that would explain observed gender differences. We do not find any evidence in support of this explanation.

D.1 Replicating Our Regression Analysis in the Pooled Sample

Table 4: Pooled OLS Regressions of Investment Outcomes in Large Endowment Treatments

Specification:	OLS (1)	OLS (2)	OLS (3)	OLS (4)
Panel A: Dep. Var. = Indicator for Investing 80 Shillings or Less				
Public or price treatment	0.049	-0.025	0.063*	-0.008
	(0.032)	(0.052)	(0.032)	(0.052)
Female	·	-0.117**	·	-0.136**
		(0.054)		(0.057)
Female × public treatments	·	0.121*	·	0.117*
		(0.066)		(0.066)
Test of H_0: public + female×public = 0 (p-value)	·	0.019	·	0.007
Panel B: Dep. Var. = Indicator for Investing Exactly 80 Shillings				
Public or price treatment	0.045*	0.018	0.053*	0.035
	(0.027)	(0.044)	(0.027)	(0.045)
Female	·	-0.048	·	-0.047
		(0.044)		(0.049)
Female × public treatments	·	0.044	·	0.030
		(0.055)		(0.057)
Test of H_0: public + female×public = 0 (p-value)	·	0.064	·	0.058
Village FEs	No	No	Yes	Yes
Additional Controls	No	No	Yes	Yes
Observations	1061	1061	1061	1061

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported; logit and probit results are similar. Sample restricted to subjects receiving the larger endowment. A constant is included in all specifications. Even-numbered columns include controls for all variables that are not balanced across genders (see Online Appendix Table 7): age, education level, the log value of household assets, the number of close relatives residing in the village, the number of distant relatives residing in the village, involvement in community groups, and indicators for having a bank savings account, participating in a ROSCA, having given a gift or loan to another household in the last 3 months, and belonging to a local ethnic minority; even-numbered columns also include controls for marital status and household size.

Table 5: Pooled OLS Regressions of Paying to Avoid Announcing

Dependent Variable:	Paid to Avoid Public Announcement			
Specification:	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
Price of exit	-0.006***	-0.007***	-0.005***	-0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Large budget	0.142***	0.02	0.12**	-0.094
	(0.033)	(0.086)	(0.049)	(0.112)
Price × large budget	.	0.002	.	0.005*
		(0.002)		(0.003)
Coin flip lands heads	.	0.173***	.	0.125**
		(0.042)		(0.06)
Heads × large budget	.	0.063	.	0.148
		(0.064)		(0.097)
Female × price of exit	.	.	-0.0005	-0.0004
			(0.001)	(0.001)
Female × large budget	.	.	0.037	0.186
			(0.062)	(0.129)
Female × price × large budget	.	.	.	-0.003
				(0.003)
Female × heads	.	.	.	0.082
				(0.079)
Female × heads × large budget	.	.	.	-0.145
				(0.124)
Village FEs	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
Observations	688	687	688	687
R^2	0.161	0.214	0.161	0.218

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported. Sample restricted to subjects assigned to the price treatments. A constant is included in all specifications. All specifications include controls for all variables that are not balanced across genders (see Online Appendix Table 7): age, education level, the log value of household assets, the number of close relatives residing in the village, the number of distant relatives residing in the village, involvement in community groups, and indicators for having a bank savings account, participating in a ROSCA, having given a gift or loan to another household in the last 3 months, and belonging to a local ethnic minority; all specifications also include controls for marital status and household size.

D.2 Predicting Participation in the Experiment

61 percent of subjects in our experiment are female. Though we do not have exact figures for the villages in our sample, this proportion is roughly consistent with the percentage of women in the rural village population.[3] This pattern is largely explained by the differential probability of (temporary) migration: many married men reside in cities and town, where more jobs are available, but leave their wives and children at home in the village. In our sample, 97.4 percent of married men live with their spouse, compared to only 77.6 percent of married women.[4] 70.3 percent of men in our sample (versus 19.4 percent of women) had moved to a town or city in search of work at some point in the past. Thus, the village is to some extent a selected sample, at least in the Western Kenyan context, but the proportion of women in our sample is not out of line with the proportion of women in the village population.

To examine whether the factors predicting participation in our experiment differ by gender, we regress the indicator for participation on a range of individual and household characteristics (Table 6). Our sample is restricted to subjects who were surveyed in their homes one day prior to the experiment (because we have survey data on both those who participated and those who did not for this population). In Columns 1 and 2, we include only the $Female_i$ dummy. In Columns 3 and 4, we include a broad range of characteristics. In Columns 5 and 6, we interact our individual and household characteristics with the $Female_i$ dummy to test for differential selection. Even-numbered columns absorb differences across villages through community-level fixed effects.

We find that women are significantly less likely to participate in the experiment (women are roughly 5 percentage points more likely to participate), but only when we do not include controls for individual characteristics. Once individual characteristics are included, we find that participation is positively and significantly related to household size and participation in transfer networks (having made a transfer in the last 3 months); participation is negatively associated with belonging to a (local) minority ethnic group. We do not find evidence of differential selection by gender.[5]

Table 6: OLS Regressions Predicting Participation in the Experiment

Dependent Variable:	PARTICIPATED IN EXPERIMENTAL SESSION					
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-0.057***	-0.052***	-0.032	-0.03	0.199	0.267
	(0.019)	(0.019)	(0.023)	(0.023)	(0.267)	(0.264)
Age	.	.	0.0007	0.0007	0.0008	0.001
			(0.0007)	(0.0007)	(0.001)	(0.001)
Completed primary school	.	.	0.031	0.032	0.045	0.053
			(0.021)	(0.021)	(0.034)	(0.034)
Completed secondary school	.	.	-0.027	-0.029	-0.075	-0.075
			(0.036)	(0.037)	(0.05)	(0.05)
Married	.	.	-0.015	-0.013	-0.022	-0.03

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[3]As part of another project in other rural villages in Kenya's Western Province, we conducted a census of all the adults living in each of two communities. We found that females accounted for 58 percent of adults in each of these villages. If we restrict attention to those who would be able to participate in the experiment (being neither blind, deaf, inebriated, or unfamiliar with Swahili), women account for 59 and 64 percent, respectively, of the capable adults residing in those two villages.

[4]There is some polygyny (9 men in our sample live with more than one spouse), which also contributes to the differential likelihood of living in a household that does not include your spouse.

[5]It is not possible to test for differential selection on unobservables. However, given the high overall participation rate and the lack of evidence of differential selection across a wide range of observable characteristics, differential selection on unobservables is unlikely to be a serious issue.

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Dependent Variable:	PARTICIPATED IN EXPERIMENTAL SESSION					
Specification:	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
			(0.023)	(0.023)	(0.042)	(0.043)
Household (HH) size	.	.	0.011***	0.011***	0.01**	0.011**
			(0.004)	(0.004)	(0.005)	(0.005)
No. of close kin in village	.	.	0.002	0.004	0.004	0.008
			(0.005)	(0.005)	(0.006)	(0.007)
No. of distant kin in village	.	.	-0.0005	-0.0005	-0.0005	-0.0007
			(0.0007)	(0.0007)	(0.001)	(0.001)
Close kin attended game	.	.	0.036	0.032	0.034	0.029
			(0.025)	(0.025)	(0.034)	(0.034)
Natural log of HH assets	.	.	-0.017	-0.017	-0.0001	-0.0008
			(0.013)	(0.013)	(0.019)	(0.019)
Any HH member employed	.	.	-0.03	-0.033	0.024	0.011
			(0.022)	(0.022)	(0.036)	(0.036)
Self-employed	.	.	-0.015	-0.018	0.0008	0.0009
			(0.021)	(0.021)	(0.036)	(0.036)
Has bank savings account	.	.	-0.035	-0.032	-0.057	-0.053
			(0.029)	(0.03)	(0.043)	(0.043)
Participates in ROSCA	.	.	0.024	0.02	0.019	0.008
			(0.023)	(0.023)	(0.037)	(0.037)
HH gave gift or loan in last 3 mos.	.	.	0.071**	0.08**	0.065	0.074
			(0.031)	(0.032)	(0.057)	(0.058)
HH received gift or loan in last 3 mos.	.	.	0.032*	0.031	0.04	0.046
			(0.019)	(0.019)	(0.031)	(0.031)
No. of community groups	.	.	0.005	0.004	-0.0006	0.002
			(0.006)	(0.006)	(0.01)	(0.01)
Ethnic minority	.	.	-0.063***	-0.054**	-0.06	-0.039
			(0.024)	(0.026)	(0.042)	(0.047)
Distance from village to paved road	.	.	0.014***	.	0.009***	.
			(0.002)		(0.003)	
Female × age	.	.	.	.	-0.0002	-0.0006
					(0.001)	(0.001)
Female × completed primary school	.	.	.	.	-0.026	-0.035
					(0.044)	(0.043)
Female × completed secondary school	.	.	.	.	0.095	0.089
					(0.072)	(0.073)
Female × married	.	.	.	.	0.017	0.026
					(0.052)	(0.052)
Female × household size	.	.	.	.	0.003	0.001
					(0.007)	(0.007)
Female × no. of close kin in village	.	.	.	.	-0.006	-0.008
					(0.01)	(0.01)
Female × no. of distant kin in village	.	.	.	.	0.0001	0.0002
					(0.001)	(0.001)
Female × close kin attended game	.	.	.	.	0.0004	-0.0002
					(0.051)	(0.05)
Female × natural log of HH assets	.	.	.	.	-0.029	-0.027
					(0.026)	(0.026)
Female × any HH member employed	.	.	.	.	-0.079*	-0.065
					(0.045)	(0.045)
Female × self-employed	.	.	.	.	-0.021	-0.024
					(0.044)	(0.044)
Female × has bank account	.	.	.	.	0.043	0.04
					(0.06)	(0.06)
Female × participates in ROSCA	.	.	.	.	0.007	0.018

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Dependent Variable:	Participated in Experimental Session					
Specification:	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
					(0.047)	(0.047)
Female × HH gave gift or loan in last 3 mos.	.	.	.	.	0.01	0.011
					(0.069)	(0.068)
Female × HH gave gift or loan in last 3 mos.	.	.	.	.	-0.012	-0.023
					(0.039)	(0.039)
Female × community groups	.	.	.	.	0.007	0.003
					(0.013)	(0.013)
Female × ethnic minority	.	.	.	.	0.002	-0.017
					(0.051)	(0.054)
Female × distance to road	.	.	.	.	0.008**	.
					(0.004)	
Constant	0.843***	0.839***	0.756***	0.826***	0.617***	0.639***
	(0.015)	(0.016)	(0.136)	(0.135)	(0.203)	(0.2)
Village FEs	No	Yes	No	Yes	No	Yes
Observations	1799	1799	1773	1773	1773	1773
R^2	0.004	0.053	0.055	0.074	0.061	0.077

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported, logit and probit results are similar. Sample restricted to individual surveyed in their homes one day prior to the experiment.

D.3 Do Other Characteristics Explain the Differential Gender Impacts?

Even in the absence of differential selection into the experiment, gender may be associated with other household and individual characteristics. As discussed above, many women in the village live in households that do not include a male household head (who is physically present), so women's living conditions may be systematically different from those of men (even when they are married). Women also differ from men in other ways — for example, they have lower educational attainment and are less likely to work for a wage. Of course, it is not possible to fully separate gender differences from the impacts of factors that differ systematically across genders (and have little overlap in support) — this is not, and should not be, our goal. For example, in our sample, we cannot distinguish gender differences from the impacts of living in an exogamous society where members of one sex are expected to leave their own blood relatives and reside among their spouse's kin after marriage; such systematic differences in the social and economic lives of men and women are part of the explanation for gender differences, not alternative hypotheses.

Instead, we seek to test whether gender may simply be proxying for another characteristic that better explains the observed impact heterogeneity. A better explanation, in this context, should satisfy several criteria: its inclusion as a control should change the coefficient on the gender difference in treatment effects; it should explain treatment effect heterogeneity within gender as well as across genders; and it might have a higher explanatory power on its own than gender has. Philosophically and econometrically, characteristics that are perfectly correlated with gender in this sample are not separable from gender, so they would be equivalent explanations, in our view.

To identify potential factors that might explain observed gender differences, we first identify those factors that are associated with gender (being female) — gender differences in behavior within the experiment cannot be explained by factors that are not strongly associated with gender. After identifying a set of candidate factors that are associated with gender, we test whether said traits predict treatment effect heterogeneity and dampen our observed gender differences in treatment effects. We then ask whether these factors operate in similar ways within gender, and whether these factors represent better stand-alone explanations for treatment effect heterogeneity than gender does.

To identify factors that are associated with gender, we regress the $Female_i$ indicator on a broad set of individual characteristics: age, education level, marital status, household size, the numbers of close and more distant relatives present in the village, the natural log of the value of household assets, the number of community groups an individual participates in, and indicators for being self-employed, having an employed household member, having a bank account, participating in a ROSCA, having made or received an interhousehold transfer in the last three months, and belonging to a local minority ethnic group. Results are reported in Table 7.

Among our subjects, being female is negatively and significantly associated with age, the likelihood of having completed primary school, the number of close kin and distant kin residing in the village, the log of household assets, the likelihood of having a bank account, the probability that one's household made a transfer in the last three months, and the number of community groups one participates in; being female is positively associated with participating in a ROSCA and belonging to a local minority ethnic group. Results with and without village fixed effects are nearly identical. Results (not shown) are also nearly identical (patterns of sign and significance are unchanged) if we omit those subjects who were present at the same experimental session as their spouse.

Next, we explore the possibility that some factor other than gender explains the observed gender differences in behavior within the experiment. We begin by testing the robustness of observed gender differences to the inclusion of controls for other interactions between observable characteristics and treatment status. We focus on our most robust result: the impact of assignment to the public and price large endowment treatments on the probability of investment no more than 80 shillings. In Table 8, we estimate OLS regressions in which $LTE80_i$, the indicator for investing no more than 80 shillings, is the dependent variable. In Column 1, we replicate Column 2 of (Online Appendix) Table 4, including only the $PublicTreatments_i$ indicator, the $Female_i$ dummy, and an interaction between the two. As discussed above, the interaction term is marginally significant (p-value 0.066) and the sum of the coefficient on $PublicTreatments_i$ and the interaction term is highly significant (p-value 0.019). In Column 2, we add village fixed effects plus, for completeness, every term from (Online Appendix) Table 6 that was not significantly associated with gender (household size plus indicators for completing secondary school, being married, having an employed household member, being self-employed, and having received a gift or loan in the last 3 months) and interactions between those terms and the $PublicTreatments_i$ indicator. The $Female_i \times PublicTreatments_i$ interaction term remains significant, and the magnitude of the coefficient is (almost) unchanged. In Columns 3 through 12, we add each individual control that is associated with gender (in Online Appendix Table 6) plus an interaction between each term and the $PublicTreatments_i$ indicator. Coefficient magnitudes (on the gender-treatment interaction) are similar across all specifications, and the interaction term remains statistically significant in 7 of the 10 specifications. In the final column, we include all the controls simultaneously. Though the $Female_i \times PublicTreatments_i$ term is insignificant in this last column, it is not because the coefficient changed; it is because the test has become under-powered, as the standard error is larger.

Thus, none of the factors under consideration substantially alters the coefficient of interest (on $Female_i \times PublicTreatments_i$). For the three factors that did bring about its statistical insignificance (age, ethnic minority status, and the number of close kin in the village), we conduct an additional test: we ask whether a regression of $LTE80_i$ on $PublicTreatments_i$, a covariate, and that covariate interacted with $PublicTreatments_i$ produces a higher R^2 than does the same regression with gender as the covariate. In other words, we estimate the OLS regression

$$LTE80_i = \alpha + \beta PublicTreatments_i + \gamma Z_i + \delta Z_i \times PublicTreatments_i + \epsilon_i$$

and compare the R^2 to that of the specification reported in (Online Appendix) Table 4. None of the three characteristics in question (age, ethnic minority status, and the number of close kin in the village) has greater explanatory power than gender itself. We take this as further evidence that it is highly unlikely that gender is simply proxying for another observable characteristic.

As a final step, we probe each of these three variables in turn. Looking first at ethnic minority status, we note that women are more likely to be members of local ethnic minorities (in part because of the traditions of patrilocal exogamy described above), but the estimated coefficient on the minority-treatment interaction is negative (both in Column 12 of Table 7 and when other controls and interactions are omitted). Thus, minority status cannot explain observed gender differences because gender predicts larger treatment effects of observability while minority status is associated with dampened treatment effects, though the two variables (gender and minority status) are positively correlated.

We now turn to the two remaining variables that, while unable to completely explain observed gender differences in the previous tests, may partially explain the observed gender differences: age and the number of close relatives residing in the village. For both variables, we observe considerable within-gender variation and overlap an in support across genders. We would therefore expect that, if gender were merely proxying for age or the number of close kin in the village, we would find similar treatment effect heterogeneity within genders.

Gender (being female) is negatively related to the number of close kin in the village: women in our sample have an average of 1.5 close relatives in the village, versus 3.5 for men. Thus, if gender differences were actually caused by variation in the number of close kin in the village, we would expect the interaction between the number of relatives in the village and $PublicTreatments_i$ to be negative in both the pooled sample and within each gender. As shown in (Online Appendix) Table 9, that is what we find in the pooled sample, when we do not control for gender, and that is what we find among men. However, among women, the effect goes in the opposite direction. The equality of the coefficients on the interaction term in the male and female subsamples can be rejected statistically. This is not to say that close kin do not play a role: for women, they do appear to intensify the effect of the public treatment, as we also find elsewhere in the paper (though statistically insignificantly in this particular specification). However, we can reject the hypothesis that gender is merely proxying for an effect of the number of close relatives in the village that does not, itself, depend on gender.

As for age, we see in Table 9 that the treatment effect varies in the same direction with age for both men and women. However, this does not explain away the variation associated with gender: the effect of the public treatment is strongest for young women, but is statistically significant at all ages under 40, for example; for men, the effect is always smaller, and is never statistically significant, at all ages under 40. Moreover, though women in our sample are, on average, younger than men, the difference in mean ages is quite small (the average age for men is 37.9 years vs. 35.5 for women). Point estimates in (Online Appendix) Table 4 suggest that women are approximately 12 percentage points more likely to invest no more than 80 shillings in the public treatments, as compared to men. Given the magnitude of the point estimates reported in Table 9, women would need to be roughly 24 years younger than men, on average, for age to explain the observed gender differences. So, while the variation in treatment effect with age is an interesting pattern, it does not explain away the gender differences in treatment effects.

Table 7: Associations between Gender and Individual and Household Characteristics

Dependent Variable:	Subject Is Female	
Specification:	OLS	OLS
	(1)	(2)
Age	-0.007***	-0.008***
	(0.0006)	(0.0007)
Completed primary school	-0.148***	-0.147***
	(0.021)	(0.021)
Completed secondary school	-0.036	-0.032
	(0.032)	(0.032)
Married	-0.012	-0.008
	(0.022)	(0.022)
Household (HH) size	-0.004	-0.004
	(0.004)	(0.004)
No. of close kin in village	-0.072***	-0.071***
	(0.004)	(0.004)
No. of distant kin in village	-0.002***	-0.002***
	(0.0007)	(0.0007)
Natural log of HH assets	-0.038***	-0.039***
	(0.013)	(0.013)
Any HH member employed	0.007	0.0008
	(0.021)	(0.022)
Self-employed	0.004	0.002
	(0.019)	(0.019)
Has bank savings account	-0.099***	-0.095***
	(0.026)	(0.027)
Participates in ROSCA	0.246***	0.239***
	(0.021)	(0.022)
HH gave gift or loan in last 3 mos.	-0.067**	-0.071**
	(0.029)	(0.029)
HH received gift or loan in last 3 mos.	0.03*	0.025
	(0.018)	(0.018)
No. of community groups	-0.026***	-0.025***
	(0.006)	(0.006)
Ethnic minority	0.036*	0.044**
	(0.021)	(0.022)
Constant	1.547***	1.573***
	(0.128)	(0.129)
Village FEs	No	Yes
Observations	2124	2124
R^2	0.325	0.339

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported, logit and probit results are similar.

Table 8: Do Other Observable Characteristics Explain Gender Differences in Treatment Effects?

Dependent Variable:	Invested 80 Shillings or Less												
Specification:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Public treatments	-0.025	0.04	0.204	0.018	0.061	0.033	-0.544	0.003	0.027	-0.087	0.03	0.044	-0.182
	(0.052)	(0.117)	(0.142)	(0.123)	(0.126)	(0.121)	(0.453)	(0.12)	(0.118)	(0.141)	(0.12)	(0.117)	(0.491)
Female	-0.117**	-0.107**	-0.096*	-0.117**	-0.121**	-0.111**	-0.113**	-0.136**	-0.112**	-0.113**	-0.106*	-0.103*	-0.148**
	(0.054)	(0.054)	(0.055)	(0.055)	(0.057)	(0.055)	(0.055)	(0.056)	(0.055)	(0.054)	(0.054)	(0.054)	(0.062)
Female × public treatments	0.121*	0.114*	0.109	0.122*	0.099	0.117*	0.127*	0.149**	0.116*	0.121*	0.116*	0.108	0.119
	(0.066)	(0.067)	(0.068)	(0.068)	(0.072)	(0.068)	(0.068)	(0.068)	(0.067)	(0.066)	(0.067)	(0.066)	(0.078)
Age	.	.	0.003	.	.	.	.	.	.	.	.	.	0.004*
			(0.002)										(0.002)
Public × age	.	.	-0.004	.	.	.	.	.	.	.	.	.	-0.006**
			(0.002)										(0.003)
Completed primary school	.	.	.	-0.066	.	.	.	.	.	.	.	.	-0.029
				(0.06)									(0.062)
Public × completed primary school	.	.	.	0.046	.	.	.	.	.	.	.	.	-0.012
				(0.074)									(0.076)
No. of close kin in village	.	.	.	.	-0.008	.	.	.	.	.	.	.	0.0009
					(0.011)								(0.012)
Public × no. of close kin in village	.	.	.	.	-0.006	.	.	.	.	.	.	.	-0.018
					(0.014)								(0.015)
No. of distant kin in village	.	.	.	.	.	-0.001	.	.	.	.	.	.	-0.001
						(0.002)							(0.002)
Public × no. of close distant in village	.	.	.	.	.	0.0007	.	.	.	.	.	.	0.001
						(0.003)							(0.003)
Natural log of HH assets	.	.	.	.	.	.	-0.025	.	.	.	.	.	-0.009
							(0.038)						(0.04)
Public × log of HH assets	.	.	.	.	.	.	0.061	.	.	.	.	.	0.041
							(0.046)						(0.047)
Has bank savings account	.	.	.	.	.	.	.	-0.122*	.	.	.	.	-0.164**
								(0.07)					(0.076)
Public × has bank savings account	.	.	.	.	.	.	.	0.179**	.	.	.	.	0.23**
								(0.09)					(0.096)
Participates in ROSCA	.	.	.	.	.	.	.	.	0.026	.	.	.	0.045
									(0.055)				(0.067)
Public × participates in ROSCA	.	.	.	.	.	.	.	.	0.009	.	.	.	-0.004
									(0.068)				(0.083)
HH gave gift or loan in last 3 mos.	.	.	.	.	.	.	.	.	.	-0.137*	.	.	-0.081
										(0.083)			(0.087)
Public × HH gave gift or loan	.	.	.	.	.	.	.	.	.	0.164	.	.	0.081
										(0.104)			(0.108)
No. of community groups	.	.	.	.	.	.	.	.	.	.	0.001	.	0.00009
											(0.015)		(0.019)
Public × community groups	.	.	.	.	.	.	.	.	.	.	0.005	.	0.0009
											(0.018)		(0.022)
Ethnic minority	.	.	.	.	.	.	.	.	.	.	.	0.139**	0.132*
												(0.069)	(0.071)
Public × ethnic minority	.	.	.	.	.	.	.	.	.	.	.	-0.022	-0.013
												(0.083)	(0.084)
Village FEs	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1061	1061	1050	1061	1061	1061	1061	1059	1059	1061	1061	1061	1048
R^2	0.007	0.059	0.062	0.061	0.062	0.06	0.062	0.063	0.06	0.062	0.06	0.067	0.082

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported; logit results are similar. Sample restricted to subjects receiving the larger endowment. A constant is included in all specifications. Columns 2 through 13 include controls for all variables that are balanced across genders (see Online Appendix Table 7): household size and indicators for being married, being self-employed, having an employed household member, and having received a transfer in the last three months, and interactions between these control variables and the $PublicTreatments_i$ indicator.

Table 9: Treatment Effect Heterogeneity: Kin in Village

Dependent Variable:	Invested 80 Shillings or Less		
Sample:	Both Genders	Women	Men
Specification:	OLS	OLS	OLS
	(1)	(2)	(3)
Public treatments	0.079^{*}	0.058	0.088
	(0.044)	(0.053)	(0.082)
No. of close kin in village	0.002	-0.005	-0.005
	(0.01)	(0.016)	(0.014)
Public × no. of close kin in village	-0.012	0.026	-0.03^{*}
	(0.013)	(0.022)	(0.018)
Village FEs	Yes	Yes	Yes
Additional Controls	No	No	No
Observations	1061	644	417
R^2	0.044	0.061	0.126

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported; logit results are similar. Sample restricted to subjects receiving the larger endowment. A constant is included in all specifications.

Table 10: Treatment Effect Heterogeneity: Age

Dependent Variable:	Invested 80 Shillings or Less		
Sample:	Both Genders	Women	Men
Specification:	OLS	OLS	OLS
	(1)	(2)	(3)
Public treatments	0.229^{**}	0.361^{***}	0.039
	(0.09)	(0.122)	(0.143)
Age	0.004^{*}	0.005	0.002
	(0.002)	(0.003)	(0.003)
Public × age	-0.005^{**}	-0.007^{**}	-0.002
	(0.002)	(0.003)	(0.004)
Village FEs	Yes	Yes	Yes
Additional Controls	No	No	No
Observations	1050	633	417
R^2	0.046	0.067	0.104

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported; logit results are similar. Sample restricted to subjects receiving the larger endowment. A constant is included in all specifications.

E. The Endogeneity of Subject Characteristics: Kin Presence

We next explore the association between individual characteristics and the likelihood that one's close relatives attended the experiment. In Table 11, we report OLS regressions of the indicator for kin presence at the experiment on a range of individual and household characteristics. After controlling for village fixed effects, the only factor strongly associated with kin presence at the game in the sample of women is the number of close kin living in the village. Being married, household size, household asset holdings, and having an employed household member are weakly associated with kin presence at the game.

In Table 12, we test the robustness of the impact heterogeneity in terms of kin presence at the experiment that we document in the paper. In Columns 1 and 4, we replicate Columns 1 and 4 from Table 4 in the paper. In the remaining columns, we test the robustness of our main results by including controls for having one's spouse at the game and having an above median number of distant kin in the village; we interact each of these controls with the dummy for random assignment to the public or price treatment. In all cases, we find that none of the other variables alone can explain the observed heterogeneity in treatment effects. (Since having close kin in the village and having many distant kin in the village are very strongly correlated with kin presence at the experiment, it is sometimes necessary to look at their combined effect when added to the kin presence variable rather than the marginal impact of kin presence at the game.)

Table 11: Predicting the Presence of Close Relatives at Experiment

Dependent Variable:	Any Close Relatives Attended the Experiment					
Sample Restriction:	All Subjects		Women Only		Men Only	
Specification:	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.0009	0.001*	-0.00003	-0.00003	0.002*	0.002**
	(0.0006)	(0.0006)	(0.0008)	(0.0008)	(0.001)	(0.001)
Completed primary school	0.042**	0.042**	-0.007	-0.005	0.106***	0.103***
	(0.019)	(0.019)	(0.021)	(0.021)	(0.035)	(0.035)
Completed secondary school	-0.058*	-0.052*	-0.004	0.001	-0.098**	-0.084*
	(0.03)	(0.029)	(0.036)	(0.037)	(0.045)	(0.044)
Married	0.049***	0.046**	0.043**	0.038*	0.057	0.038
	(0.018)	(0.018)	(0.019)	(0.02)	(0.042)	(0.044)
Household (HH) size	-0.005	-0.005	-0.005	-0.006*	-0.005	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)	(0.006)
No. of close kin in village	0.054***	0.055***	0.056***	0.056***	0.052***	0.056***
	(0.004)	(0.004)	(0.007)	(0.007)	(0.006)	(0.006)
No. of distant kin in village	0.0001	1.00e-05	0.0005	0.0006	0.00002	-0.0006
	(0.0007)	(0.0007)	(0.001)	(0.001)	(0.001)	(0.001)
Natural log of HH assets	-0.029**	-0.027**	-0.021*	-0.018	-0.04*	-0.042*
	(0.011)	(0.011)	(0.011)	(0.011)	(0.022)	(0.022)
Any HH member employed	-0.005	-0.004	-0.035*	-0.036*	0.034	0.05
	(0.019)	(0.019)	(0.02)	(0.019)	(0.038)	(0.038)
Self-employed	0.009	0.008	0.008	0.007	0.008	0.023
	(0.017)	(0.017)	(0.019)	(0.019)	(0.034)	(0.034)
Has bank savings account	-0.011	-0.01	0.033	0.032	-0.043	-0.033
	(0.024)	(0.024)	(0.029)	(0.03)	(0.037)	(0.038)
Participates in ROSCA	0.007	0.009	0.006	0.008	0.003	0.0007
	(0.02)	(0.02)	(0.022)	(0.023)	(0.037)	(0.037)
HH gave gift or loan in last 3 mos.	0.028	0.029	0.03	0.041	0.032	0.019
	(0.024)	(0.025)	(0.025)	(0.026)	(0.057)	(0.056)
HH received gift or loan in last 3 mos.	0.004	0.009	0.012	0.013	-0.008	0.0003
	(0.016)	(0.016)	(0.018)	(0.018)	(0.031)	(0.031)
No. of community groups	-0.007	-0.006	-0.01	-0.01	-0.002	-0.002
	(0.005)	(0.006)	(0.006)	(0.006)	(0.01)	(0.01)
Ethnic minority	0.006	0.016	0.011	0.017	-0.003	0.005
	(0.02)	(0.021)	(0.021)	(0.023)	(0.041)	(0.047)
Female	-0.083***	-0.074***	.	.	.	.
	(0.02)	(0.021)				
Constant	0.34***	0.299**	0.229**	0.19	0.381*	0.384*
	(0.117)	(0.117)	(0.114)	(0.117)	(0.229)	(0.227)
Village FEs	No	Yes	No	Yes	No	Yes
Observations	2125	2125	1281	1281	844	844
R^2	0.161	0.18	0.111	0.132	0.12	0.173

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported; logit results are similar. Sample restricted to subjects receiving the larger endowment. A constant is included in all specifications.

Table 12: Treatment Effect Heterogeneity: Potential Confounds of Kin Presence

Dependent Variable:	Invested 80 Shillings or Less			Invested Exactly 80 Shillings		
Specification:	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Close kin attended game	-0.245***	-0.243***	-0.24***	-0.093	-0.093	-0.09
	(0.09)	(0.09)	(0.091)	(0.073)	(0.073)	(0.074)
Close kin at game × public	0.418***	0.407***	0.376***	0.152	0.16*	0.131
	(0.109)	(0.112)	(0.121)	(0.093)	(0.095)	(0.104)
No close kin at game × public	0.069	0.062	0.036	0.058	0.063	0.042
	(0.045)	(0.047)	(0.058)	(0.038)	(0.039)	(0.048)
Spouse at game	.	-0.042	.	.	0.018	.
		(0.117)			(0.097)	
Spouse at game × public	.	0.084	.	.	-0.063	.
		(0.149)			(0.119)	
Above median number of distant relatives in village	.	.	0.016	.	.	0.007
			(0.075)			(0.063)
Above median distant relatives × public	.	.	0.073	.	.	0.036
			(0.084)			(0.071)
Constant	0.054	0.061	0.065	-0.037	-0.043	-0.032
	(0.284)	(0.284)	(0.285)	(0.253)	(0.254)	(0.254)
Village FEs	Yes	Yes	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	642	642	642	642	642	642
R^2	0.117	0.118	0.121	0.089	0.089	0.09

Robust standard errors in parentheses. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. OLS specifications reported; logit results are similar. Sample restricted to women receiving the larger endowment. A constant is included in all specifications. All specifications include controls for all variables that are not balanced across genders (see Online Appendix Table 7): age, education level, the log value of household assets, the number of close relatives residing in the village, the number of distant relatives residing in the village, involvement in community groups, and indicators for having a bank savings account, participating in a ROSCA, having given a gift or loan to another household in the last 3 months, and belonging to a local ethnic minority; all specifications also include controls for marital status and household size.

F. Additional Figures

Figure 1: Histograms of Investment in the Business Cup by Treatment

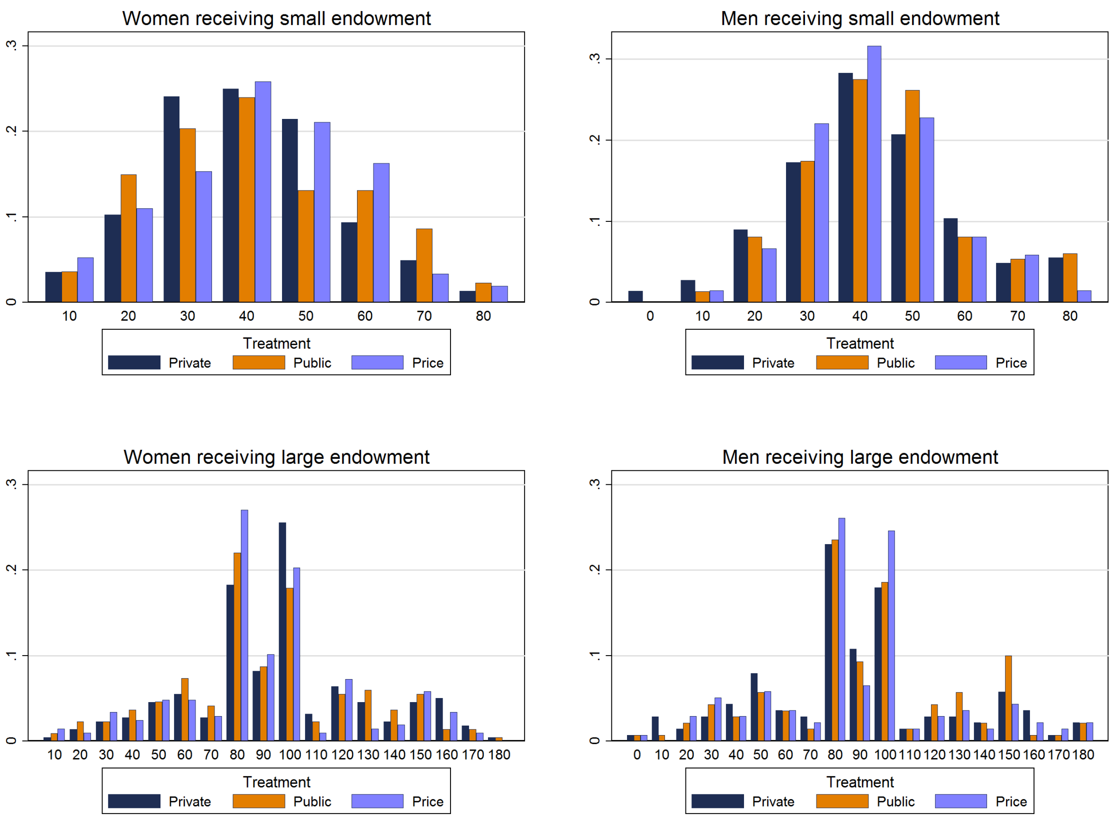


Figure 2: Histograms of Simulated Investments in the Business Cup by Treatment

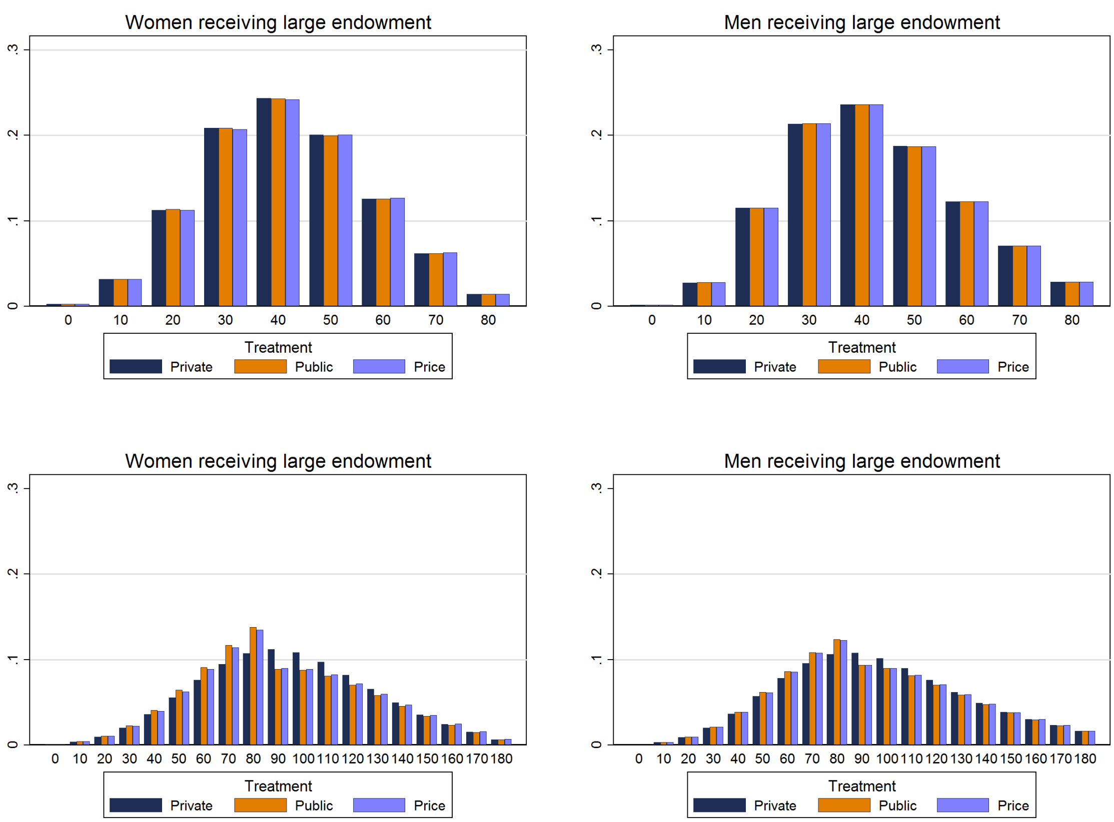
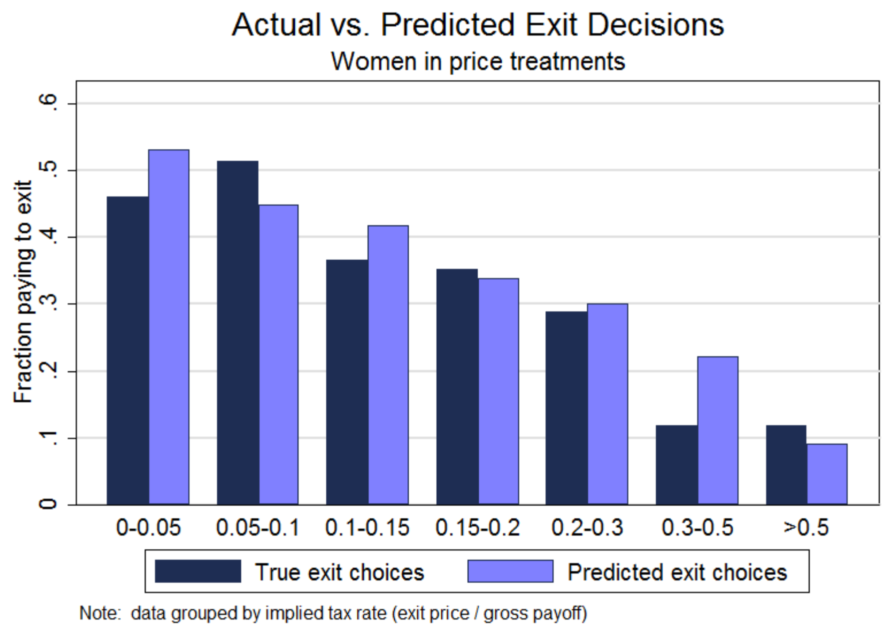
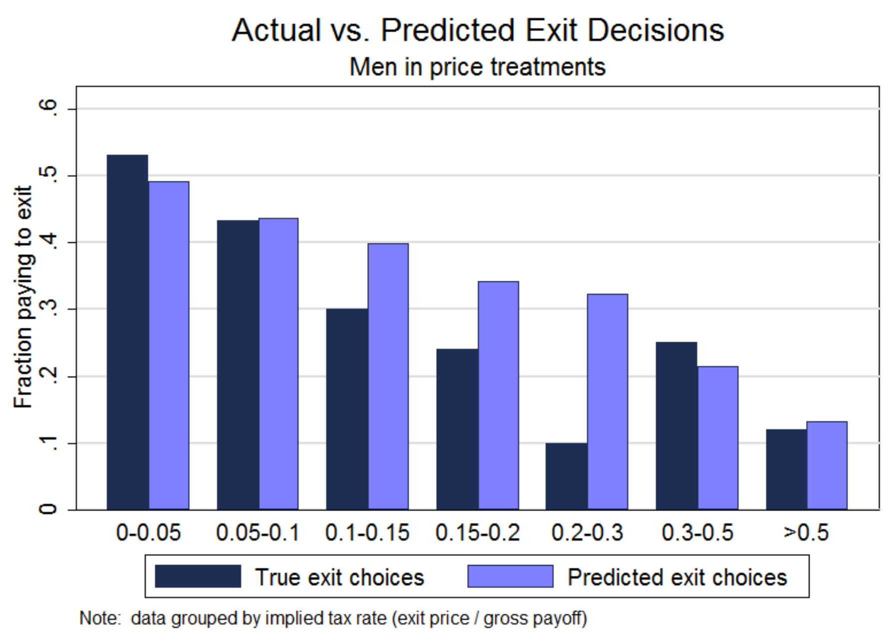


Figure 3: Actual vs. Predicted Exit Decisions





G. CRRA Parameter Estimation Sensitivity Analysis

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 $EV_{ik} < EV_{ij}$.[6] 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:

$$EU_{ij} = \underbrace{\frac{1}{2\eta_i}(m_i - b_j)^{1-\rho_i} + \frac{1}{2\eta_i}(m_i + 4b_j)^{1-\rho_i}}_{EV_{ij}} + \varepsilon_{ij}, \tag{24}$$

while η_i would be replaced with $(1 - \rho_i)$ if we instead used the scaling in Equation (19). When the conventional scaling is used, as in Equation (19), 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 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 13. We report the parameter estimates using UR scaling in Column 1, parameters estimated using the utility function defined in Equation (19) 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 24. 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.

[6] 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.

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 13).[7] Estimated levels of risk aversion are higher than those typically reported for undergraduate subjects (cf. Holt and Laury 2002, Goeree, Holt, and Palfrey 2003) but in line with those reported in Cardenas and Carpenter (2008) who survey the experimental literature measuring risk preferences in developing countries. 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 13, Column 2).

Table 13: Comparing Estimated Distributions of CRRA Parameters

SCALING:	UR (1)	$1-\rho$ (2)	CE (3)	CU (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 $400^{1-\rho} - 10^{1-\rho}$.

G.1 An alternative distributional assumption

Though the analysis in the manuscript assumes normally distributed risk preferences in the population, the results do not hinge on this assumption. As a robustness check, here we replicate Table 9 from the paper, but instead of a normal distribution, we use a symmetric triangular distribution, with parameters μ and ω such that:

$$f_\rho(\rho) = \begin{cases} \frac{\rho+\omega-\mu}{\omega^2} & \text{if } \mu-\omega \leq \rho < \mu \\ \frac{\omega+\mu-\rho}{\omega^2} & \text{if } \mu \leq \rho \leq \mu+\omega \\ 0 & \text{otherwise} \end{cases}$$

[7] 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.

Table 14: Parameter Estimates using Triangular Distribution of Risk Preferences

	(1)	(2)	(3)
Panel A: Women in All Treatments			
μ_ρ	0.752***	0.751***	0.753***
	(0.011)	(0.011)	(0.011)
ω_ρ	0.203***	0.202***	0.204***
	(0.011)	(0.011)	(0.011)
σ_ϵ	0.012***	0.012***	0.012***
	(0.001)	(0.001)	(0.001)
τ	0.043***	0.044***	0.040***
	(0.013)	(0.011)	(0.010)
γ		0.059***	0.058***
		(0.009)	(0.009)
κ			0.003
			(0.005)
Panel B: Men in All Treatments			
μ_ρ	0.754***	0.752***	0.753***
	(0.013)	(0.013)	(0.014)
ω_ρ	0.241***	0.240***	0.241***
	(0.011)	(0.012)	(0.012)
σ_ϵ	0.010***	0.011***	0.011***
	(0.001)	(0.001)	(0.001)
τ	0.027***	0.026*	0.025
	(0.006)	(0.013)	(0.015)
γ		0.061***	0.090***
		(0.012)	(0.033)
κ			-0.031
			(0.029)

Standard errors in parentheses.

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