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Kinship Taxation as an Impediment to Growth: Experimental Evidence from Kenyan Microenterprises

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Abstract

This paper documents strong pressure on productive entrepreneurs in a developing country setting to share their income. This 'kinship tax' can distort productive decisions, including investment. I conduct a lab experiment with a sample of 1805 Kenyans to quantify the importance of this tax. In my sample, one in three men men and one in five women face distortionary pressure to share income. Strikingly, this share is strongly increasing in ability, suggesting potentially large aggregate production consequences. Male entrepreneurs who receive cash grants expand their business only if they do not face distortionary kinship taxation as measured in the lab.

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Introduction

Given limited access to financial markets, people living near subsistence often engage in a remarkable amount of resource sharing. Economists such as Townsend (1994) have written extensively on the insurance benefits of such communal arrangements. However, this empirical literature largely abstracts from potential implications for incentives.[1] This stands in contrast to early development economists, such as Lewis (1955), who worried that sharing of resources among kin could act as an informal tax and dilute productive incentives. Anthropologists and sociologists have similarly identified this as a key characteristic of poverty, and a barrier to accumulation and development.[2] This paper shows that this distortion is greater for individuals with higher productive ability, and estimates its effect on the investment decisions of entrepreneurs in rural Kenya.

Using a sample of 1800 Kenyans in 17 villages, I propose and implement a lab experiment, building on Jakiela and Ozier (2016), to elicit from each participant the extent to which they face pressure to share income. Since this acts as an informal analogue to a tax, I refer to it as a "kinship tax", and show that my measure identifies which participants would choose to distort productive activity in response to this pressure to share income. I find that kinship taxation disproportionately affects high-ability participants, measured using schooling, cognitive ability and entrepreneurship. I also find kinship taxation affects men more than women, which is consistent with gender norms in this setting where men are primarily responsible with inter-household transfers.

To test what effect kinship taxation has on productive decisions, I use variation from a pre-existing experiment which randomized receipt of large cash transfers. Strikingly, I find that it is only male entrepreneurs unaffected by kinship taxation who use these cash grants to invest in their firms. Entrepreneurs who face kinship taxation (that is, are willing to pay to hide income) do not invest any of the cash into their firm, despite having higher ability and seeming to have higher returns to capital.

Conversations with entrepreneurs support the idea that these distortions are important. As a 40 year old entrepreneur from a slum near Nairobi explained:

> "I sell second-hand clothes without anyone knowing, far from home. I hide from my friends because I believe not all friends will be happy with my success, and from family to create a picture that I have no money, for them to work hard for

[1] This is especially relevant in light of evidence that these transfers are in large part involuntary (Comola and Fafchamps, 2014; Kinnan, 2021; De Weerdt et al., 2019).

[2] Stack (1974); Hydén (1980); Harvey (1993). This idea goes back to Max Weber, who felt that strong economic links within kin groups ("fetters of the sib") discouraged entrepreneurial activity (Weber, 1915).

their own money. My previous business, a street-side restaurant, failed due to my in-laws using me for money, yet I wanted to expand it."

A key feature of kinship taxation is that transfers to relatives may directly contribute to utility, for example because of altruism or a desire for status. In Section 1, I provide an explicit formalization of marginal kinship tax rates, defined as being net of this direct utility from transfers. An undistorted entrepreneur either faces no demands on additional income, or is indifferent between increasing private consumption and making additional transfers. This implies that data on inter-household transfers alone cannot identify the extent or distribution of distortions induced by these transfers, since these data cannot distinguish 'taxation' from gifts. Two individuals with the same income and who face the same requests may differ in the extent to which they value the warm glow or status they derive from these transfers, and hence whether they choose to reduce their productive effort in response. The correct measure must take into account preferences over these transfers.

This approach abstracts from the specific forces that determine inter-household transfers. This is in contrast to the bulk of the related theoretical literature, which focuses on modelling self-enforcing risk sharing arrangements, usually in the presence of limited commitment or hidden information.[3] Instead, I allow for arbitrary equilibrium distributions of transfer obligations, and of motivations for transfers, and focus on the sufficient statistic for an individual's productive decisions: their marginal kinship tax rate. I show that this can be measured using willingness-to-pay to hide income. If an entrepreneur is compelled to make transfers, they will be willing to take costly actions to hide additional income. This allows me to measure marginal kinship tax rates in a lab-in-the-field experiment.

Section 2 describes the experiment I use to elicit willingness-to-pay (WTP) to hide income from friends and family. Participants are asked to choose whether they prefer \$5 'in public' (announced to members of their community) or a smaller amount of money 'in secret.' I elicit the lowest amount they are willing to accept in secret, which gives me their marginal kinship tax rate. Income in this experiment is framed as payment for an effort task to elicit tax rates on earned, rather than windfall, income.

Section 3 presents an anatomy of kinship taxation, describing its correlates for a sample of 1805 rural Kenyans. I find substantial variation in marginal tax rates. One fifth of women in my sample and a third of men are willing to pay to hide income, i.e., face positive marginal kinship tax rates. For these 'taxed' individuals, the mean marginal tax rate is above 50%. What distinguishes them from their fellow community members?

[3]See Kocherlakota (1996); Ligon et al. (2002); Ambrus et al. (2014); Bourlès et al. (2017); De Weerdt et al. (2019).

Most notably, marginal kinship tax rates are increasing in ability. They are higher for those with more schooling, higher cognitive ability, and among entrepreneurs. This suggests that the aggregate efficiency cost of these taxes may be disproportionately high. Comparatively, the effect of family and household characteristics is less clear. Marginal rates are higher for those with more siblings, and with more children living in the household. But they do not vary by marital status, and are higher for men than for women, which suggests variation in tax rates is not driven by household bargaining power.

To estimate the effect of kinship taxation on productive activity, Section 4 focuses on the entrepreneurs in my sample, and uses data from their firms to show how kinship taxation constrains firm growth. To do so I exploit a separate cash transfer experiment conducted with the participants in my sample. These cash grants were large, equivalent to 13 months of median firm value-added. Male-owned microenterprises who receive this cash transfer almost double their capital stock, but only if they do not face kinship taxation.[4] Entrepreneurs who face positive kinship tax rates do not invest their cash transfers in firm capital.

Related literature

The primary contribution of this paper is to the empirical literature on informal transfers between members of cohesive communities, which can provide insurance for households with unpredictable income streams. While villages as a whole typically depart from perfect insurance (Morduch, 1991; Townsend, 1994; Kurosaki and Fafchamps, 2002), more closely connected groups seem to achieve something close to it. This has been documented within kinship networks (Ferrara, 2003; Fafchamps and Lund, 2003; Cox and Fafchamps, 2008; Witoelar, 2013), as well as ethnic groups and castes (Udry, 1994; Munshi and Rosenzweig, 2016). Recent evidence shows that in addition to consumption-smoothing, these transfers can play a role in the provision of public goods (Miguel and Gugerty, 2005; Olken and Singhal, 2011; Walker, 2017) and investments in private (Angelucci et al., 2017; Karaivanov et al., 2014) and human capital (Angelucci et al., 2010; Baland et al., 2016).

What is less understood is the effect of these transfers on productive incentives. Do people in these communities shirk or otherwise distort their economic activity because of the transfers they expect to make? My contribution to this literature is to quantify how this system of transfers affects productive incentives, and its effect on investment. I do this by shifting the focus away from measuring transfers, and instead directly elicit the effect of kinship taxation on individual incentives–the marginal kinship tax rate.

This analysis is limited in two important ways. The first is that it measures the incentive

[4]Consistent with evidence summarized in Bernhardt et al. (2019), only male-owned firms grow as a result of these cash transfers.

cost of kinship taxation insofar as it directly affects productive incentives at the margin, but does not also measure what effect obligations to share income have on welfare or even productivity.[5] Regardless of the net welfare effects of kinship taxation, understanding how it distorts productive decisions is a first-order question in understanding how to increase efficiency in developing countries. The second limitation is that, given my data, the analysis is static. I do not observe the firms in my sample over many periods, and therefore cannot study dynamic effects of kinship taxation on firm outcomes. I also do not consider the extensive margin decision of whether to operate a business.[6]

This paper joins an emerging literature focused on the distortionary effects of sharing obligations among the poor, most notably Jakiela and Ozier (2016). There is evidence this pressure to share income can reduce effort (Carranza et al., 2021), savings (Boltz et al., 2019; Goldberg, 2017), investment (Riley, 2020; Fiala, 2018; Grimm et al., 2016; Di Falco and Bulte, 2011), human capital (Wantchekon et al., 2014), and lead people to take costly and unnecessary loans (Baland et al., 2011).[7]

My first contribution to this literature is to identify the marginal kinship tax rate as the key object of interest in understanding potential productive distortions. This idea and the lab experiment I use to elicit this marginal kinship tax rate build on Jakiela and Ozier (2016), who offer investment decisions in the lab to participants who can reduce expected earnings to reduce their visibility. The second innovation is to quantify how kinship taxation affects the investment decisions of entrepreneurs.

A partly overlapping literature considers the decision to hide resources to understand relationships in social networks. Jakiela and Ozier (2016), Boltz et al. (2019) and Beekman et al. (2015) elicit as in this paper the decision to hide income from a broader community, while Ashraf (2009), Mani (2020), Castilla and Walker (2013), Castilla (2019) and Fiala (2018) focus on spouses hiding from each other. This literature is motivated in part by the finding that income received privately is spent differently than if it is public (Castilla and Walker, 2013; Boltz et al., 2019; Goldberg, 2017), suggesting a preference for keeping income hidden.

The only other paper in this literature that uses results from the lab to study real-world productive decisions is Fiala (2018). He uses the choice of paying to hide income from one's

[5]The insurance this provides is an obvious gain for welfare, but it may also in some settings encourage certain types of risky investment, as in Angelucci et al. (2017).

[6]If kinship taxation also distorts the decision to become a business owner, or the type of business to operate, this could add to its efficiency cost.

[7]This idea has entered the economics literature mostly through the work of Platteau (2000), who introduced the idea that kinship taxation, especially in Africa, is an important determinant of productive behaviour. Some theorists have also modelled the origins and consequences of this phenomenon (Hoff and Sen, 2006; Alger and Weibull, 2010).

spouse to study heterogeneous effects on investment of a cash transfer field experiment. His findings are consistent with mine: for men, being willing to pay to hide income is associated with investing less, while for women investment is low regardless.[8] I treat this result as complementary to my findings, with a focus on intra-household rather than inter-household sharing obligations as in my study.

Understanding marginal kinship tax rates and their effect on productive decisions is key to various micro-policies in developing countries aiming to increase productivity. Crucially this matters for the design of effective formal state and private financial interventions that can substitute for existing informal mechanisms, while allowing productive firms to expand without facing stifling requests for social support.

1 Conceptual framework

Taxes distort productive activity if they create a wedge between private and social returns from a productive activity. In formal tax systems, this gap is the marginal tax rate. In the case of informal taxation, the obvious analogue would be to measure the change in net transfers (outgoing minus incoming) with a change in income. A person who increases net transfers by fifty cents with every dollar of income faces a 50% marginal transfer rate.[9] However, this marginal transfer rate may not be a correct measure of the wedge between private and social returns. Unlike payments made to tax collectors, transfers to your social network are likely valued in their own right. Entrepreneurs who work to help support their parents value these transfers and may internalize the full social value of their productive decisions. The gap between the private and social returns from productive activity depends not just on the transfers made, but also on the extent to which these transfers are valued by the person making them. For transfers to one's kin to be distortionary, it must therefore be the case that, on the margin, these transfers are valued less than private consumption. This is what I refer to as the marginal kinship tax rate: the part of the marginal transfer rate which is not valued by the person making the transfers.

I propose a simple model of utility maximization with productive decisions, and allow for a kinship tax rate that depends flexibly on income. In contrast with a formal tax, which would appear directly in the budget constraint, I model kinship taxation as an additional constraint which specifies a minimum level of net transfers, given an agent's income. This minimum transfer may be negative, and indeed will be for all net recipients of transfers.[10]

[8] See Bernhardt et al. (2019) for more on the differential effects of cash transfers by gender.

[9] The literature on crowding-out of informal transfers as formal transfers increase refers to this as the 'transfer derivative,' as in Cox and Fafchamps (2008).

[10] While net transfer recipients have negative average tax rates, they are likely to nevertheless face positive

1.1 Kinship taxation

In this static model, an entrepreneur makes productive decisions and divides their income between individual consumption c, and net transfers d to their social network. Both consumption and transfers enter directly into the utility function $u(c, d)$. Utility from transfers may come for example from warm-glow altruism, increased social status, or anticipated reciprocity.

The key part of this model is an economic obligation to an agent's social network. Given some income y, agents are required to make a minimum net transfer $T_i(y)$, such that $d \geq T_i(y)$. Entrepreneurs may choose to transfer more than they are required to, but not less. The minimum transfer function T_i is individual specific, and may be a function of personal characteristics such as gender or age, as well as ability A_i.

To illustrate this minimum net transfer function, take as a benchmark a world of anonymous relationships, where no demands are made on anyone's income. In this case, $T(y) = 0$ such that, for any y, agents are free to set $d = 0$. If one of these agents is altruistic, she may choose to set $d > 0$ despite the lack of social obligation—this would depend on the shape of her utility function with respect to d. But she would have no recourse to make demands of others, and hence cannot choose $d < 0$.

Consider if now, by begging, an agent can receive transfers from others. In this case, $T_i(y) = -B$, where B is the amount that can be collected by someone who chooses to beg full time. If agents can costlessly pretend to be poor, B is not a function of income, though it may still be individual-specific because of differences in begging skill, or because of characteristics such as age, sex, or visible disability.

Consider alternatively if the T function were to represent transfer obligations to a state's fiscal authority rather than to one's social network. $T(y)$ would now be increasing in y. With no direct utility from paying taxes and no disutility from receiving government transfers, $\partial u/\partial d = 0$ and hence the constraint $d \geq T(y)$ would bind for all agents.

Production decisions consist of how much capital k to hire at rental cost r, which is the only input in a value-added production function $f(k)$, with heterogeneous entrepreneurial ability A. The agent's problem consists of:

$$\max_{c,d,k} \; u(c, d),$$

marginal tax rates, as in the analogous case of having welfare benefits withdrawn as income increases.

s.t.

$$\begin{aligned} c + d &= A_i f(k) - rk, \\ d &\geq T_i(y), \end{aligned} \tag{1}$$

where $y \equiv A_i f(k)$ and u, f, and T_i are differentiable and weakly increasing in their arguments. The optimal choice of each input depends on whether the kinship tax constraint (1) binds at the optimum. If $d^* > T_i(y)$, then the choice of capital satisfies $A_i f_k = r$. If (1) does bind, $d^* = T_i(y)$ and the choice of capital instead satisfies:

$$\left[1 - \frac{\partial T_i}{\partial y}\left(1 - \frac{u_d}{u_c}\right)\right] A_i f_k = r,$$

where u_c and u_d are marginal utilities of consumption and transfers, respectively.

Deviations from the productively efficient choice of capital therefore arise if the following term is non-zero: $\partial T_i/\partial y \times (1 - u_d/u_c)$. This term, which I define as the marginal kinship tax rate t_i, consists of the marginal transfer rate $\partial T_i/\partial y$, attenuated by the marginal utility of transfers u_d. If an entrepreneur receives no direct benefits from transfers at the margin, then $u_d = 0$ and the kinship tax rate is simply the transfer rate, $\partial T_i/\partial y$. If on the other hand the kinship tax constraint is slack, such that $u_d = u_c$, then the marginal kinship tax rate is zero. This is a crucial point, and highlights the problem with measuring transfers to analyze distortions from kinship taxation. Doing so ignores the distinction between transfers increasing with income without the tax constraint binding (insofar as transfers are a 'normal good'), and transfers that increase with income because of a binding kinship tax constraint. Identifying who faces kinship taxation then is not straightforward—it requires knowledge both of an implicit transfer obligation ($\partial T_i/\partial y$) that may be individual-specific, as well as marginal utilities u_d/u_c.

Conveniently, under certain assumptions, an individual's marginal kinship tax rate equals their willingness-to-pay to hide income. Since this can be elicited in a lab setting, it allows me to measure marginal kinship tax rates directly, without separately estimating the marginal transfer rate and the marginal utility from transfers.

1.2 Willingness-to-pay to hide income

To measure marginal kinship tax rates I make use of a common insight: a costly tax evasion technology will be used up to the point where its marginal cost equals the marginal tax rate. This point is made explicitly in Slemrod (2001), and underlies the Allingham and Sandmo (1972) model of tax evasion. Here tax evasion is modelled as a binary choice $H \in \{0, 1\}$,

where $H = 1$ corresponds to the decision to hide 1 unit of income at cost g. In practice, hiding income in this setting can take many forms: choosing an occupation or workplace where income flows are harder to observe, or hiding cash at home despite a risk of theft. Paying to hide income reduces the agent's 'taxable income' by 1 unit, such that the agent's problem becomes:

$$\max_{c,d,k,H \in \{0,1\}} \quad u(c,d),$$

s.t.

$$\begin{aligned} c + d + Hg &= A_i f(k) - rk, \\ d &\geq T_i(y - H). \end{aligned}$$

Optimal input choices remain the same, and an entrepreneur chooses to hide income ($H = 1$) if the cost is sufficiently small:

$$g \leq \frac{\partial T_i}{\partial y}\left(1 - \frac{u_d}{u_c}\right).$$

Notice that the term on the right hand side is the same as what was defined above as the marginal kinship tax rate, t_i. Thus, offered a choice to hide income from those who might make demands on it, this result implies an entrepreneur would choose to hide so long as the fraction lost by doing so were less than the marginal kinship tax rate. Willingness-to-pay to hide income, then, is equal to the distortion to productive incentives caused by kinship taxation.[11]

This measure of kinship taxation is robust to allowing for distortions that come not from having to make transfers, but from avoiding envy or jealousy. It captures transfers in the broadest sense, including not just cash payments but also in-kind transfers, preferential loans, and services of any kind. It also accounts for the fact that productive distortions come from expected future transfers rather than realized past transfers.[12]

[11] Crucially, this method is not meant to measure the amount of hiding outside the lab, or its costliness. It is possible that participants never choose to hide income, perhaps because they have no access to a technology to do so. Conversely, this result does not rely on the assumption that participants do *not* hide their income outside of the lab. Instead, it simply assumes that if, outside the lab, they would be willing to pay marginal cost g' to hide income, they would also be willing to pay marginal cost g' to hide income in the lab. What is important is that they are willing to hide at a marginal cost equal to their marginal kinship tax rate.

[12] Given risk aversion this would imply measured transfers underestimate the distortions from anticipated transfers, which my measure captures correctly. For example, you may choose to consume your income rather than invest it if you know that whenever your brother is sick he comes and asks for help. If your

However, one important concern is that earned and unearned income may face different kinship tax rates. This could be either because pressure to share ($\partial T_i/\partial y$) or benefits from sharing (u_d/u_c) depend on the source of income. Intuitively, these could act in opposite directions. On the one hand, it may be that the disutility cost of making transfers is lower for windfall gains. On the other, groups may tax earned income less heavily than a windfall, to attenuate moral hazard. This is particularly concerning given the finding that in some settings, framing income as having been earned has been shown to reduce giving in dictator games (Cherry et al., 2002).

While the distortions of primary economic importance would come from taxes on earned income, lab experiments typically rely on unearned income. I address this concern in part by having participants complete a real effort task to 'earn' the income they can choose to hide. I also directly test in section 3.2 whether, in this setting, people have different willingness-to-pay to hide earned versus windfall income.

The following intuition underpins the proposed method to identify kinship taxation: paying to hide income from one's social network signals that the value of potential future income is not fully internalized when making productive decisions.

2 Eliciting kinship tax rates

This section describes a lab-in-the-field experiment which presents each participant with an incentivized choice of whether to pay to hide income. As argued above, I interpret this decision to take costly actions to hide as an indicator of distortionary pressure to share income.

2.1 Experimental setup

In each location, eligible participants gathered in a designated area outside the local primary school, and were registered by the team supervisor. Each participant was assigned to a separate enumerator. Enumerators were distributed in teams of two to four, each in a separate classroom. These sessions were synchronized, so that once all the participants assigned to one team of enumerators completed the experiment, they left at the same time and a new set of participants were ushered in. From the beginning to the end of a session, participants worked one-on-one with their enumerator, first signing a consent form, then answering survey questions, receiving experimental instructions, making decisions in the

brother remains healthy, this distortionary effect will not be observable in measures of transfers. Realized transfers do not accurately measure the pressure to share resources when agents are risk averse.

experiment, and finally receiving any experimental winnings. These steps are described in turn below.

Consent forms were read aloud to participants in their native language (Somali), emphasizing the confidentiality of their answers. Participants then went through a short survey, answering questions about basic demographics and family characteristics. They were asked for their relationship to all other participants in their session (whether family, friend, or stranger). They were administered a short version of the Raven's Progressive Matrices test, to measure cognitive ability. Next they were presented with hypothetical situations and asked whether, in these situations, they would choose to pay to hide income. If they said they owned a business, they were administered an additional microenterprise module, discussed in section 4. After completing the survey, they moved on to the experimental section of the interview. All responses were recorded using data entry software (SurveyCTO) on smartphones, which allows randomization of questions asked to participants.

Before starting the experiment, participants were told they would be given the chance to earn money if they agreed to do some work. This was done to frame the money they might receive as having been earned. The task was to sort types of beans, which usually took less than five minutes.[13]

The experimental instructions explain the game in two steps.[14] Each participant was first told they would soon have a chance to receive a prize, but that before determining whether they do, they would have some choice over what the prize would be. They were told they would make a series of binary decisions, and that one of their decisions would be chosen as the prize they might win.

The following question was asked eight times to each participant: "If the prize were either \$5 announced or \$$x$ in secret, which would you choose?", where x is a different value for each question. Before being asked these questions, they were told that a prize 'announced' would be given to them in full view of the other participants in their session. Receipt of a prize 'in secret' would be known only to them.

The value of the 'announced' prize was held fixed at \$5 for each of the eight questions. The value 'in secret' went from \$1.50 to \$5 in \$0.50 increments. Before making these choices, each participant was shown eight small paper tickets. They were told that each represents one of the decisions they would have to make. After a participant made the first of their eight binary decisions (as in Table C.1), their choice was recorded on the associated ticket for that decision, and that ticket was then folded and placed into a small plastic basket, and

[13]Tasks similar to this are commonly used in lab-in-the-field experiments to induce effort. See for example Jakiela (2015); Barr et al. (2015).

[14]See Appendix F for detailed experimental instructions.

so on for each of the questions.[15]

After making these choices, they were asked to confirm their decisions and told that once the next stage begins they would no longer be able to amend them. One of their eight decisions was then chosen at random by drawing paper slips from a small basket, a simple and transparent way to implement a Multiple Price List elicitation procedure. The participant was shown the ticket they drew, and their choice for that ticket (whether \$5 in public or the specified value in secret) was emphasized.

Participants were allowed to make dominated choices. That is, they could choose \$3 in secret over \$5 in public, but also choose \$5 in public rather than \$4 in secret. (Hence, by transitivity, prefer \$3 in secret to \$4 in secret.) After all eight decisions were made, if any decisions were inconsistent the enumerator explained that one of their choices implies choosing less cash instead of more cash. They were asked to make each of the eight choices again. If they again made inconsistent choices, these were kept and the lottery progressed using their inconsistent choices.

The next step determines whether or not the participant receives the prize they chose, and was added to provide deniability to anyone who chose to take money in secret. This was done by putting small different-colored tokens in a second opaque plastic container. Before putting them in the container, the participant was asked to choose one color, which became the winning color. The container was then lifted above the eye-level of the participant, and they were asked to reach in and choose one of the tokens.

If a participant won the lottery and their chosen prize was to receive some amount of money in secret, it was given to them discreetly by their interviewer. Prize money was given in the form of cash, to more easily hide it.[16] If they won and their chosen prize was to have \$5 announced, this was announced to the other participants in their session and the cash prize given to them in full view of these participants. It was not announced more generally, to members of other sessions. Since the probability of winning the lottery was low, participants could plausibly deny having won anything. It is important that someone who did not publicly win a prize was thought to have lost the lottery, rather than have won but chosen to hide.

To help ensure comprehension, immediately before the lab experiment each participant played an almost identical "airtime" game. In this game, participants chose between a \$0.50 mobile airtime card and an equal or smaller amount of cash. Participants who chose the smaller cash prizes were effectively paying for liquidity, since airtime cards can in principle

[15]The order in which they made these eight decisions was randomized, as described in section 3.2.

[16]When piloting this experiment in slums of Nairobi, some participants preferred receiving their prize through a mobile payment (MPesa) for fear of theft. Participants in Garissa told us they preferred the liquidity of cash.

be resold at face value. These were not framed as 'practice' decisions, but real ones with a real prize. All details of the practice game, from the randomizing device to the text of the instructions, was as similar as possible to the subsequent income hiding game. If they won airtime or coins in the practice game, the prize was given to them on the spot. Once completed, participants were encouraged to ask any questions they had about the experiment before proceeding to the income hiding game.

2.2 Sample description

Data collection for this study took place from mid-November 2014 to mid-January 2015, in 17 villages across Garissa County in Eastern Kenya. The population of this area is mostly Somali, Islamic and agro-pastoralist. Subjects were recruited from the pool of participants in a separate cash transfer field experiment, which aimed to increase primary school enrollment and attendance. The cash transfer experiment is described in more detail in section 4. Participants in the cash transfer experiment were chosen by village committees under the guidance that each had to be responsible for at least one school-age child, and be from a relatively poor household. The condition related to having a child was chosen because the cash transfers were aimed at increasing school enrollment.

Advertisement for participation in the lab-in-the-field experiment was done in each village by a local leader shortly before my team of enumerators and I arrived in that village. This person, usually contacted by mobile phone, was told that we would be at the village's elementary school on a given day. They were asked to notify potential participants that we would like to interview them, and that there would be some compensation. No details were provided at this stage about the content of the interviews. Of the 2310 participants in the cash transfer experiment in the 17 villages, 1805 participated in the lab experiment and the associated survey measuring firm outcomes.[17] Descriptive statistics of participants in the lab experiment are presented in Table 1.

Mean household daily income of participants is 251 Kenyan Shillings, or approximately \$2.51.[18] Subjects earned an average of \$1.60 through their participation, including a \$1.10 show-up fee (paid in the form of 1kg of sugar) and experimental winnings ranging from \$0 to \$5.50.

[17] See Appendix E.1 for an analysis of which CT participants take part in the lab experiment.

[18] All monetary values are converted from KES to USD at the rate of 100 to 1. The exchange rate in January 2015 was 90.2, which I approximate to 100 for simplicity.

Table 1: Descriptive Statistics

Variable	Mean	Std. Dev.	N
(A) Individual characteristics			
Female	0.705	0.456	1804
Age	40.803	13.418	1804
Married (living with spouse)	0.804	0.397	1804
Separated	0.072	0.258	1804
Single (never married)	0.01	0.099	1804
Divorced	0.034	0.182	1804
Widowed	0.08	0.271	1804
Raven's score (%)	0.399	0.275	1725
Schooling (yrs)	0.311	1.386	1804
Islamic schooling (yrs)	0.99	1.339	1804
Num children in household	5.66	2.367	1804
Num own children	5.873	2.868	1804
Number of siblings	7.402	4.131	1804
Num parents (living)	0.995	0.771	1804
(B) Session co-participants			
Co-participants: Spouse	0.011	0.105	1804
Co-participants: Relatives	0.741	0.778	1804
Co-participants: Friends	0.129	0.446	1804
Co-participants: Acquaintances	0.874	0.892	1804
Co-participants: Strangers	0.07	0.3	1804

Notes: All data are from the lab experiment and its accompanying survey. *Panel A*: 'Married,' 'Separated,' 'Single,' 'Divorced' and 'Widowed' are dummy variables equal to 1 for the appropriate marital status and 0 otherwise. 'Raven's score (%)' is the percent of correct answers from a cognitive ability test. 'Num children in household' is the number of children living with the respondent, whether they are related to them or not. 'Num own children' is the number of biological children the respondent has who are living. 'Number of siblings' refers to siblings who are still alive. 'Num parents (living)' takes the value of 2 if both the mother and father are alive, and 1 or 0 if either or both are not. *Panel B*: Identity of co-participants in experimental session, as reported by the person being interviewed.

3 Anatomy of kinship taxation

Paying to hide income in the lab experiment provides a measure of kinship taxation. Before using studying the link between kinship taxation and investment decisions, this section provides an anatomy of kinship taxation: how do participants who face kinship taxation differ from those who do not? I then present various pieces of evidence which support the notion that participants understood the game, that their choices reflect their preferences, and that paying to hide in the lab is an informative measure of distortionary kinship taxation.

I find that 30% of men and 20% of women in my sample choose to pay to hide income. I interpret this as prima facie evidence that, within these communities, transfer arrangements have an efficiency cost. People are willing to reduce the total income of their network to reduce the visibility of their own income.[19]

Further, of those willing to pay, just over half are willing to pay the maximum elicited price of 70%. That is, they prefer \$1.50 in private to \$5 in public (Figure B.1). This suggests these pressures to share income may have potentially large efficiency costs. What predicts being willing to pay to hide income from your community members?

3.1 Who faces distortionary kinship taxation?

Table 2 presents a description of the individual-level predictors of kinship taxation. Column 1 starts with the most basic of these, age and gender. Column 2 includes characteristics that may proxy for earning ability: schooling, cognitive ability, and being a firm owner. Column 3 instead includes various family and household characteristics. Column 4 combines these, and column 5 adds control variables related to the experiment that might have affected their propensity to pay to hide income. The most notable of these are variables that capture the relationship to co-participants in the experimental session.[20] These are meant to control for the fact that participants face the decision to hide from people of varying degrees of closeness, which could influence their decision to pay to hide. The effect of relationships to co-participants is discussed in Section 3.2.

Female participants are substantially less likely to face kinship taxation. This is unsurprising in the context of the Somali communities in this study. Men are widely seen as the ones who bear the responsibility for inter-household transfers, and women who are unmarried, divorced or widowed are under the care of a male kin member (typically a father or

[19] This is analogous to the literature on non-cooperative intra-household bargaining, which finds non-Pareto efficient outcomes even within households (Udry, 1996; Duflo and Udry, 2004).

[20] Each person was asked about their relationship to every other participant in their session, classified as one of the following categories: spouse, family, friend, acquaintance and stranger. The most common relationships are family and acquaintance, as shown in Table 1.

Table 2: Correlates of kinship taxation

Dependent variable:	(1)	(2)	(3)	(4)	(5)
	Kin Tax (Marginal kinship tax rate > 0)				
Female	-0.117	-0.0706	-0.114	-0.0729	-0.0710
	(0.0239)	(0.0251)	(0.0238)	(0.0272)	(0.0269)
Age	0.00436			0.00518	0.00367
	(0.00380)			(0.00415)	(0.00413)
Age squared	-0.0000588			-0.0000577	-0.0000411
	(0.0000395)			(0.0000434)	(0.0000431)
Schooling (yrs)		0.0186		0.0190	0.0192
		(0.00837)		(0.00845)	(0.00837)
Islamic schooling (yrs)		0.0197		0.0191	0.0209
		(0.00831)		(0.00847)	(0.00855)
Raven's score (SD)		0.0263		0.0254	0.0122
		(0.0108)		(0.0110)	(0.0112)
Microentreprise owner		0.0649		0.0574	0.0579
		(0.0271)		(0.0272)	(0.0268)
Married (living with spouse)			0.0214	0.0170	0.0131
			(0.0250)	(0.0269)	(0.0269)
Num children in household			0.00941	0.0116	0.0134
			(0.00541)	(0.00559)	(0.00540)
Num own children			-0.00364	-0.00566	-0.00667
			(0.00461)	(0.00493)	(0.00467)
Number of siblings			0.00519	0.00412	0.00491
			(0.00257)	(0.00263)	(0.00263)
Num parents (living)			0.0159	0.00979	0.0112
			(0.0136)	(0.0149)	(0.0147)
Observations	1804	1725	1804	1725	1725
Experimental controls					X

Notes: Robust standard errors in parentheses. Each column is an OLS regression with a dependent variable equal to 1 if the participant pays to hide income in the lab experiment, and equal to 0 otherwise. 'Schooling' is years of formal secular and of islamic education. 'Raven's score (z-score)' is the score from a cognitive ability test, normalized to a standard deviation of 1. 'Married' is equal to 1 if married and not separated. 'Num children in household' is the number of children living with the respondent, whether they are related to them or not. 'Num own children' is the number of biological children the respondent has who are living. 'Num parents (living)' takes the value of 2 if both the mother and father are alive, and 1 or 0 if either or both are not. Experimental controls (column 5) consist of the number of each type of co-participant in the experimental session, and indicators for the randomized order of the MPL questions (ascending or descending price) and for being randomly selected to receive the 'hiding' income with certainty. All regressions include location (village) fixed effects.

brother).[21] Further, in this setting women have less independent ability to invest or save than men, and therefore may be more willing to make transfers now in the expectation of future reciprocity. The finding that women are less likely to pay to hide income is consistent with the findings in Boltz et al. (2019), Fiala (2018) and Beekman et al. (2015), while in contrast Jakiela and Ozier (2016) find that women are more likely to pay to hide income than men.[22]

The relationship with age, in contrast, is weak and not robust. Figure B.2 illustrates the relationship between age and kinship taxation non-parametrically. This figure suggests that kinship tax rates have a modest inverted-U pattern, increasing up to age 30 and starting to decline around age 50. Men see a particularly noticeable increase in kinship taxation in their 20s, and no discernible decline in old age.

Each of the four proxies for earning ability is positively associated with kinship taxation. An added year of either formal (secular) or Islamic schooling is associated with a 2 percentage point increase in the likelihood of facing kinship taxation.[23] In addition to education, I also use Raven's matrices to measure cognitive ability, or what is termed 'general intelligence' in psychometric studies. Kinship taxation seems to be increasing with performance in this test, though the link is not robust. Owning a firm is also associated with higher kinship taxation, and the choice of being an entrepreneur may be a proxy for ability.[24] Together, these results suggest that kinship tax rates are increasing in ability.

Figure 1 shows the relationship between predicted earnings and the rate of kinship taxation. The measure of predicted earnings comes from a regression of pre-treatment income on years of schooling, raven's score, and an indicator for being a firm owner. The figure illustrates the positive relationship between earning ability and kinship taxation.[25] In contrast, kinship taxation appears to not vary with actual earnings. Appendix C.2 presents evidence that kinship taxation does not vary with observed income, or with the receipt of a large cash

[21] See for example Lewis (1999).

[22] This may be related to cultural differences between our settings. Gender inequality seems more significant in Garissa County than in Western Kenya, where Jakiela and Ozier (2016) takes place. For example, average years of schooling for adults in the region of Kenya they study is almost equal for men and women, whereas in mine, men have almost three times more years of schooling than women. (Using the 2008/2009 DHS.) This is consistent with the idea that the reason women hide less than men in this setting is that they have fewer opportunities outside the home.

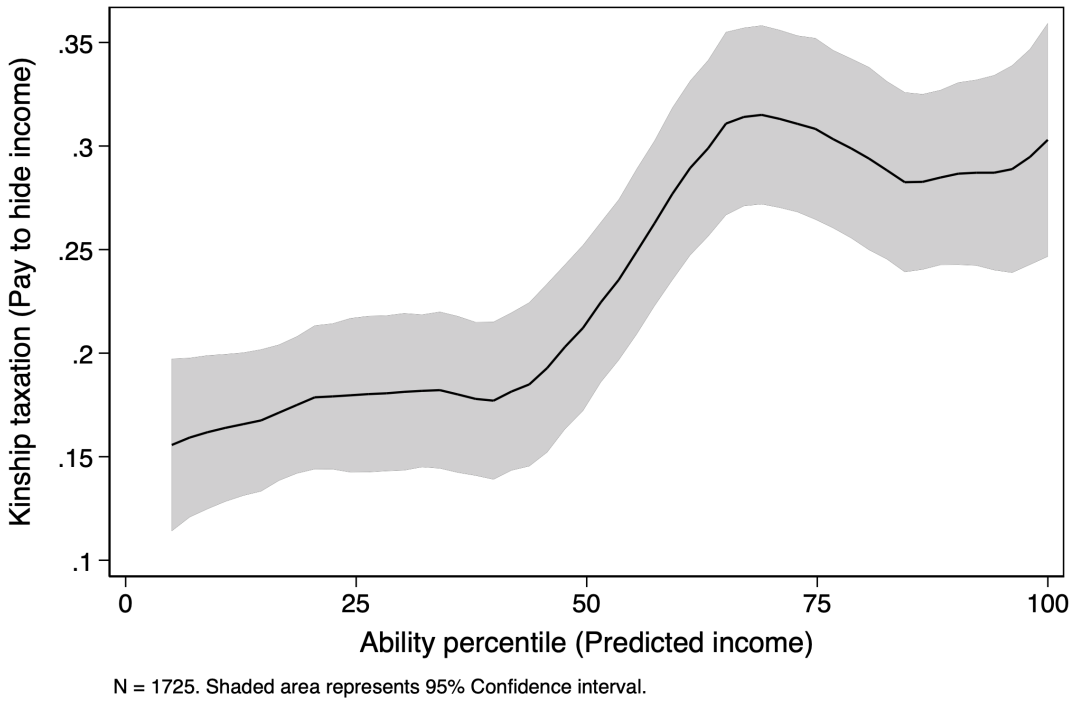
[23] Islamic education (mainly memorizing the Quran) is more common in this setting than formal secular education. These two types of education are not mutually exclusive: they are often undertaken simultaneously, as Islamic education is done mostly in evenings and weekends, and during school holidays.

[24] Indeed, entrepreneurs in this sample have higher schooling and cognitive ability (see Table A.1).

[25] This relationship holds if we summarize ability proxies (schooling, raven's score and firm ownership) using either their first principal component (Figure B.3) or a simple additive index (Figure B.4). It also holds when we include controls for female, age (quadratic), and lab session co-participants (Figure B.5), or when we omit the indicator for owning a firm (Figure B.6).

grant.

Figure 1: Kinship taxation and ability (predicted income)



Notes: 'Kinship taxation' measures the proportion of participant who are willing to pay to hide income. 'Ability' is a measure of predicted baseline income, using years of schooling, Raven's score, and an indicator for being a firm owner. The line in the figure is a local polynomial which describes the non-parametric relationship between these two variables.

Results for family and network characteristics are more mixed. Being married is not a significant predictor of kinship taxation.[26] Children are measured in two different ways to capture subtleties that are important in this setting. The first is the number of children living with the respondent, presumably under their care. This often includes children who are not their immediate offspring, such as nieces/nephews, grandchildren, or even fostered children of non-kin. The second is the number of biological children who are alive, but are not necessarily living with the respondent, including adult children. Kinship taxation increases with the first, but is uncorrelated with the latter. While not very robust, this suggests there is some channel through which intra-household issues do affect kinship taxation.[27]

Having more siblings is associated with higher kinship tax rates. However, whether both, only one, or neither of a respondent's parents are alive does not predict kinship taxation.

[26]While not shown in this table, this result holds for both men and women separately.

[27]An alternative explanation however is that people partly fulfill their kinship tax obligations by fostering children of their relatives (and hence causality runs in the opposite direction). This is a common practice in much of Africa, and has been documented in Serra (2009).

This mirrors the result on one's own children, and suggests that, to the extent that parents and children rely on each other for support, these transfers are not distortionary.

The most consistent conclusion from this set of results is that marginal kinship tax rates increase with earning ability. This suggests there may be disproportionately large effects of kinship taxation on aggregate output. Before exploring whether kinship taxation does affect productive decisions, this section ends with a discussion of the reliability of the lab experiment used to elicit kinship tax rates.

3.2 Comprehension and validity

Results from the lab experiment rely on participants understanding the questions they are asked and choosing answers carefully. This is of particular concern in a context where participants are unaccustomed to such exercises and have little formal education. I provide evidence that participants understood the lab experiment, that we can interpret their choices as reflective of their true willingness-to-pay to hide income, and that this measure does reflect their kinship tax rate.

About one in six men and one in nine women stated a preference for receiving \$1.50 in private rather than \$5 in public (Figure B.1). Is a willingness-to-pay of 70% credible, or does it reflect a mis-understanding of the decision being made? A first piece of evidence that these participants understood the choice they were making comes from their (open-ended) answers to why they chose to pay to hide their income. The most common reason these participants gave was to avoid having to share the income. The following quotes are illustrative: "I don't want to share with others that is why I prefer \$1.50 not announced"; "Nobody will ask me to give something"; "The \$5 announced might get finished due to demands from friends and family"; "There are so many people who are poor, they will ask you to give them something." Other common reasons, such as avoiding the evil eye, are consistent with the idea that paying to hide was done out of a desire to avoid social pressure to share income.[28] More details about the reasons participants gave for paying to hide income, as well as support for the remaining pieces of evidence in this section, are presented in Appendix D.

If choosing to pay to hide were a function of comprehension problems, one would expect that participants with less schooling or who seemed more confused during the experiment would be more likely to pay to hide. Instead we find the opposite pattern. Participants with more schooling and higher cognitive ability are more likely to hide as emphasized in Figure 1. Similarly, participants coded by enumerators as having better understood the experimental instructions are more likely to pay to hide (Figure D.1).

[28]This use of magical curses to enforce redistribution of income is consistent with Platteau (2009); LeMay-Boucher et al. (2012) and Gershman (2015).

Additional supportive evidence comes from the composition of experimental groups. Each participant was assigned to a 2-4 person group, from whom income could be hidden (or not). As expected, participants in larger groups are more likely to pay to hide income (Figure D.2). Further, the identity of these co-participants matter. Participants seem to hide from relatives and friends, but not from group members they are more distantly connected to (Table D.1).

The pattern of participants' choices given the eight prices at which they could choose to pay to hide also supports the idea that they understood the decision they were making. Participants' decisions were insensitive to the (randomized) order in which they made these eight binary choices (Figure D.3), and only 23 of 1805 participants made dominated decisions.[29]

It is reasonable to be concerned that the complexity of the lab experiment added confusion to the straightforward decision of whether to pay to hide income. As part of the accompanying survey, each participant answered a simple unincentivized question about willingness-to-pay to hide income at a single price. Responses to this question are highly correlated with decisions in the lab experiment (Table D.2).

Finally, to ensure they understood the lab experiment, participants played a practice 'airtime' game, as described in section 2.1. While its purpose was simply to teach participants how to play the hiding game, there is evidence that they understood the airtime game: willingness-to-pay to convert an airtime card into cash is small on average, but increases by almost half in locations without reliable mobile network coverage. This is what we would expect given lower liquidity of airtime cards in locations without a reliable network, and suggests an understanding of the mechanics of the experiment.

A second striking feature of the data is that most participants chose to either always or never hide for all eight decisions, as shown in Figure B.1. That is, relatively few participants (24% of men and 15% of women) chose to hide at a low price but not at a higher price. I present evidence in Appendix D.3 that this behavior is not a product of low comprehension on the part of participants, but rather reflects the true bimodal distribution of marginal kinship tax rates in this population.

A final set of concerns about the lab measure of kinship taxation is that, while participants' choices may accurately reveal their willingness to pay to hide income, they may not reflect the true pressure to share income that these individuals face when making (real-world) productive decisions.

Participants' willingness to pay to hide income in the lab may differ from their kinship

[29]Dominated decisions entail choosing to pay to hide at a certain price while choosing not to pay to hide at a lower price.

tax rate on real-world income for a number of reasons. First, income in the lab may be treated as a windfall gain, which may affect the pressure to share this income relative to income earned outside the lab. I show evidence in Appendix D that, for this sample, the distinction between earned and windfall income appears to not be important. Participants are equally likely to say they would pay to hide earned or unearned income when asked in a way that holds all other considerations fixed (Table D.4). This result is consistent with a literature that finds that in traditional societies such as this one, whether income is earned or not does not affect dictator game allocation decisions (Cappelen et al., 2013; Barr et al., 2015; Jakiela, 2015).[30] It is also consistent with the cross-cultural psychology literature (Schäfer et al., 2015), and with Platteau (2000, 2009) who argues that societies such as the one studied here operate on egalitarian norms where success is attributed entirely to luck, and all income is seen as fundamentally unearned.

A second concern is that participants may be more (or less) willing to take costly actions to avoid sharing larger sums than the $5 in the lab. I show that increasing (or decreasing) this amount by an order of magnitude does not affect the share of people who say they would choose to pay to hide (Figure D.3). I also show suggestive evidence that the reason participants hide is not in order to spend that money in ways their family or community considers reproachable, such as on cigarettes (Table D.5).

Finally, it is unclear exactly how observable income is both in and out of the lab. I show that behavior in the lab is not driven by the concern that money received privately will not remain private. Participants were not concerned about their ability to keep cash physically hidden, nor about the observability of the resulting consumption. Conversely, lab sessions were relatively small, and information about income received may not fully diffuse to the rest of the community. A related concern is that real-world income may not be fully observable, which would mean the kinship tax rate measured in the lab might apply only to the fraction of real-world income which is observable. If reducing the observability of income is costly (an entrepreneur choosing to hire fewer workers or operating their business farther from home, for example) this cost would form part of the distortion from kinship taxation.

Together, the evidence from this section suggests that participants did understand the game and that their choices are informative of their true willingness-to-pay to hide income. Using these as measures of kinship tax rates, we can estimate the distortions in productive activity from kinship taxation.

[30] In contrast, university students from WEIRD populations (Henrich et al., 2010) choose to give less money away in dictator games when income is framed as having been earned (Cherry et al., 2002; Cappelen et al., 2007).

4 Kinship taxation and investment

This section explores whether kinship taxation, as measured in the lab, can explain real-world behavior. To do so, I exploit variation created by a pre-existing RCT that provided large cash grants to a subset of the participants in the lab experiment. I focus on business owners and test whether kinship taxation affects investment. Do entrepreneurs who would otherwise invest their cash transfers in expanding their business choose not to do so because they might be compelled to share their increased profits? That is, does kinship taxation hinder firm growth?

This section uses variation from a pre-existing cash transfer RCT (hereafter "CT experiment") run by a non-profit organization (Save the Children) with the aim of increasing school attendance. To do so, they enrolled 4000 households in 31 villages, assigning 3000 to the treatment group and 1000 to control.[31] Randomization was done publicly, such that a household's assignment to the treatment or control group was public knowledge within a village. Treatment consisted of 18 monthly cash transfers of $30, a total of $540 over the entire duration of treatment. Lab participants in the treatment and control groups are balanced across a wide range of characteristics, as shown in Appendix E.2.[32] The timing of this cash transfer RCT, and how it relates to the lab experiment that is the focus of this paper, is in Figure 2. An important limitation of the analysis in this section is that the lab experiment happened after, rather than before, the CT experiment. I address this issue and provide more details on the design of the CT experiment in Appendix E.3. Another important limitation is that not all CT participants took part in the lab experiment and therefore can be included in this analysis. In Appendix E.1 I show how participants differ from non-participants.

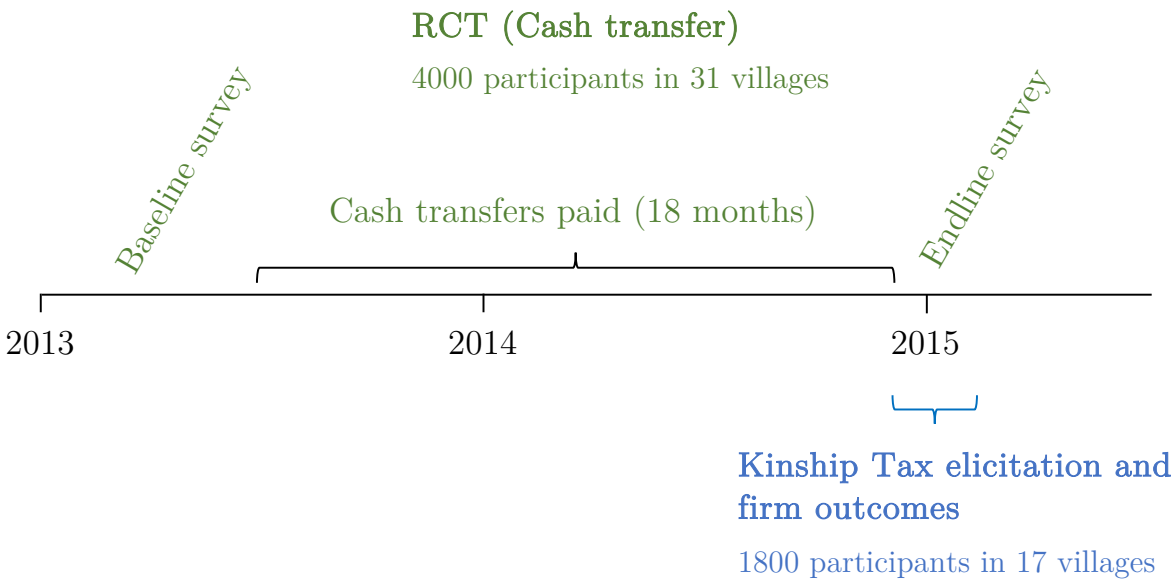
4.1 Sample and data

The analysis in this section focuses on business owners, since they make regular investment decisions which may be affected by kinship taxation. Respondents are included in this sample if they answered yes to the following question: "Do you personally own a business of any kind, for example a kiosk, a shop, selling charcoal, vegetables, a restaurant?" Of the 1805 participants in the lab experiment, 20% answer yes, which provides a sample of 361 entrepreneurs. These entrepreneurs, as shown in Table A.1, differ systematically from non-entrepreneurs in the sample. They are more likely to be female, have more years of schooling,

[31] The treatment group consisted of conditional and unconditional cash transfer arms as in Baird et al. (2011). For the purposes of this paper they are treated indistinguishably.

[32] Importantly, entrepreneurs in the control and treatment groups are also balanced on observables (Table E.3).

Figure 2: Timing of the cash transfer experiment



are less likely to be married, and have more siblings. They also face higher kinship tax rates, which suggests distortions in this sector may be particularly important.

Entrepreneurs were asked about firm-level outcomes using questions adapted from De Mel et al. (2008). Their responses are summarized in Table 3. Firms are on average just under 4 years old, and 15% of them are registered with their local government. Capital stock is calculated as the sum of the value of inventory, equipment and any structure (premises) used for the business. Labor use is the sum of the number of wage workers, unpaid workers (family members, for example) and any owners who provide labor to the business, including the respondent.

Profits are calculated as the total income earned from the business, including any wage entrepreneurs pay themselves. The precise question is: "What was the total income the business earned last month after paying all expenses including wages of employees, but not including any income you or another owner paid yourselves. That is, what were the profits of your business last month?" For similar firms in Sri Lanka, De Mel et al. (2009b) find that this question is a more accurate measure of business income than detailed questions on revenues and expenses. Unlike in more formal settings, these entrepreneurs do not keep formal records, and hence finding a measure of output which is reasonably well-measured is crucial. Using the "total income" question as a measure of profits is reasonable since in this context we cannot distinguish between payments entrepreneurs earn from their labor, from entrepreneurial rents, and from returns to capital. Consistent with this society's gender roles,

Table 3: Descriptive Statistics (Firms)

Variable	Mean	Std. Dev.	N
Microentreprise owner	0.2	0.4	1805
Business age (yrs)	3.905	4.13	317
Hours worked daily	8.006	3.319	361
Business registered	0.15	0.357	361
Capital: Inventory	251.564	706.131	358
Capital: Equipment	113.564	562.525	358
Capital: Structures	180.175	396.716	357
Monthly sales	299.683	1536.818	350
Monthly profits	85.783	190.35	351
Labor use	1.665	1.391	361

Notes: All data are from the lab experiment and the accompanying survey. Hours worked daily only includes work related to their business. Capital, sales and profits are in USD. Labor use is the sum of wage workers, unpaid workers, and owners who work in the business.

discussed in Section 3, male firms are much larger than female firms (Table A.2). Notably, male-owned firms have more than twice as much capital stock as female-owned firms.

4.2 Analysis

The question this section aims to address is whether kinship taxation can act as a deterrent to investment. To test this, I exploit variation in access to income from the CT experiment, which could be used to purchase capital. I use the following empirical specification to test whether the cash transfer treatment effect differs for entrepreneurs who face kinship taxation:

$$\ln k_i = \beta_1 \text{Treat}_i + \beta_2 \text{KinTax}_i + \beta_3 \text{Treat}_i \times \text{KinTax}_i + \gamma X_i + \epsilon_i. \tag{2}$$

Treat_i and KinTax_i are indicator variables for being in the CT experiment treatment group (that is, receiving \$540 in cash transfers), and paying to hide income in the lab, respectively. X_i is a set of controls. The first parameter of interest is β_1, which represents the effect of cash grants on capital stock for entrepreneurs who do not face kinship taxation.

The other parameter of interest is β_3, which is the difference in the treatment effect for entrepreneurs who *do* face kinship taxation. If pressure to share income does have a constraining effect on investment, we would expect that $\beta_3 < 0$. To answer the question of whether kinship taxation attenuates incentives to invest, it is necessary to study a population

for which $\beta_1 > 0$. That is, entrepreneurs who do not face kinship taxation must increase their capital stock with the cash transfer in order to test the hypothesis that kinship taxation reduces investment.

In order to focus on a population most likely to invest the cash transfer in their firm, I split the sample by sex. Research in this literature has consistently found that female-owned microenterprises do not typically benefit from such cash grants. Bernhardt et al. (2019) find that this seems to be because capital is instead invested in male-controlled businesses in the household. See Kevane and Wydick (2001); De Mel et al. (2008), and Fafchamps et al. (2014) for evidence of this phenomenon in Guatemala, Sri Lanka, and Ghana. De Mel et al. (2009a) discuss this finding at greater length.

4.3 Results

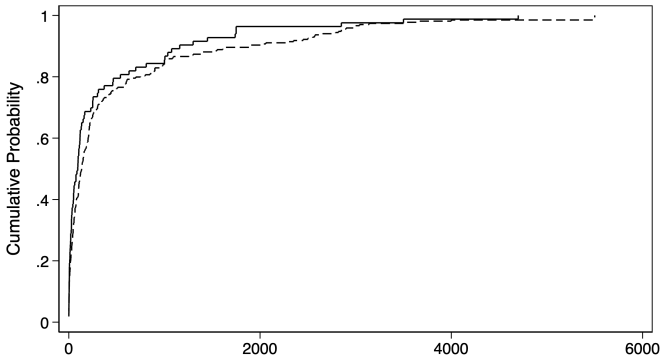
Using the sample of entrepreneurs who participated in the lab experiment, I study the effect of the cash transfer treatment on the amount of capital in their firms. First, I find that while the cash transfer treatment seems to have increased the capital stock of entrepreneurs overall the effect is modest, as shown in Figure 3a. This is a cumulative density plot, where the dotted line is mostly to the right of the solid line, indicating entrepreneurs in the treatment group have somewhat higher amounts of capital. Second, figures 3b and 3c show the effect separately for male and female entrepreneurs. Consistent with the literature cited above, I find that cash transfers lead to a large increase in capital stock for male-owned firms, while female-owned microenterprises do not respond to treatment. Female entrepreneurs in this setting appear to face constraints to firm growth that are not alleviated by cash transfers alone. Since the disincentive effect of kinship taxation on investment can only be tested for entrepreneurs who would otherwise invest, the remainder of this section focuses on male entrepreneurs.

Does kinship taxation deter investment? Table 4 reports results from regression equation (2). Column 1 shows that male-owned microenterprises who receive cash transfers more than double their capital stock if they do not face kinship taxation. However, summing coefficients from row 1 and row 3 shows that, conditional on facing a positive kinship tax rate, the effect of the cash transfer is small and not significantly different from zero. This relationship holds when controlling for a range of individual characteristics. For women (columns 3-4), the effect of the cash transfer is small and not statistically significant whether or not they face kinship taxation. That is, kinship taxation does not explain the low investment rate of female entrepreneurs in this setting.

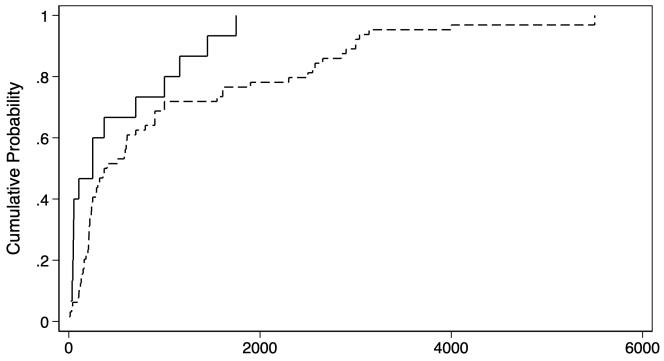
Table A.3 in the appendix reproduces these results using capital stocks in levels (rather

Figure 3: Effect of cash transfer on capital stock of firms

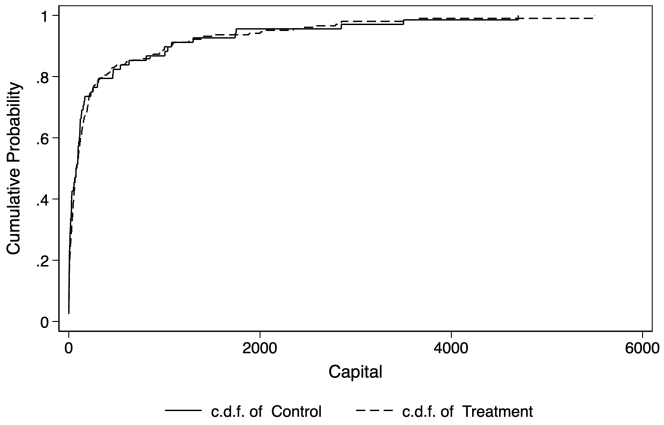
(a) All firms



(b) Male-owned firms



(c) Female-owned firms



Notes: Cumulative density function of firm capital in USD. Total values are then winsorized at 1% to attenuate the effect of outliers. The dashed lines represent the distribution of capital for entrepreneurs whose households received cash transfers in the CT experiment, while the solid lines are for those in the control group. (a) $N = 352$, (b) $N = 79$, (c) $N = 273$.

than log) as the outcome measure. Results are consistent though significant only at the 10% level. The magnitude of these results suggests that roughly the entire value of the $540 cash transfers was invested in the firms of male entrepreneurs who do not face kinship taxation. Results on the effect of treatment on firm profits is in Table A.4. These results are also consistent with the ones on capital stock, though they are substantially noisier.

Table 4: Effect of Cash Transfer Treatment on Capital Stock

	(1)	(2)	(3)	(4)
Sample:	Male entrepreneurs		Female entrepreneurs	
Dependent variable:	Capital (log)	Capital (log)	Capital (log)	Capital (log)
Treat	1.883	1.636	-0.0543	-0.0689
	(0.689)	(0.785)	(0.298)	(0.290)
KinTax	1.774	1.600	-0.120	0.111
	(0.873)	(0.887)	(0.548)	(0.556)
Treat × KinTax	-1.999	-2.264	0.308	0.166
	(1.014)	(1.034)	(0.637)	(0.639)
P-values				
Treat = 0	0.00850	0.0427	0.856	0.812
Treat + (Treat × KinTax) = 0	0.870	0.343	0.653	0.864
Observations	79	79	265	263
Experimental controls	X	X	X	X
Individual controls		X		X

Notes: Robust standard errors in parentheses. All columns are OLS regressions, with a dependent variable equal to the (log) capital stock of the entrepreneur. *Treat* is a indicator variable for being randomized into receiving a large cash transfer in the CT experiment. *KinTax* is an indicator variable for paying to hide in the lab experiment. *Treat + (Treat × KinTax)* represents the effect of treatment for entrepreneurs with *KinTax* = 1. Individual controls include age, schooling, raven's score, and number of siblings. All regressions include experimental controls described in Table 2 and location (village) fixed effects.

This suggests that kinship taxation discourages productive entrepreneurs from investing in their firms, and hence providing them with access to finance is likely to have small returns. However, an alternative explanation for this result is that entrepreneurs who face kinship taxation have lower returns to capital. Kinship taxation might simply proxy for these low returns to capital, rather than directly reducing investment. I show that this does not appear to be the case.

I first test whether entrepreneurs who face kinship taxation produce less output per unit of capital.

This measure of the average product of capital will be informative about each firm's

marginal product of capital if firms produce using a simple production function such as Cobb-Douglas.[33] Column 1 of Table 5 shows the results of a regression of (log) profits-to-capital ratio on an indicator variable for kinship taxation. Column 2 includes the treatment indicator, since a cash grant might lower a firm's average (and marginal) product of capital by relaxing credit constraints, and column 3 adds individual controls. While the results are imprecise, the point estimates suggest that the firms of entrepreneurs who face kinship taxation may have substantially higher profits per unit of capital. And as expected in a setting where credit constraints are likely to bind for many entrepreneurs, receiving a cash grant is correlated with lower profits-to-capital ratio. Columns 4 to 6 show results from a similar analysis using (log) productivity as the dependent variable, where the outcome is equal to (log) y/k^{α}, with $\alpha = 2/3$. These results are imprecise but suggest that the reason entrepreneurs who face kinship taxation do not invest their cash transfers is not because they are less productive or have lower returns.

Table 5: Output per unit of capital (Male entrepreneurs)

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Profits/capital (log)			Productivity (log)		
Kin Tax > 0	0.333	0.215	0.561	0.354	0.284	0.507
	(0.461)	(0.434)	(0.585)	(0.382)	(0.361)	(0.483)
Treatment (Cash transfer)		-0.876	-0.613		-0.512	-0.382
		(0.487)	(0.501)		(0.430)	(0.429)
Observations	75	75	75	75	75	75
Experimental controls	X	X	X	X	X	X
Individual controls			X			X

Notes: Robust standard errors in parentheses. All columns are OLS regressions. In columns 1-3 the dependent variable is equal to the (log) ratio of profits to capital stock of the entrepreneur. In columns 4-6 the dependent variable is the (log) of the residual productivity term A for each entrepreneur using the following production function: $y = Ak^{\alpha}$, with $\alpha = 2/3$. *Kin Tax* > 0 is an indicator variable for paying to hide in the lab experiment. *Treatment (Cash transfer)* is a indicator variable for being randomized into receiving a large cash transfer in the CT experiment. Individual controls include age, schooling, raven's score, and number of siblings. All regressions include location (village) fixed effects.

Finally, I test whether proxies for returns to investment (schooling and cognitive ability) can explain the relationship described between kinship taxation and investment behavior. If kinship taxation were only proxying for low (or high) ability, including ability interacted with

[33]Assume, for example, that firms share a common production function of the form $y = Ak^{\alpha}$, where y is profits, A is an entrepreneur's productivity, k is capital stock, and α is a positive constant. Then each firm's marginal product of capital is a linear function of y/k. This abstracts from non-convexities in the production function, including fixed costs.

treatment status should attenuate the results on kinship taxation. I test for this in Table A.5, and find that ability differences do not explain the lower returns for entrepreneurs who face kinship taxation. This is consistent with evidence from section 3 that kinship taxation is increasing, rather than decreasing, in ability.

An alternative test of the differential returns to capital for entrepreneurs who face kinship taxation would be to use the cash grant as an instrument for capital stock, as in De Mel et al. (2008). This would require separately estimating the relevant two-stage regression for entrepreneurs who face kinship taxation and those who do not. However, this strategy requires that the instrument (cash grant) increases the capital stock of both types of entrepreneurs. Since those who face kinship taxation do not invest their cash transfers, it is not possible to use this method to estimate their returns to capital. Said differently, there is no first stage for the entrepreneurs who face kinship taxation (and hence we cannot measure their returns to capital in this way).

To summarize, I find that male entrepreneurs in my sample invest a large share of these cash transfers into their business. I then show, however, that male entrepreneurs invest these cash transfers only if they do not face kinship taxation. That is, male entrepreneurs who pay to hide income in the lab do not invest in their firms when provided with large cash transfers. I test for and confirm that this is not because these entrepreneurs have lower returns to capital.

5 Conclusion

This paper documents the cost of kinship taxation in a poor rural setting. First, I propose a formalization of kinship taxation which emphasizes the difference between transfers made voluntarily (as gifts) from transfers made under pressure (as a tax). I argue that eliciting willingness-to-pay to hide income is a credible way of measuring individual-level marginal kinship tax rates. I find that these tax rates are higher for men, and are strongly increasing in ability. I show a broad range of evidence that suggests participants understood the decisions they made to pay to hide income in the lab, and that these decisions are reflective of real pressure to share income.

I then show that kinship taxation as measured in the lab has important real-world consequences for productive efficiency. Male entrepreneurs who face kinship taxation abstain from investing in their business despite seeming to have high returns. Since this disproportionately affects high-ability entrepreneurs, the losses from this distortion are potentially large.

The degree to which these findings can be generalized to the broader economy is not

clear. This depends in part on whether the phenomenon of kinship taxation is more or less important in urban settings than in rural ones. The greater mobility of urban workers and the erosion of traditional identities may lead to a reduction of kinship tax rates, and formal substitutes for insurance provided by informal transfers may also be more readily available. On the other hand, whatever distortions there are from kinship taxation among the urban poor may be more costly if they have more opportunities for productive investment available. Piloting this study in Nairobi, I found that participants living in urban slums faced similar kinship tax rates as the rural sample. This may be because in an urban environment, the perceived benefits from making transfers are relatively small, so what transfers do happen are more distortionary. Arthur Lewis describes this potential problem well:

> "[The extended family] is at any time a deterrent to making superior effort, and it is especially so at times when the family concept is narrowing, and the community is passing from wider to narrower recognition, since it is then that men are least likely to accept claims which they would previously have taken for granted." (Lewis, 1955, p 114)

Measuring marginal kinship tax rates across societies by eliciting willingness-to-pay to hide income would provide a more reliable way of measuring where we would expect this distortion to matter most. It may also shed light on the societal transitions that weaken the grip of the extended family on economic activity.

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A Appendix: Additional Tables

Table A.1: Entrepreneurs

	(1) Non-Entrepreneur	(2) Entrepreneur	(3) p-value of diff.
Kin Tax > 0	0.220	0.291	0.005
Treatment (Cash transfer)	0.781	0.765	0.501
Female	0.687	0.776	0.001
Age	41.193	39.244	0.014
Schooling (yrs)	0.254	0.540	0.000
Islamic schooling (yrs)	0.990	0.989	0.986
Raven score (%)	0.390	0.434	0.007
Married (living with spouse)	0.814	0.765	0.033
Num children in household	5.644	5.720	0.587
Num own children	5.938	5.612	0.053
Number of siblings	7.224	8.116	0.000
Num parents (living)	0.963	1.125	0.000
N	1443	361	
Proportion	0.800	0.200	

Notes: Data are from the lab experiment and the accompanying survey. Entrepreneurs include anyone who answered Yes to whether they "personally own a business of any kind." 'Kin Tax > 0' is an indicator for being willing to pay to hide income in the lab. 'Treatment (Cash transfer)' is an indicator for being randomized to receive the treatment in the CT experiment. 'Schooling' is years of formal secular and of islamic education. 'Raven's score (SD)' is the score from a cognitive ability test, normalized to a standard deviation of 1. 'Married' is equal to 1 if married and not separated. 'Num children in household' is the number of children living with the respondent, whether they are related to them or not. 'Num own children' is the number of biological children the respondent has who are living. 'Num parents (living)' takes the value of 2 if both the mother and father are alive, and 1 or 0 if either or both are not. P-values are computed after including location fixed effects and experimental controls as in Table 2.

Table A.2: Descriptive Statistics (Firms), by sex of owner

	(1) Male-owned	(2) Female-owned	(3) p-value of diff.
Business age (yrs)	3.627	3.992	0.504
Hours worked daily	7.840	8.054	0.610
Business registered	0.272	0.114	0.000
Capital: Inventory	449.377	193.719	0.004
Capital: Equipment	282.192	64.255	0.002
Capital: Structures	267.165	155.455	0.027
Monthly sales	398.204	272.356	0.528
Profits	135.831	71.718	0.009
Labor use	1.679	1.661	0.917
N	81	280	
Proportion	0.224	0.776	

Notes: All data are from the lab experiment and the accompanying survey. Hours worked daily only includes work related to their business. Capital, sales and profits are in USD. Labor use is the sum of wage workers, unpaid workers, and owners who work in the business.

Table A.3: Effect of Cash Transfer Treatment on Capital Stock

	(1)	(2)	(3)	(4)
Sample:	Male entrepreneurs		Female entrepreneurs	
Dependent variable:	Capital	Capital	Capital	Capital
Treat	738.9	580.2	20.16	-5.136
	(428.0)	(476.8)	(142.6)	(152.3)
KinTax	627.6	485.0	39.90	114.9
	(517.1)	(536.5)	(241.5)	(232.9)
Treat × KinTax	-807.8	-922.8	1.783	-27.81
	(583.2)	(602.1)	(272.4)	(288.4)
P-values				
Treat = 0	0.0901	0.230	0.888	0.973
Treat + (Treat × KinTax) = 0	0.857	0.331	0.926	0.888
Observations	79	79	273	270
Experimental controls	X	X	X	X
Individual controls		X		X

Notes: Robust standard errors in parentheses. All columns are OLS regressions, with a dependent variable equal to the capital stock of the entrepreneur (in levels, not in logs). *Treat* is a indicator variable for being randomized into receiving a large cash transfer in the CT experiment. *KinTax* is an indicator variable for paying to hide in the lab experiment. *Treat + (Treat × KinTax)* represents the effect of treatment for entrepreneurs with $KinTax = 1$. Individual controls include age, schooling, raven's score, and number of siblings. All regressions include experimental controls described in Table 2 and location (village) fixed effects.

Table A.4: Effect of cash transfer treatment on profits (value added)

	(1)	(2)	(3)	(4)
Sample:	Male entrepreneurs		Female entrepreneurs	
Dependent variable:	Profits (log)	Profits (log)	Profits (log)	Profits (log)
Treat	0.930	0.740	0.198	0.120
	(0.609)	(0.694)	(0.203)	(0.204)
KinTax	1.529	1.446	0.367	0.319
	(0.712)	(0.867)	(0.399)	(0.395)
Treat × KinTax	-1.408	-1.369	-0.146	-0.0234
	(0.734)	(0.870)	(0.439)	(0.429)
P-values				
Treat = 0	0.133	0.293	0.332	0.556
Treat + (Treat × KinTax) = 0	0.369	0.242	0.898	0.804
Observations	75	75	273	270
Experimental controls	X	X	X	X
Individual controls		X		X

Notes: Robust standard errors in parentheses. All columns are OLS regressions, with a dependent variable equal to the (log) profits of the entrepreneur. *Treat* is a indicator variable for being randomized into receiving a large cash transfer in the CT experiment. *KinTax* is an indicator variable for paying to hide in the lab experiment. *Treat + (Treat × KinTax)* represents the effect of treatment for entrepreneurs with *KinTax* = 1. Individual controls include age, schooling, raven's score, and number of siblings. All regressions include experimental controls described in Table 2 and location (village) fixed effects.

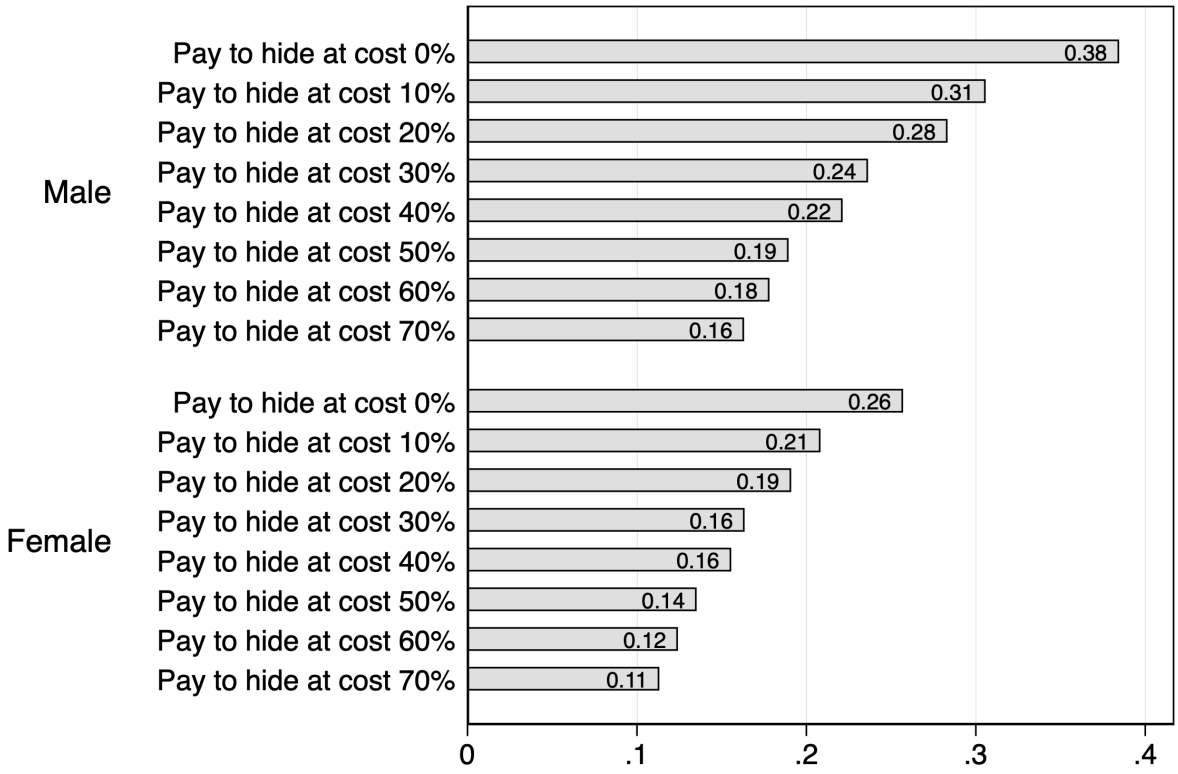
Table A.5: Treatment by Ability

	(1) Capital (log)	(2) Capital (log)	(3) Capital (log)	(4) Capital (log)	(5) Capital (log)	(6) Capital (log)
Treat	1.883	1.636	1.236	1.632	1.711	2.191
	(0.689)	(0.785)	(1.523)	(1.462)	(1.410)	(1.502)
KinTax	1.774	1.600			1.311	1.401
	(0.873)	(0.887)			(0.919)	(1.028)
Treat × KinTax	-1.999	-2.264			-1.900	-2.107
	(1.014)	(1.034)			(0.995)	(1.116)
High Ability			1.526	1.212	1.234	0.793
			(0.959)	(0.906)	(1.022)	(0.983)
Treat × High Ability			-0.628	-0.958	-0.227	-0.550
			(1.048)	(0.970)	(1.138)	(1.084)
P-values						
Treat = 0	0.00850	0.0427	0.421	0.270	0.231	0.152
Treat + (Treat × KinTax) = 0	0.870	0.343			0.921	0.963
Treat + (Treat × Ability) = 0			0.358	0.317	0.0308	0.0624
Observations	79	79	79	79	79	79
Experimental controls	X	X	X	X	X	X
Individual controls		X		X		X

Notes: Robust standard errors in parentheses. All columns are OLS regressions, with a dependent variable equal to the (log) capital stock of the entrepreneur. *Treat* is a indicator variable for being randomized into receiving a large cash transfer in the CT experiment. *KinTax* is an indicator variable for paying to hide in the lab experiment. *High Ability* is an indicator variable for having above-median predicted profits using schooling and Raven's score. *Treat + (Treat × KinTax)* represents the effect of treatment for entrepreneurs with *KinTax* = 1. *Treat + (Treat × Ability)* represents the effect of treatment for entrepreneurs with *High Ability* = 1. Individual controls include age, schooling, raven's score, and number of siblings. All regressions include experimental controls described in Table 2 and location (village) fixed effects.

B Appendix: Additional Figures

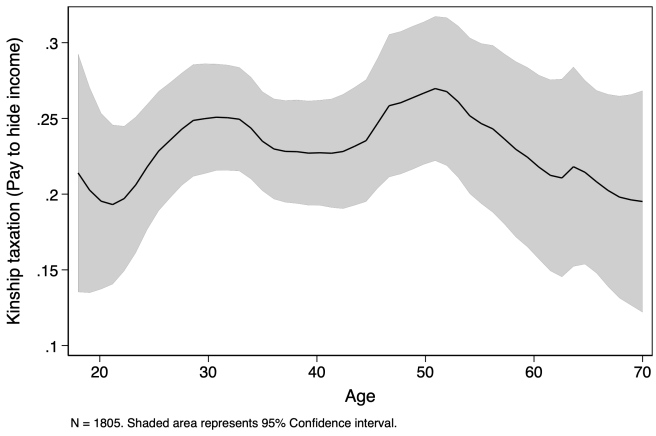
Figure B.1: Fraction of participants who pay to hide at each price



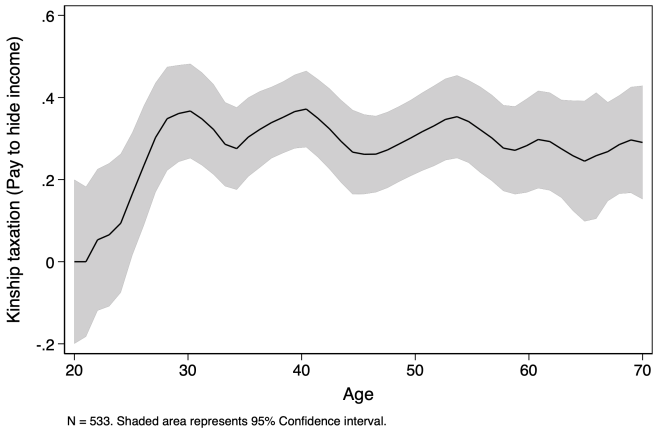
Notes: Each bar represents the fraction of participants in the lab experiment who chose to hide income at various prices. Each participant chose to hide or not at all eight prices, and only 1% made dominated choices.

Figure B.2: Kinship taxation and age

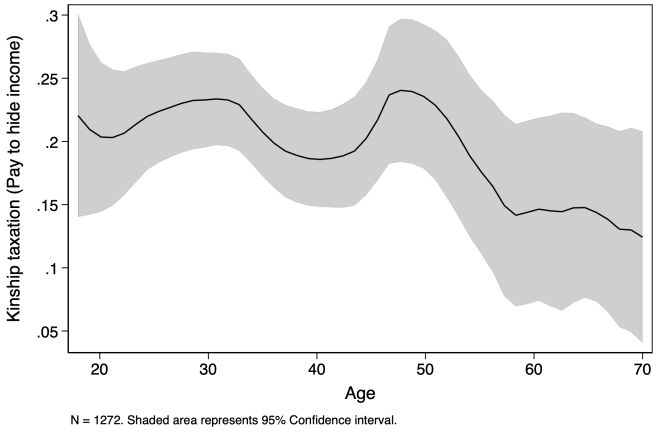
(a) All participants



(b) Male only

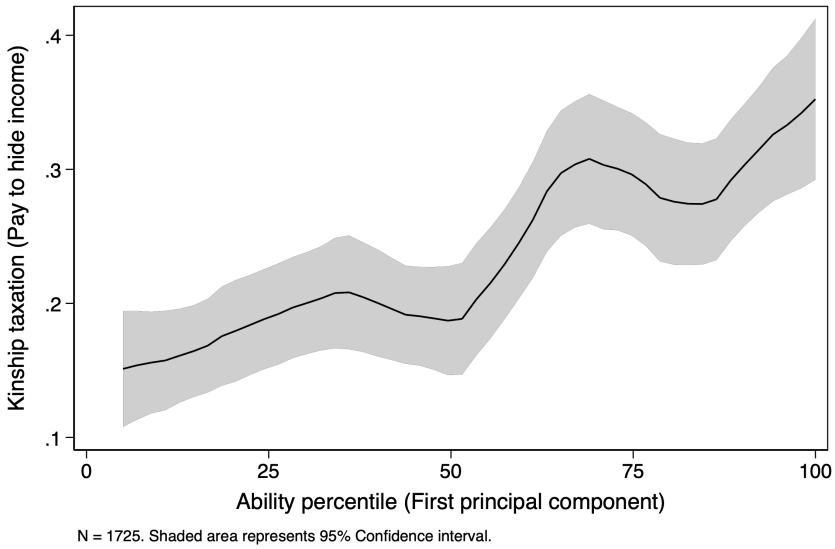


(c) Female only



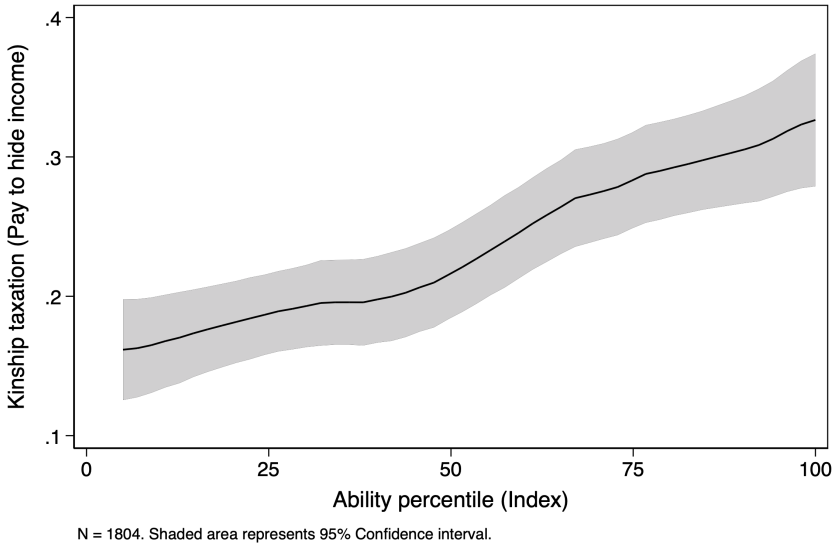
Notes: Data are from the lab experiment and the accompanying survey. 'Kinship Taxation' is an indicator for being willing to pay to hide income in the lab. Ages greater than 70 have been set equal to 70 (this affects less than 3% of the sample).

Figure B.3: Kinship taxation and ability (first principal component)



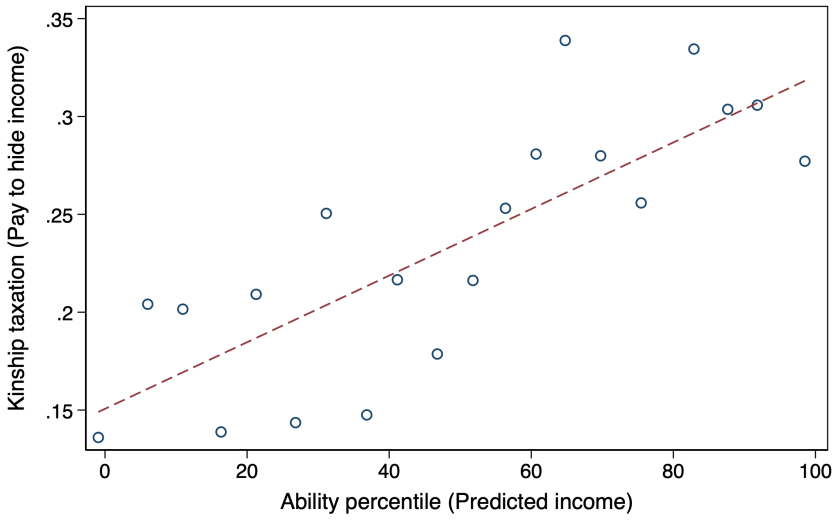
Notes: 'Kinship taxation' measures the proportion of participant who are willing to pay to hide income. 'Ability' is the first principal component of the following variables: years of schooling, Raven's score, and an indicator for being a firm owner. The line in the figure is a local polynomial which describes the non-parametric relationship between these two variables.

Figure B.4: Kinship taxation and ability (index)



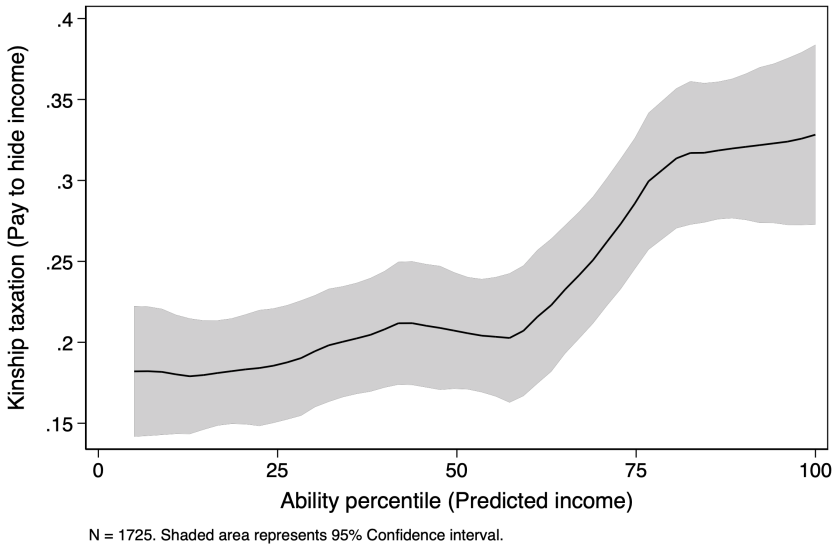
Notes: 'Kinship taxation' measures the proportion of participant who are willing to pay to hide income. 'Ability' is the sum of z-scores for the following variables: years of schooling, Raven's score, and an indicator for being a firm owner. The line in the figure is a local polynomial which describes the non-parametric relationship between these two variables.

Figure B.5: Binscatter of kinship taxation and ability (predicted income)



Notes: 'Kinship taxation' measures the proportion of participant who are willing to pay to hide income. 'Ability' is a measure of predicted baseline income, using years of schooling, Raven's score, and an indicator for being a firm owner. The figure is a 'binscatter' representation of the relationship between these two variables, after controlling for the following variables: the participant's sex, age, age squared, and the number of each type of co-participant in the lab experiment.

Figure B.6: Kinship taxation and ability (predicted income) omitting 'Entrepreneur' dummy



Notes: 'Kinship taxation' measures the proportion of participant who are willing to pay to hide income. 'Ability' is a measure of predicted baseline income, using years of schooling and Raven's score. The line in the figure is a local polynomial which describes the non-parametric relationship between these two variables.

C Appendix: Lab experiment

C.1 Calculating tax rates

Before interpreting the results of the lab experiment, the eight binary decisions of whether to hide income at a given price need to be converted into a single willingness-to-pay. Table C.1 demonstrates how I do this with four hypothetical sets of responses. Person A never chooses to pay to keep their income hidden, and hence has a willingness-to-pay of 0%. Person B is willing to pay \$2 to hide \$5. That is, they are willing to accept a 40% reduction of the potential prize to keep it hidden. Person C prefers as little as \$1.50 in secret to \$5 publicly, which means they have at least a 70% willingness-to-pay to hide income.

Table C.1: Choice list for Willingness-to-pay to hide income

If the prize were either...	Person A	Person B	Person C	Person D
1) \$5 announced or \$5.00 secret	Secret	Secret	Secret	Secret
2) \$5 announced or \$4.50 secret	Announced	Secret	Secret	Secret
3) \$5 announced or \$4.00 secret	Announced	Secret	Secret	Secret
4) \$5 announced or \$3.50 secret	Announced	Secret	Secret	Announced
5) \$5 announced or \$3.00 secret	Announced	Secret	Secret	Secret
6) \$5 announced or \$2.50 secret	Announced	Announced	Secret	Announced
7) \$5 announced or \$2.00 secret	Announced	Announced	Secret	Announced
8) \$5 announced or \$1.50 secret	Announced	Announced	Secret	Secret
Imputed WTP to hide income	0%	40%	70%	20%

Notes: Given four hypothetical sets of choices in the lab experiment, the bottom row specifies the corresponding imputed willingness-to-pay to hide income. In each case, the price paid for the last contiguous "secret" choice, starting from the first row, is the highest price we know with certainty the person is willing to pay. Person D is treated as if they chose "announced" for rows 4 to 8.

In each of these cases the imputed willingness-to-pay is conservatively chosen as the lower bound on their true willingness-to-pay. Person A, for example, may be indifferent between \$4.80 in secret and \$5 in public, implying a willingness-to-pay of 4%. This is especially true for person C, whose revealed willingness-to-pay is in the range of 70-100%.

Of the 1805 participants, 23 made dominated choices, as illustrated by Person D. In such cases, they are coded conservatively as having permanently switched to 'Announced' after their first such decision. In the case of Person D, this means all decisions below the first Announced decision (at \$5 announced versus \$3.50 secret) are treated as if they were also Announced.[34]

[34]The box around the first three choices (the set of contiguous "secret" choices, starting from the top) illustrates how inconsistent choices are interpreted.

C.2 Kinship tax and income

By analogy to formal progressive tax schedules, the primary source of heterogeneity in marginal tax rates may be income. Given the high tax rates measured in the experiment, however, an important concern is reverse causation: high tax rates could lead to lower effort and investment, and hence lower income. An income shock from the cash transfer field experiment allows us to address this concern, isolating the causal impact of income on marginal tax rates.

Surprisingly, kinship tax rates appear to be flat with respect to this income shock. Column 1 of Table C.2 shows that tax rates of participants who received cash transfers are no different from those who did not. Adding a range of individual-level characteristics as controls, in column 2, does not alter this result. The absence of a difference between the control and treatment group is not simply a result of an insufficiently large treatment: The sum of cash transfers received by the treatment group is about 9 months of median household income.

Table C.2: Kinship tax rates and Income

	(1) All	(2) All	(3) Control	(4) Control	(5) Treatment	(6) Treatment
Treatment (Cash transfer)	0.00302	-0.00663				
	(0.0237)	(0.0240)				
Baseline income (log)			-0.0278	-0.0366	-0.00801	-0.00895
			(0.0277)	(0.0285)	(0.0147)	(0.0148)
Observations	1804	1725	343	329	1215	1164
Experimental controls	X	X	X	X	X	X
Individual controls		X		X		X

Notes: Robust standard errors in parentheses. All columns are OLS regressions, with a dependent variable equal to 1 if kinship tax rate is strictly positive, and equal to 0 otherwise. 'Cash transfer' is equal to 1 if the participant is in the cash transfer RCT treatment group, and equal to 0 if in the control group. Baseline income is measured as total monthly household income in USD, prior to randomization into the cash transfer treatment or control groups. Columns (3) and (4) include only participants in the CT experiment control group, and columns (5) and (6) include only participants in the CT experiment treatment group. Individual controls consist of age (quadratic), schooling, Raven's score, number of siblings and of children living in the household. All regressions include experimental controls described in Table 2 and location (village) fixed effects.

An alternative to using the cash transfer treatment as a proxy for income is to use household income measured before participants were randomly assigned to the control or treatment groups. Separating the sample into control and treatment in columns 3 and 5 and regressing the tax rate on the log of household income suggests that the slope is close to

zero. Adding controls, in columns 4 and 6, does not alter this result.

There are a few caveats to this result. Respondents with high kinship tax rates may have understated their household income, which would be reasonable if they felt their answers may not be fully private. Household income is also hard to measure well in this context, given that much of household production is not marketed, and hence difficult to value accurately. Also there may be a disconnection between individual kinship tax rates and household (rather than individual) income. Each of these issues might attenuate the slope of the relationship. Despite these concerns, the randomized cash transfer study provides compelling evidence that marginal kinship tax rates do not vary substantially with income.

The finding that kinship taxation does not vary with income can be reconciled with the result that it is increasing in earning ability in the following three ways. First, it is reasonable to assume that the relationship between income and kinship taxation varies within and across groups. Within groups, those with the highest income should face the highest rates (consistent with the results on ability). However the groups with the highest mean incomes may in turn face lower average tax rates, as their members need less assistance, and when assistance is needed there are more members who can help. When looking at individuals across many groups, these two forces go in opposite directions, and attenuate the within-group relationship between income and tax rates. The second way to reconcile these findings is that households vary in the amount of consumption needed to maintain comparable levels of welfare, and this is correlated with the household income. If tax rates are extracted on income in excess of the required level of consumption, then this correlation with the size or the needs of the household would also attenuate the relationship between tax rates and income. Finally, there may simply be reverse causation: high kinship tax rates discourage production and reduce income, which again attenuates the relationship between these two. Ability is a measure of potential income, and hence that relationship is not distorted by this reverse causal channel.

D Appendix: Lab experiment comprehension and validity

D.1 Choosing to hide does not result from low comprehension

D.1.1 Stated reason for hiding

After completing the lab experiment, participants were asked why they chose to hide or not. In particular, we might be worried about the 225 people who reported that they prefer receiving \$1.50 in secret to \$5 in public. Responses were open-ended, and combined into categories ex-post. Of these 225 participants, 89 said they were hiding because they did not want to share with others.

A further 69 said they chose to hide because they did not want others to see what they received. While they did not explicitly say they did not want to share the income, it seems reasonable to believe that the reason they did not want others to know they received money is that they would be asked to share some of it. "I don't want people to know that I got money, I just want it to be secret"; "I don't want people to see what I am receiving"; "I never want people to see what I am receiving."

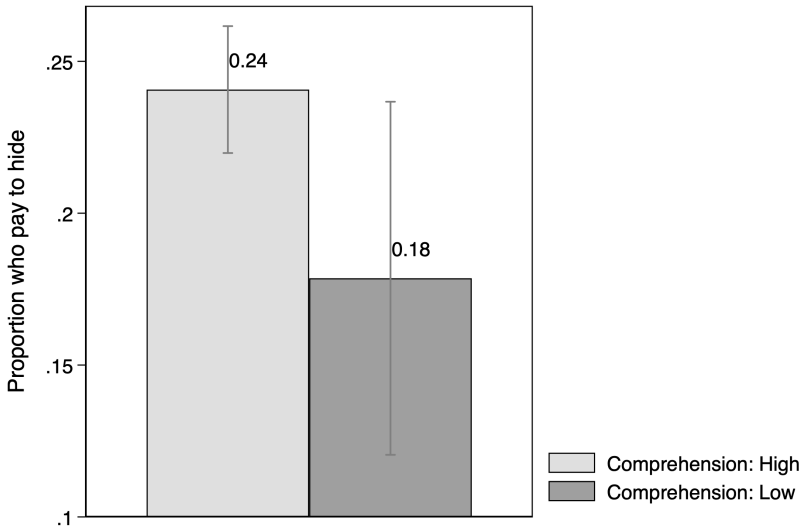
37 people said they were hiding to avoid an 'evil eye': "I don't want people to notice, they may curse me with evil eye." As noted above, LeMay-Boucher et al. (2012) and Gershman (2015) explore the use of magical beliefs to underpin income sharing.

Of the remaining 30 participants with high willingness-to-pay to hide income, 12 simply said they preferred having money in secret, with no further explanation, and 7 gave uninformative answers. The latter include "I don't know"; "If God wishes that I get that money, it is ok." The remaining responses are: wanting to choose who to give the money to (4 people), being worried about safety (4), and wanting to avoid hatred, suspicion or humiliation (3). Taken together, it is hard to reconcile these responses with simple confusion or misunderstanding.

D.1.2 Those with high comprehension hide more

Enumerators coded how well they felt a participant understood the experiment. Enumerators ranked 91% of participants as having "excellent" or "good" understanding, while 9% were coded as "not so good" and less than 0.3% were "very bad." Figure D.1 shows that participants with low comprehension (coded as anything below 'good') were less likely to pay to hide.

Figure D.1: Comprehension



Notes: Each bar represents the fraction of participants in the lab experiment who chose to pay to hide income in the lab experiment. Participants are divided by how well their enumerator judged them to have understood the experiment. Those coded as "good" or "excellent" are categorized into High comprehension, whereas those coded as "not so good" or "very bad" are categorized into Low comprehension.

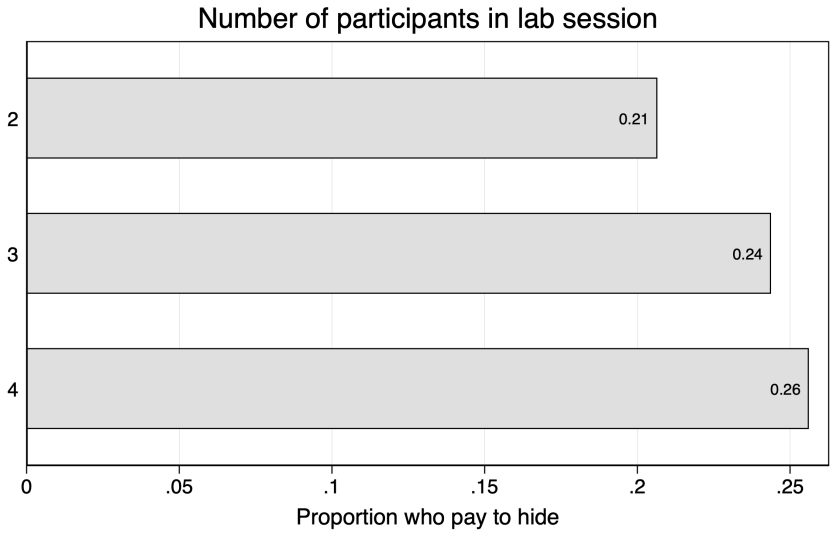
D.1.3 Co-participants

The experiment can also help us understand whom participants are hiding their income from, which may suggest whom they are being taxed by. We can do this by focusing on the relationship between people in a given experimental session. Participants were told that if they chose \$5 'in public' as a prize and they won, this would be announced to the other participants in their session. These sessions had significant variation in the relationship between participants. Panel B of Table 1 summarizes the number of co-participants of a given type of relationship (spouse, family, friend, acquaintance, or stranger). The categories were left somewhat ambiguous, to let each person decide how they thought of their relationship with a given person. They were each asked about every other participant in their experimental session, so in a given pair one person could say they were friends, while the other said they were acquaintances. Friends included all close relationships between people unrelated by blood or marriage. Acquaintances were people known to respondents, but with no close ties. Strangers were people who did not know each other.

I find that having more co-participants in an experimental session is correlated with a higher likelihood of paying to hide, as in Figure D.2. Further, Table D.1 presents results from a regression of kinship tax rates on the number of co-participants who are members of the participant's social network. These correlations, however, should not be interpreted

causally.[35] I find that an additional close friend or relative in the session is correlated with an increase in the probability of hiding in the experiment. An additional stranger in the experimental session does not seem to increase the probability of paying to hide money. While these relationships are not causal, they suggest the choice to hide in the experiment is driven by a desire to prevent demands from one's social network, rather than a fear of theft.

Figure D.2: Number of participants in experimental session



Since information can spread, hiding from a particular person is not evidence that this person will tax the recipient. It may be for example that when anyone wins, their co-participants will immediately tell all relevant people about this, and hence the relationship between participants should have no effect on tax rates. To the degree that information does spread, these results represent the sum of taxation by co-participants and anyone they convey this information to.

It may seem surprising that people hide as much from friends as from relatives, given that it seems clear from the literature that the majority of informal transfers are made between relatives (hence 'kinship' taxation). It may be that while transfers to relatives are larger than those to friends, it is the latter that are more distortionary because the value of these transfers is not internalized, as it is for close family members. Qualitative evidence supports this idea. For example, a 53 year-old man who chose to hide his earnings from

[35]The experimental design calls for the supervisor to record the relationship between each participant in a session, before they are randomly allocated an enumerator. This would have allowed me to estimate the causal impact of having an additional relative or friend as a co-participant. Because of data recording problems, this relationship data was recorded for only 242 of 1805 participants, and is not used in this analysis.

Table D.1: Kinship Tax correlations: Co-participants

	(1)	(2)	(3)
Sample:	All participants	Male only	Female only
Dependent variable:	Kin tax > 0	Kin tax > 0	Kin tax > 0
Co-participants: Spouse	0.00694	0.0832	-0.0789
	(0.0931)	(0.155)	(0.0975)
Co-participants: Relatives	0.0339	0.0705	0.0234
	(0.0193)	(0.0384)	(0.0220)
Co-participants: Friends	0.156	0.192	0.136
	(0.0297)	(0.0512)	(0.0363)
Co-participants: Acquaintances	0.0126	0.0353	0.00735
	(0.0168)	(0.0337)	(0.0191)
Observations	1805	533	1272

Notes: All data are from the lab experiment and the accompanying survey. Robust standard errors in parentheses. All columns are linear probability models (OLS), with a dependent variable equal to 1 if kinship tax rate is strictly positive, and equal to 0 otherwise. Each 'Co-participants' variable is equal to the number of people in the same experimental session who were identified by the participant as being a member of the given category.

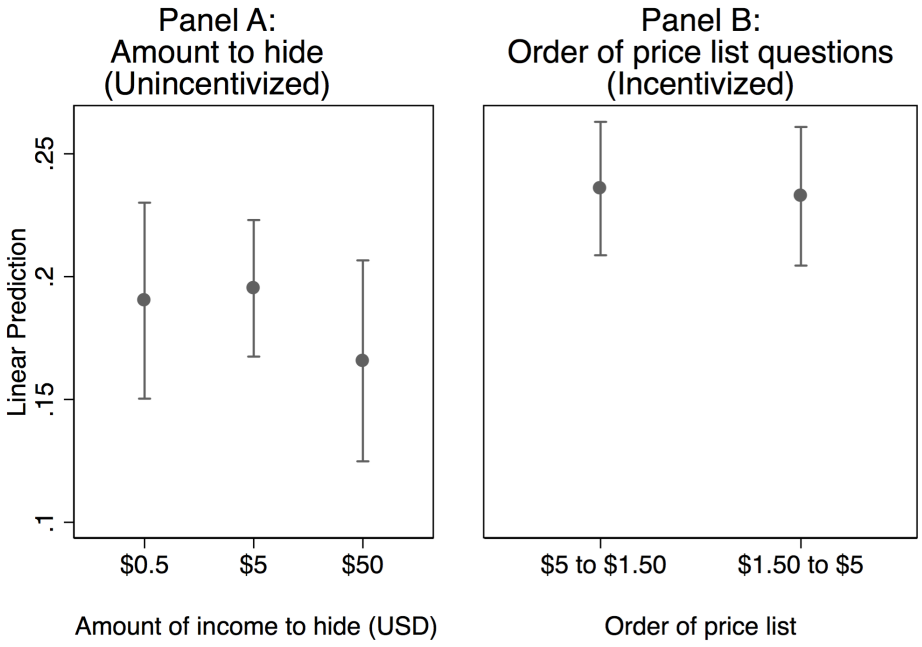
'acquaintances' reported that he did so because: "I want to use [the money] alone with my family."

D.1.4 Random order of experimental questions

The order of the eight binary choices in the experiment, shown in Table C.1, was randomized for each participant. For half of them, the first choice was \$5 announced or \$5 in secret and moved down, top to bottom. For the other half, the first choice was between \$5 announced or \$1.50 in secret and moved up, bottom to top. If people were making decisions based on their true preferences, the order of questions ought not to matter.

Panel B of Figure D.3 shows that revealed willingness-to-pay to hide in the incentivized lab experiment is insensitive to the order of questions. Participants did not, for example, make the first binary decision carefully, and then stick to their decision for the remaining seven questions. This supports the conclusion that participants were not systematically making mistakes when choosing to pay to hide income.

Figure D.3: Willingness-to-pay to hide income, incentivized and unincentivized



Notes: Each bar represents a mean and 95% confidence interval for whether participants choose to pay to hide income in a given condition. Panel A shows the difference in responses across amounts to hide (in each case the cost of hiding was 20% of the total amount) in an unincentivized, hypothetical question. Panel B shows differences across order of price list questions in the incentivized lab experiment.

D.1.5 Simplified hiding decision

As part of the survey and before doing the experiment, each participant answered a simple unincentivized question about willingness-to-pay to hide income. Specifically they were asked:

> Imagine that I offer you $5 today. Imagine also I did not offer money to the other participants in this room. Now what if I gave you the choice of not telling anyone that I gave you money. Then the others would not know that you received any money from me. If you could choose either, (1) I give you $5 and I announce to the other participants in this room, or (2) I give you $4 and do not tell the others, which would you prefer?

Responses to this simpler and more straightforward question are closely correlated to those in the more complex lab experiment. Table D.2 shows in 82% of cases, participants made the same decision of whether to hide or not in this hypothetical question as in the incentivized lab experiment.[36] This suggests choices in the lab experiment are not driven by its complexity.

Table D.2: Decision to pay to hide, incentivized versus hypothetical

		Choose to pay to hide (Hypothetical question)		
		Hide	Don't hide	Total
Choose to pay to hide (Incentivized question)	Hide	181 12%	164 11%	345 23%
	Don't hide	95 6%	1036 70%	1131 77%
	Total	276 19%	1200 81%	1476 100%

Notes: The rows in this table (Incentivized) correspond to a choice in the lab experiment to pay a positive amount to hide $5. The columns (Hypothetical) correspond to an unincentivized stated preference for receiving either $5 announced to co-participants, or $4 in private. The shaded cells highlight the 82% of participants whose decision to hide income is consistent across the incentivized and hypothetical questions.

[36] For those whose decisions in the lab did not match their hypothetical answer, most switched to hiding in the lab experiment after having said they would not hide in the hypothetical question. This is consistent with the idea that some participants are embarrassed to admit they want to hide income from their group, and will only choose to do so when the stakes are real.

D.2 WTP to hide income and kinship taxation outside of the lab

D.2.1 Earned versus windfall income

Kinship tax rates may vary for different types of income. Specifically, participants may feel differently about paying to hide earned versus windfall income. This is important, as all estimates in this paper of the cost of kinship taxation relate to income which would be earned, either through judicious investment or additional effort.

As mentioned in section 2.1, the money to be hidden in the lab experiment was framed as payment for an effort task, to induce participants to see the income they might receive as having been earned. However, the need to ensure deniability for participants who chose to hide meant there had to be some element of chance to winning the prize.

To test whether this element of chance reduced the salience of the 'earned' framing, some participants were randomly assigned to receive the income from this experiment with certainty. That is, some participants chose whether to pay to hide or not knowing with certainty that they would receive the money.

Specifically, for a small subset of locations I further randomized 1/8 participants to receive income from the hiding experiment with certainty. They were told the following before deciding whether to hide: "Because of the quality of your answers and your work cleaning the beans, you have been chosen to EARN WITHOUT DOING ANY LOTTERY. The reward for answering my questions and cleaning the beans will be two prizes. The first time you will receive airtime or some coins, the second time you will receive a few hundred shillings."

Contrast this to the message given to all other participants: "As a reward for answering my questions and cleaning the beans, I will now give you two chances to receive some money. The first time you might receive airtime or some coins, the second time you might receive a few hundred shillings."

Randomization into this "automatic win" experimental treatment was only done for a small subset of participants, so these results should be interpreted with caution. It was only implemented in the last few days of the experiment, and hence in just a few villages. This was in part for budgeting reasons (participants who chose not to pay in the airtime and hiding experiments received \$5.50 in the 'automatic win' treatment, versus an average of \$0.50 otherwise).

If the lottery element of the experiment primed participants to interpret the money they might receive (and could pay to hide) as being a windfall rather than earned, the 'automatic win' treatment should have dealt with this. As shown in Table D.3, we cannot reject that being randomized to win with certainty had no effect on the likelihood of paying to hide.

This is consistent with two interpretations: the first is that the framing of the experimental income as being earned was successful, so that removing the lottery aspect had no effect. The second is that increasing the degree to which participants see the income as earned has no effect on their willingness to pay to hide it.

Table D.3: Effect of automatic win on hiding in the lab

	(1) Kin Tax > 0	(2) Kin Tax > 0	(3) Kin Tax > 0
Automatic win	-0.0173	-0.0134	-0.0243
	(0.0682)	(0.0691)	(0.0681)
Observations	275	275	275
Mean of dependent variable	0.218	0.218	0.218
Experimental session controls		X	X
Individual controls			X

Notes: Robust standard errors in parentheses. Each column is an OLS regression with a dependent variable equal to 1 if the participant pays to hide income in the lab experiment, and equal to 0 otherwise. Experimental controls are described in Table 2. Individual controls consist of age (quadratic), schooling, Raven's score, number of siblings and of children living in the household.

This latter interpretation is consistent with results from a hypothetical question a subset of participants answered which asked whether they would pay to hide in a situation involving earned income. After giving an answer to the hypothetical question described in section **??** (which was framed as windfall income), they were also given the following scenario:

> Now, imagine that I asked you to do a difficult job, and offered $5 as payment for work. Imagine also I do not offer this job to the other participants in this room.

They were then again given the same choice between receiving $4 in secret or $5 in public. The results, for people who were asked both questions, are in table D.4. First, the fraction of respondents choosing to hide in both scenarios is almost equal (18% versus 17%). Second, the shaded cells in the table show that these choices are highly consistent: for 88% of participants, the hiding decision is unaffected by whether the income is a windfall or is earned.

D.2.2 Amount of income to hide

It may be that participants pay to hide income not to reduce transfers to others, but because of a desire to avoid being the subject of an announcement. The preference for money received

Table D.4: Hiding earned versus windfall income

		Choose to pay to hide EARNED income		
		Hide	Don't hide	Total
Choose to pay to hide WINDFALL income	Hide	79 11%	38 6%	117 17%
	Don't hide	46 7%	533 77%	579 83%
	Total	125 18%	571 82%	696 100%

Notes: The rows in this table (Windfall) correspond to an unincentivized stated preference for receiving either \$5 announced to co-participants, or \$4 in private. The columns (Earned) correspond to a similar choice, but where the income which can be hidden comes as payment for "a difficult job". The shaded cells highlight the 88% of participants for whom the hiding decision is unaffected by whether the income is a windfall or is earned.

in secret comes for these people from this social embarrassment, rather than any pressure to share the announced income. If this were the case, the desire to avoid this announcement would be roughly independent of the amount received. That is, willingness-to-pay to hide (in percentage terms) should be decreasing in the amount to be hidden.

To test whether hiding of income is fixed rather than proportional, participants were asked one of three questions, chosen at random. One of the three is the hypothetical income-hiding question discussed above, where the choice is between receiving \$4 in secret or \$5 in public. The other two are identical, except for the fact that the amounts participants chose between were either (1) \$0.40 in secret or \$0.50 announced, or (2) \$40 and \$50. That is, they were asked whether they would be willing to give up 20% of potential income to keep it hidden, across three orders of magnitude.

Panel A of Figure D.3 shows that the decision to pay to hide seems relatively insensitive to the size of the sum to be hidden. Given the consistency of decisions across the three very different amounts, it seems that the motivation to hide is roughly proportional in the amount.

D.2.3 Socially sanctioned consumption

The link between behavior in the lab and outside of it is clearly a first-order concern. You raise the concern that participants may have been paying to hide their income not to avoid having to share it but instead having to spend it in a socially acceptable manner. The most

direct piece of evidence I have which speaks to this is a question in the endline survey of the cash transfer RCT, that asks the following question: "Does any member of your household chew miraa or smoke/chew tobacco?" (Miraa is the local name for khat, which is a mild narcotic Somalis commonly chew, and which is more common than cigarette smoking in this region. Note also that alcohol is not commonly consumed in these communities.) 22% respond yes to the question above. If individuals were paying to hide in order to spend a larger share of their income on the consumption of khat or tobacco, we would expect a positive correlation between consuming khat/tobacco and the desire to hide income. Instead, I find no such correlation in Table D.5, where the dependent variable is an indicator for paying to hide income in the lab.

Table D.5: Khat/tobacco consumption and kinship taxation

	(1) All	(2) Male	(3) Female	(4) All	(5) Male	(6) Female
HH member consumes khat/tobacco	0.00292	0.0144	-0.00976	0.00623	0.0267	-0.00847
	(0.0251)	(0.0493)	(0.0286)	(0.0252)	(0.0501)	(0.0288)
Observations	1633	470	1163	1633	470	1163
Mean of dependent variable	0.231	0.302	0.203	0.231	0.302	0.203
Experimental session controls				X	X	X

There is also evidence from other African settings that is consistent with the idea that hiding income is done to avoid having to share it. Most directly, a larger share of income received publicly is shared with others (Boltz et al., 2019). Public income is also spent more quickly, likely to pre-empt future requests for help (Goldberg, 2017). While I cannot directly test for these effects with my data, the investment results in section 4 of the revised paper suggest that the income hiding I measure in the lab is indeed leading to real-world distortions.

Finally, there is a sense in which some social proscriptions on how income should be spent can function as a kinship tax. Take, for example, spending which has local positive spillovers. A man's kin group may feel they benefit if he pays his child's school fees instead of buying cigarettes. In that sense, the redirection of consumption is a way of 'taxing' his income by inducing him to spend it in socially beneficial ways. Importantly, this type of pressure would lead to productive distortions that parallel those resulting from a pressure to share income.

D.2.4 Visibility of income

The framework linking WTP to hide income and kinship taxation implicitly assumes that the visibility of income to one's network goes from 0% to 100% if it is announced. In reality, this is unlikely to be true. One particular concern is that participants may be concerned that income they pay to hide in the lab may nonetheless be revealed to others.

Before the end of each experimental session, participants were asked whether they believed that money received 'in secret' in the lab would remain secret or whether others would find out. 94% of participants believed it would remain hidden. Notably, the 6% of participants who did *not* were far less likely to have chosen to pay to hide. When asked why they thought others would find out about the money, the most common response by far is that the recipient would end up telling others. None mentioned an inability to keep cash hidden, and only 1% (16 of 1804) mentioned that others would observe the resulting consumption. For example, one answered that the reason the money would not remain secret is "Because people know that I don't have money, then if I receive money in secret they will know me."

D.3 'Extreme' values of WTP

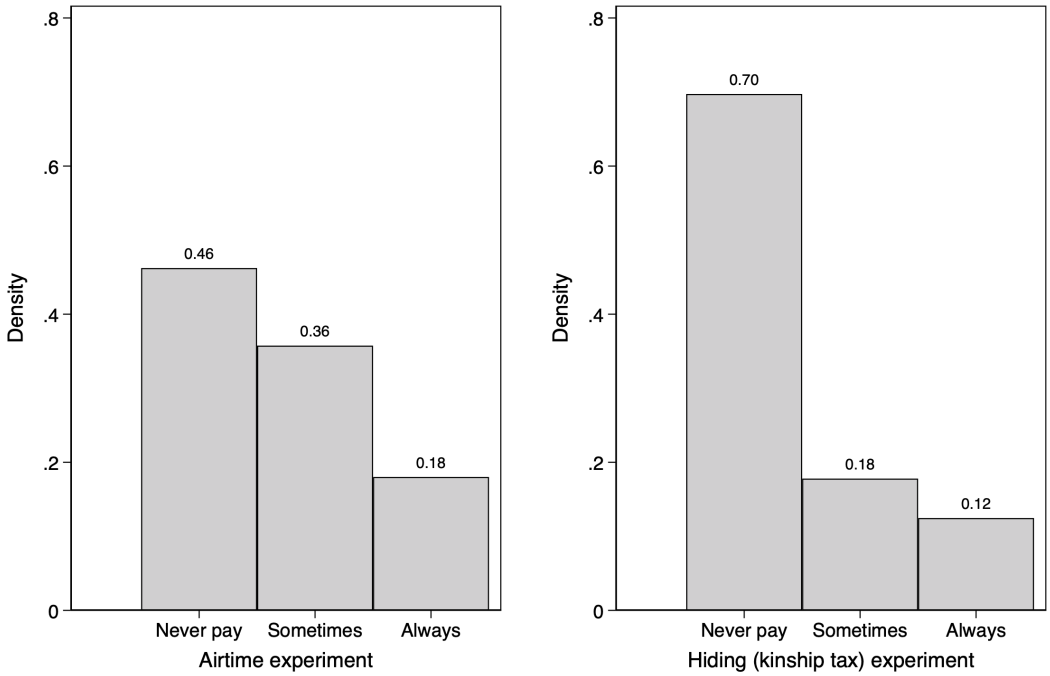
This section considers whether the distribution of willingness-to-pay to hide income in the lab experiment is a product of misunderstanding or confusion on the part of participants.

D.3.1 Interior choices in the practice game

Behavior in the practice 'airtime' game provides evidence that extreme values of WTP to hide are not simply an artefact of the lab. This was played before the hiding game and had real payoffs. It followed an identical structure to the hiding game, including making eight binary choices with the same (proportional) increments in price and having a lottery to determine which of the eight choices was selected. Figure D.4 shows for both the airtime and hiding experiments the distribution of participants who chose either never to pay, to sometimes pay, or to always pay. In the case of the airtime game, participants were paying for liquidity by converting an airtime card into cash, at a cost.

I find that the fraction of participants who chose an interior WTP in the airtime multiple price list is significantly higher than for the hiding game. While only 18% of the sample chose interior allocations in the hiding game, the share was 36% in the airtime game. (Note that this share includes those who hide only when the cost for doing so is zero.) That is, a far greater share of participants chose to 'sometimes' pay in the airtime game, rather than always or never. As you suggest, this is inconsistent with the idea that choosing to always or never pay in the price list experiment is due to comprehension problems. This is especially

Figure D.4: Fraction of participants who chose interior and extreme values in airtime and hiding experiments



Notes: Each bar represents the fraction of participants in the lab experiment who chose to either never pay at any price (including a price of 0), those who paid at some prices but not others, and those who chose to pay at all eight prices. The airtime experiment refers to the (incentivized) 'practice' game which asked participants whether they were willing to pay for liquidity by converting an airtime card into cash. The stakes were 1/10 the stakes of the hiding experiment, which allowed participants to pay to hide $5 at various prices.

true since (a) the airtime experiment was done prior to the hiding experiment, whereas we would expect comprehension problems to decrease with experience, and (b) the stakes were much higher for the hiding game (10x larger) which should have led participants to make their decisions more carefully.

D.3.2 Hiding when cost is zero

Another piece of evidence comes from the decision to hide when the price is zero. Figure B.1 shows the share of male and female participants who chose to pay to hide for each of the eight decisions they made. The largest dropoff is when the cost goes from 0% to 10%. People who chose to hide at a cost of 0% but not a cost of 10% are inferred to have a kinship tax rate in the range [0, 0.1) which, since it includes zero, I classify as being in the category of not paying to hide income. That the dropoff in demand is highest near a price of zero is consistent with findings across many settings (e.g. Berry et al. (2020)).

As further evidence, I also test in Table D.6 whether measures and correlates of cognitive ability (schooling, Raven's score, entrepreneur) predict who chooses these extreme values. The puzzle is presumably why, conditional on paying at some price, so many choose to pay up to the maximum price. Therefore I focus on the sample of participants who chose to pay to hide at least once. That is, I compare the 12% of participants who chose the extreme value (always pay to hide) with the 18% of participants who chose interior values (those who hide at some prices but not others). Columns 1-3 include participants who (only) chose to pay to hide when the cost was zero as being 'sometimes hiders', while columns 4-6 exclude these participants and focus on those willing to pay a strictly positive price. While none of the coefficients are statistically distinguishable from zero, they are mostly positive. This is the opposite of what we would expect if being an 'always hider' were a comprehension problem, and if such problems were decreasing in cognitive ability.

D.3.3 Level of understanding

The final piece of evidence comes from the subjective coding enumerators gave as to how well participants understood the experiment. In 91% of cases enumerator coded participants as having "good" or "excellent" understanding, while 9% were coded as "not so good" and less than 0.3% were "very bad". Importantly, I show in Figure D.5 that the fraction of participants making extreme decisions is not different for those who showed relatively low understanding (coded as anything below 'good'). While the share of those with low comprehension who choose interior values is slightly lower, this is not surprising given the correlation between positive kinship tax rates and ability. Notice that the fraction who

Table D.6: Always and Sometimes payers (Hiding experiment)

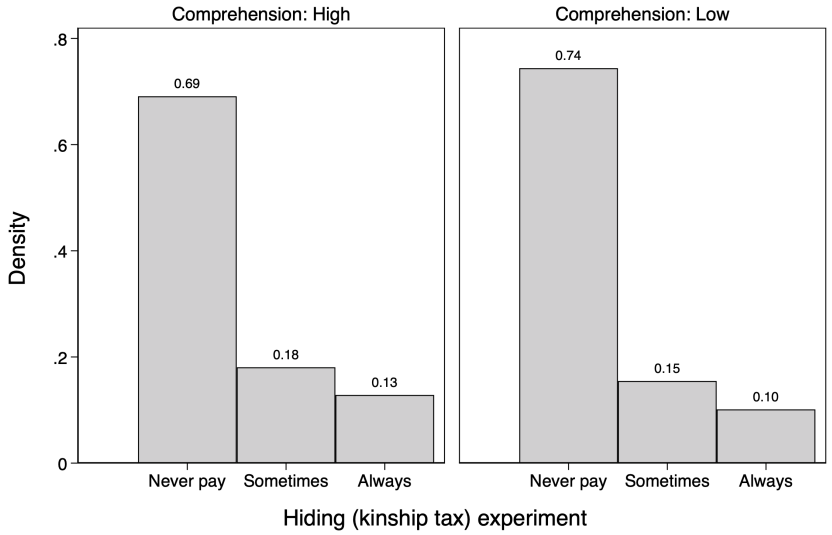
	(1) Always pay	(2) Always pay	(3) Always pay	(4) Always pay	(5) Always pay	(6) Always pay
Female	0.0401	0.0244	0.000892	0.0308	0.00701	-0.0152
	(0.0498)	(0.0506)	(0.0554)	(0.0570)	(0.0586)	(0.0656)
Schooling (years)	0.0109	0.00864	0.00723	0.00599	0.00183	0.000750
	(0.00963)	(0.00973)	(0.00991)	(0.0105)	(0.0107)	(0.0109)
Raven's score (SD)	-0.00498	-0.00307	-0.00713	-0.0126	-0.00559	-0.0111
	(0.0221)	(0.0231)	(0.0233)	(0.0257)	(0.0261)	(0.0265)
Microentreprise owner	0.0430	0.0383	0.0348	0.0205	0.0187	0.0220
	(0.0537)	(0.0542)	(0.0548)	(0.0602)	(0.0597)	(0.0610)
Observations	517	517	517	403	403	403
Mean of dependent variable	0.410	0.410	0.410	0.526	0.526	0.526
Include sample with $0 \leq t < 10\%$	X	X	X			
Experimental session controls		X	X		X	X
Individual controls			X			X

Notes: Robust standard errors in parentheses. Each column is an OLS regression with a dependent variable equal to 1 if the participant chooses to pay to hide at all eight prices (0% to 70%). The sample in this table excludes participants who did not pay to hide at any price (chose not to pay for all eight decisions). Columns with "Include sample with $0 \leq t < 10\%$" includes participants who paid to hide only when the cost was zero. The remaining columns only include participants who chose to hide at least once when the price was strictly positive.

always pay to hide is also lower for those with low comprehension. Conditional on paying to hide at least once, the proportion who always pays to hide is 42% and 40% for participants with high and low comprehension, respectively.

To summarize, the evidence suggests that the choice of extreme values (either unwilling to pay to hide at any price or willing to pay at all prices up to 70%) is not driven by comprehension problems. First, I show that in the incentivized low-stakes airtime WTP elicitation, the fraction of participants who made an interior decision was twice as high as the hiding WTP elicitation. Second, choosing to pay at any price is, relative to making an interior decision, not correlated with lower levels of ability. Third, while they are coded as having no kinship tax, a substantial number of participants who chose to hide when the cost was zero. Finally, participants coded as having higher comprehension of the game are not more likely to make an interior choice, relative to always paying.

Figure D.5: Fraction of participants who chose interior and extreme values, by comprehension



Notes: Each bar represents the fraction of participants in the lab experiment who chose to either never pay at any price (including a price of 0), those who paid at some prices but not others, and those who chose to pay at all eight prices. Participants are divided by how well their enumerator judged them to have understood the experiment. Those coded as "good" or "excellent" are categorized into High comprehension, whereas those coded as "not so good" or "very bad" are categorized into Low comprehension.

E Appendix: Field experiment (RCT)

E.1 Selection into lab experiment participation

To judge external validity, it is useful to know how the lab sample was drawn from the larger population. Unfortunately, participants in the cash grant RCT ("CT participants") were not chosen to be representative of their villages, as discussed in Section 2.2. Without data on non-participants, I cannot know exactly how important these differences are. However, within each village the share of households with a CT participant is between 10 and 26%.

What my data can speak to is the selection of CT participants into the lab-in-the-field experiment. All data in Table E.1 are household characteristics from the baseline survey of the cash transfer study. Table E.1 shows that people who participated in the lab-in-the-field experiment do indeed differ systematically from the CT participants who did not.

CT participants who chose to take part of the lab experiment are younger and come from households which are less likely to be female-headed, and are larger. Their primary source of household income is more likely to be a household-owned business, wages from casual labor, or farming. It is less likely to be from livestock holdings. This is consistent with what people told us when we asked why certain CT participants could not be found. The most common response was that they were grazing their livestock away from the village. People whose income comes from a business, from casual labor or from farming are presumably less likely to be in this situation.

Household income seems to be comparable, though participants have slightly more assets. This is also consistent with the livestock explanation: three of the six possible assets are primarily useful for people whose income does not come from livestock. They also live closer to their local school, which is reasonable given that the lab experiments were carried out in said schools, and a not-insignificant number of households live a fair distance from their school.

Table E.1: Household characteristics of CT participants

	(1) Non-participant (lab)	(2) Participant (lab)	(3) p-value of diff.
Age of beneficiary	43.347	39.506	0.000
Female beneficiary	0.739	0.790	0.014
Female head	0.356	0.316	0.089
HH members	6.125	6.426	0.004
HH members above 16	2.552	2.493	0.304
Somali	1.000	0.996	0.173
Income source: Business	0.105	0.154	0.006
Income source: Livestock	0.683	0.630	0.029
Income source: Casual labor	0.115	0.151	0.043
Income source: Farming	0.006	0.018	0.048
Household income (monthly)	66.939	68.103	0.604
Education expenditure	4.547	4.664	0.748
Number of assets	0.962	1.145	0.001
Distance from school (hrs)	0.377	0.327	0.000
Safe walk to school	0.941	0.954	0.240
N	505	1806	
Proportion	0.219	0.781	

Notes: All data from baseline survey of the cash transfer RCT, mid-2013, before being randomized into control or treatment. Column 1 includes CT participants who did not participate in the lab experiment. Column 2 are all those CT participants who did take part in the lab experiment. 'Beneficiary' refers to the household member assigned to receive the cash transfers (if randomized into treatment group). Each household could only have one beneficiary. Income source values are 1 if the category is the main source of income. ('Other' is excluded.) Household income and expenditure are in USD, converted at the rate of 100 KES to 1 USD. Number of assets is from a list of six potential assets: mobile phone, radio, wheelbarrow, plough, donkey cart, and bicycle. 'Distance from school' is number of hours walking to nearest primary school. 'Safe walk to school' is equal to 1 if the respondent considers it safe for children to walk to school alone. 'Somali' is equal to 1 if the participant is Somali, 0 otherwise.

E.2 RCT Treatment status balance

All participants in the lab experiment are also participants in the cash transfer RCT ("CT participants"). Their treatment status reflects whether they were randomized to receive series of monthly cash grants that totalled $540 over 18 months, as described in Section 4. Table E.2 tests whether the lab experiment participants in the treatment and the control group are balanced on two sets of characteristics. Panel A in Table 1 presents the first set of characteristics, measured during the lab experiment survey. These are mostly fixed individual characteristics which are unlikely to be affected by the treatment itself. Panel B presents characteristics measured during the RCT baseline survey, prior to randomization to the control or treatment groups. Both sets of variables are generally balanced, with only a few that show a significant difference. Differences between the treatment and control group are significant for two variables (age at baseline, and age during the lab) at the 5% level, and an additional two are significant at the 10% level (share who are single, and the proportion who say it is safe for children to walk from their home to the nearest primary school). In all cases the magnitude of the difference is small. Control group participants are about 1.5 years older than those in the treatment group in both the baseline and lab surveys (mean ages are higher in the lab survey simply because this survey was conducted approximately 18 months after the baseline).

Since the analysis in Section 4 exploits variation in the treatment and control groups for just those participants who are operate a firm, I also present balance for this sub-sample. Table E.3 includes the same set of variables, but restricts the sample to entrepreneurs. The control and treatment groups are again balanced on covariates. Years of islamic schooling are slightly higher in the treatment group (0.3 additional years, significant at 10% level), and the share of participants who are Somali slightly higher (1 percentage point higher, significant at 10% level).

Table E.2: Balance

	(1) Control	(2) Treatment	(3) p-value of diff.
(A) Characteristics from lab survey			
Female	0.721	0.701	0.438
Age	42.017	40.456	0.040
Married (living with spouse)	0.793	0.808	0.518
Separated	0.070	0.072	0.882
Single (never married)	0.002	0.012	0.087
Divorced	0.040	0.033	0.491
Widowed	0.095	0.076	0.211
Ravens score (%)	0.384	0.404	0.212
Education (yrs)	0.217	0.338	0.124
Islamic educ (yrs)	0.948	1.002	0.472
Num children in household	5.733	5.639	0.481
Num own children	6.035	5.827	0.200
Number of siblings	7.551	7.360	0.414
Num parents (living)	0.975	1.001	0.557
(B) Characteristics from RCT baseline survey			
Age of beneficiary	40.699	39.180	0.047
Female beneficiary	0.803	0.787	0.510
Female head	0.330	0.312	0.522
HH members	6.301	6.459	0.189
HH members above 16	2.445	2.506	0.365
Somali	0.997	0.996	0.764
Income source: Business	0.163	0.152	0.586
Income source: Livestock	0.625	0.630	0.860
Income source: Casual labor	0.146	0.152	0.785
Income source: Farming	0.020	0.018	0.828
Household income (monthly)	69.199	67.782	0.591
Education expenditure	4.807	4.627	0.651
Number of assets	1.223	1.123	0.126
Distance from school (hrs)	0.331	0.326	0.694
Safe walk to school	0.972	0.948	0.064
N	401	1403	
Proportion	0.222	0.778	

Notes: The RCT baseline survey was pre-treatment, while the lab survey was post-treatment. See Figure 2 for the timing of the lab and RCT baseline survey rounds. Variables from Panel A are described in Table 1. Variables from Panel B are described in Table E.1. Column 1 includes all participants in the lab experiment who were in the control group of the cash transfer RCT, and column 2 are those who were in the treatment group.

Table E.3: Balance (sample of entrepreneurs only)

	(1) Control	(2) Treatment	(3) p-value of diff.
(A) Characteristics from lab survey			
Female	0.824	0.761	0.227
Age	39.224	39.250	0.985
Married (living with spouse)	0.776	0.761	0.768
Separated	0.071	0.076	0.867
Single (never married)	0.000	0.022	0.171
Divorced	0.071	0.069	0.956
Widowed	0.082	0.072	0.763
Ravens score (%)	0.451	0.429	0.507
Education (yrs)	0.329	0.605	0.251
Islamic educ (yrs)	0.776	1.054	0.086
Num children in household	5.506	5.786	0.355
Num own children	5.776	5.562	0.523
Number of siblings	8.271	8.069	0.715
Num parents (living)	1.165	1.112	0.553
(B) Characteristics from RCT baseline survey			
Age of beneficiary	41.013	39.286	0.226
Female beneficiary	0.850	0.829	0.667
Female head	0.362	0.345	0.779
HH members	6.338	6.303	0.888
HH members above 16	2.575	2.627	0.744
Somali	0.988	1.000	0.076
Income source: Business	0.287	0.317	0.615
Income source: Livestock	0.450	0.397	0.401
Income source: Casual labor	0.200	0.226	0.623
Income source: Farming	0.025	0.012	0.404
Household income (monthly)	72.159	75.377	0.591
Education expenditure	4.211	5.224	0.305
Number of assets	1.125	1.167	0.739
Distance from school (hrs)	0.321	0.311	0.662
Safe walk to school	0.988	0.964	0.291
N	85	276	
Proportion	0.235	0.765	

Notes: This table excludes all participants who are not entrepreneurs. The RCT baseline survey was pre-treatment, while the lab survey was post-treatment. See Figure 2 for the timing of the lab and RCT baseline survey rounds. Variables from Panel A are described in Table 1. Variables from Panel B are described in Table E.1. Column 1 includes all participants in the lab experiment who were in the control group of the cash transfer RCT, and column 2 are those who were in the treatment group.

E.3 Kinship taxation measured post-treatment

The interpretation of the interaction of treatment status and kinship taxation in the lab experiment is problematic given that behavior in the lab experiment is potentially itself a result of the cash transfer treatment status. There is unfortunately no way to directly fix this problem. However, I show in Table E.4 that rates of kinship taxation do not vary with treatment status. That is, participants in the lab experiment pay to hide income at the same rate whether they are in the treatment or control group for the cash transfer RCT. This suggests that kinship tax rates are not affected by treatment status, and hence can plausibly be treated as an independent characteristic.

Table E.4: Does treatment affect kinship taxation

	(1) Kintax	(2) Kintax	(3) Kintax	(4) Kintax	(5) Kintax
Treatment (Cash transfer)	0.00519	0.00257	0.000618	-0.00663	-0.00908
	(0.0237)	(0.0234)	(0.0235)	(0.0240)	(0.0253)
Female		-0.110	-0.105	-0.0667	-0.0675
		(0.0230)	(0.0228)	(0.0264)	(0.0287)
Observations	1804	1804	1804	1725	1562
Mean of dependent variable	0.234	0.234	0.234	0.234	0.230
Experimental controls			X	X	X
Individual controls				X	X
Baseline controls					X

Notes: Robust standard errors in parentheses. Each column is an OLS regression with a dependent variable equal to 1 if the participant pays to hide income in the lab experiment, and equal to 0 otherwise. Individual controls consist of age (quadratic), schooling, Raven's score, number of siblings and of children living in the household. Experimental controls are described in Table 2. Baseline controls consist of baseline household size, sex of household head, and dummies for income from various sources, as in Table E.1. All regressions include location (village) fixed effects.

However, despite having the same average rates of kinship taxation, one may worry that the cash grants increased kinship tax rates for some subsets of participants and reduced them for others, leaving the net change equal to zero. To test for this, I interact treatment status with a range of characteristics which are either fixed or which were measured pre-treatment. Table E.5 consistently shows small and statistically insignificant effects (at the 10% level), for both for the main 'treatment' dummy and for the interaction terms. While I cannot fully rule out that the cash transfer treatment had an effect on kinship tax rates, the evidence in Tables E.4 and E.5 suggests that if it did have any effect it was small.

Table E.5: Does treatment affect kinship taxation

	(1) Kintax	(2) Kintax	(3) Kintax	(4) Kintax	(5) Kintax	(6) Kintax	(7) Kintax
Treatment (Cash transfer)	0.0000997	0.00157	-0.00215	0.00214	-0.000538	-0.000483	-0.00870
	(0.0236)	(0.0238)	(0.0238)	(0.0238)	(0.0252)	(0.0252)	(0.0254)
Female	-0.139						-0.108
	(0.0495)						(0.0638)
Treat × Female	0.0429						0.0363
	(0.0554)						(0.0704)
Age		-0.00142					-0.00288
		(0.00140)					(0.00164)
Treat × Age		0.00206					0.00275
		(0.00163)					(0.00189)
Total schooling (years)			0.0351				0.0241
			(0.0116)				(0.0137)
Treat × Schooling			-0.0117				-0.00671
			(0.0130)				(0.0153)
Married (living with spouse)				0.0272			-0.0226
				(0.0494)			(0.0589)
Treat × Married				0.0234			0.0357
				(0.0562)			(0.0666)
Household size					-0.00292		0.000335
					(0.0113)		(0.0115)
Treatment × Household size					0.00956		0.00601
					(0.0126)		(0.0127)
Female head						-0.0282	-0.00251
						(0.0476)	(0.0505)
Treatment × Female head						0.00593	0.00628
						(0.0532)	(0.0563)
Joint significance of Treatment and interaction effects (p-value)	0.734	0.445	0.665	0.915	0.749	0.994	0.873
Observations	1804	1804	1804	1804	1634	1635	1634
Mean of dependent variable	0.234	0.234	0.234	0.234	0.232	0.232	0.232
Experimental controls	X	X	X	X	X	X	X

Notes: Robust standard errors in parentheses. Variables interacted with the 'treatment' variable are all de-meaned. 'Household size' and 'Female head' are measures from the (RCT) baseline survey. All other variables are from the lab experiment and its accompanying survey. All regressions include location (village) fixed effects.

F Appendix: Experimental instructions

Text in italics are instructions for the enumerator that were not read aloud. **Bold text indicates skipping logic.**

As a reward for answering my questions and cleaning the beans, I will now give you two chances to receive some money. The first time you might receive airtime or some coins, the second time you might receive a few hundred shillings.

F.1 Airtime

Read exactly from script. Do not add information that is not in script unless the participant asks you a question.

You will play a Lottery where you may win some Safaricom airtime or some coins. As you can see, here are some coloured pencils. Soon we will do a lottery. I will ask you to decide on one colour then put all of them in this basket and choose one without looking. If you draw same colour as you decided, you will win the 50 Sh airtime card or coins as a prize. If you draw any other colour you win nothing. But first, we will play the Ticket Game to see whether the prize of the lottery will be coins or the airtime card.

So, we will do two games. First we will play the Ticket Game to decide whether the prize is coins or airtime. Second we will do a Lottery with the coloured pencils to see whether you win the prize

F.1.1 Ticket game

Here is what we will do for the Ticket Game: As you can see, here are eight (8) tickets with different numbers on them. Each ticket gives you a choice between the airtime card and some value of coins you might win, for example 40 Sh. For each ticket, you must tell me whether you prefer receiving the value on the ticket in cash, or the airtime card. After you make this decision for each of the tickets, I will put each ticket in this basket and ask you to draw one without looking.

The prize in the lottery will be either the airtime card or the value in cash on the ticket you pick, depending on the decision you made for that ticket. You cannot change your choice after you draw from the basket – your decisions are final. The best strategy for you is to make your decision for each ticket based on whether you really would prefer airtime or the cash, without thinking about the other tickets. Do you have any questions?

One of the following two sets of questions was chosen randomly by the data entry software.

Alternative 1 *Start with the 50 Sh ticket, and show it to the participant*

- If the prize was either this 50 Sh airtime card or 50 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 45 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 40 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 35 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 30 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 25 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 20 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 15 Sh coins, which would you choose?

Alternative 2 *Start with the 15 Sh ticket, and show it to the participant*

- If the prize was either this 50 Sh airtime card or 15 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 20 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 25 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 30 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 35 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 40 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 45 Sh coins, which would you choose?
- If the prize was either this 50 Sh airtime card or 50 Sh coins, which would you choose?

If the data entry software detects an inconsistent decision: One of your decisions does not make sense, and means that your participant prefers a smaller amount of cash to a larger amount of cash. *Go back and confirm each of the eight decisions with the participant.*

If the data entry software detects decisions are still inconsistent: *You have not fixed the mistake. Confirm that you have asked the questions a second time and you are sure that this is what the participant wants.*

If participant chose 15 Sh coins instead of 50 Sh airtime card: Why did you decide 15 Cash instead of the Airtime card?

If participant chose 40 Sh coins instead of 50 Sh airtime card, but not 15 Sh coins instead of 50 Sh airtime card: Why did you decide 40 Cash instead of the Airtime card?

If participant chose 50 Sh airtime card instead of 40 Sh coins: Why did you decide the Airtime card instead of 40 Cash?

Now you will pick one ticket from the basket. Please draw one ticket, quickly and without looking.

Which ticket did they pick from the basket? According to the eight decisions you selected, their choice was [...] *for the ticket you drew.*

Based on the ticket you drew, the prize could either be [...] Sh in coins, or the airtime card. Earlier you decided for this ticket you would rather have the [*coins*/*airtime*] and that is what we will use. You cannot change your decision now.

F.1.2 Lottery

Now we have completed the Ticket Game, and are ready to do the lottery. Here is what we will do for the Lottery: As you can see, here are eleven coloured pencils. They will all go in this basket. These six pencils have bright colours, and you can decide one of these to be the winning pencil.

If chose airtime over coins for ticket that was drawn: Remember that during the Ticket Game, you decided you would rather get the 50 Sh airtime card than [...] Sh. If you draw the colour you decided, you will win the 50 Sh airtime card as a prize. If you draw another colour you win nothing.

If chose coins over airtime for ticket that was drawn: Remember that during the Ticket Game, you decided you would rather get the [...] Sh coins than the 50 Sh airtime card. If you draw the colour you decided, you will win [...] Sh coins as a prize. If you draw another colour you win nothing.

Now, choose one of the six bright colour pencils. You cannot change your mind later. *Add the 11 pencils to the basket, shake it, and ask the participant to draw only one. Did they win or lose the lottery?*

If drew the winning colour, and prize was airtime: *Give them the prize, which is an airtime card*

If drew the winning colour, and prize was coins: *Give them the prize, which is* [...] *Sh in coins*

We are going to play this game again, with a larger prize. It is important you understand how the game is played. Do you have any questions about the game?

F.2 Hiding

Read exactly from script. Do not add information that is not in script unless the participant asks you a question.

You will play a Lottery where you may win some money. Everything we do now will be the same as we just did, but with different prizes Soon we will do another lottery. Again you will choose one colour then choose one from the basket without looking. If you draw same colour as you decided, you will receive up to 500 Sh and this will be announced to the other

participants in your group. If you draw any other colour you win nothing. But first, I would like to offer you the chance to receive money without announcing it, using the Ticket Game

So, we will do two games. First we will play the Ticket Game to decide whether you might receive money that is announced or money that is not announced. Second we will do a Lottery with the coloured pencils to see whether you win the prize

F.2.1 Ticket game

Here is what we will do for the Ticket Game: As you can see, here are eight (8) tickets with different numbers on them. Each ticket gives you a choice between the 500 Sh ANNOUNCED and some money NOT ANNOUNCED, for example 400 Sh. After you make this decision for each of the tickets, I will put each ticket in this basket and ask you to draw one without looking.

The prize in the lottery will be either 500 Sh ANNOUNCED or the amount NOT ANNOUNCED on the ticket you pick, depending on the decision you made for that ticket. You cannot change your choice after you draw from the basket – your decisions are final. The best strategy for you is to make your decision for each ticket based on whether you really would prefer ANNOUNCED or NOT ANNOUNCED, without thinking about the other tickets. Do you have any questions?

One of the following two sets of questions was chosen randomly by the data entry software.

Alternative 1 *Start with the 500 Sh ticket, and show it to the participant*

- If the prize was either 500 Sh ANNOUNCED or 500 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 450 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 400 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 350 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 300 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 250 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 200 Sh NOT ANNOUNCED, which would you choose?

- If the prize was either 500 Sh ANNOUNCED or 150 Sh NOT ANNOUNCED, which would you choose

Alternative 2 *Start with the 150 Sh ticket, and show it to the participant*

- If the prize was either 500 Sh ANNOUNCED or 150 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 200 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 250 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 300 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 350 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 400 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 450 Sh NOT ANNOUNCED, which would you choose?
- If the prize was either 500 Sh ANNOUNCED or 500 Sh NOT ANNOUNCED, which would you choose?

If the data entry software detects an inconsistent decision: One of your decisions does not make sense, and means that your participant prefers a smaller amount of NOT ANNOUNCED money to a larger amount of NOT ANNOUNCED money. *Go back and confirm each of the eight decisions with the participant.*

If the data entry software detects decisions are still inconsistent: *You have not fixed the mistake. Confirm that you have asked the questions a second time and you are sure that this is what the participant wants.*

If participant chose 150 Sh NOT ANNOUNCED instead of 500 Sh ANNOUNCED: Why did you decide 150 Sh NOT ANNOUNCED instead of 500 Sh ANNOUNCED?

If participant chose 400 Sh NOT ANNOUNCED instead of 500 Sh ANNOUNCED, but not 150 Sh NOT ANNOUNCED instead of 500 Sh ANNOUNCED: Why did you decide 400 Sh NOT ANNOUNCED instead of 500 Sh ANNOUNCED?

If participant chose 500 Sh ANNOUNCED instead of 400 Sh NOT ANNOUNCED: Why did you decide 500 Sh ANNOUNCED instead of 400 Sh NOT ANNOUNCED?

Now you will pick one ticket from the basket. Please draw one ticket, quickly and without looking.

Which ticket did they pick from the basket? According to the eight decisions you selected, their choice was [...] *for the ticket you drew.*

Based on the ticket you drew, the prize could either be [...] not announced, or [...] announced. Earlier you decided for this ticket you would rather have the [*announced/notannounced*] and that is what we will use. You cannot change your decision now.

F.2.2 Lottery

Now we have completed the Ticket Game, and are ready to do the lottery. Here is what we will do for the Lottery: As before, here are eleven coloured pencils. They will all go in this basket. These six pencils have bright colours, and you can decide one of these to be the winning pencil.

If chose 500 Sh ANNOUNCED over NOT ANNOUNCED for ticket that was drawn: If you draw the colour you decided, you will win the 500 Sh ANNOUNCED as a prize. If you draw another colour you win nothing. Remember that during the Ticket Game, you decided you would rather get the 500 Sh ANNOUNCED than [...] Sh NOT ANNOUNCED.

If chose NOT ANNOUNCED over 500 Sh ANNOUNCED for ticket that was drawn: If you draw the colour you decided, you will win the [...] NOT ANNOUNCED as a prize. If you draw another colour you win nothing. Remember that during the Ticket Game, you decided you would rather get the [...] Sh NOT ANNOUNCED than the 500 Sh ANNOUNCED.

If someone in this group were to receive money without us announcing it and they did not tell anyone, would others find out? **If answer yes:** Why do you think it would not stay secret?

Now, choose one of the six bright colour pencils. You cannot change your mind later. *Add the 11 pencils to the basket, shake it, and ask the participant to draw only one. Did they win or lose the lottery?*

If drew the winning colour, and prize was airtime: *Give them the prize, which is 500 Sh ANNOUNCED*

If drew the winning colour, and prize was coins: *Give them the prize, which is* [...] *Sh NOT ANNOUNCED*

Ask this only after giving the secret money: Do you think anyone has seen me give you the money? If yes please explain.