How Fair Shares Compare: Experimental Evidence from Two Cultures*

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Abstract

We use a suite of economic experiments to study social preferences governing the distribution of earned and unearned income in rural villages in western Kenya. Our experiments vary the extent to which income depends on individual effort while holding other aspects of the economic environment constant. Results suggest that, in rural villages, the relative weight placed on others does not depend on the extent to which those individual increased the total surplus through their own effort. However, more educated subjects and those drawn from villages closer to the road do reward others for their effort; their allocation decisions are consistent with models of reciprocity.

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1 Introduction

People are sometimes willing to sacrifice their own payoffs to help strangers, reward helpful actions by others, and punish uncooperative behavior; the social preferences underlying such behavior have been documented in many societies by experimental economists. Social preferences are likely to play a particularly important role in the economic lives of those living poor, rural communities in the developing world: in such settings, people often rely on neighbors and relatives to help them cope with negative shocks, and voluntary transfers between households are common. This has prompted scholars to describe the rural village as a "moral economy" in which individuals are motivated by concern for their neighbors' welfare and aversion to inequality within the community.[1] Scott (1976), for example, highlights the primacy of the universal right to subsistence in the moral code of poor agricultural households, while Platteau (2000) argues that many traditional societies are characterized by egalitarian norms which discourage individual wealth accumulation. Though standard models of pure self-interest can partially explain transfers between households who interact repeatedly (cf. Coate and Ravallion 1993), economic theory suggests that individual social preferences can both directly motivate some transfers and shape the space of enforceable informal insurance contracts (Foster and Rosenzweig 2001). Existing experimental evidence suggests substantial heterogeneity in individual social preferences and conceptions of fairness; much of this variation occurs within societies (cf. Fisman et al. 2014a), but we also observe large cross-cultural differences (Henrich et al. 2010). It is therefore important to provide a positive characterization the social preferences of individuals living in poor, rural communities, as they are a key input into any model of informal insurance or measure of social welfare.

In this paper, we use experimental economic methods to study the social preferences of villagers in rural western Kenya, focusing on the differential treatment of earned and unearned income. A key component of the egalitarian conception of fairness is the idea that all inequality is unfair (Cappelen et al. 2007); this distinguishes egalitarianism from theories of justice that view inequality as fair if it is caused by differences in individual ability or effort (Konow 2003). Experimental studies in university labs in the developed world document what Fahr and Ir-

[1] The term "moral economy" was popularized in this context by Scott (1976). See Fafchamps (1992) and Ravallion and Dearden (1988) for examples of its use in the economics literature.

lenbusch (2000) term an "earned property rights" effect: subjects allocate more to those who have increased aggregate income through individual effort.[2] Such findings suggest that a non-negligible proportion of university students in rich countries hold conceptions of fairness that distinguish between different sources of or justifications for inequality; in other words, they are not egalitarians. We ask whether similar earned property rights effects are observed in poor, rural communities with strong traditions of solidarity and mutual assistance. Arguments presented in, for example, Platteau (2000) suggest that poor villagers engaged in subsistence agriculture may view the distinction between earned and unearned income differently:

> "Inasmuch as 'work, in the sense of persistent individual effort, is never recognized as the reason of success' and success is attributed entirely to 'luck' and 'is never believed to be brought about or furthered by personal effort and initiative' [Rogers, 1969: 118–9], private appropriation of persistent surpluses is deemed to be unfair. In other words, a worldview that tends to consider any income as essentially 'unearned' naturally leads to a progressive concept of justice according to which rich people ought to share their income with others."
>
> — Platteau (2000, p. 198)

This characterization of poor, rural communities in the developing world motivates the present study: we test the extent to which individuals living in agricultural village economies hold social preferences characterized by the egalitarian ideal of fairness which does not distinguish between different sources of inequality.

Our experimental design measures social preferences in benchmark treatments where luck alone governs income, and compares them to the social preferences revealed in treatments where income is determined by individual effort. The experimental setting allows us to vary the

[2] See, for example, Fahr and Irlenbusch (2000), Cherry (2001), Cherry et al. (2002), and List (2007) for evidence that university student subjects treat earned and unearned income differently. In a similar vein, Hoffman et al. (1994) and Hoffman et al. (1996) show that dictators are less generous when they have earned the right to make the allocation decision. Cappelen et al. (2007), Cappelen et al. (2010), and Cappelen et al. (2013a) arrive at a similar conclusion using a different experimental design, documenting the prevalence of fairness ideals that dictate that individuals are entitled to a share of total output that reflects their relative contributions of effort and ability. Notably, Cappelen et al. (2013b) observe similar patterns among university students in Uganda and Tanzania, suggesting that young elites in developing countries may hold conceptions of fairness similar to those held by educated young people in the developed world.

extent to which income depends on luck as opposed to effort while holding other aspects of the economic environment — and the individual attributes of those making and receiving transfers — constant. We then contrast the choices made by subsistence farmers in rural Kenya with those of individuals drawn from a standard university lab subject pool at a top US university. We view the university lab subject pool as a particularly interesting comparison population because of the tremendous importance of individual ability and hard work in competitive academic environments. Additionally, the use of a standard university student subject pool allows us to demonstrate that our experimental design replicates the earned property rights effects shown in other experiments involving student subjects.

Our experimental design includes four distinct treatments, each a modified dictator game in which one subject (the "dictator") divides a budget between herself and an anonymous "recipient" (another subject chosen at random from the same experimental session). Our experimental treatments differ along two dimensions: how the budget is generated, and who decides how to divide it. In each of the treatments we consider, the size of the dictator's budget is determined by the actions of one of the subjects — either the dictator or the recipient. In LUCK treatments, one subject rolls a die, and the outcome of the roll determines the size of the budget. In EFFORT treatments, one subject engages in a real effort task for which she is paid a piece rate; that subject's earnings constitute the dictator's budget. We also vary whether the subject making the allocation decision (i.e. the dictator) is the same person who won or earned the budget. In GIVING treatments, dictators divide money that they themselves won or earned; in TAKING treatments, dictators divide money won or earned by the recipient. Our cross-cut design involves a total of four experimental treatments: LUCK-GIVING, LUCK-TAKING, EFFORT-GIVING, and EFFORT-TAKING. Our analysis examines the differences between allocations in the LUCK and EFFORT treatments. If subjects drawn from rural villages hold egalitarian ideals of fairness, the conditions of production (luck vs. effort) will not impact allocation decisions. Alternative models of social preferences predict different types of earned property rights effects.

We report three main findings. First, we find only weak evidence of respect for earned property rights in our Kenyan village sample.[3] Subjects allocate recipients somewhat less in

[3]This result was first reported in Jakiela (2011). That paper explores the sociodemographic correlates of dictator game giving in Kenya and the United States, and also documents the absence of earned property rights effects in the Kenyan village sample. The analysis presented here goes substantially further, exploring heterogeneity

the Effort-Giving treatment than in the Luck-Giving treatment, but the effect is only marginally significant. There is no evidence of respect for the earned property rights of others; instead, dictators allocate recipients less in the Effort-Taking treatment than in the Luck-Taking treatment. Second, our experimental design generates substantial earned property rights effects in the university student sample. Thus, the absence of effects in the Kenyan village sample reflects differences in social preferences and is not an artifact of our specific design. Finally, within the Kenyan sample, we find that both more educated subjects and those drawn from more developed communities (that are located closer to paved roads) show more respect for others' earned property rights. Specifically, they allocate recipients almost exactly the same amount, on average, in the Luck-Taking and Effort-Taking treatments, and their allocation decisions are consistent with models of reciprocity which predict that individuals will reward high effort and punish low effort. Less educated subjects and those drawn from more remote communities do not demonstrate this qualified form of respect for others' earned property rights.

Our work is most closely related to other laboratory and lab-in-the-field experiments exploring the variation in social preferences across and within societies. Surveying the many studies documenting substantial cross-cultural variation in social preferences governing the distribution of unearned income is beyond the scope of this paper; seminal contributions include Roth et al. (1991), Hermann et al. (2008), and Henrich et al. (2006, 2010). Our study extends this literature by exploring cross-cultural variation in the differential treatment of earned and unearned income; by focusing on poor, rural villagers rather than students, our work complements recent contributions by Cappelen et al. (2013b) and Barr et al. (2011).

The rest of this paper is organized as follows. In Section 2, we present our experimental design. Section 3 describes our experimental procedures and subjects, with particular emphasis on our lab-in-the-field procedures. Section 4 presents our analysis and results, and Section 5 concludes.

within the Kenyan village sample (with respect to community and individual level markers of development) and testing whether behavior in the Effort-Taking treatment is consistent with the predictions of theoretical models of reciprocity.

2 Experimental Design

Dictator games measure social preferences in the absence of strategic or reputational considerations (Kahneman et al. 1986, Forsythe et al. 1994). Within any dictator game, i (the "dictator") divides a fixed budget between herself and another player, j (we refer to this player as "the recipient"). Interactions between players are anonymous and one-shot — the recipient does not even have a choice to make. Hence, dictator games provide an unconfounded measure of individual social preferences since purely self-interested players will always allocate the entire budget to themselves.

We conduct four variants of the dictator game which differ along two dimensions: how the budget is generated, and who decides how to divide it. In all treatments, each dictator decides how to divide a budget, $m > 0$, between herself and an anonymous recipient — another subject chosen at random from the same experimental session. Dictators do not learn recipients' identities during or after the experiment. In each of the games we consider, either the dictator or the recipient takes an action that determines the size of the dictator's budget. In Luck treatments, one of the two subjects rolls a die to determine the budget size, so m depends only on chance. In Effort treatments, either the dictator or the recipient earns the budget by engaging in a simple real-effort task for which she is paid a piece rate. Luck treatments serve as a benchmark which allow us to evaluate the extent to which dictators reward effort — either their own or the recipient's — in the Effort treatments. Numerous studies suggest that university student subjects typically allocate less to recipients in settings where they have earned the money being divided, relative to settings where the dictator's budget is unearned (cf. Cherry et al. 2002). A smaller but growing body of work also suggests that many student subjects respect the "earned property rights" (Fahr and Irlenbusch 2000) of other subjects (cf. Ruffle 1998, Cappelen et al. 2007). To explore the latter issue, we also vary whether the subject making the allocation decision is the same person who won or earned the budget. In Giving treatments, subjects decide how to divide money that they either won (by rolling the die) or earned (by completing the real-effort task) themselves; in Taking treatments, subjects divide money won or earned by the recipient.[4] Our study builds on existing work by measuring

[4]Dictator games which allow for both giving and taking have been employed by List (2007), Bardsley (2008), and Fisman, Jakiela, and Kariv (2014b). Greig (2006) and Jakiela (2013) use taking (only) treatments similar to

respect for earned property rights in a population characterized by strong egalitarian traditions.

We employ a cross-cut experimental design that includes four distinct treatments: Luck-Giving, Effort-Giving, Luck-Taking, and Effort-Taking. The Luck-Giving treatment is most similar to a standard dictator game experiment: dictators divide their own unearned income. In the Luck-Taking treatment, on the other hand, the dictator divides the recipient's unearned income. In the Effort-Giving treatment, the dictator earns money and decides how to divide her earnings between herself and the recipient; and in the Effort-Taking treatment, the recipient earns the income by completing the real effort task, and the dictator decides how to divide it.

Our design creates two key dimensions of variation across treatments. First, the conditions of production differ between Luck and Effort treatments, allowing us to explore the extent to which dictators reward themselves — and others — for effort. A second source of variation is the nominal ownership of the budget. In all dictator games, the dictator owns the budget in the sense of Grossman and Hart (1986) because she controls the final budget allocation. In standard dictator games, and in our Giving treatments, the budget is also provisionally allocated to the dictator. In fact, the Giving treatments considered here go further because the dictator also generates the budget, either by rolling the die or by completing the real effort task. In the Taking treatments, on the other hand, ownership is more ambiguous: by construction, the dictator still holds decision rights; however, the recipient is responsible for generating the budget and it is provisionally allocated to her. Our cross-cut experimental design addresses the possibility that such variation in the nominal ownership of the budget may have a direct effect, allowing us to compare allocation decisions in settings where only the type of activity carried out to generate the budget differs across treatments.

3 Experimental Procedures and Data

We conduct lab experiments in two very different cultural contexts. Our main focus is our Kenyan village sample — adult Kenyans drawn from poor, rural communities. We describe our recruitment and implementation procedures in our lab-in-the-field setting in detail below. For

the one employed here.

comparison, we also replicated our experiments in a standard university lab setting in the United States. Experiments using our university lab subject pool were conducted at the Xlab at the University of California, Berkeley, using the undergraduate subject pool and following standard procedures.

3.1 Lab-in-the-Field Setting and Subject Pool

We conducted 14 experimental sessions in field laboratories which we set up in Busia District, a poor, predominantly rural area in western Kenya. We conducted four sessions of each EFFORT treatment and three sessions of each LUCK treatment. Each session took place in a different rural community located less than one hour from the district's main urban center. In each location, we recruited participants from the catchment area of a single primary school. Approximately one week before each experimental session, members of the research team worked with the school's head teacher and the local village elders to compile a list of adults between the ages of 18 and 35 residing in the community.[5] Letters of introduction explaining the presence of the research teams in the community were sent to these individuals. One day prior to each experimental session, members of the research team visited the household of each individual who had been sent an invitation letter. Potential subjects completed a short survey and were invited to attend the experimental session the following day. 548 individuals were surveyed prior to the experiment, 78 percent of whom attended the experiment. The sample also includes 118 individuals who were not surveyed prior to the experimental session; these individuals had received introductory letters, but were not at home when the survey team visited their household. Our Kenyan village sample includes a total of 533 subjects.

Summary statistics on experimental subjects are reported in Table 1. The average age among subjects in the Kenyan rural village sample is 27 (Table 1, Panel A). 41 percent of Kenyan subjects are female. 55 percent have at least a primary school education, while 20 percent have completed secondary school. The vast majority of Kenyan subjects are involved in home production: 96 percent farm their own plot, while 52 percent own at least one cow and 87

[5]We chose to target adults aged 18 to 35 because almost all adults from western Kenya who fall in that age range are at least conversant in Swahili. It was not possible to conduct sessions in multiple tribal languages at the same time, and many older adults have limited formal schooling and are only comfortable speaking their tribal languages. Because village elders were not always sure of the exact ages of potential subjects, our sample includes a small number of slightly older individuals (14 subjects aged either 36 or 37, and 2 subjects in their early forties.

percent own at least one chicken. However, 82 percent of Kenyan subjects come from households in which at least one adult member works in either the informal or the formal sector.

Relative to the Kenyan villagers, the university lab subjects are younger, better educated, and more likely to be female. The average age in the US university sample is only 20 (Table 1, Panel B), as opposed to 27 in the Kenyan village sample. 63 percent of university lab subjects are female, versus 41 percent of Kenyan subjects. 30 percent of US subjects are economics or business majors, and an additional 27 percent are majoring in another social science discipline.

3.2 Experimental Protocol

The experimental protocol used to administer lab sessions is common to all four treatments; to the extent possible, the same protocol was followed in both the lab-in-the-field setting in Kenya and the university lab sessions in the US. All experimental sessions in Kenya were held using two empty primary school classrooms;[6] experimental sessions in the US were conducted at the UC Berkeley Xlab, which was also partitioned into separate rooms. At the start of the experiment, participants were randomly assigned to one of the two rooms. Sessions began with a detailed explanation of the structure of the experiment. Experimental instructions were presented orally and further illustrated using wall posters and hypothetical scenarios acted out by members of the research team.[7] The presentation of the instructions included a trial period during which subjects were allowed to practice rolling the die (in the Luck treatments) or carrying out the piece rate task (in the Effort treatments).

The decision-making phase of the experiment occurred immediately after the instructions. Each subject was randomly matched with a recipient in the other room whose identity was not revealed during or after the experiment. Each subject had to decide how to divide a budget between herself and the matched recipient in the other room. Subjects were called outside one at a time to make their allocation decisions. Not all Kenyan subjects were literate, so the decisions of all subjects were recorded by members of the research team (in both Kenya and the US).[8]

[6] Sessions were held in the afternoon, by which time lower grades had departed school for the day leaving their classrooms vacant.

[7] Copies of the instructions are included in the Online Appendix. For the Kenyan sessions, English instructions were first translated into Swahili and then verified via back-translation.

[8] Each subject sat with a single enumerator who recorded her decisions. Before recording any decisions, enumerators quizzed subjects on the structure of the game to verify comprehension. In the few cases where participants did not fully understand the instructions, the enumerators reviewed the protocol with them before proceeding.

To ensure that budget sizes were comparable across all four treatments, we used the strategy method: for every feasible budget size that could be realized in the experiment, each subject indicated how she would like to divide that amount between herself and the recipient.[9]

Once the decisions of all subjects had been recorded, the income-generation phase of the experiment began. In EFFORT treatments, subjects were paid for completing a simple piece-rate task: each subjects was given a bucket containing three different varieties of dried beans; subjects were paid a fixed amount per gram of a designated variety of bean that she collected from the bucket during a 10 minute period.[10] After 10 minutes, subjects' beans were weighed to determine their earnings. In LUCK treatments, each player was given one opportunity to roll a twenty-sided die to determined her winnings and, consequently, the dictator's budget.

After the income generation phase of the experiment, subjects returned to a single classroom. Subjects in both classrooms played the role of the dictator and generated income. At the end of the experiment, one of the two rooms was chosen at random, and the decisions of subjects in that room determined the final payouts. The decisions of the dictators in the chosen room were implemented. All subjects received their payments in cash at the end of the experimental session. Experimental sessions in both Kenya and the US lasted approximately three hours.

4 Analysis

We begin our analysis by testing the hypothesis that individuals living in traditional rural communities in the developing world conform to the egalitarian ideal of fairness which suggests that all inequality is unfair (Konow 2003, Cappelen et al. 2007). First, we test whether subjects in our Kenyan village sample allocate recipients exactly half the budget. Our analysis expands upon existing work on social preferences in poor communities because we are able to test whether allocation decisions differ from the egalitarian ideal in treatments involving earned income. This is important because a key aspect of the egalitarian ideal of fairness is that the fair allocation does not depend on the conditions of production.

[9] See Fischbacher et al. (2012) for a recent discussion of the costs and benefits of using the strategy method.

[10] Subjects were informed that, if they wished to stop collecting beans at any time, their earnings would be calculated immediately based on their work up to that point. No subjects in either Kenya or the US chose to stop early. The total quantity of beans was calibrated such that a hard-working player could remove most, but not all, of the chosen variety during a 10 minute period — thus, the task became harder over the course of the experiment. No player ever exhausted their supply of beans during the allotted 10 minutes.

Figure 1 summarizes the allocation decisions made by subjects in our experiment. The average fraction of the budget allocated to the recipient is presented in Panel A, while Panel B reports the frequency with which subjects divide the budget evenly. In the benchmark Luck-Giving treatment, subjects in our Kenyan village sample allocate an average of 26.5 percent of the budget to recipients; 16.1 percent of observed allocation decisions are even splits of the budget. Kenyan subjects allocate more to recipients than university students (who allocate an average of 19.9 percent of the budget to recipients in the Luck-Giving treatment[11]), but their allocation decisions are still quite far from the egalitarian ideal of fairness. Subjects drawn from rural villages allocate more than 40 percent of the budget to recipients in the two Taking treatments, and they are also more likely to split the budget evenly (doing so 27.5 and 34.4 percent of the time in the Luck-Taking and Effort-Taking treatments respectively). We can nonetheless reject the hypothesis that subjects in our Kenyan village sample allocate an average of half the budget to recipients in each of the four experimental treatments (all p-values < 0.001).

4.1 Earned Property Rights

Though their choices do not conform to the egalitarian ideal, subjects in the Kenyan village sample may still hold egalitarian concepts of what constitutes a fair allocation: subjects in experiments make tradeoffs between their own self-interest and their ideals of fairness, so even those holding egalitarian conceptions of fairness may not split the budget evenly in dictator games (Levitt and List 2007, Cappelen et al. 2007). However, subjects holding egalitarian conceptions of fairness should make the same allocation choices in all four experimental treatments (Cappelen et al. 2007, Almas et al. 2010). In light of this, we test whether deviations from the egalitarian ideal are equally large in Luck and Effort treatments in a regression framework. Our main outcome of interest is the fraction of the budget that dictator i in treatment t allocates to the recipient (given a budget size b); since very few subjects allocate more than half the budget to the recipient, this also measures proximity to the egalitarian ideal of equality in payoffs. Following Leider et al. (2009), we model the fraction of the budget that a dictator allocates to

[11]The average allocation to the recipient observed in our university student sample (in the Luck-Giving treatment) is comparable to the levels reported in Camerer (2003).

a recipient in a simple linear framework:

$$z_{itb} = \theta_i + \eta EG_{it} + \gamma T_{it} + \delta ET_{it} + \varepsilon_{itb} \tag{1}$$

where T_{it} is an indicator equal to one if subject i is assigned to one of the TAKING treatments and EG_{it} and ET_{it} are indicators for being assigned to the EFFORT-GIVING and EFFORT-TAKING treatments, respectively.

In this framework, θ_i denotes subject i's level of baseline altruism in a standard, anonymous dictator game (our LUCK-GIVING treatment). Baseline altruism provides a simple, reduced form summary of tradeoffs between self-interest and one's ideal of fairness in neutral environments where no one exerts effort.[12] Numerous studies suggest heterogeneity in baseline altruism that is not explained by observable characteristics.[13] Since we only observe each subject in a single treatment, we model heterogeneity in baseline altruism by assuming that:

$$\theta_i = \theta_0 + \nu_i \tag{2}$$

where ν_i is mean-zero and normally-distributed. In our empirical analysis, we estimate random-effects tobit models which allow for such unobservable heterogeneity.

As discussed above, many experimental studies suggest that university students do not hold egalitarian fairness ideals: they are less generous with earned income than with unearned funds, and they allocate more to those who have increased total surplus through effort and ability. In the spirit of Locke (1980[1690]), Fahr and Irlenbusch (2000) refer to the sense of entitlement associated with exerting effort as an "earned property right." If earned property rights are

[12]Specifically, in the model proposed by Cappelen et al. (2007), the optimal interior allocation to the recipient is the fair share (for the recipient) dictated by one's fairness ideal minus a term reflecting the marginal utility of own income, as compared with the cost of violating one's own ideals of fairness. For those holding egalitarian conceptions of fairness, the fair share for the recipient is always one half. The meritocratic fairness ideal also dictates that the fair share for the recipient in the LUCK-GIVING treatment is one half, while the libertarian fairness ideal suggests that the fair share for the recipient in the LUCK-GIVING treatment is zero. Alternatively, one can interpret baseline altruism in the constant elasticity of substitution (CES) utility framework proposed by Andreoni and Miller (2002) and Fisman et al. (2007): here, baseline altruism provides a summary measure of fairmindedness (the relative weight on one's own payoff vis-a-vis the payoffs to others) and the willingness to tradeoff equality and efficiency in situations where the money being divided is unearned. In general, the linear framework presented in Equation 1 can be viewed as a summary representation of any structural model of homothetic social preferences. We consider the possibility that social preferences may not be homothetic in greater detail below.

[13]See, for example, Andreoni and Miller (2002), Leider et al. (2009), and Fisman et al. (2014a).

important, dictators will feel justified in being less generous in the EFFORT-GIVING treatment than in the LUCK-GIVING treatment, but will also feel an obligation to be more generous in the EFFORT-TAKING treatment than in the LUCK-TAKING treatment.[14] However, if rural villagers hold egalitarian conceptions of fairness, then we should not see evidence of earned property rights effects; instead, allocations should be similar in LUCK and EFFORT treatments.

We examine respect for such earned property rights effects in our Kenyan village sample by testing whether η and δ (in Equation 1) are equal to zero; in other words, we test whether dictators allocate more to recipients who have generated income through effort rather than luck, and whether dictators allocate recipients less when the dictators themselves have generated the budget through effort rather than luck. If a non-negligible proportion of subjects in our Kenyan village sample view inequality related to differences in individual effort or production as just, we might expect to see evidence of earned property rights effects. In that case, η will be less than zero (dictators will allocate less to recipients in the EFFORT-GIVING treatment than in the LUCK-GIVING treatment) and δ will be greater than zero (dictators will allocate more to recipients in the EFFORT-TAKING treatment than in the LUCK-TAKING treatment).

We test these hypotheses by estimating Equation 1 in a regression framework. We estimate tobit specifications that include individual random effects (to control for heterogeneity in baseline altruism) and adjust for censoring of the dependent variable at zero and one. Results are reported in Table 2. Even-numbered columns include a linear control for the size of the dictator's budget, and Columns 3 and 4 also control for individual sociodemographic characteristics (gender, age, marital status, and educational attainment). The parameter η (in Equation 1) measures respect for one's own earned property rights; it indicates the difference between allocations in the EFFORT-GIVING treatment and allocations in the benchmark LUCK-GIVING treatment. In the Kenyan village sample, we see that the estimated coefficient is negative but only marginally statistically significant in two of the four specifications (Table 2, Panel A, p-values 0.073). The estimate of the parameter δ, which captures respect for the earned property rights of others, is negative and never statistically significant. It therefore indicates that, if

[14] In the CES framework proposed by Andreoni and Miller (2002) and extended by Cox et al. (2007), earned property rights effects will arise if those whose efforts and abilities determine the size of the budget have higher status than other subjects. In the model proposed by Cappelen et al. (2007), earned property rights effects may arise when the population includes individuals holding meritocratic fairness ideals; whether or not such individuals will behave in a manner consistent with respect for earned property rights depends on the structure of their beliefs.

anything, dictators allocate less to recipients who have exerted effort than to those who have not (though the effect is never significant). Thus, we cannot reject the hypothesis that subjects in our Kenyan village sample do not view earned and unearned income differently. There is very weak evidence that they respect their own earned property rights and no evidence that they respect the earned property rights of others.

Interestingly, the coefficient measuring the nominal ownership effect (the coefficient on the indicator for TAKING treatments, represented by γ in Equation 1) is consistently positive and significant (as Figure 1 suggests); thus, dictators in our Kenyan village sample allocate substantially more to recipients who have generated the budget through either luck or effort than to those who have not. One interpretation of this result, consistent with the model of Cox et al. (2007), is that generating the budget (through either luck or effort) increases the recipient's status vis-a-vis the dictator. Alternatively, in the model of Cappelen et al. (2007), we would expect to observe a nominal ownership effect if society contains sufficient numbers of subjects holding libertarian fairness ideals that suggest that one is entitled to anything one produces. Nominal ownership effects are also consistent with the model of guilt aversion presented in Charness and Dufwenberg (2006) if recipients expect to receive a larger share of the budget in TAKING treatments than in GIVING treatments.[15] Irrespective of the theoretical underpinning driving the result, the finding highlights a strength of our cross-cut experimental design: a comparison of allocations in the EFFORT-TAKING and more standard LUCK-GIVING treatments would grossly overstate respect for the earned property rights of others by not controlling for the nominal ownership effect.

One possible concern with our design is that we assume that subjects narrowly bracket their allocation decisions, ignoring the fact that all subjects engage in the luck or effort task and focusing on the actions linked to the size of the budget in the decision randomly chosen to determine final payouts.[16] Our replication of our experimental protocol in a comparison sample at a US university allows us to test whether this aspect of our experimental design explains the absence of earned property rights effects. Panel B of Table 2 replicates our regression analysis

[15] Ellingsen et al. (2010) test the guilt aversion model and find no evidence that allocation decisions in dictator games are influenced by second order beliefs about recipients' expectations.

[16] Such assumptions of narrow bracketing on a particular investment or allocation problem are not uncommon in the experimental literature. See Andreoni and Miller (2002), Holt and Laury (2002), Fisman et al. (2007), and Rabin and Weizsacker (2009) for examples and discussion.

in our US university sample. Here, we find strong evidence of respect for earned property rights. Dictators allocate recipients between 16.1 and 19.5 percentage points less of the budget in the Effort-Giving treatment than in the Luck-Giving treatment (depending on the specification), replicating the earned property rights effects documented in previous work. Dictators also allocate 19.6 to 23.6 percentage points more to recipients in the Effort-Taking treatment than in the Luck-Taking treatment. Coefficients are significant at the 99 percent confidence level in all specifications. Thus, our experimental design cannot explain the absence of earned property rights effects in the Kenyan village sample; we observe ample evidence of such effects in the merit-focused environment of a US university.[17]

4.1.1 Variation within and between Villages

Our results thus far suggest that subjects drawn from rural villages in Kenya make only a limited distinction between earned and unearned income. We find weak evidence of respect for one's own earned property rights and, on average, no evidence of respect for the earned property rights of others. These patterns differ from those observed in a comparison sample of university students in the US; there, consistent with previous work, we find strong evidence of respect for earned property rights — both one's own earned property rights and those of others.

One possible interpretation of our results is that individual social preferences are impacted by some aspect of the process of economic development — specifically, that respect for earned property rights is a consequence of development. Such a finding would resonate with an enormous body of research suggesting that development impacts individual values and preferences. One potential channel highlighted in existing work is education. For example, Inkeles (1969) and Armer and Youtz (1971) argue that formal education leads to the adoption of modern values such as a belief in science. Another potential causal pathway is experience transacting in markets. For instance, Henrich et al. (2010) show that the extent of market integration is associated with higher allocations to recipients in dictator games involving unearned income, while Siziba and

[17]It is, of course, theoretically possible that subjects in the U.S. university sample narrowly bracket their decisions while subjects in the Kenyan village sample do not. Some evidence against this possibility comes from lab experiments measuring risk aversion; existing evidence does not suggest that subjects from developing countries are substantially less risk averse than university students (as one would expect if university students engaged in narrow bracketing and non-students from poor countries did not). As in all cross-cultural studies, we cannot fully rule out the possibility that subjects in different societies bracket or interpret experimental choices differently.

Bulte (2012) find that market integration lowers levels of trust.

We test for similar associations between community and individual markers of development and behavior in our experiments within the Kenyan village sample. At the village level, we calculate the distance to the nearest paved road and to the electricity grid and classify communities closer to the road/grid as more developed.[18] At the individual level, we lack detailed consumption data that would allow us to calculate market integration, so we focus instead on the educational channel by partitioning the sample into those above and below the median level of education (complete primary school) observed in our sample. We use both markers of development to split the sample into more and less developed subsamples, and examine behavior within each subsample across our four experimental treatments. Importantly, our individual-level analysis is not simply replicating the village-level analysis: community fixed effects explain only 5.7 percent of the variation in primary school completion; most of this variation occurs within rather than between communities.

In Table 3, we replicate the analysis presented in Table 2 (specifically, Table 2, Panel A, Column 4, which includes a linear control for the size of the dictator's budget and individual-level sociodemographic controls) in our more and less developed subsamples. Community-level results (more developed vs. less developed communities) are reported in Columns 1 and 2, and individual-level results (more educated vs. less educated subjects) are reported Columns 3 and 4. Two results are notable. First, dictators from less developed communities (Column 2) and those with less education (Column 4) allocate significantly less to recipients in the Effort-Taking treatment than in the Luck-Taking treatment. In contrast, dictators from more developed communities (Column 1) and those with more education (Column 3) allocate almost exactly the same amount to recipients in the two Taking treatments. Thus, the evidence is weakly supportive of the hypothesis that development leads to greater respect for others' earned property rights. Second, we observe significant nominal ownership effects in both the more and less developed subsamples; allocations to recipients are always significantly higher in the two Taking treatments than in Giving treatments, and there is no obvious correlation between the level of development and the size of this effect.

[18] Distance to the nearest paved road and distance to the neared electrified community are highly correlated (correlation coefficient 0.878), and generate identical rankings of more/less developed communities within each treatment.

4.2 Are Dictators Motivated by Reciprocity?

Our analysis thus far has focused on the average fraction of the budget allocated to the recipient, pooling data across a range of budget sizes. If subjects hold egalitarian conceptions of fairness and social preferences are homothetic, the size of the budget should not impact the proportion allocated to the recipient.[19] In contrast, models of reciprocity (cf. Charness and Rabin 2002, Cox et al. 2007) predict that dictators will allocate more to those who have increased aggregate income through their actions. Specifically, the model of reciprocity proposed by Cox et al. (2007) makes two predictions. First, a dictator's allocation to the recipient should increase with recipient production in the Effort-Taking treatment. Second, a dictator's allocation in the Effort-Taking treatment should equal the allocation she would make in the Luck-Taking treatment at some intermediate level of production that is considered "neutral;" at production levels below (above) the neutral amount, a dictator will allocate less (more) to the recipient in the Effort-Taking treatment than in the Luck-Taking treatment. The model of negative reciprocity proposed by Charness and Rabin (2002) makes a very similar prediction: a dictator will allocate less to a recipient in the Effort-Taking treatment than in the Luck-Taking treatment when the recipient has produced below the neutral level (when she has "behaved badly"); otherwise, she will allocate the same amount to the recipient in the two Taking treatments.[20]

We begin by exploring the relationship between budget size and dictators' allocations to recipients graphically. Figure 2 shows the average fraction of the budget allocated to the recipient in each treatment at each feasible budget size, plus a separate linear trend for each treatment. Surprisingly, the fraction of the budget allocated to the recipient clearly decreases with budget size in both of the Luck treatments and in the Effort-Giving treatment. This pattern is inconsistent with many existing models of social preferences which assume that preferences in neutral environments are homothetic (cf. Cappelen et al. 2007, Cox et al. 2007, Fisman et al. 2007). However, the slope with respect to budget size is markedly less negative — approximately zero

[19] In the framework proposed by Cappelen et al. (2007), for example, this is true in the more general case where the population comprises only egalitarians and libertarians; it may also be true if the population includes individuals holding meritocratic ideals of fairness (depending on the structure of their beliefs).

[20] Almas et al's (2010) model of meritocratic ideals also predicts a positive relationship between the budget share allocated to the recipient and the amount produced by the recipient in the Effort-Taking treatment if one assumes that the dictator's belief about the amount that she would have earned if she were generating the budget is independent of the amount actually produced by the recipient.

— in the Effort-Taking treatment. Interestingly, the linear trends for the Luck-Taking and Effort-Taking treatments do not intersect in the range of feasible budget sets, though they come close. As discussed above, in the model of reciprocity of Cox et al. (2007), this intersection point indicates a level of recipient effort or output considered neutral; effort levels above this neutral benchmark are rewarded, and effort levels below this neutral benchmark are punished (Cox et al. 2007). Thus, our aggregate results are consistent with negative but not positive reciprocity: dictators' allocations in the Effort-Taking treatment are increasing in recipient effort (relative to the Luck-Taking treatment), but the neutral level of effort below which recipients are sanctioned is larger than the maximum feasible level of output.

Next, we test the specific prediction of the reciprocity model — that the slope with respect to budget size should be more positive in the Effort-Taking treatment than in the Luck-Taking treatment — in a regression framework by adding treatment-specific linear budget trends to the tobit specifications described above. Specifically, in Columns 1 and 2 of Table 4, we replicate Columns 2 and 4 of Table 2 (which, respectively, do not and do include individual sociodemographic controls) including treatment-specific linear budget trends. We find that the fraction of the budget allocated to the recipient decreases with budget size in both of the Luck treatments (Table 4, Columns 1 and 2, p-values < 0.001), confirming the graphical results presented above. In addition, the slope with respect to budget size is significantly more positive in the Effort-Taking treatment than in either of the two Luck treatments (Table 4, Columns 1 and 2, p-values < 0.001). This finding is consistent with models of reciprocity in which the utility weight on the recipient (vis-a-vis the dictator) increases with recipient effort.[21] Estimated coefficients indicate that the predicted allocations to the recipient in the Effort-Taking and Luck-Taking treatments are equal at a budget size of 382.5 — well outside the range of feasible budget sizes. This confirms our graphical analysis: our results are consistent with (a qualified form of) negative rather than positive reciprocity; dictators appear to punish all feasible levels of production because they are not large enough.[22]

[21]However, it is important to note that this finding contradicts the typical prediction of reciprocity models that the share allocated to the recipient should increase with recipient effort (cf. Cox et al. 2007), since those models assume that allocations would be independent of budget size in the absence of reciprocity considerations.

[22]It is important to note that — though we focus on testing the predictions of prominent models of reciprocity — there may be other models of social preferences that could explain the patterns of behavior that we observe; models of other-regarding preferences are numerous, and testing the predictions of all such models in our data is beyond the scope of this paper. In particular, we are unable to test models in which allocations to others depend

4.2.1 Variation within and between Villages

In our final piece of analysis, we test the predictions of the reciprocity model in our more and less developed subsamples. As in the analysis presented in Section 4.1.1, we split the sample into, first, more developed and less developed communities (those closer to and further from paved roads and the electricity grid) and, second, more and less educated subjects (those who have and have not completed primary school).

Figure 3 presents our community-level analysis graphically, replicating Figure 2 separately for the more and less developed subsamples. It is clear that patterns of behavior are very different in more developed communities than in less developed communities. As in the pooled data, in the more developed communities we observe a substantially more positive slope (with respect to the size of the dictator's budget) in the Effort-Taking treatment than in the Luck-Taking treatment (Figure 3, Panel A). However, in this case, the estimated linear trends intersect near the middle of the range of budget sizes, suggesting that dictators in more developed communities reward high effort and punish low effort. In the less developed villages, by contrast, we observe no evidence of reciprocity: dictators allocate substantially less to recipients in the Effort-Taking treatment than in the Luck-Taking treatment (Figure 3, Panel B), and the slopes with respect to budget size are both downward-sloping. Next, we examine behavior in the Giving treatments. In the more developed communities, dictators allocate slightly less to recipients in the Effort-Giving treatment than in the Luck-Giving treatment, suggesting a very weak effect of one's own earned property rights. This effect is even more pronounced in the less developed communities, where dictators allocate substantially less to recipients in the Effort-Giving treatment than in the Luck-Giving treatment. Thus, there is evidence that villagers from less developed communities respect their own earned property rights, but not the earned property rights of others.

In Figure 4, we replicate our analysis using our individual-level marker of development: educational attainment, spliting the sample into those who have and have not completed primary school. The patterns apparent in Figure 4 are remarkably similar to those shown in Figure 3. In the more educated sample of Kenyan villagers, we again find evidence consistent with both

on second order beliefs since we did not collect such beliefs data.

positive and negative reciprocity, and some evidence of respect for one's own earned property rights. Less educated villagers, in contrast, consistently allocate less to recipients in the EFFORT treatments than in the LUCK treatments.

We explore these patterns more formally in a regression framework in Table 4. In Columns 3 through 6, we replicate the pooled specification reported in Column 2 (of Table 4) separately for each of our distinct subsamples (specifically, we estimate Equation 1 in a random effects Tobit framework, including controls for individual sociodemographic characteristics and treatment-specific linear budget trends). We turn first to the results for more developed communities, reported in Column 3. We find that the slope coefficient associated with the EFFORT-TAKING treatment is positive and significant, confirming the graphical analysis. Coefficient estimates suggest that the neutral level of production, above which high effort is rewarded and below which low effort is punished, is approximately 198 shillings — just below the mean level of production in the EFFORT-TAKING treatment (200 grams of beans, which translated into a budget of 200 shillings). Thus, dictators from more developed villages make allocation decisions that are consistent with models of reciprocity which suggest that subjects will reward or punish others for exerting high or low effort.

Results for more educated subjects are strikingly similar (Table 4, Column 5). Again, the slope with respect to budget size in the EFFORT-TAKING treatment is positive and significant. In this case, coefficient estimates suggest that the neutral budget size is approximately 207 shillings, which is just above the mean level of production. Above this level, allocations to recipients are higher in the EFFORT-TAKING treatment than in the LUCK-TAKING treatment; below this level, the opposite is true.

We find a very different pattern in the less developed communities (Table 4, Column 4), and among less educated subjects (Table 4, Column 6). As discussed in Section 4.1.1, in these subsamples, the estimated coefficient on EFFORT-TAKING is negative, large in magnitude, and at least marginally statistically significant — indicating that, on average, recipients receive less in the EFFORT-TAKING treatment than in the LUCK-TAKING treatment. The slope with respect to budget size is small and magnitude and not statistically significant in the less developed communities; in other words, the allocation decisions of dictators from those commonties are not consistent with reciprocity. We do observe a positive slope in the EFFORT-TAKING treat-

ment among less educated dictators, but because the average allocation is so much lower in the Effort-Taking treatment than in the Luck-Taking treatment, the estimated intersection between the two linear trends would only occur at a budget of 660 shillings (which is more than double the maximum feasible budget size in the experiment).

Taken together, these findings suggest a shift toward respect for others' earned property rights may be associated with the process of economic development (though the direction of causality is unclear). We find that less educated dictators and those drawn from more remote villages allocate less to recipients who have exerted effort than to those who have not. In contrast, more educated dictators and those drawn from communities nearer to the road network reward recipients' effort by allocating more to those who produce more output; in this subsample, we do not observe an overall tendency to allocate less in the Effort-Taking treatment than in the Luck-Taking treatment.

5 Conclusion

We conduct a suite of dictator games designed to measure the extent to which individual social preferences governing the distribution of earned and unearned income differ. We conduct identical lab experiments in two distinct cultural contexts: in lab-in-the-field settings in rural villages in western Kenya, an environment with strong egalitarian traditions and norms of mutual assistance, and in a standard university lab setting in the US. Our experiments vary whether the size of the dictator's budget depends on luck as opposed to individual effort, and whether the dictator is asked to divide her own winnings or earnings or the those of another subject.

Our main finding is that subjects drawn from Kenyan villages do not treat earned and unearned income differently. Specifically, we find weak evidence of respect for one's own earned property rights, but no evidence of respect for the earned property rights of others. This contrasts with our findings in the university lab: consistent with previous evidence, we document a substantial earned property rights effects in our university subject pool.

Our findings resonate with descriptive work on the values and norms underlying individual behavior in poor communities. For example, in his study of firms in rural Indonesia, Geertz (1963) writes "Traditional values supporting collective benefits as against individual enrichment

induce a strong resistance to the rationalization of [enterprises] once they are formed... This essentially conservative kind of approach to change can be very inhibiting to long-run development." By using experimental economic tools, we are able to show that, on average, rural villagers do appear less inclined to reward others' effort than subjects in the more individualistic environment of a US university. More interestingly, we document substantial heterogeneity within our Kenyan village sample. More educated subjects, and those drawn from more developed villages, do reward others' effort in a manner consistent with positive reciprocity. Thus, our work complements larger cross-cultural studies, and deepens our understanding of the interplay between economic development and individual preferences.

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Table 1: Summary Statistics — Subject Characteristics

	Mean	S.D.	Median	Minimum	Maximum	N
Panel A: Kenyan rural village sample						
Female	0.41	0.49	.	.	.	533
Age	27.50	5.40	27	18	45	533
Completed primary school	0.55	0.50	.	.	.	530
Completed secondary school	0.20	0.40	.	.	.	530
Married	0.70	0.46	.	.	.	527
Number of children	2.54	2.05	2	0	10	533
Household size	5.97	2.46	6	1	15	530
HH farms own plot	0.96	0.19	.	.	.	532
HH member employed	0.82	0.38	.	.	.	533
HH has tin roof	0.40	0.49	.	.	.	533
HH has latrine	0.89	0.31	.	.	.	533
Bicycles owned by HH	1.04	0.79	1	0	7	533
Cows owned by HH	1.42	2.15	1	0	20	533
Chickens owned by HH	7.83	8.56	6	0	70	533
Christian	0.97	0.16	.	.	.	533
Luhya	0.70	0.46	.	.	.	532
Teso	0.25	0.43	.	.	.	528
Luo	0.05	0.22	.	.	.	528
Community groups	2.08	1.65	2	0	8	533
Panel B: US university sample						
Female	0.63	0.48	.	.	.	185
Age	20.13	1.44	20	18	25	185
Year in university	2.91	0.96	3	1	4	185
Economics major	0.30	0.46	.	.	.	185
Social science (not economics) major	0.27	0.45	.	.	.	185
Math, science, or engineering major	0.41	0.49	.	.	.	185
Art or humanities major	0.16	0.36	.	.	.	185

Table 2: Tobit Regressions of Dictators' Allocation Decisions: Earned Property Rights Effects

Dependent variable: proportion of dictator's budget allocated to recipient

Specification:	Tobit (1)	Tobit (2)	Tobit (3)	Tobit (4)
Panel A: Kenyan rural village sample				
Own earned property right (EG treatment)	-0.05*	-0.05*	-0.043	-0.043
	(0.028)	(0.028)	(0.028)	(0.028)
Recipient's earned property right (ET treatment)	-0.034	-0.034	-0.035	-0.035
	(0.027)	(0.027)	(0.028)	(0.028)
Nominal ownership (Taking treatments)	0.194***	0.194***	0.195***	0.195***
	(0.029)	(0.029)	(0.03)	(0.03)
Constant	0.241***	0.241***	0.219***	0.219***
	(0.022)	(0.022)	(0.043)	(0.043)
Observations	10660	10660	10480	10480
Panel B: US university sample				
Own earned property right (EG treatment)	-0.161***	-0.195***	-0.176***	-0.166***
	(0.057)	(0.07)	(0.056)	(0.049)
Recipient's earned property right (ET treatment)	0.236***	0.196***	0.218***	0.201***
	(0.037)	(0.062)	(0.046)	(0.041)
Nominal ownership (Taking treatments)	-0.154**	-0.17***	-0.185***	-0.167***
	(0.062)	(0.063)	(0.055)	(0.047)
Constant	0.134**	0.132**	-0.012	0.01
	(0.056)	(0.056)	(0.055)	(0.045)
Observations	3700	3700	3700	3700
Individual random effects	Yes	Yes	Yes	Yes
Budget size control (linear trend)	No	Yes	No	Yes
Additional individual controls	No	No	Yes	Yes

Standard errors in parentheses. All specifications estimated via random effects tobit, controlling for censoring of the fraction of the budget allocated to the recipient at 0 and 1. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. Individual controls included in Panel A are: gender (indicator for being female), educational attainment (indicators for completing primary and secondary school), marital status, household size, and an index of durable asset ownership. Individual controls included in Panel B are: gender (indicator for being female) and indicators for majoring in the sciences or the humanities.

Table 3: Tobit Regressions: Heterogeneity in Earned Property Rights Effects

Dependent variable: proportion of dictator's budget allocated to recipient

	More developed community?		More educated subject?	
Sample restriction:	Yes	No	Yes	No
Specification:	Tobit	Tobit	Tobit	Tobit
	(1)	(2)	(3)	(4)
Own earned property right (EG treatment)	-0.011	-0.092*	-0.039	-0.046
	(0.035)	(0.05)	(0.038)	(0.043)
Recipient's earned property right (ET treatment)	0.003	-0.073*	-0.0003	-0.093**
	(0.044)	(0.037)	(0.035)	(0.047)
Nominal ownership (Taking treatments)	0.207***	0.153***	0.178***	0.229***
	(0.045)	(0.043)	(0.036)	(0.051)
Constant	0.163***	0.312***	0.231***	0.204***
	(0.056)	(0.067)	(0.054)	(0.063)
Observations	6120	4360	5820	4660
Individual random effects	Yes	Yes	Yes	Yes
Budget size control (linear trend)	Yes	Yes	Yes	Yes
Additional individual controls	Yes	Yes	Yes	Yes

Standard errors in parentheses. All specifications estimated via random effects tobit, controlling for censoring of the fraction of the budget allocated to the recipient at 0 and 1. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. Individual controls are: gender (indicator for being female), educational attainment (indicators for completing primary and secondary school), marital status, household size, and an index of durable asset ownership.

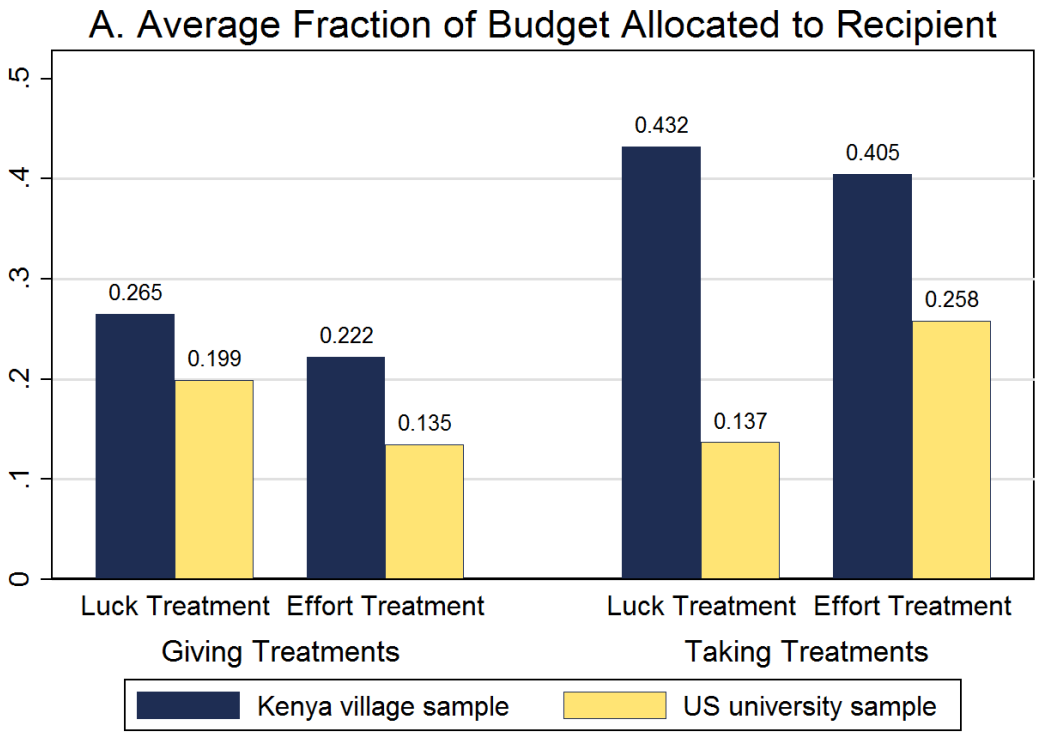
Table 4: Tobit Regressions of Dictators' Allocation Decisions: Testing for Reciprocity

Dependent variable: proportion of dictator's budget allocated to recipient

			More developed community?		More educated subject?	
Sample restriction:	None	None	Yes	No	Yes	No
Specification:	Tobit	Tobit	Tobit	Tobit	Tobit	Tobit
	(1)	(2)	(3)	(4)	(5)	(6
Own earned property right (EG treatment)	-0.05*	-0.043	-0.011	-0.092*	-0.039	-0.046
	(0.028)	(0.028)	(0.035)	(0.05)	(0.038)	(0.043)
Recipient's earned property right (ET treatment)	-0.034	-0.035	0.003	-0.073*	-0.0003	-0.093**
	(0.027)	(0.028)	(0.044)	(0.037)	(0.035)	(0.047)
Nominal ownership (Taking treatments)	0.195***	0.195***	0.207***	0.153***	0.179***	0.23***
	(0.029)	(0.03)	(0.045)	(0.043)	(0.036)	(0.051)
Budget size	-0.004***	-0.004***	-0.005***	-0.002***	-0.004***	-0.004***
	(0.0005)	(0.0005)	(0.0006)	(0.0009)	(0.0006)	(0.0008)
Luck-Taking Treatment × budget size	0.0008	0.0009	0.0004	0.00007	0.0007	0.001
	(0.0007)	(0.0007)	(0.0009)	(0.001)	(0.0008)	(0.001)
Effort-Giving Treatment × budget size	0.002***	0.002***	0.002***	0.001	0.002**	0.001
	(0.0006)	(0.0006)	(0.0007)	(0.001)	(0.0009)	(0.0009)
Effort-Taking Treatment × budget size	0.003***	0.003***	0.004***	0.0005	0.002***	0.003***
	(0.0006)	(0.0006)	(0.0008)	(0.001)	(0.0008)	(0.001)
Constant	0.241***	0.218***	0.163***	0.311***	0.231***	0.204***
	(0.022)	(0.043)	(0.056)	(0.067)	(0.054)	(0.063)
Observations	10660	10480	6120	4360	5820	4660
Individual random effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional individual controls	No	Yes	Yes	Yes	Yes	Yes

Standard errors in parentheses. All specifications estimated via random effects tobit, controlling for censoring of the fraction of the budget allocated to the recipient at 0 and 1. *** indicates significance at the 99 percent level; ** indicates significance at the 95 percent level; and * indicates significance at the 90 percent level. Individual controls are: gender (indicator for being female), educational attainment (indicators for completing primary and secondary school), marital status, household size, and an index of durable asset ownership.

Figure 1: Allocation Decisions across Experimental Treatments



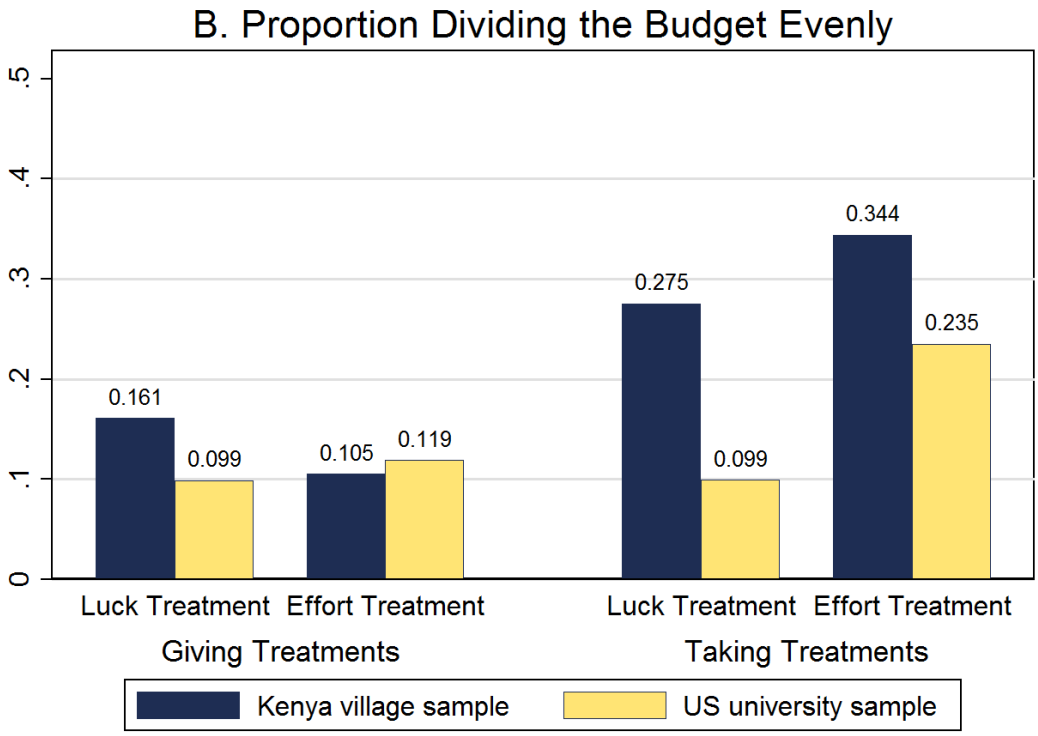


Figure 2: Allocation Decisions by Budget Size

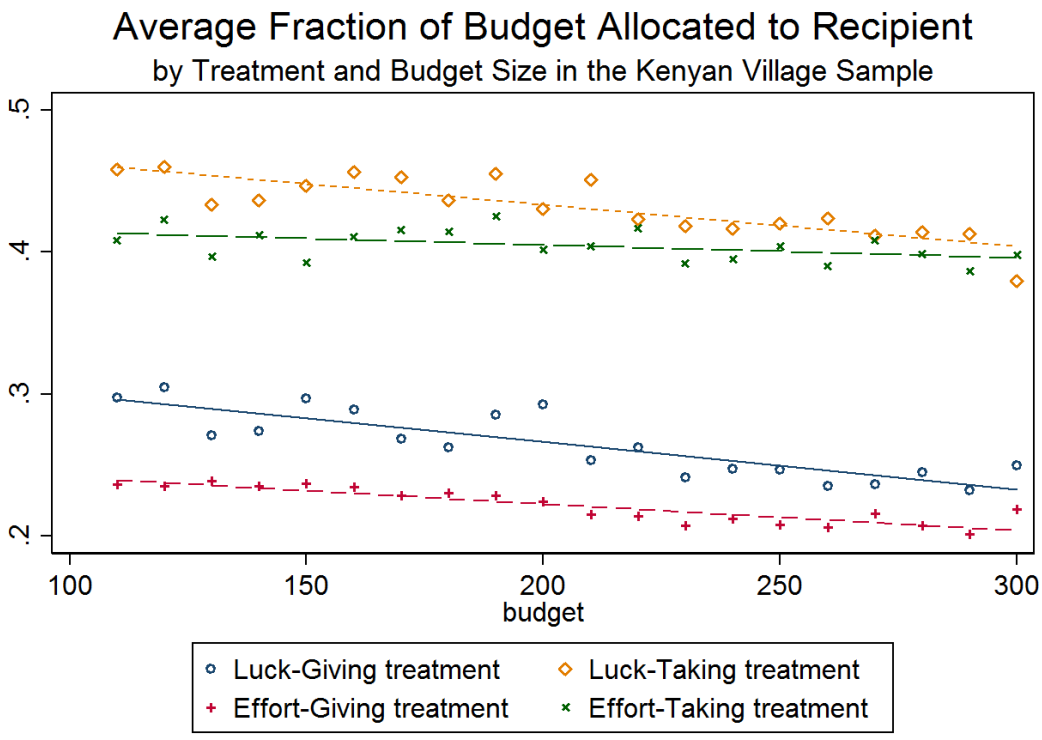
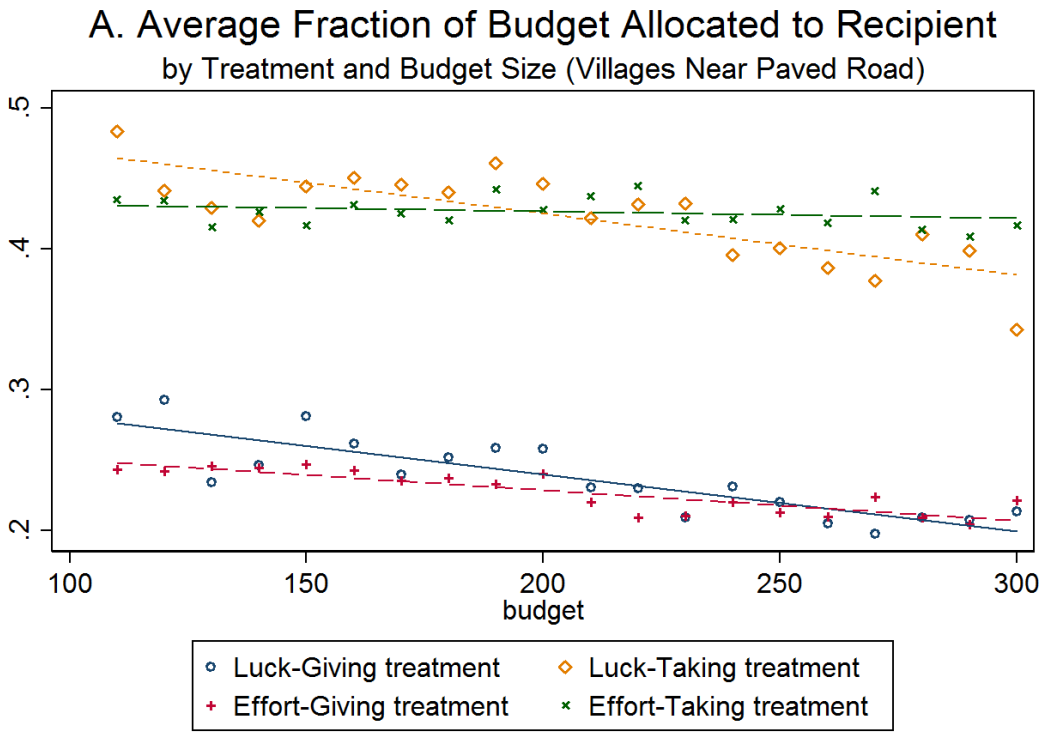


Figure 3: Allocation Decisions by Budget Size: Community-Level Heterogeneity



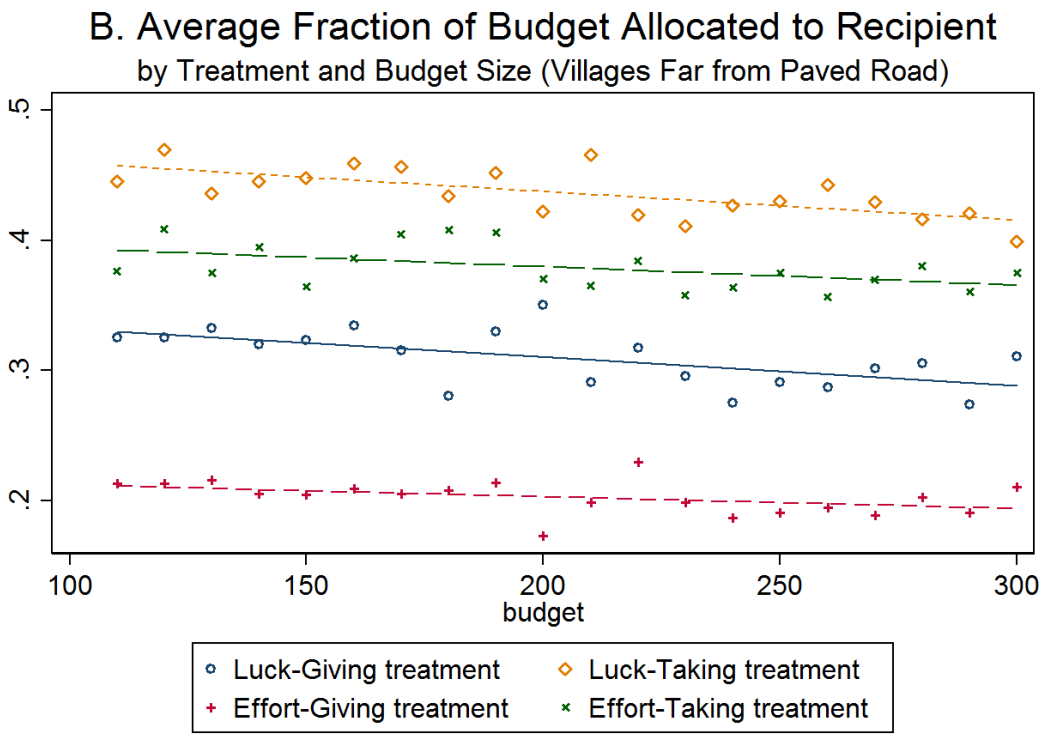


Figure 4: Allocation Decisions by Budget Size: Individual-Level Heterogeneity