# Using Economic Experiments to Measure Informal Institutions*

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## 1   Introduction

In many developing countries, formal legal institutions play a minimal role in the lives of most citizens, particularly those living in rural areas. Many states do not have the capacity to protect property rights and enforce contracts consistently; for example, many African states have struggled to project power outside the national capital in the post-independence period (Herbst 2000). Even when state capacity is present, formal rules are not always enforced. In such contexts, the *informal* rules of the game may be more salient determinants of economic outcomes than laws and regulations. However, the amorphous nature of social norms and conventions makes them substantially more difficult to analyze than legal or government institutions. From a development perspective, the gradual evolution of norms makes it difficult to distinguish between those informal institutions that facilitate growth and modernization from those that result from economic progress.

In this paper, I explore the potential for using economic experiments to analyze informal institutions. North (1990) defines institutions broadly, as "the humanly devised constraints that shape human interaction." Though some institutions — for example, written consti-

tutions — are devised intentionally and explicitly, many constraints on human interaction evolve organically without ever being formally codified. The set of all constraints can thus be partitioned into formal, explicit institutions such as laws, and informal institutions, which are "conventions, norms of behavior, and self imposed codes of conduct" and the enforcement mechanisms associated with these norms (North 1995). By this definition, the set of informal institutions comprises both internalized moral *preferences* over the outcomes of human interactions — preferences which shape individual behavior even in the absence of social enforcement mechanisms — and social conventions which occur in equilibrium.[1] Individual moral preferences include the desire to adhere to social norms of fair or correct behavior — for example, conventions related to charitable giving. However, agents may also have a preference for punishing the antisocial actions of others, even at a cost to oneself.

An obstacle to understanding how informal institutions differ across cultures is the impossibility of observing behavior out of equilibrium, which makes it difficult to disentangle individual preferences from individual beliefs about others' strategies and the economic environment. In particular, it is hard to tease apart moral preferences — trustworthiness, respect for property rights, egalitarianism, etc. — from strategic considerations arising from the repeated nature of social interactions. That patterns of behavior differ across cultures is apparent. What is not immediately apparent is whether such differences result from different moral preferences — both preferences for good behavior, subjectively defined, and preferences for punishing violators of standards of good behavior — or are simply different feasible Nash equilibria in the repeated game of human social interaction.

Economic experiments allow for the creation of interpersonal interactions with no possible social or reputational repercussions, so they can be used to measure "internally enforced standards of conduct" in disparate societies and groups, and to separate these revealed moral preferences from social mechanisms of norm enforcement (North 1990, p. 40). Thus, experiments provide a means of disentangling the internal and external components of informal

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[1]We use the term *preferences* in the economic sense, as a complete and transitive ordering over potential final outcomes or payoffs. Young (1998) defines a "convention" as "equilibrium behavior in a game played repeatedly by many different individuals in society, where the behaviors are widely known to be customary."

institutions, allowing for the measurement of individual values and norms that are difficult to capture in survey data.

In this paper, I discuss the use of economic experiments to measure internalized informal institutions, by which I mean the component of informal institutions that results from the specific moral preferences of a population, and not the (potentially arbitrary) selection of one of many equilibria. Section 2 describes the main experimental economic tools which have been used to measure individual moral preferences and differences in moral norms across societies. Section 3 summarizes the key findings from cross-cultural experiments. Section 4 explores variation in individual moral preferences within a single culture. Here, I test the hypothesis that the correlations between societal characteristics and individual choices in experiments which have been documented across cultures also exist within a specific culture. To do this, I present new results from dictator games conducted in rural, agricultural areas of western Kenya, highlighting the relationship between access to roads, and consequently markets, and individual decisions in the laboratory environment. After discussing the robustness of these finding and possible alternative interpretations, Section 5 concludes.

## 2    Measuring Informal Institutions: the Experimental Approach

The vast majority of cross-cultural experimental research has focused on four simple experiments designed to measure altruism and reciprocity: dictator, ultimatum, trust, and public goods games. These four games have been conducted all over the world, with subject populations ranging from university students in the United States and Europe to hunter-gatherers in Tanzania and Papua New Guinea (cf. Camerer 2003, Henrich et al. 2004).[2] The robust conclusion of all experimental studies is that human behavior is almost never consistent with the so-called "standard" model of self-interest. However, variation in play points to the diversity of fairness norms both within and across human societies. Roth et al. (1991)

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[2]See Henrich et al. (2010b) for an overview of the ways in which subjects in western university experimental labs are not representative of humanity in general.

conduct ultimatum games in four wealthy populations — Israel, Japan, the United States, and Yugoslavia — and report significant, though relatively small, cross-cultural differences in individual behavior. Examining a far wider range of societies, including some extremely primitive peoples, Henrich et al. (2010a) report far more dramatic cultural differences.

Dictator, ultimatum, trust, and public goods games are all designed to measure the tendency to be generous and cooperative, and to punish non-cooperation, in anonymous one-shot interactions. Thus, the experiments all seek to measure aspects of individual social preferences[3], which characterize an individual's willingness to trade off person gain and perceived moral rectitude or fairness (Levitt and List 2007).

In dictator games, one player is allocated a sum of money, and she decides how to divide that money between herself and another player (Forsythe et al. 1994). Games are anonymous, so subjects never learn the identity of the sender (or receiver) with whom they have been matched. Hence, dictator games provide a measure of generalized altruism unconfounded by strategic or reputational concerns, and have been used to measure the strength of egalitarian norms within and across societies (cf. Cappelen et al. 2007, Barr et al. 2009). If dictators were self-interested money-maximizers, they would clearly allocate nothing to other players. In practice, subjects in dictator games in university experimental labs typically allocate an average of 20-30 percent of the budget to the other player, and the vast majority of dictators transfer a positive amount (Camerer 2003).

Ultimatum games are similar to dictator games, except that the responder is given a choice between accepting the proposer's offered division of the budget or rejecting it, in which case both parties receive nothing (Güth et al. 1982). As with dictator games, the subgame perfect Nash equilibrium among self-interested players is clear: proposers allocate as little as possible to responders, who prefer any positive amount to leaving the game empty-handed. In practice, rejections of low offers are common in most, but not all, societies; anticipating this, proposers generally offer responders a substantial fraction of the budget (Camerer 2003).

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[3]An agent has *social preferences* if the consumption or welfare of another individual enters into her utility function.

Dictator games measure internal constraints on behavior directly, since an internally enforced moral standard is the only reason to rein in unobservable selfish behavior. In contrast, ultimatum experiments cannot be used to measure internalized norms of generosity: the willingness to share in an ultimatum game may demonstrate altruism, but may also be motivated by the fear of having a proposal rejected. Thus, the key deviation from the standard model demonstrated by individual choices in ultimatum games is the willingness of responders to turn down unequal offers. Ultimatum games measure negative reciprocity — the willingness to punish unfair behavior at a cost to oneself. A measure of the strength of such enforcement mechanisms is the "minimum acceptable offer" (MAO), the smallest fraction of the budget responders are willing to accept. When the MAO is zero, responder behavior is consistent with the standard model of pure self-interest.

A third variety of simple experiment used to measure internalized norms if the trust game (Berg et al. 1995). In trust games, one player — the "sender" or "trustor" — is allocated an amount of money, $X$. The sender chooses an amount $Y \leq X$ to send to a second player, the "trustee." The money sent by the trustor is multiplied by a factor $\lambda > 1$ such that the trustee receives $3Y$. The trustee then chooses an amount $Z \leq 3Y$ to send back to the trustor. Hence the sender's total payout is $X - Y + Z$, and the trustee's payout is $3Y - Z$. Giving by trustees demonstrates positive reciprocity — the willingness to reward kindness with kindness at expense to oneself. Within trust games, this is often referred to as "trustworthiness." Overall, the evidence that trustees are more generous than players in dictator games is limited (Cox 2004). Moreover, trust is a bad investment: the expected return to sending money is typically close to zero and often negative (Berg et al. 1995, Ashraf et al. 2006).

A final experimental measure of cooperation is the public goods game. In public goods games, players are randomly assigned to groups of size $n \geq 2$. Each player is endowed with a budget, $m$, which they divide between a private account and an allocation to a public account. Each token that an individual allocates to the public account is multiplied by a

factor $\eta$, where $1 < \eta < n$. Public goods games are often repeated, and can include the possibility of punishment; at the end of each round, players in punishment treatments are allowed to pay an amount $c$ to reduce the payout to another player by $\gamma c$. In a typical public goods game, contributions to the public account are common. Contribution levels tend to decline over time in the absence of punishment. However, when opportunities to sanction uncooperative behavior are available, high levels of giving to the public account are observed throughout most games (Fehr and Gächter 2002).

The four types of experiments described above are the most commonly used tools for measuring the individual social preferences underlying informal institutions in the economics lab. They are not, however, the only options available. First, many authors use variants of these basic designs which have been modified to explore specific development questions.[4] Other lab experiments are also often suitable for use in the field to measure social norms and individual values. For instance, Henrich et al. (2010a) include the third party punishment game proposed by Fehr and Fischbacher (2004). Finally, new experiments can be designed for the specific purpose of exploring informal institutions in poor communities — see, for example, Barr and Genicot (2008) and Attanasio et al. (2009) on risk-pooling and Goldberg (2010) and Jakiela and Ozier (2010) on sharing norms.

# 3    How Informal Institutions Differ Across Societies

Henrich et al. (2010a) explore cross-cultural variation in dictator and ultimatum game play across a wide range of development levels. They conduct dictator and ultimatum games in fifteen societies, ranging from the Hadza hunter-gatherers of Tanzania to wage workers in Ghana and the United States.[5] Mean offers within the dictator game range from 26 percent of the budget among the Hadza hunter-gathers of Tanzania to 47 percent of the budget among both wage workers in rural Missouri and the Sanquianga, a sedentary fishing population

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[4]See Jakiela et al. (2010), which is discussed in Section 5, for an example.

[5]They also conduct third party punishment games, which are not discussed here. Earlier waves of the project are documented in Henrich et al. (2004).

in Colombia (Barr et al. 2009); mean offers in the ultimatum games are strikingly similar, ranging from 26 to 48 percent of the budget. Comparing behavior across societies, the authors find a strong association between giving in both dictator and ultimatum games and the average level of market integration — measured by the share of total household calories purchased, rather than grown, hunted, or gathered, averaged across all subject households from a given society (Henrich et al. 2010a). They argue that the association between market integration and dictator game giving is evidence that markets lead to the emergence of shared norms of cooperation and fairness, rather than self-interest:

> "The efficiency of market exchange involving infrequent or anonymous transactions improves with an increasingly shared set of motivations and expectations related to trust, fairness, and cooperation. This lowers transaction costs, raises the frequency of successful transactions, and increases long-term rewards."

> — Henrich et al. (2010a)

How should we reconcile this striking empirical pattern with the conventional wisdom that markets promote self-interest? One interpretation is that markets clarify the concept of ownership, and thereby constrain acts of self-interest in settings where agents do not have a clear property right.[6] This suggests that market integration should be positively correlated with self-interest in settings where the dictator has a clear claim to the budget. We return to this point in Section 4.

The same set of authors also explore the enforcement side of informal institutions using the ultimatum game (Henrich et al. 2006, Henrich et al. 2010a). The range of offers observed in the dictator games is strikingly similar to that seen in the dictator games; and the correlation between the average DG offer and the average UG offer is 0.775. Not surprisingly,

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[6]Clearly, there is ambiguity about property rights in experiments only in the sense that the dictator was chosen at random and has no "natural" claim to ownership (Locke 1980[1690]); if property rights are characterized as "control rights" following Grossman and Hart (1986), then the dictator holds unambiguous property rights.

market integration is also strongly associated with ultimatum gave giving. Of course, this giving may be motivated either by altruism or by a fear of rejection. To explore internalized norms of enforcement — specifically, the willingness to reject unequal offers — the authors examine variation in the average minimum acceptable offer across (MAO) societies. MAOs range from 0.06 among both the Yasawa people of Fiji and the Kenyan Samburu to 0.38 among the Kenyan Gusii. Interestingly, the correlation between the MAO and the mean offer at the society level is quite low — only 0.161 — suggesting that both internal and external enforcement of fairness norms are important determinants of behavior in ultimatum games.

Henrich et al. (2010a) argue that punishing unfair or uncooperative behavior by strangers is critical in large populations, and indeed they find a strong correlation between community size and MAO. Interestingly, they also report that hyper-fair offers of more then fifty percent of the budget are rejected in many societies. The tendency to do so appears to be correlated with the level of giving observed in the dictator game: hyper-fair offers are never rejected only in the five societies with the lowest average dictator game offers. This suggests that dictators who share a large fraction of their budgets may be motivated by an aversion to inequality, which would also lead them to reject overly generous ultimatum game offers which create an uneven distribution of income.

Hermann et al. (2008) also document a willingness to punish generosity among university students in Asia, Europe, the Middle East, and North America. They conduct repeated public goods games in fifteen industrialized countries. Subjects in a number of countries are observed punishing those making relatively large contributions to the public good, though these punishments are often in retaliation (for previous punishment of low contributions). The authors use data from the World Values Survey to examine the association between behavior in the experiment and country-level norms of civic participation and respect for formal institutions. In particular, they regress the prevalence of "antisocial punishment" within the experiment on an index of responses to questions about whether tax evasion, fare dodging, and illegitimate benefits claims are ever justified. They find that respect for the

rule of law is negatively and significantly related to the frequency of antisocial punishment.

Though these cross-cultural patterns are striking, they raise as many questions about the evolution of informal institutions as they answer. In particular, though several studies discuss plausible mechanisms of cultural change, none report empirical evidence of a causal link. More broadly, the focus on cross-society comparisons means that observed differences may be due to variation in the genetic make-up of a population, its history and political culture, its ecological and economic environment, and formal institutions themselves.

# 4 How Informal Institutions Evolve within Societies

How do cross-cultural differences arise? If market integration — or any other factor — is causally related to differences in informal institutions across societies, we would also expect to observe similar correlation *within* societies. Moreover, evidence from a single society is more likely to convincingly demonstrate causal relationship: differences between societies result from the interplay of a variety of complex geographic and historical factors; it is often not feasible to disentangle such factors empirically. Within a community, *ceteris paribus* assumptions can be more reasonable, both because individual differences can be controlled for directly, and because it is sometimes possible to exploit exogenous variation in causal factors of interest.

To illustrate the potential for exploring variation in informal cultural norms within a society, I test Henrich et al.'s (2010a) hypothesis that market integration is related to giving in experiments using data from dictator games I conducted in seven rural communities in western Kenya in 2007. The experiments were designed to compare the willingness to share earned and unearned income in rural villages. The experimental design builds on numerous studies in university labs which suggest that dictators are less generous with earned income than with the unearned income typically used in experiments (Hoffman et al. 1994, Cherry et al. 2002). Fahr and Irlenbusch (2000) term this increase in entitlement which results from earning the budget an "earned property right." Prior to my experiments, this disparity had

not been documented in a non-undergraduate population; and I find only weak evidence of earned property rights in rural Kenya.[7]

I conducted dictator games which differed in terms of the source of the dictator's budget. In Luck treatments, dictators rolled a 20-sided die to determine the size of their budgets. In Effort treatments, dictators were paid a piece rate for completing a simple, but tedious task — collecting a specific type of dried bean from a bucket containing a variety of types. The Luck treatments allow us to test the hypothesis that market integration is associated with greater generosity in a standard dictator game, where neither player has a strong claim to the budget. The Effort treatments, on the other hand, allow for consideration of the relationship between market integration and generosity when income is earned and, consequently, ownership claims are clearer.

Experimental sessions took place in Busia District, a poor, predominantly rural area in western Kenya. Each experimental session took place in a different community.[8] A total of 272 subjects participated across all sessions. Table 1 provides summary statistics on the participants. The median subject is a 27 year old married man with less than eight years of formal education. Fewer than half the subjects — 48.5 percent — completed primary school, and only 14.3 percent completed secondary. The education level for the sample is therefore slightly below the average for the region: according to the 2008 round of the Kenya Demographic and Health Survey, 59.3 percent of males and 53.9 percent of females in rural Western Province completed primary school, and 19.6 percent of males and 16.7 percent of females completed secondary. Almost all subjects — 95.7 percent — are involved in subsistence agriculture, though many also engage in other labor market activities. The median subject is a Christian who belongs to the Luhya ethnic group, a Bantu-speaking people which form second largest ethnic group in Kenya and the majority group in Western Province. 66.5 percent of subjects in the sample are Luhya, and all but four of the remaining subjects are either Teso or Luo, two locally significant minorities. Subjects participate in

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[7]Additional results are reported in Jakiela (2011) and Jakiela (2009).

[8]A detailed description of the experimental protocol is provided in Jakiela (2009). Experimental instructions are available on the author's website.

an average of 2.0 community groups, the most popular being rotating savings and credit associations (ROSCAs), Bible study groups, and women's groups. 75.0 percent of subjects claim to trust their neighbors and family completely or somewhat, but only 40.8 percent trust people they've just met.

Table 2 reports summary statistics on experiment outcomes in the Luck and Effort treatments. The main outcome variable, *DG Offer*, is the fraction of the budget allocated by a dictator to her partner within the experiment. In my sample, dictators allocate their partners an average of 26.5 percent of the budget in the Luck treatments and 24.4 percent of the budget in the Effort treatments. Thus, though subjects allocate less of their budget to others when the money being divided is earned, the effect is quite small. However, a t-test of the equality of the means across treatments does reject the null hypothesis (p-value < 0.001). There is a similar small, but significant difference across treatments in the likelihood that a dictator allocates her partner keeps the entire budget for herself: subjects do this 15.1 percent of the time in the Luck treatments and 17.1 percent of the time in the Effort treatments. Differences in the likelihood of allocating one's partner exactly half the budget also move in the same direction, but they are not statistically significant.

My main goal is to examine the relationship between market integration and individual choices within the dictator game. As a proxy for market integration, I look at whether or not a community is located on a paved road. This measure differs from that used by Henrich et al. (2010a): they calculate the proportion of a household's total calories which were purchased. Unfortunately, their measure requires extremely detailed data which is unavailable to me *ex post*. To my knowledge, proximity to roads has not yet been used as a proxy for market exposure in the experimental literature; however, the relationship between access to paved roads and market integration has long been recognized (cf. Fafchamps 1992, Goletti et al. 1995). Though proximity to roads is quite likely to be correlated with market exposure, roads are an imperfect measure since they may also be correlated with other factors. We explore possible channels of impact and alternative interpretations further below.

Figure 1 presents histograms of the share of the total budget dictators allocated to their partners in the Luck and Effort dictator game treatments, disaggregated by proximity to the road. All four distributions are bimodal, with significant spikes at zero and fifty percent of the total budget. Looking first at the Luck treatments, it is apparent that the pattern observed across societies is also evident within this particular society: subjects in communities that are not on the paved road allocate their partners half the budget 11.4 percent of the time, while those near the paved road split the budget evenly 23.9 percent of the time. However, the pattern is reversed in the Effort treatments, where is is apparent that dictators in communities located on a road are *less* likely to split the budget evenly, doing so 11.0 percent of the time as opposed to 19.4 percent of the time in more remote areas.

To test the robustness of this finding and explore potential demographic confounds, I estimate the association between living near a paved road and giving in the dictator game in a regression framework. I estimate the OLS regression

$$DGOffer_{i,b} = \alpha + \beta Paved_i + \zeta Budget + \gamma X_i + \epsilon_{ib} \tag{1}$$

where $DGOffer_{i,b}$ is the fraction of the budget that subject $i$ allocates to the other player at budget size $b$,[9] $Paved_i$ is an indicator for residing in a community located on the paved road, $Budget$ is a control for the size of the budget, $X_i$ is a vector of individual controls such as gender and education level, and $\epsilon_{ib}$ is a mean-zero error term. Standard errors are clustered at the subject level in all specifications.

In the OLS regression with only budget size controls, the coefficient on $Paved$ is positive and significant in the Luck treatments (Column 1, Table 3). The point estimate suggests that being in a community located near the road increases the amount allocated to one's partner by 7.1 percentage points, relative to a base level of $DGOffer_{i,b}$ of approximately 31

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[9]The strategy method was used to record individual decisions within the experiment, so each subjects indicated her preferred division of the a series of feasible budgets.

percent of the budget. Including controls for gender, marital status, educational attainment, and ethnic group increases the coefficient estimate for the *Paved* indicator to 8.0 percentage points. The coefficient is significantly different from zero at the 95 percent confidence level in both specifications.

As the histograms suggest, the pattern is reversed in the Effort treatments. OLS regressions of giving in the Effort dictator games with and without controls are reported in Columns 3 and 4, respectively (Table 3). Here, the coefficient on *Paved* is consistently negative and significant at the 90 percent confidence level. Including the controls has little impact on the estimated coefficient magnitude: the point estimate is $-0.41$ without controls and $-0.42$ when they are included. Thus, the association between proximity to a paved road and giving in dictator games appears context-specific, and disappears once entitlements within the experiment become less ambiguous.

An important question is whether market integration impacts the proportion of subjects who behave in a self-interested manner or, alternatively, induces those sharing a positive amount to give more (or less). Figure 1 suggests that proximity to a paved road may shift dictators to (or away from) the particularly salient 50-50 split of the budget. To test these hypotheses, I estimate Probit regressions of the likelihood of allocating the other player nothing (Table 4, Panel A) and exactly one half of the budget (Table 4, Panel B).

Probit regressions of the indicator for giving nothing to the other player do not suggest a significant association with proximity to a paved road in either the Luck or Effort treatments. The coefficient estimate for the *Paved* indicator is negative but insignificant in all specifications (Table 4). Thus, my proxy for market exposure does not appear to influence the probability of behaving in a strictly self-interested manner. In contrast, proximity to a paved road is strongly associated with the likelihood of allocating the other player exactly half the budget. In Luck treatments, the *Paved* indicator is positive and statistically significant with and without controls; it is negative and significant in the Effort treatments. The coefficient estimate indicates that being in a community on the paved road is associated

with a 13.5 percent increase in the probability of splitting the budget evenly in the Luck treatments, but a 9.2 percentage point decrease in the likelihood of doing so in the Effort treatments.

Thus far, I have replicated and extended Henrich et al.'s (2010a) cross-society findings within a single cultural environment. As in their work, my proxy for market exposure is positively and significantly associated with giving in standard dictator games. However, I find that the market integration proxy is negatively and significantly associated with DG giving when the dictator earns the budget she divides. This is consistent with the interpretation that markets induce generous behavior when entitlement claims are ambiguous, but do not lead to greater altruism in all contexts. My results also suggest that these associations between proximity to a paved road and DG giving are not driven by significant differences in the proportion of strictly self-interested subjects; rather, the patterns suggest that those who share a positive amount use a different calculus when determining how much to allocate the other player.

One concern about my results is that proximity to the paved road may be correlated with characteristics other than market integration. In fact, this critique extends beyond the present research to all studies, including the results presented in Henrich et al. (2010a), which explore associations between variables without a credible source of exogenous variation in the independent variable of interest. For example, while Henrich et al. (2010a) control for age, sex, education, income, wealth, household size, and community size, it is impossible to control for, or even measure, all possible confounds. Market integration may be correlated with community group participation, numeracy, comprehension of the structure of the experiment, a desire to conform to the experimenter's expectations, access to credit, and unobserved innate abilities (such as cognitive ability), all of which may have an impact on behavior within the dictator game. Moreover, the correlation between exposure to markets and giving in the dictator game can never tell us whether markets lead to greater generosity, or markets arose in those locations initially characterized by altruism and interpersonal trust.

Thus, the aim of this paper is not to make a causal case, but to document the cross-cultural associations observed in previous work within an individual society. However, in an attempt to shed some light on the possible mechanisms underlying the observed association between my proxy for market exposure, proximity to a paved road, and behavior within the dictator game, I present the results of a series of regressions on individual characteristics on the *Paved* indicator, controlling for age and gender. Results are presented in Table 5. The outcomes I consider are completion of primary school, completion of secondary school, whether a household farms, the number of community groups to which a subject belongs, and an index of trust, which combines answers to questions on whether one trusts one's neighbors, one's family, and people one is meeting for the first time.

Unsurprisingly, the point estimate suggests that those living near a road are slightly less likely to farm (Table 5, Column 3); however, this result is not significant. More interestingly, those near roads are not significantly more likely to complete primary or secondary school than other subjects, and both estimated coefficients, while positive, are quite small (Table 5, Columns 1 and 2). This absence of an impact on education is particularly intriguing since, as we discuss in more detail below, education is often seen as one of the primary drivers of modernization and the adoption of market and science oriented values (Inkeles 1969). Proximity to the paved road is positively correlated with both participation in community groups and the trust index, though only the later result is statistically significant, and then only at the 90 percent confidence level (Table 5, Columns 4 and 5). Though only suggestive, these results suggest that my proxy variable is likely correlated with various forms of social capital. However, whether this represents a confound associated with my proxy for market exposure or a potential causal pathway underlying both my results and those of others is an open question.

# 5 Conclusions

The evidence presented thus far demonstrates the use of experimental economic methods to measure the "self imposed codes of conduct" underlying informal institutions, and the variation in these moral preferences within and across societies. More specifically, I have shown that giving in dictator games is significantly associated with market integration, or some factor correlated with it, in rural western Kenya, just as it is when one looks across cultures around the world. However, I have not yet provided a satisfactory answer to the question of causal identification. In fact, very little work to date has done so.

One exception is Jakiela et al. (2010). That paper, like this one, explores variation in individual social preferences in rural Kenya. In that case, we exploit the exogenous variation in success in primary school that resulted from a randomized trial of a scholarship program for girls in western Kenya. Social scientists have long hypothesized that formal schooling is causally related to the emergence of "modern" values, and empirical studies demonstrate a robust correlation (cf. Inkeles 1969). We show that random assignment to the scholarship treatment group is a strong predictor of performance on the Kenyan primary school leaving exam, allowing us to use an instrumental variables approach to estimate the impact of academic achievement on social preferences. We recruit a sample of girls from the treatment and control groups in the scholarship program to participate in a variant of the Effort dictator game treatments describe above which allow dictators to take from other players' earnings. We find that academic success is causally related to respect for earned property rights in the economic experiments.

The study is one of the first to exploit the exogenous variation resulting from field experiments to isolate the causal mechanisms of cultural change. It is closely related to Di Tella et al. (2007), who use a *natural* experiment to create exogenous variation in property rights and estimate the impact of land ownership on individual beliefs. Studies such as these, which focus on the micro-level to demonstrate how informal institutions change over time, are the next step to understanding how to identify and strengthen the types of infor-

mal institutions which promote development. Equally importantly, these studies translate some of the fundamental insights of Douglass North into the empirical language of modern development economics. Just as the work of Acemoglu et al. (2001) brought a new focus on institutions to the literature on economic growth, empirical micro-level studies of the correlates and determinants of values and internalized constraints on behavior can shed light the links between behavioral economics, informal institutions, and economic development.

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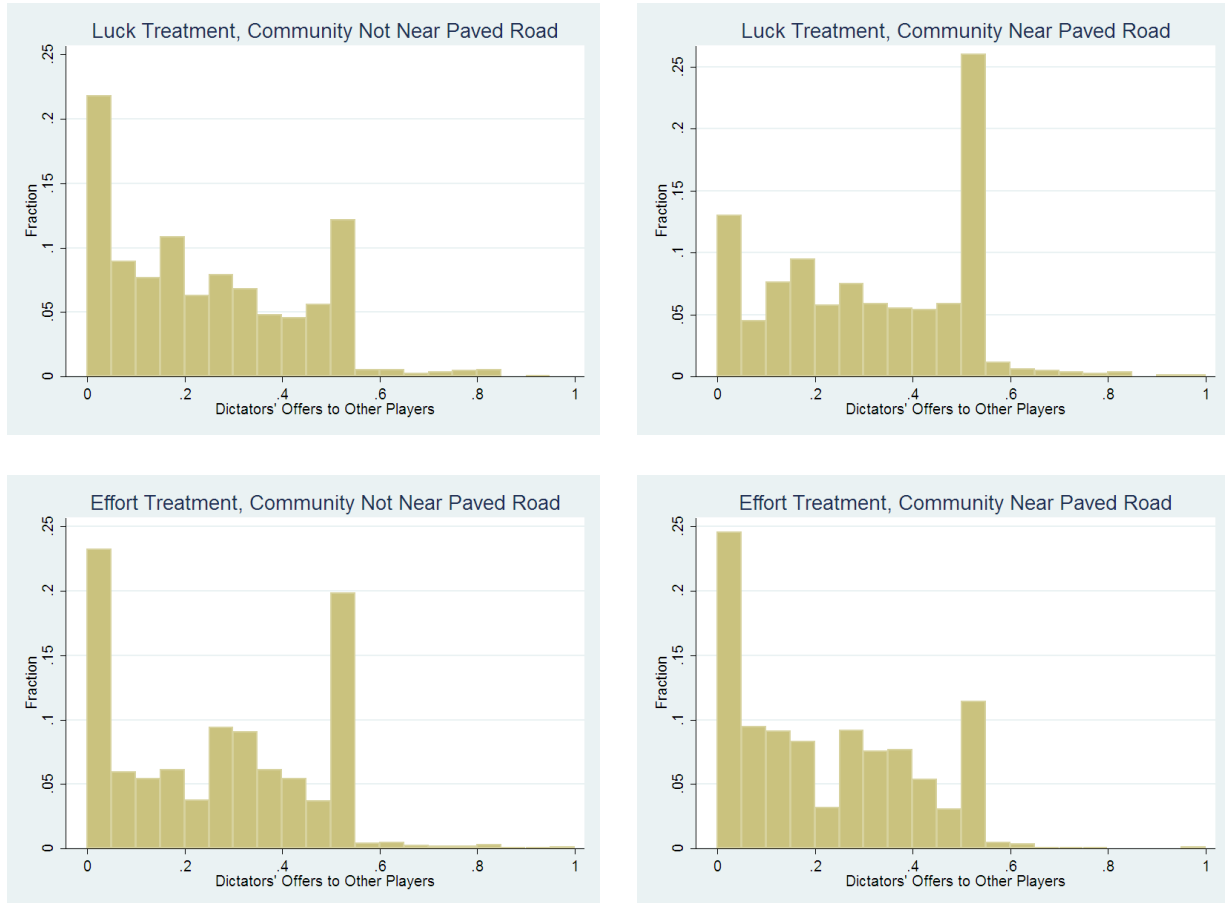Figure 1: **Share of Budget Allocated to Other Player**

Table 1: **Summary Statistics**

| Variable | Mean | S.D. | Median | N |
|---|---|---|---|---|
| Female | 0.404 | 0.492 | 0 | 272 |
| Married | 0.697 | 0.460 | 1 | 271 |
| Household Size | 5.871 | 2.448 | 5 | 271 |
| Age | 27.325 | 5.286 | 27 | 271 |
| Completed Primary School | 0.485 | 0.501 | 0 | 272 |
| Completed Secondary School | 0.143 | 0.351 | 0 | 272 |
| Farm Household | 0.956 | 0.206 | 1 | 272 |
| Luhya Ethnic Group | 0.665 | 0.473 | 1 | 272 |
| Teso Ethnic Group | 0.283 | 0.451 | 0 | 272 |
| Luo Ethnic Group | 0.037 | 0.189 | 0 | 272 |
| Christian | 0.974 | 0.159 | 1 | 272 |
| Community Groups | 2.011 | 1.674 | 2 | 272 |
| Trusts Neigbors and Kin | 0.750 | 0.434 | 1 | 272 |
| Trusts Strangers | 0.408 | 0.492 | 0 | 272 |
| Near Paved Road | 0.452 | 0.499 | 0 | 272 |

Table 2: **Dictator Game Outcomes**

| Outcome | Luck Treatments | Effort Treatments |
|---|---|---|
| Partner Share | 0.265 | 0.244 |
| | (0.194) | (0.190) |
| Partner Share Zero | 0.151 | 0.171 |
| | (0.359) | (0.377) |
| Partner Share Half | 0.161 | 0.152 |
| | (0.367) | (0.359) |

Standard errors in parentheses.

Table 3: **OLS Regressions of DG Offer on Proximity to Road**

*Dependent Variable = DG Offer*

| Treatment: | Luck | Luck | Effort | Effort |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Community on Paved Road | 0.071** | 0.080** | -0.041* | -0.046* |
| | (0.033) | (0.033) | (0.024) | (0.024) |
| Female | . | -0.006 | . | -0.025 |
| | | (0.031) | | (0.026) |
| Age | . | 0.002 | . | 0.001 |
| | | (0.004) | | (0.003) |
| Married | . | -0.06 | . | 0.044 |
| | | (0.043) | | (0.03) |
| Completed Primary School | . | 0.024 | . | 0.017 |
| | | (0.037) | | (0.027) |
| Completed Secondary School | . | -0.023 | . | 0.046 |
| | | (0.048) | | (0.047) |
| Teso | . | 0.044 | . | -0.029 |
| | | (0.041) | | (0.025) |
| Other Ethnic Group | . | 0.063 | . | -0.082* |
| | | (0.054) | | (0.045) |
| Observations | 2140 | 2140 | 4946 | 4886 |
| $R^2$ | 0.041 | 0.064 | 0.04 | 0.079 |
| Budget Controls | Yes | Yes | Yes | Yes |
| Constant | Yes | Yes | Yes | Yes |

Coefficients significantly nonzero at .99 (***), .95 (**) and .90 (*) confidence levels. Standard errors clustered at the player level in all specifications.

Table 4: **Probit Regressions of DG Giving on Proximity to Road**

| Treatment: | LUCK | LUCK | EFFORT | EFFORT |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Panel A: Dependent Variable = DG Offer Zero* | | | | |
| Community on Paved Road | -0.334 | -0.373 | -0.156 | -0.156 |
| | (0.265) | (0.265) | (0.174) | (0.178) |
| Female | . | 0.198 | . | 0.069 |
| | | (0.233) | | (0.195) |
| Age | . | -0.016 | . | -0.01 |
| | | (0.026) | | (0.019) |
| Married | . | 0.477 | . | -0.034 |
| | | (0.349) | | (0.223) |
| Completed Primary School | . | 0.039 | . | 0.3* |
| | | (0.273) | | (0.178) |
| Completed Secondary School | . | -0.054 | . | -0.696** |
| | | (0.373) | | (0.298) |
| Teso | . | -0.578* | . | 0.171 |
| | | (0.348) | | (0.178) |
| Other Ethnic Group | . | -1.323*** | . | 0.51 |
| | | (0.458) | | (0.369) |
| Observations | 2140 | 2140 | 4946 | 4886 |
| Pseudo $R^2$ | 0.015 | 0.055 | 0.003 | 0.032 |
| *Panel B: Dependent Variable = DG Offer Exactly Half* | | | | |
| Community on Paved Road | 0.497** | 0.579** | -0.373** | -0.426** |
| | (0.225) | (0.234) | (0.175) | (0.183) |
| Female | . | -0.001 | . | 0.12 |
| | | (0.231) | | (0.188) |
| Age | . | 0.043* | . | 0.006 |
| | | (0.025) | | (0.018) |
| Married | . | -0.392 | . | 0.181 |
| | | (0.288) | | (0.204) |
| Completed Primary School | . | 0.335 | . | 0.201 |
| | | (0.274) | | (0.197) |
| Completed Secondary School | . | -0.193 | . | -0.027 |
| | | (0.31) | | (0.381) |
| Teso | . | 0.227 | . | -0.149 |
| | | (0.309) | | (0.19) |
| Other Ethnic Group | . | -0.45 | . | -0.31 |
| | | (0.291) | | (0.369) |
| Observations | 2140 | 2140 | 4946 | 4886 |
| Pseudo $R^2$ | 0.032 | 0.06 | 0.049 | 0.062 |
| Budget Controls | Yes | Yes | Yes | Yes |
| Constant | Yes | Yes | Yes | Yes |

Coefficients significantly nonzero at .99 (***), .95 (**) and .90 (*) confidence levels. Standard errors clustered at the player level in all specifications.

Table 5: **OLS Regressions of Individual Characteristics on Proximity to Road**

| Outcome: | PRIMARY SCHOOL | SECONDARY SCHOOL | HOUSEHOLD FARMS | COMMUNITY GROUPS | TRUST INDEX |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | |
| Community on Paved Road | 0.007 | 0.012 | -0.033 | 0.28 | 0.162* |
| | (0.061) | (0.043) | (0.025) | (0.2) | (0.09) |
| Female | 0.029 | 0.009 | -0.003 | 0.283 | -0.106 |
| | (0.062) | (0.043) | (0.026) | (0.209) | (0.09) |
| Age | -0.008 | -0.009* | 0.003 | 0.068*** | 0.009 |
| | (0.006) | (0.005) | (0.002) | (0.02) | (0.009) |
| Constant | 0.699*** | 0.373*** | 0.883*** | -0.089 | 0.882*** |
| | (0.165) | (0.13) | (0.065) | (0.553) | (0.248) |
| Observations | 271 | 271 | 271 | 271 | 271 |
| $R^2$ | 0.009 | 0.018 | 0.014 | 0.061 | 0.023 |

Coefficients significantly nonzero at .99 (***), .95 (**) and .90 (*) confidence levels. Standard errors clustered at the player level in all specifications.