Three Essays on Field and Lab-in-the-Field Experiments in Economics

by

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PREFACE

The second chapter of my dissertation has been co-authored by Gael Raballand, Rebecca Thornton, Dean Yang, Jessica Goldberg, and Niall Keleher.

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ABSTRACT

The first chapter contributes to the debate about culture as potential determinant of economic growth by investigating the prevalence of six widely discussed norms of distributive justice in poor, rural communities of Malawi. Specifically, the distribution over strict egalitarianism, inequality aversion, liberal egalitarianism, luck egalitarianism, libertarianism, and the principle of equality of opportunity is determined using a lab-in-the-field experiment. I am using a two-person dictator game with a production phase. I find that decisions are mainly guided by strict egalitarianism and libertarianism, and less strongly by other norms. Interestingly, despite the large influence of strict egalitarianism, subjects react significantly to all experimental variations.

The second chapter draws lessons from an original randomized experiment in Malawi. In order to understand why roads in relatively good condition in rural areas may not be used by buses, a minibus service was subsidized over a six month period over a distance of 20 kilometres to serve five villages. Using randomly allocated prices for use of the bus, this experiment demonstrates that at very low prices, bus usage is high. Bus usage decreases rapidly with increased prices. However, based on the results on take-up and minibus provider surveys, this experiment demonstrates that at any price, low (with high usage) or high (with low usage), a bus service provider never breaks even on this road.

The third chapter explores the effect of incomplete information about pie size on social norms motivated by the following observations: (a) cultural norms regarding distributive justice may be fully internalized by a population or be domain-specific

to a particular informational environment and (b) each scenario may lead to different impacts on growth and development in dynamic surroundings. The chapter studies how divisions of an effort-generated social surplus depend on whether the receiver in a dictator game knows the size of the social surplus or not. I find that while dictators act more selfish under incomplete information the effect is small compared to the effects norms of distributive justice have on decision making.

CHAPTER I

Norms of Distributive Justice in Rural Malawi

1.1 Introduction

Throughout the world, across all societies, individuals engage in various forms of redistribution, be it unemployment insurance benefits from taxes paid by employers in the USA, the meticulous distribution of the spoils of a hunt among the Dobe !Kung of the Kalahari Desert,[1] or the provision of financial support by friends in times of hardship. As the examples suggest, redistribution can be organized through formal or semi-formal institutions or occur in entirely informal settings. Its different aspects have been studied by researchers from disciplines as diverse as law, anthropology, sociology, or economics. In economics, a lot of emphasis has been placed on the rigorous analysis of the incentives for productive activities set by redistributive systems.

Arguably, the more informal the setting in which redistribution takes place, the more can be learned about these incentives by studying the underlying norms of distributive justice of the population. Due to the relative paucity of formal institutions in developing countries, particularly in *rural* areas, the study of sharing norms among African villagers is therefore highly suitable to inform the long standing and con-

[1] See, for example, Lee (1993) for a description of the social life of the Dobe !Kung, including their sharing rituals.

troversial debate about culture as one key determinant of economic development,[2] beyond providing an answer to the question of what constitutes distributive justice in the minds of the study population, which, by itself, may be highly relevant for enriching models in development economics.

Specifically, this paper studies the distribution of six frequently discussed norms of distributive justice in one of the poorest regions of Sub-Saharan Africa. It informs the debate about the relationship between culture and economic growth by investigating the prevalence of strict egalitarianism, inequality aversion, liberal egalitarianism, luck egalitarianism, libertarianism, and the principle of equality of opportunity for a representative population from villages of Malawi's Central Region. To this end, a lab-in-the-field experiment in form of a one-shot two-person dictator game with production stage was employed. In addition to the experimental data, I collected baseline information on demographic and socio-economic characteristics of the participants.

The outcomes of the game reveal how effort-generated income is shared between individuals who may differ with respect to *assigned* rates of return, income shocks, and opportunities to exert effort, as well as *chosen* effort levels. Randomly selected participants were assigned to one of seven treatments. The treatments differed with respect to how many effort levels were available to the participants and whether their initial endowment could be reduced by a random income shock. In the "Benchmark Treatment", neither could happen to either player. In the three "Equality of Opportunity Treatments", either the dictator, or the receiver, or both players faced a reduced choice set of effort levels, i.e. either one or both players could not exert the highest level of effort available to the players in the Benchmark Treatment. In the "Income Shock Treatments", either the dictator, or the receiver, or both players

[2]See Weber (1992) for a seminal paper.

faced a negative income shock to their monetary endowment which was made known to both players just prior to the distribution of the pie.

Common across all treatments was the way social surplus (i.e. the pie to be shared between dictator and receiver) had to be generated: Subjects needed to separate two types of beans. This effort task was not only familiar to the participants, but also to the greatest possible extent free of any inherently motivating aspects, independent of skill, level of practice,[3] education, gender, and other demographic or socio-economic characteristics (based on piloting outcomes).[4] Another common aspect among all treatments was that participants could be assigned either a high or low rate of return for their effort. This experimental variation was introduced to be able to distinguish between contribution-rewarding norms and primarily effort-rewarding norms.

My results suggest that dictators are not merely concerned about their own income, but also take several other factors into account when making distributional choices: First, dictators react as intuitively expected to *all* (exogenous) experimental variations, i.e. dictators reward higher rates of return, and take reduced choice sets and income shocks into account when making their decisions. Second, dictators clearly reward both own and others' effort. Third, despite the first two findings, the highest percentage of observations *and* individuals must be categorized as strict egalitarian. Finally, demographic and socio-economic characteristics generally matter little for determining a player's overall generosity, or which norm predictions her decisions match.

Though this study relates to classical empirical work on culture and economic growth,

[3]The average time it took the author and members of her data entry staff to sort a specific amount of beans did not differ significantly from the average time it took an "experienced" female participant to sort the same amount of beans.

[4]The only potential exception is high age. A minority among the elderly participants had to exert more effort than the average participant for sorting the same amount of beans, due to limited mobility. I will return to and discuss this observation in section 4, during data analysis.

the approach taken puts it more in line with the growing experimental evidence shedding light on the nature of social norms in developing countries, a line of research that emerged with the application of behavioral economics to the development literature. Most of this experimental evidence, however, pertains to *unearned* income (see Camerer (2003) for a survey of dictator game application). Since it is conceivable that different norms are applied to windfall as compared to earned or, more specifically, effort-generated income, the transferability of most of the current evidence to typical growth-relevant economic situations may be very limited, i.e. the majority of documented behavior might be highly domain-specific and less relevant to learning about incentives for productive activities in a community.[5]

One notable exception is the recent study of Jakiela (2009) who investigated how subjects in rural Kenya divide windfall as opposed to effort-generated income in comparison to U.S. American subjects. For this purpose, she employed four different types of dictator games which can be categorized according to two questions: (a) How the social surplus was generated (whether through rolling a dice or through subjects' effort), and (b) who generated the social surplus (i.e. dictator or receiver). She finds that subjects in both locations clearly reward their own effort. However, the evidence for the Kenyan sample on the question of whether subjects also reward others' effort is mixed. This can be clearly established only for the U.S. sample. Also, while her study is closest to this research in underlying motivation, it is not aimed at distinguishing various norms of distributive justice, nor at highlighting any potential trade-off between selfishness and fairness as will be established in section 4 of this paper.

[5]In addition, this literature is of limited usefulness to inform any discussion about norms of distributive justice (as compared to sharing behavior) since only few such norms are defined on windfall income.

More generally, the present study is closer in design to experiments that have been conducted in developed countries regarding effort rewarding behavior of individuals (see Konow (2000) for a prominent example of this sub-branch of the literature). Specifically, the experimental design is based on the work of Cappelen et al. (2007) who employ one-shot two-player dictator games and find that out of those 35 percent of their subjects who mostly act in line with their ideas of fairness, 43.5 percent are strict egalitarians, 38.1 percent are liberal egalitarians, and 18.4 percent act libertarian. I modify and extend the design in Cappelen et al. (2007) in several important ways. First, investment is replaced by a simple effort task for the study of strict egalitarianism, libertarianism, and the traditionally effort-defined principle of liberal egalitarianism. Second, whether people consider equality of opportunity is determined by adding novel treatments: restrictions on the opportunity to exert effort are introduced for subjects assigned to these treatments. Third, luck egalitarianism is explored. It is based on income differences, which are *not* due to production choices. Thus, I bring in various income shock treatments to investigate whether this principle plays a role in decision making. Fourth, the information structure of the game is altered to credibly test for a simple version of inequality aversion, defined as individuals striving for exactly equalizing *final payoffs* (rather than merely sharing the social surplus equally). Fifth, dictators' preferences are elicited using the strategy method, which allows me to talk about self-consistency of individuals. Sixth, I link the experimental results to a rich set of demographic and socio-economic data. Last, I study the norms of randomly chosen participants from Malawian villages, as opposed to a student population in an industrialized country. Arguably, the former has more significance for enriching the discussion about norms and incentives set for productive activities in a society due to the widespread lack of formal institutions in rural Malawi, as laid out above.

The rest of the paper is organized as follows. Section 2 describes the structural model and provides definitions of the norms of distributive justice. In section 3, a discussion of the experimental design is provided. Section 4 offers an overview of the field experimental procedures. Section 5 discusses empirical results. Section 6 concludes.

1.2 Model

The subsequent discussion closely follows Cappelen et al. (2007). I study two-person dictator games with a production phase. Individual i is denoted $i = 1$ when i is the dictator and $i = 2$ when i is the receiver. The players may differ with respect to effort chosen, e_i, and rate of return, a_i. Dictators and receivers both participate in production which takes place using the function $x_i = e_i a_i$. Thus, the dictator's and the receiver's effort-generated income is determined by the production functions $x_1 = e_1 a_1$ and $x_2 = e_2 a_2$, respectively. The total effort-generated income[6] that is to be distributed among team members by the dictator is therefore given by $X(e, a) = x_1(e_1, a_1) + x_2(e_2, a_2)$, where e and a are defined as $e = (e_1, e_2)$ and $a = (a_1, a_2)$. Specifically, each dictator is asked to allocate an amount y of the social surplus to herself, leaving $X - y$ for the receiver. Dictators and receivers have additional income in the form of their endowments, and through foregoing options to exert effort as will be explained in detail in section 3. For modeling purposes, it suffices to denote the entirety of their respective non-effort-generated incomes as z_1 and z_2, with $z = (z_1, z_2)$.

Based on the framework in Cappelen et al. (2007), I assume that individuals care about both their own income and the fairness of the distribution between themselves

[6]Total effort-generated income is also referred to as "social surplus" or "money in the common pot" throughout this paper.

and the player matched with them, with dictators maximizing the utility function

$$V(y; e, a, z, b, s) = \gamma y - \sum_k \beta_k \frac{(y - m^k(e, a, z, b, s))^2}{2X(e, a)}.$$

Here, $m^k(e, a, z, b, s)$ is the fair amount of effort-generated income that the dictator should keep for herself according to norm k; b_1 and b_2 are the sets of effort choices available to the players with $b = (b_1, b_2)$; $\gamma > 0$ and $\beta_k \geq 0$ are parameters expressing the importance subjects assign to income and fairness, respectively; $s = (s_1, s_2)$ denotes negative shocks to z,[7] where $s_1 \leq 0$ and $s_2 \leq 0$.

Maximizing this function determines the optimal share y^* to be

$$y^* = \frac{\sum_k \beta_k m^k(e, a, z, b, s)}{\sum_k \beta_k} + \frac{\gamma X(e, a)}{\sum_k \beta_k},$$

assuming that an interior solution exists. Note, that the optimal amount a specific dictator allots to herself depends on her fairness ideals and how much money is in the common pot.

Expanding on the set of norms studied in Cappelen et al. (2007), I assume that every dictator follows at least one of the following principles of distributive justice: strict egalitarianism (SE), inequality aversion (IA), liberal egalitarianism (LE), the principle of equality of opportunity (EO), libertarianism (L), or luck egalitarianism (LuE), all of which satisfy the no-waste condition.[8] These concepts are explained below, using the optimal dictator share they imply.

According to strict egalitarianism it is optimal that dictators and receivers receive

[7] Note, that these shocks are known to the dictator prior to making any distributional decisions.

[8] Strict egalitarianism, liberal egalitarianism, and libertarianism are specified in a similar way to that found in Cappelen et al. (2007), but with effort replacing investment.

equal shares of the social surplus. The fair share for a dictator can thus be denoted as

$$m^{SE}(e, a, z, b, s) = \frac{X(e, a)}{2}. \tag{1.1}$$

The principle of liberal egalitarianism bases optimal shares on the relative effort choices of the players, hence,

$$m^{LE}(e, a, z, b, s) = \frac{e_1}{e_1 + e_2} X(e, a). \tag{1.2}$$

In contrast to liberal egalitarianism, libertarianism is a purely outcome based principle. The dictator's optimal share equals her effort-generated income:

$$\begin{aligned} m^{L}(e, a, z, b, s) = \frac{a_1 e_1}{a_1 e_1 + a_2 e_2} X(e, a) \\ = a_1 e_1. \end{aligned} \tag{1.3}$$

The principle of equality of opportunity alters the latter two principles' outlook in that it allows for choice sets to be taken into account. According to this norm, individuals should compensate themselves and others for reduced possibilities to exert effort, if in general they follow a norm that includes effort as a sharing criterion and is defined over the effort-generated income alone. The fair share for the dictator can be characterized as

$$m^{EO}(e, a, z, b, s) = m^{l} + \delta^{+}_{EO}(I_{DRC}) + \delta^{-}_{EO}(I_{RRC}), \tag{1.4}$$

where $l \in \{LE, L\}$, and $\delta^{+}_{EO}(I_{DRC}) \geq 0$ and $\delta^{-}_{EO}(I_{RRC}) \leq 0$ are parameters that determine how much the dictator cares about the relative size of choice set, where DRC stands for only the dictator having a reduced choice set and RRC stands for

only the receiver having a reduced choice set. A relatively smaller choice set implies a larger fair share for the affected player according to this norm. Note, that we only know the direction but not the magnitude of this effect.

Inequality aversion in its simplest form requires total incomes of players to be equalized in an optimal allocation. The fair share for the dictator is given by

$$m^{IA}(e, a, z, b, s) = (z_2 - z_1) + \frac{X(e, a) - (z_2 - z_1)}{2} \tag{1.5}$$

if $z_1 \leq z_2$, and

$$m^{IA}(e, a, z, b, s) = \frac{X(e, a) - (z_1 - z_2)}{2} \tag{1.6}$$

if $z_1 > z_2$.

In addition, a specific form of luck egalitarianism may influence distributional decisions. In this case, the fair share for the dictator additionally depends on s, i.e. the dictator takes differences in income which are *independent* of productive choices into account (and occur ex post to the latter being carried out):

$$m^{LuE}(e, a, z, b, s) = m^{j}(e, a, z, b, s) + \delta^{+}_{LuE}(I_{DS}) + \delta^{-}_{LuE}(I_{RS}). \tag{1.7}$$

Here, $j \in \{SE, LE, LL, L\}$, and $\delta^{+}_{LuE}(I_{DS}) \geq 0$ and $\delta^{-}_{LuE}(I_{RS}) \leq 0$ are parameters that determine how much the dictator cares about the relative shock to players endowment, where DS stands for only the dictator receiving a shock to her monetary endowment and RS stands for only the receiver receiving a shock to her monetary endowment. A one-sided negative shock implies a larger fair share for the affected player according to this norm. Again, we only know the direction but not the magnitude of this effect, which is an empirical matter.

1.3 Experimental Design

The analysis in this paper is based on seven treatments of a one-shot, two-person dictator game with a production stage.[9] Participants were randomly selected among the rural population of Ntchisi District in the Central Region of Malawi and assigned to different treatments prior to the instruction phase. During the recruitment and consenting phase, subjects were informed that they were eligible to participate in a scientific experiment about community norms, including survey parts. More specifically, individuals were told that the experiment would involve decision making and potentially carrying out a simple task, similar to one that they might do at home or work, as well as the distribution of money between themselves and another participant of the experiment. They were informed that they would receive a token gift worth approximately 30 Malawi Kwacha (MK) irrespective of their or others' decision-making (including the decision to end participation early) and between 0 MK up to 350 MK[10] depending on the outcome of the experiment. Additionally, potential participants learned that total participation time (including travel, consenting, experiment, surveys, and payment) would not exceed three hours.[11]

To assess the value of the incentives to participants, note that in 2005, 46.7 percent of the population in the Central Region lived at or below the national poverty line of 16,165 MK per year (i.e. approximately 44.29 MK per day) according to the World Bank (2007). 16.1 percent of these individuals fell substantially below this line and were classified as "ultra-poor", where ultra-poor indicates the inability of individuals to meet their recommended daily food needs. In addition to the generally

[9]The entire experiment consisted of eight treatments. The analysis of the eighth treatment in conjunction with the benchmark treatment is the subject of Mueller (2011a), which studies the effect of incomplete information the receiver has about the cake size on distributional decisions of the dictator (see,f.ex., Ockenfels and Werner (2011), for related literature).

[10]At the time of the experiment, 350 MK corresponded to 2.18 US-Dollar (typical cash bid rate).

[11]De facto participation time was approximately 1.5 hours, see section 4. Three hours was mentioned as an upper limit based on an outlier during piloting.

prevailing deep poverty, note that the data used in this paper was collected in July and August 2010. Both months fall in the dry season which is characterized by especially low, constant opportunity costs for participants. Goldberg (2010), for example, who conducted a labor supply study in Malawi's Central Region, offered various wages for sessions of hard physical labor, and found that over 70 percent of her sample of 529 subjects chose to work at a rate of only 30 MK *per day* during the dry season. Thus, the rewards provided by my experiment were substantial and clearly able to meet the participation constraint of most individuals in the study region.

In terms of incentivizing strategic behavior we need to understand whether variations of shares were meaningful to participants. I allowed dictators to vary shares in steps of 10 MK. Goods prices in the study region start at 0.5 MK, to the author's knowledge. However, in an informal focus group discussion conducted by study staff, it became clear that variations of up to 5 MK were not uniformly perceived as meaningful by individuals living in immediate proximity to the second biggest trading center of the district (i.e. these individuals are likely to, on average, care less about small variations of income as compared to the average study participant). To be on the safe side, I doubled this amount so that the smallest possible variation in the experiment was 10 MK.[12] Given the extreme level of poverty of large parts of the population in the study region, in combination with my findings from the focus group discussion, we can be confident that variations available to dictators were large enough to incentivize strategic behavior.

The rest of this section proceeds as follows. I begin by detailing the general experimental procedure. I then turn to a description of the benchmark treatment, which is based on the experimental design of Cappelen et al. (2007), and elaborate how it

[12]Examples of goods priced 10 MK in rural areas of the study district (at the time the study took place) are a large piece of bread or a package of pain killers.

allows for the identification of strict egalitarianism, liberal egalitarianism, libertarianism and inequality aversion. I then introduce new measures of equality of opportunity and luck egalitarianism, captured in the remaining six treatments. A description of the respective contributions of each of these treatments towards identifying the presence of these two norms concludes.

1.3.1 General Experimental Procedure

For an overview of the steps involved in the experiment, please also refer to Figure 1 in the appendix. At the beginning of the instruction phase, subjects learned that they were matched with another subject in the same location and that matching was anonymous both during and after the experiment. Then they received their endowments. Players' monetary endowments were given to them in the form of bottle caps, with one bottle cap representing 10 MK.[13] This conversion rate was made known to the players. Their non-monetary endowment was handed out in form of unsorted bags of beans. Players were informed that one bag of beans was worth 20 MK if they chose to return it to the experimenter without sorting. They learned that the alternative was to return either one or - if they had more than one bag - all of their bags sorted. Great care was taken to ensure that subjects understood that the money generated by sorting was higher than that from returning unsorted bags, but that the former would go into a common pot to be shared between them and the player matched with them, while the latter would be their own with certainty. Players were assigned either a low (40 MK per sorted bag) or high (80 MK per sorted bag) rate of return. Individuals learned their own, but not their partner's rate of return.

Players were then informed that just one subject in each group would be asked to

[13] A higher divisibility of currency was possible only to a limited extent. Since, in addition, denominations lower than 10 MK did not appear to be economically significant to *all* potential subjects, as explained above, the option of including those was not explored any further.

make sharing decisions but it would only be revealed *who* were to be the dictators after all sorting decisions had been carried out. With this important exception, the strategy method was used (as detailed further in section 3), i.e. dictators would be asked to share hypothetical common pots for all potential effort-choice/rate of return combinations of the receiver *given* their own effort choice and rate of return.[14] Payments were to be determined by the experimenter upon completion of the game and would be based on the strategies specified by the dictators and the matched receivers' sorting decisions and rates of return, i.e. the experimenter would look up what each dictator's payment plan specified for the *actual* number of bags sorted and rate of return of the respective receiver and pay players accordingly. Subjects were told that they would receive payment immediately after their participation in the study concluded and were informed that all players would learn about their partner's sorting decision and rate of return at that point. It was further conveyed that the receiver would only learn about the payoff relevant decisions of the dictator, but not have the right to learn about the dictator's entire strategy.

Summing up, players were given complete information about the game, including production, distribution, and payment phase, prior to any decision making. Importantly, the instruction phase was also designed to guarantee common knowledge among matched players. Following the instruction phase and a brief test-sorting,[15] players were asked to make their sorting decisions. After they carried out these de-

[14]The alternative would have been to elicit sharing preferences from both players for all possible effort/rate of return combinations of their partner, *then* randomly determine who is to be the decision maker. Though this would have had obvious logistical and financial benefits, in the 30+ rounds of piloting for this experiment, evidence mounted that subjects viewed the game entirely differently if randomization between players for the role of dictator took place *after* sharing decisions were made. According to their own statements, subjects frequently kept less (more) than what they would have perceived as optimal, had they been the dictator with *certainty*, when they thought their opponent might be generous (selfish). In other words, they maximized their expected utility taking into account how - according to their own expectations - their opponent might share the common pot for each scenario.

[15]See section 4 for details.

cisions they learned who had been assigned the role of the dictator. During the subsequent distribution phase, in which the dictator made her sharing decisions, each decision could be altered once before it was made final. During the payment phase the experimenter determined final payoffs for each individual (given by the sum of the monetary endowment, payment for bags which had been returned unsorted, and the share of the common pot the individual was to receive based on the dictator's wishes). While subjects were paid, they learned as announced about the other player's rate of return and payoff relevant decisions.

This concludes the description of the general experimental procedures. I now provide an in-depth discussion of each treatment and its contribution towards generating a distribution over the norms of distributive justice listed in section 2, before turning to matters of implementation in the field in section 4.

1.3.2 Benchmark Treatment

In the benchmark treatment, each participant was given an endowment of 3 bottle caps and 2 bags of beans. Sorted bags generated an income of 4 bottle caps per bag for individuals who had been assigned a low rate of return, and 8 bottle caps per bag for individuals who had been assigned a high rate of return. Each player had the option to sort either 0, 1, or 2 bags, with unsorted bags being automatically returned to the experimenter at the rate of 2 bottle caps per bag, independent of the subject's assigned rate of return.[16]

As in Cappelen et al. (2007), the benchmark treatment is used to learn about the distribution over strict egalitarianism, liberal egalitarianism, and libertarianism among subjects. Additionally, due to our modified experimental procedures in which sub-

[16]Please refer to Table 1 in the appendix for a graphical depiction of the production function.

jects were able to observe endowments and money from returning unsorted bags not only for themselves, but also for the other player in addition to the common pot prior to each allocation decision, it is sensible to also test for and determine the frequency of inequality aversion as guiding norm for allocation decisions of the dictator. Allocations in which the dictator took the entire common pot are called purely selfish for the purpose of this discussion.

Graphically depicting the various norms' predictions highlights important features of the decision making process of the subjects and (through that) how the benchmark treatment allows us to identify the discussed norms: Decisions in line with libertarian predictions (see Figure 3) have the lowest informational requirements of all allocations. The dictator only needs to know her own marginal contribution to the social surplus since it always equals her optimal share. Purely selfish and strict egalitarian allocations are based on information about the size of the common pot, i.e. *both* players' contributions: individuals keep 100 percent and 50 percent of the common pot, respectively (see Figure 4). Liberal egalitarian allocations are proportional to relative effort levels, varying with the common pot size. As the dictator increases her effort, her optimal share increases due to the associated increase in common pot size *and* the higher relative effort level of the dictator as compared to the receiver. The norm's predictions are therefore depicted for different effort levels of the dictator *given* the receiver's effort level and both players' rate of return (see Figure 5 and Figure 1 in the appendix).[17] Inequality aversion (see Figure 6) is depicted in the same way. However, the mechanism through which higher effort levels of a player lead to higher optimal shares for her is more indirect compared to liberal egalitarianism, working through the increase in common pot size, as well as the fact that subjects who exert more effort receive less income from returning unsorted bags, which must be taken

[17]Note, that effort levels and rates of return jointly determine common pot size.

into account for equalizing final payoffs.

We can distinguish four categories of distributional scenarios:

1) **Matched players are assigned the same rate of return and choose to sort an identical number of bags.** In case social surplus is generated,[18] all norms prescribe equal shares for the players.[19]

2) **Matched players are assigned identical rates of return, but choose to sort a different number of bags.** With the exception of strict egalitarianism, all norms prescribe an unequal distribution of the common pot.[20]

3) **Matched players have different rates of return, but choose to sort an identical number of bags.** In such a case only libertarianism prescribes different optimal shares for the two players.[21]

4) **Matched players are assigned different rates of return and choose to sort a different number of bags.** Here, two sub-cases can be distinguished: i) If the respective effort-generated earnings of the players do not coincide, optimal shares for

[18] If both players choose not to sort, no social surplus is generated.

[19] For example, if the rate of return is low for both players ($a_i = 1$, $a_j = 1$), and each player sorts two bags ($e_i = 2$, $e_j = 2$), the size of the social surplus is $X = 160$ and all three norms tested for in the benchmark treatment predict a dictator share of $y_i = 80$. Only purely selfish allocations are characterized by $y_i = 160$. This is the case since both players exerted same effort (hence liberal egalitarianism suggests equal shares to be optimal), contributed the same amount to the social surplus (hence libertarianism suggests equal shares to be optimal), and do not differ in their monetary endowment and payment for returning unsorted bags (hence inequality aversion suggests equal shares to be optimal). Equal shares are (trivially) optimal under strict egalitarianism.

[20] For example, if the rates of return are high for both players ($a_i = 2$, $a_j = 2$), and the dictator sorts one bag ($e_i = 1$), while the other player does not sort at all ($e_j = 0$), the size of the social surplus is $X = 80$. In this case, a dictator share of $y_i = 40$ is optimal only under strict egalitarianism. Libertarianism determines the optimal dictator share to be the contribution to the social surplus, i.e. $y_i = 80$. Liberal egalitarianism also prescribes $y_i = 80$, since the receiver did not put any effort into generating money for the common pot. Inequality aversion takes into account that the player who sorted one bag less received 20 MK more from returning an additional unsorted bag compared to the dictator. To equalize final amounts subjects take home, the dictator's share must therefore be 20 MK higher than that of the receiver, i.e. $y_i = 50$.

[21] For example, if the rate of return is high only for the dictator ($a_i = 2$, $a_j = 1$), and each player sorts two bags ($e_i = 2$, $e_j = 2$), the size of the social surplus is $X = 240$. Libertarianism predicts a dictator share of $y_i = 160$ since this equals her contribution to the common pot. A dictator following strict egalitarianism trivially keeps $y_i = 120$. This is also the optimal share according to inequality aversion and liberal egalitarianism because both players exerted the same amount of effort.

the players are different under all norms with the exception of strict egalitarianism.[22] ii) If the effort-generated earnings of both players coincide, libertarianism and strict egalitarianism prescribe equal shares. Liberal egalitarianism and inequality aversion prescribe shares to be different.[23]

1.3.3 Equality of Opportunity Treatments

There are three treatments aimed at investigating whether and to what degree equality of opportunity plays a role for distributional decisions among the subject population. One treatment introduces an "effort cap" only for the dictator, another only for the receiver, and the third treatment introduces an effort cap for both players. Players with an effort cap received only one bag of beans, but 5 bottle caps as monetary endowment. The endowment is chosen such that participants with an effort cap have the same non-effort-generated income as benchmark treatment participants in case of sorting either 0 or 1 bag. The goal is to make these treatments comparable to the benchmark treatment for these effort choices in all respects other than the existence of the effort cap.[24]

In treatments with unequal choice sets, the player with the smaller choice set should

[22]For example, if the rate of return is high only for the dictator ($a_i = 2$, $a_j = 1$), and she sorted two bags while the receiver sorted only one ($e_i = 2$, $e_j = 1$), the size of the social surplus is $X = 200$. While strict egalitarianism prescribes $y_i = 100$, libertarianism asks for the dictator to keep her marginal product to the social surplus, hence $y_i = 160$. Liberal egalitarianism rewards the dictator with two thirds of the money in the common pot since she exerted double as much effort as the receiver, hence $y_i = \frac{2}{3}200$. Inequality aversion reimburses the dictator for foregoing the money from returning her second bag unsorted (as compared to the receiver), thus the dictator's share is 20 MK larger than the receiver's, i.e. $y_i = 110$.

[23]For example, if the dictator has a low rate of return, $a_i = 1$, and sorts two bags, $e_i = 2$, while the receiver has a high rate of return, $a_j = 2$, and sorts one bag, $e_j = 1$, the social surplus generated is $X = 160$. Strict egalitarianism trivially prescribes $y_i = 80$. Since this equals the dictator's marginal product, it is also the optimal share under libertarianism. According to liberal egalitarianism $y_i = \frac{2}{3}160$ should be kept by the dictator, since she exerted double as much effort as the receiver. $y_i = 90$ is optimal according to inequality aversion, because the receiver has returned an unsorted bag while the dictator has not, i.e. the dictator needs to get 20 MK more of the social surplus than the receiver in order to equalize final payoffs.

[24]Please refer to Table 2 for the production function.

receive a higher share of the social surplus as compared to that in the benchmark treatment if a dictator follows the principle of equality of opportunity. In situations where players' rates of return and/or effort choices differ, the optimal amount for an individual with a smaller choice set should exceed the optimal amount under libertarianism or liberal egalitarianism. Note, however, that we only know the direction, not the magnitude of the effect, which is to be determined empirically. Comparisons of allocation decisions of the treatment in which both players have a limited choice set to those of the benchmark treatment will allow us to control for treatment effects *independent* of decision making which takes equality of opportunity into account.

The predictions for all other norms are identical to those for the benchmark treatment, with the sole exception that the player(s) with a limited choice set cannot exert an effort level of sorting two bags.

1.3.4 Income Shock Treatments

The income shock treatments aim at investigating whether and to what degree luck egalitarianism forms a basis for distributional decisions among the subject population. During instruction, participants assigned to these treatments learned about the possibility that either they, the player matched with them, or both of them might lose their monetary endowment, i.e. the three bottle caps given to them at the start. Only after their sorting decision was carried out, was it revealed to them who had in fact lost their endowment, i.e. production decisions took place in a symmetric set-up. The goal was to create differences in income that were *entirely independent* of production decisions and to analyze whether dictators would take such income differences into account when distributing the social surplus generated on the basis of those production decisions.

In treatments with only one player experiencing a shock, this player would receive a higher amount of the social surplus than she would in the benchmark treatment if the dictator followed luck egalitarianism. Note, that we only know the direction, but not the magnitude of the effect prior to estimation, as in the case of the 'equality of opportunity' treatments. Comparisons of allocation decisions made when both players experience a negative income shock to those of the benchmark treatment allow me to control for treatment effects that are independent of luck egalitarianism.

The predictions for the other norms are the same as for the benchmark treatment with the exception of inequality aversion (see Figure 7). Since inequality aversion is defined over final outcomes, the norm's prescriptions for treatments that are *not* symmetric in income shocks must trivially differ from those for the benchmark treatment. Specifically, the player who experiences a negative shock of 30 MK should be fully compensated for this loss in the optimal allocation.

1.4 Field Experimental Procedures

The field implementation of this study comprised three distinct parts: a baseline survey, the game, and an opinion survey. For an overview of all steps involved, please refer to Figure 2 in the appendix. The project took place in Ntchisi District, a poor, rural district in Malawi's Central Region. 80 rounds of the experiment were conducted over a period of 20 days, 4 on each day. Each round required a minimum of 40 households out of which 16 participant households were randomly selected.

To avoid contamination of the experiment due to subjects learning about the game from interaction with prior participants, precautions were taken regarding the locations of all rounds. To understand these precautions, it is useful to think of the choice of location as being divided into two stages: the choice of where to perform

the 4 sessions on a particular day ("area" for the sake of expositional clarity), and the locational choice of each of these 4 places relative to each other ("location" for clarity).

Regarding the first stage, the area for each day was selected such that word of mouth could not be reasonably expected to spread overnight between two areas that were used on consecutive days. Areas were visited sequentially by moving outward from a trading center, which is located close to the border of Ntchisi District. Each area had to have at least 160 (4 times 40) households.[25] Once an area had been chosen, four clusters of dwellings with a minimum of forty households in each were identified. In each of these locations, one experimental round was conducted. These four locations were chosen such that they were segregated enough geographically to render communication between participants in different rounds infeasible.[26]

In each location, participants were recruited in the following way: Upon arrival, the study team asked for assistance from either group village headmen[27] or chiefs in drawing up a map of the houses in that particular location. The houses were numbered based on the order in which they were drawn. To determine which households were eligible we then drew 16 numbers out of an envelope with as many numbered paper slips as there were houses.[28]

Eligible households were approached with recruitment scripts. To determine who in the household was eligible for participation, a household listing was compiled. It

[25]Information on electoral ward boundary demarcations provided by the Office of the District Commissioner, in combination with location scouting data from an earlier joint project, Raballand et al. (2011), aided in the selection of these areas.

[26]In some cases, a larger cluster of dwellings had two locations in them. However, for these cases, two precautions were taken. First, two locations within one cluster were without exception used for consecutive rounds only. Second, these places were always sufficiently spread out to have some natural barrier between the two locations.

[27]A group village headman is the direct superior of several chiefs whose villages form a cluster.

[28]The remaining households were kept as back-ups to be visited in the order they would be drawn should the need arise.

included all adult members of the household who had been present in the household the previous night.[29] These household members were assigned numbers based on the order in which they were mentioned. We then drew a numbered piece of paper out of an envelope that contained as many numbered paper slips as candidates for participation in that particular household. This determined the eligible household member. A back-up household was approached in case of absence of any adult members for household listing purposes, absence of the eligible household member, or the latter's refusal or inability to participate.

All eligible household members were asked for consent at a local chief's residence. This guaranteed that each participant was aware of the identity of all other 15 participants in her location, ensuring that each participant knew the average characteristics of her anonymously assigned partner and that the same degree of anonymity was maintained for different rounds, independent of particpants' houses' relative locations to each other. After consent, a short baseline survey was conducted. Enumerators then delivered treatment specific game instructions one on one in subjects' houses or at a mutually agreeable place which guaranteed privacy.[30]

The participants were assigned to the same combination of treatments in all locations. Enumerators rotated through instructing different treatments for each round, i.e. each enumerator instructed four different treatments per day. The rotation scheme guaranteed that each enumerator collected sharing data for each treatment the same number

[29] Adults are individuals 18 years and above according to Malawi law.

[30] Both alternatives - group instruction sessions as well as treatment group instruction sessions would have had considerable disadvantages compared to the method of instruction chosen. There was a high chance of signalling between participants for both alternatives. In case of *treatment group* instructions, there would have been a non-negligible opportunity for collusion, since all treatments were played at each location, implying a maximum of 6 subjects instructed using the same script at the same time. For *group* instruction sessions, on the other hand, the script would have needed to comprise information about *all* treatment groups which reduced participants level of understanding drastically as determined during piloting.

of times over the course of the study.

Instructions were delivered orally. During instruction, the monetary consequences of each potential sorting decision a player could make was demonstrated to her using bottle caps[31] and bags of beans. The consequences of several potential actions of the other player, given an assumed rate of return, were illustratively demonstrated to her in the same manner. At several pre-defined points, participants were allowed to ask questions. If possible, the part of the instruction script that had remained unclear to the participant was re-read, otherwise standardized answers were given. After a final check for understanding upon completion of the script, all subjects were asked to sort a standardized sample amount of beans, to get a clear idea of the difficulty and duration of the task before making their (irreversible) sorting decisions.

After sorting, the enumerators contacted the supervisors to learn whether their participant had been assigned the role of decision maker, and - for the monetary shock treatments - whether their participant or the person matched with the participant or both had lost their endowment. Neither information was revealed to the enumerators ex ante, to not influence performance. After participants learned about their status, non-decision makers were brought back to the chief's residence where they completed the opinion survey. Decision makers first had to make sharing decisions. The decision making process was aided in the following way: the monetary consequences of the decision makers' actions given her rate of return were reviewed and displayed in front of her with bottle caps. Enumerators then demonstrated the consequences of a possible effort/rate of return combination of the other player with bottle caps and bags of beans *given* the sorting decision of (and associated outcome for) the dictator. Next, the dictator was asked whether she wanted to keep more, equal, or less of the

[31]Bottle caps were turned upside down so that different colors would not lead to framing effects. All bottle caps were white on the inside.

common pot as compared to her counterpart. Afterwards, she was asked exactly how much of the social surplus she would like to keep. If her answers were consistent, the monetary consequences of her sharing decision were demonstrated with bottle caps (otherwise, the enumerator would point out the inconsistency and ask the participant how to correct it before proceeding). Importantly, the enumerators performed simple algebra for the participants at this stage, summing up the distributional decisions in terms of a) how much of the social surplus would be left for the other player, if the decision maker's choice was carried out, and b) calculating final amounts for both players. Subjects were allowed to change their mind about their preferred allocation once at this point, before the enumerator moved on to demonstrate the consequences of another possible effort/rate of return combination of the matched player. This two-step procedure allowed me to overcome educational limitations of parts of the subject population which otherwise might have tainted results.

After all sharing decisions were made, the decision makers also returned to the chief's residence for completion of the opinion survey. Enumerators turned in the sheets on which dictators' and receivers' decisions were recorded to the experimenter who then matched these answer sheets according to a pre-specified matching scheme and paid participants, calling them one by one to the project bus to keep payments private. At the bus, during payment, they were informed about the payoff relevant sorting and sharing decisions of their partner as well as their partner's rate of return, as mentioned in section 3. Baseline, game, and opinion survey together took approximately 1.5 hours on average, out of which less than 45 minutes were spent on game instruction.

Data entry took place in Lilongwe, the capital of Malawi, by native Chichewa speaking data entry staff (Chichewa being the language in which the project was conducted).

I programmed the data entry forms in CSPro. Operators performed double entry for all data.

1.5 Results

The analysis is based on the structural model of optimal choices of dictators described in section 2. First, I present an empirical assessment of whether the norms postulated in the model are appropriate for analyzing the allocation decisions in the data set. After successfully establishing that subjects react as predicted to all variations for which at least some norms predict changes of the optimal share, the analysis proceeds with assigning observations and individuals to norms. First, I match observations with norm predictions. I present frequencies and discuss the broad patterns of the results before turning to more subtle analyses for those observations that do not match any *one* norm prediction exactly. I then turn to individual level matching. Here, the issue of self-consistency is discussed in addition to repeating the previous analyses. An OLS analysis to identify demographic and socio-economic predictors for conformity with specific norms follows. Last, I present OLS results for reduced form versions of the model before turning to a mixed logit estimation.

1.5.1 Which Experimental Variations Do Subjects Respond To?

This sub-section assesses which experimental variations and which endogenous decisions the subjects of this study responded to, in order to assess the appropriateness of analyzing the data set with respect to the norms defined in the model section. Specifically, I investigate whether and describe how the common pot shares that dictators kept vary with effort, rate of return, and contribution size. I further determine whether and how dictators take limited choice sets and shocks to personal endowments into account when making decisions about the social surplus.

The results are summarized in Table $3-7$.[32] Sample sizes are stated in terms of individuals as well as in terms of observations. In addition to the mean and median dictator share, the number and percentage of observations falling into each of four categories is reported: purely selfish allocations, dictator shares between 100 and 50 percent of the common pot, equal splits of the social surplus, and "generous" allocations, for which the dictator share is lower than 50 percent of the total effort-generated income.[33] Based on these data, Wilcoxon rank-sum tests and nonparametric equality-of-median tests allow me to assess which experimental variations are most relevant for explaining the outcome data, thus building the foundation for a more detailed analysis of the data in the following sub-sections.

1.5.1.1 Do Subjects Reward Effort?

The hypothesis that individuals reward both own and others' effort is tested using the data summarized in rows $2-4$ of Table 3. To help the reader visualize the results presented in the tables, the data is once more additionally displayed in histograms (see Figures $9-11$) for this case.

Histogram 9 restricts the data to observations for which the dictator and receiver of a team choose identical effort levels and have been assigned identical rates of return (also see row 3 in Table 3). We see that the majority of decisions (63.5 percent) is to keep exactly half of the common pot. Dictator shares between 100 and 50 percent of the social surplus make for over a quarter of the observations. In comparison, purely selfish and generous allocations are rare: they occur in just 6.6 percent and

[32]Note, that since the goal of this section is to understand whether and how subjects respond to exogenous and endogenous variations of the experimental environment, rather than explicitly testing model predictions, I restrict the data in various ways to isolate the respective effects. For example, for analyzing whether subjects responded to income shocks, I restrict the data to observations for which the rate of return and effort level between dictators and receivers is held constant.

[33]The usefulness of summarizing the data in these four categories is exemplified not only in Figure 8, but also in histograms $9-11$ (see sub-section 5.1.1).

3.3 percent of all cases, respectively.[34]

Histogram 10 displays those cases in which the dictator has a higher effort level but identical rate of return compared to the receiver (also see row 2 in Table 3). We observe large shifts in the distribution towards higher dictator shares compared to histogram 9, as would be intuitively expected for subjects who reward own effort. Most strikingly, purely selfish allocations make up close to 40 percent of all observations now, while equal splits of the common pot occurred in only 25.5 percent of all cases. A comparison of the location of the distributions summarized in rows 2 and 3 of Table 3 verifies that dictators keep a significantly higher share when they exert more effort than matched receivers with the same rate of return, as compared to a scenario in which both players exert the same effort at identical rates of return.

Histogram 11 restricts the data to observations for which the dictator has a lower effort level than the receiver while both players have been assigned the same rate of return (row 4 in Table 3). Again, we observe large, intuitive changes in the distribution compared to histogram 11. Especially striking is the marked increase in generous allocations to 15.7 percent. Additionally, a smaller percentage of observations falls into the categories in which the dictator takes more than 50 percent of the common pot. The tests confirm that dictators, who at identical rates of return exert less effort than matched receivers, keep a significantly smaller median share of the common pot.

Taken together, the evidence clearly suggests that dictators reward both own and others' effort. Therefore, it is appropriate to investigate whether individuals follow

[34]Note, that this supports the model's assumption that individuals do not keep less than what their preferred norms prescribe: For the cases described in histogram 9, only a sub-set of those dictators who follow luck egalitarianism or the principle of equality of opportunity prefer generous allocations (depending on the relative weight these individuals place on their own income). Therefore, we should expect the majority of observations to not be generous.

effort-based norms of distributional justice as postulated in the model section.

1.5.1.2 Do Subjects Reward Higher Rates of Return?

We explore the hypothesis that individuals reward a higher rate of return by analyzing the data in rows 5, 6, and 7 of Table 3.

Rank-sum and equality-of-median tests comparing the location of the distribution of dictator shares when dictators and receivers have identical effort levels and rates of return to when they have identical effort levels but dictators are assigned a higher rate of return (row 5 of Table 3), reveals that dictators increase their median share significantly in the latter case. The opposite is not true if dictators have been assigned a lower rate of return than receivers (see row 7 of Table 3): differences turn out insignificant.

Summing up, we can be confident that dictators generally do take rates of return into account when making distributional choices. Therefore, it is sensible to investigate whether individuals follow norms of distributive justice that take rates of return into account as postulated in the model section.

1.5.1.3 Do Subjects Reward Higher Contributions?

We assess the hypothesis that individuals reward their own and others' contributions by analyzing the distributions summarized in rows $8-19$ of Tables 3 and 4. The data in rows 8 and $11-14$ describe situations where the dictator contributes more to the common pot than the receiver. The data in rows 10 and $16-19$ describe the converse situations.

A comparison of the median shares for the distributions summarized in rows 8 and

$11 - 14$ to the median share kept by dictators who contribute the same as receivers reveals that individuals reward own contribution: shares kept by the dictators increase significantly; in addition, dictators take the magnitude of the difference between their own and others' contribution level into account when determining payoffs - for the maximum (minimum) difference in contribution between dictator and receiver the highest (lowest) average dictator share of those in rows $11 - 14$ is kept. If the other player's contribution exceeds the dictator's, we observe *all* previously described effects in reverse, though of slightly smaller magnitudes. To sum up, both own and others' contributions to the social surplus are clearly rewarded by dictators.

1.5.1.4 Do Subjects take Equality of Opportunity into Account?

In order to answer the question whether subjects take equality of opportunity into account, I restrict the data to the treatments where at least one player was limited in her effort choice, and compare these to the data for the benchmark treatment (for which I exclude the observations of dictators, receivers or both players if they sorted 2 bags to ensure broad comparability to the various equality of opportunity treatments).

Tests on the data in rows $20 - 25$ reveal that while the distributions of the dictator shares for low rate of return dictators shift in the intuitively expected direction depending on whose choice set is restricted, these changes in location are not strong enough to be mirrored in the test statistics of either test.

This picture, however, changes when we investigate the sharing behavior of high rate of return dictators in the context of effort caps: Row 27 shows the distribution of dictator shares for all cases in which only the dictator was limited to sorting maximally one bag. Comparing the median of this distribution to the median of the benchmark treatment (excluding dictators who sorted more than one bag, i.e. row 26 of Table

4), we see that the share kept by the dictator is significantly higher if she has a reduced choice set, as would be intuitively expected for individuals taking equality of opportunity into account.

Row 29 summarizes the distribution of dictator shares for those observations for which only the receiver is limited to sorting maximally one bag. I compare the location of this distribution to the location of the benchmark treatment excluding receivers who sorted two bags, i.e. row 28. Here, the median share kept by the dictator is significantly lower for the case in which the receiver faces an effort cap, as would be expected if the principle of equality of opportunity influences decision making for the analyzed sample.

Row 31, finally, displays the distribution of dictator shares for situations in which both players faced an effort cap. A comparison of its location to the location of the benchmark treatment (excluding dictators and receivers who sorted two bags, i.e. row 30 of Table 5) reveals no significant difference. Therefore, I conclude that the two previous results are not in any significant way tainted by other effects but due to the difference in opportunities between players. Therefore, the experimental data should be analyzed for the principle of equality of opportunity.

1.5.1.5 Do Subjects care about Income Shocks unrelated to Productive Activities?

In order to answer this question, we compare the data of the treatments in which at least one of the players experienced a shock to their endowment, to the benchmark treatment data. In all cases, I hold effort level constant between players.

The rank-sum and equality-of-median tests show that low and high rate of return

dictators do not reimburse themselves for income shocks in a way that would show up as significant in these simple location tests (see rows 32, 33 and 37, 38). At the same time, test results are significant regarding reimbursement of receivers for such shocks (see rows 33, 34 and 38, 39), independent of dictators' rate of return.

A comparison between the treatment in which both players face an income shock, to the benchmark treatment (see rows 35, 36 and 40, 41), reveals no significant difference of the median share kept by the dictator independent of her rate of return. I therefore conclude, that subjects truly respond to differences in non-effort-generated income. It is thus sensible to include luck egalitarianism into the set of norms analyzed in this paper.

1.5.2 Regression Analysis With Respect To Norm Predictions

In this subsection, I assess the average influence of effort, rates of return, contributions, income shocks, and limited opportunities on monetary outcomes using the *entire* sample for a reduced form regression analysis. While certain effects may not come out as clearly in such an analysis (since they only concern a small sub-part of the sample, such as *one* income shock treatment), the benefits of proceeding this way are clear: In the previous sub-sections, I restricted the data in numerous ways in order to hold all factors but the one of interest constant. Running an OLS allows me to look at whether effects hold more generally without losing sample size while controlling for other factors through inclusion of multiple dependent variables. Hence, this analysis is able to deliver confirmation that absent to theory being imposed, effects hold with (or despite) inclusion of controls. In other words, it provides a more comprehensive picture of patterns found in the data relative to the previous subsection's pairwise comparisons, while theoretical specifications (which might obscure these patterns) are absent and become relevant only for the structural estimation of the mixed logit

model of multinomial choice.

The outcome variable is the fraction of the common pot kept by the dictator. For Table 8 and 9 I estimate the regression equations

$$y_{is} = \alpha + V_{is}\eta + \epsilon_{is} \tag{1.8}$$

and

$$y_{is} = \alpha + T_i\gamma + V_{is}\eta + T_i RE_{is}\xi + \epsilon_{is}, \tag{1.9}$$

where y_{is} represents the fraction of the common pot that dictator i keeps for herself in scenario s, where a scenario is a specific (hypothetical) effort/rate-of-return combination of the receiver. V_{is} is a matrix of scenario-dependent norm-relevant variables. For different columns/specifications of Tables 8 and 9, it is either defined as (a) DE_{is}, a vector of (hypothetical) effort levels of the dictator, measured in number of bags sorted, (b) RE_{is}, a vector of (hypothetical) effort levels of the receiver, measured in number of bags sorted, (c) a vector of (hypothetical) differences in rates of return between dictator and receiver, (d) a vector of (hypothetical) differences in dictator's and receiver's contribution to the social surplus, measured in bottle caps, or (e) a combination of the first three vectors.[35] T_i is a vector of treatment group dummies.

For controls set to zero, dictators keep an average between 47.9 and 71.2 percent of the common pot for themselves. These values are in line with findings of earlier studies using dictator games to elicit sharing preferences in developing countries (see Henrich et al. (2001)).

Dictators keep a significantly higher share of the common pot as their own effort

[35] Other combinations can not be included due to collinearity concerns.

increases. In the specification of column 1 of Table 8, in which dictators keep an average of 48 percent if their own effort is 0, dictators keep 9 percent more of the (then increased) common pot as the receiver's effort increases through sorting one bag. Pooling data across treatments confirms therefore that subjects strongly reward own effort.

Reversely, dictators keep a significantly lower share of the common pot as the receiver's effort increases. In the specification of column 2 of Table 9, in which dictators keep an average of 67.1 percent if the receiver's effort is 0, dictators keep 12 percent less of the (then increased) common pot as the receiver's effort increases through sorting one bag. Pooling data across treatments confirms therefore that subjects strongly reward others' effort.

Differences in rates of return can take the values -2, 0, or 2, subtracting the rate of return of the receiver from that of the dictator. Dictators keep a significantly higher average share when they have a higher rate of return, as intuitively expected. I interpret this effect using the specification of column 3 of Table 8. Dictators keep an average of 55 percent when both players have the same rate of return. If the dictator has a higher rate of return than the receiver, she increases her average share by 6 percent. Interestingly, the average effect of the exogenously assigned rates of return on the dictator's share is smaller than that of effort.[36]

A positive difference in contributions, meaning that the dictator contributed more to the social surplus than the receiver, leads to significantly higher average shares the

[36]Regarding comparability, please note that relative to situations in which both players have the same rate of return, a change of one player's rate of return causes a change in common pot size of the same magnitude as can be achieved by either an increase or decrease of the effort level of one of the players. Cases in which both players have effort levels of zero do not enter the analysis, since no social surplus was to be shared.

dictator allots to herself. The effect is slightly smaller when controls are added. [37] According to the specification of column 4, dictators keep an average of 57.4 percent of the social surplus in cases where both players contribute the same amount to the common pot, but increase their share by an additional 1.3 percent for each 40 MK that their contribution exceeds that of the receiver.

Income shocks that the receiver alone is facing have highly significant effects independent of the exact regression specification. The sign of the coefficients is as intuitively expected: Dictators keep less of the social surplus when receivers lose part of their endowment. However, income shocks that only the dictator faces have - on average - no significant effects on shares. Finally, if both players face an income shock, we also cannot observe any significant changes of average dictator shares. The OLS results hence fully confirm the findings from the previous sub-sections, i.e. the picture created by the analysis of the data summarized in Tables $3-6$ is matched by the OLS regression results in all aspects.

Last, we look at the effects of reduced choice sets. It is most sensible to look at the coefficients of the specification of column 10 since we can expect pure effort effects being picked up in the other specifications. If we compare the coefficients of the three treatments with effort caps we see that they are ordered in the way we would intuitively expect them to be, even though the effect of an effort cap of the dictator turns out to be not significant.

Exclusion of baseline variables gives a similar picture for all effects. I conclude this subsection by noting that we found clear evidence for the effects we would expect to

[37] The magnitude of the change of this point estimate across specifications is not different to the one of the point estimate for receiver's effort level. However, the former point estimate needs to be multiplied by at least 4: the difference in contributions is measured in bottle caps, with 40 MK being the minimal difference in contributions for players whose contributions do not coincide.

see if subjects adhered to the norms proposed in section 2.

1.5.3 Matching the Experimental Data to Norm Predictions

The last subsection successfully established that subjects react as intuitively expected to *all* variations for which at least some norms predict changes of the optimal share. Therefore, I proceed by analyzing the data with respect to the principles of distributive justice specified in the model section.

1.5.3.1 Matching Observations to Norms

In order to categorize observations by norms, I proceed as follows. Initially, I calculate the absolute difference between the share kept by a dictator in a specific allocation and what each norm would predict for that particular scenario.[38] If, for example, a dictator kept all 80 MK of a common pot generated only by her partner's effort, the absolute difference to what strict egalitarianism would predict is 40 MK. I call categorizations based on zero absolute differences "strong" matches.[39] If individuals care about income as well as norms of distributive justice, we should expect at least some observations to be close to, but not *at* the point of prediction of any norm. In such cases, I specify which norm(s) an observation is "closest to" by determining the smallest absolute difference(s) for the observation. Categorizations based on minimal absolute difference are called "weak" matches for the remainder of the paper.

Strong Match There are multiple scenarios for which the predictions of different norms are identical to each other, as was highlighted in the discussion of the interpretation of the benchmark treatment outcomes (see sub-section 3.2). Therefore, I

[38] Although "selfishness" is not an actual norm of distributive justice, I do include the term in the analysis for categorizing the data.

[39] Inequality aversion occasionally predicts amounts not divisible by 10 MK, (which is the smallest monetary unit in this experiment - recall that subjects worked with bottle caps, where one bottle cap represented 10 MK), but only by 5 MK. In such cases, if the dictator share is +/- 5 MK compared to the prediction, it is treated as a zero absolute difference for the purpose of the analysis.

distinguish between "unconditional" (see Table 10) and "exclusive" (see Table 11) matches: the table of unconditional matches includes *all* observations that have been categorized as conforming with the prediction of a particular norm. Take, f.ex., an observation classified as libertarian: if the observation is an unconditional match it might *also* fall into an additional category besides libertarianism. Exclusive matches, on the other hand, are those for which an observed dictator share is exactly equal *only* to the prediction of the norm(s) specified.

We see that among the unconditional matches 46.24 percent of all observations conform with strict egalitarianism and approximately a third with libertarianism and liberal egalitarianism, respectively. Inequality aversion can be observed in 25.92 percent of all cases. Less than 20 percent of all observations are purely selfish or cannot be matched to any of the norms, respectively.[40]

Comparing exclusive to unconditional matches, the most striking difference is the marked reduction of all norms with the exception of strict egalitarianism (see Table 11). We can observe, that even in cases in which strict egalitarianism predicts different shares from all other norms, 21.66 percent of all observations can be classified as strict egalitarian.

Weak Match Once again, we distinguish between unconditional and exclusive matches. A comparison of weak to strong unconditional matches (Tables 12 and 10, respectively) reveals that observations which could not be classified previously, are closest to all norms other than inequality aversion: the percentage of observations which can be matched with those norms increases by approximately 10 percent, while the

[40]Note, that the latter may well be in line with the model, since strong matches take neither the balance between norm adherence and selfishness nor the balance between two (or more) competing norms into account.

percentage of observations which can be matched with inequality aversion increases by 5.85 percent. Exclusive weak matches at the observation level (Table 13), show only slight changes in percentages compared to Table 11.

The overall picture we gain from matching observations to norms is that while we observe high percentages of matches for all norms, only strict egalitarianism is frequently followed if it predicts values different from those of other norms' predictions.

1.5.3.2 Matching Individuals to Norms

In order to categorize individuals by norms, I first calculate the sums of the absolute differences between the shares kept by a dictator (for all allocation decisions she had to make) and what a specific norm would predict for each case. Second, I assign an individual to a norm if the above described sum of absolute differences is zero for the individual. I call categorizations based on a zero sum "strong" matches. Third, I specify which norm(s) an individual is "closest to" by determining the smallest sum(s) of absolute differences for the individual. I call categorizations based on minimal total absolute difference "weak" matches.

Strong Match Looking at Table 14, we observe that 19.82 percent of individuals are consistently acting according to strict egalitarianism, while 7.5 percent can be classified as libertarian decision makers. 9 individuals act according to inequality aversion and pure selfishness, respectively, while only 5 individuals consistently act according to liberal egalitarianism. This picture does not change much if we look at exclusive matches (see Table 15).

Weak Match As we can see in Table 16, over 50 percent of individuals are closest to following strict egalitarianism across all of their decisions. Another 20.00 percent

of individuals are closest to following inequality aversion, and 17.32 percent can be classified as closest to being libertarian decision makers. 73 decision makers can be classified as liberal egalitarian under weak matching.

The picture changes most drastically for strict egalitarianism and inequality aversion under exclusive matching (see Table 17) as would be expected since the optimal shares according to these norms can most easily coincide across different distributional decisions: about 10 percent of individuals assigned to either of these norms are as close to another norm.

The overall picture we gain from matching individuals to norms is that consistent decision making in our sample occurs according to strict egalitarianism, inequality aversion, libertarianism, *and* liberal egalitarianism, noted in the order of influence. Pure selfishness clearly also plays a role when describing self-consistent behavior of the experimental subjects.

Taking observation level and individual level matching results together, we conclude that a surprisingly large percentage of observations/individuals strictly follows only one of the norms postulated. However, the majority of individuals optimizes differently, which, taken together with the fact that experimental subjects do react as intuitively expected to experimental variations, points to the appropriateness of estimating a mixed logit choice model, which will allow subjects to follow a weighted sum of pure selfishness and multiple norms.

Before that, I carry out a simple analysis to determine whether demographic or socioeconomic variables serve as predictors for following particular norms as identified above.

1.5.3.3 Demographic and Socio-Economic Predictors of Norm-Abiding Behavior

This subsection analyzes whether the matching outcomes at either the observation or individual level are influenced by demographic or socio-economic characteristics (see Tables 18 and 19). Presented are strong and weak unconditional matches on the observation level.[41]

The magnitudes of the coefficients seem small overall. Yet, we observe some strikingly intuitive results: Individuals from richer households, as measured by the household asset index, are significantly less likely to act strict egalitarian and significantly more likely to not follow any of the other norms exactly (i.e. to fall into the category "other").[42] Being more educated is a predictor for a higher likelihood of acting in line with *any* norm, i.e. subjects who are more educated adhere more strictly to norms. Interestingly, and contrary to results from regular dictator games, a higher age seems to predict a lower likelihood of strict egalitarian behavior. To understand the intuition, recall that the effort task employed in this study was sorting beans, a task for which a minority among the elderly had to exert higher effort than the average study participant due to limited mobility of their fingers.[43] Hence, it makes sense that these individuals would be less likely to act strict egalitarian.

In summary, we can draw the conclusion that demographic and socio-economic variables have very limited explanatory power for the outcomes of this study, though they are overall intuitive. Instead, outcomes vary predominantly with exogenous and endogenous variations of the experimental environment.

[41] Results for exclusive matches give the same overall picture and are available from the author upon request, individual level results can be found in the appendix.

[42] This observation is in line with these individuals weighting between selfishness and other norms.

[43] In addition, it may be that those individuals also perceived attendance at the common meeting point to entail more effort, if walking proved difficult for them.

1.5.4 Mixed Logit

The subjects of the experiment who are assigned the role of dictator by the randomization process are assumed to be self-interested but also concerned with several different fairness ideals. Looking at the results of a mixed logit estimation (see Table 20) of the model proposed in section 2, we see strongly significant effects of *all* norms on decision making, with strict egalitarianism having the largest weight, independent of whether the specification includes all norms or we compare the coefficients of the specifications in which only one norm is included at a time. The relative importance of liberal egalitarianism and libertarianism does not change across these two types of specifications, contrary to that of inequality aversion in relation to the other norms: If inequality aversion is included by itself it increases the probability for a specific choice more than libertarian or liberal egalitarian concerns (see the fourth column, Table 20). However, when included together with strict egalitarianism, many observations are attributed to an influence of the latter, implying that inequality aversion drops in influence on decision making to last place among the norms (see the fifth column, Table 20, in comparison). Not surprisingly, own interest (i.e. pure income considerations) is highly significant for decision making, independent of which norms are included (though its effect on dictators' choices seems rather small in comparison to that of the fairness considerations).

As a robustness check, I estimate McFadden's Choice Model, which is closest to the mixed logit except that it estimates fixed coefficients rather than a distribution of the latter. Results are very similar in nature (see Table 21). The only notable difference lies in the relative importance of liberal egalitarianism and libertarianism for choices, which, when both norms are included in the same regression, turns out to be opposite in this analysis to what the mixed logit stated. However, the difference in relative importance is not particularly striking, since both norms' influence is

roughly comparable and both estimation results are approximately in line with the matching exercise presented in earlier sub-sections. Most importantly, the relatively large influence of strict egalitarianism on decision making seems confirmed. Hence, we can be assured that results are not just an artefact of the estimation method employed, but that broad patterns can be confirmed independent of exact estimation strategy.

1.6 Conclusion

This paper studies the importance of several frequently discussed norms of distributive justice - strict egalitarianism, inequality aversion, luck egalitarianism, the principle of equality of opportunity, libertarianism, and liberal egalitarianism - in rural communities of Malawi. To this end, a lab-in-the-field experiment in form of a one-shot two-person dictator game with production stage is employed. The outcomes of the experiment reveal how effort-generated income is shared between individuals who may differ with respect to rate of return, effort, opportunity, or endowments.

The evidence clearly suggests that dictators are not only motivated by concerns about their own incomes but take into account several factors when making distributional choices. Behavior according to contribution based and effort based norms is widely observable, even though equality based norms have the largest influence on individuals' sharing behavior. Moreover, individuals take equality of opportunity and income shocks, which are unrelated to productive activities, into account when distributing social surplus. Socio-economic and demographic variables have little explanatory power regarding norm adherence. These findings together with the estimation of the structural model suggest that a complete model of distributional choice for a developing country context should take all of these norms into account.

The relatively large influence of strict egalitarianism and inequality aversion on decision making seems to be in line with critical voices claiming that norms may disincentivize productive activities in places like Malawi. However, three facts render doubt on whether norms truly play a significant role in lack of development: (1) the fact that effort is rewarded strongly on average, (2) the fact that a multitude of factors is taken into account when making sharing decisions, and (3) the fact that even in developed societies strict egalitarianism seems to be the prevailing norm. For a more meaningful judgment, the interaction between norms and *institutions* is crucial and will be explored in future work.

1.7 Figures

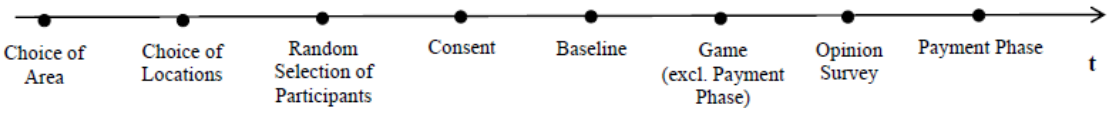

Figure 1.1: Timing of Experimental Procedures

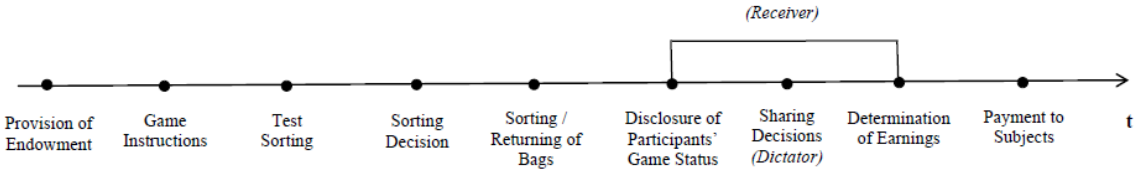

Figure 1.2: Timing of Field-Experimental Procedures

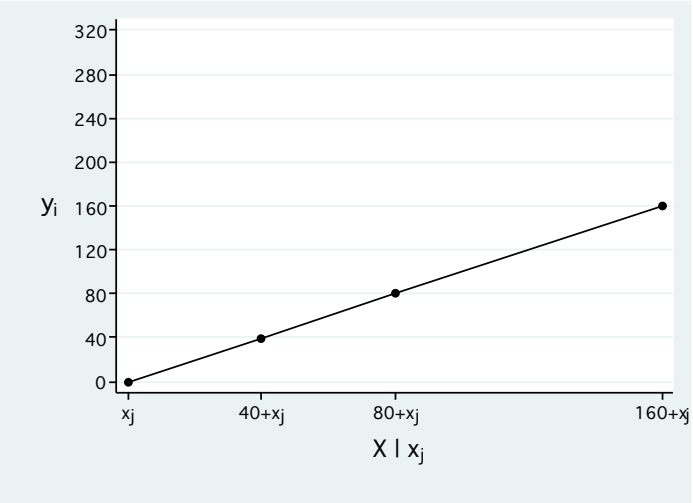

Figure 1.3: Libertarianism

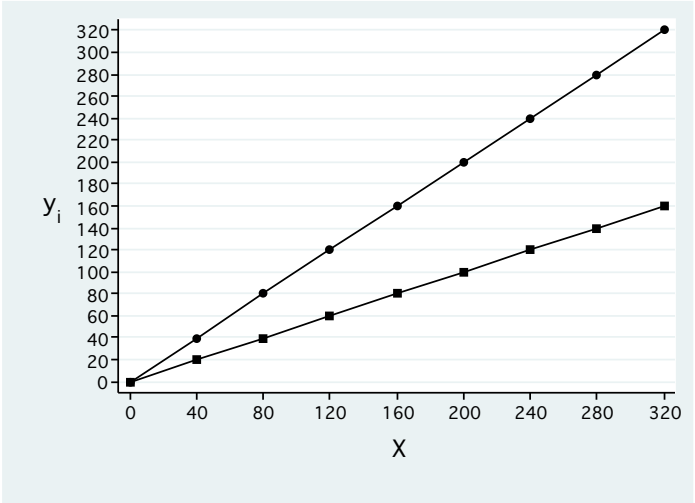

Figure 1.4: Strict Egalitarianism and Selfishness

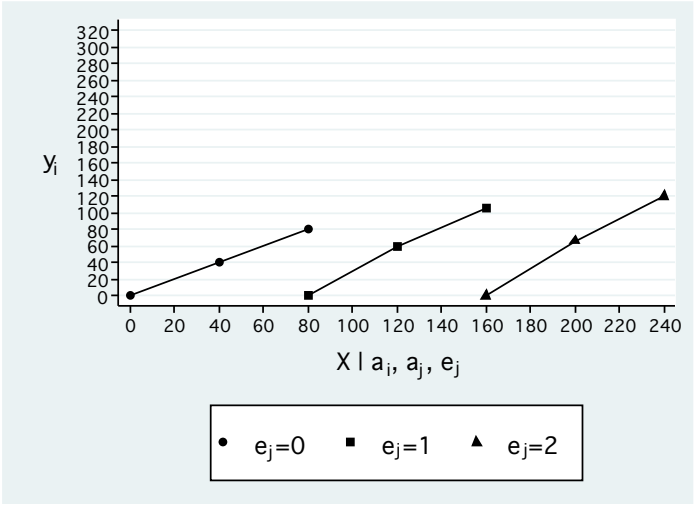

Figure 1.5: Liberal Egalitarianism; $a_i = 1$, $a_j = 2$

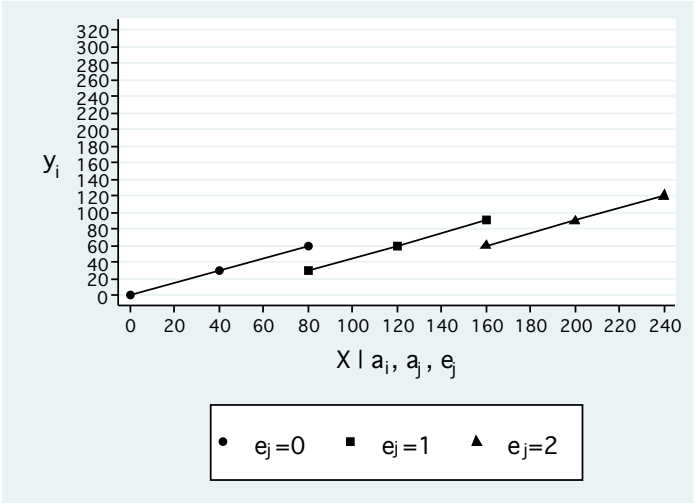

Figure 1.6: Inequality Aversion; $a_i = 1$, $a_j = 2$

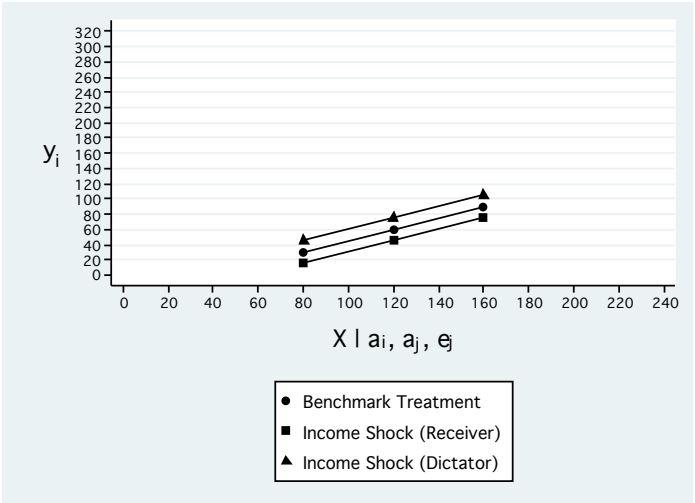

Figure 1.7: Inequality Aversion with $a_i = 1$ and $a_j = 2$ for the Benchmark Treatment and both Asymmetric Income Shock Treatments

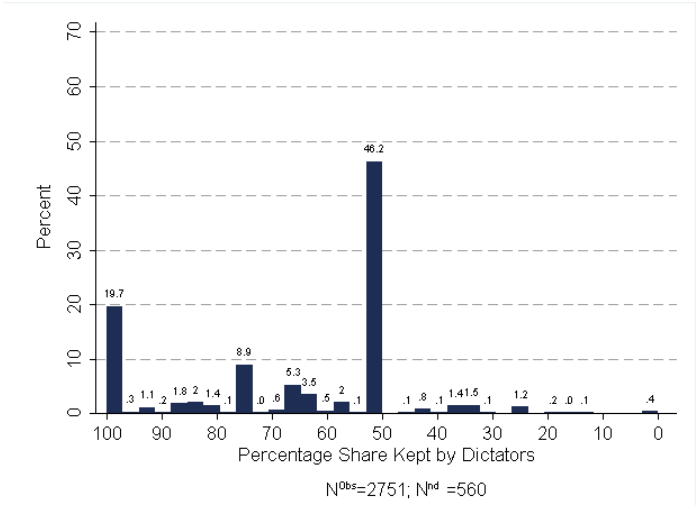

Figure 1.8: Shares of Common Pot Kept by Dictators (Strategy Method)

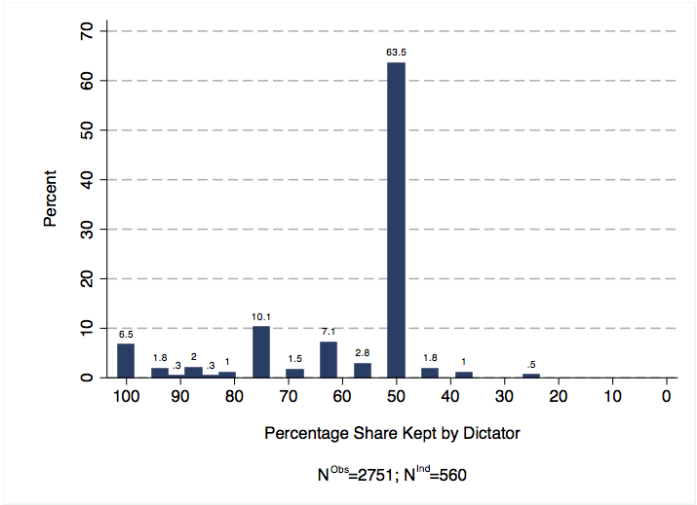

Figure 1.9: Shares of Common Pot Kept by Dictators for Dictators and Receivers Who Have Identical Effort Levels and Rates of Return (Strategy Method)

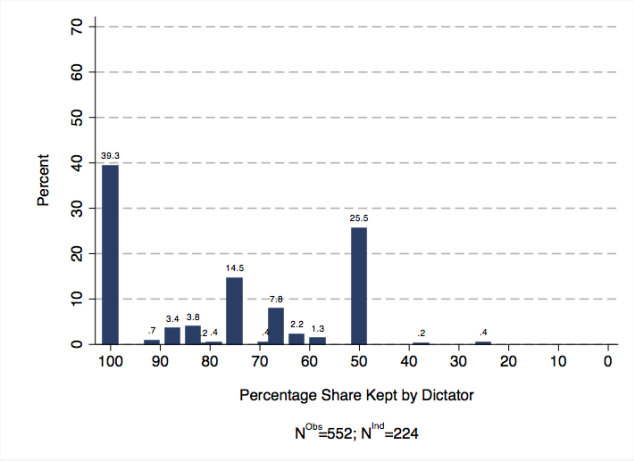

Figure 1.10: Shares of Common Pot Kept by Dictators for Dictators Who Exerted More Effort than Matched Receivers, but Have an Identical Rate of Return (Strategy Method)

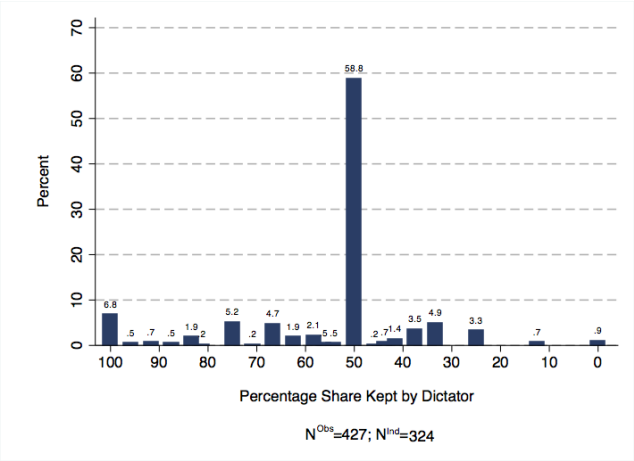

Figure 1.11: Shares of Common Pot Kept by Dictators for Dictators Who Exerted Less Effort than Matched Receivers, but Have an Identical Rate of Return (Strategy Method)

1.8 Tables

	Effort Generated Income From Sorting (Contribution to Common Pot)	Non-Effort-Generated-Income From Returning Unsorted Bags
Number of Bags Sorted	Low Rate of Return / High Rate of Return	Low and High Rate of Return
2	80 MK / 160 MK	0 MK
1	40 MK / 80 MK	20 MK
0	0 MK / 0 MK	40 MK

Table 1.1: Generated Income; Benchmark Treatment, Equality of Opportunity Treatments without Effort Cap and Income Shock Treatments (Monetary Endowment of 30 MK; 0 MK in Case of Income Shock)

	Effort Generated Income From Sorting (Contribution to Common Pot)	Non-Effort-Generated-Income From Returning Unsorted Bags
Number of Bags Sorted	Low Rate of Return / High Rate of Return	Low and High Rate of Return
1	40 MK / 80 MK	0 MK
0	0 MK / 0 MK	20 MK

Table 1.2: Generated Income; Equality of Opportunity Treatments with Effort Cap (Monetary Endowment of 50 MK)

	Data Description	A	B	C	D	E	F	G	H
1	All Observations	560	2751	65.1	50.0	19.7	28.0	46.2	6.1
2	Dictators Exerted More Effort Than Matched Receivers, Both Players Have Identical Rates of Return	416	552	77.9	75.0$^{(\dagger,\dagger)}$	39.3	34.7	25.5	0.5
3	Both Dictator and Receiver Have Identical Effort Levels and Rates of Return	397	397	58.8	50.0*	6.6	26.6	63.5	3.3
4	Dictators Exerted Less Effort Than Matched Receivers, Both Players Have Identical Rate of Return	324	427	54.4	50.0$^{(\dagger,\ddagger)}$	6.8	18.7	58.8	15.7
5	Dictator Has Higher Rate of Return, Both Players Exerted Same Effort Level	184	184	63.7	66.7$^{(\dagger,\dagger)}$	6.5	51.1	41.3	1.1
6	Both Dictator and Receiver Have Identical Effort Levels and Rates of Return	397	397	58.8	50.0*	6.6	26.6	63.5	3.3
7	Dictator Has Lower Rate of Return, Both Players Exerted Same Effort Level	213	213	57.5	50.0$^{(\S,\S)}$	6.6	27.7	55.4	10.3
8	Dictators Contribute More to the Common Pot	416	1214	76.0	75.0$^{(\dagger,\dagger)}$	35.4	35.4	28.5	0.7
9	Dictator and Receiver Contribute the Same to the Common Pot	408	556	59.1	50.0*	6.7	28.0	61.7	3.6
10	Dictators Contribute Less to the Common Pot	442	981	55.0	50.0$^{(\dagger,\dagger)}$	7.7	18.8	59.4	14.1
11	Dictator Contributes 160 MK More to Common Pot than Receiver	62	124	85.3	100.0$^{(\dagger,\dagger)}$	61.3	19.3	19.4	0
12	Dictator Contributes 120 MK More to Common Pot than Receiver	62	62	74.8	80.0$^{(\dagger,\dagger)}$	9.7	69.3	21.0	0
13	Dictator Contributes 80 MK More to Common Pot than Receiver	266	524	76.8	75.0$^{(\dagger,\dagger)}$	36.3	36.5	26.0	1.2
14	Dictator Contributes 40 MK More to Common Pot than Receiver	354	504	73.1	75.0$^{(\dagger,\dagger)}$	31.4	33.7	34.3	0.6
15	Dictator and Receiver Contribute the Same to the Common Pot	408	556	59.1	50.0*	6.7	28.0	61.7	3.6

Table 1.3: Dictator Shares (Strategy Method)

	Data Description	A	B	C	D	E	F	G	H
16	Dictator Contributes 40 MK Less to Common Pot than Receiver	294	389	56.7	50.0$^{(\dagger,\dagger)}$	9.0	20.6	59.6	10.8
17	Dictator Contributes 80 MK Less to Common Pot than Receiver	292	394	54.0	50.0$^{(\dagger,\dagger)}$	6.1	18.6	59.1	16.2
18	Dictator Contributes 120 MK Less to Common Pot than Receiver	95	95	54.8	50.0$^{(\dagger,\dagger)}$	8.4	20.1	56.8	14.7
19	Dictator Contributes 160 MK Less to Common Pot than Receiver	103	103	52.2	50.0$^{(\dagger,\dagger)}$	7.8	12.6	62.1	17.5
20	Benchmark Treatment-Excluding Dictators Sorting 2 Bags (for Dictators with Low Rate of Return)	29	150	60.5	50.0*	16.0	21.3	56.0	6.7
21	Dictator Faces Reduced Choice Set (for Dictators with Low Rate of Return)	46	243	62.4	50.0$^{(\S,\S)}$	17.7	24.7	46.5	11.1
22	Benchmark Treatment-Excluding Receivers Sorting 2 Bags (for Dictators with Low Rate of Return)	44	152	69.9	62.5*	28.3	29.6	40.1	2.0
23	Receiver Faces Reduced Choice Set (for Dictators with Low Rate of Return)	44	158	67.2	64.2$^{(\S,\S)}$	21.5	28.5	46.2	3.8
24	Benchmark Treatment-Excluding Dictators and Receivers Sorting 2 Bags (for Dictators with Low Rate of Return)	29	92	64.8	50.0*	21.7	21.7	53.3	3.3
25	Both Players Face Reduced Choice Set (for Dictators with Low Rate of Return)	44	150	66.2	50.0$^{(\S,\S)}$	24.0	23.3	46.0	6.7
26	Benchmark Treatment-Excluding Dictators Sorting 2 Bags (for Dictators with High Rate of Return)	18	90	57.2	50.0*	16.7	14.4	53.3	15.6

Table 1.4: Dictator Shares (Strategy Method) (continued from previous page)

	Data Description	A	B	C	D	E	F	G	H
27	Dictator Faces Reduced Choice Set (for Dictators with High Rate of Return)	34	176	65.6	$50.0^{(\ddagger,\ddagger)}$	19.3	26.7	44.9	9.1
28	Benchmark Treatment-Excluding Receivers Sorting 2 Bags (for Dictators with High Rate of Return)	36	126	74.4	75.0*	34.9	32.5	27.8	4.8
29	Receiver Faces Reduced Choice Set (for Dictators with High Rate of Return)	36	126	68.5	$62.5^{(\ddagger,\ddagger)}$	20.3	36.8	39.7	3.2
30	Benchmark Treatment-Excluding Dictators and Receivers Sorting 2 Bags (for Dictators with High Rate of Return)	18	54	62.7	50.0*	22.2	20.4	46.3	11.1
31	Both Players Face Reduced Choice Set (for Dictators with High Rate of Return)	36	124	68.0	$62.5^{(\S,\S)}$	21.0	34.7	40.3	4.0
32	Dictator Faces Income Shock (for Dictators with Low Rate of Return)	43	236	68.0	$60.4^{(\S,\S)}$	23.7	28.8	42.4	5.1
33	Benchmark Treatment (for Dictators with Low Rate of Return)	44	240	64.8	50.0*	20.8	26.7	47.5	5.0
34	Receiver Faces Income Shock (for Dictators with Low Rate of Return)	43	232	59.3	$50.0^{(\dagger,\dagger)}$	16.8	13.8	61.2	8.2
35	Benchmark Treatment (for Dictators with Low Rate of Return)	44	240	64.8	50.0*	20.8	26.7	47.5	5.0
36	Both Players Face Income Shock (for Dictators with Low Rate of Return)	42	236	65.0	$50.0^{(\S,\S)}$	21.2	24.6	50.0	4.2

Table 1.5: Dictator Shares (Strategy Method) (continued from previous page)

	Data Description	A	B	C	D	E	F	G	H
37	Dictator Faces Income Shock (for Dictators with High Rate of Return)	37	202	66.8	$62.5^{(\S,\S)}$	16.8	40.1	37.6	5.5
38	Benchmark Treatment (for Dictators with High Rate of Return)	36	198	67.6	62.5*	23.7	31.8	36.9	7.6
39	Receiver Faces Income Shock (for Dictators with High Rate of Return)	36	206	61.2	$50.0^{(\dagger,\dagger)}$	13.1	25.2	56.8	4.9
40	Benchmark Treatment (for Dictators with High Rate of Return)	36	198	67.6	62.5*	23.7	31.8	36.9	7.6
41	Both Players Face Income Shock (for Dictators with High Rate of Return)	39	224	65.1	$58.3^{(\S,\S)}$	17.9	33.8	43.8	4.5

Table 1.6: Dictator Shares (Strategy Method) (continued from previous page)

A	Number of Individuals
B	Number of Observations
C	Mean Share of Common Pot Kept by Dictators
D	Median Share of Common Pot Kept by Dictators
E	Share of Common Pot Kept by Dictator is 100 (in Percent)
F	Share of Common Pot Kept by Dictator is Between 50 and 100 (in Percent)
G	Share of Common Pot Kept by Dictator is 50 (in Percent)
H	Share of Common Pot Kept by Dictator is Less Than 50 (in Percent)
*	Benchmark Group
§	Not Significantly Different from Benchmark Group
‡	Significantly Different from Benchmark Group at 5 percent
†	Significantly Different from Benchmark Group at 1 percent
First Superscript	Wilcoxon Rank-Sum Test Results
Second Superscript	Nonparametric Equality-of-Median Test Results

Table 1.7: Dictator Shares (Strategy Method) (continued from previous page)

	(1)	(2)	(3)	(4)	(5)
Dictator's Effort (Measured in Bags Sorted)	0.090***				0.069***
	(0.006)				(0.006)
Receiver's Effort (Measured in Bags Sorted)		-0.120***			-0.110***
		(0.005)			(0.005)
Difference in Rates of Return			0.06***		0.006***
			(0.003)		(0.003)
Differences in Contributions (Measured in Bottle Caps)				0.013***	
				(0.001)	
Constant	0.480***	0.671***	0.550***	0.574***	0.609***
	(0.031)	(0.029)	(0.032)	(0.029)	(0.029)
No. of Obs.	2537	2537	2537	2537	2537
R^2	0.09	0.2	0.02	0.2	0.25
Demographic and Socio-Economic Controls	YES	YES	YES	YES	YES

Table 1.8: Regressions of Dictator Shares on Model Variables with Baseline Variables. * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

	(6)	(7)	(8)	(9)	(10)
Dictator's Effort (Measured in Bags Sorted)	0.098*** (0.006)				0.072*** (0.006)
Receiver's Effort (Measured in Bags Sorted)		-0.125*** (0.005)			-0.114*** (0.005)
Difference in Rates of Return			0.006*** (0.003)		0.006*** (0.003)
Differences in Contributions (Measured in Bottle Caps)				0.013*** (0.001)	
Effort Cap (Receiver)	0.017 (0.017)	-0.059*** (0.016)	0.007 (0.017)	-0.026* (0.016)	-0.046*** (0.016)
Effort Cap (Dictator)	0.021 (0.015)	-0.024* (0.014)	-0.030* (0.015)	0.015 (0.014)	0.013 (0.014)
Effort Cap (Both Players)	0.041** (0.017)	-0.063*** (0.016)	0.001 (0.017)	-0.009 (0.015)	-0.028* (0.016)
Income Shock (Receiver)	-0.059*** (0.014)	-0.064*** (0.013)	-0.062*** (0.015)	-0.059*** (0.013)	-0.062*** (0.013)
Income Shock (Dictator)	0.013 (0.015)	0.009 (0.014)	0.01 (0.015)	0.009 (0.014)	0.01 (0.013)
Income Shock (Both Players)	-0.014 (0.014)	-0.013 (0.013)	-0.008 (0.015)	-0.022 (0.013)	-0.019 (0.013)
Constant	0.479*** (0.033)	0.712*** (0.031)	0.571*** (0.034)	0.594*** (0.031)	0.634*** (0.031)
No. of Obs.	2537	2537	2537	2537	2537
R^2	0.11	0.22	0.03	0.22	0.26
Demographic and Socio-Economic Controls	YES	YES	YES	YES	YES

Table 1.9: Regressions of Dictator Shares on Model Variables with Baseline Variables (continued from previous page). * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

1.8.1 Tables of Norms at Observation Level for Strong Match

	N^{Obs}	Percent
Strict Egalitarianism	1272	46.24
Libertarianism	917	33.33
Liberal Egalitarianism	931	33.84
Inequity Aversion	713	25.92
Selfishness	542	19.70
Other	549	19.96
Total	2751	

Table 1.10: Unconditional Matches of Preferences to Norms

	N^{Obs}	Percent
Strict Egalitarianism	596	21.66
Libertarianism	85	3.09
Liberal Egalitarianism	11	0.40
Inequity Aversion	194	7.05
Selfishness	146	5.31
Other	1719	62.49
Total	2751	

Table 1.11: Exclusive Matches of Preferences to Norms

1.8.2 Tables of Norms at Observation Level for Weak Match

	N^{Obs}	Percent
Strict Egalitarianism	1577	57.32
Libertarianism	1159	42.13
Liberal Egalitarianism	1182	42.97
Inequity Aversion	874	31.77
Selfishness	803	29.19
Other	N.A.	N.A.
Total	2751	

Table 1.12: Unconditional Matches of Preferences to Norms

	N^{Obs}	Percent
Strict Egalitarianism	738	26.83
Libertarianism	106	3.85
Liberal Egalitarianism	18	0.65
Inequity Aversion	256	9.31
Selfishness	221	8.03
Other	1412	51.33
Total	2751	

Table 1.13: Exclusive Matches of Preferences to Norms

1.8.3 Tables of Norms at Person Level for Strong Match

	N^{Obs}	Percent
Strict Egalitarianism	111	19.82
Libertarianism	42	7.50
Liberal Egalitarianism	5	0.89
Inequity Aversion	9	1.61
Selfishness	9	1.61
Other	385	68.75
Total	560	

Table 1.14: Unconditional Matches of Preferences to Norms

	N^{Obs}	Percent
Strict Egalitarianism	111	19.82
Libertarianism	41	7.32
Liberal Egalitarianism	4	0.71
Inequity Aversion	9	1.61
Selfishness	9	1.61
Other	386	68.93
Total	560	

Table 1.15: Exclusive Matches of Preferences to Norms

1.8.4 Tables of Norms at Person Level for Weak Match

	N^{Obs}	Percent
Strict Egalitarianism	299	53.39
Libertarianism	97	17.32
Liberal Egalitarianism	73	13.04
Inequity Aversion	112	20.00
Selfishness	66	11.79
Other	N.A.	N.A.
Total	560	

Table 1.16: Unconditional Matches of Preferences to Norms

	N^{Obs}	Percent
Strict Egalitarianism	246	43.93
Libertarianism	77	13.75
Liberal Egalitarianism	45	8.04
Inequity Aversion	53	9.46
Selfishness	58	10.36
Other	81	14.46
Total	560	

Table 1.17: Exclusive Matches of Preferences to Norms

1.8.5 Observation Level Regressions

	SE	L	LE	IA	S
Female	0.013	-0.076***	-0.085***	0.000	-0.006
	(0.028)	(0.028)	(0.028)	(0.026)	(0.025)
Age	-0.014***	0.009***	0.007**	-0.001	0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Age Squared	0.000***	-0.000**	-0.000*	0.000	-0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years of Education	0.006*	-0.001	-0.000	0.001	-0.009***
Completed	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Head of Household	-0.005	-0.034	-0.035	0.019	-0.052**
	(0.028)	(0.028)	(0.028)	(0.026)	(0.025)
No. of Children Who Grew	-0.006	0.005	0.007*	0.001	0.004
Up in Household Together	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
No. of Children	0.010*	0.002	-0.001	0.008	-0.019***
in Current Household	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
Household Asset Index	-0.016***	0.009	0.006	-0.004	0.015***
	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)
Household Animal	0.010	-0.030***	-0.026***	0.001	-0.009
Ownership Index	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Mean of Dep. Var.	0.57	0.42	0.43	0.32	0.29
N	2537	2537	2537	2537	2537
R^2	0.01	0.01	0.01	0.00	0.02

Table 1.18: Regression of Weak Match Unconditional Dummies on Baseline Variables. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

	SE	L	LE	IA	S	O
Female	-0.007	-0.062**	-0.064**	0.004	0.001	0.007
	(0.028)	(0.027)	(0.027)	(0.025)	(0.022)	(0.022)
Age	-0.014***	0.009***	0.007**	-0.007**	0.009***	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Age Squared	0.000***	-0.000**	-0.000*	0.000***	-0.000***	-0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years of Education	0.006*	0.004	0.005*	0.006**	-0.003	-0.007**
Completed	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Head of Household	0.037	0.007	0.001	0.058**	-0.014	-0.068***
	(0.028)	(0.026)	(0.027)	(0.025)	(0.022)	(0.022)
No. of Children Who Grew	-0.005	0.006	0.007*	-0.001	0.006*	-0.005
Up in Household Together	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
No. of Children	0.012**	0.001	-0.001	0.012**	-0.013***	-0.004
in Current Household	(0.006)	(0.006)	(0.006)	(0.005)	(0.005)	(0.005)
Household Asset Index	-0.022***	0.003	0.000	-0.008	0.004	0.014***
	(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)
Household Animal	0.010	-0.018**	-0.013	0.002	-0.001	-0.004
Ownership Index	(0.009)	(0.008)	(0.008)	(0.008)	(0.007)	(0.007)
Mean of Dep. Var.	0.46	0.33	0.34	0.26	0.20	0.20
N	2537	2537	2537	2537	2537	2537
R^2	0.02	0.01	0.01	0.01	0.01	0.02

Table 1.19: Regression of Strong Match Unconditional Dummies on Baseline Variables. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

	SE	LE	L	IA	SE, LE, L, IA
Mean					
y	0.03***	0.02***	0.02***	0.02***	0.05***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
SE Loss Term	0.48***				0.55***
	(0.03)				(0.04)
LE Loss Term		0.13***			0.13***
		(0.00)			(0.02)
L Loss Term			0.11***		0.10***
			(0.01)		(0.02)
IA Loss Term				0.24***	0.08***
				(0.01)	(0.02)
SD					
SE Loss Term	0.51***				0.85***
	(0.04)				(0.05)
LE Loss Term		0.08***			0.06***
		(0.01)			(0.01)
L Loss Term			0.08***		-0.19***
			(0.01)		(0.01)
IA Loss Term				0.17***	-0.13***
				(0.01)	(0.02)
Log Likelihood	-5541.66	-5875.84	-5919.72	-5706.79	-4916.95
Number of Observations	36815	36815	36815	36815	36815
$LRChi^2$	1121.41***	247.74***	217.63***	398.58***	1645.01***

Table 1.20: Mixed Logit Estimation. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

	SE	LE	L	IA	SE, LE, L, IA
y	0.02***	0.02***	0.02***	0.02***	0.03***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
SE Loss Term	0.15***				0.09***
	(0.01)				(0.01)
LE Loss Term		0.08***			0.02***
		(0.00)			(0.01)
L Loss Term			0.08***		0.04***
			(0.00)		(0.01)
IA Loss Term				0.14***	0.04***
				(0.00)	(0.01)
Log Likelihood	-6102.37	-5999.71	-6028.54	-5906.08	-5754.51
Number of Observations	36815	36815	36815	36815	36815
Number of Cases	2751	2751	2751	2751	2751
$WaldChi^2$	719.47***	844.16***	827.30***	862.59***	918.14***

Table 1.21: McFadden's Choice Model. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

CHAPTER II

Are Rural Road Investments Alone Sufficient to Generate Transport Flows?

2.1 Introduction

This paper draws lessons from a randomized experiment in rural Malawi, which has one of the highest population densities for a Sub-Saharan Africa country.[1] In order to understand why rural roads - passable for vehicles in rural areas - may not necessarily lead to the provision of regular bus services, a minibus service was subsidized over a six month period over a distance of 20 kilometres to serve five villages.[2]

Like Hine and Rutter (2000) mentioned, "in the quest to tackle rural poverty, feeder road investment is a favored solution of many donors". Feeder roads are proposed as a potential way to take people out for poverty in rural areas (AICD 2009). With this end in view, a benchmark called the rural access index (RAI), which is the proportion of rural people who live within two kilometers (typically equivalent to a 20-minute walk) of an all-season road, has been set.[3]

[1] According to official population estimates in 2002, population density was more than 90 inhabitants per sq.km.

[2] This location was selected in order to make multiple trips per day, and given that the road was not in a very good condition, there was a need to be close enough to the market center.

[3] An all-season road is a (gravel or bitumen paved) road that is passable all year by the pre-

Measuring the cost of being isolated is a growing subject of research in economics and development. Ravallion and Jalan (1997) confirmed the existence of spatial poverty traps and the need for households to reach some asset thresholds before they will participate in markets. Therefore, rural roads investments seem to be critical. Indeed, rural road development enhances access to markets for both inputs and output through a reduction in transaction and trade costs (transport and logistics costs). The greater availability (both monetary and physically speaking) of inputs increases their use by farmers. Consequently, agricultural productivity can increase. Rural roads also allow producers to achieve additional productive opportunities, leading to a rise in production that is highlighted by numerous studies. Stifel and Minten (2008) find, in the case of Madagascar, that isolation (defined as travel time during dry season from the commune center to the nearest urban center) implies lower agricultural productivity, increased transport and transaction costs and increased insecurity. The authors found a major jump of per capita consumption from the least remote quintile to the second quintile and therefore a negative relationship between isolation and poverty.[4]

So far, most Development Partners and governments in SSA have mainly relied on the assumption that most households in rural areas in Africa are not connected to markets by paved or passable roads for motorized transport and therefore need a road passable for a truck/bus. Many investments in rural roads seem to be built on the assumption that they will lead to market provision of transport and thus, poverty reduction and income generation. Estache (2010) points out a lack of rigorous evidence

vailing means of rural transport (often a pick-up or a truck which does not have four-wheel-drive). Predictable interruptions of short duration during inclement weather (e.g. heavy rainfall) are acceptable, particularly on low volume roads (Raballand et al. 2010). Despite major measurement difficulties of the RAI, it is required for World Bank project teams to report it on a bi-annual basis and assess the number of people covered at 2 kilometers in the project area.

[4]Quoted in Raballand et al. (2010).

on these assumptions; namely there is a lack of randomized impact evaluations in the field of investments in roads, which call into question these assumptions and limit the ability to quantify the economic and even social benefits of the provision of rural roads.

In fact, there is some empirical evidence that calls into question current transportation strategies in Sub-Saharan Africa. Using the second Cameroonian national household survey (Enquête Camerounaise Auprès des Ménages II, 2001) and the Cameroon case study, Gachassin et al. (2010) demonstrate that investing in tarmac roads is likely to have a low impact on poverty. For example, isolation from a tarred road is found to have no direct impact on consumption expenditures in Cameroon. The paper reasserts the fact that access to roads is only one factor contributing to poverty reduction (and not necessarily the most important). Considering that increase in non-farming activities is the main driver for poverty reduction in rural Africa, the authors suggest that emphasis on roads investments should be given to locations where non-farming activities could be developed, which does mean that the last mile in rural areas should not be probably a road with a high road level of service (except in peculiar cases of high agricultural potential areas).

One of the main weaknesses of the current approach regarding road investments is the fact that it is built on the strong assumption among policy makers that the existence of a road in good condition in rural areas will enable service providers to come and serve rural areas. In most cases, transport services economics is completely neglected, assuming that road condition is the main determinant of transport costs.

A notable exception was Hine and Rutter (2000), who, based on surveys in almost 100 villages in Ghana and Malawi in the end of the 1990s, found out that "vehicle accessibility alone does not guarantee [...] transport service access". Moreover, Lall

(2009), which, in the case of Malawi, demonstrated that, based on trucking surveys and computation of vehicle operating costs in SSA, that both infrastructure quality and market structure of the trucking industry are important contributors to differences in transport costs. Lall (2009) points out that costs due to poor feeder roads are exacerbated by low volumes of trade between rural locations and market centers. With empty backhauls and journeys covering small distances, only a few transport service providers enter the market, charging high prices to cover fixed costs and maximize markups.

Raballand et al. (2010) finds in field surveys in Uganda, Cameroon and Burkina Faso that load consolidation at the local level decreases the need for a road accessible by truck to every farm and it decreases investment needs and increases value-added for farmers. From a cost-benefit analysis, load consolidation (or agglomeration) is probably the most effective since it mainly reduces road public investment to the secondary network and enables decrease of transport costs due to increased predictability of volumes and strengthened competition between operators.

In the case of this paper, through a randomized evaluation, a passenger bus line was introduced between a rural cluster of villages and a town that serves as a regional trading hub. The study was implemented in rural Malawi, more than two hours by car from the nearest urban location. While most residents of the villages had been to the regional market town (96 percent) and most had been a passenger on a bus prior to the project (81 percent), no regular motorized passenger transportation existed along the road between the market town and the project villages prior to this study. Prior to this study, travel outside of the village cluster was infrequent and most transport was pedestrian or by bicycle. This particular market town offers dozens of stores providing domestic goods and agricultural inputs. There is also a

weekly market in the town that brings traders from disparate areas to sell their goods.

In the study, after a baseline survey, households were randomly assigned to one of seven bus pass categories, with each category assigned a unique price between zero and five hundred Malawi Kwacha (US$ 3.57). Over the course of the project, take up of the bus was recorded (See Goldberg et al., 2010 for a full set of results).

There were significant differences in take up based on the price level assigned to a household. Even small positive prices (far below the marginal cost of a ride) lead to substantial declines in demand for a bus ride.[5]

The experiment demonstrates that households from villages are not willing or cannot afford to pay more than the break-even point for the bus operator.[6] There are two important policy implications of these findings. First, if a bus service is not subsidized, a road in relatively good condition may continue to be used mainly by bicycles and pedestrians. Second, if subsidies are not possible in most rural areas in Malawi (or more generally in SSA), there may be a need to adjust more carefully investments to the potential demand and link it to the types of services provision used in the areas.

The second section describes the experiment protocol and data. Then, results are presented in terms of take up and potential revenues for a bus operator at different prices. The fourth section discusses the policy implications of the results and the final section concludes and presents some ideas for future research in this area.

[5]This concurs with findings from Hine and Rutter (2000), who found out that, for these reasons of affordability of transport services, the poorest segment of rural areas populations continue to walk and do not necessarily benefit from rural roads investments.

[6]Results could have differed if the experiment would have been longer (over a year). However, we also think that results over six months are significant.

2.2 Description of the data and experiment

The study was conducted between the July and December 2009 in a rural area in central Malawi. The sample site includes a cluster of five villages located 17 kilometers from a market town which offers a large weekly market, a health clinic, and access to further means of transport. In total, 542 households were listed in the five villages (in a complete census). A circular circuit track connects three of the five villages, with the other two villages within 10 minutes walking distance. This track was identified to serve as the bus route to use in picking up passengers. While there is some evidence of irregular transport on the road between the village cluster and the market town, there was no evidence of a bus or any other mode of transport intended for passengers that operated along the road connecting the village cluster and the market town.[7]

We report on the second phase of a two part design.[8] Of the originally listed 542 households, 514 households were successfully interviewed and were eligible to participate; 406 of these households are included in the second phase analysis. Participation in phase one and phase two was by random assignment. After selection, a baseline survey was conducted interviewing both men and women. After the completion of the baseline survey, a meeting was called at each of the five villages. At the meeting, a household-level lottery was explained. One member of each household was asked to select a ticket from a bucket. Each ticket contained a number to signify the price of one round trip on the project bus. Prices of a round trip included the following categories: MK 0, MK 10, MK 20, MK 50, MK 100, MK 300, MK 500.[9] After the household member was assigned the lottery ticket, the number was recorded and the

[7]This road was identified as in a good condition for buses to run (during the dry season).

[8]The first Phase included conducting a listing of all households in the village cluster area and randomly sampling 100 households in four villages to participate in the first phase of the project which did not randomize the price, rather randomized overall accessibility.

[9]All households that participated in the first phase (randomly selected) were allocated MK 0.

price was stamped on a bus pass unique to that household.[10] Bus ridership records were kept by a supervising member of the field team and we report on these here.

In addition to collecting data on individuals and households, we also conducted in-depth interviews among minibus owners and drivers, of which two were providing bus services to the market town (not from the cluster of villages included in this study). Minibus owners and drivers near the market town were interviewed in order obtain a broader understanding of the market for transportation in Ntchisi district.

2.3 Results

Results are presented in two parts: the first one relates to the take-up of the bus and presents what is the affordability level in this area and bus use when it is subsidized. The second part tries to assess if there is any price level, at which the bus provider would recover operational costs and then make private bus provision a viable option for transport firms.

2.3.1 Take-up of the bus service

At the household level, more that 60 percent of households used the bus at least once with an average of 2.85 inbound rides. We focus on individual take-up in Figure 1. This figure presents proportion of individuals who used the bus at least once by randomly assigned price. There is a sharp decrease in ridership among those required to pay a positive price, especially prices above 100 Kwacha. These results are similar among men and women (not shown).

[10] One bus pass was valid for all adult members of that household. The bus pass contained a photo and name of the adult members of the pass holder's household and the price of each round trip to the market town.

The coefficients show that the likelihood of using the bus service declines monotonically with price. Other variables that significantly predict using the bus including being the head of the household, being married, and indicators of economic status (asset index or producing food; not shown). Controlling for other individual and household characteristics do not significantly affect the price coefficients. Prices were also important in the types of trips individuals made. Figures 2 and 3 present the reasons for making a trip (as asked on rider logs on the bus) separately by men and by women among those who could ride the bus for free and those who had to pay some positive price.

Among both men and women, there are differences in the types of rides made. In particular, those who were offered a free ride were more likely to go for personal purchases, farm or business reasons, and for health reasons. The difference in health care visits is particularly strong among women, with those at zero price being more than ten percentage points more likely to have used the bus for a health care visit than those at any of the positive price levels.

The findings concur with what was found in Malawi in the late 90s where collection of farm inputs, trips to markets and to health centres were respectively the fourth, fifth and sixth purposes of trips (behind visiting friends, funerals and post office/public telephone) (Rutter et al. 2000).

2.3.2 Bus providers

The interviews with transport operators reveal that most of the operators operate on long-distance routes with a minimum distance of 48 kilometers (maximum of 140 kilometers). Buses were, on average, 14 years old, and in most cases allowed to carry a maximum of 16 people. One of the major constraints identified by providers is the

quality of the road. Providers tend to operate on paved roads or maintained dirt roads. Most rural networks, such as the route leading to the project site, are not accessible during the rainy season. When asked about travels, a typical route to and from the market town involves a single bus making travels each leg of the trip twice a day, with one trip on Sundays. This route is along a paved road that stretches approximately 50 kilometers. The bus typically waits 1 to 2 hours before departure, leaving once there is a minimum of 10 passengers. The price of a one way ride is MK300 and passengers may board and get off at official and unofficial intermediate stops. It should be noted that the existing bus route, being along a primary road, does not provide direct access to remote villages. Residents of the rural villages in our sample site need to walk to the originating depot of the bus route or walk/bike to the paved road in order to board the bus.[11]

In four out of six cases the conductor is hired, and in five out of six cases paid, by the owner of the minibus. It appears that owners are interested in hiring conductors themselves in order to monitor the ability of drivers to generate side-profits. Increased monitoring ability can help to collect additional fees from intermediate passengers, thus bringing the owner's interests in line with those of the driver, i.e. to value routes with intermediate stops. Longer bus routes, while incurring higher costs, are more attractive than local rides if sufficient rents can be shared by the owner and driver in picking up intermediate passengers.

The three main factors regarding the assessment of profitability of a route are: seasonality of a route (or not), high demand, and prices of existing modes of transport.

[11]There are also special considerations with cargo. An average of three to four people brings cargo per full minibus. The general rule is that small cargo - items that can be stored on a passenger's lap - can go for free. The content of the cargo is rarely monitored by the driver. However, common types of cargo include crops, especially groundnuts, and bottle crates. Larger cargo may be charged between 15 and 50 percent of a regular passenger price, but enforcement of this charge is discretionary. Only a rough 50 percent of cargo will be charged to the passengers.

The reason why certain owners opt for certain routes which do not stand out by several of these criteria has to do with the location of the owner. Most owners are uncomfortable with lack of monitoring possibilities and thus chose to have their bus run on a route starting or ending at their place of residence. Local bus ownership can thus be seen as foundation for servicing remote areas.

Furthermore, the quality of the road network is a decisive factor mentioned by 50 percent of the sample in determining the actual route. In the case of evaluating a route on which no bus is running, the evaluation of the road condition increases in importance.

Prices for buses depend on length, origin and destination for the trip, ranging between MK300 and MK700. If a route is neither frequented by matolas nor minibuses originally, drivers will start with what they perceive as low prices given their own assessment of demand and gradually raise them or have focus groups in which they ask people about their willingness to pay. However, most drivers report that they would shy away from such a route due to uncertainty of demand.

Seasonality is one of the most important factors which lower profit, as named by half of respondents. In this context, seasonality should be interpreted as routes becoming impassable during rainy season and demand fluctuations that coincide with the agricultural calendar. Legal restrictions regarding maximum capacity, competition, and illegal callers are further threats, the latter being specifically mentioned by operators in the market town. There are also other costs for minibus providers including membership fees, repairs, and fuel.[12]

[12]Fees collected by the Minibus Association, maintenance cost, and fuel prices also have to be taken into account when assessing profitability. While fees for being part of the Minibus Association are mandatory and amount to MK200 up to MK750 per day, most operators do not see a benefit of being member. The only benefit mentioned by two of the drivers is the regulation of bus departure order; official callers prevent line-skipping - the first bus in line is first one to be allowed to start. Owners are responsible for the majority of repairs, though drivers are usually responsible for the

Using accounting costs of the bus incurred during this study and the predicted demand found in this analysis, we are able to estimate profits and any requisite subsidies that would be necessary to promote bus operation between remote areas and market towns.

In order to predict total costs, we include a fixed cost bus rental or investment term (θ) and fuel costs that have two components, one which requires travel to pick up passengers (fixed) and one that varies based on the level of demand.

$$TC = \theta + \gamma[\omega(0, d) + (\omega(Q, d).\varphi(Q, B))] \tag{2.1}$$

Where $\omega(Q, d)$ is a measure of fuel efficiency for the bus with demand, Q, and round-trip travel distance d and $\varphi(Q, B)$ is a ceiling function of number of bus trips required to meet demand with a maximum number of passengers per trip, B. γ is the price of fuel. Marginal cost of an additional passenger is therefore:

$$MC = \gamma(\omega'(Q, d).\varphi'(Q, B))] \tag{2.2}$$

Based on the cost of bus operation incurred by this study we set the cost parameters[13]

$\theta = 7000$

d = 34 kilometers

γ = MK213

$\omega(Q, d) = [0.29 + 0.0015Q]d$

$\varphi(Q, B) = min\{n \in Z \mid n \geq \frac{Q}{B}\}$

replacement of tires. Tire replacement usually occurs twice annually. This is of importance to servicing remote areas, since the probability of tire damage increases along dirt and ungraded roads. The driver will be less inclined to seek out rural routes as a result. Fuel prices reflect both distance and location of purchase. Again, bus service in more remote areas leads to increased costs of bus operation due to a lack of infrastructure, in this case, filling stations.

[13]All parameters are in the appendix.

B = 14

Due to the non-linearity of the bus trip function, $\varphi(Q, B)$, marginal costs spike at the points where an additional bus trip is required. The marginal cost of adding an additional bus trip is substantial. The fixed costs of the bus imply that carrying the first passenger costs MK11,200 (US$75). Each additional trip beyond the first trip implies a MK2100 cost increase. The marginal cost of adding an additional passenger to an existing bus trip, i.e. increasing passengers from 2 to 3, is a minimal MK10.86. Therefore, our model suggests that fixed costs matter a great deal in achieving low marginal costs.

Total revenues are estimated based on the inverse demand curve for bus use (equation 2). For demonstration, we assume a linear demand curve; however, we utilize regression estimates for the demand curve in our profit calculations.

$$p = \frac{\alpha}{\beta} - \frac{1}{\beta}Q \tag{2.3}$$

Total revenue is thus

$$TR = (\frac{\alpha}{\beta} - \frac{1}{\beta}Q)Q \tag{2.4}$$

and marginal revenue

$$MR = \frac{\alpha}{\beta} - \frac{2}{\beta}Q \tag{2.5}$$

Based on estimated total cost and total revenues, we see no point at which the bus would generate positive profit. Table 3 shows the revenue estimates based on demand estimates assuming a non-linear demand curve. We base the demand estimates of an unconditioned regression of Phase 2 bus rides on dummies for price levels. The base demand at MK0 would be 28 passengers per day, falling to 0.21 passengers per day

at MK500 price. Based on the costs of this project and the estimated demand, we see that profit is maximized between prices MK100 and MK300, but is nevertheless negative.

Our findings suggest that subsidizing a daily bus to the market cluster included in this study would be costly. We see that a daily subsidy of MK10,082 (US$67) would be at least needed to achieve a breakeven point for the bus operator (for a roundtrip). This finding is crucial since there is a usual plea to call for competition in transport services in rural areas in order to curb transport prices. This experiment demonstrates that competition, in this case, is impossible to get and, even worse, one operator can never break-even.

The requisite subsidy is due to both supply and demand factors. On the cost side, high fixed costs of bus rental and discontinuities in costs caused by the discrete jump in costs once a bus becomes full drive up the cost of bus operation. On the revenue side, we find that population density and significant price sensitivity drive down aggregate demand for the bus service to the point where the bus route would not be profitable for a private firm.[14]

2.4 Policy implications

The results from the experiment in Malawi present several policy implications as well as future directions for research. Notably, infrastructure upgrade and rehabilitation should not be the only answer to connectivity problems in rural areas; transport services provision should also be looked at in details.

[14]This is similar to the findings of Rutter et al. (2000) and Hine and Rutter (2000) with, for instance, an inverse relationship between loading times and population density: in low population density districts, loading times are longer, which is detrimental to the quality of services.

There are two possible major objectives with subsequent different public interventions[15]

1. Ensure motorized service provision (bus/truck),
2. Ensure non-motorized service provision.

In the first case, the results of the experiment clearly demonstrate that a mix of public interventions between road investments and service subsidies needs to be found. Assuming USD$ 3,000 per kilometer to maintain an earth road in a passable condition for a minibus for the year (rainy season excepted), in this case, USD$ 60,000 have to be spent[16](which is equivalent to over USD$ 100 per household). Service provision subsidies in this case would be approximately equivalent to USD$ 12,000.[17] Assuming that this would be possible to implement, 20% of the recurrent costs should be aimed at subsidizing a bus operator to make it break-even.[18] Not subsidizing services would be equivalent to a waste in road investment since villagers will either walk on this road or use intermediate means of transport (IMTs), such as bicycles or motorcycles and, therefore, a high level of service[19] for the road would not be needed.

In the second case, road level of service (technical standard, width...) should be reduced[20] aiming at ensuring passability of intermediate means of transport, which would mean the importance of working exclusively on critical points/obstacles (small

[15]Due to the existence of poverty traps (Azariadis and Stachurski, 2005) and the need to increase rural growth, subsidizing transport services could be justified in order to increase the economic impact of rural roads.

[16]Hine and Rutter (2000) gave the figure of over 50 USD per head.

[17]Assuming that a bus service during a period of six months.

[18]It would be obviously crucially important to determine how service provision would be subsidized and update regularly the amounts in order to limit waste.

[19]Level of service generally describe traffic conditions in terms of speed and travel time, volume and capacity, traffic interruptions, comfort and safety.

[20]Where repressed demand is not high.

bridges for instance).

Like pointed out in Raballand et al. (2010) using Cameroon, Burkina Faso and Uganda examples, there is a continuum of integration to markets for most households in Africa. In some cases, a road may be non-passable for cars, a motorcycle driver may, for instance, dismount the motorcycle and walk it around the trouble spot in the road and then continues his trip. Therefore, from an economic perspective, most rural populations are somehow connected to markets whereas most data analysis or policy prescriptions are based off of binary classifications of connectivity as either 0 or 1. Hence, from a public policy perspective, investments in roads might have a lower impact on economic development than expected due to the fact that transport connectivity is only one component of rural development, and sometimes not the most important.[21] It may also explain why despite major investments in rural roads in some countries, poverty reduction has not reduced significantly.

In any case, economic and social data (such as traffic data, vehicle operating costs for minibuses/IMTs, purpose of minibus usage) need to be collected for policy-makers to decide on which investment choice is required to enable which type of service provision, based primarily on demand assessment. Surveying transport demand in the Western part of the country, Zambia JCTR (2008) found that 92% of the local farmers surveyed do not produce more than 50 bags of 50 kilos each of agricultural products in a year, which means that without consolidation, the current demand does not justify transport services to develop.

There is a recurrent lack of data collection on the demand side and many investments

[21]Ruijs et al. (2004) find out that the direct effect of transport costs reductions on food prices, such as cereals, requires some nuance and tempered expectations in the case of Burkina Faso, notably due to the organization of markets.

are carried out on the multiple assumptions that roads improvement translates into reduced vehicle operating costs (VOCs), which are then assumed to be passed to the final users of transport services.

In reality, reduced VOCs do not translate to reduced transport prices (especially where volumes are low) and reduced transport prices do not translate to poverty reduction if the poor can not afford to use transport services or need other factors to increase production.

From a donor perspective, this experiment demonstrates that a rule of thumb such as the RAI is likely to be an investment waste in many rural areas in SSA since, at best, a motorized service will be provided at an unaffordable price for most households and at worse, no motorized transport will be provided, which means that most households will continue not to go to a trade center/small town or to go by cycling and even more probably by walking.

2.5 Conclusion and areas for further research

The randomized experiment summarized in this paper is the first of this kind in SSA and illustrates that road condition does not necessarily generate transport provision at an affordable price for villagers and does not necessarily enable minibus providers to break-even. It confirms that affordability in rural areas in SSA can be very low and makes service provision profitability unpredictable in most cases.

Therefore, in this context, setting rigid rules on investments across the continent is likely to increasingly add wastes since investments are likely to go further and further in remote places where demand may not necessarily justify services provision to break even. On the contrary, there is an increasing need to differentiate allocations/technical

solutions and possibly service subsidies to adjust investments to potential demand.

Areas for future research are numerous since this is only a start to use such approach in transport. Moreover, there may be some specificities related to Malawi: Hine and Rutter (2000) had found out that the use of motorized transport services in Malawi was extremely low compared to Ghana, for instance. Moreover, there may be some area-specific features

Therefore, it would be important to undertake such experiments in areas with higher population density than this study site to assess the affordability level and potential demand and for a longer period of time since some decisions to invest in agriculture could be taken on the fact that a bus provision is guaranteed for at least two seasons. Moreover, in an area like Nchtisi, where potential demand/affordability seem to be low, it would be important to have randomized experiment with IMTs: would a subsidy of bicycles, tractor, oxcart would generate more demand or not? What would be the break-even point and would villagers could then afford a trip to town at the break-even point? Moreover, it could be important to design some incentives for farmers to consolidate transport and assess the impact on transport supply and prices (like presented in Kunaka 2011).

This type of work is crucial in order to design solutions, which would then increase on the ground access to markets/services because, for the time being, despite discourse and massive investments in infrastructure, it may actually not have the expected outcome on access and then crowd out investments in other sectors, where it could have a greater impact on economic and social development.

2.6 Figures

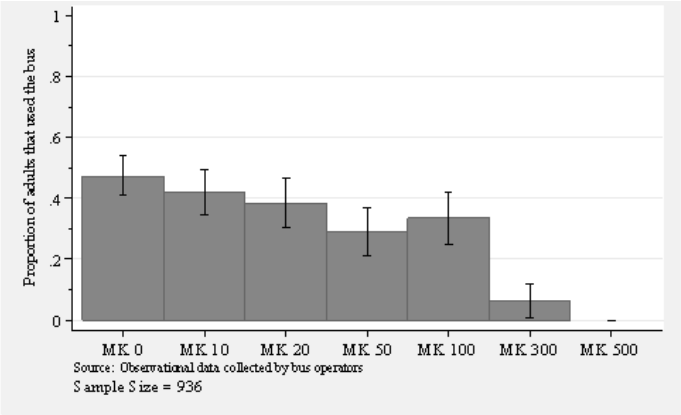

Figure 2.1: Bus Service Take-up according to Prices. Results are based on the individuals that participated in Phase 2.

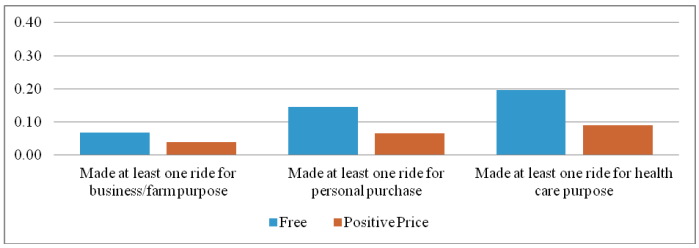

Figure 2.2: Trip Purposes for Women

Figure 2.3: Trip Purposes for Men

2.7 Tables

	Adult Used Bus	Total Inbound Rides		
	1	2	3	4
MK10	-0.06	-0.05	-0.28	-0.26
	(0.06)	(0.05)	(0.24)	(0.22)
MK20	-0.09	-0.08	-0.34	-0.33
	(0.06)	(0.06)	(0.25)	(0.23)
MK50	-0.18***	-0.15**	-0.63**	-0.56**
	(0.06)	(0.06)	(0.25)	(0.23)
MK100	-0.14**	-0.15**	-0.74***	-0.77***
	(0.06)	(0.06)	(0.21)	(0.20)
MK300	-0.41***	-0.43***	-1.20***	-1.26***
	(0.05)	(0.05)	(0.19)	(0.20)
MK500	-0.47***	-0.49***	-1.32***	-1.35***
	(0.04)	(0.04)	(0.18)	(0.18)
Male		0.02		0.41***
		(0.04)		(0.14)
Head of Household		0.14***		0.46***
		(0.04)		(0.17)
Married		0.09**		0.22*
		(0.04)		(0.13)
Adults in the Household		-0.01		-0.08
		(0.02)		(0.06)
Children in the Household		-0.00		0.04
		(0.01)		(0.04)
Constant	0.47***	0.41***	1.32***	1.55***
	(0.04)	(0.11)	(0.18)	(0.40)
Mean of Dep. Var.	0.339	0.340	0.844	0.847
N	936	933	936	933
R^2	0.094	0.155	0.053	0.124

Table 2.1: Bus Use in Response to Prices. Results are based on the individuals that participated in Phase 2. Note: This table presents OLS regressions on bus use for adults (18 years or older). Heteroskedasticity-robust household-clustered standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

	MK0	MK10	MK20	MK50	MK100	MK300	MK500
Price	0	10	20	50	100	300	500
Demand vs. MK0 (*)	0.00	-0.28	-0.42	-0.61	-0.75	-1.26	-1.34
Bus rides to market town in one month	1.35	1.07	0.93	0.74	0.60	0.09	0.01
Bus rides to market town per day	0.023	0.018	0.016	0.013	0.010	0.002	0.000
Adult Population	1200	1200	1200	1200	1200	1200	1200
Total adult market town bus rides per day	27.93	22.18	19.29	15.22	12.38	1.96	0.21
Total Revenue	0.00	221.76	385.79	761.22	1237.68	587.14	103.45
Marginal Per Passenger Revenue	0	10	8.50	24.66	38.50	-332.40	-2337.85
Average Per Passenger Revenue	0	10	20	50	100	300	500
Projected Profit (Daily)	**-13583**	**-13296**	**-13099**	**-12681**	**-10082**	**-10624**	**-11097**

Table 2.2: Revenues Estimates. Note: Values in Malawi Kwacha. Demand estimates are based on coefficients from unconditional regression of the total phase 2 bus rides on a set of dummies for each price level. (*) Calibration over the full sample

CHAPTER III

In the Public Eye: Distributional Choices in Rural Malawi Under Perfect and Imperfect Information

3.1 Introduction

People care deeply about their social image, more so in small, tightly knit communities as opposed to more fragmented, anonymous settings. In Sub-Saharan Africa, including Malawi, people predominantly live in such small communities, making social image concerns likely to influence various aspects of their decision making, including economic actions. However, living conditions are in the process of changing in even the remotest of locations as broader socio-economic transformations take place in these countries. Modernization is forcing these traditional communities to slowly disintegrate, weakening the potential influence of social image on economic behavior.

The influence of culture on economic growth is a recurrent theme in the development literature, ever since the seminal work of Weber (1992). In this context, sharing norms have often been the focus of interest due to their influence on redistribution, and hence, incentives for productive activity within a community or society.

Given the changes in community structures in rural areas of developing countries,

it is therefore of great interest for this debate to investigate if, and to what extent, these sharing norms are dependent on social image or are fully internalized by people.

If the former is the case, we might expect more abrupt changes in social behavior when communities become less tightly knit, i.e. we might see larger, faster, and clearly discernable changes in behavior due to erosion of traditional structures. In other words, as soon as the relevance of the concern about social image is altered by a change of environment, individuals would optimize differently. Fully internalized norms which are not followed due to any social image effect, on the other hand, are likely to be eroded gradually and adaptively, based on the requirements of a changed social and economic environment. Depending on two factors - whether the norms a community follows provide incentives for productive activities, and the direction in which 'social image effects' alter the behavior of individuals - these different scenarios will lead to different predictions regarding the potential interaction between modernization, changes in sharing norms, and further feedback effects on economic development.

Specifically, what this paper does is to observe whether and how sharing behavior regarding effort-generated social surplus of individuals in rural Malawi changes, when they know that their contribution to and share taken of the social surplus cannot be fully observed by other individuals, i.e. how social image concerns determine optimal shares in this setting.

This study builds on a branch of literature, started by Mitzkewitz and Nagel (1993), which primarily uses ultimatum games to investigate the effects of incomplete information of the responder about the size of the pie on sharing behavior of the proposer.[1]

[1]See Gueth et al. (1996), and Huck (1999) for other studies belonging to this branch of literature. For examples of other related literature see, f.ex. Roth and Malouf (1979) and Roth et al. (1981), who investigate bargaining behavior between two individuals when the size of the prize is not known to one of the players.

The effect generally found is that proposers pretend to be more generous but are de facto more selfish than under full information. This is achieved by creating the impression that they follow a sharing rule that would be perceived as fair by a majority of players for a small pie when in reality the pie to be shared is large, this being not known to the responder with certainty.

Specifically, Mitzkewitz and Nagel (1993) introduce the notion of "anticipation philosophy" where proposers take the expected level of acceptance of an offer by the responder into account when making their sharing decision. This acceptance level depends on the expected size of the common pot in case of an offer game (i.e. whether at least half of the expected pie is given to the responder) and whether a demand is clearly unfair (less than half of the small pie is left to the responder) in case of a demand game.

Contrary to most of this literature, I employ dictator games to distinguish whether it is the fear that the responder will reject an offer perceived to be unfair, or truly concern about ones social image, that drives expected changes to sharing behavior under incomplete information. In a dictator game only the latter consideration should play a role.

The only other study, to my knowledge, which employs dictator games in this context is Ockenfels and Werner (2011) who conducted their study online using newspaper readers in Germany as experimental subjects. The study featured a complete information and an incomplete information treatment with two different pie sizes in each treatment. In both cases subjects make their decisions about windfall income, i.e. the pie was not generated by the subjects themselves. The main finding of this study is that dictators in the incomplete information treatment are more frequently giving

less or equal to half of the small pie to the receivers than dictators in the complete information treatment, i.e. subjects pretended in a sophisticated way that they were facing a small pie and behaving in a fair manner given that.

In contrast to Ockenfels and Werner (2011), I investigate social image effects for *effort-generated* (as opposed to windfall) income in a *developing* (as opposed to highly industrialized) country setting. This aspect of the study throws light on the ongoing debate about culture as a factor in economic development as detailed above. Additionally, a novel aspect of my study is the attempt to distinguish the influence of different norms of distributive justice on sharing decisions similar to Cappelen et al. (2007) and capture trade-offs between selfishness, fairness considerations, *and* social image effects.

Ockenfels and Werner (2011) note that due to the setting of their experiment, an unnatural distance between the experimental subjects (newspaper readers) exists which may limit transferability of results. The present study, on the other hand, was conducted in rural communities of a developing country where subjects interact in their natural surroundings. This arguably renders the results less domain specific.

In addition, I make use of a rich set of demographic and socio-economic data to investigate the determinants and predictors of selfish behavior, norm-based behavior, and decision making guided by social image concerns.

The rest of the paper is organized as follows. Section 2 presents a model of decision making. Section 3 details the experimental set-up while section 4 talks about the implementation in the field. Section 5 discusses empirical results, while section 6 concludes and provides an outlook on emerging questions.

3.2 Model

The subsequent discussion closely follows Cappelen et al. (2007) and Mueller (2011b). I study two-person dictator games with a production phase. Players are denoted by i, with $i = 1$ for the dictator and $i = 2$ for the receiver. The players may differ with respect to effort chosen, e_i, and rate of return, a_i. Vectors e and a are defined as $e = (e_1, e_2)$ and $a = (a_1, a_2)$. The production functions $x_1 = e_1 a_1$ and $x_2 = e_2 a_2$ determine the dictator's and the receiver's respective effort-generated income. $X(e, a) = x_1(e_1, a_1) + x_2(e_2, a_2)$ denotes the total effort-generated income that is to be distributed among team members by the dictator.[2] Each dictator allocates an amount $y \leq X$ to herself, leaving $X - y$ for the receiver. Non-effort-generated income is denoted as z_1 and z_2, with $z = (z_1, z_2)$, and comprises monetary endowments of the players as well as money received for foregoing options to exert effort as will be explained in section 3.

Individuals are assumed to have other-regarding preferences, i.e. they care about their own income and about the fairness of a distributional situation. In addition, subjects' beliefs about how self-interested they are perceived to be by others, are assumed to directly enter their utility function by affecting their marginal utility of income. Specifically, dictators are assumed to maximize the utility function

$$V(y; e, a, z, b, s) = (\gamma_0 + \gamma_1 I(h))y - \sum_k \beta_k \frac{(y - m^k(e, a, z, b, s))^2}{2X(e, a)}.$$

$m^k(e, a, z)$ is the fair amount of effort-generated income that the dictator should keep for herself according to norm k, while $\gamma_0 > 0$, $\gamma_1 > 0$, $\delta \geq 0$, and $\beta_k \geq 0$ are parameters expressing the importance of income and fairness, respectively, to the dictator. I(.) is an indicator function, where h denotes a state of the world in which half of the

[2] Total effort-generated income is also referred to as "social surplus" or "money in the common pot" throughout this paper.

dictator's contribution to the common pot remains "hidden" from the receiver. The assumption is that dictators who believe that the receiver might perceive them as less self-interested than they really are, place a higher weight on income as opposed to fairness considerations.

Maximizing this function determines the optimal share y^* to be

$$y^* = \frac{\sum_k \beta_k m^k(e,a,z,b,s)}{\sum_k \beta_k} + \frac{(\gamma_0 + \gamma_1 I(h))X(e,a)}{\sum_k \beta_k},$$

under the assumption that an interior solution exists.

Optimal shares for each dictator are assumed to be co-determined by at least one of the following norms: strict egalitarianism (SE), inequality aversion (IA), liberal egalitarianism (LE), or libertarianism (L), all of which satisfy the no-waste condition.

According to strict egalitarianism dictators and receivers should receive equal shares of the social surplus, hence

$$m^{SE}(e,a,z) = \frac{X(e,a)}{2}. \tag{3.1}$$

According to inequality aversion in its simplest form, total incomes of players are to be equalized in an optimal allocation, hence

$$m^{IA}(e,a,z) = (z_2 - z_1) + \frac{X(e,a) - (z_2 - z_1)}{2} \tag{3.2}$$

if $z_1 \leq z_2$, and

$$m^{IA}(e,a,z) = \frac{X(e,a) - (z_1 - z_2)}{2} \tag{3.3}$$

if $z_1 > z_2$.

According to the principle of liberal egalitarianism, optimal shares should be proportional to the relative effort choices of players. Hence,

$$m^{LE}(e, a, z) = \frac{e_1}{e_1 + e_2} X(e, a). \tag{3.4}$$

Libertarianism, on the other hand, is an outcome based principle: The fair share for the dictator is given by the dictator's marginal product to the social surplus,

$$\begin{aligned} m^{L}(e, a, z) &= \frac{a_1 e_1}{a_1 e_1 + a_2 e_2} X(e, a) \\ &= a_1 e_1. \end{aligned} \tag{3.5}$$

3.3 Experimental Design

The analysis in this paper is based on two treatments of a one-shot, two-person dictator game with a production stage.[3] 320 randomly selected participants from rural parts of Ntchisi District in the Central Region of Malawi were assigned to the different treatments during recruitment. Please refer to Figure 1 for details of randomization and sample sizes. Their eligibility to take part in a scientific experiment about community norms, including survey parts, was communicated to the participants during both recruitment and consenting phase. Individuals were told the following details: First, that the experiment would involve decision making and potentially carrying out a simple task, similar to one they might do at home or work; and second, that the experiment might involve the distribution of money between themselves and another participant of the experiment.

Subjects learned that the minimum they would earn was a token gift worth ap-

[3]The entire experiment consisted of eight treatments. The other treatments are discussed in Mueller (2011b).

proximately 30 Malawi Kwacha (MK), and, depending on their and their partner's decisions, an additional monetary amount between 0 MK up to 350 MK. 350 MK corresponded to 2.18 US-Dollar (typical cash bid rate) during the time of this study. This was put into perspective for subjects by informing them that the total participation time (including travel, consenting, experiment, surveys, and payment) would not exceed three hours. De facto participation time was approximately 1.5 hours, see section 4.[4] The value of the incentives to participants can be understood in the context of the following facts: (1) 46.7 percent of the population in the Central Region lived at or below the national poverty line of 16,165 MK per year (i.e. approximately 44.29 MK per day) as recently as 2005, with 16.1 percent classified as "ultra-poor" according to the World Bank (2007), where ultra-poor indicates the inability of individuals to meet their recommended daily food needs. (2) I collected the data used in this paper during the months of July and August 2010. Both months fall in the dry season which is characterized by especially low, constant opportunity costs for participants. Goldberg (2010), for example, who conducted a labor supply study in Malawi's Central Region during the dry season, offered various wages for sessions of hard physical labor, and found that over 70 percent of her sample of 529 subjects chose to work at a rate of only 30 MK *per day*. Thus, monetary rewards provided by my experiment were clearly substantial.

The rest of this section proceeds as follows. I first detail the general experimental procedure and elaborate how the treatments allow for the identification of the norms presented in the model section. I then highlight how the differences between the benchmark and the incomplete information treatment are able to generate data aimed at answering the main question of this paper, i.e. whether and how subjects' beliefs about their own social image in this particular context directly enter their utility

[4]Three hours was mentioned as an upper limit based on an outlier during piloting.

function.

3.3.1 General Experimental Procedure

Lists of the information subjects had at various stages of the game are provided in Tables $1-4$. At the initial stage of instructions, subjects learned that they had been anonymously matched with another player from the same location before receiving their endowments. The monetary part of the endowments was given to them in the form of bottle caps, with one bottle cap representing 10 MK.[5] Players were aware of the rate of conversion. Unsorted bags of beans formed the non-monetary part of each subject's endowment. The conversion rate of 20 MK for returning an unsorted bag of beans to the experimenter was made known.

Players were then informed of their options. They could sort zero, one or both of their bags before returning them to the experimenter. Rates of return for sorting bags were either high (80 MK per sorted bag) or low (40 MK per sorted bag), and assigned and announced to the subjects ex ante. However, players did not learn their partner's rate of return. Special emphasis was placed on conveying to the subjects that the higher return from sorting would become part of a common pot belonging to them and their partner while the lower return from returning unsorted bags would go straight to them. Individuals learned that this social surplus would be divided by either them or the other player and that the dictator's identity would be revealed

[5]This level of divisibility of currency was chosen throughout the experiment based on informal focus group discussions conducted by study staff. Here, the largest unit of money which was not uniformly seen as "making a difference" was 5 MK. Note that the focus groups were conducted in the second biggest trading center of the district, i.e. an area in which individuals face higher average prices than our typical study participant. To be on the safe side I chose 10 MK to be the smallest unit for my experiment. Goods can be bought for as little as 0.5 MK in the study region. Examples of goods priced at only 10 MK in rural areas of the study district (in 2010, at the time the study took place) are a large piece of bread or a package of pain killers. Given the great overall poverty of experimental subjects and the outcomes of the focus group discussions, we can be confident that the differences between each pair of potential dictator shares was substantial enough to incentivize strategic behavior.

only after all sorting decisions had been carried out.

Subjects were told that divisions would be made according to the strategy method, i.e. dictators would be asked to share hypothetical common pots for all potential effort-choice/rate of return combinations of the receiver *given* their own effort choice and rate of return. During instruction, it was further conveyed that payment would immediately follow participation and that all players would learn about their partner's sorting decision at that point. The rate of return of the matched player, on the other hand, would only become known to a player if she was a) a dictator or b) a receiver in the benchmark treatment, i.e. receivers in the incomplete information treatment knew they would never learn the true rate of return of the dictator just as the dictators in that treatment knew that receivers would never learn their rate of return. Receivers in the benchmark treatment would learn about the payoff relevant sharing decision of the dictator in addition.[6] Hence, before subjects made decisions, they had complete information about the production, distribution, and payment phase of the game. Common knowledge between matched players was established by the end of the instruction phase.

Sorting decisions had to be made after a brief test-sorting, immediately following instruction, in which subjects could familiarize themselves with the specifics of the effort task. The status of the players (dictator versus receiver) was revealed following the implementation of the sorting decision. After this, dictators entered the distribution phase. A dictator could change her mind about each of her sharing decisions once. During subsequent payment, final payoffs for each individual - which comprised

[6]This implies that these receivers did not have the right to learn about the dictator's entire strategy and that subjects were aware of this fact. Note also, that the sharing decision could have been backed out by the player in the benchmark treatment. However, given the educational limitations of the experimental subjects in this study we chose to perform this calculation for them so that there would not be any kind of bias based on the arithmetic ability among the participants.

their monetary endowment, the payment for bags they had returned unsorted, and the share of the common pot the individual was to receive based on the dictator's wishes - were determined by the experimenter. Dictators learned the sorting decisions and rates of return of matched receivers independent of their treatment groups at this point. Receivers in the benchmark treatment learned the same information about the dictators. In addition, they were told her payoff relevant sharing decision. Receivers in the incomplete information treatment, on the other hand, only learned the sorting decision of the dictator.

3.3.2 Experimental Set-up and Norms

Both treatments in this study are designed to learn about the distribution over strict egalitarianism, liberal egalitarianism, and libertarianism, as in Cappelen et al. (2007). Additionally, some individuals might base their decisions on inequality aversion since endowments and money from returning unsorted bags for both players, as well as the money in the common pot, are displayed prior to each allocation decision subjects are required to make. Purely selfish allocations are those in which the dictator takes the entire common pot for the purpose of this discussion.

Subjects acting in line with libertarianism have the lowest informational requirements since the optimal libertarian share is always one's own marginal contribution. Information about the common pot is sufficient for both purely selfish and strict egalitarian allocations: dictators keep 100 percent and 50 percent, respectively. Liberal egalitarians reward themselves and others proportional to relative effort. Since additional effort from both players also increases common pot size, optimal amounts for liberal egalitarianism are depicted for different effort levels of the dictator *conditional* on the receiver's effort level and both players' rate of return. Inequality aversion is depicted in the same way. However, the mechanism through which higher effort levels

increase a player's optimal share is more indirect compared to liberal egalitarianism: exerting additional effort implies that fewer bags are returned to the experimenter unsorted. Hence, a player who exerted more effort must be "compensated" for giving up this type of income in addition to creating a larger social surplus, to be shared.

Four different scenarios give us the possibility to distinguish norms:

1) **Matched players are assigned the same rate of return and choose to sort an identical number of bags.** All norms prescribe equal shares for the players.

2) **Matched players are assigned identical rates of return, but choose to sort a different number of bags.** Strict egalitarianism prescribes equal shares for the players, while all other norms prescribe an unequal distribution of the common pot.

3) **Matched players have different rates of return, but choose to sort an identical number of bags.** Libertarianism prescribes an unequal distribution of the common pot, while all other norms prescribe equal shares.

4) **Matched players are assigned different rates of return and choose to sort a different number of bags.** Here, two sub-cases can be distinguished: i) If the contributions of the players to the common pot do not coincide, optimal shares are unequal under all norms except for strict egalitarianism. ii) If the contributions to the common pot coincide, libertarianism and strict egalitarianism prescribe equal shares. Liberal egalitarianism and inequality aversion prescribe unequal shares.

3.3.3 Benchmark and Incomplete Information Treatment and Dictator Beliefs

I employ two treatments to investigate whether and to what degree incomplete information of the other player about own rate of return plays a role for distributional decisions of the dictators. In the benchmark treatment, both, effort levels and rates of

return of the other player become known to each subject during the payment phase. In contrast to this, the rate of return of the dictator will never become known to the receiver in the incomplete information treatment. The dictator is aware of this fact while making her decisions. If dictators care about social image we may expect them to derive a higher marginal utility from own income relative to fairness considerations if they are assigned a high rate of return, sorted a positive number of bags, and were part of the incomplete information treatment as compared to all other situations, i.e. if half of their contribution to the social surplus cannot be seen by the receiver (please refer to section 2 above).

3.4 Field Experimental Procedures

This section describes the details of the field experiment, including data collection and game procedures, following Mueller (2011b) closely in its structure and presentation.

The study was implemented in the field through three distinct parts: a baseline survey, the game, and an opinion survey. The site of the project was Ntchisi District, a poor, rural district in Malawi's Central Region. The experiment was conducted over a period of 20 days, 4 rounds (each in a different location) per day. The requirement for each round was a minimum of 40 households in one location with random selection of 16 participant households out of those. 4 participants out of 4 households were part of *this* study, while the remaining 12 participants were part of treatments which form the basis for analysis in Mueller (2011b). Careful precautions were taken to avoid contamination of the experiment due to subjects learning about the game from interaction with prior participants.[7]

[7]For details please refer to section 4 in Mueller (2011b).

Recruitment in each location took place as follows: Upon arrival, assistance was sought from the group village headmen[8] or chiefs in drawing up a map of the houses in that particular location. The study team numbered the houses based on the order in which they were drawn. Eligibility of households was determined by drawing 16 numbers out of an envelope with as many numbered paper slips as there were houses.[9]

Recruitment scripts were used to approach eligible households. A household listing including all adult members of the household who had been present in the household the previous night was compiled.[10] Numbers were assigned based on the order in which household members were mentioned. Drawing a numbered piece of paper out of an envelope that contained as many numbered paper slips as candidates for participation in that particular household, determined the person eligible for participation.[11]

Consenting of participants and a short baseline survey were conducted at a local chief's residence. Each subject was matched with another subject in the same location. Hence, participants were aware of average player characteristics for their location, even though the precise matching with a partner was anonymous both during and after the experiment.[12] Treatment-specific game instructions were delivered one on one in a place which guaranteed privacy.[13]

All treatments were played in each location. Rotation of enumerators guaranteed that

[8] A group village headman is the direct superior of several chiefs whose villages form a cluster.

[9] The remaining households served as backups. They were visited in the order they would be drawn if it became necessary.

[10] Adults are individuals 18 years and above according to Malawi law.

[11] If no adult member of the household was available for household listing purposes, or if the eligible household member was absent, refused or was unable to participate, a back-up household was approached.

[12] This guaranteed the same degree of anonymity for different rounds, independent of participants' houses' locations relative to each other from one round to the next.

[13] Please refer to section 4 of Mueller (2011b) for a detailed discussion of the advantages of one on one instruction in this particular context.

each enumerator instructed four different treatments per day and collected sharing data for each treatment the same number of times over the course of the entire study.

Instructions were delivered orally. The monetary consequences of the player sorting 0, 1, or 2 bags of beans were demonstrated to her using bottle caps and bags of beans. Similarly, it was demonstrated to her how the monetary consequences of several sorting decisions of the matched player would look like, given an assumed rate of return for that player. Participants were prompted to ask questions at pre-defined points. To homogenize replies across different enumerators, either parts of the script were re-read or standardized answers were given. After instruction, subjects sorted a standardized sample amount of beans, to understand the difficulty and duration of the task before making their irreversible sorting decisions. Enumerators *and* participants learned only after sorting whether the participant was assigned the role of dictator, so as not to influence the performance of enumerators or alter the symmetric structure of the production stage. Receivers returned to the chief's residence for completion of the opinion survey. Dictators first made sharing decisions using the strategy method.[14]

To this end, enumerators (a) reviewed the monetary consequences of the decision makers' actions given her rate of return, and (b) demonstrated the consequences of each possible effort/rate of return combination of the other player *given* the sorting decision of (and associated outcome for) the dictator. For simplicity, bottle caps and bags of beans were used to display both, (a) and (b). Immediately following the display of each effort/rate of return combination of the other player, the dictator had to make her respective sharing decision.[15]

[14]Please refer to section 4 Mueller (2011b) for a discussion of why only one subject in each group was asked for her strategy as decision maker.

[15]The details of each sharing decision to be made were as follows: The first question aimed at the dictator established whether she would like more, the same, or less of the common pot for herself as compared to the player she was matched with for the specific situation that was displayed. She was then asked for the exact amount she would like to keep. In order to overcome educational limitations

After the elicitation of dictators' sharing preferences, dictators had to return to the chief's residence as well to also complete the opinion survey. Following the matching of answer-sheets according to a pre-specified scheme, subjects were paid in privacy by the experimenter. At the same time they learned about the payoff relevant decisions of their partner as far as their respective treatments allowed (please refer back to sub-section 3.1 for details). Subjects' participation time was approximately 1.5 hours on average, out of which less than 45 minutes were spent on game instruction.

3.5 Results

The analysis is based on the structural model of optimal choices of dictators for various distributional scenarios, which includes the trade-off between selfishness and norm-abiding behavior and incorporates the hypothesis that dictators who can 'hide' part of their contribution to the common pot are more greedy.

The first part of the empirical analysis comprises simple tests to establish whether the norms postulated in the model seem appropriate for analyzing the allocation decisions in the data set. I follow with the analysis of whether dictators' beliefs enter their utility functions directly, i.e. whether the incomplete information treatment leads to a change of allocation decisions as predicted. Last, I present a mixed logit estimation to establish the relative importance of norms and own income under the different treatments.

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of the subject population the monetary consequences of her sharing decision were demonstrated with bottle caps and the enumerators performed simple arithmetic for the participants telling the dictators how much of the social surplus would be left for the other player, if the decision maker's choice was carried out, and what the final total amounts for both players would be. Subjects were allowed to change their mind once at this stage before the enumerator moved on to displaying the monetary consequences of another potential effort/rate of return combination of the other player. For further details, please refer to section 4 of Mueller (2011b).

3.5.1 Which Experimental Variations Do Subjects Respond To?

This sub-section provides a first glance at which factors other than own income dictators take into account when making their sharing decisions. As in Mueller (2011b), I restrict the data in numerous ways to isolate effects. To exclude the possibility that subjects' sorting decisions (see Table 5) were influenced by the treatment they were assigned to, I first run a simple Chi2 test. The p-value is 0.24, indicating that the data can be pooled across different sorting decisions for this first step of the analysis.

The results are summarized in Tables $6-8$. Sample sizes are stated in terms of individuals as well as in terms of observations. In addition to the mean and median share of the common pot kept by dictators for each scenario, the number and percentage of observations falling into each of four categories is reported: purely selfish allocations, dictator shares between 100 and 50 percent of the common pot, equal splits of the social surplus, and "generous" allocations, for which the dictator share is lower than 50 percent of the common pot. Based on these data, Wilcoxon rank-sum tests and non-parametric equality of median tests allow me to assess which factors besides utility derived directly from own income are relevant for explaining the outcome data.

Table 6 shows that with the exception of an exogenously assigned low rate of return for the dictator, all variations which are arguments of at least one of the norms postulated in the model section lead to significant changes of the location of the distribution of dictator shares. Contrary to this, the differences in Table 7 are *only* significant if the possibility to 'hide' part of the effort-generated income from the receiver is given *and* the receiver has sorted a positive number of bags. To uncover these effects further, I turn to regression analysis.

3.5.2 Regression Analysis With Respect To Norm Predictions

In this subsection, I assess the average influence of the opportunity to hide effort-generated income on decision making while controlling for receiver's effort, differences in rates of return and contribution as well as numerous baseline variables, using data from the *entire* sample for a reduced form regression analysis. This step of the analysis is meant to display what type of effect information truly has without imposing theoretical specifications which might obscure these patterns in the data. Since Mueller (2011b) shows in great detail, that this particular subject population responds to all experimental and choice variations as intuitively expected and required for the model to be sensible, I will here focus on the information effect.

I estimate the regression equation

$$y_{is} = \alpha + \gamma h_i + V_{is}\eta + \epsilon_{is}, \tag{3.6}$$

where y_{is} represents the fraction of the common pot that dictator i keeps for herself in scenario s, where a scenario is a specific (hypothetical) effort/rate-of-return combination of the receiver. V_{is} is a matrix of scenario-dependent norm-relevant variables. For different columns/specifications, it is either defined as (a) DE_{is}, a vector of effort levels of the dictator, measured in number of bags sorted, (b) RE_{is}, a vector of hypothetical effort levels of the receiver, measured in number of bags sorted, (c) a vector of hypothetical differences in rates of return between dictator and receiver, (d) a vector of hypothetical differences in dictator's and receiver's hypothetical (due to the use of the strategy method) contribution to the social surplus, measured in bottle caps, or (e) a combination of the first three vectors. h_i is a variable that can take the values 0 or 1, and denotes whether a player can 'hide' part of her contribution to the common pot from her counterpart, i.e. h_i takes the value 0 for dictators who

are either in the benchmark treatment, have sorted zero bags, or have been assigned a low rate of return.

Interestingly, information has a small but significantly positive effect on the average share kept by dictators, i.e. with an extra 40 MK 'hidden', a dictator chooses to keep another percent of the common pot on average. This effect holds for nearly all specifications, with or without the inclusion of baseline variables (see Tables $9 - 12$). All other variables influence dictator shares significantly and in the direction we would intuitively expect them to.

Overall, we can conclude, that even though effects are not large enough in magnitudes to shift medians in a significant way, information nevertheless has significant effects on allocation decisions for our sample. The small magnitude of the effect should not be surprising, since strategic motives fall away in dictator games while only the social image effect is isolated. This is in line with Ockenfels and Werner (2011) who also find very small but intuitive and significant effects for their sample of German newspaper readers. Since we established the existence of an information effect successfully, I will now proceed to estimate the structural model in the following sub-section.

3.5.3 Mixed Logit

Tables 13 (mixed logit) and 1 in the appendix (conditional logit, for comparison) are included to establish the relative importance of income and norms on decision making, as compared to any potential effect the different information treatments might have. Just as in Mueller (2011b), strict egalitarianism has a much larger coefficient than the other norms. Though incomplete information has the expected effect on dictators' decision making, this effect proves insignificant if all other norms and the income motive are included in the regression. We conclude that social image concerns

are present, but not decisive for individuals' choices.

3.6 Conclusion

This paper studies whether and how sharing behavior regarding effort-generated social surplus of individuals in rural Malawi changes, when they know that their contribution to and share taken of the social surplus cannot be fully observed by other individuals, i.e. how social image concerns determine optimal shares in this setting.

The motivational background for this paper was that people supposedly care deeply about their social image, arguably more so in small, tightly knit communities as opposed to more fragmented, anonymous settings. Since the influence of culture on economic growth is a recurrent theme in the development literature, sharing norms have often been the focus of interest due to their influence on redistribution, and hence, incentives for productive activity within a community or society. Given the changes in community structures in rural areas of developing countries, it is therefore of great interest for this debate to investigate if, and to what extent, these sharing norms are dependent on social image or are fully internalized by people.

We do find significant but small effects of our information treatment on dictator shares. But while we learn that subjects take social image concerns into account when making distributional decisions, we also see that the effect is not large enough for abrupt changes in economic behavior due to changes in the informational environment caused by modernization. It seems that norms are to a large extent internalized, and hence likely to be eroded gradually and adaptively, based on the requirements of a changed social and economic environment.

3.7 Figures

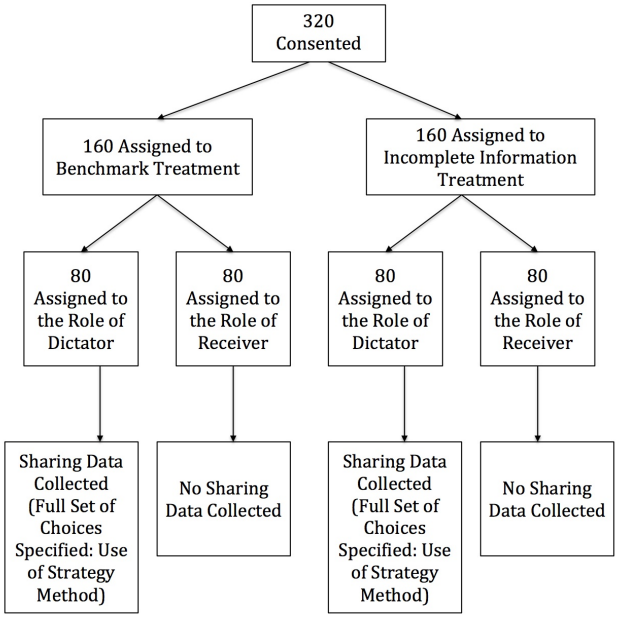

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Figure 3.1: Sample Sizes

3.8 Tables

Benchmark Treatment
Own Rate of Return
Matched Player has 50 Percent Chance of Having been Assigned High or Low Rate of Return
Equal Chance to be Assigned the Role of Either Dictator or Receiver
Own Sorting Decision, Rate of Return and − in the Case of Being Assigned Role of Dictator − Payoff Relevant Sharing Decision Will be Revealed to the Matched Player

Table 3.1: Information Known to Players Prior to Sorting Decisions

Incomplete Information Treatment
Own Rate of Return
Matched Player has 50 Percent Chance of Having been Assigned High or Low Rate of Return
Equal Chance to be Assigned the Role of Either Dictator or Receiver
Own Sorting Decision and Rate of Return Will be Revealed to the Matched Player in the Case of Being Assigned Role of Receiver
Own Sorting Decision and Share of the Common Pot Given to the Receiver Will be Revealed to the Matched Player in the Case of Being Assigned Role of Dictator

Table 3.2: Information Known to Players Prior to Sorting Decisions (continued)

Benchmark Treatment	
Dictator	Receiver
Own Rate of Return (A)	Own Rate of Return (C)
Own Number of Bags Sorted (B)	Own Number of Bags Sorted (D)
Receiver's Rate of Return	Dictator's Rate of Return
Receiver's Number of Bags Sorted	Dictator's Number of Bags Sorted
Dictator's Choices Given A and B for All Potential Rates of Return and Number of Bags Sorted of the Receiver	Dictator's Sharing Decision Given A, B, C and D

Table 3.3: Information Known to Players During Payment Phase

Incomplete Information Treatment	
Dictator	Receiver
Own Rate of Return (A)	Own Rate of Return (C)
Own Number of Bags Sorted (B)	Own Number of Bags Sorted (D)
Receiver's Rate of Return	
Receiver's Number of Bags Sorted	Dictator's Number of Bags Sorted
Dictator's Choices Given A and B for All Potential Rates of Return and Numbers of Bags Sorted of the Receiver	Amount Given to Receiver by the Dictator Given B, C and D

Table 3.4: Information Known to Players During Payment Phase (continued)

Number of Bags Sorted	Benchmark Treatment	Incomplete Information Treatment
	Number / Percent	Number / Percent
2	306/31.88	306/31.88
1	372/38.75	402/41.88
0	282/29.38	252/26.25

Table 3.5: Distribution of Number of Bags Sorted

	Data Description	A	B	C	D	E	F	G	H
1	All Observations	160	884	66.46	62.5	19.57	35.3	40.38	4.75
2	Both Dictator and Receiver Have Identical Effort Levels and Rate of Return	122	122	60.63	50.0*	8.20	32.78	57.38	1.64
3	Dictators Exerted More Effort Than Matched Receivers, Both Players Have Identical Rate of Return	122	180	79.17	$75.0^{(\dagger,\dagger)}$	38.33	43.34	18.33	0.0
4	Dictators Exerted Less Effort Than Matched Receivers, Both Players Have Identical Rate of Return	102	140	52.32	$50.0^{(\dagger,\dagger)}$	4.29	18.57	62.14	15.0
5	Both Dictator and Receiver Have Identical Effort Levels and Rate of Return	122	122	60.63	50.0*	8.20	32.78	57.38	1.64
6	Dictator Has Higher Rate of Return, Both Players Exerted Same Effort Level	54	54	66.04	$66.67^{(\dagger,\dagger)}$	7.41	64.82	24.07	3.7
7	Dictator Has Lower Rate of Return, Both Players Exerted Same Effort Level	68	68	59.56	$50.0^{(\S,\S)}$	5.88	35.3	50.0	8.82
8	Dictators Contribute More to the Common Pot	122	385	78.83	$75.0^{(\dagger,\dagger)}$	37.14	44.16	18.18	0.52
9	Dictator and Receiver Contribute the Same to the Common Pot	122	176	62.13	50.0*	8.52	40.91	49.43	1.14
10	Dictators Contribute Less to the Common Pot	131	323	54.08	$50.0^{(\dagger,\dagger)}$	4.64	21.68	61.92	11.76

Table 3.6: Dictator Shares (Strategy Method)

	Data Description	A	B	C	D	E	F	G	H
11	Benchmark Treatment	80	438	66.03	57.29*	22.15	29.0	42.69	6.16
12	Incomplete Information Treatment	80	446	66.89	62.5$^{(\S,\S)}$	17.04	41.48	38.12	3.36
13	Benchmark Treatment Excluding 0 Hypo. Sort.	80	320	58.84	50.0*	7.81	31.87	51.88	8.44
14	Incomplete Information Treatment Excluding 0 Hypo. Sort.	80	320	61.00	50.0$^{(\ddagger,\ddagger)}$	5.63	43.74	45.94	4.69
15	Benchmark Treatment, Dictator Has Higher Rate of Return	36	198	67.57	62.5*	23.74	31.81	36.87	7.58
16	Incomplete Information Treatment, Dictator Has Higher Rate of Return	35	194	67.58	66.67$^{(\S,\S)}$	12.37	55.77	27.74	4.12
17	Benchmark Treatment, Dictator Has Higher Rate of Return, Excluding 0 Hypo. Sort.	36	144	59.70	50.0*	6.94	36.11	46.53	10.42
18	Incomplete Information Treatment, Dictator Has Higher Rate of Return, Excluding 0 Hypo. Sort.	35	140	63.20	62.5$^{(\ddagger,\dagger)}$	5.71	59.29	29.29	5.71
19	Benchmark Treatment, Dictator Has Lower Rate of Return	44	240	64.77	50.0*	20.83	26.67	47.5	5.00
20	Incomplete Information Treatment, Dictator Has Lower Rate of Return	45	252	66.35	50.0$^{(\S,\S)}$	20.63	28.18	48.41	2.78
21	Benchmark Treatment, Dictator Has Lower Rate of Return, Excluding 0 Hypo. Sort.	44	176	58.14	50.0*	8.52	28.41	56.25	6.82
22	Incomplete Information Treatment, Dictator Has Lower Rate of Return Excluding 0 Hypo. Sort.	45	180	59.21	50.0$^{(\S,\S)}$	5.56	31.66	58.89	3.89

Table 3.7: Dictator Shares (Strategy Method). Continued from previous page

A	Number of Individuals
B	Number of Observations
C	Mean Share of Commonpot Kept by Dictators
D	Median Share of Commonpot Kept by Dictators
E	Share of Commonpot Kept by Dictator is 100 (in Percent)
F	Share of Commonpot Kept by Dictator is Between 50 and 100 (in Percent)
G	Share of Commonpot Kept by Dictator is 50 (in Percent)
H	Share of Commonpot Kept by Dictator is Less Than 50 (in Percent)
*	Benchmark Group
§	Not Significantly Different from Benchmark Group
‡	Significantly Different from Benchmark Group at 5 Percent
†	Significantly Different from Benchmark Group at 1 Percent
First Superscript	Wilcoxon Rank-Sum Test Results
Second Superscript	Nonparametric Equality-of-Median Test Results

Table 3.8: Dictator Shares (Strategy Method) (continued)

	(1)	(2)	(3)	(4)	(5)	(6)
Hidden	0.046**	0.015	0.037**	0.066***	-0.056***	0.006
	(0.019)	(0.018)	(0.017)	(0.024)	(0.018)	(0.021)
Dictator's Effort		0.121***				0.103***
		(0.013)				(0.012)
Receiver's Effort			-0.110***			-0.097***
			(0.010)			(0.010)
Difference in Rates of Return				-0.011		0.003
				(0.008)		(0.007)
Differences in Contributions (Measured in Bottle Caps)					0.015***	
					(0.001)	
Constant	0.651***	0.519***	0.774***	0.642***	0.685***	0.648***
	(0.012)	(0.017)	(0.016)	(0.014)	(0.010)	(0.021)
No. of Obs.	446	446	446	446	446	446
R^2	0.01	0.18	0.21	0.02	0.28	0.33
Demographic and Socio-Economic Controls	NO	NO	NO	NO	NO	NO

Table 3.9: Regression of Dictator Shares. * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

	(1)	(2)	(3)	(4)	(5)	(6)
Hidden	0.055***	0.012	0.046***	0.067***	-0.047**	0.008
	(0.020)	(0.019)	(0.018)	(0.025)	(0.019)	(0.022)
Dictator's Effort		0.120***				0.100***
		(0.015)				(0.014)
Receiver's Effort			-0.109***			-0.098***
			(0.010)			(0.010)
Difference in Rates of Return				-0.007		0.002
				(0.009)		(0.007)
Differences in Contributions (Measured in Bottle Caps)					0.014***	
					(0.001)	
Constant	0.466***	0.495***	0.605***	0.462***	0.620***	0.616***
	(0.061)	(0.057)	(0.056)	(0.062)	(0.054)	(0.053)
No. of Obs.	408	408	408	408	408	408
R^2	0.07	0.2	0.27	0.07	0.32	0.36
Demographic and Socio-Economic Controls	YES	YES	YES	YES	YES	YES

Table 3.10: Regression of Dictator Shares. * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

	(1)	(2)	(3)	(4)	(5)	(6)
Hidden	0.070***	0.042**	0.070***	0.081***	0.014	0.034
	(0.019)	(0.018)	(0.018)	(0.023)	(0.020)	(0.022)
Dictator's Effort		0.086***				0.087***
		(0.012)				(0.012)
Receiver's Effort			-0.039**			-0.039**
			(0.018)			(0.016)
Difference in Rates of Return				-0.006		0.004
				(0.008)		(0.007)
Differences in Contributions (Measured in Bottle Caps)					0.009***	
					(0.001)	
Constant	0.584***	0.500***	0.643***	0.580***	0.628***	0.561***
	(0.011)	(0.016)	(0.029)	(0.013)	(0.013)	(0.029)
No. of Obs.	320	320	320	320	320	320
R^2	0.04	0.18	0.06	0.05	0.14	0.19
Demographic and Socio-Economic Controls	NO	NO	NO	NO	NO	NO

Table 3.11: Regression of Dictator Shares (Excluding 0 Hypo. Sort.). * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

	(1)	(2)	(3)	(4)	(5)	(6)
Hidden	0.074***	0.044**	0.074***	0.080***	0.027	0.040*
	(0.018)	(0.018)	(0.018)	(0.023)	(0.020)	(0.023)
Dictator's Effort		0.077***				0.077***
		(0.013)				(0.013)
Receiver's Effort			-0.032*			-0.032**
			(0.017)			(0.016)
Difference in Rates of Return				-0.004		0.002
				(0.008)		(0.008)
Differences in Contributions (Measured in Bottle Caps)					0.007***	
					(0.002)	
Constant	0.482***	0.505***	0.530***	0.480***	0.568***	0.555***
	(0.055)	(0.052)	(0.060)	(0.055)	(0.056)	(0.057)
No. of Obs.	292	292	292	292	292	292
R^2	0.12	0.21	0.13	0.12	0.18	0.23
Demographic and Socio-Economic Controls	YES	YES	YES	YES	YES	YES

Table 3.12: Regression of Dictator Shares (Excluding 0 Hypo. Sort.). * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

	SE	LE	L	IA	SE, LE, L, IA
Mean					
y	0.01***	0.04***	0.03***	0.00	0.04***
	(0.00)	(0.01)	(0.00)	(0.00)	(0.01)
Case Hidden	0.06***	0.00	-0.01*	-0.00	0.05***
	(0.01)	(0.00)	(0.00)	(0.00)	(0.02)
SE Loss Term	1.01***				1.24***
	(0.15)				(0.21)
LE Loss Term		0.23***			-0.01
		(0.03)			(0.06)
L Loss Term			0.15***		0.11**
			(0.02)		(0.05)
IA Loss Term				0.04***	0.03
				(0.01)	(0.02)
SD					
SE Loss Term	0.88***				1.03***
	(0.15)				(0.17)
LE Loss Term		0.13***			0.02
		(0.02)			(0.03)
L Loss Term			0.07***		0.14***
			(0.02)		(0.04)
IA Loss Term				0.00	0.06**
				(0.00)	(0.03)
Log Likelihood	-684.05	-815.15	-839.25	-888.08	-661.87
Number of Observations	6264	6264	6264	6264	6264
$LRChi^2$	238.10***	44.08***	20.46***	0.00	252.92***

Table 3.13: Mixed Logit Estimation. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

APPENDICES

APPENDIX A

Appendices for "Norms of Distributive Justice in Rural Malawi"

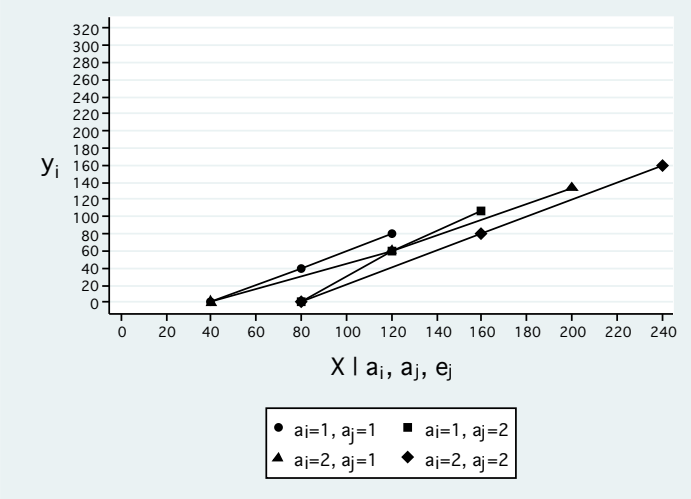

Figure A.1: Liberal Egalitarianism; $e_j = 1$, all a_i and a_j

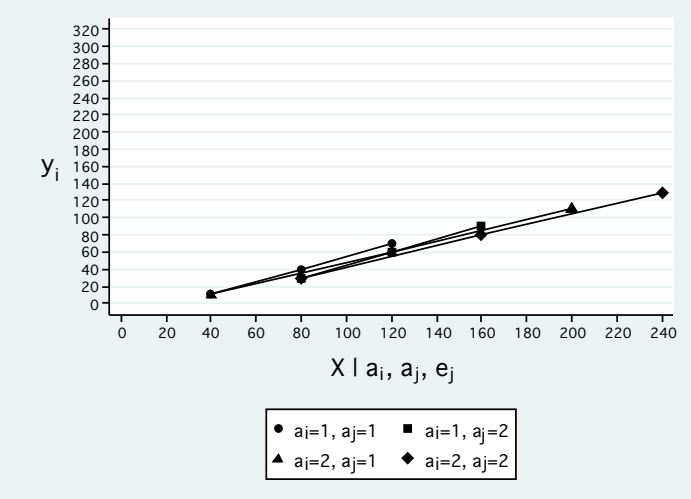

Figure A.2: Inequality Aversion; $e_j = 1$, all a_i and a_j

	(1)	(2)	(3)	(4)	(5)
Dictator's Effort (Measured in Bags Sorted)	0.088***				0.067***
	(0.006)				(0.005)
Receiver's Effort (Measured in Bags Sorted)		-0.119***			-0.109***
		(0.005)			(0.005)
Difference in Rates of Return			0.008***		0.007***
			(0.003)		(0.003)
Differences in Contributions (Measured in Bottle Caps)				0.013***	
				(0.000)	
Constant	0.554***	0.769***	0.652***	0.644***	0.686***
	(0.008)	(0.006)	(0.004)	(0.004)	(0.009)
No. of Obs.	2751	2751	2751	2751	2751
R^2	0.08	0.18	0.00	0.19	0.23
Demographic and Socio-Economic Controls	NO	NO	NO	NO	NO

Table A.1: Regressions of Dictator Shares on Model Variables. * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

	(6)	(7)	(8)	(9)	(10)
Dictator's Effort (Measured in Bags Sorted)	0.097*** (0.006)				0.070*** (0.006)
Receiver's Effort (Measured in Bags Sorted)		-0.124*** (0.005)			-0.112*** (0.005)
Difference in Rates of Return			0.008*** (0.003)		0.008*** (0.003)
Differences in Contributions (Measured in Bottle Caps)				0.013*** (0.001)	
Effort Cap (Receiver)	-0.029* (0.016)	-0.048*** (0.015)	0.018 (0.016)	-0.011 (0.015)	-0.033** (0.015)
Effort Cap (Dictator)	0.029** (0.014)	-0.017 (0.013)	-0.023 (0.015)	0.026* (0.013)	0.021 (0.013)
Effort Cap (Both Players)	0.051*** (0.016)	-0.054*** (0.015)	0.01 (0.016)	0.004 (0.015)	-0.017 (0.015)
Income Shock (Receiver)	-0.052*** (0.014)	-0.060*** (0.013)	-0.059*** (0.014)	-0.050*** (0.013)	-0.055*** (0.013)
Income Shock (Dictator)	0.021 (0.014)	0.014 (0.013)	0.014 (0.014)	0.02 (0.013)	0.019 (0.013)
Income Shock (Both Players)	-0.011 (0.014)	-0.015 (0.013)	-0.01 (0.014)	-0.016 (0.013)	-0.016 (0.013)
Constant	0.539*** (0.012)	0.797*** (0.011)	0.661*** (0.010)	0.648*** (0.009)	0.695*** (0.013)
No. of Obs.	2751	2751	2751	2751	2751
R^2	0.1	0.2	0.02	0.2	0.24
Demographic and Socio-Economic Controls	NO	NO	NO	NO	NO

Table A.2: Regressions of Dictator Shares on Model Variables (continued from previous page). * Significant at 10 Percent; ** Significant at 5 Percent; *** Significant at 1 Percent

A.0.1 Individual Level Regressions

	SE	L	LE	IA	S
Female	-0.009	-0.007	-0.038	0.057	0.050
	(0.072)	(0.061)	(0.056)	(0.059)	(0.047)
Age	-0.020**	0.010	-0.001	-0.002	0.010*
	(0.009)	(0.008)	(0.007)	(0.007)	(0.006)
Age Squared	0.000**	-0.000	0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years of Education	-0.001	0.002	-0.001	-0.008	-0.005
Completed	(0.008)	(0.007)	(0.007)	(0.007)	(0.005)
Head of Household	-0.080	0.074	0.030	-0.021	-0.028
	(0.072)	(0.061)	(0.056)	(0.059)	(0.047)
No. of Children Who Grew	-0.007	0.004	0.017**	0.003	-0.010
Up in Household Together	(0.011)	(0.009)	(0.008)	(0.009)	(0.007)
No. of Children	0.011	-0.011	-0.007	0.016	-0.012
in Current Household	(0.016)	(0.013)	(0.012)	(0.013)	(0.010)
Household Asset Index	-0.009	0.005	0.009	0.006	0.008
	(0.014)	(0.012)	(0.011)	(0.012)	(0.009)
Household Animal	0.001	-0.000	-0.013	0.031	0.008
Ownership Index	(0.023)	(0.020)	(0.018)	(0.019)	(0.015)
Mean of Dep. Var.	0.44	0.23	0.17	0.21	0.13
N	384	384	384	384	384
R^2	0.03	0.02	0.02	0.03	0.03

Table A.3: Regression of Weak Match Unconditional Dummies on Baseline Variables. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

	SE	L	LE	IA	S	O
Female	0.029	-0.007	-0.022	-0.001	0.020	-0.019
	(0.049)	(0.044)	(0.015)	(0.015)	(0.020)	(0.065)
Age	-0.008	0.006	0.001	0.002	-0.000	-0.000
	(0.006)	(0.005)	(0.002)	(0.002)	(0.002)	(0.008)
Age Squared	0.000	-0.000	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Years of Education	-0.001	-0.000	-0.001	-0.001	0.001	0.001
Completed	(0.006)	(0.005)	(0.002)	(0.002)	(0.002)	(0.008)
Head of Household	0.056	0.015	-0.025*	0.021	0.021	-0.089
	(0.049)	(0.044)	(0.015)	(0.015)	(0.019)	(0.065)
No. of Children Who Grew	0.001	-0.004	-0.001	0.001	0.003	-0.000
Up in Household Together	(0.007)	(0.007)	(0.002)	(0.002)	(0.003)	(0.010)
No. of Children	0.003	-0.002	-0.003	-0.001	-0.003	0.005
in Current Household	(0.011)	(0.009)	(0.003)	(0.003)	(0.004)	(0.014)
Household Asset Index	-0.006	0.008	-0.002	-0.001	0.001	-0.000
	(0.010)	(0.009)	(0.003)	(0.003)	(0.004)	(0.013)
Household Animal	0.003	-0.017	0.002	0.006	-0.005	0.011
Ownership Index	(0.016)	(0.014)	(0.005)	(0.005)	(0.006)	(0.021)
Mean of Dep. Var.	0.13	0.10	0.01	0.01	0.02	0.74
N	384	384	384	384	384	384
R^2	0.02	0.01	0.01	0.02	0.01	0.01

Table A.4: Regression of Strong Match Unconditional Dummies on Baseline Variables. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

APPENDIX B

Appendices for "Are Rural Road Investments Alone Sufficient to Generate Transport Flows?"

Cost Assumptions	Unit Cost	Unit
Round trip distance	34	km
Fuel efficiency (with one passenger)	0.29	liters/km
Round trips per day	2	trips
Bus Rental	6000	daily rental
Driver	1000	per day
Bus Cost	7000	daily rental
Fuel	213	Liter
Loss of fuel efficiency per passenger	0.0015	liters/km
Maximum passengers per bus	14	passengers

Table B.1: Annex 1: Cost assumptions

APPENDIX C

Appendices for "In the Public Eye: Distributional Choices in Rural Malawi Under Perfect and Imperfect Information"

	SE	LE	L	IA	SE, LE, L, IA
y	0.01***	0.02***	0.02***	0.00	0.02***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Case Hidden	0.01***	0.00	-0.00	0.00***	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
SE Loss Term	0.18***				0.13***
	(0.00)				(0.02)
LE Loss Term		0.11***			0.02
		(0.01)			(0.02)
L Loss Term			0.08***		0.04*
			(0.01)		(0.02)
IA Loss Term				0.04***	0.01
				(0.01)	(0.01)
Log Likelihood	-803.09	-837.19	-849.48	-888.08	-788.33
Number of Observations	6264	6264	6264	6264	6264
Number of Cases	320	320	320	320	320
$WaldChi^2$	124.65***	91.13***	84.14***	59.22***	129.82***

Table C.1: Conditional Logit Estimation. * Significant at 10 Percent, ** Significant at 5 Percent, *** Significant at 1 Percent

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