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by

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Abstract

Essays in Development Economics

by

Maria Pia Basurto

This dissertation consists of three self-contained chapters on development economics. The dissertation is focused primarily on one of the largest input subsidy program in the world in terms of how beneficiaries are whether households hide income. As a separate project, I also look at the impact of star students on their siblings test scores in the context of a Peruvian high-achievers national boarding school.

In the first chapter, entitled Measuring Sensitive Questions: Income Hiding and Subsidy Allocation , I document the extent to which villagers hide income from local leaders and other villagers as a strategic behavior in the context of a large scale agricultural subsidy in Malawi (FISP). My main contribution is methodological. I use three different measures of income hiding to asses the extent of this practice: direct questions, list randomization, and, willingness to pay to hide income. I find that income hiding prevalence is between 17 to 27 percent depending on the measure employed. Also, I find that villagers hide income from different people and the three most common categories are village headmen (16%), neighbors (16%), and, friends (15%).

The second chapter, entitled Decentralization and Efficiency of Subsidy Targeting: Evidence from Chiefs in Rural Malawi, is joint work with Pascaline Dupas (Stanford) and, Jonathan Robinson (UCSC). We study the trade off between centralized and decentralized subsidy targeting in the context of two large-scale subsidy programs in Malawi (for agricultural inputs and food). Decentralized targeting is carried by traditional leaders (chiefs) who are asked to target the needy. Using high-frequency household panel data on neediness and shocks, we find that nepotism exists but has only limited mistargeting consequences. Importantly, we find that chiefs target households with higher returns to farm inputs, generating an allocation that is more productively efficient than what could be achieved through a a proxy means test used for centralized targeting. This could be welfare improving, since within-village redistribution is common in the study setting.

The third chapter, entitled On the Peer Effects of Star Students, is joint work with Manuel Barron (assistant professor at Universidad del Pacifico), and, Gabriela Cuadra (Ph.D. Student, UCSC). We estimate the effect that star students have on their siblings' learning outcomes, measured by their high school grade point average (GPA) and their math grades. To this end, we couple administrative school data on grades with an unusual natural experiment in Peru that generates exogenous variation in the presence of star students at home. We find that star students increase their siblings' GPA by 0.33 standard deviations and their math grades by 0.22 standard deviations. The effect size is inversely related to number of siblings, suggesting that the remaining siblings act as substitutes for the star student.

To my parents, husband and son

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Chapter 1

Measuring Sensitive Questions: Income Hiding and Subsidy Allocation

1 Introduction

Research has found that people may have an incentive to hide income in developing countries to avoid a "kinship tax" i.e (Jakiela and Ozier, 2015), (Squires, 2017), and, (Boltz et al., 2016). Income hiding strategies have been documented to lead to significant economic costs like taking up expensive loans (Baland, Guirkinger, and Mali, 2011), buying illiquid assets (Di Falco and Bulte, 2011), increasing household present consumption (Goldberg, 2010), forgoing profitable investments and paying out of pocket in order to maintain their profits unknown to their relatives (Jakiela and Ozier, 2015), leading entrepreneurs to invest less in their business than they would have otherwise (Squires, 2017), and, forgoing laboratory gains to keep winning privates and reduce the share of gains transfered to kinship (Boltz et al., 2016) [1]. Thus far, the literature has mainly focused on income hiding to avoid kinship taxes. People may also hide income from local leaders to appear poor to qualify for government subsidies. This may cause resource misallocation due to targeting errors in anti-poverty programs.

Some research finds evidence for this. For instance, Camacho and Conover (2011) show that, in response to the expansion of a social protection program, informal employment crowded-out formal employment. The informal sector is preferred over the formal sector because, despite informal earnings being lower since they are difficult to monitor by the government[2]. Thus, people may also have an incentive to hide income from local government officials or engage in

[1]Income hiding has also been studied in the village setting as an explanation for incomplete informal insurance (Kinnan et al., 2010; Karaivanov and Townsend, 2014)

[2]This also generates government losses in forgone income taxes.

other forms of strategic behavior in order to receive subsidies.

In this paper, I use novel data from Malawi to examine and document the extent of income hiding from other villagers and also from local leaders (village headmen or chiefs). In this context, I use a variety of direct and indirect survey measurement techniques to construct descriptive statistics and compare responses under various strategies. Data was collected during 2015 from a sample of 300 Malawian farming households and 60 village heads.

This study focuses on one of the largest farming input subsidy programs in the world, Malawi's Farming Input Subsidy Program (FISP). The sample consists on Malawian farmers who are eligible to be selected beneficiaries of FISP 2015/2016. FISP beneficiary selection is done by village headmen based on local information available to them about households' economic status. Income hiding may thus generate inclusion errors in FISP allocation. Given the fixed amount of FISP available to each village, this would imply exclusion errors as well, as households who truly deserve the subsidy are left out of the beneficiary list to make room for households that do not deserve the subsidy but, to the village head's eyes, look like they do.[3]

In this paper, I document the extent of income hiding from various people and describe whether people hide income and from whom. Future research can assess the subsidy targeting consequences of income hiding in this setting. Since income hiding is a sensitive questions it is necessary to use different measurement

[3]Another source of beneficiary missallocation extensively studied is the trade off between nepotism and increased local information in decentralized subsidy targeting (Basurto, Dupas, and Robinson, 2017; Alatas et al., 2012; Bardhan and Mookherjee, 2006; Dorward et al., 2008; Kilic, Whitney, and Winters, 2013)

techniques. Thus, to document the extent of income hiding from chiefs and other villagers, I use three alternative measures: direct questions, list randomization and willingness to pay to hide income (hypothetical winnings).

Direct questioning, as its name states, consists of asking respondents directly whether they hide some income. In particular, respondents were asked if they had ever hidden income from: spouse, other relatives, friends, neighbors, village head, and, other village members. These answers gives us a lower bound on true income hiding. List randomization consists on randomly selecting half of the sample to answer to a short list of non-sensitive statements, while the other half is randomly assigned to answer a larger list which includes the short list plus one additional statement, the sensitive question. Respondents are asked for the number of true statements in the list. Since the two lists differ in only one statement, the sensitive question, the researcher can asses the prevalence of the sensitive behavior by comparing the average of the two groups (Karlan and Zinman, 2012). Willingness to pay a fee to hide income consists on asking respondent if they are willing to pay and how much in order to keep winnings private rather than announced in public. Respondents were faced with four different sizes of hypothetical winnings. This technique has been used by Squires (2017) to measure the marginal kinship tax rates and by Boltz et al. (2016) to measure income hiding from kinship.

Depending on the method, between 16% to 27% of households in the sample hide income either from relatives, neighbors, the village head, etc. Direct questions about hiding perform well in my study setting. First, households seem to be upfront in reporting that they hide income. Second, unlike willingness-to-pay to hide, direct questions allow to identify from whom is the household hiding income (relatives, friends, neighbors, village head, or a combination of them). Randomization list did not work well in this setting, a matter I discuss in depth in section 4.3, relating it to recent literature on this method.

This study has 3 main caveats. First, the sample size is small. Second, willingness to pay to hide income was measured using hypothetical winnings instead of real potential winnings that would motivate respondents to answer more truthfully according to their real preferences. Third, the study uses only one method for indirect elicitation. In addition, an open question for future research is the subsidy targeting consequences of income hiding.

The remainder of the paper is organized as follows. Section 2 institutional background on Malawi and the Farming Input Subsidy Program (FISP). Section 3 explains the data collection exercise and provides summary statistics. Section 4 explains the methods used to measure sensitive questions. Section 5 shows income hiding prevalence through the different methods, and, section 6 includes a discussion and concludes.

2 Institutional Background

2.1 Local governance in Malawi

Malawi is a presidential democracy divided into 28 districts, each administered by a District Assembly which coexist with a traditional chieftaincy hierarchy with four ranks: Paramount Chief, Traditional Authority (TA), Group Village Headman (GVH), and Village Headman, also known as village chief.[4] Therefore, District Assemblies consist of a combination of democratically elected councilors and members of parliament (elected for 5-year terms), together with ex-officio, non-voting members including higher-ranked chiefs (TAs). District Assemblies are led by a chairperson elected among their members (Local Government Act 1998, Section 5). In general, District Assemblies do not have much authority. They rely primarily on resources from the central government. However, transfers from the central government have been limited and councilors have de facto very few resources available.[5] Due in part to these problems, the functioning of local assemblies has been problematic. Most notably, local assembly elections were not held between 2000 and 2014 (such that local councils were not seated from 2005-2014).

The 1967 Chiefs Act establishes that chieftaincies are hereditary and hierarchical, however, the Chiefs Act also gives the OPC power to approve (or decline) new chiefs and to create new chieftaincies. In 2009, Malawi had more than eighteen thousand villages and VHs, nearly 2400 GVHs, 61 Sub-TAs, 171 TAs, and 28 senior chiefs (Ministry of Local Government 9 March 2009). According to the chiefs' acts their role is: to preserve the public peace; to carry out the traditional functions regarding customary law when is not contrary to the Constitution or any written law; to assist in the collection of tax; to assist in the general administration of the District; to carry out functions as the District Commissioner may require; and to carry out and enforce any lawful directions of the District

[4]This section draws on Basurto, Dupas, and Robinson (2017).

[5]The Decentralization Policy and Local Government Act of 1998 allowed the government to transfer 5% of the national revenue to District Assemblies, but in practice the sums transferred are smaller and not allocated equitably across districts (Patel et al., 2007).

Commissioner. Besides these functions, chiefs have judicial functions regarding minor disputes to keep order in the village and the promotion of development and well-being within their communities (Cammack, Kanyongolo, and O'Neil, 2009). Chiefs receive are remunerated for their work: a Paramount receives K50,000, while a Senior TA K30,000. A sub-TA receives K18,000, a GVH K5000 and a VH, K2500 (Cammack, Kanyongolo, and O'Neil, 2009).

Modern chiefs in Malawi hold little formal power: officially they serve only as non-voting advisory members of Assemblies, which themselves hold little authority. They do not have direct control over any public funds and are not allowed to raise local taxes. However, chiefs hold other customary responsibilities. The 1998 Decentralization Policy and Local Government Act recognized the rights of chiefs to allocate communal land and adjudicate matters related to customary law (in particular customary land). Chiefs also play an advisory and coordination role regarding local development projects.[6] Finally chiefs are typically relied on to identify beneficiaries for targeted government programs, one of the programs –and the focus of this paper– is the input subsidy program, which I describe in more detail in the following subsection.

2.2 The Agricultural Subsidy Program

Malawi's Farming Input Subsidy Program (FISP) is a large scale fertilizer and seed subsidy program, one of the largest agricultural input subsidy programs in

[6]Local development funds are in principle spent through groups known as Area Development Committees (headed by TAs) and Village Development Committees (chaired by Group Village Headmen and composed of ward councilors, MPs, religious leaders, business leaders and youth and women representatives)

the world. According to Wiggins and Brooks (2010), Malawi, Sri Lanka and India spend between 10% to 20% of government budget on agricultural input subsidy programs. The FISP program in Malawi started in 1998 and greatly expanded in response to a severe drought that took place in 2004. Since then, the program has maintained the principle of offering generous subsidies on fertilizer and seeds to a large share of the Malawian farming population. In 2015, the program was expected to reach 1.5 million farmers in Malawi.[7]

The subsidy package includes several farming inputs and comes in the form of (indivisible) vouchers, which are redeemable at local agricultural shops. Basurto, Dupas, and Robinson (2017) found that the four most popular items subsidized in their study period were 50 kilograms of planting fertilizer (NPK) worth about $40 at market prices in 2013; 50 kilograms of top-dressing fertilizer (urea) comparable in price to planting fertilizer; 5 kilograms of hybrid maize seeds worth about $7; and, 2-3 kilograms of hybrid groundnut seeds worth about $1.30 per kilogram.[8]

Beneficiary selection and voucher distribution is timed to precede the main planting season, which begins in November and lasts until March. Beneficiary lists are typically drawn in August, while the subsidy vouchers themselves are distributed in September and October, in advance of planting. There are three main steps in beneficiary selection (Chirwa, Matita, and Dorward, 2011). First, the government conducts a yearly national farmer registration census. Only those households registered as farmers are potentially eligible to receive a voucher. Next,

[7]http://www.times.mw/goodall-justifies-k19bn-fisp-allocation-cut/ (Access: July 28th, 2015)

[8]The redemption fee for FISP 2015 increased dramatically, by 600%, but nonetheless the subsidy remained generous covering 85% of it's market value. At the time of data collection.

the central government allocates vouchers to districts in proportion to their farming population and the acreage under cultivation. Within each district, the District Agriculture Development Office (DADO) allocates vouchers across villages based on farming population shares (Chirwa and Dorward, 2013). Finally, the number of vouchers available to each village is known, and a list of eligible villagers is made. Even though, formally, beneficiary selection is supposed to be implemented by the Village Development Committee through open community meetings, and audited by the DADO, Basurto, Dupas, and Robinson (2017) show that in a similar setting most authority appears to be *de facto* delegated to chiefs. This finding is consistent with Dorward et al. (2013) who show that around 70% of households in 2013 believed the decision on voucher recipients was made by the chiefs *before* the official meeting was held. The subsidy is explicitly targeted towards the poorest smallholders, although targeting guidelines leave plenty of leeway to the TA. The official FISP guidelines state that the subsidy is targeted toward "full-time, resource-poor, small holder Malawian farmers", but in addition, the program is meant to benefit particularly vulnerable groups, like the elderly, households with HIV positive members, households headed by a female, a child or an orphan, households with a physically challenged head, and households with physically challenged members (MoAFS 2009). As can be seen, guidelines are broad and in practice leave discretionary power to village headmen to choose beneficiaries.

3 Data and Summary Statistics

Data was collected in 60 villages in 3 Traditional Authorities (TA): Nsamala, Kalembo, and, Amidu; in the district of Balaka in Southern Malawi. Data was collected in August 2015.

3.1 Sampling Framework

3.1.1 The 2014 Farmer Annual Registry 2014

The sampling framework for the present study is the “farmer’s registry”, a census conducted yearly across the country.[9] This registry contains information on the number of households in each village that conduct farming activities and the gender of the household representative.

Balaka district has 7 TA, from which I selected 3 TA to conduct fieldwork. Two TAs were selected because they were the two largest in terms of population and the third TA was chosen due to logistical reasons: it was close to the first two TAs. The selected Traditional Authorities were: TA Amidu, TA Kalembo, and TA Nsamala. Each of them represent 10%, 18% and 34% of the farming households in Balaka, respectively, adding up to a total of 63% of farming households in Balaka (see Appendix Table 1.1). Sixty villages were chosen based on proximity to Liwonde town, were the field team operations was based.

The final sample consisted of 26 villages in TA Amidu, 2 villages in TA Kalembo and 32 villages in TA Nsmala, for a total of 60 villages. Next, I randomly

[9] I want to thank the District Agricultural and Development Office (DADO) of Balaka for kindly sharing the 2014 farmers registry.

selected 5 households per village from the 2014 farmers registry, with a total of 300 households.

3.2 Village Head and Household Surveys

3.2.1 Village Head survey

All village heads from the 60 selected villages in the sample were interviewed. The survey contained demographic characteristics, questions on tenure as chief, the number of FISP packages received by the village in the 2010-2014 period, and on how household characteristics were used for selecting FISP beneficiaries.

3.2.2 Household Survey

Household surveys were conducted prior to beneficiary selection for FISP 2015-2016. The survey collected information on household characteristics, expenditures, assets, history of FISP reception, savings, economic shocks and questions about income hiding. Income hiding questions are detailed and discussed in section 5.1.

3.3 Household Characteristics

Table 1.1 reports characteristics of the households in the sample. Panel A reports characteristics of the main respondent. Twenty-five percent of respondents were male and were on average 44.2 years old. Twenty-four percent had no formal education, around half had incomplete primary education, and 12 percent had complete primary. In turn, 9 percent had incomplete secondary, 2 percent had complete high school and only 1 percent had any post secondary education. About 18% report holding a public position at the village.[10]

Table 1.1 Panel B reports household summary statistics. In terms of demographic characteristics, the average household has 5.4 members; 2.8 under the age of 15 and 2.6 older than 15 years. Households have almost four decades of residence in the village, in practical terms, their entire life.

Eleven percent of households have a physically disabled member. Ten percent of households are polygamous. Given that the sampling framework is the annual farmer registry, all households in the sample are agricultural households and thus eligible for FISP. Besides farming, 22 percent of households have at least one member who sells at the market, seven percent have at least one member working in the town center, and in 45 percent of households at least one member owns a business. The average acreage of their land is 2.42, of which an average of 2.26 acres were used for farming in the 2014-2015 agricultural season. Regarding future plans, households estimated to farm on average 2.28 acres during the 2015-2016 agricultural season. Combined farm and non-farm earnings during 2015 were around USD 250, and if households had sold the total amount of their 2014-2015 harvest they would have received slightly above USD 80. About 56% of households have earnings from non-farm labor, and, conditional on participating in non-farm labor, average non-farm earnings were around USD 300. Average annual pecuniary expenditure was roughly USD 300. The value of livestock and

[10] These include Village Development Committee (VDC) member, chief councilor, chief clerk and member of volunteer groups (i.e community police, mother groups, school committee, village nursery, etc.).

household assets, besides land and the dwelling itself, was around USD 400 on average.

Thirty-seven percent of households reported being related to the chief, while a slightly higher figure reported being friends with the chief. On the other hand, 21 percent of households are related to the VDC, and 29 percent considered being friends with the VDC.

3.4 Characteristics of Village Heads

Table 1.2 Panel A turns to the description of the sixty village heads. Ninety-five percent of the village heads in the sample resides in the village where they are village head. Village heads are on average 52 years old, 77 percent of them are male, and their average tenure is almost 13 years. Also, eighty-five percent of chiefs in our sample declared to believe that their household would receive FISP for 2015/2016 planting season.

3.5 FISP Allocation

Table 1.2 Panel B describes the number of FISP subsidy packages received by each village in the sample between 2010 and 2014. The number of packages has not changed significantly in that time period fluctuating around 75 FISP packages per village.

4 Measurement of sensitive issues

This study aims to detect income hiding in the setting of a subsidy program. In this setting, income hiding may be considered a sensitive issue. Sensitive issues such as socially undesirable conducts, conditions or traits are generally hard to measure through self-reports since respondents may falsely deny the sensitive issues to the surveyor. In the face of this problem, the literature has found different ways to improve its measurement by indirect elicitation techniques. Two such techniques are employed in the field: list randomization and measurement of willingness to pay to receive hypothetical winnings privately. In this study, I use both direct and indirect elicitation techniques.

4.1 Indirect Elicitation

4.1.1 Randomized Response Techniques and List Randomization

A common method to measure the prevalence of sensitive issues is the randomized response technique (RRT) and its variants such as list randomization and crosswise-model (CM). The aim of all three methods is to increase the chances that the respondent will answer truthfully. However, ensuring the respondent's privacy comes at the cost of not knowing which interviewee individually exhibits the undesirable behavior.

In RRT, random noise is added to the answers which allows respondents to be confident that their answer will be completely private. For instance, the interviewee may be given a coin and asked to toss it before answering each of a number of questions, without showing the coin to the interviewer. If the result of the coin-toss is heads, the interviewee should answer with the truth, and if it is tails, the answer should be "yes" (assuming that answering yes is undesirable). Since 50% of coin tosses should be heads, the prevalence of the undesired behavior can be inferred by comparing the actual rate to 50% and multiplying times 2 (e.g., if the rate is 80%, the behavior is present in $(0.80 - 0.50) \times 2 = 60\%$ of the sample).

In list randomization, a randomly selected half of the sample gets assigned to answer to a short list of non-sensitive statements, while the other half is randomly assigned to answer the same list but with an additional statement (the sensitive question). Respondents are asked for the number of true statements in the list. Since the two lists differ in only one statement, the researcher can asses the prevalence of the sensitive behavior by comparing the average of the two groups (Karlan and Zinman, 2012).

This technique has been found to yield more accurate responses to socially undesirable behavior than direct reporting. A meta-analysis with 48 comparisons of direct and list randomization found that in 63% of the cases the socially undesirable behaviors were significantly larger with the list randomization technique (Holbrook and Krosnick, 2010). The biggest challenge to the implementation of this technique lies in the selection of the non-sensitive statements in the list. It is best to select items that pose small variance in the sample, but that pose some variation in the sample since otherwise respondents may not feel confident that their answer would indeed be anonymous. Given this, a common result in the use of list randomization is that it produces results with such high variance that are not statistically significant and this is specially the case when the behavior of interest is not high prevalence (Karlan and Zinman, 2012).

In the CM, respondents are presented with two statements, one sensitive and one not sensitive, and are asked to answer whether responses to both statements are the same or not. It is important to notice that respondents don't have to say whether the answer is "yes" or "no".

Given the small sample size it was necessary to use only one of these techniques. Based on the evidence from the field, I decided to use randomization lists, with three sets of lists. Each list had two versions that were randomly assigned to respondents. The first version contained three non-sensitive statements while the second version contained the sensitive statement in addition to the other three (see Appendix Table 1.2). For instance, the three non-sensitive statements were "all of my harvest from 2014/2015 farming season got spoiled", "I bought or sold a chicken last year", and "I ate nsima[11] at least once during last week", with the added sensitive element, present only in half the surveys, being "I hide some of my income from the village chief".

4.1.2 Willingness to pay to hide income

A second method used in this paper to assess income hiding is to estimate the willingness to pay to hide income. Squires (2017) used this technique to measure the marginal kinship tax rates and Boltz et al. (2016) to measure income hiding from kinship. I included questions about hypothetical winnings[12] and asked

[11]Nsima is a staple food made of white cornmeal and water.
[12]Winnings had to be hypothetical given budget limitations.

how much the respondent would be willing to pay in order to keep hypothetical winnings private rather than announced in public (see Appendix Table 1.3). Respondent's were faced with four different sizes of hypothetical winnings: 1,000 MWK, 10,000 MWK and 30,000 and 100,000 MWK[13] and had to decide between receiving the hypothetical winnings in public or paying a fee to receive them in private. The respondent faces a set of decreasing fees from which she can choose from in order to keep winning private rather than public. The fee is between 3% and 45% according to the amount of hypothetical winnings as seen in Table 1.3. Each respondent faced a various choices ans shown in Appendix Table 1.3. In the example shown in Appendix Table 1.3, respondents were asked to either receive 1000 MWK in public or to pay a fee to receive the winnings privately were fees were presented in a descending order such that the price for income hiding equals either 450, 400, 350, 300, 250, 200, 150, 100, 50, or 0 MWK; and the payout amount to be 1000 minus the fee.

Following Squires (2017) and Boltz et al. (2016), any positive willingness to pay to keep winnings private indicates that the respondent prefers not to disclose income to the public, which could be correlated with income hiding.

4.2 Direct questions

The third method used to measure income hiding is by asking direct questions on income hiding. The respondent is asked directly if the respondent had ever

[13]Exchange rate during the study period was 1USD equal to 450 MWK, therefore hypothetical winnings were equivalent to USD 2.22, USD 22.22, USD 66.67, and, USD 222.22 respectively which represent 0.88%, 8.76%, 26.3%, and 87.57% respectively of the average annual earnings (USD 253.51) in the sample.

hidden income from their spouse, other relatives, friends, neighbors, village head, and other village members. Additional questions were asked about how does the respondent hide income and whether it was costly to hide. This is supposed to give a lower bound of the true rate of income hiding.

4.3 Direct vs Indirect Elicitation

The literature has interpreted differences of prevalence rates of sensitive issues between direct questioning and the techniques mentioned above as the latter being more accurate than the former. However, recent studies have shown that when dealing with sensitive issues, researchers need to worry not only about false negatives (respondents denying to engage in a sensitive issue) but also false positives (respondents over reporting to have a sensitive trait when they actually don't). With the latter, the interpretation of differences between direct questioning and the answer received by the techniques explained earlier, can no longer be interpreted as the latter being more truthful answers.

Höglinger and Diekmann (2017) test for false positives in CM by comparing answers to direct questioning. The authors use a method that does not require an individual-level validation criterion. To do so, the authors use low- or zero-prevalence items "ever received a donated organ" and "ever suffered from chagas disease". While CM reported and 8% positive rate, direct questioning reported 5% prevalence rate, and since the authors know that the answer should be closer to zero due to the zero-prevalence in the sample area they show that CM can suffer from false positives and is not always a superior technique than direct questioning.

Karlan, Osman, and Zinman (2016) provide another example. The authors use three methods to measure how people spend credit loans: (1) direct elicitation, (2) indirect elicitation via list randomization, and (3) by asking about cash outflows. The study reveals limitations to both direct elicitation and indirect elicitation, and highlights the importance of using high frequency cash flows to study loan use instead.

5 Income Hiding Prevalence

5.1 Willingness to pay to hide income

Table 1.4 panel A, reports the hypothetical winnings exercise described in section 4.1.2. The Table shows that between 17% to 27% of respondents have a positive willingness to pay in order to keep hypothetical winnings private. Respondents were asked to answer in two scenarios: receiving the lump sum before FISP allocation and after FISP allocation. Both measures of willingness to pay are highly correlated, with correlation coefficients ranging from 0.83 to 0.97. It is certainly possible that the high correlation owes to the fact that it was difficult for the respondents to put themselves in a double hypothetical situation (hypothetical winnings and hypothetical timing), but an alternative explanation is that since FISP selection happens every year, even if hypothetical winnings occur after FISP selection of a given year, it will occur before beneficiary selection of the following year. Another interesting pattern that emerges from the table is that the share of respondents that have a positive willingness to pay to keep winnings private increases as the hypothetical winnings increases from 1,000 MWK to 100,000 MWK. While the share is between 17% and 20% for hypothetical winnings of 1,000 MWK, or about USD 2.2[14], the share increases to 25% and 27% for hypothetical winnings of 30,000 MWK (USD 66.67) and 100,000 MWK (USD 222.22) respectively. Correlation between before and after FISP 2015 beneficiary selection also increases with the amount of hypothetical wins.

5.2 List Randomization

Table 1.4, panel B reports the results of the list randomization exercise. The first row shows that 16% of households hide some income from other household members (significant at the 90% of confidence). The third row, in turn, shows that about 18% of respondents report knowing how to increase their chances to get FISP (statistically significant at 5% of confidence). Somewhat surprisingly, the second row shows that there is no evidence that people hide income from village head. If hiding from the village head had low prevalence, this could be a false negative (Karlan and Zinman, 2012).

5.3 Direct Questions

Table 1.5 shows that 23% of respondents report to hide income, be it from their spouse, other relatives, friends, neighbors and village head. Hiding income from village head (16%), and neighbors (16%) are the modal categories, closely followed by friends (15%) and other relatives (11%). Hiding income from the spouse is less

[14] Using the exchange rate during fieldwork in August 2015

prevalent, at 3%. When asked about whether hiding income was costly, only 6% of respondents reported it was costly. This gives an indication that households don't perceive income hiding as a costly activity, which could simply mean that the income hiding methods do not require out of pocket expenditures. A majority of respondents report that they physically hide income in a secret place (65%), while others report hiding income by not purchasing animals (12%), by asking for credit (11%), by purchasing fewer snacks (8%), or by not making improvements to their house (5%).

The lower panel in Table 1.5 reports descriptive evidence or other strategic behavior engaged by villagers in order to improve their chances of being selected beneficiaries of FISP. Around 19% of respondents report to have done something to improve their chances of getting FISP. The two most popular actions taken were: (i) by taking part in village developments and meetings (64%), and, (ii) by asking or complaining to the village head (36%). Other categories, like working hard on the land, spending time in the village, and working less in the business, had between 2 and 7 percent prevalence among those who did anything to increase their chance of receiving FISP.

6 Discussion and conclusions

I study the extent of income hiding from villagers and village headmen in the context of a large input subsidy program in Malawi (FISP) by using different measurement techniques since income hiding is a sensitive question. I use three alternative measures: direct questions, list randomization and willingness to pay to hide income (hypothetical winnings). Direct questioning, consists on asking respondents directly whether they hide some income and from whom. List randomization consists on randomly selecting half of the sample to answer to a short list of non-sensitive statements, while the other half is randomly assigned to answer a larger list which includes the short list plus one additional statement (the sensitive question). Respondents are asked for the number of true statements in the list such that the difference in the average of the two lists corresponds to the prevalence of the sensitive behavior (Karlan and Zinman, 2012). Willingness to pay to hide income consists on asking respondent if they are willing to pay and how much in order to keep hypothetical winnings private rather than announced in public. Respondent's were faced with four different sizes of hypothetical winnings.

Using novel data from Malawi, I find that, depending on the method, between 16% to 27% of households in the sample hide income either from relatives, neighbors, the village head, etc. Direct questions about hiding perform well in my study setting. First, households seem to be upfront in reporting that they hide income. Second, unlike willingness-to-pay to hide, direct questions allow to identify from whom is the household hiding income (relatives, friends, neighbors, village head, or a combination of them). Randomization list did not work well in this setting, a matter I discuss in depth in section 4.3, relating it to recent literature on this method.

This study has 4 main caveats. First, the sample size is small, which means wide confidence intervals and false negatives. Second, willingness to pay to hide income was measured using hypothetical winnings instead of real potential winnings that would motivate respondents to answer more truthfully according to their real preferences. Third, willingness to pay to hide income can also be measured as an open ended questions such that the marginal ''tax rate" is compute. Fourth, the study uses only one method for indirect elicitation. In addition, an open question for future research is the subsidy targeting consequences of income hiding. Finally, future research can assess the subsidy targeting consequences of income hiding in this setting.

Table 1.1: Household Descriptive Statistics			
	Mean	**St. Dev.**	**Observations**
Panel A: Main Respondent			
Male	0.25	0.43	300
Age in years	44.19	17.08	296
Education level			
None	0.24	0.43	300
Primary incomplete	0.53	0.50	300
Primary complete	0.12	0.33	300
Secondary incomplete	0.09	0.28	294
Secondary complete	0.02	0.14	300
Post secondary	0.01	0.10	300
Holds a public position at village	0.18	0.38	300
Panel B: Household Characteristics			
Demographic Characteristics			
Number of members younger than 15	2.82	1.82	300
Household members 15 or older	2.60	1.15	300
Years living in village	39.07	17.30	299
Any household member disabled	0.11	0.32	300
Household polygamous	0.10	0.30	300
Economic Activity			
Any member sells at market	0.22	0.41	300
Any member works at center	0.07	0.25	300
Any member owns business	0.45	0.50	300
Acres of land used for farming	2.26	1.39	300
Acres of land owned	2.42	1.87	300
Acres of land planned to farm 2015/2016	2.28	1.47	300
Farm and non farm annual earnings (USD)	253.51	854.02	300
USD if you had sold all your last harvest	83.41	102.03	300
Value of animals and household assets (USD)	409.47	3244.34	300
Average monthly expenditure (USD)	25.27	24.51	300
For those with non-farm earnings:			
Annual Non farming earnings (USD)	301.96	1120.05	169
Political Connections in the village			
Related to chief	0.37	0.48	300
Friends with chief	0.39	0.49	300
Related to VDC member	0.21	0.41	298
Friends with VDC member	0.26	0.44	300

Notes: All data are from the villagers survey. Panel B: VDC stands for Village Development Committee. Exchange rate used 1USD = 450 MKW

Table 1.2: Village Characteristics			
Variable	**Mean**	**St. Dev.**	**Observations**
Panel A: Village Head Characteristics			
Lives in the village where is village head	0.95	0.22	60
Age	51.80	15.99	59
Male	0.77	0.43	60
Tenure (years)	12.90	13.14	60
Will your household get a FISP package in 2015/2016?	0.85	0.36	55
Panel B: FISP packages for the village			
Number of packages received in 2014	76.02	36.50	60
Number of packages received in 2013	76.25	36.59	59
Number of packages received in 2013	73.45	34.57	58
Number of packages received in 2011	74.91	35.45	58
Number of packages received in 2010	75.05	37.61	57

Notes: All data are from the village head survey. Panel A: Tenure refers to the time the village head interviewed has had the village head position.

Table 1.3: Fees according to willingness to pay to hide income (MWK)

1,000		10,000		30,000		100,000	
Fee	% Fee	Fee	% Fee	Fee	% Fee	Fee	% Fee
450	45%	4,500	45%	9,000	30%	45,000	45%
400	40%	4,000	40%	8,000	27%	40,000	40%
350	35%	3,500	35%	7,000	23%	35,000	35%
300	30%	3,000	30%	6,000	20%	30,000	30%
250	25%	2,500	25%	5,000	17%	25,000	25%
200	20%	2,000	20%	4,000	13%	20,000	20%
150	15%	1,500	15%	3,000	10%	15,000	15%
100	10%	1,000	10%	2,000	7%	10,000	10%
50	5%	500	5%	1,000	3%	5,000	5%
0	0%	0	0%	0	0%	0	0%

Notes: Data was collected in the villagers survey. Each respondents was asked a total of 8 questions regarding willingness to pay to hide income: 4 before beneficiary selection and other 4 after beneficiary selection. Each set of questions used different hypothetical winnings to motivate the exercise as shown in Table 3. For more detailed information on the survey questions see Appendix Table 3

Table 1.4: Indirect questions about income hiding

Panel A: Positive Willingness to Pay to hide Hypothetical Winnings

Hypothetical winnings	Mean After Beneficiary Selection	Mean Before Beneficiary Selection	Difference (1)-(2)	P-value	Correlation (1) & (2)
	(1)	(2)	(3)	(4)	(5)
1,000 MWK	0.83	0.20	0.17	0.03	0.05
10,000 MWK	0.93	0.20	0.21	-0.01	0.26
30,000 MWK	0.97	0.25	0.25	0.00	0.56
100,000 MWK	0.96	0.27	0.27	0.00	0.66
Observations			300		

Panel B: Randomization Lists

Additional item in lists	Mean List with 4 Items	Mean List with 3 Items	Difference (1)-(2)	P-value	Observations
	(1)	(2)	(3)	(4)	(5)
Hides some income from other household members	1.69	1.53	0.16	0.08	296
Hides some income from village head	0.82	0.90	-0.07	0.37	277
Knows how to improve chances to get FISP	1.76	1.57	0.18	0.03	270
Observations			300		

Notes: Panel A: Reports the percentage of people who prefer to pay a fee to keep hypothetical winnings private. The questions used was: "Let's say that during [before or after beneficiaries are selected] you get an income shock of [AMOUNT] MWK that you can either receive publicly during a village meeting or in private with some deduction on the amount." Panel B: Using randomization list techniques, reports results of income hiding to other household members and village head and whether respondents know how to improve their chances of receiving FISP.

Table 1.5: Direct questions about income hiding		
Variable	**Mean**	**Observations**
Hides income from:		
Spouse	0.03	300
Other relatives	0.11	300
Friends	0.15	300
Neighbors	0.16	300
Village head	0.16	300
Doesn't hide	0.77	300
How do you hide money (For those who hide):		
By hiding in a secret place	0.65	65
By not purchasing animals	0.12	65
By asking for credit	0.11	65
By purchasing fewer snacks	0.08	65
Not making improvements to house	0.05	65
By spending less in general	0.03	65
By eating less meat	0.02	65
By not starting a business	0.02	65
By opening a bank account	0.02	65
By asking for food	0.65	65
Was it costly to hide income?	0.06	52
Have you ever done anything to try to get FISP?	0.19	297
If yes, what did you do?		
By taking part of village developments and village meetings	0.64	55
By complaining to the village head	0.36	55
By working hard on my land	0.07	55
By spending more time in the village	0.05	55
By working less on my business	0.02	55

Notes: Data are from villagers survey

Table A.1: Farmer Census Data

Traditional Authority	Female	Male	Total	%
Amidu	**5,544**	**8,723**	**14,267**	**10%**
Chanthunya	1,740	2,319	4,059	3%
Kachenga	5,971	6,718	12,689	9%
Kalembo	**10,233**	**14,922**	**25,155**	**18%**
Nkaya	8,719	10,592	19,311	14%
Nsamala	**18,691**	**28,158**	**46,849**	**34%**
Sawali	6,251	8,251	14,502	11%
Total	57,149	79,683	136,832	100%

Table A.2: List Randomization Questions

List Randomization Statement:
[FO read]: "I'm going to read and show you three statements. For each one, decide whether the statement is true or false, but do not say it out loud to me. At the end you have to count in your head the number of statements that are TRUE. When I am finished reading all of the statements, tell me how many out of the three statements are TRUE".

	Short List	Complete List
List 1	1. All of my harvest from 2014 got spoiled. 2. I bought or sold a chicken in the last year 3. I ate nsima at least once during the last week	1. All of my harvest from 2014 got spoiled. 2. I bought or sold a chicken in the last year 3. I ate nsima at least once during the last week 4. I hide some of my income from other household members.
List 2	1. I owned a chicken last year. 2. I planted beans in 2014/2015 farming season 3. I used a tractor to prepare my land in 2014/2015 farming season.	1. I owned a chicken last year. 2. I planted beans in 2014/2015 farming season 3. I used a tractor to prepare my land in 2014/2015 farming season. 4. I hide some of my income from the village head.
List 3	1. I used irrigation on my crops during the 2014/2015 farming season. 2. I planted maize during 2014/2015 farming season. 3. I know how to improve my chances to get a farming input subsidy.	1. I used irrigation on my crops during the 2014/2015 farming season. 2. I planted maize during 2014/2015 farming season. 3. I know how to improve my chances to get a farming input subsidy. 4. I bought sugar in the last 2 years.

Table A.3: Willingness to Pay to Hide Income

Statement:
[FO read]: Now, I'm going to ask you a series of hypothetical questions in which you have to make a decision between receiving an income shock in private (at your home) or publicly (at a village meeting).

Example Question:
Let's say that **during July** (before input subsidies are allocated) you get an income shock of 1,000 MWK that you can either receive publicly during a village meeting or in private with some deduction on the amount. Mark your preferred option. These are your options.
Please put an X in the "your choice" column to denote the respondent's final decision for the amount s/he chooses to invest.

	Private	Public	Deduction (fee)	Your choice
1	550 MWK	0 MWK	450 MWK	
2	600 MWK	0 MWK	400 MWK	
3	650 MWK	0 MWK	350 MWK	
4	700 MWK	0 MWK	300 MWK	
5	750 MWK	0 MWK	250 MWK	
6	800 MWK	0 MWK	200 MWK	
7	850 MWK	0 MWK	150 MWK	
8	900 MWK	0 MWK	100 MWK	
9	950 MWK	0 MWK	50 MWK	
10	0 MWK	1000 MWK	0 MWK	

Notes:(1) Similar questions were asked for other hypothetical amounts of winnings: 10,000 MWK, 30,000 MWK, and, 100,000 MWK which corresponds USD 22.2, USD 66.7, and, USD 222.2 respectively at an exchange rate of 1USD = 450 MWK. (2) Similar questions were asked regarding hiding hypothetical winnings after input subsidies were allocated (September). These questions showed similar willingness to pay to hide in comparison to the before beneficiary selection questions. The lack of difference between the two measures of willingnes to pay to hide income may be explained by the yearly nature of the input subsidy program were every year beneficiaries are selected and therefore hiding income after FISP 2015 allocation would also be hiding income prior to FISP 2016 allocation.

Chapter 2

Decentralization and Efficiency of Subsidy Targeting: Evidence from Chiefs in Rural Malawi

1 Introduction

Targeting programs such as subsidies to needy households is an important part of what governments do. To do this effectively, governments must first identify who is truly needy, which may be difficult in developing countries where government infrastructure and information technology is limited (particularly in rural areas). Governments typically have the choice to administer such selection of eligibles centrally, or to decentralize authority to local communities (usually these programs are officially administered by local leaders).[1] Decentralization has two main benefits: (1) local leaders are almost surely more informed about the relative neediness of people in their village than a centralized bureaucracy, especially in a context in which most people do not file a tax return; and (2) local leaders will be more accountable to villagers, particularly if leaders face village electoral pressure or are motivated by reputation concerns. On the downside, decentralization may open the door for corruption or nepotism.

This paper uses rich panel data collected from a sample of 1,559 households over four survey rounds in 2011-2013 to explore this fundamental trade-off in the context of two subsidy programs in Malawi – the well-known farming input subsidy program which provides subsidies for fertilizer and hybrid seeds once a year, and a one-time food aid relief program put in place after a drought in 2012. These programs were conceived as anti-poverty programs and the selection of beneficiaries was decentralized to local traditional leaders, called chiefs. How well do chiefs target the program?

[1] See Coady et al. (2004) for a detailed discussion of various forms of targeting.

This is a setting in which the tradeoff between nepotism and information is particularly salient. On the one hand, nepotism is possible since chiefs cannot be held accountable via electoral pressure (in contrast to papers such as Bardhan and Mookherjee 2000, 2005; Bardhan 2002) – the position of chief in Malawi is hereditary and chiefs face fairly weak oversight. There is also no strict eligibility rule provided by the government (only general guidance on who should be "considered") and no government backchecking of allocations.[2] But on the other hand, local information is critical, along two main dimensions: (1) shocks are common and chiefs likely have good information on recent household-specific economic conditions; and (2) the return to inputs will likely be heterogenous across households within a village and related to factors such as soil type, access to credit, household composition, etc. Since there is some non-negligible level of income pooling, targeting the inputs where they will increase output the most (rather than to the neediest) may be welfare improving: the input subsidy can be allocated based on productive efficiency to increase the size of the pie, and *ex post* inter-household transfers can be used to reduce poverty.

In this paper, we compare the allocation chosen by chiefs to that which would have obtained under an alternative of a proxy-means test (PMT) based on a household survey. We are interested in two sets of questions. First, how common are errors of exclusion (truly needy households not getting the subsidy) under the chiefs, and is this error rate higher or lower than what would have occurred under the PMT?

[2]See Niehaus et al. (2013) for a discussion of optimal targeting rules when programs are implemented via local, possibly corruptible, agents, but in which agents can be punished if they do not follow the rule.

Do chiefs use local information to target households which have suffered recent negative shocks? Do they favor relatives? Second, do chiefs take into consideration productive efficiency when allocating the input subsidies? Specifically, do they target the agricultural subsidies to households with higher returns to fertilizer?

To answer the first question, we follow Alatas et al. (2011, 2013) and use observed food expenditures in the immediate pre-subsidy period as our measure of neediness. We find evidence that both the chiefs and the PMT miss a substantial fraction of poor people, but that the chiefs miss significantly more: chiefs wrongly exclude about 15% of people for both the input and food subsidy, whereas the PMT would have excluded only about 10% for the input subsidy and 14% for the food subsidy. We also find evidence of nepotism: chiefs are more likely to target food subsidies to relatives. However, this nepotism appears to have minimal aggregate welfare consequences, since chiefs' relatives are similarly poor to other villagers. Ultimately, we find that both the PMT and the observed chiefs' allocation are pro-poor, consistently outperforming a random allocation (especially in terms of mean squared error). While chiefs do worse than the PMT in terms of poverty targeting, we find that chiefs use their informational advantage to the benefit of households hit with negative shocks: people who have experienced droughts, floods, cattle death, or crop disease are significantly more likely to receive subsidies.

The second part of the paper tests whether chiefs target input subsidies to people with higher returns to agricultural inputs. The test is derived from a model of subsidy allocation in which chiefs have preferences over households (each household has a given welfare weight), but also have information about household-specific returns to agricultural inputs. We assume that there is little heterogeneity in productive returns to food, in which case the allocation of the food subsidy is reflective of the welfare weights. To back out the relative importance of productivity considerations in the chief's objective function, we can thus exploit the wedge between the allocations of the food and input subsidies. Taking this to the data, we find that chiefs indeed allocate relatively more inputs to households with higher gains from fertilizer use, while the PMT would not, suggesting productive efficiency gains from a decentralized system.

Our paper ultimately paints a nuanced view of the targeting of chiefs. On the one hand, chiefs do significantly better than a random allocation would have (as in Galasso and Ravallion 2005 for a food-for-education program in Bangladesh), and we find evidence that decentralization has the benefit of improved information on recipients (see Bardhan and Mookherjee 2006 for evidence on the benefits of decentralization for a credit and farming input subsidies in West Bengal). On the other hand, we do find strong evidence of nepotism. As in Alatas et al. (2013), we find that the ultimate welfare consequences of nepotism are likely small, however.

Our paper makes several contributions to the literature. Starting with the more narrow contribution, our paper highlights a number of pitfalls for studies aiming to assess the efficiency of poverty targeting in government programs. We show that assessing the true quality of targeting is very data-demanding: assets like land are noisy predictors of consumption – the R-squared for our PMT regression is only 0.32 in Malawi, and we document similar figures for datasets from Kenya and Uganda. This may be one reason we find lower levels of mistargeting and elite capture here than in previous work which used assets as a proxy for need (i.e. Bardhan and Mookherjee 2006), including several previous studies in Malawi (Dorward et al. 2008, 2013; Kilic et al. 2013). We also bring attention to the difference between poverty targeting and *poverty reduction* (the ultimate goal of subsidy programs). In communities with informal income pooling, productive efficiency targeting may be the more effective (albeit indirect) way of reducing poverty. Thus looking only at who gets input subsidies rather than how the produced output is allocated may not be sufficient to gauge the poverty impacts. PMTs and chiefs are not the only mechanisms that can be used to select beneficiaries. Another common mechanism is community-based targeting (CBT, where communities get together to decide on beneficiaries). Two studies that do careful comparisons between PMT and CBT in the context of cash transfer programs tend to give a modest advantage to the PMT: Alatas et al. (2012, 2013) in Indonesia, where the relationship between assets and consumption is somewhat stronger than in contexts we consider; and Stoeffler et al. (2016) in Cameroon, where the CBT implementation appear poor. While the results of these two studies suggest community targeting could at best marginally improve on the chiefs' allocation, exploring the impact of community-based targeting in contexts like ours is an interesting area for future research.

More broadly, we contribute to the literature on the role of traditional authorities in African development. While survey evidence from the Afrobarometer suggests that traditional leaders are perceived to regulate important aspects of the local economy in numerous African countries (Logan, 2011; Michalopoulos and Papaioannou, 2013), the question of whether their existence further undermines weak governance, or instead palliates it, is still unsettled. Acemoglu et al. (2014) find that areas of Sierra Leone where competition among potential chieftaincy heirs was low during and after British colonial rule have significantly worse development outcomes today, but higher levels of respect for traditional authorities. They hypothesize that this reflects the ability of uncontested traditional ruling families to simultaneously capture resources and civil society organizations. Our evidence from Malawi mitigates this view: in our context, traditional leaders are uncontested and popular, as in Acemoglu et al. (2014), but effective at targeting input subsidies to productive farmers, possibly putting their village on a higher growth path.[3]

The layout of the paper is as follows. Section 2 presents some background on the Malawian local governance structure and decentralized subsidy programs. Section 3 discusses the sample and data. Section 4 presents evidence on poverty-based (mis)targeting. Section 5 tests for productive efficiency targeting. Section 6 concludes.

[3]Outside of Africa, Anderson et al. (2015) also finds evidence of poor governance by elites in Maharashtra, India. Though democracy appears to be vibrant, there exists elites an entrenched clientilisic vote-trading system in which elites landholders are able to enact policies which lower rural wages in exchange for insurance. As in Acemogulu et al. (2014), this system is so engrained that people report high levels of satisfaction.

2 Institutional Background

2.1 Local governance in Malawi and the role of chiefs

Malawi is a presidential democracy with a single federal legislative body (parliament). At the sub-national level, Malawi is divided into 28 districts each administered by a District Assembly. District Assemblies consist of a combination of democratically elected councilors and members of parliament, together with ex-officio, non-voting members. This local government coexists with a traditional chieftaincy hierarchy. There are four ranks within this hierarchy: Paramount Chief, Traditional Authority (TA), Group Village Headman (GVH), and Village Headman (also known as village chief). In our data, TAs have authority over areas smaller than a district. They oversee from 10 to 45 GVHs, and GVHs oversee between 2 and 10 villages.[4]

Chiefs in Malawi hold little formal power. They do not have direct control over any public funds and are not allowed to raise local taxes. However, chiefs hold other

[4] A brief history of the coexistence of these two systems of local governance is as follows (this note relies heavily on Lihoma 2012, Eggen 2011 and Cammack et al. 2007). Prior to colonialism, local government structures in Malawi varied across regions and ethnic groups. Most local governments included chiefs, but the role of chiefs varied between centralized systems in which the chief's authority was paramount and more decentralized, participatory systems (Lihoma, 2012). Malawi was colonized by Britain in 1891, which attempted a system of direct rule which minimized the authority of chiefs. In 1912, Britain moved towards a system of indirect rule which recognized chiefs as traditional authorities, reporting to the colonial district administrator. In 1933, traditional powers were extended such that chiefs could perform some functions of local government (such as administering communal land and arbitrating disputes in traditional courts), though chiefs were still financially dependent on colonial administrators. Beginning in 1953 and continuing until independence in 1964, the British transferred local authority from chiefs to district councils. While higher-ranked chiefs (TAs) served as ex-officio members of these councils, their powers to act unilaterally were limited (and were officially subordinate to the council itself).

customary responsibilities. The 1998 Decentralisation Policy and Local Government Act recognized the rights of chiefs to allocate communal land and adjudicate matters related to customary law (in particular customary land). Chiefs also play an advisory and coordination role regarding local development projects: local development funds are in principle spent through groups known as Area Development Committees (headed by TAs) and Village Development Committees (chaired by GVH and composed of ward councilors, MPs, religious leaders, business leaders and youth and women representatives). Finally – and this is the focus of this paper – chiefs are typically relied upon to identify beneficiaries for targeted government programs.

Traditional leadership positions are hereditary. Chiefs who pass away are replaced by someone from the chieftaincy clan. In patrilineal communities, such as in the northern part of the country, chieftancy is inherited from father to son, while in matrilineal communities, like in the southern and central parts of the country where our data comes from, chieftancy is inherited from maternal uncle to nephews (so the first son of the first sister inherits the right to the position) (Chirwa 2014). There are a few female chiefs, but they are often seen as "holding the place for a brother" (Peters 2010).

Chiefs are paid a salary by the government that is known as *mswahala*, but it is fairly small.[5] Chiefs do occasionally charge fees to villagers (in our sample, 44 percent of villagers report having ever made a payment to the village chief). Interestingly, chiefs are favorably viewed by the majority of the Malawian popu-

[5]In 2014, a village chief in Malawi received 2,500 MWK (about US$6 in 2014) per month as *mswahala*, around a week's worth of labor at the prevailing casual wage.

lation. In 2008-2009, 74% of Afrobarometer respondents in Malawi perceived traditional leaders as having “some” or “a great deal” of influence, and 71% thought the amount of influence traditional leaders have in governing the local community should increase – for comparison, the average across 19 African countries for these two questions were both 50% (Logan, 2011). Possibly as a result of this high popularity, chiefs appear able to influence local villagers on whom to support in general elections and local government elections (Patel et al., 2007), an influence that may limit their accountability to elected representatives.

2.2 Subsidy Programs

2.2.1 Fertilizer Subsidy Program

Malawi’s Farming and Agricultural Input Subsidy Program (FISP) is one of the largest fertilizer and seed subsidy programs in the world.[6] Though the program has existed since 1998, it greatly expanded after a drought in 2004 and has steadily increased in size since then. In 2012-13, the program reached 4.4 million recipients and took up 10-15% of the government’s budget (Dorward et. al 2013, Baltzer and Hansen 2011). In our data, the percentage of people benefitting from subsidies has increased steadily over time, from 63% in 2008 to 82% in 2012.

The subsidy program covers several inputs and comes in the form of vouchers, which are redeemable at a local agricultural shop in exchange for the items. The four most common items covered by the voucher subsidy during our study period

[6] Fertilizer subsidies are one of the more popular (and expensive) aid programs across the developing world, in some cases taking up significant fractions of government budgets. For example, Sri Lanka, Malawi and India spend 10-20% of their government’s budget on fertilizer subsidies (Wiggins and Brooks 2010). The countries of Zambia and Tanzania also devote 1-2% of their budget to subsidies (Baltzer and Hansen 2011).

were planting fertilizer (a 50 kilogram bag of NPK, worth about $40 at market prices in 2013), top-dressing fertilizer (a 50 kilogram bag of Urea, comparable in price to NPK), hybrid maize seeds (a 5 kg bag, worth about $7), and hybrid groundnut seeds (a 2 kg bag, worth $2.60). The price of the voucher is only $1.7, so the subsidy is extremely high, at over 98%, and as a result take-up of the vouchers is reported to be 100%.

There is no strictly defined, official eligibility criteria for the subsidy, but the intention is to target the poor and vulnerable. The official FISP guidelines reads that beneficiaries "will be full time resource poor smallholders Malawian farmers" but no threshold is provided for what defines "resource poor". The program guidelines does hint at particular groups however: ".the following vulnerable groups should also be considered: elderly, HIV positive, female headed households, child headed households, orphan headed households, physically challenged headed households and heads looking after the elderly and physically challenged" (MoAFS 2009).

The identification of beneficiaries has three main stages (Chirwa et al. 2010). First, the government conducts a national farmer registration census. Second, the central government allocates vouchers to districts as a function of the area's farming population and the acreage under cultivation. Within each district, the District Agriculture Development Office (DADO) allocates vouchers across villages based on farming population shares (Chirwa and Dorward 2013). Finally, within each village, once the number of subsidies available to the village is known, a list of eligible villagers is made. Formally, the selection of beneficiaries at this stage is supposed to be done by the Village Development Committee through open community meetings, and audited by the DADO. However, as we will show below, most authority appears to be *de facto* delegated to chiefs.[7] Once the list of beneficiaries have been received by the DADO, it establishes a date and venue for the distribution of the vouchers themselves. The distribution is done by a staff member from the DADO. Listed beneficiaries have to show their voter registration card in order to receive the vouchers and also to redeem the vouchers at the retail stores (MoAFS 2009).

The identification of beneficiaries and distribution of vouchers is timed to precede the main planting season (which begins in November and lasts until the harvest in March). Beneficiary lists are typically drawn in August, while the subsidy vouchers themselves are distributed in September/October, in advance of planting.

2.2.2 Food Subsidy Program

Malawi devalued its currency in 2012, causing inflation of 20-30% in 2012-13 (World Bank 2015) and making food imports prohibitively costly. There was also a drought in 2012, causing the harvest to be poor. In response, a food subsidy program was implemented in late 2012, lasting from November 2012 to January 2013. In our area of study, the subsidies were distributed in kind. As with the input subsidy, the program was targeted at the "poor" but without a precise threshold or formula for what constitutes poverty. Of those receiving the subsidy in our data, the average amount received was 103 kg of maize, 14 kg of soy blend,

[7] For example, Dorward et. al (2013) show that around 70% of households in 2013 believed the decision on voucher recipients was made by the chiefs *before* the official meeting was held.

18 kg of pigeon peas, 10 kg of beans, and 3 liters of oil. We estimate that this package was worth about $72 in 2013 USD. As with the farming input subsidy program, chiefs were given primary responsibility for identifying which households should receive the food aid.

3 Data

3.1 Sample

The data we use for this paper was collected as part of a separate research project focused on estimating the impact of providing savings accounts to unbanked households (Dupas et al., 2017a). The project took place around the catchment areas of NBS bank branches in two districts of Southern Malawi – Machinga and Balaka. The sampling frame for Dupas et al. (2017a) relied on a census of market businesses and a census of households conducted at the end of 2010 – we use only the household sample for this analysis. The household census listed 9,297 households from 68 villages in three Traditional Authorities (TA) areas – Kalembo, Sitola, and Nsamala. Of these, 78.8% met the eligibility criteria set by Dupas et al. (2017a): they did not have a bank account and had a female head of household (that is, the sample includes female widows and married or cohabitating couples). Dupas et al. (2017a) randomly sampled a subset and completed 2,107 baseline surveys. Of this baseline sample, 354 did not complete one of the three follow-up surveys used in this paper (16.9%). In addition, the module to measure food subsidy receipt was introduced only partially through the endline survey, and another 185 households were not asked these questions (9.7%). We are thus left with a sample of 1,568 households. The analysis in this paper requires comparing subsidy recipients to non-recipients in the same village, so we need a sufficient number of people in each village. For this reason we drop five villages with less than 5 households in our data, and are left with 1,559 households in 61 villages for our analysis.

Given this sampling frame, our data includes only a subset of people in each village (around 10% of households). Since our question of interest is to understand how chiefs allocated subsidies *within this sample*, and our basic thought experiment is to ask what the gains would be from re-allocating subsidies *within this sample*, our results are still internally valid and of interest, however. Second, our sample excludes the 15% or so of households who already had a formal place to save as of 2011. Since formal sector employee are those most likely to have an account, our sample likely excludes them and hence the top tail of the income distribution. We may therefore underestimate overall targeting errors (if any of the relatively better-off people with bank accounts ended up receiving subsidies) though we think this is quite unlikely since they typically do not cultivate land.

3.2 Data Sources

A timeline of project activities is included as Figure B.1. We have four waves of survey data for each household in the sample: (1) a baseline conducted from February to March 2011 (2) a first follow-up survey conducted from February to March 2012; (3) a second follow-up survey conducted from September to December 2012; and (4) an endline survey conducted from February to May 2013. The baseline survey includes a standard set of demographic variables, including a module on asset ownership which can be used to construct the allocation that would have obtained under a counterfactual allocation based on a proxy-means test from baseline assets. Each of these survey rounds included detailed expenditure modules.

The follow-up and endline surveys include a module on the farming subsidy. This is used to construct a time series of subsidies received from 2008-2013, for each household. The module includes information on which input subsidy was received, whether the household received the voucher itself or shared another household's voucher, and what the household actually did with the subsidized products (used them, sold them, shared them, etc.). The endline survey also asked these questions for the food subsidy, which was introduced in 2012. Finally, the endline included a separate module with questions on how the input and food subsidies were allocated. These include questions on how (in the respondent's opinion) the vouchers were allocated, whether a public meeting was held, whether the respondent participated in the meeting, etc. We use this module to provide some descriptive evidence on how the programs were implemented.

In addition, between August and October 2014 we collected a fifth wave of data for a random subset of 563 households in the initial sample. This survey asked additional questions on the process through which subsidies were allocated and on respondents' attitudes towards the allocation process as well as their perception of the traditional authorities' role, beliefs and objectives in this allocation. Importantly, we also elicited households' beliefs on the returns to farming inputs on their own land.

Between August and October 2014 we collected surveys with all of the 105 traditional leaders in our study area of 61 villages, including 76 village headmen (chiefs) and 29 group village headmen (GVH).[8] The survey included questions on their tenure and responsibilities, and included questions about the details of how the FISP and Food subsidy programs were allocated. We also measured village chiefs' self-reported knowledge of the distribution of returns to inputs in their village, and of the realizations of household-specific shocks.

3.3 Characteristics of households, chiefs and villages in the sample

Table 2.1 presents basic summary statistics on the households in our sample. Panel A includes time-invariant characteristics collected at baseline. The first variable shown is the household's self-reported relationship to the chief. We asked the following question to each respondent: "Are you related to the chief?," to which 27% reported yes. In a follow-up question, we asked: "How are you related?" The modal answer was the chief is an uncle (20% of the related cases), followed by brother (13%), brother-in-law (12%) and grandfather (12%). In what follows, we refer to those who reported as being related to the chief as "kin".[9]

[8]The reason why there are more chiefs than villages is that 19 villages were divided into multiple villages between our initial data collection in 2011-13 and the time of the survey in 2014.

[9]Given an average village size of 300 households (Table B.1), the numbers imply that in an average village the chief is uncle to 16 heads of households, the brother to 11 households, the brother in law to 10 households, and the grandfather to 8 households. While high, these numbers are not implausible given high fertility rates.

Households in the sample are very poor: 90% have mud floors or worse quality, 77% have thatch roofs, and less than 1% have electricity. Only 59% are literate, and average years of education for the household head is just below 5.[10] The FISP program specifically targets single-headed households (the majority of which are widowed women), and there are a large number of these: 28% of households are headed by females alone. Ninety-seven percent of households in our sample own some land.

Panel B shows time varying expenditure, shocks and transfers. Across rounds, households report spending a total of only $9.66 per month per capita, $6.80 on food, and $2.71 on non-staple food. These figures place these households well below the global extreme poverty threshold of $1.25 per day. Shocks are also quite common: 26% lost at least 1 day of work in the past month due to illness, 69% had another household member sick over the past month, 28% experienced a drought or flood, and 20% experienced crop loss or livestock death. Across survey rounds, 72% of households report being worried about having enough food to eat in the past 3 months. Transfers across households within the village are very common, with 58% of households reporting being recipients of transfers in the last 90 days, and 25% reporting having made transfers.

Columns 3 and 4 of Table 2.1 show, for each variable, the gap between kin and non-kin and its standard error. This reveals that if anything, kin are poorer than non-kin – they are significantly less educated (Panel A), and have slightly lower consumption (Panel B).

[10] The school system in Malawi is composed of 8 years of primary school and 4 years of high school.

Not only are households in rural Malawi poor, they area also facing a lot of variation in resources over time. Column 5 of Table 2.1 presents the correlation between survey rounds for the variables in Panel B. In general, the correlation is fairly low: shocks are largely uncorrelated across rounds, and the correlation for our neediness measure, non-stable food expenditure, is just 0.35. The correlations for chiefs' kin are not higher than for the overall sample, suggesting that chiefs' kin do not face less risk.

Table B.1 presents summary statistics on villages and village chiefs in our sample. The average village in our sample has 309 households and over 7,000 acres of customary land. The average village chief is 53 years old and has just over 5 years of education. Eighty two percent of chiefs are male. They have been chief for 13 years on average, and 90% inherited the position (most of the remainder were appointed). The vast majority faced no competition from within the family blood line for the position. In principle, traditional leaders can be removed from office or reprimanded, but our data suggests this almost never happens: only one of the chiefs we surveyed mentioned having ever been suspended. These basic statistics suggest that chiefs are *de facto* not accountable to anyone.

When chiefs were asked about their main responsibilities, the five most common responses were resolving conflicts among villagers (90%); reporting issues to higher level chiefs (61%); monitoring village projects (56%); disseminating information to villagers (33%); and overseeing subsidy programs (20%). Note that land allocation was not mentioned. Indeed, while customary land management traditionally falls under the chief's responsibilities, *de facto* land rarely changes hand – over the period of our panel we see no household in our sample receive or lose right to customary land.

3.4 Summary statistics on the allocation of subsidies in our sample

Table B.2 presents summary statistics on the process through which input and food subsidies were allocated among households in our sample. Panels A and C rely on the latest round of survey data (2014) and presents evidence on how both chiefs and villagers experience and perceive the subsidy allocation mechanisms. Panel B presents data from the earlier household survey waves.

The data confirms that chiefs are the primary decision-makers in allocating subsidies. Turning first to panel A, the great majority of village chiefs report that they have control over the subsidy allocation: 62% declare deciding by themselves, and an additional 3% report deciding in collaboration with others. Of the remainder, 13% report that the village development committee (of which the chief is a member) decides the allocation, and 13% report that subsidies are allocated in a village meeting (which the chief typically runs). When asked about selection criteria, chiefs report need as the primary criterion. Chiefs also put significant weight on female-headed households, households which recently received a shock households taking care of orphans, and households that the chief believes are hard-working.

Panel B suggests that community meetings regarding the selection appear to happen quite regularly, with approximately 95% of villagers reporting that a meeting was held to discuss input subsidies, and high attendance at the meeting (82% for inputs and 65% for food subsidies). Nevertheless, Panel C shows that households report that the village chiefs has a considerable role in the allocation: 49% report that the chief alone decides on the input subsidies, and 73% report that the chief alone decides on the food subsidies. Like chiefs, households list neediness as the main criterion, but they also mention demographic characteristics of the household (elderly and female headed households, which are considered priority households in the allocation due to increased neediness). The great majority of households perceive the allocation of subsidies as "very good." There seems to be two factors that households consider when deciding whether the allocation is good – one is whether the allocation benefits the poorest, and the other is whether the allocation reaches the largest possible share of households. The concern for reaching as many households as possible is echoed by chiefs: when asked how they would allocate additional vouchers if they were to receive them, all but one chief say they would give vouchers to more households so that the number of beneficiaries expand (Panel A).

This relates to an important fact about the allocation of subsidies that transpired from our data: while the FISP guidelines do not endorse sharing of subsidy packages, in practice sharing is quite common. We show this in Table B.3. Villagers report large levels of sharing, primarily orchestrated by the chief. Seventy-seven percent (0.46/0.60) of households who received a subsidy voucher report sharing it; of those 83% say they received instructions from the chief on whether to share it, and 79% received specific instructions from the chief on *whom* to share with.

They also overwhelmingly report that the chief decides how food subsidies are shared.

Table B.3 also shows summary statistics on subsidy receipt. The percentage of households receiving input subsidies has increased steadily over time, from 58% in 2008 to 81% in 2012. Receipt of the input subsidy is quite correlated over time, with 48% of households receiving some input subsidy in all five years covered in our data, and 10% never receiving any input subsidy. Conditional on receiving the subsidy, the quantity of fertilizer received (summing over the two types of fertilizer, for planting and top-dressing) was about 77 and 64 kg during 2011 and 2012 respectively. This is smaller than the official package that subsidy beneficiaries are entitled to get (100 kg) due to sharing. Sharing seems to increase over time, explaining part of the growth in the coverage rate: in 2012, more households receive some subsidized inputs but they receive smaller quantities. The food subsidy of 2012 was more limited in scope than the input subsidy, reaching only 59% of households, though sharing was common for food as well.[11]

Since our aim is to think about the efficiency of the chief's allocation, in what follows we consider the allocation observed in our data *after* sharing. That is, if a household answered "yes" to the question "Did you receive an input (food) subsidy in that year?", we consider this household as a beneficiary, irrespective of whether, in subsequent survey questions, the households reveals that it did not receive the actual voucher but a share from another household instructed to share their voucher.

[11] In 2012, 53% of households received both the input and food subsidy, 13.6% received neither, 5% received the food subsidy only and 27.9% received the input subsidy only.

Other safety net programs

Beegle, Galasso and Goldberg (2015) document that chiefs are also involved in deciding which households are eligible for Malawi's public work program (PWP) – though the responsibility falls more on the Group Village Headmen and the villagers themselves. They report that Malawi's PWP "has been operational since the mid-1990s and aims to provide short-term labor-intensive activities to poor, able-bodied households for the purpose of enhancing their food security." While we unfortunately did not collect data on participation in the PWP directly from respondents in our surveys, a fuzzy name match between the original household sample and administrative data on PWP participants obtained from the two districts in our sample yields 167 matches for the 2012-2013 budget year, out of 2,107 households in the Dupas et al. (2017a) baseline survey, suggesting that the PWP coverage in our study area is about 8%. Verification surveys with a subset of those matched and unmatched conducted in March 2015 suggests that an additional 3% may have been participating in PWP, bringing our estimates to roughly 11%.[12] While name matching is always prone to significant error, this ballpark figure is not far from the 15% coverage targeted by the program. While studying how the PWP is targeted and the specific role of chiefs would have been interesting, omitting it due to data limitations should not affect our analysis of the other subsidy progams. Notably, Beegle, Galasso and Goldberg (2015) find no correlation between receipt of PWP and receipt of other benefits, suggesting no

[12]We are extremely grateful to Santiago Saavedra for obtaining the administrative records and performing the matching analysis and verification surveys.

strategic allocation across programs, in particular, no compensation of non-PWP households with input or food subsidies.

4 Poverty targeting

4.1 Measuring Neediness

To measure neediness, we follow Alatas et al. (2012) and use whether households would have qualified based on their food expenditure distribution, which we consider a proxy for consumption. Food expenditures have been shown to be better predictors of neediness than other measures such as income (Deaton 1997; Meyer and Sullivan 2012).

While there are 12 broad food categories (covering all food types) measured in each survey wave, we focus on the 10 categories that are typically purchased rather than self-produced. These are all but the staples and grains: vegetables, fruits, meat, dairy/eggs, salt, sugar, other cooking items (oil, margarine), coffee and tea, snacks, and juice/sodas. Ligon (2017) identifies those foods as elastic goods among a similar population of households in Uganda, and thus useful for drawing inferences regarding household's index of marginal utility of contemporaneous expenditures, or neediness. The two categories excluded were recorded in the survey as "staple" and "grains/nuts", which the great majority of household produce for home consumption.[13]

[13]We expect expenditure to be negatively rather than positively correlated with total con-

We compute the sum of expenditures on these 10 food categories over the 30 days preceding the survey and then divide the sum by the number of household members to construct "per capita non-staple food expenditure" or PCF, our measure of need going forward (we report this figure in USD).[14] The distribution of log PCF in our data is plotted separately for the two main years of analysis, 2011 and 2012, in the top panel of Figure B.1.

The food expenditure we would ideally use to determine "true need" (PCF eligibility) would be measured right around the time when subsidy beneficiaries are identified (which is around August for the input subsidy and November for the food subsidy). However, the timing of our surveys does not precisely correspond to these periods. Our food expenditure module covered the last 7-30 days (depending on the question) before the survey date. Thus, given the dates of the surveys mentioned in Section 3.2, we have consumption data for the following periods: January 2011 to February 2011; January 2011 to February 2012; August to November 2012; and January to April 2013. To study the targeting of the 2011 input subsidy, we thus have to rely on the January 2011 to February 2011 expenditure data, which is not ideal because it is substantially before the period of interest. In particular, it is before the March 2011 harvest, which is likely an

sumption for such goods: those who need to buy them from the market are those whose harvest was poor and ran out faster.

[14]We choose to compute things per capita (PC) rather than per adult equivalent (PAE) because commonly used equivalence scales between children and adults may be an underestimate of how much communities actually value children consumption (Olken 2005). We have done the entire analysis in the paper using PAE instead of PC and the results are identical. See Deaton and Zaidi (2002) for a discussion of constructing poverty indices.

important determinant of actual neediness as of August - November 2011.[15] This is less of a concern for the 2012 subsidies, where we have consumption data from August to November, which is concurrent with the identification of beneficiaries and exactly precedes any actual distribution of food.

4.2 Constructing counterfactual allocations

4.2.1 Neediness Rank

For each village, we observe the total number of households within our sample who benefited from a subsidy (be it full or partial) – call this number $\bar{s}$. To construct the counterfacult in which vouchers (voucher shares) were distributed based on true consumption, we rank households (within each village) by their per capita non-staple food expenditure (PCF). We consider a household "PCF eligible" if they are ranked at or below the $\bar{s}$th farmer in the PCF distribution. We break ties based on total food expenditures and then total expenditures on all items.

4.2.2 PMT Score Rank

To construct the counterfactual in which voucher shares were allocated via PMT, we repeat this procedure but this time we rank households (within each village) by a "PMT score". We consider households PMT eligible if they are ranked at

[15] In principle, we could also use the January 2012 to February 2012 data since no food subsidies were distributed that year and the proceeds of the maize planted with the subsidized inputs of 2011 were not reaped until March 2012. Results look very similar using this data.

or below the $\bar{s}$th household in the PMT score distribution. We compute the PMT score as follows: we regress log PCF on household characteristics, including demographic characteristics, dwelling characteristics, assets and occupation, and use the estimated coefficients to predict a score for each household. As in Alatas et al. (2012), we do this in two steps: we first run kitchen sink regressions with all available characteristics and then, using a backward stepwise procedure, keep only those characteristics significant at the 10 percent level in the regression used to predict the score.

The PMT regressions are shown in Table B.4. We show the results for both per capita and per adult equivalent food expenditure, and find slightly higher predictive power for per capita values.[16] From Column 1, we obtain a R-Squared of 0.32, which is somewhat lower than the 0.40 obtained by Alatas et al. (2012) in Indonesia (when pooling districts together). To test the extent to which our lower R-Squared is due to a particularly pronounced measurement error problem in our dataset, in Table B.5 we use data from the third wave of the Integrated Household Survey (IHS3), a representative household survey collected by Malawi's National Statistics Office. We restrict that dataset to the two districts in our sample, and estimate PMT regressions using the same backward stepwise method to identify covariates. We obtain a R-Squared of 0.39 when we include all potential covariates available in the IHS3, and essentially the same when we restrict the potential regressors to the set available in our own dataset (we call it "BDR variables," with slightly different variables than the IHS3). This suggests that the lower R-

[16]We use a per adult equivalent formula of 1 child under 18 equal to 0.5 adults.

Squared we obtain in our own dataset is not due to our survey instrument having failed to measure important predictors. Instead, our R-squared of 0.32 in our dataset is possibly lower than the 0.39 found in IHS3 data because our sample is somewhat poorer than a representative sample, and their consumption may be more volatile due to lower access to insurance. In comparable samples from neighboring countries we find similarly low R-squared values: Table B.6 shows regressions for samples of rural unbanked households in Kenya (Dupas et al., 2017b) and Uganda (Dupas et al., 2017a). We find an R-squared of 0.31 in Kenya and 0.28 in Uganda.

4.3 Poverty-targeting results

4.3.1 PMT vs. Chief Allocation

Our first main result is Figure 2.1, which plots the probability of receiving the subsidies by quintile of the PMT score distribution (top panel) and quintile of the PCF distribution (bottom panel). We show the "true" allocation (made by the chiefs, solid line) as well as two counterfactual allocations: the PMT allocation, our "benchmark" for what could be done under centralization; and the PCF-based allocation, the "optimal" allocation from the point of view of need targeting. We pool across villages, which vary in their underlying distributions as well as in the number of subsidies available, which explains why neither of the two counterfactual allocations are perfect step functions of their respective distributions. It also explains why even the PCF-based allocation does not reach perfect targeting: there appears to be substantial mistargeting across villages which explain that even a perfect allocation within village would yield evidence of mistargeting on Figure 2.1 since it is aggregated across villages. The PCF gradient of PCF-based allocation in Figure 2.1 should therefore be considered as the "best possible targeting" given the across-village allocation in our data.[17]

From the top panel, it is clear that chiefs target different people than the PMT would: while the PMT, by definition, would allocate subsidies to 100% of people at the bottom of the distribution, the chiefs' allocation has a much flatter gradient with respect to the PMT score. In isolation, this result looks similar to Dorward et al. (2008, 2013) and Kilic et al. (2013), who look at how well chiefs target based on assets and conclude that there is widespread mistargeting.

The bottom panel, which show targeting based on PCF, also show that the PMT does better than chiefs – but the gap is much smaller than in the top panel. In the allocation decision of 2011, contemporaneous to the PMT formula data collection, the gradient for the PMT allocation is quite a bit steeper than that of the chiefs, but by 2012 a lot of the PMT edge has ebbed already, suggesting that the advantage of the PMT may be short-lived. While the PMT does better than the chiefs at least initially in terms of poverty-targeting, it still generates a substantial number of errors. This is true even if we use the PMT formula from the IHS3 rather than the one derived in our dataset (see Figure B.3). The relatively poor targeting performance of the PMT seems due to the fact that assets (the most

[17] While understanding the determinants of subsidy allocations across villages is of great interest, our data does not allow us to study this question.

important factor in the PMT) are a relatively poor proxy for need in our study context, because PCF eligibility is not time-invariant (the correlation between food expenditures across rounds is only 0.35 as previously discussed and shown in Table 2.1) and because there are important unobservables in the determinants of PCF.

4.3.2 Error rates

Table 2.2 shows the poverty-targeting error rate under the two allocation schemes (chiefs and PMT). The table shows the average village error rate (averaging first over individuals within villages, and then across villages). For these calculations, we include only those villages in which the probability of getting a subsidy is between 0 and 100% (so that targeting errors are possible).[18] Following Alatas et al. (2012), what we call the poverty-targeting error rate is the probability that a household is (1) eligible based on its position in the PCF distribution within the village; but (2) does not make it onto the actual beneficiary list (chief error) or on the counterfactual PMT beneficiary list (PMT error). Note that since the number of beneficiaries within the village is kept fixed in this exercise, this error rate also provides the probability that a household is (1) categorized as ineligible based on its position in the PCF distribution and (2) gets the subsidy. In other words, mechanically there are as many people who don't get the subsidy when they should (exclusion errors) as there are people who get the subsidy when they

[18]The table reports the probability of all or none of the villagers getting the subsidy. The odds that all villagers got the subsidy was 9,8% for the 2011 input subsidy, 14.8% for the 2012 input subsidy, and 4.9% for the 2012 food subsidy. In addition, the food subsidy was not given out in 4.9% of villages.

should not (inclusion errors). We also show what the expected error rate would be if subsidies were allocated randomly. These are calculated from a permutation test with 1,000 draws (the distribution of results for those are shown in Figure B.2). Finally, we also compute the squared error for each allocation.

We can see that both allocations make a significant number of errors compared to the PCF-based allocation, but that the PMT always performs better for input subsidies. The average error rate for the PMT is 10-12%, compared to 14-16% observed by chiefs (these differences are statistically significant). Perhaps a better metric for measuring error is mean squared error, which punishes error far from the eligibility threshold more than errors closer. We again find strong evidence that the PMT does better. Note that we consider somewhat of a "best-case" PMT: we assume perfect compliance with the allocation rule, ignoring potential implementation issues; what's more, our PMT formula is based on data that predates the consumption and subsidy allocation measures by only a year, while any actual PMT allocation rule would likely rely on older data in most years (since measuring household-held assets, a key component of the PMT formula, is costly, especially once households know their eligibility depends on their survey responses – see Besley 1990).

While chiefs do worse than the PMT, they do not seem to make random allocations (see Table 2.4 and Figure B.2). The simple error rate for the input subsidies is clearly worse than would most likely be obtained from random targeting (see Figure B.2), but the mean squared error is much lower, suggesting that chiefs trade PCF-eligible for ineligible only around the PCF cutoff. Chiefs also do better than random on the food subsidy, by both metrics.

An interesting pattern in these results is that, compared to the PMT, chiefs look worse at targeting the truly needy for the input subsidy than for the food subsidy. A central hypothesis of this paper is that this may be due to productivity targeting of the input, which we will argue is less relevant for food. We dive into this issue in detail in Section 5.

4.3.3 Who is favored and who is left out by chiefs?

Table 2.3 shows the results of a multivariate regression of subsidy receipt on background characteristics and village fixed effects. We show the covariates of actual receipt (the true allocation, made by the chiefs) in columns 1-4. In columns 5-8 we show the characteristics targeted under the counterfactual PMT. Comparing the coefficient estimates across the two sets of analyses tells us who is favored and who is left out under each scheme. We consider both the extensive margin (receiving any subsidy) and the intensive margin (value of the subsidy received, since this varies across households due to sharing).[19] The first row of Table 2.3 confirms the poverty-targeting results discussed above: the gradient in PCF is more negative under the PMT than under the chiefs, and the gap in the gradient is more pronounced for the input subsidy than for the food subsidy.

[19]For the intensive margin, we construct the counterfactual PMT allocation keeping the distribution of input subsidy values the same as under the chief, but assigning the largest value subsidy to the household with the lowest PMT score, the second largest value to the housheold with the second lowest PMT score, etc. This inflates the targeting performance of the PMT compared to allocating fixed subsidy values to every household eligible. This is the relevant benchmark insofar as there is no reason (other than logistical constraints) why subsidy amounts under the PMT cannot be varying with the PMT score.

We find evidence of nepotism: conditional on covariates, chief's kin are 11 percentage points more likely to receive the food subsidy under the chief, whereas they would not be favored under the PMT. For the input subsidy, nepotism appears much less pronounced: while chief's kin receive a greater input subsidy package (an extra 3.30 kg off of a mean of 50.5 kg, significant at 10%), the PMT would also award kin higher subsidy packages (+2.3 kg, also significant at the 10 percent level). This is due to the fact that chiefs' kin are marginally asset poorer than non-relatives. Turning to other covariates, we find that chiefs target older households, as per the official FISP recommendation. Chiefs also target households that received negative shocks: households who experienced a drought or flood are 4 percentage points more likely to receive subsidized food, while households who experienced crop loss or cattle death are 8 points more likely to get it.

4.4 Discussion of Poverty Targeting Results

Summing up, the results in Table 2.3 epitomize the tradeoff between local information and capture: we find that chiefs are able to use local knowledge to benefit households hit by recent negative shocks, while the PMT misses them; but they also favor their kin. These results raise several questions.

First, are food expenditures a good measure of need? There is undoubtedly measurement error in food expenditures, which would tend to flatten the gradient in PCF for both the chiefs and the PMT. In addition, our measure is based on expenditures, not consumption, and consumption will depart from expenditures if people receive transfers. In Table B.7 we show fixed effects regressions of our primary measure of non-staple food expenditures on other measures of poverty, including total (measured) expenditures,[20] total food expenditures on all categories, the share of food in expenditures, the percent of days in which respondents report not having enough food, and an indicator equal to 1 if a respondent reported being worried about food. All measures are strongly correlated, with p-values less than 0.01. The (within) R-squared for total and food expenditures is 0.37-0.39, though lower for other measures.

We redo our main analysis using total expenditures and total food expenditures instead of non-staple food expenditures. Error rates and mean squared errors are shown in Table B.8. Results look qualitatively similar, though error rates for chiefs are higher with these measures than for non-staple food. Our interpretation of the difference between these results and the previous set is that chiefs are able to target people who are poor, even conditional on total expenditures. Indeed, as mentioned in footnote 20 our expenditures module did not include all possible expenditures categories. To the extent that Engel curves are not linear (see Ligon 2017 for evidence that they are not among rural households in Uganda), and the categories we did not measure represent a higher share of total expenditures for relatively richer households, our measure of total expenditures may be less informative about household's relative neediness than expenditures on an exhaustively measured list of items in a subcategory (e.g. food).

We also re-do our analysis of the correlates of subsidy receipt in Table B.9, using these alternative measures of expenditures. Results are unchanged: we still find

[20] Our expenditures module did not include all possible expenditures categories, therefore our measure of total expenditures is only total over measured categories.

strong evidence that chief's kin are favored. Note that for the kin variable to be biased, it would need to be the case that the correlation between food expenditures and true need is correlated with kinship. While this is theoretically possible, we see no reason to expect it to be the case, given that kin and non-kin look very similar on a host of baseline characteristics (Table 2.1, Columns 3-4).

Second, is the fact that kin are more likely to get subsidies evidence of nepotism? An alternative hypotheses is that chiefs have better information on relatives, and therefore are more likely to target kin because they can be certain that they are truly poor. If this is the case, we would expect that subsidies to kin would be more responsive to consumption than to non-relatives. We investigate this in Table B.10, in which we include an interaction between log food and kinship. We find no evidence in favor of the information hypothesis: targeting actually appears somewhat worse for relatives for the input subsidy, though there is no effect for the food subsidy. While we lack data to definitively rule out an information story, our evidence appears more consistent with nepotism.

Third, how important is the mistargeting to relatives? Alatas et al. (2013) show that the "cost" of nepotism in terms of average consumption level among beneficiaries can be approximated with the following formula:

$$\triangle C = \alpha \frac{\triangle\beta}{\beta} \frac{(c_e - c_b)}{c_b}$$

where α is the share of kin, $\frac{\triangle\beta}{\beta}$ is how much more likely kin are to receive benefits, and $\frac{(c_e - c_b)}{c_b}$ is how much richer kin are. Taking the following values from our data: $\alpha = 0.27, \frac{\triangle\beta}{\beta} = 0.19$ (for the food subsidy) and $\frac{(c_e - c_b)}{c_b} = 0.053$, we obtain

$\triangle C \approx 0.0027$. In other words, nepotism in the allocation of the food subsidy reduces consumption among the truly eligible by 0.26%. This is a very small cost, in fact surprisingly similar to that obtained by Alatas et al. (2013) for Indonesia ($\triangle C \approx 0.003$ for a cash transfer program). The main reason why nepotism is not very costly in terms of consumption targeting is that kin and non-kin are equally poor in our sample. Nepotism could however mean that kin can achieve the same level of consumption alongside much more leisure (if they do not need to work as hard to achieve the consumption). The distributional impacts of nepotism in terms of overall welfare could thus be non-trivial if leisure is valued highly.

5 Productive Efficiency Targeting

In this section, we investigate whether some of the apparent mistargeting of input subsidies by chiefs is due to targeting on farming productivity: if returns to input subsidies are heterogeneous and chiefs have information on this, then they might allocate subsidies in a way that takes both poverty targeting and productive efficiency into account. In this section we present a model that allows for heterogeneity in returns as well as heterogeneity in the welfare weights that chiefs assign to households, and propose a method to test whether the mistargeting we observe for input subsidies is in part driven by productive efficiency considerations.

5.1 Model and prediction

We consider the problem of allocating subsidies across households within a village.

The intra-village allocation is done by the village chief.

Suppose that allocation of subsidy s ($s \in \{\text{fertilizer, food}\}$) to household i enables that household to generate additional income:

$$y_i = A_{is} s^{\mu}$$

where A_i denotes individual-specific returns to the subsidized resource and $\mu \in (0, 1)$ denotes potentially diminishing returns in the subsidized resource. In the nested special case where the subsidized resource is food, rather than farming inputs, we set $\mu = 1$ and $A_{is} = 1$ for all households (and thus start by abstracting away from a case in which there is a productive response to nutrition – we relax this assumption later).

We assume that households share a common homothetic, CRRA utility function defined over total income:

$$u_i = \frac{(y_i + e_i)^{1-\rho}}{1 - \rho}$$

with $\rho > 0, \neq 1$ and where e_i is the income that household i gets in addition to the subsidy-enabled income.

The aggregate supply of subsidies to the village is denoted by $\bar{s}$. Under a proxy-mean test, the subsidies would go to the $\bar{s}$ households in the village with the lowest PMT score. In contrast, when allocating subsidies across households within the village, and assuming for now that there is no *ex post* redistribution orchestrated by the chief, the chief chooses the subsidy levels s_i so as to maximize the weighted sum of villagers' utility:

$$\sum \omega_i \frac{(A_{is} s_i^{\mu} + \hat{e}_i)^{1-\rho}}{1-\rho} \tag{1}$$

subject to

$$\sum_i s_i = \bar{s}$$

In equation 1, $\hat{e}_i$ is the income that the village chief expects household i to have at the time the subsidy benefits are realized, and ω_i is the relative welfare weight of household i. Since chiefs do not face reelection incentives and have limited accountability (see section 2.1), the relative welfare weight of a household may not reflect its role in the political process as in earlier models (Bardhan and Mookherjee, 2000, 2003, 2006) but may instead depend on the preferences of the chief (e.g. if the chief favors his kin, the relative welfare weight of kin will be higher).

While $\hat{e}_i$ could be endogenous, we assume that the chief can take the households' best response distribution of $\hat{e}_i$ as given when maximizing the objective function shown in 1.

Taking the first order conditions for input subsidies ($s = fert$) for two households i and j yields:

$$\omega_i \left(A_i fert_i^{\mu} + \hat{e}_i\right)^{-\rho} A_i fert_i^{\mu-1} = \omega_j \left(A_j fert_j^{\mu} + \hat{e}_j\right)^{-\rho} A_j fert_j^{\mu-1} \tag{2}$$

For food subsidies, where $A = 1$ and $\mu = 1$ for all households, we have an analogous but simplified expression:

$$\omega_i \left(food_i + \hat{e}_i\right)^{-\rho} = \omega_j \left(food_j + \hat{e}_j\right)^{-\rho} \tag{3}$$

Taking the ratio of 2 over 3, the welfare weights cancel and we obtain:

$$\frac{\left(A_i fert_i^{\mu} + \hat{e}_i\right)^{-\rho} A_i fert_i^{\mu-1}}{\left(food_i + \hat{e}_i\right)^{-\rho}} = \frac{\left(A_j fert_j^{\mu} + \hat{e}_j\right)^{-\rho} A_j fert_j^{\mu-1}}{\left(food_j + \hat{e}_j\right)^{-\rho}} \tag{4}$$

From this expression we can derive the relationship between a household's productivity parameter A_i and the difference in value between the fertilizer and the food subsidy that that household receives $(fert_i - food_i)$. In Figure B.4, we plot that relationship setting $\mu = 0.9$ and either $\rho = 0.5$ or $\rho = 1.2$. The relationship is positive: as the returns to fertilizer increase, a household receives relatively more fertilizer subsidies than food subsidies. The intuition here is the following: if productivity considerations matter, then if a household has a higher return to the fertilizer subsidy than average, then that household should be relatively more favored when it comes to the input subsidy than for the food subsidy. Unsurprisingly, when the utility function is very concave ($\rho = 1.2$), the impacts of productive efficiency considerations is considerably muted, since increases in the resources of the already better off have lower value.

This leads us to the prediction we can test in the data:

If chiefs take into consideration productive efficiency when allocating farming subsidies, $d\left(fert_i - food_i\right)/dA_i > 0$*. Namely, the higher the return to fertilizer for a household, the higher the gap between fertilizer and food subsidies received by that household.*

Below we show that this prediction holds under a number of extensions to the basic model.

As shown in Table 2.1 Panel C, there is a significant amount of transfers between households within the village. In the presence of a redistribution instrument, the chief's objective function would be modified as follows: the chief now chooses the sets of subsidies s_i and transfers t_i so as to maximize:

$$\sum_i \omega_i \frac{(A_i s_i^{\mu} + t_i + \hat{e}_i)^{1-\rho}}{1-\rho}$$

subject to

$$\sum_i s_i = \bar{s}$$

$$\sum_i t_i = 0$$

where t_i is the net ex-post income transfer received by household i, which can be either negative or positive.

It is evident that redistribution will allow chiefs to target productivity more than the autarkic case. Thus the more rdistribution is possible, the greater the optimal wedge between the fertilizer and the food subsidy a given household receives. In the extreme case in which income is fully pooled, the objective function of the chief can be rewritten as $\max \sum_i \beta_i (A_i s_i^{\mu})$. In this case, the allocation of fertilizer subsidies will be entirely driven by productive efficiency since redistribution will happen ex post.[21]

[21] The two subsidies we study could be complementary: the input subsidy as a growth instrument and the food subsidy as a redistribution instrument. This is an interesting insight which suggests that the introduction of the food subsidy may lead to an increase in the extent to which the input subsidy can be used by chiefs as a growth instrument going forward. Note that this does not invalidate our test: since the food allocation at any point in time should be based on

It is possible that food subsidies increase productivity for very poor households due to improved nutrition (Strauss, 1986). Such a nutrition-productivity link would not change the prediction. To see this, note that allowing for the efficiency of an hour worked to increase with food subsidies implies a negative correlation between the relative productivity of inputs and the relative productivity of food, given the complementarity between farm inputs and efficient labor units. This increases $d\,(fert - food)\,/dA$.

In many African countries, rural economies are poorly connected to markets and thus local prices may be responsive to local output. If so, allocating subsidies to the most productive may reduce prices by increasing output. Since 90% of farmers in our sample are *net buyers* of grain (in other words, they consume more grain than they produce), such a price effect would translate into a positive income effect for most villagers and thereby increase welfare. This increases $d\,(fert - food)\,/dA$ for any ρ because allocating inputs to households with higher returns increases the welfare of the rest of the village through lower prices.

the pareto weight and current consumption – irrespective of whether the current consumption level was secured through enhanced yields in the previous period thanks to inputs subsidies or not – relative pareto weights can still be backed out from jointly observing the food allocation and current consumption, as we do. Also, in our data, the food subsidy was announced after the 2012 input subsidy allocation had been decided.

5.2 Results

To test the prediction, we need a measure of A, the household (farm) specific productivity of fertilizer. In this or any context, estimating the productivity of an input is very difficult, since input choices are endogenous and farmers with higher returns are presumably more likely to use fertilizer in a given season. Returns are also volatile across years and even within farms, so estimating this well would typically require a long panel.

Instead of estimating productivity, we therefore opted to simply ask farmers for their expectations of yields with and without fertilizer use. We collected this data in the fifth survey round conducted in the summer 2014. There are several important caveats. First, due to budget constraints the survey could only be done with a random subset of households in each village. The sample includes only about one third of the sample. Second, the questions are about total output with and without fertilizer, rather than marginal returns. The main issue here is that farmers may have bought some fertilizer even in the absence of the subsidy, and so some of the transfer may be inframarginal. Another potential issue would be that returns might be concave in quantities. We argue this is less true here than in most settings, since the size of the subsidy does not cover the whole farm. Farmers typically use a given amount of fertilizer per plant, so that an increase in inputs would involve an increase in acreage under fertilizer rather than an increase in fertilizer per plant. While it still may be the case that fertilizer is allocated to parcels of land with higher returns first, we argue that average and marginal returns are likely correlated. Nevertheless, we acknowledge that these measures are not perfect.

We show the means of the reported expected yield in panel C of Table 2.1, and we plot the distribution of the reported gain in total output in Panel B of Figure B.1. There is substantial heterogeneity in these reported gains from input use. What drives it? Table B.11 examines correlates of yield increases with inputs. We regress the log yield increase on log acres and other observables. We find that yield increases are correlated with many variables, including household demographics (yield increases are increasing in the age of the head of household), education, log assets, and household size (though this is not statistically significant). We expect that these are the types of proxies that the chief may use to allocate subsidies, in addition to other characteristics that are unobservable to us, such as land quality. Also of note is that the correlation between estimated production gains from using fertilizer are and our measure of neediness, PCF, is fairly weak (Panel C of Figure B.1). We also find no systematic differences by kinship status (Table 2.1 Panel C, column 3, and Table B.11).

To test for productive efficiency targeting, in Table 2.4 we regress the value of the fertilizer and food subsidies received, as well as their gap ($fert_i - food_i$), on the log of reported gains in output when using fertilizer. We find clear evidence in favor of targeting based on productive efficiency: the value of the input subsidy received increases significantly with the reported gains from fertilizer use. The food subsidy, by contrast, is not correlated with gains. When we look at our primary outcome – the gap betwen the two – we find that the gap increases significantly with the gain from fertilizer, as predicted by the model. In Figure

2.2 we plot the estimated relationship between the subsidy values and the gain when using a quadratic instead of log. The positive slope for input subsidy values under the chief's allocation is very clear, compared to the flat relationship for food subsidies.

These results are in sharp contrast with those for the counterfactual PMT distribution, in which the value of the subsidy is actually (insignificantly) declining in the gains to fertilizer (because of a negative correlation between returns and assets). In that case, the PMT undermines the effect of the subsidy on total farm output at the village level. In contrast with the chief's allocation, the gap between fertilizer and food subsidy values does not significantly increase with reported gains from fertilizer under the PMT allocation (Table 2.4, column 6).[22] Overall, the results in Table 2.4 and Figure 2.2 are consistent with chiefs taking productive efficiency into consideration when allocating input subsidies – something that the PMT cannot do since information on who has more to gain from

[22]The analysis in Table 2.4 only controls for log farm size, log PCF, and relation to chief in this regression: we omit other household controls such as demographics, since these controls themselves are predictors of log gains as shown in Table B.11. Including all other controls attenuates coefficients (see Table B.12), causing the relationship between yield increases and the value of the input subsidy to lose significance at conventional levels (p=0.16). However, the gap in coefficients on log gains between the chief and conterfactual PMT allocation remains large, and for our primary outcome – the value gap between the fertilizer and food subsidies, the object of the model's prediction – the coefficient on log gains remains large and significant at 5% for the chief's allocation and small and insignificant under the PMT. The results are also robust to controlling semi-parametrically for farm size instead of using a log-linear specification (Panel A of Table B.13). Panel B of Table B.13 uses *per acre yield gain* as the independent variable of interest. The results are weaker than for total yield gain, which we argue is not surprising since the per acre yields are not as important as total gains (since on small enough plots of land, high returns do not translate into large effects on total income). Nevertheless, the coefficient is of the correct sign. Note that we cannot regress input per acre on yield gain per acre because this would generate very serious division bias and hence a spurious, very significant relationship between the two.

fertilizer use is not something that can be elicited in an incentive-compatible way if people expect their subsidy package to depend on it. The magnitude of the effects is not trivial: a household with an extra log point gain from fertilizer gets about 6.5 more kgs of input subsidies under the chiefs than under the PMT.

Chiefs appear to have substantial power to enforce redistribution (as evidenced by the fact that they control how subsidy packages are shared among villages). This redistributive power may be what allows chiefs to use the input subsidies as a growth instrument, bringing their village closer to the production possibility frontier, and then enforcing sharing of food after harvest. Interestingly, allocating input subsidies based on returns is not what they are asked to do. The official guidelines of the inputs subsidy program is to target the poor, and thus when asked chiefs report targeting the poor rather than taking productivity into account (see Table B.3) – even though our careful analysis of their allocation decision suggests that they do.

As above, another question is whether chiefs have better information on relatives. We check this in Table B.14 in which we include interactions between productivity and kinship. We find little evidence of better targeting towards relatives.

5.3 Supportive evidence

Is information on the relative productivity of various potential beneficiaries of the input subsidy embedded in the chief, or does it rest in the people themselves? People who have high value for the input subsidy could wait in line more, lobby more or protest more if they don't get the subsidy, such that the allocation of the chief ultimately favors them in a way that looks as if the chief was himself aware of the heterogeneity.

Disentangling whether the information rests in the chief or can be elicited in an incentive-compatible way from the people is interesting, since a PMT scheme could possibly achieve some level of productive efficiency if the lobbying made to the chief was made to the outside government agent coming to the village to distribute the subsidy vouchers based on the PMT. Yet the outside government agent would not be able to know the level of redistribution in the village; that is, she would not be able to gauge the "poverty mistargeting" cost of responding to the lobbying and targeting based on productive efficiency.

To provide descriptive evidence on this question, in the 2014 survey, we asked respondents if they had ever lobbied the chief to obtain subsidies. Only nine percent of respondents reported lobbying for input subsidies, and 4% reported lobbying for food subsidies (Table B.3). The likelihood of having lobbied is not positively correlated with returns to fertilizer overall (see Table 2.4, column 7). It is among chiefs kin (see Table B.14, column 7), but those lobby much less on average, and overall the targeting efficiency is not higher among kin as shown in Table B.14 columns 1-3.

In the survey of chiefs also conducted in 2014, we asked chiefs a number of questions about what they could observe about households, which we present in Appendix Table B.15. We find that 86% of chiefs report that they can easily categorize farms in their villag in terms of productivity of inputs. Chiefs also report that they know who works harder, who has money for inputs, and whose returns are highest. While descriptive, these responses suggest significant local knowledge on the part of chiefs.

6 Conclusion

Traditional leaders, often known as "chiefs," have maintained a significant amount of *de facto* if not *de jure* power in sub-Saharan Africa. Possibly owing to the weakness of local governance in most of the continent, chiefs are commonly involved in the decisions of how to allocate government resources. One prominent type of resource is subsidies. Developing country governments allocate an important portion of their national budget to subsidies targeted at the poor, and it is common for chiefs to be asked to identify who should be eligible for such subsidies. Do chiefs identify the right beneficiaries? Previous work on this question concluded that there was widespread elite capture (Dorward et al., 2008; Kilic et al., 2013). These conclusions are based on evidence that "connected" households are more likely to receive subsidies, and that household assets measures do not strongly predict subsidy receipt. We show that such evidence may not directly speak to the issue of poverty-targeting in environments where assets are a poor predictor of need, and where the subsidized items are productive inputs.

Using detailed food expenditure and shocks data to better proxy for neediness, we show that since chiefs' kin are no better off than non-kin, the nepotism that is evident in the data does not imply greater poverty mistargeting. Chiefs do make more errors than a perfectly implemented PMT scheme would, but the gap reduces over time as the information used for the PMT becomes less accurate due to frequent shocks, while chiefs appear able to target based on such shocks. Importantly, chiefs also appear to allocate input subsidies to farmers with larger returns to input use. This result underscores how a naive measure of targeting based solely on the neediness of households (even when needinesss is well measured) may understate the poverty-alleviation impacts of the allocation: when ex post redistribution is possible through informal transfers, targeting input subsidies based on productive efficiency (i.e. using input subsidies as a *growth instrument*) can have a larger impact on aggregate welfare than targeting based on poverty would. This issue has not received much attention in the literature up to this point, even though most of the inputs subsidized by governments are productive (farming inputs, health products) that have heterogeneous returns. Future work should explore whether our results generalize to other contexts and countries.

Table 2.1. Summary Statistics on Households in the sample

	(1)	(2)	(3)	(4)	(5)	(6)
	Overall Mean	Std. Dev.	Difference kin vs. non-kin		Correlation between rounds (all)	Correlation between rounds (kin only)
Panel A. Time-Invariant Baseline Variables			Diff.	Std. Err.		
Related to chief ("Kin")	0.27	-				
Mud/dirt floor	0.90	-	0.02	0.02		
Thatch roof	0.77	-	0.01	0.02		
Has electricity in dwelling	0.006	-	0.002	0.004		
Reads or writes chichewa	0.58	-	-0.07	0.029*		
Years of education	4.86	3.54	-0.50	0.205*		
Widowed or divorced female	0.29	-	0.03	0.03		
Household size	4.57	2.07	-0.06	0.12		
Number of children	2.49	1.72	-0.06	0.10		
Respondent age	40.14	17.09	0.50	0.99		
Owns land	0.97	-	0.03	0.01*		
If yes, acres of land owned	2.36	1.96	0.19	0.11		
Value of durable assets owned (USD)	98.04	384.06	-11.27	22.32		
Value of animals owned (USD)	36.76	105.51	-2.43	6.15		
Number of households	1559					
Panel B. Time-varying Variables						
Total expenditures per capita (monthly)[1]	9.66	10.85	-0.476	0.313	0.45	0.43
Total food expenditures per capita (monthly eq.)	6.80	7.77	-0.349	0.224	0.35	0.35
PCF: Total non-staple food expenditures per capita (monthly eq)	2.71	3.45	-0.186	0.099**	0.38	0.35
Shocks						
Experienced drought or flood (past 3 months)	0.28	-	0.005	0.013	-0.33	-0.33
Experienced cattle death or crop disease (past 3 months)	0.20	-	0.013	0.012	0.04	0.06
Respondent missed work due to illness (past month)	0.26	-	-0.002	0.015	0.16	0.11
Other household member was sick (past month)	0.69	-	0.007	0.013	0.16	0.18
Report being worried about having enough food to eat (past month)	0.72	-	-0.023	0.012	0.14	0.13
Share of days with enough food to eat	0.67		0.004	0.016	0.19	0.18
Informal redistribution						
Received transfers from other villagers in past 90 days	0.58		-0.017	0.014	0.11	0.14
Made transfers to other villagers in past 90 days	0.25		-0.003	0.013	0.07	0.06
Number of observations	6236					
Number of households	1559					
Panel C. Reported returns to fertilizer (2014 Survey)						
Self-reported total production without fertilizer use (50 kg bags)	3.87	2.62	0.25	0.25		
Self-reported total production with fertilizer use (50 kg bags)	18.48	9.41	0.42	0.87		
Gain in production from using fertilizer (50 kg bags)	14.50	8.05	0.20	0.76		
Gain in production from using fertilizer (50 kg bags), per acre	7.83	4.92	0.20	0.47		
Number of households	532					

Note: All monetary amounts are in US dollars. Years of education is highest in the household (husband or wife).

[1]Expenditures are winsorized at the 99th percentile.

Table 2.2: Targeting errors: Comparison of chief and PMT allocations with consumption-based allocation

	(1)	(2)	(3)
	2011 Input Subsidy	2012 Input Subsidy	2012 Food Subsidy
Percentage of population receiving subsidy	0.753	0.812	0.586
Percentage of villages in which 0% received subsidy	0.000	0.000	0.049
Percentage of villages in which 100% received	0.098	0.148	0.049
If between 0 and 100%			
Simple error rate under following allocation mechanism:[1]			
Chief (True allocation)	0.161	0.140	0.151
PMT (Counterfactual)	0.108	0.104	0.141
PMT (Counterfactual) based on IHS3 formula	0.124	0.116	0.145
Random (Counterfactual)	0.153	0.126	0.162
P-val Chiefs = PMT	<.001	0.003	0.365
P-val Chiefs = PMT (IHS3)	0.002	0.031	0.585
P-val Chiefs = Random	0.479	0.227	0.266
P-val PMT = Random	<.001	<.001	<.001
Mean squared error in log consumption under following allocation mechanism:[2]			
Chief (True allocation)	0.497	0.269	0.375
PMT (Counterfactual)	0.199	0.126	0.193
PMT (Counterfactual) based on IHS3 formula	0.282	0.154	0.250
Random (Counterfactual)	0.543	0.802	1.082
P-val Chiefs = PMT	<.001	0.005	0.057
P-val Chiefs = PMT (IHS3)	0.436	<.001	<.001
P-val Chiefs = Random	<.001	<.001	<.001
P-val PMT = Random	<.001	<.001	<.001

Notes: IHS3 = Malawi Third Integrated Household Survey, a representative survey conducted by Malawi's National Statistical Office from March 2010 to March 2011.

[1]Error rate is defined as the percentage of people who received the subsidy and shouldn't have. Since the total number of beneficiaries is fixed, this error rate is equal to the percentage of people who didn't receive the subsidy and should have.

[2]Mean squared error is calculated as deviations from the log PCF threshold.

Table 2.3. Multivariate correlates of Subsidy Receipt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Actual (Chief's) allocations				Counterfactual PMT allocation			
	Got input subsidy	Value (USD)	Got food subsidy	Value (USD)	Eligible for input subsidy	Value (USD)[a]	Eligible for food subsidy	Value (USD)[a]
Log PCF (total non-staple food expenditures per capita in past month	-0.01	-0.99*	-0.03**	-2.12*	-0.04***	-5.54***	-0.04***	-4.08***
	(0.01)	(0.59)	(0.01)	(1.21)	(0.01)	(0.50)	(0.01)	(0.77)
Time-Invariant Baseline Variables								
Related to chief	0.04	3.24*	0.12***	10.87***	-0.01	1.87	0.04	2.83
	(0.02)	(1.71)	(0.03)	(2.89)	(0.02)	(1.19)	(0.03)	(1.75)
Log (acres farmed)	0.04**	5.58***	0.02	1.9	-0.05***	-6.00***	-0.09***	-9.24***
	(0.02)	(1.14)	(0.02)	(1.48)	(0.02)	(1.05)	(0.01)	(0.93)
Years of education (divided by 10)	-0.14	-16.03	-0.1	-22.52	-0.13	9.08	-0.12	-8.24
	(0.31)	(22.19)	(0.31)	(20.15)	(0.19)	(20.59)	(0.31)	(16.13)
Widowed or divorced female	0.02	2.7	-0.06	-3.38	-0.35***	-	-0.29***	-
	(0.03)	(2.66)	(0.05)	(4.10)	(0.03)	(2.16)	(0.04)	(3.19)
Household size (divided by 10)	0.02	0.52	0	1.94	0.05*	9.92***	0.12***	15.90***
	(0.02)	(1.60)	(0.03)	(2.88)	(0.03)	(1.67)	(0.03)	(2.28)
Respondent age: 2nd quartile (26-35)	0.09*	6.86	-0.04	-0.69	0.44***	47.01***	0.53***	72.39***
	(0.05)	(4.26)	(0.05)	(5.55)	(0.07)	(5.06)	(0.08)	(7.98)
Respondent age: 3rd quartile (36-51)	0.12***	8.78***	0.06*	4.32	0.05	1.89	0.07**	0.16
	(0.03)	(2.41)	(0.03)	(3.04)	(0.03)	(1.46)	(0.03)	(2.07)
Respondent age: highest quartile (over 52)	0.17***	13.20***	0.12**	11.34***	0.09**	6.40***	0.11***	5.38*
	(0.04)	(2.90)	(0.05)	(3.96)	(0.04)	(2.05)	(0.04)	(2.85)
Log (value of animals owned)	0.21***	15.00***	0.24***	22.01***	0.11***	14.47***	0.20***	23.47***
	(0.04)	(2.80)	(0.05)	(4.33)	(0.04)	(2.21)	(0.04)	(2.89)
Shocks								
Experienced drought or flood (past 3 months)	0.00	1.17*	-0.01	0.74	-0.03***	-2.05***	-0.02**	-2.52***
	(0.01)	(0.67)	(0.01)	(0.88)	(0.01)	(0.57)	(0.01)	(0.80)
Experienced cattle death or crop disease (past 3 months)	0.04**	-0.85	0.08**	5.45*	0.04	-0.38	0.03	1.93
	(0.02)	(1.50)	(0.03)	(2.74)	(0.03)	(1.56)	(0.03)	(2.59)
Number of Observations	3094	3043	1559	1559	3118	3043	1559	1559
Number of Households	1558	1558	1559	1559	1559	1558	1559	1559
Number of Villages	61	61	61	61	61	61	61	61
Mean of dependent variable	0.78	50.47	0.59	42.03	0.78	50.47	0.59	42.03
Years	2011 & 2012		2012	2012	2011 & 2012		2012	2012

Note: Regressions for input subsidies pool years 2011 and 2012 and control for the year. Omitted age category is less than 26. Standard errors clustered at the village level. All regressions include village fixed effects.

[a] Counterfactual quantities have the same distribution as actual quantities.

* significant at 10%; ** significant at 5%; *** significant at 1%

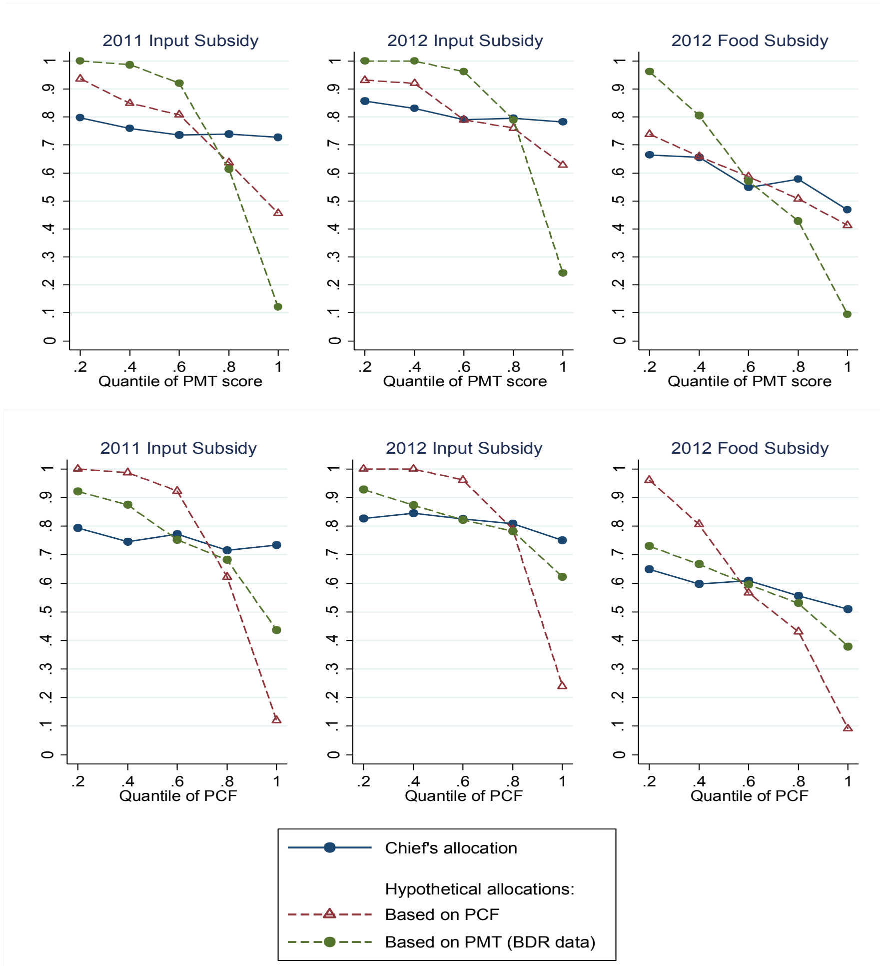
Table 2.4. Productive efficiency

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Actual (Chief's) allocations			Counterfactual PMT allocation			
	Value (USD) of input subsidy	Value (USD) of food subsidy	Value gap (input-food)	Value (USD) of input subsidy under PMT[a]	Value (USD) of food subsidy under PMT[a]	Value gap (input-food) under PMT	Ever Lobbied Chief to try to get Input Subsidy
Log (gain in farm production from fertilizer use)	4.04**	0.23	8.16**	-2.49	-3.97	2.35	0.00
	(1.70)	(2.93)	(3.44)	(2.54)	(3.28)	(2.83)	(0.03)
Log (total non-staple food expenditures per capita in past month)	0.01	-0.58	0.23	-11.46***	-10.92***	1.87	0.01
	(0.73)	(1.57)	(2.14)	(1.06)	(1.96)	(1.71)	(0.01)
Time-Invariant Baseline Variables							
Related to chief	2.34	10.74***	-7.27	7.34***	7.87*	-0.96	0.03
	(2.95)	(4.02)	(5.37)	(2.48)	(4.17)	(3.82)	(0.03)
Log (acres farmed)	6.14**	-1.28	6.28	-2.87	-7.23***	3.86**	0.00
	(2.58)	(2.87)	(3.77)	(2.45)	(2.71)	(1.84)	(0.03)
Number of Observations	1048	530	529	1048	530	529	530
Number of Households	530	530	529	530	530	529	530
Number of Villages	61	61	61	61	61	61	61
Mean of dependent variable	51.83	37.78	11.94	52.96	40.53	9.26	0.089
Years	2011-2012	2012	2012	2011-2012	2012	2012	

Note: Sample restricted to households surveyed in 2014 and asked about perceived returns to fertilizer use. Regressions for input subsidies pool years 2011 and 2012 and control for the year. 2011 input allocation information comes from 2011 survey. 2012 input and food allocations information comes from 2012 survey. Omitted age category is less than 26. Standard errors clustered at the village level. All regressions control for village fixed effects.

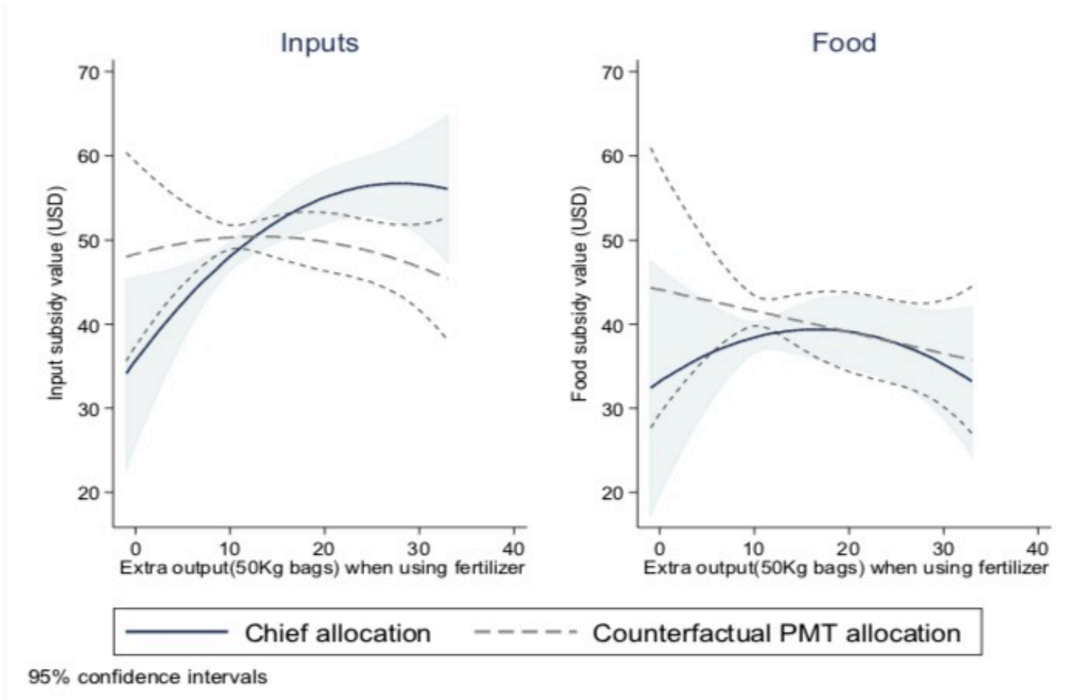
* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 2.1. Comparing chiefs' allocation to counterfactual allocations



Notes: See main text section 4.2. The PMT formula is obtained using 2011 data. The PCF is contemporaneous of the subsidy allocation decision. The chief allocation is the allocation observed, made by chiefs. Because the share of households that receive subsidies vary across villages, the threshold PMT (PCF) score for eligibility varies across villages, which explains why the allocations by PMT (PCF) quantile are not either 1 or 0.

Figure 2.2. Productive Efficiency of Chief's Allocation: Polynomial Estimates



Notes: 2012 data. Estimates from OLS regressions with second-order polynomial in the variable shown on the x-axis as well as controls for PCF, log land size, and chief kinship.

Table B.1. Summary statistics on villages and chiefs in the sample

	(1)	(2)
	Villages (N=61)	
	Mean	SD
Panel A. Characteristics of Villages		
Village population	3169	4149
Number of households in village	309	333
Number of family clans in village	63	186
Total acres of customary land in village	7640	7294
	Village Chiefs (N=79)	
	Mean	SD
Panel B. Characteristics of Chiefs		
Age	53.96	14.99
Male	0.82	
Years of education	5.25	3.54
Religion		
Christian	0.61	
Muslim	0.39	
How were you selected to be chief?		
Hereditary	0.90	
Appointed	0.09	
Elected	0.01	
For how many years have you been chief?	13.24	13.23
For how many years have you lived in this village?	44.90	17.25
For how many years have you farmed the land you currently farm?	24.19	14.70
At the time you became chief, was there someone else considered for the position?	0.05	
Do you receive a payment *(mswahala)* from the government?	0.89	
Have you ever been suspended from your position as village head	0.01	
Describe your responsibilities as village chief		
Solve conflicts among villagers	0.90	
Report to group village headman and traditional authority	0.61	
Monitor village projects	0.56	
Disseminate information to villagers	0.33	
Oversee subsidy programs	0.20	
Preserve local traditions	0.14	
Demarcate and supervise use of customary land	0.06	
Supervise government laws	0.01	

Notes: Data from surveys conducted from August to October 2014 in the study districts.

Table B.2. Chiefs' role in the allocation of subsidies

	(1) Input subsidy	(2) Food subsidy
Panel A. Surveys of Chiefs in 2014 (N = 79)		
Who decides which households in the village will be beneficiaries of the subsidy program?		
Village head (chief) alone	0.62	
Village Development Committee alone	0.13	
Village meeting	0.13	
District agricultural officer alone	0.06	
Group village head alone	0.04	
Chief in consult with others	0.03	
What selection criteria are used to allocate vouchers in your village? (multiple answers possible)		
Neediness	0.97	1.00
Absence of male head	0.62	0.54
Recent negative shocks	0.54	0.34
Child headed households and households taking care of orphans	0.24	0.20
How hard-working the household is	0.16	-
Farm size	0.11	-
Elderly, disabled, or chronically ill	0.05	0.76
Land quality	0.01	-
Panel B. Survey of villagers in 2013 (N = 1,381)		
The chief organized a meeting to talk about the program	0.95	0.81
If yes, did you attend the meeting?	0.82	0.65
At the meeting, was there a discussion about:		
Who should be included in the program?	0.77	0.81
Sharing the subsidies (i.e.: who should share with who, how much should be shared)?	0.75	0.71
Panel C. Survey of villagers in 2014 (N=542)		
Have you ever made a payment to the chief? (not specific to subsidy)	0.44	
Who decides which households in the village will be beneficiaries of the subsidy program?		
Village head (chief) alone	0.49	0.73
Chief in consult with others	0.23	0.04
Village meeting	0.15	0.02
Village Development Committee alone	0.10	0.09
District agricultural officer alone	0.01	-
NGO alone	-	0.08
Group village head alone	0.02	0.03
Other	0.01	0.02
Have you ever asked the village head to give you an input subsidy voucher?	0.09	0.03
Have you ever complained to the village head about the allocation?	0.16	0.05
What selection criteria are used to allocate vouchers in your village?		
Neediness	0.71	0.88
Elderly, disabled, or chronically ill	0.46	0.75
Child headed households and households taking care of orphans	0.16	0.29
Absence of male head	0.12	0.37
Recent negative shocks	0.10	0.34
How hard-working the household is	0.13	-
Farm size	0.01	-
Households with more children	-	0.32
Households with poor land	-	0.27
Households not receiving other subsidies	-	0.26
Do you think the subsidy is allocated in a good way?		
Very good	0.49	0.67
Somewhat good	0.36	0.27
Not so good	0.13	0.05
Very bad	0.02	0.01
What is your definition of a "good" allocation? An allocation that...		
... benefits the poorest	0.47	0.65
... increases total village production so that there is more food to share	0.07	-
... rewards those who work hard	0.03	-
... provides at least something to most households	0.37	0.58
... benefits those not receiving subsidies from other programs	-	0.26
On a scale from 1 to 5, how much do you agree with the selection of subsidy beneficiaries?	3.65	3.96

Notes: Panel A and C come from surveys administered in August-October 2014. Panel B comes from a survey administered Feb - May

Table B.3. Exposure to Subsidy Programs

	(1) Mean	(2) Std. Dev.
Panel A. Input subsidy		
Received input subsidy in 2008	0.58	
....in 2009	0.66	
....in 2010	0.73	
....in 2011	0.75	
....in 2012	0.81	
If any, kgs of fertilizer received in 2011	81.58	26.54
.... 2012	64.22	25.47
If any, kgs of seeds received in 2011	5.07	3.44
.... 2012	4.80	3.05
If received subsidy, value of 2011 package[1]	72.28	24.14
....2012 package	58.07	22.68
Received input subsidy all 5 years	0.48	
Never received input subsidy	0.10	
Sharing (based on 2014 villagers survey, N=504)		
Received voucher and didn't share	0.14	
Received voucher and shared	0.46	
Received share of someone's voucher	0.30	
Didn't receive a voucher or share	0.10	
Who decided the voucher would be shared? (Asked of voucher recipients)		
Village Chief	0.85	
Villagers themselves	0.13	
Other	0.02	
Who decided with whom the voucher would be shared? (Asked of voucher recipients)		
Village Chief	0.73	
Villagers themselves	0.23	
Other	0.04	
Who decided with whom the voucher would be shared? (Asked of share recipients)		
Village Chief	0.85	
Villagers themselves	0.09	
Other	0.06	
Panel B. Food Subsidy		
Received food subsidy in 2012	0.59	
If received subsidy, value of package	72.00	37.40
Received both food and input subsidy in 2012	0.53	
Sharing (based on 2014 villagers survey, N=504)		
Who decided with whom the food would be shared?		
Village Chief	0.75	
Group Village Chief	0.03	
Villagers themselves	0.13	
Other	0.09	

Note: All monetary amounts are in US dollars. Exchange rate was roughly 150 MWK to $1 at the time of the baseline, and it was 300 MWK to $1 in late 2012.

Table B.4. PMT formula

	(1) log PCF	(2) log PAEF
Household size (divided by 10)	-4.73***	-3.04***
	(0.74)	(0.55)
Household size (divided by 10) squared	1.91***	1.22***
	(0.48)	(0.42)
Number of children under 5 (divided by 10)	-	1.23***
		(0.34)
Total number of children (divided by 10)	0.54*	-
	(0.32)	
Log durable assets	0.40***	0.40***
	(0.03)	(0.03)
Log animal assets	-	-
Owns land	-	-
Owns land * log acres owned	0.10**	0.09**
	(0.04)	(0.04)
Widowed or Divorced Female Head	-0.27***	-
	(0.07)	
Age of respondent (divided by 100)	-	-
Age of respondent (divided by 100) squared	-1.28***	-1.30***
	(0.28)	(0.27)
Highest education within household	0.05***	0.06***
	(0.01)	(0.01)
Household head is literate	-	-
Home has mud or dirt floors	-0.16*	-
	(0.10)	
Home has thatch roof	-	-
Home has mud or dirt walls	-	-
Toilet is private covered latrine	-	-
Toilet is uncovered latrine	-	-
No toilet	-	-
Water source is public tap	0.69***	0.64***
	(0.19)	(0.20)
Water source is well	0.58***	0.55**
	(0.19)	(0.21)
Water source is piped water	1.02***	1.05***
	(0.32)	(0.30)
Has electricity	-	-
Has a mobile phone	-	-
Main occupation = vendor	-	-
Main occupation = owner of other business	-	-
R-squared	0.32	0.28
Households	1559	1559
Villages	61	61

Notes: Baseline data. PC(PAE)F = per capita (per adult eq) expenditures on non-staple food (monthly eq.), in USD. Sequential selection of variables done using Stata backward stepwise regression. Standard errors, clustered by village, in parentheses. *, **, and *** denote significance at 10%, 5%, and 1%.

Table B.5. PMT formula from IHS3 Data

	(1)	(2)
	BDR variables only	All IHS3 Variables
Household size (divided by 10)	-5.82***	-5.63***
	(0.66)	(0.68)
Household size (divided by 10) squared	2.66***	2.58***
	(0.56)	(0.57)
Number of children under 5 (divided by 10)	-1.24***	-1.16***
	(0.39)	(0.42)
Total number of children (divided by 10)	1.01**	0.84*
	(0.42)	(0.46)
Log durable assets	0.17***	0.17***
	(0.03)	(0.03)
Log animal assets	-	-
Owns land	-	-0.18*
		(0.09)
Owns land * log acres owned	-	-
Widowed or Divorced Female Head	-0.26***	-0.24***
	(0.09)	(0.09)
Age of respondent (divided by 100)	-	-
Age of respondent (divided by 100) squared	-0.38**	-0.40**
	(0.18)	(0.18)
Highest education within household	0.03**	0.03**
	(0.01)	(0.01)
Household head is literate	-	-
Home has mud or dirt floors	-	-
Home has thatch roof	-0.37***	-0.35***
	(0.08)	(0.08)
Home has mud or dirt walls	-	-
Toilet is private covered latrine	-0.26**	-
	(0.11)	
Toilet is uncovered latrine	-0.28**	-
	(0.12)	
No toilet	-0.30*	-
	(0.16)	
Water source is public tap	0.48***	0.48***
	(0.12)	(0.13)
Water source is well	-	-
Water source is piped water	-	-
Has electricity	0.35*	0.39**
	(0.18)	(0.18)
Has a mobile phone	-	-
Main occupation = vendor	-	-
Main occupation = owner of other business	0.29***	0.30***
	(0.09)	(0.09)
Variables in IHS3 but not BDR		
Value of house (USD)	-	-
Has trash pit for garbage	-	-
R-squared	0.40	0.39
Households	763	763
Villages	48	48

Notes: Data comes from Malawi Integrated Household Survey Wave 3 (IHS3). PCF = per capita (per adult eq) expenditures on non-staple food (monthly eq.), in USD. Sequential selection of variables done using Stata backward stepwise regression. Standard errors, clustered by village, in

Table B.6. PMT in Kenya and Uganda

	(1) Kenya	(2) Uganda
	Dependent variable: log PCF	
Household size (divided by 10)	-3.43***	-3.09***
	(0.42)	(0.35)
Household size (divided by 10) squared	1.12***	1.10***
	(0.26)	(0.28)
Number of children under 5 (divided by 10)	-	-
Total number of children (divided by 10)	-	-
Log durable assets	0.19***	0.28***
	(0.04)	(0.03)
Log animal assets	-	0.05***
		(0.02)
Owns land	-	-
Owns land * log acres owned	-	-
Widowed or Divorced Female Head	-0.83***	-0.20***
	(0.09)	(0.06)
Age of respondent (divided by 100)	5.87***	-0.89***
	(1.41)	(0.21)
Age of respondent (divided by 100) squared	-6.76***	-
	(1.46)	
Highest education within household	0.03**	-
	(0.01)	
Household head is literate	-	0.12**
		(0.05)
Home has mud or dirt floors	-	-0.17***
		(0.05)
Home has thatch roof	-	-
Home has mud or dirt walls	-0.61***	-0.11*
	(0.23)	(0.07)
Toilet is private covered latrine	-	-
Toilet is uncovered latrine	-	-
No toilet	-	-
Water source is public tap	-	-
Water source is well	-	-
Water source is piped water	-	-
Has electricity	-	-
Has a mobile phone	-	-
Main occupation = vendor		-0.26***
		(0.05)
Main occupation = vendor	-	0.43***
		(0.08)
Main occupation = owner of other business	-	-
R-squared	0.31	0.28
Households	845	2160

Notes: Data from surveys conducted in 2010 in Kenya (Dupas, Keats and Robinson 2016) and Uganda (Dupas et al. 2016). Dependent variable is total household log per capita food expenditures. *, **, and *** denote significance at 10%, 5%, and 1%.

Table B.7. Correlations between neediness measures

	(1) Log (total expenditures per capita)	(2) Log (total food expenditure per capita)	(3) Food share of expenditures	(4) Percent of days without enough food	(5) Dummy = 1 if not worried about having enough food
Log PCF (non-staple food expenditures per capita)	0.60***	0.69***	0.03***	-0.03***	0.04***
	(0.02)	(0.02)	(0.00)	(0.00)	(0.01)
Observations	6,027	6,027	6,027	6,018	4,507
Number of villages	61	61	61	61	61
Number of households	1,558	1,558	1,558	1,558	1,558
R-squared (within)	0.37	0.39	0.07	0.00	0.00
Mean of Dep. Var.	1.78	1.35	0.70	0.28	0.33

Notes: Regressions pooled across 4 survey rounds (only 3 rounds for col 5). All variables are measured over previous 30 days, with exception of days with enough food (Col 4), which was measured over 30 days in the first 2 surveys, and 90 days over the last 2 surveys. Standard errors in parentheses.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table B.8. Targeting errors, based on total expenditures and total food expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
Neediness proxied using:	Total expenditures			Total food expenditures		
	2011 Input Subsidy	2012 Input Subsidy	2012 Food Subsidy	2011 Input Subsidy	2012 Input Subsidy	2012 Food Subsidy
Simple error rate under following allocation mechanism:[1]						
Chief (True allocation)	0.155	0.145	0.169	0.153	0.145	0.165
PMT (Counterfactual)	0.135	0.116	0.140	0.116	0.102	0.123
PMT (Counterfactual) based on IHS3 formula	0.138	0.120	0.143	0.127	0.099	0.137
Random (Counterfactual)	0.152	0.126	0.162	0.153	0.125	0.162
P-val Chiefs = PMT	0.075	0.006	0.003	0.005	<.001	<.001
P-val Chiefs = PMT (IHS3)	0.080	0.022	0.014	0.025	<.001	0.003
P-val Chiefs = Random	0.754	0.100	0.485	0.992	0.061	0.758
P-val PMT = Random	<.001	<.001	<.001	<.001	<.001	<.001
Mean squared error in log consumption under following allocation mechanism:[2]						
Chief (True allocation)	0.651	0.512	0.796	0.487	0.542	0.881
PMT (Counterfactual)	0.350	0.236	0.495	0.281	0.288	0.618
PMT (Counterfactual) based on IHS3 formula	0.499	0.329	0.616	0.422	0.360	0.866
Random (Counterfactual)	0.699	0.826	2.081	0.524	1.002	2.559
P-val Chiefs = PMT	0.036	0.010	0.032	0.463	0.016	0.822
P-val Chiefs = PMT (IHS3)	0.767	<.001	<.001	0.611	<.001	<.001
P-val Chiefs = Random	0.009	<.001	<.001	<.001	<.001	<.001
P-val PMT = Random	<.001	<.001	<.001	<.001	<.001	<.001

Notes: See Table 2 notes

Table B.9. Multivariate regressions with total expenditures or total food expenditures

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Actual (Chief's) allocations				Counterfactual PMT allocation			
	Got input subsidy	Value (USD)	Got food subsidy	Value (USD)	Eligible for input subsidy	Value (USD)[a]	Eligible for food subsidy	Value (USD)[a]
Panel A. Total expenditures								
Log per capita total expenditures	-0.01*	0.06	-0.04***	-3.06**	-0.05***	-5.32***	-0.03***	-3.54***
	(0.01)	(0.69)	(0.01)	(1.24)	(0.01)	(0.45)	(0.01)	(0.63)
Related to chief	0.04	3.19*	0.12***	10.99***	-0.01	1.93	0.04	3.07*
	(0.02)	(1.71)	(0.03)	(2.85)	(0.02)	(1.17)	(0.03)	(1.79)
Number of Observations	3094	3043	1559	1559	3118	3043	1559	1559
Number of Households	1558	1558	1559	1559	1559	1558	1559	1559
Number of Villages	61	61	61	61	61	61	61	61
Mean of dependent variable	0.78	50.47	0.59	42.03	0.78	50.47	0.59	42.03
Panel B. Total food expenditures								
Log per capita total food expenditures	-0.01*	-0.39	-0.04***	-2.85**	-0.03***	-3.46***	-0.02**	-1.87***
	(0.01)	(0.58)	(0.01)	(1.22)	(0.01)	(0.41)	(0.01)	(0.65)
Related to chief	0.04	3.14*	0.11***	10.85***	-0.01	1.8	0.04	3.15*
	(0.02)	(1.71)	(0.03)	(2.86)	(0.02)	(1.20)	(0.03)	(1.80)
Number of Observations	3094	3043	1559	1559	3118	3043	1559	1559
Number of Households	1558	1558	1559	1559	1559	1558	1559	1559
Number of Villages	61	61	61	61	61	61	61	61
Mean of dependent variable	0.78	50.47	0.59	42.03	0.78	50.47	0.59	42.03

Note: Regressions control for all variables in Table 3, but only key variables are shown. Standard errors clustered at the village level. All regressions include village fixed effects.

[a] Counterfactual quantities have the same distribution as actual quantities.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table B.10. Is poverty targeting more efficient among chiefs' kin?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Actual (Chief's) allocations				Counterfactual PMT allocation			
	Got input subsidy	Value (USD)	Got food subsidy	Value (USD)	Eligible for input subsidy	Value (USD)[a]	Eligible for food subsidy	Value (USD)[a]
Log PCF (total non-staple food expenditures per capita in past month)	-0.01*	-1.48**	-0.03**	-1.93	-0.04***	-5.81***	-0.04***	-4.28***
	(0.01)	(0.63)	(0.01)	(1.34)	(0.01)	(0.55)	(0.01)	(1.00)
Log PCF * Related to chief	0.02**	1.91**	0.00	-0.69	0.01	1.07	0.00	0.72
	(0.01)	(0.93)	(0.03)	(2.67)	(0.01)	(0.87)	(0.02)	(1.52)
Time-Invariant Baseline Variables								
Related to chief	0.04**	5.59***	0.02	1.89	-0.05***	-6.00***	-0.09***	-9.23***
	(0.02)	(1.13)	(0.02)	(1.48)	(0.02)	(1.06)	(0.01)	(0.94)
Log (acres farmed)	-0.14	-16.41	-0.1	-22.45	-0.14	8.86	-0.12	-8.32
	(0.31)	(22.29)	(0.31)	(20.16)	(0.20)	(20.60)	(0.31)	(16.10)
Years of education (divided by 10)	0.02	2.72	-0.06	-3.38	-0.35***	-29.34***	-0.29***	-30.12***
	(0.03)	(2.66)	(0.05)	(4.10)	(0.03)	(2.16)	(0.04)	(3.19)
Widowed or divorced female	0.02	0.54	0.00	1.94	0.05*	9.93***	0.12***	15.91***
	(0.02)	(1.60)	(0.03)	(2.88)	(0.03)	(1.66)	(0.03)	(2.26)
Household size (divided by 10)	0.09*	6.97	-0.04	-0.68	0.45***	47.07***	0.53***	72.38***
	(0.05)	(4.30)	(0.05)	(5.58)	(0.07)	(5.05)	(0.08)	(7.98)
Respondent age: 2nd quartile (26-35)	0.12***	8.67***	0.06*	4.36	0.04	1.83	0.07**	0.12
	(0.03)	(2.41)	(0.03)	(3.03)	(0.03)	(1.46)	(0.03)	(2.07)
Respondent age: 3rd quartile (36-51)	0.17***	13.11***	0.12**	11.40***	0.09**	6.35***	0.11***	5.32*
	(0.04)	(2.90)	(0.05)	(3.95)	(0.04)	(2.05)	(0.04)	(2.85)
Respondent age: highest quartile (over 52)	0.21***	14.91***	0.24***	22.05***	0.11***	14.42***	0.20***	23.43***
	(0.04)	(2.81)	(0.05)	(4.31)	(0.04)	(2.23)	(0.04)	(2.88)
Log (value of animals owned)	0.00	1.16*	-0.01	0.74	-0.03***	-2.06***	-0.02**	-2.52***
	(0.01)	(0.67)	(0.01)	(0.89)	(0.01)	(0.57)	(0.01)	(0.81)
Shocks								
Experienced drought or flood (past 3 months)	0.04**	-0.84	0.08**	5.46*	0.04	-0.38	0.03	1.92
	(0.02)	(1.48)	(0.03)	(2.73)	(0.03)	(1.55)	(0.03)	(2.59)
Experienced cattle death or crop disease (past 3 months)	0.08***	-1.76	0.00	0.2	0.05***	-1.98	0.04	1.63
	(0.01)	(1.44)	(0.02)	(2.21)	(0.01)	(1.26)	(0.02)	(1.77)
Number of Observations	3094	3043	1559	1559	3118	3043	1559	1559
Number of Households	1558	1558	1559	1559	1559	1558	1559	1559
Number of Villages	61	61	61	61	61	61	61	61
Mean of dependent variable	0.78	50.47	0.59	42.03	0.78	50.47	0.59	42.03

Note: Regressions for input subsidies pool years 2011 and 2012 and control for the year. Omitted age category is less than 26. Standard errors clustered at the village level. All regressions include village fixed effects.

[a] Counterfactual quantities have the same distribution as actual quantities.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table B.11. Multivariate correlates of Returns to Fertilizer

	Dependent variable: Log (gain in farm production from fertilizer use)
Log (acres farmed)	0.13***
	(0.04)
Related to chief	-0.01
	(0.04)
Years of education (divided by 10)	0.25***
	(0.08)
Widowed or divorced female	-0.01
	(0.05)
Household size (divided by 10)	0.23*
	(0.12)
Respondent age: 2nd quartile (26-35)	0.13**
	(0.06)
Respondent age: 3rd quartile (36-51)	0.21***
	(0.06)
Respondent age: highest quartile (over 52)	0.22***
	(0.08)
Log (value of animals owned)	0.06***
	(0.02)
Ever made a payment to the village chief	-0.01
	(0.05)
Number of Observations	530
Number of Villages	61
Mean of dependent variable	2.04
SD of dependent variables	0.74
R-squared (no village FE)	0.14

Note: Omitted age category is less than 26. Standard errors clustered at the village level. Regression includes village fixed effects.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table B.12. Productive efficiency with longer list of controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Actual (Chief's) allocations			Counterfactual PMT allocation			Ever Lobbied Chief to try to get Input Subsidy
	Value (USD) of input subsidy	Value (USD) of food subsidy	Value gap (input-food)	Value (USD) of input subsidy under PMT[a]	Value (USD) of food subsidy under PMT[a]	Value gap (input-food) under PMT	
Log (gain in farm production from fertilizer use)	2.58	-0.45	7.29**	-2.53	-4.96*	2.80	0.00
	(1.58)	(2.54)	(3.37)	(1.88)	(2.92)	(2.92)	(0.03)
Log (total non-staple food expenditures per capita in past month)	1.01	1.83	-1.29	-5.69***	-4.38***	0.05	0.00
	(0.87)	(1.93)	(2.57)	(1.06)	(1.48)	(1.59)	(0.01)
Time-Invariant Baseline Variables							
Related to chief	0.89	7.55*	-4.73	4.70**	5.28*	-0.30	0.04
	(3.06)	(3.97)	(5.38)	(2.30)	(2.93)	(3.55)	(0.03)
Log (acres farmed)	3.21	-4.38	7.09*	-5.66**	-9.49***	4.71**	0.01
	(2.67)	(2.95)	(3.77)	(2.46)	(2.23)	(1.81)	(0.03)
Years of education (divided by 10)	1.83	-8.01	10.35	-30.05***	-33.16***	7.90	0.01
	(5.57)	(7.21)	(8.21)	(4.22)	(5.05)	(4.96)	(0.06)
Widowed or divorced female	-1.35	0.80	-1.93	10.17***	16.49***	-4.99	0.00
	(2.76)	(3.93)	(4.40)	(2.55)	(3.65)	(3.78)	(0.03)
Household size (divided by 10)	-0.22	3.68	-0.10	50.23***	69.14***	-22.05*	0.02
	(6.19)	(9.64)	(12.86)	(9.00)	(13.82)	(11.87)	(0.08)
Respondent age: 2nd quartile (26-35)	11.32**	4.23	4.37	1.52	-1.37	2.75	-0.10**
	(4.32)	(5.82)	(6.95)	(3.52)	(4.75)	(4.55)	(0.05)
Respondent age: 3rd quartile (36-51)	15.68***	11.85	1.46	4.42	-1.21	4.44	-0.12*
	(4.67)	(7.53)	(7.83)	(3.70)	(4.67)	(5.30)	(0.06)
Respondent age: highest quartile (over 52)	19.07***	24.02***	-8.67	13.84***	12.83**	-0.52	-0.13**
	(4.93)	(6.95)	(7.24)	(4.62)	(5.77)	(6.16)	(0.06)
Log (value of animals owned)	0.62	0.37	0.08	-2.88***	-3.22**	0.27	0.03
	(1.09)	(1.46)	(1.79)	(0.99)	(1.43)	(1.41)	(0.02)
Shocks							
Experienced drought or flood (past 3 months)	0.33	4.34	-3.60	-2.56	0.62	0.05	-0.08**
	(3.65)	(4.81)	(6.17)	(3.05)	(4.69)	(4.84)	(0.03)
Experienced cattle death or crop disease (past 3 months)	3.47	-1.42	5.25	2.23	5.02	-3.24	-0.02
	(2.54)	(4.29)	(4.49)	(2.30)	(3.11)	(3.12)	(0.02)
Information from villager survey							
Ever made a payment to the village chief	-4.33*	-7.70*	5.38	-3.95	-3.56	0.28	0.01
	(2.23)	(4.39)	(4.79)	(2.38)	(3.63)	(3.51)	(0.03)
Number of Observations	1048	530	529	1048	530	529	530
Number of Households	530	530	529	530	530	529	530
Number of Villages	61	61	61	61	61	61	61
Mean of dependent variable	51.83	37.78	11.94	52.96	40.53	9.26	0.09

Note: Sample restricted to households surveyed in 2014 and asked about perceived returns to fertilizer use. Regressions for input subsidies pool years 2011 and 2012 and control for the year. 2011 input allocation information comes from 2011 survey. 2012 input and food allocations information comes from 2012 survey. Omitted age category is less than 26. Standard errors clustered at the village level. All regressions control for village fixed effects.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table B.13. Productive efficiency results: alternative specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Actual (Chief's) allocations			Counterfactual PMT allocation			Ever Lobbied Chief to try to get Input Subsidy
	Value (USD) of input subsidy	Value (USD) of food subsidy	Value gap (input-food)	Value (USD) of input subsidy under PMT[a]	Value (USD) of food subsidy under PMT[a]	Value gap (input-food) under PMT	
Panel A. Non-parametric controls for farm size							
Log (gain in farm production from fertilizer use)	4.34**	-0.01	8.46**	-2.69	-4.65	2.79	0.00
	(1.72)	(2.91)	(3.46)	(2.54)	(3.19)	(2.79)	(0.03)
Log (total non-staple food expenditures per capita in past month)	-0.05	-0.66	0.34	-11.60***	-10.90***	1.87	0.02
	(0.75)	(1.56)	(2.16)	(1.04)	(1.95)	(1.71)	(0.01)
Time-Invariant Baseline Variables							
Related to chief	2.36	11.01***	-7.67	7.51***	7.92*	-0.93	0.03
	(2.93)	(3.97)	(5.28)	(2.52)	(4.24)	(3.91)	(0.03)
Q2 of acres farmed	4.80	6.58	-4.62	0.26	-8.76	5.28	-0.02
	(4.20)	(6.97)	(7.40)	(4.41)	(6.23)	(4.80)	(0.04)
Q3 of acres farmed	9.37***	5.22	1.20	1.36	-2.94	1.44	-0.01
	(3.35)	(4.78)	(6.00)	(2.84)	(4.47)	(3.66)	(0.04)
Q4 of acres farmed	7.61*	-1.41	9.47	-4.75	-10.37**	4.99	-0.02
	(4.02)	(4.53)	(5.81)	(3.73)	(4.74)	(3.08)	(0.04)
Panel B. Targeting on productivity per acre							
Log (per acre yield gain from fertilizer use)	2.63	1.37	5.96*	-1.50	-1.98	0.96	-0.01
	(1.88)	(3.15)	(3.52)	(2.52)	(3.18)	(2.56)	(0.03)
Log (total non-staple food expenditures per capita in past month)	0.01	-0.74	0.56	-11.70***	-11.10***	2.02	0.01
	(0.77)	(1.56)	(2.16)	(1.03)	(1.94)	(1.73)	(0.01)
Time-Invariant Baseline Variables							
Related to chief	2.11	10.96***	-7.99	7.38***	7.90*	-0.95	0.03
	(2.94)	(3.99)	(5.23)	(2.54)	(4.28)	(3.94)	(0.03)
Q2 of acres farmed	6.20	7.30	-1.44	-0.78	-9.91	5.80	-0.02
	(4.48)	(7.24)	(7.43)	(4.74)	(6.45)	(4.94)	(0.04)
Q3 of acres farmed	11.97***	6.26	6.70	-0.22	-4.90	2.40	-0.02
	(3.91)	(5.54)	(6.52)	(3.78)	(5.22)	(3.91)	(0.04)
Q4 of acres farmed	11.92**	0.33	18.82***	-7.45	-13.86**	6.74	-0.03
	(4.79)	(5.69)	(6.86)	(4.85)	(6.27)	(4.50)	(0.04)
Number of Observations	1038	525	524	1038	525	524	525
Number of Households	525	525	524	525	525	524	525
Number of Villages	61	61	61	61	61	61	61
Mean of dependent variable	52.20	38.05	11.98	53.39	40.86	9.41	0.09

Note: Sample restricted to households surveyed in 2014 and asked about perceived returns to fertilizer use. Regressions for input subsidies pool years 2011 and 2012 and control for the year. 2011 input allocation information comes from 2011 survey. 2012 input and food allocations information comes from 2012 survey. Omitted age category is less than 26. Standard errors clustered at the village level. All regressions control for village fixed effects.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table B.14. Is productive efficiency targeting stronger among chiefs' kin?

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Actual (Chief's) allocations			Counterfactual PMT allocation			Ever Lobbied Chief to try to get Input Subsidy
	Value (USD) of input subsidy	Value (USD) of food subsidy	Value gap (input-food)	Value (USD) of input subsidy under PMT[a]	Value (USD) of food subsidy under PMT[a]	Value gap (input-food) under PMT	
Log (gain in farm production from fertilizer use)	3.99*	-2.52	9.16**	-1.74	-3.20	1.87	-0.030
	(2.35)	(3.55)	(4.22)	(2.99)	(4.04)	(3.33)	(0.031)
Log (gain in farm production from fertilizer use) * relative	0.33	8.87	-3.37	-2.33	-2.44	1.53	0.10**
	(4.19)	(6.55)	(8.08)	(4.22)	(6.33)	(4.50)	(0.05)
Log PCF (total non-staple food expenditures per capita in past month	6.25**	-1.27	6.37	-2.84	-7.27***	3.87**	0.000
	(2.59)	(2.85)	(3.84)	(2.43)	(2.72)	(1.88)	(0.027)
Log PCF * Related to chief	4.38**	-2.91	4.05	1.58	-0.58	-0.51	-0.03
	(1.79)	(3.66)	(4.68)	(1.91)	(4.17)	(3.87)	(0.03)
Time-Invariant Baseline Variables							
Related to chief	-1.26	0.27	-0.97	-11.92***	-10.74***	2.02	0.020
	(0.93)	(1.97)	(2.62)	(1.21)	(2.35)	(1.94)	(0.015)
Log (acres farmed)	1.55	-10.32	-0.48	13.20	14.24	-4.60	-0.20*
	(10.84)	(16.60)	(20.16)	(10.39)	(15.61)	(11.38)	(0.111)
Number of Observations	1048	530	529	1048	530	529	530
Number of Households	530	530	529	530	530	529	530
Number of Villages	61	61	61	61	61	61	61
Mean of dependent variable	51.83	37.78	11.94	52.96	40.53	9.26	0.09

Note: Sample restricted to households surveyed in 2014 and asked about perceived returns to fertilizer use. Regressions for input subsidies pool years 2011 and 2012 and control for the year. 2011 input allocation information comes from 2011 survey. 2012 input and food allocations information comes from 2012 survey. Omitted age category is less than 26. Standard errors clustered at the village level. All regressions control for village fixed effects.
* significant at 10%; ** significant at 5%; *** significant at 1%

Table B.15. Perceived Within-Village Heterogeneity among Village Chiefs

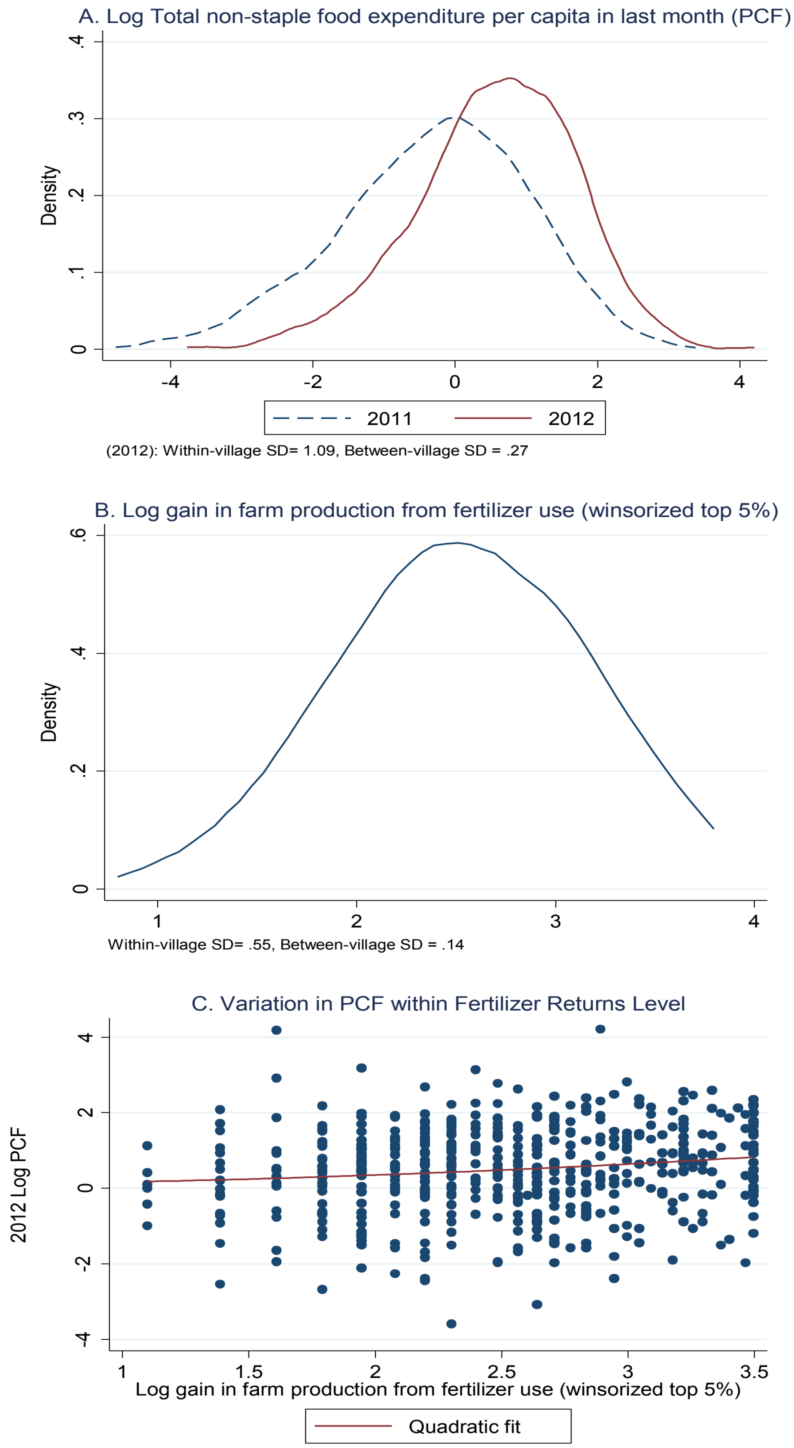
Can you easily categorize households in the village with land better suited for fertilizer and those with land not so well suited for fertilizer?	
Yes	0.86
Can you easily categorize households in the village in two groups, those who are very poor and those who are less poor?	
Yes	0.96
with money at a given time?	
I know how everyone is doing	0.65
I know how some people are doing	0.32
I do not know	0.04
planting season and who will not?	
I know how everyone is doing	0.49
I know how some people are doing	0.27
I do not know	0.24
Number of observations	79

Notes: From survey of village chiefs conducted in 2014. See text for details.

Figure B.1. Timeline

Activity	Dates
Census	November-December 2010
Baseline Survey	February-March 2011
Account Opening	June-July 2011
Follow-up I	February-March 2012
Follow-up II	September-December 2012
Follow-up III (endline)	February-May 2013
Villager survey	August-October 2014
Chiefs survey	August-October 2014

Figure B.1. Distributions of key variables of interest



Notes: Gain in farm production expressed in 50 Kg bag units.

Figure B.2. Permutation test, village average error rate

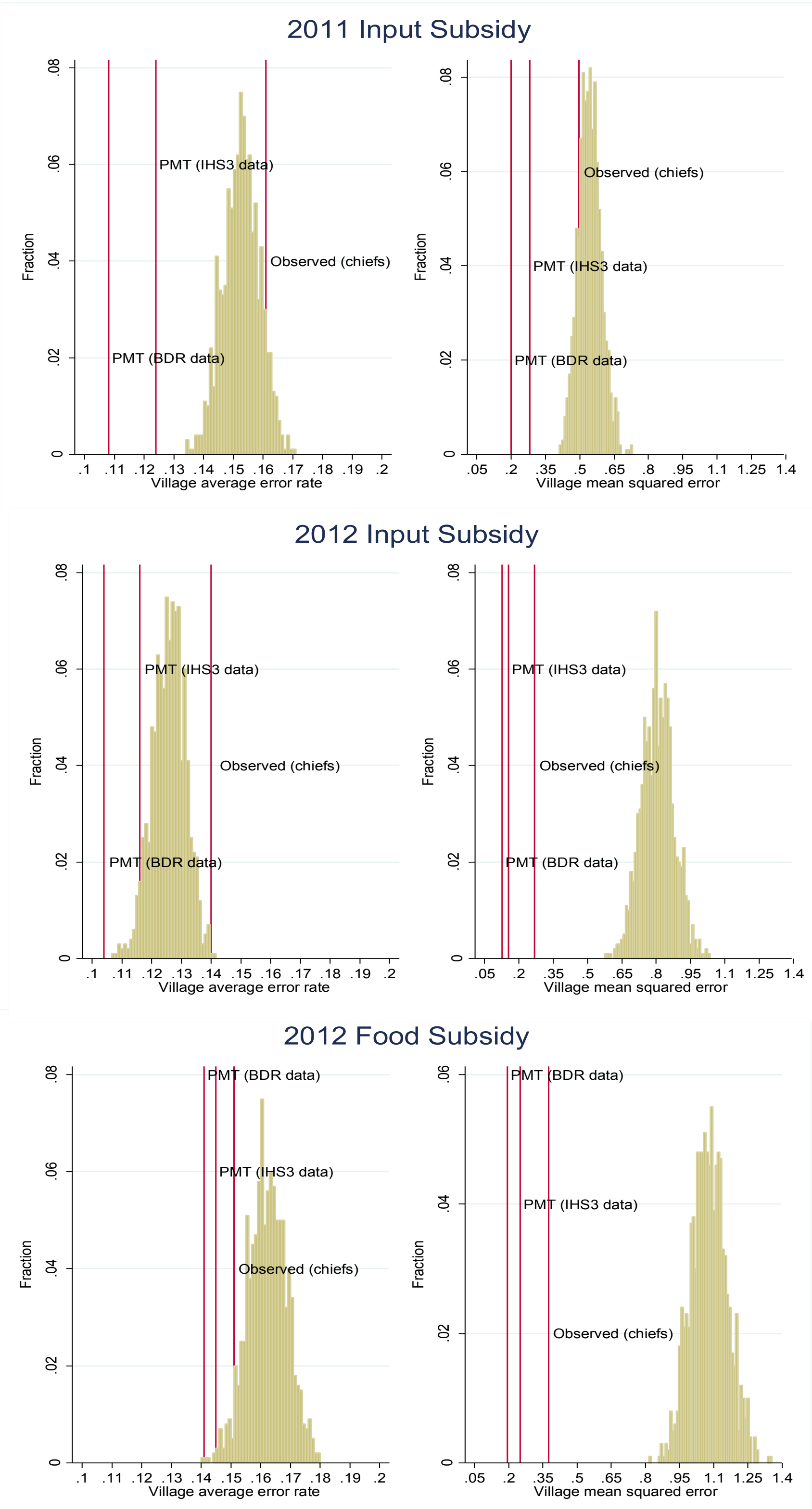
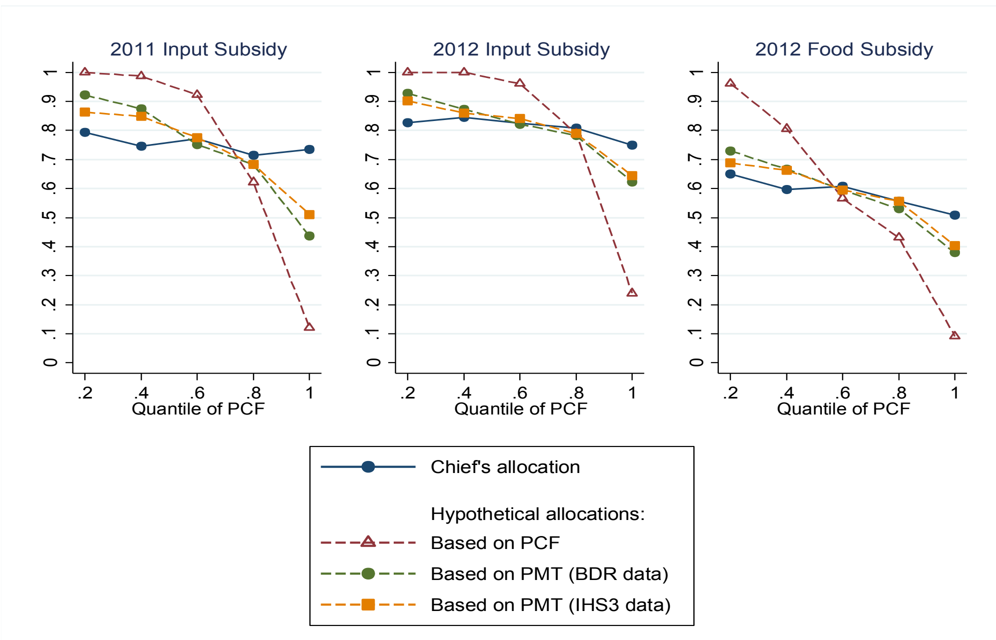
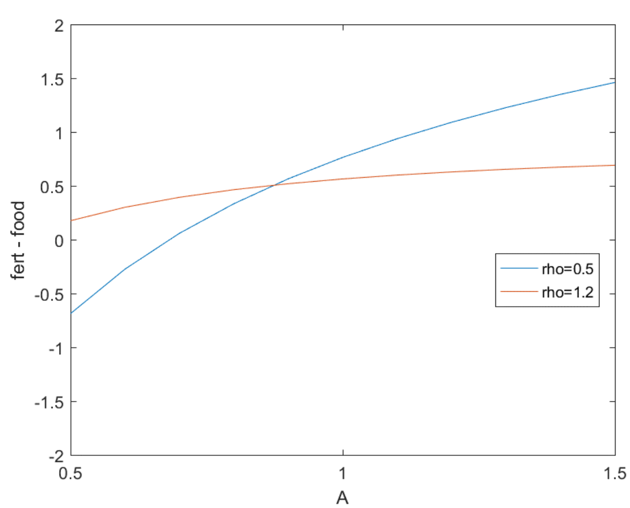


Figure B.3. Comparing chiefs' allocation to counterfactual allocations



Notes: See main text section 4.2 and figure 1 notes.
IHS3 = Malawi Third Integrated Household Survey, a representative survey conducted by Malawi's National Statistical Office from March 2010 to March 2011.

Figure B.4. Model simulation: Optimal Allocation with Productive Efficiency Consideration



Notes: see text section 5.

Chapter 3

On the Peer Effects of Star Students

1 Introduction

Peer effects are believed to play a crucial role in the process of human capital formation. However, most empirical papers find impacts only on non-academic outcomes (Moreira 2016). Moreover, there is scarce evidence on the peer effects among siblings, arguably a person's most influential peers.[1] To shed light on the nature of these effects, we estimate the peer effects of star students on their siblings' learning outcomes.[2] This task poses a peculiar problem, since unbiased estimation of these effects requires exogenous variation in the presence of star students in the household. We exploit an unusual natural experiment which does precisely this: the admission process to a high-achiever public boarding school in Peru.

Essentially, our empirical strategy consists in comparing the siblings of two star students who obtained very similar scores in the final stage of the admission process to the high-achiever school; one of whom scored barely below the admission cutoff, and therefore still lives at home, and one of them who scored barely above the threshold, and therefore has moved out to the boarding school. This small, plausibly exogenous, differences in scores generate plausibly exogenous variation in the presence of star students at home. It is key to note that only the top 0.10-0.15% of students in each cohort reach the final stage, so both admitted and non-admitted applicants can be classified as "star students".

Our population of interest is formed by the star students' siblings, which we

[1]Two interesting exceptions are Dustan (2015) and Goodman et al. (2015) who study how siblings affect school and college choice, respectively.

[2]Moreira (2016) studies a related phenomenon: the peer effects of star students on their classmates' performance.

sort in two groups. The treatment group is formed by students whose (star student) siblings barely gained admission to the high-achiever school. The control group is formed by students whose (star student) siblings almost gained admission to the high-achiever school. In this setting, treatment consists in having the star student move out from home to the boarding school. This strategy allows estimating the effect of star students on their siblings' learning outcomes by comparing sibling grade point average (GPA) and math scores across treatment arms.

We find that star students increase their siblings' GPA by 0.33 standard deviations, and their math grades by 0.22 standard deviations. These effects are large by international standards and comparable to those of the Education Ministry's teacher mentoring program (Rodriguez, Leyva and Hopkins 2016), which is one of its flagship interventions. Heterogeneity analysis shows that effect magnitude is inversely related to number of siblings, suggesting that the remaining siblings act as substitutes for the star student.

The next section describes the study setting and and the datasets used in our analysis. Section 3 discusses our empirical approach. Section 4 presents our results and section 5 concludes.

2 Setting and Data

Colegio Mayor is a high-achiever public boarding school located to approximately one hour east of Lima, Peru's capital city. When it started operations, in 2010, it was the only of its kind in the country. As the years passed, its model got replicated and now there are 20 high-achiever public boarding schools nation-wide. During our study period, to be eligible to apply, students must have ranked first or second of their class in second year of high school school (corresponding to eighth grade in US education), or must have ranked in the top five spots in nation-wide academic competitions organized by the Ministry of Education during first or second year of high school.[3] The admission process has two stages;[4] at the end of each stage successful applicants were invited to continue on to the next one. Each year, public schools in Peru have approximately 400,000 to 450,000 students in second year of high school. Between 2011 and 2014, there have been approximately 3,700 applicants per year to Colegio Mayor, of which a select group of 500-650 applicants (i.e., the top 0.10-0.15% of their cohort), who for our purposes are "star students", reached the final stage.

The school has admission quotas for 26 regions in the country[5], which implies the existence of as many admission cutoff points per admission process. We had access to admission scores and region of residence for the 2011, 2013, and 2014 admission processes. De facto, this amounts to 78 natural experiments that we leverage to find our parameter of interest.

The data on school grades by subject were provided by the Ministry of Education. Grades in the Peruvian education system range from 0 to 20, with 10.50 required for a passing grade. Both data sources were merged by father's and mother's last name, similar to the procedure followed by Dustan (2015) and Dustan, De Janvry and Sadoulet (2015). To protect anonymity, the match was

[3]In addition, students must have been enrolled in public schools during first and second year of secondary school.

[4]With the exception of the 2013 process, which had three stages.

[5]Metropolitan Lima, rest of Lima, Callao, and the remaining 23 regions.

performed by the staff at the Ministry of Education's Monitoring and Strategic Evaluation Office. The matched dataset contains 646 high school students matched to 520 applicants. Conditional on matching, there is an average of 1.24 matches per applicant.

Since matching was performed on father's and mother's last name, children who share only one parent are not identified as siblings in our dataset. It is worth noting that the effects among them may be different. However, we consider this strategy superior to matching only on one last name, since it may generate a large number of false matches.

3 Empirical Approach

We use the admission process to the high-achiever school as a natural experiment, within the framework of regression discontinuity (RD) design.[6] If we assume that each applicant's admission score (the running variable) has a random component with a continuous density, then the probability of scoring marginally above or below the cutoff is the same. In other words, admission is as good as randomly assigned within a sufficiently small neighborhood of the cutoff point, even if the expected score depends on individual characteristics (Lee 2008). If the probability of scoring marginally above or below the threshold is the same, applicants at either side of the threshold should share the same individual and household characteristics. As a corollary, their siblings should also share the same characteristics. We

[6] Admission processes have been used extensively in RD designs. See, e.g., Solis (2017), Smith et al. (2017).

provide supportive evidence for this assumption in section 4.

Before estimating the model we rescale all scores by subtracting their respective region's minimum admission score that year, so that the threshold is zero in every region and year. Let S_h be the standardized score obtained by household h's applicant (in our sample no household has more than one applicant). With this, we estimate

$$y_{iht} = \beta_0 + \beta_1 T_i + f(S_h) + \varepsilon_{iht} \tag{1}$$

, where y_{iht} is the outcome variable for student i from household h in year t. T indicates whether student i's sibling gained admission to the school, β_0 is the expected value of the outcome variable for students whose (star student) sibling scored just below the cutoff, and β_1 is the change in expected value of the outcome variable for students whose (star student) sibling scored just above the cutoff, our parameter of interest. $f(\cdot)$ is a local polynomial function of S_h, and ε is the residual term.

4 Results

The main results are plotted in Figure 3.1, panels (a)-(d). In all panels, the vertical axis indicates student's GPA or math grades, and the horizontal axis indicates the running variable, his or her (star student) sibling's score in the admission process. Treated students appear to the right of the cutoff, and control students, to the left. Panels (a) and (b) show that there were no discontinuities in GPA or in math grades before the star student gained admission to the high achiever school, supporting the validity of our research design[7]. Panel (c) shows our main result: at the threshold, the presence of a star student at home increases siblings' GPA by 0.6 points, or 0.33 standard deviations. Panel (d) shows that the effect on math grades is also large and statistically significant, at 0.8 points (0.22 standard deviations). These results remain unchanged if we remove one cohort at a time. An effect this size is comparable to a flagship mentoring program run by the Ministry of Education (Rodriguez, Leyva and Hopkins 2016).

Figure 3.2 shows the results by number of siblings. Approximately 35% of the students in our sample have one school-age sibling, 35% have two, and 30% have three or four (our data is capped at five school-age siblings per family). Panels (a) and (b) show the effects on GPA and math grades, respectively, for students whose only school-age sibling, the star student, left home. Panels (c) and (d) show the treatment effects for students with an additional sibling besides the star student, while Panels (e) and (f) show the results for households with two or three siblings besides the star student. Effects are largest in small households, and fade away for households with three or more siblings. The negative relation between effect size and number of siblings suggests that siblings act as substitutes of the star student. Understanding how this substitution takes place is an interesting path for future research.

[7]There were no statistically significant differences in the other subjects or in density across the threshold (not shown due to space constraints).

5 Conclusions

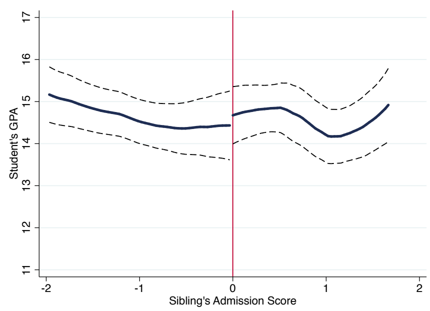
We estimate the effects of star students on their siblings' learning outcomes, measured by their GPA and math scores. We pair a rich dataset on school grades with an unusual natural experiment that generates exogenous variation in the presence of a star student at home. In our sample, star students increase their siblings' GPA by 0.33 standard deviations and their math grades by 0.22 standard deviations. These effects are large, either compared with international literature or with some of the most successful programs run by the Ministry of Education in Peru.

Our study has some important caveats. First, as in any RD design, the effects are valid for students around the admission threshold only. Second, with the data at hand, we are not able to disentangle the peer effects of the presence of a sibling from those of said sibling being a star student. Third, our data says little about the mechanisms through which the effects the peer effects reported here operate. Fourth, there are likely additional effects on non-academic outcomes that our study does not address.

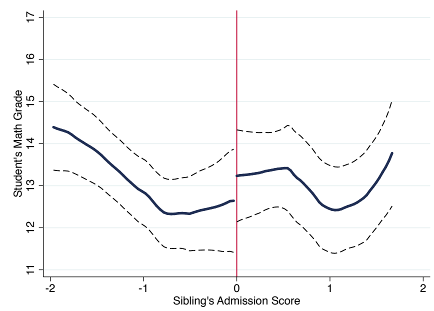
However, our findings have an interesting corollary. Under the assumption that peer effects are a monotonic function of student quality, our estimates could be used to place an upper bound on the peer effects of "non-star" students on their siblings.

Figure 3.1: Main Results

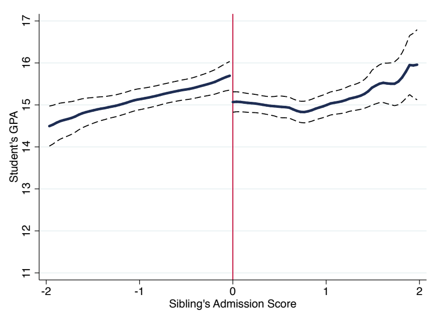
(a) Pre-Treatment Differences, GPA



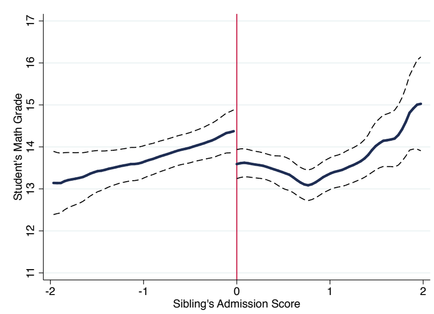
(b) Pre-Treatment Differences, Math



(c) Treatment Effect, GPA



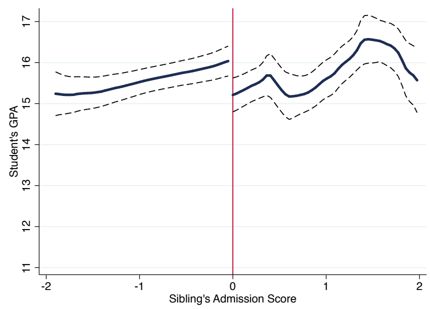
(d) Treatment Effect, Math



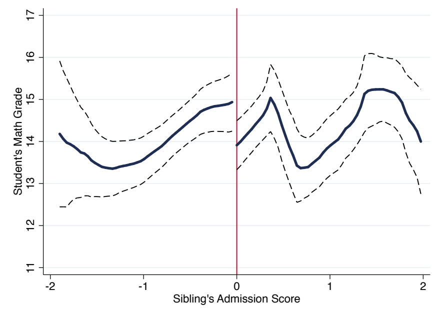
Notes: Dashed lines represent the empirical 95% confidence bands. Source: Ministry of Education and Colegio Mayor

Figure 3.2 Effect by Number of Siblings

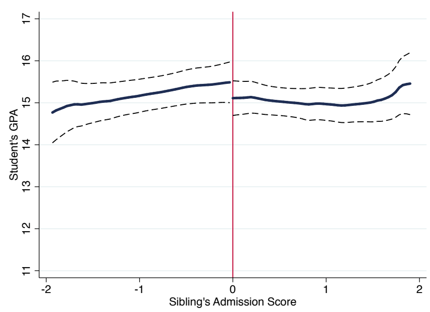
(a) Effect on GPA, 1 sibling



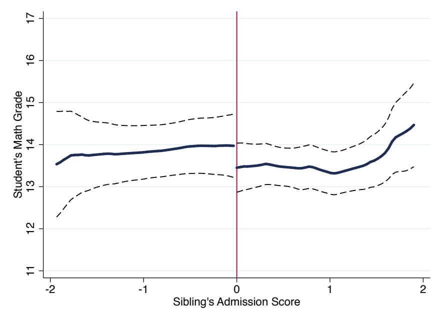
(b) Effect on Math Grade, 1 sibling



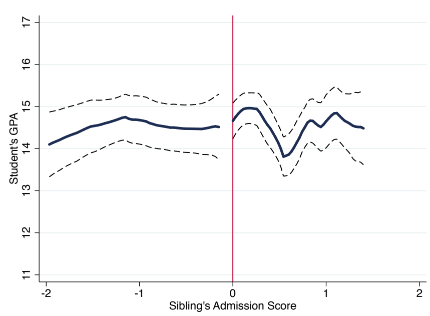
(c) Effect on GPA, 2 siblings



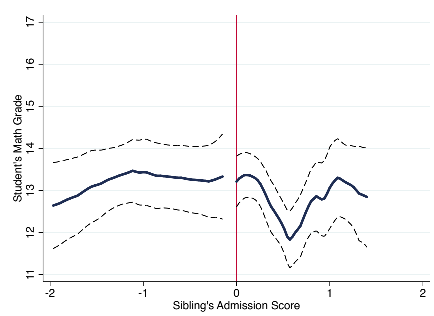
(d) Effect on Math Grade, 2 siblings



(e) Effect on GPA, 3 siblings



(f) Effect on Math Grade, 3 siblings



Notes: Dashed lines represent the empirical 95% confidence bands. Source: Ministry of Education and Colegio Mayor

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