Informal Taxation in Development Projects: The Role of Chiefs in Sierra Leone

Andrés F. Rodríguez*

November 17, 2023
[Click Here For Latest Version](#)

Abstract

This paper investigates the relative merit of traditional leaders in promoting cooperative behavior when informally taxing citizens' labor in low-income states and whether this comes at the expense of relatively poor households. I designed a field experiment to measure whether citizens distort their behavior when facing informal taxation schemes by engaging in costly actions to avoid contributing their labor to a public good. I randomize communities into different methods to select contributors and compare the status quo of selection by chiefs versus two alternatives: random lotteries and progressive selection based on household surveys. I use the random selection arm as a benchmark and estimate the relative merits of chiefs and the possible costs of progressive selection. My experiment finds that chiefs do not distort individual behavior but promote pro-social actions by making citizens less likely to avoid the public good than the other selection methods. However, selection by chiefs does create distortions, particularly among the poorest households, which are more likely to accept less compensation in the experiment to avoid being selected by chiefs. Progressive taxation does not create significant distortions, but it does exhibit regressive patterns similar to those of selection by chiefs. This stems from differences in self-perceptions of wealth and how surveys proxy for material well-being in these low-income rural contexts. (JEL C93, D91, H21, H23, H41, I32, O12, O17)

I Introduction

Development policy in low-capacity states across sub-Saharan Africa often relies on traditional or customary leaders to operate. These leaders, often called chiefs, have played essential roles in various policies by assisting with targeting subsidies, collecting taxes, or managing local infrastructure projects (Basurto *et al.*, 2020; Balan *et al.*, 2022; Casey *et al.*, 2023). Chiefs appear particularly important when development initiatives need community support as they can efficiently mobilize people and resources towards community goals (Baldwin and Raffler, 2019; van den Boogaard and Santoro, 2023). This stems from the fact that in these areas that fall beyond the reach of central states, chiefs are often in charge of organizing collective action for local public goods (Honig, 2022). As a result, customary leaders seem critical for promoting cooperative behavior needed for some development programs.

However, previous work also points out that involving chiefs in public goods provision might lead to regressive informal taxation schemes, where very poor households devote significant amounts of money, labor, or goods to finance local public goods (Olken and Singhal, 2011; Walker, 2018; van den Boogaard *et al.*, 2019). This pattern is consistent with how chiefs have historically relied on communal work, mostly from young and poor men (Peters and Richards, 2011; Bulte *et al.*, 2018). More broadly, these concerns are amplified by work suggesting chiefs are mainly local elites with no electoral pressure and thus little incentive to redistribute or promote broad community development (Acemoglu *et al.*, 2014b). Put together, this suggests a tradeoff for development policy. Involving chiefs in development programs promotes cooperative behavior needed to mobilize resources towards public goods but at the expense of relatively poor households bearing significant costs.

In this paper, I examine the tradeoff highlighted above through a field experiment in Sierra Leone that offered simple one-day jobs in rural communities and created a public good problem around this setup framed as a development program. The experiment required some participants to be selected as "community workers." This meant that instead of working for themselves, selected participants would work to help fund a real local public good, the local health clinic. This experimental feature resembles real development programs that often rely on local labor for implementation and allow informal taxation to occur (Bulte *et al.*, 2018; van den Boogaard *et al.*, 2019). Then, I compare the status quo approach of delegating the selection of community workers to local chiefs relative to two benchmarks: selecting community workers by random lotteries or using a progressive scheme based on wealth measures from household surveys. The former guarantees that selection is transparent and not targeted towards any particular type of citizen. The latter tests a simple policy instrument commonly used to target subsidies or calibrate low-income countries' tax rates. I make this comparison by randomizing across communities the selection method used to pick who should work to fund the public

good.

To assess if involving chiefs in the selection of community workers is relatively effective at promoting cooperative behavior at the expense of relatively poor households, I focus on measuring two key outcomes: (i) the extent to which citizens engage in costly actions to avoid contributing to a public good and (ii) conditional on being asked to contribute, how much effort citizens exert towards funding the public good. Although both outcomes reflect cooperative behavior, the first is critical to assessing if redistribution through informal taxation imposes costs on citizens through distortions of individual behavior. If citizens distort their behavior by engaging in costly actions to avoid being selected, coercion might play an important role in driving contributions. This intuition stems from simple taxation models where individuals do not internalize the benefits of what is being funded — for example Agranov and Palfrey (2015). However, citizens can be pro-social and interested in funding local public goods, particularly in low-income rural settings where some public services are severely underfunded. This can lead to contributions being better understood as voluntary giving in a context where citizens internalize the benefits of public goods. Distinguishing between these two perspectives is necessary to study the incidence of informal taxation schemes, which is the primary goal of this study. This approach stems from the literature studying the distortionary effects of redistributive pressure from kin or peers in low-income settings (Jakiela and Ozier, 2016; Boltz *et al.*, 2019; Carranza *et al.*, 2022).

To study the incidence of informal taxation, I implemented the experimental jobs program over two days per community. Then, upon recruitment, I advertised that community workers using their labor to fund the local health clinic could only selected among people working on the second day. This feature implies that participants who want to avoid being selected should prefer to work on the first day of the program, as working earlier guarantees they get to keep their earnings. As a result, I can measure whether participants bear private costs to shift their work toward the first day of the program[1]. In the experiment, this takes the form of participants being willing to accept a lower compensation to work during the first day of the program. I use this behavior as the primary outcome of my experiment as it captures privately and socially costly behavior that helps participants avoid an informal tax on their labor.

I first analyze the random selection arm, which serves as a baseline for how participants behave in this experiment. I leverage within-community variation to cleanly estimate the elasticity of costly actions to avoid contributions to changes in the likelihood of being selected as a community worker. In this arm, some participants are told they can never be selected as community workers, while others are told they will face a lottery to decide if they are selected. When they cannot be selected, 24% of participants accept all wage cuts, and 49% accept at least one

---

[1]This resembles the behavioral response identified by Exley (2016) where participants in an experiment display relatively more risk aversion when risk can be used as an excuse to avoid charitable donations.

wage cut. Not surprisingly, these outcomes are negatively correlated with participants' wealth. When the likelihood of selection increases, I find participants react as expected by increasing the likelihood of accepting wages, although this effect is small in magnitude. This result is consistent with participants internalizing the value of the public good. However, I observe more significant behavioral responses among participants who display relatively low valuation of investments directed toward their local health clinic. This result is consistent with participants reacting more to informal taxation schemes via random lotteries when they do not value the public good being funded.

Using random selection as a benchmark, I then leverage the experimental variation across communities to investigate the effects of delegating selection to chiefs or using a progressive selection method to select community workers. I find evidence consistent with chiefs being relatively effective at promoting cooperative behavior as participants assigned to chief selection are less likely to accept wage cuts and distort their behavior to avoid the public good relative to random selection. The estimated effect size is large as participants are 25% less likely to always prefer working on the first day when selection is delegated to chiefs relative to random selection. I do not find evidence that progressive selection affects behavior relative to random selection, which suggests no relative benefits in terms of socially aligned behavior or no costs in terms of making citizens more likely to avoid the public good. Participants in the experiment showed almost no reaction to either selection method via the intensive margin to effort towards the public good. This finding reveals the importance of measuring participants' willingness to participate in the public good and whether they would bear private costs to avoid having to contribute.

Finally, I conduct a pre-specified heterogeneity analysis based on differences in household wealth across participants. This analysis allows me to check whether involving chiefs in selecting community workers leads to a higher prevalence of costly actions to avoid the public good among the poorest households and if a simple progressive mechanism can counteract this pattern. Results show evidence consistent with regressive informal taxation by chiefs. In particular, although the average participant acts more pro-socially when selection is delegated to chiefs relative to random selection, participants at the bottom of the wealth distribution under selection chiefs significantly distort their behavior by accepting more wage cuts to avoid the public good. For example, participants in the bottom 10% of material wealth increase their likelihood of accepting a lower wage to work earlier by 40% relative to random selection. This effect reveals a large behavioral response and shows the prevalence of regressive informal taxation in this context, where coercion plays a more important role in how customary or traditional leaders informally tax the poorest households.

More surprisingly, progressive selection backfired and generated regressive patterns very similar in magnitude to selection by chiefs. This result is mainly driven by differences in how

4

households perceive their relative living conditions versus survey approaches to proxy for rural material wealth. This result points towards the perils of implementing redistributive policies in contexts where wealth is hard to measure and where policymakers disagree with citizens on the standards used to redistribute resources.

I.A Related work

The first line of work I contribute to has focused extensively on how chiefs affect development in sub-Saharan Africa (Herbst, 2000; Baldwin, 2015; Bulte *et al.*, 2018). In this context, scholars have focused on the how lack of electoral accountability affects how chiefs operate (Acemoglu *et al.*, 2014b; Baldwin, 2013; Baldwin and Holzinger, 2019). This has lead to multiple studies on whether customary forms of governance complement or substitute democratic ones (Holzinger *et al.*, 2016; Henn, 2023; van der Windt *et al.*, 2019). I contribute to this comparative study of the chieftaincy by providing empirical evidence of whether chiefs are regressive when providing local public goods. If so, this would be consistent with chiefs representing the interests of local elites and opens the door to redistribution through more democratic institutions (Acemoglu and Robinson, 2000).

Multiple studies have focused on specific roles chiefs can play in development policy. This includes tasks like collecting taxes (Balan *et al.*, 2022), targeting subsidies (Basurto *et al.*, 2020), managing local projects (Casey *et al.*, 2023), or land allocation in rural areas (Goldstein and Udry, 2008; Honig, 2017). My work adds to these studies by studying another instance in which chiefs have to choose beneficiaries of a development program. However, the novelty of my study comes from focusing on how this selection selection process alters citizens incentives to contribute to local public goods.

I particularly contribute to the work assessing how traditional leaders solve local public goods problems and promote cooperation —work not exclusively focused on chiefs but generally on customary leaders in different contexts (Diaz-Cayeros *et al.*, 2014; Alatas *et al.*, 2019). This literature also includes the extensive work on informal taxation and how traditional leaders fund local public goods Olken and Singhal (2011); van den Boogaard *et al.* (2019); Walker (2018). Within this space, this study is closely related to the work of Beekman *et al.* (2014), who studies how corruption by chiefs in Liberia creates disincentives to cooperate, and the work of Goist and Kern (2018), who implement a lab-in-the-field experiment to study how chiefs promote cooperation. I add to this work by implementing a field experiment framed as a real development policy where chiefs make consequential decisions and carefully measure how this alters individual decision making in the face of redistribution.

Finally, my work also adds to the extensive work on development economics on instruments to target anti-poverty measures to the individuals who need it the most(Hanna and Olken,

2018; Elbers *et al.*, 2007; Haushofer *et al.*, 2022). This includes work on targeting instruments relying on community leaders (Alatas *et al.*, 2019; Basurto *et al.*, 2020), peer information (Dupas *et al.*, 2022; Trachtman *et al.*, 2022), or poverty indices based on household surveys (Banerjee *et al.*, 2020; Brown *et al.*, 2018). I contribute to this strand of work by adding evidence on how citizens respond to different redistribution schemes involving traditional leaders, random mechanisms, and progressive schemes. This piece of evidence is key to expand this literature by understanding how redistribution mechanisms are perceived by citizens, the incentives they generates, and thus their social and political feasibility.

II   Setting

Sierra Leone is a relatively small country in West Africa and is one the poorest countries in the world, ranking 181 out of 191 in terms of the Human Development Index reported by the Unite Nations in 2022. On top of this, the central state often lacks the capacity to provide quality public services. As a result, it often ranks in the bottom 10th percentile of Government Effectiveness according to the World Bank. These problems are magnified in rural areas, where food insecurity is prevalent and economic opportunities outside of subsistence agriculture are rare. It is in this rural context where traditional or customary leaders are the most influential.

Given the lack of state presence, customary institutions then become highly relevant for people's livelihoods by regulating multiple dimensions of people's lives partially in response to the harsh economic conditions of these areas (Bulte *et al.*, 2018). Not surprisingly, this leads to citizens overwhelmingly having higher contact with their traditional leaders called chiefs, as well as displaying more trust in them relatively to any type of publicly elected leader[2]. This is an important reason why chiefs are in such a good position to coordinate local public good provision in this context.

In practice, chiefs in Sierra Leone are local elites that have important political roles in the areas they control. Their power is made legitimate using arguments that reference tradition and culture and it is often unrelated to ethnic allegiances (Honig, 2022). As a result, they take on the role of chiefs for life, need to be descendants of specific ruling families that vary by chiefdom, and are appointed using internal elections among local elites (Acemoglu *et al.*, 2014a; Bulte *et al.*, 2018). All these characteristics makes them institutionally very different from government officials and democratically elected leaders. Nonetheless, the Chieftaincy in Sierra Leone is not at all separate from the state and in many ways is a formal institution. The

---

[2]Based on the Afrobarometer 2021, 48% of respondents have contacted more than a few times their traditional leaders about local problems relatively to only 19.5% for Local Councillors, the lowest level elected official in the country. Similarly, 32% of citizens significantly trust their traditional leader relative to only 13.3% for their Local Councillor.

national constitution recognizes the role that chiefs play in rural governance, the Chieftaincy Act of 2009 codifies in law the exact roles that customary leaders play within the country, and they even have seats in the national parliament. All of this is evidence of how the state and democratic institutions have heavily relied on the chieftaincy to exert control of its territory and often act as complements even when there are some conflicts of interest (Baldwin, 2015; Henn, 2023).

In terms of how they operate, traditional or customary leaders across sub-Saharan Africa tend to be in charge of land management, conflict resolution, and public good provision (Baldwin, 2015; Honig, 2022; Casey *et al.*, 2023). Sometimes chiefs are even entitled to collect certain taxes and partially share revenues with government (Acemoglu *et al.*, 2014a). In Sierra Leone, all of these tasks are distributed along an internal hierarchy, which has Paramount Chiefs on top, goes down to section chiefs, and ends in village chiefs or headman. Importantly, the experiment conducted in this paper was done by collaborating with these lower level village chiefs, as they are the face of the chieftaincy for most citizens and are in charge of coordinating local public good provision. In my sample, these chiefs are on average 56 years old and only 16% of them completed primary schooling. This is in line with previous work concerned with these local leaders not being highly educated and thus limiting their ability as local project managers (Casey *et al.*, 2023).

In terms of financing local public goods, chief often rely on methods that the literature has called informal taxes (Olken and Singhal, 2011). In Sierra Leone, these contributions of cash, goods or labor to community projects are quite common, and chiefs are often coordinating their collection. According to van den Boogaard *et al.* (2019), 64% of all taxes paid in a year by an average household are collected by chiefs. Within the sample of participants surveyed for this study, 80% of them reported having made an informal contribution to local chiefs within the last 6 months. Moreover, 56% of respondents made monetary contributions to chiefs, 73% of respondents made labor contributions, and 22% contributed in-kind. Overall, this reveals the prevalence of the types of informal taxes studied in this paper.

III   Experimental design

The experiment is embedded in a simple program offering one days jobs to people in rural Sierra Leone with an associated public goods problem, where I randomize communities into different methods to select who works for the public good and who works to earn for themselves. The program was implemented in 88 communities across six districts of Sierra Leone. I proceed by explaining in details how the program worked and how it created a collective action problem within the communities visited as participants essentially faced an informal tax on their labor. Then, I go over the different selection methods randomized across communities.

I then explain the different outcomes this experiment allows me to measure and conclude this section with a description of my experimental sample and how the randomization was balanced across communities.

III.A   The jobs program

The basic idea of the program was to provide one-day jobs to people in each community visited, thus creating an activity where participants are individually rewarded for their effort. The job task is classifying Sierra Leonean names by gender and ethnicity; a task that is framed as useful for a local NGO which is outsourcing it at scale in rural communities. Importantly, this implied that participants needed to have very basic reading skills to be part of the program and thus the results of this experiment should be interpreted bearing in mind that they apply to relatively young and educated citizens of rural Sierra Leone. By taking up this one-day job offer, participants needed to spend around one hour to earn maximum 2 USD, which is equivalent to twice the average daily income reported by participants.

To create a public good problem, participants were informed that some of them will be selected to work for their community. I call them "community workers". In practice, selected participants had to complete the job task as all others, but the majority (90%) of their earnings was automatically donated by the program to a real local public good. Participants who were not selected kept all of their experimental income. As a result, community workers did not profit directly from their effort which should have made the program less attractive to them if they do not internalize the benefits of the public good. In all communities visited the public good to which community workers donated their labor was the closest clinic or health center. This was explained to all participants.

It is worth noting that citizens did not operate as intermediaries and the program directly took the earnings of community workers and donated them to their local clinic. This means the informal labor tax created in this experimental setting was fully enforced. This means that enforcement of contributions is an important dimension this experiment abstracts away from. Since it is likely that chiefs have a clear advantage on this dimension, the decision to implement perfect enforcement lets me focus on the inherent redistribution tension of informal taxation schemes.

In order to capture each participant's willingness to be be selected to contribute their labor to fund their local health clinic, I implemented the jobs program over the course of two days in each community. This allowed me to inform participants that the selection of community workers was only taking place on the second day. The two stage of the jobs program is explained graphically in Figure 1. This key feature of the experimental design implies that participants who want to avoid being selected should prefer to work on the first day of the program as

working earlier guarantees they get to keep their earnings. I leverage this feature to elicit participant's preferences over which day to work allowing participants to accept a wage-cut in order to work earlier. With this strategy I am able to measure which participants want to avoid the public good and are willing to engage in costly actions to do so. This is one of the main outcomes of the experiment and it is explained in more detail in subsection III.D.

Figure 1: Two-day design of the experiment for each community
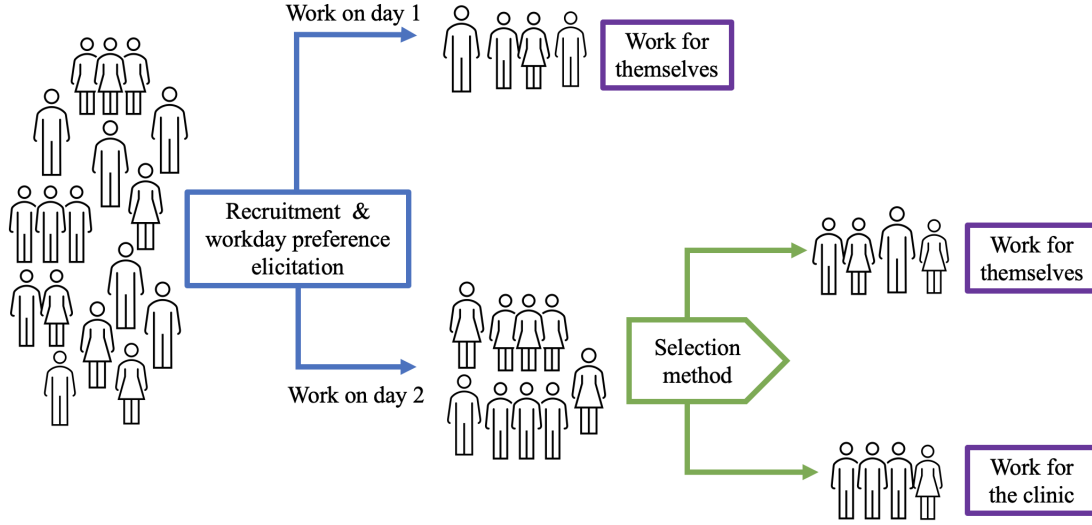
During the program participants answer two individual surveys which provide useful information used in the experimental analysis. Upon recruitment, I collect participant's preferences over which day to work and an extensive battery of demographic questions. Importantly, this survey allows me to measure multiple proxies for the relative household wealth of all participants, as well as other variables that might meditate how participants might react to the experiment. I survey participants again when they complete the name classification task offered in the jobs program. This second survey allows me to precisely measure how much effort did participants exert in the task, both for selected and non-selected participants, as well as additional exit questions measuring their satisfaction with how the program operated in their community.

III.B   Sample

Data collection for this experiment was conducted in six districts across Sierra Leone, 3 in the northern region of the country and 3 in the south. Within districts, 88 communities were sampled from 44 sections that were selected due to having a health center for which we know its location and such that the area was relatively low in population density. The first requirement was for ease of the experimental protocol as public good contributions were donated to local

clinics and the second requirement was set in order to study remote locations far from the state and were chiefs tend to be more important. Figure A.1 in the Appendix shows the locations in Sierra Leone where the experiment took place.

Table 1: Balance Table

| Variables | N | Outcome Mean | | | | F Test |
		Full Sample	Chief	Progressive	Random	P value
Panel A: Community Characteristics						
No. of Households	88	54.057	52.567	51.077	57.875	0.758
Distance to HQ town (Min)	88	95.239	107.667	96.115	82.875	0.527
Chief's Age	88	56.409	59.033	50.615	58.656	0.014
Educated Chief	88	0.159	0.100	0.192	0.188	0.489
Panel B: Household Characteristics						
Age	1,361	25.393	24.968	25.069	26.042	0.275
Male	1,361	0.631	0.603	0.664	0.631	0.113
Years in Community	1,361	13.972	12.871	14.522	14.564	0.050
Household Size	1,361	7.762	7.777	7.453	7.990	0.076
Years of Education	1,361	9.896	9.991	9.827	9.863	0.827
Job Test (Min)	1,361	2.536	2.503	2.529	2.571	0.894
Works Outside Home	1,361	0.351	0.358	0.326	0.365	0.733
Subsistence Farming	1,361	0.359	0.367	0.346	0.363	0.792
Weekly Income (USD)	1,088	4.922	4.982	5.016	4.787	0.900
Wealth Index	1,361	0.000	0.004	-0.028	0.018	0.611
Clinic Visits (per year)	1,361	13.877	14.019	14.476	13.275	0.687
Panel C: Relationship to Chiefs						
Seeks Advice of Chief	1,361	0.711	0.702	0.746	0.691	0.429
Close to Chief (Index)	1,361	0.000	0.017	-0.007	-0.010	0.859
Freq. of Informal Tax (6 months)	1,361	1.284	1.241	1.464	1.184	0.424

Notes: Community Characteristics were obtained from surveying the village chief of each community visited. Educated chief measures if they completed primary. Remaining variables are measured for each participant through individual surveys. Job Test measures the time spend by participants completing a test of the job task used in the experiment and proxies productivity. Works Outside Home and Subsistence Farming are based the main activity each respondent engaged with in the last week. Weekly income is only measured for people reporting positive income. The Wealth Index combines multiple questions about asses ownership, materials of dwelling, ability to cope with shocks, food security and health security. This index is standardized to have mean zero. Seeks Advice of Chief measures if a respondent asks for advice once a month or more frequently. The index measuring closeness to the chief standardizes and averages multiple variables proxying for how close is each participant to their chief, using variables like distance to the chief's house, how often they seek advice from them, whether they support land titling, and if they are related to the chief. Frequency of informal taxation measures the number of times in a six month window that a responded has contributed money, labor, or goods to the chief for a community activity or project. The F Test jointly tests of balance on treatment assignment using a regression with district fixed-effects and standard errors clustered at the community lever.

A more detailed description of the experimental sample can also be found in Table 1. The communities were the experiment was conducted, which had on average 54 households and were on average one hour and a half away by motorbike from their chiefdom headquarter town. These are relatively small communities far from urban centers. Participants tend to be young, with the average age begin 25 years old. This is by construction, as the experiment required participants to know how to read in order to work, which also explained why on average participants have completed 9 years of education.

Two other set of variables are also very relevant for this paper, variables that proxy for rural wealth and variables that proxy for how close is each participant to their local chief. It is important to note that participants have significantly low incomes, way below any monetary poverty line, and work mostly in agriculture, either mostly for subsistence or to sell. Thus, in this context I measure household wealth through a standardized set of questions measuring how many assets or animals do they own, the materials used to construct their dwelling, whether they own any land, and their ability to cope with shocks. I validated this measure through multiple pilots in this context. Within the experimental sample, this index is an important predictor of food and health insecurity[3].

their exposure to food or health insecurity. Regarding participants' relationship to their local chiefs, I also create an index that standardizes and averages many variables proxying for being socially proximate to the village chief, such as distance from participants' dwelling to the chiefs house, how often do they seek advice from their chief, whether they believe government should not title land, and family connections to the chief. For reference, 71% of participants regularly seek advice from them and on average they have been asked to pay an informal 1.3 times in the last six months —including labor, money and in-kind contributions.

III.C   Selection methods

The treatment arms in this experiment are defined by the selection method used in each community to select community workers. The selection method assigned to each community was extensively explained to participants during the recruitment survey. However, to maintain the incentives for participants to work on the first day if they wanted to avoid being selected, each of these selection methods explained below was only implemented with participants assigned to work on the second day. The three selection methods randomized in this experiment are:

1) Random Selection: This treatment arm serves as a benchmark for the experiment as it uses a simple transparent lottery to decide whether each participant working on the second day is selected to work for the clinic or not. Within communities assigned to this method, I cross-randomize participants into three different lotteries to determine their selection: 0% chance of selection, 25% chance of selection, and 50% chance of selection. Participants were approximately split equally among these three lotteries. This randomization allows me to use

---

[3]For example, a one standard deviation increase in this wealth index is associated to a 17% decrease in the likelihood of participants worrying about not having enough food to eat over the last month, and a 60% decrease in the likelihood of a participant reporting not eating for a day during the last month. Also, a one standard deviation increase in this wealth index is associated to a 50% decrease in the likelihood that a participant reports someone in their household had a health emergency that could not be treated during last month, and a 20% decrease in the likelihood that someone in their household had to skip school or work due to health problems. These estimates come from simple regressions controlling for regional differences and are robust to controlling for other personal characteristics.

individual-level variation to estimate how participants react to different likelihoods of being selected in an environment without any discretion or any bias in the selection process.

2) Chiefs Selects: In communities assigned to this selection method, I asked village chiefs at the end of the first day of work in each community to select who they believe should be a community worker and help fund the local clinic. This involved explaining to the chief the nature of the program, particularly how some people were going to work and keep their earnings while others were working to help the community. Chiefs selected community workers by choosing half of the people from the list of all participants in the program. However, as some participants completed the work task on the first day, I only selected as clinic workers participants selected by the chief and available to work on the second day. I interpret this method as the status quo approach used in rural communities to decide who bears the costs of funding or providing local public goods via informal labor taxes(Olken and Singhal, 2011).

3) Progressive Selection: In communities assigned to this arm participants were explained that the program wanted to make sure people in relatively better conditions were selected as community workers. To do so, the program used survey questions about participants' living conditions asked during recruitment to select relatively wealthier households to work for the clinic. In practice, I created an index of household wealth tailored to rural areas in this context. This was done to mimic a simple implementation of Proxy-Means Tests (PMT) targeting (Brown *et al.*, 2018). Field teams had access to this index for each participant and selected half of workers available on the second day based on this wealth index. This treatment arm allows me to measure precisely how participants react to progressive redistribution schemes based on simple surveys as an alternative to usual informal taxation by traditional leaders.

The randomization used to assign these three selection methods was stratified at the district level for the 6 districts where the program was implemented. From the 88 communities visited, 32 were assigned to to random selection, 30 were assigned to selection by chiefs, and 26 to progressive selection. In each community roughly 15 participants were enrolled in the study. This lead to 502 participants assigned to random selection, 466 assigned to selection by chiefs, and 393 assigned to progressive selection. Table 1 also shows balance on observable characteristics among communities and participants assigned to each selection method.

III.D   Outcomes

I separate the outcomes analyzed in this experiment into two stages based on the timing of measurement within the jobs program. The first stage outcomes capture participants behavior upon recruitment. This includes their preferences over which day to work on and initial reactions towards the selection method used in their community before any selection is made. Second stage outcomes measure behavior once participants know whether they are selected or

not as community workers. This includes for example the effort participants exert in the job task for the public good. These outcomes are explained below:

(i) First stage outcomes:

Eliciting participant's preferences over which day to work allows me to capture whether people engage in costly actions to avoid contributions to a public good. To measure this, I use a Multiple Price List (MPL) to elicit how big of a wage-cut are participants willing to accept in order to work on the first day[4]. These decisions are made after recruitment so participants know how selection of community workers will be done in their community. Participants know that their the program will try to use their choices to decide if they work on the first or second day the program, guaranteeing incentive compatibility. However, some random set of participants are only offered jobs on the second day in order to study second stage outcomes without selection bias from decisions in the first stage.

When deployed on the field at scale, this MPL procedure was not very successful at measuring interior answers for the wage-cut participants were willing to accept in order to work earlier. In the data 75% of participants either accept no wage-cut or accept all of them[5]. This probably reveals challenges when explaining this relatively simple protocol among the procedures used in the literature to measure impatience. However, this procedure still measures participants preferences. The fact that only 1.69% of respondents displayed multiple crossings or inconsistent answers during the MPL questions suggests this approach is relatively reliable and participants understood the questions. Therefore, all the analysis below will use two binary variables, one taking value of one when a participant accepts all wage-cuts and one taking the value of one when a participant accepts at least one wage-cut. Importantly, these alternative variables still capture whether participants distort their behavior when facing an informal tax on their labor. Moreover, it captures uncooperative behavior that is inefficient by reducing the resources the program brings to the community. Conversely, a selection method that decreases this outcome reflects an informal tax that distorts behavior less and aligns individual incentives with social outcomes.

Measuring behavior at this experimental first-stage is also an important contribution to the literature on informal taxation which often fails to distinguish voluntary giving from contributions under coercion. This is an important distinction as concerns over the incidence of informal taxation should particularly focus on the latter and avoid measuring the former. I improve upon previous work as incentives in the experiment are such that participants who voluntarily wants to help the public good should be less likely to accept wage-cuts to work earlier. I am then able to capture behavior changes reflecting distortions to individual incen-

---

[4]Participants making multiple decisions of the form "do you prefer working for \$X today or \$Y tomorrow" for different values of X and a fixed value of Y, where $X \leq Y$.

[5]The MPL procedure offered wage-cuts from 12% to 75% of their experimental income.

tives due to coercion. If I just focused on measuring contributions to a public good this might overestimate the burden of informal taxation and how it is distributed due to voluntary giving. On top of this, the two-day design also allows participants to mask their decision to avoid the public good under other reasons that might lead them to prefer working on the first day. This is an important feature of this experimental design as other types of questions about public good contributions involving traditional leaders and development programs might suffer from social desirability bias.

(ii) Second stage outcomes:

After the selection of community workers was done in each community participants could then engage with the job task knowing whether they would earn for themselves or whether they were selected as community workers. At this stage participants were presented with the name classification task which involved classifying local names by gender and ethnicity to help a local NGO. This involved completing 8 pages with 10 names each page. Participants know that to receive they payment that was agreed upon recruitment they had to complete all the pages, but they could stop before hand if they wanted and be paid proportionally to completion. In practice, 92% of the participants completed 8 pages, so this outcome measuring the intensive margin of effort provides very little information useful for this experiment.

In order to get an additional measures of cooperation, I added extra question for both community and regular workers after they completed the main job task. First, I asked community workers whether they would be interested in completing three extra pages of classification to increase the money donated to their local clinic. Participants could either accept or refuse this offer, which I use as another outcome in this second stage to measure how each selection method motivates selected participants to exert additional effort for the public good. Second, for participants working for themselves I surprised them after the job by asking them if they wanted to donate any percentage of their earning towards the public good. I use this outcome to measure altruism by people who are not informally taxed but still could display cooperative behavior.

The fact that this experiment allows for participants to avoid public good contributions by taking lower wages and work earlier means that second stage outcomes suffer from selection bias towards relatively pro-social participants or participants with strong beliefs of not being selected. To overcome this issue, the experiment only used the work day preferences on a random subset of participants to decide if they could avoid selection. Therefore, this experimental design is also able to inform what is the effect of chiefs or progressive selection both conditionally and unconditionally of their first stage decisions. The only caveat of this feature is that it decreases the sample used to study second-stage outcomes. The only caveat for this exercise is the limited sample used to study the relevance of selection.

IV   Results

I analyze this experiment in the following order. I begin by describing participant's behavior under the random selection arm as it provides a natural benchmark for citizens willingness to engage with this experimental public good. This arm also introduces individual-level variation useful to characterize how first-stage outcomes vary with controlled changes in the selection method. I then analyze the effect of delegating the selection of public good workers to chiefs and the effect of implementing a progressive selection method. Finally, I explore effect heterogeneity of both treatment arms, particularly along participant's household wealth.

IV.A   Random selection

The random selection treatment arm provides a useful starting point to analyzing this experiment as it uses simple and transparent lotteries to decide who works for the clinic. It allows me to describe how participants behave in this experiment when no particular citizen is targeted by an informal tax on their labor and the mechanism used to select community workers is clearly understood. To take advantage of this arm, I create individual-level variation in terms of the exact lottery used to decide if participant's are selected as community workers. Participants are randomly assigned to one of three possible lotteries, facing a likelihood of selection of either 0, 25%, or 50%. Let's denote this probability for participant i as $Prob_i$. I use this variation to study how responsive are participants to changes in this random selection procedure in order to validate this experimental setup. This is useful to then interpret the main experimental results discussed in Section IV.B

I study the random selection treatment arm by using regression 1 below. I begin by estimating the elasticity of costly actions to avoid contributions to random increases in the informal tax-rate participants face. I expect this elasticity to be positive as participants should prefer to work earlier in the experiment for a lower wage when they are more likely to be picked as community workers. This is captured by $\theta_0 > 0$ in regression 1. Importantly, in this specification I control for individual characteristics measured upon recruitment[6] and add enumerator times district fixed effects to capture implementation and geographic differences in behavior.

$$Y_{icd} = \mu_d + \theta_0 Prob_i + \psi X_i + \varepsilon_{icd} \tag{1}$$

Moreover, I study two additional hypothesis that might influence how responsive are participants to changes in the likelihood of selection by allowing specification 1 to capture heteroge-

---

[6]Control include demeaned variables measuring age, sex, education, occupation, wealth levels by the wealth index, productivity in the job task measured by a test upon recruitment, closeness to the village chief, and how often do participants visit the local clinic.

nous responses to changes in $Prob_i$. First, I explore heterogeneity by wealth and test if relatively poor households are more or less responsive changes in the likelihood of selection. Given this experiment recreates a two-period consumption problem, we can expect relatively rich households to have less incentives to smooth consumption over periods and act as risk-neutral agents. This will lead to larger behavior responses to changes in the likelihood of selection among the relatively wealthy. Second, we can also expect that participants who internalize the benefits of working for a public good will be less responsive to being informally taxed by this random selection process. Conversely, participants who who do not value the public good should be more likely to react by working earlier then they are more likely to be selected.

Results from this section are shown in Table 2. To start, this table shows how on average, when participants cannot be selected as community workers, approximately 24% of participants accept all wage-cuts and 50% accept at least one in order to work earlier in the experiment. The first row of this table shows estimates for θ_0 under different specifications. Columns 1 and 4 display positive estimates as expected, although not statistically different from 0 at normal significance levels. Nonetheless, they imply that participants that a increasing the likelihood of selection from 0 to 50%, leads to a 2.7 percentage point increase in the likelihood of accepting all wage-cuts and a 5 percentage point increase in the likelihood of accepting at least one. They both represent roughly 10% increases relative to each outcomes mean, however estimates are noisy and not statistically different from zero. This can also be explained by participants displaying relatively high levels of altruism an internalizing the benefits of the public good, which would decrease this estimated elasticity to the point where very high sample sizes are needed to detect them.

Table 2 also shows evidence consistent with the two additional hypothesis involving heterogenous responses changes in the likelihood of success. Columns 2 and 4 show as expected that relatively poor participants with negative wealth index are more likely to accept lower wages to work earlier, and more importantly that they tend to react less to changes in the likelihood of selection. Although noisy, these results suggest for example that increasing the likelihood of selection from 0 to 50% for a participant 1/2 std above average wealth, leads to a 17 percentage point increase in the likelihood of accepting all wage-cuts.

On the other hand, columns 3 and 6 of Table 2 show that, as expected, participants that do not prioritize development expenditures directed towards their local clinic display substantially higher elasticity of behavior to changes in the likelihood of selection. Moreover, the estimated elasticity for both outcomes is basically zero for participants who do prioritize expenditures directed towards the experimental public good. For example, column 6 suggests that increasing the likelihood of selection from 0 to 50% for the average participant that does not prioritize the clinic leads to a 12 percentage point increase in the likelihood of accepting any wage cut. This result is statistically different from 0 using a 10% significance level. Column 4 shows an

estimate of similar magnitude.

Table 2: Results from Random Selection

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Accept All Wage-cuts			Accept Any Wage-cut		
Lottery Likelihood	0.055	0.048	0.210	0.104	0.092	0.253*
	(0.089)	(0.088)	(0.129)	(0.106)	(0.106)	(0.150)
Wealth Index	-0.033	-0.099	-0.029	-0.068	-0.181*	-0.064
	(0.057)	(0.078)	(0.057)	(0.071)	(0.093)	(0.071)
Likelihood X Wealth Index		0.302			0.521*	
		(0.249)			(0.305)	
Prioritizes Clinic	0.059		0.121**	0.012		0.072
	(0.041)		(0.058)	(0.046)		(0.066)
Likelihood X Prioritizes Clinic			-0.288			-0.276
			(0.185)			(0.213)
Outcome Omitted Group	0.247	0.250	0.216	0.493	0.497	0.464
Observations	502	502	502	502	502	502
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include enumerator times district fixed effects. Lottery Likelihood is a variable randomized at the individual level within this treatment arm and it takes values 0, 0.25 or 0.5. The Wealth Index averages multiple standardized variables that proxy for wealth in rural areas. Prioritizes Clinic is a binary variable of whether a respondent believes their health clinic should be prioritized by development programs. Accept All Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted. Robust standard errors in parentheses ***p<0.01, **p<0.05, *p<0.1

Overall, these results are consistent with simple predictions that could be tested using the random selection arm and thus provide evidence this experimental setup can be used to study informal taxation. I do not want to over-interpret these results as they are not the main contribution of this paper, but they serve as validation that the experimental setup is capturing the desired behavior. That is, results are in line with basic predictions about how participants should behave when trading-off working for their own income and working for a public good —even if not always statistically significant which might be due to the small sample size. I now proceed to the main section of this paper, which takes random selection as a benchmark and studies how participants behave under alternative selection methods.

IV.B Main experimental results

The main results of this paper leverage the community level randomization to compare on aggregate how selection by chiefs and progressive selection differ from random selection. As outcomes, I use both first stage and second stage outcomes. The former capture if a participant

engages in costly actions to work earlier in the experiment by accepting all or any wage-cuts. The latter measure if community workers exert extra effort for the public good and whether regular workers behave altruistically and donate from their earnings to the public good. All of these outcomes speak to how efficient are these selection methods when using resources made available to communities, but the first stage outcomes reflect if participants distort their individual behavior when being informally taxed.

The regression I use in this section is shown in Equation 2. This specification again includes some individual controls for efficiency and adds enumerator times district fixed effects which capture implementation and regional differences. Standard errors in this specification are clustered at the community (village) level as this was the relevant unit for treatment assignment within each district, The effect of delegating selection to chiefs is captured by β_1 and the effect of implementing a progressive selection method is captured by β_2, both of which are estimated relative to participants facing random selection with positive probability.

$$Y_{icd} = \mu_d + \beta_1 Chief_c + \beta_2 Progressive_c + \psi X_i + \varepsilon_{ics} \tag{2}$$

All the outcomes Y_{icd} analyzed in this section are related to cooperative behavior and reveal the extent to which participants act such that resources from the program going towards the community are maximized. First-stage outcomes reveal how participants distort their behavior when facing an informal labor tax and choose to work earlier for a lower wage to avoid selection. Thus positive effects on these outcomes reveals uncooperative behavior and participants being willing to take on private costs to avoid contributions. I only analyze these outcomes for workers eligible to be selected, which means I drop participants in the random selection arm with zero chances of being selected. As selection by chiefs is the status quo selection method in this context and previous work emphasizes the role chiefs play in solving collective action problems, I hypothesize that for these outcomes $\beta_1 < 0$. As progressive selection bypasses local decision making and might be uncertain to communities, I expect $\beta_2 > 0$ for these outcomes. This would reveal an efficiency cost of progressive selection.

For second stage outcomes, I focus on whether selected workers agree to exert extra effort towards the public good and whether participants working for themselves donate money from their earnings to the public good. The former measures participants contributing effort towards the public good beyond what was originally asked and the latter captures altruism among people that were not selected. As before, due to the role chiefs play in solving collective action problems, I expect $\beta_1 > 0$ for these outcomes. Conversely, due to how progressive selection will assign relatively wealthy participants to work for the public good and relatively poor ones to work for themselves, I expect this to have negative effects on cooperation and hence expect that $\beta_2 < 0$. This would confirm that implementing a more progressive informal taxation scheme

has efficiency costs.

Results are shown in Table 3, which shows evidence of chiefs being particularly effective at promoting cooperative behavior. From columns 1 and 2 which study participants' preferences for only working earlier and avoiding selection, we can see participants in the Selection by Chief arm are approximately 8 percentage points less likely to accept all wage-cuts relative to behavior under random selection. From a base rate of 32%, this is a 25% significant decrease in this outcome as a result of delegating selection to chiefs. Although selection by chiefs seems to increase the likelihood of workers doing extra effort for the public good, this effect is noisy and not significant. Altruism by regular workers is basically not effect by chiefs being in charge of selection.

Overall, these results confirm the hypothesis that chiefs are specially important to align individual incentives with social goals, thus promoting more potential contributions to local public goods (Baldwin, 2015; Honig, 2022). Moreover, this is not driven by participants feeling pressured by chiefs, as direct coercion in the experiment was not possible and the experiment allowed participants to escape selection by working earlier. The fact that they could claim being busy and unable to work on the second day created plausible deniability which protected citizens from any direct type of social punishment for not contributing. This result does not disprove that chiefs rely on coercion to solve collective action problems, however it shows that the advantage of working through them also stems from promoting cooperative norms of behavior. Moreover, as this experiment finds not evidence of chiefs being particularly distortionary when informally taxing labor, this underscores work suggesting chiefs might hurt economic development by capturing civil society (Acemoglu *et al.*, 2014b). On the contrary, this experiment suggests chiefs are on average beneficial to promote collective investments through socially-aligned behavior.

Table 3 also shows that Progressive Selection had little effects across the board, generating no differences in behavior relative to random selection. The fact that coefficients associated with this treatment arm are mostly small and not significant at least show that progressive selection does not create important efficiency costs as was initially expected. The experiment does not show that participants suddenly shifted to working earlier under progressive selection or stoped exerting effort towards the public good, at least on this one-shot implementation of progressive informal taxation. This is then consistent with progressive taxation being feasible in this context without important negative repercussions, but more studies on this topic are needed. Nevertheless, progressive selection does not show negative effects only relative to random selection. Coefficients for both treatment arms in columns 1 and 2 are statistically different from each other (p-value=0.08) and thus, the experiment shows significantly more cooperation when chiefs are delegated with selection. This would indeed be a possible cost of replacing informal taxation schemes with progressive mechanisms commonly used by governments.

Appendix Table A.1 shows suggestive evidence supporting the idea that selection into the second stage of this experiment can explain why effects on effort might be small. As the experiment allowed for some participants to avoid the public good by working earlier, the main estimates for second-stage outcomes relies on a sample of participants more likely to act in favor of collective interests. Within this sample, it is possible that selection by chiefs or progressive selection might not lead to higher effort relative to random selection. However, when citizens do not have options to avoid being informally taxed and all citizens can be selected, chiefs might be more effective at promoting cooperation. Evidence in favor of this theory is shown in Appendix Table A.1. It shows that restricting the sample to a random set of participants whose preferences were disregarded in the first stage leads to larger coefficients for the chief selection arm and the progressive selection arm on variables measuring the intensive margin of effort. The main limitation of this exercise is the restricted sample size of these regressions and thus limited power to detect any effects.

Table 3: Main Comparison of Selection Methods

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Accept All Wage-cuts		Accept Any Wage-cut		Extra Effort		Donations	
Chief Selects	-0.085**	-0.075**	-0.032	-0.019	0.074	0.080	-0.004	-0.001
	(0.037)	(0.037)	(0.044)	(0.044)	(0.070)	(0.066)	(0.012)	(0.009)
Progressive Selection	-0.019	-0.009	-0.020	-0.011	0.058	0.042	0.003	0.010
	(0.041)	(0.040)	(0.042)	(0.042)	(0.070)	(0.071)	(0.013)	(0.011)
Omitted Outcome Mean	0.327	0.319	0.552	0.541	0.257	0.236	0.087	0.085
Observations	1,154	1,154	1,154	1,154	421	421	732	732
Sample		All Eligible Workers			Public Good Workers		Workers for Self	
Controls	No	Yes	No	Yes	No	Yes	No	Yes

Notes: All regressions include enumerator times district fixed effects. Chief and Progressive selection are binary variables reflecting treatment assignment at the community level. The omitted group are participants in communities facing random selection by lotteries. Workers in the random selection ineligible for selection by being assigned to zero chances of selection were dropped. Accept All Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted. Extra Effort is a binary variable of whether participants working for the public good agree to do extra work. Donations is measured for participants not selected to work for the public good and reflects the percentage of their income donated to the public good after completing the job. Standard errors clustered at the community level in parentheses ***p<0.01, **p<0.05, *p<0.1.

IV.C   Distributional implications

I now focus on the distributional properties of each selection mechanism in the experiment by comparing selection by chiefs and progressive selection to random selection for participants with different wealth levels within the communities visited. The main motivation of this analysis is to study if selection by chiefs leads to regressive patterns of informal taxation where the poorest households pay larger private costs to avoid selection. The progressive selection arm allows me to test if a simple policy tool can revert that pattern and whether the relatively rural

households resist this type of redistribution. As a result, this heterogeneity analysis mostly focuses on first-stage outcomes measuring costly actions by participants to avoid being selected.

To answer these questions, I run the same specification as before but now focusing on heterogenous treatment effects of the chiefs and progressive arms along the wealth dimension. I do this through two variables associated to household wealth and wellbeing: (a) the wealth index used capturing household's material wealth, and (b) average rankings of living conditions asked to participants about others in their community. Both of these measures were pre-specified and included in the pre-analysis plan for this study.

Doing this exercise reveals important heterogeneity in how participants reacted the to different selection methods, particularly showing that both chief progressive selection generated relatively regressive behavior relative to random selection. This is shown in Figure 2, which displays the conditional average treatment effect of both chief and progressive selection along the wealth index.

Regarding chief selection, this figure shows an estimated downward slopping effect, with some tendency to increase also towards the top of the wealth distribution. More precisely, this figure shows that participants half a standard deviation below average wealth do distort their behavior by being approximately 18 percentage points more likely to accept at least one wage-cut to work earlier and avoid selection. Relative to a baseline of approximately 45%, this is a massive and statistically significant effect suggesting a 40% increase for this outcome. Contrasting this with the average negative effect discussed in the previous section revealing chiefs actually promoted socially aligned behavior, this shows evidence of regressive informal taxation as suggested by previous work (Olken and Singhal, 2011; van den Boogaard *et al.*, 2019). Importantly, this regressiveness is now quantified in terms of distortions to individual behavior with particularly very poor households being willing to accept lower paying jobs to avoid being selected by their chiefs.

This result implies that for the average household, chiefs do seem to affect behavior by leading them towards socially desirable behavior. Nonetheless, this effect fades for households of lower wealth and actually leads to positive distortions among the poorest households in this context. This result is thus consistent with informal taxation under customary or traditional leaders targeting poor households and relying more heaving on coercion among this group. This has important implications for development policy since it suggests the most vulnerable citizens either end up contributing more to development projects that act as public goods or incur in private costs to avoid informal taxation. Both of these are undesirable results when policy aims to particularly improve the wellbeing of the poorest households in these contexts.

I also present evidence in the Appendix showing this result is robust to multiple ways of measuring wealth in this context. Table A.2 shows these results in regression form, both for a linear

Figure 2: Heterogenous Treatment Effects by Wealth
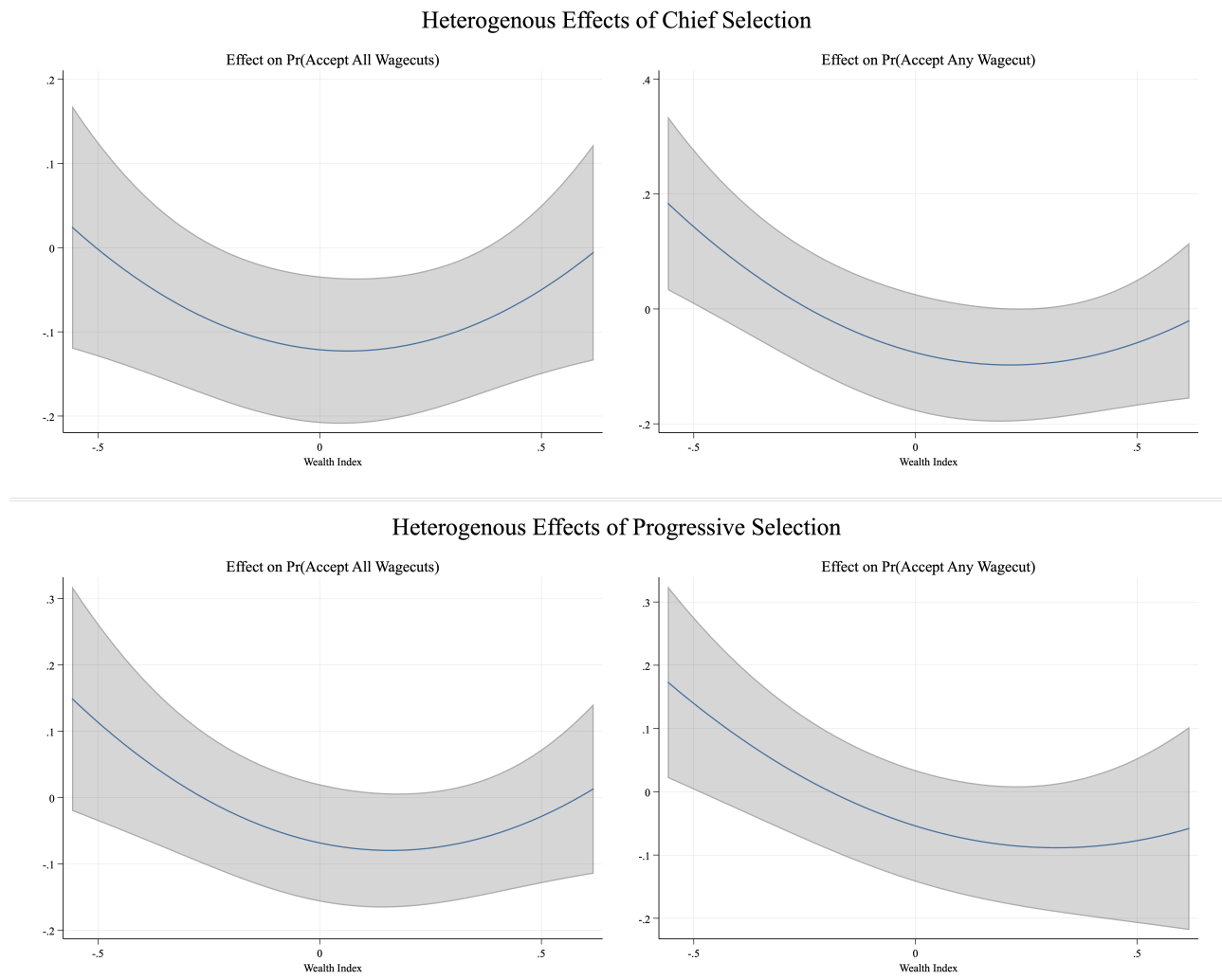
Heterogenous Effects of Chief Selection

Heterogenous Effects of Progressive Selection

Notes: Estimates obtained from main specification which includes enumerator time distric fixed effects and individual controls. Standard Errors clustered at the community level. Effects are relative to participants in the random selection arm who faced lotteries with positive probability of selection. Wealth Index is standardized to have mean zero and averages multiple proxies of wealth in rural areas. Accept all Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted.

interaction of the wealth index with the treatment variables as well as splitting the wealth index by terciles. Both of these specifications also show evidence of regressiveness in how chiefs informally tax labor in this context. Appendix Figure A.2 additionally shows that informal taxation by chiefs in this experiment is even more regressive when using an alternative proxy for wealth, which relies on the main principal component of all variables proxying for wealth as done by the Demographic Health Surveys (DHS). This resulting negative slope for the effects of delegating to chiefs even remains when using subjective wealth ratings by other community members by other participants, as shown in A.3. Regression estimates for these alternative specifications are shown in Tables A.3 and A.4.

Figure 2 additionally has important implications for the progressive selection arm. It shows

that this strategy drastically failed in making very poor households less likely to incur in private costs to avoid selection. Table A.2 shows this same result in regression form. Similarly to the effect of delegation to chiefs, participants half a standard deviation below average wealth do distort their behavior by being approximately 16 percentage points more likely to accept at least one wage-cut to work earlier and avoid selection. This effects is also quite large in magnitude and provides evidence that simple methods to implement progressive selection based on household surveys can backfire in contexts where people are not familiar with these approaches.

A theory that might explain this result is that households have perceptions of their wealth relative to others that substantially differ from surveys measures of material wealth used in the progressive selection arm. This could have led some households to feel targeted by progressive selection, when in fact they were not. Evidence supporting this theory is shown in Appendix Table A.5. Upon recruitment, I asked participants to rank themselves relative to others in their communities and I classify as them as overestimating wealth when they claim to be above average living conditions, but my standardized wealth index puts them in the bottom tercile. Columns 5 of Table A.5 shows that progressive selection particularly distorted behavior of people overestimating their relative wealth, making them 23 percentage points more likely to accept all wage-cuts. This has important implications for any policy that implements redistribution in contexts like rural Sierra Leone, as using assignment mechanisms that target low and high levels of wealth might backfire if participants do not have beliefs that align with these strategies to approximate material wellbeing.

V Conclusion

In this paper, I study a particular instance of a development project that required community contributions and allowed for informal taxation to take place. This happened around a simple jobs program that allowed some citizens in rural Sierra Leone to work and earn money for themselves, while others in their community were selected as community workers and exerted effort to fund a real local public good —their closes health clinic. I randomly implement three different methods to select community workers: random selection, selection by chiefs, and progressive selection using household surveys to proxy for rural wealth. Importantly, this experiment allowed me to measure whether participants incurred in private costs to avoid being selected as community workers by participating earlier in the jobs program and earning less money. This on top of measuring effort contributions towards the public good once community workers were selected and altruism among unselected workers.

This setup allowed to study whether chiefs are relatively effective at promoting socially-aligned behavior due to their established role promoting cooperating and organizing local public good

provision (Baldwin and Raffler, 2019; Honig, 2022). I also leveraged differences in participant's wealth to study if this potential benefit on including local elites burdened particularly poor households by making them targets of informal taxation (Olken and Singhal, 2011). Moreover, by simultaneously implementing a progressive selection method in this context, I could study the implication of using a simple policy tool to bypass local institutions and shift informal taxation pressure towards the relatively wealthy.

I find that chiefs are indeed quite important to fund local public goods by promoting cooperative behavior among citizens. In the experiment, this took the form of participants being more likely to delay work and be available to be selected as community workers relative to alternative selection mechanisms. Such behavior meant higher efficiency in communities where chiefs selected public good workers, as citizens accepted less wage-cuts to avoid selection and thus increased the total pool of resources that communities received. Importantly, this results also suggest that chiefs are on average less distortionary than random and progressive selection, which speaks to their importance for local public good provision and why they take such important roles around development policy in contexts with low state capacity (Balan *et al.*, 2022; Basurto *et al.*, 2020). This contrasts work suggesting chiefs are highly extractive and negatively impact economic development (Herbst, 2000; Acemoglu *et al.*, 2014b).

In terms of distributional implications, I do find evidence that chiefs are regressive, which in this context implies very poor households do react by distorting their behavior and accept lower paying jobs to avoid selection when chiefs are in charge. This confirms findings by previous work which relied on accounting for past contributions (van den Boogaard *et al.*, 2019; Walker, 2018). Instead, what this paper shows is that traditional or customary forms of governance not only collect more contributions from very vulnerable households, but also create incentives for them to avoid these informal redistribution schemes. Although this paper does not study the full consequences of such distortions, this paper does show evidence that households that really might benefit from extra earnings are willing to reduce them to avoid informal taxation. This points towards an important redistributive cost for development programs when relying on local leaders to operate.

Moreover, I also find that trying to implement progressive taxation in rural areas of low-income countries where wealth is hard to measure can backfire and create undesired incentives. This stems from the fact that randomly implementing progressive selection actually led to regressive patterns of behavior similar in magnitude to those observed when chiefs selected public good workers. I show this result stems from differences in how participants perceive their own wealth relative to others and how surveys measure material wealth. This happened because some participants classified as relatively poor believed they were relatively wealthy and distorted their behavior towards working earlier for lower wages. Broadly, this speaks to the challenge of implementing redistributive policies in contexts where wealth is hard to measure,

where citizens might be relatively uninformed of others' living conditions, or where policy-makers have different standards of wealth and wellbeing than citizens.

References

Acemoglu, D., Chaves, I. N., Osafo-Kwaako, P. and Robinson, J. A. (2014a). Indirect Rule and State Weakness in Africa: Sierra Leone in Comparative Perspective. In *African Successes, Volume IV: Sustainable Growth*, University of Chicago Press, pp. 343–370.

—, Reed, T. and Robinson, J. A. (2014b). Chiefs: Economic Development and Elite Control of Civil Society in Sierra Leone. *Journal of Political Economy*, **122** (2), 319–368, publisher: The University of Chicago Press.

— and Robinson, J. A. (2000). Democratization or repression? *European Economic Review*, **44** (4-6), 683–693.

Agranov, M. and Palfrey, T. R. (2015). Equilibrium tax rates and income redistribution: A laboratory study. *Journal of Public Economics*, **130**, 45–58.

Alatas, V., Banerjee, A., Hanna, R., Olken, B. A., Purnamasari, R. and Wai-Poi, M. (2019). Does Elite Capture Matter? Local Elites and Targeted Welfare Programs in Indonesia. *AEA Papers and Proceedings*, **109**, 334–339.

Balan, P., Bergeron, A., Tourek, G. and Weigel, J. L. (2022). Local Elites as State Capacity: How City Chiefs Use Local Information to Increase Tax Compliance in the Democratic Republic of the Congo. *American Economic Review*, **112** (3), 762–797.

Baldwin, K. (2013). Why Vote with the Chief? Political Connections and Public Goods Provision in Zambia. *American Journal of Political Science*, **57** (4), 794–809, _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/ajps.12023.

— (2015). *The Paradox of Traditional Chiefs in Democratic Africa*. Cambridge Studies in Comparative Politics, Cambridge: Cambridge University Press.

— and Holzinger, K. (2019). Traditional Political Institutions and Democracy: Reassessing Their Compatibility and Accountability. *Comparative Political Studies*, **52** (12), 1747–1774, publisher: SAGE Publications Inc.

— and Raffler, P. (2019). Traditional leaders, service delivery, and electoral accountability. *Decentralized governance and accountability*, pp. 61–90, publisher: Cambridge University Press Cambridge, UK.

Banerjee, A., Hanna, R., Olken, B. A. and Sumarto, S. (2020). The (lack of) distortionary effects of proxy-means tests: Results from a nationwide experiment in Indonesia. *Journal of Public Economics Plus*, **1**, 100001.

Basurto, M. P., Dupas, P. and Robinson, J. (2020). Decentralization and efficiency of subsidy targeting: Evidence from chiefs in rural Malawi. *Journal of Public Economics*, **185**, 104047.

Beekman, G., Bulte, E. and Nillesen, E. (2014). Corruption, investments and contributions to public goods: Experimental evidence from rural Liberia. *Journal of Public Economics*, **115**, 37–47.

Boltz, M., Marazyan, K. and Villar, P. (2019). Income hiding and informal redistribution: A lab-in-the-field experiment in Senegal. *Journal of Development Economics*, **137**, 78–92.

Brown, C., Ravallion, M. and van de Walle, D. (2018). A poor means test? Econometric targeting in Africa. *Journal of Development Economics*, **134**, 109–124.

Bulte, E., Richards, P. and Voors, M. (2018). Chiefs and Chieftaincy. In E. Bulte, P. Richards and M. Voors (eds.), *Institutions and Agrarian Development: A New Approach to West Africa*, Palgrave Studies in Agricultural Economics and Food Policy, Cham: Springer International Publishing, pp. 85–112.

Carranza, E., Donald, A., Grosset, F. and Kaur, S. (2022). The Social Tax: Redistributive Pressure and Labor Supply. *Working Paper*, p. 69.

Casey, K., Glennerster, R., Miguel, E. and Voors, M. (2023). Skill Versus Voice in Local Development. *The Review of Economics and Statistics*, **105** (2), 311–326.

Diaz-Cayeros, A., Magaloni, B. and Ruiz-Euler, A. (2014). Traditional Governance, Citizen Engagement, and Local Public Goods: Evidence from Mexico. *World Development*, **53**, 80–93.

Dupas, P., Fafchamps, M. and Houeix, D. (2022). Measuring Relative Poverty through Peer Rankings: Evidence from ivory coast.

Elbers, C., Fujii, T., Lanjouw, P., Ozler, B. and Yin, W. (2007). Poverty alleviation through geographic targeting: How much does disaggregation help? *Journal of Development Economics*, **83** (1), 198–213.

Exley, C. L. (2016). Excusing Selfishness in Charitable Giving: The Role of Risk. *The Review of Economic Studies*, **83** (2 (295)), 587–628, publisher: [Oxford University Press, The Review of Economic Studies, Ltd.].

Goist, M. and Kern, F. G. (2018). Traditional institutions and social cooperation: Experimental evidence from the Buganda Kingdom. *Research & Politics*, **5** (1), 2053168017753925, publisher: SAGE Publications Ltd.

Goldstein, M. and Udry, C. (2008). The Profits of Power: Land Rights and Agricultural Investment in Ghana. *Journal of Political Economy*, **116** (6), 981–1022, publisher: The University of Chicago Press.

Hanna, R. and Olken, B. A. (2018). Universal Basic Incomes versus Targeted Transfers: Anti-Poverty Programs in Developing Countries. *Journal of Economic Perspectives*, **32** (4), 201–226.

Haushofer, J., Niehaus, P., Paramo, C., Miguel, E. and Walker, M. W. (2022). Targeting Impact versus Deprivation.

Henn, S. J. (2023). Complements or Substitutes? How Institutional Arrangements Bind Traditional Authorities and the State in Africa. *American Political Science Review*, **117** (3), 871–890.

Herbst, J. (2000). *States and Power in Africa: Comparative Lessons in Authority and Control*. Princeton University Press, stu - student edition, 2 edn.

Holzinger, K., Kern, F. G. and Kromrey, D. (2016). The Dualism of Contemporary Traditional Governance and the State: Institutional Setups and Political Consequences. *Political Research Quarterly*, **69** (3), 469–481, publisher: [University of Utah, Sage Publications, Inc.].

Honig, L. (2017). Selecting the state or choosing the chief? the political determinants of small-holder land titling. *World Development*, **100**, 94–107.

— (2022). *Land Politics: How Customary Institutions Shape State Building in Zambia and Senegal*. Cambridge University Press.

Jakiela, P. and Ozier, O. (2016). Does Africa Need a Rotten Kin Theorem? Experimental Evidence from Village Economies. *The Review of Economic Studies*, **83** (1), 231–268.

Olken, B. A. and Singhal, M. (2011). Informal Taxation. *American Economic Journal: Applied Economics*, **3** (4), 1–28.

Peters, K. and Richards, P. (2011). Rebellion and Agrarian Tensions in Sierra Leone. *Journal of Agrarian Change*, **11** (3), 377–395, _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/j.1471-0366.2011.00316.x.

Trachtman, C., Permana, Y. H. and Sahadewo, G. A. (2022). How much do our neighbors really know? The limits of community-based targeting. p. 83.

van den Boogaard, V., Prichard, W. and Jibao, S. (2019). Informal taxation in Sierra Leone: Magnitudes, perceptions and implications. *African Affairs*, **118** (471), 259–284.

— and Santoro, F. (2023). Co-financing community-driven development through informal taxation: Evidence from south-central Somalia. *Governance*, **36** (2), 499–531, _eprint: https://onlinelibrary.wiley.com/doi/pdf/10.1111/gove.12678.

van der Windt, P., Humphreys, M., Medina, L., Timmons, J. F. and Voors, M. (2019). Citizen Attitudes Toward Traditional and State Authorities: Substitutes or Complements? *Comparative Political Studies*, **52** (12), 1810–1840, publisher: SAGE Publications Inc.

Walker, M. (2018). Informal Taxation Responses to Cash Transfers: Experimental Evidence from Kenya. *Working Paper*, p. 75.

A   Figures
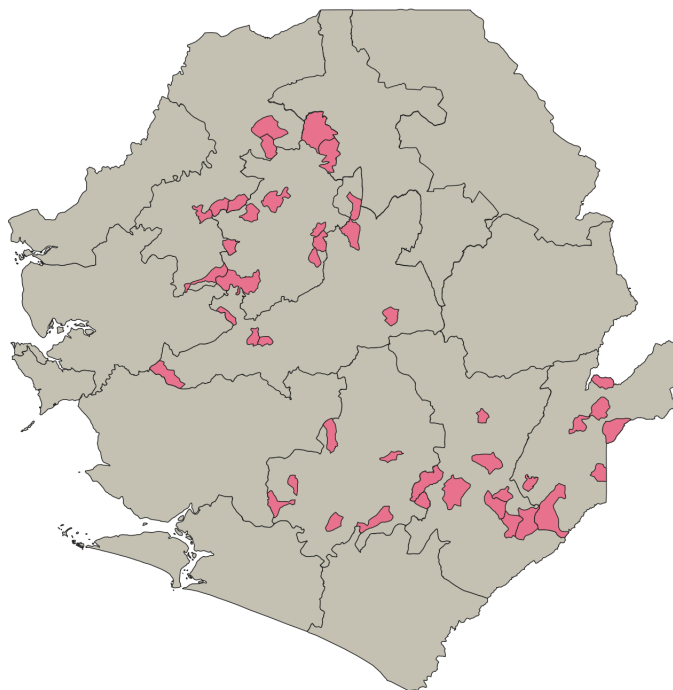
Figure A.1: 44 Sampled Sections

Figure A.2: Heterogenous Treatment Effects by Wealth (Version 2)
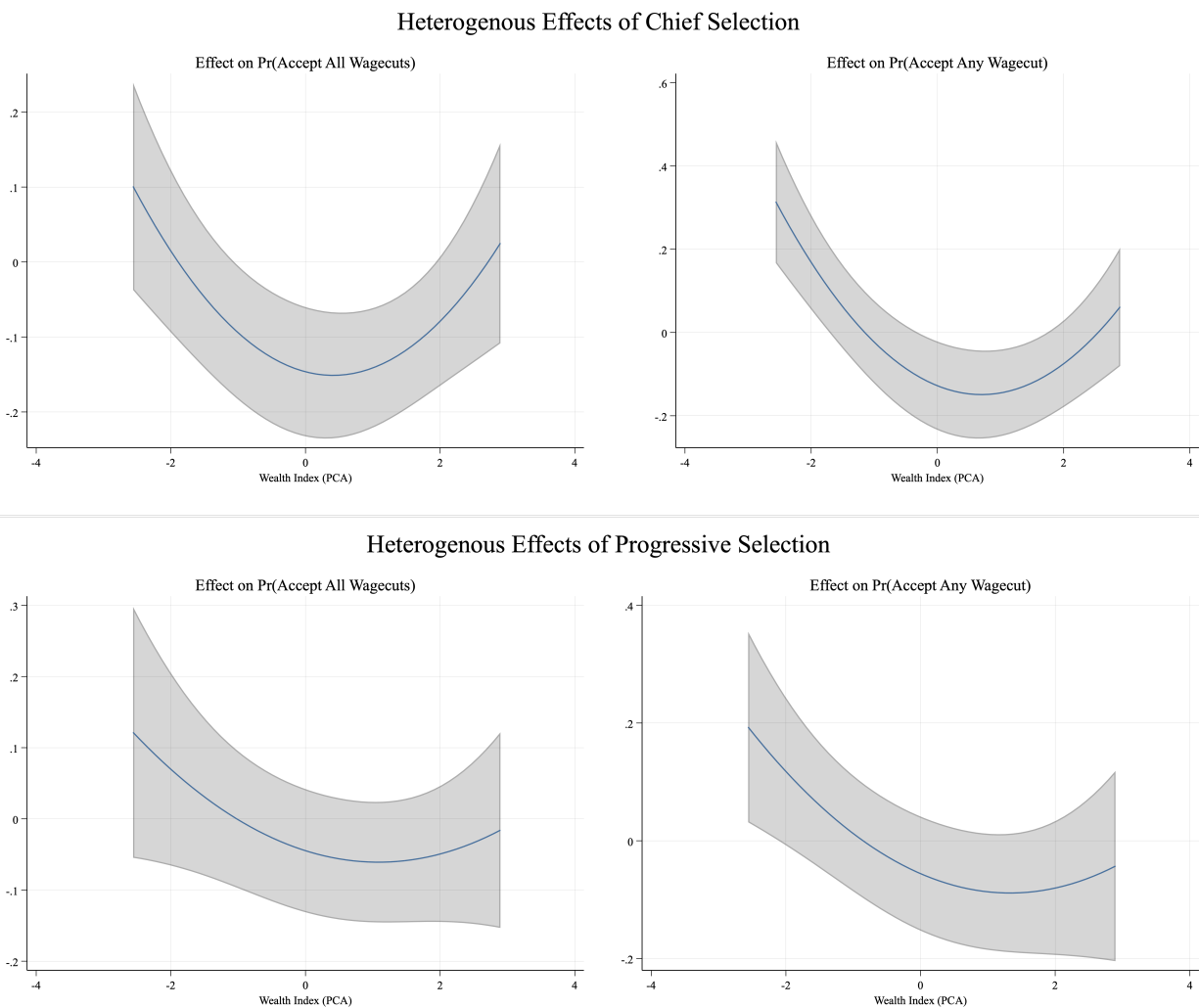
Heterogenous Effects of Chief Selection

Heterogenous Effects of Progressive Selection

Notes: Estimates obtained from main specification which includes enumerator time distric fixed effects and individual controls. Standard Errors clustered at the community level. Effects are relative to participants in the random selection arm who faced lotteries with positive probability of selection. Wealth Index is the first component of a Principal Component analysis from multiple proxies of wealth in rural areas. Accept all Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted.

Figure A.3: Heterogenous Treatment Effects by Wealth
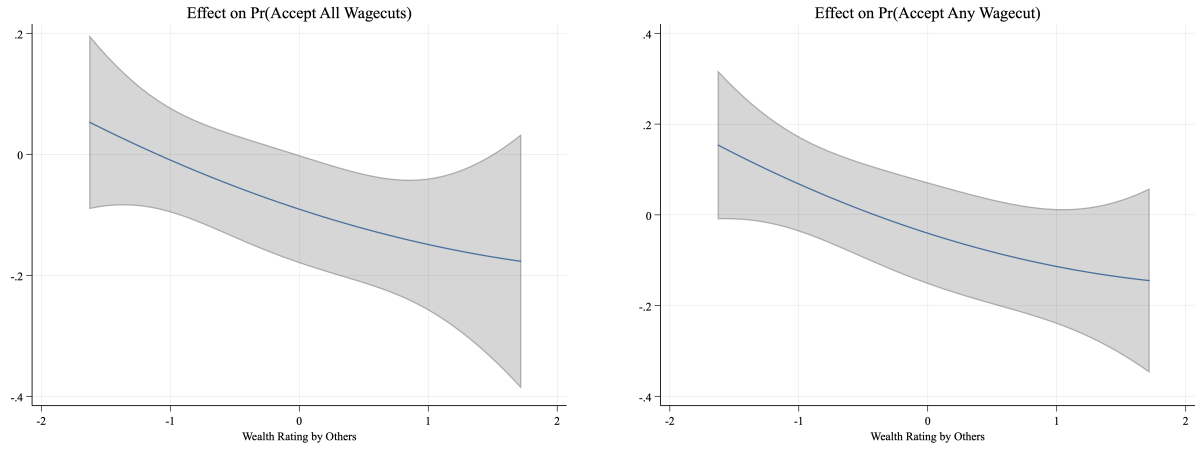
Heterogenous Effects of Chief Selection

Heterogenous Effects of Progressive Selection
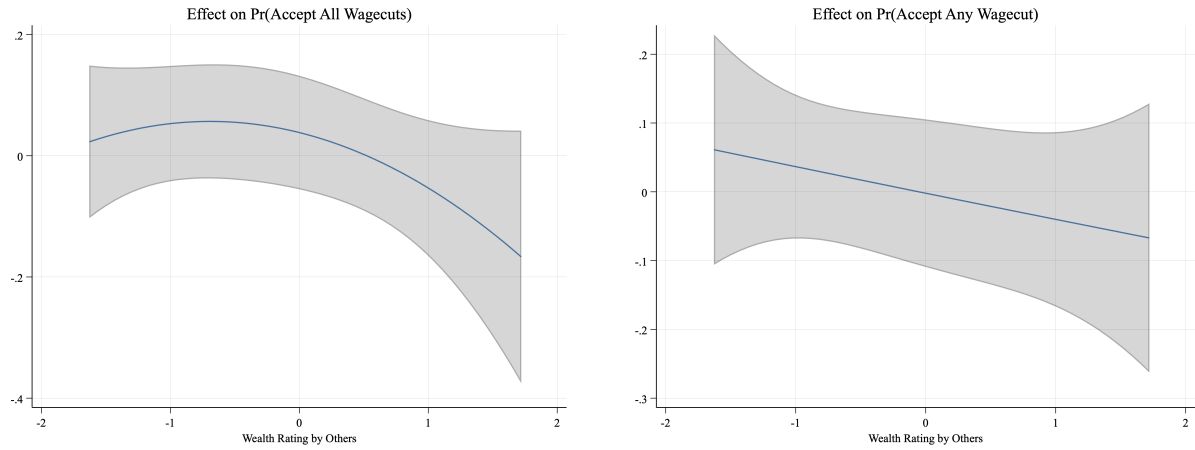
Notes: Estimates obtained from main specification which includes enumerator time distric fixed effects and individual controls. Standard Errors clustered at the community level. Effects are relative to participants in the random selection arm who faced lotteries with positive probability of selection. Wealth Rating is standardized to have mean zero and averages how each participant was rated by others in terms of living conditions. Accept all Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted.

B Tables

Table A.1: Main Effects on Second Stage Correcting for Selection

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Complete Main Task		Extra Effort		Donations	
Chief Selects	-0.021	0.011	0.094	0.102	-0.003	-0.005
	(0.051)	(0.050)	(0.075)	(0.081)	(0.012)	(0.015)
Progressive Selection	0.041	0.065	0.038	0.071	0.004	0.016
	(0.053)	(0.063)	(0.071)	(0.085)	(0.013)	(0.020)
Constant	0.880***	0.855***	0.229***	0.224***	0.088***	0.102***
	(0.043)	(0.047)	(0.062)	(0.068)	(0.007)	(0.009)
Observations	421	280	421	280	732	333
Sample	Public Good Workers				Workers for Self	
Selected Sample	Yes	No	Yes	No	Yes	No

Notes: Even columns restrict the sample to the random set of participants for which their preferences were not taken into account to decide which day they work on, thus removing selection bias on second stage decisions about contributions towards the public good. All regressions include enumerator fixed effects which also capture regional differences. Chief Selects and Progressive selection are binary variables reflecting treatment assignment at the community level. The omitted group are participants in communities facing random selection by lotteries. Workers in the random selection ineligible for selection by being assigned to zero chances of selection were dropped. Complete Main Task is a binary variable taking value of one is participant exerted 8 units of effort in the job task which was advertised as the goal of the program. Extra Effort is a binary variable of whether these same participants agree to do extra work for the public good. Donations is measured for participants not selected to work for the public good and reflects the percentage of their income donated to the public good after completing the job. Standard errors clustered at the community level in parentheses ***p<0.01, **p<0.05, *p<0.1.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Accept All Wage-cuts		Accept Any Wage-cut		Extra Effort		Donations	
Chief Selects	-0.074**	-0.126**	-0.017	-0.130**	0.072	0.007	-0.001	-0.010
	(0.036)	(0.059)	(0.042)	(0.061)	(0.073)	(0.126)	(0.011)	(0.019)
Progressive Selection	-0.012	-0.057	-0.012	-0.077	0.035	0.002	0.012	0.001
	(0.039)	(0.061)	(0.040)	(0.056)	(0.074)	(0.120)	(0.013)	(0.021)
Wealth Index	0.023		0.066		-0.021		0.005	
	(0.061)		(0.075)		(0.135)		(0.020)	
Chief X Wealth Index	-0.019		-0.171*		0.179		0.004	
	(0.087)		(0.093)		(0.160)		(0.024)	
Progressive X Wealth Index	-0.136		-0.204*		0.115		0.022	
	(0.099)		(0.106)		(0.154)		(0.028)	
Low Wealth		-0.102		-0.180***		-0.004		-0.021
		(0.063)		(0.058)		(0.136)		(0.017)
High Wealth		-0.059		-0.132**		-0.098		-0.024*
		(0.059)		(0.056)		(0.110)		(0.012)
Chief X Low Wealth		0.094		0.222***		-0.001		0.017
		(0.081)		(0.077)		(0.153)		(0.026)
Chief X High Wealth		0.073		0.134		0.203		0.016
		(0.074)		(0.086)		(0.159)		(0.019)
Progressive X Low Wealth		0.154		0.177**		-0.032		0.005
		(0.095)		(0.083)		(0.149)		(0.022)
Progressive X High Wealth		-0.003		0.040		0.123		0.028
		(0.075)		(0.079)		(0.129)		(0.022)
Outcome Mean Omitted	0.313	0.367	0.536	0.636	0.232	0.261	0.088	0.102
Observations	1,154	1,154	1,154	1,154	421	421	732	732
Sample		All Eligible Workers			Public Good Workers		Workers for Self	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include enumerator times district fixed effects. Chief and Progressive are binary variables reflecting treatment assignment at the community level. The omitted group are participants in communities facing random selection by lotteries. Workers in the random selection ineligible for selection by being assigned to zero chances of selection were dropped. Wealth Index is standardized to have mean zero and averages multiple proxies of wealth in rural areas. Low and High Wealth are binary variables representing the low and top terciles of the Wealth Index distribution, hence the omitted category is the middle tercile. Accept all Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted. Extra Effort is a binary variable of whether participants working for the public good agree to do extra work. Donations is measured for participants not selected to work for the public good and reflects the percentage of their income donated to the public good after completing the job. Standard errors clustered at the community level in parentheses ***p<0.01, **p<0.05, *p<0.1.

Table A.3: Heterogenous Effects by Wealth (Version 2)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Accept All Wage-cuts		Accept Any Wage-cut		Extra Effort		Donations	
Chief Selects	-0.074**	-0.055	-0.017	-0.094	0.061	0.037	-0.001	-0.019
	(0.037)	(0.055)	(0.043)	(0.071)	(0.073)	(0.137)	(0.011)	(0.018)
Progressive Selection	-0.012	0.021	-0.012	-0.056	0.022	0.050	0.014	0.009
	(0.039)	(0.057)	(0.041)	(0.073)	(0.072)	(0.133)	(0.014)	(0.022)
Wealth Index	0.003		0.011		-0.027		-0.002	
	(0.013)		(0.019)		(0.031)		(0.005)	
Chief X Wealth Index	-0.008		-0.037*		0.063*		0.004	
	(0.019)		(0.021)		(0.033)		(0.006)	
Progressive X Wealth Index	-0.022		-0.037		0.055		0.009	
	(0.022)		(0.025)		(0.035)		(0.006)	
Low Wealth		0.001		-0.133*		0.049		-0.014
		(0.059)		(0.068)		(0.149)		(0.016)
High Wealth		-0.016		-0.081		-0.125		-0.022
		(0.058)		(0.072)		(0.136)		(0.019)
Chief X Low Wealth		-0.061		0.159*		-0.073		0.024
		(0.074)		(0.080)		(0.169)		(0.022)
Chief X High Wealth		-0.005		0.072		0.187		0.037
		(0.077)		(0.090)		(0.156)		(0.025)
Progressive X Low Wealth		-0.008		0.121		-0.154		-0.001
		(0.099)		(0.104)		(0.169)		(0.023)
Progressive X High Wealth		-0.087		0.020		0.099		-0.005
		(0.082)		(0.098)		(0.151)		(0.032)
Outcome Mean Omitted	0.319***	0.324***	0.540***	0.612***	0.250***	0.268**	0.085***	0.097***
Observations	1,154	1,154	1,154	1,154	421	421	732	732
Sample	All Eligible Workers				Public Good Workers		Workers for Self	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include enumerator times district fixed effects. Chief and Progressive are binary variables reflecting treatment assignment at the community level. The omitted group are participants in communities facing random selection by lotteries. Workers in the random selection ineligible for selection by being assigned to zero chances of selection were dropped. Wealth Index is the first component of a Principal Component analysis from multiple proxies of wealth in rural areas. Low and High Wealth are binary variables representing the low and top terciles of the Wealth Index distribution, hence the omitted category is the middle tercile. Accept all Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted. Extra Effort is a binary variable of whether participants working for the public good agree to do extra work. Donations is measured for participants not selected to work for the public good and reflects the percentage of their income donated to the public good after completing the job. Standard errors clustered at the community level in parentheses ***p<0.01, **p<0.05, *p<0.1.

Table A.4: Heterogeneity Results Based on Wealth Ratings by Others

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Accept All Wage-cuts		Accept Any Wage-cut		Extra Effort		Donations	
Chief Selects	-0.078**	-0.113**	-0.024	-0.089	0.087	0.053	-0.001	0.005
	(0.035)	(0.054)	(0.043)	(0.074)	(0.075)	(0.121)	(0.012)	(0.017)
Progressive Selection	0.001	0.081	-0.001	0.007	0.060	-0.019	0.007	0.017
	(0.039)	(0.053)	(0.043)	(0.066)	(0.075)	(0.105)	(0.014)	(0.020)
Wealth Rating	0.071**		0.053		-0.025		0.005	
	(0.028)		(0.032)		(0.062)		(0.007)	
Chief X Wealth Rating	-0.068*		-0.090**		0.034		-0.001	
	(0.037)		(0.040)		(0.073)		(0.010)	
Progressive X Wealth Rating	-0.054		-0.039		0.013		-0.004	
	(0.035)		(0.041)		(0.070)		(0.010)	
Low Wealth Rating		-0.008		-0.087		0.094		-0.017
		(0.067)		(0.087)		(0.135)		(0.012)
High Wealth Rating		0.162***		0.008		-0.115		0.013
		(0.060)		(0.067)		(0.145)		(0.015)
Chief X Low Wealth Rating		0.118		0.190*		-0.069		0.007
		(0.092)		(0.110)		(0.160)		(0.020)
Chief X High Wealth Rating		0.006		0.038		0.175		-0.030
		(0.072)		(0.089)		(0.188)		(0.022)
Progressive X Low Wealth Rating		-0.067		0.038		0.072		-0.001
		(0.081)		(0.107)		(0.149)		(0.019)
Progressive X High Wealth Rating		-0.188**		-0.049		0.210		-0.033
		(0.073)		(0.083)		(0.161)		(0.023)
Outcome Mean Omitted	0.311	0.262	0.535	0.555	0.229	0.235	0.089	0.089
Observations	1,120	1,120	1,120	1,120	415	415	704	704
Sample		All Eligible Workers			Public Good Workers		Workers for Self	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include enumerator fixed effects which also capture regional differences. Chief and Progressive are binary variables reflecting treatment assignment at the community level. The omitted group are participants in communities facing random selection by lotteries. Workers in the random selection ineligible for selection by being assigned to zero chances of selection were dropped. Wealth Rating is standardized to have mean zero and averages how each participant was rated by others in terms of living conditions. Low and High Wealth Ratings are binary variables representing the low and top terciles of the Wealth Rating distribution, hence the omitted category is the middle tercile. Accept all Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted. Extra Effort is a binary variable of whether participants working for the public good agree to do extra work. Donations is measured for participants not selected to work for the public good and reflects the percentage of their income donated to the public good after completing the job. Standard errors clustered at the community level in parentheses ***p<0.01, **p<0.05, *p<0.1.

Table A.5: Additional Heterogeneity Analysis

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Accept All Wage-cuts						Accept Any Wage-cut			
Chief Selects	-0.038	-0.097**	-0.087	-0.037	-0.080**	0.041	-0.034	-0.060	0.018	-0.033
	(0.056)	(0.046)	(0.052)	(0.049)	(0.039)	(0.052)	(0.060)	(0.063)	(0.062)	(0.047)
Progressive Selection	0.038	-0.040	-0.016	0.028	-0.032	0.025	-0.035	-0.023	0.041	-0.021
	(0.062)	(0.048)	(0.057)	(0.051)	(0.041)	(0.053)	(0.056)	(0.061)	(0.056)	(0.043)
Chief X Men	-0.118					-0.133*				
	(0.072)					(0.079)				
Progressive X Men	-0.103					-0.050				
	(0.071)					(0.077)				
Chief X High Education		0.045					0.030			
		(0.062)					(0.071)			
Progressive X High Education		0.054					0.046			
		(0.064)					(0.072)			
Chief X Prioritizes Clinic			0.021					0.077		
			(0.068)					(0.070)		
Progressive X Prioritizes Clinic			0.013					0.020		
			(0.090)					(0.081)		
Chief X Close to Chief				-0.070					-0.068	
				(0.064)					(0.073)	
Progressive X Close to Chief				-0.076					-0.101	
				(0.066)					(0.068)	
Chief X Overestimates Wealth					0.062					0.144
					(0.100)					(0.119)
Progressive X Overestimates Wealth					0.264**					0.113
					(0.103)					(0.140)
Outcome Mean Omitted	0.247	0.307	0.321	0.322	0.324	0.472	0.537	0.556	0.511	0.551
Observations	1,154	1,154	1,154	1,154	1,154	1,154	1,154	1,154	1,154	1,154
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All regressions include enumerator times district fixed effects. Chief and Progressive are binary variables reflecting treatment assignment at the community level. The omitted group are participants in communities facing random selection by lotteries. Workers in the random selection ineligible for selection by being assigned to zero chances of selection were dropped. High Education means having above median years of schooling. Prioritizes Clinic is a binary variable of whether a respondent believes their health clinic should be prioritized by development programs. Close to chief is a binary variable capturing being above the median of an index averaging multiple variables proxying citizens being close to their chiefs. Overestimates wealth is also a binary variable taking value of one when a participant is in the bottom tercile of wealth according to the wealth index, but believes their household wealth is above average. Accept all Wage-cuts is a binary variable and captures accepting all wage-cuts to work earlier in the multiple price list exercise. Accept any Wage-cut similarly captures whether any of the multiple price list options for working earlier was accepted. Standard errors clustered at the community level in parentheses ***p<0.01, **p<0.05, *p<0.1.