Final report

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Taxation and political participation in Sierra Leone

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Final Report: Taxation and Political Participation

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Contents

I. Introduction

Canonical theories in political economy argue that taxation provokes demands from citizens for greater representation and political accountability (e.g., Bates and Lien 1985; North & Weingast 1989). A complementary literature on the "resource curse" argues that undemocratic, but resource-rich, leaders are more likely to survive, because they do not derive revenues from their constituents. In "rentier states" with considerable oil wealth, for example, these leaders have little need to directly tax citizens, and citizens, in turn, make few demands (see Ross 2015 for a review).

These arguments provide micro-foundations for democratization and the emergence of a "fiscal contract" between taxpayers and public officials (e.g., Timmons 2005). Yet, we have little credible evidence to support the claim that taxation provokes demands for political accountability (Weigel 2019 providing a notable exception). In many settings, taxation is already ubiquitous, making it impossible to study the extensive margin; in nearly all settings, it's rare to find a government partner willing to experiment with tax collection.

During the 2019, we worked with the Kono District Council (KDC) to set up basic administrative infrastructure for their incipient tax reform (detailed in final report for Project # 39423). In addition to supporting KDC to develop tax collection protocols, train tax collectors, project property tax revenues and manage incoming tax data, we also worked with KDC to develop and implement a subsidy policy to increase tax collector reach within the district. Specifically, we worked with Kono District Council during the 2019 tax seasons to develop a "travel subsidy program". While tax collectors are in general responsible to cover their own travel costs, this subsidy program subsidized tax collectors' travel to some difficult to reach villages. From a pool of potential "hard-to-reach" villages, we randomly selected villages to which travel was subsidized. This project examines the possibility of leverage this Travel Subsidy Program to study the effects of taxation on citizen political participation.

In Section II, we outline our research design. We describe the Travel Subsidy Program, the intervention which provides causal leverage for addressing our research question and discuss how we identified communities as eligible for the travel subsidy program. We also provide details on our estimation strategy and data.

In Section III, we report results. First, we describe and discuss some basic patterns of where tax collectors visited to collect taxes. Second, we report the effectiveness of the travel subsidy program in increasing the probability that a village was visited by a tax collector. We find that tax collectors are significantly more likely (in both statistical and substantive terms) to visit subsidized villages compared to control villages— villages that were eligible for the subsidy but were not selected into the subsidy program. The point estimate for this effect is that the subsidy program increases probability of a village being visited by 57.75 percentage points (CI 47.77 to 67.77 percentage points). While the magnitude of the direct effect of the subsidy is large, it does not appear that the effect of the subsidy "spills over" to increase the probability of visiting village geographically proximate to subsidized villages.

In Section IV, we conduct a power analysis. First, we consider the possibility of leveraging the 2019 Travel Subsidy Program to estimate the effect of taxation on political participation. We find that we are severely underpowered to leverage the Subsidy Program to study the effect of taxation on *village-level* measures of political participation. We are better powered to detect *individual-level* outcomes of political participation, but achieving adequate power requires an effect size of about (depending on number of villages sampled) a third of a standard deviation. We feel this is too heavy an assumption to justify a large data collection effort.[1] Second, we make a power calculations for a research design that leverages a district-wide Travel Subsidy Program, randomized at a village-level. We reason that this is a feasible research design because there is little evidence that the effects of the subsidy on increased tax collector visit probability spills over to neighboring villages. Looking at standardized village-level outcomes, we achieve 80% power with effect sizes of about a third of a standard deviation (depending on number of villages in study). We suggest village-level voter turnout in the 2022 local election as a village-level measure of political participation and outline a strategy for estimating village level turnout from administrative records. We note that individual-level, self-reported measures of participation in political events in the run-up to the local elections well complement this approach.

In Section V we describe recent developments in the relationship between our research team and Kono District Council. We also note that our research team has opened conversations with Koinadugu District council on tax reform. Section VI concludes with a discussion.

[1] Moreover, the COVID-19 crisis likely suspends required large-scale in-person data collection for much of the remaining 2019.

II. Research Design

In this section we first describe the motivation and operationalization of intervention— the Travel Subsidy Program. Second, we lay out our experimental design, describing how the travel subsidy program provides experimental leverage for causally estimating the effects of 1) taxation on political participation and 2) tax collector travel subsidies on total tax revenue.

A. Intervention Description: Principals & Implementation of the Travel Subsidy Program

i. Motivations for the Travel Subsidy Program

The idea for the Travel Subsidy program sprung from the experience of the Kono District Council's 2018 tax collection season. The District Council identified "transportation constraints" as a major reason for limited tax collection in 2018. As documented in the Final Report associated with IGC Project 39423, our research team recommended a Travel Subsidy Program, which would subsidize travel to "hard to reach villages".

There are both normative and fiscal motivations for subsidizing access to "hard to reach villages".[2] From a normative perspective, without subsidies, tax burden falls on villages that are easy for the tax collectors to access, which are likely villages along good roads. In this way subsidies for visiting "hard to reach" villages are a mechanism for sharing the tax burden across a larger share of the district's population.

From a fiscal perspective, without subsides, there are many villages where tax collector has no incentive to visit, *even though visiting these villages would increase net tax revenue.* To see this, note that a tax collector will only visit a village if travel costs to that village are less than 10% of what he expects to collect there.[3] More concretely, if a tax collector expects to collect 100,000 LE in taxes, he knows that he will only get to keep 10,000 LE and thus he won't visit that village at all if travel cost is more than 10,000 LE.[4] However, consider that District Council and Chiefdom Council combined receives 80% of revenue collected. While the tax collector is indifferent about visiting the village in the above situation (ignoring opportunity costs), District Council and Chiefdom Council stand to generate 80,000 LE from visiting that village. Moreover, and related to normative issues described above, if certain taxpayers feel that they are carrying a disproportionate share of the tax burden, they may be less willing to pay taxes, lowering the rate of voluntary tax compliance.

ii. Implementation of Travel Subsidy Program

How should such a Travel Subsidy Program be implemented? Through an iterative process with the KDC and our local partners at KoCEPO, we developed a set of protocols and implementation plan for the Travel Subsidy Program. A draft protocol of the Travel Subsidy Program was submitted to Kono District Council on August 31, 2019 (Appendix 1). The District Council approved the protocol of the Program and the finalized protocol was incorporated into tax collector training for 2019 (Appendix 2, slides 24 – 25).

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[2] This paragraph borrows, in parts verbatim, from the IGC Final Report for project 39423, which is written by the authors of this report.

[3] I use "he" for tax collectors, as all tax collectors for 2019 season are male.

[4] Note that this simple example assumes that a tax collector will collect taxes for free. If we consider the opportunity cost of his time – the tax collector is likely also a farmer— the tax collector will only visit a village is 10 percent of the expected collection is greater than transportation cost plus opportunity cost.

The core elements of the program are as follows:

1. *Define Hard to Reach Villages* – It was agreed that villages that were "hard to reach" were those that had high travel cost from where the Tax Collector lived. However, there was a balance that needed to be struck. It was acknowledged that there was little chance that a village that lay very far from any motorable would be visited, subsidy or not. Therefore, villages far from motorable roads were excluded.[5]

With these principals in mind, our Research Team was tasked with devising a strategy to systematically define and identify "hard to reach" villages in Kono District. Our strategy leveraged three key pieces of information: i) geo-location for (nearly) every village in Kono District from the 2015 National Census, ii) open source road network data,[6] iii) shapefiles of all "tax zones", the areas in which tax collectors collected taxes.[7]

To identify hard to reach villages, we first identified spatial points in the district that were hard to reach, which we refer to as "end nodes". We defined hard to reach end nodes as spatial points that lay either a) at the end of a road network, or b) where a road network crossed a tax-zone border. Note that "unconnected" roads— pieces of road that did not connect to the larger district-wide network— were dropped from this analysis. Across the District, we identified 466 "end nodes".

From this set of end nodes, we randomly selected 110 to subsidize. Figure 1 displays subsidized and unsubsidized endpoints in Kono. Red and Blue dots are "end nodes" that lie either at the end of a road network or at the point where a road network crosses a "tax zone". Blue dots are end nodes to which travel was subsidized; travel to red dots is unsubsidized. The black lines are tax zone boundaries. Appendix 3 zooms in on a single tax zone (tax zone 11) and displays end nodes and road networks.

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[5] This is a feature we recommend changing in a future iteration of the Travel Subsidy Program. Probability of tax collector visits to all villages is low; we now believe an appropriate subsidy will increase probability of tax collectors to visit even difficult to reach villages.

[6] We verified the accuracy of this road network data through a field exercise. This activity is not described here, but more details are available upon request.

[7] For 2019 there were 27 tax zones, which in most instances are entire wards. However, three wards that cross chiefdom boundaries were split into two tax zones. The motivation and creation of these "tax zones" is described in more detail in our report for IGC Project 39423

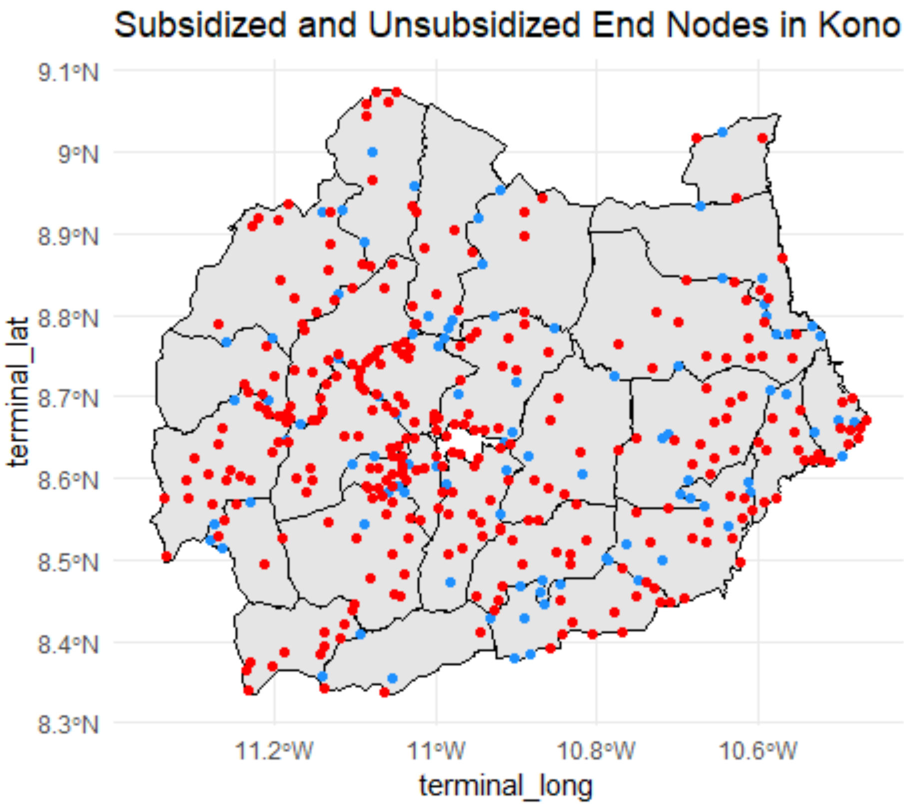

Figure 1 - Subsidized and Unsubsidized "End Nodes"

2. *Identifying Subsidized Villages* – The goal of the Travel Subsidy Program is of course to increase the likelihood that tax collectors visit *villages*, not "end nodes". Therefore, we next identified a set of villages to subsidized based on the set of selected subsidized nodes. We subsidized travel for the closest village to each subsidize end node; each end node was matched to a different village, as villages were removed as potential matches after they had been matched to a subsidized end node.

Figure 2 displays subsidized "end nodes" and corresponding subsidized villages. The black dots represent villages that have been subsidized; blue does represent subsidized "end nodes". Note that some blue dots completely cover black dots. (See Appendix 4 for list of subsidized villages).

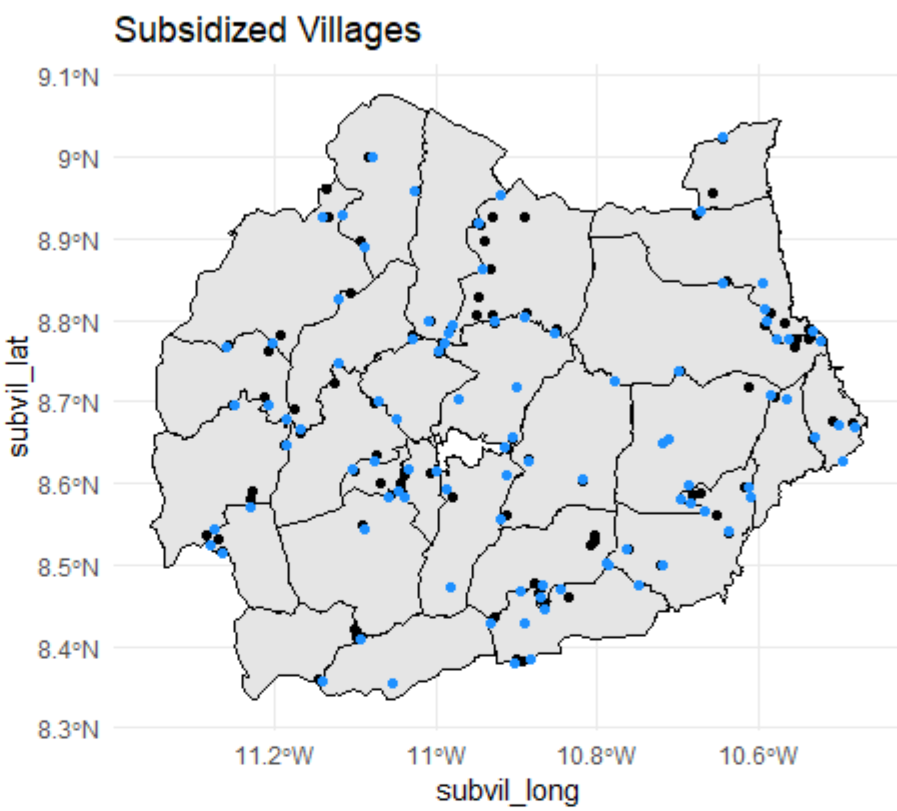

Figure 2 - Subsidized End Nodes and Subsidized Villages

3. *Establishing Visit to Subsidized Village* – For the program to be incentive compatible, tax collectors should only receive a subsidy if they visit an eligible village. We devised a strategy to verify if tax collectors had visited a subsidized village, by using tablets to establish geo-locations. Per request of the District Council, tax collectors were instructed to fill out a short survey in every village they visited, which captures the total tax potential of the village (Appendix 5 for report of Kono District Potential Revenue); at the end of this survey the tax collector captures their geo-location. We used 2015 Census data (which contains geo-locations) to verify that geo-locations collected by the tax collected matched the geo-location of the subsidized villages.

4. *Level and timing of the subsidy* – It was agreed that the level of the subsidy should *at minimum* fully cover the transportation cost of visiting a village. We conducted an exercise to capture travel costs associated with traveling from the home of the tax collector to each of the subsidized locations in his jurisdiction, by visiting bike parks near the residence of the tax collector. Appendix 6 provides information about the cost of each subsidized trip and the source of the information. The value of the subsidy to each village was set to 150 % of the estimated cost to travel to that village.

At the onset of the program it was agreed that the subsidy payment would be made *after* Tax Collectors could demonstrate that they had visited a “hard to reach” village. However, “hard to reach” villages are costly to visit by definition, and this protocol requires that tax collectors finance the costly trip up front. It became apparent during the beginning of the program that many tax collectors were not making trips to subsidized villages due to liquidity constraints. Therefore, the decision was made to provide 50% of one subsidy to a tax collector in advance. When the tax collector could demonstrate that he had visited the subsidized village, he would receive the remaining 50% and an additional 50% subsidy towards another subsidized community. If a tax collector did not visit a village for which they had been advanced part of the subsidy this amount was deducted from their final incentive payout.

B. Experimental Design

In this section we describe how we leverage the Travel Subsidy Program to estimate the effects of taxation on political participation. In doing so, we also lay our strategy for estimating the effect of the Travel Subsidy program on a) the probability that a village experiences a tax collector visit and b) tax revenue.

i. Subsidy as an instrument for Taxation Demands

“Tax Demand” is the independent variable for our main research question, which asks: how does increased tax demands affect political participation? We operationalize “tax demand” as a dummy variable equal to 1 if the tax collector visited a given village. Simply regressing a measure of political participation on our tax demand dummy will produce biased estimates, as political participation is likely to be correlated with the probability that a tax collector visits the village. For example, (as we’ll demonstrate below) tax collectors are more likely to visit villages on the main road, and we might well imagine that citizens in these villages have different political behavior than their fellow citizens further off the road.

The Travel Subsidy Program supplies exogenous variation in the probability that a tax collector visits a subset of villages. The Travel Subsidy Program can influence the probability that a village is visited both directly and indirectly. We’ll say that the travel subsidy program directly increases the probability that a village is visited when that village has been selected to be subsidized under the Travel Subsidy Program. The Travel Subsidy program can also indirectly increase the probability that a village is visited by a tax collector, if a village is easily accessible from a village to which travel has been subsidized. Below, we’ll define exactly what we mean by “easily accessible”.

Adopting instrumental variable framework, the Travel Subsidy program allows us to casually estimate the impact of tax demands on political participation. Theoretically, the Travel Subsidy program is a solid candidate to instrument for tax demands. The Travel Subsidy Program provides financial incentives for a tax collector to visit villages and attempt to collector taxes from citizens. Our defense that the Travel Subsidy program meets the exclusion restriction is straightforward: because subsidizes were randomly assigned to end nodes (and therefore villages) using researcher’s computers (ie, with R), the subsidizes cannot be correlated with our outcome of interest, political participation.

ii. Directly Subsidized Villages & Comparison villages

Above we argued that the Travel Subsidy Program can be thought of as an instrument for "tax demands", as the Travel Subsidy Program might increase the probability that tax collectors visit a given village. But recall that not all villages were eligible for the Travel Subsidy Program, as the program is targeted at villages that are defined as "hard to reach". This means that not all villages can be credibly compared to villages that received the travel subsidy program. So what are the appropriate comparison villages?

There are 356 end nodes that were not selected for subsidy. We again match these 356 end points to the closest village, pulling from the full set of villages, but again dropping villages once they have been matched. These Villages are *Control* villages, for which we can credibly compare tax collector visits, to understand the effect of the subsidy on the probability of tax collector visits.

Note that Subsidized Villages and Control Villages are not necessarily unique, as a single village could be the closest village to both a subsidized endpoint and an unsubsidized endpoint. 28 villages that are subsidized are also coded as control. We will drop these villages in our estimates, as the expected treatment effect for these villages is zero. [8]

iii. Indirectly Subsidized villages & Comparison Villages

In the previous sub-section we focused on villages that were directly subsidized. In this section, we consider villages where the probability of a tax collector visit may be *indirectly* affected by its proximity to a subsidized village. To see why this might be the case, note that a tax collector may refrain from visiting a cluster of villages that are all along a road axis that is costly to visit. If one of those village is subsidized, the tax collectors is now more likely to visit *all* of the villages in that cluster, compared to a situation where there was not subsidized village in the cluster. This means that any village in the proximity to a village that has been directly subsidized can be considered "encouraged", as a tax collector will be more likely to visit compared to if there was no Travel Subsidy Program.

But how should we define which villages are indirectly encouraged? Our approach is to use a distance measure from the directly subsidized village. But what then is the appropriate distance? Ultimately, we will select a distance for coding villages as indirectly subsidized (or controls for indirectly subsidized communities) that maximizes of power for estimating the effect of tax demands on political participation. For now, we'll simply define this distance at several value, and observe how estimates of the effect of the Travel subsidy program on probability of village visits changes at each distance value.

[8] It could be argued that the proper set of comparisons villages are actually the 110 villages that are closest to a random sample of 110 of the 356 unsubsidized endpoints. For each additional end node that is matched to a closest village, the pool of villages to draw from is smaller and the expected value of the distance between the end node and the closest village is larger. This means that if the pool of control village is larger than the pool of treatment villages, control villages will be on average further from end nodes, which could plausibly affect the characteristics of the set of villages. If the pool of treatment and control villages is equal, then the expected value of the distance between end node and matched village is equal. Finally, note that the number of treatment villages that are also found in control should be *smaller* if the pool of control villages is smaller, ie, we can leverage *more* treatment villages.
An analysis that limits the pool of control villages to 110 is not included in this version of the report.

C. Estimation

Ultimately, we want to estimate the effect of the Travel Subsidy Program on Political Participation. As noted, we plan to use an instrumental variable approach, where we instrument tax collector visits (our operationalization of "tax demands") with a dummy variable for if the village has been subsidized as part of the Travel Subsidy Program. As our instrument is binary, we plan to use a Wald Estimator.

The first stage (the denominator) of the Wald Estimator is the difference in means of the key explanatory variable, between treatment and control. In our context, this is the difference in means of the dummy for visited villages, in villages that were subsidized compared to villages that were not subsidized. Specifically, we estimate the equation:

$$\text{ATE} = Mean(Y_i|\ Subsidy) - Mean(Y_i|\ No\ Subsidy)$$

Where Y_i is a dummy variable equal to 1 if village *i* was visited by a tax collector. Note that depending on how we define the population, can estimate the direct and indirect effects of the travel subsidy program. In our secondary analysis, we estimate the effect of the travel subsidy program on village level tax revenue, using the same estimator. For this analysis, Y_i is sum total of reported collected taxes in village *i*.

D. Data

i. Dependent Variables

Visited Village - Our primary dependent variable is a dummy variable equal to 1 if a village was visited by a tax collector during the 2019 tax collection season. We have information about tax collector visits from two sources. First, every tax collector was asked to complete a "village tax assessment" in every village, where he counted the total number of properties for several different property types. This survey was conducted using a tablet, and the tax collector recorded the village in which he was conducted the property tax assessment. Using this data, we identify 304 villages as visited.

Second, when tax collectors collected taxes from citizens, they wrote tax receipts. A carbon copy of each receipt was turned into the valuation department at district council. We worked with the District Council to digitize these receipts (See Appendix 7 for 2019 Kono District Tax Revenue Report). Using this data, we identify 197 villages where receipts have been collected, of which 154 also appear in our "tax assessment" data. This means that we record tax receipts in 43 villages where we do not have geo-location information from tax collectors (remember that tax collectors are only incentivized to fill in the "tax assessment" in subsidized villages, as subsidy is given conditional on proof of visit from geo-location; in non-subsidized villages tax collectors are instructed to fill the tax assessment in as part of their job, but doing so is not incentivized).

We code the village visit variable in two ways. Our preferred coding considers a village as visited if it appears in either the source of information. Using this coding, 344 villages are coded as visited. Our second coding considers only villages that are marked a visited by the tax assessment data. As noted above, using this coding, 304 villages are coded as visited. We avoid a coding that relies on collected tax receipts because, as we'll discuss below, it was often the case that a tax collector visited a village but did not report collecting taxes.

Tax Revenue – The dependent variable for our secondary analysis is village level tax revenue. As noted above, we worked with the Evaluation Office at District council to digitize tax receipts. From this, we construct a village level measure of total tax revenue. Total tax revenue includes taxes collected from business tax, plantation tax, and property tax.

ii. Independent Variables for descriptive analysis

Before conducting our primary and secondary analysis on the effects of the travel subsidy program on village visits and tax revenue, we first conduct a descriptive analysis of correlates of tax collector visits. We model tax collector visits as a function of four variables.

Distance to Road – We used open source road network data, which we then verified on the ground to find out which "level" of road networks in the open source data were motorable. This variable is the distance, in kilometers, between a given village and the closest point on a road network.

Distance to Tax Collector Residence - This variable measures the distance, in kilometers, between a given village and the relevant tax collectors village of residence.

Number of Building Structures (Census) – We use the 2015 National Census to calculate the number of building structures in each village. We winsorize this variable below the 2.5 percentile and above the 97.5 percentile.

Ethnic Fractionalization Index (Census) - The Census contains information on the ethnic composition of each village. We construct a measure of ethnic fractionalization where Fractionalization Index of Village j is the sum of the square of the percent share of each ethnic group in village j, subtracted from 1:

$$FRACT_j = 1 - \sum_{i=1}^{N} S_{ij}^2$$

Where S_{ij} is the percent share of ethnic group *i* in village *j.* A perfectly homogenous village has an fractionalization index of zero, as a single group makes has a village level ethnic group share of 1, which subtracted from 1 is equal to zero. This variable is also windsorized below the 2.5 percentile and above the 97.5 percentile.

Summary statistics for these variables are displayed below:

Table 1: Tax Collection covariates				
Variable	**mean**	**Std Dev.**	**min**	**max**
Distance to Road (KM)	0.175563	0.421795	0	3.738057
Building Structures (n)	25.74359	24.09199	1	100
Ethnic FRACT	0.234992	0.240271	0	0.752894
Distance Collector Residence (KM)	8.001508	6.323576	0	37.53651

Table 1 - Summary Statistics, Explanatory Variables for Descriptive Analysis

III. Results

A. Map & Describing Visit Patterns

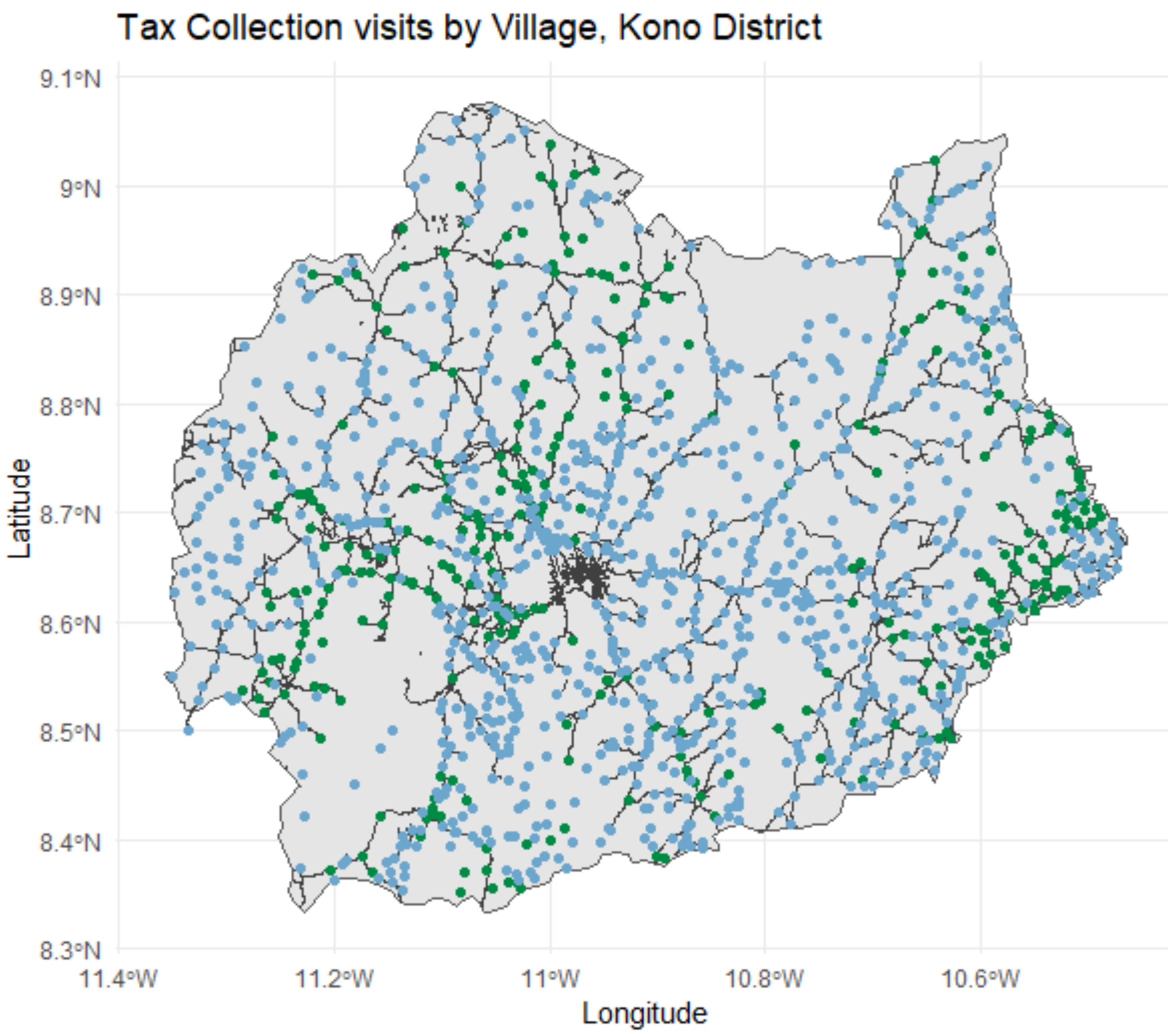

Figure 3 - Tax Collection Visits

Figure 4 displays visited and unvisited villages in Kono District. Blue dots represent unvisited villages and green dots represent visited villages. The dark brown lines snaking throughout the map are the district's road networks. During the 2019 tax season, tax collectors visited 344 villages, exactly 25% of the total villages in Kono District. Before moving to the effects of the Travel Subsidy program, we first explore the relationship between several village-level factors and tax collector visits, using a logistic regression.[9]

[9] Coefficient estimates presented in Appendix 13, Table 14

We model a tax collectors' decision to visit a village or not as a function of four village-level characteristics:

- Distance of a village from the tax collector village of residence
- Distance of a village from the road network
- Number of building structures in a village
- Ethnic Fractionalization Index of a village

The motivation for included the first three variables is straight forward: villages closer the a tax collectors' village of residence and closer to a road network should be cheaper and less time consuming to visit; villages with more structures have greater potential tax revenue (tax collectors keep 10% of all taxes collected). There are multiple stories for why village-level ethnic diversity might influence a tax collector's decision to visit a village.

On the one hand, more diverse communities may with more varied economies— either because different ethnic groups have been attracted to a trading town, or because different ethnic groups bring with them different types of economies— that may be easier (or harder) to tax.[10] There also might be a political logic at play. Tax collectors' are appointed by local politicians, ward councilors. Minority communities may be less (or more) important for building political coalitions for local politicians, making villages with large shares of minorities less (more) important for obtaining political support and therefore more (less) likely to be taxed. An author of this report attended tax collector training sessions in Kono District. When asked why Local Councilors chose people as tax collectors, a common refrain was that it was a reward for important service during the 2018 election.

Table 2: Tax Collector visits – Predicted Probabilities					
Model	**dist2road**	**structures**	**Fract Index**	**distance2collector**	**Predicted Probability**
1.	0.175563	25.74359	0.234992	8.001508	**0.304526**
2	*0*	25.74359	0.234992	8.001508	**0.330431**
3	*0.597358*	25.74359	0.234992	8.001508	**0.250287**
4	0.175563	25.74359	0.234992	*1.677932*	**0.459625**
5	0.175563	25.74359	0.475262	8.001508	**0.396762**
6	0.175563	25.74359	0	8.001508	**0.235095**
7	0.175563	*49.83558*	0.234992	8.001508	**0.467956**
8	0.175563	*1.651597*	0.234992	8.001508	**0.198172**

Table 2 - Correlates of tax collector visits

Table 2 displays predicted probabilities, from the aforementioned logit model, for villages with various sets of characteristics. Row 1 display the predicted probability of a tax collector visit for a village that has mean values for each of the model's four factors. A village that is 175 meters from the closest road, has 26 structures, has a fractionalization index of .235 and is 8 kilometers from the tax collector's residence has a predicted probability of being visited of 30%.

Now, consider a village that sits squarely on the road network, but has mean values of each of the other three variables (row 2). For this village, the predicted probability of tax collector visit rises to 33%. Now

[10] For example, Fula minorities (in the Kono majority district) are (said to be) more likely to rear cattle, but also are may be more engaged in trade.

consider a village that is .6 kilometers from the closest road, one standard deviation further away than the mean village (row 3). The predicted probability of a village visits drops to 25%.

Row 4 in Table 2 considers a village that is one standard deviation closer to a tax collector's residence than the mean distance. The predicted probability for a visit to this village jumps to 46%, nearly 16 percentage points above the predicted probability when a village is the mean distance of 8 kilometers from a tax collectors' village of residence

Row 5 looks at the relationship between village level ethnic fractionalization and probability of tax collector visit. When ethnic fractionalization increases from the mean of .23 by one standard deviation to .48, predicted probability of a village visit increases to nearly 40%, 10 percentage points above the predicted probability when a village is set of the mean value of ethnic fractionalization. To get a sense of the ethnic fractionalization measure, consider a village with two ethnic groups, Kono and Fula, where 86.5% of the village is Kono and 13.5% of the village are Fula. The ethnic fractionalization index for this village is .2335, just about the mean level across villages in Kono District. Now consider a village that is 60% percent Kono and 40% Fula; the ethnic fractionalization index for this village is .48. The predicted probability of a village visit for this second village is nearly 10 percentage points higher than the first village. Row 6 examines a perfectly homogenous village (Fract index = 0). The predicted probability of a village visit is 6.5 percentage points less than if the ethnic fract is at its mean level.

Rows 7 and 8 consider the numbers of structures in a community. A one standard deviation increase in the number of structures, from a mean of 26 to 50 structures, increases the predicted probability of a visit to 46.7%. the predicted probability for a small village of two structures is about 20%, a 10 percentage point decrease compared to the mean level of villages.

B. Effects of Direct Subsidy on Tax Collector Visits

In this section we present estimates of the effect of the travel subsidy program on tax collector visits, for villages that were directly subsidized. Villages are coded as visited using our preferred coding method, where a village is coded as visited if it appears in either the tax collection or tax assessment data.[11] We consider only villages that are *uniquely* coded as either treatment or control villages.

Table 3: Effect of Travel Subsidy Program on Village Visits			
Outcome = Village Visited			
	Control Villages	**Treatment Villages**	**Total**
N	330	82	412
Visited Villages	71	65	136
Visited / N	0.215	0.7926	0.5775
			(0.0504)

Table 3 displays difference in means calculations for the probability of being visited by a tax collector between treatment and control villages. Tax collectors visited 21.5% (71 of 330) of control villages. Remember that in this context a control village is a village that was labelled as "hard to reach" but was not selected as a community to which transportation was subsidized. Of the 82 treatment villages that were directly subsidized, 65 (79.3%) were visited. The difference in means point estimate is that the

[11] Results with alternative coding are nearly identical (Table 15, Appendix 13)

subsidy program increased the probability that a tax collector visited a hard to reach village by 57.8%. The standard error for this estimate is 5%. This means that the 95% confidence interval upper bound is 67.7% and the lower bound is 47.8%.

C. Indirect Effect of Subsidy on Tax Collector Visits

In this section we expand our analysis, incorporating villages on which the Travel subsidy program might have an indirect effect.

Table 4: Number of "encouraged" & Control units			
Distance	**Encouraged (n)**	**Control (n)**	**Total (n)**
KM = 1	103	411	514
KM = 1.5	130	491	621
KM = 2	127	547	674
KM = 2.5	116	566	682
KM = 3	80	536	616
Note: This table shows the number of *uniquely* "encouraged" villages and number of villages that are uniquely control units. Villages that are coded as both "encouraged" and "control" not counted here.			

Table 4 displays the number of villages that are coded as either "encouraged" or "control", dropping those villages that are listed as both. In Row 1, we see that the total number of villages within 1 Kilometer of a subsidized village is only slightly more than the number of villages are directly subsidized. While there are 82 unique directly subsidized villages (row 1, table 3), there are only an additional 21 villages that are within 1 kilometer of a directly subsidized village (but not themselves directly subsidized). The number of uniquely encouraged villages reaches its peak when we consider villages within 1.5 KM of a directly subsidized village; there are an additional 48 villages beyond those that are directly subsidized that can be considered "encouraged". Beyond this distance, the number of additionally encouraged villages starts to fall, as villages fall within the given distance of both an encouraged villages and control villages.

Table 5 shows the estimated difference in means of being visited by a tax collector, for villages within a given distance to either a subsidized (treatment) or a control village. Again, we consider villages that are either unique coded as encouraged or control. In table 5, villages that are directly subsidized (and their controls) are included. The estimates for Table 5 can be interpreted as the weighted average of direct and indirect effects of the subsidy.

Table 5: "Total" Effects of Subsidy on tax collector visit					
Distance	**Effect**	**Std Error**	**CI Low**	**CI High**	**N**
KM = 1	0.426665	0.051644	0.32457	0.528759	514
KM = 1.5	0.31692	0.047684	0.22282	0.41102	621
KM = 2	0.228879	0.047566	0.134974	0.322784	674
KM = 2.5	0.205526	0.04877	0.109148	0.301904	682
KM = 3	0.157836	0.056302	0.046071	0.269601	616

As we might expect, the treatment effect of the subsidy diminishes as we expand the number of villages that we consider as "encouraged". But are there actually indirect effects? That is, are tax collectors more likely to visit villages "in the neighborhood" of directly subsidized villages. We can't answer this question looking at Table 5, because directly subsidized villages are still included.

Table 6: Indirect effect of subsidy program on tax collector visit					
Distance	**Effect**	**Std Error**	**CI Low**	**CI High**	**N**
KM = 1	0.022629	0.091317	-0.16135	0.206604	125
KM = 1.5	0.019097	0.060293	-0.10049	0.138682	255
KM = 2	-0.00159	0.052591	-0.10571	0.10252	354
KM = 2.5	0.003501	0.049494	-0.09449	0.101489	416
KM = 3	-0.10956	0.044032	-0.197	-0.02212	403

When we dig deeper in Table 6 and remove villages that are either directly subsidized or controls for subsidized villages, we find no evidence of indirect effects. Point estimates for (nearly) all distance cutoffs are near zero; while estimates are imprecise, upper bounds of confidence intervals allow us to confidently state that, if indirect effects do exist, they are much small than the direct effects. This is not what we expected. We expected that, with travel to difficult to reach places mostly financed, tax collectors would be willing to take the extra time and take on the small additional travel costs to visit nearby villages. If anything, its possible that travel subsidy program *reduced* visits to villages slightly further from the subsidized village (Table 6, row 3).

D. Effect on Revenue Raised

The ultimate goal of the Travel Subsidy Program is to raise revenue. Table 7 displays the total amount of receipts collected and total revenue generated during 2019 tax collection season (See appendix 7 for 2019 tax receipt report). In total, 1,553 receipts were collected, which generated SLL 24,816,00 (USD 2,559).

Table 7: Tax revenue by Chiefdom		
Chiefdom	**Number Receipts**	**Total Revenue (Leones)**
Gbane	12	430,000
Gbane Kandor	100	1,555,000
Gbense	2	80,000
Goroma Kono	108	1,821,000
Kamara	373	6,265,000
Lei	80	1,775,000
Nimikoro	178	2,550,000
Nimiyama	155	2,440,000
Sandor	397	5,690,000
Soa	96	1,465,000
Tankoro	27	375,000
Toli	25	370,000
Totals	**1,553**	**24,816,000**
Note: Only 12 of the 14 chiefdoms are listed. No taxes were collected in Fiama or Mafindor Chiefdoms		

Table 8 provides some descriptive statistics of village level revenue reported by tax collectors. Row 1 and Row 2 report the average village-level property tax revenue and total revenue reported by tax collector.

The average total revenue per village is just over 19,000 SLL (about USD 2). Row 3 and row 4 describe and interesting pattern— while 26% of the total villages in Kono district were visited by tax collectors, tax collectors only reported collecting taxes in 15% of total villages.

Table 9 reports these same statistics, but only across *visited* villages. Table 9 allows a better understanding of the amount of revenue a tax collector was able to extract, given that they visited the village. Row 2 (table 9) tells us that the average tax revenue collected in a visited village is about SLL 72,000 (USD 7.4). Turing to row 3 in Table 9, we can see that tax collectors only report collecting tax revenue in 59.7% of the villages that the visited.

Table 8: Village Level Tax Revenue, SLL (All villages)				
Variable	**mean**	**sd**	**min**	**max**
Property Revenue	16.04535	75.99534	0	1610
Total Revenue	19.02844	91.31531	0	2060
Taxes Collected	0.150653	0.357848	0	1
Village Visited	0.264412	0.441189	0	1
Note: Revenue data Winsorized at 95%				

Table 9: Revenue, Visited Villages				
Variable	**mean**	**sd**	**min**	**max**
Property Revenue	60.68314	138.4637	0	1610
Total Revenue	71.96512	166.682	0	2060
Taxes Collected	0.569767	0.49583	0	1
Village Visited	1	0	1	1
Note: Only looking at visited villages (as can be see that "Village Visited" is equal to 1.				

These revenue collection patterns might shed light on one of the interesting patterns we found above: it seems that subsidies did not encourage tax collectors to visit villages in close proximity to the subsidized village. As we see in table 9, tax collectors do not collect much revenue in villages that they visit. Remember that a tax collector only keeps 10% of the revenue collected, so on average the tax collector is pocketing SLL 7,000 for collecting taxes in a village, or about $.70. Even if a village is close by, its difficult to take transport between any two villages for less than SLL 7,000.

We might be concerned that the Travel Subsidy Program is driving the pattern that tax collectors are not collecting taxes in villages they visited. The Travel subsidy offered to tax collectors was 1.5 times the estimated cost of the round-trip visit to the subsidized village. It could be the case that tax collectors are happy to pocket 50% of the travel cost to visit a subsidized village, take the GPS coordinate to claim the subside, then leave. If this is true, the low revenue collection to visited village ratio would be perverse effect of the Travel Subsidy Program, but not a consistent feature of tax collection.

This does not appear to be the case. Taxes were collected in 42 of the 71 (59%) control villages visited and in 30 of the 65 (46%) subsidized villages visited. While it's possible that tax collectors are slightly less likely to collect taxes in subsidized villages, it is clear that taxes are not collected in a substantial proportion of villages visited without the aid of the travel subsidy (tables 16/17 in Appendix 13)

Next, we turn to the effect of the travel subsidy on revenue collection. To make a back of the envelope calculation for the revenue returns from the 2019 Travel Subsidy Program we can estimate what would have been collected in the absence of the travel subsidy program by multiplying the amount collected in subsidized villages by the ratio of the tax collection rate in control villages to treatment villages.[12] The total revenue raised in (unique) subsidized villages is SLL 1,415,000 (Tables 17, Appendix 13). The tax visit rate in control communities is .215 and in treatment communities is .7926 (Table 3). Village visit rate in control communities is .27126 of that in treatment villages. We estimate the amount of revenue that would have been collected in subsidized communities had they not been subsidized at SLL 383,832 (1,415,000*.27126). Subtracting this from the amount actually collected in subsidized villages, we get SLL 1,031,168 (USD 106.28). That's 4.2% of the total taxes collected.

More generally, we can try to estimate the effect of the travel subsidy program on village-level revenue. Table 10 reports difference in means estimates for the effect of the travel subsidy program on village level tax revenue. Estimates are reported with tax revenue winsorized below 2.5 percentile and above 97.5 percentile. Estimates show that subsidized communities get more revenue, but not by much. Point estimates for the effect of the subsidy program on total revenue and on property tax revenue are SLL 8,500. While point estimates are small, they are nearly the size of baseline means in the control group. Table 16 (appendix 13) shows mean total revenue collection in control villages is just under 11,000 SLL and mean property tax collection is just under 9,000 SLL. That said, standard errors for the estimates are fairly large, with 95% confidence intervals for total revenue dip below zero. Moreover, if we leave more extreme values in the data, winsorizing at 99th percentile, point estimates for the effect of the travel subsidy program shrink and are no longer statistically significant.

Table 10: Effect of Subsidy Program on Total Revenue

Variable	Coef.	std error	CI Low	CI High	N
Total Rev (95th Wind)	8.515152	4.753389	-0.87037	17.90067	440
Total Rev (99th Wind)	4.227273	6.2409	-8.05982	16.51436	440
Prop Rev (95th Wind)	8.424242	3.992663	0.539237	16.30925	440
Prop Rev (99th Wind)	6.515152	5.434969	-4.19747	17.22778	440

Note: This table is a difference in means of revenue between subsidized communities and subsidized control communities. This is a rather shocking result given how much more likely these communities are to be visited.

[12] This calculation also assumes that the average village revenue collection is the same across villages that would be visited travel subsidy and villages that would be visited in the absence of travel subsidy.

IV. Power Analysis

As outline in Section II, our intention was to leverage the Travel Subsidy Program to estimate the effect of increased taxation on citizen political participation. Given that can already observe the instrument for this design (tax collector visits), we now ask if this provides us with enough statistical power to justify measuring out dependent variable, political participation. In this section we conduct power simulations for the effect of taxation (visited village) on the following outcomes measures for participation:

- 4-point Likert outcome on individual level survey
- Standardized village-level outcome measure

We only conduct a power calculation for directly subsidized villages, as we saw in Section 3C that the indirect effects of a subsidy of tax collector village visits is, if anything, limited (for completeness, we produce these power calculations in Appendix 13, table 18/19). In these power calculations, the explanatory variable is a dummy equal to one if the village was visited by a tax collector. As described in Section II, the Travel Subsidy Program serves as an instrument for tax collector visits. Our power analysis considers as "instrumented" only villages that have directly received the subsidy.

A. Individual survey (village level cluster)

- We first consider an individual-level survey measure. The outcome variable is a 4-point Likert scale.
- 10 respondents are sampled in each village
- An individual's outcome value is a function of two factors: a) a random draw from a normal distribution with mean 0, sd 1, b) village visit. If the sum of these two components is below -1, the outcome value is 1; if it is between -1 and zero the outcome is 2; between zero and 1 the outcome value is 3; above 1 the outcome value is 4.
- The value of the "village visit" factor is the effect size. I start by setting the treatment effect size (effect of village visit) to .2. In practice, this is as though having one's village visited increases outcomes from baseline of 2.5 to 2.7; or 1 in every 5 treated individuals increase their response on the Likert scale by 1 point.
- Village-level ICC is set to .1.
- I use the "iv_robust" estimator from the *estimatr* package. Estimate clustered at village level.

Table 11 maps power across different effect size. Column 1 lists effect sizes and column 2, 3, and 4 report power estimates. Recall that there are 330 control villages. Column 2 reports power with 10 respondents sampled in each of the 330 villages. Column 3 reports power when 10 respondents are sampled in a randomly sampled 200 control villages, and Column 4 reports power with 10 respondents sampled in a randomly sampled 100 control villages. (Note, there are 82 treatment villages). As noted above, effect sizes represent movements along a four-point Likert scale. Across all columns, power is limited when effect sizes are small. In the design that selects only 100 control villages, power never reaches adequate levels, even when effect sizes are .33 on the Likert scale. The design with 200 villages requires an effect size of about .33 to achieve 80% power and the design with all 330 control villages requires an effect size of around .3 to achieve 80% power. It seems difficult to justify the such a substantial data collection— ten respondents in 330 control villages plus 82 treatment villages is 4,120 respondents across 412 villages— given the that we are insufficiently powered to detect medium-sized effects.

Table 11: Power, Individual Likert outcome			
Effect Size	**Power (all)**	**Power (200 Villages)**	**Power (100 Villages)**
0.1	0.188	0.17	0.136
0.15	0.324	0.29	0.246
0.2	0.576	0.416	0.384
0.25	0.762	0.58	0.504
.3	0.80	0.744	0.608
.33	0.88	0.828	0.64
Note: There are 330 control villages. Column 2 reports power assuming that 10 respondents are randomly sampled from all 330. Column 3 reports power assuming that 10 respondents sampled from 100 villages			

B. Standardized village-level outcome

We next consider a standardized village-level outcome. Table 12 displays power estimates when only directly subsidized villages are considered instrumented across several effect sizes. Unlike the calculations presented in Table 11, these calculations are for village level outcomes. Column 1 Table 12 displays effect sizes, which can be interpreted as fractions of a standard deviation of a village-level outcome. Even when we consider all 330 control villages, the effect sizes required for a well powered study are unrealistic. An effect size of four-tenths of a standard deviation still only provides 53% power.

Table 12: Standardized village-level outcome	
Effect Size	Power
0.1	0.07
0.15	0.098
0.2	0.202
0.25	0.242
0.3	0.366
0.4	0.526
Note: Standardized village-level outcome. Assumes 82 treatment villages and 330 control villages	

C. Powering village-level outcomes: Expanding the Travel Subsidy Program

What would a well-powered village level study look like? Would expanding the scope of the Travel Subsidy Program give us leverage to study village level outcomes? We designed the travel subsidy program in 2019 to target villages we though tax collectors would have difficulty visiting. As it turns out, tax collectors did not visit the great majority of villages in 2019. For the 2019 Travel Subsidy Program, we shied away from randomizing the subsidy across large swaths of the district because we reasoned that spillovers would occur from subsidized villages to nearby villages; that assumption is at the heart of our design. Following this logic, treating a high portion of villages means that subsidizes spillover to the remaining untreated villages, hindering our ability to detect effects of the subsidy. At least for the 2019 tax season, this spillover appears not to have occurred. If we assume no/limited spillover, then an optimal research design might randomize subsidies at a village level across the district. We explore this design in the following way. We sample with replacement from our existing data to create more treatment and control communities. Note that because these observations are sampled with replacement from existing sample, the expected value of the key explanatory variable, village visit, is the same as in our initial sample. By doing this we assume that the first stage effect remains the same. That is, the central assumption we are baking into these power simulations is that we can sample a given amount of villages for random assignment to treatment (ie subsidy), maintaining a difference in probability of tax collector visit between treatment and control of about 58 percentage points.

Table 13 presents power simulations for such an expanded subsidy design, across varying effect sizes and number of villages in the study. Column 1 is effect size in terms of stand deviation of some village-level outcome. Column 2 is power estimates if we capture outcomes from 600 villages, evenly split between treatment and control. Column 3 and 4 are power estimates if we capture village level outcomes in 800 and 1000 villages respectively. (We will address in a moment the question of how we can capture village level outcomes on political participation in 1000 villages).

Table 13 shows us that, even with a large sample size, we still need to decently large effects, about a third of a standard deviation of our outcome of interest, to be considered well powered. How realistic might such an effect be for a village-level outcome and what outcome could we measure across 800 or 1000 villages?

Table 13: Expanded Subsidy Program - Standardized Village Level Outcome

Effect	n = 600	n = 800	n = 1000
0.2	0.316	0.356	0.484
0.3	0.554	0.66	0.724
0.35	0.792	0.812	0.932
0.4	0.786	0.902	0.948

Note: The "baked in" assumption here is a first stage effect of about 58 percentage points

Voter turnout, captured at a village-level, might fit the bill. Below, we lay out a plan for obtaining measures of village-level voter turnout from administrative records. But first, we consider if the effect sizes are realistic. In the 2018 national elections, average polling station voter turnout in Kono was 84% and standard deviation of voter turnout across polling stations was 6.4% (author's calculations). 80 percent power would be achieved in a study across 1000 villages, if a tax collector visits increased voter turnout by 2 percentage points. If treatment was assigned across 800 villages, we would achieve 80% power with an effect of 2.2 percentage points (calculations not shown). Is that an achievable effect? Consider that a simple text message reminder to vote during the 2018 election increased polling station level turnout by about 1% (point estimates .7%).[13] Moreover, 2022 local elections do not coincide with national elections, and therefore levels of voter turnout should be lower, potentially leaving more room for larger effects.

Our plan for obtaining village level estimates of voter turnout is as follows: On election day, voters vote at Voting Centers where they are registered. To verify that they have voted, (to curb repeat voting and ballot stuffing) voters place their thumbprints on a large sheet of paper, next to their name and voting ID.

[13] Grieco, Meriggi, and Voors. "Voter Mobilization in Sierra Leone using SMS". Technical Report.

These cards are then stored at District National Election Commission (NEC) offices. Our plan is to work with NEC to get access to these voting cards, and record voting ID numbers of voters who turned out at each polling station, indicated by the marked thumbprint next to the name/voting ID. We can then turn to voter registration records (which we already have access to), which records the name of the village of residence for very registered voter. This allows us to construct a village-level measure of voter turnout for every village in the District. This village-level design could be supplemented by self-reported measures of participation in political events held in the runup to the 2022 elections. In the run-up to the elections, our research team could compile a comprehensive list of political activity and events. Respondents could then be asked about their participation in these events. Survey measures that capture participation in political events would be well powered.[14]

Finally, the above power calculation does not include a blocked randomization. This may be possible in the study design that we have proposed, blocking on either turnout from corresponding voting stations or village level covariates of turnout obtained from census data.

To briefly summarize some of the key takeaways of the above power calculations:

- We are likely insufficiently powered to leverage the 2019 Travel Subsidy Program to study the effects of taxation on political participation.
- If we attempted to leverage 2019 Subsidy Program, the most promising avenue would be to study individual level outcomes, clustering the analysis at a village level (Table 11). We conclude that the required effect sizes are too large to justify data collection. More importantly, the onset of COVID-19 makes this option unfeasible.
- Leveraging the 2019 Subsidy Program to capture the effect to taxation on *village level* measures of political participation is severely underpowered (Table 12)
- If the travel subsidy program was extended to cover most of the district's villages, we may be sufficiently powered to capture village-level outcomes. We believe a research design where subsidies are randomized throughout the district will be effective, due to the lack of spillover of tax collector visits from subsidized villages to nearby villages
- One village-level measure for political participation is village-level voter turnout in the 2022 local elections.[15] Effect sizes of about 2 percentage points would achieve 80% power.
- The possibility of a blocked randomization should be investigated.
- This could be complemented by individual level measures of self-reported political participation in the run up to the 2022 local elections.

[14] While we don't present comprehensive results, results from a single simulation are worth noting. A design that captures individual level self-reported participation in several political events for 10 respondents across 300 villages is 86% powered to detect effects .2 standard deviations in some participation index.

[15] So far the experimental literature has not capture the effect of increased taxation on voting behavior.

V: Partnerships & Collaboration

Maybe the most encouraging outcome of this project is the continued relationship building between the research team and Kono District Council (KDC). Above we detailed how our research team supported the Kono District Council to set up a tax collection data system that a) recorded geo-locations of villages visited by tax collectors, b) estimated village-level property tax potential revenue, and c) recorded and digitized tax revenue receipts at a village level. We plan to maintain technical support on this system in 2020. While the COVID-19 crisis has slowed tax collection in Kono District in 2020, a "refresher" training has been conducted with tax collectors in one chiefdom; our research team supported the District Council Valuator (head of revenue mobilization) in this training.

Above we noted that the true problem facing revenue mobilization in Kono district is the lack of compliance. We have been working with KDC to develop strategies for increasing compliance. One factor identified by the KDC as limiting tax compliance during the 2019 season was lack of community level awareness / sensitization (see report for IGC project 39423). The KDC has issued an official letter of support to work with our research team to develop a "Town Hall" intervention, "to bring community members together with relevant stakeholders for revenue mobilization" (Appendix 8).

Two other factors that were identified as limiting factors for tax collection during 2019 were a) lack of enforcement for non-compliance and b) lack of tax collector monitoring. We developed a concept note outlining strategies for improving tax compliance through a) enforcement measures for non-compliance taken by Local Courts and b) community level tax collector monitoring (Appendix 9). This note has been shared with Kono District Council has received verbal support; the Valuator has agreed to put support for these ideas in writing when necessary.

It is worth noting that the Kono District Council has also reached out directly to our research team for technical support for revenue mobilization. On February 27th, the Valuator of Kono District Council wrote to a member of the research team requesting technical support in developing a geo-tagged data base for communication poles and mining sites in Kono District (Appendix 10). A member of our research team will be traveling to Kono as soon as possible (ie, when the inter-district travel ban is lifted) to facilitate this request.

Finally, working with our partners at KoCEPO— the Kono based civil-society organization supporting tax reform in Kono— we have opened dialogue with the Koinadugu District Council on tax reform (see Appendix 11 for Letter of Support from KoCEPO, which mentions expansion of support to Koinadugu). In February 2020, Mr. Grieco traveled to Koinadugu District to meet with members of the Koinadugu District Council to discuss tax reform. The district council is interested in taking similar reform steps as in Kono, starting by fully engaging district Paramount Chiefs. Notably, the only place where the District Council is successfully collecting taxes— within the Kabala Township where the Council is located— they are relying on the support of relevant Paramount Chiefs. The District Council was further interested in the tax collector training and tax data system that is being built in Kono District. The next step is to try to hammer out a MoU between District Council and District Paramount Chiefs. If this is successful, Mr. Grieco has offered to support with technical assistance for tax collector training and data management.

VI. Concluding Discussion

The travel subsidy cost about SLL 5,071,164 ($522).[16] Even the most optimistic estimate of the revenue generating effect of the Travel Subsidy Program would fall far short of this. Still, the Travel Subsidy Program should not be considered a failure: $522 seems a relatively small price to pay for dramatically extending tax collector reach. Above, we made the case from a research design perspective that the Travel Subsidy Program should be extended throughout the district. But there is a strong policy motivation for this as well. The KDC has taken extraordinary strides in the last several years just by starting up the process of generating own source revenue by collecting taxes in the district. But the reform is still new and in many places there is limited awareness. Expanding the Travel Subsidy Program should be seen as an investment— it's a strategy for getting tax collectors out to all parts of the districts, many parts where the state rarely ventures, to spread news about the new tax reform initiated by KDC and District Paramount Chiefs.

Additional factors hindered revenue generation in 2019. First, tax rates during 2019 were too low. Property tax rates for most residential structures are SLL 10,000 – 15,000. By way of comparison, a medium size locally made baguette sold on the street side will cost you SLL 2,000 and a large one SLL 3,000. A yearly property tax just several times the price of bread is probably too small in any circumstance. Encouragingly, Kono District Council has taken steps to address this issue by increasing tax rates for 2020 (See Appendix 12). The minimum property tax has doubled to 20,000. An extra tax category has been added such that the tax on some types of mud brick house, easily the most popular housing type in the District (see report on potential tax revenue in Appendix 5).

Second, and more importantly, compliance rates are low. In our short report on district revenue submitted to District Council, we estimate that only 8.9 percent of total potential revenue is collected *in villages that tax collectors visited*. While tax rates are low, less than 10% of property owners are paying taxes in the villages that tax collectors visit (remember that only about 26% of villages are visited). Full attention needs to be given to compliance.

In conclusion, while the Travel Subsidy Program cannot claim to have "paid for itself" in 2019, we argue that the program was very effective at doing what it was intended to do— get tax collectors to villages they wouldn't otherwise visit for a relatively cheap cost. While the decision by Kono District Council to increase tax rates for 2020 tax season is a good first step, the current level of compliance is the biggest limiting factor on District tax revenue. Undoubtedly, strategies to increase tax compliance should be the focus of Kono District Council for future tax seasons.

[16] We say "about" because the total amount available for subsidizes was SLL 6,132,000 and 91/110 (82.7%) of subsidies were paid out. (SLL 6,132,000 * .827 = SLL 5,071,164). The exact amount of the subsidies can be checked by matching the visited villages to the subsidy amount for each village.

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