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Local Taxation and Development Finance in the DRC: A Comment on Balán et al. (2022)

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Abstract:

Balán et al. (2022) evaluate the impact of “local elites” involvement in local tax collection in a large city in the Democratic Republic of Congo. Using a randomized controlled trial to vary the identities of tax collectors, they find that local elites’ involvement raises tax compliance and total revenue by 50 and 44 percent, respectively. The paper argues that the primary mechanism behind the results is better targeting made possible by local elites’ superior information about property holders’ willingness and ability to pay. In this replication comment, we first reproduce the paper’s main results. Then, we assess the robustness of the results by (1) employing randomization inference for statistical tests; (2) controlling for baseline characteristics that are not balanced; and (3) using an alternative method to examine the claims supporting the preferred mechanism of better targeting. We find robust estimates in (1). However, the results are less robust both in terms of statistical significance and magnitude for (2) and (3). We conclude that the average treatment effect is robust, while the main claim about mechanisms, the information channel, is less robust to alternative estimation approaches. We contextualize and discuss the significance of these results, including the negligible revenue potential even under full compliance.

1. Introduction

Balán et al. (2022) (henceforth, BBTW) use a randomized controlled trial in Kananga, the capital city of Kasai-Central province of the Democratic Republic of Congo (DRC), to assess whether tax compliance depends upon the type of tax collector, local elites or state officials. The study addresses the broader issue of how fragile states build state capacity. Should such a state leverage local power structures to improve its capacity to govern in the short run? Or will greater reliance on local elites interfere with building state capacity in the long run? The paper provides evidence on the short-run effects of delegation and/or collaboration with local elites on tax collection in DRC, a state with very low tax compliance and a tax-GDP ratio of 7.3% in 2020, less than half of Africa's average and one of the lowest in the world (OECD, AUC, and ATAF 2022).

The previous literature has highlighted the role of local elites in establishing the state's legitimacy and improving the state's knowledge about local conditions (Basurto, Dupas, and Robinson 2020). However, local elites could also use this increased control to extract greater rents and divert tax revenue for private gain. To investigate these tradeoffs, the study randomized the assignment of 356 neighborhoods, consisting of 45,162 properties, to five groups, each exposed to different types of tax collectors: i) provincial officials (Central), ii) local elites (Local), iii) provincial officials who benefited from consultation with local elites (CLI), iv) a provincial official paired with a local elite (CXL), or v) a control group consisting of a declarative payment process (Control).¹ The main comparison is made between Local and Central. In the Local treatment, local elites are assigned the full tax collection responsibilities. In the Central treatment, state agents are assigned the full tax collection responsibilities without interaction between state agents and local elites. The primary outcome variables are tax compliance and tax revenues collected, and both variables come from administrative data. The authors also collected information using a household survey to measure willingness to pay, household income, property quality, and interactions with tax collectors.

The paper's main result, which is statistically significant at all conventional levels, is that the Local treatment achieves a 50% increase in tax compliance and a 44% increase in tax revenue relative to the Central treatment (p. 764). If the treatment had been implemented across all properties and neighborhoods of Kananga, a “city of 1 to 2 million inhabitants” (p. 767 of BBTW),

¹ The experiment also cross-randomized messages to encourage tax compliance, but we do not focus on this aspect of the study.

these effect sizes would translate into a total revenue gain of \$2,517 (USD 2022). Tax collectors in the Local treatment are also more likely to properly grant tax exemptions to the elderly and the disabled as required by law (p. 764). The analysis suggests that a revenue-maximizing government would need to weigh the social cost of bribing, which tends to increase in the Local treatment, much higher than net tax revenue to not prefer to delegate tax collection to local elites.

The paper claims that the benefit of delegating tax collecting to local elites comes from their superior ability, due to an informational advantage, to target taxpayers more efficiently. This claim is established by adjudicating among various pieces of evidence in favor or against three potential channels: i) local elites simply visit the property owner's place of residence more frequently; ii) local elites better target property owners that are more compliant (regardless of who would be collecting the tax); and iii) conditional on visiting a property owner's place of residence, local elites achieve higher compliance from the property owner. Each of these channels is consistent with the main finding. Based on a multitude of tests in the paper and the online appendix, the paper argues that there is strong evidence in support of ii) and the body of evidence is largely inconsistent with i) and iii).

Specifically, the paper argues that a direct test of ii), which is built into their experimental design, is the comparison of the CLI to the Central treatment (p. 765). It shows that the CLI treatment, which should also benefit from the informational advantage of local elites, indeed outperforms the Central treatment, recovering nearly 70% of the compliance obtained with Local.

To confirm that CLI is more effective than Central because of the information transmitted, the paper shows that CLI consultations, where local elites offered their assessments about property holders' willingness and ability to pay, correlate positively with CLI tax collectors' decisions to visit a property holder and with tax compliance outcomes. They then show that predictions of property holders' willingness to pay and ease of payment, based on a set of household characteristics (available from the household survey data), positively correlate with both tax visits and tax compliance in the Local treatment and only positively correlate with tax compliance in the Central treatment (pp. 784-86). Since the tax collectors in the Central arm could not have benefited from consultations with local elites and the information transmitted is likely difficult for them to observe, we would not expect to see any relationship between these predictions and tax visits in the Central arm. Yet, if the information transmitted does have value in predicting tax compliance,

then we would expect that the predicted variables would be associated with both tax visits and tax compliance in the Local arm, while only tax compliance in the Central arm.

The paper also develops and presents sophisticated tests to rule out the two other mechanisms, including, but not limited to, comparing the number of visits to households that tax collectors make, comparing tax outcomes when a peculiarity of the tax campaign constrained the ability of tax collectors to target households, as well as various treatment effect heterogeneity analyses. Thus, having established a preferred and plausible mechanism, the paper concludes that the delegation of functions of the government to local elites or collaboration with them in the short run is a successful strategy for building state capacity in this setting.

We conduct a robust replication of the paper. Our approach is to select a claim in the paper's abstract and narrowly evaluate it with alternative methods. The main claim is that local elites *"achieved higher compliance by using local information to more efficiently target households with high payment propensities, rather than by being more effective at persuading households to pay conditional on having visited them."* Our interest in this claim is mainly due to its policy implications. The paper, having reported "no erosion of tax morale", concludes that *"... a revenue-maximizing government would likely prefer chief to state collection in this setting (p. 792)."* Focusing on one claim and electing for a thin set of robustness checks keeps our analysis tidy and, we hope, allows the replication to send a sharper message. Given the complexity of the research design as well as the analysis in the paper, a more extensive replication analysis would dramatically increase the potential number of discrepancies without substantially increasing the force of this evidence. These self-imposed constraints come at a cost since we must remain silent on many aspects of the paper. We encourage the reader to read the paper in full and consider the balance of evidence presented by the authors before updating their beliefs about the paper using our results.

We believe the argument to establish the paper's claim requires at least two positive results (finding evidence that local information matters and that local information is costly to obtain by the state) and at least one negative result (ruling out the most plausible alternative mechanism). The negative result gives rise to our first adjustment to the approach adopted in the paper. We leverage the rerandomization design to argue that randomization inference would provide a more judicious assessment of whether we should fail to reject the null hypothesis. The next adjustments aim to better evaluate the CLI treatment as the direct test of the informational advantage. First,

since the baseline characteristic, trust in the city chief, is not balanced (BBTW Table A2 of the Online Appendix, p. 49) and that trust and both the ability of chiefs to persuade residents to pay as well as the quality of information that chiefs transmit in the CLI treatment are likely positively correlated, it is important to see whether the results change when accounting for this imbalance. Second, we use an alternative procedure to predict the ease of payment and willingness to pay. A robustness exercise concerning *Predicted ease of payment* and *Predicted willingness to pay* is important since the results using these variables represent smoking-gun evidence that better targeting due to an informational advantage is indeed at work.

The results of our efforts are as follows: We were able to computationally reproduce the results nearly perfectly in the paper. We were not as successful with our robust replication. The main treatment effect's magnitude and statistical significance stand: the Local treatment increases tax compliance and tax revenues collected relative to the Central treatment. However, the evidence that leads the paper to make a claim about the main mechanism, a claim that appears both in the abstract and the title, is less robust. Our alternative analysis casts some doubt on the original analysis and research design. We, therefore, learn from the paper that the Local treatment can outperform the Central treatment, but, in our minds, the evidence is insufficient to support the paper's main claim regarding the mechanism.

The rest of the comment proceeds as follows: Section 2 presents our attempt to reproduce the results. Section 3 covers the robust replication. Section 4 discusses the results and what we have learned from this exercise. Section 5 concludes.

2. Reproducibility

The replication files are well-structured and easy to navigate. The different file categories are organized and grouped into separate folders. The do-files are set to relative paths. Thus, it was easy to adapt the folders to any computer by changing the main directory. Nevertheless, when trying to reproduce the paper, a few coding issues needed to be fixed. But these issues did not change the results. First, even though the authors conveniently included in the replication files a do-file that automatically installed any missing package needed to run all the code at once, we needed to install a few additional packages and the replication code depends upon the Stata packages that had been previously installed on the authors' computers relative to the computer used to reproduce. In our case, the Stata version we used is SE 17.0 and it was run on a MacBook

Pro 1.4 GHz Quad-Core Intel Core i5 laptop. We needed to complete the list of packages with `cibart`, `binscatter`, `ranktest`. It would be helpful to amend the list of Stata packages required to run the entire code. Second, in the do-file, `TableA31.do`, the code attempted to apply labels to the variables `hetXt_1` and `het` on lines 108 and 109 before creating the variables themselves on lines 123 and 126. Once these issues are debugged, the codes produce nearly the same results as in the paper.² The one discrepancy occurs in BBTW Table 5. For the dependent variable, *Incorrect exemption*, the coefficient on *Local* has the opposite sign, but the magnitude and standard error are the same. The statement, “[local elites] were more likely to exempt the elderly and the disabled,” on p. 764 should be revised or the authors should clarify the discrepancy. Finally, the formatting of the results occasionally needs to be adjusted manually to match the formats appearing in the paper.

3. Replication

This section describes our approach to robust replication and presents the results³.

3.1. Randomization inference

Given the rerandomization design that the authors employ, the approach to inference that the authors use is overly conservative for BBTW Tables 4, 5, 6 and 7 (Li et al. 2018). While this is not an issue for the main result in BBTW Table 4, it does suggest that the authors may be more prone to failing to reject the null for negative results. To safeguard against this potential criticism, we employ randomization inference. Randomization inference will also provide an alternative inference approach for BBTW Table 8, which uses generated regressors in columns (5)-(8) without making any correction.

To implement randomization inference, our procedure first randomly permutes the treatment assignment at the neighborhood level 1,000 times. To obtain the p-value, we take the share of

² In BBTW Tables 4-8, we identified a total of six coefficient-standard error pairs that had differences that could not be attributed to rounding to the hundredth decimal. We do not report five of these since they do not substantially change the authors’ presentation of the results. In addition, in the last column of Panels C and D of BBTW Table 5, we were unable to computationally reproduce any of the means of the Central treatment. Since these are not results, we do not include them in our assessment of computational reproducibility.

³ The replication files along with all the outputs are located at the following link:
<https://github.com/SossouAdjisse/LocalTaxReplicationProject.git>.

estimates from the permutation exercise that are below the reported estimate in absolute value. In Tables 4R1-8R1, we show that the randomization inference exercise produces roughly similar p-values. Some p-values are larger than the ones based on the OLS t-stats, most likely reflecting the fact that there are relatively few neighborhoods for four treatment arms and a control. Some minor changes are of note. First, the effect of the Local treatment on tax revenue in Column 1 of Table 4R1 and the effect of CLI in Column 2 of Table 7R1 goes from 5% to 10% statistical significance. Second, Table 8R1 shows that Column 5 goes from 10% to 5% statistical significance and for willingness to pay from 10% to 1%. Third, and the only substantive change, the effect of CLI on tax compliance in Column 5 of Table 7R1 loses statistical significance at conventional levels.

3.2. Controlling for the respondents' trust in the city chief

Our next approach is to control for the baseline characteristic measuring trust in the city chief, *Trust in chief*, while balanced between Central and Local, is unbalanced in the experiment and should matter for the tax compliance and revenue mobilization outcomes.⁴ Before discussing the results, we note the paper uses the terms, city chiefs and local elites, interchangeably, meaning *Trust in chief* is a variable that represents the trust that the respondent places in the local elite and would therefore seem like an important factor in the effectiveness of the treatments. We also note that while the variable is called *Trust in chief*, the English translation in the online appendix says this question is about confidence (coded on a 4-point scale: No confidence, not very much confidence, Some confidence, A lot of confidence). We wondered, and the authors confirmed, reassuringly, that this is merely a mistaken translation of the French word, *confiance*. The original survey, which was administered in both the French and Tshiluba languages, uses the concept of trust.

We must first warn the reader that this robust replication exercise forces us to restrict attention to the properties of property holders who appear in the baseline sample, covering roughly 10% of all properties in the administrative data (i.e., the universe of such properties). Only respondents in the baseline sample were asked the question used to construct the variable *Trust in chief*. Therefore, it is prudent to first check whether our analysis reflects the difference in samples or their sizes. An alternative approach, one suggested to us by the authors in correspondence, would be to take the

⁴ We note that the paper tests for balance treating this variable as a continuous one. The variable is also unbalanced when treating it as a categorical variable as we do in our subsequent analysis.

baseline average-level of *Trust in chief* in a neighborhood and use this neighborhood-level variable as a control, circumventing the sample restriction. However, such an exercise does not capture the individual-level heterogeneity of *Trust in chief* within a neighborhood that a tax collector encounters.

We alert the reader that this sample restriction is, in principle, neither arbitrary nor severe. First, the authors, in fact, use the same sample for various analyses in the paper for both dichotomous and continuous outcomes (e.g., Tables 3, 5, and 6, among others) without concerns about sample differences or statistical power. Accordingly, we should presume that the study is adequately powered when restricting to the baseline respondents. Second, the baseline sample of respondents is randomly selected within a neighborhood and is stratified by the same set of neighborhoods. Since the treatment is randomized at the neighborhood level, we suspect that the number of neighborhoods (which remain the same in both samples) is of considerable consequence for statistical power since we anticipate that the intracluster correlation is nonnegligible and the desired minimal detectable effect size is moderate given the underlying objective of building state capacity.⁵ In correspondence with the authors, we learned that the study is, in fact, severely underpowered for *Tax compliance* when using the baseline sample, which gives us unease in interpreting their balances tests and some of their null results that do not use the full sample of administrative data.

Inference aside, we would still expect the results of this exercise to yield estimates similar to those when using the full administrative data set; therefore, this sample restriction is a useful robustness exercise on its own. In Tables 4B-8B, we redo BBTW Tables 4-8 by restricting the sample to baseline respondents. In Table 4B, we see results that are quite similar to those in BBTW Table 4. We should note the loss of statistical significance in column (1), but the results in BBTW Table 4 are preserved in the remaining columns. We also see minor differences in Tables 6B and

⁵ For illustrative purposes only (using the Stata command *power*), the decrease in statistical power between the two samples, assuming a modest intracluster correlation of 0.15 and setting a baseline compliance rate at 0.05 and an effect size of 0.065, which would bring the baseline compliance rate to the level of an earlier RCT on property tax compliance in the DRC (Weigel 2020), is only 4% (95.4% vs. 99.2%). If the ICC were equal to one, then the two samples would have the same statistical power. Holding the ICC constant, decreasing the effect size would increase the difference in power between the two samples. For example, decreasing the effect size to 0.035, would yield 57% power for the smaller sample and 73% power for the larger one. In such a case, we could achieve 73% and 91% power, respectively if we were to split all the 356 neighborhoods across Local and Central. For reference, the baseline sample would have 80% power to detect an effect size of 0.035 with an ICC as high as 0.0485 (using the original number of neighborhoods), while the larger sample would have 99% power. This discussion highlights the importance of the authors' input in determining whether the study is adequately powered.

Tables 7B relative to BBTW Tables 6 and 7, respectively. Table 8B exhibits noisier estimates, but qualitatively similar results as in BBTW Table 8 for columns (1)-(4) in both Panels A and B. The comparison between Table 8B and BBTW Table 8 for columns (5)-(8) is mixed and characterized by less precision. The analysis reveals that the sample restriction does not alone drive discrepancies with the results in BBTW Tables 4, 6 and 7, while it may be a source of discrepancy for the results in BBTW Table 8. Participation in the baseline survey could make property holders behave differently than they otherwise would have behaved. Alternatively, the results in BBTW Table 8 columns 5-8 could simply be less robust than other results in the paper due to a heightened sensitivity to sample size.

Returning to the main analysis of this second robust replication exercise, we find that controlling for *Trust in chief* introduces some uncertainty in the interpretation of the results. If the results were robust to baseline imbalances, then we would not expect to see substantive changes in the estimates. The magnitude of the effect of CLI on tax compliance, relative to the outcome in the Central arm, increases by 20% in column (1) and by over 50% in column (6) in Table 7R2. The change in estimates is so dramatic that we can no longer reject the null hypothesis that the effect of CLI is equal to the effect of Local. Additionally, the effect on revenue of CLI is no longer statistically significant in columns (2) and (7).

In Table 8R2, which considers CLI neighborhoods in columns (1)-(4), Local in columns (5)-(6) and Central in columns (7)-(8), also differs from Table 8B and BBTW Table 8 in important ways. In Panel A of column (5), once we account for *Trust in chief*, the sign of the relationship between *Predicted ease of payment* and *Tax visits* reverses and the coefficient is statistically significant at the 10% level. Therefore, the positive correlation between chiefs' recommendations and chiefs' own pattern of visits is not robust to controlling for *Trust in chief*. In addition, we see that respondents that have no trust in the chief are more positively associated with compliance in the Local neighborhoods than those with some trust, conditional on *Predicted ease of payment*. As in Table 8B, *Ease of payment* and *Willingness to pay* recommendations are no longer statistically significant for *Tax visits* and *Predicted ease of payment* and *Predicted Willingness to pay* are no longer statistically significant for *Tax compliance* in Central neighborhoods.

For comparison, to show that we can recover what one would expect to see for a balanced variable, since *Trust in chief* is balanced across Local and Central arms, we verify that BBTW Table 4 is robust to controlling for *Trust in chief* in Table 4R2. Similar to Table 4B, in column (1),

while we see that the main result loses statistical significance, the magnitude of the coefficient is only slightly reduced. The coefficients in columns (2)-(4), which include the preferred specification, are still statistically significant and of similar size. Table 6R2, again a comparison between Local and Central, is also very similar to Table 6B and BBTW Table 6 is robust to controlling for Trust in chief. This analysis clearly indicates BBTW Table 7 is less robust than BBTW Table 4 and we require more assessment to determine the robustness of BBTW Table 8.

3.3. Using an alternative prediction method

Next, we turn our attention to the prediction method in the interest of improving the stability of the results in BBTW Table 8. The method used in the paper regresses an ordered categorical variable on a set of predictors using a linear regression.⁶ Due to the ad-hoc nature of the construction of the *Predicted ease of payment* and *Predicted willingness to pay* variables, we construct these variables in an alternative way that better reflects the nature of the data-generating process. Instead of linear regression, we use an ordered Probit model that selects cutoff levels as part of the estimation. Table 8P shows that, for *Ease of payment*, this approach outperforms the approach with ad-hoc cutoffs, but it does not perform dramatically better. We also see separation in the predictions of the two approaches since the ordered Probit prediction does not perfectly correlate with the BBTW prediction. For *Willingness to pay*, there is no noticeable change in performance and the two approaches yield similar but not the same predictions. We then use *Predicted ease of payment* and *Predicted willingness to pay* from our approach and rerun the specifications in columns 5-8 of BBTW Table 8. In columns (5)-(8) of Table 8R3i, the main results hold for both *Predicted ease of payment* and *Predicted willingness to pay*. In terms of magnitudes, the results become even stronger for *Predicted ease of payment*, consistent with attenuation bias due to measurement error in the original approach.

We next build on the previous robust replication exercise by introducing additional observable characteristics in the prediction stage that are excluded in BBTW because they do not vary at the

⁶ The set of predictors are chosen in a two-step process. First, a large set of predictors consisting of various household characteristics are chosen by the authors (unfortunately, the larger set of included variables is unknown to us). Second, OLS is run on this set of predictors and those that are statistically insignificant are discarded, yielding the final set of predictors. After the set of predictors is selected, the method requires predicting the ordered outcome using estimates from the linear regression. Finally, one should construct the *Predicted ease of payment* and *Predicted willingness to pay* by choosing two arbitrary cutoff levels that split the real line into three parts.

household level. Given that *Trust in chief* appears to matter for the effectiveness of CLI, it is instructive to verify whether chiefs' characteristics or other neighborhood characteristics are correlated with the nature of the information that is being transmitted to the tax authorities in CLI consultations.⁷ Controlling for chief characteristics is particularly important because of the subjective nature of these assessments. More generally, community-level variables matter for understanding willingness to pay – here, we consider only a few variables that are available and discernable to us. Accordingly, we redo the selection stage by incorporating such observable characteristics together with the original five predictors (since we did not have access to the original initial large set of predictors, this is the best that we can do).⁸ To keep the same degrees of freedom in the prediction stage, if the second step produces more than original number of predictors, for lack of a better way to discard variables, we take those with the highest t-stats to generate the same number of predictors. Once the new set of predictors (plus ethnic dummy fixed effects and other fixed effects used by the authors) is selected, we then perform the prediction using ordered Probit. For *Ease of payment*, the selected predictors are the respondent's sex, respondent has a salary, a dummy for the respondent's neighborhood's chief being older than the median chief, a dummy for the respondent's neighborhood being above-median in remoteness, and a dummy for the respondent's neighborhood's chief having above-median trust in the government (and ethnic group fixed effects) and, for *Willingness to pay*, the selected predictors are respondent's sex, respondent has a salary, a dummy if the family member of respondent's neighborhood's chief previously served as chief in the neighborhood, a dummy for respondent's neighborhood's chief having above-median view of government's responsiveness to needs of neighborhood, and respondent's neighborhood's treatment status during the 2016 tax campaign (and ethnic group fixed effects).⁹

⁷ While the authors are cognizant of the general problem associated with subjective assessments, (e.g., the paper shows that state tax collectors who consulted with "better informed" chiefs achieve higher compliance in CLI (p. 786), none of their robustness checks use chief characteristics in the prediction stage.

⁸ The original set consists of six predictors, age, sex, employment status, has a salary, government job status and ethnic group. However, since ethnic group variable is a set of ethnic group dummy variables, we decided it would be better to treat this variable as ethnic group fixed effects and include them in all predictions for the purposes of this exercise.

⁹ We note that the original sets of predictors are surprisingly the same for *Willingness to pay* and *Ease of payment*. There are three explanations: 1) it is merely a coincidence; 2) it is due to an oversight; and 3) the selection procedures are not based on *Willingness to pay* and *Ease of payment*, but rather some other outcome, for example, poverty status. The first is unlikely, the second is troublesome and the third is plausible albeit a puzzling choice.

In Table 8R3ii, we rerun the results in BBTW Table 8 by substituting *Predicted ease of payment* and *Predicted willingness to pay* in columns (5)–(8) with our predictions. Table 8P shows similar levels of accuracy, despite the lack of household-level variation in the included neighborhood and chief characteristics, and there is a small improvement in the prediction for *Willingness to pay*. Reassuringly, the city chiefs’ assessments of ease of payment and willingness to pay are still predictors of visits and tax compliance in the CLI arm. However, the new sets of predictors lead to changes in the results that do not fit with the preferred narrative.¹⁰ *Predicted ease of payment* no longer predicts tax visits and tax compliance in the Local and the Central arms. *Predicted willingness to pay* gives mixed results: it remains predictive for tax compliance in the Central arm, which is consistent with the authors’ findings, but perplexingly does not for Local.

4. Discussion

The paper presents a rich set of results and discussion that we cannot fully cover here other than to say that we commend the authors for their contribution to the literature. Our discussion is instead limited to the objective of the robust replication to improve confidence in the findings highlighted in the abstract.

The main result, the average treatment effect, is robust to our analysis. Indeed, given that this is a randomized controlled trial, our prior was that the result would hold. Nevertheless, the main result in column (1) of BBTW Table 4 is no longer statistically significant when we restrict to the baseline survey sample. To be precise, the robust replication estimate is 0.020 (SE=0.016), a 13% decrease in the magnitude of the original effect of 0.023 (SE=0.008) and a doubling of the standard error. Naturally, one would suspect the reduced precision in the smaller sample as the main culprit for the lack of robustness. However, without a better understanding of the research design, we are unable to judge how damaging this discrepancy is in terms of confidence in the original results, and we invite the reader to defer to the authors' input on this matter. We only note

¹⁰ We also get similar results if we do not impose the five highest t-stat restriction and instead use all statistically significant predictors.

that, using the baseline sample, we were able to more closely replicate other results that also compare Local to Central.

Based on our understanding of the context and after reading the paper, we had a prior against elevating the proposed mechanism to a higher status relative to the others for several reasons that we discuss below. Despite this, our view of an effective, robust replication is to avoid subjecting a result to a battery of tests in search of a reversal. Rather, we should limit our investigation to only alternative methods that are fully consistent with the authors' own research design and should, in principle, tangibly improve confidence in the analysis. As such, we undertook relatively minimal and justifiable deviations from the authors' empirical approach to establish robustness. Having considered the results of the robust replication presented in the previous section, we conclude that the differences are sufficient to cast significant doubt on the elevated status of the information/targeting mechanism as the most likely explanation for the results. This conclusion is based on the assessment of both the positive and negative results underlying our analysis, that is, lack of strong evidence supporting simultaneously that i) local information matters, ii) local information is costly to obtain by the state, iii) and ruling out other plausible alternatives.

With respect to the robustness of the negative tests on evidence for other channels, it should be clear that the aim of this replication is not to establish an alternative mechanism that could then supplant the paper's preferred one. The aim is to assess confidence in the paper's stated findings, which include an assertion that the targeting/information channel is a likely mechanism while others that they consider are not. In the interest of transparency, we view this assertion as problematic given the research design. First, the three channels analyzed are not mutually exclusive, so that any one of them could be at work supporting the impact of the others (e.g., chiefs could visit property owners more (less) intensively when they know that will be more (less) likely to pay and these visits may be essential to get property owners to pay), nor exhaustive of all possible channels (e.g., property owners may be more likely to voluntarily pay property tax unconditional of a visit by the chief because they know the high opportunity cost of the chief spending time to collect taxes). Second, even if they were mutually exclusive and exhaustive, the current research design does not adequately hold the other mechanisms fixed while randomly varying one. For example, the CLI variant randomly varies access to local knowledge, while preserving other aspects of the Central treatment, yet it does not succeed in randomly varying the

targeting-based-on-superior-information mechanism since state tax collectors are free to choose to ignore this advice (62% of CLI tax collectors report that they did not only target households recommended by the chief, p. 784) and city chiefs are free to encourage those property holders that they have recommended in consultations to comply. Of course, identifying mechanisms in this setting is challenging and the authors do a commendable job given their objective. Despite our concerns, according to their research design and our analysis plan, since the negative results are all robust to randomization inference, the first robust replication exercise should increase confidence in the results that the paper uses as evidence to downgrade the status of other mechanisms.

That said, we believe this evidence is not as conclusive as the paper puts forward. The incertitude is buttressed by crucial contextual information that affects the meaning of key variables and makes it difficult to rule out other mechanisms adequately. We discuss two important examples here. First, the paper rules out greater collection efforts as a mechanism by measuring the reported number of visits by tax collectors to respondents' homes. This variable is likely a very imprecise measure of tax collection efforts by city chiefs, while it may perfectly measure tax collection efforts by provincial authorities. In theory, because the chief lives in the community, any encounter can count as a visit because the non-compliant taxpayers will be directly or indirectly reminded of their tax obligation. Such encounters include various community places like marketplaces, unrelated community meetings, or just being a neighbor of the chief. In this case, even meeting the chief's relatives in the community can count as informal tax visits. We note that the paper documented self-reported proxy variables for such encounters suggesting that, outside of the tax campaign, the chiefs' encounters with their community members were not statistically different from those of Central. However, considering that the role of the chiefs has been defined as " ... *local notables whose main responsibilities include (i) mediating local disputes, especially over property; and (ii) helping maintain local infrastructure through an informal labor tax (salon) in which citizens help repair roads, bridges, and other local public goods. Chiefs are nominated by elders in the neighborhood—typically for being longstanding and respected residents—and then rubber-stamped by the government,*" even if, for the purposes of the study, actions were taken to limit these roles and responsibilities, it is inconceivable that such measures can be sustained without altering the chiefs' role in the community. Therefore, we wonder whether these proxies adequately capture the interaction between city chiefs and local residents. Second, using the term

“local elite” in this context can be a misnomer and obviously has implications for the reader’s understanding of what city chiefs are optimizing. Additionally, we note that this intervention occurred in the wake of the 2006 decentralization law that formally defined and significantly reformed some of these city chief positions and, consequently, may have introduced new or strengthened old responsibilities and accountability mechanisms to the governor or the interior ministry.¹¹ Simply put, we question whether the experiment adequately accounts for differences across the types of tax collectors to elucidate mechanisms.

More generally, the type of relationship between city chiefs and local residents that comes across in the paper’s narrative is an extractive one, both in terms of information and tax revenue (the paper acknowledges that the relationship is more of a reciprocal one in footnote 37 and elsewhere). The paper offers no discussion of how city chiefs’ information about local residents may change with their new tax collection responsibilities. Introducing the extractive aspect of taxation into the relationship between city chiefs and local residents could inhibit information flows moving forward. Important for the paper’s narrative to be one of general interest is the tradeoff between the benefit of better targeting and the cost of revenue leakage. Due to possessing superior information that is unobservable to the tax officials, city chiefs can divert some tax revenue to their pockets and even use their new power to extort. However, if city chiefs are part of a larger implicit contract, they may use their new responsibility as an additional tool to spread risk and redistribute resources within the community. One might associate a finding of Local having higher bribe incidence and bribe payments as evidence consistent with the extraction view.¹² In contrast, under the implicit contract view, there need not be a strong correlation and most likely should not be one over time. Table 1 presents suggestive evidence that reality may not fit the simple extractive view. Rows 1 and 3 are from BBTW Table 5 in the paper and, for illustrative purposes, we add rows 2 and 4 on the amounts of bribes paid. Bribe amounts paid are lower at the midline and they are not statistically different at the endline in Local relative to Central Table 1, which is more consistent with the implicit contract view. We are not in a position to do a full accounting of extraction or establish evidence in favor of one view over the other. However, we include Table 1 here to show that the tradeoff that motivates the paper is too simplistic for this

¹¹ See Chapter 3 of the 2006 decentralization law.

¹² This simplistic view of extraction, while it motivates the paper, should not necessarily be attributed to the authors. The authors indeed have research underway that develops a much more sophisticated view of equilibrium bribe incidence and payment.

context (as the authors appear to be keenly aware). To put this point another way, we wonder whether the paper adequately identifies and measures the true cost side of the tradeoff that the government faces when working with local elites or city chiefs to tax its citizens.

Table 1: Local vs. Central in Bribe Payments

	Coefficient	Std. Err.	p-value	N	Mean in Central
Pays bribe (midline)	-0.001	0.003	0.843	18596	0.016
Bribe amounts paid (midline)	-12.527	5.008	0.013	18570	26.353
Pays bribe (endline)	0.018	0.009	0.051	1169	0.014
Bribe amounts paid (endline)	19.857	12.514	0.114	1169	13.732
<i>Notes: Each row is a separate regression. Coefficient is the coefficient on Local treatment variable. Pays bribe and Bribe amounts paid are self-reported bribe payments, measured at the midline and endline.</i>					

With respect to the robustness of the positive tests on evidence for information/targeting, we show that the result championed as a direct test of the informational advantage is not robust. Specifically, the paper's findings that Local outperforms CLI in terms of the extensive margin of tax compliance and that CLI is more effective than Central in terms of tax amounts collected are not robust to controlling for an imbalanced baseline covariate. Simply controlling for baseline levels of *Trust in chief* is sufficient to make CLI indistinguishable from Local. Following the paper's logic, this would constitute even stronger evidence of information transfer, which already in the original result recovered 70% of Local's effect. However, in the context of a robust replication, the results should not have changed, and this dramatic difference *prima facie* reduces confidence in the stated findings. It also now questions a claim made in the paper that not all information can be transferred through consultation (p. 786). More importantly, from our point of view, this evidence rather negates the need for delegation. That is because, if CLI is as effective as Local, then there is no reason to delegate (which is a core contribution of the paper) and unnecessarily burden city chiefs with tax collection. All that is needed is a brief consultation with city chiefs before tax season (or obtain the same information through other means).

Alternatively, CLI may be just as effective, not because of easily transferrable information, but because chiefs' direct or indirect involvement in ensuring tax compliance in the CLI neighborhoods, such as, but not limited to, city chiefs taking actions, unobservable to the researchers, based on their stake in the success of their consultations to the state (e.g., tax officials

report to their supervisor that the city chief is helpful and competent). This latter interpretation certainly would undermine the paper's narrative and would suggest a role for persuasion, whereby certain chiefs are better able to elicit greater tax compliance among property holders in the CLI arm. This interpretation, as opposed to the full information transfer interpretation, is bolstered by the results of the third robust replication exercise. Once we account for other observable characteristics in the prediction stage of the CLI consultations, the null results that we find on *Predicted ease of payment* and *Predicted willingness to pay* also suggest that chiefs may have identified individuals that the chief could have more easily persuaded in the CLI arm instead of those with observable characteristics that predict compliance as the authors argue.

Finally, the paper appeals to Weber when characterizing a fragile state trying to build state capacity. Historically, states have delegated the function of tax collection to private entrepreneurs to economize on administrative costs (Hoffman 2017). Weber (1922) explicitly refers to tax farming's ability to reduce administrative costs and spread the risk of uncertain revenue streams (White 2004). Of course, the literature has long discussed relying on tax farming to increase fiscal capacity in developing countries (Stella 1993). The paper distances itself from the tax farming literature (see footnote 2 of BBTW; Weber's tradeoff is less relevant in the modern era due to digital technologies and the modern financial system); however, examining this suggestion in more detail is instructive.¹³ In contrast to delegation, tax farming could decouple tax collection from electoral incentives. Decoupling matters because the argument put forward by Baldwin (2016), a work that the paper cites to support the research question, to leverage local chiefs' power in state-building in weak states, relies on traditional leaders having a long-term interest in the local population in contrast to the state's short-term interest. Rather than an illustration of Baldwin's paradox, we see that the treatment effect of Local is close to zero and is not statistically significant for chiefs that presumably have a long-term interest in the locality (those that also hold a customary office or have dynastic succession).¹⁴ Moreover, delegating local tax collection to city chiefs undermines Baldwin's argument because city chiefs' incentives will be increasingly contaminated with electoral incentives. As already discussed, for example, one could speculate about whether city chiefs had an interest in the performance of the tax collectors in the CLI arm in hopes of

¹³ While we note that Stella (1993) argues that property tax would be one of the more attractive types of taxes to farm, we are not suggesting that the DRC should use tax farming for property tax collection.

¹⁴ The analysis that allows for heterogeneous treatment effects by the type of city chief is from BBTW Table A27 in the Online Appendix.

pleasing provincial authorities who could appoint them. From this point of view, Local and CLI could be equally effective, and more effective than Central, because city chiefs face the same short-term interest in both treatments. Better neighborhood performance boosts their reputation as an effective agent for state activities in contrast to tax officials who already hold their positions.

5. Conclusion

In this paper, we replicate the analysis in BBTW to assess the robustness of the central claim of the paper on the prominence of information as a mechanism over alternatives such as persuasion.

Before embarking on our robust replication, we computationally reproduced the results with minor discrepancies, except for one result whose coefficient had the opposite sign, negating a statement that “[local elites] were more likely to exempt the elderly and the disabled.” This statement had been used to further support a claim in the paper (using city chiefs as tax collectors does not undermine tax morale) that we chose not to directly examine in our analysis.

This initial exercise, coupled with the first part of our robust replication, employing randomization inference, increased our confidence in the average treatment effect reported. Further analysis of the mechanism, however, raised several doubts, including whether the relevant information for targeting is, in fact, difficult to obtain by the tax authorities and whether the paper rules out alternative mechanisms. This analysis led us to conclude that the mechanism remains an open question to which an answer is likely a mixture of effort, information and persuasion.

While the differences between our robust replication results and those in BBTW Tables 7 and 8 alone caution against drawing broader policy conclusions from this study, this conclusion is supported by two points of discussion that immediately arise when engaging substantively with the paper as we have done in this replication. First, recommending that the state grant some power to local elites to collect taxes or influence tax collection is risky. The proposition is tempting because, as the paper points out, a virtuous cycle can emerge through the initial increase in tax revenue mobilization, which can then be used to increase state capacity. Since the informational advantage of local elites will dissipate as state capacity increases, there can be a gradual transition away from the local elites to salaried officials. However, if persuasion instead explains the positive treatment effect of Local, then delegating control could initiate a vicious cycle instead of a virtuous one. Control over tax mobilization by local elites would only strengthen their relative position vis-

à-vis the state. Moreover, casting city chiefs in the DRC as local elites and then using their behavior to estimate the tradeoff between corruption and compliance could vastly underestimate the level of extraction by local elites in other contexts. While it is likely that an informational advantage exists, other mechanisms, such as persuasion, could be at work, and both the study design and the evidence do not allow us to understand how important persuasion is or whether a vicious or virtuous cycle is likely to emerge.

Second, even if there were a large informational advantage that would lead to a virtuous cycle, policymakers should think carefully about risking the disruption of the social fabric for rather minuscule financial gains. City chiefs, among other things, help resolve neighborhood disputes and thus play the role of moral authorities, as evidenced by the higher trust the people place in them relative to elected officials (Logan and Amakoh 2022). Turning them into tax collectors may yield hidden unintended consequences over time that outweigh the revenues' value – even if the revenues were much higher than the total treatment effect. We estimate that, even at full compliance, the government would only raise \$87,000 USD in revenue. Since incentives to extract are only likely to get stronger and the informational advantage, as the paper argues, is likely to get weaker when the potential gains in tax revenue increase, the chance of a virtuous cycle emerging through delegation to local elites would certainly need to be reevaluated and could likely not exist at policy-relevant levels. Therefore, using these results to inform short-term revenue maximization is short-sighted and potentially misleading. In our opinion, the results, while valuable on their own, are severely limited in application to important debates about state capacity and institutional development in low-income countries.

Based on the evidence presented in this robust replication, together with our priors and our general criticism, we conclude that more research is needed before accepting the paper's bold policy claims, such as *“local elites are important allies for fragile states seeking to establish rudimentary fiscal capacity (p. 794 of BBTW).”* For example, one would want to know, among other things, if there were other, less disruptive means of gathering the necessary information to improve targeting, given that this information appears to be easily transferable and correlated with observable characteristics. More generally, one would want to know if there were more effective ways of increasing tax compliance that do not involve extracting information about a taxpayer's ease of payment or willingness to pay. In the exact same setting, Weigel (2020) shows that a tax campaign that informs property holders of their obligation to pay taxes can generate an effect size

that is over three times higher in absolute terms and many times higher in relative terms than the effect size of the Local vs Central treatment.

Notwithstanding the findings of this replication exercise, this research is a welcomed contribution and encourages deep reflection on issues at the intersection of domestic revenue mobilization, local governance, and broader institutional development. We hope this replication exercise invites a richer discussion on these issues in the profession.

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Tables Section 3.1

Table 4R1: Local versus Central: Compliance and Revenues: Randomization Inference

	(1)	(2)	(3)	(4)	(5)
	Tax Compliance	Tax Compliance	Tax Compliance	Tax Compliance	Tax Compliance
	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues
Local	0.0226 (0.00809) [0.016]	0.0322 (0.00715) [0.001]	0.0320 (0.00830) [0.000]	0.0331 (0.00719) [0.001]	0.0396 (0.00827) [0.000]
Month FE	No	Yes	Yes	Yes	Yes
House FE	No	No	No	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes
Observations	28872	27764	213	27764	23803
Clusters	221	213		213	213
Mean	.068	.063	.065	.063	.073
	(1)	(2)	(3)	(4)	(5)
	Tax Revenues	Tax Revenues	Tax Revenues	Tax Revenues	Tax Revenues
	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues
Local	57.63 (25.69) [0.051]	79.64 (22.86) [0.005]	81.83 (38.59) [0.019]	68.86 (20.56) [0.010]	81.99 (23.56) [0.003]
Month FE	No	Yes	Yes	Yes	Yes
House FE	No	No	No	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes
Observations	28872	27764	213	27764	23803

Authors' Notes: This table reports estimates from equation (1), comparing property tax compliance in Local and Central (the excluded category). The two panels show estimates from separate regressions of compliance and revenues (in Congolese francs) on treatment, respectively. All regressions include fixed effects for randomization strata and cluster standard errors at the neighborhood level. Column 1 regressions do not include time period fixed effects described in Section IV while those in other columns include them. Regressions in columns 1–3 do not include house fixed effects. Column 3 shows results when the data are collapsed to the neighborhood level. We use robust standard errors and assign the minimum value for time period fixed effects to a neighborhood. Regressions in column 5 exclude exempt properties. The data include all properties registered by tax collectors merged with the government's property tax database. We discuss these results in Section IVA. **Replication Notes:** Below standard errors, we report p-values in brackets calculated using the distribution of coefficients estimated from 1,000 corresponding regressions of the outcome variable on random permutations of treatment assignment.

Table 5R1: Local versus Central: Mismanagement and Views of Government, Chiefs, and Taxes: Randomization Inference

	beta	SE	p	r ²	N	Central mean
Assigned Exemption	0.039	0.021	0.142	0.055	13772.000	0.266
Incorrect Exemption	0.012	0.007	0.175	0.020	13771.000	0.044
Assigned High Band	0.030	0.021	0.221	0.230	27764.000	0.114
Incorrect Assignment	-0.013	0.006	0.054	0.041	27764.000	0.031
Paid Bribe (Midline)	-0.001	0.003	0.859	0.007	18596.000	0.016
Gap Self v. Admin (Midline)	0.016	0.009	0.108	0.018	14309.000	0.077
Paid Bribe (Endline)	0.018	0.009	0.089	0.049	1169.000	0.014
Other Payments (Endline)	0.031	0.014	0.046	0.041	2407.000	0.094
	beta	SE	p	r ²	N	Central mean
View of government (index)	0.023	0.049	0.681	0.100	2411.000	-0.033
Trust in government	0.127	0.057	0.054	0.075	2286.000	-0.079
Responsiveness of government	-0.049	0.045	0.338	0.099	2282.000	-0.002
Performance of government	-0.060	0.052	0.264	0.060	2179.000	0.033
Integrity of government	0.043	0.047	0.403	0.058	2313.000	-0.038
Perceived tax compliance on avenue	0.100	0.055	0.136	0.073	1851.000	-0.065
Trust in tax ministry	0.085	0.061	0.205	0.073	2259.000	-0.075
Property tax morale	0.075	0.047	0.158	0.057	2343.000	-0.025
Fairness of property taxation	-0.004	0.053	0.941	0.046	2407.000	-0.007
Perception of enforcement	-0.019	0.058	0.776	0.070	2379.000	-0.021

Authors' Notes: Each row summarizes an OLS estimation of equation (1), comparing Local and Central, with the dependent variable noted in the first column. The column header $\hat{\beta}$ is the coefficient on the treatment indicator, followed by the cluster-robust standard error, R^2 , number of observations, and the Central group mean \bar{x}_{Central} . In panel A, row 1 shows differences in whether the collector designated the property exempt from taxes. Properties owned by the elderly, widows, government pensioners, and handicapped individuals, among others, are legally supposed to be exempt. Row 2 shows differences in whether an independent enumerator disagreed (in either direction) with the exemption status of a given property. Row 3 shows differences in whether a property was assigned to the high-value category, and row 4 shows whether enumerators' independent evaluations diverged with the collectors' designation. In panel B, the outcomes in rows 5 and 7 are self-reported bribe payment as measured during the midline and endline surveys, respectively. The outcome in row 6 indicates property owners who reported paying the tax but who were not recorded as having paid in the administrative data. The outcome in row 8 is self-reported payment of any informal fees at endline. We discuss the results from panels A and B in Section IVB. Panels C and D control for the baseline value except when analyzing perceived tax compliance and fairness of property taxation, outcomes we only measured at endline. Each dependent variable, described briefly in Section IVB and in detail in online Appendix A2.6, is standardized to facilitate interpretation of coefficient magnitudes. We discuss the results in panels C and D in Section IVB. In all panels, regressions include fixed effects for randomization strata, and cluster standard errors at the neighborhood level. Regressions estimating effects on midline and property assessment outcomes include time period fixed effects described in Section IV and house type fixed effects. We do not include house type fixed effects for endline outcomes to maximize the analysis sample, as discussed in Section V. The number of observations varies across regressions due to (i) outcomes being drawn from different surveys, and (ii) nonresponse for specific survey questions. **Replication Notes:** Below standard errors, we report p-values in brackets calculated using the distribution of coefficients estimated from 1,000 corresponding regressions of the outcome variable on random permutations of treatment assignment.

Table 6R1 Local versus Central: Tax Visits: Randomization Inference

	(1)	(2)	(3)	(4)
	Visited Post Carto	Visits Post Carto	Visited Other Contact	Visits Other Contact
	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues
Local	-0.00901 (0.0258) [0.739]	0.0138 (0.0459) [0.773]	0.00819 (0.00673) [0.295]	0.0187 (0.0125) [0.215]
Month FE	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
Observations	18162	18151	3513	3513
Clusters	209	209	206	206
Mean	.417	.552	.025	.039
<p>Authors' Notes: This table reports estimates from equation (1), comparing the tax visits collectors made after registration in Local and Central (the excluded category). All regressions include fixed effects for house type, randomization strata, and time periods described in Section IV, and cluster standard errors at the neighborhood level. Columns 1 and 2 report differences in tax visits—after the registration visit—by the extensive and intensive margins, respectively. Columns 3 and 4 report differences in citizen-reported contact with collectors outside of the tax campaign by the intensive and extensive margins, respectively. We exclude property type fixed effects in online Appendix Table A17. We discuss these results in Section VA. Replication Notes: Below standard errors, we report p-values in brackets calculated using the distribution of coefficients estimated from 1,000 corresponding regressions of the outcome variable on random permutations of treatment assignment.</p>				

Table 7R1: Central versus CLI: Randomization Inference

	(1)	(2)	(3)	(4)	(5)	(6)
	Tax Compliance	Tax Revenues	Visited	No. of Visits	Tax Compliance	Tax Compliance
	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues
Central + chief Info	0.0242 (0.00914) [0.032]	46.57 (21.20) [0.083]	-0.0162 (0.0279) [0.622]	-0.0255 (0.0441) [0.619]	0.0264 (0.0140) [0.851]	0.0221 (0.00909) [0.024]
Local						0.0462 (0.00688)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	20636	20636	13884	13877	5283	33746
Clusters	165	165	163	163	161	267
Mean	.051	150.66	.387	.497	.097	.052
CLI vs Local						.007
<p>Authors' Notes: This table compares the CLI arm to the Central arm, which is the excluded category. Columns 1, 5, and 6 report effects on compliance. Column 2 reports effects on revenues. Columns 3 and 4 report differences in tax visits by collectors after registration by the extensive and intensive margins, respectively. All regressions include fixed effects for house type, randomization strata, and time periods and cluster standard errors at the neighborhood level. All specifications include time fixed effects defined to maximize overlap between the treatments under comparison, as discussed in Section IV. Column 5 restricts to the subsample of properties that received any tax visits after registration. Column 6 includes a dummy for the Local treatment. The bottom row reports the p-value from a test for equality between the CLI and Local. We discuss these results in Section VB. Replication Notes: Below standard errors, we report p-values in brackets calculated using the distribution of coefficients estimated from 1,000 corresponding regressions of the outcome variable on random permutations of treatment assignment.</p>						

Table 8R1: Values of Chiefs Information: Randomization Inference

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues
Ease of payment	0.0448	0.0561	0.0287	0.0442				
	(0.0125)	(0.00710)	(0.0137)	(0.00804)				
	[0.000]	[0.000]	[0.010]	[0.000]				
Predicted Ease of payment					0.0403	0.0428	0.00361	0.0274
					(0.0211)	(0.0125)	(0.0158)	(0.00929)
					[0.039]	[0.000]	[0.861]	[0.002]
Wall quality			0.0252	0.0216	0.0116	0.0150	0.0251	0.0118
			(0.0126)	(0.00714)	(0.0111)	(0.00693)	(0.0108)	(0.00471)
Roof quality			0.00506	-0.0000607	0.00607	0.000955	0.0180	-0.00991
			(0.00611)	(0.00179)	(0.00793)	(0.00441)	(0.00793)	(0.00632)
Erosion threat			0.0171	-0.00349	-0.00335	-0.0113	-0.00180	-0.00502
			(0.0108)	(0.00438)	(0.0120)	(0.00706)	(0.00999)	(0.00470)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5574	8135	4551	5150	4980	4994	4820	4826
Clusters	79	80	66	66	82	82	77	77
Mean	.376	.072	.352	.065	.449	.112	.413	.061

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues
Willingness	0.0342	0.0374	0.0329	0.0384				
	(0.0114)	(0.00731)	(0.0116)	(0.00780)				
	[0.000]	[0.000]	[0.000]	[0.000]				
Predicted Willingness to pay					0.0381	0.0324	0.0152	0.0260
					(0.0203)	(0.0116)	(0.0159)	(0.00845)
					[0.000]	[0.000]	[0.300]	[0.000]
Wall quality			0.0219	0.0213	0.0119	0.0155	0.0249	0.0121
			(0.0132)	(0.00863)	(0.0111)	(0.00691)	(0.0107)	(0.00475)
Roof quality			0.0107	0.000726	0.00615	0.000974	0.0179	-0.00977
			(0.00799)	(0.00226)	(0.00794)	(0.00439)	(0.00790)	(0.00621)
Erosion threat			0.0163	-0.00492	-0.00325	-0.0111	-0.00210	-0.00521
			(0.0117)	(0.00455)	(0.0120)	(0.00716)	(0.00997)	(0.00471)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3933	5521	3929	4461	4980	4994	4820	4826
Clusters	50	50	50	50	82	82	77	77
Mean	.357	.062	.357	.066	.449	.112	.413	.061

Authors' Notes: This table explores the extent to which chiefs' recommendations in CLI predict tax visits after registration and tax payment. Columns 1–4 show correlations in CLI between chiefs' recommendations and outcomes. Columns 5–8 report correlations between predicted propensity measures described in Section VB and outcomes in Local (columns 5 and 6) and Central (columns 7 and 8). Columns 1, 3, 5, and 7 show correlations between propensity and tax visits; columns 2, 4, 6, and 8 show correlations between propensity and compliance. All regressions include house type and randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 5–8 include controls for visible household characteristics. We show results excluding house fixed effects in online Appendix Table A21. We discuss these results in Section VB. **Replication Notes:** Below standard errors, we report p-values in brackets calculated using the distribution of coefficients estimated from 1,000 corresponding regressions of the outcome variable on random permutations of treatment assignment.

Tables Section 3.2

Table 4B: Local versus Central: Compliance and Revenues: Restricting the Sample to Baseline Respondents

	(1)	(2)	(3)	(4)
	Tax Compliance	Tax Compliance	Tax Compliance	Tax Compliance
Local	0.020	0.033**	0.034**	0.044**
	(0.016)	(0.014)	(0.014)	(0.018)
Month FE	No	Yes	Yes	Yes
House FE	No	No	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
Observations	2530	2439	2439	1878
Clusters	221	213	213	212
Mean	.088	.082	.082	.103

	(1)	(2)	(3)	(4)
	Tax Revenues	Tax Revenues	Tax Revenues	Tax Revenues
Local	44.846	92.149	86.269	119.750*
	(61.880)	(58.166)	(55.974)	(68.963)
Month FE	No	Yes	Yes	Yes
House FE	No	No	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
Observations	2530	2439	2439	1878
Clusters	221	213	213	212
Mean	275.851	252.785	252.785	310.011

Authors' Notes: This table reports estimates from equation (1), comparing property tax compliance in Local and Central (the excluded category). The two panels show estimates from separate regressions of compliance and revenues (in Congolese francs) on treatment, respectively. All regressions include fixed effects for randomization strata and cluster standard errors at the neighborhood level. Column 1 regressions do not include time period fixed effects described in Section IV while those in other columns include them. Regressions in columns 1–3 do not include house fixed effects. Column 3 shows results when the data are collapsed to the neighborhood level. We use robust standard errors and assign the minimum value for time period fixed effects to a neighborhood. Regressions in column 5 exclude exempt properties. The data include all properties registered by tax collectors merged with the government's property tax database. We discuss these results in Section IVA. **Replication Notes:** This table restricts the sample to only properties of baseline survey respondents. Not all residents participated in the baseline survey. We omit the specification in column (3) of Table 4, so that columns (3) and (4) correspond to columns (4) and (5) of Table 4.

Table 6B: Local versus Central: Tax Visits: Restricting the Sample to Baseline Respondents

	(1)	(2)	(3)	(4)
	Visited Post Carto	Visits Post Carto	Visited Other Contact	Visits Other Contact
Local	-0.054	-0.007	0.004	0.001
	(0.035)	(0.082)	(0.013)	(0.029)
Month FE	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
Observations	1813	1813	1105	1105
Clusters	208	208	188	188
Mean	.477	.65	.036	.064
<p>Authors' Notes: This table reports estimates from equation (1), comparing the tax visits collectors made after registration in Local and Central (the excluded category). All regressions include fixed effects for house type, randomization strata, and time periods described in Section IV, and cluster standard errors at the neighborhood level. Columns 1 and 2 report differences in tax visits—after the registration visit—by the extensive and intensive margins, respectively. Columns 3 and 4 report differences in citizen-reported contact with collectors outside of the tax campaign by the intensive and extensive margins, respectively. We exclude property type fixed effects in online Appendix Table A17. We discuss these results in Section VA. Replication Notes: This table restricts the sample to only properties of baseline survey respondents. Not all residents participated in the baseline survey.</p>				

Table 7B: Central versus CLI: Restricting the Sample to Baseline Respondents

	(1)	(2)	(3)	(4)	(5)	(6)
	Tax Compliance	Tax Revenues	Visited	No. of Visits	Tax Compliance	Tax Compliance
	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues	b/se/pvalues
Central + Chief Info	0.0293250 (0.0148) [0.104000]	39.03621 (33.85) [0.417000]	-0.0567658 (0.0389) [0.228000]	-0.0591125 (0.0688) [0.476000]	0.00132616 (0.0290) [0.992000]	0.0336676 (0.0146) [0.054000]
Local						0.0513096
						(0.0134)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1797	1797	1347	1347	594	2984
Clusters	163	163	161	161	152	265
Mean	.066	202.935	.45	.61	.112	.066
CLI vs Local						.022
<p>Authors' Notes: This table compares the CLI arm to the Central arm, which is the excluded category. Columns 1, 5, and 6 report effects on compliance. Column 2 reports effects on revenues. Columns 3 and 4 report differences in tax visits by collectors after registration by the extensive and intensive margins, respectively. All regressions include fixed effects for house type, randomization strata, and time periods and cluster standard errors at the neighborhood level. All specifications include time fixed effects defined to maximize overlap between the treatments under comparison, as discussed in Section IV. Column 5 restricts to the subsample of properties that received any tax visits after registration. Column 6 includes a dummy for the Local treatment. The bottom row reports the p-value from a test for equality between the CLI and Local. We discuss these results in Section VB. Replication Notes: This table restricts the sample to only properties of baseline survey respondents. Not all residents participated in the baseline survey.</p>						

Table 8B: The Value of Chiefs' Information: Restricted to the Baseline Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Ease of payment	0.0283541	0.0711433	0.00702058	0.0522117				
	(0.0342)	(0.0171)	(0.0369)	(0.0210)				
Predicted Ease of payment					0.0545056	-0.0190308	-0.0466741	-
					(0.0863)	(0.0513)	(0.0910)	0.00830370
								(0.0289)
Wall quality			0.0279269	0.0215279	0.0154988	0.0320104	-	0.0292822
			(0.0298)	(0.0123)	(0.0358)	(0.0258)	0.00809016	(0.0179)
							(0.0475)	
Roof quality			-	0.00133516	0.0292939	0.00462332	0.0656725	-
			0.00453369	(0.00347)	(0.0155)	(0.00500)	(0.0113)	0.00317143
			(0.0189)					(0.00235)
Erosion threat			0.0293743	-0.0273417	0.0397188	0.0218713	0.0596103	-0.0252513
			(0.0263)	(0.00849)	(0.0410)	(0.0308)	(0.0597)	(0.0171)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	585	757	447	488	296	296	280	281
Clusters	77	78	62	62	67	67	68	68
Mean	.433	.098	.404	.073	.449	.095	.511	.057

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
	b/se	b/se	b/se	b/se	b/se	b/se	b/se	b/se
Willingness	0.00887750	0.0615637	0.0104895	0.0519131				
	(0.0351)	(0.0195)	(0.0355)	(0.0193)				
Predicted Willingness to pay					0.0736877	-0.0529316	0.114637	0.0237012
					(0.106)	(0.0705)	(0.0800)	(0.0357)
Wall quality			0.00148085	0.0169365	0.0174777	0.0310563	-0.00379301	0.0300821
			(0.0306)	(0.0127)	(0.0358)	(0.0252)	(0.0479)	(0.0179)
Roof quality			0.0144685	0.00466046	0.0292559	0.00416590	0.0668476	-0.00295030
			(0.0201)	(0.00444)	(0.0153)	(0.00532)	(0.0113)	(0.00207)
Erosion threat			0.0140241	-0.0246636	0.0354333	0.0256338	0.0609685	-0.0250401
			(0.0290)	(0.00765)	(0.0411)	(0.0316)	(0.0608)	(0.0174)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	394	500	394	433	296	296	280	281
Clusters	50	50	50	50	67	67	68	68
Mean	.416	.077	.416	.077	.449	.095	.511	.057

Authors' Notes: This table explores the extent to which chiefs' recommendations in CLI predict tax visits after registration and tax payment. Columns 1–4 show correlations in CLI between chiefs' recommendations and outcomes. Columns 5–8 report correlations between predicted propensity measures described in Section VB and outcomes in Local (columns 5 and 6) and Central (columns 7 and 8). Columns 1, 3, 5, and 7 show correlations between propensity and tax visits; columns 2, 4, 6, and 8 show correlations between propensity and compliance. All regressions include house type and randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 5–8 include controls for visible household characteristics. We show results excluding house fixed effects in online Appendix Table A21. We discuss these results in Section VB. **Replication Notes:** This table restricts the sample to only properties of baseline survey respondents. Not all residents participated in the baseline survey.

Table 4R2: Local versus Central: Compliance and Revenues: Controlling for Trust in chief.

	(1)	(2)	(3)	(4)
	Tax Compliance	Tax Compliance	Tax Compliance	Tax Compliance
Local	0.020	0.033**	0.034**	0.044**
	(0.207)	(0.021)	(0.019)	(0.012)
Not very much confidence	-0.016	-0.020	-0.018	-0.022
	(0.515)	(0.432)	(0.480)	(0.497)
Some confidence	0.012	0.007	0.007	0.007
	(0.556)	(0.706)	(0.723)	(0.790)
A lot of confidence	-0.015	-0.023	-0.024	-0.030
	(0.369)	(0.174)	(0.165)	(0.160)
Month FE	No	Yes	Yes	Yes
House FE	No	No	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
Observations	2517	2426	2426	1868
Clusters	221	213	213	212
Mean	.088	.082	.082	.103
<p>Authors' Notes: This table reports estimates from equation (1), comparing property tax compliance in Local and Central (the excluded category). The two panels show estimates from separate regressions of compliance and revenues (in Congolese francs) on treatment, respectively. All regressions include fixed effects for randomization strata and cluster standard errors at the neighborhood level. Column 1 regressions do not include time period fixed effects described in Section IV while those in other columns include them. Regressions in columns 1–3 do not include house fixed effects. Column 3 shows results when the data are collapsed to the neighborhood level. We use robust standard errors and assign the minimum value for time period fixed effects to a neighborhood. Regressions in column 5 exclude exempt properties. The data include all properties registered by tax collectors merged with the government's property tax database. We discuss these results in Section IVA. Replication Notes: Each regression includes as an additional control variable, trust in chief. We omit the specification in column (3) of Table 4, so that columns (3) and (4) correspond to columns (4) and (5) of Table 4.</p>				

Table 6R2: Local versus Central: Tax Visits: Controlling for Trust in chief.

	(1)	(2)	(3)	(4)
	Visited Post Carto	Visits Post Carto	Visited Other Contact	Visits Other Contact
Local	-0.056	-0.010	0.003	0.000
	(0.034)	(0.080)	(0.013)	(0.029)
Not very much confidence	0.037	0.009	-0.031	-0.010
	(0.055)	(0.100)	(0.025)	(0.049)
Some confidence	-0.020	-0.062	-0.028	-0.023
	(0.039)	(0.072)	(0.018)	(0.031)
A lot of confidence	0.038	0.052	-0.012	-0.008
	(0.040)	(0.088)	(0.021)	(0.037)
Month FE	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes
Observations	1807	1807	1102	1102
Clusters	208	208	188	188
Mean	.477	.65	.036	.064
<p>Authors' Notes: This table reports estimates from equation (1), comparing the tax visits collectors made after registration in Local and Central (the excluded category). All regressions include fixed effects for house type, randomization strata, and time periods described in Section IV, and cluster standard errors at the neighborhood level. Columns 1 and 2 report differences in tax visits—after the registration visit—by the extensive and intensive margins, respectively. Columns 3 and 4 report differences in citizen-reported contact with collectors outside of the tax campaign by the intensive and extensive margins, respectively. We exclude property type fixed effects in online Appendix Table A17. We discuss these results in Section VA. Replication Notes: Each regression includes as an additional control variable, Trust in chief.</p>				

Table 7R2: Central versus CLI: Controlling for Trust in chief.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Tax Compliance	Tax Revenues	Visited	No. of Visits	Tax Compliance	Tax Compliance	Tax Revenues
Central Plus Local Info	0.029*	39.844	-0.061	-0.063	0.005	0.035**	50.228
	(0.015)	(34.943)	(0.039)	(0.069)	(0.030)	(0.015)	(41.740)
Local						0.052***	141.085**
						(0.013)	(53.612)
Not very much confidence	0.007	90.243	-0.032	-0.023	0.091	-0.012	-34.970
	(0.029)	(127.200)	(0.064)	(0.113)	(0.073)	(0.024)	(100.461)
Some confidence	0.032*	58.213	0.037	0.040	0.063	0.008	-11.333
	(0.019)	(55.981)	(0.047)	(0.085)	(0.044)	(0.016)	(49.419)
A lot of confidence	0.004	30.414	-0.021	-0.045	0.040	-0.019	-60.381
	(0.018)	(55.466)	(0.045)	(0.080)	(0.039)	(0.015)	(51.165)
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1791	1791	1344	1344	591	2973	2973
Clusters	163	163	161	161	151	265	265
Mean	.066	202.935	.45	.61	.112	.066	.066
CLI vs Local						.259	.071

Authors' Notes: This table compares the CLI arm to the Central arm, which is the excluded category. Columns 1, 5, and 6 report effects on compliance. Column 2 reports effects on revenues. Columns 3 and 4 report differences in tax visits by collectors after registration by the extensive and intensive margins, respectively. All regressions include fixed effects for house type, randomization strata, and time periods and cluster standard errors at the neighborhood level. All specifications include time-fixed effects defined to maximize overlap between the treatments under comparison, as discussed in Section IV. Column 5 restricts to the subsample of properties that received any tax visits after registration. Column 6 includes a dummy for the Local treatment. The bottom row reports the p-value from a test for equality between the CLI and Local. We discuss these results in Section VB. **Replication Notes:** Each regression includes as an additional control variable, Trust in chief.

Table 8R2: The Value of chiefs' Information: Controlling for Trust in chief.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
Ease of payment	0.037	0.072***	0.016	0.053**				
	(0.033)	(0.017)	(0.036)	(0.021)				
Predicted Ease of payment					-0.141*	0.068*	0.034	0.031
					(0.072)	(0.038)	(0.075)	(0.043)
Not very much confidence	-0.164	-0.007	-0.228*	-0.029	0.042	-0.143*	0.125	0.081
	(0.111)	(0.059)	(0.126)	(0.043)	(0.127)	(0.079)	(0.169)	(0.097)
Some confidence	0.008	0.012	0.015	0.048	-0.063	-0.112*	0.000	0.040
	(0.070)	(0.038)	(0.080)	(0.036)	(0.082)	(0.062)	(0.103)	(0.039)
A lot of confidence	-0.131**	-0.008	-0.134*	0.048	0.014	-0.096*	0.014	0.007
	(0.065)	(0.035)	(0.077)	(0.029)	(0.087)	(0.056)	(0.099)	(0.028)
Wall quality, Roof quality, Erosion threat	No	No	Yes	Yes	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	583	755	446	487	294	294	280	281
Clusters	77	78	62	62	66	66	68	68
Mean	.433	.098	.404	.073	.449	.095	.511	.057

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
Willingness	0.016	0.063**	0.018	0.054**				
	(0.035)	(0.020)	(0.036)	(0.020)				
Predicted Willingness to pay					-0.084	0.057	-0.050	0.027
					(0.083)	(0.043)	(0.078)	(0.036)
Not very much confidence	-0.193	-0.030	-0.201	-0.032	0.032	-0.139*	0.121	0.077
	(0.143)	(0.045)	(0.144)	(0.043)	(0.127)	(0.080)	(0.168)	(0.099)
Some confidence	0.076	0.018	0.073	0.045	-0.065	-0.112*	-0.014	0.038
	(0.082)	(0.036)	(0.081)	(0.037)	(0.085)	(0.061)	(0.103)	(0.038)
A lot of confidence	-0.023	0.047	-0.024	0.067**	0.010	-0.093	-0.002	0.005
	(0.079)	(0.032)	(0.078)	(0.032)	(0.089)	(0.056)	(0.101)	(0.029)
Wall quality, Roof quality, Erosion threat	No	No	Yes	Yes	Yes	Yes	Yes	Yes
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	393	499	393	432	294	294	280	281
Clusters	50	50	50	50	66	66	68	68
Mean	.416	.077	.416	.077	.449	.095	.511	.057

Authors' Notes: This table explores the extent to which chiefs' recommendations in CLI predict tax visits after registration and tax payment. Columns 1–4 show correlations in CLI between chiefs' recommendations and outcomes. Columns 5–8 report correlations between predicted propensity measures described in Section VB and outcomes in Local (columns 5 and 6) and Central (columns 7 and 8). Columns 1, 3, 5, and 7 show correlations between propensity and tax visits; columns 2, 4, 6, and 8 show correlations between propensity and compliance. All regressions include house type and randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 5–8 include controls for visible household characteristics. We show results excluding house fixed effects in online Appendix Table A21. We discuss these results in Section VB. **Replication Notes:** Each regression includes as an additional control variable, Trust in chief.

Tables Section 3.3

TABLE 8P: PREDICTION PERFORMANCE

No	Outcome and Approach	Accuracy	MSE	MAE
Against The True Values of Ease of payment and Willingness to pay				
01	Predicted Ease – BBTW Table 8	57.056	0.495	0.451
02	Predicted Ease – Table 8R3i	57.568	0.468	0.439
03	Predicted Ease – Table 8R3ii	56.928	0.474	0.445
04	Predicted Willingness – BBTW Table 8	53.150	0.559	0.499
05	Predicted Willingness – Table 8R3i	52.930	0.565	0.502
06	Predicted Willingness – Table 8R3ii	54.286	0.541	0.485
Against The Predicted Values of Ease of payment and Willingness to pay in BBTW Table 8				
07	Predicted Ease - Table 8R3i	90.916	0.091	0.091
08	Predicted Ease - Table 8R3ii	83.663	0.163	0.163
09	Predicted Willingness - Table 8R3i	96.850	0.032	0.032
10	Predicted Willingness - Table 8R3ii	85.751	0.142	0.142
<p><i>Notes: This table reports the performance metrics of the predictions from ordered Probit to the one from a linear regression and arbitrary thresholds. The first panel compares all the predictions to the true value of Ease of payment and Willingness to pay. The second panel compares the ordered Probit predictions to the prediction from the linear regression and arbitrary thresholds.</i></p>				

Table 8R3i: The Value of chiefs' Information: Ordered Probit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
Ease of payment	0.045***	0.056***	0.029**	0.044***				
	(0.012)	(0.007)	(0.014)	(0.008)				
Predicted Ease of payment					0.057**	0.046**	0.006	0.045***
					(0.023)	(0.014)	(0.016)	(0.008)
Wall quality			0.025**	0.021**	0.011	0.015**	0.025**	0.011**
			(0.012)	(0.007)	(0.011)	(0.007)	(0.011)	(0.005)
Roof quality			0.005	-0.000	0.006	0.001	0.018**	-0.010
			(0.006)	(0.002)	(0.008)	(0.004)	(0.008)	(0.006)
Erosion threat			0.017	-0.004	-0.003	-0.011	-0.002	-0.005
			(0.011)	(0.004)	(0.012)	(0.007)	(0.010)	(0.005)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5574	8135	4551	5150	4980	4994	4820	4826
Clusters	79	80	66	66	82	82	77	77
Mean	.376	.072	.352	.065	.449	.112	.413	.061

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
Willingness	0.034**	0.037***	0.033**	0.038***				
	(0.011)	(0.007)	(0.012)	(0.008)				
Predicted Willingness to pay					0.042**	0.033**	0.005	0.016*
					(0.020)	(0.012)	(0.017)	(0.008)
Wall quality			0.022	0.021**	0.012	0.015**	0.025**	0.012**
			(0.013)	(0.009)	(0.011)	(0.007)	(0.011)	(0.005)
Roof quality			0.011	0.001	0.006	0.001	0.018**	-0.010
			(0.008)	(0.002)	(0.008)	(0.004)	(0.008)	(0.006)
Erosion threat			0.016	-0.005	-0.003	-0.011	-0.002	-0.005
			(0.012)	(0.005)	(0.012)	(0.007)	(0.010)	(0.005)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3933	5521	3929	4461	4980	4994	4820	4826
Clusters	50	50	50	50	82	82	77	77
Mean	.357	.062	.357	.066	.449	.112	.413	.061

Authors' Notes: This table explores the extent to which chiefs' recommendations in CLI predict tax visits after registration and tax payment. Columns 1–4 show correlations in CLI between chiefs' recommendations and outcomes. Columns 5–8 report correlations between predicted propensity measures described in Section VB and outcomes in Local (columns 5 and 6) and Central (columns 7 and 8). Columns 1, 3, 5, and 7 show correlations between propensity and tax visits; columns 2, 4, 6, and 8 show correlations between propensity and compliance. All regressions include house type and randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 5–8 include controls for visible household characteristics. We show results excluding house fixed effects in online Appendix Table A21. We discuss these results in Section VB. **Replication Notes:** The first four columns are the same as columns (1)–(4) of BBTW Table 8. We include them here for the reader's convenience. In columns (5)–(8), Predicted Ease of Payment and Willingness to Pay are generated using an ordered Probit model with the same set of predictors (unreported) as for Predicted Ease of Payment and Willingness to Pay in BBTW Table 8.

Table 8R3ii: The Value of chiefs' Information: Ordered Probit + Controlling for chiefs' Characteristics.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
Ease of payment	0.045***	0.056***	0.029**	0.044***				
	(0.012)	(0.007)	(0.014)	(0.008)				
Predicted Ease of payment					0.009	0.023*	0.022	0.002
					(0.022)	(0.012)	(0.026)	(0.010)
Wall quality			0.025**	0.021**	0.011	0.015**	0.022**	0.012**
			(0.012)	(0.007)	(0.011)	(0.007)	(0.011)	(0.005)
Roof quality			0.005	-0.000	0.006	0.001	0.018**	-0.010
			(0.006)	(0.002)	(0.008)	(0.004)	(0.008)	(0.006)
Erosion threat			0.017	-0.004	-0.004	-0.011	-0.001	-0.005
			(0.011)	(0.004)	(0.012)	(0.007)	(0.010)	(0.005)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5572	8133	4551	5150	4908	4922	4725	4731
Clusters	79	80	66	66	80	80	76	76
Mean	.376	.072	.352	.065	.448	.112	.41	.059

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance	Visited Post Carto	Tax Compliance
Willingness	0.034**	0.037***	0.033**	0.038***				
	(0.011)	(0.007)	(0.012)	(0.008)				
Predicted Willingness to pay					-0.010	0.012	-0.019	0.025**
					(0.028)	(0.009)	(0.033)	(0.009)
Wall quality			0.022	0.021**	0.011	0.016**	0.023**	0.012**
			(0.013)	(0.009)	(0.011)	(0.007)	(0.011)	(0.005)
Roof quality			0.011	0.001	0.006	0.001	0.018**	-0.009
			(0.008)	(0.002)	(0.008)	(0.004)	(0.008)	(0.006)
Erosion threat			0.016	-0.005	-0.004	-0.011	-0.001	-0.005
			(0.012)	(0.005)	(0.012)	(0.007)	(0.010)	(0.005)
House FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stratum FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3933	5521	3929	4461	4908	4922	4725	4731
Clusters	50	50	50	50	80	80	76	76
Mean	.357	.062	.357	.066	.448	.112	.41	.059

Authors' Notes: This table explores the extent to which chiefs' recommendations in CLI predict tax visits after registration and tax payment. Columns 1–4 show correlations in CLI between chiefs' recommendations and outcomes. Columns 5–8 report correlations between predicted propensity measures described in Section VB and outcomes in Local (columns 5 and 6) and Central (columns 7 and 8). Columns 1, 3, 5, and 7 show correlations between propensity and tax visits; columns 2, 4, 6, and 8 show correlations between propensity and compliance. All regressions include house type and randomization stratum fixed effects and cluster standard errors at the neighborhood level. Columns 3, 4, and 5–8 include controls for visible household characteristics. We show results excluding house fixed effects in online Appendix Table A21. We discuss these results in Section VB. **Replication Notes:** The first four columns are the same as columns (1)–(4) of BBTW Table 8. We include them here for the reader's convenience. In columns (5)–(8), Predicted Ease of Payment and Willingness to Pay are generated using an ordered Probit model with an alternative set of predictors for Predicted Ease of Payment and Willingness to Pay. Instead of the five predictors used to predict the generated regressors in BBTW Table 8, we use the five predictors (Respondent's sex, Respondent is employed with a salary, a dummy for the respondent's neighborhood's chief being older than the median chief, a dummy for the the respondent's neighborhood being above-median in remoteness, a dummy for the respondent's neighborhood's chief having above-median trust in the government) plus ethnic group fixed effects for Ease of Payment and five predictors (Respondent's sex, Respondent is employed with a salary, A dummy if the family member of respondent's neighborhood's chief previously served as chief in the neighborhood, a dummy for respondent's neighborhood's chief having above-median view of government's responsiveness to needs of neighborhood, respondent's neighborhood's treatment status during the 2016 tax campaign from Weigel (2020)) plus ethnic group fixed effect for Willingness to Pay.