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**Working Paper**

## Digitalization and the politics of tax in low-income states

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# Digitalisation and the politics of raising tax in low-capacity states

Supplementary materials

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## **A Data collection**

The experiment was included in an original face-to-face survey in Malawi in October 2024, working with the Institute for Public Opinion Research (IPOR). The survey focused on two of the country's 27 districts, Blantyre and Salima, selected to ensure broad variation in partisanship and ethnicity.

Sampling followed a multi-step randomisation process as follows:

1. Within each district, randomly select census-defined enumeration areas (communities) from each ward, weighted by the overall 2023 census-projected population of the ward. In total, 69 EAs were chosen in each district.
2. Within each enumeration area, select households with a random walk protocol. Specifically, a 10-interval pattern was used, meaning every 10th household was selected while walking in a designated direction from the starting point. In each selected EA, on average at least 8 interviews were conducted.
3. Within each household, take a roster of individuals and randomly select who to interview. This alternates between male and female to ensure gender representation in the sample.

## B Research design

### B.1 Intervention

The conjoint experiment was embedded within a face-to-face survey, in which enumerators read information to respondents as part of the broader questionnaire.

To begin, respondents were read the following preamble, which introduced the topic of mobile money taxes in Malawi. The phrases in bold were highlighted by enumerators, since these represent the attributes that are randomised in the experiment.

In Malawi, many people use mobile payment platforms, like Airtel money or TNM Mpamba, to send and receive funds. You have to pay transaction fees to send money, and cash-out fees to withdraw it from an agent. Most of the money collected in fees goes to mobile operators, like Airtel or TNM.

To fund public spending, many governments across Africa collect taxes on mobile money payments. There are many ways governments might do this, and we are interested in what kind of tax policies you would prefer.

First, the government decides the **tax rate**. This is the amount of tax you pay when you make a payment. It is usually between 1% to 5%. For example, if you send 1,000 kwacha to your relative, the government will receive between 10 to 50 kwacha in tax, on top of the fees you pay the operator.

Second, the government decides the **exemptions**. Sometimes you can send a certain amount of money per day, often around 5,000 kwacha, before you pay any mobile money taxes. In addition, some types of payments might never be taxed, like those to registered businesses.

Third, the government decides **how tax revenues are spent**. Sometimes taxes go into the overall national budget, while other times they are reserved in a special fund for a particular purpose, like investing in healthcare facilities in your district.

Fourth, the government can select a **monitor** to make sure tax money is spent properly. In Malawi, this could be the Ministry of Finance, the Anti-Corruption Bureau, or an international organisation like USAID.

Lastly, the government chooses **how to collect taxes**. Sometimes your mobile operator collects the tax and will pass it on to the government for you. Other times, the money gets sent directly to the government from your account.

In the questions that follow we will present you with pairs of different tax policies that the government of Malawi could implement. We will then ask you to choose the policy that you would prefer. You don't need to explain your reasoning, just tell us which one you pick.

Your choices are anonymous, and we will combine them with those of more than a thousand other Malawians taking part in our survey. The overall results will be made public, so that organisations across Malawi, including the government, can use them to inform future decisions.

Respondents were then be presented with pairs of policies, and asked the following outcome question:

*Suppose the government was going to introduce one of the below mobile money tax policies. Which would you prefer they implemented?*

The intervention contained two randomisations. First, the attribute levels presented to respondents were fully randomised at the *task* level, since there are no combinations which are logically inconsistent with one another. Second, the order in which attributes are presented was randomised at the *respondent* level, so that I can test for row-order effects. This means that row-ordering will stay constant across the four choices put to a given individual respondent, but that the order will randomly vary *between* them and other respondents.

## **B.2 Ethical considerations**

The experiment received institutional ethics approval from both the author's home university, and an independent IRB in Malawi.

All participants were required to provide informed consent before commencing the survey, and were given contact details for IPOR and the independent IRB should they have questions about the study after taking part. Moreover, the survey data used for the analysis is fully anonymised, as respondents' names, addresses and identifiable information was deliberately not recorded. The conjoint intervention stresses the hypothetical nature of proposed taxes, while suggesting realistic scenarios based on conversations with policymakers across Malawi.

## C Main results and robustness

### C.1 Balance

Table SM1 presents covariate balance tests, which model the probability of a respondent seeing a given level of an attribute, relative to its baseline. As can be seen, there is broad demographic balance for each attribute level, which suggests randomisation was successfully implemented.

**Table SM1:** Balance tests

Attribute	Rate	Earmarking	Payment	Monitoring		Exemptions	
Dependent Variables:	Lower	Yes	Direct	ACB	Int org	5000 daily	Reg. business
Model:	(vs upper)	(vs no)	(vs operator)	(vs none)	(vs none)	(vs none)	(vs none)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>Variables</i>							
Age	$7.15 \times 10^{-5}$ ( $7.91 \times 10^{-5}$ )	$3.09 \times 10^{-5}$ ( $9.91 \times 10^{-5}$ )	$6.77 \times 10^{-5}$ ( $5.18 \times 10^{-5}$ )	0.0001 ( $9.94 \times 10^{-5}$ )	0.0001** ( $6.88 \times 10^{-5}$ )	$5.34 \times 10^{-5}$ ( $5.56 \times 10^{-5}$ )	$4.66 \times 10^{-5}$ ( $5.26 \times 10^{-5}$ )
Male	0.003 (0.010)	-0.003 (0.010)	-0.013 (0.010)	0.014 (0.012)	-0.003 (0.012)	0.008 (0.012)	0.020* (0.012)
Education = Primary	-0.009 (0.017)	-0.018 (0.018)	0.028 (0.018)	-0.013 (0.021)	-0.004 (0.023)	-0.026 (0.022)	-0.004 (0.024)
Education = Secondary	-0.031* (0.018)	-0.017 (0.019)	0.027 (0.019)	-0.030 (0.023)	-0.019 (0.025)	-0.032 (0.024)	-0.014 (0.026)
Education = Uni/vocation	0.025 (0.031)	0.014 (0.030)	0.032 (0.030)	-0.014 (0.034)	0.0009 (0.037)	-0.037 (0.043)	0.045 (0.039)
Ethnicity = DK/none/refuse	0.057 (0.044)	-0.080 (0.076)	-0.064** (0.030)	0.076 (0.098)	-0.019 (0.060)	-0.163 (0.106)	0.077 (0.106)
Ethnicity = Lomwe	0.006 (0.017)	-0.019 (0.018)	-0.037** (0.018)	-0.018 (0.021)	0.007 (0.021)	-0.012 (0.022)	-0.017 (0.021)
Ethnicity = Other	0.003 (0.013)	-0.009 (0.014)	-0.019 (0.014)	-0.026 (0.016)	-0.003 (0.016)	0.005 (0.017)	-0.0007 (0.017)
Wealth basket	0.007 (0.007)	-0.0008 (0.007)	-0.001 (0.007)	0.007 (0.008)	0.004 (0.008)	0.010 (0.008)	$-3.05 \times 10^{-5}$ (0.008)
District = Salima	0.023* (0.013)	-0.008 (0.014)	-0.020 (0.014)	-0.007 (0.017)	-0.003 (0.016)	-0.024 (0.017)	-0.020 (0.017)
<i>Fit statistics</i>							
Observations	9,936	9,936	9,936	6,610	6,610	6,638	6,586
R <sup>2</sup>	0.002	0.0005	0.0009	0.002	0.0008	0.002	0.001
F-test	0.188	0.061	0.115	0.197	0.098	0.187	0.174

Clustered (id) standard-errors in parentheses

Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Source: Analysis of author-collected survey data

## C.2 Overall results

Table SM2 below presents the overall results of the conjoint. Column (1) presents the baseline specification shown in the main text, column (3) introduces the same demographic covariates used in the balance checks, and (2) and (4) add district fixed effects to these respectively. As can be seen, the substantive and statistical significance of the AMCEs are highly consistent across models.

**Table SM2:** Overall results of the conjoint

Attribute	Baseline		Covariates	
Dependent Variable: Model:	(1)	Pr (choose policy) (2)	(3)	(4)
<i>Variables</i>				
Lower (vs upper)	0.077*** (0.011)	0.077*** (0.011)	0.077*** (0.011)	0.077*** (0.011)
5000 daily (vs none)	0.039*** (0.013)	0.039*** (0.013)	0.040*** (0.013)	0.040*** (0.013)
Reg. business (vs none)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)
Earmarking (vs no)	0.122*** (0.011)	0.122*** (0.011)	0.122*** (0.011)	0.122*** (0.011)
Monitoring by ACB (vs none)	0.018 (0.012)	0.017 (0.012)	0.018 (0.012)	0.018 (0.012)
Monitoring by int org (vs none)	-0.031** (0.013)	-0.031** (0.013)	-0.031** (0.013)	-0.031** (0.013)
Pay direct to govt (vs operator)	0.010 (0.011)	0.010 (0.011)	0.009 (0.011)	0.009 (0.011)
<i>Fixed-effects</i>				
District		Yes		Yes
<i>Fit statistics</i>				
Observations	9,944	9,944	9,936	9,936
R <sup>2</sup>	0.024	0.024	0.024	0.024

Clustered (id) standard-errors in parentheses

Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Note: Columns 1–2 present baseline results from the conjoint; columns 3–4 include controls for age, gender, education, ethnicity, and a wealth basket. Columns 2 and 4 include district fixed effects. SEs clustered by respondent throughout. Source: Analysis of author-collected survey data.

## **C.3 Diagnostics**

### **C.3.1 Column-order effects**

I first consider column order effects, seeing whether results differ when the attribute level was on the left or right-hand side of the contrast presented to respondents. Since respondents were read the contrast by the enumerator, this likely represents the order in which they received the information.

The results are in table [SM3](#). The effect of the rate and earmarking attributes are consistent across the board, while other attributes are less consistent on the right hand side. Looking at the 5000 daily exemption, registered business, and USAID monitoring, we see on the right hand side similarly signed effects that fail to reach statistical significance, suggesting that this may stem from the reduced power of the subsetting sample. Nonetheless, the fact that this only occurs on the right hand side suggests a slight bias in how respondents interpret the information provided, suggesting the level they receive first might have a disproportionate weight on their overall policy choice.

**Table SM3:** Testing column-order effects

Task order	Left-hand column				Right-hand column			
Specification	Baseline		Covariates		Baseline		Covariates	
Dependent Variable:			Pr (choose policy)					
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Variables</i>								
Lower (vs upper)	0.091*** (0.015)	0.092*** (0.015)	0.090*** (0.015)	0.090*** (0.015)	0.065*** (0.014)	0.065*** (0.014)	0.064*** (0.014)	0.063*** (0.014)
5000 daily (vs none)	0.049*** (0.018)	0.050*** (0.018)	0.050*** (0.018)	0.050*** (0.018)	0.028 (0.018)	0.028 (0.018)	0.030* (0.018)	0.029* (0.018)
Reg. business (vs none)	0.039** (0.018)	0.040** (0.018)	0.040** (0.018)	0.040** (0.018)	0.014 (0.017)	0.014 (0.017)	0.014 (0.017)	0.013 (0.017)
Earmarking (vs no)	0.128*** (0.015)	0.128*** (0.015)	0.129*** (0.015)	0.129*** (0.015)	0.115*** (0.015)	0.115*** (0.015)	0.115*** (0.015)	0.114*** (0.015)
Monitoring by ACB (vs none)	0.008 (0.017)	0.008 (0.017)	0.010 (0.017)	0.010 (0.017)	0.025 (0.018)	0.025 (0.018)	0.026 (0.018)	0.026 (0.018)
Monitoring by int org (vs none)	-0.043** (0.017)	-0.044** (0.017)	-0.042** (0.017)	-0.042** (0.017)	-0.021 (0.018)	-0.021 (0.018)	-0.021 (0.018)	-0.021 (0.018)
Pay direct to govt (vs operator)	-0.0007 (0.014)	-0.0009 (0.014)	-0.0009 (0.014)	-0.0007 (0.014)	0.021 (0.014)	0.021 (0.014)	0.019 (0.014)	0.019 (0.014)
<i>Fixed-effects</i>								
District	Yes		Yes		Yes		Yes	
<i>Fit statistics</i>								
Observations	4,972	4,972	4,968	4,968	4,972	4,972	4,968	4,968
R <sup>2</sup>	0.029	0.030	0.033	0.034	0.020	0.020	0.024	0.024

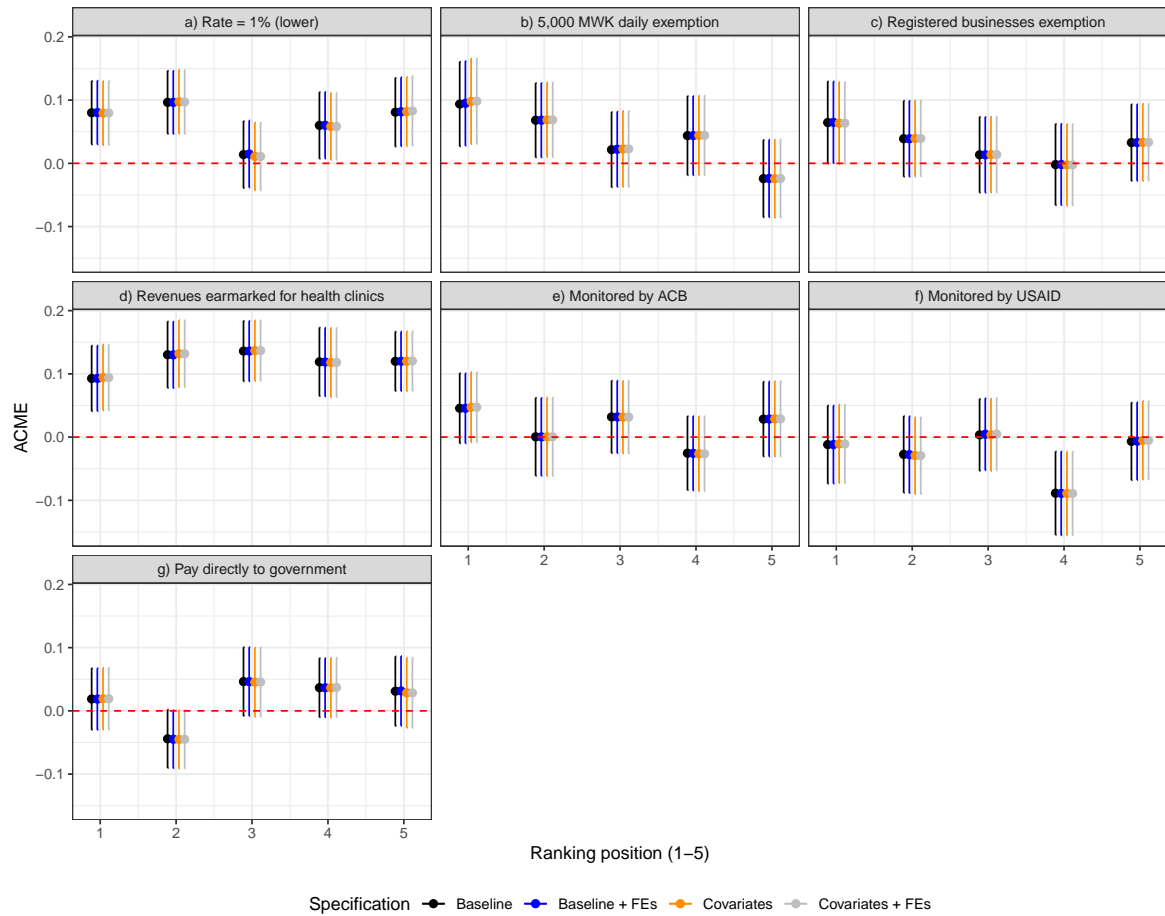
Clustered (id) standard-errors in parentheses  
Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Note: Tables represent ACME estimates from specifications run individually on each subset of the data, broken by attribute levels presented on the left or right hand side of the data. SEs clustered by respondent throughout. Source: Analysis of author-collected survey data.

### **C.3.2 Row-order effects**

I next turn to row-order effects. Here, I separate responses into subgroups based on the row-order in which a given attribute level appeared (which, to recall, was randomised between respondents). Since this leads to many estimates, I present the results visually in figure [SM1](#).

The results show a similar pattern to the column-based subsets. Earmarking and the lower tax rate have consistent positive effects across the board (with the exception of rate when placed in the third row). Here we can also see that exemption attributes have generally positive and similarly sized effects, though these sometimes lose statistical significance. For monitoring the story is more complicated, with overall negative USAID effects appearing to derive from contrasts where the monitoring attribute is presented fourth. There is also some variation on tax collection, with respondents showing a preference for paying directly to the government when the attribute is ranked lower down the list of items (3rd, 4th, or 5th). By contrast, the null overall effect appears driven by a negative ACME when presented second.



**Figure SM1:** Row-order effects. Source: Analysis of author-collected survey data.

### C.3.3 Task-order effects

Finally, I turn to the task-order, i.e. the order in which respondents choose between policies. Once again, there is a consistent preference across tasks for a lower tax rate and for revenues to be earmarked for local health clinics. And again, effects on exemptions are substantively similar and positively signed across the board though not always significant, something likely due to reduced sample size. Monitoring provides more nuanced outcomes once more, with negative effects of USAID deriving from the second and third task. The mode of payment collection is null across the board.

**Table SM4:** Testing task-order effects (tasks 1 and 2)

Task order	Task 1				Task 2			
Specification	Baseline		Covariates		Baseline		Covariates	
Dependent Variable: Model:	(1)	(2)	(3)	Pr (choose policy) (4)	(5)	(6)	(7)	(8)
<i>Variables</i>								
Lower (vs upper)	0.108*** (0.020)	0.109*** (0.020)	0.108*** (0.020)	0.108*** (0.020)	0.088*** (0.020)	0.089*** (0.020)	0.088*** (0.020)	0.088*** (0.020)
5000 daily (vs none)	0.030 (0.024)	0.031 (0.024)	0.031 (0.025)	0.031 (0.025)	0.051*** (0.025)	0.051*** (0.025)	0.053*** (0.025)	0.053*** (0.025)
Reg. business (vs none)	0.025 (0.024)	0.026 (0.025)	0.027 (0.025)	0.027 (0.025)	0.042 (0.025)	0.041 (0.025)	0.043* (0.025)	0.043* (0.026)
Earmarking (vs no)	0.119*** (0.019)	0.120*** (0.019)	0.121*** (0.019)	0.121*** (0.019)	0.131*** (0.020)	0.131*** (0.020)	0.131*** (0.020)	0.131*** (0.020)
Monitoring by ACB (vs none)	0.051*** (0.025)	0.051*** (0.025)	0.052*** (0.025)	0.052*** (0.025)	0.011 (0.024)	0.011 (0.024)	0.011 (0.024)	0.011 (0.025)
Monitoring by int org (vs none)	-0.015 (0.024)	-0.016 (0.024)	-0.016 (0.025)	-0.016 (0.025)	-0.036 (0.024)	-0.037 (0.024)	-0.037 (0.024)	-0.037 (0.024)
Pay direct to govt (vs operator)	-0.001 (0.020)	-0.0010 (0.020)	-0.002 (0.020)	-0.002 (0.020)	0.022 (0.020)	0.022 (0.020)	0.022 (0.020)	0.022 (0.020)
<i>Fixed-effects</i>								
District	Yes		Yes		Yes		Yes	
<i>Fit statistics</i>								
Observations	2,486	2,486	2,484	2,484	2,486	2,486	2,484	2,484
R <sup>2</sup>	0.029	0.029	0.030	0.030	0.029	0.029	0.030	0.030

Clustered (id) standard-errors in parentheses  
Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Note: Tables represent ACME estimates from specifications run individually on each subset of the data, broken by task. Columns 1-4 present the first task, columns 5-8 the second. SEs clustered by respondent throughout. Source: Analysis of author-collected survey data.

**Table SM5:** Testing task-order effects (tasks 3 and 4)

Task order	Task 3				Task 4			
Specification	Baseline		Covariates		Baseline		Covariates	
Dependent Variable:			Pr (choose policy)					
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Variables</i>								
Lower (vs upper)	0.040** (0.020)	0.040** (0.020)	0.039** (0.020)	0.039** (0.020)	0.072*** (0.020)	0.072*** (0.020)	0.071*** (0.020)	0.071*** (0.020)
5000 daily (vs none)	0.059** (0.024)	0.059** (0.024)	0.060** (0.025)	0.060** (0.025)	0.019 (0.025)	0.019 (0.025)	0.018 (0.025)	0.019 (0.025)
Reg. business (vs none)	0.035 (0.024)	0.034 (0.024)	0.034 (0.024)	0.034 (0.024)	0.008 (0.025)	0.009 (0.025)	0.008 (0.025)	0.009 (0.025)
Earmarking (vs no)	0.115*** (0.020)	0.115*** (0.020)	0.115*** (0.020)	0.115*** (0.020)	0.123*** (0.020)	0.122*** (0.020)	0.124*** (0.020)	0.124*** (0.020)
Monitoring by ACB (vs none)	-0.019 (0.025)	-0.019 (0.025)	-0.018 (0.025)	-0.017 (0.025)	0.026 (0.024)	0.026 (0.024)	0.027 (0.024)	0.027 (0.024)
Monitoring by int org (vs none)	-0.072*** (0.025)	-0.071*** (0.025)	-0.070*** (0.025)	-0.070*** (0.025)	-0.003 (0.024)	-0.003 (0.024)	-0.002 (0.024)	-0.002 (0.024)
Pay direct to govt (vs operator)	0.008 (0.020)	0.009 (0.020)	0.007 (0.020)	0.007 (0.020)	0.009 (0.020)	0.009 (0.020)	0.009 (0.020)	0.009 (0.020)
<i>Fixed-effects</i>								
District	Yes		Yes		Yes		Yes	
<i>Fit statistics</i>								
Observations	2,486	2,486	2,484	2,484	2,486	2,486	2,484	2,484
R <sup>2</sup>	0.022	0.022	0.023	0.023	0.021	0.021	0.022	0.022

Clustered (id) standard-errors in parentheses  
Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Note: Tables represent ACME estimates from specifications run individually on each subset of the data, broken by task. Columns 1-4 present the third task, columns 5-8 the fourth. SEs clustered by respondent throughout. Source: Analysis of author-collected survey data.

## D Heterogeneity

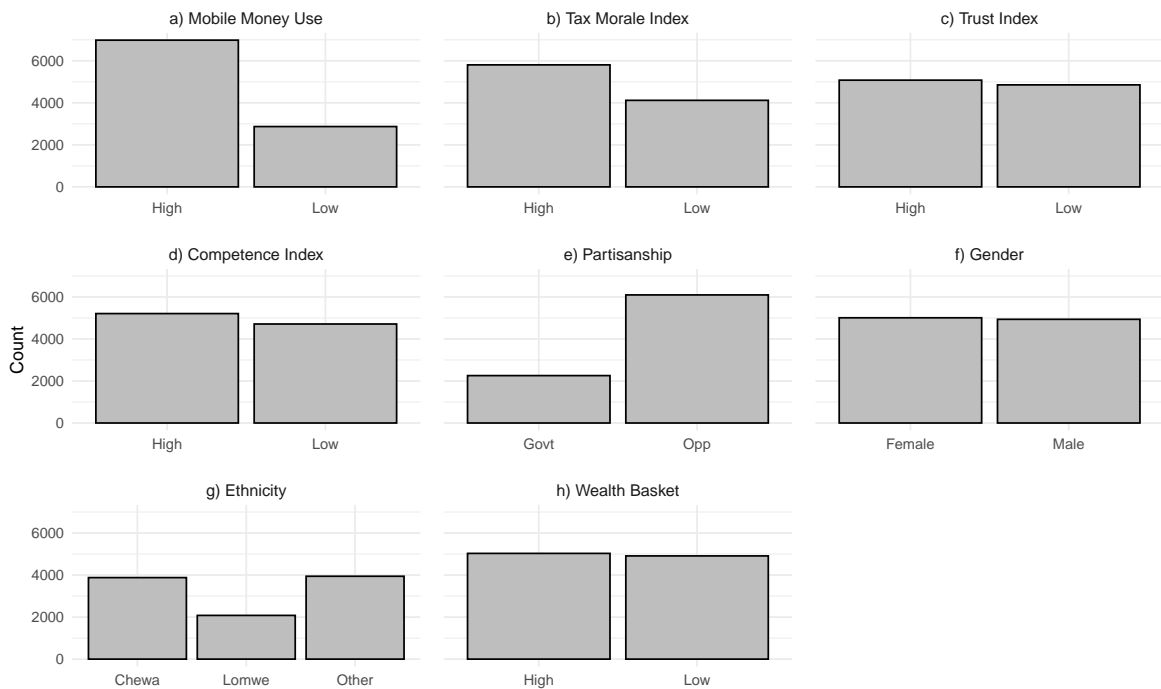
### D.1 Subgroup analyses

As discussed in the pre-analysis plan, I re-run the analysis on various theoretically motivated subgroups of interest. In a slight deviation, I draw on recent literature and present results based on marginal means, rather than subgroup ACMEs, when subsetting the data.

The groups are plotted in figure [SM2](#) below, and cover a range of political and demographic outcomes. First, respondents are split by how often they use mobile money, with “high” defined as at least a few times a week. Second, I create indices of how respondents respond to batteries of questions about tax morale, trust in government, and perceptions of how competently the government has handled various issues. Those in the “high” group are equal to or above the median score on this index, and “low” otherwise. Third, I split respondents by whether they intend to vote for the ruling MCP party (“Govt”) in the country’s upcoming election, or any other party (“Opp”). Fourth, I split by gender (Male/Female) and ethnicity (Chewa, Lomwe, or Other). Finally, I class respondents on a wealth basket index of access to basic goods and services, again defining “high” as above or equal to the median score.

Figure [SM3](#) presents marginal means for each attribute level across each subgroup. Overall, patterns are broadly consistent across groups, suggesting limited evidence of heterogeneity in respondent preferences.

First, support for earmarking revenues and a lower tax rate draw the most notable response, with means consistently above 0.5. Inversely, the alternative levels, either putting funds into the national budget or charging a higher tax rate, have means that fall below this. This again highlights the importance of these two attributes in the overall results, for virtually every subgroup tested.

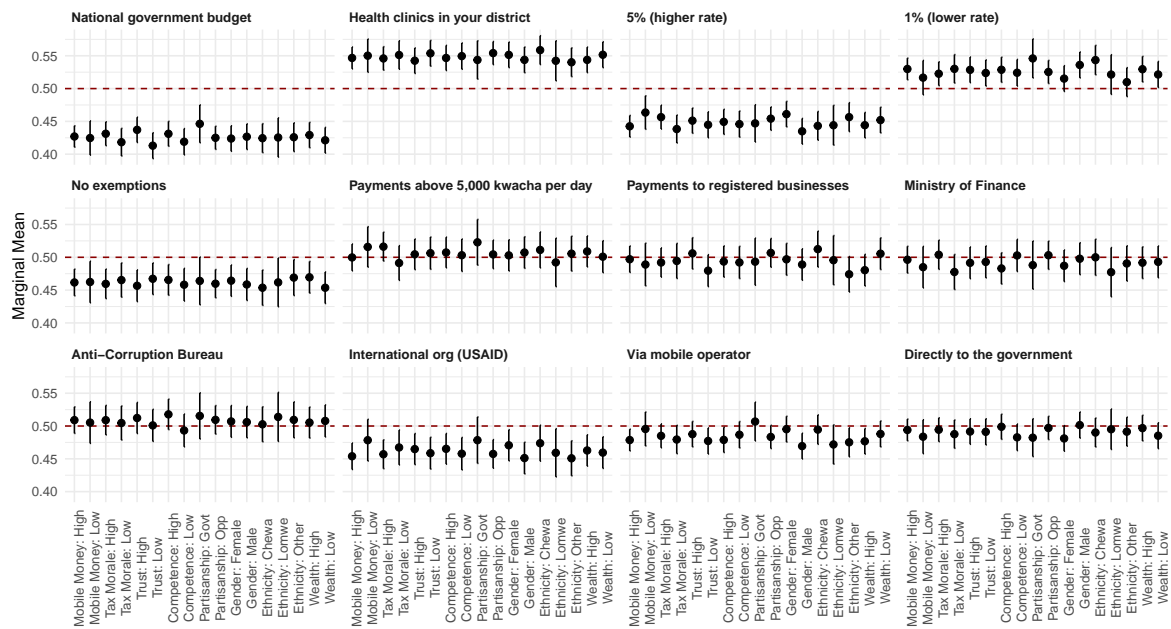


**Figure SM2:** Distribution of subgroups. Source: Analysis of author-collected survey data.

Other variables show limited variation across groups. Support for “no exemptions” is generally low, with the possible exception of government intended voters though this difference is not statistically significant from the opposition. Meanwhile marginal means for the possible exempted categories, a daily payment allowance and those made to registered businesses, are mixed and oscillate around 0.5. This suggests respondents are somewhat indifferent, and that the overall AMCE stems from a specific *dislike* of a tax without exemptions, rather than explicit support for any one given thing being made exempt.

A similar story holds for monitoring. Support for USAID-based oversight channels is consistently low, and in fact on a par with marginal means for the 5% higher tax rate attribute level. But for other bodies, like the ministry of finance or anti-corruption bureau, effects are more mixed. The subgroup marginal means appear to match the aggregate results here, in which USAID has a negative effect relative to either of its alternatives, but those alternatives are not actively preferred.

Lastly, both modes of payment collection (via mobile operator or direct to government) display means that oscillate around 0.5 or just below. This is consistent for both levels, showing the aggregate null effect in the AMCE holds more widely.



**Figure SM3:** Marginal means across full suite of attribute levels, run separately on each subgroup. Source: Analysis of author-collected survey data.

## **D.2 Interaction effects**

As discussed in the main text, I test several pre-registered interaction effects between different attributes. This includes seeing if the impact of earmarking or monitoring depends on the tax rate, and whether monitoring shapes support for earmarking. Lastly, beyond the pre-analysis plan, I interact the payment exemptions and monitoring attributes.

In general, none of the interaction terms are statistically significant while their baseline effects remain comparable to the aggregate results. This suggests that the general AMCEs on the tax rate, earmarking, or monitoring are not conditional on the level of the other particular attribute provided to respondents.

There is some indication that earmarking shapes support for a daily tax-free payments exemption, with a negative interaction term that is significant at the 10% level. Substantively, this suggests that support for the exemption falls when revenues are earmarked. One explanation may be that earmarking increases confidence that a respondent will personally benefit from the transactions tax, and so they are more willing to pay it (and hence less concerned about exemptions).

**Table SM6:** Interaction effects (I)

Interaction	Rate by earmark				Rate by monitor			
Specification	Baseline		Covariates		Baseline		Covariates	
Dependent Variable: Model:	(1)	(2)	(3)	Pr (choose policy) (4)	(5)	(6)	(7)	(8)
<i>Variables</i>								
Upper (vs lower)	-0.091*** (0.015)	-0.091*** (0.015)	-0.090*** (0.015)	-0.090*** (0.015)	-0.069*** (0.018)	-0.069*** (0.018)	-0.068*** (0.018)	-0.068*** (0.018)
Earmarking (vs no)	0.108*** (0.015)	0.108*** (0.015)	0.109*** (0.015)	0.109*** (0.015)	0.122*** (0.011)	0.122*** (0.011)	0.122*** (0.011)	0.122*** (0.011)
5000 daily (vs none)	0.039*** (0.013)	0.039*** (0.013)	0.039*** (0.013)	0.040*** (0.013)	0.039*** (0.013)	0.039*** (0.013)	0.040*** (0.013)	0.040*** (0.013)
Reg. business (vs none)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)
Monitoring by ACB (vs none)	0.018 (0.012)	0.018 (0.012)	0.018 (0.012)	0.018 (0.012)	0.025 (0.017)	0.025 (0.017)	0.026 (0.017)	0.026 (0.017)
Monitoring by int org (vs none)	-0.031** (0.013)	-0.031** (0.013)	-0.031** (0.013)	-0.031** (0.013)	-0.026 (0.017)	-0.026 (0.017)	-0.026 (0.017)	-0.026 (0.017)
Pay direct to govt (vs operator)	0.010 (0.011)	0.010 (0.011)	0.009 (0.011)	0.009 (0.011)	0.010 (0.011)	0.010 (0.011)	0.009 (0.011)	0.009 (0.011)
Upper (vs lower) × Earmarking (vs no)	0.027 (0.020)	0.026 (0.020)	0.026 (0.020)	0.026 (0.020)				
Upper (vs lower) × Monitoring by ACB (vs none)					-0.015 (0.024)	-0.015 (0.024)	-0.015 (0.024)	-0.015 (0.024)
Upper (vs lower) × Monitoring by int org (vs none)					-0.010 (0.024)	-0.010 (0.024)	-0.011 (0.024)	-0.011 (0.024)
<i>Fixed-effects</i>								
District		Yes		Yes		Yes		Yes
<i>Fit statistics</i>								
Observations	9,944	9,944	9,936	9,936	9,944	9,944	9,936	9,936
R <sup>2</sup>	0.024	0.024	0.024	0.025	0.024	0.024	0.024	0.024

Clustered (id) standard-errors in parentheses

Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Note: Columns 1-4 present results including a rate by earmark interaction term, columns 5-8 with a rate by monitor interaction term. As before, other columns vary the inclusion of demographic controls and district fixed effects. SEs clustered by respondent throughout. Source: Analysis of author-collected survey data.

**Table SM7:** Interaction effects (II)

Interaction	Earmark by monitor				Exempt by monitor			
Specification	Baseline		Covariates		Baseline		Covariates	
Dependent Variable:				Pr (choose policy)				
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<i>Variables</i>								
Upper (vs lower)	-0.077*** (0.011)	-0.077*** (0.011)	-0.076*** (0.011)	-0.077*** (0.011)	-0.077*** (0.011)	-0.077*** (0.011)	-0.076*** (0.011)	-0.077*** (0.011)
5000 daily (vs none)	0.039*** (0.013)	0.039*** (0.013)	0.039*** (0.013)	0.040*** (0.013)	0.060*** (0.017)	0.060*** (0.017)	0.060*** (0.017)	0.060*** (0.017)
Reg. business (vs none)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.027** (0.013)	0.019 (0.018)	0.019 (0.018)	0.019 (0.018)	0.019 (0.018)
Earmarking (vs no)	0.121*** (0.018)	0.121*** (0.018)	0.121*** (0.018)	0.121*** (0.018)	0.130*** (0.017)	0.130*** (0.017)	0.131*** (0.017)	0.131*** (0.017)
Monitoring by ACB (vs none)	0.009 (0.017)	0.009 (0.017)	0.010 (0.017)	0.010 (0.017)	0.018 (0.012)	0.018 (0.012)	0.019 (0.012)	0.019 (0.012)
Monitoring by int org (vs none)	-0.024 (0.018)	-0.024 (0.018)	-0.025 (0.018)	-0.025 (0.018)	-0.031** (0.013)	-0.031** (0.013)	-0.031** (0.013)	-0.031** (0.013)
Pay direct to govt (vs operator)	0.010 (0.011)	0.010 (0.011)	0.009 (0.011)	0.009 (0.011)	0.010 (0.011)	0.010 (0.011)	0.009 (0.011)	0.009 (0.011)
Earmarking (vs no) × Monitoring by ACB (vs none)	0.017 (0.025)	0.017 (0.025)	0.017 (0.025)	0.018 (0.025)				
Earmarking (vs no) × Monitoring by int org (vs none)	-0.014 (0.025)	-0.014 (0.025)	-0.013 (0.025)	-0.013 (0.025)				
Earmarking (vs no) × 5000 daily (vs none)					-0.042* (0.024)	-0.042* (0.024)	-0.041* (0.024)	-0.041* (0.024)
Earmarking (vs no) × Reg. business (vs none)					0.015 (0.024)	0.015 (0.024)	0.015 (0.024)	0.015 (0.024)
<i>Fixed-effects</i>								
District	Yes		Yes		Yes		Yes	
<i>Fit statistics</i>								
Observations	9,944	9,944	9,936	9,936	9,944	9,944	9,936	9,936
R <sup>2</sup>	0.024	0.024	0.024	0.025	0.024	0.024	0.025	0.025

Clustered (id) standard-errors in parentheses

Signif. Codes: \*\*\*: 0.01, \*\*: 0.05, \*: 0.1

Note: Columns 1-4 present results including a earmark by monitor interaction term, columns 5-8 with a exempt by monitor interaction term. As before, other columns vary the inclusion of demographic controls and district fixed effects. SEs clustered by respondent throughout. Source: Analysis of author-collected survey data.