What Politicians Don't Know Can Hurt You: The Effects of Information on Politicians' Spending Decisions

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Do well-informed politicians make more effective spending decisions? In experiments with 70% of all elected politicians in Malawi (N=460), we tested the effects of information on public spending. Specifically, we randomly provided information about school needs, foreign aid, and voting patterns prior to officials making real decisions about the allocation of spending. We show that these information interventions reduced inequalities in spending: treatment group politicians were more likely to spend in schools neglected by donors and in schools with greater need. Some information treatment effects were strongest in remote and less populated communities. These results suggest information gaps partially explain inequalities in spending allocation and imply social welfare benefits from improving politicians' access to information about community needs.

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Introduction

Do well-informed politicians make more effective spending decisions? Despite implicit assumptions in many theories of spending, politicians are seldom comprehensively informed about the characteristics of their constituencies. Such gaps in politician knowledge are particularly problematic in developing countries, where development resources are limited and public officials lack the capacity to collect

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and disseminate information. In Malawi, the context of our study, we show that most politicians lack knowledge core to their official duties. For instance, over two-thirds are unable to answer questions about the distribution of school enrollments or foreign aid in their constituencies. These information gaps are greatest for communities that are distant from politicians' homes.

In this research we document distortions in politicians' knowledge of their constituencies and explain how different types of information can change spending decisions. In doing so we contribute to debates in theories of distributional politics, politician responsiveness, and politician knowledge. Theories of distributional politics have long suggested that decisions about whether to target core or swing voters, or rely on clientelism or patronage, are contingent on politician knowledge about voting intentions and needs. But we have too little basic evidence on the source and variation in such knowledge. Similarly, theories of government responsiveness have focused on the question of when spending decisions are welfare-maximizing, but often implicitly assumed away politicians' challenges at getting basic information about constituency needs. Finally, a growing body of scholarship has explored biases and gaps in politician priors and preferences. However, this work has rarely looked at real policy decisions or considered how effects might differ in contexts with high poverty and weak state capacity.

To evaluate the effects of information on spending allocation decisions, we conducted an experiment with 70% of elected politicians in Malawi.[1] In the experiment, which focused on the education sector, we randomly assigned politicians to receive or not receive three pieces of information about schools in their constituencies: need, aid, and voting. The Need Information Treatment provided information about class and teacher overcrowding and insufficient teacher housing. The Aid Information Treatment provided information about the number and types of aid projects at the schools. The Voting Information Treatment provided information about the percentage of votes the politician received in the last election at the nearest polling station to the school. These information treatments were randomly assigned within respondent blocks in a fully-crossed factorial design.

After receiving (or not receiving) one or more of these information treatments, politicians made

[1] A pre-analysis plan (PAP) for this experiment was pre-registered prior to data analysis and is available at `https://osf.io/kazfp`. See Jablonski and Seim (2023) for replication data and code.

real decisions about the allocation of development resources (school supplies) to these same schools. Following the experiment, each politician's constituency was allocated school supplies in accordance with the politician's preferences and the outcome of a public lottery.

We find that the information treatments affected the allocation of spending. We estimate politicians in the Need Information Treatment group are about 13% more likely to spend on schools in the highest quartile of need. Further, some information treatments had larger effects when politicians were making decisions about allocations in more remote communities. We provide evidence that politicians face high costs of gathering information in these remote communities and that these communities lack access to government officials. Politicians in the Aid Information Treatment group are about 8% less likely to spend on schools with an existing foreign aid project. However, politicians in the Voting Information Treatment group appear no more likely to spend on schools with a particular vote share.

Our results suggest that information gaps are an important and under-recognized reason why politicians allocate spending to some areas more than others. However, some caution is warranted in the interpretation of our findings. Due to the limited number of politicians in Malawi, our sample size – and therefore the power of our study to detect small- to medium-sized effects – is limited.[2]

Contribution to Research and Policy

This study speaks to three branches of research. First, we contribute to debates in distributional politics regarding the ways in which incomplete information affects spending strategies (Basurto, Dupas and Robinson 2020; Diaz-Cayeros, Estévez and Magaloni 2016; Dixit and Londregan 1996; Keefer and Vlaicu 2008; Oates 1999; Ravanilla, Haim and Hicken 2022; Stokes et al. 2013). Whether politicians allocate to core or swing voters (or, more accurately, groups of voters) is widely regarded to be contingent on politicians holding accurate information about voter partisanship and needs (Dixit and Londregan 1996; Golden and Min 2013; Stokes et al. 2013). Dixit and Londregan (1996), for instance, propose that politicians target core voters in part due to the informational advantages that politicians have in understanding the needs of core voter communities. Yet, there is limited research attempting to

[2] We provide power simulations for the study design in the Supplemental Information (SI) 4.1.

understand how politicians learn about their constituencies, or the impact of information interventions on spending targeting. We contribute to this body of work by providing experimental evidence of a relationship between information and spending outcomes. Our findings validate the premise that incomplete information shapes distributional strategies, and that providing information to politicians changes these strategies.

Our research is also aligned with work on the responsiveness of politicians to citizen needs and demands (Buntaine, Nielson and Skaggs 2021; Golden, Gulzar and Sonnet 2023; Grossman, Humphreys and Sacramone-Lutz 2020; Hawkins, Wolferts and Nielson 2018; Keefer and Khemani 2005; Liaqat 2020; Loewen, Rubenson and McAndrews 2022; Todd et al. 2021). Much of this literature attempts to assess the conditions under which policy decisions respond to information about citizen demands. The findings of this literature have been mixed, with many finding little evidence that policy improves with new information. In our study we provide one explanation for this heterogeneity: politician priors about community needs are more uncertain for some communities and policy domains than others. As a result, politicians tend to be more responsive to some citizens and some information types than others. We provide evidence suggesting that this heterogeneity in priors and responsiveness is related to the costs of collecting information in some communities and the role of biased heuristics.

In making this contribution we also speak to policy debates around the best ways to improve responsiveness. Interventions such as decentralization and community-driven development are premised on the notion that politicians lack constituency knowledge (Bardhan and Mookherjee 2006; Mansuri and Rao 2013). Yet we have little data on when and how such knowledge gaps persist. We think it plausible that variation in politicians' access to and demand for information may help explain heterogeneity in the impacts of some of these policies. Relatedly, our research suggests that policy interventions to provide information to politicians about constituency needs – particularly about the needs of citizens with limited access to government officials – could improve the effectiveness of public spending. We elaborate in the conclusion on how such interventions might be better designed in light of these findings.

We also contribute to research on distortions in politicians' access and response to information. Largely relying on survey data from the United States and Europe, this literature documents that public officials have distorted perceptions of constituency preferences and needs (Broockman and Skovron 2018; Erikson, Luttbeg and Holloway 1975; Gulzar, Hai and Paudel 2021; Hertel-Fernandez, Mildenberger and Stokes 2019; Kalla and Porter 2021; Kertzer 2020; Pereira 2020; Rogger and Somani 2019). This literature points out that politicians often lack sufficient information to allocate resources efficiently. Politicians in the United States, for instance, often believe that the preferences of constituents are more ideologically extreme than they are in practice (Broockman and Skovron 2018; Hertel-Fernandez, Mildenberger and Stokes 2019). In Sweden, politicians are more likely to misperceive the policy positions of low-status than high-status voters, likely due to greater exposure to the opinions of high-status voters (Pereira 2020). Similarly, Liaqat (2020) studies the effects of providing information about citizen preferences to Pakistani politicians. Mirroring some of our conclusions, Liaqat demonstrates that politicians' priors about citizen preferences are mostly inaccurate and that politician responses to information are greater for female constituents, for whom the costs of information collection are greater. We contribute to this discussion by examining the effect of information on politician decision making in a context of high poverty and weak state capacity. We argue that in such contexts, perceptual biases are especially shaped by inter-personal networks and geographic and social disparities in access to political power. Our study also documents the likely policy consequences of such biases.

Finally, this research also extends work by Seim, Jablonski and Ahlbäck (2020) who analyze one treatment arm in this set of experiments to estimate the crowding out effect of foreign aid and impacts on citizen welfare. We build on this research by considering how and when politicians respond to different kinds of information and by analyzing the consequences of information for spending allocations.

Theory: How Information Affects Public Spending Allocation Decisions

Politicians often struggle to obtain sufficient information to efficiently allocate resources to constituents. To illustrate why this is, and the implications of knowledge gaps for spending decisions, consider a simple model. Suppose a politician wants to make an educational investment in one of two communities in her constituency. We assume she wants to maximize her chances of re-election; though some results of our study are also consistent with politicians weighing community welfare for non-electoral reasons: for instance, to please NGOs or aid donors. We discuss some of these alternatives below.

Let $c_i(a) > 0$ be the utility that a resident i of community j might get from the politician making an investment $a > 0$ in their school. Let $p_i \in [-\infty, \infty]$ be the prior utility that i might get from voting for a challenger over the incumbent. Let $d_i > 0$ be the resident's cost of voting. Following other theories of government responsiveness, we assume that citizens will weigh c against p and d when deciding how and whether to vote. If so, we can model a resident's utility from voting for the incumbent as $x_i = c_i(a) - p_i - d_i$. The politician's return on their investment a will equal the number of residents for whom the increase in utility they get from a exceeds the disutility they might get from voting for the incumbent:

$$v_j(a) = \sum_{i=1}^{n} \mathbb{1}(x_i > 0, p_i + d_i \geq 0) \tag{1}$$

A politician's decision problem is to choose a community with the highest return on votes: to choose the community that maximizes $\frac{\partial v_j}{\partial a_j}$. This might lead politicians to target in a socially optimal way (e.g., if voter preferences differ little across communities), but could also lead to socially sub-optimal outcomes if electorally pivotal communities are not the neediest. Many studies have considered problems of this sort, often to derive the conditions under which voters with different political preferences or incomes might be targeted with spending, or to derive the conditions under which elections will improve welfare (see, for example, Cox 2009; Diaz-Cayeros, Estévez and Magaloni 2016; Dixit and Londregan 1996; Keefer and Vlaicu 2008; Stokes et al. 2013).

Note that any such model of responsiveness has stringent requirements of politician knowledge. Accurately ranking communities on $v_j(a)$ often requires that politicians have well-informed priors on the community-level distribution of c, p, d and n. This is not a reasonable assumption. While politicians may have the capacity to collect detailed information from citizens about political and spending preferences, the opportunity costs of being completely informed are prohibitive.

One major cost is time. As Robert Fenno noted of US Congress, a politician's "scarcest and most precious" dilemma is the allocation of time (Fenno 1977). Such time constraints may be particularly binding in low-income contexts, where politicians are more likely to rely on personal communication as a primary means to learn about constituent needs, a fact we establish in the context of Malawi below.[3] As a result gathering information is often more time-intensive in low-income contexts. Politicians will particularly struggle to get information about difficult-to-access communities where communication networks are limited.

Another significant source of information costs is coordination. In Malawi, governments have to coordinate their spending allocations across several layers of administrative and customary authority. But even more challenging is the problem of coordinating with the 36 official donors and over 800 registered NGOs engaged in development-related work (NGORA Malawi 2022). Official statistics on the totality of such spending is frequently incomplete and unreliable. When politicians fail to coordinate, resource allocation will often be duplicative and non-complementary (Seim, Jablonski and Ahlbäck 2020).

Politicians can use several strategies to fill information gaps. First, they can prioritize gathering information in areas where information costs are low. When information is collected interpersonally, politicians often find it particularly costly to gather information about remote and marginalized communities (Gwiriri and Bennett 2020). In African democracies, for instance, over 70% of MPs consider the costs of constituency travel to be a burden (Barkan et al. 2010).[4] Likewise, transportation costs to visit or engage with politicians are unlikely to be paid by anyone other than the wealthiest and most invested citizens (Gwiriri and Bennett 2020).

A complementary strategy is to rely on heuristic shortcuts and intermediaries to learn about constituents. Politicians can infer voter preferences and voting behavior from employment, party memberships, ethnicity, geography and other factors (Fenno 1977). In developing contexts, politicians often rely on local brokers to gather information about voter preferences and voting behavior (Stokes et al. 2013). Politicians also learn about constituency preferences from special interests, civic groups, constituent letters, and expert pollsters (Erikson, Luttbeg and Holloway 1975; McClendon 2016; Pereira

[3] See also Bussell (2019); Grossman, Humphreys and Sacramone-Lutz (2020); Gulzar, Hai and Paudel (2021) for evidence in other contexts.

[4] Malawian MPs estimate that they pay \$1,256 for a single constituency visit: 12% of their official yearly income in 2020 (Barkan et al. 2010).

2020).

The problem with these strategies is that the resulting information will often be imprecise and biased. When the interests and preferences of intermediaries differ from those of citizens, it is likely that politicians' beliefs and policy preferences will be likewise biased, often in favor of elites and politically influential groups (Broockman and Skovron 2018; Gilens and Page 2014; Pereira 2020). Further, if politicians prioritize gathering information where information costs are low, they will be particularly uninformed about areas with higher information costs, a point which we establish in the case of Malawi below.

The consequence of having uncertain and biased information about constituency characteristics and preferences is that spending allocation decisions will be inefficient. In the SI, we expand on this point and derive the conditions under which accuracy and uncertainty about v_j lead to inefficient spending.[5] Below, we expand on this theory to consider how spending allocation decisions change when politicians are provided with accurate information.

Hypotheses about Treatment Effects

Our experimental treatments provide politicians with information relevant for their assessment of needs, foreign aid, and voting preferences at schools in their constituency. As we justify below, we expect this information to be particularly useful for politicians seeking to efficiently allocate resources to constituents. After receiving these information treatments, politicians were asked to allocate NGO-funded school supplies to a school.

In this section, we explain our hypotheses about how information affects these allocation decisions.[6] We base our discussion on the theory of responsiveness outlined above and in Equation 1. We assume that politicians will allocate supplies to the school that maximizes politician utility (v_j). We further assume that the information treatments will cause politicians to update their beliefs about the distribution of voter preferences and consumption (c or p). Under these two assumptions, we expect the information treatments to cause politicians to be more likely to allocate supplies in a way that

[5] See SI Section 2.

[6] In the SI, we test additional pre-registered hypotheses (Section 10).

increases politician utility.[7]

Need Information As we detail below, the Need Information Treatment provides information about the ranking of school needs in each community based on statistics about structural, facility, and teacher overcrowding. We expect that this treatment will increase the accuracy of politicians' beliefs about the mean consumption utility that residents get from spending at their school ($\bar{c}_j$). From equation 1, it follows that politicians will likely expect greater utility from allocating to communities where consumption utilities are high (we assume $\frac{\partial v_j}{\partial \bar{c}_j} > 0$). If so, the Need Information Treatment will shift spending allocations towards schools which are ranked as having relatively greater need.

Hypothesis 1

When politicians receive information about school needs, they will be more likely to allocate to high-need schools.

Aid Information The Aid Information Treatment provides details on the number of foreign aid projects in each school as well as a categorization of the types of aid goods provided. Given the high costs of coordinating with development actors in Malawi, we expect that this information will allow politicians to better take alternative spending into account when making their allocation decision.

Existing research suggests competing hypotheses regarding how government spending might respond to foreign aid.[8] First, politicians might believe that the marginal returns to overlapping spending are negative ($c''(a) < 0$). If so, politicians seeking to maximize c should respond to aid information by reducing allocation in high-aid areas, leading to a "crowding out" effect of foreign aid on government allocation.

Alternatively, politicians might expect complementarity between foreign aid and government spending ($c''(a) > 0$) – for example, they might expect spending to be more effective where donor-funded infrastructure is in place. This would imply that the treatment will cause more spending

[7] In the SI, we elaborate on the conditions necessary for these conclusions to hold (SI Section 2).

[8] For discussion of these debates and analysis of how foreign aid information might impact spending, see Seim, Jablonski and Ahlbäck (2020).

allocations in high-aid areas. Thus, depending upon politician beliefs about the complementarities between aid and spending allocation, we expect the Aid Information Treatment to have one of two effects:

Hypothesis 2

When politicians receive information about foreign aid, they will be less likely to allocate to high-aid schools.

Hypothesis 3

When politicians receive information about foreign aid, they will be more likely to allocate to high-aid schools.

Voting Information The Voting Information Treatment provides politicians with information about the percentage of votes they received in the community around each school. This information should increase the accuracy of politicians' beliefs about mean voting preferences in a community ($\bar{p}_j$). There are multiple ways this information might impact politicians' spending utility ($\frac{\partial v_j}{\partial \bar{p}_j}$). If voter mobilization costs (d) are low, better information could cause politicians to more efficiently target communities with a high density of swing voters (those for whom p_i is near 0) in order to persuade voters who are somewhat indifferent. Alternatively, if the costs of mobilization are high, politicians will instead have incentives to target supportive communities to encourage greater mobilization among their base.[9] In more complex models, such core and swing voter targeting decisions might also depend upon factors like the costs of coordinating brokers (Stokes et al. 2013), the costs of learning about efficient spending options in core versus swing communities (Dixit and Londregan 1996), or the credibility of campaign promises in core versus swing areas (Keefer and Vlaicu 2008).

Our pre-registered hypothesis is that voting information increases allocations to communities with greater support for the politician: those with core voters.[10] One reason for this preference might be that

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[9] To see this, note that to persuade a voter, it must be the case that $c_i(a) > p_i + d_i$. The most efficient voter to target will therefore be the one for whom $p_i + d_i$ is nearest 0. Thus as d increases, the value of p for this efficient voter must decrease.

[10] In the SI, we also consider and reject alternative swing voter hypotheses (SI Section 4.6).

the costs of voting (d) are relatively large in Malawi and politicians (particularly at the local level) have strong incentives to mobilize supporters (Duwa 2014). Politicians' preference to target core voters in Malawi might also be shaped by low voter loyalty in Malawian local elections.[11] Where politicians cannot rely on core voters to remain loyal, politicians might prefer to invest in retaining their voting coalition rather than in persuading swing voters.

Hypothesis 4

When politicians receive information about voting, they will be more likely to allocate to high-support schools.

Hypotheses about Heterogeneous Effects

Transparency In an independently randomized, overlaid experiment, we assign a fourth treatment that varies the level of transparency of the politician's decision (the Transparency Treatment). Politicians' decisions are either shared on local radio or distributed in a report to all major donors in Malawi(or both or neither). This treatment clarifies both attribution (who is responsible for the allocations) and recipient (which school is intended for the allocations).

To the extent voters and donors prefer to maximize social welfare and are willing to sanction politicians accordingly, transparency raises the costs for politicians of allocating along dimensions other than social welfare. Thus this treatment effectively manipulates the weights that politicians put on consumption utilities versus political preferences ($c(a)$ versus $p + d$). The implication is that transparency will magnify the effect of need information and diminish the effect of voting information, resulting in the following heterogeneous effects hypothesis:[12]

Hypothesis 5

[11] For example, only 18% of incumbent councillors held their seats in 2019.

[12] Transparency could mediate the effect of the Aid Information Treatment, but in practice citizens have insufficient information about aid to sanction politicians (Seim, Jablonski and Ahlbäck 2020). We find little evidence for such an interaction (SI Section 4.7).

When politicians know that their decisions will be transparent, the effect of the Need Information Treatment will be larger and the effect of the Voting Information Treatment will be smaller.

Information Costs We theorize that politicians will be more uncertain about allocation decisions in communities where the costs of obtaining information are high. If so, the effects of the information treatments will increase with the costs of information.[13]

We consider three proxies for the costs of obtaining information. First, when politicians are resource-constrained and rely on personal interactions to gather information, information costs will be greatest in geographically remote communities. Second, information costs will be higher in less densely populated communities due to their often lower levels of political representation and political importance. Third, as politicians are likely to have more robust social networks in politically supportive communities, information costs will be lower in supportive areas and higher in areas with low support.

We test this argument in two steps. First, in the section that follows, we describe the Malawian local government context and use survey and interview data to evaluate how politicians' knowledge of their constituencies varies over space and correlates with these proxies for information costs. Second, we experimentally test whether the effects of the information treatments are larger in communities where information costs are higher, as measured by these proxies, which leads to the following hypothesis:

Hypothesis 6

Information treatment effects will be greater where schools are: (a) further from a politician's hometown; (b) in less populous communities; or (c) in communities who are less supportive of the politician.

A couple caveats are in order. First, to avoid priming effects, we did not measure politician priors or posterior beliefs at the school level. We therefore cannot measure updating directly.[14] We do show evidence from post-treatment surveys that politicians learned from the information treatments and incorporated the information into their decision making. Nonetheless, there are politicians who are

[13] We demonstrate this point formally in SI Section 2.

[14] As we demonstrate in SI Section 2, the direction of effects are unlikely ever to be conditional on biases in priors. We also consider the implications of biased updating (Adida et al. 2017).

ineligible to update because their priors were more accurate. Thus, our estimates should be interpreted as intent-to-treat (ITT) effects.

Information and Spending Allocation Decisions in Malawi

Our experiment takes place among elected local councillors (LCs) and members of parliament (MPs) in Malawi. Every five years, LCs and MPs are elected from single-member electoral wards (LCs) and constituencies (MPs). For simplicity, we refer to all electoral units as constituencies. The politicians in our experiment were elected in 2014. The experiment took place in 2017.

While Malawi has a multiparty system of government, party organizations tend to be weak and often fail to articulate clear programmatic policies (Lembani 2008). Most Malawians instead expect politicians to deliver public goods or development projects in exchange for electoral support. There are many ways politicians can control the allocation of development resources. At the local level, both MPs and LCs are members of the district councils. Councils have an average budget of approximately US$5 million, 11% of which is dedicated to education, the sector on which we focus our study.[15] Additionally, MPs each have access to a discretionary Constituency Development Fund (about $40,000 in 2016) for development projects in their constituencies. Finally, politicians rely on their influence with local and international development organizations to bring development projects to constituents.

In this section, we validate two assumptions underpinning our theory in the Malawi context. We first demonstrate that Malawian politicians seek information to aid them in allocation decisions, and that their ability to do so varies with information costs. We next demonstrate that politicians face gaps in their knowledge of constituency characteristics.

Assessing Politician Sources of Information

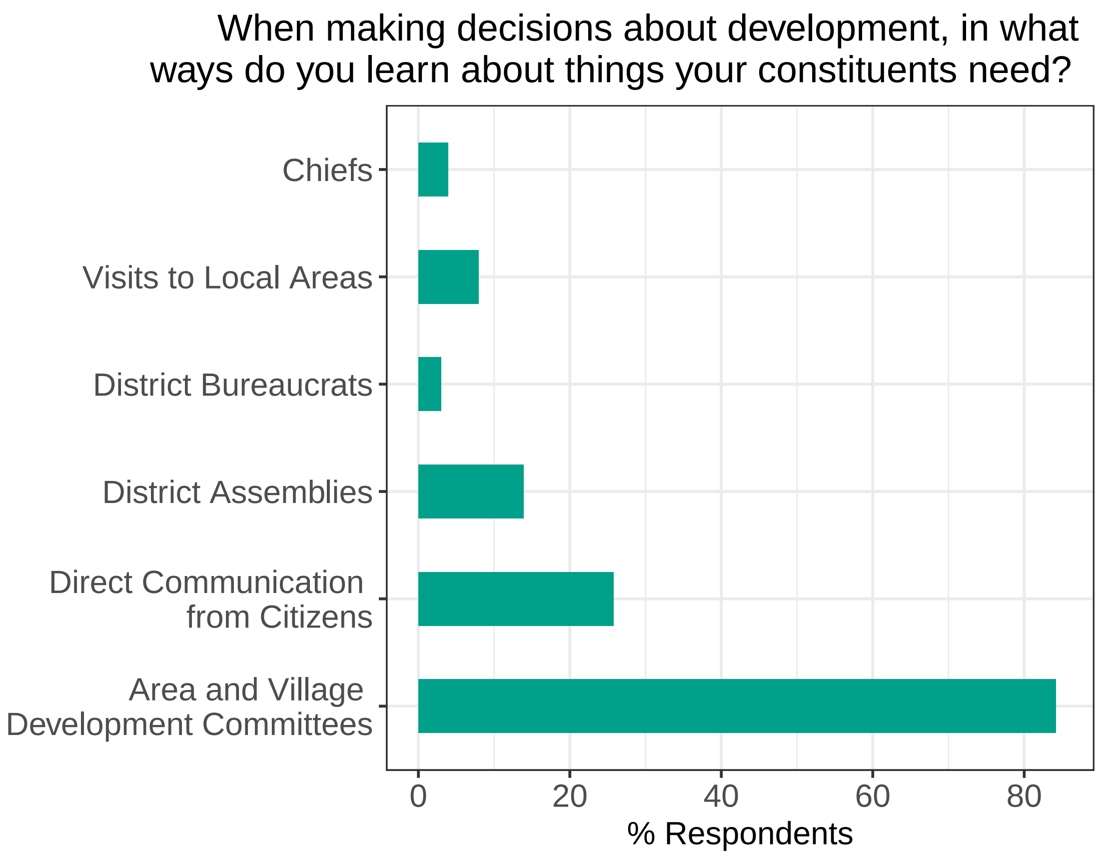
Prior to treatment, we explored how elected officials in Malawi gather information relevant to spending allocation decisions by conducting phone interviews with 101 LCs in Malawi.[16] We asked each to

[15] 2016 Ministry of Local Government data.

[16] See SI Section 7.1 for details on these interviews.

describe where they learn about the needs of their constituents. We summarize responses in Figure 1. Most commonly, councillors get information from Area Development Committees (ADCs) and Village Development Committees (VDCs). ADCs are oversight committees at the chiefdom level and VDCs are analogous committees at the village level. The primary role of these committees is to aggregate community preferences and liaise between communities and governments. Similar development-focused community associations exist around the world (Bardhan and Mookherjee 2006).

FIGURE 1. Sources of Constituency Information for Elected Councillors



Note: This figure summarizes responses from a survey of a randomly selected sample of 110 councillors involved in this study.

Another common information source is communication with citizens. This information channel relies on a politician's personal connections and the initiative of individual citizens, and is therefore also vulnerable to bias.

Interestingly, no councillor mentioned relying on any government or non-governmental data. It is possible that politicians did not expect such information to be useful; though we think a more likely explanation is that these data are difficult to access. Partly due to inconsistent funding and capacity of statistical offices, it is very difficult for politicians in Malawi to access and consume disaggregated statistics on voting, school characteristics, and foreign aid. As we document below and in the SI, all of the data we use in the information treatments required considerable processing to be meaningful.

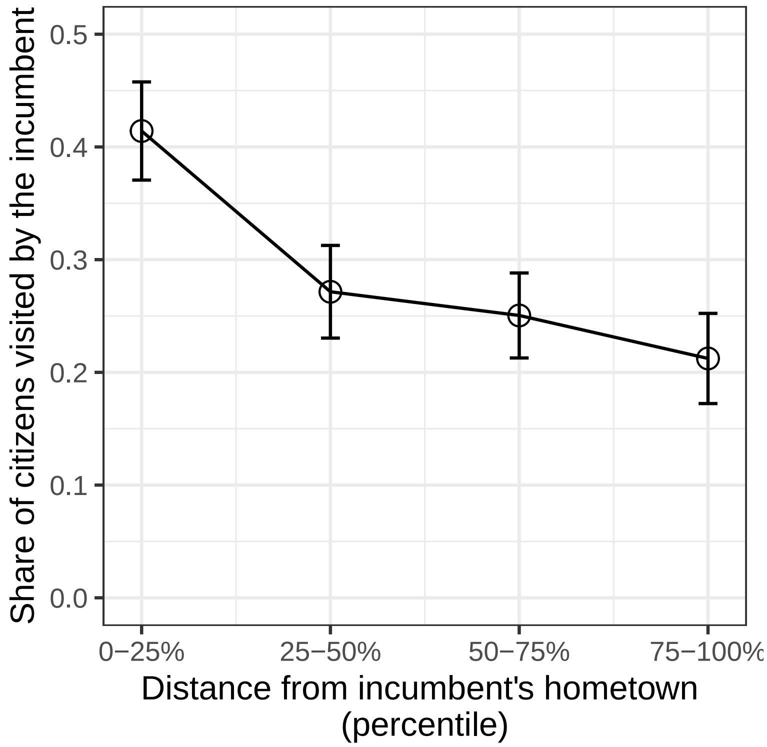
We also conducted in-person interviews with five MPs.[17] Given their greater resources, MPs were more likely to rely on government bureaucrats for information, especially the District Education Manager, who is responsible for managing education resources in the district. Others mentioned communication from chiefs, non-governmental organizations (NGOs), or teachers.

As discussed above, one implication of relying on personal networks is that information costs will be lower for communities proximate to the politician. In Malawi, for instance, many councillors complain that the government never fulfilled pledges to finance motorbikes to lower costs of travel to distant constituents (Chauwa 2016). To illustrate the implications of distance for constituency information gathering, we examine whether the number of citizen-reported politician visits to schools covaries with the school's distance to the politician's self-reported home town (Figure 2).[18] Councillors visited about 41% of villages within 6 km (the 25th percentile) from the councillor's home village, but they visited only 21% of villages more than 18 km away (the 75th percentile).

[17] See SI Section 7.1.

[18] Distance equals the most efficient driving or walking distance from politicians' self-reported home town per Google's API. See SI Section 7.2 for information about the survey of Malawian citizens and teachers on which these data are based.

FIGURE 2. Distance and Councillor Visits



Note: This figure shows the mean number of citizens reporting at least one visit from their councillor grouped by how far away (in percentiles) they are from the councillor's hometown. Vertical lines show 95% confidence intervals adjusted for village-level clustering. See SI Table S1 for tabular estimates.

Assessing Politician Knowledge

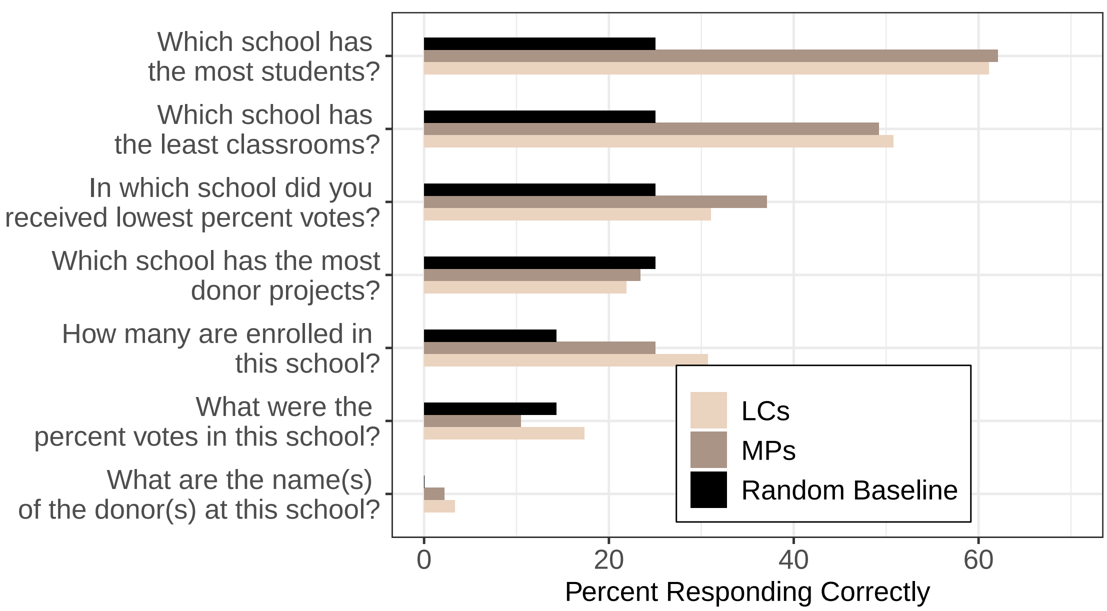
In this section, we establish that politicians have incomplete knowledge of their constituencies and that this knowledge is biased in systematic ways. To measure knowledge, we asked the politicians enrolled in our experiment to take a post-treatment quiz about their constituencies. There were seven, mostly multiple-choice questions on this quiz. Each question asked politicians to evaluate traits of three randomly selected primary schools in their constituency. The quiz questions aligned with information provided in the Need Information Treatment, Aid Information Treatment, and Voting Information Treatment (described below). Since local councils are statutorily responsible for for most education provision and coordination, these questions assess knowledge that is core to politicians' official duties. We nonetheless identify large gaps in knowledge.

We summarize the proportion of correct answers to each quiz question in Figure 3. In brief, politicians had relatively high levels of knowledge regarding constituency needs (school enrollment and classroom infrastructure), but relatively low levels of knowledge regarding the distribution of donor projects across the constituency. For instance, only 22% of politicians could identify the school with the most foreign aid projects, which is indistinguishable from answering randomly.

Knowledge of voting was mixed. Politicians were generally able to evaluate their support in a relative sense, with 33% able to identify the school where they received the fewest votes. However, they were less aware of their exact level of support in a given area.

Contrary to the theoretical literature on decentralization (see, for example, Bardhan and Mookherjee 2006), we find little evidence that knowledge varies systematically by political office: On average, MPs got 31% of questions correct and local councillors got 32% of questions correct.

FIGURE 3. School Knowledge Questions



Note: The x-axis shows the percentage of politicians responding correctly to questions about the characteristics of three randomly selected schools in their constituencies. All questions are multiple-choice except for the question on the name of the donor. The top dark line shows the expected proportion of correct answers we would expect if politicians answered randomly.

Politicians' knowledge of their constituencies also varied over space. In SI Section 5.2, we assess how answers on this quiz vary depending on the features and locations of schools in a politician's constituency. Political support and distance are particularly strong predictors of knowledge. A standard deviation increase in distance from a politician's hometown is associated with a 4-6% *decrease* in the proportion of correct answers ($p = 0.03$). Similarly, a one standard deviation increase in votes for a politician is associated with a 9% *increase* in the proportion of correct answers to questions about voting patterns ($p < 0.01$).

These data demonstrate that politicians have gaps in their understanding of their constituency, and that these gaps are greatest where communities are socially or geographically distant. This is consistent with politicians relying on biased heuristics when learning about their constituency.

Research Design

To study the effects of information on spending allocation decisions, we reached out to all the MPs and LCs in Malawi. We successfully recruited 125 in-office Members of Parliament (MPs) and 335 in-office Local Councillors (LCs) in Malawi, or 63% and 73% of each population, respectively. We show a map of sampled constituencies in Figure 4.[19]

In partnership with a UK-based NGO (Tearfund), we offered each politician the opportunity to allocate school supplies to schools in their constituency. In face-to-face interactions with Malawian research assistants (RAs), each politician was presented with a map of their constituency with three schools marked on it. The three schools that appeared on the map were randomly selected from the government's list of primary schools in the constituency. The politician was then asked to decide which of the three schools should be allocated a type of school supplies. Specifically, the decision prompt was: "When you are ready, please tell me which school you would like to choose to receive a set of [*school supply*]. Please take your time in making this decision." The maps, an example of which is shown in Figure 5 below, were shown to the politician on portable tablets and could be studied by him or her in detail before the allocation decision was made.[20]

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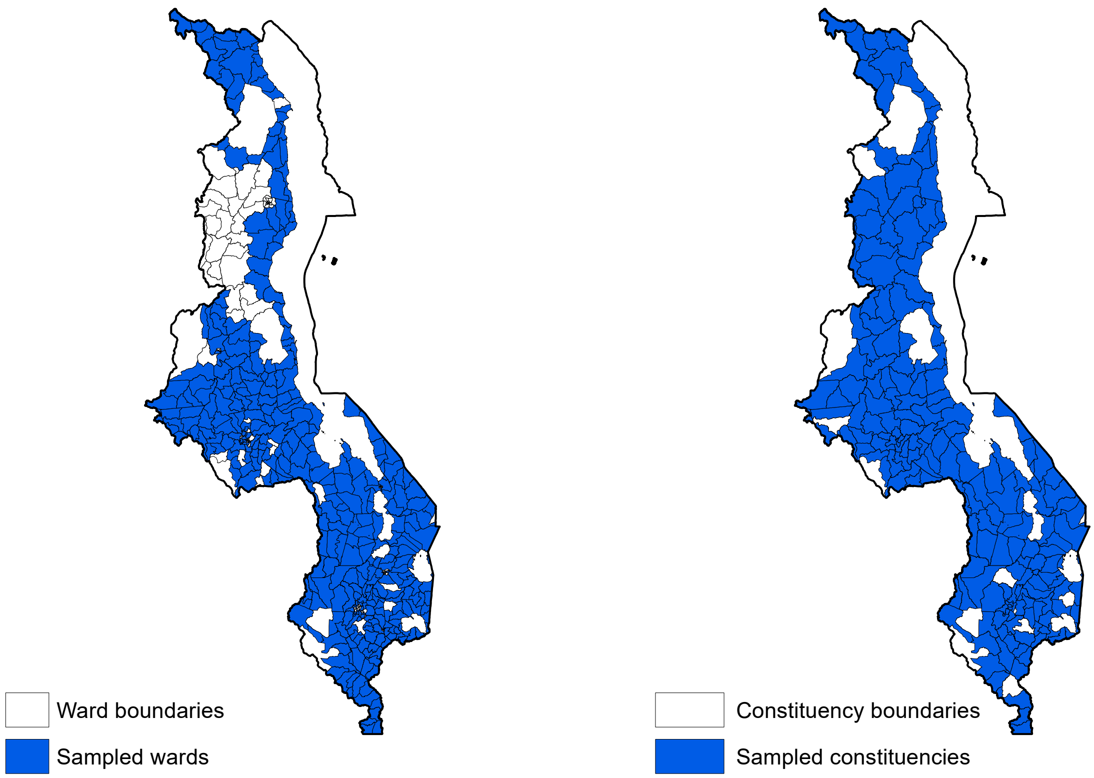
[19] See SI Section 6 for sample and attrition statistics.

[20] This design draws on methods used in the choice experiment literature to model consumer preferences; see Clark et al. (2014). We show example maps for all information treatment combinations in SI Section 8.2.

FIGURE 4. Sampled Constituencies



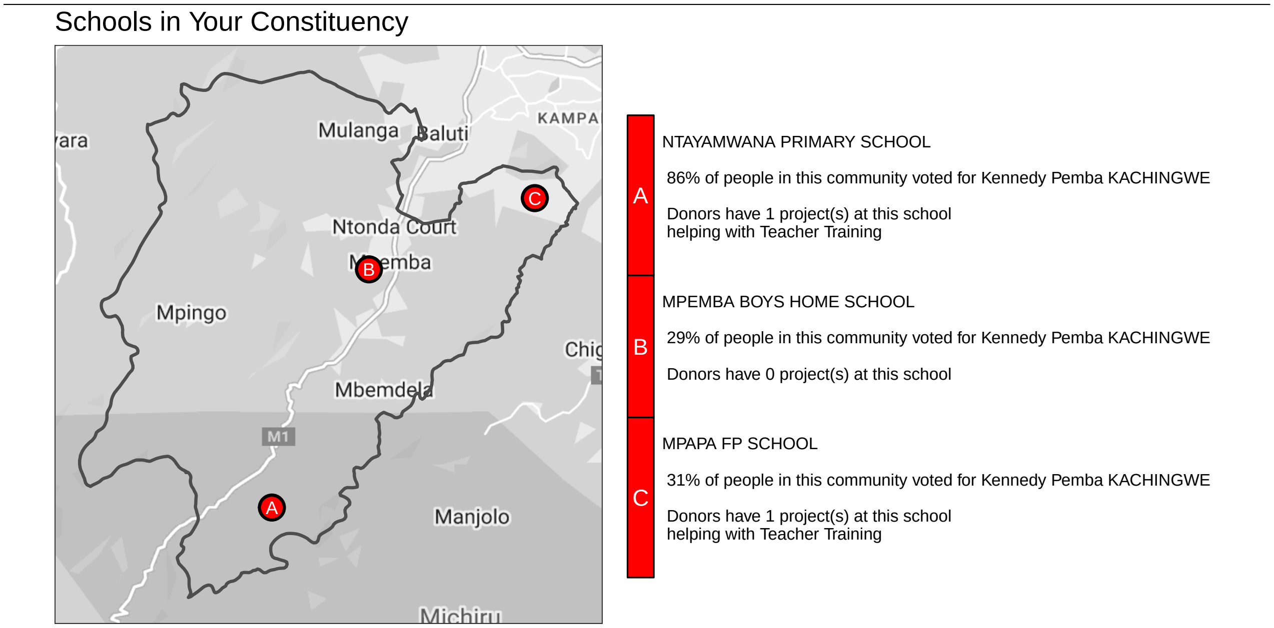
Note: This figure shows the constituencies of politicians in the sample.

Each politician repeated this process three times, so they ultimately selected three schools out of nine to receive school supplies.[21] Each decision involved the allocation of a different type of school supplies–either a set of 10 solar lamps, 10 teacher supply kits, or 10 English dictionaries. Our focus group discussions with project stakeholders suggest that these school supplies are valued by politicians and schools. The portable, stand-alone solar lamps are useful to allow students and teachers to work after dark. The dictionaries are helpful in lesson planning and studying. The teacher supply kits consisted of a box of chalk, rubbers, pens, notebooks, and tote bag - basic supplies considered necessary for teachers to carry out their work.[22] The ordering of maps, supplies, and schools was random.

[21] Because of the small number of schools in some constituencies, some politicians (21%) received fewer than three maps.

[22] See SI Section 8.4 for more details and pictures.

FIGURE 5. Example Map with School Need and Voting Information



These were not hypothetical decisions. Following the experiment, the selected schools were entered into a public lottery. Approximately 20% of the selected schools were chosen in this lottery to receive supplies. The details of the lottery were provided to politicians before they made their decisions. Politicians appeared to value the school supplies and make the decisions about allocation carefully. About a third of our sample either participated in the lottery or called to inquire about the results. Many also showed up at schools to participate in delivery. The funds for school supplies were provided by research grants.

The decisions in the experiment mimic decisions politicians make as part of their official duties. Almost all politicians we interviewed pre-experiment mentioned working with NGOs.[23] When asked to cite an example of a development project they brought to their constituency, most politicians mentioned a project implemented (and funded) in partnership with an NGO rather than one implemented directly by the government. As one MP described, "I also have a close relationship with [an education NGO]. Using my influence, they have constructed 18 school blocks in my constituency." A councillor stated, "I interact with donors on [a] monthly basis and they consult when they want to come up with a project."

Further, much of the education budget for councils comes from donors. For example, an average of approximately $200,000 within each district is allocated to individual schools through the USAID

[23] See SI Section 7.1.

School Improvement Grants (SIG) program.[24] In fact, so much of the local budget comes from donor funding, neither politicians nor voters consider the funding source when thinking of government projects. For example, in a survey of teachers in Malawi, 27% could identify a particular project completed at their school that they attributed to an elected official. Out of these, the majority (71%) were projects that could have been funded by either a NGO or government (or by the official personally). Only four percent were identified as government-funded projects, and 24% were identified as NGO-funded projects.[25] In light of the equivalence of different funding sources in the Malawian context, we cannot tie the effects we report below to a particular funding source: it is possible the effects would be different if the source of funding was specified or primed before the allocation decisions were made.

Treatment Assignment

Prior to making the allocation decisions, politicians were informed about the transparency of their decisions. Specifically, we randomly assigned politicians to two transparency treatments in a crossed-factorial design within paired blocks. Politicians in one of the transparency treatment groups were told (truthfully) that their allocation decisions would either be announced on local radio and/or compiled in a report for distribution to major donors.[26] To ensure that politicians understood the transparency treatments, they were played a sample radio broadcast and/or shown a sample report to donors.[27]

Once politicians were informed of their transparency treatment conditions, they went on to make the allocation decisions based on the maps described above. Three information treatments were independently and randomly assigned at the map level within respondent-level blocks: Need Information, Aid Information, and Voting Information. The treatment was assigned factorially, so each map received between zero and three information treatments. We chose this set of information treatments based on the theories discussed above and extensive year-long pre-experiment scoping

[24] Data collected by authors from District Education Managers.

[25] See SI Section 7.2.

[26] The radio broadcast occurred ten months after data collection on Zodiak radio. The donor report was disseminated ten months after data collection to 13 donor agencies.

[27] See examples in SI Section 8.5.

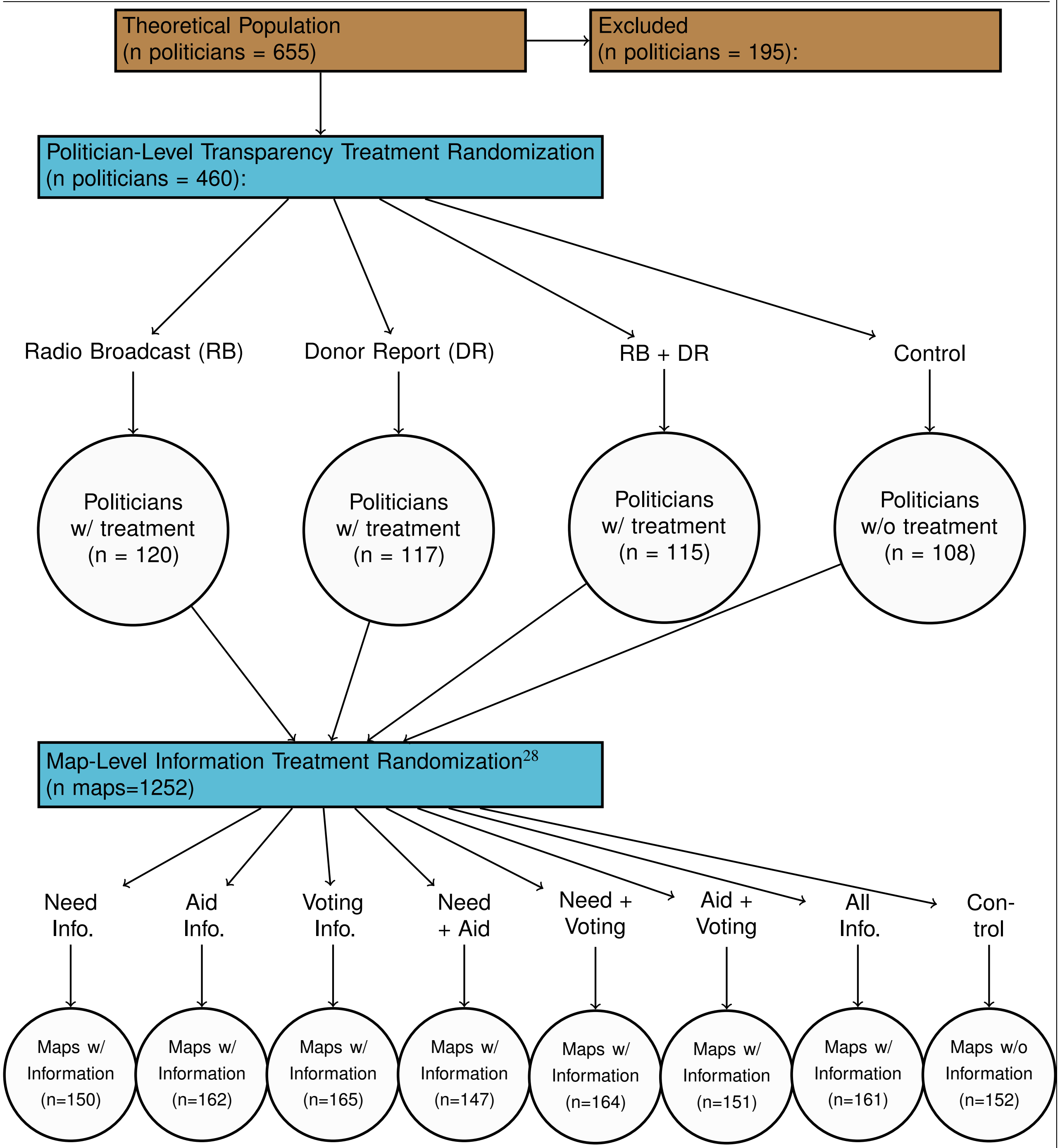
activities. Specifically, before the experiment, we conducted 32 semi-structured interviews with local councillors, members of parliament, District Commissioners, and Area Development Committees, as well as four focus group discussions with Malawian citizens. We also conducted phone interviews with 101 randomly selected local councillors to further evaluate how they gather information about their constituencies and make allocation decisions, and then we ran a pilot of the experimental protocol. Finally, we conducted a survey of over 2,000 citizens and teachers associated with 180 schools across Malawi to assess community needs and preferences. Further details on these activities are in the SI.

All the estimates reported below reflect within-respondent and within-map treatment estimates. In 6 we show a CONSORT diagram that depicts the broader experimental design. In Figure 1, we summarize the information provided in each treatment, and next we discuss each information treatment in detail.

TABLE 1. Summary of Information Treatments

Information Treatment	Information Provided
Need Information	For each school on a Need Information Treatment map, shows the ranking of school needs, the number of students per classroom, the number of students per teacher, and the number of temporary and permanent classrooms.
Aid Information	For each school on an Aid Information Treatment map, shows the number and type of aid projects supported by international donors at each school in the past 5 years.
Voting Information	For each school on a Voting Information Treatment map, shows the percentage of votes received by the politician in the polling station nearest to the school.

FIGURE 6. Experiment CONSORT Diagram



[28] An additional 83 maps were excluded post-treatment due to discrepancies in constituency boundaries or issues in plotting. See SI Section 6.4.2.

Need Information Treatment

The goal of the Need Information Treatment is to improve politicians' assessments of the welfare and consumption consequences of spending allocations at a particular school. We chose the information to include in this treatment based on pre-experiment piloting activities. In a survey of teachers, we asked teachers to prioritize the needs at their schools. The highest priority issues (named by over 60%) were overcrowding in classrooms and teacher houses, both of which suggest a need for infrastructural support. Teachers also frequently mentioned needing more staff, more learning materials, and various facility improvements, including electricity.[29] Similarly, in our interviews with politicians about how they make development decisions in the education sector, they most frequently mention considering enrollment levels, the number of classrooms, and the number of teachers ho[uses.[30] Other politicians consider the "look of the infrastructure," or "the nature of the school."

Accordingly, maps in the Need Information Treatment show politicians information about three dimensions of school need: structural overcrowding (number of students per classroom), teacher overcrowding (number of students per teacher), and the quality of classrooms at each school (the ratio of temporary to permanent classrooms).[31] To simplify the interpretation and analysis of this information, we also use these three dimensions of need to provide politicians with an ordinal ranking of the needs in each school relative to others in the constituency as illustrated in Figure 5.

Education statistics reinforce the importance of these dimensions of need: on average, primary school classrooms have 138 students each, though some have more than 300. Due to chronic problems of low or unpaid salaries, teachers in Malawi are often heavily over-committed and underpaid. Primary school teachers are expected to teach 75 students on average, though some have more than 200. The quality of temporary classrooms varies, but these are often of extremely poor quality, sometimes

[29] See SI Section 7.2.

[30] See SI Section 7.1.

[31] The data for these three measures come from 2014 official school-level statistics from the Education Management Information System at the Malawi Ministry of Education Science and Technology.

consisting of lean-to structures and borrowed residences.

Though not an exhaustive assessment of school need, the aforementioned dimensions are three highly visible characteristics of need. In addition, the number of students per classroom and the number of students per teacher are robustly linked to education quality (Birdsall, Levine and Ibrahim 2005).

To construct the ordinal ranking of needs, we create an index, *School Need Index*, which is equal to the sum of the z-scores of the three measures of school need.[32]

Aid Information Treatment

The Aid Information Treatment is designed to improve politicians' ability to assess the international development interventions at each school. Such considerations are highly relevant to spending allocation decisions in Malawi, which is among the most aid dependent countries in the world (Seim, Jablonski and Ahlbäck 2020). Between 2011 and 2016, donors directly funded projects in approximately 34% of primary schools, which is roughly comparable to the percent of schools (38%) that received projects funded by the local government.[33] As noted above, many government-allocated projects in schools are still funded by donors through budgetary support.

To collect information on school-based aid interventions, we focused on the 11 international donors active in the education sector.[34] We asked each donor to provide data on their project activities since 2011, including the type of intervention and the name and location of the recipient school(s).

In total, 3,151 primary schools received 4,566 foreign aid projects from this set of donors between 2011 and 2016. The number of foreign aid projects in each school varied from 0 to 4. We use these data to populate maps in the Aid Information Treatment with details on the number of foreign aid

[32] $SchoolNeed = \frac{x-\mu_1}{\sigma_1} + \frac{x-\mu_2}{\sigma_2} + \frac{x-\mu_3}{\sigma_3}$ where μ_i and σ_i indicate the within-constituency means and standard deviations of students per teacher, students per classroom, and proportion of temporary classrooms for all available primary schools in Malawi.

[33] See SI Section 7.2.

[34] An additional 4 donors did not respond to our queries. See SI Section 8.3 for the protocol and donor list.

projects (*Aid Project Count*) and the number of development goods types (*Aid Good Types*) at each school (see SI Section 8.2 for example).[35] Seventy-three (73%) of the Aid Information Treatment maps contained variation across schools in the number of foreign aid projects. Since politicians might care about both the volume and breadth of donors' involvement in a school, we consider both the number of projects and number of goods types in our analysis below (as pre-specified).

Voting Information Treatment

The Voting Information Treatment improves politicians' ability to assess the political preferences of their constituencies. Our interviews and piloting activities suggest politicians often consider voter preferences when making spending allocation decisions.[36] For instance, in an interview, one District Commissioner said, "Whenever [we] conduct a meeting with the elected officials to identify the area where the development should go, most of them choose the area where he got more votes." Politicians also justify their decisions similarly. One politician, when asked to justify his allocation decisions in our piloting activities, explained that he "was taking into consideration how people voted for [him] so [he] wanted to please [his] people."

In order to measure voter preferences at the community level, we collect polling station-level data on the votes received by all politicians in the most recent (2014) election. A large proportion (68%) of the schools in our sample are also polling stations, allowing us to directly measure voter preferences in those communities. For those schools in our sample which are not used as polling stations (32%), we measure voter preferences by using the geographically nearest polling station to the school. In the Voting Information Treatment, we use these data to populate maps with the percentage of votes politicians received at or near each school shown (*Percent Votes*).[37]

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[35] We classified the goods into capacity building, construction, health services, food provision, community support, gender issues, and teacher training. Some projects encapsulate multiple types.

[36] See SI Section 7.1.

[37] See SI Section 8.2 for example

Estimation

To test our hypotheses, we estimate how Need Information, Aid Information, and Voting Information change the odds a politician allocates to a school with certain traits.

Formally, let $P(Y_{nsi} = 1)$ indicate the probability politician n chooses school i in map s. In the absence of any information treatment, we expect that this probability will vary depending on the levels of *School Need Index*, *Aid Project Count*, *Aid Good Types* and *Percent Votes*, as defined above. Let these characteristics of each school equal z_{is}. Let X_{is} be a vector of school-specific controls.

To estimate information treatment effects, we evaluate how the effects of z_{is} vary with treatment assignment. Let $t_s \in [0, 1]$ be our randomly assigned treatments of information at the map level. Our treatment equals one if map s has been assigned to a treatment group and zero if it is in a control group. To estimate the effects of treatment, we interact t_s with z_{is} as in equation 2.

$$P(Y_{nsi} = 1) = \phi(\beta_1 z_i + \beta_2 t_s z_i + \gamma X_{is} + e_{nsi}) \tag{2}$$

We estimate ϕ using a conditional logit model (conditioned on map s). The conditional logit is an extension of the logit model for discrete choice experiments where individuals make decisions between more than two outcomes (McFadden 1973). The estimates from a conditional logit estimator are generally less biased than alternative estimators in this setting.[38] In SI Section 4.9, we also show consistent estimates using a linear probability model with fixed effects for each s. The conditional logit model averages the odds of a school being selected for allocation within each choice map, so variables that do not vary within s drop out of the estimating equation (such as t_s). Since politicians each make more than one choice, we cluster our errors at the politician level. Conservatively, we use two-tailed hypothesis tests throughout.

Below we show estimates from three separate equations, one for each t_s and z_{is} pair. In SI Section

[38] For discussion of trade-offs in the estimation of discrete choice experiments, see Clark et al. (2014); McFadden (1973).

4.7, we also show results jointly estimating all treatment effects and their interactions. We are primarily interested in β_2, which corresponds to the change in the effect of z_{is} in treatment versus control.

We are also interested in estimating how the information treatment effects vary with information costs. We estimate these conditional average treatment effects using a triple interaction term. That is, for each conditioning variable w_i, we estimate the following equation and then analytically calculate the treatment effect and standard error conditional on w_i.

$$P(Y_{nsi} = 1) = \phi(\beta_1 z_i + \beta_2 w_i + \beta_3 t_s z_i + \beta_4 t_s w_i + \beta_5 z_i w_i + \beta_6 t_s z_i w_i + \gamma X_{is} + e_{nsi}) \tag{3}$$

We include estimates with and without control variables. Our pre-specified controls include *Log Permanent Classrooms, Log Temporary Classrooms, Log Teacher Houses Permanent, Log Teacher Houses Temporary, Opposition Percent Votes (for MP and LC), Log Enrollment, Number of Aid Projects, Family Attends School, Incumbent Percent at Polling Station*, and *School Need Index*.[39] We normalize continuous variables in our analysis; coefficients can be interpreted as the effects of a standard deviation change in a continuous variable (or a one-unit change in a count variable) on the odds or log odds of a school being selected for allocation by the politician. We also discuss within-sample predictions on a probability scale.

Results

Allocation Patterns in the Absence of Information

We begin by considering patterns in the allocation decisions of politicians when information about the schools is not provided. In Figure 7, we show the coefficients from eight different regressions of school selection on school characteristics. In each regression, we subset the data to include only maps in the relevant control group: those maps without information about that school characteristic (e.g., the

[39] Summary and coding details are in SI Section 6.

regression of school selection on school needs excludes maps with the Need Information Treatment).

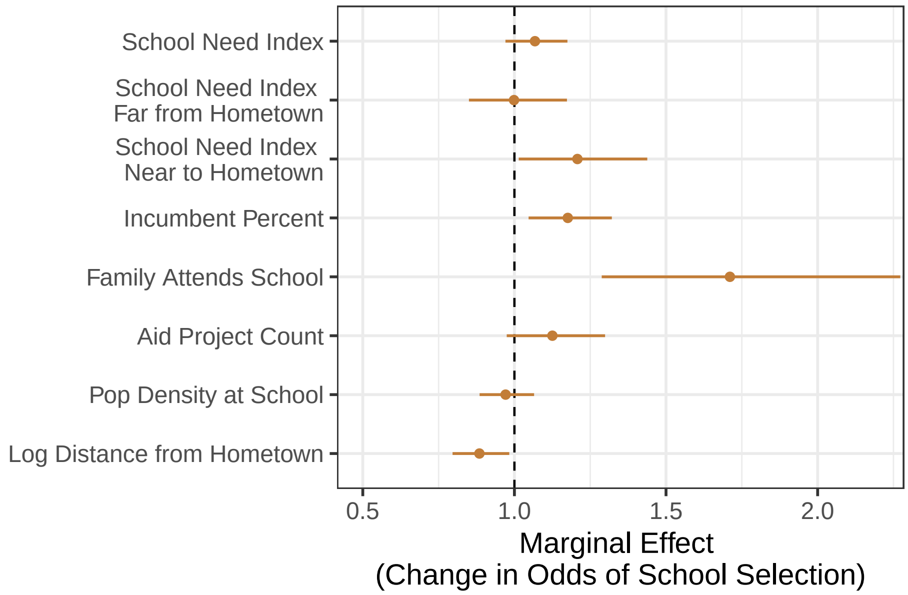
These estimates are not causally identified, however, they are consistent with our assumption that politicians prefer to spend in areas we identify as having greater need and areas that are more electorally supportive. Each standard deviation increase in *School Need Index* is associated with an increase of 1.07 in the odds that politicians select a school, though this effect is not significant (p=0.17). However, consistent with our argument about information costs, these odds increase to 1.21 when a school is a standard deviation closer than average to a politician's hometown (14km). This observations implies that politicians are most responsive to the needs of communities that are geographically proximate.

Likewise, schools in electorally supportive communities are also more likely to be selected—as we would expect if politicians weigh the electoral consequences of their actions. A standard deviation increase in percent votes for a politician (21 pp) is associated with a 1.18 increase in the odds of a school being selected.

Geographic and familial proximity are also associated with higher odds of allocation. For each standard deviation increase in distance from a politician's hometown, the odds of a school being selected decrease by 0.12. The odds that a school with a family member is selected are nearly double (1.7 times) the odds that a school without a family member is selected.

These results might seem unexpected in light of the fact that politicians score relatively low in tests of constituency knowledge. However, considered together, we think that these results are consistent with politicians relying on biased heuristics rather than government data. A politician might infer from talking to village leaders whether a community is politically supportive and that politician will be able to effectively – even if unintentionally – target based on voting patterns. Such a politician, however, might still do quite poorly in differentiating between communities that voted for her, for example, at 20% as opposed to 30%, or in identifying needs in less proximate areas.

FIGURE 7. Association between School Characteristics and School Selection



Note: This figure shows exponentiated coefficients from separate conditional logistic regressions of school selection on each variable. The sample is limited to maps that do not contain the information treatment related to each school characteristic. 95% confidence intervals are shown in the horizontal lines. Standard errors are clustered on politician. Continuous variables are normalized for comparison purposes. See SI Tables S2-S7 for tabular estimates.

Effects of Need Information

We next consider the tests of our hypotheses regarding the effects of the information treatments. We first hypothesized that the Need Information Treatment would cause politicians to be more likely to allocate to high-need schools (H1).

For ease of interpretation, we plot our treatment estimates in Figure 8. The figure shows the estimated effect of a standard deviation change in *School Need Index* on the odds a school is selected by the politician. We show effects among schools included in the Need Information Treatment group in light-colored lines. We show effects among schools omitted from the Need Information Treatment group in dark-colored lines. The p-values on the left show the probability the effect of the Need Information Treatment is consistent with a null effect. Coefficients are presented in Table 2.

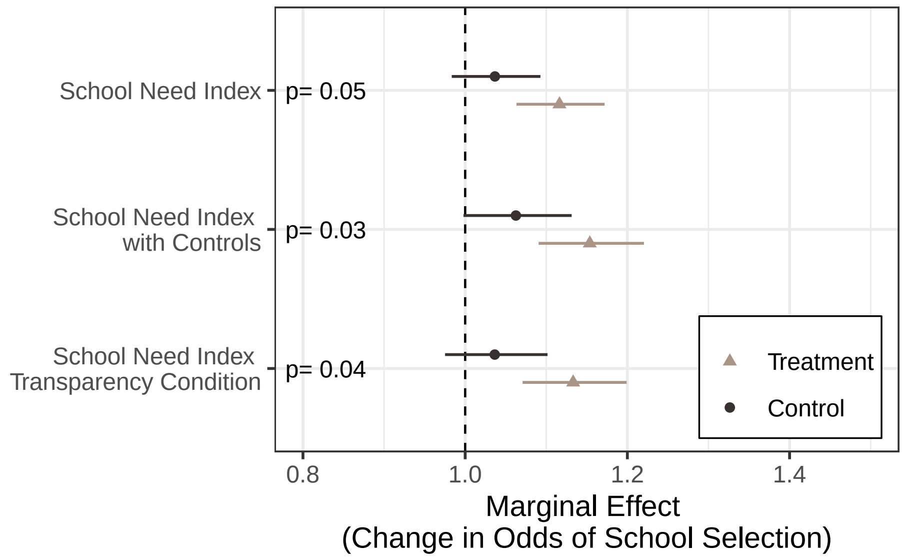
The results are broadly consistent with our hypotheses. A standard deviation increase in *School*

Need Index increases the odds of a school being selected by 1.04 in control and 1.12 in treatment, for a net treatment effect of the Need Information Treatment of 0.08 ($p = 0.05$).[40] These effects imply a potentially large shift in the allocation of resources among politicians with better information. In within-sample predictions, we estimate that politicians are about 13% more likely to select a school in the highest quartile of the *School Need Index* when they are exposed to need information.

We fail to see evidence that treatment effects are significantly larger among politicians in the combined transparency treatment (H4), or in the individual Donor Report and Radio Broadcast transparency arms. We discuss in the Conclusion some potential explanations for the weak effect of transparency.

[40] Here we report uncorrected p-values for each of our hypotheses. In SI Section 4.2, we show our estimates after correcting for multiple comparisons within each family of hypotheses. The p-values on our treatment effects are larger after these corrections. However, particularly in specifications with controls, p-values on H1 and H2 remain near 0.10 (0.05 in a one-tailed test) after correction.

FIGURE 8. Effects of Need Information on School Selection



Note: Circles indicate estimated effects of *School Need Index* on the odds of a school being selected in the control group (those appearing on maps without the Need Information Treatment). Triangles indicate estimated effects in the treatment group (those appearing on maps with the Need Information Treatment). Horizontal lines indicate 95% confidence intervals. The p-values on the left indicate the probability our treatment estimate is consistent with a null effect. For estimates in tabular form, see SI Tables S8-S9.

TABLE 2. The Effect of School Need Information on School Selection

	(1)	(2)
Need Treatment* School Need Index	0.074**	0.082**
	(0.038)	(0.039)
School Need Index	0.036	0.061*
	(0.027)	(0.031)
Controls	No	Yes
N Maps	1164	1164
N Schools	3492	3492
Pseudo-R^2	0.005	0.020

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. See SI 3.3, Table S8 for complete model results.

Effects of Aid Information

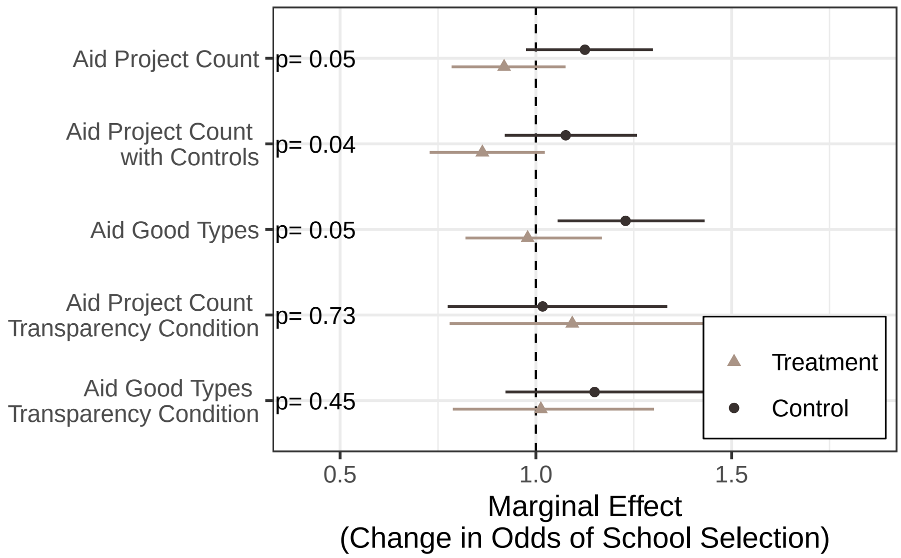
We hypothesized that the Aid Information Treatment would cause politicians to be less likely to select schools with more foreign aid projects or more types of aid goods. Our estimates in Figure 9 and Table 3 are consistent with this "crowding out" effect. On average, Aid Information decreases the odds of a school with one foreign aid project being selected by 0.21 (p = 0.05).[41] In our sample, we estimate that receiving the Aid Information Treatment reduces the probability of allocating to a school with at least one aid project by 8%.

In addition to the number of aid projects, we consider the effect of information about the number of donor-provided goods types (*Aid Good Types*). We find that when a politician learns from the Aid Information Treatment that there are three types of goods being delivered by donors at a school (the average is 2.6), the odds of the politician allocating to that school decrease by 0.91 (p = 0.05).

We see little evidence of a different treatment response for politicians in the Transparency Treatment group.

[41] On average, schools have 0.9 aid projects.

FIGURE 9. Effects of Aid Information on School Selection



Note: Circles indicate estimated effects of *Aid Project Count* or *Aid Good Types* on the odds of a school being selected in the control group (those appearing on maps without the Aid Information Treatment). Triangles indicate estimated effects in the treatment group (those appearing on maps with the Aid Information Treatment). Horizontal lines indicate 95% confidence intervals. The p-values on the left indicate the probability our treatment estimate is consistent with a null effect. For estimates in tabular form, see SI Tables S10-S11.

TABLE 3. The Effect of Foreign Aid Information on School Selection

	(1)	(2)	(3)	(4)
Aid Treatment* Aid Project Count	−0.203*	−0.220**		
	(0.113)	(0.115)		
Aid Project Count	0.118	0.073		
	(0.079)	(0.083)		
Aid Treatment* Aid Good Types			−0.227*	−0.239*
			(0.120)	(0.122)
Aid Good Types			0.206***	0.165**
			(0.086)	(0.089)
Controls	No	Yes	No	Yes
N Maps	1164	1164	1164	1164
N Schools	3492	3492	3492	3492
Pseudo-R^2	0.001	0.019	0.002	0.019

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. See SI 3.3, Table S10 for complete model results.

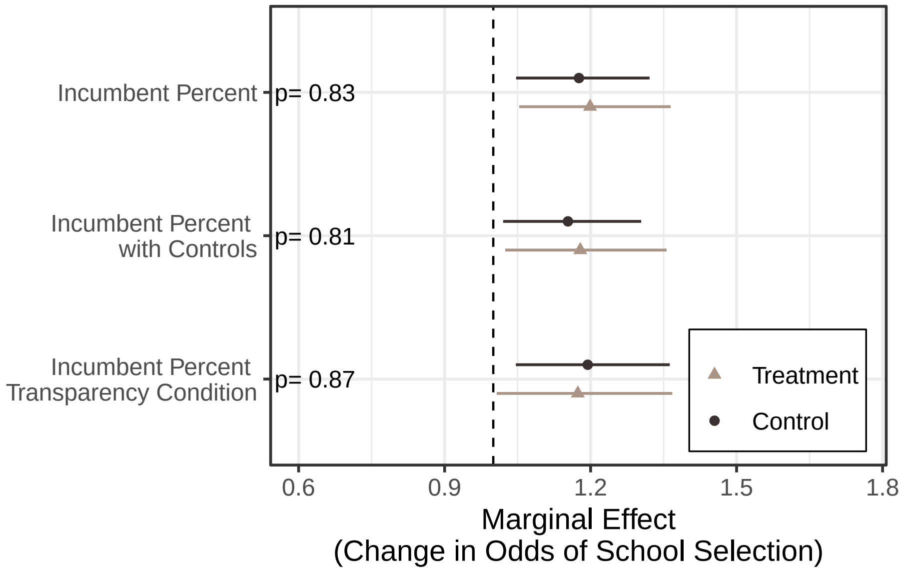
Effects of Voting Information

Finally, in Figure 10 and Table 4, we consider the effects of the Voting Information Treatment. We see little evidence that information about voting changes politicians' allocation decisions (H3).

These weak effects are somewhat surprising given the low-level of knowledge about voting that politicians exhibited in our knowledge quiz (Figure 3). One possible explanation is that politicians can obtain voting information at low cost from party and election politicians, which would imply the Voting Information Treatment did not fill any unmet demand. Alternatively, information about voting in 2014 may have been too stale to be useful due to rapidly changing voter preferences (most councillors ended up losing their seats in 2019). In SI Section 4.8, we consider a number of sub-group interactions in an attempt to distinguish between these explanations. Among other things, we evaluate whether treatment effects differ when politicians anticipate contesting elections or when they have more experience in the constituency. We do not see effects inconsistent with the null hypothesis in any subgroup.

A final possibility is that there are other unmeasured ways in which politicians are incorporating voting information. It is difficult to rule this out entirely; however in SI Section 4.6 we consider if information causes more targeting of marginal communities, as one would expect if politicians were targeting swing voters. We do not see evidence consistent with this alternative.

FIGURE 10. Effects of Voting Information on School Selection



Note: Circles indicate estimated effects of *Percent Votes* on the odds of a school being selected in the control group (those appearing on maps without the Voting Information Treatment). Triangles indicate estimated effects in the treatment group (those appearing on maps with the Voting Information Treatment). Horizontal lines indicate 95% confidence intervals. The p-values on the left indicate the probability our treatment estimate is consistent with a null effect. For estimates in tabular form, see SI Tables S12-S13

TABLE 4. The Effect of Political Information on School Selection

	(1)	(2)
Voting Treatment* Incumbent Percent	0.019	0.022
	(0.090)	(0.091)
Incumbent Percent	0.162***	0.143**
	(0.065)	(0.069)
Controls	No	Yes
N Maps	1161	1161
N Schools	3482	3482
Pseudo-R^2	0.004	0.019

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results are in SI 3.3, Table S12.

Heterogeneous Effects by Information Costs

In H5, we posit that the effects of information vary with the costs associated with obtaining information in the absence of our treatments: the treatment effects will be greater when the costs of otherwise obtaining information are higher. We anticipate that it is particularly costly to obtain information about areas far away from the politician's home town, areas with lower population density, and areas where the politician received fewer votes.

In Figure 11, we depict the conditional average treatment effect estimates for each combination of the information treatments and the proxies for information costs. The estimated treatment effect in odds is shown on the y-axis and the percentile of the conditioning variable is shown on the x-axis.

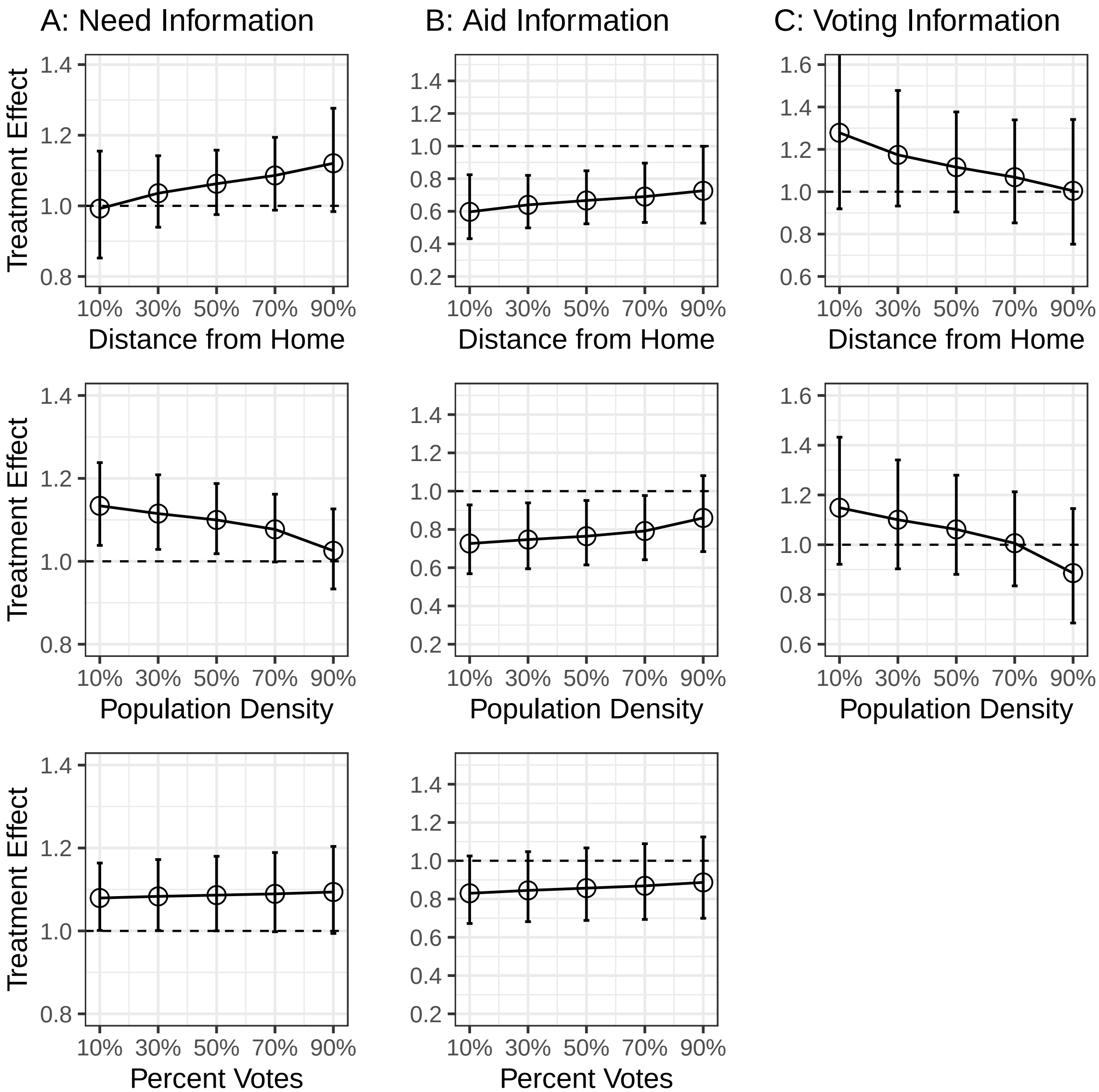
The results are mixed yet broadly consistent with H5. The effects of the Need Information Treatment are larger in communities that are farther from a politician's hometown or in areas with low population density. In communities that are at the 70th percentile of distance, for instance, we estimate the conditional average treatment effect of *School Need Index* is 1.09 times higher in treatment versus control. In contrast, in nearby communities (those around the 10th to 30th percentile of distance), we estimate conditional average treatment effects near zero. We do not see evidence that treatment effects differ for communities with more supporters of the politician.

The conditional average treatment effects of the Aid Information Treatment are mixed. While this treatment is no more likely to shift spending allocations to near or far schools, we do see a larger conditional average treatment effect in less densely populated and lower vote communities.

These different effects of the Aid Information and Need information Treatments may be due to the different ways in which politicians collect information about aid and school needs. While citizens have institutionalized means to communicate community needs to politicians, there is no similar mechanism for politicians to learn about foreign aid. It may be that politician knowledge about foreign aid is better predicted by politicians' networks with elites and development actors, as proxied by population density and political support.

Consistent with our results elsewhere, we do not see meaningfully different conditional average treatment effects for the Voting Information Treatment.

FIGURE 11. Interaction Effects of Information Treatments and Distance, Population, and Voting



Note: This figure shows conditional average treatment effects of each information treatment (in odds). In columns A, B, and C, we show the effects for Need Information, Aid Information, and Voting Information, respectively. In rows 1, 2, and 3 we show how these conditional average treatment effects vary by the school's distance from the politician's hometown, population density at the school, and the percent of votes for the politician at the nearest polling station to the school. All x-axes are shown in percentiles. For estimates in tabular form, see SI Tables S14-S16.

Evaluating Mechanisms and Generalizability

We consider several alternative explanations for our effects of information. One possibility is that our estimates are influenced by social desirability or experimenter/donor demand. While we emphasized that there were "no restrictions" on the politicians' decisions and that selected schools would be randomly selected to receive school supplies in a public lottery, some politicians still may have believed that a donor, their constituents, or the research team expected a particular decision. Relatedly, responses might be influenced by Hawthorne effects: that is, politicians may have made different decisions because they knew they were being observed.

It is difficult to rule out such effects entirely. The intention of our study was to mimic fairly typical interactions between NGOs and politicians rather than to provide information in a lab-like setting. The value of this setting is that our treatment effects are likely generalizable to similar kinds of real-world decision contexts. However, the cost is that it is difficult to identify the motivations underlying politicians' decisions. It is certainly possible that the behavior we observe is specific to the decision context and we caution against generalizing the findings to vastly different decisions: for example, those made in a legislative context.

Nonetheless, there are reasons to think that politicians' decisions were primarily motivated by the consequences of their spending decisions for constituents. First, politicians did not always allocate goods in ways that donors or NGOs would consider desirable. Politicians allocated more to political supporters and family members and often justified their decisions with reference to electoral consequences. Moreover, politicians responded to aid information in a way that is contrary to the way donors usually portray their interests. Donors often take steps to avoid exactly the kind of re-allocation of resources we observe in this experiment (Morrissey 2015). Moreover, we see no evidence from our Transparency Treatment that sharing politician decisions with donors altered decision-making. Nor did decisions differ among politicians with more interaction with donors or those who knew our partner

NGO.[42] We also think it unlikely that politicians were responding to the interests of the research team. Because implementation was done through an NGO and RAs identified themselves (honestly) as NGO representatives, it seems unlikely that politicians would align their behavior with research expectations.

Politicians' post-treatment behavior also suggests that they took the decision seriously and were motivated by concerns for their constituents. Many politicians followed up with our research team to learn details of the lottery and delivery and a number physically attended one or both. Additionally, when asked to justify their allocation decisions, only five politicians specifically mention our partner (Tearfund) and only six mention "you" (the RA). Instead many politicians refer specifically to the information provided during the experiment and justify their decision with reference to constituent needs.[43] For instance, 174 politicians outright said that they were choosing a school because it had *not* been supported by donors. That said, to the extent donor and voter preferences are aligned, it is impossible to fully eliminate the concern that donors, in addition to or instead of voters, are affecting politician decision making.

One might question whether our findings would generalize to other settings. While the decisions of politicians in our experiment might seem removed from traditional budgetary processes, politicians make these kind of budgetary decisions with donors and NGOs on a regular basis in many low-income countries. The kind of decision setting is also similar to the ways politicians make other forms of discretionary spending decisions. For instance, the allocation of constituency development funds frequently requires politicians to select among multiple competing projects and locations and make binding recommendations (Harris and Posner 2019). Still, an important caveat to our results is that we cannot say for certain that results would not differ if the decisions were over budgetary allocations or if funds came from government budgets or tax revenue. It is also possible that the preferences of the NGO (or donors in general) may be given more weight in decisions about NGO funding due to the perceptions of greater donor oversight or concerns about repercussions for "incorrect" decision making.

[42] See SI Section 4.3.

[43] See SI Section 5.1.

One might also question whether our findings would generalize to policy interventions that use alternative modes of information dissemination. One potentially important difference is that some policy interventions rely on empowering citizens to communicate with politicians rather than providing information to politicians directly (see, for example, Grossman, Humphreys and Sacramone-Lutz (2020); Gulzar, Hai and Paudel (2021)). Such interventions may cause politicians to respond based on the status of voters rather than on the type of information (Grossman and Slough 2021). Additionally the fact that we deliver highly targeted information at the point of decision making may be important. Other interventions that focus on lowering the costs of information through dashboards or regular reporting sometimes may find different effects due to higher search costs and the fact that politicians can opt-in or out of being well-informed.

Conclusion and Policy Implications

Our study establishes that providing information to in-office politicians shifts the allocation of spending. Need information increases allocation to high-need areas and aid information reduces allocation to high-aid areas. However, voting information does not affect allocations across high- and low-support areas. Information appears to have a larger effect on politician spending decisions in harder-to-access (and therefore harder to learn about) areas, such as those far from the politician's hometown or those in an area with low population density. These heterogeneous effects suggest that information gaps may explain disparities in public spending, and imply that information has the potential to reduce these disparities.

We do not find that transparency makes politicians respond differently to information. This may be because our study is relatively under-powered to detect such effects. Our power to identify the moderating effect of transparency is less than half that of our main hypotheses.[44] Another possibility is that citizens and donors are not willing to sanction politicians for ineffective allocations. But,

[44] See discussion in SI Section 4.1.

contrary to this explanation, our focus groups with citizens suggest a high degree of willingness to sanction politicians for targeting political supporters or family members. Instead, we think that a likely explanation is that citizens themselves lack sufficient information to sanction politicians. Citizens' ability to assess community needs, public spending, and foreign aid outside their own community is quite weak. Indeed, in our survey we find that only 10% of citizens were aware of anything a councillor had done outside of their own community.[45] Donors likewise often struggle to stay informed of the activities of governments (Easterly and Pfutze 2008).

Certainly we need more research and theoretical refinement to answer some of the questions we have posed. Our sample is relatively small sample and we cannot confidently rule out the null hypothesis for some of our treatments. We are especially under-powered to answer some questions about interactions across treatments, or to estimate heterogeneous effects. We think especially that there is productive work to be done to better understand the source of knowledge distortions among politicians and politicians' incentives to consume new information. Future research might also productively explore alternative types of information and modes of information delivery.

Still, from a policy perspective, our study provides evidence that programs to increase administrative capacity and lower the costs of information could have welfare benefits, especially for communities which have been marginalized in their access to government. Programs that make it easier to learn about areas of their constituency that are socially or geographically distant could be particularly effective. Likewise, the effects of foreign aid information in our study suggest that mechanisms for more substantial coordination and information dissemination between donors and government officials could improve the efficiency of policy.

Our results also suggests some reasons why policies to lower the costs of information might fail. The heterogeneity in responsiveness we document suggests that such interventions need to take context and incentives into account. Information is likely to be most effective when it aligns with officials' policy priorities, when politicians do not already have access to alternative and cheap sources of

[45] See SI Section 7.2.

information, and when officials are able to easily and immediately consume relevant information while making policy decisions. We think a useful area of future research is to explore the ways in which such programs might influence officials' demand for information. Programs which help empower marginalized citizens and civic groups with better information to sanction poor spending decisions may be a particularly effective mechanism to incentivize well-informed policy.

Data Availability Statement

Research documentation and data that support the findings of this study are openly available in the APSR Dataverse at `https://doi.org/10.7910/DVN/HS5R5S`.

Human Subjects Statement

The authors declare the human subjects research in this article was reviewed and approved by the London School of Economics Research Ethics Committee and the Malawi National Commission on Science and Technology of Malawi and certificate numbers are provided in the SI. The authors affirm that this article adheres to the APSA's Principles and Guidance on Human Subject Research.

Ethics and Conflicts of Interest Statement

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Supplemental Materials: What politicians don't know can hurt you: The effects of information on politicians' spending decisions.

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Contents

1 Overview

These Supplemental Materials (SM) are intended to provide additional information useful for understanding the experiment and the results in the main text. In addition to the material here, you can find on the associated dataverse an example survey, replication files, extended versions of some results tables, and ethics approvals.

1. Section 2 provides an illustrative model of Bayesian updating. We derive the conditions under which information improves distributional decisions and under which we can expect positive treatment effects.

2. Section 3 provides tables for all the estimates plotted in the main text.

3. Section 4 provides additional tests that might aid in understanding the results of the study, including a power analysis, multiple comparison tests, assessments of experimenter demand effects, compliance checks and interactions across treatment arms. We also consider alternative ways politicians may use voting information in their spending strategies.

4. In Section 5, we discuss evidence of learning and updating. We discuss post-treatment surveys that indicate that politicians retained information and found it useful. We also show tests of conditional treatment effects by politician knowledge of their constituencies.

5. Section 6 provides statistics on sample representativeness, attrition, variable correlations, and variable distributions and coding details.

6. Section 7 provides an overview of the pre-treatment interview protocol and a description of the survey of citizens and teachers referenced in the main text.

7. Section 8 provides a detailed description of the randomization process, example maps, details on the goods used in the experiment, and example transparency treatments.

8. Section 9 provides a discussion of the ethics of this experiment and the steps we took to ensure the protection of all research participants.

Additionally, these SM serve as a compendium of all the tests of the information treatment arms which were pre-specified in our pre-analysis plan (PAP). This pre-analysis plan was filed with EGAP on January 23, 2018 r to any analysis being undertaken. You can see the full pre-analysis plan at `https://osf.io/kazfp`. Additionally, in Section 10, we summarize all of the pre-specified hypothesis tests and where the tests can be found. Finally, we discuss deviations from the PAP in Section 11.

2 Formal Model of Information Updating

In this section, we formally derive the assumptions required for our hypotheses about information updating to hold.

In line with our experimental setup, consider a politician that has to make a decision about how to allocate a fixed development budget of value $a > 0$ to a set of schools n. In making this decision, the politician has to consider the returns (e.g., in terms of votes or welfare) to each investment, $v_i ... v_n$. We assume $v_i(a) > 0 \quad \forall i$. We define $v_i > v_{i+1}$; which implies that a completely informed politician will always prefer to spend on school one.

We represent the politician's prior beliefs about each v_i as independent random variables $\phi_i ... \phi_n$. We assume that these priors are normally distributed with means $m_i ... m_n$ variances $\sigma_i^2 ... \sigma_n^2$. To simplify the exposition, we will assume for now that $n = 2$ and that prior variance is constant ($\sigma_i^2 = \sigma_{i+1}^2$). Later we discuss the implications of relaxing these assumptions.

Let θ equal the probability that $\phi_1 > \phi_2$. θ is therefore equal to the probability that the politician obtains maximum returns to her investment, which can be represented as follows:

$$\theta = Pr(\phi_1 - \phi_2 > 0) = \Phi_0(m_2 - m_1, \sigma_1^2 + \sigma_2^2) \tag{S1}$$

Where Φ_0 is the normal CDF evaluated at 0.

From Equation S1, it follows that there is a positive relationship between information precision and accuracy and the probability of making an effective spending decision. From the properties of the normal CDF, it follows that the probability of inefficient distributional decisions (θ) is increasing in prior inaccuracy ($m_2 - m_1$). Further when priors are inaccurate ($m_2 > m_1$), the probability of an inefficient decision is increasing in total uncertainty ($\sigma_1^2 + \sigma_2^2$).

This simple model likely also underestimate the effects of uncertainty. Many models assume a negative correlation between v_j and σ_j^2. For instance, if politicians require some knowledge about a community in order to optimize the way spending is delivered, or in order to claim political credit for that spending, then politician utility is likely going to be higher where politicians have better knowledge (for discussion see, e.g., Keefer and Vlaicu 2008; Stokes 2007; Dixit and Londregan 1996). In the context of our experiment, for instance, politicians often showed up during deliveries of school goods to engage in claim some credit for the aid. Such credit claiming activities are likely only possible where politicians have close personal connections with a community.

A negative correlation between v_j and σ_j^2 implies that communities with higher information costs will be especially disadvantaged in spending decisions.

In line with our experiment, we assume that some politicians receive a treatment signal about the correct values of v_1 and v_2. We represent these signals as random variables τ_1 and τ_2. We assume these signals are normally distributed with truthful means equal to v_1 and v_2 and constant variance s^2.

If the politician updates using Bayes' rule, their posterior beliefs in the treatment condition are as follows:

$$\phi_i(\tau_i) = N[m_i + (v_i - m_i)\lambda_i, \frac{\sigma_i^2 s^2}{\sigma_i^2 + s^2}] \tag{S2}$$

Where $\lambda_i = \frac{\sigma_i^2}{\sigma_i^2 + s^2}$ equals the precision of the information signal. A politician's decision problem is to determine the probability that $\phi_1(\tau_1) > \phi_2(\tau_2) = \theta(\tau_1, \tau 2)$, which is equal to the cumulative distribution function of $\phi_2(\tau_2) - \phi_1(\tau_1)$ evaluated at zero:

$$\theta(\tau_1, \tau_2) = Pr[\phi_2(\tau_2) - \phi_1(\tau 1) > 0] = \Phi_0\{[m_2 + (v_2 - m_2)\lambda_2] - (m_1 + (v_1 - m_1)\lambda_1], \frac{\sigma_2^2 s^2}{\sigma_2^2 + s^2} + \frac{\sigma_1^2 s^2}{\sigma_1^2 + s^2}\} \tag{S3}$$

We can now derive the conditions under which information improves spending decisions ($\theta(\tau_1, \tau 2) > \theta$), and, by implication, those conditions under which treatment effects will be positive. Under reasonable assumptions, we can show that politicians will never be worse off with information than without information, and will most often be better off. Unlike in updating models with one-sided information, this conclusion does not depend upon the accuracy or ranking of a politician's priors, m_1 and m_2.

To illustrate why this is, first consider the case where a politician correctly ranks schools ($m1 > m2$). If information causes incorrect decisions, it would have to be the case that information causes a politician to switch to the school with lower returns, which would occur only if the posterior means implies higher returns for school two than school one. From Equation S3 this would imply the following would have to be true:

$$m_1 + (v_1 - m_1)\lambda_1 < m_2 + (v_2 - m_2)\lambda_2 \tag{S4}$$

Since our assumption that $v_1 > v_2$ and $m_1 > m_2$ would contradict equation S4, it follows that this can never be the case.[1]

Proposition 1 *When a politician has correct priors ($m_2 > m_1$) with consistent variance ($\lambda_1 = \lambda_2$), the probability of correctly ranking the schools will never be lower in the treatment condition than the control condition ($\theta(\tau_1, \tau 2) \not< \theta$).*

1 After simplifying, $m_1 - m_2 + \lambda v_1 - \lambda v_2 < \lambda m_1 - \lambda m_2$. Since λ is positive and bounded between zero and one, this can never be the case.

Now consider the case where a politician has an incorrect ranking ($m_2 > m_1$). If information causes worse decisions, it would have to be the case that the informative signal makes it *more* likely that a politician retains rather than switches their ranking. From Equation S3, this would imply

$$Pr[m_1 + (v_1 - m_1)\lambda_1 < m_2 + (v_2 - m_2)\lambda_2] > Pr(m_1 < m2) \tag{S5}$$

Since, by assumption, $v_1 - m_1 > v_2 - m_2$, the probability of switching to a more accurate ranking of schools is always higher in the treatment condition and this can never be the case.[2]

Proposition 2 *When a politician has incorrect priors ($m_2 > m_1$) with consistent variance ($\lambda_1 = \lambda_2$), the probability of a correct school ranking is higher in the treatment condition than the control condition ($\theta(\tau_1, \tau 2) > \theta$).*

It follows similarly that the probability that a politician switches to a more effective spending decision is greater when priors are more diffuse or when the information signal is more precise. To illustrate, note that when a politician has an incorrect ranking ($m_2 > m_1$), the probability she changes her ranking is equal to $Pr[m_1 + (v_1 - m_1)\lambda > m_2 + (v_2 - m_2)\lambda]$. Again, since $v_1 - m_1 > v_2 - m_2$, it follows that this probability is strictly increasing in λ. For this reason, we predicted in our experiment that politicians would be more responsive to information treatments when the precision of information priors are limited by high information costs (e.g., due to the costs of travel to distant schools).

Proposition 3 *When a politician has incorrect priors ($m_2 > m_1$) with consistent variance ($\lambda_1 = \lambda_2$), the probability of a correct ranking is increasing in the precision of the signal ($1/s^2$) and the variance in prior beliefs (σ_i^2).*

The conclusions above assume that the politician's priors are similarly precise for both schools ($\lambda_1 = \lambda_2$). We might doubt this is the case. As we discuss in the main manuscript, politicians are better informed about some communities than others, for instance due to the greater costs of citizen lobbying in more distant communities. Additionally, motivated reasoning or partisan bias might also motivate differences in the precision of priors (Gerber and Green, 1999).

It follows directly from Equation S3 that the change in a politician's posterior beliefs is proportional to λ_i and m_i. Therefore a primary effect of relaxing this assumption is to vary the probability that a politician updates their beliefs about a particular school. Formally, it follows from Equation S3 that:

Proposition 4 *When a politician has less precise priors about school i than school $i+1$ ($\sigma_i^2 < \sigma_{i+1}^2$), the difference between priors and posteriors will likewise be greater for school i than school $i+1$. Formally, $(m_i + (v_i - m_i)\lambda_i) - [m_{i+1} + (v_{i+1} - m_{i+1})]\lambda_{i+1} > (m_i - m_{i+1})$.*

Another implication of relaxing the constant variance assumption is that Propositions 1 and 2 will not hold under all conditions. To see this, note that equations S4 and S5 are not contradicted with certainty if we do not restrict the distribution of λ_1 and λ_2. When $\lambda_1 \neq \lambda_2$, politicians might become less likely to select school one in the treatment condition ($\theta(\tau_1, \tau_2) < \theta$). To illustrate, suppose a politician received information that causes her to update negatively ($m_1 > v_1$ and $m_2 > v_2$) but at different rates ($\sigma_1^2 > \sigma_2^2$). In such a scenario, if differential updating is considerably greater for school one than school two, then it is conceivable that a politician will switch from preferring school one to preferring school two in the treatment condition.

We refer to this scenario as negative updating. Negative updating will occur especially when the precision of priors are much greater for the second school ($\sigma_1^2 > \sigma_2^2$) and when politicians are close to indifferent in their priors ($m_1 \sim m_2$). The range of values where this occurs are narrow and occur rarely across reasonable simulations.[3]

There are also empirical reasons to discount negative updating. Negative updating requires that politicians know *less* about schools with a higher investment return. This contradicts what we show in the main manuscript (e.g., politicians know more about high need schools). Additionally, we think it unlikely that politicians who are relatively indifferent in their priors will have large differences in the precision of their beliefs. Most evidence and theory suggests instead that indifference is correlated with less confident beliefs (Feddersen and Pesendorfer, 1996; Druckman and Lupia, 2000).

We next relax the assumption that politicians are only selecting between two schools. While the logic becomes more complex, our core conclusion about the beneficial effects of information do not change. Assume, for instance, the setting in our experiment of a politician allocating across three schools with returns v_1, v_2, and v_3. Consider, first, the case of consistent priors ($m_1 > m_2 > m_3$). Here m_3 is irrelevant to the decision and the logic simplifies to the comparison between v_1 and v_2. Alternatively, a politician might have incorrect priors ($m_3 > m_2 > m_1$) or ($m_3 > m_1 > m2$). In order for $\theta > \theta(\tau_1, \tau_2, \tau_3)$, one of the following must hold:

$$Pr(m_1 + (v_1 - m_1)\lambda_1 < m_2 + (v_2 - m_2)\lambda_2) > Pr(m_1 < m2) \tag{S6}$$

$$Pr(m_1 + (v_1 - m_1)\lambda_1 < m_3 + (v_3 - m_3)\lambda_3) > Pr(m_1 < m3) \tag{S7}$$

We already ruled out the first possibility (Equation S5) and the second inequality is impossible for an identical reason: By assumption, $v_3 - m_1 > v_3 - m_1$ so this inequality cannot hold. We could make a similar argument for any set of n schools.

[2] After simplifying, $v_1 - v_2 > m_1 - m_2$. By assumption, $v_1 - v_2$ is strictly positive and $m_1 - m_2$ is strictly negative.

[3] In simulations assuming independent and uniform distributions on m_i and v_i bounded between 0 and 1, we observed negative updating less than 2% of the time and positive updating over 70% of the time.

It's important to note that this model of information updating is different in important ways from one-sided information problems that we see, e.g., in theories of voting behavior. In models and experiments of how information affects voting, voters generally only receive information about the quality of incumbents (e.g., the level of incumbent corruption). How voters respond to that information depends upon whether the information causes voters' relative ranking of incumbent and challenger to shift positively or negatively. Because the direction of this shift depends in turn on politician priors, the average effects of information, independent of priors, can be indeterminate (for discussion see Arias et al. 2018 and Izzo, Dewan and Wolton 2018). In contrast, in our setting, politicians receive information about the full set of possible schools. In this setting, and under the assumption discussed above, a politician's ranking about the optimal investment cannot shift in a direction adverse to the ranking provided in the experiment.

3 Tables Showing Estimates from Main Text Figures

In the main manuscript, we show most treatment effect estimates in coefficient plots. In this section, we show estimates in Table form for all these plots.

3.1 Figure 2

Table S1: Estimates from Main Text Figure 2

	Linear Effect	0-25 perc.	25-50 perc.	50-75 perc.	75-100 perc.
	(1)	(2)	(3)	(4)	(5)
Log Distance from Hometown	-0.095^{***}				
	(0.022)				
Intercept	0.513^{***}	0.414^{***}	0.272^{***}	0.250^{***}	0.212^{***}
	(0.058)	(0.039)	(0.042)	(0.037)	(0.029)
Observations	1,864	495	453	511	405
R^2	0.027	-0.000	0.000	0.000	0.000

Note: *p<0.1; **p<0.05; ***p<0.01

3.2 Figure 7

Table S2: Estimates from Figure 7 (School Need Index)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
School Need Index	0.066	0.107	0.091	0.011
	(0.049)	(0.063)	(0.059)	(0.087)
Observations	1,743	1,743	1,197	546
Pseudo-R^2	0.001	0.021	0.002	0.00003

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 73).

Table S3: Estimates from Figure 7 (School Need Index*Distance)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
School Need Index*Log Distance from Hometown	-0.095	-0.095	-0.133^{**}	0.057
	(0.064)	(0.064)	(0.072)	(0.146)
School Need Index	0.094	0.094	0.116^{*}	0.034
	(0.059)	(0.059)	(0.069)	(0.118)
Log Distance from Hometown	-0.092	-0.092	-0.053	-0.179
	(0.069)	(0.069)	(0.081)	(0.133)
Observations	1,287	1,287	926	361
Pseudo-R^2	0.005	0.005	0.007	0.006

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 75).

Table S4: Estimates from Figure 7 (Incumbent Votes)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Incumbent Percent	0.162***	0.162**	0.201**	0.104
	(0.065)	(0.073)	(0.084)	(0.103)
Observations	1,683	1,683	1,161	522
Pseudo-R^2	0.004	0.020	0.005	0.002

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 77).

Table S5: Estimates from Figure 7 (Family Attends School)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Family Attends School	0.537***	0.428***	0.550***	0.458
	(0.144)	(0.149)	(0.156)	(0.384)
Observations	3,492	3,492	2,439	1,053
Pseudo-R^2	0.004	0.019	0.005	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 79).

Table S6: Estimates from Figure 7 (Aid Project Count)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Aid Project Count	0.118	−0.215	0.121	0.110
	(0.079)	(0.164)	(0.094)	(0.147)
Observations	1,752	1,752	1,218	534
Pseudo-R^2	0.001	0.025	0.001	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 81).

Table S7: Estimates from Figure 7 (Population Density)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Pop Density at School	−0.030	0.105	−0.006	−0.095
	(0.049)	(0.296)	(0.059)	(0.120)
Observations	3,375	3,375	2,427	948
Pseudo-R^2	0.0001	0.021	0.00000	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 83).

3.3 Figure 8

Table S8: Estimates from Main Text Figure 8 (part 1)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Need Treatment* School Need Index	0.074**	0.082**	0.089**	0.031
	(0.038)	(0.039)	(0.046)	(0.068)
School Need Index	0.036	0.061*	0.050	0.006
	(0.027)	(0.031)	(0.033)	(0.047)
Need Treatment				
	(0.000)	(0.000)	(0.000)	(0.000)
Aid Good Types		0.364		
		(0.232)		
Aid Project Count		−0.428		
		(0.314)		
Family Attends School		0.430***		
		(0.149)		
Incumbent Percent		0.710***		
		(0.234)		
Log Enrollment		0.122***		
		(0.044)		
Log Permanent Classrooms		−0.075		
		(0.118)		
Log Permanent Houses		0.023		
		(0.062)		
Log Teachers		0.041		
		(0.101)		
Log Temporary Classrooms		−0.091		
		(0.070)		
Log Temporary Houses		0.029		
		(0.063)		
Log Turnout		−0.208**		
		(0.084)		
Opposition Percent (LC)		−0.207		
		(0.273)		
Percent Votes (MP)		0.196		
		(0.240)		
Pop Density at School		−0.003		
		(0.003)		
Observations	3,492	3,492	2,439	1,053
Pseudo-R^2	0.005	0.020	0.009	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S9: Estimates from Main Text Figure 8 (part 2)

	Transparency Interactions
Need Treatment*School Need Index*Transparency Treatment	0.065
	(0.089)
Need Treatment*School Need Index	0.024
	(0.077)
School Need Index*Transparency Treatment	−0.001
	(0.063)
Need Treatment*Transparency Treatment	
	(0.000)
School Need Index	0.037
	(0.055)
Need Treatment	
	(0.000)
Transparency Treatment	
	(0.000)
Observations	3,492
Pseudo-R^2	0.006

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

3.4 Figure 9

Table S10: Estimates from Main Text Figure 9 (part 1)

	All Surveys	with Controls	Alternate Coding	Councillors	MPs
	(1)	(2)	(3)	(4)	(5)
Aid Treatment*Aid Project Count	-0.203^{*}	-0.220^{**}		-0.372^{***}	0.164
	(0.113)	(0.115)		(0.136)	(0.206)
Aid Project Count	0.118	0.073		0.121	0.110
	(0.079)	(0.083)		(0.094)	(0.147)
Aid Treatment*Aid Good Types			-0.227^{*}		
			(0.120)		
Aid Good Types			0.206^{***}		
			(0.086)		
Aid Treatment					
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Aid Project Count		0.424^{***}			
		(0.149)			
Family Attends School		0.723^{***}			
		(0.234)			
Incumbent Percent		0.118^{***}			
		(0.044)			
Log Enrollment		-0.060			
		(0.118)			
Log Permanent Classrooms		0.031			
		(0.062)			
Log Permanent Houses		0.063			
		(0.101)			
Log Teachers		-0.086			
		(0.070)			
Log Temporary Classrooms		0.029			
		(0.063)			
Log Temporary Houses		-0.227^{**}			
		(0.084)			
Log Turnout		-0.175			
		(0.273)			
Opposition Percent (LC)		0.201			
		(0.240)			
Percent Votes (MP)		-0.003			
		(0.003)			
Pop Density at School		0.104^{***}			
		(0.024)			
Observations	3,492	3,492	3,492	2,439	1,053
Pseudo-R^2	0.001	0.019	0.002	0.003	0.004

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S11: Estimates from Main Text Figure 9 (part 2)

	Transparency Interactions
Aid Treatment*Aid Project Count*Transparency Treatment	−0.359
	(0.265)
Aid Treatment*Aid Project Count	0.072
	(0.231)
Aid Project Count*Transparency Treatment	0.141
	(0.177)
Aid Treatment*Transparency Treatment	
	(0.000)
Aid Project Count	0.017
	(0.150)
Aid Treatment	
	(0.000)
Transparency Treatment	
	(0.000)
Observations	3,492
Pseudo-R^2	0.001

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

3.5 Figure 10

Table S12: Estimates from Main Text Figure 10 (part 1)

	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)
Voting Treatment*Incumbent Percent	0.019	0.022	−0.040	0.116
	(0.090)	(0.091)	(0.115)	(0.149)
Incumbent Percent	0.162***	0.143**	0.201**	0.104
	(0.065)	(0.069)	(0.084)	(0.103)
Voting Treatment				
	(0.000)	(0.000)	(0.000)	(0.000)
Aid Good Types		0.357		
		(0.232)		
Aid Project Count		−0.421		
		(0.313)		
Family Attends School		0.427***		
		(0.149)		
Log Enrollment		0.125***		
		(0.044)		
Log Permanent Classrooms		−0.063		
		(0.118)		
Log Permanent Houses		0.024		
		(0.062)		
Log Teachers		0.032		
		(0.102)		
Log Temporary Classrooms		−0.099		
		(0.070)		
Log Temporary Houses		0.026		
		(0.063)		
Log Turnout		−0.242***		
		(0.088)		
Opposition Percent (LC)		−0.181		
		(0.273)		
Percent Votes (MP)		0.198		
		(0.240)		
Pop Density at School		−0.003		
		(0.003)		
School Need Index		0.106***		
		(0.024)		
Observations	3,482	3,482	2,429	1,053
Pseudo-R^2	0.004	0.019	0.004	0.005

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S13: Estimates from Main Text Figure 10 (part 2)

	Transparency Interactions
Voting Treatment*Incumbent Percent*Transparency Treatment	−0.149
	(0.211)
Voting Treatment*Incumbent Percent	0.132
	(0.184)
Incumbent Percent*Transparency Treatment	0.065
	(0.153)
Voting Treatment*Transparency Treatment	
	(0.000)
Incumbent Percent	0.112
	(0.134)
Voting Treatment	
	(0.000)
Transparency Treatment	
	(0.000)
Observations	3,482
Pseudo-R^2	0.004

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

3.6 Figure 11

Table S14: Estimates from Main Text Figure 11 (Need Interactions)

	Distance Interactions	Density Interactions	Voting Interactions
	(1)	(2)	(3)
Need Treatment*Log Distance from Hometown*School Need Index	0.048		
	(0.047)		
Need Treatment*Incumbent Percent*School Need Index			0.022
			(0.039)
Need Treatment*Pop Density at School*School Need Index		−0.104*	
		(0.065)	
Need Treatment*School Need Index	0.057	0.073*	0.073*
	(0.045)	(0.039)	(0.038)
Need Treatment*Log Distance from Hometown	−0.070		
	(0.097)		
Need Treatment*Pop Density at School		−0.084	
		(0.139)	
Need Treatment*Incumbent Percent			−0.109
			(0.091)
Log Distance from Hometown*School Need Index	−0.052		
	(0.035)		
Pop Density at School*School Need Index		0.159***	
		(0.054)	
Incumbent Percent*School Need Index			−0.019
			(0.028)
Need Treatment			
	(0.000)	(0.000)	(0.000)
School Need Index	0.052	0.044	0.038
	(0.033)	(0.028)	(0.027)
Incumbent Percent			0.230***
			(0.065)
Log Distance from Hometown	−0.093		
	(0.069)		
Pop Density at School		−0.075	
		(0.096)	
Observations	2,612	3,375	3,482
Pseudo-R^2	0.009	0.011	0.010

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S15: Estimates from Main Text Figure 11 (Aid Interactions)

	Distance Interactions	Density Interactions	Voting Interactions
	(1)	(2)	(3)
Aid Treatment*Log Distance from Hometown*Aid Project Count	0.077		
	(0.092)		
Aid Treatment*Incumbent Percent*Aid Project Count			0.110
			(0.084)
Aid Treatment*Pop Density at School*Aid Project Count		0.174	
		(0.124)	
Aid Treatment*Aid Project Count	-0.412^{***}	-0.232^{**}	-0.206^{*}
	(0.131)	(0.115)	(0.114)
Aid Treatment*Log Distance from Hometown	-0.120		
	(0.098)		
Aid Treatment*Pop Density at School		0.147	
		(0.128)	
Aid Treatment*Incumbent Percent			0.098
			(0.091)
Log Distance from Hometown*Aid Project Count	-0.117		
	(0.072)		
Pop Density at School*Aid Project Count		-0.053	
		(0.086)	
Incumbent Percent*Aid Project Count			0.028
			(0.058)
Aid Treatment			
	(0.000)	(0.000)	(0.000)
Aid Project Count	0.181^{**}	0.147^{**}	0.127^{*}
	(0.094)	(0.080)	(0.080)
Incumbent Percent			0.126^{**}
			(0.063)
Log Distance from Hometown	-0.061		
	(0.074)		
Pop Density at School		-0.094	
		(0.090)	
Observations	2,612	3,375	3,482
Pseudo-R^2	0.008	0.002	0.007

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S16: Estimates from Main Text Figure 11 (Voting Interactions)

	Distance Interactions	Density Interactions
	(1)	(2)
Voting Treatment*Log Distance from Hometown*Incumbent Percent	−0.095	
	(0.094)	
Voting Treatment*Pop Density at School*Incumbent Percent		-0.268^{*}
		(0.161)
Voting Treatment*Incumbent Percent	0.117	0.003
	(0.109)	(0.096)
Voting Treatment*Log Distance from Hometown	0.175	
	(0.102)	
Voting Treatment*Pop Density at School		−0.173
		(0.129)
Log Distance from Hometown*Incumbent Percent	−0.019	
	(0.065)	
Pop Density at School*Incumbent Percent		0.032
		(0.121)
Voting Treatment		
	(0.000)	(0.000)
Incumbent Percent	0.117^{*}	0.175^{***}
	(0.079)	(0.070)
Log Distance from Hometown	-0.171^{**}	
	(0.073)	
Pop Density at School		0.010
		(0.090)
Observations	2,602	3,365
Pseudo-R^2	0.009	0.007

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

4 Additional Analysis

4.1 Power Analysis

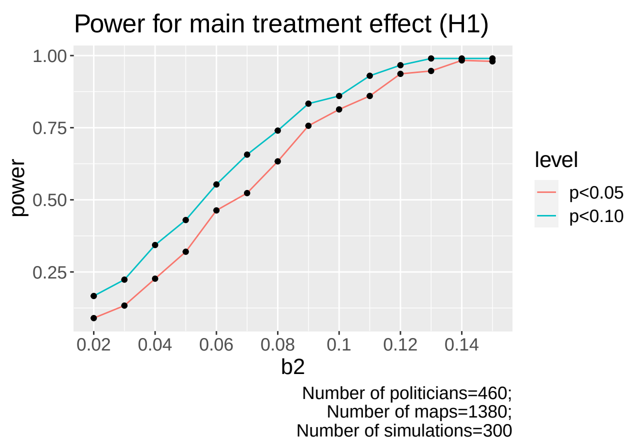
One possible reason why we cannot reject the null for some hypotheses is that our sample size is necessarily limited by the number of politicians in Malawi. This limitation on sample size makes it particularly hard to rule out the null for hypotheses that require multiple interactions (e.g., our transparency effect).

To aid in interpreting our treatment effects, in this section we show simulations of the statistical power of the study as designed. For each power simulation, we repopulate our dataset by sampling from the true distribution of schools and politicians. We then randomly assign treatment using simple randomization at the map and politician level.[4]

The results of this simulation are shown in Figures S1 and S2. For main effects (H1-H3), we obtain 80% power assuming a true normalized treatment effect (in log odds) of 0.09; which is equivalent to about a 50% increase on our expected baseline effect of z on y (0.06). While it is difficult to derive precise priors on treatment effects for a study like this one, we think these assumptions are reasonable given the low baseline levels of knowledge in our sample.

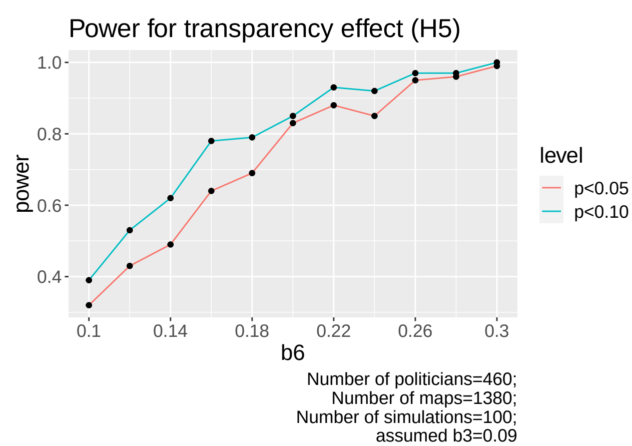
Our power to identify interactions is lower. We estimate that the power to identify significant interaction effects for the transparency arm (H4) is less than half that of our main treatment effects. We obtain 80% power assuming that the interaction is 2x the main treatment effect (at 0.18). Small treatment effects is a reasonable explanation for our inability to reject the null on H5.

Figure S1



Note: This figure shows the expected power of our study (y-axis) to rule out the null for H1 under different assumptions about the true treatment effect (x axis).

Figure S2



Note: This figure shows the expected power of our study (y-axis) to rule out the null for H5 under different assumptions about the true treatment effect (x axis).

[4] This is somewhat conservative since the transparency treatment was blocked on politician characteristics.

4.2 Multiple Comparisons Adjustments

In the main manuscript, we report uncorrected p-values for each of our hypotheses about the effects of information. It is possible that these over-state the overall evidence in favor of our hypotheses since they do not consider the multiplicity of hypotheses associated with each treatment arm. Here we show how our estimates differ after correcting for the false-discovery rate.

In our pre-analysis plan we proposed three families of hypotheses about the main effects of need information, foreign aid information, and political information. In our pre-analysis plan we also proposed additional hypothesis families which explore the ways in which the treatment might interact with different sub-groups. Since these are mostly intended to decompose the main treatment effects in order to evaluate mechanisms, these violate the assumptions of a standard false discovery rate correction and we do not include corrections for these families of hypotheses.

Following our pre-analysis plan, we adjust for the false discovery rate within each pre-registered family of hypotheses using the Benjamini-Hochberg correction; which generally has greater power relative to comparable methods (Benjamini and Hochberg, 1995). For comparison, we also show estimates using the more conservative Bonferroni adjustment. For consistency, we show estimates using two-tailed hypotheses for both directional and non-directional hypotheses.

To summarize the findings of this analysis, in Figure S3 we show how the p-values on our main hypotheses vary under alternative assumptions about multiple comparison, sample size and control variables. Below we clarify exactly how each multiple comparison test was executed and which hypotheses were included. We also show corrected p-values for all hypotheses within each family. As Figure S3 illustrates, the p-values on our treatment effects are larger after these corrections. However, particularly in specifications with controls, p-values on H1 and H2 remain near 0.10 (0.05 in a one-tailed test) after correction.

In Table S17 we show adjusted estimates for the need information treatment. In our pre-analysis plan, we proposed three main hypotheses of the effects of need information.[5] These hypotheses are listed in Table S17 as we originally formulated them in the pre-analysis plan. After adjusting for the multiplicity of hypotheses, we see stronger evidence in favor of a null hypothesis ($p = 0.15$ and $p = 0.1$). It is worth remembering however that our predictions for need information are directional, so these two-tailed tests may overstate the evidence in favor of a null.

In Table S18 we show adjusted estimates for the aid information treatment. In our pre-analysis plan, we only proposed one main hypothesis for the average effect of the aid information treatment (H1). However we also proposed that treatment effects might differ depending upon the frequency of donor interaction and the characteristics of the school (H2-H4).[6] Since H2-H4 are intended to decompose the main treatment effect, a standard multiple comparison correction is not appropriate or informative.[7] However, to remain as consistent as possible to our pre-specified approach, we nonetheless estimate corrected p-values. We show adjusted p-values both for the effects of treatment on the number of aid categories at a school (columns 2-4) and for the number of past aid projects (columns 5-7). The adjusted p-value estimates for H1 remain near conventional significance levels ($p = 0.12$ and $p = 0.09$).

In Table S19 we show adjusted estimates for the political support information treatment. In our pre-analysis plan, we proposed two main hypotheses of the effects of political information.[8] After adjusting for the multiplicity of hypotheses, the adjusted p-values for the main effects are above typical levels of statistical significance.

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5 These hypotheses are referred to as HB1-HB3 in the pre-analysis plan.

6 These hypotheses are referred to as HD1-HD5 in the pre-analysis plan. Note that HD1 and HD3 refer to the same estimate with different hypothesized signs. Since we rely on two-tailed tests throughout, we can combine these two hypotheses in this table.

7 Note that H2-H4 are not hypotheses about the treatment, but rather hypotheses about whether treatment effects differ across sub-groups.

8 These hypotheses are referred to as HC1-HC2 in the pre-analysis plan.

Figure S3: Multiple Comparison Adjustments

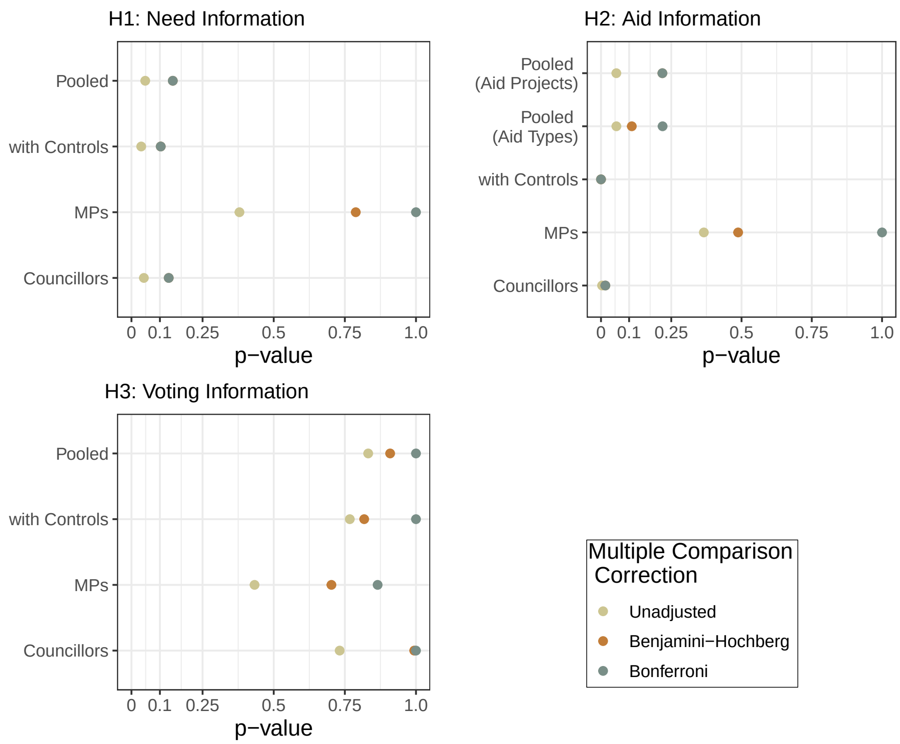


Table S17: Multiple Comparison Adjustment, School Need Information

Hypothesis	Unadjusted	BH	Bonferroni	Unadjusted with controls	BH with controls	Bonferroni with controls
H1. Politicians will be more likely to allocate to schools in areas with high need.	0.0484	0.1453	0.1453	0.0343	0.1029	0.1029
H2. Politicians will be be more likely to allocate to schools located in areas with higher support in the last election.	0.2247	0.3371	0.6741	0.3138	0.4707	0.9415
H3. Politicians will be less likely to allocate to schools located in their home community or where family members attend.	0.5241	0.5241	1.0000	0.9614	0.9614	1.0000

Table S18: Multiple Comparison Adjustment, Foreign Aid Information

Hypothesis	Unadjusted Aid Types	BH Aid Types	Bonferroni Aid Types	Unadjusted Aid Projects	BH Aid Projects	Bonferroni Aid Projects
H1. Politicians will be more likely to allocate to schools that have already benefitted from more past aid projects and where donors have provided more categories of goods.	0.0548	0.1096	0.2191	0.0546	0.2184	0.2184
H2. Treatment effect will be greater when politicians interact frequently with donors.	0.7903	0.7903	1.0000	0.5043	0.5043	1.0000
H3. Treatment effect will be greater where the politician did not receive a high proportion of votes.	0.0295	0.1096	0.1178	0.1630	0.3260	0.6521
H4. Treatment effect will be greater where schools are less needy.	0.2661	0.3549	1.0000	0.4279	0.5043	1.0000

Table S19: Multiple Comparison Adjustment, Political Support Information

Hypothesis	Unadjusted	BH	Bonferroni	Unadjusted with Controls	BH with Controls	Bonferroni with Controls
H1. Politicians will be more likely to allocate to schools located in areas with higher support for the politicians in the last election.	0.8320	0.9092	1.0000	0.7674	0.8180	1.0000
H2. Politicians will be less likely to allocate to schools in areas with high need	0.9092	0.9092	1.0000	0.8180	0.8180	1.0000

4.3 Assessing Experimenter Demand and Social Desirability

As discussed in the main text, one might worry that politicians are responding to the information provided in this experiment because of social desirability. In particular, politicians may believe that donors in general or our research partner, Tearfund, in particular expects them to respond to the information in a certain way. While we cannot completely rule out this possibility, one way to explore such effects is to see if responses to the treatment vary when politicians interact more with donors, or with Tearfund.

We conduct this analysis in Tables S20, S21, and S22. Overall we see little evidence of heterogeneous treatment effects. Politicians who have worked with Tearfund or worked more frequently with other donors are not significantly more likely to respond to the information treatments.

Table S20: Treatment Effects Conditional on Donor Interaction and Tearfund Knowledge

	All Surveys	All Surveys	All Surveys
	(1)	(2)	(3)
Need Treatment* School Need Index* Frequency of Donor Interaction	−0.015		
	(0.037)		
Need Treatment* School Need Index* Heard of Tearfund		0.056	
		(0.077)	
Need Treatment* School Need Index* Worked with Tearfund			0.072
			(0.103)
Need Treatment* School Need Index	0.090*	0.041	0.063
	(0.051)	(0.059)	(0.041)
School Need Index* Frequency of Donor Interaction	0.001		
	(0.025)		
School Need Index* Heard of Tearfund		−0.017	
		(0.055)	
School Need Index* Worked with Tearfund			−0.014
			(0.069)
School Need Index	0.033	0.046	0.039
	(0.036)	(0.042)	(0.030)
Observations	3,486	3,486	3,492
Pseudo-R^2	0.005	0.006	0.006

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S21: Treatment Effects Conditional on Donor Interaction and Tearfund Knowledge

	All Surveys	All Surveys	All Surveys
	(1)	(2)	(3)
Aid Treatment* Aid Project Count* Frequency of Donor Interaction	0.067		
	(0.107)		
Aid Treatment* Aid Project Count* Heard of Tearfund		−0.195	
		(0.228)	
Aid Treatment* Aid Project Count* Worked with Tearfund			−0.107
			(0.321)
Aid Treatment* Aid Project Count	−0.269*	−0.103	−0.183
	(0.156)	(0.173)	(0.122)
Aid Project Count* Frequency of Donor Interaction	−0.068		
	(0.074)		
Aid Project Count* Heard of Tearfund		−0.081	
		(0.161)	
Aid Project Count* Worked with Tearfund			−0.134
			(0.231)
Aid Project Count	0.185*	0.165	0.136*
	(0.112)	(0.123)	(0.085)
Observations	3,486	3,486	3,492
Pseudo-R^2	0.001	0.002	0.001

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S22: Treatment Effects Conditional on Donor Interaction and Tearfund Knowledge

	All Surveys	All Surveys	All Surveys
	(1)	(2)	(3)
Voting Treatment* Incumbent Percent* Frequency of Donor Interaction	−0.005		
	(0.089)		
Voting Treatment* Incumbent Percent* Heard of Tearfund		−0.248	
		(0.183)	
Voting Treatment* Incumbent Percent* Worked with Tearfund			−0.230
			(0.241)
Voting Treatment* Incumbent Percent	0.018	0.165	0.059
	(0.128)	(0.138)	(0.100)
Incumbent Percent* Frequency of Donor Interaction	−0.050		
	(0.064)		
Incumbent Percent* Heard of Tearfund		0.073	
		(0.131)	
Incumbent Percent* Worked with Tearfund			0.165
			(0.176)
Incumbent Percent	0.215**	0.121	0.134**
	(0.095)	(0.099)	(0.071)
Observations	3,476	3,476	3,482
Pseudo-R^2	0.005	0.005	0.005

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

4.4 Compliance and Validation

We took steps to validate that respondents correctly interpreted the treatment instruments, and we pre-specified several variables that we would use to test whether issues of compliance introduce bias into our estimates. First, we conducted a test of whether respondents could correctly interpret the maps we provided. Prior to participating in our experiment, respondents' were given an example map and asked to interpret the information provided. If they could not interpret the information, respondents were given detailed instructions to make sure they could correctly interpret the maps. Only 4% failed to understand the map on the first try. Of these, 76% were LCs, who tend to have lower levels of education than MPs. Second, we asked our RAs to record (1) whether respondents requested other schools than those shown on the maps, (2) whether respondents disputed whether particular schools were in their constituency, and (3) whether the respondent requested goods other than those Tearfund was provisioning.

In Table S23, S24 and S25 we show how our treatment effects differ across these measures. While there is some evidence of stronger treatment effects among those who understood the maps (especially in Table S24), we cannot reject the null of no difference between compliers and non-compliers.

Table S23: Treatment Effects by Compliance

	(1)	(2)	(3)
Aid Treatment* Aid Project Count* Misunderstood Maps (Q1.22)	−0.316		
	(0.339)		
Aid Treatment* Aid Project Count* Requested Other School (Q1.71)		−0.279	
		(0.733)	
Aid Treatment* Aid Project Count* Requested Other Goods (Q1.73)			0.002
			(0.758)
Aid Treatment* Aid Project Count	−0.181	−0.216*	−0.219*
	(0.126)	(0.118)	(0.118)
Aid Project Count* Misunderstood Maps (Q1.22)	0.059		
	(0.227)		
Aid Project Count* Requested Other School (Q1.71)		−0.019	
		(0.514)	
Aid Project Count* Requested Other Goods (Q1.73)			0.507
			(0.497)
Aid Project Count	−0.151	−0.148	−0.150
	(0.132)	(0.129)	(0.130)
Observations	3,492	3,492	3,492
Pseudo-R^2	0.021	0.020	0.021

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S24: Treatment Effects by Compliance

	(1)	(2)	(3)
Need Treatment* School Need Index* Misunderstood Maps (Q1.22)	−0.168		
	(0.121)		
Need Treatment* School Need Index* Requested Other School (Q1.71)		−0.006	
		(0.252)	
Need Treatment* School Need Index* Requested Other Goods (Q1.73)			−0.153
			(0.216)
Need Treatment* School Need Index	0.103**	0.082**	0.087**
	(0.042)	(0.039)	(0.040)
School Need Index* Misunderstood Maps (Q1.22)	0.241***		
	(0.088)		
School Need Index* Requested Other School (Q1.71)		−0.073	
		(0.162)	
School Need Index* Requested Other Goods (Q1.73)			0.126
			(0.155)
School Need Index	0.031	0.063*	0.057*
	(0.033)	(0.032)	(0.032)
Observations	3,492	3,492	3,492
Pseudo-R^2	0.022	0.020	0.020

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S25: Treatment Effects by Compliance

	(1)	(2)	(3)
Voting Treatment* Incumbent Percent* Misunderstood Maps (Q1.22)	−0.190		
	(0.304)		
Voting Treatment* Incumbent Percent* Requested Other School (Q1.71)		0.013	
		(0.519)	
Voting Treatment* Incumbent Percent* Requested Other Goods (Q1.73)			−0.483
			(0.551)
Voting Treatment* Incumbent Percent	0.044	0.032	0.041
	(0.097)	(0.093)	(0.093)
Incumbent Percent* Misunderstood Maps (Q1.22)	−0.048		
	(0.209)		
Incumbent Percent* Requested Other School (Q1.71)		−0.423	
		(0.402)	
Incumbent Percent* Requested Other Goods (Q1.73)			0.221
			(0.402)
Incumbent Percent	6.315	7.886	6.243
	(16.380)	(16.327)	(16.325)
Observations	3,482	3,482	3,482
Pseudo-R^2	0.020	0.021	0.020

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

4.5 Transparency Treatment Interactions

In our pre-analysis plan, we predicted similar treatment effects from the donor and radio transparency treatment arms, and to maximize power we analyse these two treatment arms together in the main text. One exception is that we anticipated that the effects of donor information would be greater in the donor transparency group (PAP HI1). In this section we evaluate this HI1 hypothesis and consider whether there are substantial differences in effects across the two arms.

In Tables S26, S27, S28 we interact each of the information treatments with each transparency treatment arm. We find no evidence that any transparency treatment conditions the effect of aid information (inconsistent with HI1).

In Table S27 we do find evidence of a larger need information treatment in the donor transparency condition;

however, the effect is null on average when we include politicians who received both the donor and radio transparency condition (as we note in the main text). It is difficult to know why politicians responded differently to combined transparency treatment, and–given our low power to identify effects across each factorial arm–we hesitate to over-interpret these effects. One possibility is that donor-transparency had an effect but that politicians discounted the donor-transparency treatment in the combined treatment arm. This could be because politicians perceived this combined intervention as being less credible or because they found it more difficult or time-consuming to understand.

Table S26: Interaction of Aid Information Treatment with Transparency Treatments

	Any Treatment	Donor Treatment	Radio Treatment	All Treatments
	(1)	(2)	(3)	(4)
Aid Project Count	0.017	0.130	0.076	0.017
	(0.150)	(0.108)	(0.113)	(0.150)
Aid Treatment*Aid Project Count	0.072	−0.173	−0.099	0.072
	(0.231)	(0.155)	(0.163)	(0.231)
Aid Project Count*Transparency Treatment	0.141			
	(0.177)			
Aid Treatment*Aid Project Count*Transparency Treatment	−0.359			
	(0.265)			
Aid Project Count*Donor Transparency		−0.026		0.136
		(0.159)		(0.229)
Aid Project Count*Aid Treatment*Donor Transparency		−0.060		−0.340
		(0.226)		(0.330)
Aid Project Count*Radio Transparency			0.084	0.239
			(0.159)	(0.219)
Aid Treatment*Aid Project Count*Radio Transparency			−0.198	−0.460
			(0.226)	(0.316)
Aid Project Count*Radio Transparency*Donor Transparency				−0.330
				(0.320)
Aid Treatment*Aid Project Count*Radio Transparency*Donor Transparency				0.520
				(0.456)
Observations	3,492	3,492	3,492	3,492
Pseudo-R^2	0.001	0.001	0.001	0.002

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S27: Interaction of Need Information Treatment with Transparency Treatments

	Any Treatment	Donor Treatment	Radio Treatment	All Treatments
	(1)	(2)	(3)	(4)
School Need Index	0.037	0.023	0.019	0.037
	(0.055)	(0.038)	(0.038)	(0.055)
Need Treatment*School Need Index	0.024	0.073	0.112^{**}	0.024
	(0.077)	(0.053)	(0.054)	(0.077)
School Need Index*Transparency Treatment	−0.001			
	(0.063)			
Need Treatment*School Need Index*Transparency Treatment	0.065			
	(0.089)			
School Need Index*Donor Transparency		0.026		−0.033
		(0.054)		(0.076)
School Need Index*Need Treatment*Donor Transparency		0.002		0.176
		(0.075)		(0.109)
School Need Index*Radio Transparency			0.034	−0.027
			(0.054)	(0.077)
Need Treatment*School Need Index*Radio Transparency			−0.074	0.092
			(0.076)	(0.107)
School Need Index*Radio Transparency*Donor Transparency				0.122
				(0.108)
Need Treatment*School Need Index*Radio Transparency*Donor Transparency				-0.336^{**}
				(0.152)
Observations	3,492	3,492	3,492	3,492
Pseudo-R^2	0.006	0.006	0.006	0.007

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S28: Interaction of Voting Information Treatment with Transparency Treatments

	Any Treatment	Donor Treatment	Radio Treatment	All Treatments
	(1)	(2)	(3)	(4)
Incumbent Percent	0.112	0.146*	0.206**	0.112
	(0.134)	(0.097)	(0.095)	(0.134)
Voting Treatment*Incumbent Percent	0.132	0.051	0.008	0.132
	(0.184)	(0.130)	(0.130)	(0.184)
Incumbent Percent*Transparency Treatment	0.065			
	(0.153)			
Voting Treatment*Incumbent Percent*Transparency Treatment	−0.149			
	(0.211)			
Incumbent Percent*Donor Transparency		0.030		0.187
		(0.131)		(0.191)
Incumbent Percent*Voting Treatment*Donor Transparency		−0.061		−0.247
		(0.181)		(0.260)
Incumbent Percent*Radio Transparency			−0.083	0.069
			(0.131)	(0.193)
Voting Treatment*Incumbent Percent*Radio Transparency			0.017	−0.162
			(0.182)	(0.261)
Incumbent Percent*Radio Transparency*Donor Transparency				−0.286
				(0.263)
Voting Treatment*Incumbent Percent*Radio Transparency*Donor Transparency				0.339
				(0.365)
Observations	3,482	3,482	3,482	3,482
Pseudo-R^2	0.004	0.004	0.004	0.005

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

4.6 Alternative Effects of Votes

In the main manuscript, we assume that politicians will prefer to target core voters in their allocation decisions. This pre-registered assumption was based on pilot experiments and focus groups. These strongly suggested that politicians would have a preference to target core supporters.

As we discuss in the main manuscript an alternative hypothesis is that politicians might target swing voters or use a mixed strategy. In this section we consider whether that might be the case.

Such a hypothesis is difficult to test. Formally, a swing voter strategy suggests that politicians should prefer to target communities where there is a high density of voters for whom $p_i \approx 0$. Lacking individual voting data, we cannot identify the density of swing voters in each school catchment areas. Like many scholars in this literature, we instead assume that the density of swing voters is likely higher in those communities which are more competitive.

Specifically, we create a variable, *Victory Margin*, which equals the percent votes received by the incumbent minus the percent votes received by the leading challenger in the last election (2014). We include specifications with and without a squared term to test the possibility that effects are non-linear. Using this variable, we re-estimate the results in Figures 7 and 10 in the main text. A swing voter hypothesis suggests the effects of *Victory Margin* on school selection should be negative. Or, at the least, the effect of *Victory Margin* should be highly non-linear, with the communities with the highest percentage of votes receiving fewer goods.

The data are not consistent with these alternative hypotheses. As shown in Table S29, the effect of *Victory Margin* is positive and significant. We can identify no non-linearities in the effect.

In Table S30, we interact *Victory Margin* with the Voting Information Treatment. Again we cannot reject the null of no treatment effect.

Table S29: Effect of Incumbent Votes on School Selection with Non-Linear Effects

	All Surveys	All Surveys	with Controls	Councillors	MPs
	(1)	(2)	(3)	(4)	(5)
Victory Margin	0.143**	0.136**	0.194**	0.176**	0.083
	(0.063)	(0.066)	(0.086)	(0.086)	(0.102)
Victory Margin Squared		−0.018	−0.030	−0.022	−0.011
		(0.050)	(0.051)	(0.061)	(0.087)
Observations	1,683	1,683	1,683	1,161	522
Pseudo-R^2	0.003	0.003	0.021	0.004	0.002

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. It is intended to assess whether there are non-linear effects of victory margin. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 25).

Table S30: The Effect of Political Information with Non-Linear Voting Effects

	All Surveys	All Surveys	with Controls
	(1)	(2)	(3)
Voting Treatment* Victory Margin	−0.006	0.014	0.015
	(0.086)	(0.090)	(0.091)
Voting Treatment* Victory Margin Squared		0.050	0.053
		(0.067)	(0.067)
Victory Margin	0.143**	0.136**	0.151**
	(0.063)	(0.066)	(0.076)
Victory Margin Squared		−0.018	−0.033
		(0.050)	(0.050)
N Maps	1161	1161	1161
N Schools	3482	3482	3482
Pseudo-R^2	0.003	0.003	0.019

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. It is intended to assess whether there are non-linear effects of victory margin. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 25).

4.7 Interactions between Information Treatments

In our pre-analysis plan, we anticipated that the information treatments might cause politicians to substitute one form of targeting for another. We specifically hypothesized that need information might cause politicians to target fewer schools where they received more votes. Conversely, we predicted that voting information might cause politicians to target fewer needy schools. In Tables S31, S32 and S33 below we consider interactions between all school characteristics and all information treatment arms. We see little evidence of interaction or substitution effects. One exception is that in Table S32 we see evidence that politicians who see the aid information treatment and the need information are especially likely to avoid spending on schools with existing foreign aid project. One possibly explanation is that need and aid information are mutually reinforcing: because donors often target larger and more populous schools, providing information on school needs can reinforce incentives for politicians to target more marginalized communities.

Table S31: Information Treatment Interactions with School Need Index

	(1)	(2)
School Need Index	0.037	0.037
	(0.038)	(0.038)
Need Treatment*School Need Index	0.071	0.115**
	(0.054)	(0.054)
Voting Treatment*School Need Index	−0.001	
	(0.054)	
Aid Treatment*School Need Index		−0.002
		(0.054)
Need Treatment*Voting Treatment*School Need Index	0.006	
	(0.076)	
Need Treatment*Aid Treatment*School Need Index		−0.079
		(0.076)
Observations	3,492	3,492
Pseudo-R^2	0.005	0.006

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S32: Information Treatment Interactions with Aid Projects

	(1)	(2)
Aid Project Count	−0.005	0.122
	(0.111)	(0.120)
Need Treatment*Aid Project Count	0.251	
	(0.160)	
Aid Treatment*Aid Project Count	0.151	−0.091
	(0.162)	(0.165)
Voting Treatment*Aid Project Count		−0.007
		(0.160)
Need Treatment*Aid Treatment*Aid Project Count	−0.695***	
	(0.228)	
Aid Treatment*Voting Treatment*Aid Project Count		−0.223
		(0.226)
Observations	3,492	3,492
Pseudo-R^2	0.004	0.002

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

Table S33: Information Treatment Interactions with Percent Votes

	(1)	(2)
Incumbent Percent	0.230**	0.128
	(0.092)	(0.092)
Need Treatment*Incumbent Percent	−0.136	
	(0.130)	
Voting Treatment*Incumbent Percent	−0.001	−0.012
	(0.129)	(0.126)
Aid Treatment*Incumbent Percent		0.069
		(0.130)
Need Treatment*Voting Treatment*Incumbent Percent	0.042	
	(0.181)	
Voting Treatment*Aid Treatment*Incumbent Percent		0.070
		(0.181)
Observations	3,482	3,482
Pseudo-R^2	0.005	0.005

Note: $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician.

4.8 Other Heterogeneous and Pre-Registered Treatment Effects

In our pre-analysis plan, we anticipated that some treatment effects would be conditioned by gender, plans to contest upcoming elections, time living in the constituency, and perceptions of the usefulness of the information in the experiment. We estimate each of these heterogeneous treatment effects in Figures S4, S5 and S6.

The effects of treatment do not differ meaningfully across most of these sub-groups. We see some evidence of stronger treatment effects among politicians that have lived longer in their constituency; though we only sampled 18 politicians who had lived in their constituency less than 10 years, so our power to identify effects by tenure is quite limited. We face similar challenges in identifying heterogeneous effects by gender since only 11% of our respondents were female.

We do see some evidence of stronger treatment effects among politicians who found the information useful (particularly for the aid information treatment). This is consistent with information updating; however it is important to note that this conditional effect is not necessarily well identified. We asked about information usefulness after the treatment, and it's plausible that beliefs about information usefulness are endogenous to treatment assignment.

Additionally, in Figure S7 we include estimates of the effect of need information *Incumbent Percent, Family Attends School* and *Distance from Hometown*. These estimates were pre-registered but not tested in the main text.

Figure S4: Heterogeneous effects of the need information treatment [9]

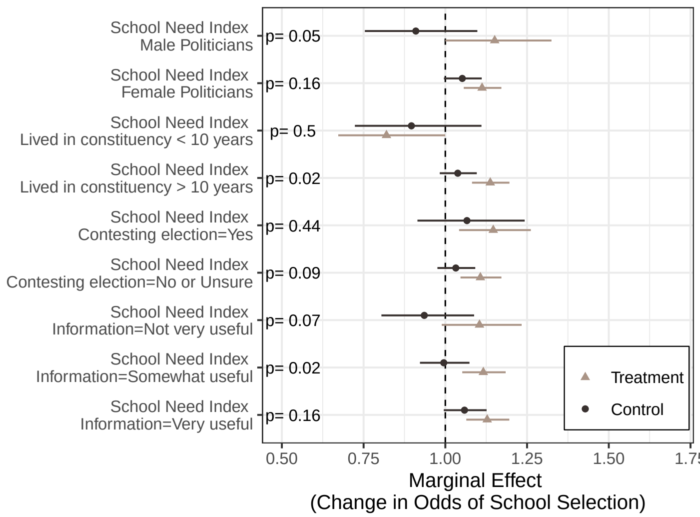
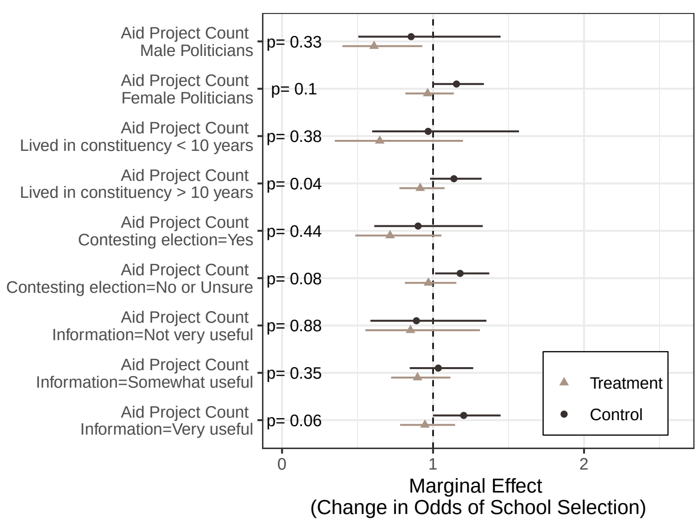


Figure S5: Heterogeneous effects of the aid information treatment[10]



[9] Tabular estimates can be found on the APSR dataverse "Replication Notes and Output.pdf" file here (Table 9).

[10] Tabular estimates can be found on the APSR dataverse "Replication Notes and Output.pdf" file here (Table 36).

Figure S6: Heterogeneous effects of the voting information treatment[11]

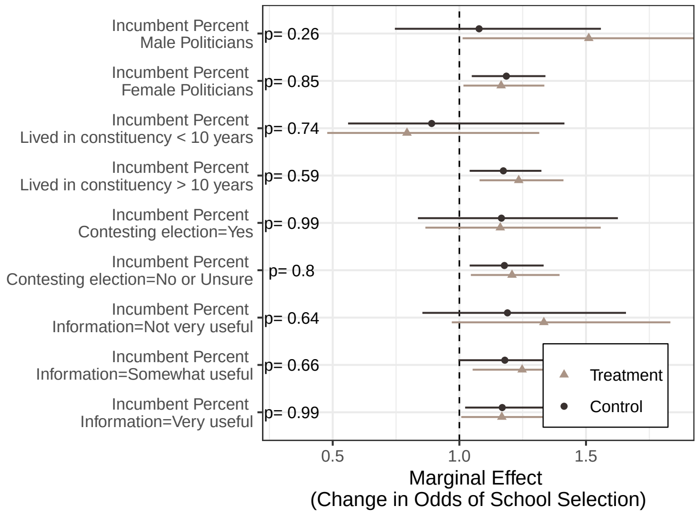
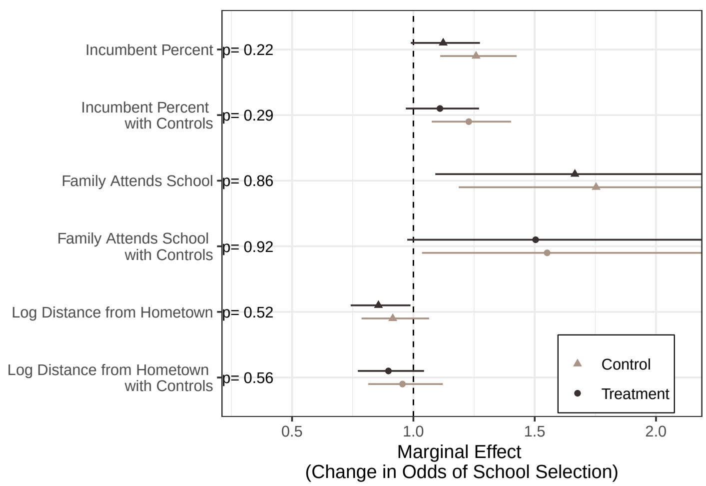


Figure S7: Additional Pre-Registered Tests of the Need Information Treatment[12]



[11] Tabular estimates can be found on the APSR dataverse "Replication Notes and Output.pdf" file here (Table 19).

[12] Tabular estimates can be found on the APSR dataverse "Replication Notes and Output.pdf" file here (Table 10).

4.9 Linear Estimates of Main Effects

In the main text, we estimate treatment effects using a conditional logit estimator, following our pre-analysis plan. We recognize however that there is some debate in the econometric literature over the possibility for bias in experimental treatment effect estimates from logistic models. In this section, we re-estimate our main treatment effects using a linear probability model.

Let $Y_{nsi} \in [0, 1]$ indicate whether politician n chooses school i in map s. Let z_{is} be the variables specific to a school i, such as whether previous donor projects have been carried out there. We can estimate the probability of selecting a given school in a set s conditional on z_{is} using the fixed effect specification in equation S8.

$$Y_{nsi} = \beta_1 z_i + \beta_2 t_s z_i + \gamma X_{is} + d_s + e_{nsi} \tag{S8}$$

Where d_s is a map-specific fixed effect. Errors are clustered on politician as in the main specification.

The results in Figures S8, S9 and S10 align closely with the results in the main text. The p-values for the estimates differ little from those in the main text. The coefficient estimates likewise align closely with probability estimates from a conditional logit specification.

Figure S8: Effects of need information treatment[13]

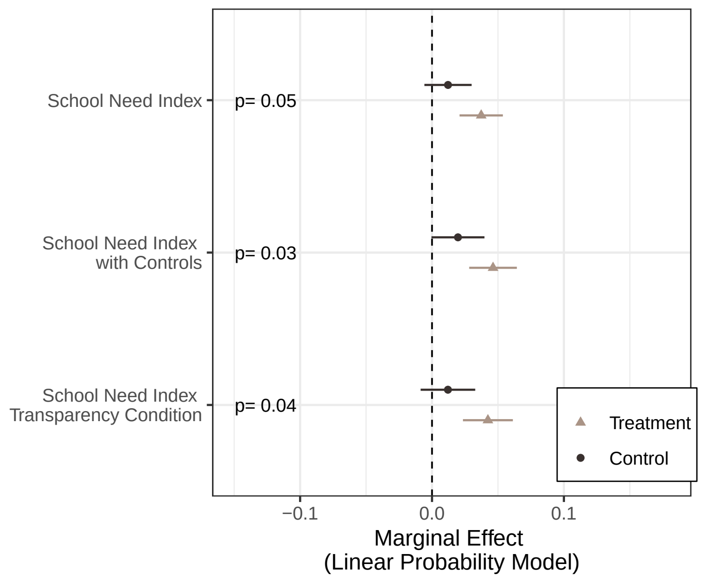
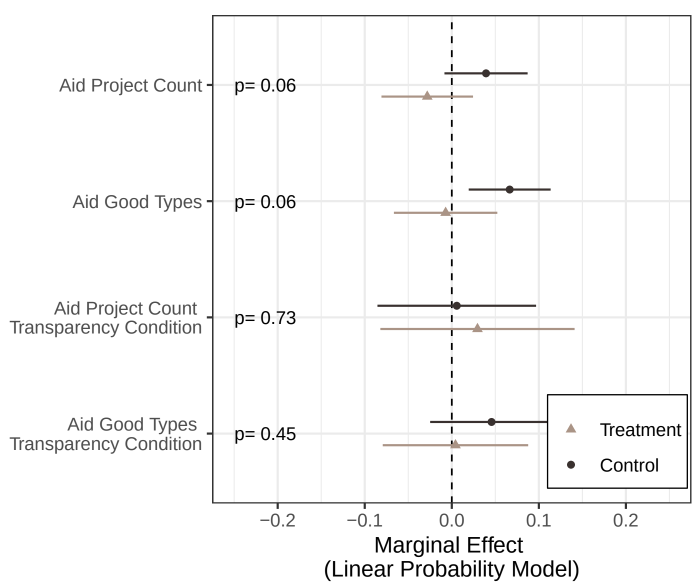
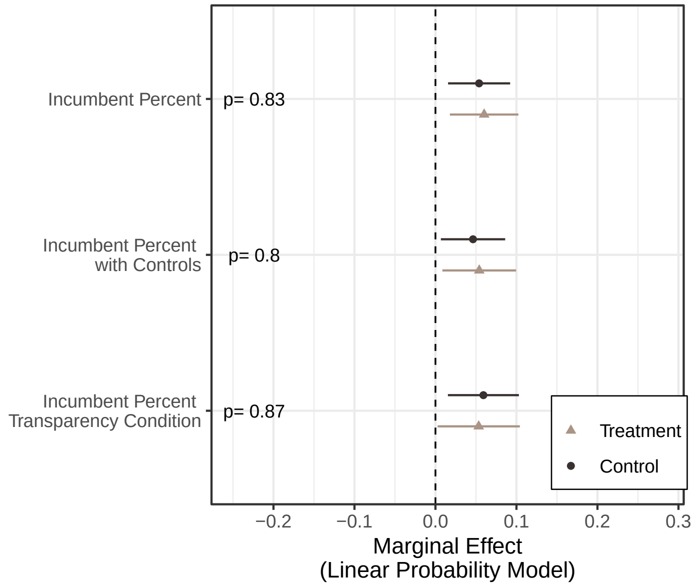


Figure S9: Effects of aid information treatment[14]



[13] Tabular estimates can be found on the APSR dataverse "Replication Notes and Output.pdf" file here (Tables 7 and 8).

Figure S10: Effects of voting information treatment[15]



[14] Tabular estimates can be found on the APSR dataverse "Replication Notes and Output.pdf" file here (Tables 34 and 35).

[15] Tabular estimates can be found on the APSR dataverse "Replication Notes and Output.pdf" file here (Tables 17 and 18).

5 Evidence of Learning and Updating

Our theory assumes that politicians update their priors from the information we provide in the experiment. To avoid priming effects, we did not measure politician's priors or posteriors directly; so it is not possible to directly test for information updating. In this section, we consider two alternative pieces of evidence.

First, we asked politicians questions about the usefulness of the information in the experiment. In Section 5.1 we show that most politicians claimed to find the information useful and were able to describe specific pieces of information that they used in their decision-making. Second, in 5.3 we look at whether treatment effects differed among those politicians who did better or worse in a quiz about their constituencies. Under the assumption that this quiz predicts politician priors about specific schools used in the experiment, this offers an alternative test of updating.

5.1 Survey Desponses on Information Usefulness and Learning

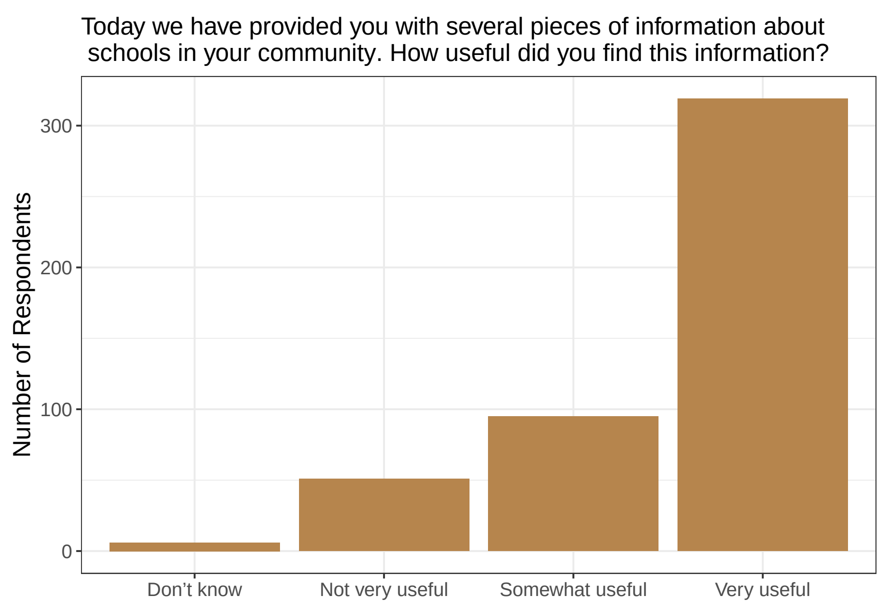
In this section we summarize politician survey and open ended responses on information usefulness and learning. These questions were asked at the conclusion of the experiment in order to assess politicians' self-assessment of the information in the study. Politicians were not able to refer back to any of the treatment information when answering these questions, so we think these questions also provide an indication of how well politicians retained the information we provided.

First, in Figure S11 we show that the modal politician found the information very useful and almost all politicians (88%) claimed to find the information very or somewhat useful.

We also asked politicians to describe how the information was useful. We summarize answers to that open ended question in Figure S12. Out of the 362 valid responses to this question, 140 specifically mentioned information in the treatments. 100 mentioned something about school needs related to the treatment. An additional 60 mentioned school facilities, enrollment numbers, foreign aid or voting.

We also directly asked politicians whether they learned something new about schools in their community (Figure S13). This may have been a more difficult question since some politicians seemed reticent to admit ignorance.[16] Nonetheless, 154 politicians admitted to learning something new in the experiment. We also asked these 154 politicians to describe what they learned. Again, almost all of these 154 politicians were able to accurately describe information provided in the treatment, such as voting, foreign aid or features of schools.

Figure S11: Information Usefulness



[16] In the question about usefulness, politicians often preferred to talk about being "reminded" rather than admitting ignorance.

Figure S12: Information Usefulness Detail

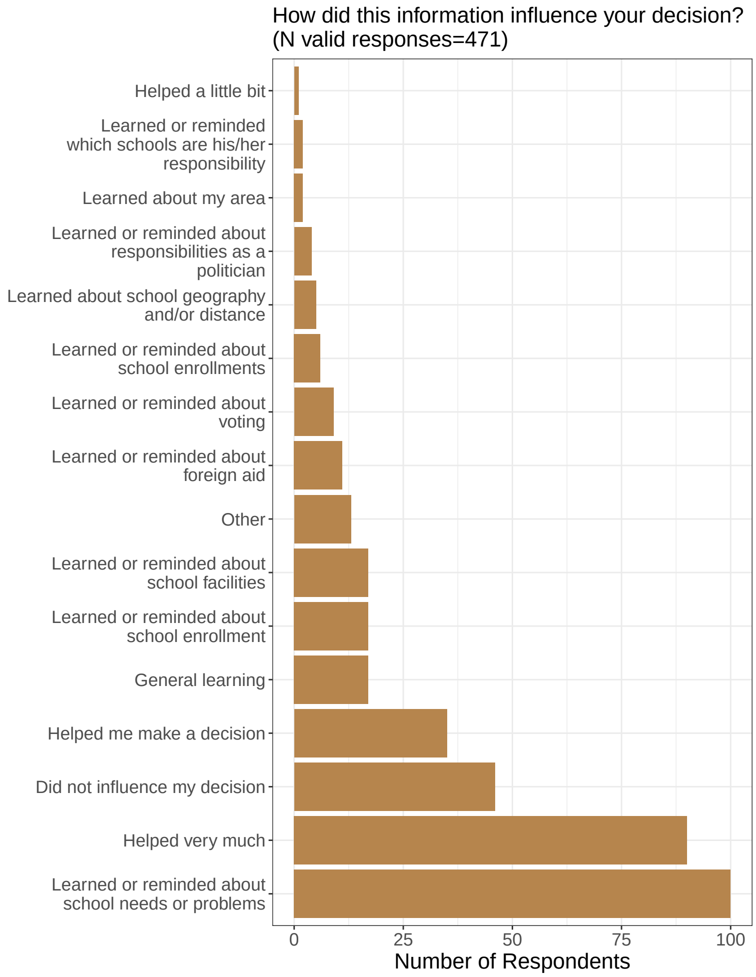
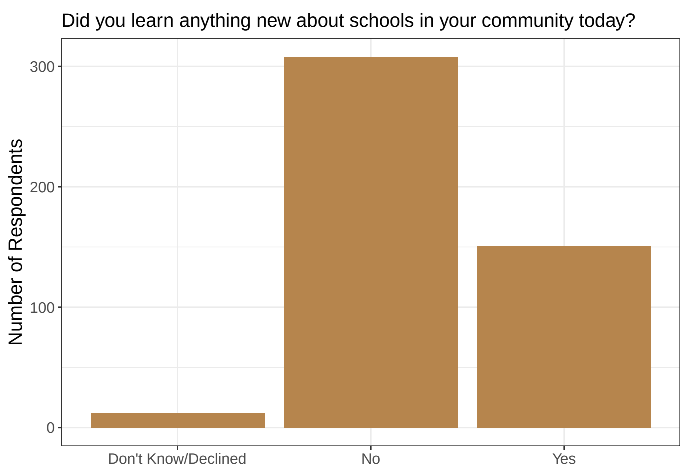


Figure S13: Information Learning

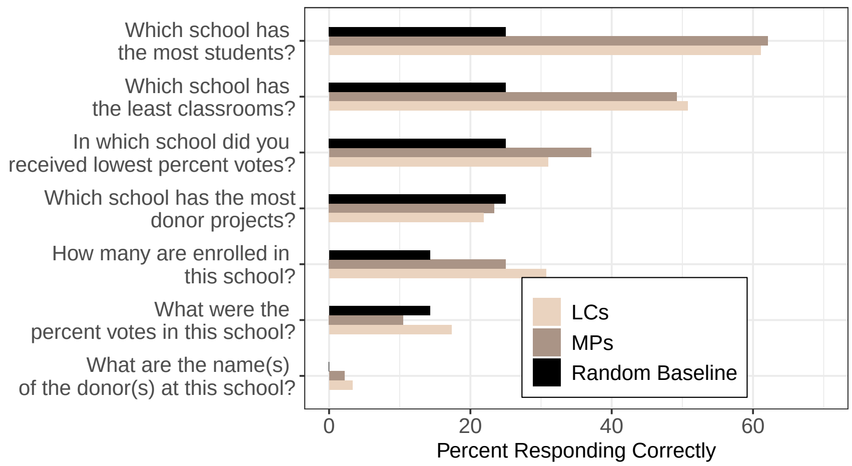


5.2 Post-Treatment Quiz Assessing Politician Knowledge

We concluded the experiment with an additional (fourth) map containing three randomly selected schools from each politician's constituency. We then asked seven, mostly multiple-choice, questions about the number of donor projects, the percent votes, or the number of classrooms or students at these schools. Since we only ask about three schools, the information provided by this quiz is incomplete. It nonetheless offers credible insights into what politicians know about their constituencies.

In Figure S14, we show the results of this quiz. Light-colored bars show the percentage of correct responses to each question. The dark bar is the expected distribution of correct answers if politicians were answering randomly. Knowledge of school characteristics was relatively high: politicians could correctly identify over half (56%) of the schools based on enrollment and classroom numbers. Knowledge of donor activities was low: only 22% could identify which school had the most foreign aid projects, which is indistinguishable from answering randomly. Less than 5% could name even one major donor who had invested at this school. Knowledge of voting was mixed: while 33% could identify the least supportive school, we find little evidence that politicians can systematically identify the share of votes they received at that school.[17]

Figure S14: School Knowledge Questions



Note: The x-axis shows the percentage of politicians responding correctly to questions about the characteristics of three randomly selected schools in their constituencies. All questions are multiple-choice except for the question on the name of the donor. The top dark line shows the expected proportion of correct answers we would expect if politicians answered randomly.

We next consider how politician knowledge varies by school. We measure the proportion of questions that a politician got correct for each of the three schools in the knowledge quiz. We then regress this proportion of correct answers on school characteristics.[18] The results are shown in Table S34. Political support and distance are particularly strong predictors of knowledge. A one standard deviation increase in the distance from a politician's hometown is associated with a 4-6 percentage point decrease in the share of correct answers (a 17-28% change relative to the mean of correct answers). A one standard deviation increase in votes for an incumbent is associated with a 9 percentage point increase in the proportion of correct answers to questions about voting patterns. Politicians appear to know less about the characteristics of schools in areas where they received more votes.[19] Politicians also appear more informed about high need schools. We see no evidence that politicians know more about populous or less impoverished areas.

[17] It is probable that knowledge of voting would have been higher nearer to the 2014 election. In a pilot study among councillors in 2015, 40% were able to identify a school based on the percentage of votes. Knowledge of school characteristics in this pilot was similar.

[18] Not all schools included in the quiz are included in this sample. While the schools included in the quiz were randomly assigned, we did not ask questions about each of the schools on the map, making the effective sample unrepresentative.

[19] One possible reason for this is that politicians were asked about the lowest vote school in the quiz. Because of this, the distribution of schools for which it is possible for a politician to show knowledge is limited to those schools which have relatively low support for a politician. Given this truncated distribution, what these data may indicate is that politicians are better identifying vote shares in communities that strongly support the opposition than in communities with more mixed levels of support, which may not be as surprising.

Table S34: Correlates of School Knowledge

	All Questions	School Questions	Voting Questions
	(1)	(2)	(3)
Log Distance from Hometown	−0.082	−0.060	0.004
	(0.052)	(0.067)	(0.084)
Incumbent Percent	−0.020	−0.009	0.099
	(0.036)	(0.040)	(0.071)
Pop Density at School	0.092	0.083	−0.117
	(0.087)	(0.102)	(0.108)
School Need Index	−0.011	−0.065	0.110^{*}
	(0.041)	(0.048)	(0.055)
School Enrollment	−0.016	−0.019	-0.251^{*}
	(0.064)	(0.075)	(0.133)
Number of Permanent Classrooms	−0.076	−0.115	0.115
	(0.066)	(0.078)	(0.103)
Poverty at School	0.008	0.034	0.066
	(0.037)	(0.045)	(0.045)
Observations	104	92	58
Pseudo-R^2	0.465	0.602	0.929

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the results of a linear fixed-effect regression of school covariates on the proportion of correct answers about each school included in the post-experiment quiz. The outcome variable is the share of correct questions about each school.

This effect of distance is consistent with our theory and politicians' self-reported behavior. Most politicians are cost and resource constrained and rely on development committees and personal networks to learn about constituency needs. This fact tends to increase the cost of getting information about more remote areas.

5.3 Heterogeneous Treatment Effects by Politician Knowledge

In this section, we assess how responses to treatment differ based upon politician responses to the quiz discussed in Section 5.2. If we assume that knowledge of these three schools used in the quiz is correlated with prior uncertainty on average, then politicians which have less certain knowledge about voting, foreign aid, and school needs should be more likely to respond to information treatments. We therefore hypothesized in our pre-analysis plan that treatment effects would be larger among politicians who do poorly in this quiz.

We create three variables to measure politician knowledge: *Aid Knowledge*, *Need Knowledge* and *Political Knowledge*. These variables equal the proportion of questions that politicians answered correctly about foreign aid, school needs and voting.

We estimate three separate models to test the effects of politician knowledge: one for each treatment and knowledge pair. Our estimation approach follows the one specified in equation 4 in the main text.

These results should be interpreted with some caution. For one, the effects of politician knowledge are not causally identified. Politicians who are well-informed are different in many respects from those who are not. Additionally, knowledge is plausibly endogenous to the treatment response. If politicians seek knowledge when their demand for knowledge is high, then knowledgeable politicians might be precisely those politicians most likely to respond to treatment.

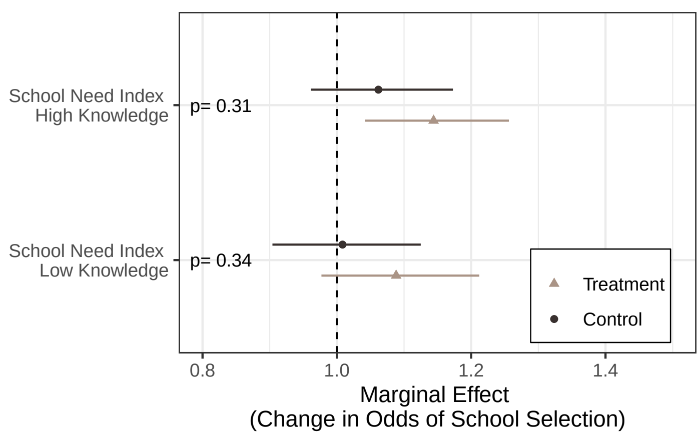
Additionally, we have limited power to identify these effects both due to the noisy nature of the measurement and our relatively small number of politicians. Our ex-ante power analysis suggests our power to identify heterogeneous effects is less than half that of our main treatment effect. Power will further differ across treatment arms due to differences in knowledge across issue areas.

In Tables S35, S36 and S37 we show results for each treatment arm. To simplify interpretation in Figures S15, S16 and S17, we also plot conditional average treatment effects for politicians at the minimum and maximum levels of knowledge.

We see mixed evidence that less knowledgeable politicians are more likely to respond to treatment. The effect of the Aid Information Treatment is larger and more precisely estimated among low-knowledge versus high-knowledge politicians. We do not see larger effects of the Need Information Treatment among low-knowledge politicians. Nor do we see evidence that the effects of the Voting Information Treatment were larger among low-knowledge politicians.

The weak effects for the Voting Information Treatment are as expected given the weak treatment effects overall. The weak effects for the Need Information Treatment are surprising, however, we think there are a couple reasonable explanations. Part of the explanation may be the fact that politicians were much less knowledgeable about foreign aid; so our sample of politicians in the low-knowledge sub-group is larger. The effects of aid knowledge might also be better identified since politicians do not have ready access to statistics on foreign aid investments. In contrast, political and need information can be collected more readily by politicians willing to pay the costs of connecting with those communities.

Figure S15: Effects of the Need Information Treatment by Need Knowledge[20]



[20] Tabular estimates are shown in Table S35.

Figure S16: Effects of the Aid Information Treatment by Aid Knowledge[21]

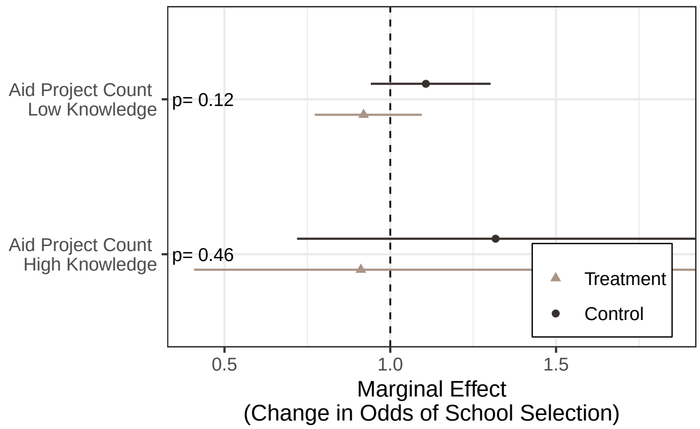
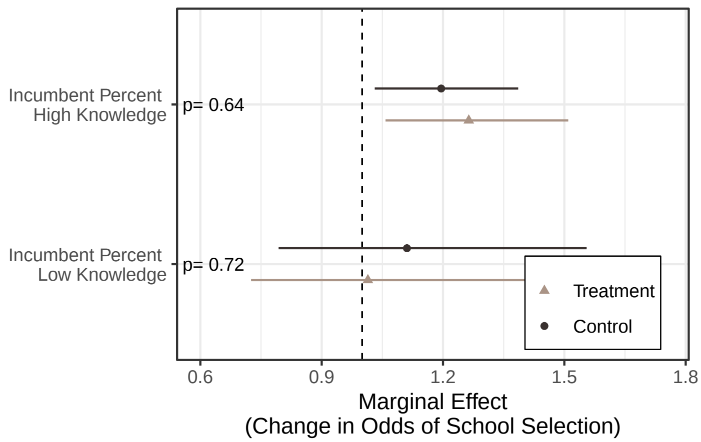


Figure S17: Effects of the Voting Information Treatment by Voting Knowledge[22]



[21] Tabular estimates are shown in Table S36.

[22] Tabular estimates are shown in Table S37.

Table S35: Interactions of Need Information Treatment with School Knowledge

	without controls	with controls
	(1)	(2)
Need Treatment*Need Knowledge*School Need Index	0.002	0.038
	(0.130)	(0.134)
Need Treatment*School Need Index	0.075	0.066
	(0.073)	(0.075)
Need Knowledge*School Need Index	−0.052	−0.091
	(0.092)	(0.096)
Need Treatment*Need Knowledge		
	(0.000)	(0.000)
School Need Index	0.060	0.104*
	(0.050)	(0.055)
Need Treatment		
	(0.000)	(0.000)
Need Knowledge		
	(0.000)	(0.000)
Observations	3,492	3,492
Pseudo-R^2	0.006	0.020

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 5).

Table S36: Interactions of Aid Information Treatment with Aid Knowledge

	without controls	with controls
	(1)	(2)
Aid Treatment*Aid Knowledge*Aid Project Count	−0.183	−0.442
	(0.584)	(0.594)
Aid Treatment*Aid Project Count	−0.186	−0.180
	(0.124)	(0.126)
Aid Knowledge*Aid Project Count	0.174	0.315
	(0.409)	(0.412)
Aid Treatment*Aid Knowledge		
	(0.000)	(0.000)
Aid Project Count	0.102	0.043
	(0.088)	(0.092)
Aid Treatment		
	(0.000)	(0.000)
Aid Knowledge		
	(0.000)	(0.000)
Observations	3,492	3,492
Pseudo-R^2	0.001	0.019

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 32)

Table S37: Interactions of Voting Information Treatment with Voting Knowledge

	without controls	with controls
	(1)	(2)
Voting Treatment*Voting Knowledge*Incumbent Percent	−0.146	−0.114
	(0.321)	(0.325)
Voting Treatment*Incumbent Percent	0.056	0.052
	(0.117)	(0.118)
Voting Knowledge*Incumbent Percent	−0.073	−0.116
	(0.237)	(0.242)
Voting Treatment*Voting Knowledge		
	(0.000)	(0.000)
Incumbent Percent	0.179**	0.168**
	(0.084)	(0.087)
Voting Treatment		
	(0.000)	(0.000)
Voting Knowledge		
	(0.000)	(0.000)
Observations	3,482	3,482
Pseudo-R^2	0.005	0.020

Note: *p<0.1; **p<0.05; ***p<0.01

This table shows the coefficients (in log odds) from conditional logit regressions on school selection. Standard errors are clustered on politician. Full model results can be found on the APSR dataverse 'Replication Notes and Output.pdf' file at https://doi.org/10.7910/DVN/HS5R5S (Table 15).

6 Summary Data

6.1 Statistics on Sample vs Theoretical Population

Out of 462 LCs, 335 were included in our sample. Out of 193 MPs, 125 were included in our sample. Politicians were excluded largely due to missing data on key variables (e.g., due to by-elections) or because there were not enough schools to make the treatment protocol feasible. Additionally, a few MPs were excluded because they were travelling or otherwise unavailable. No politicians refused to participate.

Our sample is reasonably representative of the country as a whole. In Tables S38 and S39 below, we show variable means for included and excluded wards and constituencies with standard deviations in parentheses.

Across both groups, population characteristics (turnout and number of registered voters) are well balanced. Since we were forced to exclude some smaller wards, our LC sample includes, on average, more schools and lower average enrollment. We generally see good balance on political characteristics of MPs and LCs. It is perhaps noteworthy that we sampled fewer ruling party (DPP) MPs. This is likely due to the fact that ruling party MPs are more likely to travel on a regular basis and were therefore harder to contact.

Table S38: Local Councillor Sample Statistics

Variable	In_Sample	Out_of_Sample	Difference
Mean School Enrollment	938.859 (411.212)	1566.974 (964.155)	-628.115 (152.893)
Mean Number of Teachers	13.26 (5.631)	20.948 (12.028)	-7.688 (1.937)
Mean Student to Teacher Ratio	72.946 (18.749)	77.365 (26.933)	-4.42 (4.787)
Number of Aid Projects	11.03 (10.836)	4.681 (7.567)	6.349 (1.928)
Number of Schools	12.94 (6.226)	6.447 (5.295)	6.493 (1.192)
Turnout	0.699 (0.086)	0.678 (0.129)	0.021 (0.023)
Incumbent Victory Margin	0.259 (0.193)	0.172 (0.148)	0.088 (0.035)
Registered Voters	18090.91 (7642.809)	15736.553 (14056.628)	2354.357 (2333.846)
Incumbent Percent	0.49 (0.143)	0.436 (0.12)	0.054 (0.027)
DPP Incumbent	0.334 (0.471)	0.468 (0.504)	-0.134 (0.101)
UDF Incumbent	0.036 (0.186)	0.021 (0.146)	0.015 (0.034)
MCP Incumbent	0.232 (0.422)	0.234 (0.428)	-0.002 (0.088)
PP Incumbent	0.104 (0.306)	0.043 (0.204)	0.062 (0.054)
Independent Incumbent	0.069 (0.253)	0.064 (0.247)	0.005 (0.052)
Average School Population Density	11.356 (15.838)	39.7 (63.663)	-28.344 (9.569)

Table S39: MP Sample Statistics

Variable	In_Sample	Out_of_Sample	Difference
Mean School Enrollment	938.859 (411.212)	1566.974 (964.155)	-45.519 (69.649)
Mean Number of Teachers	13.26 (5.631)	20.948 (12.028)	-0.894 (0.897)
Mean Student to Teacher Ratio	72.946 (18.749)	77.365 (26.933)	3.813 (3.123)
Number of Aid Projects	11.03 (10.836)	4.681 (7.567)	6.085 (3.344)
Number of Schools	12.94 (6.226)	6.447 (5.295)	0.068 (2.144)
Turnout	0.699 (0.086)	0.678 (0.129)	-2345.005 (2137.803)
Incumbent Victory Margin	0.259 (0.193)	0.172 (0.148)	-0.04 (0.028)
Registered Voters	18090.91 (7642.809)	15736.553 (14056.628)	NA (NA)
Incumbent Percent	0.49 (0.143)	0.436 (0.12)	-0.035 (0.022)
DPP Incumbent	0.334 (0.471)	0.468 (0.504)	-0.162 (0.073)
UDF Incumbent	0.036 (0.186)	0.021 (0.146)	0.022 (0.021)
MCP Incumbent	0.232 (0.422)	0.234 (0.428)	0.087 (0.064)
PP Incumbent	0.104 (0.306)	0.043 (0.204)	0.006 (0.043)
Independent Incumbent	0.069 (0.253)	0.064 (0.247)	-0.095 (0.039)
Average School Population Density	11.356 (15.838)	39.7 (63.663)	-2.222 (2.63)

6.2 Summary Statistics and Coding for All Variables

Below are summary statistics and sources for all variables used in the analysis. Note that per our pre-analysis plan, missing data in control variables are imputed using the mean value for the lowest aggregation available (map, constituency, or district).

Table S40: Summary Statistics, MPs

Variable	Mean	SD	Details
Log Population	10.521	0.462	Log Constituency/Ward Population (WorldPop)

Log Area	9.91	0.884	Log Constituency/Ward Area in Square Km (WorldPop)
Log Enrollment	6.104	1.548	Log Number of Students in School +1 (Malawi Dept of Education)
Log Teachers	2.46	0.545	Log Number of Teachers in School +1 (Malawi Dept of Education)
ChildrenAttend=Yes	0.795	0.404	Whether incumbent's or family members' children attend school in the constituency=Yes (survey)
ChildrenAttend=No	0.205	0.404	Whether incumbent's or family members' children attend school in the constituency=No (survey)
ChildrenAttend=Don't Know	0	0	Whether incumbent's or family members' children attend school in the constituency=Don't Know (survey)
Incumbent's Children Attends School	0.043	0.204	Whether incumbent's children attends this school (survey)
Incumbent's Relatives Attend School	0.059	0.236	Whether incumbent's family members' children attends this school (survey)
Family Attends School	0.078	0.268	Whether incumbent's children or family members' children attends this school (survey)
Incumbent Understood Maps	0.858	0.349	Whether incumbent correctly indicated a response in a test map (survey)
Log Temporary Classrooms	0.396	0.645	Log Number of Temporary Classrooms in School +1 (Malawi Dept of Education)
Log Permanent Classrooms	1.855	0.712	Log Number of Permanent Classrooms in School +1 (Malawi Dept of Education)
Log Temporary Houses	0.42	0.665	Log Number of Temporary Teacher Houses in School +1 (Malawi Dept of Education)
Log Permanent Houses	1.096	0.739	Log Number of Permanent Teacher Houses in School +1 (Malawi Dept of Education)
Choice=Dictionary	0.324	0.468	Allocation decision on this map was about dictionaries (survey)
Choice=Teacher Bags	0.333	0.472	Allocation decision on this map was about teacher bags (survey)
Choice=Solar Lamps	0.343	0.475	Allocation decision on this map was about solar lamps (survey)
Opposition Votes (LC)	315.08	294.439	Votes at Polling Station for Leading Opposition Candidate in Councillor Election (Malawi Electoral Commission)
Opposition Percent (LC)	0.239	0.157	Percent Votes at Polling Station for Leading Opposition Candidate in Councillor Election (Malawi Electoral Commission)
Opposition Votes (MP)	341.167	352.331	Votes at Polling Station for Leading Opposition Candidate in MP Election (Malawi Electoral Commission)
Percent Votes (MP)	0.252	0.179	Percent Votes at Polling Station for Leading Opposition Candidate in MP Election (Malawi Electoral Commission)
Victory Margin (MP)	0.179	0.335	Victory Margin at Polling Station for incumbent MP (Malawi Electoral Commission)
Pop Density at School	9.679	16.634	Population per hectare (World Pop Project)
Pop Density at School	9.666	16.598	Population per hectare (World Pop Project)
Poverty at School	0.616	0.164	Proportion of Area in Poverty (World Pop Project)
Poverty at School	0.616	0.164	Proportion of Area in Poverty (World Pop Project)
Turnout	1347.38	945.126	Turnout at Polling Station
Log Turnout	7.007	0.647	Log Votes at Polling Station
Gender	0.902	0.297	Gender of respondent, male=1 and female=0 (survey)
Education Plan=Yes	0.67	0.47	Incumbent's council has an education plan=Yes (survey)
Education Plan=No	0.322	0.467	Incumbent's council has an education plan=No (survey)
Education Plan=Don't Know	0	0	Incumbent's council has an education plan=Don't Know (survey)
IncumbentTribe=Chewa	0	0	Incumbent is from Chewa tribe (survey)
IncumbentTribe=Lomwe	0	0	Incumbent is from Lomwe tribe (survey)
IncumbentTribe=Ngoni	0	0	Incumbent is from Ngoni tribe (survey)
IncumbentTribe=Other	1	0	Incumbent is from Other tribe (survey)
IncumbentTribe=Sena	0	0	Incumbent is from Sena tribe (survey)

IncumbentTribe=Tumbuka	0	0	Incumbent is from Tumbuka tribe (survey)
IncumbentTribe=Yao	0	0	Incumbent is from Yao tribe (survey)
ConstituencyTribe=Chewa	0	0	Constituency is predominately from Chewa tribe (survey)
ConstituencyTribe=Lomwe	0	0	Constituency is predominately from Lomwe tribe (survey)
ConstituencyTribe=Ngoni	0	0	Constituency is predominately from Ngoni tribe (survey)
ConstituencyTribe=Other	1	0	Constituency is predominately from Other tribe (survey)
ConstituencyTribe=Sena	0	0	Constituency is predominately from Sena tribe (survey)
ConstituencyTribe=Tumbuka	0	0	Constituency is predominately from Tumbuka tribe (survey)
ConstituencyTribe=Yao	0	0	Constituency is predominately from Yao tribe (survey)
Contest=Yes	0.766	0.424	Plan to contest election=Yes (survey)
Contest=No	0.034	0.182	Plan to contest election=No (survey)
Contest=Don't Know	0	0	Plan to contest election=Don't Know (survey)
Contest=Undecided	0.2	0.4	Plan to contest election=Undecided (survey)
Opposition Percent Votes in Ward	0.23	0.073	Percent votes for leading opposition candidate in ward (Malawi Electoral Commission)
Opposition Votes in Constituency	0.165	0.165	Percent votes for leading opposition candidate in constituency (Malawi Electoral Commission)
HighestEd=Certificate	0.317	0.466	Incumbent's highest education level=Certificate (survey)
HighestEd=Degree	0.025	0.155	Incumbent's highest education level=Degree (survey)
HighestEd=Diploma	0.096	0.294	Incumbent's highest education level=Diploma (survey)
HighestEd=PhD	0	0	Incumbent's highest education level=PhD (survey)
HighestEd=Primary	0.013	0.115	Incumbent's highest education level=Primary (survey)
HighestEd=Secondary	0.549	0.498	Incumbent's highest education level=Secondary (survey)
Income1	0.362	0.481	Incumbent household income 100,000-200,000 kwacha/month (survey)
Income2	0.304	0.46	Incumbent household income 200,000-400,000 kwacha/month (survey)
Income3	0.119	0.324	Incumbent household income 400,000-1,000,000 kwacha/month (survey)
Income4	0.018	0.134	Incumbent household income 1,000,000-5,000,000 kwacha/month (survey)
Income5	0	0	Over 5,000,000 kwacha/month (survey)
Income6	0.197	0.398	Under 100,000 kwacha/month (survey)
IncomeDeclined	0	0	Incumbent declined to declare income (survey)
LengthResidence1	0.005	0.07	Incumbent resided in constituency less than 5 years (survey)
LengthResidence2	0.031	0.172	Incumbent resided in constituency 5-10 years (survey)
LengthResidence3	0.214	0.411	Incumbent resided in constituency more than 10 years (survey)
LengthResidence4	0.738	0.44	Incumbent resided in constituency all their life (survey)
Length of Residence	2.706	0.547	0-3 index of how long incumbent resided in constituency (<5 yrs, 5-10 yrs, >10yrs or entire life) (survey)
LengthResidenceDontKnow	0	0	Incumbent doesn't know how long s/he resided in constituency (survey)
VoteAFORD	0	0	Incumbent would vote for AFORD party (survey)
VoteDPP	0.38	0.485	Incumbent would vote for DPP party (survey)
VoteIndependent	0.004	0.061	Incumbent would vote for Independent party (survey)
VoteMCP	0.333	0.472	Incumbent would vote for MCP party (survey)
VoteDeclined	0.147	0.354	Incumbent declined to declare party vote (survey)
VotePP	0.048	0.213	Incumbent would vote for PP party (survey)
VoteUDF	0.088	0.284	Incumbent would vote for UDF party (survey)

Pop Density	0.611	0.901	Average number of persons per grid cell in ward/constituency (WorldPop)
Incumbent Percent	0.488	0.216	Percent votes at polling station for incumbent (Malawi Electoral Commission)
Incumbent Votes	657.79	555.811	Votes at polling station for incumbent (Malawi Electoral Commission)
Aid Treatment	0.502	0.5	Equals one if a map was assigned the aid information treatment and zero otherwise
Need Treatment	0.507	0.5	Equals one if a map was assigned the school need information treatment and zero otherwise
Voting Treatment	0.522	0.5	Equals one if a map was assigned the percent votes information treatment and zero otherwise
Need Knowledge	0.475	0.306	Average score in school knowledge questions (survey)
Voting Knowledge	0.241	0.295	Average score in political knowledge questions (survey)
Aid Knowledge	0.122	0.222	Average score in donor knowledge questions (survey)
Aid Good Types	-0.007	1	A count of the number of types of aid projects delivered by donors at this school (donors)
Information Usefulness	1.552	0.676	A 0 to 2 scale indicating how useful the information was to the respondent (survey)
Learning from Experiment	0.271	0.444	Whether the respondent indicated that they learned something from the experimental interaction (survey)
Frequency of Donor Interaction	0.771	0.97	A 0 to five scale indicating how frequently incumbents interact with donors (survey)
Student to Teacher Ratio	72.719	33.563	Number of students per teacher in a school (Ministry of Education EMIS Statistics)
Student to Classroom Ratio	135.787	262.25	Number of students per class in a school (Ministry of Education EMIS Statistics)
Temporary Classroom Ratio	0.492	0.913	Number of temporary to permanent classrooms in a school (Ministry of Education EMIS Statistics)
School Need Index (ward)	-0.014	1.805	Index of school need within the ward (Ministry of Education)
School Need Index (constituency)	-0.013	1.88	Index of school need within the constituency (Ministry of Education)
School Need Index	-0.014	1.805	Index of school need within the constituency or ward (Ministry of Education)
Aid Project Count	-0.006	1.002	Number of aid projects at school (various donors)
Test Question Classes	0.51	0.5	Whether the respondent could correctly identify a school with the least number of permanent classes
Test Question Votes	0.31	0.463	Whether the respondent could correctly identify a school with the least percentage of votes for the incumbent
Test Question Enrollment	0.613	0.487	Whether the respondent could correctly identify a school with the highest number of students
Test Question Projects	0.213	0.41	Whether the respondent could correctly identify a school with the most donor projects
Test Question Enrollment Specific	0.304	0.46	Whether the respondent could correctly identify the range of enrollment at a chosen school
Test Question Votes Specific	0.173	0.378	Whether the respondent could correctly identify the range of percent votes at a chosen school
Test Question Aid Projects Specific	0.031	0.166	Whether the respondent could correctly identify one or more donors with projects on a map
Radio Transparency	0.506	0.5	Treatment telling incumbent that decisions would be broadcast on radio
Donor Transparency	0.5	0.5	Treatment telling incumbent that decisions would be shared with donors
Transparency Treatment	0.759	0.428	Either transparency treatment
Incumbent Percent	0.488	0.217	Percent votes at polling station for incumbent (Malawi Electoral Commission)
Log Distance from Hometown	2.366	0.848	Log distance in km (+1) from incumbent's self-reported hometown
Heard of Tearfund	0.801	0.399	Equals one if the incumbent has heard of Tearfund and zero otherwise (survey)

Worked with Tearfund	0.229	0.42	Equals one if the incumbent has worked with Tearfund and zero otherwise (survey)
Victory Margin	0.249	0.327	Victory Margin at Polling Station for incumbent (Malawi Electoral Commission)

Table S41: Summary Statistics, MPs

Variable	Mean	SD	Details
Log Population	11.257	0.392	Log Constituency/Ward Population (WorldPop)
Log Area	10.698	0.727	Log Constituency/Ward Area in Square Km (WorldPop)
Log Enrollment	6.163	1.475	Log Number of Students in School +1 (Malawi Dept of Education)
Log Teachers	2.448	0.537	Log Number of Teachers in School +1 (Malawi Dept of Education)
ChildrenAttend=Yes	0.592	0.492	Whether incumbent's or family members' children attend school in the constituency=Yes (survey)
ChildrenAttend=No	0.399	0.49	Whether incumbent's or family members' children attend school in the constituency=No (survey)
ChildrenAttend=Don't Know	0.008	0.092	Whether incumbent's or family members' children attend school in the constituency=Don't Know (survey)
Incumbent's Children Attends School	0.003	0.053	Whether incumbent's children attends this school (survey)
Incumbent's Relatives Attend School	0.027	0.163	Whether incumbent's family members' children attends this school (survey)
Family Attends School	0.028	0.166	Whether incumbent's children or family members' children attends this school (survey)
Incumbent Understood Maps	0.887	0.317	Whether incumbent correctly indicated a response in a test map (survey)
Log Temporary Classrooms	0.389	0.637	Log Number of Temporary Classrooms in School +1 (Malawi Dept of Education)
Log Permanent Classrooms	1.857	0.678	Log Number of Permanent Classrooms in School +1 (Malawi Dept of Education)
Log Temporary Houses	0.406	0.647	Log Number of Temporary Teacher Houses in School +1 (Malawi Dept of Education)
Log Permanent Houses	1.124	0.727	Log Number of Permanent Teacher Houses in School +1 (Malawi Dept of Education)
Choice=Dictionary	0.329	0.47	Allocation decision on this map was about dictionaries (survey)
Choice=Teacher Bags	0.326	0.469	Allocation decision on this map was about teacher bags (survey)
Choice=Solar Lamps	0.346	0.476	Allocation decision on this map was about solar lamps (survey)
Opposition Votes (LC)	301.308	271.395	Votes at Polling Station for Leading Opposition Candidate in Councillor Election (Malawi Electoral Commission)
Opposition Percent (LC)	0.242	0.16	Percent Votes at Polling Station for Leading Opposition Candidate in Councillor Election (Malawi Electoral Commission)
Opposition Votes (MP)	322.779	304.401	Votes at Polling Station for Leading Opposition Candidate in MP Election (Malawi Electoral Commission)
Percent Votes (MP)	0.262	0.187	Percent Votes at Polling Station for Leading Opposition Candidate in MP Election (Malawi Electoral Commission)
Victory Margin (MP)	0.154	0.344	Victory Margin at Polling Station for incumbent MP (Malawi Electoral Commission)
Pop Density at School	9.755	26.318	Population per hectare (World Pop Project)
Pop Density at School	9.731	25.028	Population per hectare (World Pop Project)
Poverty at School	0.631	0.156	Proportion of Area in Poverty (World Pop Project)
Poverty at School	0.631	0.156	Proportion of Area in Poverty (World Pop Project)
Turnout	1261.678	748.263	Turnout at Polling Station
Log Turnout	6.984	0.572	Log Votes at Polling Station

Gender	0.884	0.321	Gender of respondent, male=1 and female=0 (survey)
Education Plan=Yes	0.875	0.33	Incumbent's council has an education plan=Yes (survey)
Education Plan=No	0.116	0.321	Incumbent's council has an education plan=No (survey)
Education Plan=Don't Know	0	0	Incumbent's council has an education plan=Don't Know (survey)
IncumbentTribe=Chewa	0	0	Incumbent is from Chewa tribe (survey)
IncumbentTribe=Lomwe	0	0	Incumbent is from Lomwe tribe (survey)
IncumbentTribe=Ngoni	0	0	Incumbent is from Ngoni tribe (survey)
IncumbentTribe=Other	1	0	Incumbent is from Other tribe (survey)
IncumbentTribe=Sena	0	0	Incumbent is from Sena tribe (survey)
IncumbentTribe=Tumbuka	0	0	Incumbent is from Tumbuka tribe (survey)
IncumbentTribe=Yao	0	0	Incumbent is from Yao tribe (survey)
ConstituencyTribe=Chewa	0	0	Constituency is predominately from Chewa tribe (survey)
ConstituencyTribe=Lomwe	0	0	Constituency is predominately from Lomwe tribe (survey)
ConstituencyTribe=Ngoni	0	0	Constituency is predominately from Ngoni tribe (survey)
ConstituencyTribe=Other	1	0	Constituency is predominately from Other tribe (survey)
ConstituencyTribe=Sena	0	0	Constituency is predominately from Sena tribe (survey)
ConstituencyTribe=Tumbuka	0	0	Constituency is predominately from Tumbuka tribe (survey)
ConstituencyTribe=Yao	0	0	Constituency is predominately from Yao tribe (survey)
Contest=Yes	0.864	0.343	Plan to contest election=Yes (survey)
Contest=No	0.025	0.158	Plan to contest election=No (survey)
Contest=Don't Know	0	0	Plan to contest election=Don't Know (survey)
Contest=Undecided	0.11	0.314	Plan to contest election=Undecided (survey)
Opposition Percent Votes in Ward	0.236	0.073	Percent votes for leading opposition candidate in ward (Malawi Electoral Commission)
Opposition Votes in Constituency	0.148	0.149	Percent votes for leading opposition candidate in constituency (Malawi Electoral Commission)
HighestEd=Certificate	0.11	0.314	Incumbent's highest education level=Certificate (survey)
HighestEd=Degree	0.272	0.445	Incumbent's highest education level=Degree (survey)
HighestEd=Diploma	0.354	0.478	Incumbent's highest education level=Diploma (survey)
HighestEd=PhD	0.045	0.208	Incumbent's highest education level=PhD (survey)
HighestEd=Primary	0	0	Incumbent's highest education level=Primary (survey)
HighestEd=Secondary	0.093	0.291	Incumbent's highest education level=Secondary (survey)
Income1	0.042	0.202	Incumbent household income 100,000-200,000 kwacha/month (survey)
Income2	0.102	0.303	Incumbent household income 200,000-400,000 kwacha/month (survey)
Income3	0.292	0.455	Incumbent household income 400,000-1,000,000 kwacha/month (survey)
Income4	0.479	0.5	Incumbent household income 1,000,000-5,000,000 kwacha/month (survey)
Income5	0.068	0.252	Over 5,000,000 kwacha/month (survey)
Income6	0.008	0.092	Under 100,000 kwacha/month (survey)
IncomeDeclined	0.008	0.092	Incumbent declined to declare income (survey)
LengthResidence1	0.014	0.118	Incumbent resided in constituency less than 5 years (survey)
LengthResidence2	0.034	0.181	Incumbent resided in constituency 5-10 years (survey)
LengthResidence3	0.181	0.385	Incumbent resided in constituency more than 10 years (survey)
LengthResidence4	0.754	0.431	Incumbent resided in constituency all their life (survey)

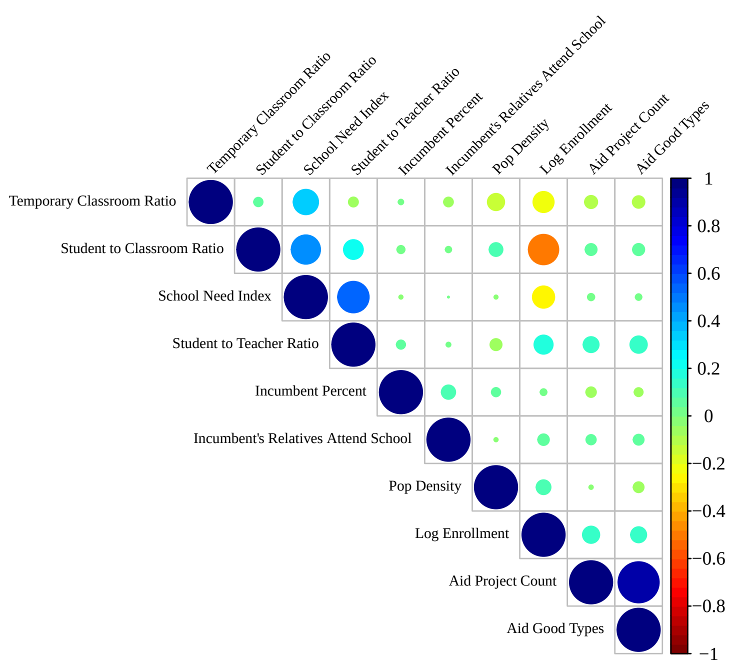
Length of Residence	2.703	0.604	0-3 index of how long incumbent resided in constituency (<5 yrs, 5-10 yrs, >10yrs or entire life) (survey)
LengthResidenceDontKnow	0	0	Incumbent doesn't know how long s/he resided in constituency (survey)
VoteAFORD	0.008	0.092	Incumbent would vote for AFORD party (survey)
VoteDPP	0.249	0.433	Incumbent would vote for DPP party (survey)
VoteIndependent	0.045	0.208	Incumbent would vote for Independent party (survey)
VoteMCP	0.292	0.455	Incumbent would vote for MCP party (survey)
VoteDeclined	0.19	0.392	Incumbent declined to declare party vote (survey)
VotePP	0.099	0.299	Incumbent would vote for PP party (survey)
VoteUDF	0.116	0.321	Incumbent would vote for UDF party (survey)
Pop Density	0.558	0.798	Average number of persons per grid cell in ward/constituency (WorldPop)
Incumbent Percent	0.418	0.215	Percent votes at polling station for incumbent (Malawi Electoral Commission)
Incumbent Votes	525.091	405.09	Votes at polling station for incumbent (Malawi Electoral Commission)
Aid Treatment	0.49	0.5	Equals one if a map was assigned the aid information treatment and zero otherwise
Need Treatment	0.484	0.5	Equals one if a map was assigned the school need information treatment and zero otherwise
Voting Treatment	0.504	0.5	Equals one if a map was assigned the percent votes information treatment and zero otherwise
Need Knowledge	0.449	0.255	Average score in school knowledge questions (survey)
Voting Knowledge	0.244	0.304	Average score in political knowledge questions (survey)
Aid Knowledge	0.128	0.229	Average score in donor knowledge questions (survey)
Aid Good Types	-0.002	1	A count of the number of types of aid projects delivered by donors at this school (donors)
Information Usefulness	1.75	0.561	A 0 to 2 scale indicating how useful the information was to the respondent (survey)
Learning from Experiment	0.474	0.5	Whether the respondent indicated that they learned something from the experimental interaction (survey)
Frequency of Donor Interaction	1.346	1.152	A 0 to five scale indicating how frequently incumbents interact with donors (survey)
Student to Teacher Ratio	75.044	33.824	Number of students per teacher in a school (Ministry of Education EMIS Statistics)
Student to Classroom Ratio	126.952	158.201	Number of students per class in a school (Ministry of Education EMIS Statistics)
Temporary Classroom Ratio	0.475	0.969	Number of temporary to permanent classrooms in a school (Ministry of Education EMIS Statistics)
School Need Index (ward)	-0.077	1.78	Index of school need within the ward (Ministry of Education)
School Need Index (constituency)	-0.056	1.826	Index of school need within the constituency (Ministry of Education)
School Need Index	-0.056	1.826	Index of school need within the constituency or ward (Ministry of Education)
Aid Project Count	-0.007	0.993	Number of aid projects at school (various donors)
Test Question Classes	0.479	0.5	Whether the respondent could correctly identify a school with the least number of permanent classes
Test Question Votes	0.377	0.485	Whether the respondent could correctly identify a school with the least percentage of votes for the incumbent
Test Question Enrollment	0.618	0.486	Whether the respondent could correctly identify a school with the highest number of students
Test Question Projects	0.238	0.426	Whether the respondent could correctly identify a school with the most donor projects
Test Question Enrollment Specific	0.252	0.434	Whether the respondent could correctly identify the range of enrollment at a chosen school

Test Question Votes Specific	0.11	0.314	Whether the respondent could correctly identify the range of percent votes at a chosen school
Test Question Aid Projects Specific	0.018	0.12	Whether the respondent could correctly identify one or more donors with projects on a map
Radio Transparency	0.518	0.5	Treatment telling incumbent that decisions would be broadcast on radio
Donor Transparency	0.533	0.499	Treatment telling incumbent that decisions would be shared with donors
Transparency Treatment	0.799	0.401	Either transparency treatment
Incumbent Percent	0.418	0.215	Percent votes at polling station for incumbent (Malawi Electoral Commission)
Log Distance from Hometown	2.751	0.814	Log distance in km (+1) from incumbent's self-reported hometown
Heard of Tearfund	0.125	0.33	Equals one if the incumbent has heard of Tearfund and zero otherwise (survey)
Worked with Tearfund	0.051	0.22	Equals one if the incumbent has worked with Tearfund and zero otherwise (survey)
Victory Margin	0.156	0.345	Victory Margin at Polling Station for incumbent (Malawi Electoral Commission)

6.3 Variable Correlation Matrix

Figure S18 shows the correlation matrix of all the variables we use to operationalize the treatment information. The dot size and color indicates the correlation coefficient of each variable.

Figure S18: Correlation Matrix



6.4 Attrition Statistics

6.4.1 Sample Attrition

In order to participate in the experiment, politicians had to be active in office and accurate data had to be available on all information treatments. By these criteria, 353 LCs and 187 MPs which were eligible for participation in the experiment. Of these, we were able to contact 335 LCs and 125 MPs. Subjects were excluded primarily because they were out of town at the time of the study. Since the information treatments were blocked on respondent, attrition is unrelated to treatment by design. However attrition also raises concerns about generalizability. In Tables S42 and S43 we show that there is little systematic difference between included and excluded subjects. Additionally in Table S44 we conduct a regression of available covariates on attrition. An F-Test easily fails to reject the null that these variables help explain patterns of attrition. We conclude that our subject pool is not biased to any large extent by attrition.

6.4.2 Post-Treatment Attrition

No politicians refused to participate in the study or answer questions about schools; however, we excluded some maps from the final analysis for technical reasons. The main reason for this is that politicians sometimes contested whether one or more schools were actually in their constituency.

Since politicians never select schools which they believe (rightly or not) to be outside their constituency, we exclude these contested maps from our analysis. We also excluded one map that was not correctly displayed on an RA tablet. These issues affect 83 out of 1,169 maps, and we exclude these 83 maps from our analysis.

To test whether this post-treatment attrition is related to treatment assignment, in Table S45, we estimate whether attritted maps are more likely to be in one or more treatment groups. We do not find evidence that this is the case.

Table S42: Summary Statistics by Survey Attrition Status

Variable	NotAttritted	Attritted	Difference
Aid Good Types	0.699 (0.668)	0.708 (0.637)	0.009 (0.155)
Aid Project Count	0.521 (0.477)	0.535 (0.45)	0.013 (0.109)
Incumbent Percent	0.492 (0.215)	0.452 (0.21)	-0.039 (0.051)
Log Enrollment	6.12 (1.544)	6.061 (1.805)	-0.059 (0.434)
Log Permanent Classrooms	1.859 (0.714)	1.899 (0.731)	0.04 (0.177)
Log Permanent Houses	1.097 (0.742)	1.196 (0.718)	0.099 (0.174)
Log Teachers	2.467 (0.545)	2.442 (0.595)	-0.025 (0.143)
Log Temporary Classrooms	0.395 (0.644)	0.254 (0.525)	-0.141 (0.129)
Log Temporary Houses	0.418 (0.662)	0.293 (0.571)	-0.126 (0.139)
Log Turnout	7.011 (0.643)	7.194 (0.581)	0.183 (0.141)
Opposition Percent (LC)	0.238 (0.156)	0.253 (0.162)	0.015 (0.039)
Percent Votes (MP)	0.253 (0.179)	0.261 (0.176)	0.008 (0.043)
Pop Density at School	9.774 (16.663)	8.045 (7.871)	-1.728 (2.066)
School Need Index	-0.015 (1.806)	-0.138 (1.835)	-0.124 (0.444)

Table S43: Summary Statistics by Survey Attrition Status

Variable	NotAttritted	Attritted	Difference
Aid Good Types	0.783 (0.688)	0.607 (0.591)	-0.176 (0.097)
Aid Project Count	0.558 (0.479)	0.481 (0.43)	-0.077 (0.069)
Incumbent Percent	0.416 (0.215)	0.45 (0.225)	0.034 (0.034)
Log Enrollment	6.158 (1.475)	6.034 (1.645)	-0.125 (0.247)
Log Permanent Classrooms	1.849 (0.684)	1.846 (0.774)	-0.003 (0.116)
Log Permanent Houses	1.121 (0.727)	1.087 (0.731)	-0.034 (0.113)
Log Teachers	2.443 (0.536)	2.466 (0.586)	0.024 (0.089)
Log Temporary Classrooms	0.389 (0.638)	0.412 (0.659)	0.023 (0.101)
Log Temporary Houses	0.41 (0.646)	0.469 (0.699)	0.06 (0.106)
Log Turnout	6.981 (0.573)	7.066 (0.676)	0.085 (0.1)
Opposition Percent (LC)	0.242 (0.16)	0.235 (0.141)	-0.007 (0.023)
Percent Votes (MP)	0.263 (0.187)	0.263 (0.182)	0 (0.029)
Pop Density at School	9.565 (24.497)	12.627 (23.117)	3.062 (3.663)
School Need Index	-0.059 (1.831)	0.035 (1.876)	0.095 (0.289)

Table S44: The Effect of Covariates on Survey Attrition

	MP Survey	Councillor Survey
	(1)	(2)
Aid Good Types	−0.488**	0.012
	(0.206)	(0.071)
Aid Project Count	0.666**	−0.011
	(0.306)	(0.102)
Incumbent Percent	0.320	−0.025
	(0.271)	(0.101)
Log Enrollment	−0.173*	−0.021
	(0.096)	(0.029)
Log Permanent Classrooms	0.294	0.027
	(0.235)	(0.074)
Log Permanent Houses	−0.123	0.041
	(0.127)	(0.041)
Log Teachers	−0.002	−0.076
	(0.178)	(0.057)
Log Temporary Classrooms	0.071	−0.028
	(0.189)	(0.057)
Log Temporary Houses	0.156	−0.029
	(0.132)	(0.044)
Log Turnout	0.108	0.085**
	(0.121)	(0.036)
Opposition Percent (LC)	−0.059	0.030
	(0.455)	(0.178)
Percent Votes (MP)	0.214	0.037
	(0.436)	(0.105)
Pop Density at School	0.0004	−0.002
	(0.003)	(0.001)
School Need Index	0.085	−0.037
	(0.081)	(0.034)
Constant	−0.047	−0.295
	(0.836)	(0.247)
Observations	187	353
R^2	0.101	0.041
F Statistic	1.376 (df = 14; 172)	1.028 (df = 14; 338)
Note:		*p<0.1; **p<0.05; ***p<0.01

Table S45: Effect of treatment on attrition due to politician contesting a school location

	(1)	(2)	(3)	(4)
Need Treatment	−0.014			−0.013
	(0.013)			(0.013)
Aid Treatment		−0.010		−0.010
		(0.013)		(0.013)
Voting Treatment			−0.018	−0.017
			(0.013)	(0.013)
N Maps	1252	1252	1252	1252
Observations	1,252	1,252	1,252	1,252
Pseudo-R^2	0.001	0.0004	0.001	0.002
Adjusted Pseudo-R^2	−0.0001	−0.0004	0.0004	−0.00001
Residual Std. Error	0.249 (df = 1250)	0.249 (df = 1250)	0.249 (df = 1250)	0.249 (df = 1248)
Note:				*p<0.1; **p<0.05; ***p<0.01

This table shows the results of a linear regression. The outcome variable is one if a map is attritted due to politicians contesting map boundaries and zero if the map remains in the analysis sample. Estimates are clustered on respondent.

7 Pre-Experiment Data Collection

7.1 Interviews and Focus Group Discussions with Government Officials and Citizens

Before the experiment, we conducted 32 semi-structured interviews with Local Councillors, Members of Parliament, District Commissioners, and Area Development Committees, as well as four focus group discussions with Malawian citizens. We used convenience sampling to sample the participants based on our networks and word of mouth. These interviews and focus group discussions asked questions about decision-making, transparency, accountability, and relationships across government stakeholders and donors. We also conducted phone interviews with 101 randomly selected local councillors to further evaluate how they gather information about their constituencies and make allocation decisions.

7.2 Teacher and Citizen Survey

This survey was designed to assess the views of recipient teachers and citizens about foreign aid and relationships with councillors. We utilized a hierarchical sampling procedure in order to select the schools to be included in this survey. We began with the sample of 333 wards which were involved in piloting activities. From these, we then randomly selected 60 wards, stratified by region, to be involved in the survey.

Within these 60 wards, we selected three schools to be involved in each survey. At each of these schools, we interviewed the head teacher and/or the assistant head teacher. Within the community surrounding each school, we used a random walk procedure to sample potential voters in the area. A team of two Malawian enumerators first located the school and recorded its GPS coordinates. Then, they spun a bottle and walked in the direction of the bottle opening. They sampled the male head of household at the first house, skipped two houses, and then sampled the female head of household at the next (fourth) house. They then continued until they had sampled six heads of households in that direction, at which point they returned to the school and repeated the process in a different direction, sampling a female head of household the second time. There were almost no instances of participants refusing to participate, but where this occurred or where the head of household was not home, the house was skipped, and the sampling procedure simply ignored this house in the random walk pattern.

This process resulted in a total intended sample of 13 people per school at three schools in 60 wards, or 2340 people. Because of logistical issues, the total actual sample was closer to 2000. All participants gave verbal consent to participate and were given between MK200 ($0.25) and MK1000 ($1.25) as a token of appreciation for their time (the payment was greater for head teachers and greater at baseline).

7.2.1 Teacher Perceptions of High-Priority Needs

Below we summarize responses from teachers about high-priority issues in their schools.

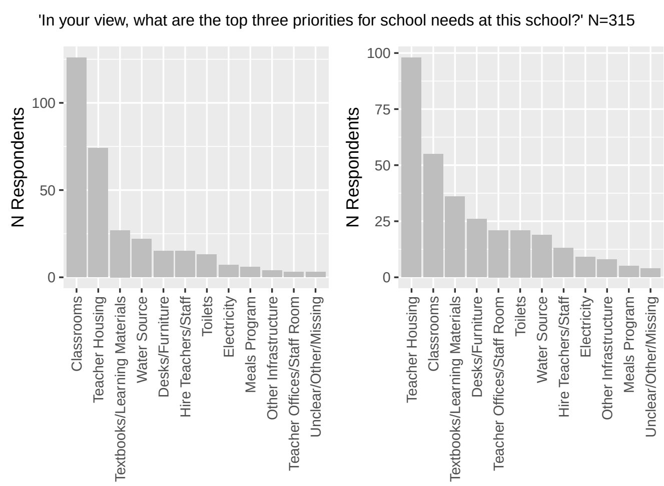


Figure S19: Head Teacher Prioritization of School Needs
NOTE: Head teachers were asked an open-ended question about the top three priorities in their school. We categorized their responses into 11 categories. The frequency of each category is shown on the y-axis. Each category is shown on the x-axis.

8 Experimental Protocol

8.1 Treatment Overview and Randomization

The experimental design included three information treatment arms which were administered to respondents via the maps following a full factorial design. The information treatments involved providing the respondent information about school need, political support, and foreign aid projects at a given school. For political support we used the vote-share of the MP/LC at the nearest polling station in the previous election in 2014. To measure school-level need, we used official data on student per classroom, teacher-to-student ratio and permanent-to-temporary classroom ratio. We also categorized schools into high, low or average needs relative to other schools in the same constituency/ward based on their scores in these indices. Table S46 below outlines the treatment conditions for each information treatment.

Table S46: Information on Treatment and Control Maps

	Treatment	Control
Political Support Information	Information on the map designates the level of support for the MP or LC at the nearest polling station to the school	Political support information is not provided
School Need Information	Information on the map designates the level of need at the school	School need information is not provided
Aid Information	Information on the map designates the number and type of foreign aid projects supported by international donors at the school	Foreign aid project information is not provided

The randomisation proceeded so that each sampled politician was first assigned into one transparency treatment arm. The transparency treatments were blocked on partisanship, the number of schools in a ward, and incumbent vote percentage. Each politician was then randomly assigned three information treatments within respondent blocks.

All treatments were assigned factorially. Altogether, therefore, there were 32 different possible combinations of transparency and information treatments. Table S47 below provides an overview of the number of individual maps that received each of the combinations of treatments.

Table S47: Number of maps receiving different combinations of transparency and information treatments

		Transparency Treatments				
		Control	Donor Audit	Radio	Donor Audit + Radio	Total
Information Treatments	Control	43	36	35	38	152
	Political Support (PS)	32	47	43	43	165
	School Need (SN)	36	38	41	35	150
	Aid	35	38	40	49	162
	PS + SN	47	40	37	40	164
	PS + Aid	33	44	40	34	151
	SN + Aid	36	42	35	34	147
	PS + SN + Aid	27	45	47	42	161
	Total	289	330	318	315	1252

8.2 Example Maps

All information treatments were presented in legends on the side of the map. In line with the factorial design, each map displayed either one of the individual information treatments, a combination of several information treatments, or no information treatment at all (full control). Due to the factorial design, these treatments were orthogonal to each other, enabling independent analysis of each information treatment separately. Figures S20 through S27 provide examples of maps containing each of the possible combinations of information treatment.

Figure S20: Map containing political support information treatment

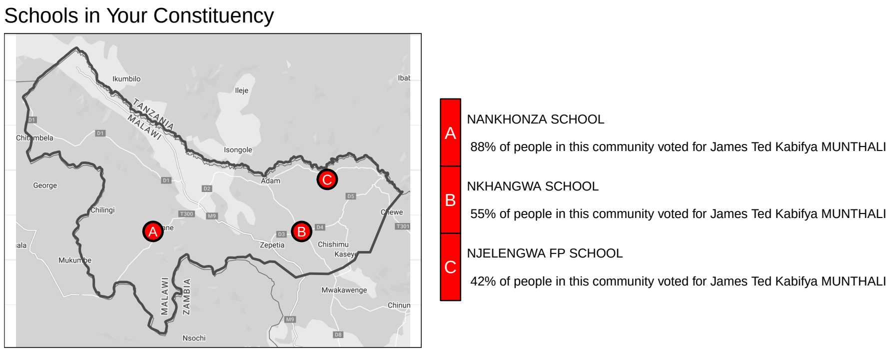


Figure S21: Map containing school need information treatment

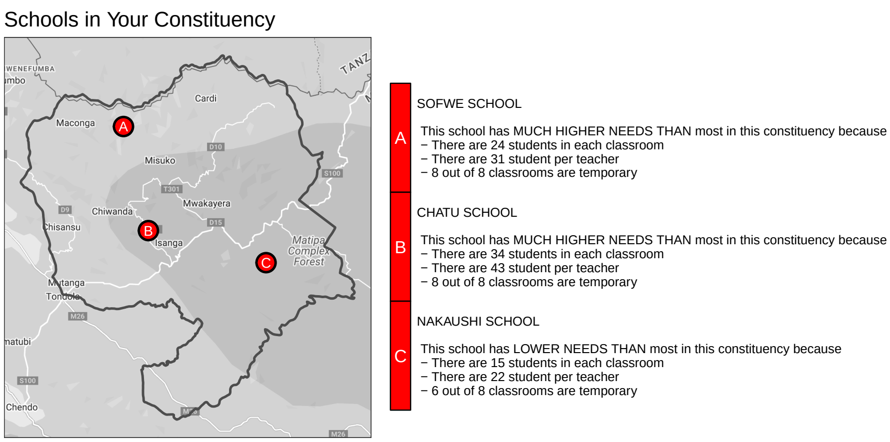


Figure S22: Map containing aid information treatment

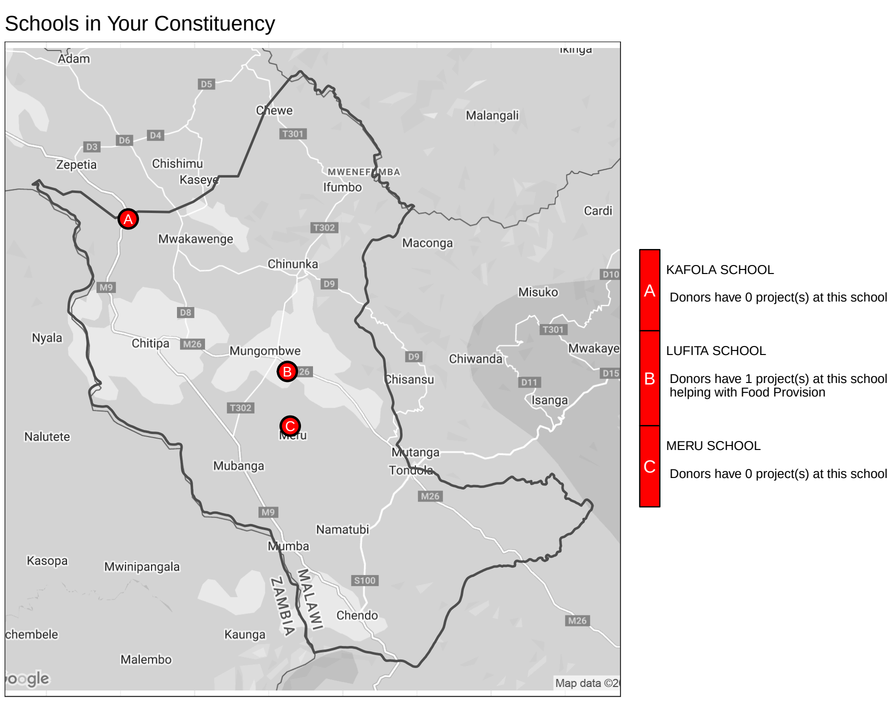


Figure S23: Map containing political support information and school need information treatments

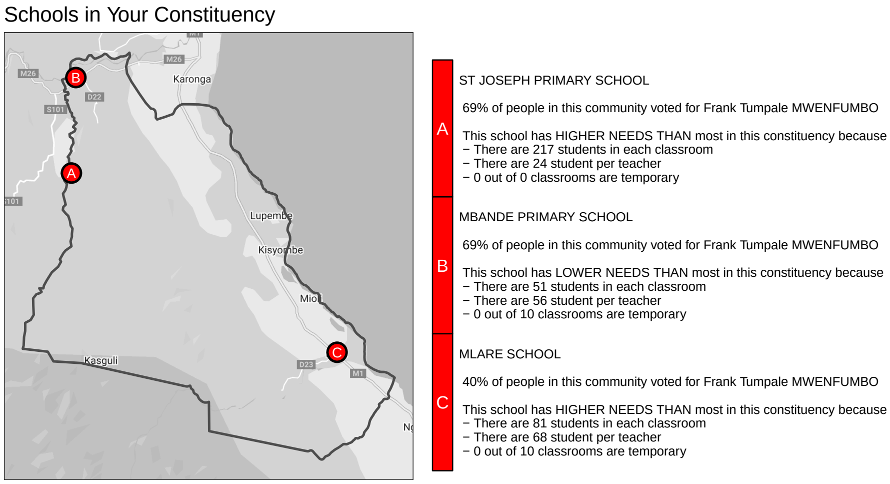


Figure S24: Map containing political support information and aid information treatments

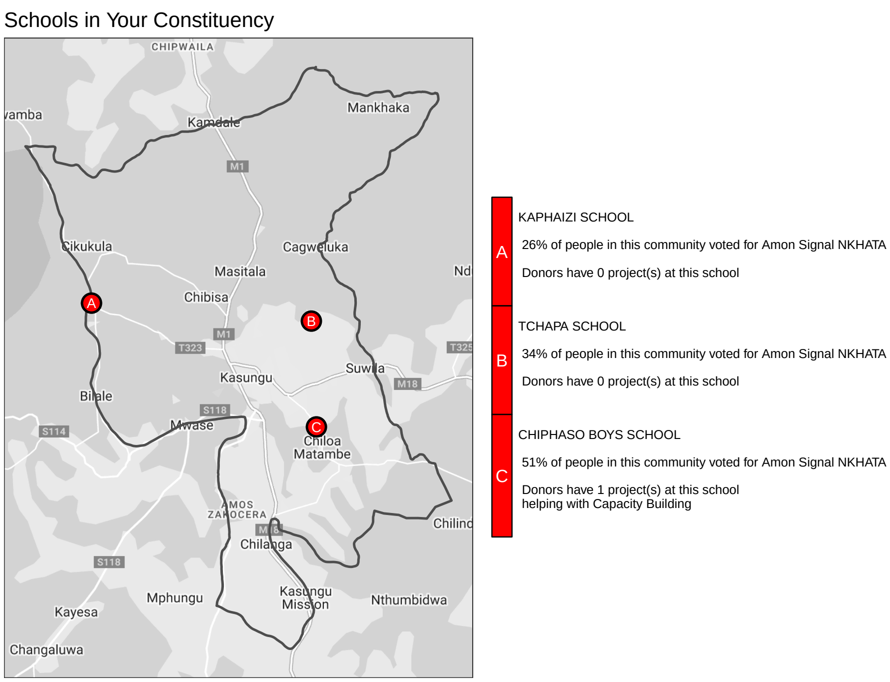


Figure S25: Map containing school need information and aid information treatments

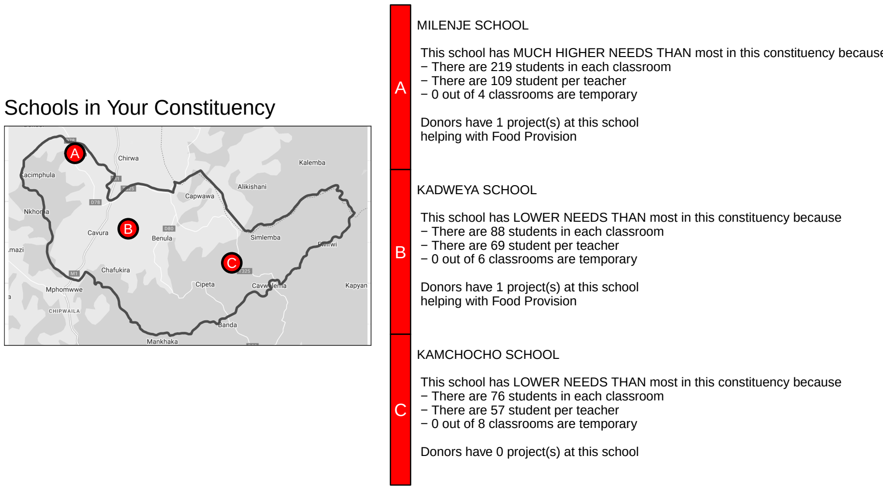


Figure S26: Map containing political information, school need information, and aid information treatments

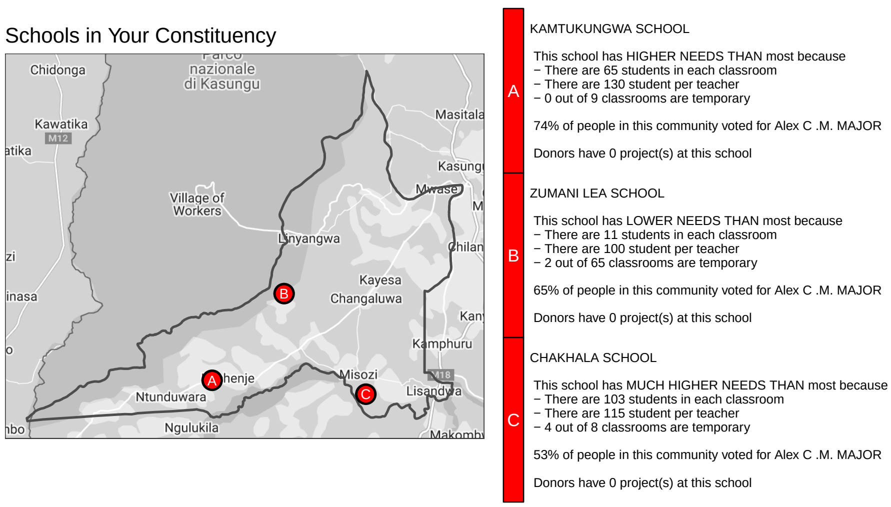
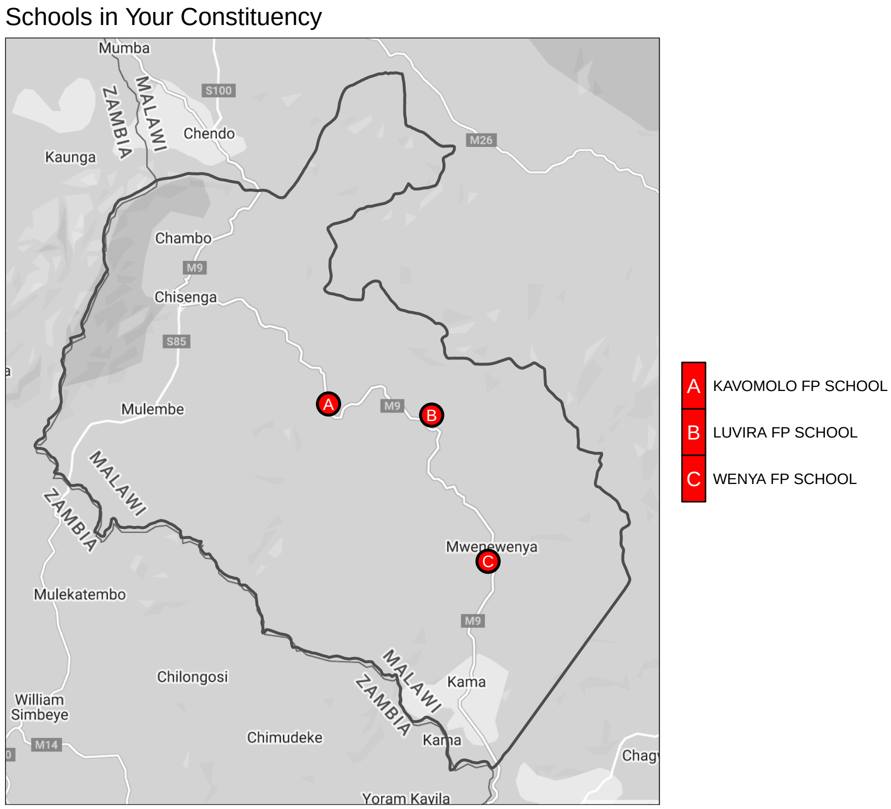


Figure S27: Map containing no information treatment



8.3 Collection of Information about Foreign Aid Projects

To collect information on foreign aid used for the aid information treatment, we focused the data collection on the main foreign donors active in the primary education sector in Malawi, and the projects these donors had carried out in individual primary schools in the past five years (since 2011). Following consultations with local stakeholders and practitioners active within the aid sector in Malawi, we identified the main donors whose project activities included the primary education sector. When approaching each of these donors, we asked them to provide detailed data on their project activities since 2011, including the type of intervention and the name and location of the recipient school. Donors were also asked to cross-validate our list of active donors in the sector, and to suggest organizations that were not on the list.

The organizations from which data on aid projects were obtained include Department for International Development (DFID), Deutche Gesellschaft fur Internationale Zusammenarbeit (GIZ), German Development Cooperation (KFW), Norweigan Embassy, Save the Children, United States Agency for International Development (USAID), United Nations Children's Fund (UNICEF), United Nations Development Programme (UNDP), Volunteer Service Overseas (VSO), World Food Programme (WFP), and the World Bank. Organizations that were identified as active in the education sector, but that failed to respond to our queries, include Japan International Cooperation Agency (JICA), OXFAM, United Nations Population Fund (UNFPA), and World Vision.

Like most of the Malawian aid portfolio for these donors, these education projects were almost entirely off-budget and implemented by donors or non-governmental implementing partners. Government ministries were consulted on some projects. However, we could find no evidence that council authorities or parliamentary representatives in benefiting constituencies had influence or insight into the process of allocating these projects.

8.4 Details on Development Goods Provided to Schools

In partnership with a UK-based NGO operating in Malawi (Tearfund), we offered to deliver school supplies to schools selected by the respondents, following a lottery. These school supplies consisted of either a set of 10 solar lamps, 10 English dictionaries, or 10 teacher supply kits. Examples of these school supplies are displayed in the pictures below.

Our focus group discussions with project stakeholders suggest that these goods are highly valued by politicians and schools. The portable, stand-alone solar lamps were intended to allow students and teachers to continue working even after dark fall, which is often difficult, due to the lack of electricity in the vast majority of schools in Malawi. The dictionaries were standard Oxford English language dictionaries to help with lessons, aid teachers with planning and teaching, and support students in independent studies. The teacher supply kits consisted of a box of chalk, rubbers, pens, notebooks, and a tote bag - basic supplies considered necessary for teachers to carry out their work.

The economic value of these goods was as follows:

1. 10 solar lamps: Malawi Kwacha 50,000 (approximately 69 US dollars)
2. 10 dictionaries Malawi Kwacha 55,000 (USD 76)
3. 10 teacher kits Malawi Kwacha 26.500 (USD 36)

One indication of the value recipients and politicians placed on the goods was the high turnout when delivering goods to the selected schools. On average, some 10 local leaders (i.e. village headmen, chiefs, church leaders, etc.) turned out at the handover events. Furthermore, several LCs, as well as headteachers, contacted Tearfund to inquire about the goods and their delivery.

We show pictures of a delivery for each good type in Figure S28.

Figure S28: Goods

8.5 Transparency Treatment

Besides the information treatments, the experiment also randomly assigned two transparency treatments. These were designed to measure the effect of politicians facing increased visibility of their decision-making to voters or donors. Two transparency treatments were provided before the politician made any decision regarding which school in their area should receive materials. The first transparency treatment involved informing the politician that the selected school will be broadcast on community radio. The research assistant then played out a sample of this broadcast for the politician (see 8.5.1 for an example script). RAs were instructed to use the following script to explain this treatment:

> *Please note that Tearfund will distribute a report about your choices today. This report will be provided to major donors in Malawi, and will include your name and a description of the schools you have selected today. The report will also explain the lottery. I have brought with me a copy of the letter that donors will receive (show the councillor/MP the letter if he/she wants to see it).*

The second transparency treatment involved informing the politician that a report would be sent to donors with his or her name and the selected school. The research assistant showed a sample of this report to the politicians (see 8.5.2 for a sample of the report). RAs were instructed to use the following script to explain this treatment:

> *Please note that Tearfund will make an announcement on community radio about your choices today. This broadcast will be heard by many in your constituency, and will include your name and a description of the schools you have selected today. The broadcast will also explain the lottery. I have brought with me an excerpt of the broadcast your constituents will hear (show the councillor/MP the broadcast if he/she wants to see it).*

Politicians appeared to take both treatments seriously. RAs were asked to identify all cases where politicians chose not to listen to the broadcast or read the report. They were also asked to indicate any politicians who did not read or listen to the end. We recorded no instances where politicians failed to review the report or listen attentively.

The provision of the transparency treatments followed a full-factorial design similar to that of the information treatments. Therefore, in addition to the information treatments detailed above, randomly assigned politicians were provided with either of the transparency treatments, both transparency treatments, or neither.

There was no deception involved in this intervention. At the conclusion of the study, the research project purchased a broadcast on the nationally syndicated radio station, Zodiac. In this broadcast, we shared information about the decisions that politicians made about where to allocate funding. Additionally, the research project sent an email to all major donors in Malawi. In this email, we provided a report about the decisions that politicians made about where to allocate funding.

8.5.1 Example Radio Transparency Treatment Broadcast

Politicians in the radio transparency treatment were provided with an audio recording of the following text to illustrate the information that would be provided to citizens. Politicians could listen to this recording in Chichewa, English or Tumbuku.

MP Script:

> We bring you this special program from Tearfund NGO. Tearfund is distributing development materials to primary schools in Chigwe District. The first phase of this project was to ask MPs and councillors for their input. We would like to inform you, the people of Chigwe District, about the schools your elected officials recommended to receive materials from Tearfund.
>
> Please know that not all these schools will receive materials. A public lottery will be held in Lilongwe to determine which schools will receive materials.
>
> Honourable MP John Banda of Nyasa Constituency was given a choice between Mkuku Primary School, Mpenga Primary School, and Nkhande Primary School to receive teacher supplies kits. [PAUSE HERE.] He recommended Mpenga Primary School. Then, he was given a choice between Mphidza Primary School, and...

Councilors Script:

> We bring you this special program from Tearfund NGO. Tearfund is distributing development materials to primary schools in Chigwe District. The first phase of this project was to ask MPs and councillors for their input. We would like to inform you, the people of Chigwe District, about the schools your elected officials recommended to receive materials from Tearfund.
>
> Please know that not all these schools will receive materials. A public lottery will be held in Lilongwe to determine which schools will receive materials.
>
> Honourable Councilor John Banda of Nyasa Ward was given a choice between Mkuku Primary School, Mpenga Primary School, and Nkhande Primary School to receive teacher supplies kits. [PAUSE HERE.] He recommended Mpenga Primary School. Then, he was given a choice between Mphidza Primary School, and...

8.5.2 Example Donor Transparency Treatment Report (see next two pages)

DONOR REPORT

PRIMARY SCHOOL DEVELOPMENT MATERIALS PROJECT

Prepared for:

USAID, DFID, GIZ, World Food Program, UNICEF, Save the Children, World Vision

June 2017

In the first half of 2017, Tearfund NGO initiated a project to provide development materials to primary schools across Malawi. The first phase of this project was to meet with elected officials to give them the opportunity to select schools in their areas to receive materials. The schools recommended by these officials will be entered into a public lottery to determine which schools will receive materials. This report provides information about the decisions of the officials and the characteristics of the schools they selected that you may find helpful as you plan projects in the future.

Member of Parliament ***John Banda***, representing ***Nyasa Constituency***, selected the following schools to receive materials:

Selected to Receive Teacher Kits

Mkuku Primary School
Location of School: Mbeta Village, Chizwe Ward
Number of Students: 872
Number of Classrooms: 7
Number of Teachers: 12
Number of Donor Projects: 1
% Votes MP Received in Community: 35%

Selected to Receive Dictionaries

Mpenga Primary School
Location of School: Mwai Village, Chipeza Ward
Number of Students: 963
Number of Classrooms: 5
Number of Teachers: 10
Number of Donor Projects: 0
% Votes MP Received in Community: 16%

Selected to Receive Solar Lamps

Nkhande Primary School
Location of School: Mapeto Village, Nkhozwe Ward
Number of Students: 450
Number of Classrooms: 8
Number of Teachers: 15
Number of Donor Projects: 2
% Votes MP Received in Community: 68%

Please note that, because of our project guidelines, not all schools in the constituency were eligible for selection.

9 Ethical Practices

Our research directly engaged human participants as interviewees, focus group discussion participants, and those exposed to experimental interventions (both directly and indirectly). In this section, we discuss our ethical practices concerning these participants.

We confirm compliance with APSA's Principles and Guidance for Human Subjects Research, and this research was reviewed and approved by the Malawi National Commission on Science and Technology and the London School of Economics Research Ethics Committee. We obtained voluntary and informed consent from all participants prior to research activities. All participants were permitted to withdraw from the project at any time (none did). Participants were compensated via airtime credit in the amounts as follows:

- Elected officials sampled for experiment - MK2000
- Elected officials sampled for interviews, teachers sampled for survey - MK1000
- Citizens sampled for survey or focus group discussions, elected officials sampled for phone survey - MK500

Our study used no deception and we do not believe that the research caused physical, psychological, social, or economic harm to either direct participants, or to others indirectly affected by the research. Indeed, our intention with this study was to replicate as closely as possible the kinds of spending decisions that officials make regularly as part of their official duties (and to reinforce the ability of democratic accountability mechanisms to improve such decisions). However one concern in this respect might be that the project influenced the allocation of resources in a way that was not equitable or welfare-enhancing. In this respect, it is important to note that we did not run the experiment in the context of an existing donor or public project, and therefore did not shift any planned funding or allocations and no community was worse off as a result of our research. Instead, we used research funds to fund an *additional* project that benefited the school communities identified by the elected officials. Moreover, in practice, the interventions piloted in this study appear to have been welfare-enhancing.

Some of these choices by the elected officials in our study could be perceived as patronage, and one might also be concerned about the use of a research project that could have facilitated patronage. In practice, the interventions piloted in this study appear not to have shifted spending to political supporters or family members; though we recognize that there was ex ante some risk that the interventions would increase patronage. To ameliorate the risk of highly biased or unfair spending decisions, we allowed school officials and our partner donor organization to decline any project, though they never chose to do so. Officials at the partner donor organization also had the option to reallocate the development materials at their own discretion, though they never chose to do so.

Our research-related activities complied with relevant laws and regulations in Malawi. In addition to formal approval from the authority governing research in Malawi, we also conducted informational interviews and piloting activities designed to validate our research approach and ensure it aligned with both general and context-specific ethical practices. These pre-research activities involved representatives from the potential participant pools (local councillors, members of parliament, district commissioners, area development committees, Malawian citizens) as well as representatives from organizations involved in development in Malawi (National Democratic Institute, National Initiative for Civic Education, United States Agency for International Development, and the United Kingdom's Department for International Development). In addition to interviews with officials from many of these organizations, we conducted a formal workshop in which stakeholders could offer feedback on our research design.

With the exception of the information revealed as part of the transparency treatments in our experiment, we kept the identities of all participants confidential via robust data security protocols in collection and storage. There were no breaches in confidentiality and the data that will be published as part of the replication materials for this article have been fully anonymized. Regarding the transparency treatments, participants were made aware of the transparency treatments before they made their choices in the experiment and were given the opportunity to refrain from making particular choices or decline to participate in the research altogether. In addition, we note that this research involves decisions about the allocation of public development aid. This task – making decisions about development aid – is a routine component of each elected official's professional duties in Malawi and is always in the public domain.

10 Summary of Pre-Specified Hypotheses

Below we summarize all of the pre-specified hypotheses referring to the information treatments in this experiment. Table S48 below provides an overview of these hypotheses, listing their numbering, potential changes in numbers used in the text, as well as where in the main text or SM the given hypothesis was examined or discussed.

Table S48: Pre-Specified Hypotheses about Information Treatments

PAP Num.	Hypothesis	Where Examined
Effects of Need Information Treatment		
HB.1	Politicians will be more likely to allocate to schools in areas with high need.	Main Manuscript, Section 3
HB.2	Politicians will be less likely to allocate to schools located in areas with higher support in the last election.	Main Manuscript, Section 3
HB.3	Politicians will be less likely to allocate to schools located in their home community or where family members attend.	Main Manuscript, Section 3; Section 4.8
Effects of Voting Information Treatment		
HC.1	Politicians will be more likely to allocate to schools in areas with higher support for the politicians in the last election.	Main Manuscript, Section 3
HC.2	Politicians will be less likely to allocate to schools in areas with high need.	Section 4.7
Effects of Aid Information Treatment		
HD.1	Politicians will be more likely to allocate to schools that have already benefitted from more foreign aid projects and where donors have provided more categories of goods (validation effect)	Main Manuscript, Section 3
HD.2	Validation will be more likely when politicians interact frequently with donors.	Section 4.3
HD.3	Politicians will be less likely to allocate to schools that have benefited from more foreign aid projects and where donors have provided more categories of goods (crowding out effect).	Main Manuscript
HD.4	Crowding out will be more likely in areas where the politician did not receive a high proportion of votes.	Main Manuscript, Section 3
HD.5	Crowding out will be [less] likely in areas where schools are less needy.	Section 4.7
Conditioning Effects of Knowledge		
HE.1	Information effects will be weaker (stronger) among politicians with more (less) relevant knowledge of their constituency.	Main manuscript, Section 3
HE.2	Information effects will be weaker (stronger) among politicians with more (less) time living in their constituency.	Section 4.8
HE.3	Information effects will be stronger (weaker) among politicians who found the information provided in the experiment to be useful (not useful).	Section 4.8
Compliance and Understanding		
HH.1	Politicians that demonstrate the ability to read and interpret maps (Q1.22) will be more likely to respond to all treatments.	Section 4.4
Interactions Across Information and Transparency Treatments		
HI.1	The effect of aid information will be stronger among politicians in the donor transparency group.	Section 4.5
HI.2	The effects of need information will be stronger among politicians in the donor and radio transparency treatment group	Main manuscript and Section 4.5
HI.3	The effects of political support information will be weaker among politicians in the donor and radio transparency treatment groups.	Main manuscript and Section 4.5
Conditioning Effects of Gender		
HK.3	Female politicians will be more likely to respond to information about need.	Section 4.8
HK.4	Male politicians will be more likely to respond to information about votes.	Section 4.8
Conditioning Effects of Electoral Competition		
HL.3	Politicians that expect to contest upcoming elections will be more likely to respond to need and political support information treatments.	Section 4.8
Hypothesis Family M: Conditioning Effects of Office		
HM.1	MPs will be more likely to respond to information treatments than LCs	Main Manuscript, Section 3

11 Pre-Analysis Plan Deviations and Errors

- There is a typo in HD.5 in the pre-analysis plan (PAP). The hypothesis should read "Crowding out will be *less likely* in areas where schools are less needy." not "Crowding out will be *more likely* in areas where schools are less needy." This typo is clear from the contradiction between the discussion of the mechanism underlying this hypothesis (at the end of the first paragraph in section D).

- In our PAP, we anticipated that politicians would be more likely to target communities with high population density (PAP HA5); however, we did not pre-specify that treatment effects would be conditioned by population density (Figure 111). While the results from these estimates are consistent with our theoretical expectation, the specification in Figure 11 is a deviation.

- In Section B of the PAP, we discussed our expectation that politicians would be more informed about their home area and we hypothesized in HB3 that the need information treatment would therefore lead to less spending in home areas. While we think that distance is a reasonable way to capture this home area effect, we were not explicit about this choice of measurement strategy. We also only hypothesized this effect for the need information treatment. This is what we find in practice, though we feel it is more transparent and consistent with theory to consider how home towns might condition all the information treatments.

- In the main text, we test H7a and H6b by estimating triple interactions between treatment, treatment information and distance to hometown or political support. In the PAP, we propose a triple interaction in the case of the aid treatment (PAP HD4); however we proposed a double interaction (need information*distance/political support) in the case of the need treatment (PAP HB2, HB3). The latter test is more consistent with the information costs theory outlined in the main text and PAP. We show the alternative specification in Figure S7.

- In the PAP, we specified a two stage least squares estimator of complier average treatment effects. Our measure of compliance (Q1.33 and Q1.34) is only valid for the transparency treatment arms and we see no variation in compliance. We, therefore, cannot estimate this model.

- In the PAP, we specified a preference for a mixed logit model in addition to a conditional logit model since the mixed logit is often used in the choice experimental literature to model similar consumer choice problems. In practice, we omit the mixed logit estimates. We did not anticipate the complexity of modeling and interpreting random parameter estimates in this context, especially with fixed treatment interactions (e.g., see Torres, Hanley and Riera (2011); Hensher and Greene (2003)).

- In designing our study, we anticipated that there could be errors in Ministry of Education statistics on school locations. To address this concern, we included a question on the survey about whether politicians claimed that one or more schools were outside their constituency (Q1.172). However we did not pre-register a plan for how we would address such issues. Since politicians never select schools which they believe (rightly or not) to be outside their constituency, we exclude these contested maps from our analysis. We also exclude one map that was not correctly displayed on an RA tablet. These issues affect 83 out of 1,169 maps and we exclude these 83 maps from our analysis. We show in Section 6.4 that this attrition is not related to treatment.

12 References

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