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Does Information Lead to More Active Citizenship? Evidence from an Education Intervention in Rural Kenya

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Abstract

We study a randomized educational intervention in 550 households in 26 matched villages in two Kenyan districts. The intervention provided parents with information about their children's performance on literacy and numeracy tests, and materials about how to become more involved in improving their children's learning. We find the provision of such information had no discernible impact on either private or collective action. In discussing these findings, we articulate a framework linking information provision to changes in citizens' behavior, and assess the present intervention at each step. Future research on information provision should pay greater attention to this framework.

Providing information to citizens about the quality of the government services they receive has been seized upon by development organizations in recent years as a key lever for improving the welfare of the world's poorest people. The logic is straightforward: Poverty can be reduced by improvements in governance and service delivery (World Bank 2004). In turn, governance and service delivery can be strengthened by increasing bottom-up pressure from citizens (Bruns et al. 2011). And, in keeping with the rich scholarly literature on the role of asymmetric information in principal-agent relationships (Ferejohn 1986; Fearon 1999; Besley 2006), bottom-up pressure can be increased by providing citizens with comprehensible information about what their governments and elected representatives are (or are not) doing on their behalf. The causal chain runs from information to citizen pressure to improved service delivery to welfare improvements.

This logic has motivated donors to support hundreds of millions of dollars of interventions designed to alleviate the presumed informational constraints faced by citizens in developing nations. And yet, these projects risk proving as unproductive as the ones they supplanted in the absence of a deeper understanding of the conditions under which information is likely to change people's behavior. Indeed, various researchers have begun to shed light on this plausible development strategy through a series of experimental interventions to study the effects of information provision, including through the distribution of report cards on local health service provision, school quality, and legislators' performance.¹ Others have involved media campaigns to publicize the leakage of development funds.² Still others have

disseminated information about municipal spending, corruption, and other outcomes.³ However, to date, the results from these studies have been mixed, and clearly more research is needed to draw stronger conclusions about the logic and assumptions undergirding the recent enthusiasm for information campaigns as development strategy.

This paper aims to further this understanding by evaluating and then unpacking the results of a large-scale informational intervention designed to generate both citizen activism and private behavioral change on behalf of improved educational outcomes in Kenya. Our study is unique with respect to most impact evaluation research in this area in that we manage to avoid many of the typical tradeoffs between internal and external validity: We study a largely "natural" intervention in the sense that we, as investigators, did not influence the formulation of the treatment materials, the sampling, or any aspect of its implementation; but we are also able to make relatively strong inferences about causal relationships because villages and households were randomly sampled for inclusion in the program. That said, there are also some limitations associated with our study that are distinct from field experiments designed purely for research purposes: the intervention combined multiple treatments in ways that make it difficult to assess their independent impact; the information provided to citizens only indirectly addresses the outcomes the intervention seeks to produce; and the theory of change structuring the intervention relied on a set of relatively optimistic assumptions about people's willingness to take costly actions to achieve collective ends. These characteristics, however, make the project no different from many other initiatives launched by

major development organizations and, in some respects, make it more, not less, important to try to determine whether the project achieved its goals (and, if not, why).

We employ a post-treatment field study conducted in matched villages in two rural Kenyan districts. Using multiple measures, we evaluate both public citizen activism and private actions taken by members of households that did and did not receive a randomized informational intervention. The intervention involved two different kinds of information: the reporting to parents of the results of literacy and numeracy tests administered to their school-aged children, and the provision of materials describing strategies parents might employ to improve their children's learning. The objective of the former was to provide parents with factual information from which they could make an inference about the performance of their local primary school, and hence the need to take action to improve it.⁴ The goal of the supplementary materials was to expand parents' repertoires of action by providing them with ideas for concrete steps they could take in order to hold schools and government accountable for better education.

We find that these informational interventions did not have any substantial impact on parents' public or private behavior. Parents that received the informational treatments were no more likely than other parents to take actions at school or in the public sphere to improve the quality of their children's schooling or to adopt behaviors at home that might have a positive impact on their children's learning. Nor were they more likely to increase their levels of citizen activism or community participation in areas outside education.

Although disappointing from the standpoint of those who have embraced the link between information provision and service delivery improvements, our null findings provide an opportunity for exploring some of the (usually unarticulated) conditions that may be necessary for information provision to generate real behavioral change. Specifically, we suggest that for information to generate citizen action it must be understood; it must cause people to update their prior beliefs in some manner; and it must speak to an issue that people prioritize and also believe is their responsibility to address. In addition, the people at whom the information is directed must know what actions to take and possess the skills for taking these actions; they must believe that authorities will respond to their actions; and, to the extent that the outcome in question requires collective action, they must believe that others in the community will act as well. And, of course, they cannot already be doing everything that is possible for them to do.

Either these conditions must already be met prior to the informational intervention or the intervention itself must produce these conditions. The absence of any of these conditions may be enough to interrupt the presumed link between information and both private and public actions. Our articulation of these key conditions has implications not just for making sense of our findings but for the assessment, design, and understanding of informational interventions more broadly.

RELATION TO THE LITERATURE

The hypothesized link between information and citizen activism for improved service delivery has been subjected to a growing number of empirical

tests. Multiple studies have found that informed citizens are more likely to be involved in civic and political action and to engage in participatory activities such as voting, attending political meetings, contacting officials, and protesting (Neuman 1986; Zaller 1992; Brady et al. 1995; Gerber and Green 2000). Studies have also shown that such participation is associated with higher levels of service provision (e.g., Heller 2001; Bjorkman-Nyqvist et al. 2013). Yet, a great deal of empirical work has found little substantive impact from the provision of information to poor citizens. This is true both among studies (like ours) that test for a link between the provision of information and changes in citizens' public and private behaviors and among those that investigate the reduced form relationship between information and the improved public service provision that these behaviors are thought to promote. Little empirical consensus has emerged.

A number of studies in this literature focus on the impact of information on voting. Among these, Banerjee et al. (2011) find that slum dwellers in Delhi increase turnout and select for better performing candidates when equipped with pre-election report cards on incumbent performance and candidate qualifications. However, Chong et al. (2012) find that the provision of information on municipal spending and corruption to Mexican voters has no impact on turnout or vote choices. Humphreys and Weinstein (2012) also find no effect on voters' electoral behavior in Uganda two years after the dissemination of report cards detailing their MP's performance. De Figueiredo et al. (2011) investigate the impact on turnout, ballot spoilage, and electoral support of publicizing a candidate's conviction on corruption charges in Brazil. They find that the effect of providing such information

is conditional on the convicted candidate's party connection, presumably because of the differing dispositions of each party's support base vis-a-vis corruption.

These mixed findings are echoed in studies that emphasize the impact of information on citizen actions outside of voting. Banerjee et al. (2010) find that providing information to citizens in Uttar Pradesh about the role of the local village education committee and about the quality of learning in local schools had no impact on parental involvement in the school system. Keefer and Khemani (2011) employ a natural experiment in Benin built around within-commune variation in access to community radio programming to evaluate the effects of information dissemination on literacy, government inputs to education, citizen involvement in Parent-Teacher Association meetings, and private investments in children's learning. They find that increased radio access has no impact on community-level participation, although it does seem to affect private behavior supportive of children's learning, such as purchasing books or making informal or private tuition payments to schools. Bjorkman and Svensson (2009) find that providing communities in Uganda with information about the performance of their local health facilities and encouraging community members to become more involved in monitoring their performance is associated with greater citizen involvement.

Another set of studies sidesteps the intermediate link between information and citizens' public or private actions and tests for a direct connection between information provision and the quality of government service delivery. Besley and Burgess (2002) examine government responsiveness to food production shortfalls in India and find that state governments respond more aggressively where

newspaper circulation (and presumably, the flow of information) is higher. Reinikka and Svensson (2005) report that a newspaper campaign in Uganda aimed at reducing the capture of public funds by providing information about local officials' handling of a large education grant program had a strong impact on both enrollment and test scores. Andrabi, Das and Khwaja (2009) find that the distribution of report cards with information on student performance on math and language tests in Pakistan led to test score increases in subsequent years. Jensen (2010) finds that providing students in the Dominican Republic with information about the returns to schooling significantly increases the number of years of schooling they complete. Both Keefer and Khemani (2011) and Bjorkman and Svensson (2009), described above, find positive effects of information provision on pupil test scores and infant mortality rates, respectively.

Almost all of the interventions described thus far, as well as much of the existing theoretical literature, focus on the provision of factual information that increases citizens' appreciation of the (usually deficient) quality of government services. But information provision might also generate citizen activism through other channels: by informing citizens about the importance of taking action and providing ideas about the specific actions they might take in order to improve the quality of government service provision (or substitute for it). One of the advantages of the intervention we study here is that it combines these different types of information. Like Banerjee et al. (2010) and Pandey et al. (2009), the interventions we evaluate involve information about both the quality of children's learning and how citizens might improve it.

INTERVENTION, CONTEXT AND RESEARCH DESIGN

(a) The intervention

The particular intervention we study, the Uwezo initiative, is a large-scale, multi-country, information-based intervention that seeks to promote citizen action towards the improvement of children’s learning in East Africa.⁵ It does this in three linked steps: First, by providing parents with reliable, easily understood information with which they might be able to make an inference about how much their children are (or are not) learning in school; second, by providing concrete suggestions about steps that parents might take to improve education outcomes for their children; and finally, by facilitating a broad public discussion of the state of education in the country. Our study focuses on the impact of the first two of these steps during the second Uwezo assessment round in Kenya, which took place in 2011. The specific interventions we study involve a series of informational treatments that provide parents with feedback about their children’s performance (the “assessment”) and guidelines for action (the “instruction materials”). Our research exploits the random implementation of these treatments across households and villages to estimate their effects on parents’ willingness to take public and private actions on behalf of improved educational outcomes for their children, and on their degree of citizen activism more generally.

Villages and households were selected for assessment and information dissemination (hereafter described as “treatment”) as described in the next section.⁶

Selected households received the following treatment during the months of February and March of 2011:

1. *Assessment:* An Uwezo volunteer administered tests of basic literacy, numeracy, and reading comprehension—in both English and Swahili—to every child in the household aged 6-16. Parents were presented with the results of these tests at the conclusion of the assessment and told that the test provided an indication of whether or not their children had mastered basic skills in reading and math.
2. *Instruction materials:* An Uwezo volunteer presented assessed households with materials that outlined strategies that parents might pursue to improve their children’s learning. These included: a wall calendar with statements about the value of education; a poster with a checklist of strategies parents might take to improve their children’s learning;⁷ a sign-up sheet to become a “friend of education” and to receive periodic text messages from Uwezo on education themes; stories in English and Swahili intended to be read by children; and a “citizen’s flyer” with recommendations about how to get involved in local and national efforts to improve educational outcomes. The volunteers took time to talk through the checklist of strategies listed on the poster, but left the other materials for household members to consider on their own.

During the 2011 round of the Uwezo initiative, 124 districts were randomly selected (from a total of 158 districts nationwide), weighted such that the number of

districts selected in each province would be proportional to the province's population. Thirty villages were then randomly selected for treatment from each district. In each selected village, twenty households were chosen to receive the assessment and instruction materials.⁸ In total, 72,106 households were visited by Uwezo volunteers in 2011, and a total of 134,243 children were administered the literacy and numeracy tests (Uwezo Kenya 2011).

(b) The context

Although the Uwezo initiative covers three East African countries (Kenya, Tanzania, and Uganda), we focus our evaluation on Kenya. Kenya is a democratic, multi-ethnic country with a historically solid educational system that, according to the 2011 UNDP report, provides 7 average years of schooling to children (high compared to 4.5 for sub-Saharan Africa as a whole). In 2003, the government introduced universal free primary education. Since that time, primary school enrollment rates have risen dramatically. However, many observers believe that due to the absence of a commensurate infusion of new funds, children's learning has suffered (Sifuna 2007; Hornsby 2012: 702-3). Corruption, mismanagement, and a lack of resources also may have undermined educational outcomes over the past two decades (Wrong 2009).

While the country's high baseline levels of schooling might bias against finding a strong effect of the intervention, this may be counterbalanced by the country's historic propensity for citizen activism, which should make Kenya especially fertile soil for the kind of social mobilization that the Uwezo initiative was

designed to inspire. Moreover, we study the intervention in areas with both relatively low and relatively high overall levels of educational attainment.

(c) Research design

We employ a post-treatment, matched village research design for estimating the effects of the Uwezo informational treatment. We selected two specific districts that provide variation in socio-economic status and baseline literacy rates for intensive study—Kirinyaga, in Central Province, and Rongo, in Nyanza Province.⁹ Kirinyaga ranks in the top third of districts in terms of advanced schooling completed (secondary or higher); Rongo ranks in the bottom third.¹⁰ Within each district, we designed our study to maximize the possibility of detecting a treatment effect. First, as described above, we study a compound treatment of student-specific information about performance and process-oriented information concerning *how* to participate as an active citizen.¹¹ Moreover, to minimize contamination (and associated attenuation bias) from the first Uwezo assessment round in 2010, we selected both study districts in part because they were not included in the 2010 Uwezo assessment.¹² To reduce the likelihood of information diffusion from the earlier Uwezo intervention and follow-up dissemination campaign, we also selected the districts so as to be distant from Nairobi and other large population centers, and as far as possible from districts that had been included in the 2010 assessment round.

Within Kirinyaga and Rongo, we selected six villages from among the thirty that had received the Uwezo assessment (“treated villages”). We selected these

villages so as to be physically distant (at minimum, non-bordering) from one another. From among the hundreds of untreated villages in each district, we then selected as control villages the six villages that offered the closest matches with the treated villages that we had already chosen. Matching was accomplished using data from the 2009 Kenyan census on a number of village-level characteristics that we hypothesized might influence the impact of the Uwezo intervention: population size, number of households in the village, number of people currently attending school, percentage of population that had finished primary and secondary school, percentage of population with radio and mobile phone service, and percentage of households with a mobile phone. Matches were also chosen so as to contain villages from the same electoral constituency and from adjacent, albeit different, sub-locations. Because we discovered that one of the treated villages in Kirinyaga contained only four treated households (discussed more below), we selected an additional treated village and matched pair in that district, for a total of seven village pairs in Kirinyaga and six in Rongo. Table 1 summarizes the characteristics of the village pairs across the covariates on which we matched.

Table 1

Our main interest was measuring differences in parents' behavior vis-a-vis their children's learning in households that received the assessment and instruction materials and those that did not ("treated" and "untreated" households, respectively). Since treated households could only be located in treated villages, and since our control villages contained, by definition, no treated households, this meant comparing households located in treated and untreated villages. However, we were

also interested in ascertaining whether the impact of the Uwezo intervention *spilled over* within treated villages from households that received the assessment and instruction materials to those that did not. Hence, we administered our household questionnaires (described below) in three different types of households: treated households in treated villages, untreated households in treated villages, and untreated households in untreated villages. We selected these households in the following manner:

- *Treated households in treated villages:* Uwezo's protocol had called for conducting assessments in twenty households in each treatment village. However, these twenty households were selected from the village household lists before the Uwezo volunteers had been able to confirm that the households contained school-aged children (thus suitable for assessment). Approximately one-third of the time, the pre-selected households did *not* contain children aged 6-16. Since the Uwezo protocol did not provide for the replacement of such households with new ones, the number of households in which the assessment was actually carried out was far below the target of twenty per village: on average, only twelve. In order to maximize the number of treated households in our study, our protocol was to include all of them in our sample.
- *Untreated households in treated villages:* We also sought to study households in treated villages that had not themselves received the assessment and accompanying instruction materials. To do this, we returned to the original household list developed during the assessment exercise and randomly

sampled fifteen households from among those that had not received the treatment. We also selected a set of replacement households using the same procedures.

- *Households in untreated villages:* In the control villages we had no household lists to draw upon, so our field coordinators worked with village elders to develop them, following the same procedures Uwezo used in their original sampling plan. From those lists, we randomly sampled fifteen households, along with an additional set of replacements.

The final size of our sample, tabulated in terms of our three treatment conditions and village pairs, is presented in Table 2.

Table 2

To select respondents to interview within each sampled household, we employed the following protocol: Enumerators were instructed to greet the first adult they encountered when they approached the household. They were to mention that they were conducting a survey on democracy and family behavior and ask if there were children aged 6-16 living in that household. If the answer was no, then the enumerator would not proceed with the interview. If the answer was yes, then the enumerator would ask the adult if he/she was the direct caregiver of a school-aged child living in the household. If the answer was again yes, then the enumerator would continue with the survey. If the adult indicated that she/he was not a direct caregiver of a school-aged child, then the enumerator would ask to identify another adult living in the household who was. Once a direct caregiver was identified, the enumerator would interview that person. Enumerators were

instructed to return two times to households in which it was not initially possible to conduct an interview, and to select a household from the replacement list when a suitable respondent could not be identified.¹³

During the interview, the enumerator would ask the respondent to list the names of every member of the household and to identify the children for whom she/he had direct responsibility. If the respondent cared for more than one child, the enumerator would roll a die to select just one child to be the subject of questions later in the survey.

DATA

In order to gather information about outcomes and covariates of interest, we developed an extensive household survey, which was translated into Swahili and Luo and administered by trained enumerators fluent in the appropriate local language. Interviews were conducted face-to-face with adult household members in June and July 2011, selected as described above. Households that had received the Uwezo assessment were told that we were following up on a survey conducted in March; households that had not received the assessment were told that their names had been selected at random. In neither instance did the enumerators specifically mention the Uwezo initiative while introducing the survey. The household survey took approximately 1.5 hours to administer.

In order to minimize desirability bias, we wrote the introduction to the survey to reduce the possibility that respondents would be primed to think about the Uwezo assessment or that they would think that the researchers and

interviewers were principally interested in education (and thus might be assumed to support efforts to improve children’s learning outcomes). We embedded questions on education in the latter part of the survey, alongside questions about governance, health, and water service provision.

(a) Outcomes

We were interested in outcomes of two sorts: actions that parents take at home to improve their own children’s learning; and actions that parents take in public arenas to improve education provided in school. Both types of actions may have positive impacts on one’s own children’s welfare, but the latter is subject to free riding and hence may be perceived as more costly relative to private benefits. Although the Uwezo initiative was designed principally to generate public citizen action with potential externalities (i.e., the latter sort), private actions to help one’s own family members are a critical complementary (or perhaps even substitutive) response that must be studied alongside the more “civic” reactions.

Thus, one logical way that parents may have responded to the information they received about their children’s literacy and numeracy was by increasing their efforts at home to help children with their schoolwork. We therefore asked parents whether they helped their children with reading, writing, and math. We also asked them whether they had read to their children in the past week, whether they had asked their children about their teacher’s presence at school, and whether they had ever considered switching their children to another school in order to improve the quality of the education they were receiving.

In addition to these specific questions, we also asked parents for a subjective assessment of their level of involvement in their children's education. We first asked parents: "Overall, how involved would you say that you are in trying to improve the quality of your child's education?" We then asked: "Has this level of involvement changed during the past three months?" (i.e., since the time of the Uwezo assessment).

In addition to taking action at home or becoming involved in their own children's education, parents may also undertake more public activities in support of improved learning at their children's school. From a policy perspective, such activities are particularly desirable, as they will likely have positive externalities that will benefit other children as well one's own children. To measure activities of this type, we asked parents whether, in the prior three months, they had discussed their child's performance with their teacher, attended a parent-teacher meeting, organized school activities for children, assisted teaching at school, provided extra lessons outside school, provided teaching materials to school, helped with school maintenance, provided food or water to the school, or discussed learning quality with their child's teacher.

We also asked respondents a series of questions about their participation in education-related groups and/or associations. We measured such participation in three different ways. The first indicator was a dichotomous measure recording whether or not the individual had participated in any groups or associations dealing with education issues in the last three months. The second was a more fine-grained measure based on the number of meetings the individual had attended on the

subject during this period, if they in fact had participated in such a group. The third recorded whether or not the individual had contributed any money in the last three months to the group. To get a sense of whether the Uwezo assessment triggered broader collective action at the village level, we asked respondents how often in the last three months members of the village had jointly approached village officials or political leaders, such as MPs and councilors, to improve their schools.

Because we were interested in the possible spillover of the impact of the Uwezo intervention beyond the educational realm, we also asked questions that indicated the extent of citizen activism on behalf of improvements in the delivery of public services more broadly. We therefore asked individuals whether or not they had taken any of a series of actions to improve the delivery of education, health, or water services. We also asked a series of questions about village-level collective action over and above education.

In all, we studied a total of fourteen outcomes, the full details of which are provided in Appendix A.

(b) Balance of covariates

Our strategy for making meaningful comparisons between treated and untreated households rested upon the assumption that the respondents from our treatment villages were not markedly different in aggregate from respondents in the control villages with which they were matched. While some confidence for this assertion comes from the balance in village characteristics summarized in Table 1, further confidence comes from a comparison across treated and untreated

households of the mean values of the key covariates collected in the household surveys (see Appendix A for a description of these variables). As Table 3 indicates, the differences in the mean values across the three treatment groups are statistically insignificant for all covariates (see especially column 4, which compares treated and untreated households).¹⁴

Table 3

FINDINGS

Do we observe different levels of involvement and citizen activism among parents whose children received the Uwezo assessment and the accompanying instruction materials; and among those parents whose children did not? The answer is an unambiguous no: no matter how we analyze the data, we find no evidence of a substantively or statistically significant average treatment effect on any of the outcomes we investigated. Figure 1 reports public actions taken by parents at school or in the public sphere that have potential externalities; Figure 2 reports private actions taken by parents at home for the sole benefit of their own children. Although the threshold for what constitutes a meaningful effect size is somewhat arbitrary, it is reasonable to think that an average effect of at least 0.5 standard deviations—equivalent to reporting taking one additional action (out of 9 possible actions) to help to improve one’s child’s school—is a defensible benchmark. This threshold also strikes us as appropriate given the Uwezo’s initial ambitious goal of increasing primary school literacy and numeracy by 10 percentage points. However, as the figures make clear, none of the point estimates exceed even 0.2 standard deviations,

and since the 95 percent confidence intervals include zero in every case, we cannot reject the null hypothesis of no effect for any of the aspects of citizen activism we measure.

Figures 1 and 2

Figures 1 and 2 only compare average responses across treated and untreated units, but our (non-) findings are robust to an alternative regression specification in which we control for a host of covariates that might have differed slightly across these two populations.¹⁵

We also find no evidence for conditional effects. In further analyses we estimated a series of models in which we interacted the highest grade achieved by the household head, the literacy status of the household head, and the number of meals that household members consume each day (a standard, if rough, proxy for household income) with the treatment variable, and found no impact of these interaction terms on any of our measures of private action or citizen activism. (All additional analyses are available upon request).

In addition to our household survey, we spent two months carrying out in-depth fieldwork in each study village, including interviews with village elders and head teachers, and focus groups with village elites. These studies were designed to help us identify the mechanisms linking the informational treatment to changing attitudes and behaviors. In the absence of any detectable treatment effect at the household level, we used these studies to confirm that there was also no effect at the village level. If there had been any new substantial discussions or outbreaks of

citizen activism in any of the treated villages, we almost certainly would have learned about these outcomes through this research, but this was not the case.

What about spillover effects? Thus far, we have focused on the differences in citizen activism between members of households that received the Uwezo assessment/instruction materials and members of households in control villages that were not part of the Uwezo assessment. However, our research design also makes it possible to identify spillover of the treatment effect from treated households to non-treated households located in the treated villages.¹⁶ This involves comparing the 200 control households with the 204 households that were located in treatment villages but did not receive the assessment or instruction materials. If there is spillover, we should see a difference in average outcomes across these two categories of households (although given the lack of a treatment effect among households that actually received the treatment, a finding of a treatment effect here would have been quite surprising). We can also compare outcomes among the 146 households that received the treatment and the 204 households that were located in the same villages but did not. There is, again, no evidence of an average treatment effect in any of the models, and thus no evidence of spillover.

UNDERSTANDING THE LACK OF TREATMENT EFFECT

What, then, explains the lack of a treatment effect? As noted, the project we were evaluating was a high profile intervention that embodied the new wisdom of how to improve the welfare of citizens of developing countries. It is thus important to try to figure out why we find no effects of the treatments. Of course, with

hindsight, it might appear "obvious" that a particular treatment might not have an effect on desired outcomes, but we seek to contribute to the development of future interventions by providing a more systematic consideration of the potential limitations of the intervention we studied (and also our own study of its impact).

First, it may be that our analysis is simply underpowered. A sample of 146 treated and 200 control units should be sufficient to identify treatment effects of 0.5 standard deviations given an intensive treatment such as the multi-pronged Uwezo intervention. But we cannot rule out the possibility that a larger sample of households might have made it possible to detect substantively smaller effect sizes, and hence altered our conclusions.

A second possible explanation is that the number of treated households in each treated village was too small—that is, that the treatment itself was insufficiently powerful. To the extent that real behavioral change requires collective action, a critical mass of citizens must be mobilized to act. It may be that the number of treated households in each village (an average of just twelve out of between fifty and 1,400 households in the village as a whole) was too few to generate this critical mass.

A third possibility is that inadequate time had elapsed between the assessment and our measurement of its impact. Real behavioral change may require reflection, discussions with other community members, and a rearrangement of commitments and prior obligations to make room for new activities and behaviors. Three months may simply have been too short an interval for these processes to work themselves through. Of course, it is also possible that the impact of the

intervention was extremely short-lived, in which case three months may have been too *long* an interval.

However, given our "clean" matched-village design, along with the confirmation provided by our subsequent qualitative fieldwork, we do not believe that we have missed a causal effect that actually exists. In light of the enthusiasm for such interventions among scholars and practitioners, our null finding thus demands further explanation.

We propose that a potentially important explanation for the absence of a strong treatment effect was the likely widespread absence of key conditions that logically ought to be present for an informational intervention to have a plausible chance of generating citizen activism. In figure 3, we offer a systematic framework for articulating these conditions. The framework can be thought of as responding to the question: What must be true for us to expect the provision of information to citizens to have a reasonable probability of causing a change in their behavior? Although we present it as a flow chart, the framework can be equally—and perhaps more usefully—understood simply as an enumeration of key constraints that might, alone or in combination, dilute the impact of an informational intervention.

Figure 3

The framework can be used in two ways. Here, we use it to structure an investigation into the possible explanations for our null findings. However, the framework can also be used for broader purposes to design more successful informational interventions and, more fundamentally, to better understand how, why, and under what conditions information might affect behavior—a contribution

for which development scholars and practitioners have recently been calling (O’Meally 2013; Joshi 2013; Booth 2011; McGee and Gaventa 2010; Foresti et al. 2007; Joshi forthcoming). Indeed, despite the growing awareness that pre-existing conditions matter for understanding whether informational interventions are likely to affect citizen action, there has been little progress on identifying exactly which factors actually matter (Joshi 2013; O’Meally 2013).

The factors we identify in Figure 3 fall into two categories. The first category focuses on the relationship of individuals to the specific content of the information provided by the treatment: Is the informational treatment easily understood? Does it provide new information that causes people to update their beliefs about the quality of service delivery? The second category focuses on the attitudes and beliefs of individuals about their political environment more generally: Do people prioritize the given issue (in our case, education)? Do they feel responsible for doing something about it? Do they have the knowledge and skills to take action? Do they feel their actions can have an impact? The key insight of the framework is that if, after the provision of the information, the answer to any of these questions is no—either due to the pre-existing characteristics of the people receiving the treatment or due to the contents of the treatment itself—then the intervention is less likely to have a significant effect on their behavior.

For each of the steps in Figure 3, we draw upon data from our study population to assess whether the answer to the question is likely to have been “yes” or “no” in the context we study. Where possible, we also evaluate whether the characteristic at issue is in fact correlated with differential treatment outcomes.

Ideally, we would also evaluate whether treatment effects depend on the joint presence of all nine conditions listed in Figure 3, but our limited sample size makes this impossible.

First, we note that the people at whom the information is directed must understand its content. As Fox (2007) has argued, if the information is provided in a form that is “opaque”—not understandable or not actionable—then it is unlikely to result in behavioral change. We are unable to assess this condition with our survey data since we did not administer a test of comprehension, but our extensive qualitative research suggests that parents did seem to have a relatively good grasp of the assessment and the ideas for action. We therefore do not believe that a lack of understanding was the source of our null findings.

Assuming that parents understood the information they received, the information should also be *new* for it to increase the probability of behavioral change. “Newness” may not be absolutely necessary. Mere repetition (Allport and Lepkin 1945; Schwarz et al. 2007) or receiving the information from a particularly trustworthy source (Malhotra et al. 2012) may increase its impact, even if the information has already been received previously. In addition, if people receiving the information know that others are receiving it as well, it may play a coordinating role that is independent of its novelty. This said, whether the receipt of information leads to changes in behavior is plausibly much more powerfully related to whether it causes people to update their prior beliefs.

For our study population, this condition was largely unmet. In our household survey, we asked parents of assessed children whether their children’s test scores

were higher or lower than they expected them to be. Among those who could remember the results, fully 60 percent reported that their child's scores were about the same as they expected.¹⁷ Parents also seemed fairly well informed about the performance of their children's schools more generally. All Kenyan schoolchildren take the Kenya Certificate of Primary Education (KCPE) examination upon completion of primary school, and all schools are ranked by their students' performance on this test. More than 72 percent reported that they knew the KCPE rank quartile of their child's school.¹⁸ It therefore seems unlikely that the Uwezo intervention was providing most parents with entirely new information about their children's school performance.¹⁹

Closely linked to the question of whether the information causes people to update their priors is the question of the direction in which those priors are updated. Most informational interventions in the service delivery sector are designed to cause people to believe that services are worse than they had thought—that is, they are designed to provide “bad news.” This is because it is usually assumed that bad news is what galvanizes people and motivates them to take action.²⁰

It turns out, however, that for many of the households in our study, the Uwezo assessment may have inadvertently given parents the impression that their children were learning at a level beyond their actual capabilities. The assessment tested literacy and numeracy for all children 6-16 years old but only at a Class 2 level, which is typically attended by children 6-7 years old. Since the majority of the assessed children were well beyond Class 2, most received passing scores on the

assessment—despite the fact that many were almost certainly performing below grade level and would have received failing scores had the Uwezo tests been grade appropriate. As a consequence, many parents received (erroneous) positive information about their children’s performance. In our study villages, 61 percent of children passed the English assessment, 62 percent passed the Swahili assessment, and 62 percent passed the numeracy assessment. More than half of the children—54 percent—received passing scores on all three tests. Not surprisingly, passage rates were especially high among older pupils. As shown in Figure 4, more than three quarters of assessed children above the age of 12 received passing grades on the literacy and numeracy.

Figure 4

Consistent with the failure of the assessment to provide bad news, large numbers of parents reported high levels of satisfaction with the quality of their children’s learning. More than half of parents in untreated households reported being very satisfied with the quality of English teaching at their child’s school and more than 85 percent reported being at least somewhat satisfied. These percentages are nearly identical among parents in treated households, which suggests that the treatment may have generated little motivation for the vast majority of parents to expend energy on improving their children’s learning.

But were parents who received “bad” news more likely to take action? Again, we are hamstrung in answering this question by the fact that parents in control households received no news about their children’s performance. Hence we cannot estimate treatment effects by conditioning on whether parents in treated

households received “bad” news. What we can do, however, is look at the treatment effect only among parents of children under ten years old, who failed the literacy and numeracy assessments at rates of greater than 50 percent. This is tantamount to limiting the sample to households where, with reasonable probability, parents received (or, in the case of parents in control households, we can infer *would have received*) bad news about their children’s performance on the assessment.²¹ To the extent that the intended treatment of the Uwezo campaign was not just the provision of information but the relaying of “bad” news about one’s children’s learning achievements, the analysis of this limited sample can be thought of as an analysis of the “treatment-on-the-treated” rather than (as in the results presented earlier) an analysis of the “intent-to-treat.”

We find no treatment effect on any of our fourteen outcomes in this analysis (results available on request). While not definitive, these findings cast doubt on the hypothesis that the lack of a treatment effect in our study was due primarily or exclusively to the improper calibration of the Uwezo test. Although most parents in the intervention did not receive “bad news,” there is no strong evidence that bad news led parents to increase dramatically the priority given to education as an issue for action.

Next, we move to assessing the attitudes and beliefs of individuals about their political priorities and the political environment. Even if the information is new, action to effect change is only likely if the recipients of the information prioritize the issue in question. Citizens in developing countries face problems of many kinds. Information about a particular problem, no matter how compelling,

may not inspire action if people rank its importance below that of other concerns. Citizens also may not be aware of the potential benefits of action, and this may reduce the extent to which they care about the outcome in question.²²

Data from our survey suggested that many parents *do* value education relative to other public goods such as health care and drinking water infrastructure. In a supplementary survey conducted in one of our two research districts (Kirinyaga), but in villages that had not received the Uwezo assessment, we asked 261 parents what they would do if they were given 1000 shillings to spend on improving the local health clinic, school, or village well. Most tended to split the contributions relatively evenly. On average, respondents allocated 380 shillings for education, 343 for health, and 272 for water improvement. Forty-three percent of respondents allocated the most money to education.

Assuming the information is well understood, is relatively new, and speaks to an issue that parents prioritize, then the next question is whether parents think it is their responsibility to do something about the problem. Feeling a sense of responsibility for an issue (or, more generally, a sense of civic duty) is especially important when one individual's action is unlikely to have an effect by itself.²³ In many cases, citizens in poor countries believe that monitoring the government and applying pressure to improve the quality of public services is the responsibility of local leaders, NGOs, professional inspectors, journalists or other individuals, but not citizens themselves. To the extent that this is the case, the recognition that something needs to be done may not produce behavior by citizens to do something

about it. In such an environment, we should not expect the provision of information about service delivery failures to generate citizen action.

There is evidence that this consideration may have been relevant in our study. Only six percent of our respondents reported that parents were most responsible for making sure that teachers come to school and teach the children, while 83 percent thought the school or headmaster was most responsible for doing so.

Next, if all these criteria are satisfied, the would-be actors must have useful knowledge about the concrete actions they could take. They need to know whom to contact, how the political and educational systems work, and where they can most effectively apply pressure for improvements (Tarrow 1998). If they do not have ideas for concrete actions and some knowledge of the public decision making process, then they may take actions that have no impact or, anticipating their inability to effect meaningful change, may not take any actions at all. This roadblock appears to have been potentially salient in our study population: When asked whether they knew what action to take when addressing problems with their child's school, the vast majority of parents in our study—72 percent—said they would not know, or would not know how to figure out, what specific actions to take.

To test whether the low level of political knowledge and sophistication among members of our study population was responsible for the lack of impact of the informational intervention (independently of the presence or absence of other conditions) we again looked for heterogeneous treatment effects. However, as we report in row 1 of Table 4, we find that even when we limit the sample to parents

who do report knowing what actions to take, parents who received the informational treatment were no more likely than parents who did not to become actively engaged in their children's learning. Again, this result suggests that the source of our null finding either lies elsewhere or that lack of political sophistication is responsible for the null impact jointly with the absence of other key conditions.

Table 4

Next, individuals must possess the skills to take these actions. As Brady et al. (1995) have highlighted in their resource model of political participation, citizens need to be able to voice their concerns, either verbally or in writing. They need to be able to organize themselves and others to take action. These skills are typically learned through schooling and through participation in non-agricultural employment and civic and religious organizations. In our sample, these skills appear to have been lacking. Only 17 percent of our respondents had experience contacting an official, and only 21 percent had written a letter as part of a community group. Only about a quarter had planned a meeting, although a fairly larger number—41 percent—claimed to have given a speech in a community group.

In addition to knowledge about actions to take and the skills for these actions, the next consideration is whether citizens have a subjective sense of efficacy and think that their efforts will have an impact. Even if they know who to contact, when and where to hold the meeting, and which buttons to push, they may still believe that their appeals will fall on deaf ears, that their pressure will generate no change, or that their efforts will, in the end, do nothing to change the status quo.²⁴ If

this is the case, then skills and knowledge alone will not be enough to lead to citizen action.

Parents in our sample displayed a reasonable level of internal efficacy or confidence in their ability to affect their environment: 65 percent said that people like them they had “a lot” (30 percent) or “some” (35 percent) influence in making their village a better place to live. Forty-seven percent said that people like them had “a lot” (16 percent) or “some” (31 percent) influence over local government decisions that affect their village.

Parents, however, expressed significant reservations about their external efficacy (i.e. the government’s trustworthiness and capacity to respond to their demands). For example, 39 percent of parents thought that it was very likely that they would be punished if they spoke out or complained about corruption at the village school or clinic, and another 28 percent thought punishment was somewhat likely. When asked a question about whether a hypothetical businessman who supported the opposition would receive equal treatment in his application for a business license from the local government, only 25 percent answered in the affirmative; the other 75 percent thought he would face at least some problems.

Respondents also estimated the general level of corruption in government to be very high, which we interpret as a likely barrier to citizen efficacy. When asked how much money people would actually receive if the government initiated a program that was supposed to provide each Kenyan with 10,000 shillings in drought relief payments, 85 percent thought that the average person would receive half or less than half of the payment. On average, people thought that only 2678 out

of 10,000 shillings allocated by the government would make it into their hands—in other words, they anticipated that almost three-quarters of the allocation would be siphoned off by corrupt officials.

The results from our surveys are echoed in the 2008 Afrobarometer data. On a question that asked respondents how much they thought an ordinary person could do to improve problems with how local government is run in their community, 72 percent said “nothing” or “a small amount.” When asked how likely it was that people be punished by government officials if they make complaints about poor quality services or misuse of funds, 34 percent said “somewhat” or “very likely.” When respondents were asked how easy or difficult they thought it was for an ordinary person to have his/her voice heard between elections, 53 percent said “very difficult” and another 24 percent said “somewhat difficult.”

There was some evidence to suggest that higher levels of efficacy were associated with greater citizen activism. Respondents who indicated confidence in their ability to shape outcomes in their village and to influence the local government were more likely to take action. Expectations of punishment were correlated with willingness to speak out. People who thought they would receive a greater percentage of the relief payments were also more likely to have taken actions to improve the school and to report that people in the village had asked officials to improve governmental performance in these areas.

But is greater efficacy associated with differential response rates to the information treatment? The results reported in Table 4 (rows 2-5) provide little evidence to support such a conclusion. If the lack of efficacy among our subjects

independently and exclusively lies behind the lack of impact of the Uwezo intervention, then our measures and estimation procedures are insufficient to capture this channel.

Finally, for citizens to act on behalf of change, they must believe either that their own individual actions can make a difference or, if they think that generating real change will require collective action, that others in the community will act with them. This is a key insight in Barr et al. (2012), which shows that information alone has a weaker impact on the success of community-based monitoring of schools in Uganda than information plus engagement in a dialogue with other members of the school monitoring committee, which the authors suggest aids community members in overcoming collective action problems.²⁵

To the extent that collective action is a real constraint, we might see stronger treatment effects on private actions to improve learning, like reading to one's own children or helping them with their math and writing, and weaker effects on participation in group activities, like going to a meeting. As we saw in Figures 1 and 2, however, there is no evidence for this pattern: the informational intervention generated no more private actions than public ones. To the extent that collective action problems are binding, we might also expect to find stronger treatment effects in places where respondents say they live in communities where there is higher "social capital."²⁶ As the results reported in Table 4, row 6 make clear, however, respondents in such communities are no more likely to have responded to the Uwezo intervention than respondents in communities with lower levels of social capital.

We are left, once again, with a plausible explanation for the lack of treatment effect but little evidence in our data that it independently and exclusively drives the null impact of the intervention. It could be that we simply lack the appropriate measures or statistical power to estimate these effects properly. Or it could be that several of the factors we investigated are complicit, but that each alone is insufficient to generate effects that are large enough to be detected in our analyses. Whatever the reason, the framework we applied to guide our inquiry strikes us as the right way to go about understanding the possible source of our null findings.

The framework also has utility beyond the uses to which we put it here. By identifying the factors that mediate the effects of an informational intervention on behavioral change, the framework provides a template for investigating why informational interventions frequently fail to generate the behaviors they are hypothesized to produce. The framework can also be used to design and to target informational interventions that have a higher likelihood of success. And it can be used as a structure for thinking more deeply and theoretically about the relationship between information and citizen action.

CONCLUSIONS

A large and growing number of interventions in developing countries are built around the assumption that information deficits are responsible for poor governance and service delivery failures. In recent years, development agencies have spent hundreds of millions of dollars underwriting projects designed to improve welfare by filling this purported information deficit. In this paper, we

evaluate the impact of a prominent example of such an intervention and, consistent with the findings of many similar studies, we find no substantial impact on any of a range of outcomes associated with public or private citizen activism.

Our findings lead us to be skeptical of the transformative effects of supplying citizens in poor countries with information about the quality of service provision. As the framework we develop makes clear, many factors are likely to mediate – and frankly, to dissipate – the effects of even very well-designed and well-implemented informational interventions on citizen attitudes and behaviors. Both efforts to promote citizen action through the provision of information and attempts to understand the failures of previous informational interventions would benefit from a more explicit engagement with this framework.

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¹ Examples include Bjorkman and Svensson (2009), Banerjee et al. (2010), Pandey et al. (2009), Humphreys and Weinstein (2012), and Banerjee et al. (2011).

² Examples include Reinnika and Svensson (2005) and Chong et al. (2012).

³ For reviews, see McGee and Gaventa (2010) and Pande (2011).

⁴ Assessment scores provide imperfect information about school quality, inasmuch as they reflect both a child's own aptitudes and the quality of the schooling they have received. Nonetheless, parents who were informed that their children failed the assessments should, on average, have had lower opinions of the quality of their children's schooling, and hence be more motivated to take actions to improve it, than parents whose children were not assessed.

⁵ Uwezo is a multi-year project involving annual assessments of children's learning for five years, beginning in 2010. The initiative is a sub-project of Twaweza, a non-governmental organization based in Dar es Salaam, Tanzania. For a fuller discussion of the theory of change underlying the Twaweza project, see Twaweza (2011: ch. 2).

⁶ Throughout the paper, when we refer to "villages" we mean village or urban areas, as the latter were also included in Uwezo's random sample. Our own evaluation, however, was limited to rural districts and included no urban locations.

⁷ These were in the form of questions: Do you read to your kids? Do you tell them stories? Do you ask your children to read to you? Do your children see you reading?

⁸ To select these households, an Uwezo volunteer worked with the village elder to draw up a list of all the households in the village. The volunteer then divided the total number of households by twenty to generate a value n , and selected every n^{th} household on the list. Five additional households were selected as alternates using a similar method.

⁹ In 2011, Kenya adopted a new system of devolved government in which counties replaced districts as the key units of sub-national administration below the province level. However for ease of exposition, we refer to these units as districts. Also, Rongo district became part of a broader Migori county, so adopting the “county” designation would lead to ambiguity as to the boundaries of our research site, which corresponds to the boundaries of the old Rongo district. Kirinyaga district became Kirinyaga county, so there would be no loss of precision if we adopted the new label.

¹⁰ Based on 2009 census data.

¹¹ In a supplementary study of villages and households that received only the assessment treatment, the substantive (null) findings were largely the same as those described below (results available upon request).

¹² Uwezo’s sampling protocol calls for the progressive expansion of assessed districts in each assessment round to ensure that each round (after the first) will contain a combination of districts that had previously received the assessment/information materials and those that had not.

¹³ We contacted a total of 732 households and completed 550 interviews, for an overall response rate of 75.1%. The vast majority of non-completed interviews were in households that were disqualified from our protocol because the household did not contain a child aged 6-16 years. Among households that did contain children in this age range, our overall response rate was 91.2 percent. This rate was slightly higher in Uwezo villages (92.9 percent vs. 88.4 percent) that had been previously contacted during the Uwezo assessment.

¹⁴ Note that, since all covariates were measured post-treatment, questions regarding behaviors that might have been affected by treatment were phrased to refer to the period “before the start of this year’s rainy season” (i.e., before the assessment).

¹⁵ We can be fairly certain that the lack of a treatment effect is not due to a poorly implemented intervention. Fully 84 percent of treated households had at least one of the instruction materials visible in their home at the time of our enumerators' follow-up visit three months after the intervention, and about half of the households had three or more (out of six) instruction materials visible.

¹⁶ Because control villages were selected so that they did not border villages in the Uwezo assessment, and since our household survey work was conducted prior to the launch of Uwezo's dissemination campaign, spillover to households in the control villages should be minimal.

¹⁷ Only about half of the parents in our survey could recall how their children had performed on the assessment, suggesting either that they did not grasp the import of the information or that they felt it was not worth remembering—perhaps because it confirmed their prior beliefs.

¹⁸ We did not, however, assess the accuracy of their knowledge of their child's school's KCPE ranking. Here, and in all subsequent analyses in this section, we report averages for the control group only.

¹⁹ Were the parents who received "new" information more likely to take action? In answering this question we are handicapped by the fact that while our data permit us to sort parents in treated households into those who received "new" information and those who did not, we cannot similarly sort parents in the control households, who did not receive information of any kind (the receipt of the information being the treatment). This means that we cannot estimate the treatment effect on subjects who received "new" information. Nor can we meaningfully compare levels of activism among treated parents that did and did not receive "new" information, since "new" information was not randomized within the treatment villages. Unobservable background characteristics that explain the gap between the parents' expectations and the child's actual performance on the test (which determines whether the information was "new") may also be correlated with the parents' levels of activism. We are thus left with a plausible candidate explanation for our null findings that, unfortunately, we cannot test.

²⁰ This assumption may not, however, be correct. It is at least possible that an equally powerful way of motivating behavior is by providing information that conditions are *better* than people had imagined. This might occur if, for example, “good news” caused people to have a greater sense of pride in the outcome, made them feel like part of a “winning team,” increased feelings of efficacy, or triggered an aversion to seeing service delivery decline.

²¹ The key, and we believe plausible, assumption here is that children in control households would have failed the test at the same rate as children in the treatment households.

²² Jensen’s (2010) finds that providing students with information about the returns to schooling has a dramatic positive impact on the number of years of schooling they ultimately attain is consistent with this argument if we interpret the response as being due to an increase in the extent to which students care about schooling.

²³ Downs (1957); Riker and Ordeshook (1968); Blais and Achen (2010); Blais (2000); Campbell (1960: 156).

²⁴ Researchers distinguish between two types of efficacy: internal and external. Internal efficacy refers to an individual’s confidence in their ability to understand and participate in the world around them. External efficacy refers to an individual’s belief that the government will be responsive to their demands. For reasons of data availability, we focus here mainly on external efficacy.

²⁵ Bjorkman and Svensson’s (2010) finding that community based monitoring of health facilities is more effective in ethnically homogeneous districts is constituent with this argument, insofar as citizens in ethnically homogeneous districts are better able to overcome collective action problems.

²⁶ Our measure of social capital is an index, as described in Appendix A.

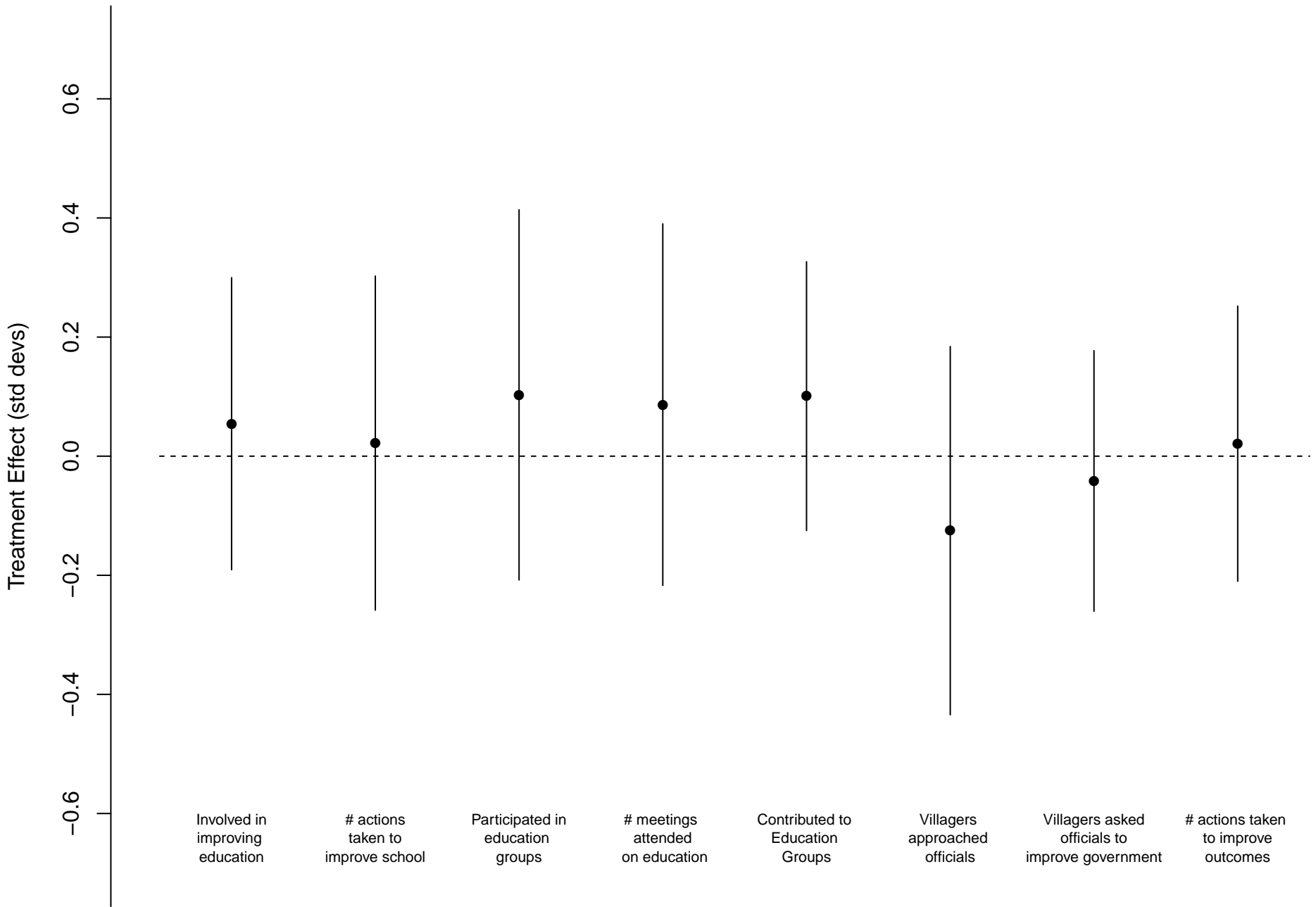


Figure 1
Average Treatment Effects: Public Actions

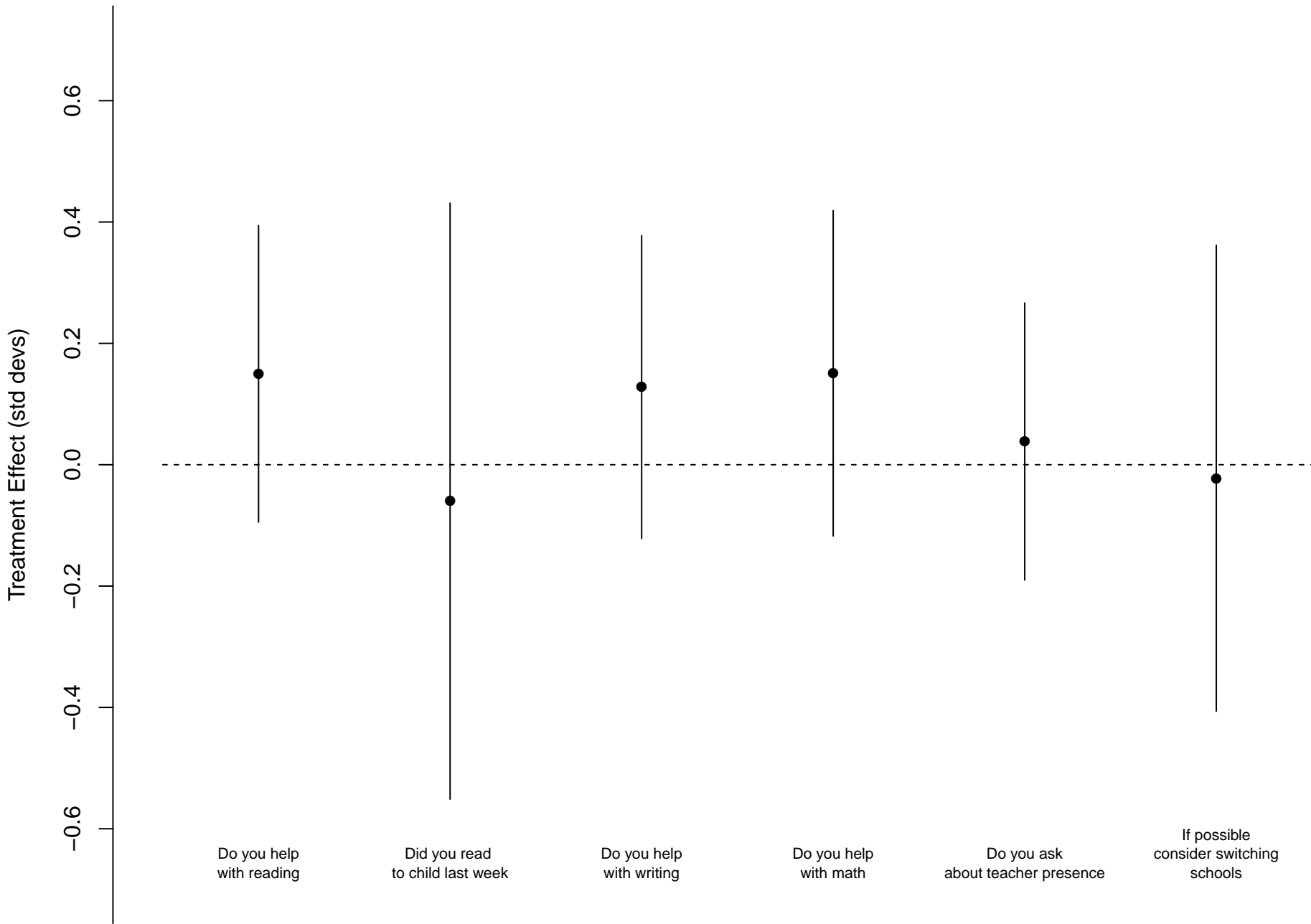


Figure 2
Average Treatment Effects: Private Actions

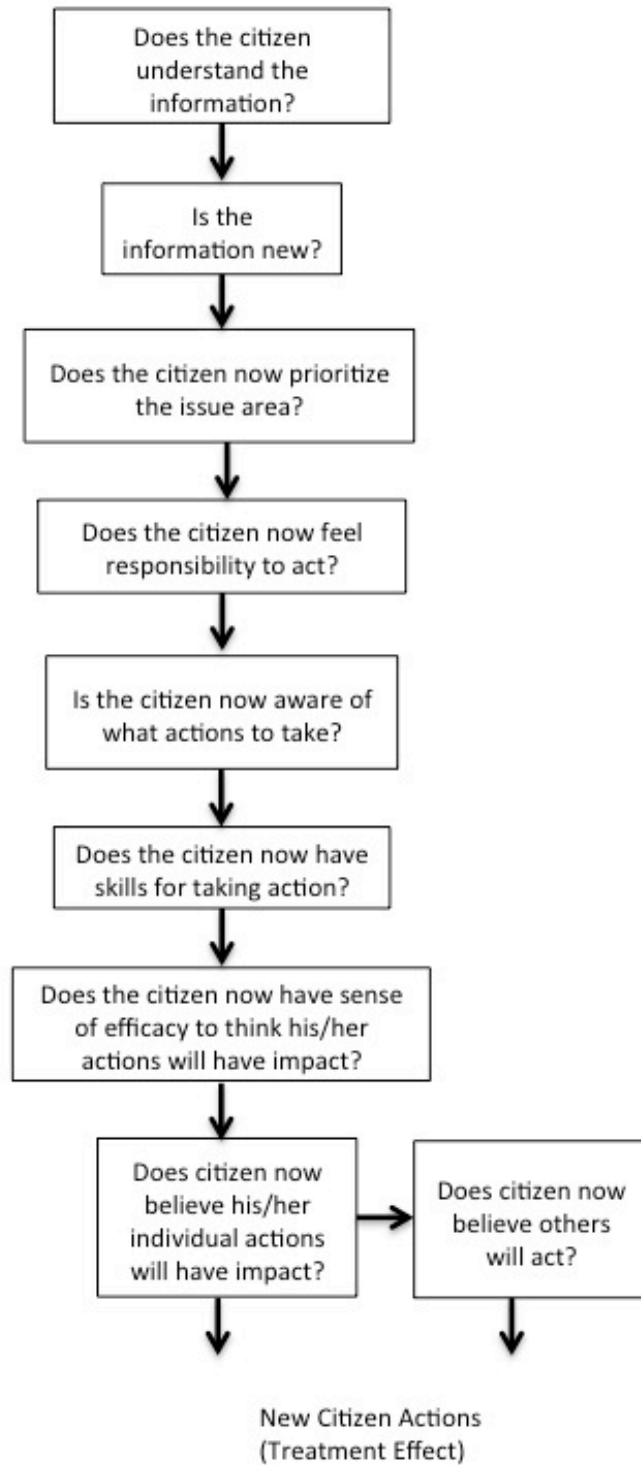


Figure 3: When Might Information Generate Citizen Action?

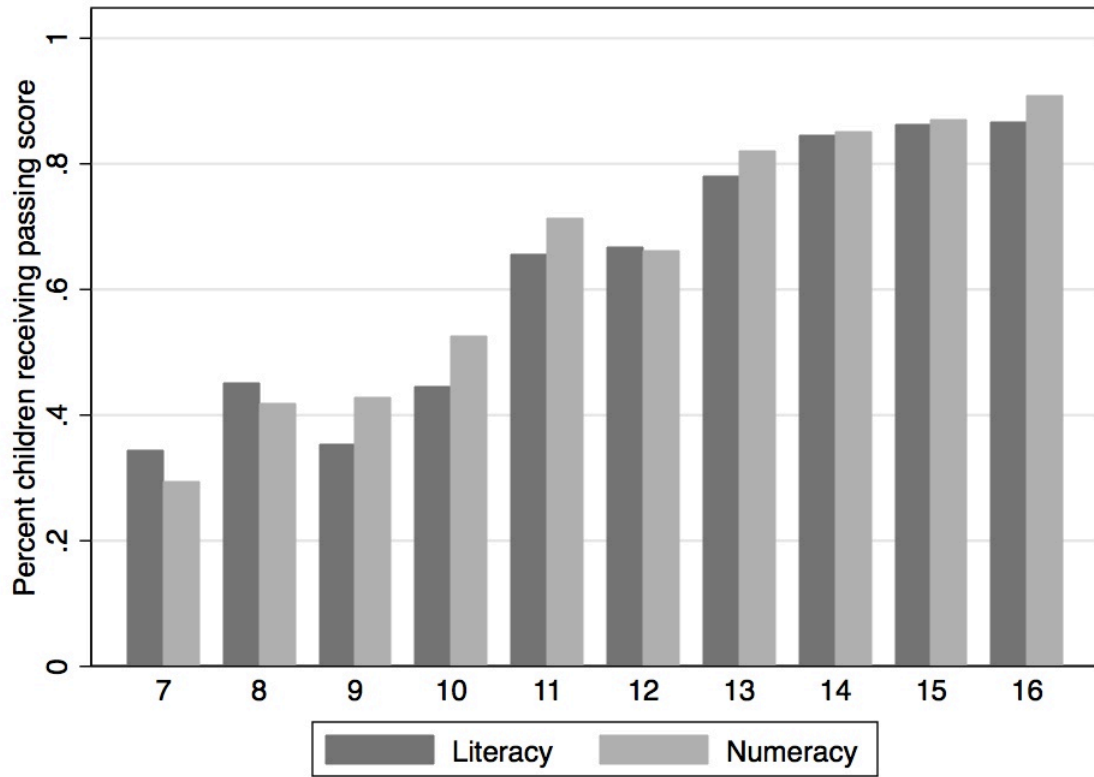


Figure 4: Percent of Children Receiving Passing Scores on Uwezo Assessments, by Age

Table 1: Characteristics of Matched Village Pairs

<i>District</i>	<i>Pair code</i>	<i>Treatment status</i>	<i>2009 pop</i>	<i># of hhlds</i>	<i># children at school</i>	<i>% adults only primary school</i>	<i>% adults only 2ndary school</i>	<i>% hhlds w radio service</i>	<i>% hhlds w mobile phone service</i>	<i>% hhlds w mobile phone</i>
Kirinyaga	A	Treated	800	200	200	0.5	0.2	0.9	1	0.8
	A	Control	700	200	200	0.5	0.3	0.9	0.8	0.8
	B	Treated	200	100	100	0.6	0.2	0.9	0.7	0.6
	B	Control	200	100	100	0.6	0.2	0.8	0.4	0.6
	C	Treated	400	100	100	0.4	0.4	1	0.7	0.8
	C	Control	400	100	100	0.5	0.2	0.9	0.9	0.7
	D	Treated	1400	500	500	0.5	0.3	0.8	0.6	0.8
	D	Control	1200	400	400	0.6	0.2	0.9	0.8	0.8
	E	Treated	800	200	200	0.6	0.2	0.8	0.4	0.7
	E	Control	700	200	200	0.6	0.1	0.8	0.5	0.6
	F	Treated	900	300	200	0.6	0.2	0.8	0.5	0.5
	F	Control	1000	300	300	0.6	0.1	0.8	0.7	0.7
	F2	Treated	300	100	100	0.6	0.2	0.7	0.4	0.6
	F2	Control	200	100	100	0.5	0.2	0.9	0.5	0.6
Rongo	G	Treated	200	50	100	0.5	0.2	0.9	0.4	0.5
	G	Control	200	50	100	0.5	0.1	0.8	0.4	0.4
	H	Treated	1200	200	700	0.3	0.4	0.9	0.8	0.7
	H	Control	700	100	300	0.6	0.1	0.9	0.4	0.8
	I	Treated	1300	200	700	0.4	0.3	0.4	0.3	0.7
	I	Control	1000	200	400	0.5	0.1	0.7	0.4	0.6
	J	Treated	900	200	400	0.6	0.1	0.6	0.4	0.4
	J	Control	800	200	400	0.5	0.1	0.6	0.5	0.4
	K	Treated	300	100	100	0.6	0.1	0.6	0.4	0.4
	K	Control	300	100	100	0.5	0.1	0.6	0.4	0.4
L	Treated	300	100	200	0.5	0.2	0.8	0.5	0.6	
L	Control	400	100	200	0.5	0.1	0.7	0.4	0.6	

Note: Village names are not provided and numbers are rounded to protect the identities of respondents in villages, following our protocol for the protection of human subjects.

Table 2: Distribution of Survey Households Across Treatments

		<i>Control Villages</i>	<i>Treated Villages</i>		
		<i>Control</i>	<i>Untreated</i>	<i>Treated</i>	<i>Total</i>
	<i>Pair</i>	<i>households</i>	<i>households</i>	<i>households</i>	
Kirinyaga	Total	109	105	77	291
	A	16	20	13	49
	B	15	12	10	37
	C	15	14	11	40
	D	17	13	4	34
	E	16	15	11	42
	F	15	16	18	49
	F2	15	15	10	40
Rongo	Total	91	99	69	259
	G	15	16	11	42
	H	16	16	10	42
	I	14	20	10	44
	J	15	17	16	48
	K	16	14	11	41
	L	15	16	11	42
		200	204	146	550

Table 3: Covariate Balance Across Treatment Groups

	(1)	(2)	(3)	(4)	(5)	(6)
	Untreated Households	Treated Households	Untreated Households in Treated Villages	Difference of Means (1)-(2)	Difference of Means (1)-(3)	Difference of Means (3)-(2)
Gender	0.41 (197)	0.33 (146)	0.37 (194)	0.077 (0.05)	0.040 (0.05)	0.037 (0.05)
Age	43.3 (196)	42.5 (146)	40.28 (192)	0.832 (1.63)	3.010* (1.46)	-2.178 (1.61)
Highest grade	4.08 (193)	3.97 (145)	4.06 (192)	0.110 (0.20)	0.026 (0.18)	0.085 (0.19)
Mother's educ.	1.8 (181)	1.79 (134)	2.14 (186)	0.005 (0.18)	-0.344 (0.18)	0.349 (0.20)
Can write	0.76 (195)	0.76 (145)	0.74 (204)	0.000 (0.05)	0.019 (0.04)	-0.018 (0.05)
Reading materials	7.63 (196)	7.81 (146)	7.68 (199)	-0.176 (0.16)	-0.051 (0.13)	-0.125 (0.16)
Meals per day	2.73 (196)	2.66 (146)	2.71 (204)	0.077 (0.06)	0.024 (0.05)	0.053 (0.06)
Information	10.1 (191)	10.04 (145)	9.99 (201)	0.063 (0.11)	0.115 (0.10)	-0.051 (0.11)
Ethnic outsider	0.07 (195)	0.03 (145)	0.06 (204)	0.032 (0.02)	0.008 (0.02)	0.024 (0.02)
Social capital	2.24 (190)	2.21 (141)	2.23 (197)	0.033 (0.11)	0.007 (0.10)	0.026 (0.11)
Request index	23.83 (193)	24.37 (146)	23.85 (201)	-0.536 (0.29)	-0.017 (0.28)	-0.519 (0.28)
Pretest active	8.51 (192)	8.56 (143)	8.56 (202)	-0.054 (0.23)	-0.059 (0.17)	0.005 (0.21)

Mean values reported in columns 1-3, with sample size in parentheses. Difference of means reported in columns 4-6, with standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

**Table 4: Heterogeneous Treatment Effects
(Treated vs. Untreated Households)**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	helped children with school	involvement in improving education	# actions taken to improve school	participated in education groups	# meetings attended on education	contributed to education groups	villagers approached officials	# actions to improve outcomes	villagers asked officials to improve government
Respondent knows, or could figure out, what specific actions to take to improve child's school	-0.012 (0.23)	-0.245 (0.21)	0.478 (0.83)	0.048 (0.15)	-0.091 (0.41)	-0.119 (0.19)	-0.143 (0.20)	-0.172 (0.76)	-0.237 (0.23)
Respondent believes that someone like myself can have a lot or some influence over local government decisions that affect my village	-0.153 (0.11)	0.151 (0.14)	0.329 (0.33)	0.227 (0.11)	0.622 (0.33)	0.271 (0.16)	-0.208 (0.13)	0.392 (0.71)	-0.161 (0.17)
Respondent believes that people like myself can have a lot or some influence in making this village a better place to live	-0.026 (0.22)	0.144 (0.17)	-0.022 (0.30)	0.120 (0.12)	0.565 (0.33)	0.197 (0.15)	-0.447* (0.17)	0.290 (0.80)	-0.207 (0.13)
Respondent believes that people who speak out or complain about corruption at the school or clinic are not very likely to be punished	0.409* (0.17)	0.208 (0.16)	0.556 (0.49)	-0.104 (0.15)	-0.463 (0.32)	0.025 (0.19)	-0.137 (0.15)	0.423 (0.89)	0.161 (0.17)
Share of the hypothetical KSh 10,000 in relief payments that respondent believes a person in their village would actually receive	-0.000 (0.00)	-0.000 (0.00)	-0.000 (0.00)	0.000 (0.00)	0.000 (0.00)	-0.000 (0.00)	0.000 (0.00)	-0.000 (0.00)	0.000 (0.00)
Respondent lives in a community with above average levels of social capital	-0.136 (0.21)	-0.132 (0.17)	0.657 (0.48)	-0.002 (0.11)	-0.030 (0.31)	-0.265 (0.13)	-0.059 (0.20)	-0.724 (0.62)	0.086 (0.16)

Reported coefficient is the interaction term between the treatment dummy and the variable listed at left. Observations vary in each cell. Robust standard errors, clustered by village, are in parentheses. All specifications include the following controls: Gender, Age, Highest grade attained, Mother's education, Can write, Reading mat'ls, Meals per day, Information, Ethnic outsider, Social capital, Request index, Pretest active; the treatment dummy, and the variable of interest. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.