

# Information Technology and Political Engagement: Mixed Evidence from Uganda\*

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Draft Version 1.1: October 17, 2015

## Abstract

Marginalized populations engage in politics at lower rates. Not only are their demands less likely to be addressed, they are also less likely to be articulated in the first place. This study uses a large-scale field experiment—implemented in partnership with the national Democratic Institute and the Parliament of Uganda—to learn about how technological change can affect who gets heard and what gets communicated to politicians. The nationwide field experiment was implemented following a national pilot undertaken under more controlled settings. The controlled experiment provided evidence that ICT can lead to significant “flattening”: a greater share of marginalized populations used this SMS-based communication compared to existing political communication channels. Estimated relations from the scaled-up intervention, however, look a lot like politics as usual, where participation rates are low and marginalized populations engage at especially low rates. We examine possible reasons for these differences, and then present the design and analysis of a third “mechanism experiment” that helps parse rival explanations for these divergent patterns.

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\*We thank the National Science Foundation for its generous support, as well as the National Democratic Institute for a fruitful collaboration. We also wish to thank Michael Findley and Daniel Nielson for useful comments.

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## **A Note to EGAP members**

Thank you for the time effort you are putting into reading our paper. As you can see this is a relatively early draft, so all comments are welcome. We are especially interested in whether you buy into the basic frame of the paper and our decision to focus on external validity and more specifically, issues arising from scaling up programs (rather than a focus on the field experiment per se). If so, do you think that there are other explanations we should be addressing in addition to “scale”, “players” and “design”? We are also interested of knowing what additional information we need to provide on the uSpeak program, the messages that came in, etc.

Thanks again. We very much look forward for your constructive comments Guy, Macartan and Gaby

# 1 Introduction

Marginalized populations may fail to engage in politics for many reasons. They may lack the means to engage; they may lack motives in the sense of not having formulated a distinct set of policy positions or not presuming a right to express them; or they may see no reasonable opportunities to engage, calculating that engagement is ineffective because more powerful actors will determine outcomes. These different types of explanation broadly map onto older discussions of the exercise of power, with work by Dahl (1961) emphasizing the role of resource disparities, Bachrach and Baratz (1962) highlighting opportunities, and Lukes (1974) highlighting motives. Additional perspectives also highlight challenges of collective action that render coordinated action by large groups difficult (Olson, 1965).

This paper contributes to our understanding of inequalities in political participation by assessing the heterogeneous causal effects of access to the *means* of communication; in doing so we seek to shed light also on the underlying willingness to engage in politics and on citizen beliefs about the effectiveness of engagement. We do so through the examination of three related experiments. The primary experiment examines a nation-wide parliament-led intervention that sought to increase and diversify political engagement by focusing on the means of political engagement. The intervention established and subsidized a mobile technology platform for political communication in order to increase political participation, especially for marginalized groups such as women and the poor. as we describe below this context is an interesting one in that the change in access was led by political elites and thus provided an invitation to citizens to engage in politics. Our overall conclusions are mixed. On one hand we find evidence that increased access *can* have a flattening effect and that in some settings marginalized voters are willing to engage at relatively high rates, on the other hand we find evidence that voters doubt the efficacy of their actions and that larger inequalities prevent modest interventions from having effects at scale.

In addition to our contribution to the study of communication, we also contribute to an ongoing discussion on the usefulness of more controlled experiments to shed light on larger political processes. Most field experiments are implemented on a small scale but seek to make claims about large-scale processes. Experiments sometimes test new approaches or are designed as a proof of concept. Sometimes micro logics are examined in small controlled settings but seek to shed light on general features of human behavior. Indeed the ‘credibility revolution’ in the study of international development is premised on the idea that small-scale controlled field experiments can create a body of knowledge that allows promoting “what works” and eliminating programs and policies that do not work (Banerjee and Duflo, 2009). Yet, it is often contestable whether the results of smaller-scale field experiments can accurately inform theory or form the basis for more general policy (Manski, 2013).

Our study contributes to this discussion by comparing the findings from three distinct experiments. Prior to the launch of the national program, we implemented a more controlled experiment conducted alongside a nationally representative in-person survey. This “scaled-down” experiment differs from the scaled up intervention in which entire constituencies were randomly selected into the program, and constituents were invited to take advantage of the new communication service using 30-second long radio spots (ads), aired

in the 19 languages spoken in our study areas, over a six-month period. Results from the pilot—described at greater length at [authors’ citation omitted]—suggested that there is underlying demand for the ability to contact one’s MP using mobile communication. The results of the controlled experiment also suggested that opening a new Information Communication Technology (ICT) channel has a potential of *flattening* political access; marginalized populations were likely to take advantage of a low-cost, impersonal, alternative technology to contact representatives.<sup>1</sup> Finally our pilot suggested that, consistent with the law of demand, prices matter for political participation, even when the size of subsidization of the cost of political communication is quite small. The depressing effect of prices was however no stronger for more marginalized populations.

Encouraged by the pilot’s findings, the research team collaborated with the National Democratic Institute (NDI) and the Parliament of Uganda to scale up the program to about half of the country. The scaled up version of the program, which covered the constituencies of 186 Members of Parliament, yielded, however, very different results: uptake in treatment constituencies was low, marginalized populations largely refrained from using the ICT platform, and there was no effect of price. In fact, because of the disappointing level of citizen engagement and apparent low interest among Members of Parliament (MPs), the Ugandan Parliament ultimately decided to phase out the SMS service altogether.

The differences between the findings from the two experiments are large and striking. The pilot suggested that mobile technology could democratize political communication; the scale up suggests no change in real politics. Since the controlled pilot was implemented using subjects from every constituency in Uganda, both experiments (the small-scale pilot and the scaled-up program) have been conducted with similar populations, eliminating a common external validity concern—that replications tend to fail because of unobserved features of the experimental subject pool (see (Allcott, 2015; Stokes, 2014)).

Especially striking have been the differences in the flattening effects in the two experiments. Past work suggests that the existing participation gaps, such as a gender gap, can be keenly sensitive to the ways that engagement is structured (Karpowitz and Mendelberg, 2014). In our discussion we consider a set of possibly important differences between the interventions.

The first is the simple difference in *scale*. Often described as a problem of general equilibrium effects, this concern is of particular salience when there are risks of treatment spillovers. In our case it is quite possible that collective action problems get altered substantially as scale increases. Insofar as political communications complement each other, or substitute for each other, increases in scale could lead to greater or lower overall levels of communication. Whether scale effects lead to differences in flattening can depend on complementarities in different subgroups.

A second possible reason for the divergent results relates to *players*. Whereas the research team implemented the controlled pilot, the National Parliament led the scaled up system. Such differences in player across scales is common, if not typical: for example the

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<sup>1</sup>A for underlying demand, uptake in the controlled experimental pilot was about 5%. As for flattening, almost half of users (47% of those sending messages to politicians) were women whereas only 26% of citizens that can be considered “highly engaged” using traditional forms of political participation were women.

Millennium Villages initiative sought to assess the scope for government led development change by examining an intervention in which government was not the primary actor. In our case this change in players not only might have affected citizen expectations regarding responsiveness to their message, but also opened up the possibility for more engagement by MPs who could for example encourage constituents to make use of the system in ways not anticipated in the controlled experiment.

The third possibility relates to *experimental design* and specifically, to the possibility that details of the mode of treatment delivery are consequential for uptake. We focus here on two possibilities. The first is the possibility that the method of delivery introduced a *treatment compliance effect*: that marginalized populations were simply less likely to hear (and not less likely to respond, conditional on hearing) appeals issued through mass media invitations. The second possibility is that the method has an *invitational effect*. Provided in the context of a survey, the invitation may appear as a more personal invitation to engage in politics. A direct personal invitation may have an empowering effect, signaling receptiveness and the possibility that political communication will make a difference. A direct personal invitation to participate in politics may have a large marginal effect for marginalized populations who have, on average, lower political efficacy to begin with (Isaksson et al., 2014).

These last two mechanisms are closely related yet distinct: one is about whether an invitation was deemed personal and the other is whether an invitation gets heard at all. A feature of the pilot experiment is that, by construction, enumerators’ invitations were personal and they were clearly heard and internalized. In the scaled-up national intervention, the radio ads were impersonal, and there was no guarantee that they were being heard.

We used a number of strategies to adjudicate between these three possibilities. First we exploit a feature of the scaled up experiment in which there was variation in the *feedback* provided to voters on the behavior of others. This allows us to examine whether exacerbating collective action problems due to scale can, at least partially, account for the divergent results. Second we conducted a citizen endline survey with a nationally representative sample, which allows us to assess—albeit with some lag—ex-post differences in treatment compliance. Third, we implemented an additional “mechanism experiment” in one district in Uganda in which we specifically varied the invitational component. From these explorations we find relatively strong evidence in favor of the *treatment compliance effect*; i.e., that, at least some of the differences in female participation between the controlled pilot and the program at scale can be attributed to differential take up due to differential access to radio. By contrast, though we find that personal invitations indeed increase uptake, we do not find evidence that women respond differently to personal appeals to contact their representatives. We also do not find evidence that scale itself is driving our divergent results.

In the remainder of this paper we introduce the research questions that the different field experiments were designed to answer and present the design and results from the scaled-up national program. We then present analyses designed to assess mechanisms that could account for differences in outcomes. Our conclusions focus on the implications for efforts to democratize political communication, and on the implications for learning about political processes from controlled experiments.

## 2 Access as a Constraint on Political Communication

In many low-income countries, the aggregation of preferences and priorities is limited, by the weakness of civil society organizations, labor unions and political parties. Potential preference aggregators, such as unions and non-government organizations (NGOs), tend to be located in urban centers and to have a narrow membership base. Political parties may have a wider reach, but many are weakly institutionalized and lack resources and elite cohesion (LeBas, 2011). That parties are often organized on ethnic or geographic basis—rather than by class or religion—further contributes to the non-programmatic nature of almost all political parties, in Africa and beyond (Riedl, 2014). Although voting rates among the poor are sometimes higher (Kasara and Suryanarayan, 2014) this may reflect differences in mobilization (or the repression of mobilization efforts) in different contexts and does not extend immediately to other types of engagement.

The political implications of weak preference aggregations are manifold. Existing preference aggregators, such as political parties, often focus on valence (rather than wedge) issues that offer voters little policy differentiation (Bleck and van de Walle, 2012). A focus on valence issues, such as fighting corruption and improving security, means that parties have a weak incentive to elucidate the preferences of marginalized populations, such as poor people, women, and those living in more remote areas (Grossman, 2015). When parties are non-programmatic, the accountability relationship between office holders and voters can narrow down to local clientelistic exchange. In addition, the high cost of traditional forms of political communication—e.g., traveling large distances to meet public officials in person—further reduce citizens’ incentive to be proactive in reaching out to politicians and parties in order to articulate interests, policy priorities and preferences.

Unsurprisingly, Members of Parliament (MPs) we have surveyed in Uganda describe themselves as insufficiently informed when they vote in plenary and in committee meetings, and surveyed citizens generally feel disempowered, commonly expressing low levels of political efficacy. Given the above background conditions, we ask: can simple technological innovations to improve channels of political communication alter MP-constituency relations? Against a backdrop of studies that seek to improve accountability relationship by increasing the information that citizens have about politicians, this research project focuses instead on the fact that representatives cannot represent if they lack information on their constituents’ preferences and needs. Indeed, our study is premised on the idea that improving the information that politicians have—and citizens’ awareness that politicians possess such knowledge—may be just as important as improving the information in the hands of voters.

A starting point of our study is the rapid penetration of mobile technology across the developing world and the scope this has for altering patterns of political communication (Leo et al., 2015). Naturally, however, implementing costly IT communication platforms can be an engine of political change only if there exists an underlying demand, particularly among the marginalized, to communicate preferences, interests and needs to their representatives via text-messaging.

In (Reference Omitted) we explored these questions using a controlled experiment delivered alongside a survey conducted in every political constituency in Uganda. The experi-

ment sought to assess whether demand existed, especially among marginalized populations, and whether this kind of platform would lead to flattening of political access or, rather, an exacerbation of existing inequalities. Indeed, many observers have raised a concern that IT communication platforms would be adopted disproportionately by male constituents, the better educated, and the rich who are also more likely to be technologically savvy.

In the controlled experiment, sampled respondents were invited to send a text message to their MP at randomly assigned prices (full, free, or subsidized). Discussed in more detail in [authors' citation omitted], the uptake recorded in the controlled small-scale pilot—about 5%—suggests that a sizable number of citizens value the opportunity for interest articulation provided by the introduction of an SMS channel to MPs. We note that five percent uptake is only slightly lower than the share of voters participating in party primaries in the U.S (Gans, 2010), and equivalent to the number of attendees in relatively large scale public deliberations over oil revenues in São Tomé e Príncipe. (Humphreys et al., 2006) and to the number of citizens joining online discussions during Iceland's deliberative process over a new constitution (Magnusson, 2013).

We found that rates were higher among more marginalized populations, possibly reflecting the fact that these populations have fewer opportunities to access politicians and may therefore place a higher value on impersonal and inexpensive communication channels. We were able to probe the demand side by experimentally manipulating the price of sending a text message to one's MP, and found, as expected, that reducing the cost of communication encouraged usage.<sup>2</sup> Moreover, consistent with the idea that marginalized populations place a higher value on cheap impersonal communication, we found that marginalized populations were not more sensitive to the cost of political communication than less marginalized populations.

The controlled experiment allowed only a 'one-shot' opportunity to communicate with MPs, and thus was unable to examine usage patterns over time, in which citizens' behavior is likely (also) a function of both the usage of *other* citizens and the response of their MP to past messages. Moreover, it was implemented in an usual environment— in the context of an in-person survey in which subjects had an extended interaction with an enumerator regarding their political views— which in turn may have made politics more salient to interviewed and invited subjects. Building on core findings from the Get-Out-The-Vote literature, the one-on-one contact with the surveyor and the personalized invitation to contact one's MP may have increased both the sense of empowerment and civic obligation to act politically and raise one's voice. Subjects may have also perceived the controlled experiment as closer to a civil society effort than an official government program.

These considerations raise the question of whether whether similar effects would be found when the intervention was brought to scale, and shifted from being a researcher-led and controlled intervention to being an institutionalized part of national politics.

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<sup>2</sup>Take-up was almost 50% higher for those who were randomly assigned to a free SMS treatment arm, as compared to those who were assigned to a treatment group that was not offered any subsidy for texting their MP.

### 3 Scaling Up

The scaled up field experiment was a part of the Parliament of Uganda’s national strategy for widening political voice. As part of that strategy, a case management platform hosted in the Ugandan National Parliament was developed, allowing citizens to send messages to their MP via SMS or a voice call to a call center. MPs randomly assigned to participate in the program (“*uSpeak*”) were given access to the platform and trained in its use. The platform allowed MPs to log onto a dashboard where they could read tagged SMS messages from constituents, reply, and see simple descriptive statistics (queries) about the messages they received, such as what the priority issues in their constituency were within a selected time-frame. A screenshot of the query dashboard is presented in the Appendix (Figure 9). Only treated MPs were able to receive text messages from their constituents via the case management system.

The study used a group of 186 MPs who volunteered to be part of a six-month pilot phase. It was expected that, if deemed successful, all MPs would be phased into the program at the end of the randomized control trial. Given the sensitivities of providing a new service to only some constituencies, it was agreed that MPs would be selected into the program using a public lottery, managed by the National Democratic Institute (NDI). NDI used block randomization to assign MPs to treatment groups; MPs were sorted into bins based on their *type* (District Woman MP or Constituency MP), *political party*, and *region*.<sup>3</sup> Over the following 6 months selected MPs and their constituencies took part in the *uSpeak* program while non-selected MPs did not. Again prices were randomly varied (across MPs and over time; we provide more detail on the randomization scheme below).

The ICT platform was promoted to citizens through short radio advertisement spots, played twice daily on local radio stations over a six months period. The 30 seconds advertisements were in local languages, and featured a skit where actors portraying constituents talked about how *uSpeak* could be used to draw the MP’s attention to important issues, specifically service delivery deficiencies. These skits were first tested using focus groups of ‘ordinary’ residents in Kampala. A second tier of randomly assigned treatments—price and feedback—was also delivered via the radio ads.

#### 3.1 Variation in Price

To assess the effects of price on uptake and message content, we randomly varied the cost of sending a message to MPs via the *uSpeak* system, across and within constituencies. Each constituency was assigned 3 months in which *uSpeak* would be provided free of charge and 3 months without any subsidization.<sup>4</sup> Being sensitive to potential sequence effects, all possible sequences of full price and free months were randomly assigned to constituencies in

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<sup>3</sup>Each bin was used to implement a separate public lottery with a target number of MPs selected into treatment based on that MP type’s prevalence in the subject pool.

<sup>4</sup>The cost of sending text-messages to the *uSpeak* system was 110sh, which was equivalent to about 2 cents, and about double the cost of sending a typical person-to-person SMS during this period (prices in Uganda have since dropped). The system was more expensive than typical messaging because the system relied on a shortcode, a special telephone number that is shorter than a full telephone number, and is used to address SMS messages from mobile phones.



the treatment group using a blocked design. Note that while the variation in prices in the first period provides a clean separation into price groups, identification based on variation in subsequent months must assume no carryover effects.

### 3.2 Variation in Feedback

In order to examine whether information on *others'* usage encourages greater uptake, we added a 'feedback' treatment arm delivered through modification of the base radio ads. In this version, voters heard that others had been sending messages to the system about the need to do more in the educational sector. A second variation also highlighted the educational sector but without communicating that others had been using the system to lobby in that area. To the extent that there are complementarities in public goods messaging, we expect that hearing that others are sending messages about education should increase the willingness to contact one's MP. Indeed, our feedback skit was written explicitly in a way that made this sort of complementarity more apparent to radio listeners.<sup>5</sup>

There were eight unique price sequences that fulfilled the balance requirement (3 of each type), and six unique combinations of the feedback treatments. Together there were 48 unique combinations of price and feedback sequences. These were assigned in a balanced way to treatment constituencies, resulting in roughly 2 constituencies of each unique treatment schedule. Figure 10 (Appendix) provides an example of treatment schemes for a subset of treatment constituency.

### 3.3 Data

Data for testing the effects of the national scaled-up experiment come from four key sources: (1) a baseline survey of all constituencies in Uganda, conducted immediately following the 2011 Parliamentary election, (2) the SMS messages sent by constituents to the uSpeak system/their MPs, tagged with the date and time they were received, (3) a callback phone survey we conducted with users of the uSpeak program roughly 2 months from the date they sent their message, and (4) an end-line survey of a nationally-representative sample of Ugandan adults. In addition, we conducted a follow up small-scale field experiment with about 3000 experimental subjects in Arua district.

## 4 Main Results

We focus on the main results related to overall uptake, price and feedback effects, and the voter population that used the ICT system. Other results, such as price effects on the type of message sent, are available in the online appendix.

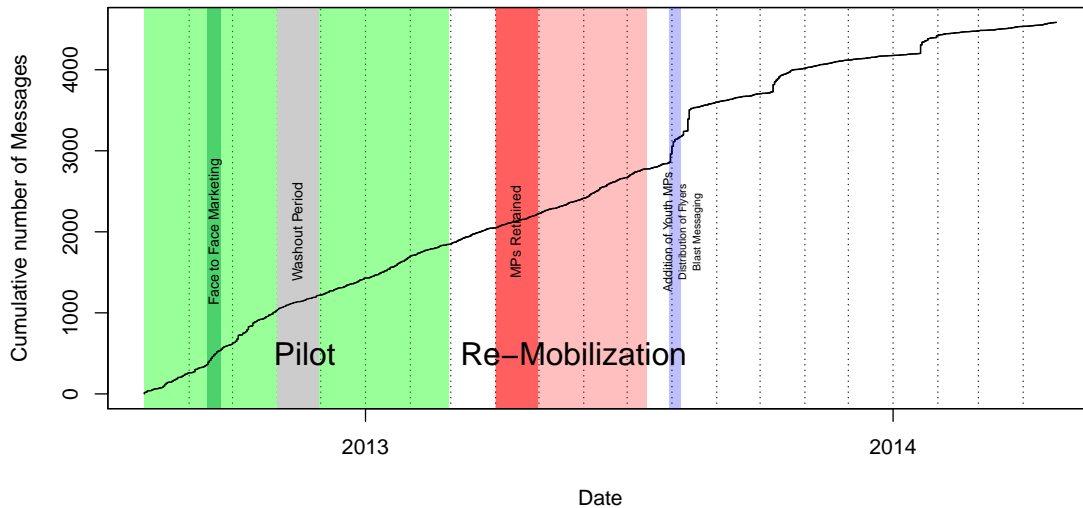
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<sup>5</sup>By contrast, if people view text messages as substitutes, then hearing that others are using the IT system is expected to exacerbate the collective action problem.

## 4.1 Weak Uptake

Unlike our controlled experiment, uptake in the full-scale intervention was very low.<sup>6</sup> Despite twice daily radio ads and price subsidization throughout the country, MPs in the treatment group received a total of 1946 messages during the 6-month study period. Although census data for Uganda is outdated (2002), we estimate conservatively that the radio ads were played over an area where 10 million voters live. This uptake then corresponds to a monthly usage of about 1 in 30,000. Figure 1 (see also Figure 2) shows the cumulative messaging over time, extending well beyond the pilot phased to show uptake in the post pilot period including various periods in which an assortment of mobilization efforts were used by parliament and NDI—none of which produced sustained effects.

Figure 1: **Full Scale Intervention: Uptake**



**Note:** Cumulative messaging over time. Gray area represent the wash-up period after the third month in which no radio spots were played.

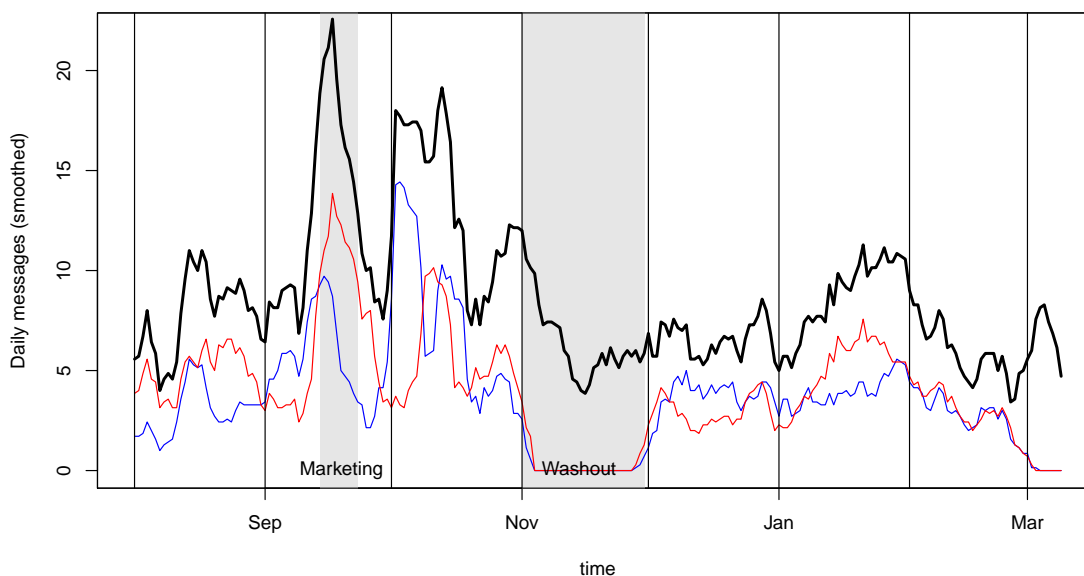
## 4.2 Insensitivity to Price

Unlike the controlled pilot we see almost no evidence of sensitivity to price in the scaled up interventions. Figure 2 shows the month to month rate of messaging in the free and full-price treatment conditions. In order to test for a price effect more formally, we run a linear regression of the number of messages received in a given month on price—a binary

<sup>6</sup>Note that the level of uptake is not an experimental treatment effect; rather it is a level assessed under controlled conditions.

variable that takes the value of one for full price and zero for months of free messaging—controlling for the month and whether or not we also included feedback, with fixed effects for MPs. Results presented in Table 1 suggests that contrary to earlier findings, in the full scale national program, price was not a significant factor in determining the amount of messaging.

Figure 2: **Price Effects**



**Note:** The number of messages per day sent at full price are represented by the blue line, while messages sent for free are represented by the red line. Price factor was irrelevant to uptake.

### 4.3 No flattening effects

One of the key findings of the controlled small-scale field experiment was that the share of marginalized populations—including women and the poor—among system users was higher than the share of marginalized people participating in traditional forms of political engagement. That finding formed the basis of our conclusion that ICT platforms have a real potential of flattening political access, since they allow marginalized populations to capture a larger share of the voices that are being heard by MPs. We thus turn to analyze whether such flattening is also present in the scaled national experiment.

In order to identify the characteristics of those who sent text-messages to their MP using the uSpeak system, we conducted a phone-based callback survey of system users. Using a call center that the research team had set up in the capital Kampala, local enumerators contacted all uSpeak users no longer than two months after they had sent a text message

Table 1: Uptake as a function of price and feedback

	<i>Dependent variable:</i>	
	MESSAGES	
	(1)	(2)
Price	-0.081 (0.262)	-0.079 (0.262)
Education prompt		-0.275 (0.453)
Education plus Feedback Prompt		-0.132 (0.452)
Observations	660	660
R <sup>2</sup>	0.055	0.055
Adjusted R <sup>2</sup>	0.045	0.045
F Statistic	5.251*** (df = 6; 544)	3.972*** (df = 8; 542)
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01	

to their MP. The short callback survey was designed to elicit information on users' demographics, on whether they received a response from their MP, and their general satisfaction with the ICT service. More information on the logistical aspects of the callback survey can be found in the online appendix.

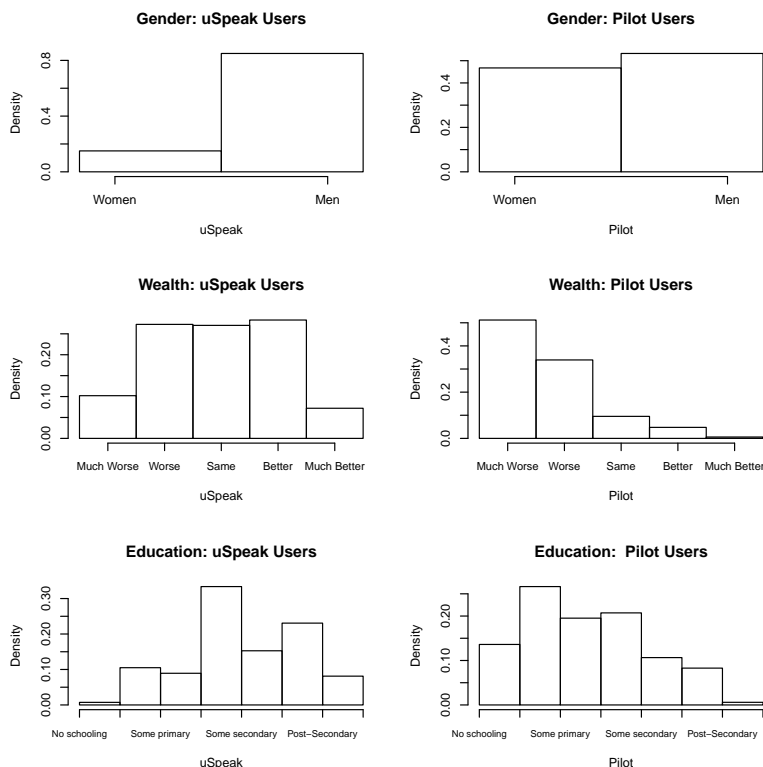
Comparing results from our callback survey to information culled from the control pilot, it is clear that our scaled-up national program failed to replicate the flattening effect identified in the controlled small-scale experiment. Specifically, the users of the uSpeak system were wealthier, more highly educated, overwhelmingly male, and younger compared to those sending text-messages in the controlled pilot. Put plainly, the uSpeak program failed to elicit participation from marginalized population in the way the controlled experiment did. This conclusion is clearly shown in Figures 3 and 4 that provide information on the distribution of wealth, gender, education, and age of SMS senders across the two experiments.

#### 4.4 No price effects

That only a small number of politically engaged citizens from traditionally powerful group chose to send an SMS to their MPs via the new IT system, can help explain the lack of evidence of a price effect. Given that at the time of the study the average cost of text-messages was lower than 110 UGX (equivalent to about 2 US cents over the course of the program), it is reasonable to interpret the null effect of the price treatment as stemming

from the fact that highly engaged relatively well-off citizens are not price sensitive when communicating directly with MP is at stakes.<sup>7</sup>

Figure 3: **Demographic Differences: Users in the Pilot versus Full Scale Program**



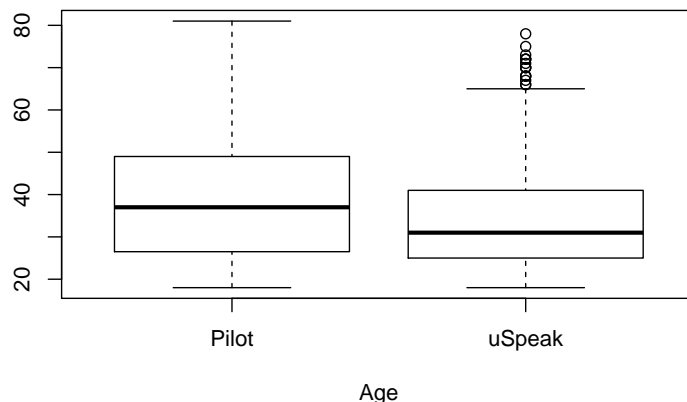
**Note:** Users in the controlled experiment were more likely to be female, less educated, and poorer.

## 4.5 No Evidence of Downstream Effects

Thus far we have shown that uptake in scaled-up uSpeak program was very low and that fully subsidizing the cost of messaging did not increase voters' proclivity to contact their MP via SMS. Notwithstanding the low rates of usage, it may still be that uSpeak has a positive effect on voters' sense of efficacy and their satisfaction with politics in Uganda. This would be the case if citizens view the implementation of ICT platforms, irrespective of one's own usage, as an important tool for strengthening citizen voice. This was a goal of the intervention and we report on it here briefly. Note that the results in this section and supplementary material use experimental estimates of the effects of the intervention, exploiting the random assignment of the scaled up program. Previous results on uptake and differential uptake do not make use of this random assignment.

<sup>7</sup>The divergence observed in price effect across treatments is reminiscent of the way subjects of controlled laboratory experiments react to even small monetary manipulations that are insignificant and thus inconsequential outside the laboratory setting.

Figure 4: **Age: Pilot versus Full Scale Program**



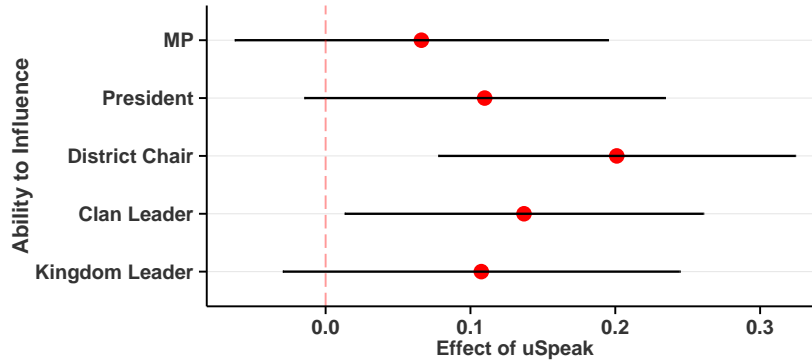
**Note:** Users of the uSpeak program were younger than pilot users.

To test for the effect of the scaled program on voters' efficacy we conducted a national representative survey in both treatment (i.e., constituencies that their MP was selected to be part of the uSpeak program) and control constituencies. The survey, which took place in July-August 2014, included 2,714 people from 76 constituencies and 304 villages in 52 districts across Uganda. We provide descriptive statistics of the endline citizen survey in the online appendix.

To measure efficacy we asked our survey respondents whether they agree or disagree with the following statement *People like you can do things that can have an influence on the actions of ... [your constituency MP]*; we then repeated the question for the president, district chairperson, clan and kingdom leaders which serve as placebo tests. Our key dependent variable is a binary indicator that is equal to one for the 60% of respondents who had agreed that citizen action could influence their MP. We then run a simple OLS model regressing the efficacy outcome on a treatment indicator and district fixed effects. Results, presented in Figure 5 suggest that the uSpeak program had no discernible effect on voters' sense of efficacy. We then repeat the analysis, conditioning the effect of uSpeak on those actually hearing—or more accurately, report hearing—about the program. Again, we find no discernible effect of the uSpeak program on efficacy, even among the subset of voters that are aware of the ICT platform.

In the online appendix we provide further evidence that the national scaled intervention did little to effect citizens' level of satisfaction with their MPs, or their level of political participation more broadly.

Figure 5: **Efficacy Effects**



**Note:** The marginal effect of uSpeak on political efficacy measured as respondents’ perception of their ability to impact their MP. Block analysis takes account of blocked assignment.

## 5 Explaining Conflicting Results

Our findings from the scaled up intervention conflict with what were predicted by the controlled experiment. The Parliament’s communication strategy resulted in very low uptake, even when the service was offered to voters at no cost. In addition the program did not have a discernible effect on voters’ sense of political efficacy. Moreover, when it was used, it was used by exactly those citizens whose voice is already heard. In other words, the groups that have the weakest access to political processes were also the least likely to access and use the new ICTs platform.

Although the pilot led by the research team was meant to capture the key features of the scaled up IT system, the introduction of relatively tight experimental control introduces a number of differences.

One set of differences include features uniquely related to the scale of the intervention: for example the collective action problem may be greater when implemented at scale, especially when subjects can accurately infer the magnitude of the program from its delivery method, as is clearly the case in our study. We refer to this possible cause of the conflicting results as a *scale effect*. A second possibility relates to the political context: the fact that the scaled intervention was implemented by Parliament and promoted by politicians may have altered incentives of populations, and politicians, to engage. We refer to this as a *player effect*: when taken to scale different players commonly get implicated.

Two other explanations relate more directly to experimental controls. WE will call these *design effects*. The first design effect we are interested in examining is the possibility that Ugandans simply did not hear about the program since radio listenership is low, especially among women, less educated and poor constituents. This differs from the controlled setting where there is no doubt over the delivery of the intervention. We refer to this design effect as the *treatment compliance effect*—that women and poor were simply less likely to hear (and not less likely to respond, conditional on hearing) appeals issued

through impersonal mass media invitations. A different possibility is that uptake was relatively high in the controlled small-scale experiment not simply because we ensured that all experimental subjects were aware of the new ICT platform, but because the enumerators had personally invited respondents to contact their MP at the end of our baseline survey. As mentioned above, such direct personal invitation may have had an empowering effect, signaling receptiveness and the possibility that political communication will make a difference. Since enumerators handed out fliers with explicit graphical explanations of how to send a text-message to one’s MP, the in-person invitation also allowed overcoming technological barriers. Specifically, an invitation to participate in politics may have a larger marginal effect for marginalized populations who have lower political efficacy and greater technological barriers to begin with.

We have some evidence from the intervention that shed light on some of these possible explanations and we gathered additional evidence to parse others.

## 5.1 Scale effects

Political communication involves a problem of collective action. If many others are lobbying a politician for a private good the gains from additional lobbying may be lower than when few are. However when many others are lobbying for a common good there may be increasing returns to lobbying (or here too, there may also be increasing incentives to free ride). In short, if the incentives to engage in political communication depend on how others are engaging then outcomes at a small scale may look very different to outcomes at a large scale. From this perspective weaker participation from the scaled up program may reflect a simple failure of collective action.

One piece of evidence that sheds light on this logic is our evidence on the effects of feedback messages. Recall that in the scaled up experiment there was variation in the communication to voters about the level of activity by other voters in the previous periods. In particular a random subset were told that, through the radio commercials, that other voters had been using the system to raise issues around education. Under a free riding logic, such information would depress engagement among those exposed to it.

Returning to Table 1, however, we find no evidence of sensitivity of engagement to information on uptake by others—neither the difference between feedback on education messages and standard marketing, nor differences between education marketing with and without feedback is significant. This is consistent with a set of analyses of the controlled experiment that found no evidence for strategic engagement with the system. We conclude that scale by itself does not seem the factor driving our divergent results.

## 5.2 Player Effects

Another reason for the divergent results across experiments might stem from what we have termed above *player effects*.

Plausibly citizens’ usage of mobile messaging increases with the belief that there is a receptive MP at the other side of the interaction. Which system should voters expect to



produce greater responsiveness by politicians? On one hand, citizens may believe that their MP will take their messages seriously if an NGO mediates the relationship between voters and their representatives; for example, if it follows up in case some messages get ignored. On the other hand, the scaled up system is formally owned and led by parliament, which signals some level of commitment by politicians. In addition the dynamic nature of the scaled-up program —i.e., the ability of MPs to interact with citizens directly via the IT platform—allows MPs to signal their responsiveness directly. This sort of dynamic reciprocal relationship could not have been established in the controlled one-shot pilot. Thus was hard to predict *a priori* how the change in the identity of the implementer would affect citizens’ proclivity to use the system.

We explore (non-experimentally) player effects in two ways. First we use the callback survey ( $n = 2,517$  system users) to calculate response rate at the constituency level and then test for a correlation between MP’s responsiveness and the volume of messaging at the constituency level. We find that only 9 percent of uSpeak users report ever hearing back from their MP; in fact, in almost half of constituencies (44) *not a single uSpeak user had received any response from their MP*. Moreover according to records available to NDI, the majority of MPs did not even read most of the messages sent to them. As expected, we find a positive correlation between messaging and responsiveness, which is consistent with citizens low engagement being a response to their MP’s (in)action during the scaled-up study period.

This analysis focuses on system users—a self-selected group, but we can also assess broader expectations regarding MP responsiveness may have contributed to the low response rate among the general population. Here we examine responses in the endline survey when our national representative sample was asked to indicate reasons for why people might not use SMS systems like uSpeak to communicate with their MPs. Figure 6 provides information on the share of respondents in treatment constituencies that indicate each of the possible reasons, which were read out loud by the enumerator, broken down by gender. We find that close to 50 percent of respondents report that they did not send a message because they believe their MP would not be responsive.

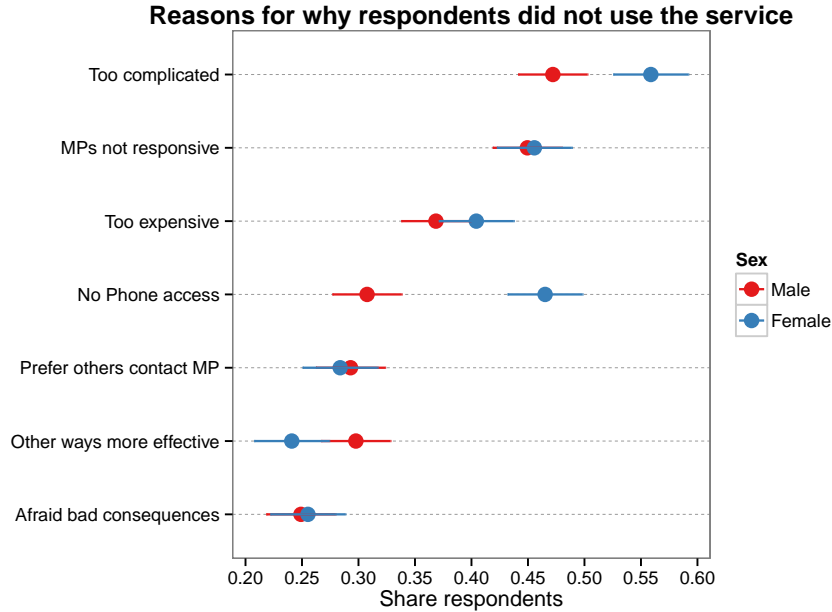
We do not have information on the expectations of responsiveness from MPs in the controlled experiment and so cannot compare directly. Nevertheless the statements by citizens and the very weak responsiveness by politicians suggests that the very weak engagement with the scaled up program was a rational response on the part of citizens.

### 5.3 Design Effects I: Treatment compliance

To test for compliance effects we asked respondents in our endline national representative survey directly whether they have ever heard about uSpeak. The survey was implemented a year after the six month radio campaign, though at a time when the system was still active and promoted by parliament. In light of the time gap, we used a deliberately strong prime in which we play the original uSpeak radio ad and ask respondents if they have heard of uSpeak.

We find that 17% of respondents in control constituencies and 24% of respondents in treatment areas have self-reported that they ever heard of uSpeak. We note that this is

Figure 6: **Reasons citizen give for not using uSpeak**



**Note:**Reasons Ugandans gave for not using uSpeak.

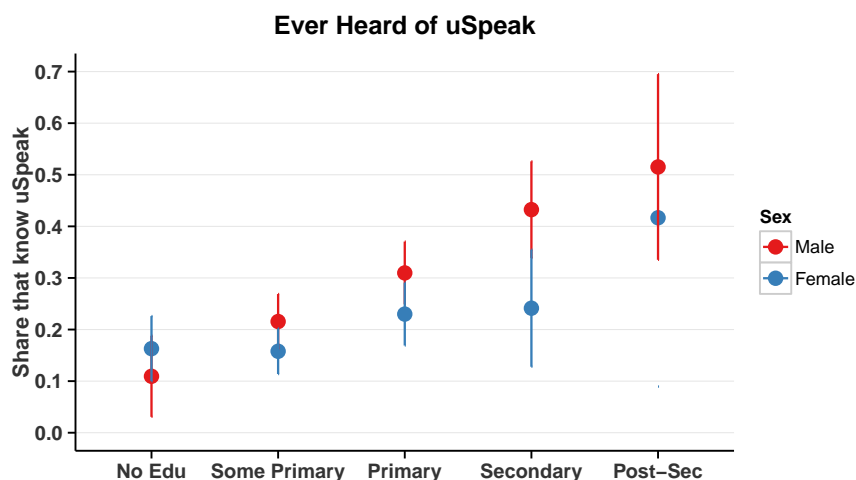
likely an upper bound due to social desirability bias. Also note that not all the control respondents are necessarily lying about their knowledge of the program; this is because radio signals normally have a range that encompass more than a single parliamentary constituency.<sup>8</sup> When probing deeper about respondents’ knowledge of the program we find that only 6 percent of treatment respondents were able to confidently say that their MP had participated in the program. Moreover, when asked to repeat the short code, less than half a percent of treated constituencies claimed to know the short-code to send a text-message to their MP and an additional 3% report they once knew the number but have forgotten it. These findings strongly point to the limitation of radio spots to garner sufficient awareness to the new service.

Turning to the differential finding regarding marginalized groups, Figure 7 breaks down the level of awareness of the uSpeak program by gender and education in treatment constituencies, showing that not only women are less likely to hear about uSpeak than men, but also that awareness of uSpeak increases with level of education. This finding provides some tentative evidence of the relevance of a treatment compliance effect.

To further test whether a treatment compliance effect is responsible to the large gender gap in participation, as compared to the controlled pilot, we asked our respondents about radio ownership. We find that 0.71 of female respondents reside in a household that owns a radio in good working condition, compared to 0.76 of men. More so, since owning and listening to radio are distinct, we further asked our nationally representative sample

<sup>8</sup>We minimized spillover by using the name of treated MPs in the 30 second adverts.

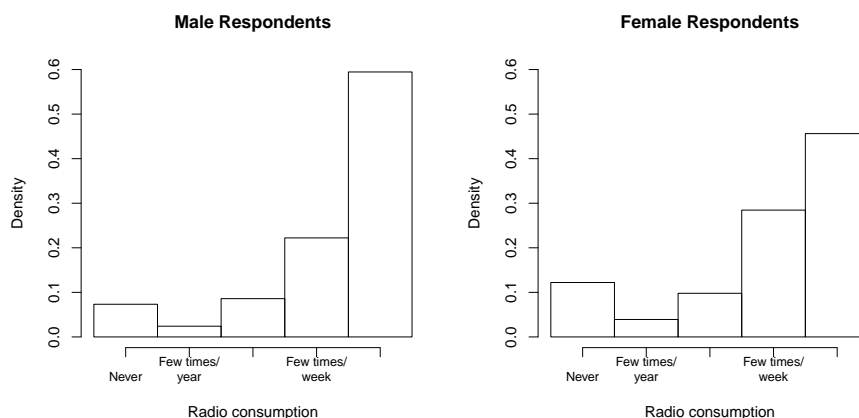
Figure 7: Awareness of uSpeak by gender and education



**Note:** The share of Ugandans who report ever hearing about uSpeak.

about their frequency of radio consumption on a five-point scale (from “never” to “daily”).<sup>9</sup> Figure 8 that provides information on the distribution of responses broken down by gender, indicates clearly that women listen to radio much less frequently than men. Together, the above findings provide a rather strong evidence of treatment compliance effect as one explanation for the relatively low participation rate of marginalized groups in the uSpeak program.

Figure 8: Radio listenership by gender



**Note:** Distribution of radio consumption by gender. Source: citizen end-line survey.

<sup>9</sup>The question verbatim was: *How often do you get news from the following sources?*

## 5.4 Design Effects II: Invitational Effects

An alternative explanation for the differences in findings lies in the invitational nature of the controlled pilot. Recall that the two experiments differed in their mode of ‘marketing’: whereas the national program used impersonal radio 30 second spots, in the controlled pilot, survey respondents were personally invited by the enumerator to contact their MP. As mentioned, direct personal invitation may itself have an empowering effect, signaling receptiveness and the possibility that political communication will make a difference. If these effects operate differentially for marginalized and non-marginalized populations this could account for the differences in observed flattening effects.

To assess this possibility we implemented a third experiment, this time exploiting an existing SMS platform, UBridge, developed in partnership between UNICEF’s Ureport platform and Uganda’s Governance, Accountability, Participation and Performance [GAPP] project and operative in Arua district, Uganda.<sup>10</sup>

UBridge is designed to open a new channel of communication from citizens to local government officials to report public service deficiencies. Based in the Arua district, it is a voluntary program, where citizens actively register to participate in the sending and receiving of messages. Using the UBridge technology, citizens can engage with Arua district government officials in three ways. First, they can send unsolicited “anonymous” messages to the UBridge short code (8500), at *no cost*. District officials, in both technical and political positions, are equipped with 3G tablets that enable them to access the messages anywhere, provided they have Internet access. Second, citizens can respond to short weekly polls (usually a single question) solicited from UBridge registered members by the research team. The polls are conducted on weekends using a robocall system operated by VotoMobile. Third, citizens have an opportunity to attend community meetings, implemented and organized by GAPP, where they received information about national service standards, the performance of their own district and community, and learn about the actions they can take to communicate with local officials. The first round of meetings was held by GAPP in September to October 2014 as part of the service launch.

The UBridge system was launched as a pilot study in September 2014 in 90 villages across Arua district, which were clustered into 24 village-clusters. Clusters in the GAPP program are defined as the group of nearby villages that are serviced by the same public health center. Arua has 48 government health centers (i.e. 48 clusters), half of which were randomly sampled as control areas and half as treatment areas. The UBridge system has been introduced only in treatment clusters.<sup>11</sup> A study evaluating the effect of getting access to the UBridge system is underway and is not the subject of this paper.

At the time of our ‘mechanism’ experiment there were 4,568 UBridge registered users, the majority (3,947) selecting into the program through a door-to-door registration campaign that took place between October and November 2014 in the 24 treatment clusters (all

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<sup>10</sup>With some loss in external validity, our design aims to keep the *treatment compliance* effect constant by focusing on respondents in the UBridge system. We hope that parsing the outcome compliance effect will be the focus of future studies.

<sup>11</sup>The mean number of villages per cluster is 3.75; the minimum number of villages per cluster is 1 and the maximum is 6

such users have provided explicit consent to participate in the program). An additional 373 users registered following GAPP’s community meetings, where sign-up sheets were passed between attendees. Finally 248 system users have registered with UBridge independently; for example, by following registration instruction on flyers that were distributed in cluster areas during the registration campaigns and the community meetings.

In order to verify which of the UBridge registered numbers are valid, a team of local enumerators was hired by UBridge to personally call registered numbers. The enumerators, who called each number up to three times, were able to positively verify 2,810 mobile numbers: out of which 2,335 completed the survey, 75 began talking to one of the enumerators but the phone disconnected before the poll was completed, and 400 refused to complete the phone survey. Additional 1,750 numbers were not ‘purged out’ of the system since they at least answered the phone in one of UBridge’s past weekend polls, for a total of 4,568 UBridge registered users. The verification exercise was completed on June 8, 2012.

On Saturday June 13, UBridge conducted a baseline poll using VotoMobile’s robocall system asking about attitudes with respect to budgetary processes. The key outcome of interest is a binary variable that receives the value of 1 if the UBridge user responded to the poll, and 0 otherwise. 12% of 2,720 respondents (verified users whose gender is known to UBridge) responded and shared their views with UBridge. Consistent with past findings, there is a significant difference in response rates across gender groups: 7.6% of women responded compared to 12.2% of men. Thus in this baseline setting in which ICT is used, but invitations are weak and impersonal, there is again evidence of a large and significant gender gap in participation rates

Our experimental intervention sought to assess whether the gender gap that is evident in the UBridge panel will be reduced if messages are accompanied by a stronger invitational component. Note that our interest is not simply in whether invitations increase participation but whether they do so disproportionately for women.

To address these questions we asked UBridge to run a modified version of their baseline poll but now introducing a modest variation in treatment. All groups would be able to participate in a poll regarding taxation, similar to the previous UBridge poll. In a randomly selected treatment group however we preceded the call with a set of SMS encouragements that explicitly invited participants to take part in the weekend poll and that highlighted the importance of individual responses in the message. Full text of the intervention messages is provided in an appendix.

Our primary measure is the response (or non-response) by UBridge users to the weekend poll. The encouragement messages were delivered on 24, 25, and 26 June 2015, with the poll taking place on 26 June.

We used blocked random assignment, blocking on the following variables: Gender (2 categories); Age (3 categories); Constituency (4 + unknown); Recruitment method (3 categories: individual, group, Ureport); Verified sample (2 categories); Past responses (2 categories).<sup>12</sup>

We begin with a basic cross-tabulation of the raw data by treatment and gender for

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<sup>12</sup>Note that the “past response stratum” is nested within the verified respondent sample. Collectively these strata produce  $2 \times 3 \times 5 \times 3 \times 3 = 270$  possible subgroups.

our verified sample. Using the verified subsample, we find that in the control (baseline) condition the response rate of registered male users was almost double that of female users (11.2% as against 5.7%). Moving to the personalized invitation treatment, for both male and female UBridge registered users, overall response rate has increased by about 2 percentage points (13.4% against 7.5%). In order to more formally test whether there is a differential effect of the invitational treatment by gender we implement analyses pre-registered at EGAP’s registration web page.<sup>13</sup>

Specifically, we estimate average treatment effects using a regression, accounting for block fixed effects. Our analysis takes account of the variables used for blocking but introduces no further controls. Our primary regression uses only the verified subset of UBridge users, whereas our secondary analysis includes all registered users whether or not they have been positively verified. Results, reported in Table 2, suggest that though invitation had a strong positive effect on response rate (column 1), its effect was almost identical across men and women (columns 2 and 3). We conclude that personal invitations can have a powerful effect on rates of participation, but they do not necessarily have a differential effect by gender.

Table 2: Mechanism Experiment

	<i>Dependent variable:</i>		
	CompleteSurvey		
	Base	Primary	Secondary
	(1)	(2)	(3)
Invitation	0.021* (0.011)	0.021 (0.020)	0.021 (0.014)
Flattening (Male*Invitation)		0.0004 (0.024)	-0.003 (0.017)
Observations	2,717	2,717	3,957
R <sup>2</sup>	0.153	0.153	0.165
Adjusted R <sup>2</sup>	0.108	0.107	0.115
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01		

## 6 Conclusions

This study described the results of three field experiments in Uganda undertaken to explore the extent to which innovations in mobile technology can be used to alter constituent-politician communication in low-income countries. Evidence from a pilot study suggested not only that there is underlying demand to contact representatives using mobile technology, but also that SMS technology can produce increased levels of political engagement

<sup>13</sup>Note the primary and secondary analyses are as registered. The base column is added for reference and reports the simple effect of invitation without including heterogeneous effects.

in a way that flattens access for marginalized populations. By contrast, when brought to scale, we find very low levels of citizen engagement, with marginalized populations especially refraining from using a cheap and unmediated communication platform to raise their voice. These results have both policy and methodology implications

The implications for method relate to external validity and scale up. The literature on scaling up has largely focused on assessing the extent to which experimental estimates in one context apply in another. Some of this literature highlights the problems in using a small handful of studies as the basis for inferences to different contexts (Collaboration, 2015). Other work highlights the costs of extrapolation. Comparing non-experimental and experimental estimates that rely on the same data, Pritchett and Sandefur (2013) conclude that non-experimental estimates with the same subject population can better predict treatment effects versus experimental effects from other contexts as contextual variation can drive bigger differences in the estimated effectiveness of a program than selection bias.<sup>14</sup>

In the case of our pilot study and subsequent scale up, however, there should not have been significant differences in the subject population. The pilot study was offered to subjects in every constituency in Uganda. The scaled-up nationwide program was offered to a random sub-sample of a set of 186 constituencies<sup>15</sup> out of a total of 238. Thus the difference we observe draw attention to a distinct problem, which is the external validity across the nuts and bolts of *interventions* and not necessarily across *populations*.<sup>16</sup> This kind of validity problem is especially critical when lessons from carefully controlled small-scale studies are intended to inform policies to be implemented at a larger scale. Our results provide a cautionary tale for researchers and policy makers seeking to make such claims.

In our analysis we identified several distinct reasons why outcomes of small-scale experiments may fail to replicate when brought to scale. These include already well-appreciated effects that relate directly to scale (see also Deaton (2010) on general equilibrium effects). In addition we highlight possible effects related to the changing players involved when interventions are implemented at scale (see also (Bold et al., 2013) on capacity and motivation of implementing organizations), and we identify differences related to details in the design between controlled interventions and interventions implemented in the political wild.

Using an array of data sources—including two national representatives surveys, a call-

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<sup>14</sup>There are a couple completing methodological approaches to generating out of sample predictions based on experimental data. Hotz et al. (2005) suggest using subject’s observed characteristics as predictive of treatment effects independent of context. Gechter (2015) proposes a method that uses differences in outcome distributions for individuals with the same characteristics and treatment status in the original study and the context of interest to learn about unobserved differences across contexts. He is able to use his proposed framework and experimental data from an education intervention in one city in India to accurately predict outcomes in another city.

<sup>15</sup>Constituencies that were part of in the study were included on the basis that their MPs volunteered to participate in the program.

<sup>16</sup>Some of our divergent findings do relate to endogenous changes in populations as a consequence of the factors outside the control of the research team. For example, one reason we do not find price effects in the scaled-up program which we found in the controlled experiment, can be attributed to the fact that the national intervention was taken up by relatively well off and engaged citizens who are unlikely to be sensitive to a very small price subsidy.

back user survey and a third ‘mechanism’ experiment—we find evidence consistent with the idea that design details are what mattered. Specifically we find that due to its strong delivery method, the controlled experiment achieved a level of first stage compliance that could not be achieved by the scaled intervention, even one that relied on an aggressive campaign. Our mechanism experiment provides further evidence that personal invitations can drive up participation substantially, although it does not find evidence that this feature is what explains differences in flattening effects.

Ironically when design details matter, a first response is to resort to controlled conditions to get those details right. This might be an appropriate approach when seeking to control for all factors but a manipulated parameter of interest, but a lesson from this case is that the importance of those details may only become apparent once control is removed.

The variation in results however also provides substantive insights. Rather than simply noting differences, we have been able to learn from them.

First we learned from the controlled experiment that many people, including marginalized people, do want to communicate with their representatives in government using new technologies, and are willing to pay to do so. This stands in contrast to accounts of disengagement as reflecting alienation or apathy. We also know that many—though clearly not all (see Figure 6)—have the capacity to do so. The results from the controlled experiment support the idea that mobile technology could, under the right conditions, change the relationships between voters and representatives in the developing world. An examination of the scaled up system alone would have hidden these features. Second, from the variation in the scaled up intervention we see little evidence that the differences across experiments is due to pure scale effects: we do not see users responding strategically as a function of how many others are using the system. Third, from the mechanism experiment we learned that there is a strong responsiveness to invitations to engage, but we do not see evidence of the kind of differential responsiveness that would be needed to account for differences in flattening effects.

All of these findings suggest that problems are not present on the demand side. In contrast survey evidence suggests weaknesses in the system itself. Moreover, our analysis of *player effects*; i.e., that the change in the identity of the implementer, which was easily observed by experimental subjects, might have been consequential, suggest that general trust in the responsiveness of politicians is preventing engagement but is also rational. Interestingly in our case, player effects do not stem from motivation differences between implementers (as for example identified by Berge et al. (2012)), but rather from the way player identities interact with citizen expectations.

With the multiple pieces of evidence available to us we infer that the failure of the national system is not simply a function of weak demand on the part of citizens but is a function of larger inequalities that the intervention did not address—but which perhaps parliament may be able to address by tinkering with its outreach strategy—and in part a function of more fundamental weaknesses in the broader political system, which parliament likely cannot, or will not, address easily.



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## 7 Appendix: Extra Figures and Tables

Figure 9: Dashboard screenshot

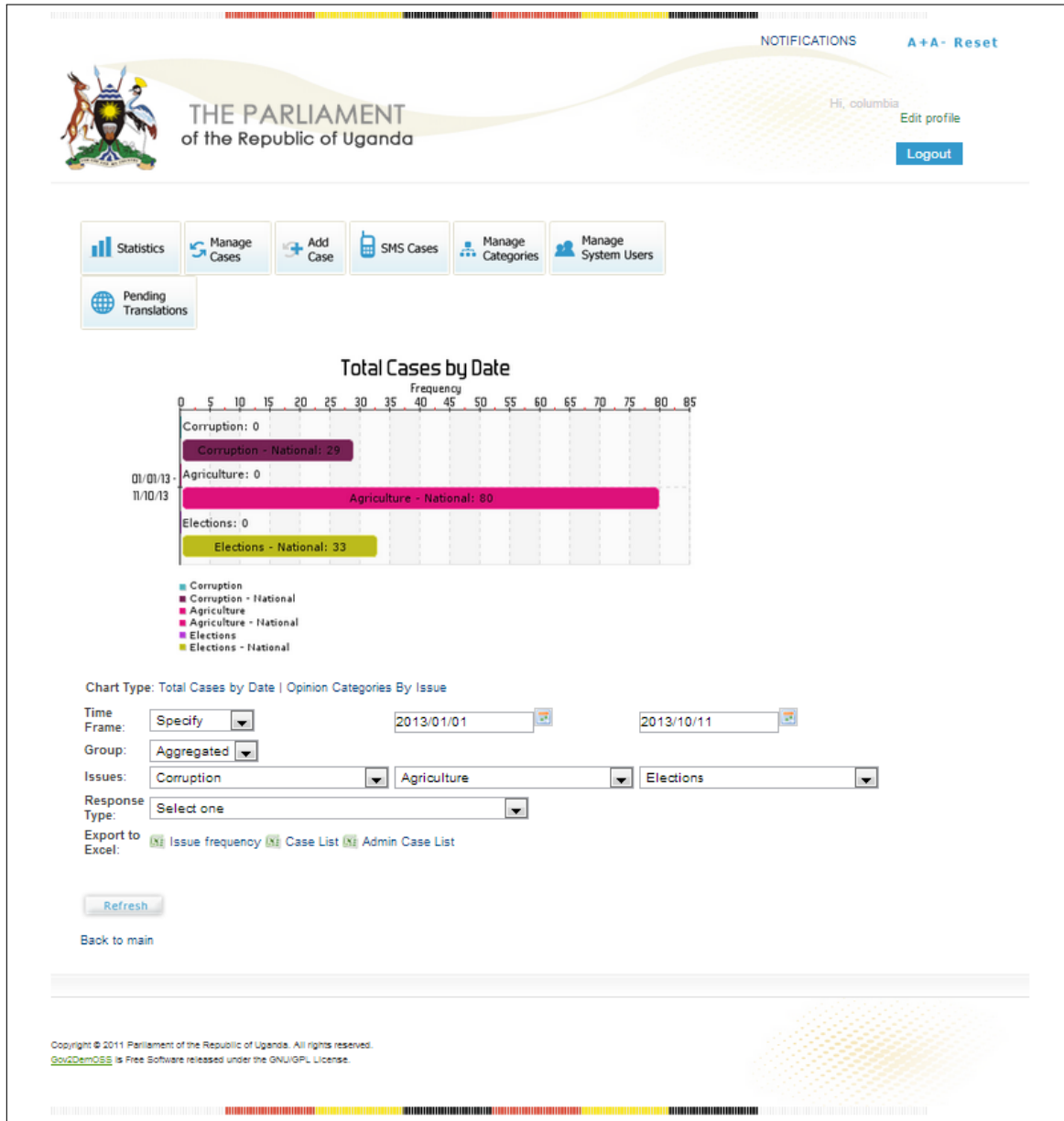


Figure 10: Treatment Scheme Example

District Code	District Name	District Type	MPs	Number of MPs	Month	Price	Feedback
1	APAC	MIXED	AJOK LUCY & AKORA MAXWELL EBONG PATRICK & AYOO TONNY	3	1	FREE	NONE
1	APAC	MIXED	AJOK LUCY & AKORA MAXWELL EBONG PATRICK & AYOO TONNY	3	2	FULL	NONE
1	APAC	MIXED	AJOK LUCY & AKORA MAXWELL EBONG PATRICK & AYOO TONNY	3	3	FULL	NONE
1	APAC	MIXED	AJOK LUCY & AKORA MAXWELL EBONG PATRICK & AYOO TONNY	3	4	FREE	SCHOOL + USERS
1	APAC	MIXED	AJOK LUCY & AKORA MAXWELL EBONG PATRICK & AYOO TONNY	3	5	FULL	NONE
1	APAC	MIXED	AJOK LUCY & AKORA MAXWELL EBONG PATRICK & AYOO TONNY	3	6	FREE	SCHOOL
2	ARUA	OPP	OKUONZI SAM AGATRE & WADRI KASSIANO EZATI	2	1	FULL	NONE
2	ARUA	OPP	OKUONZI SAM AGATRE & WADRI KASSIANO EZATI	2	2	FREE	NONE
2	ARUA	OPP	OKUONZI SAM AGATRE & WADRI KASSIANO EZATI	2	3	FULL	NONE
2	ARUA	OPP	OKUONZI SAM AGATRE & WADRI KASSIANO EZATI	2	4	FREE	SCHOOL + USERS
2	ARUA	OPP	OKUONZI SAM AGATRE & WADRI KASSIANO EZATI	2	5	FREE	NONE
2	ARUA	OPP	OKUONZI SAM AGATRE & WADRI KASSIANO EZATI	2	6	FULL	SCHOOL
3	BUNDIBUGYO	NRM	NTABAZI HARRIET	1	1	FULL	NONE
3	BUNDIBUGYO	NRM	NTABAZI HARRIET	1	2	FREE	NONE
3	BUNDIBUGYO	NRM	NTABAZI HARRIET	1	3	FREE	NONE
3	BUNDIBUGYO	NRM	NTABAZI HARRIET	1	4	FULL	SCHOOL
3	BUNDIBUGYO	NRM	NTABAZI HARRIET	1	5	FULL	NONE
3	BUNDIBUGYO	NRM	NTABAZI HARRIET	1	6	FREE	SCHOOL + USERS
5	GULU	MIXED	OULANYAH JACOB L'OKORI & ACIRE CHRISTOPHER	2	1	FREE	NONE
5	GULU	MIXED	OULANYAH JACOB L'OKORI & ACIRE CHRISTOPHER	2	2	FULL	NONE
5	GULU	MIXED	OULANYAH JACOB L'OKORI & ACIRE CHRISTOPHER	2	3	FREE	NONE
5	GULU	MIXED	OULANYAH JACOB L'OKORI & ACIRE CHRISTOPHER	2	4	FULL	NONE
5	GULU	MIXED	OULANYAH JACOB L'OKORI & ACIRE CHRISTOPHER	2	5	FREE	SCHOOL
5	GULU	MIXED	OULANYAH JACOB L'OKORI & ACIRE CHRISTOPHER	2	6	FULL	SCHOOL + USERS
6	HOIMA	NRM	KAHWA TOPHACE BYAGIRA	1	1	FULL	NONE
6	HOIMA	NRM	KAHWA TOPHACE BYAGIRA	1	2	FREE	NONE
6	HOIMA	NRM	KAHWA TOPHACE BYAGIRA	1	3	FULL	NONE
6	HOIMA	NRM	KAHWA TOPHACE BYAGIRA	1	4	FREE	SCHOOL + USERS
6	HOIMA	NRM	KAHWA TOPHACE BYAGIRA	1	5	FULL	SCHOOL
6	HOIMA	NRM	KAHWA TOPHACE BYAGIRA	1	6	FREE	NONE