

IMPACT EVALUATION OF COMMUNITY ADVOCACY FORUMS (BARAZAS) IN UGANDA

A PROPOSED RESEARCH STUDY DEVELOPED BY THE INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE (IFPRI) FOR FUNDING BY THE INTERNATIONAL INITIATIVE FOR IMPACT EVALUATION (GDN-3IE)

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ACRONYMS

CAO	Chief Administrative Officer
CG	Central Government
GoU	Government of Uganda
HH	Household
IFPRI	International Food Policy Research Institute
LC	Local Council
LG	Local Government
LLG	Lower Local Government
NAAD	National Agricultural Advisory Services
S	
OPM	Office of the Prime Minister
RCTs	Randomized Control Trials
RDC	Resident District Commissioner
SC	Subcounty
TA	Treatment Arm

OVERVIEW

This study is proposed to rigorously evaluate the impacts of the community advocacy forums (or baraza) initiative in Uganda. Barazas were introduced in Uganda about four years ago as an empowerment tool to enhance citizens' involvement in monitoring and holding the government accountable for service delivery in relation to the resources spent. As barazas continue to be rolled out beyond the pilot communities, a rigorous impact study of their effectiveness is still outstanding. This study proposes a mixed methods approach, using sufficient, representative and temporal datasets, to control for any biases and consistently estimate the impacts of barazas in Uganda.

Results of this study will be disseminated in several outlets and will be used to inform policy on the effectiveness of barazas and other related governance tools on service delivery and public accountability.

1. INTRODUCTION

Since Uganda's independence in 1962, the country's development efforts have been thwarted by political turmoil and economic mismanagement. In the mid-1980s, after attainment of relative stability, the Government of Uganda (GoU) supported by development partners, initiated reforms to address development challenges of the time. Notable among these initiatives was the liberalization of the economy and the introduction of a decentralized system of governance (Francis and James, 2003, Benin et al., 2007). Decentralization was particularly viewed as a suitable mechanism for addressing welfare and political challenges by improving efficiency of public service delivery, formulating more appropriate services, bringing representative governance closer to citizens and empowerment of local stakeholders (Steiner, 2007, Francis and James, 2003). This is in contrast to centralized governance where participation in decision making is restricted to political and economic elites only (Steiner, 2007), and the cost of information sharing between central government actors and beneficiaries is relatively high. A major ingredient of decentralization is to enhance empowerment and build a sense of ownership of the local citizens to actively participate in planning, implementation and evaluation of development interventions in their locations, so as to improve accountability and responsiveness of local leaders and service providers (Burki et al., 1999).

Until recently, this empowerment component has been largely lacking in Uganda's decentralization process. The realization of benefits of decentralization in Uganda has been greatly affected by ineffective monitoring and weak accountability mechanisms, especially with respect to beneficiaries holding the service providers accountable (Björkman and Svensson, 2009, Reinikka and Svensson, 2004). In this regard, the GoU, under the stewardship of the Office of the Prime Minister (OPM), initiated community advocacy forums (or citizen barazas) in 2009 with the general objective of "enhancing public involvement in holding the government accountable for service delivery in relation to the resources spent" (OPM, 2013).

Citizen Barazas (or barazas) are viewed as platforms for enhancing information sharing between policy makers, development partners and beneficiaries of public goods and services. In addition, it provides the opportunity for citizens to ask questions to their leaders and deliberate among themselves, ultimately contributing to effective monitoring, accountability and transparency among all stakeholders. With barazas, citizens in particular have the opportunity to participate in the development cycle by directly engaging with service providers, and to demand accountability of the use of public

resources.

Barazas have been implemented in Uganda for the last four years. Barazas were first piloted in the financial year 2009/10.¹ The initial pilot barazas were undertaken in eight lower level local governments² (hereon generically referred to as “subcounties”³) of the four districts of Masaka, Bushenyi, Kumi and Nebbi, which are respectively located in the four geographical regions of Uganda: Central region, Western region, Eastern region, and Northern region. Since then, efforts have been underway to roll out barazas in all subcounties in the country. During the full-scale implementation phase in the financial year 2010/2011, 16 more subcounties in 8 districts had held a baraza meeting. And, by the last quarter of 2011/2012, 267 out of the country’s total of 1,340 subcounties, spread in 112 districts had held a baraza meeting. At the beginning of 2012/2013 however, changes in implementation were suggested: subsequent barazas would target district-level reporting so as to increase participation at a higher level. Currently, about 18 district-level barazas have been conducted. Our field observations in the recent district-level barazas we attended and other anecdotal information indicate that the level of intensity is significantly lower in district-level barazas as compared to subcounty-level barazas. For this reason, subsequent discussions in this proposal exclusively focus on subcounty barazas, unless otherwise indicated.

It is imperative that rigorous empirical studies are carried out to independently evaluate and document the impacts arising from the implementation of barazas in Uganda. The GoU shares the same aspiration so as to inform policy on program effects of service delivery to local communities (OPM, 2013) since there has not been any formal study conducted to test the actual achievements of the baraza initiative so far against the set objectives. The proposed study intends to fill this gap by examining impacts of barazas using careful analytical strategies and testing the sensitivity of the results to varying assumptions.

The remainder of this proposal document is structured as follows: The next Section discusses the objectives of the study. Section 3 theoretically motivates the analysis, situates it in the relevant literature, and proposes a theory of change on the basis of which the hypothesized impacts of the barazas design can be tested. This is followed by a description of the baraza institutional setup in Section 4. The methods proposed for data collection and analysis are laid out in Section 5, while Section 6 presents the expected outputs. Administrative and logistical details are covered in Sections 7 and 8: The former contains information about the project management, the activities and

¹ Uganda’s financial year starts on 1st July of one year and ends on 30th June of the following year.

² Lower level local governments include mainly subcounties but also town councils and municipal divisions. Lower level governments form a district and are expected to have fully-fledged political and administrative units. All major sectoral heads at the district are represented at the lower level as well.

³ For the sake of this proposal a generic term “subcounty” is used to also represent town councils and municipality divisions

work plan, and the latter presents the budget.

2. OBJECTIVES OF THE STUDY

The main aim of the proposed study is to quantify, in a rigorous manner, the impacts of the baraza initiative, and to establish a credible baseline and benchmark for assessing future impacts of any changes or modifications in the baraza program or any other governance monitoring tools that the GoU would introduce in future (beyond the lifetime of this study).

The study will address two major objectives:

- a. To assess the impact of barazas on citizen empowerment and demand for accountability. As noted earlier, the overarching goal of the baraza initiative is to empower citizens so that they can hold government accountable for service delivery in relation to the resources spent and to enhance government's responsiveness to citizens' demand by improving service delivery. In this study, we will compare the perceptions of citizens on empowerment and assess participation in planning and decision making on the services citizens require and the content and quality of the services.
- b. To assess the impact of barazas on service delivery. Beside empowerment and accountability mentioned above, what ultimately matters to the citizens are the actual changes that take place in service delivery due to barazas. This study will therefore analyze changes in the indicators of service delivery as follows:
 - (i) *Agriculture*: This study will assess changes and comparisons in access to agricultural extension advisory services and technology development service delivery by examining the frequency of households' contact with the extension service providers per year; awareness, knowledge and access to existing and new types of technologies provided (e.g. mechanized and animal drought technology, improved seed, use of agrochemical and fertilizers, etc.); the distance to a nearest extension office in case of an emergency; the availability of storage and processing facilities and the distance from the household to the facility, etc.
 - (ii) *Health*: The analysis will examine the availability of public health facilities in the community, the distance to the nearest facility for households and the services offered (including maternity care, ambulance, child care, immunization, etc.) The study will also establish if there are enough health workers, their behavior, and quality of services provided and whether citizens pay for any services at the facility.
 - (iii) *Infrastructure*: This study will focus primarily on water and road infrastructure, and may consider other selected infrastructure types. It will analyze changes in households' access to protected and

unprotected water sources. This will also involve estimating the distance to the mentioned water sources, and the households own perception of water quality. The study will further assess the changes over time, in households' access to road infrastructure by asking households to mention the distances to nearest seasonal and all-weather roads. Data on other infrastructural developments such as access to electricity will also be examined.

- (iv) **Education:** In this sector, the analysis will assess the changes in the availability of primary, secondary and technical schools within the community and the distance from the household to the nearest of each of these. The study will also obtain information on the households' perception of the standard quality indicators of services provided by the mentioned educational establishments such as teacher absenteeism, class size, availability of books, etc.

3. THEORETICAL CONTEXT OF BARAZA INTERVENTION

The baraza intervention fundamentally seeks to improve public services through improving downward accountability of local public decision makers and service providers. Within this scope, two features of the barazas contrast in terms of their focus on *information* versus *deliberation* (more specifics on the baraza design are provided in Section 4). Figure 1 below illustrates the conceptual role of each of these two elements representing the hypothesized pathway from the baraza design features to improved public service delivery. These are both important in downward accountability of service providers and local public leaders, and have been analyzed in the academic literature both theoretically and empirically.

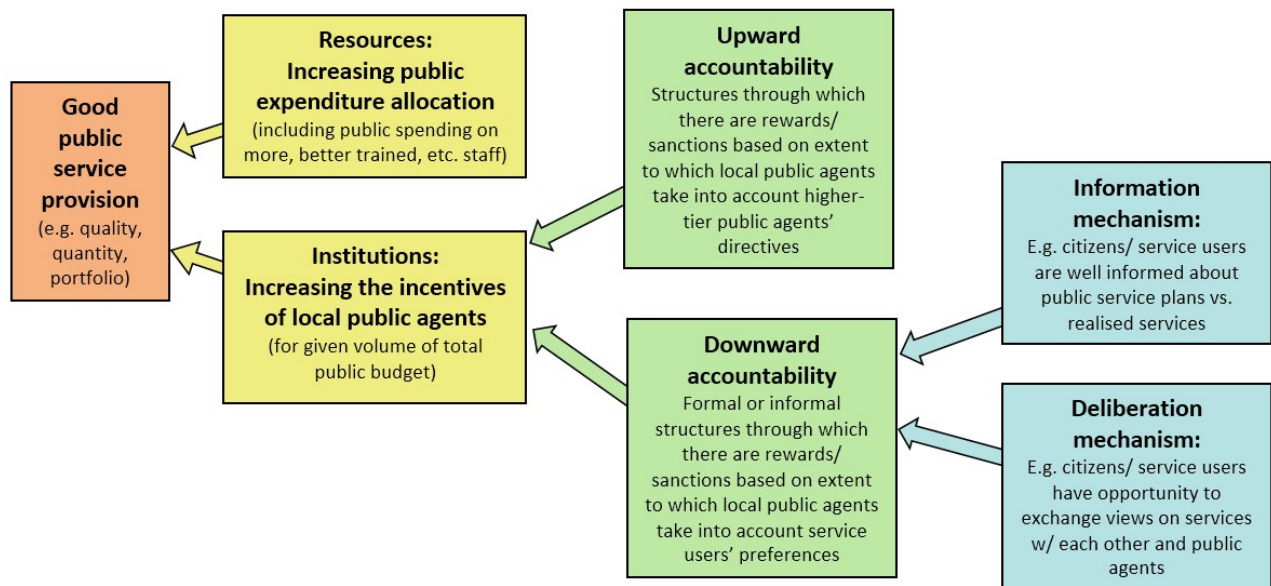


Figure 1: Pathway from information and deliberation features to improved service delivery

a. The Information Mechanism

One of the ‘political market’ imperfections that looms large in public service outcomes for the poor is lack of information on the part of citizens about the actions and performance of public agents (Keefer and Khemani, 2005). Even where democratic institutions are strong and public decision makers are compelled to take into account citizen preferences in their policy and public expenditure decision making, limited access to information by citizens can be a serious hindrance to their policy preferences being realized.

Information imperfections in this context can take two fundamental forms: Lack of information about available publicly provided goods, services, and infrastructure; and lack of information about public agents’ efforts (including actions, policies and expenditures). The first type of information constraint can lead residents to fail to take advantage of the services that are in fact available to them, and thus it ultimately brings about poor usage of even extant services. The second type of information constraint makes it difficult for residents to hold public decision makers accountable, leading to potential undersupply of the needed services. When either type of information constraint is, furthermore, more binding among the poor in society than among the non-poor, all else equal, this results in negative distributional effects of service provision. The hardest type of information deficit to overcome—and potentially the most important for outcomes—is a particular combination of both lack of knowledge about services and about public efforts, namely the challenge for citizens to be aware of the causal link between public actors’ efforts and the existing services.

Rigorous channeling of information to citizens about the quantity, modality, and quality of public services, as well as about the investments and policy decisions made by politicians, bureaucrats, and service providers, can increase the ability of the users to hold the leaders accountable to improve service provision. For example, Pandey, Goyal and Sundararaman (2011) establish using a field experiment in India that community information campaigns about states’ school management obligations had a positive impact on school performance. Gilens (2001) identifies a significant influence of providing policy facts on the public’s political judgment.

b. The Deliberation Mechanism

Theory on the deliberative process explores the effects of greater both-way interaction between citizens and leaders (versus only one-way information provision, as in the first mechanism described above), and of interaction among citizens with each other. Much of the work on local deliberative processes have as their foundation broader theories of deliberative democracy (e.g. Gutmann and Thompson, 1996; Gutmann, 1996; Rawls, 1997). Features

of deliberative democracy include, among others, that agents are given the opportunity to articulate their diverse positions to each other; that these positions are conscientiously mutually vetted based on the evidence presented in their support; and that an agent's political standing, economic status, or other characteristics reflecting their power is not a factor in the weight placed on their arguments (Fishkin, 1991).

The seminal literature on this topic especially highlights three contributions of deliberation. Firstly, it has a legitimating effect on decisions arrived at in this fashion (Cohen, 1989). These follow from the particular features, as described above, that highlight equal voice of the arguments of both marginal and advantaged agents, and the role of evidence that support the positions articulated. Secondly, it is judged to have the ability to more effectively distil social choice than simple voting and majoritarian rule, in part by building of consensus both among citizens and between public servants and citizens (Miller, 1992; Fishkin, 1997). Thirdly, it is said to have a positive impact on the vigor and breadth of subsequent citizen involvement in community affairs (Cohen, 1997). In deliberation with public officials, citizens get to observe how leaders respond to their queries and expression of dissatisfaction with services, which can have an effect of feeling empowered that they can in fact ask for, and are entitled to, better or more resource allocation to the public services that they need. The perception that one is entitled to and can demand changes in public action is an important precondition for holding public servants accountable to improve services. There are several reasons why this perception on the part of citizens can be low—especially in authoritarian systems but to some extent even in democratic systems. Thus, the fact that this feature is being addressed through the baraza intervention allows testing for its hypothesized effectiveness to improve service delivery. Impacts of deliberative processes have also been the subject of empirical analysis. For example, experimental evidence shows that deliberative processes make decision outcomes less sensitive to the institution (e.g. voting) rules that bring them about (Goeree and Yariv, 2011).

4. THE BARAZA INSTITUTIONAL SETUP

Uganda has a structured decentralized system characterized by highly technical processes across all decision-making levels of public service. According to official documentation (see OPM, 2012), the decentralization process is guided by seven pillars as follows:

- a. Power is devolved from the center to local governments (LGs) to enable them to make decisions and allocate resources based on local priorities. LGs then devolve power to Lower Local Governments (LLGs) to allow decisions to be made at the lowest levels;
- b. Focus is placed on efficiency and effectiveness, and equitable, transparent and accountable use of power and resources in a manner that observes democracy, human rights and the rule of law;

- c. LGs have powers to make own plans based on local priorities. Non-prioritized goals are subject to negotiation with the Central Government (CG);
- d. Development planning and budgeting are carried out in a participatory manner based on democratic principles to enable active participation of the people at all LGs in matters that affect them;
- e. Government is supposed to develop mechanisms that ensure strengthening and regulation of program implementation by the civil society, private sector and the academia among others, in order to strengthen decentralized service delivery;
- f. Much as there are devolved powers, LGs cannot dominate others or LLGs under them. LGs have unfettered powers to make decisions but within the confines of the law;
- g. Local officials are accountable to both the CG and local citizens over the use of public resources. At the same time, citizens are supposed to respond to calls for participation, payment of taxes and fees and ensuring that they keep to the law.

In 2009, the barazas initiative was conceived to create space as a citizens' advocacy forum in order to fulfill the pillars of the decentralization process listed above. Barazas are expected to bring together stakeholders from government (both CG and LGs), who are the policy makers; public service providers, who are policy implementers or civil servants; and the public, the users of services as the citizens. Barazas are thought to provide a platform for interface between the citizens and their leaders on sharing of public information with a focus on effective monitoring of public service provision (on the part of leaders) and demand for accountability and transparency (on the part of citizens). The following are the five stated objectives of barazas:

- a. To establish a public information sharing mechanism, providing citizens with a platform to influence government development programs;
- b. To institutionalize downward accountability so as to bring about improvement in public service delivery and transparency in the use of public resources;
- c. To instill a homegrown culture of independent citizens monitoring for constructive criticism that improves public service delivery and sustain the wellbeing of the people;
- d. To enhance central government's responsiveness to citizens development demands and public service delivery concerns; and,
- e. To create a corrective strategy aimed at enhancing public accountability which through central government's quick responsiveness shall rebuild the government's popularity towards its citizens.

Essentially, the baraza initiative should provide a platform to enhance information exchange and deliberation mechanisms as explained in Section 3 above. Operationally in Uganda, a single baraza event (or simply called, baraza) is expected to be conducted twice a year at the level of the subcounty

to track the planned activities and achieved outcomes. In the current form, the OPM coordinates the baraza activities in every district by mobilizing district and subcounty officials including the Chief Administrative Officer (CAO), the Resident District Commissioner (RDC), the District Local Council Chairperson (LCV), sector heads, and subcounty administrative officers (chiefs), among others. The baraza operation rolled out in several phases, with each phase corresponding to one financial year. Prior to the 2011/2012 baraza implementation phase, a baraza would be preceded by a three-week training of community facilitators in each district where a baraza was to be undertaken. The role of community facilitators is to disseminate information and explain the baraza concept to the populace. In recent times however (i.e. beginning with the 2011/12 phase), village mobilizers and community resource persons are used instead of community facilitators, so as to enhance information dissemination efficiency and optimize resources. These community mobilization efforts are further reinforced by adverts in the local media in the form of radio announcements; printed banners, posters and fliers; and mobile public address systems, a few days before the baraza event.

Until now, every baraza in Uganda has been initiated, coordinated and logistically supported by the OPM, independent of the local governments. If a baraza is to be held in a given location, the OPM, in consultation with the district leaders (RDC, CAO and the LCV) and other stakeholders, agree on the date and a neutral venue in which to hold the baraza event. Again, in consultation with the district leaders, a viable moderator and an interpreter into the local language where applicable are identified to guide the baraza forum, in which sector heads make presentations and respond to queries raised by the community. Baraza moderators are expected to be objective, of high integrity and have the trust of the community. Moreover, they should be knowledgeable about government programs in a given locality.

On the chosen date and venue, a baraza meeting is chaired by the Office of the RDC in each district. In front of the audience, including local citizens and invited opinion leaders and elders, the RDC seeks accountability and feedback from each head of major sectors, which are: health, education, physical infrastructure (mainly water facilities and roads), and agriculture (particularly national agricultural advisory services – NAADS) by asking them to present on:

- a. What services were planned to be delivered in the subcounty?
- b. What was actually delivered and in what quantity and quality?
- c. What issues and challenges have emerged and what is the way forward?

The RDC then seeks reactions and feedback from citizens on whether what has been presented is what was planned for and actually implemented in different locations. Sector heads are then given another opportunity to clarify on or react to any issues raised by the citizens. At the end of the process, the RDC makes a report to the OPM, indicating issues that arose in the baraza

meeting. This report particularly points out policy and program implementation weaknesses and challenges, which further feeds into the general government performance management system.

5. STUDY DESIGN AND DATA

a. Impact pathway and the identification problem

We will combine qualitative and quantitative methods to identify the key impact pathways, outcomes and actual impacts of the baraza initiative. To identify impact mechanisms, we will use the participatory impact pathway methods as suggested by Douthwaite et al (2008), which helps to attribute impacts to specific interventions such as the baraza initiative. To identify the potential impact pathways, we will first carry out a qualitative assessment by holding further consultations with OPM staff involved in baraza and activities, political and leaders and technical officers in selected districts and lower level governments where barazas have been undertaken, community facilitators, and other key informants in all the four regions of Uganda, eliciting key political and socioeconomic differences. The qualitative assessment will identify the expected intermediate and final outcomes and impacts of barazas, and the likely mechanisms of impacts. This qualitative work will further guide the design of the quantitative surveys that will be conducted and in interpreting the analytical results.

One important expected impact of baraza is improved service delivery for individuals or households living in locations where barazas have been implemented, which may result from many intermediate pathways. On the part of service providers, there may be improved allocative efficiency of public resources and improved responsiveness of civil servants to the needs of the community in the expectation of being held to account by beneficiaries at a future baraza. On the other hand, barazas may enhance the beneficiaries' empowerment and build a sense of ownership thereby increasing their active participation in planning, implementation and monitoring of public service delivery systems. The impact pathway analysis will be used to suggest hypotheses about key relationships and indicators for the quantitative work.

In this study, we plan to analyze changes in the quality and quantity of public service delivery in agricultural extension and advisory services, health services, water and road infrastructure, and education services by assessing differences across sampled households in baraza and non-baraza subcounties. The challenge however, is to attribute these outcomes to baraza initiatives. There is a potential selection bias that results from either program placement or self-selection into treatment, implying that a unit of study may have characteristics leading to better service delivery outcomes even without baraza interventions. For example, some subcounties may have more vocal and astute political and opinion leaders that would constantly monitor and

demand for public service delivery in their communities. This would result in better service delivery even without baraza interventions. Moreover, some subcounties may have highly motivated service providers that even without monitoring say, in form of barazas, they are still able to deliver on identified societal needs.

To control for this potential selection problem, we will identify proper counterfactuals, similar in all respects to the group that received the intervention except that the counterfactual does not receive the intervention (White, 2013a). We will use a combination of sampling and analytical techniques to identify the counterfactual and use it to robustly test the validity and sensitivity of baraza impact across different contexts. We thus propose to employ a mixed methods approach of data collection and analysis (White, 2008, White, 2013b) based on randomized controlled trials (RCTs) alongside other panel and cross-sectional based approaches as explained below.

b. The Experimental Treatment Design

(i) Selection of to-date untreated districts and subcounties

As noted earlier, until now, a small share of all subcounties, albeit located throughout all of Uganda's 112 districts across the four regions of the country, have received an SC-level baraza intervention. Two facts, when combined, point to favorable conditions for undertaking rigorous evaluation of the barazas. Firstly, the implementing agency, OPM, has the sole political mandate and the financial resources to further roll out barazas to new areas. Secondly, we have learned from our prior discussions with OPM that the agency is willing to randomly assign the remaining baraza interventions in selected subcounties and to delay it in others until after the endline survey. This gives us a chance to evaluate baraza impact in an experimental setting by comparing outcomes across the different sets of conditions.

This randomization solves the problem of selection bias because baraza and non-baraza areas will be drawn randomly from the same underlying population and therefore, the average characteristics of these groups will not systematically vary, and any differences observed in the outcomes of interest can therefore be attributed to the intervention. To further minimize the possibility that our study design to this end would be compromised, we will assign a research assistant to regularly visit the study sites, engage with the local implementing staff, and report back to us on a regular basis on the implementation of the barazas in the treatment areas of the study and the conditions in the control areas of the study.

With this in mind, we propose to design a social experiment covering districts, subcounties, and households across the four regional blocks (Northern, Western, Central and Eastern) of Uganda. Each regional block has somewhat

unique characteristics in terms of ethnicity, geographical and agro-ecological conditions, as well as cultural history. We make our sample selection of districts from among ‘eligible districts’, and our sample selection of subcounties from ‘eligible subcounties’ (or ‘eligible SCs’). An ‘eligible district’ is defined here as a district in which a district-level baraza has not already been implemented prior to this proposed evaluation. The districts with already implemented district-level barazas are excluded from the sampling frame because we cannot track which members of which subcounty attended the district-level baraza. An ‘eligible SC’ is defined as a subcounty to which two conditions apply: (i) a subcounty-level baraza has not already taken place, and (ii) the subcounty is not located in a district in which a district-level baraza has already been implemented i.e. the subcounty is located in an eligible district. Preliminary analysis of the baraza implementation data from OPM shows that there are 20 or more eligible districts per region, amounting to a total of 94 eligible districts. In each region, there are at least 147⁴ subcounties that have never been treated as well as are in eligible districts; the total of such eligible subcounties is 722.

(ii) The administrative placement dimension in a nested randomization design

As a first step, this study proposes a nested, or two-step, randomization design that will test the effects of two different administrative placement dimensions of the baraza intervention in a nested way. The first dimension has implicitly already been alluded to in various places above: The baraza intervention can be distinguished by the administrative level at which it is implemented: Barazas had been originally planned to be implemented at the subcounty level but in recent years, the focus is being turned to implementing barazas at the district level. This administrative placement dimension immediately points to a potential tradeoff between attempting to achieve breadth of coverage (through the district-level barazas), and attending to depth and quality of coverage (through subcounty-level barazas). While conducting a district-level baraza may be cheaper than conducting subcounty-level barazas in all subcounties of that district, it is not clear *a priori* how these cost savings justify potential reduction in effectiveness of district-level barazas in any given subcounty of the concerned district. Therefore, one vital aspect of this study will be to compare the effectiveness of these two different level administrative placements of the treatment against control areas without any baraza intervention.

(iii) The modality dimension in the nested randomization design

The second dimension of the study is directly derived from the theoretical framework and empirical literature discussed earlier in Section 3, covering similar governance interventions in developing countries to enhance the

⁴ This number excludes subcounties that were subsequently treated by the district level barazas, in order to avoid contamination.

quality of public services and local accountability. As mentioned before, barazas are viewed as platforms for enhancing *information sharing* between policy makers, development partners and beneficiaries of public goods and services. In addition, barazas provide the opportunity for citizens to *ask questions to and go into debate with their leaders*, ultimately contributing to effective monitoring, accountability and transparency among all stakeholders. It is useful to know if there is a differential effect between these two major features within the treatment, which may then be useful information to improve the effectiveness of the intervention in the future.

To this end, we will appropriately design a randomization experiment that will have untreated subcounties as a control group and treated subcounties where subcounty-level barazas will be conducted. However, instead of just dividing the sample into treatment and control, we will differentiate between two key components that can grossly be identified within the current barazas: (i) the *information sharing component* and (ii) the *deliberative component*, as theoretically motivated in Section 3, and described in Section 4 in the specific barazas context as executed by OPM to date.

We propose to use a factorial design. If we define two levels within each factor, a 2 by 2 fully crossed factorial design is appropriate and can allow us to describe the baraza intervention in terms of two separate and one crossed treatment. As such, we can structure the evaluation based on 3 treatment arms in an experiment as follows (a graphic illustration follows in Figure 2 further below):

- **Crossed treatment arm (S^0_{ID}):**- includes sampled subcounties within the context of the administrative placement dimension (baraza treatments and no-treatments at district-level) explained earlier. This comprises of a subset of treated subcounties that will receive the current form of the baraza as it is currently implemented, with both the information sharing component and the deliberative component (i.e. the crossed treatment).
- **The deliberation treatment arm (S^0_D):**- is directly derived from the existing modality of the barazas as they are conducted at the subcounty level heretofore, i.e. as described in Section 4 above. However, since this treatment wants to separate out the effect of citizens challenging government officials and deliberating among one another, this treatment will not involve the information sharing components of the existing barazas as described in Section 4. In other words, this treatment will not involve the presentations mentioned, but only involve a facilitated session where citizens can engage with each other and with government officials.
- **The information sharing treatment arm (S^0_I):**- while S^0_D above is

focused on the deliberative process, S^0_I will concentrate on one-way information provision and sharing. This treatment also follows the model of the pre-existing baraza, but focuses on only the first part that consists of presentations from each head of major sectors on planned activities, on what has been delivered and on the challenges that have emerged. In this treatment, there will be no space for interaction with the government officials or of the community members among each other. It will be seen as an information dissemination event. In other words, this treatment will not involve the facilitated session where citizens can engage with government officials, but only involve the presentations by different sector heads.

The treatment arms described, will examine in absolute terms the effectiveness of information provision, and the deliberative process and the interaction effect thereof, as well as will allow a relative comparison of these two avenues of improving accountability of service providers to the service users. Moreover, such a crossed design allows us to learn which aspects of the baraza is most effective. For instance, not only does this allow us to learn how much more (or less) effective barazas are compared to the control, but also how much more effective barazas are compared to a treatment that consists only of the information sharing component, or only of the deliberation component. We will operationalize this examination in our experimental design after more detailed discussions with OPM on the above proposed partition of the baraza in the two treatment arms, and based on these discussions, more detailed specifications of the two intervention elements.

While comparing control areas to subcounties that received treatment S^0_D , treatment S^0_I or both is of main interest, it may also be instructive to compare each treatment to the crossed treatment. For example, it may be that sector heads, through disseminating information, are able to influence the deliberative process. Sector heads may be able to play down expectations of citizens by presenting unambitious plans and exaggerating delivered services. This may result in the communities being far less critical and a muted deliberative process may result, as compared to a treatment with only a deliberative component. On the other hand, in a treatment with only a deliberative process, expectations of citizens may be completely unaligned to planned activities, leading to an unproductive deliberative process.

(iv) Approach in selection of districts and subcounties in nested treatment design

Figure 2 summarizes how the selection of districts, subcounties, and households will proceed in the context of this nested randomization design. The green boxes represent the district-level randomization pertinent for the first (administrative placement) dimension in the nested design. From all eligible districts, B^0 districts will be randomly assigned to remain control

districts, in the sense that these will not receive district-level baraza interventions for the full duration of the study. Another set of B^{ID} districts will be randomly assigned to begin receiving the baraza-level intervention immediately after the baseline survey (see subsections c) and d) further below for details sample size determination and data). The nature of these barazas will comprise of the full intervention in the sense of crossed treatment dimension i.e. these district-level barazas will contain both information and deliberation components.

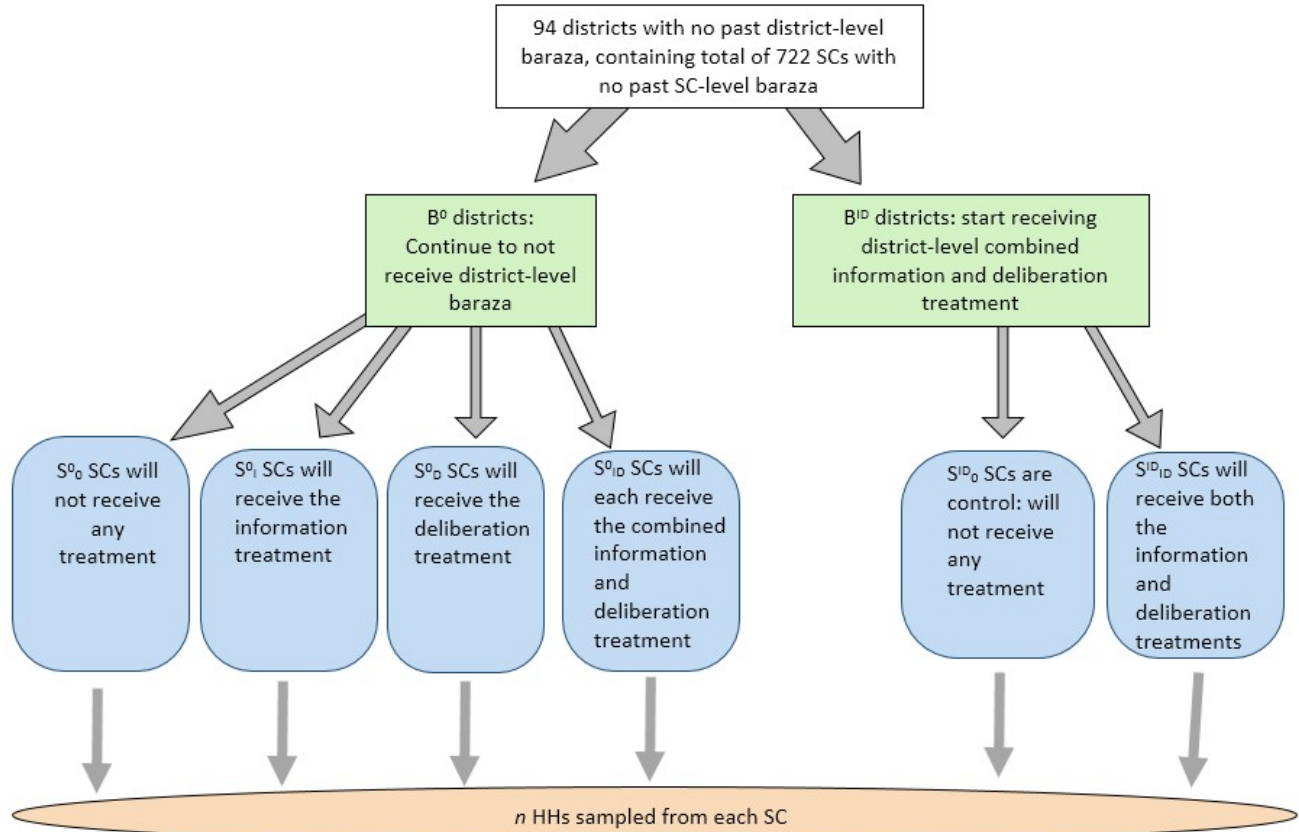


Figure 2: Illustration of treatment design

As is apparent from this, in our notation B refers to numbers of districts assigned to some treatment group, and superscripts identify the treatment group to which the relevant district belongs. From these districts, assignment for subcounties (blue boxes in Figure 2) will be as follows: From among all eligible SCs in each district out of the B^0 districts, a total of S^0 SCs will be randomly assigned to 3 baraza treatment arms along the second dimension and the control:

- S^0_0 SCs are control subcounties and will not receive any subcounty-level baraza,
- S^0_1 SCs will receive subcounty-level barazas that use the information modality,
- S^0_D SCs will receive subcounty-level barazas that use the deliberation

- modality,
- S_{ID}^0 SCs will receive subcounty-level barazas that combine the information and deliberation components.

where, analogous to the notation description above, S then refers to the number of subcounties in some treatment type or set of treatment types, the superscripts here too are indicators of the treatment type to which the district is assigned in which a given subcounty is located, and subscripts show the treatment group to which the subcounty itself is assigned.

From among all eligible SCs in each district out of the B^{ID} districts, a total of S^{ID} SCs will be randomly assigned to two treatment types along the second dimension:

- S_{ID}^0 SCs are control subcounties and will not receive any SC-level baraza,
- S_{ID}^{ID} SCs will receive subcounty-level barazas that combine the information and deliberation components.

Before proceeding to discussion on the determination of specific sizes of samples and subsamples for this study, it is important that we first lay out the *relative* numbers of districts and subcounties assigned across the treatment groups in each of the two treatment dimensions, relying on basic principles as well as the empirical context of this study.

Districts:

We equalize the assignment of districts into the two groups, i.e. $B^0 = B^{ID}$.

Subcounties:

- The ratio of the number of subcounties in one treatment type and one district to that in another treatment type in the same district needs to be equalized across districts. That is,

$$S_{T,b}^{T''} / S_{T',b}^{T''} = S_{T,b'}^{T''} / S_{T',b'}^{T''} \quad \forall b, b' \quad (1)$$
where b and b' refer to any two selected districts with the same district-level treatment type, and T and T' refer to any two subcounty-level treatment types (e.g. T could be control, while T' could be the combined information and deliberation treatment).
- In principle, within the constraints of (1), the number of subcounties in some treatment type T could be proportional-to-size, that is, districts with a larger number of subcounties would have a larger $S_T^{T''}$. However, because the number of eligible SCs in any given district is likely not to be large, we will not be able to closely tailor the number of subcounties to be proportional-to-size.⁵ We can therefore use the simpler rule such that

⁵ Table 1 shows that there are on average less than 10 subcounties per district. Thus, e.g. if two districts b_1 and b_2 have 8 and 12 eligible SCs, respectively, and if $SC_{b_1}^{b_1} = 3$, then using the proportional-to-size rule would have required $SC_{b_2}^{b_2} = 4.5$, which is of course not possible, and using the feasible 4 or 5 subcounties instead, leaves us with a relatively high inaccuracy.

$$S_{T,b}^T = S_{T,b'}^T \quad \forall b, b' \quad (2)$$

- In the case of multiple treatment groups (within one dimension of randomization) and the use of crossed interventions, Duflo, Glennerster and Kremer (2007)⁶ point to the need to have the number of pure control and full intervention units larger than the number of single-treatment units. Therefore, we have $S_{I,b}^0 = S_{D,b}^0 < S_{ID,b}^0 = S_{0,b}^0$ for any district b .

Households:

We randomly sample an equal number of households from each selected subcounty, i.e. $n_s = n_{s'} \quad \forall s, s'$, for any subcounties s and s' .

c. Power analysis and determination of sample size

To determine the appropriate sample size, we formulate a series of primary and secondary outcome variables along the lines of the categories mentioned in Section 2(b). For each of these outcome variables, we specify a minimum detectible difference and standard deviation of the outcome. We also add the average for each outcome variable obtained from the data source used to calculate the standard deviation, for illustrative purposes only. These figures can be found in the first three columns of Table 1. In addition to the effect size and the standard deviation, one needs to specify the power and the significance level. We specify a power level of 0.80 and a significance level of 0.05 to calculate the sample size to compare the means of two independent samples.

Table 1: Determination of sample size

	mdd	sd	avg	ρ_{dist}	ρ_{sc}	n@dist - n@sc	Data source
Health							
Weight for age z-scores	0.2	1.20	-0.90	0.045	?	1,814- ?	DHS 2011
Number of days unable to work as percentage of days sick	10	39.72	51.02	0.052	0.108	880- 1,560	UNHS 09/10
Education							
Average years children go to school in household	1	3.50	6.61	0.159	0.225	1,697- 2,321	UNHS 09/10
% children in hh currently attending	5	27.72	31.71	0.032	0.057	1,241- 1,832	UNHS 09/10

⁶ Handbook of Development Economics Vol 4, "Using Randomization in Development Economics Research: A Toolkit"

school							
Agriculture							
Maize yield (kg/ha)	500	1,998	1,628	0.048	0.155	842-2,158	UNHS 05/06
Visited by extension worker (share of hhs, %)	5	26.20	7.41	0.013	0.045	980-1,920	UNHS 05/06
Infrastructure							
Time to get drinking water (incl. waiting time; in minutes)	15	53.76	57.7	0.1805 0	0.300	1,983-3,171	UNHS 09/10
Improved drinking water sources (share of hhs, %)	10	44.08	73.60	0.1834 1	0.395	2,372-4,844	UNHS 09/10

Notes: mdd = minimum detectible difference; sd = common standard deviation of the outcome variable; avg = average of the outcome variable; ρ_{dist} = intra-cluster correlation at district level; ρ_{sc} = intra-cluster correlation at subcounty level; n@dist = determined sample size at the district level; n@sc = determined sample size at the subcounty level.

There are two reasons why we need to also obtain the intra-cluster correlation of observations within groups. Firstly, in this design, random assignment occurs at a group level, not at the level at which we want to measure outcomes. In other words, our setup is a clustered randomized experiment, with clustering at two different levels. In such experiments, additional information is needed on the correlation within each group among observations' values on the outcome variable. This is known as the intra-cluster correlation (ρ). Indeed, if experimental units within a group show a high degree of correlation, you will need more units to obtain the same information out of a sample. This is why we also add estimates of intra-cluster correlation in Table 1. We estimate intra-cluster correlation at two levels: first at the district level and then at the subcounty level.

The second reason why we need the intra-cluster correlations is because our two stage experimental design, which includes an examination of the administrative scope of the baraza intervention, calls for the use of clustered sampling. If we were only interested in the effectiveness of district-level barazas, we could calculate the sample size using the intra-cluster correlation at the district level (ρ_{dist}). However, districts are large and random sampling at the district level may involve interviewing people that are a significant distance apart. While clustering observations at the subcounty level may result in a larger sample size because of the higher intra-cluster correlation at

this level, the cost of these extra observations may be smaller than the cost associated with interviewing individuals spread over a much larger area. In addition, we are also going to administer treatments at the subcounty level anyway, so we may want to draw on data from these households to learn about the treatment at the district level. This is why we will consider the intra-cluster correlation at the subcounty level to determine sample sizes.

We use the following formula to determine sample size for continuous variables:

$$n = \frac{\left(\Phi_{\frac{\alpha}{2}} + \Phi_{\beta} \right)^2 [1 + (m-1)\rho]}{\left[\frac{\mu_1 - \mu_2}{\sigma} \right]^2} \quad (3)$$

where μ_1 and μ_2 refer to the average of the outcome variable in the control and treatment groups respectively. m is the number of observations in each group, n denotes the sample size in each treatment arm needed to be able to detect a minimal relevant difference of $\mu_1 - \mu_2$ at a significance level of α with power β , and the combined standard deviation⁷ of the outcome is σ . Furthermore, Φ_x stands for the x^{th} quantile of the normal distribution. The formula clearly shows how the term $[1 + (m-1)\rho]$ in the equation inflates the sample size if ρ , the intra-cluster correlation, increases. And assuming intra-cluster correlation is strictly positive, the size of the groups (m) also increases the required sample size.

We next discuss the outcome variables in Table 1 for the four sectors considered.

Health: For health related outcomes, we take weight-for-age z-scores for children, motivated by the fact that this is an important outcome variable in Bjorkman and Svensson's (2009) study on public service delivery in Uganda. The standard deviation and the intra-cluster correlation were calculated using the latest available Demographic and Health Survey carried out in 2011. Unfortunately, the DHS does not provide subcounty location information so we could not calculate intra-cluster correlation at that level. Filling in the numbers leads to a sample size of 1,814 individuals in groups of 50 individuals. Assuming the same intra-cluster correlation at the subcounty level as at the district means that we have to sample at least 37 subcounties in each treatment arm (Table 1). The second outcome variable for health measures at the household level the average reported number of days unable to work as a percentage of days sick. If we expect a minimum detectable difference of about 10 percent, we need a sample of about 1,560 individuals at the SC level

⁷ By combined, we mean it is the standard error of the outcome in both samples. We assume that the standard error will be the same in both treatment and control groups and so the combined standard error will (the square of) two times the estimated standard error of the outcome.

or about 30 subcounties in each treatment type. Standard deviations and intra-cluster correlations are obtained from the Uganda National Household Survey (UNHS) 2009/10.

Education: We calculate the number of years the average child within the household goes to school. We find this to be about 6.6 years in the UNHS 2010/11. We expect the intervention to add more than one extra year. We would then need to collect information of about 2,321 individuals at the subcounty level. Another outcome variable is the percentage of children within the household that is enrolled in school at the time of the survey. When we expect at least a 5 percent increase, we need to survey 1,832 households.

Agriculture: We look at yields in a central food crop, namely maize. Maize yields in Uganda are about 1.6 tons per hectare (much lower than the potential of say 8.6 tons per hectare as observed in developed nations). If we expect an increase of at least half a ton per hectare, we need 2,158 observations per treatment arm. Another outcome variable is on agricultural extension service provisions by the government to farmers. Only 7 percent reported to have been visited by an extension worker. An expected minimal detectable increase of 5 percent leads to 1,920 observations. All data used to estimate standard deviations and intra-class correlations used come from the UNHS 2005/06, as this survey is the latest available that has extensive module on agriculture.

Infrastructure: Finally we also look at some sample size calculations for infrastructure, for the outcome variable on the time it takes to get to a drinking water source, and the proportion of households that report to have access to improved drinking water sources. For this last question, detecting a 10 percent increase would require almost 5,000 observations per treatment arm. This is due to the fact that infrastructure, such as the source of water, affects large numbers of people in the same way. The resulting high intra-cluster correlation leads to very large required sample sizes.

In summary, Table 1 looks at a wide range of outcomes. Since we have 6 treatment arms, we need a minimum of 9,360 observations in total to answer the average share of working days lost due to illness when sick and up to a maximum of almost 30,000 observations in total to answer the question on access to an improved drinking water source. In groups of 50 households, this translates into between 187 and 581 subcounties to be sampled. In an ideal world, with no time and budget restrictions, we would choose 30,000 households (the largest number in Table 1) as our final sample size. Further, statistical power analysis based on this variable (see Figure 3) shows that even at lower power and larger minimal detectable size effect, the required number of observations is still very large.

A compromise would be to steer away from infrastructure variables, which, by

their nature, often suffer from large intra-cluster correlation. Outcome variables such as “time to reach the nearest paved road” are likely to provide little variation within villages or subcounties. If we don’t consider such infrastructural outcomes, our next highest required sample size is found for the outcome variable “average number of years of schooling within the household”. Detecting a minimal effect of 1 additional year at the usual 0.8 power level requires about 2,300 observations in each treatment arm (or about 14,000 households for the 6 treatment arms in 280 subcounties) (Figure 4). While this outcome still suffers from high intra-class correlation (it should be no surprise as years of schooling is likely to be correlated to infrastructure, which in turn is likely to have a high intra-cluster correlation), the sample size is more reasonable.

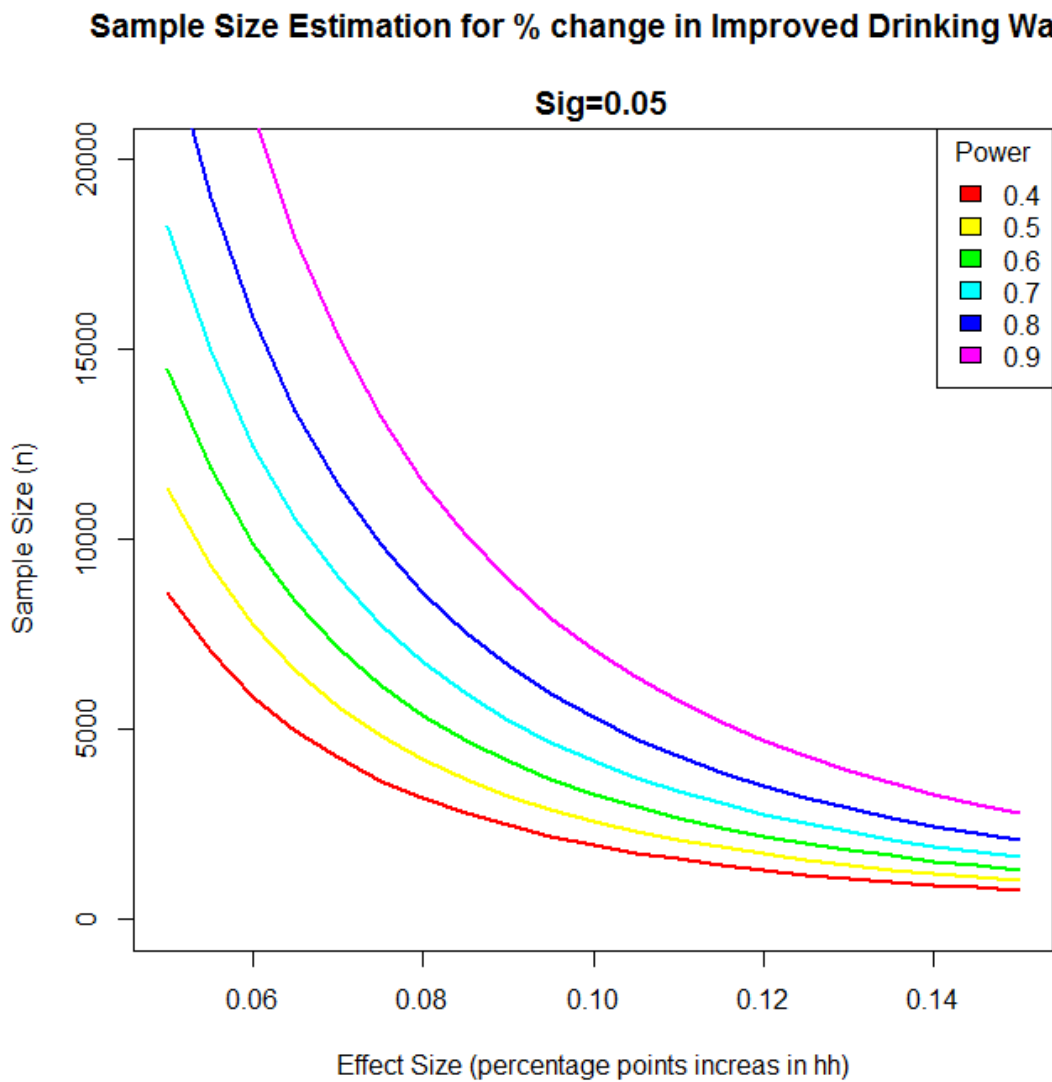


Figure 3: Statistical power analysis for change in proportion of

households with access to improved drinking water

An alternative approach to reduce the sample size that would somewhat alter the design would be to do away with one of the treatments. For instance, we could decide to drop the subcounty-level treatment in the treated districts arm (S^{ID}_{ID} in Figure 2). This would still enable us to answer the question of whether subcounty-level barazas are significantly different in terms of outcomes than district-level barazas. It may also be more realistic to implement in practice, as non-researchers may have difficulties understanding why a group of people should get two barazas (i.e. both district-level and subcounty-level barazas). Deleting this treatment arm in the experiment will reduce the treatment arms to 5 and reduce our sample size to about 10,900 observations.⁸ However, it is likely that cluster sampling at the subcounty level will still be more appropriate than random sampling within the entire district. As such, the sample size in this arm should also be calculated using the subcounty-level intra-cluster correlation. Thus, in this case the total number of observations needed would be about 11,500.

Sample Size Estimation for Average # years of schooling in hh

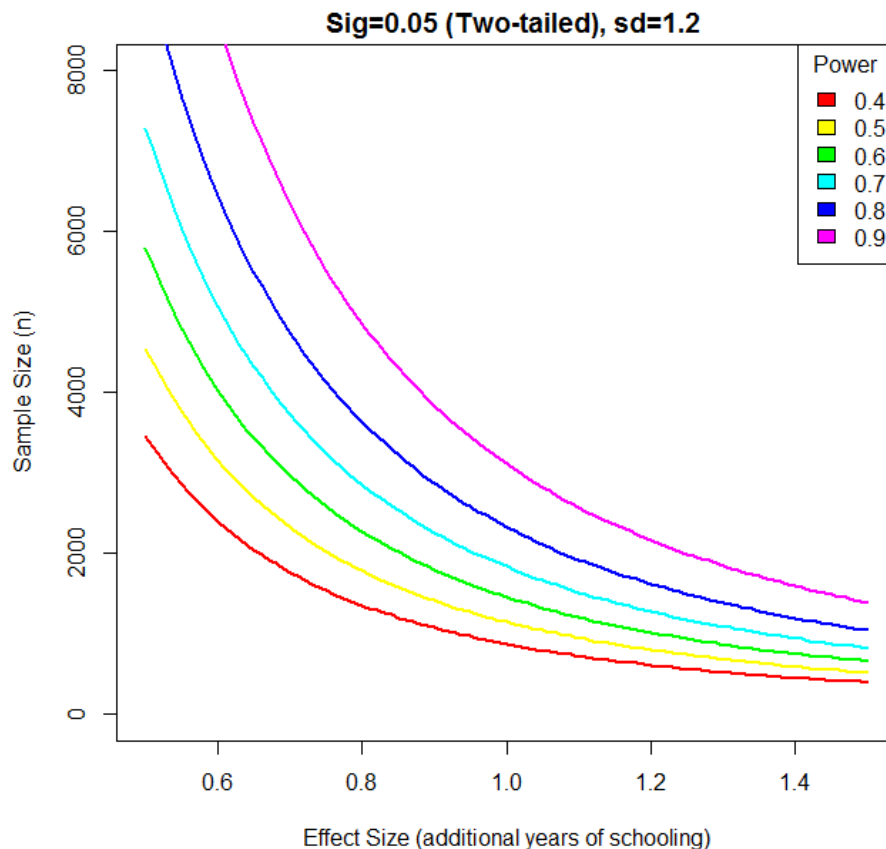


Figure 4: Statistical power analysis for the outcome variable average

⁸ Since the district level baraza treatment has a lower intra-class correlation, we would only need 1690 observations in this arm, as opposed to the 2300 observations needed if we sample at the subcounty level.

years of schooling within household

In summary, we lay out three options with different budget implications, as follows:

- i. We keep all the 6 treatment arms and outcome variables in all four sectors, as originally proposed. For this, we will need to interview about 30,000 households distributed over about 582 subcounties.
- ii. We disregard variables that are likely to exhibit large intra-cluster correlation, such as outcomes related to public infrastructure provision, and keep the 6 treatment arms as originally proposed. This reduces the sample size to about 14,000 observations, clustered into about 280 subcounties.
- iii. We reduce the number of treatment arms to 5 as explained earlier, as well as disregard variables that are likely to exhibit large intra-cluster correlations. For this, we need about 11,500 observations, distributed over about 230 subcounties.

In this study, we propose to conduct the study using the most economical of the three options, i.e. Option (iii) above, which has 5 treatment arms and focuses on outcomes in three instead of four sectors (dropping infrastructure).

d. Data collection

We aim to conduct interviews and secondary data collection at the district level, surveys and interviews at the subcounty level, and surveys at the household level. The data will be used to synthesize views on and assess the impact of the baraza program on improving the quality and efficacy of service delivery in the short- and medium-term. We will therefore carry out two household surveys, two subcounty-level surveys, and one subcounty-level set of interviews as follows:

- i. Subcounty-level and district-level interviews;
- ii. A baseline household-level survey and subcounty-level survey, immediately following the interviews of (i) above;
- iii. An endline household-level survey and subcounty-level survey, 24 months after the baseline.

This study will be implemented in three components that reinforce each other as follows:

Quantitative household survey: Household surveys, with the household head as the main respondent, will be conducted through a structured questionnaire with the selected households in all the selected subcounties. The household survey will include modules to elicit information regarding household characteristics; household access to major services such as health, agricultural advisory services, agricultural technologies, water and road infrastructure, among others; participation in the baraza activities; and other topics.

Quantitative subcounty survey: At the level of the subcounty, there will be a quantitative survey, administered through a structured questionnaire to key informants. The quantitative component will take place in the two survey rounds and in all study subcounties. It will capture core characteristics of the subcounty, including the public services and infrastructure available in the subcounty. It will complement the household survey in documenting changes in the quality and quantity of public service delivery in the study subcounties.

Qualitative subcounty interviews: At the SC level there will be as well a qualitative component, administered through a semi-structured interview instrument to a focus group of residents/citizens. The qualitative component differs both in temporal and geographic coverage of SCs from that of the household survey and quantitative SC survey. It will take place once, right before the baseline data collection. Furthermore, it will be conducted in a selection of 12 subcounties: four of these are drawn from among the study subcounties, specifically one from each region. Another four are drawn from outside the study subcounties, specifically, in subcounties that have already received / been receiving the subcounty-level baraza intervention. Again, these four are selected one from each region. The remaining four (also one from each region) are in subcounties that have not received a subcounty baraza but are located in districts that have received a district-level baraza. All 12 subcounties will be from different (i.e. from 12 in total) districts, to ensure that we capture geographic and administrative variety in each of the three treatment types, i.e. (i) not-yet-treated, (ii) treated at the SC level, and (iii) treated at the district level.

This qualitative component will serve three major purposes: Firstly, the insights will provide a first exploratory assessment of impact of the already conducted barazas. This means that the OPM will not have to wait until the analysis of the endline survey for a first evaluation of the role that the barazas are having for the quality and quantity of public services—or shortcomings in that regard. The qualitative analysis will provide a first view into the effectiveness of the barazas. Secondly, analysis of the qualitative data will give insights on the impact pathways and mechanisms of the barazas and will allow an indicative empirical validation of or correction on the theory of change underpinning the study. Finally, the in-depth interviews in the subcounties will inform the quantitative household and subcounty-level survey instrument, by

pointing to previously unforeseen outcomes, issues, and questions that will need to be captured in the analysis and thus in the questionnaires.

The core experimental study design, described above, can be classified as a ‘gold-standard’ strategy to assess the impact of the baraza intervention in Uganda. It therefore yields robust and reliable results. However, the data collected as described can allow the use of qualitative methods, as well as inclusion of quasi-experimental methodological elements, to explore the impact of the program and its impact pathways. Mixed methods approaches are encouraged in impact assessment (White, 2008, 2011), and multiple quantitative methods allow comparing and testing the sensitivity of results of one approach against the other. In the next paragraphs, we explain the quasi-experimental methods we intend to use.

e. Key robustness and sensitivity analyses

In order to further explore the robustness of our findings, in light of the potential limitations in experimental design, we employ selected methods from the body of quasi-experimental approaches, each of which addresses selection bias in different ways and has different strengths and limitations, with none being unambiguously superior in all circumstances (Ravallion, 2007).

One of the possible limitations even in randomized designs is that the units selected for treatment may in fact not receive the treatment, or may not receive it in the fashion that was intended by the intervention. Conducting standard analysis that does not account for this potential discrepancy between treatment and intention to treat could understate the impact of the intervention. In the case of the baraza program, we have initiated careful discussions with the implementing agency about the importance in adhering to a pre-determined design have. We will also deploy a research assistant to review the process of implementation in addition to other measures so as to minimize this discrepancy. Even with these measures in place, however, it is impossible to completely rule out any potential discrepancies between design and actual treatment, because in some cases it may be hard to obtain full field information about the actual features of treatment, subcounty by subcounty. Therefore, we will supplement our core analysis with analysis that uses the intention-to-treat data as an instrument for treatment (Abadie et al., 2002).

A second approach from the quasi-experimental toolbox that we will employ on our data from the experimental design is double-differencing. Randomization fundamentally avoids the existence of systematic differences between the treated and non-treated elements. However it does not prevent nontrivial differences that may be present by chance. Therefore, we will conduct a difference-in-differences analysis, rather than derive results only on the basis of the endline surveys and rather than limiting the use of the baseline survey for the purposes of stratification (White, 2013a). The double-

difference analysis will compare service delivery change outcomes before and after the baraza interventions for households located in subcounties that held a baraza and households in subcounties without a baraza intervention. The baseline and the endline wave of survey data can be used to effect this analysis.

Finally, we will use statistical analysis to account for other important, although less critical limitations of the experimental approach. One such issue we immediately think of is spillover effects, whereby untreated villages also profit from the intervention. The existence of spillovers would lead to an underestimation of the treatment effect. To test the robustness of our results to spillover, we will follow Miguel and Kramer (2004) and add distance to the nearest treated subcounty as a control variable. In the same vein, we will include a host of other controls to rule out alternative explanations of the effects we identify.

6. EXPECTED OUTPUTS

The study will have several outputs including working papers, high quality peer-reviewed international journal articles, research briefs, field and other documentation and manuals, and several oral presentations in Ugandan and international forums. The results of this study will help inform the government on the impacts and effectiveness of barazas in general on service delivery and will serve as a baseline and benchmark with the help of which the impact of related future programs can be evaluated. Importantly, we will disseminate findings of the study to the program implementers in Uganda (OPM) through workshops as well as more informal small meetings. In addition to providing information about and dissemination of the study findings, these workshops and meetings will also provide theoretical and practical guidelines that can be used by program implementers and evaluators to measure impact of barazas and other related interventions. During the course of the study, we also plan to hold seminars, the objective of which is to develop analytical capacity of staff members of OPM and other analysts and researchers in Uganda. The fact that this study would be implemented by researchers from the International Food Policy Research Institute's (IFPRI's) office in Kampala, with its long history of engaging with policy makers in Uganda, is likely to be beneficial for the policy impact of this evaluation.

7. PROJECT MANAGEMET, RESEARCH ACTIVITIES AND WORK PLAN

Overall management including internal communication and collaboration, collection of the baseline and subsequent datasets will be the responsibility of the project coordinator at IFPRI-Kampala office, Dr. Nassul Kabunga. Other IFPRI team members, namely, Dr. Bjorn van Campenhout and Dr. Tewodaj Mogues will support the project in various capacities. Dr. Haroon Sseguya,

who has been identified by IFPRI Kampala Office as a national collaborator, is based at Makerere University, a key partner in this study.

The project will be divided into a series of sequential research activities. Table 2 presents a work plan detailing the main activities, persons responsible, and annotated timelines. Any adjustments in any of these will be done in consultation with OPM and 3ie.

Table 2: Study Work plan

Date	Activity	Responsible
YEAR 1: QUALITATIVE INTERVIEWS AND BASELINE SURVEY		
May 2014	<ul style="list-style-type: none"> Planning for the qualitative survey: interviews with: <ul style="list-style-type: none"> OPM baraza M&E staff key informants at the local government level Area and household sampling 	Nassul, Bjorn and Haroon in consultation with OPM M&E staff
June 2014	<ul style="list-style-type: none"> Development of survey tools Recruitment and training of field supervisors and enumerators 	Nassul, Bjorn, Tewodaj and Haroon in consultation with OPM staff
July-Aug 2014	<ul style="list-style-type: none"> Implementation of the baseline survey <ul style="list-style-type: none"> Pilot testing of the survey tools Actual data collection 	Nassul, survey supervisors and enumerators
Sept 2014	<ul style="list-style-type: none"> Data entry and cleaning 	Nassul, Bjorn and data entry clerks
Oct-Nov 2014	<ul style="list-style-type: none"> Data analysis and report writing 	Nassul, Bjorn, Tewodaj and Haroon in consultation with OPM staff
YEAR 2: TRAINING AND BASELINE REPORT		
January 2015	<ul style="list-style-type: none"> Hold training workshop with OPM staff Submit preliminary reports to OPM and 3ie based on the baseline analysis 	Nassul, Bjorn, Tewodaj and Haroon in consultation with OPM staff
February 2015	<ul style="list-style-type: none"> Submit final report to OPM and 3ie 	Nassul and Bjorn
	<ul style="list-style-type: none"> Hold a dissemination workshop, targeting all stakeholders 	Nassul, Bjorn, Tewodaj and Haroon
YEAR 3: ENDLINE SURVEY		
March-May 2016	<ul style="list-style-type: none"> Hold consultations with: <ul style="list-style-type: none"> OPM baraza M&E staff key informants at the local governments of selected districts 	Nassul, Bjorn and Haroon
June 2016	<ul style="list-style-type: none"> Recruitment and re-orientation of field supervisors and enumerators, where necessary 	Nassul and Bjorn
July-Aug 2016	<ul style="list-style-type: none"> Implementation of the endline survey 	Nassul, field supervisors and enumerators
Sept 2016	<ul style="list-style-type: none"> Data entry and cleaning 	Nassul, data entry clerks
Oct-Nov 2016	<ul style="list-style-type: none"> Data analysis and report writing 	Nassul, Bjorn, Tewodaj and Haroon
Nov-Dec 2016	<ul style="list-style-type: none"> Hold a training workshop at OPM Submit preliminary report comparing changes between the baseline and end-line to OPM and 3ie 	Nassul and Bjorn
YEAR 4: FINAL REPORT		
Jan 2017	<ul style="list-style-type: none"> Submit final report 	Nassul and Bjorn

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