Can Journalism Improve Government Responsiveness?

Experimental Evidence from Tanzania

Dylan W. Groves [*].

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Abstract

I evaluate the influence of journalism on government responsiveness using a national-scale randomized controlled trial in Tanzania. I collaborated with 15 regional radio stations to identify 206 communities experiencing service delivery problems like flooded roads, broken water points, and missing medical supplies. I then randomly assigned half the communities to the treatment group and half the communities to a pure control condition. In treatment communities, journalists investigated the service delivery problem, broadcast their findings on regional radio, and conducted follow up reports several months later. Seven months after the reports were broadcast, independent auditors evaluated the service delivery problem in all 206 communities. I find that treatment communities received higher audit scores on average (coefficient = 0.25 standard deviations, randomization inference p-value = 0.033), amounting to one road or water point repair in every four treated communities. The investigations generated observable responses by un-elected government ministries but *not* citizens, local government officials, or members of parliament.

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1 Introduction

Can journalism promote government responsiveness? Classic models of electoral accountability suggest that journalism can help citizens motivate public officials or replace those who under-perform (Barro 1973; Ferejohn 1986), and a growing body of observational studies conclude that investigative reporting reduces corruption and contributes to good governance (Berry 2008; Gentzkow et al. 2006; Hamilton 2018; Lublinski et al. 2016; Norris and Bank 2010; Spurk and Dingerkus 2017).

But the correlation between media coverage and government responsiveness does not necessarily imply a causal relationship between the two. The features of stories that attract journalist's attention, such as outraged citizens or visible human suffering, may lead to a government response even in the absence of journalists' reports. Researchers' efforts to account for these selection effects are often confounded by journalists' reliance on insider sources and local information that academic researchers cannot readily observe. As a result, fundamental questions about whether and how investigative journalism enhances government responsiveness remain unanswered.

One strategy for overcoming these challenges is to work directly with journalists and media organizations to randomly vary the topics and communities that they cover. Recent research has employed this approach to study whether online opinion magazines influence public expression (King et al. 2017) and whether newspaper evaluations of incumbent performance influence budget allocations (Banerjee et al. 2020). However, no study has worked with journalists to experimentally evaluate the activity at the core of their profession: investigating the causes of citizen's grievances and publicizing the findings (Schudson 2002). As a result, the causal effect of investigative journalism, a central question at the intersection of political science and media studies, remains unresolved.

This paper reports the results of a national-scale field experiment in Tanzania designed to evaluate the influence of investigative journalism on government responsiveness to service delivery failures. I worked with local radio stations and journalists in 15 regions to identify service delivery failures in 206 communities, each serving approximately 1,500 citizens. I randomly selected 103 communities to be the target of local media investigations which were collectively entitled *Wakati Ni Sasa* (*WNS*) (*The Time is Now*). The remaining 103 communities made up a pure control condition. Investigations focused on one problem in each community specified prior to randomization, such as a broken bridge, unpotable water, absentee nurses or an overcrowded school. In treatment communities, journalists spoke with citizens, elected officials, local bureaucrats and relevant ministries to document the problem, then produced and broadcast 20 minute radio news programs about their findings. Journalists returned to treatment communities 4-6 months later for follow-up reports.

To measure the effects of the *Wakati Ni Sasa* investigations, a team of 16 enumerators conducted unannounced audits of the pre-specified service delivery problem of all communities in the sample seven months after the initial

investigations were broadcast. In each community, auditors conducted 2-3 surveys with local leaders and bureaucrats, collected photographic, video, and audio documentation of service delivery changes, and compiled 1-2 page qualitative assessments of how the targeted problem was addressed over the project period. Auditors then filled out assigned a report card score between -10 and 10 to overall improvements in the targeted public good. They also assigned scores to the responses of citizens, non-governmental organizations, and different government actors.

The primary finding is that journalism improves public service provision. Auditors gave better report card scores to public services in treatment communities than in control communities (coefficient = 0.25 standard deviations, randomization inference p-value = 0.033). The effect corresponds to one repaired road, clinic, or water point for every four communities where a *WNS* investigation was conducted.

The second finding is that journalism spurs responses from unelected ministry officials but not citizens, non-governmental organizations, community-level government officials or members of parliament. While a precise identification of mechanisms is beyond the scope of this research design, quantitative and qualitative evidence suggest that local media investigations improve public service provisions by helping the ruling party monitor district-level ministry officials. These officials respond to *WNS* investigations because negative news coverage threatens their career trajectory within the bureaucracy, while positive news coverage of their response could boost their trajectory.

There are important limitations to these findings. The experiment was implemented outside of an election year, so mechanisms related to electoral accountability were less likely to be operative, and the experiment took place only over the course of seven months, so the full range of plausible citizen and government responses may not have had time to manifest. It is therefore possible that different mechanisms may be more salient under different experimental conditions. In-person interviews with citizens and some leaders were made impossible by COVID-19 restrictions, so outcomes are limited to audit reports. Finally, the experimental design cannot rigorously determine whether journalists' investigations influenced the *amount* of resources provided to citizens or simply their *allocation*. Each of these concerns point to directions for future research.

Nonetheless, this study makes important substantive, theoretical, and methodological contributions. Substantively, the paper demonstrates that local "watchdog" journalism can improve government responsiveness to service delivery failures, even in the "hard-case" of (i) a low-income country (ii) ruled by a hegemonic party regime (iii) outside of an election year. The paper also provides insights into processes of political accountability and distribution in hegemonic party regimes that are common Sub-Saharan Africa (Levitsky and Way 2012). Popular models of government responsiveness emphasize accountability relationships between citizens and politicians (Grossman et al. 2020), citizens and local bureaucrats (Björkman and Svensson 2009; Kosack and Fung 2014), and politicians and bureaucrats (Raffler 2019; Slough 2020). Without discounting these accountability relationships, this paper highlights how media facilitates top-down accountability *within* ruling parties (Lorentzen 2014). Methodologically, this

study offers a template for future collaborations with media professionals to investigate whether and how their work influences political and economic outcomes.

This essay is organized as follows. I begin by reviewing existing theories of journalism. I then briefly describe politics, media, and service delivery in the Tanzanian context. Having set the backdrop for the study, I describe the *WNS* treatment and lay out the key features of the experimental design: sampling of radio stations and communities, random assignment, outcome measurement, and estimation procedures. I then assess the effects of the treatment on public service delivery and explore both quantitative and qualitative evidence for mechanisms. I conclude by discussing the implications of the findings and suggesting avenues for further investigation.

2 Theory

Existing empirical work identifies a robust correlation between the presence of mass media and a range of positive governance outcomes (Besley and Burgess 2002; Ferraz and Finan 2008; Larreguy and Marshall 2020a,b; Snyder 2008). However, mass media exposure encompasses a bundle of programs and content, and efforts to theoretically and empirically disentangle specific components are rare (Larreguy and Marshall 2020a). This paper homes in on a specific element of mass media: investigative journalism.

The organization Investigative Reporters and Editors (IRE) defines investigative journalism as *"the reporting, through one's own initiative and work product, of matters of importance to readers, viewers or listeners. In many cases, the subjects of the reporting wish the matters under scrutiny to remain undisclosed."* Hamilton (2018) identifies three distinct elements in this definition: (1) original reporting on (2) issues of public concern that (3) powerful actors would prefer not be made public. This definition is to the common understanding of "watchdog journalism", which Norris and Bank (2010) define as *"asking hard or probing questions of the powerful to maximize transparency and to serve the public interest...[watchdog journalism] commonly highlights failures in government, especially those arising from...incompetent management of public service delivery."* The notion that investigative journalists contribute to political accountability and responsive governance is widely accepted, but even proponents of investigative journalism acknowledge that rigorous empirical support for their position is lacking (Norris 2014).

In this section, I develop a theory of the relationship between investigative journalism and government responsiveness that is particularly applicable to hegemonic party regimes that are common Sub-Saharan Africa. I draw on 12 months I spent working with local radio stations and shadowing investigative journalists in Tanzania, along with more than 50 interviews I conducted with journalists, station managers, local politicians, and ministry officials across Tanzania.

I argue that media investigations help hegemonic party regimes overcome a central obstacle to effective governance: monitoring the performance of local ministry officials. Central governments face a classic principal-agent

problem in which they would like district-level ministry officials to deliver services and respond to citizen needs, but can only imperfectly monitor those officials' performance (Egorov et al. 2009; Lorentzen 2014). Local ministry officials would prefer to shirk and expropriate funds, but can be spurred to execute the central government's priorities if they are sufficiently worried that the central government would observe and sanction incompetence and malfeasance. When district-level ministry officials believe that a local service delivery failure is likely to be investigated by the media, they become more motivated to address the problem (Anderson et al. 2019).

In Tanzania, government resources for services like water, health care, education, and roads are distributed at the district level by local government authorities (LGAs) or district-level ministry offices (see Figure 2). Tanzania's President appoints regional and district commissioners to oversee the execution of these projects (Mdee and Mushi 2021). Regional and district commissioners are often referred to as the "eyes and ears" of the President, and they have the authority to influence promotions and demotions within the regional bureaucracy. Bureaucrats in ministry and district councils are motivated to curry favor with regional and district commissioners in order to advance their careers within the ruling party (Cheeseman et al. 2021; Fukuyama 2014). However, the oversight capacity of regional and district commissioners is imperfect, creating incentives to shirk for district-level bureaucrats (Carlitz 2017; Mdee and Mushi 2021).

In this context, local media investigations can serve an important function for the Tanzanian ruling party by exposing governance failures at community level. According to an award-winning local journalist in Mbeya,

> "*The (district-level) ministry officials have to worry that if they don't do anything, they will get in trouble with the government because they have a budget so as to create those roads, so if a journalist goes and asks about that budget, the ministry officials have a worry, usually about the regional commissioner or of course from the Minister.*"

This theory of journalism's influence contrasts with "bottom-up" theories premised on media's capacity to mobilize citizens to respond to service delivery problems directly or through the ballot box. It suggests that journalists are effective as much because of their relationships and connections within the ruling party apparatus as because of their ability to generate public outrage. Media's role promoting "top-down" accountability is deeply rooted in Tanzania's political history. Veteran journalists from Tanzania's era of single-party politics recall waiting by the phone after publishing investigations into local government malfeasance in the hopes that Tanzanian's first president, Julius Nyerere, would call them and thank them for alerting him to the issue (Mwafissi, interview). The media's role facilitating top-down accountability is also reminiscent of descriptions of media influence in China (Anderson et al. 2019; Pan and Chen 2018; Truex 2016). However, journalism's role facilitating top-down channels of accountability in a hegemonic party regime like Tanzania has yet to be tested experimentally.

3 Context

Three factors make Tanzania an apt context for studying journalism and responsiveness in a hegemonic party regime. First, Tanzania's ruling party, Chama Cha Mapinduzi (CCM), exhibits a level of hegemonic control and institutionalization that reflects many shared features of hegemonic party regimes in Sub-Saharan Africa and throughout the developing world. Second, Tanzania's media industry is underdeveloped but growing rapidly, so Tanzania's citizens and government officials are familiar with local press reports without being saturated by them. Finally, public service delivery in Tanzania is limited, leaving the door open for external interventions to improve development outcomes (Croke 2021).

3.1 Hegemonic party regimes

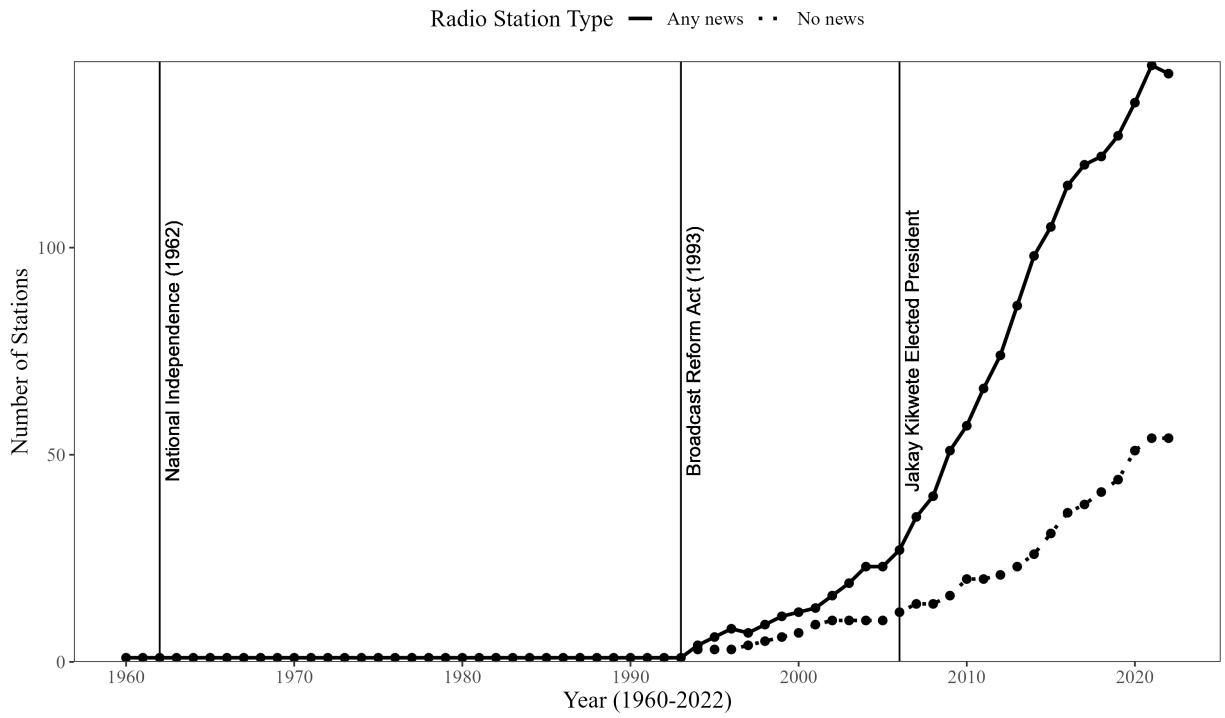
Tanzania has been governed by a single political party, Chama Cha Mapinduzi (CCM), since its unification in 1964.[1] Through this history, CCM has rarely faced a meaningful threat from opposition parties. CCM has never won less than 58% of the presidential vote or 70% of seats in parliament (Morse 2012, 2019; Weghorst 2022). CCM's electoral hegemony is attributable in part to a sophisticated and highly institutionalized party apparatus with operations that extend to the community level.[2] In both its sustained control of the executive and parliament and its moderate toleration of opposition political parties, CCM is broadly representative of other hegemonic party regimes in Sub-Saharan Africa, including Namibia, Senegal, and Uganda (Morse 2019; Weghorst 2022). CCM's highly institutionalized party apparatus is reminiscent of ruling party structures in Ethiopia, Rwanda, Senegal, and Zimbabwe as well as throughout Eastern Europe, Latin America and Southeast Asia (Weghorst 2022).

3.2 Growth of independent mass media

Like citizens of many post-colonial states, Tanzanians have witnessed a dramatic expansion of local and independent media over the last thirty years (Sturmer 1998). From independence until 1992, there were just two radio stations broadcasting in the entire country: the government-owned *Sauti ya Dar es Salaam* and *Sauti ya Injili*, which is operated by the Catholic Church. After political liberalization and the repeal of Tanzania's most onerous media restrictions in 1993, the number of independent radio stations climbed to three in 1995 and 33 in 2002 Figure 1). Today, there are just over 200 radio stations.

[1] Prior to unification, Tanzania was ruled by CCM's precursor, TANU, from independence in 1961 through 1964. It was a one party state until the introduction of multipartyism in 1992.
[2] Often called the "ten cent system"

Figure 1: **Growth of Independent Radios in Tanzania**



While many of Tanzania's newly minted media houses are focused on urban centers like Mwanza and Dar es Salaam, about 70% of Tanzania's registered radio stations are based in regions that, prior to 2005, had never carried a regionally-focused news outlet. These local and independent radio stations are often owned by local business people and politicians (47%), religious organizations (21%), and community-based organizations (30%). While most privately-owned radio stations focus on music and entertainment, about one quarter are focused on news, politics, and social issues, and almost all local radio stations broadcast daily local news coverage and employ teams of journalists (Katunzi and Spurk 2019; Spurk and Dingerkus 2017). In comparison to other East African countries, however, Tanzanian journalists have less training and fewer resources (Katunzi and Spurk 2019). Nonetheless, local media investigations are a standard component of local radio programming in Tanzania. The Tanzanian Development Information Organization (TADIO), an umbrella organization of more than 34 local radio stations representing every region of Tanzania, shares weekly local news reports broadcast by its partners. These stories range from sharing a community's grievances about the the distances children walk to secondary school to tracking government responses to water shortages.[3]

In sum, Tanzania's media sector is growing rapidly but remains underdeveloped relative to other countries in East Africa. Most citizens can listen to or read press coverage about social and political issues, but it is rare that their local grievances will be the subject of press reports, especially in rural regions. As I discuss in more detail

[3]https://radiotadio.co.tz/dodomafm/2022/09/01/umbali-wa-shule-wapelekea-baadhi-ya-wanafunzi-pandambili-kuacha-shule/, https://radiotadio.co.tz/nurufm/2022/09/05/mtendaji-na-katibu-bandia-mbaroni-kwa-uchochezi-mkoani-morogoro/

below, the *Wakati Ni Sasa* press reports discussed in this paper were not significantly different than the standard practice of Tanzania's local radio stations. Instead, *WNS* influenced the frequency, targeting, and quality of local media investigations.

3.3 Delivery of Public Goods and Services

Tanzanian citizens often lack access to basic public goods and services like health care, education, water, and roads, a bundle of basic public goods often collectively referred to as "maendeleo" (development). In Afrobarometer surveys, just 21% of Tanzanian citizens reported having access to piped water, 68% to schools, and 37% to health services in their community. More than half of low-income Tanzanians report moderate or high difficulty gaining access to medical care or water, sanitation, and electricity services.

Since 1999, the Tanzanian government has pursued a strategy of "decentralization by devolution" (Mclellan 2021; Mdee and Mushi 2021). The centerpiece of this strategy has been transferring authority for implementing development programs from the central government to officials at the district level. In theory, decentralization brings development decisions to the level of the community, improving targeting, responsiveness, and citizen input. In practice, decentralization has been both less effective and less complete than originally planned (Mdee and Mushi 2021). Responsibility for water and roads projects was first devolved then re-centralized in 2016, while responsibility for health and education remains blurred. The result is that citizens are often unaware which government bodies are responsible for guaranteeing the provision of different government services.

During in-depth interviews, village, ward, and district officials outlined three routes to government service delivery in Tanzania (see Figure 2). In the "standard route" to public goods provision, the parliament allocates budgetary support to districts (known as Local Government Authorities, or LGAs) earmarked for specific development priorities. Unelected district executive officers work with elected district councils to allocate funds to wards and villages. This route is particularly common for the provision of health and education services.

A second route runs through national ministries and parastatals, such as the Rural Water Supply and Sanitation Agency (RUWASA), the Tanzania Rural and Urban Roads Agency (TARUA), and the Tanzania Electric Supply Company Limited (TANESCO), which maintain offices in each region and are empowered to directly pursue development projects at the community and ward level. When funding from district councils and ministries is not forthcoming, community and ward governments often raise money directly specific development projects by taxing local businesses or soliciting citizen contributions.

Finally, in some cases Members of Parliament contribute money and supplies to ongoing development projects, especially during election years. In the absence of government support, public goods and services are often provided by citizen self-help initiatives and non-governmental organizations.

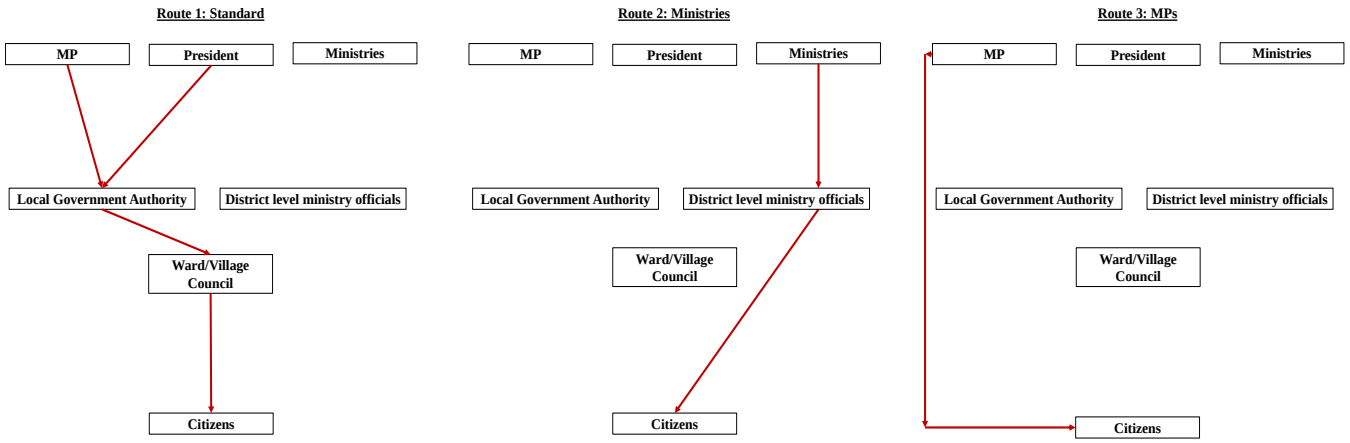
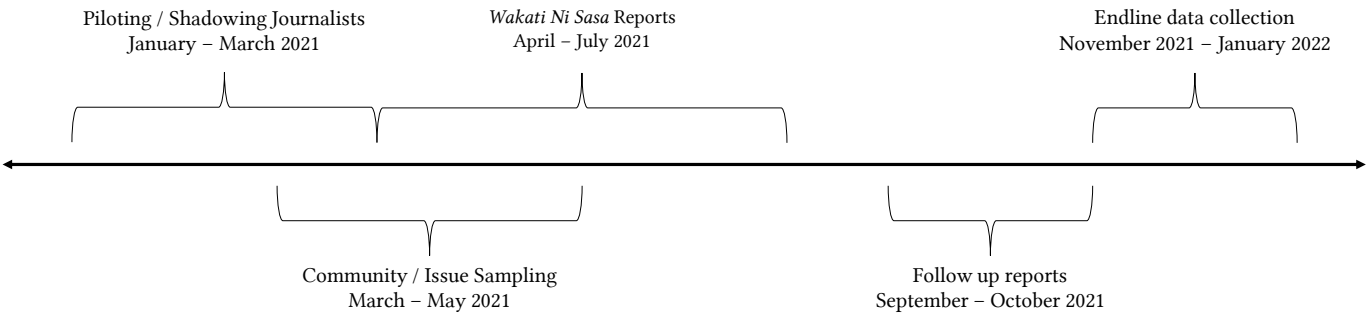


Figure 2: **Three routes to service delivery**

4 Intervention

The focus of this paper is the influence of journalism on the provision of public goods like water, health care, education, and roads. I spent more than 12 months shadowing local journalists in an effort to develop a template for local journalist investigations that reflected procedures of high quality reporters. I then collaborated with radio station partners and journalists standardize a template for reporting, producing, and broadcasting investigations about service delivery failures in a project entitled *Wakati Ni Sasa* (*The Time is Now*).[4] The *WNS* intervention represents a bundle of three related components: (1) investigations into the causes and consequences of service delivery problems; (2) production and broadcast of 20 minute news reports about the results of the investigations; and (3) follow-up contacts with responsible officials to track responses to the initial investigations. The procedure represents "best practices" for trained journalists in Tanzania (Katunzi and Spurk 2019). Figure 3 shows the timeline of piloting, reporting, and data collection in the study.

Figure 3: **Study Timeline**



Wakati Ni Sasa journalists began each investigation by reaching out to local leaders and key informants in tar-

[4]Audio recordings and written summaries of reports are available here.

geted communities. Preliminary outreach served both a pragmatic and ethical purpose. Pragmatically, preliminary conversations gave journalists a preview of the service delivery problem and let them know whether any background interviews (for example, with water or road engineers) were necessary prior to visiting the community. Ethically, preliminary conversations with government officials reduced the risk of unexpected confrontations with local leaders during the community visit and helped journalists identify safety issues (COVID-19, crime, or unsafe transportation) they needed to consider before departing.

After conducting prelimenary outreach, *WNS* journalists travelled to the targeted community to investigate the pre-specified service delivery problem. The journalists were trained to begin their investigations by interviewing a range of citizens about the history of the problem and the way the problem was affecting their community. Journalists then conducted follow-up interviews with government officials. While *WNS* journalists were given wide latitude to pursue their investigations as they and their station manager saw fit, they were expected at minimum to interview the village/street chairperson and executive officer, ward councilor and executive officer, district commissioner, and representative from the relevant ministry. *WNS* were also required to contact the relevant Member of Parliament to request comment. Requiring *WNS* journalists to contact a range of government officials adhered to best practices of investigative journalism as well as Tanzanian media law.

Community visits usually lasted between one and three days, and follow-up interviews with higher level government officials lasted anywhere between one week and several months. The most common reason for extended reporting times was the refusal of district and regional officials to make themselves available for interviews. *WNS* partners then produced 15 minute narrative reports for radio broadcast. By agreement, the reports always consisted of an introduction explaining the broader relevance of service delivery issue, interviews with citizens emphasizing how the service delivery issue was affecting their lives, and interviews with relevant government officials about the history of the problem, the reasons for government inaction, and the government's plans for addressing the issue in the future.

All reports were broadcast twice over the course of a week, and listeners were given the opportunity to call or text their comments. In addition to the radio broadcast, reporters were free to post pictures and discussion about the stories to their website and social media. In anticipation of the broadcast, *WNS* journalists called key citizen informants and government officials to inform them that the episode would be put on air. They also informed all parties that they would conduct a follow-up episode in the ensuing months to track the community's progress towards resolving the issue.

Three to four months after the first broadcasts, journalists conducted follow-up investigations in targeted communities. Follow-up reports were shorter than the original reports, and included phone call interviews with citizens and government officials. The reports were conducted regardless of whether the government had resolved or failed

to resolve the specified issue.

4.1 Was *Wakati Ni Sasa* realistic?

How closely did *Wakati Ni Sasa* investigations reflect real-world news reports in Tanzania? As the description of media context in Tanzania makes clear, local news investigations are a weekly feature of at least one (and often more) radio station in every region in Tanzania. I took several steps to ensure that the *WNS* reporting template reflected best practices by local journalists. Radio station partners picked journalists for *WNS* who were already on the station's staff, and *WNS* journalists did not receive additional formal training. The sampling process was modelled on the weekly routine radio newsrooms follow to select communities for local news reports. The resulting news stories were produced and broadcast entirely by radio station staff. The primary deviation from standard reporting procedures was that journalists submitted the results of their investigations to a research team member, and in some cases received feedback about additional governmental officials they should contact.

Rather than introducing a qualitatively different type of press coverage, then, *WNS* changed the *frequency*, *targeting*, and *quality* of local media investigations. *WNS* changed increased the frequency of media investigations by providing funds to journalists to report and produce more stories than their station managers could otherwise afford. *WNS* influenced the *targeting* of media investigations by introducing a randomized component and by providing transportation funds so that journalists could reach rural communities they otherwise could not reach. Finally *WNS* influenced the *quality* of media investigations at some radio stations by introducing a shared template for reports so that journalists at lower-capacity stations still followed the best practices of high performing journalists I shadowed in the qualitative lead-up to the project.

5 Research design

The experiment is a matched-pair randomized controlled trial with randomization and outcome measurement at the community level. 103 treatment communities received the *WNS* intervention, which included investigations, broadcasts, and follow-up reports. The remaining 103 communities made up a pure control condition. This section reviews the project's sampling process, random assignment, data collection, and estimation strategy.

5.1 Sampling

Radio stations. I conducted the experiment in collaboration with 15 local radio stations located across Tanzania. I partnered with radio stations that met three conditions: they were located in mainland Tanzania (rather than Zanzibar), they had district or regional (rather than national) radio broadcast permits from the Tanzania Communications and Regularly Agency (TCRA), and they were identified by professors of journalism at the University of Dar es Salaam and St. Augustine University School of Journalism for broadcasting high quality news reports. I ultimately identified radio stations in 15 of Tanzania's 26 mainland regions. Excluding Dar es Salaam, regions with

selected radio stations are comparable to regions without selected radio stations in average size (35,000 vs 36,000 km^2) and average population (2.0 vs 1.8 million).

Villages and streets. The study's unit of randomization is the community (referred to as a village in rural districts and a street in urban and peri-urban districts). To identify communities with service delivery problems, radio stations asked listeners to call or send text messages about water, health, education, and infrastructure challenges in their area. They also included these requests after *Wakati Ni Sasa* broadcasts. In the week-long preparation stage, participating radio stations received between 60 and 180 text messages, and continued to receive SMS texts and calls throughout the project period. A sample of texts across different issue areas appears below:

> "*[Ward name] has 4 teachers and does not have a single female teacher and has about 500 students and only 6 classrooms is a big problem problem over the problem. This is [name]*"

> "*I am called [name] of [village]....The biggest challenge is the poor school toilets and I really ask the government to help us*

> "*I am called [name] I am found in the village of [village name], the challenges of our village is in the health side, with insurance if you go to another [clinic] you are written to buy [from] the pharmacy, it means the government does not have medicines.*

> "*Water crisis in the Dodoma region of [district name] of [village name] has a shortage of water...Please we need help to solve this problem of water. Water shortage Thank you [phone number]*"

> "*My name is [name] in the village of [village name] Region Dodoma. The challenge facing [school name] children is a road full of water that prevents children from school: [mobile number]*"

Radio stations also learned about service delivery problems through informal networks of informants. Radio stations submitted communities in pairs to facilitate the matched-pair randomization described below. The research team excluded communities if they could not verify the problem through follow-up phone calls to community members, if the proposed investigation posed risks to the safety of citizens or journalists, or if another community in the same ward was already included in the sample. Ultimately, the research team selected 103 community pairs (206 total) for *WNS* program eligibility. This strategy limited the sample to communities where at least some members were aware of the service delivery problem; this study does not consider the role of media in *uncovering* previously unrecognized governance failures.

5.2 Random Assignment

I conducted random assignment to experimental conditions at the community. Paired communities are generally located in the same district and requested media investigations in the same week. Within each matched pair, one community was randomly assigned to treatment and one community as randomly assigned to control. Figure 4

shows the geographic distribution of treatment and control communities. Treated communities are colored blue and control communities are colored red, while each shape represents a distinct service delivery issue.

Figure 4: **Random Assignment**

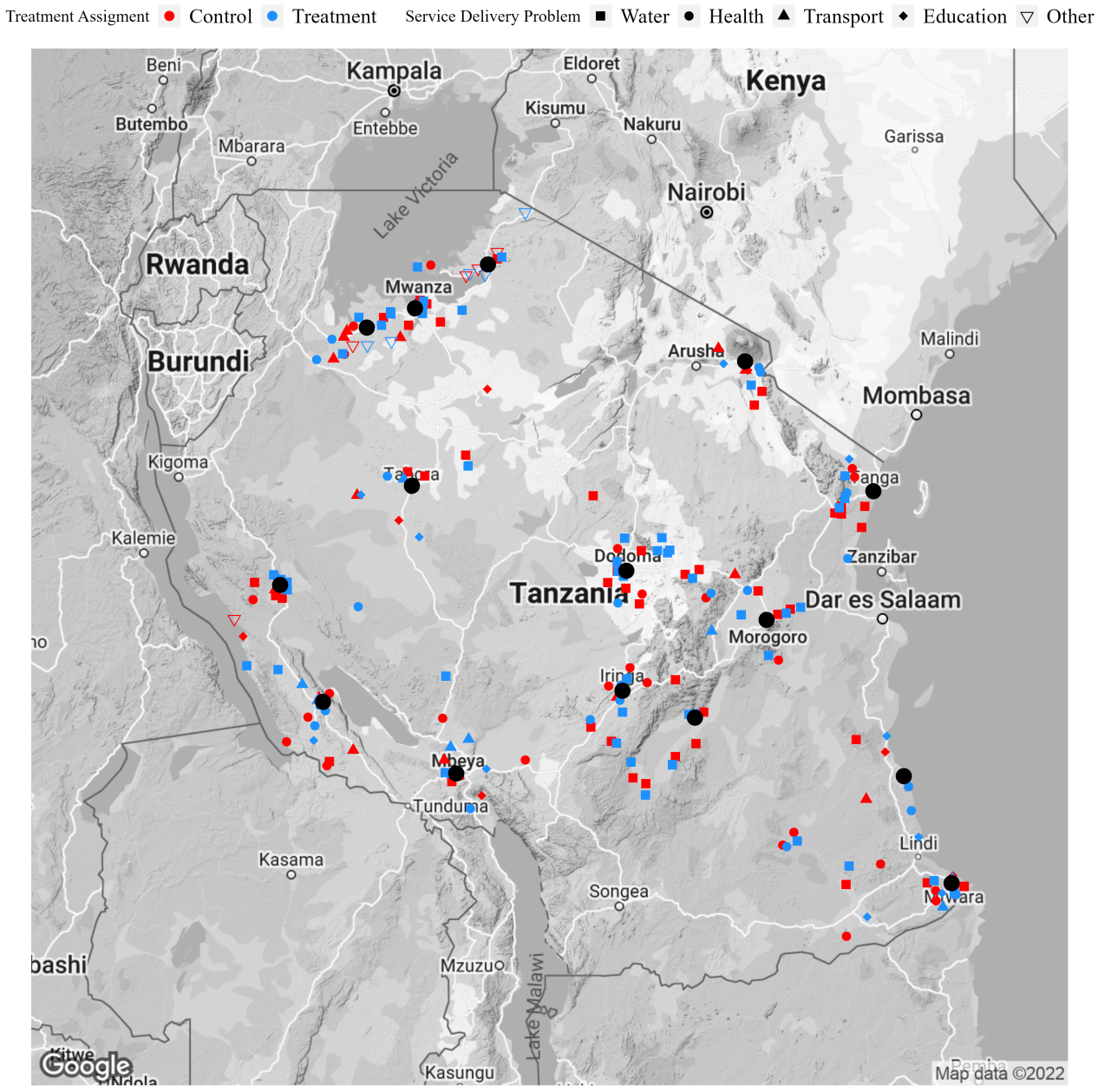


Table 1 demonstrates that random assignment was not significantly correlated with administrative measures of pre-treatment population, electoral competition, or distance from roads, towns, and service centers, nor was it correlated with pre-treatment measures of the nature and severity of the specified service delivery problem.[5] Out of 19 pre-treatment covariates, one (5%) generates a randomization inference p-value of 0.05 or smaller, and only

[5]I did not conduct a baseline survey due to concerns that baseline data collection would triggered two potential channels of media influence: mobilizing community focus on a specific service delivery issue and making local leaders feel that their performance was being observed.

two (11%) generate a randomization inference p-value of 0.1 or smaller, almost exactly what is expected to occur by chance alone.

Table 1: **Balance on Pre-Treatment Covariates**

Variable	Treatment	Control	RI p-value	Observations
Politics				
Village Established Afer 2015	0.175	0.126	0.370	206
Share CCM Ward Councillor Vote - Ward (2015)	0.611	0.621	0.420	190
CCM Ward Councillor Dummy - Ward (2015)	0.700	0.700	0.850	200
Population				
Male Population	1372.872	1640.483	0.110	165
Female Population	1412.936	1729.782	0.085	165
Household population	583.462	684.954	0.120	165
Service Delivery Problem Severity				
Pre-Treatment Problem Score (0-10)	7.010	6.853	0.580	203
Pre-Treatment Problem Pair Winner	0.059	−0.059	0.455	203
Service Delivery Problem Type				
Problem - Education	0.097	0.087	0.560	206
Problem - Health	0.233	0.262	0.450	206
Problem - Transport	0.107	0.136	0.340	206
Problem - Water	0.515	0.466	0.390	206
Problem - Other	0.049	0.049	0.735	206
Distances (km)				
Distance from Radio Station	44.063	47.526	0.155	206
Distance from Regional Capital	48.836	54.028	0.035	206
Distance from Dodoma	352.708	351.524	0.660	206
Distance from Clinic	33.832	35.842	0.335	206
Distance from Schools	4.067	4.104	0.935	206
Distance from Secondary Roads	21.378	21.815	0.775	206

Note: "Village established after 2015" is a binary variable that takes the value 1 if the village was established after 2015 and 0 otherwise. The remaining politics variables from publicly release vote totals. Dummy variables take the value 1 if the Ward Councillor or Parliamentary seat was occupied by a CCM party member in 2015. Population values are taken from Administrative data shared by Tanzania's Data and Statistics Agency. Data are from 2015, and so is unavailable for newly established villages. Service delivery problem severity data comes from independent audit evidence collected about the original service delivery issue. Service delivery type is a series of binary variables based on the pre-treatment identification of the issue area in each village. Distances are calculated using GPS coordinates of the village and geo-located data on the location of cities and administrative units.

Table 1 also offers a useful profile of the community sample. On average, sampled communities are small and rural: their populations range between 1,000 and 2,000 citizens and are on average 21 kilometers from the nearest paved road and 4 kilometers from the nearest school. Politically, most communities supported the ruling *Chama Cha Mapinduzi* party, but the support was not overwhelming. On average, 61% of voters in sample communities voted for the CCM candidate for Ward Councillor, and the CCM candidate won 70% of 2015 Ward Councillor elections in sampled wards. With respect to the service delivery problems in sampled communities, Table 1 shows a clear focus on water shortages (48% of communities), followed by health services (23%), infrastructure (11%), and education (9%). *WNS*'s emphasis on water service delivery presaged a broader shift in governmental focus on rural water supply

in 2022: just as endline data collection was ending, The Tanzanian Ministry of Water and Irrigation announced a country-wide inspection of all government water projects.[6]

5.3 Outcome Measures

Treatment uptake. The first group of measurements centers on program implementation. To assess whether *Wakati Ni Sasa* reports were produced and broadcast, I asked radio stations to submit an audio file of the finished report, and assigned a research team member to listen to the station to confirm that the program was broadcast at two agreed-upon dates.

I also assessed whether *Wakati Ni Sasa* reports were noticed and remembered by local leaders in targeted communities. Enumerators conducted interviews with three community leaders: the village chairperson, the village councilor most directly responsible for the targeted service delivery issues (e.g. the head of the education committee), and the chairperson of the largest sub-village. To measure whether local leaders encountered journalists during the investigations, enumerators asked leaders "How many times has a journalist visited this community or street to address the [targeted issue] problem in the past seven months?" To measure whether leaders heard the reports broadcast on air, enumerators also asked "Have you heard a radio report about a [targeted issue] problem in your community in the past seven months?" For both responses, I coded the community-level outcome as 1 if any community leader responded "yes" and 0 otherwise. I then constructed an index taking the value 1 if any village leader remembered a journalist investigation about the targeted problem *or* heard a report broadcast about the targeted problem.

I also measured how *WNS* reports influenced leaders' perceptions about the likelihood of future reports. Enumerators asked the chairperson "How likely do you think it is that a journalist will visit your village/street if it has a [targeted issue] problem in the future?" and coded the response as 1 if the chairperson said "very likely" and 0 otherwise (very few leaders responded "not at all likely," even in communities that had never received a report).

Service delivery. The outcome of primary interest is the quality of core services like education, health care, water provision, and roads. To measure changes in service delivery, trained auditors conducted unannounced audits in every community in the sample five to seven months after the original *WNS* reports aired. Trained auditors, blind to both treatment assignment and the nature of the intervention, contacted located leaders, citizens, and district officials in each community and asked them about their experiences with the pre-specified target problem. Based on these interviews, auditors wrote narrative reports about the original service delivery issue as well as steps that had been taken to address the problem in the previous seven months. In addition to these qualitative accounts, enumerators collected photographs, videos, and audio recordings to document observable changes in the targeted issue area. They also collected evidence of changes in *other* service delivery areas to test for potential spillover

[6] *WNS* cannot claim credit for Aweso's announcement.

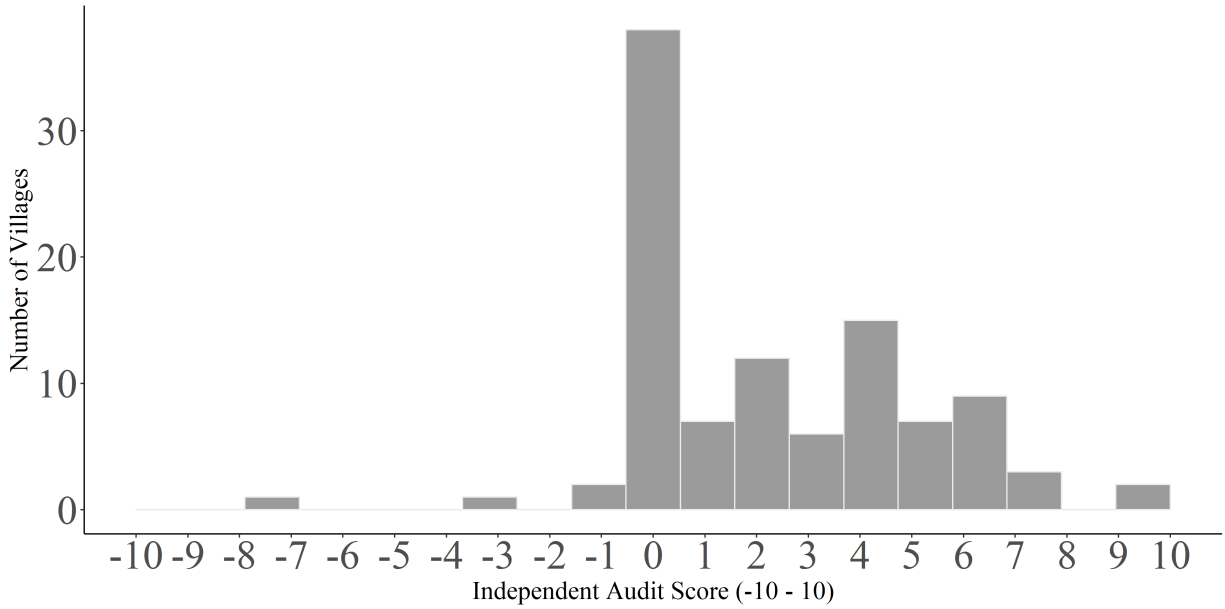
effects. Auditors submitted their findings in the form of 1-2 page audit reports that are available in the Online Appendix.

After collecting interviews and visual evidence, auditors scored service delivery improvements in each community. Auditors were asked, "On a score of -10 (much worse) to 10 (fully solved), how would you say the [targeted issue] problem has changed from seven months ago to today?" To calibrate scores across regions, I removed any mention of media reports from the qualitative reports and gave the reports and accompanying evidence to an independent reviewer to re-score. The correlation between the original auditor score and independent reviewer score is 0.68, and Cronbach's Alpha for the two measures is 0.81. The outcome presented in the main text is a simple average between the original auditor and independent reviewer's scores, although the primary results are almost identical regardless of grader. Figure 5 shows the distribution of scores in the control group sample. The modal outcome is 0 (no improvement), the mean outcome is 2.3 (marginal improvement or planned improvement), and the standard deviation in the control sample is 2.7.

Independent audits and report card scores offer an attractive outcome measure for several reasons. First, the procedure was "double-blind" in the sense that auditors were not told about treatment assignment or purpose of the intervention, and community leaders and citizens were not aware that the independent audits were connected to the *WNS* investigations.[7] Another attractive feature of the design is that it focuses on concrete outcomes rather than citizen or leader perceptions. This distinction is especially important in the context of a media study in which reports themselves may have influenced citizen and leader expectations and satisfaction even in the absence of objective improvements in service delivery. Finally, the audit measure is easily to replicate or augment. Because the basis for evaluations is including in qualitative report cards, future analysts could easily create a separate metric for evaluating service delivery success and give a community an updated score. For example, the scores provided here emphasize a holistic assessment of service delivery improvements (giving points for repairs, partial improvements, and pledges). If researchers were interested in giving credit only for new and completed service delivery projects, the raw data is available for creating such a score for each community.

[7] Even if auditors had found out that a given community received *WNS* investigations and allowed the knowledge to influence their scores, any mention of local press coverage was removed from the audits before being passed to the independent reviewer.

Figure 5: **Distribution of Independent Audit Scores**, Control Sample



What do these scores mean in practice? To make the meaning of the report card scores more concrete, I can connect each auditor's quantitative score to their qualitative narratives about the community's experience. In communities that received a score of 0, service delivery improvements were predictably nil, or minimal progress was offset by comparable setbacks. In the village of Liwiti in the southeastern coastal region of Lindi, for example, citizens have been requesting assistance to repair a road since 2018. Over the 7 month project period, no progress was made towards repairing the road. In the rural village of Ngoji, the absence of piped water forces villagers to regularly dig shallow wells by hand or retrieve pond water that often makes them sick. Villagers dug two wells during the project period, but the wells only replaced wells that dried up in the same period. Stories like Liwiti and Ngoji are by far the most frequent outcome in the control group sample; 28% of control communities received a score of 0. Citizens told auditors that they wait for election season to receive even the promise of future support.

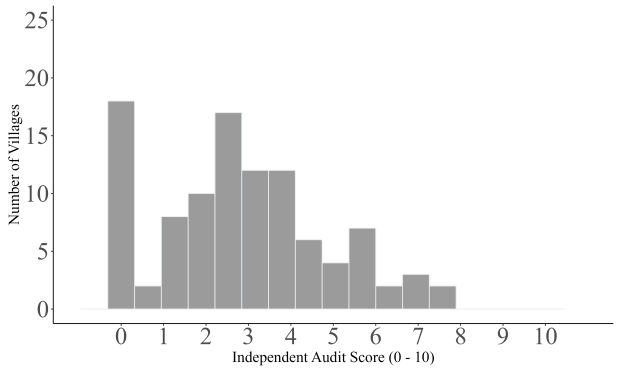
In communities that received scores between 1 and 3, citizens experienced positive but marginal improvements in service delivery, or plans for significant improvements were set in motion but had yet to be carried out. In the village of Ipagusa in Tanzania's central Morogoro Region, for example, more than half of the community lacks access to potable water because the local well is small and often broken. During the project period, the district government responded by investing just 60,000 TZS (approximately 30 US dollars) to clean dirt and grime around local wells, and did not drill additional wells to confront the severe water shortage. In Nyregete village in the Mbeya highlands, the local health clinic routinely runs out of supplies, the doctor is often unavailable because there are no staff quarters connected to the clinic and he lives in another village, and citizens from distant sub-villages need to walk 2-3 km to access health services. The government announced plans to build a new clinic in 2023, but the only action taken during the project period was to construct a new staff quarters for the doctor. Ipagusa and Nyregete received a score

of 1 and 3, respectively.

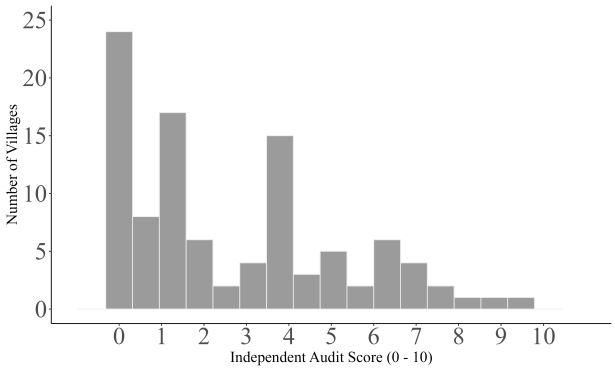
Communities that received audit scores about one standard deviation above the treatment mean (a score of 4, 5 or 6) experienced more tangible improvements in the targeted issue area. In Tutuo village of western Tabora district, which is home to over 1,500 students by only one school with ten classrooms and ten teachers, the government contributed 48 million TZS (about 20,000 USD) and citizens contributed labor to construct a new school. Construction of the school was underway but far from complete, and no action had been taken to resolve the teacher shortage. In Mabogini village at the base of Mt. Kilimanjaro, the absence of drainage ditches means that roads become flooded and impassible during the rainy season. During the project period, TARURA moved in to repair the roads: they laid gravel on the primary road and built two small roads to avoid the flooded area. Nonetheless, the new roads have severe potholes and still lack drainage ditches, so worries remain that the roads will flood next rainy season and soon fall into disrepair. Tutuo and Mkingala received a score of 4.5 and 5, respectively. In short, a one standard deviation improvement in a community's audit score relative to the control mean signifies the difference between the government promising development projects and the government actually initiating them, or the difference between a marginal response to a service delivery problem and a direct (albeit short-term) solution to it.

At the higher end of the audit score, a score from 7-10 means the difference between the government initiating a project and the government completing it, or the difference between a short-term solution to a service delivery problem and a more permanent solution. Just 5 communities in the control group (3%) received a score of 7 or higher. In Idunda village in the Udzungwa mountains, the scarcity of functioning water points forced villagers to rely on polluted water from the Ruli river. During the project period, the district government sent the Rural Water and Sanitation Agency (RUWASA) to repair broken water points, and the village council raised money to pay a local contractor to drill ten new water points in the Idunda and surrounding sub-villages. Idunda received a score of 7. One of the highest scoring control villages was Ismani in Iringa region, where the government responded to an under-resourced health center by contributing 400 million TZS (171,000 USD) to upgrade the health facility with a laboratory, a doctor's quarters, an operating room, and patient's ward. Villagers and villagers' relatives from outside the community also made significant donations. Idunda received a score of 9.

Responsivenes After scoring overall improvements in service delivery, auditors assigned scores to the responses from specific actors. Auditors were also required to include specific statements about the activities of key actors in their narrative reports, and these narrative reports were re-scored by an independent reviewer.



(a) Citizen Response Scores (0-10)



(b) Government Response Scores (0-10)

Auditors first evaluated whether citizens of the targeted community. Auditors were asked "on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the overall response by the citizens in this village/street to the X problem?" Auditors were trained to account for a broad range of citizen activities to resolve service delivery problems, including financial contributions, physical labor, and lobbying of government officials. The modal citizen responsiveness scores were 0 and 2.5 (17% of communities each), and the median score was 2.5. Citizens receiving scores between 1 and 3 held village meetings, lobbied government officials, or made limited financial or labor contributions, although they were often insufficient to resolve the problem entirely. For example, villagers held community meetings about the problem issue in Tabora region (score = 1), filled potholes with sand as a short-term solution to broken road infrastructure in Geita region (score = 2), and lobbied ministry officials to request additional funds to resolve a water crisis in Tanga region (score = 2.5). Citizens earned scores between 4 and 7 if they made significant financial and labor contributions or enacted dramatic accountability measures, such as recalling an ineffective village leader. Citizens did not receive a score above a 7 in any community.

Auditors also scored the responses of the Tanzanian government. In a similar vein to citizen scores, auditors were asked "on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the overall response by the government to the X problem?" As Figure 6b makes clear, the government was more likely to take no or limited action than citizens: auditors scored government responsiveness a 1 or less in 42% of control communities. On the other hand, governments were also much more likely to significantly mobilize resources to fully address the service delivery problem, scoring 7 or higher in 10% of the control sample. After scoring the actions of the Tanzanian government as a whole, auditors scored the actions of specific sectors of government, including local elected officials, local bureaucrats, district-level ministry officials, and the Member of Parliament. As I discuss in more detail in the results section, government responsiveness to service delivery issues varies significantly from community to community and problem to problem.

5.4 Estimation

The estimand of primary interest is the intent-to-treat effect (ITT). Treatment "uptake" (communities where a journalist visited, produced, and broadcast a report) was 98.1%. As previously discussed, journalists were unable to complete reports in three communities at the request of government officials. These communities are still considered partially treated because reporters spoke with citizens and government officials, even though reports were never broadcast. Enumerators collected endline data in the three communities where *WNS* reports were forbidden.

The units of analysis are either community leader respondents or direct observation measures taken at the community level. I estimated the ITT using OLS regression. The regression model

$$y_i = \beta d_i + \gamma_1 pair_i + u_i$$

expresses the outcome y_i as a linear function of the randomly assigned treatment, binary variables for each of the community pairs (blocks), and an unobserved error term u_i. β represents the Intent-To-Treat (ITT) effect. The regression estimator is identical to the difference-in-means estimator, since the block variables are orthogonal to treatment assignment. I calculate p-values using randomization inference (RI) (2,000 permutations).

In keeping with the pre-analysis plan, the analysis of substantive outcomes also reports covariate-adjusted regression results. The LASSO procedure selects prognostic covariates from a set of administrative variables listed in Table 1. The number of selected covariates ranges from to zero to ten depending on the outcome, but due to the similarity across experimental groups at baseline, the estimates after adjustment closely resemble estimates without adjustment across all analyses.

6 Results

6.1 Compliance with assigned treatment

Was *Wakati Ni Sasa* implemented as planned? Table 2 shows the effect of treatment assignment on three first-stage outcomes: whether *WNS* reports were produced and broadcast, whether the reports were noticed and remembered by community government officials, and whether the reports influenced officials' beliefs about the likelihood of future media investigations.

Column 1 of Table 2 shows that *Wakati Ni Sasa* journalists produced and broadcast local news reports in 100 of 103 communities (98.1%) assigned to the treatment condition.[8] As I discuss in the previous section, the government forbade journalists from pursuing investigations in three communities. Endline data collection was conducted in all three communities.

[8] An assigned research team member verified that each report was broadcast at least twice in the agreed-upon week.

Did local leaders in targeted communities notice and remember *Wakati Ni Sasa* investigations? Enumerators asked three leaders in each community "how many times has a journalist visited this village/street in the past seven months to address [targeted problem]" and "have you heard a radio report about [targeted problem] in your village/street in the last seven months?" I coded the outcome as 1 if at least one official in the community remembered a journalist visit or heard a radio broadcast. While control communities were by no means saturated by local media reporting in the absence of *WNS*, local news reports were also not uncommon: at least one local leader recalled a journalist visit in 29% of control communities and reported hearing a radio report in 25% of control communities. Even so, *WNS* dramatically increased perceived press coverage in treated communities. On average, local officials in treated communities were 34 percentage points more likely to remember a journalist visiting their community and 28 percentage points more likely to remember hearing a news report about their community (one-tailed randomization inference p-value < 0.001). Overall, local officials recalled either a journalist visit or hearing a broadcast report in 75% of treated communities compared to 38% of control communities (one-tailed randomization inference p-value < 0.001).

I also observe evidence that *WNS* investigations influenced local leaders' beliefs about the likelihood of *future* media coverage. Enumerators asked village/street chairpersons, "how likely do you think it is that a journalist will visit your village/street if it has a [targeted problem] problem in the future?" 63% of leaders in the treatment group said a report was "very likely" compared to 46% in the control group. Taken together, Table 2 suggests that local media reports in the absence of *WNS* were neither common nor unheard of, and *WNS* investigations left a lasting impression that local news reports would become more likely in the future. In other words, *WNS* reports increased the intensity of familiar styles of journalism rather than introducing a new accountability mechanism.

Table 2: **Effect of Treatment Assignment on Treatment Uptake**

	Reports	*Local Official Awareness*			*Local Official Expectations*
	Report Broadcast	Aware of WNS Index	Journalist Visit	Heard Report	Future Report Likely
	(1)	(2)	(3)	(4)	(5)
WNS Treatment	0.971	0.374	0.343	0.283	0.214
Standard Error	0.017	0.059	0.065	0.062	0.093
RI p-value	<0.001	<0.001	<0.001	<0.001	0.011
Hypothesis	+	+	+	+	+
Control Mean	0.00	0.38	0.29	0.25	1.31
Control Village SD	0.00	0.49	0.46	0.43	0.73
DV Range	0-1	0-1	0-1	0-1	0-2
Matched-pair controls	Yes	Yes	Yes	Yes	Yes
Lasso-selected controls	No	No	No	No	No
Adj-R^2	0.94	0.32	0.17	0.22	0.10
Observations	206	202	202	202	206

Note: The dependent variable in Column 1 comes from monitoring data on whether a *Wakati Ni Sasa* report was completed and broadcast on the radio, and takes the value 1 if the report was broadcast at least twice and 0 otherwise. Column 2 reports an index of local official awareness of *Wakati Ni Sasa* reports which is the simple arithmetic mean of Columns 3 and 4. The dependent variable in Column 3 takes the value 1 if any community government official in the community answers "at least once" to the question "*How many times has a journalist visited this community or street to address the X problem in the past seven months?*" The dependent variable in Column 4 takes the value 1 if any local official in the community answers "yes" to the question "*Have you heard a radio report about an X problem in your community in the past seven months?*" The dependent variable in Column 5 is the community chairperson's response to the question "*How likely do you think it is that a journalist will visit your village/street if it has a [targeted problem] problem in the future?*" Responses took the value 2 if the chairperson responded "very likely" and ' if the chairperson responded "somewhat likely".

6.2 Impact on public goods and services

I begin the analysis of results by considering the outcome of primary interest: change in the quality of public goods and services. In the *absence* of press reports, some targeted problems were addressed over the 7 month project period, but average improvements were small: the mean audit score in control communities was just 2.03 on a -10 to 10 point scale (standard deviation = 3.05), and auditors reported that the problem remained unchanged (37%) or had grown worse (8%) in nearly half of control communities.

How did *Wakati Ni Sasa* reports affect the quality of public goods and services? Table 3 shows that communities targeted by *WNS* received audit scores that were 0.68 points higher than control communities on average, which amounts to more than one-fourth of a control group standard deviation. The one-tailed randomization inference p-value for the result is 0.03. Recall that a one standard deviation audit score improvement above the control group mean means the difference between a minimal response to the problem and a tangible short-term fix (e.g. gravel filling in an impassable road) or the initiation (but not completion) of a longer-term solution (e.g. breaking ground on a new clinic).

Another way to conceptualize the treatment effect is to ask how often treatment communities received higher audit scores than their matched-pair counterpart in the control group. Treatment communities scored higher than their control community counterparts in 50% of all matched pairs, tied in 13%, and scored lower in 31% (one-tailed randomization inference p-value = 0.03). The final pre-specified operationalization of the primary outcome is a simple binary indicator of whether the auditor recorded *any* improvement in the targeted issue area. Treated communities were about 7 percentage points more likely to see some improvement, although this result is only significant

at the 0.1 level after adjusting for LASSO-selected covariates.

Bear in mind that these estimates reflect the intent-to-treat effect of a community being targeted by a *WNS* report, and that the research team did not prevent radio stations from reporting on control communities for ethical reasons. Given that 38% of communities received press coverage of the targeted area during the project period, the effect of *WNS* reports relative to a pure control may have been considerably larger.

Table 3: **Primary Outcome** - Report Card Score

	Overall Score		*Matched-Pair Winner*		*Any Improvement*	
	(1)	(2)	(3)	(4)	(5)	(6)
WNS Treatment	0.684	0.684	0.155	0.155	0.068	0.091
Standard Error	0.361	0.361	0.086	0.086	0.067	0.063
RI p-value	0.033	0.033	0.028	0.028	0.112	0.077
Hypothesis	+	+	+	+	+	+
Control Mean	2.33	2.33	0.31	0.31	0.54	0.54
Control SD	2.77	2.77	0.47	0.47	0.50	0.50
DV Range	-10-10	-10-10	0-1	0-1	0-1	0-1
Blocked FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	0	No	0	No	2
Adj-R^2	0.14	0.14	-0.59	-0.59	0.07	0.14
Observations	206	206	206	206	206	206

Note: Columns 1 and 2 come from an independent auditor's response to the question: "First, on a score of -10 (much worse) to 10 (fully solved), how would you say the [pre-specified problem name] problem has changed from seven months ago to today?" Columns 3 and 4 report results from the same question, but where responses are coded 1 if the community had a higher report card score than its matched-pair partner, 0 if the community had the same report card score as its matched-pair partner, and -1 if the community had a lower score than its matched-pair partner. Columns 5 and 6 report results from the same question, but where responses are coded 1 if there was any improvement and 0 otherwise.

6.3 Robustness and Threats to Validity

The findings are robust to a variety of alternative outcome measures and model specifications. Table A1-Table A3 demonstrate that the treatment effects are similar whether I consider the score given by the original auditor or the score given by the follow-up auditor who only had access to evidence collected by the original auditor. The primary outcome measure is somewhat larger when using the first audit score alone. I also show that the results are robust to a variety of alternative specifications, including the exclusion of matched-pair fixed effects and the inclusion of a political, demographic, and economic covariates not chosen by the LASSO-selection procedure specified in the pre-analysis plan.

I also consider two threats to validity introduced by the possibility of interference between experimental units. First, *Wakati Ni Sasa* reports about treatment communities may have improved government responsiveness in nearby control communities. For example, *WNS* reports may have catalyzed citizens in control communities who heard the reports to demand service improvements in their own areas. Similarly, government officials in control communities

may have heard *WNS* reports, interpreted them as a sign that reports in control communities were more likely in the future, and improved their performance in anticipation of that eventuality.

Three observations help alleviate this concern. First, it is unlikely that the effect of *WNS* reports on the perceptions of citizens or government officials in untreated communities was dramatic. Table 2 shows that more than 30% of control communities were the target of local news reports even in the absence of *WNS*, and *WNS* reports were spread out between districts and over time so that no district would be subject noticeable surge in press attention. Second, I observe no evidence of spillover effects of *WNS* reports on government or citizen actions on other issue areas in treatment communities (see Table A11), so it seems unlikely that the reports generated spillovers in different communities. Finally and most importantly, this form of interference would bias the estimated effect of *WNS* reports *downward* by improving public service scores in control communities. To the degree that *WNS* generated expectations about future media coverate in untreated communities, this paper understates the causal effect of journalism.

The second threat to validity from interference between experimental units is government actions in treatment communities trading off with government actions in control communities. Again, it seems unlikely that such a trade-off would be dramatic, given that reports were spread about across districts and across time and that districts are responsible for service delivery in 40 or more communities. Moreover, district governments notoriously fail to spend down their allocated budgets, and it strains credulity to assume that ministries and district governments operate at full capacity (Carlitz 2017). Nonetheless, it is a weakness of the design that it cannot definitively determine whether *WNS* reports increased government effort and resource distribution in the aggregate or only influenced how a fixed level effort and resources were distributed between communities. Considering the extreme case of a 1:1 trade-off between support for treated communities and support for control communities, the estimated treatment effect would be at most twice the size of the true effect. Estimating the precise nature of trade-offs between communities represents an important avenue for future research.

7 Mechanisms

How do local media investigations influence service delivery? Based on 12 months shadowing local journalists, I outlined six plausible channels of media influence on public service delivery (see Appendix): mobilizing citizens, attracting support from non-governmental organizations, helping citizens oversee local bureaucrats, helping citizens hold elected officials accountable, helping politicians hold bureaucrats accountable, and helping the national government monitor and sanction its own bureaucracy. In qualitative interviews, most journalists foregrounded the first two channels; they described their role primarily as catalyzing citizen action. As I describe below, more experienced journalists tend to highlight their role activating accountability relationships within the ruling party apparatus.

However, adjudicating between these competing views of media influence is challenging because journalists cannot observe community outcomes in the counterfactual world where they do not conduct an investigation.

Which channel of influence predominates? A designed-based approach to investigating mechanisms is ideal but beyond the scope of this paper. However, in this section I provide suggestive evidence from qualitative interviews and independent audit scores that local news investigations activate top-down channels of accountability *within* the ruling party bureaucracy. National and regional ministries are motivated to promote social welfare and avoid long-term threats to ruling party dominance (Lorentzen 2014; Magaloni and Kricheli 2010). Local news reports increase the likelihood that the grievances of local communities will reach district and regional commissioners, who control promotions within the regional party apparatus. Journalists help commissioners monitor and sanction district and ministry bureaucrats. These bureaucrats are keenly aware that public embarrassments may result in their demotion, firing, or transition to another region.

7.1 Qualitative evidence

A a district-level official in Tanga voiced this paper's argument clearly. Asked about the potential outcome of a negative report about their issue area, the official said "I could be fired, or sometimes moved from one place to another. I could be moved from Tanga to Mbeya (a regional with a much colder climate)." A journalist in southern Tanzania explained that effective journalists know and exploit this principal-agent dynamic between district-level ministry officials and commissioners during the reporting process. He said,

> *I don't tell them I am going to share the findings from the [reporting] with the minister, but they know that with journalists will go there and publish the story...they know that if that the story is published, everyone will see it, even leaders or ministers or the regional commissioner....Sometimes you have to publish even on Facebook or Instagram where [regional leaders] will see it.*

If media investigations influence district and ministry officials, why don't they influence community politicians and members of parliament? The most straightforward reason why local politicians do not respond to local media investigations is that they are well aware of service delivery problems in their area, and those who have not already taken steps to solve the problem have few extra resources to do so. More than 80% of local elected officials told enumerators that they would *welcome* negative media reports about their communities, and several journalists told stories about local leaders lobbying and in some cases paying transportation costs to journalists to attract attention to service failures in their area. Why would local elected officials court negative press coverage about embarrassing governance failures in their own villages? One village chairperson voiced a common explanation: "if we don't get on the radio [about our water point problem], RUWASA (the rural water ministry) might never show up to help."

Members of parliament, on the other hand, appear to be neither interested nor capable of responding to service delivery problems outside of election years. In two independent qualitative interviews, citizen's referred to their MP

as *muwa*, or sugarcane, because "their promises during election season taste so sweet, but their support disappears so quickly." Several journalists said that MPs are notoriously difficult to reach except during campaign season, because MP's spend most of their time in Dodoma and Dar es Salaam and assiduously avoid engaging in controversial issues in their home districts. It remains possible that media investigations influenced ministry outcomes by activating "fire-alarm" accountability: helping MPs monitor and sanction ministry officials to improve bureaucratic performance. However, evidence for "fire-alarm" accountability is limited. According to local leader surveys, MPs were no more likely to visit or contact local government officials in treated communities compared to control communities.

7.2 Audit scores

These qualitative insights are supported by evidence from audit scores of citizens, non-governmental organizations, and specific government actors. Auditors were asked to score the responses by these actors on a scale of -10 (made the targeted problem much worse) to 10 (solved the targeted problem entirely).

In the control sample, citizen self-help (average audit score 2.9, score of 0 or less in 16% of communities) was more common than government support (average audit score 2.7, score of 0 or less in 25% of communities). In Mbangala village in the southeastern region of Lindi, for example, citizens contributed funding and labor to build the foundation for a new clinic. In Weruweru village at the base of Mount Kilimanjaro, villagers raised 32 million Tanzanian shillings (13,000 USD) to build the first four classrooms of a new school. In many control communities where government officials contributed funding and supplies, they did so only *after* citizens initiated service delivery improvements. Mbangala's Member of Parliament Mariam Kasembe, for example, contributed 100 bags of cement for additional bricks after villagers completed construction of the clinic's foundation.

Non-governmental organizations, on the other hand, were less likely to respond to service delivery problems than either citizens or the Tanzanian government. Nonetheless, non-governmental organizations provided crucial support to some control communities, especially around water issues. For example, the Islamic Foundation, a Saudi Arabian-funded community development organization based central Tanzania, sponsored the construction three water points in Dakawa village. Elsewhere in central Tanzania, the Israeli-based non-governmental organization Innovation: Africa provided technical support for construction of a piped water system connecting a primary village to two sub-villages.

How did *Wakati Ni Sasa* reports influence the relative contributions of government, citizens, and non-governmental organizations? Table 4 shows that the *WNS* increased average government response by about 0.5 points (one-tailed RI p-value = 0.03) while *decreasing* average citizen responses by 0.36 points, although the latter result falls short of conventional statistical significance (two-tailed RI p-value = 0.180). *WNS* reports also did not appear to attract support from civil society: the effect of *WNS* reports on non-governmental actions was small and statistically in-

significant (coefficient = 0.11, one-tailed RI p-value = 0.290).

Table 4: **Government, Citizen, and NGO Response** - Report Card Score

	Government Score		*Citizen Score*		*NGO Score*	
	(1)	(2)	(3)	(4)	(5)	(6)
WNS Treatment	0.490	0.622	-0.325	-0.362	0.107	0.160
Standard Error	0.343	0.358	0.275	0.276	0.331	0.346
RI p-value	0.076	0.047	0.210	0.180	0.290	0.260
Hypothesis	+	+	two-tailed	two-tailed	+	+
Control Mean	2.54	2.54	2.83	2.83	1.63	1.63
Control SD	2.72	2.72	2.01	2.01	2.54	2.54
DV Range	-10-10	-10-10	-10-10	-10-10	-10-10	-10-10
Blocked FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	7	No	2	No	7
Adj-R^2	0.18	0.22	0.10	0.14	0.18	0.15
Observations	206	206	206	206	206	206

Note: Columns 1 and 2 come from an independent auditor's response to the question: ""First, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the overall response by the government to the X problem?" Columns 3 and 4 come from an independent auditor's response to the same question, except referring to citizens of the targeted community. Columns 5 and 6 come from an independent auditor's response to the same question, except referring to non-governmental organizations.

Turning to actors within the Tanzanian government, Table 5 shows report card scores for responses by community politicians, community bureaucrats, Members of Parliament, and ministries. There is no indication that *Wakati Ni Sasa* reports increased the average response of community politicians like village/street chairpersons (*mwinyikiti*) and ward councilors (*diwani*). This is not because community politicians lacked the resources or capacity to respond; local elected officials received the highest audit scores in the control group (average score 4.49). Their responses usually took the form of raising and distributing village funds, but also included rallying citizens to contribute labor to certain projects and lobbying higher levels of government for additional aid. However, local elected officials were almost entirely unaffected by *WNS*'s investigations on average (coefficient = 0.055, one-tailed RI p-value = 0.51).

Table 5: **Government Response Outcomes** - Report Card Score

	Local Elected		*Local Bureaucracy*		*MP*		*Ministry*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WNS Treatment	-0.005	-0.005	-0.019	-0.019	-0.320	-0.320	0.573	0.573
Standard Error	0.254	0.254	0.249	0.249	0.312	0.312	0.346	0.346
RI p-value	0.506	0.506	0.500	0.500	0.828	0.828	0.048	0.048
Hypothesis	+	+	+	+	+	+	+	+
Control Mean	4.47	4.47	4.22	4.22	2.13	2.13	2.46	2.46
Control Village SD	1.97	1.97	1.89	1.89	2.60	2.60	2.81	2.81
DV Range	0-8	0-8	0-8	0-8	-5-9	-5-9	-1-10	-1-10
Matched-pair controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lasso-selected controls	No	0	No	0	No	0	No	0
Adj-R^2	0.02	0.02	0.10	0.10	0.12	0.12	0.26	0.26
Observations	206	206	206	206	206	206	206	206

Note: These questions about specific government agencies are responses to the question: "Now we will ask you to give a score to different individuals and organizations in government. For each one, we are asking you to score only that person or organization's contribution to solving the X problem" Columns 1 and 2: "How do you score the Mwenyekiti's response?" and "How would you score the Diwani's response?"; Columns 3 and 4: "How would you score the Village Executive Officer or Ward Executive Officer response?"; Columns 5 and 6: "How would you score the response by the Member of Parliament?" ; Columns 7 and 8: "How would you score the response by the Ministry (examples: TARURA, TANESCO, RUWASA)?"

What about local bureaucrats? Village/street and Ward Executive Officers are charged with managing the implementation of local projects and overseeing the disbursement of village and district funds. According to the social accountability model of media influence, press coverage casts light on incompetence and malfeasance in local bureaucracies, helping citizens motivate service delivery providers to perform more effectively. Columns 3 and 4 offer sparse support for this story. Local bureaucrats are already among the most responsive actors in the absence of *WNS* reports (average score 4.25), but their activities were not discernibly affected by increased press attention (coefficient = -0.01, one-tailed RI p-value = 0.6).

WNS investigations also do not appear to influence Members of Parliament. There are some instances of Members of Parliament visiting sample communities and gifting bags of cement for clinic construction, new school supplies, or a diesel engine to power the local borehole. Nonetheless, according to report card scores MP's are are the least responsive actors in Tanzanian government: columns 5 and 6 show that MP's received the lowest average responsiveness score among all sectors of government (average score 2.12), and their efforts were slightly *lower* in communities that received *WNS* investigations (coefficient = -0.27, one-tailed RI p-value = 0.82).

Rather than influencing local elected officials, Members of Parliament, or local bureaucrats, it appears *Wakati Ni Sasa* investigations primarily triggered action by officials from the Tanzanian Rural and Urban Roads Agency (TARURA), the Ministry of Rural Water Supply (RUWASA), and the health and education ministries. In control communities, these ministry officials were nearly as unresponsive as MPs (average score 2.45). However, *WNS*

reports increased average response scores by 0.67 points (RI p-value = 0.02), approximately one fourth of a control group standard deviation. Figure A2 shows that overall treatment effects were strongest across the three issue areas that fall most directly under the purview of ministry responsibilities: roads, water, and wildlife/environment. I observe no treatment effects on issue areas under the purview of local governments, health and education.

8 Discussion

An independent press is considered a bedrock of accountable governance in industrialized democracies, but its role in developing countries, especially those ruled by hegemonic party regimes, is less certain. This study examines the impact of local media investigations on the delivery of public goods and services in the "hard case" of Tanzania, where the reach of local media, the delivery of public services, and the extent of political competition is limited.

How can local media influence the provision of public goods and services under these circumstances? Drawing on more than 12 months of field work with radio stations and journalists, I developed a template for local media investigations modelled on the best practices of local reporters. We tested the effect of high-quality local media investigations through a national-scale field experiment conducted in partnership with 15 regional radio stations.

The primary finding is that local media investigations generated modest but meaningful improvements in the delivery of public goods and services seven months after they were are aired. However, I observe no evidence that *WNS*'s influence was attributable to increased citizen self-help, NGO support, citizen mobilization of local service providers, or oversight of the bureaucracy by members of parliament. Instead, in keeping with theories of top-down intra-party accountability, only ministry officials appear to consistently respond to negative press reports. Qualitative evidence suggests that district level ministry officials are motivated to respond to the prospect of negative news coverage to prevent bad news from reaching their superiors and threatening their upward trajectory in the ruling regime. Taken together, the results suggest a novel channel of media influence in hegemonic party regimes: helping ruling parties monitor and sanction their own bureaucratic apparatus.

These findings have important implications for scholarship on political accountability and distribution in hegemonic party regimes. First, they represent the first experimental evidence that journalism improves government responsiveness outside of industrialized democracies. Evidence for the efficacy of information and social accountability campaigns outside of developed democracies is decidedly mixed, especially with respect to tangible outcomes like service delivery (Grossman and Slough 2022). The results raise the intriguing possibility that media investigations, conducted by journalists with long-standing ties to the communities where they operate, activate channels of accountability that information and transparency campaigns do not.

Second, the findings highlight the importance of accountability relationships within ruling party bureaucracies. Extant scholarship on government responsiveness emphasizes the role of principal-agent relationships between

citizens and politicians, politicians and bureaucrats, and citizens and bureaucrats (Grossman and Slough 2022). The findings suggest that in hegemonic party regimes, accountability relationships within the executive bureaucracy, and within the ruling party apparatus more broadly, may be especially important for understanding barriers to government responsiveness.

Finally, paper offers important lessons for policy. Funding local media investigations is inexpensive: each *Wakati Ni Sasa* report cost less than 100 USD to conduct. Crucially, *WNS* reports engendered responses from government institutions rather than citizens or non-governmental organizations. As democratic institutions come under increasing strain around the world, identifying ways to support, strengthen, and sustain local news reporting offers a promising avenue for ensuring that governments remain responsive to the basic needs of their citizens.

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Supplemental Materials for

Can Journalism Promote Government Responsiveness?

July 12, 2023

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Appendix

A Six Theories of Media Influence

How might press coverage influence the quality of essential public goods and services like water, education, health care, and transportation? To identify channels of media influence, I spent more than 12 months in Tanzania prior to the launch of *Wakati Ni Sasa* working with local radio stations and shadowing investigative journalists. I also conducted in-depth interviews with more than 50 journalists, station managers, local politicians, and ministry officials across Tanzania. This prelimenary qualitative field work helped identify six potential channels of media influence government responsiveness, which I then pre-registered as part of the *Wakati Ni Sasa* pre-analysis plan.[1] I discuss each in turn below.

A.1 Citizen Self-Help

The most direct channel of media influence on public goods provision is that coverage motivates citizens to engage in collective self-help. This channel of resonates with many "old-guard" media members in Tanzania because it reflects Julius Nyerere's philosophy that mass media's primary role is to educate and motivate citizens towards their own economic development (Mwaffisi, interview). Journalists shared three strategies for promoting citizen self-help: informing citizens about underappreciated problems in their area, persuading citizens that certain problems merit prioritization, and directly coordinating or advising appropriate citizen responses. A fourth strategy, generating "common knowledge" about a priority that is held privately by many citizens, is common in theoretical literature but was not emphasized by journalists (Barberà and Jackson 2019; Chwe 2013).

During a village visit to a peri-urban village suffering from water shortages in Morogoro Region, for example, independent journalist Mohammed Rajab spoke as much like a community mobilizer as a reporter, interspersing standard journalistic questions with exhortations to invest in private water pipelines. "We don't know when the government will come, my friends," Rajab reminded the crowd of 15-20 citizens who had gathered around the household where he was interviewing a key informant, "You need to work together!" The news item he later submitted for broadcast on Planet FM in Morogoro included reminders that government support for new water pipes would only be possible once citizens started digging trenches on their own.

Rajab's focus on local collective self-help is consonant with Elinor Ostrom's classic work on the co-production of public goods and services in settings with weak institutions (Ostrom 1996). While cooperative self-help is susceptible to classic coordination and collective action problems, Ostrom and others have documented a wide range of strategies that communities and local employ to overcome these challenges. Might local media represent one such strategy? Evaluations of *external* interventions to promote local self-help initiatives, often termed "community-

[1] See pre-analysis plan.

driven development", have yielded unimpressive results (Casey et al. 2012; Humphreys et al. 2019). Observational data on the influence of *media* on collective action is also mixed (Campante et al. 2022).[2] However, these studies focus on the effects of a community gaining *access* to new forms of media, rather than the effects of a community *being the subject* of press coverage. Local media coverage of type practiced by Mohammed Rajab in Morogoro has yet to be rigorously tested.

A.2 Non-Government Organizations

A second channel of media influence emphasized by both journalists and station managers is that media reports attract support from non-governmental organizations and philanthropists. This is an especially popular channel among media professionals in larger cities like Dodoma, Mwanza, and Arusha. In Tanzania's capital city of Dodoma, for example, a team of two journalists listed, off the top of their heads, more than 10 water-rights NGOs that they turn to when Tanzania's Ministry of Rural Water and Sanitation (RUWASA) is unresponsive. To attract support from civil society, radio journalists seek out dramatic personal-interest stories and convert them into social media posts (primarily Facebook and Instagram) that are most likely to be observed by NGO officials in urban centers. They also share this content directly with village chairpersons, ward councilors, and Members of Parliament, who use them as leverage to advocate for NGO support.

A robust literature examines the influence international and non-governmental development organizations in low-income countries, including the potential that public goods provision by these actors may undermine government accountability and long-term state capacity (Green 2014). If local media attracts support from NGOs but not governments, classic arguments for media's role in promoting government accountability would be undermined. To my knowledge, however, few studies have evaluated the influence of media reports on when and where NGOs provide development assistance.

A.3 Social Accountability (Citizen Oversight of Local Bureaucrats)

Media may also promote public goods provision by helping citizens monitor and sanction ineffective local service providers - contractors, doctors, teachers, and local bureaucrats. Many journalists have a favorite story of helping citizens bring a malfeasant or incompetent local bureaucrat to account. In theory, this "short route" to accountability offers a direct, cost-effective way for citizens to influence the design and implementation of public services (Kosack and Fung 2014). There is substantial observational evidence that communities which engage in local monitoring receive more and better services from the state (Tsai 2007). There is also some promising evidence that external interventions to support citizen monitoring of service providers generated substantial improvements in service quality (Björkman and Svensson 2009). By and large, however, interventions to support social accountability have not pro-

[2]Existing studies emphasize the distinction between traditional news media, which tends to facilitate collective action, and entertainment media, which tends to reduce social capital and political engagement.

duced anticipated benefits (Lieberman and Zhou 22ed; Lieberman et al. 2014; Olken 2007) and have been susceptible to capture by local elites (Banerjee et al. 2010), while rigorous attempts to replicate the most promising models of social accountability have not been successful (Raffler et al. 2020). In Tanzania, an exceptionally careful evaluation of a social accountability intervention found that it produced no discernible effect service delivery or citizen satisfaction (Arkedis et al. 2021). The most optimistic interpretation of the existing base of evidence is that social accountability depends on external support for social accountability works only when paired with clear, direct, and actionable mechanisms for sanctioning under-performing bureaucrats (Fox 2015).

Media offers a promising tool for overcoming some of primary obstacles to effective social accountability. Like many of the interventions just described, news investigations may resolve information asymmetries between citizens and service providers and help citizens coordinate campaigns focused on specific service delivery issues and responsible actors. Journalists may also stimulate effective social accountability by providing clear templates for political action and acting as brokers by citizens and service delivery providers Kruks-Wisner (2022). There is some promising non-experimental evidence suggesting that local media improves service delivery through a social accountability channel Reinikka and Svensson (2005). However, therea are no experimental evaluations of independent media's contributions to social accountability.

A.4 Electoral Accountability (Citizen Oversight of Politicians)

Even when citizens do not directly monitor and sanction service delivery providers, they can still contribute to government responsiveness through their decisions at the ballot box (the "long route" to accountability (Kosack and Fung 2014). A journalist from Mbeya region said, "reports on those [service delivery] issues, can make [members of parliament] be scared they will not be selected if they are not responding." At the same time, many journalists emphasized that the influence of media was limited outside of election season. In the same interview, the Mbeyan journalist later explained, "…but you know, the MP has five years so maybe they are just making plans when they do not accomplish the assignment." When community politicians and members of parliament do make commitments to support local development projects, many journalists derisively refer to their statements as "*ahadi hewa*" (promises of the air).

In classic theories of democratic accountability, elections help citizens both *select* effective representatives and *motivate* their representatives once in office. For these systems of electoral accountability to be effective, voters need accurate and credible signals of politician's performance (Barro 1973; Fearon 1999). A robust literature finds that voters update their voting preferences in response to new information about about candidate malfeasance (Ferraz and Finan 2011) and overall performance (Bhandari et al. 2021; Bowles 2020), although evidence is mixed (Dunning et al. 2019; Incerti 2020). A smaller body of evidence suggests that incumbent politicians increase their effort when

they know that voters are better informed about their performance (Bobonis et al. 2016; Grossman and Michelitch 2018; Grossman et al. 2020).

A functioning media environment has an obvious role to play in promoting electoral accountability in democratic countries. Both observational and experimental findings suggest that the threat of media reports motivate improved government responsiveness. However, media's favorable influence becomes less clear outside of industrialized democracies, where limited media reach (Larreguy et al. 2020), and more fragile electoral systems may undermine media's contribution to electoral accountability. Studies of transparency campaigns on politician behavior outside of industrialized democracies have met limited success (Humphreys and Weinstein 2012; Malesky et al. 2012).

A.5 Horizontal Accountability (Political Oversight of Bureaucracy)

A fifth potential channel of media influence on government responsiveness is helping elected officials to monitor and sanction service delivery providers. Politicians can improve the performance of service delivery providers by monitoring and sanctioning poor performance or by selecting better bureaucrats (McCubbins and Schwartz 1984; Raffler 2019; Slough 2020). Both strategies require politicians to be informed about the performance of service delivery providers, which depends on politicians having access to information about service delivery outputs and/or citizen preferences and grievances. In every investigation that I observed, journalists reached out to the relevant Diwani (Ward Councilor) and Mbunge (Member of Parliament) to share their findings and encourage the official to take action.

A growing body of empirical evidence evaluates programs that attempt to improve politicians' oversight of the bureaucracy. Olken (2007) finds that top-down monitoring of bureaucrats reduces corruption and increases performance of public works projects in Indonesia. Raffler (2019) finds that a program to train politicians to monitor bureaucrats in Uganda increased oversight behaviors and some measures of service delivery, although only in areas where politicians were not aligned with the ruling party. Slough (2020) argues that formal complaint systems in Colombia allow citizens to instigate political oversight of front-line service delivery providers.

However, there are significant barriers to improving political oversight of bureaucracies. Grossman et al. (2018) and Buntaine et al. (2021a) observe significant obstacles to citizens communicating their grievances to elected officials in Uganda. (Buntaine et al. 2021b) find that an initiative to help citizens report their problems to politicians in Uganda failed to improve waste service delivery because the quality of the information was low quality and did not fit into existing decision-making processes. Buntaine and Daniels (2020) find that a similar initiative in Uganda to failed to improve government responsiveness because politicians deliberately avoided knowledge of the monitoring in order to evade accountability.

A.6 Within-Party Accountability

Finally, journalists report influencing government responsiveness by activating accountability relationships *within* the ruling party hierarchy. Tanzania's executive bureaucracy is sprawling, and includes ministry officials as well as regional, district, ward, and village-level executive officers. It is also deeply tied to, and often indistinguishable from, CCM party structures. Its internal promotion system is reminiscent of the cadre system in China (Chen et al. 2016; Fukuyama 2014; Pye 1986)

A distinguishing mark of a seasoned journalist in Tanzania is that they understand dynamics within the ruling party's local power structure. For example, the first thing Rodrik Makundi, a journalist in Kilimanjaro Region, does when he begins reporting on a story is to snap a picture or video and send it via WhatsApp to the relevant district executive officer and ministry official; this, he says, is even more crucial to his reporting process than the ultimate broadcast of the story. Makundi, like many Tanzanian journalists, talks about his job as a partnership with government officials. Sometimes, Makundi can get officials at TARUA (roads), TANESCO (electricity), or RUWASA (water) to respond to his stories with a video clip of the problem before the story is even broadcast. When these officials are unresponsive, he reaches out to Regional Commissioners and even Ministers. Makundi often plans his investigations to coincide with visits by major party officials to the region in the hopes of attracting their attention.

Rodrik Makundi's approach reflects a channel of media influence that is common in journalist's accounts of their own reporting process but sits uneasily with the academic literature on media and accountability in democracies. Rather than relying on the threat of electoral sanction, Makundi and other journalists form alliances with government officials, often sharing information about local governance failures and advice about how the government should respond. To elected officials, journalists provide a valuable mouthpiece for conveying their community's priorities to higher-level officials in the bureaucracy. To unelected ministry officials, journalists offer a valuable way to keep tabs on emerging challenges in their constituency. Ministry officials have an interest in CCM's continued success, but they do not hold elected office; their primary concern is signaling strong performance to regional and national superiors to continue their advancement within the ruling party. In a country where re-assignments of district and regional commissioners with regularity, they are especially keen to avoid embarrassment (Fatah, Interview).

The qualitative account of media's influence on intra-party accountability most closely resembles theories of media influence in autocratic regimes like China (Egorov et al. 2009; Lorentzen 2014). In these accounts, media facilitates top-down accountability by providing credible information to ruling elites about citizen grievances and the performance of local officials. Information from independent media may be especially useful when the state lacks the capacity to gather information itself or local officials misrepresent citizen grievances to their superiors (Chen

and Xu 2017). A number of recent studies suggest that civil society improves autocratic government responsiveness in China by providing bureaucrats with accurate information (Anderson et al. 2019; Buntaine et al. 2021b). However, the degree to which these findings hold in lower capacity governments like Tanzania remains very much in doubt.

B Supplemental Results

B.1 Comparison of In-Person and Follow-Up Audit Scores

Table A1: **Overall Improvement** - By Audit Scorer

	Combined Score		*In-Person Audit Score*		*Blind Audit Score*	
	(1)	(2)	(3)	(4)	(5)	(6)
WNS Treatment	0.684	0.684	0.311	0.311	0.068	0.091
Standard Error	0.361	0.361	0.172	0.172	0.067	0.063
RI p-value	0.031	0.031	0.028	0.028	0.112	0.077
Hypothesis	+	+	+	+	+	+
Control Mean	2.33	2.33	-0.16	-0.16	0.54	0.54
Control SD	2.77	2.77	0.87	0.87	0.50	0.50
DV Range	-10-10	-10-10	-10-10	-10-10	-10-10	-10-10
Blocked FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	0	No	0	No	2
Adj-R^2	0.14	0.14	-0.95	-0.95	0.07	0.14
Observations	206	206	206	206	206	206

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Columns 1 and 2 come from a simple average of the next two outcomes. Columns 3 and 4 come from an independent auditor's response to the question: "First, on a score of -10 (much worse) to 10 (fully solved), how would you say the [pre-specified problem name] problem has changed from 7 months ago to today?" Columns 5 and 6 come from an independent auditor's response to thes same question based solely on the documentation submitted by the original auditor.

Table A2: **Government Response**, by Audit Scorer

	Combined Score		*In-Person Audit Score*		*Blind Audit Score*	
	(1)	(2)	(3)	(4)	(5)	(6)
WNS Treatment	0.490	0.622	0.427	0.564	0.553	0.608
Standard Error	0.343	0.358	0.390	0.401	0.346	0.344
RI p-value	0.076	0.047	0.143	0.087	0.050	0.039
Hypothesis	+	+	+	+	+	+
Control Mean	2.54	2.54	2.34	2.34	2.74	2.74
Control SD	2.72	2.72	2.99	2.99	2.71	2.71
DV Range	-10-10	-10-10	-10-10	-10-10	-10-10	-10-10
Blocked FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	7	No	3	No	4
Adj-R^2	0.18	0.22	0.09	0.14	0.21	0.27
Observations	206	206	206	206	206	206

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Columns 1 and 2 come from a simple average of the next two outcomes. Columns 3 and 4 come from an independent auditor's response to the question: "First, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the overall response by the government to the X problem?" Columns 5 and 6 come from an independent auditor's response to thes same question based solely on the documentation submitted by the original auditor.

Table A3: **Citizen Response**, by Audit Scorer

	Combined Score		*In-Person Audit Score*		*Blind Audit Score*	
	(1)	(2)	(3)	(4)	(5)	(6)
WNS Treatment	-0.325	-0.362	-0.350	-0.313	-0.301	-0.354
Standard Error	0.275	0.276	0.332	0.368	0.282	0.293
RI p-value	0.210	0.180	0.310	0.360	0.890	0.890
Hypothesis	+	+	+	+	+	+
Control Mean	2.83	2.83	3.17	3.17	2.49	2.49
Control SD	2.01	2.01	2.41	2.41	2.07	2.07
DV Range	-10-10	-10-10	-10-10	-10-10	-10-10	-10-10
Blocked FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	2	No	8	No	5
Adj-R^2	0.10	0.14	0.12	0.16	0.08	0.09
Observations	206	206	206	206	206	206

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Columns 1 and 2 come from a simple average of the next two outcomes. Columns 3 and 4 come from an independent auditor's response to the question: "First, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the overall response by citizens to the X problem?" Columns 5 and 6 come from an independent auditor's response to thes same question based solely on the documentation submitted by the original auditor.

B.2 Primary Results Without Matched-Pair Controls

Table A4: **Primary Outcome**, No Matched-Pair Controls

	Overall Score		*Matched-Pair Winner*		*Any Improvement*	
	(1)	(2)	(3)	(4)	(5)	(6)
WNS Treatment	0.684	0.684	0.155	0.155	0.068	0.091
Standard Error	0.361	0.361	0.086	0.086	0.067	0.063
RI p-value	0.033	0.033	0.028	0.028	0.112	0.077
Hypothesis	+	+	+	+	+	+
Control Mean	2.33	2.33	0.31	0.31	0.54	0.54
Control SD	2.77	2.77	0.47	0.47	0.50	0.50
DV Range	-10-10	-10-10	-10-10	-10-10	-10-10	-10-10
Blocked FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	0	No	0	No	2
Adj-R^2	0.14	0.14	-0.59	-0.59	0.07	0.14
Observations	206	206	206	206	206	206

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Columns 1 and 2 come from an independent auditor's response to the question: "First, on a score of -10 (much worse) to 10 (fully solved), how would you say the [pre-specified problem name] problem has changed from 7 months ago to today?" Columns 3 and 4 come from an independent auditor's response to the question: "First, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the overall response by the government to the X problem?" Columns 5 and 6 come from an independent auditor's response to the question "Next, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the overall response by the citizens in this village/street to the X problem?"

C Heterogeneous Treatment Effects

C.1 Aggregate

Table A5 reports heterogeneous treatment effects of *WNS* investigations on overall service delivery by (1) electoral competitiveness, (2) geographic remoteness, and (3) district bureaucratic capacity. Counter to expectation, I observe no consistent evidence of heterogenous treatment effects. Prior versions of this paper observed treatment effect heterogeneity by electoral competitiveness, however I discovered a codeing error in vote counts that incorrectly magnified CCM's vote share in some regions.

Table A5: **Heterogeneous Treatment Effects**, by electoral competition, remoteness, and bureaucratic capacity

	Electoral competition		Remoteness		Bureaucratic capacity	
	Ward councilor	Parliament	District capital	Secondary road	Per capita budget	% taxes collected
	(1)	(2)	(3)	(4)	(5)	(6)
***WNS* Treat**	0.960	0.711	0.722	0.645	-3.576	-3.576
(S.E.)	(0.560)	(0.630)	(0.695)	(0.460)	(5.272)	(5.272)
***WNS* Treat * Covariate**	1.216	-0.630	-0.003	0.001	0.437	0.437
(S.E.)	(1.962)	(1.978)	(0.008)	(0.007)	(0.560)	(0.560)
RI p-value	0.590	0.790	0.734	0.926	0.320	0.320
Covariate	0.540	-1.543	-0.020	-0.002	6.487	-0.002
(S.E.)	(8.658)	(2.720)	(0.010)	(0.021)	(1.608)	(0.021)
Blocked FE	No	No	No	No	No	No
Adj-R^2	0.06	0.04	0.07	0.05	0.07	0.05
Observations	206	190	206	206	206	206

Note: The p-values for interaction terms are one-tailed tests in line with pre-analysis plan. Treatment is a binary indicator taking the value 1 if the village was assigned to the *Wakatni Ni Sasa* treatment. The covariate in column 1 is the margin of victory of the winning ward councilor in Tanzania's 2015 elections × − 1. The covariate in column 2 is the margin of victory of the winning member of parliament in Tanzania's 2015 election × − 1. The covariate in column 3 is the distance between the village and the district capital. The covariate in column 4 is the distance between the village and the nearest secondary road. The covariate in column 5 is the per capita budget allocated to the district in 2021. The covariate in column 6 is the percentage of taxes that a district was supposed to collect that was actually collected in 2021.

C.2 Electoral Competitiveness

Table A6: **Heterogeneous Treatment Effects**, by 2015 Opposition Party MP Vote Share

	(1) Overall Improvement	(2) Ministry	(3) Politicians
WNS Treat * Opp. Strength	3.158	-1.203	4.147
	(3.830)	(3.808)	(2.467)
RI p-value	(0.232)	(0.387)	(0.053)
WNS Treat	-0.615	0.919	-1.703
	(1.594)	(1.565)	(1.031)
Hypothesis	+	+	+
Opposition Strength	0.815	6.318	-8.103
	(17.174)	(12.701)	(4.795)
Blocked FE	No	No	No
Adj-R^2	0.05	0.23	0.13
Observations	204	204	204

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. THe p-values for interaction terms are one-tailed tests in line with pre-analysis plan. Opposition party strength is defined as the percentage of vote captured by opposition party MP candidates in the 2015 national election. Results are the same outcome measures reported in Table 3 and Table 5.

Table A7: **Heterogeneous Treatment Effects**, by 2015 Opposition Party Ward Vote Share

	(1) Overall Improvement	(2) Ministry	(3) Politicians
WNS Treat * Opp. Strength	-1.684	0.774	-0.463
	(3.773)	(4.247)	(2.079)
RI p-value	(0.334)	(0.411)	(0.439)
WNS Treat	1.466	0.157	0.030
	(1.566)	(1.784)	(0.819)
Hypothesis	+	+	+
Opposition Strength	-2.144	-2.742	-1.186
	(4.704)	(2.887)	(1.895)
Blocked FE	No	No	No
Adj-R^2	0.05	0.23	0.20
Observations	190	190	190

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. The p-values for interaction terms are one-tailed tests in line with pre-analysis plan. Opposition party strength is defined as the percentage of vote captured by opposition party Diwani (ward councilor) candidate in the 2014/2015 electoral cycle. Results are the same outcome measures reported in Table 3 and Table 5.

C.3 By Community Remoteness

Table A8: **Heterogeneous Treatment Effects**, by Distance from Secondary Road

	(1) Overall Improvement	(2) Ministry	(3) Politicians
WNS Treat * Distance	0.001	0.012	0.002
	(0.007)	(0.007)	(0.006)
RI p-value	(0.443)	(0.056)	(0.363)
WNS Treat	0.645	0.243	-0.101
	(0.460)	(0.433)	(0.300)
Distance	-0.002	0.019	-0.004
	(0.021)	(0.019)	(0.018)
Blocked FE	No	No	No
Adj-R^2	0.05	0.26	0.10
Observations	206	206	206

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Treatment is a binary indicator taking the value 1 if the village was assigned to the *Wakatni Ni Sasa* treatment. Distance from secondary road is measured in kilometers and calculated in R. Outcomes measures are identical to Table A5.

C.4 By LGA Efficacy

Table A9: **Heterogeneous Treatment Effects**, by LGA % Taxes Collected

	(1) Overall Improvement	(2) Ministry	(3) Politicians
WNS Treat * % Taxes	0.004	-0.010	-0.019
	(0.020)	(0.016)	(0.015)
RI p-value	(0.496)	(0.492)	(0.475)
WNS Treat	0.248	1.462	1.777
	(1.850)	(1.502)	(1.423)
% Taxes Collected	-0.222	-0.205	-0.046
	(0.057)	(0.013)	(0.061)
Blocked FE	No	No	No
Adj-R^2	0.06	0.23	0.13
Observations	204	204	204

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Treatment is a binary indicator taking the value 1 if the village was assigned to the *Wakatni Ni Sasa* treatment. LGA Efficacy is measured in by percentage of projected LGA tax collection actually collected in the according to the 2020-2021 Auditor's report. Outcomes measures are identical to Table A5.

Table A10: **Heterogeneous Treatment Effects**, by District Budget

	(1) Overall Improvement	(2) Ministry	(3) Politicians
WNS Treat * log budget	0.437	0.358	0.809
	(0.560)	(0.494)	(0.366)
RI p-value	(0.160)	(0.247)	(0.027)
WNS Treat	-3.576	-2.917	-7.736
	(5.272)	(4.735)	(3.511)
Log per capita budget	6.487	6.222	1.272
	(1.608)	(0.459)	(1.751)
Blocked FE	No	No	No
Adj-R^2	0.07	0.23	0.15
Observations	204	204	204

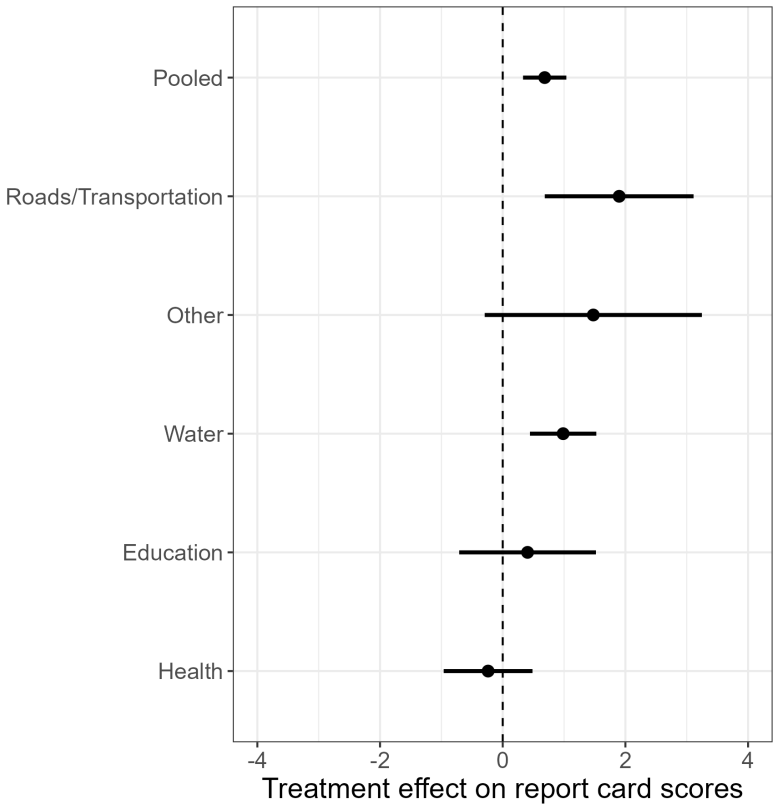
Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Treatment is a binary indicator taking the value 1 if the village was assigned to the *Wakatni Ni Sasa* treatment. District budget is the log of the total national budget allocation in 2020/2021 to the community's local government authority. Auditor's report. Outcomes measures are identical to Table A5.

D Conditional Average Treatment Effects

D.1 By Pre-Specified Problem Area

Figure A2 shows the standardized effect of the *WNS* treatment on overall report card score score by pre-specified topic area. Treatment effects are concentrated among communities with transportation, water, and other (primarily environment and crime) service delivery problems, and are weaker for communities experienced education and health issues.

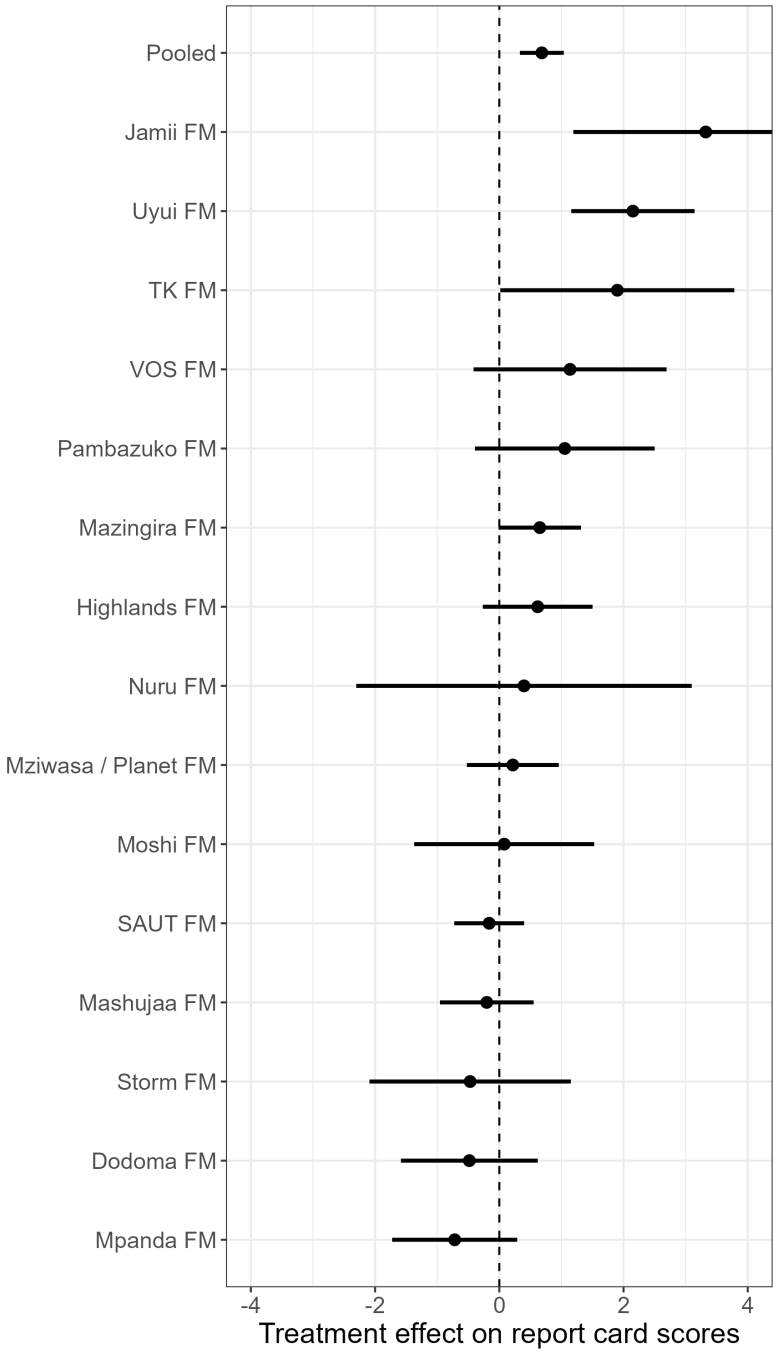
Figure A1: **Standardized Treatment Effect**, by problem type



D.2 By radio station

Figure A2 shows the standardized effect of the *WNS* treatment on overall report card score score by radio station. There is dramatic variation in average report card score.

Figure A2: **Standardized Treatment Effect**, by radio station



E Spillovers

To measure spillover effects, independent auditors also scored government and citizen actions on issue areas not targeted by *Wakati Ni Sasa Reports*. We find no evidence that *WNS* stimulated government or citizen actions on topics not covered by the investigative reports.

Table A11: **Government and Citizen Actions on Other Topics**

	Government		*Citizen Actions*	
	(1)	(2)	(3)	(4)
WNS Treatment	0.147	0.147	-0.146	-0.194
Standard Error	0.315	0.315	0.233	0.230
RI p-value	0.340	0.332	0.722	0.788
Hypothesis	+	+	+	+
Control Mean	4.27	4.27	0.75	0.75
Control SD	2.61	2.61	1.85	1.85
DV Range	-10-10	-10-10	-10-10	-10-10
Blocked FE	Yes	Yes	Yes	Yes
Controls	No	0	No	7
Adj-R^2	0.29	0.29	0.13	0.21
Observations	205	205	206	206

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Columns 1 and 2 come from an independent auditor's response to the question: "On a score of -10 to 10, how wuld you score GOVERNMENT's actions to address OTHER problems in their village (NOT XX problem) in the past 7 months?" Columns 5 and 6 come from an independent auditor's response to the same question but focused on citizen actions.

F Do Citizens Care About Negative News?

I conducted a survey experiment to test a core assumption underlying the standard models of media influence. According to the standard account, media broadcasts are embarrassing to the ruling party because they spread news about governance failures beyond the borders of a particular affected community (Besley and Burgess 2001). To avoid these embarrassments, ministry officials and politicians might be especially motivated to respond to villages where they expect media reports to occur. But it is not obvious that citizens update their views of the national government and ruling party based on events they hear about in other communities. Citizens might not trust news stories from communities they are unfamiliar with, or they might place greater weight on their observations of the government's performance in their own community.

I implemented a survey experiment to test the influence of local news reports about government service delivery failures on citizens' opinions about community government, national government, and the ruling party. The primary hypothesis is that when citizens hear news reports about government service delivery failures in other communities in their region, they negatively update their views about the government and ruling party. The second hypothesis is that when negative news reports are followed by reports of a government response to remedy the service delivery failure, the negative impact on public opinion is attenuated.

The survey experiment is a two-armed intervention with a "positive" arm and a "no response" arm. Both treatment arms include a two minute news clip about a service delivery issue Tanga Region. The clip is a shortened version of a 15 minute news story that was broadcast on Tanga Kunani FM (TK FM) as part of the *Wakati Ni Sasa* radio program. The news item introduces water as an important issue in Tanzania, then introduces a journalist who is conducting investigations of water issues in Tanga Region. The journalist then shares a story about the lack of water service delivery in a near-bye village in Tanga and plays interviews with both villagers complaining about the impact that the lack of water has had on their health and safety, as well as an interview with a community government leader saying that the national government promised support but had not yet followed through.

After the initial news clip, the two treatment arms diverge:

- **Treatment 1 (Positive Response):** In the "positive response" report, the news follows up on the initial report by describing how the government successfully responded to villagers' needs and includes citizens and local officials thanking the government for their response.
- **Treatment 1 (No Response):** In the "no response" report, the report follows up the initial clip with the information that 5 months after the initial report, the government has still not responded to villagers' needs. The report indicates that the journalist will continue to investigate the effects of the water service delivery

failure.[3]

The two arms were designed to model the choice facing the Tanzanian government after the *Wakati Ni Sasa* report aired: they could respond to the problem, generating a "positive response" follow up report, or they could ignore the report, leading to a "no response" follow up report. My goal was to understand the public's reactions to these two outcomes.

Table A12: **Survey Experiment**, Positive Response Clip vs No Response Clip

	Beliefs	*Attitudes*	
	Posterior	Nat. Gov	Loc. Gov
	(1)	(2)	(3)
Gov. Response vs No Gov. Response	0.134	0.095	0.025
Standard Error	0.054	0.054	0.056
RI p-value	0.007	0.039	0.326
Hypothesis	+	+	+
Control Mean	0.29	0.59	0.49
Control SD	0.46	0.49	0.50
DV Range	[0-1]	[0-1]	[0-1]
Blocked FE	Yes	Yes	Yes
Controls	No	No	No
Adj-R^2	0.02	0.01	-0.00
Observations	309	321	324

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Posterior Beliefs [1 if Sometimes or Always, 0 Otherwise]: "Some people say that citizens receive the development support they need from government, and other people say citizens do not receive enough support. How about you, when citizens have development problems do you think the government always, usually, sometimes, rarely, or never responds?", Attitudes [1 if Very or Somewhat Satisfied]: "Now, I would like to ask you about how satisfied you are with different groups...[National Government, Community Government]"

Table A12 shows the effect of the "positive government response" news story relative to the "no government response" news story on respondent's beliefs and attitudes about the Tanzanian government. Respondents who listened to a story in which the government responded to a service delivery issue were 13 percentage points more likely to say that the Tanzanian government sometimes or always gives citizens the development support they need (RI p-value = 0.007) when compared to respondents who heard the "no response" clip. Beliefs about government responsiveness translated into more general attitudes towards the national government. Respondents who heard the "positive response" clip were 9 percentage points more likely to say they were satisfied with the national government (RI p-value = 0.039). Negative updating towards the government was focused more on national government than local officials; the treatment effect on attitudes towards community government was small and statistically

[3]It is important to note that the survey experiment does not involve direct deception because respondents are never provided by inaccurate information. The "no response" report accurately reports that after 5 months, the government had not yet responded. The "positive response" report accurately states that after 6 months, the government resolved the water issue. After the outcome measures were collected, the surveyor played a final news clip about Tanzanian government water policy in Tanzania and actions in the village to ensure that all respondents received the full set of information by the end of the interview.

insignificant.

Instead of comparing the two treatment arms (an investigation followed by no government response versus an investigation followed by a positive government response), Table A13 shows the impact of each treatment arm independently relative to a pure control.

Interestingly, the "no reponse" treatment does *not* influence citizens' beliefs about whether the government responds to the needs of citizens, but it *does* negatively influence attitudes about the national government. The "positive response" treatment had the inverse effect: it positively influenced citizens' beliefs about whether the government responds to the needs of citizens, but did *not* influence attitudes about the national government.

Table A13: **Survey Experiment**, Positive Government Response vs No Government Response

	Beliefs	*Attitudes*	
	Posterior	Nat. Gov	Loc. Gov
	(1)	(2)	(3)
No Gov Resp. vs Control	-0.013	-0.093	-0.065
Standard Error	0.082	0.081	0.083
RI p-value	0.401	0.037	0.116
Hypothesis	+	+	+
Yes Gov Resp. vs Control	0.120	0.002	-0.040
Standard Error	0.054	0.054	0.056
RI p-value	0.027	0.976	0.479
Hypothesis	+	+	+
Control Mean	0.31	0.69	0.56
Control SD	0.46	0.47	0.50
DV Range	0-1	0-1	0-1
Blocked FE	Yes	Yes	Yes
Controls	No	No	No
Adj-R^2	0.01	0.00	-0.00
Observations	463	484	493

Note: * p < 0.1, ** p < 0.05, *** p< 0.01. Posterior Beliefs [1 if Sometimes or Always, 0 Otherwise]: "Some people say that citizens receive the development support they need from government, and other people say citizens do not receive enough support. How about you, when citizens have development problems do you think the government always, usually, sometimes, rarely, or never responds?", Attitudes [1 if Very or Somewhat Satisfied]: "Now, I would like to ask you about how satisfied you are with different groups...[National Government, Community Government]"

G Distributional Effects

Did *Wakati Ni Sasa* reports generate dramatic improvements in a few communities or modest improvements across many communities? **??** shows a smoothed distribution of audit scores for treatment (blue) and control (red) communities. Communities assigned to receive *WNS* investigations were more somewhat more likely to receive positive scores ranging from 2 to 10, and were less likely to receive zero or negative scores. While it it is impossible to rule out the possibility that *WNS* reports inspired large-scale responses in some communities that were then offset by negative responses in others, **??** lends credence to the interpretation that *WNS* had modest-to-moderate influence across many communities rather than large scale influence in a few. *WNS* may also have forestalled the *deterioration* of public goods and services (reflected in a negative audit score), which occurred in 8 control communities but just 2 treated communities.

Figure A3: **Smoothed Distribution of Independent Audit Scores**, Treatment versus Control Comparison

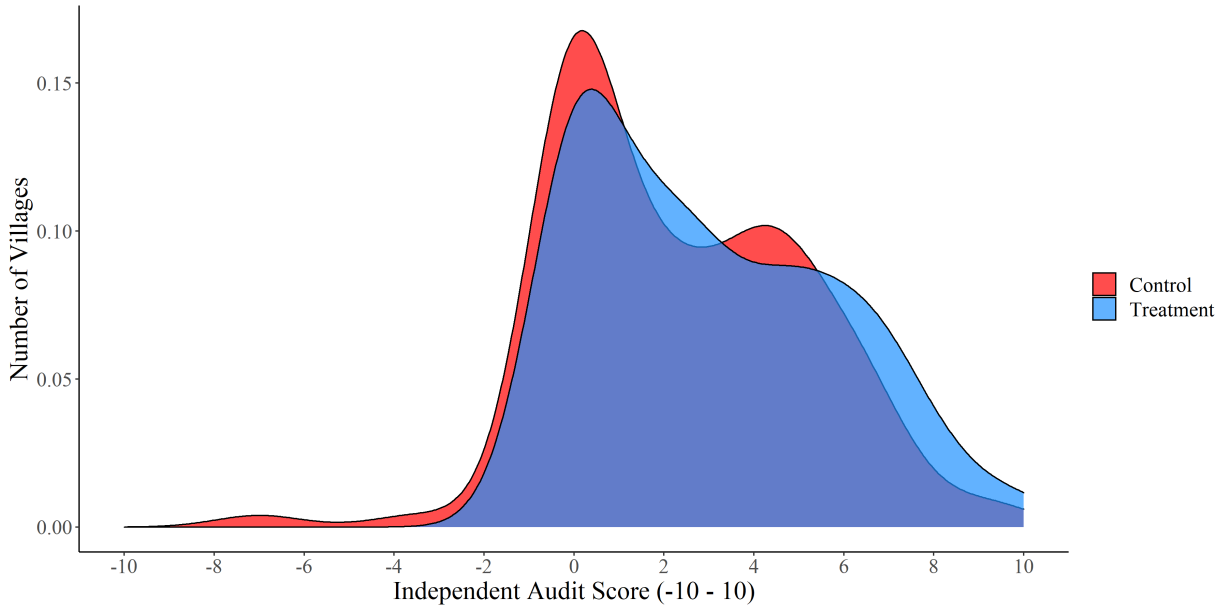
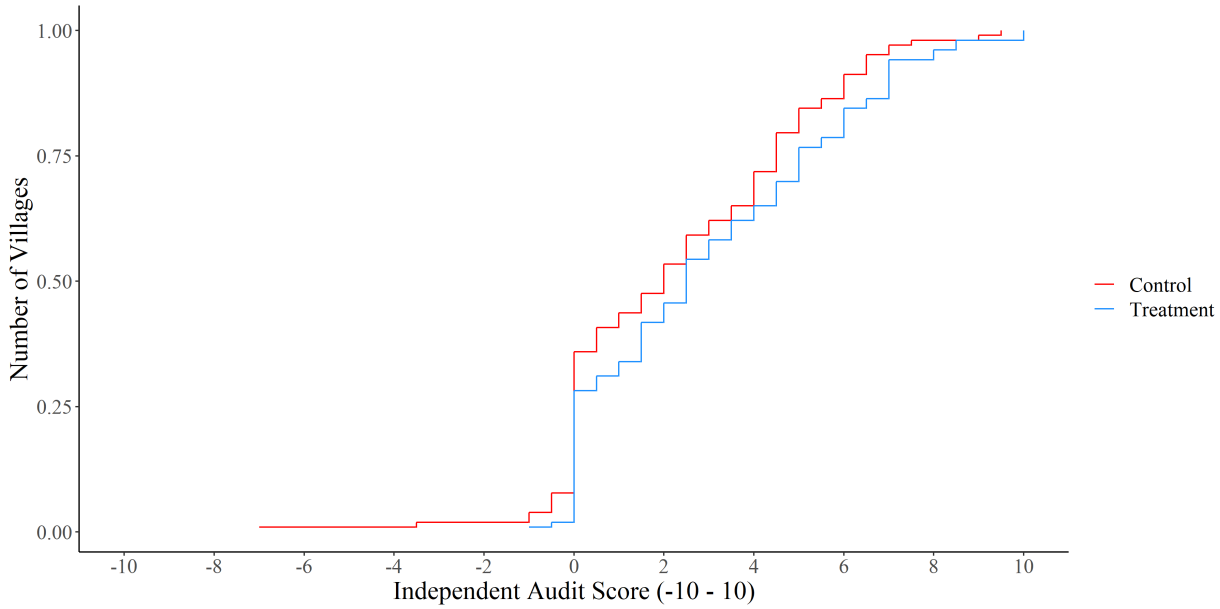


Figure A4 offers an additional way to visualize this finding. By-and-large, we do not observe substantial differences in the treatment effect across quantiles, suggesting that the treatment did not have significant distributional effects (eg effecting communities that tended towards less government responsiveness more than communities that tended towards higher government responsiveness).

Figure A4: **Cumulative Density of Audit Scores**, Treatment versus Control Comparison



H Power Calculations

I performed power analyses for three measures of the primary outcome: the report card score for primary service delivery, the matched-pair winner of report card score, and a binary indicator. Table A14 shows minimal detectable effects (MDE) in standard deviation units using input parameters from the dataset for 80% power at α = 0.05 (one-sided test), as well as the estimated average treatment effect for reference. The explanatory power of matched-pari blocks is 0.55.

Table A14: **Minimal Detectable Treatment Effects**

Outcome	MDE (Power=0.8)	ATE
Overall Response (-10-10)	0.69	0.684
Overall Response (Matched Pair)	0.306	0.311
Overall Response (Any)	0.125	0.06

I Ethics

I.1 Equipoise

The principle of equipoise holds that an experimental intervention is ethically justifiable only if there is reasonable uncertainty among researchers that any arm of the intervention may be better for experimental subjects than any other arm. Recently, MacKay has introduced the term "policy equipoise", which holds that all treatment arms should also be in a state of equipoise with the best reasonably attainable alternative intervention.

The uncertainty around the effects of media, accountability, and community empowerment programs in low-income countries suggests that the current project satisfies the equipoise standard. There are no experimental studies of the effects of investigative media reports on government responsiveness, and more general studies on the effect of transparency programs have produced mixed results (?). Just as importantly, there is no consensus around better *alternative* interventions to address government service delivery failures at a similar cost and scale to this study's intervention (+/- 125 USD per village). Like media interventions, community empowerment programs and information treatments have produced mixed and/or modest results.

In short, current scholarly equipoise around the appropriate bundle of interventions to address government responsiveness is the central ethical justification for this project. A village receiving a media report is not obviously better off than a village that does not receive a media report, and neither village is obviously better off than a village that receives any other intervention attainable at a similar cost.

I.1.1 Scarcity

Even in the absence of equipoise, the study could be justified on the grounds of resource scarcity. Given a budget constraint by the radio station and research team, it would have been impossible to support media reports in all 200 villages. Even in the absence of policy equipoise, resource scarcity can justify randomization if no participant has a stronger moral claim to the intervention than other participants. We did consider the possibility that certain villages had a unique moral claim to media coverage relative to the rest of the sample based on the nature of their problem.

However, two considerations cut against this concern (apart from the equipoise argument from above). The first is that it would have been very difficult to differentiate the villages in "extreme need" from other villages in the sample in the absence of further investigation. All villages that radio stations identified expressed a strong need for government support. The second consideration is that the study design did not prohibit the village from receiving a media report treatment from the partner radio station or any other media outlet. If a village's situation proved particularly dire, there were still options for delivering the intervention, albeit outside the scope of this study.

Nonetheless, the research team was attentive to any village reports proposed by radio stations that seemed to demand urgent attention from the station, such that it would have been unethical to assign the village to the

control group. In one case (a story of ongoing evictions and physical abuse of citizens by police), the research team recommended removing the village from the sample and arranged support for a media report outside the research design. The removal of this village occurred prior to random assignment.

I.2 Researcher Role with Respect to Implementation

This study is not a case of a researcher independently evaluating an intervention that would have occurred in the researcher's absence: the investigative media reports would not have occurred when, where, and how they did in the absence of the researcher's participation and financial support. This level of researcher involvement in the intervention design implies a unique ethical responsibility by the researcher to the participants in the study.

The research team spent 1.5 years reaching out to radio stations and building relationships with station managers and reporters to understand the scope of their reporting, safety and ethical concerns, and their interest in a large-scale research project. The investigative report template was developed in collaboration with media houses and journalists, and radio stations held primary control and ultimate veto power over where to conduct reports, whom to interview, and what to include the broadcasts. Radio stations controlled the sampling process and carried out the reporting without researcher interference. The research team, in turn, provided funds for transportation, per diem, and production totally +/- 125 USD per report. These funds did not cover the full expenses of the reporting, which were shared by the media houses. The research team also reviewed all episodes before they were broadcast to ensure that no content would be aired that would put the radio station, journalists, communities, or research team at risk. Ultimately, no content was censored by the research team, although some recommendations were made to interview government officials for "balance" that is legally mandated by the Tanzanian government.

It is important to note that in the absence of the researcher's involvement, the radio stations would still have reported, produced, and broadcast local news stories. However, the researcher's involvement undeniably shaped the nature of the reports in three ways. First, transportation support allowed journalists to visit villages further away from the station than they would otherwise generally visit. Second, the production support enabled reporters to carry out more thorough investigations than standard news reports. Finally, the research design meant that radio stations produced more local news stories than they would typically produce.

I.3 Potential Harms to Participants or Non-participants

There are three categories of participants in this research project.

I.3.1 Journalists and Media Houses

The first category of participants is participating media houses and journalists. In the worst case scenario, journalists could have been harassed, arrested, or otherwise harmed by communities who did not want certain topics to be broadcast or aired. We took the following steps to minimize potential risks to media houses and journalists.

- The research team engaged in 1.5 years of planning conversations with media houses to ensure that the design of the intervention would not put them at legal risk.
- The research team agreed that all reports would include opportunities for government responses, in line with Tanzanian media law.
- The research team also agreed to exclude topics that media houses felt would invite government reprisal. These topics varied from region to region. In Mtwara Region, for example, we did not pursue stories related to sensitive issues around refugees entering the country from Mozambique.
- The research team made clear to journalists that they could abandon an investigation with no financial penalty if they felt that continuing to report would put them at risk, which they chose to do in two cases.
- The research team gave ultimate veto power over whether and when to air broadcasts to media houses.
- For all reports, the radio station invited government officials to respond on the record to citizen concerns in the report.
- The research team elected not to implement the project in the run-up to Tanzanian presidential election to avoid the perception of interfering with the electoral process.

I.3.2 Community Members in Treatment Areas

The second category of participants is community members in treatment areas. It is important to note that individual community members who elected to speak to journalists are not technically research subjects, in that no research data was collected from or about them. Nonetheless, it is easy to imagine a scenario in which the research project resulted in harm to community members who spoke with the media for the reports. For example, they could have experienced retribution from community members for speaking out about controversial issues. We took the following steps to minimize potential risks to community members who collaborated with journalists and media reports:

- No community member was forced to participate in an investigative report or was named in the report without their consent.
- Any community member was free to request that their contribution be considered off the record so that their name and identifying information was not associated with the reports
- All community members were given the contact information of participating journalists and allowed to follow up if had concerns of questions about the reports.
- The research team was in weekly contact with every media house for updates on any threats to participant safety. There were no reports of adverse consequences for participating community members.

It is also plausible that the reports may have led to government responses that left community members in participating villages worse off, whether or not they were interviewed by journalists. Uncertainty around the net effects of the reports on treatment communities is part of what generates equipoise, discussed in the sections above. If we had observed systematic negative consequences for participating communities, we would have closed the project. However, no such consequences were observed.

I.3.3 Government Officials in Treatment Areas

The third category of participants is government officials in treatment areas. The actions or inactions of government officials were often the subject of media reports. Reports may therefore have had negative consequences on the careers of elected officials and bureaucrats, although more severe harm was deemed unlikely and was not observed during the research period.

We ultimately deemed negatve career outcomes for government officials an acceptable negative risk, in line with interventions to promote accountability of bureaucrats and elected officials. In line with Tanzanian media law, government officials at the village, ward, and district level were in all cases invited to respond to media reports (on or off the record) and invited to follow-up with the media house after the report was broadcast.

I.3.4 Community Members in Control Areas

The final category of individuals potentially harmed by the project is community members in control areas. There are two plausible pathways by which individuals could be harmed. First, if a given region has limited financial resources, media reports in one village could have resulted in the diversion of resources from control villages towards treatment villages (also a violation of the non-interference assumption). Second, if media houses were focused on treatment areas, they may have ignored important social concerns in control areas to the detriment of communities in those areas.

The research team took several steps to avoid these concerns:

- The research team explicitly informed media houses that they were *not* restricted from reporting on issues in control areas. This avoided the risk that serious harms occuring in control areas would be ignored.
- The research team conducted the study *after* the budget process was underway. This meant that the primary focus of the intervention was on mobilizing action by at the Ward level (the unit of randomization) rather than the District or Regional level (where budget tradeoffs would be more likely to negatively impact control areas.

Nonetheless it is impossible to discount the possibility that media reports on one area may result in less attention on other areas. On the other hand, it is possible that media reports on one area may have increased attention on other areas as political actors sought to preempt the possibility of future reports. This uncertainty is part of what motivates and ethically justifies the research design.

I.4 Potential Harm from Data Collection or Research Protocols

I.4.1 Informed Consent for Randomization

Because of the nature of the intervention, community members were not given informed consent over the randomization process. As described in the section above, however, consent was implied by voluntary participation in the reports in much the same way that consent is implied by voluntary participation in community meetings during

community development interventions

I.4.2 Informed Consent for Interview Participation

Data collection was conducted independently of the intervention and the enumeration team was blind to the treatment status of villages. All subjects will be provided with both written and oral explanations of the consent process and given the option to refuse participation. All data will be collected on password protected tablets using SurveyCTO/Open Data Kit software. Upon completion of the the survey, survey data is synched to an encrypted SurveyCTO server and downloaded to a laptop held by the research team and encrypted using Boxcryptor.

I.4.3 Community Consent

Survey enumerators will secure consent from Regional, District, Ward, and Village officials before conducting any surveys. This consent is both legally mandatory and ensures the safety of enumerators and research participants.

I.5 Potential Harms to Field Staff

Because of the geographic coverage of the study, field staff will be required to travel long distances, sometimes on public transportation or motorcycle. The research team takes the health and safety of staff extremely seriously. All staff are provided with medical insurance in case of accidents. All staff are provided per diem sufficient to cover overnight stays in safe lodgings so that they do not need to travel at night. All staff are paid salaries in compliance with Tanzanian employment law and their salaries are reported to government.

Additional risks introduced by the COVID-19 pandemic are discussed in an independent section below.

I.6 Financial or Reputational Conflicts of Interest

The research team reports no financial or reputation conflicts of interest in this study.

I.7 Intellectual Freedom

The research team reports no contractual limitations to reporting the results of the study. The Tanzanian government requires that any research be submitted to their research compliance office before publication, but there are no restrictions on what findings can be published.

I.8 Feedback to Participants or Communities

Because this study involves collaboration with radio stations, there is a in-built opportunity for sharing research findings with participating communities. Radio stations have agreed to broadcast reports about the findings, which will enable treatment, control, and non-sampled communities to hear about the results.

I.9 COVID-19

This project was implemented and data was collected in the midst of the COVID-19 pandemic. The research team takes its responsibilities related protection of research staff and subjects seriously. We obtained approval from Columbia University's COVID-19 review board to carry out the data collection, and designed transportation and

data collection procedures with COVID-19 risks in mind. Shortly before the start of endline data collection, the Omicron variant of COVID-19 was discovered, so endline data collection was converted to phone interviews.

J Pre-Analysis Plan

Spotlights:
Local Media and Government Responsiveness in Tanzania

Pre-Analysis Plan

Dylan W. Groves

Columbia University *

December 10, 2021

*s.

Contents

*

1 Introduction

In this document I describe the research and analysis strategy for the research project entitled *Spotlights: Local Media and Government Responsiveness in Tanzania*. The study features a national-scale randomized controlled trial implemented in collaboration with 15 radio stations in Tanzania.[1] This document predates endline data collection,[2] and has been approved by the Tanzania Commission for Science and Technology and Innovations for Poverty Action Institutional Review Board (IRB), protocol 15487. Any contingency not accounted for in this PAP will be dealt with according to the Standard Operating Procedures for Don Green's lab.

2 Motivation

Does local media coverage improve government responsiveness? The relationship between media and government responsiveness is a central concern in political science. It is particularly relevant in post-colonial states, where independent media has grown exponentially since the 1990's and significant proportions of the citizenry rely on government programs for fundamental human needs like water and health care.

Classic theories of political accountability suggest that the rise of local and independent media should be a boon for responsive governance. According to theory, local media engenders electoral accountability by publicizing the performance of politicians to their constituents and bureaucratic accountability by publicizing the performance of bureaucratic agents to their political or bureaucratic principals. Media coverage may also spur citizens to coordinate collective action to resolve local problems themselves.

In practice, the effect of independent media on government responsiveness is less clear. A robust literature utilizing observational methods finds that media improves service delivery (Besley and Burgess 2002; Snyder and Strömberg 2010), but a growing body of experimental evidence finds only limited evidence that delivering information to citizens and politicians influences political outcomes (Dunning et al. 2019; Raffler 2019). What accounts for this disconnect? One possibility is that non-experimental research overestimates the effect of media because observational research designs suffer from classic selection problems. A more hopeful prospect is that extant experimental research underestimates the effect of media coverage because it tests artificial treatments that do not capture important channels of media influence. It is difficult to adjudicate between these explanations without experimentally varying real-world media coverage.

This research project seeks to remedy the impasse. In cooperation with a network of 30 journalists working at 15 independent media houses across Tanzania, we implement a nation-wide randomized evaluation of investigative media reports about government service failures and track the effects of the reports on citizen mobilization,

[1] The radio stations are: Jamii FM (Mtwara), Mushajaa FM (Lindi), Tanga Kunani FM (Tanga), Moshi FM (Moshi), Planet FM (Morogoro), Pambazuko FM (Morogoro), Highlands FM (Mbeya), Nuru FM (Iringa), Dodoma FM (Dodoma), Voice of Sumbawanga (Rukwa), Uyui FM (Tabora), SAUT FM (Mtwara), Mazingira FM (Mara), Storm FM (Geita), Mpanda FM (Katavi).

[2] Note that the updated draft of this pre-analysis plan was revised shortly after launch of data collection to account for changes to the data collection plan that were made necessary by the discovery of the Omicron variant of COVID-19

government responsiveness, and service delivery outcomes.

3 Mechanisms

When and why do media reports influence government responsiveness? We identify several models of government responsiveness and discuss what they predict about the effect of independent mediage reports. These models rarely make directly contrasting predictions. This project will provide an exploratory investigation of causal mechanisms rather than decisive tests.

3.1 Bottom-up Accountability

In standard accounts of bottom-up accountability (Besley and Burgess 2001; Fearon 1999), an elected official chooses whether to expend effort and resources to services to constituents or to shirk and extract private rents. Citizens then choose whether to vote for the elected official. Citizens' ability to select better politicians and sanction worse politicians depends on the degree to which they can observe government performance. Media coverage contributes to bottom-up accountability by increasing the probability citizens observe poor performance by elected officials.

Bottom-up accountability improves government responsiveness through a selection channel and a motivation channel. First, bottom-up accountability allows citizens to select leaders who are better at providing public goods. Second, accountability motivates elected officials to provide public goods in order to avoid electoral sanction. In the present study, we are primarily interested in the motivation channel, because the treatment and end-line data collection take place in-between Tanzania's national elections.

Theories of bottom-up accountability predict that negative news reports increase responsiveness by elected officials. Media reports are a credible signal about the perceived probability of future reports (as part of the experimental design, journalists told government officials that they would return for follow up reports). Elected officials respond in the hopes of avoiding the spread of further negative news coverage about their performance or the performance of the ruling party. If media reports operate through bottom-up accountability channels, we expect to see increased responsiveness (meeting attendance, village visits, financial contributions, and lobbying) by elected officials.

A second prediction of theories of bottom-up accountability is that the effect of improved citizen monitoring is strongest when electoral outcomes are closely contested (Besley and Burgess 2002). In areas where an elected official is already unlikely to win re-election (such as when an incumbent is term limited) or very likely to win re-election (such as areas with deeply entrenched ruling party support), the officials' incentive to deter or respond to negative news coverage is reduced.

3.2 Top-down Accountability

A second class of theories linking media reports and government responsiveness centers on the accountability relationship between autocrats or elected officials and local bureaucrats (Egorov et al. 2009; Lorentzen 2014; Raffler

2019). Theories of top-down accountability may be especially helpful for understanding government responsiveness in non-democratic regimes like Tanzania where bottom-up accountability processes are weak.

The basic framework for top-down accountability processes is a principal-agent problem between a higher-level government official (the principal) who has imperfect oversight over a lower-level bureaucrat who is responsible for implementing government programs (Pande 2020). The government official wants local level officials to deliver constituency services, but the lower-level government official would prefer to shirk and extract rents without the threat of punishment.

Theories of top-down accountability predict that negative news reports will increase bureaucratic responsiveness. Media reports about under-performance could improve bureaucratic responsiveness through a selection channel (allowing higher level officials to identify and replace poorly performing bureaucrats) or a motivation channel (motivating lower level bureaucrats to perform better to avoid being punished or replaced). Therefore, if media reports operate through a top-down accountability channel, we should expect treatment to increase the replacement of bureaucrats, the pressure put on bureaucrats (eg by Ministry officials and ward/village executive officers) by elected officials and/or increased direct actions by bureaucrats.

3.3 Citizen Oversight of the Bureaucracy

A related theoretical perspective focuses on the role of citizen oversight of local bureaucrats. In this "short-route" to accountability, citizens directly pressure service providers rather than making appeals to politicians who then sanction or reward bureaucrats. Media reports potentially enable citizen oversight by sharing information about bureaucratic performance, allowing citizens to leverage social sanctions (Tsai 2007) or complaint systems (Slough 2020) to improve bureaucratic performance. Media reports may also help citizens understand which government officials are responsible for a given service delivery failure, allowing them to focus their lobbying efforts. Finally, media reports may help citizens overcome collective action obstacles to community monitoring of bureaucrats by generating shared understandings about the importance of the targeted service. If media reports operate through a citizen oversight channel, we should expect to see increased citizen meeting attendance and pressure of bureaucrats, as well as increased bureaucratic responsiveness.

3.4 Citizen Mobilization

Another of class of theories linking media reports and government responsiveness focuses on local collective action. Citizen-led responses to development challenges are especially resonant in rural Tanzania, where the historical legacy of Julius Nyerere's ideology of "ujamaa" (local, cooperative development) remains deeply embedded in social and political life. Local governments regularly ask citizens to directly contribute labor or resources to development projects before requesting support from higher level government officials. The theoretical literature on collective action emphasizes that it depends in part on common knowledge and effective coordination. Visits by journalists and

broadcast reports have the potential to generate shared understandings about the importance of a local problem and the viability of local responses, engendering citizen-led solutions. If media reports operate through a citizen mobilization channel, we should expect the treatment to increase citizen contributions of resources and effort to directly address the service delivery challenge or engage in more effective oversight of elected officials and bureaucrats (see above sections).

3.5 Government Knowledge

Government responsiveness may also be hindered by a more banal problem: government officials may simply not know the preferences and needs of their citizens (Liaqat 2020). Even if politicians are motivated to serve their constituents, media reports may improve government responsiveness by helping officials understand where citizens are upset. If media reports operate through a politician information channel, we should expect to see media reports to increase responsiveness by elected officials. Unlike the bottom-up accountability channel, however, this effect should not be moderated by baseline levels of political contestation.

3.6 Non-Governmental Responses

In many low-capacity states, non-government organizations, business, and private-citizens fill the role of service delivery in communities where the state cannot. In Tanzania, local development projects are regularly funded through these non-traditional means: domestic and international NGOs build water points and clinics; rural citizens ask wealthy family members in cities to contribute funds for development; and governments negotiate with local mining companies or large-scale farms to support development projects in return for land titles.

Media reports could influence service delivery through non-governmental channels in several ways. First, NGOs or private citizens could hear media reports and use that information to determine where they implement new projects. Second, elected officials could feel pressure from negative news reports and opt to lobby NGOs, businesses, and private citizens to provide services if government resources are not available. Finally, media reports could mobilize collective action by local citizens who contact NGOs or citizens outside the community to assist them.

If media reports affect service delivery through a non-governmental channel, we should expect to see increased village visits and financial (or in-kind) contributions by NGOs to the development problem.

3.7 Government Allocation

The final model of responsiveness centers on the difficulty that highly centralized political systems have allocating limited government resources among competing priorities. In Tanzania, ward councillors meet on a quarterly basis to make decisions about how district resources will be allocated to development projects. Ward leaders also compete with one another to secure support from central government Ministries of Water, Roads, and Energy. This process of negotiating for access to limited resources creates a dilemma for centralized governments. Every ward councillor has an incentive to ask for as many resources as possible. As a result, central governments have no reason

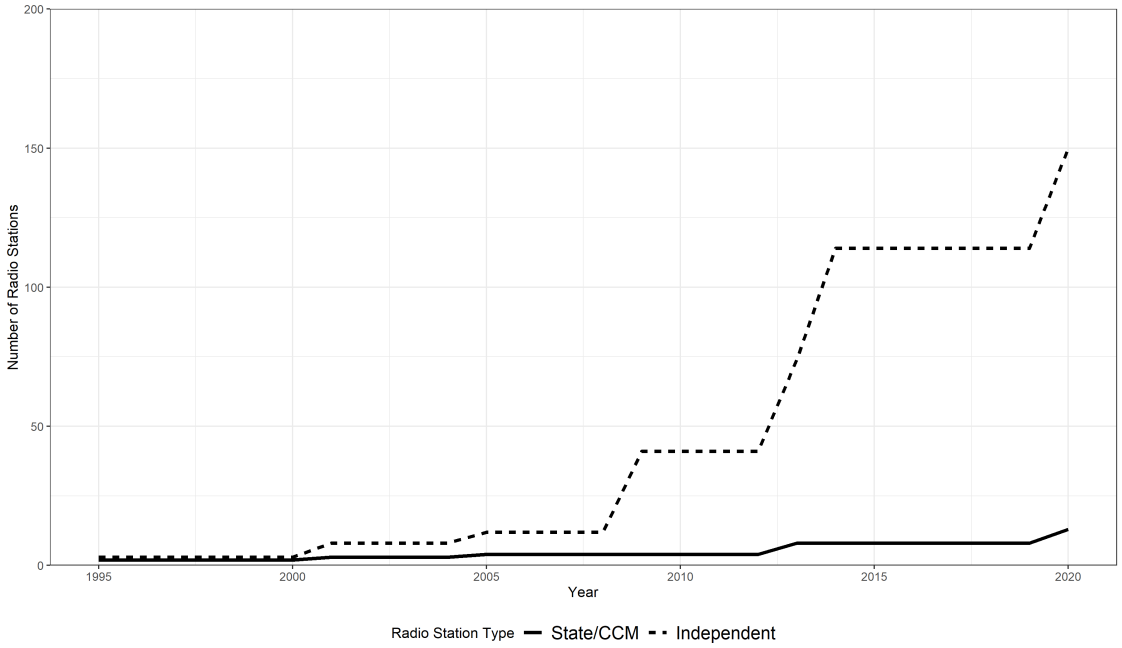
to trust the signals sent by ward councillors about their constituents' needs (other than the ward councillor's shared interest in the best interest of the ruling party).

In this context, independent media reports may be helpful to both ward councillors and central governments. Negative independent media reports threaten the legitimacy of both ward councillors. Encouraging negative media reports is therefore a way for ward councillors to send a "costly signal" about the needs of their ward, separating themselves from ward councillors who might claim severe need but would be unwilling to pay the reputation cost of inviting a negative media report. If media reports influence government responsiveness through a government allocation mechanism, we should expect local news reports to increase responses by higher level government officials (but not lower level elected officials), and local elected officials from villages with more severe service delivery problems to actively support visits by journalists.

4 Context

Like citizens of many post-colonial states, Tanzanians have witnessed a dramatic expansion of local and independent media over the the last thirty years (Sturmer 1998). From independence in 1962 until 1993, Tanzanians were able to access just two radio channels: state-owned *Radio Tanzania Dar es Salaam* and *Sauti ya Injilli (Voice of the Gospel)*, a non-news radio station owned by the Catholic Church (Sturmer 1998). After political liberalization and the repeal of Tanzania's most onerous media restrictions in 1993, the number of independent radio stations climbed to three in 1995 and 11 in 2005, the year that Jakaya Kikwete was inaugurated as Tanzania's fourth president. Under Kikwete, the number of independent mass media houses grew dramatically: there were 125 independent radio stations in Tanzania by the time he left office in 2015 (see Figure 1). Today, there are 188.

Figure 1: **Growth of Independent Radio in Tanzania Since 1990**



While many of Tanzania's newly minted media houses are focused on urban centers like Mwanza and Dar es Salaam, about 70% of Tanzania's registered radio stations are based in regions that, prior to 2005, had never carried a regionally-focused news outlet. These local, independent radio stations are owned by a medley of local business people and politicians (47%), religious organizations (21%), and community-based organizations (30%) (Mwafisi 2015). While most privately-owned radio stations focus on music and entertainment, almost all make room for daily local news coverage and many employ teams of journalists dedicated to covering issues of local concern (Katunzi and Spurk 2019; Spurk and Dingerkus 2017).

5 Intervention

The intervention is an investigative report of a government service delivery failure in the targetted village. Although implementation varied from station to station and village to village, all radio stations agreed on the following template for outreach, reporting, broadcasting, and follow-up. It is important to note that the intervention is modelled on high quality local news reporting advocated by journalism NGOs in Tanzania. For many radio stations, it improves upon their standard reporting in three ways: (1) reports were more likely to include follow-up interviews with government officials, since journalists had permission from their managers to spend time on the reports, and (2) reports were longer than many standard news reports, since journalists had the opportunity to travel to villages for in-depth interviews. Audio and written summaries of all reports are available here.

5.1 Outreach

In each targeted village, reporters contacted the Ward Councillor (Diwani), Village Chairperson (Mwinyikiti), and any citizens in the area who had expressed concern about the issue. Reporters informed the contacts that they would travel to the village to record citizen concerns and speak to the government about the problem.

5.2 Reporting

Radio stations sent 1-2 journalists to the targeted village to report the story. Reporters were instructed to speak with villagers about the specified issue, how it was affecting their lives, who they thought was responsible for change, and what they hoped would be done. Reporters were given wide latitude for conducting follow up interviews based on the nature of the problem, but in all cases were expected to speak to government officials at the village/street, ward, and District level. Where appropriate and possible, they also followed up with Members of Parliament, Regional Commissioners, and representatives of relevant bureaucracies.

Visits to the village usually lasted 1-3 days, and reporters took anywhere from one week to one month to conduct follow up interviews. The most common reason for extended reporting times was the refusal of District and Regional officials to make themselves available for interviews. Reporters were not allowed to produce and air episodes without a government response to ensure fairness and avoid conflicts with the Tanzanian government. In two villages, episodes were never aired because the government refused to respond to journalists. These villages are counted as "non-compliers" for purposes of analysis.

5.3 Production

Journalists and radio station production teams worked together to turn reporting into 15 minute narrative reports for radio broadcast. The reports generally had four parts: an introduction by the narrative explaning the problem and context, interviews with villagers explaining how the problem is impacting their lives and what they would like to see change, interviews with local government officials about the history of the problem and reasons for inaction, and follow-ups with higher level government officials about their plans for addressing the issue. The reports did not advocate for specific government actions, but they almost always included bringing requests from citizens to higher level government officials.

5.4 Broadcast

All reports were broadcast twice, generally over the course of a week. The broadcast included a short introduction by the radio presenter, who invited citizens to call or text comments or feedback to the report. Following the report, presenters took calls and read text messages live on air. Broadcasters also informed listeners that they would be following up on the story in the village over the following months. In addition to the radio broadcast, reporters were free to post pictures and discussion about the stories to their website and social media. In anticipation of the broadcast, journalists called villagers and government officials to inform them that the episode would be put on air.

They also informed all parties that they would conduct follow up episodes in the ensuing months to track progress in resolving the issue.

5.5 Follow-Ups

3-4 months after the first broadcast, journalists conducted follow up reports of the problem in the village. Follow-up reports were shorter than the original reports, and usually included phone call interviews with villagers and government officials about the state of the problem in the village. Follow up reports were conducted regardless of whether the government had resolved or failed to resolve the specified issue. With the exception of the two villages where the government rejected the first report, all villages had received follow up reports a the time of data collection.

6 Experimental Sample

6.1 Radio Stations

We began by seeking out radio station partners who were willing and able to implement high quality local investigative reports. Selected radio stations met several requirements. First, we excluded radio stations in Zanzibar, where we did not have research permissions, and Dar es Salaam, which has a saturated media environment. To ensure that radio stations focused on local news issues, we only partnered with radio stations with a District or Regional (rather than National) broadcasting permit from the Tanzanian Communications and Regulatory Authority (TCRA). Finally, we asked journalists and journalism professors at the University of Dar es Salaam and St. Augustine School of Journalism to identify stations with 1-3 talented journalists with experience reporting on local government. Some radio stations were also dropped after a pilot episode (practice episodes that were dropped before random assignment) and discussions with management about the budget and project plan.

We ultimately identified radio stations in 15 of Tanzania's 26 mainland regions: Jamii FM (Mtwara), Mushajaa FM (Lindi), Tanga Kunani FM (Tanga), Moshi FM (Moshi), Planet FM (Morogoro), Pambazuko FM (Morogoro), Highlands FM (Mbeya), Nuru FM (Iringa), Dodoma FM (Dodoma), Voice of Sumbawanga (Rukwa), Uyui FM (Tabora), SAUT FM (Mtwara), Mazingira FM (Mara), Storm FM (Geita), Mpanda FM (Katavi).

Regions where we selected radio stations were roughly comparable to regions where did not select radio stations (excluding Dar es Salaam) in size (35 vs 36,000 km^2 and population (2.0 vs 1.8 million). Given the idiosyncratic reasons why radio stations in some regions did not participate (reasons for exclusion ranged from a low quality pilot episode to lack of airtime available for weekly broadcasts to a difficult station manager personality), we concluded that regions of participation radio stations are broadly representative of mainland Tanzania as a whole, excluding Dar es Salaam.

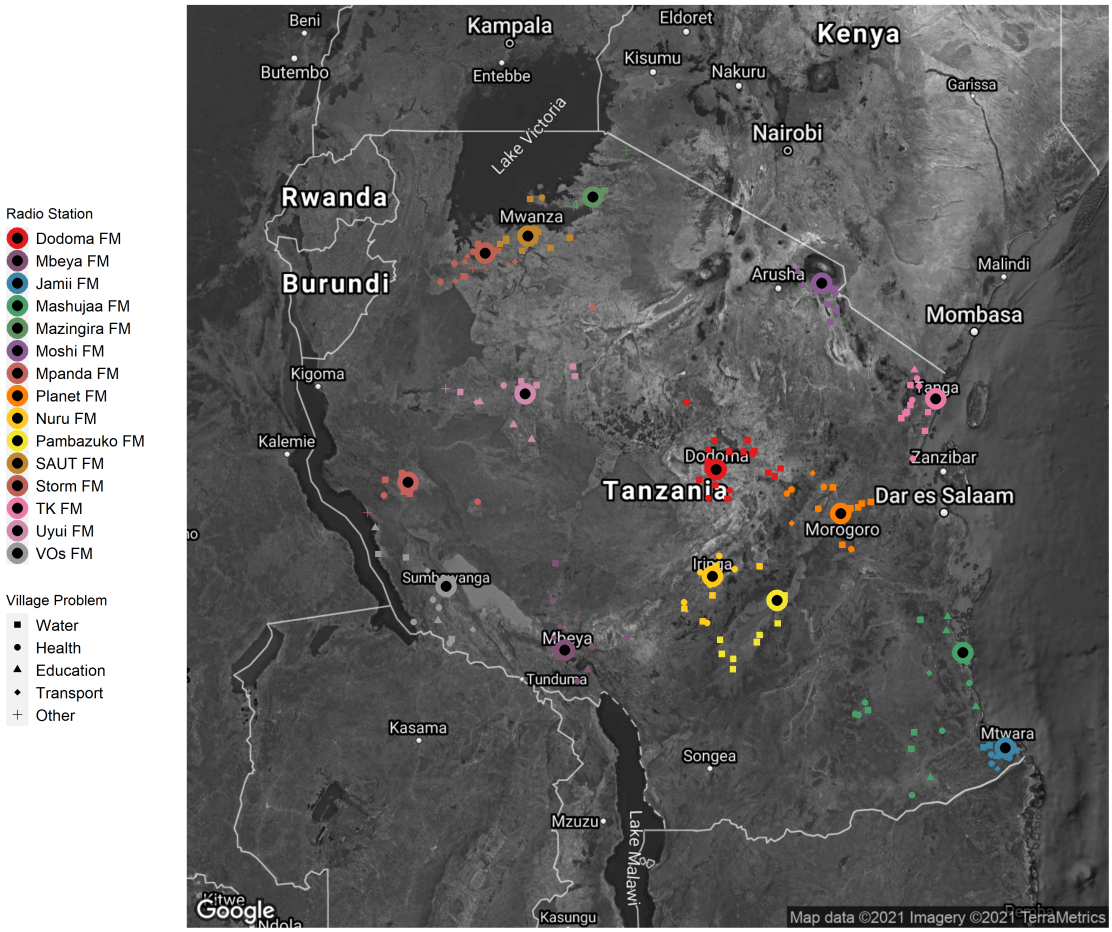
6.2 Villages

This study's unit of randomization is the village. Over the course of the implementation period, radio stations provided a list of 206 villages that they believed were suffering from a government service delivery problem. The strategies journalists use to identify report-worthy villages, especially in absence of meaningful travel budgets, is worthy of its own investigation. Journalists typically identify villages where they want to conduct reports in one of three ways: by receiving calls and texts during daily news programs from villagers; through Whatsapp groups radio stations set up for citizens to voice their concerns to journalists; or through informal social networks built through prior reporting.

To reduce the risk of interference between experimental units, we imposed the constraint that no village could be in the same ward as any other village in the sample, and no more than four villages in the sample could be from the same district.[3] Along with the village name and location, radio stations provided a brief overview of the challenge facing the village as well as proposed metrics for evaluating the impact of the radio report on government responsiveness.

[3]The only exception is in Mtwara and Mara, which are more dispersed than usual

Figure 2: **Treatment and Control Map**



6.3 Survey Respondents

Due to health restrictions related to COVID-19, primary data collection will be conducted through cell-phone interviews with the following respondents.

An important feature of endline data collection is that (1) surveyors are blind to treatment assignment as well as the purpose of data collection, and (2) participants are blind to the connection between endline data collection and media reports. This reduces the risk that either surveyors or village leaders will bias their assessments/responses to align with the goals of the experiment.

6.3.1 Elected Officials

The first sample of survey respondents is elected officials. In each village, we will sample three elected officials: the ward councillor, the village chairperson, and the village council-member focused on the topic of the reports. If either the village chairperson or targeted village council-member is not available, we will randomly sample from the remaining members of the village council.

6.3.2 Bureaucrats

The second sample of survey respondents is bureaucrats. In each village, we will sample two bureaucrats: the ward executive officer (WEO) and village executive officer (VEO).

6.3.3 Citizens

NOTE: Due to the emergence of the Omicron variant of the COVID-19 virus, we abandoned citizen data collection for the first round of surveying, since effective sampling of citizens was not possible by phone.

The final sample of survey respondents is villagers. In each village, we will sample two villagers. The first villager will be the villager who originally reported the problem. The second villager will be randomly selected from a village list provided by the village chairperson.

6.4 Direct Observation

The final sample will consist of direct observations at the village level. Enumerators will ask village leaders for pictures, audio recordings, and videos documenting the original service delivery problems. Using objective evidence and information from interviews with elected officials, bureaucrats, and villagers, enumerators fill in a report card about each village that includes their rating of the quality of response by villagers, government officials, and non-governmental organizations. These reports will then be reviewed by a member of the research team to ensure that no references to media reports appear and handed over to two independent scorers (also blind to treatment assignment) who will independently rate different actors on the quality of their response. These steps to blind the scorers to the presence of media reports are implemented to maintain symmetry between treatment and control.

7 Randomization

7.1 Blocking

All villages were submitted by radio stations to the research team in pairs. Pairs are almost always in the same district but do not necessarily share the same problem. The sample is therefore comprised of 103 two-village blocks. Each block-pair shares the same radio station and date on which the problem was identified by the radio station.

7.2 Random Assignment

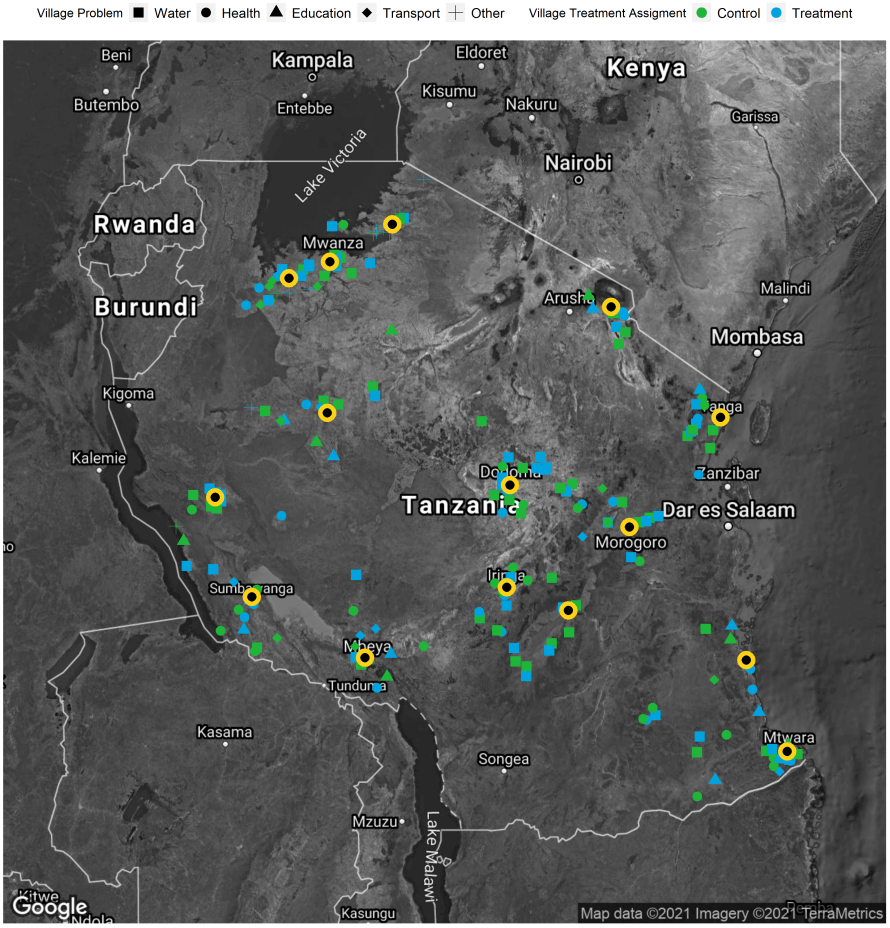
Within each pair of villages, one village was assigned to treatment using the following code[4]:

[4] due to travel by the research team, the initial 20 villages were selected using an online random number generator selecting between 1 (for first village) and 2 (for second village)

```
/* Randomize ____________________________________________________________________*/
        set seed 1956

        gen rand1 = runiform()
        gen rand2 = runiform()

        sort rand1 rand2
        bys block_uid : replace rank = _n

        gen treat = .
                replace treat = 1 if rank == 1
                replace treat = 0 if rank == 2
```

The map of the resulting sample appears in Figure 3

Figure 3: **Treatment and Control Map**



7.3 Balance on Covariates

We did not conduct a baseline survey out of the fear that an external survey would trigger two potential channels of media influence: mobilizing community interest in the selected problem and making local leaders think that they

are being observed. We therefore examine balance on observable pre-treatment covariates using a combination of satellite-based, administrative, and election data, as well as a series of dummy variables for the problem the radio stations identified in the village.Table 1 reports the treatment and control village means alongside the p-value calculated using Randomization Inference.[5] Out of 17 pre-treatment covariates, one (6%) generates a p-value of 10% or less, slightly fewer than expected by chance alone.

Table 1: **Balance** Village-Level Pre-Treatment Covariates

Variable	Treatment	Comparison	p-value	Observations
Population				
Male Population	1407.076	1612.126	0.124	166
Female Population	1444.215	1703.494	0.097	166
HH population	596.797	672.667	0.162	166
Distances				
Distance from Radio Station (km)	44.855	48.665	0.912	204
Distance from Regional Capital (km)	49.624	55.264	0.979	204
Distance from Dodoma (km)	353.600	350.143	0.322	204
Politics				
New Village (2018)	0.183	0.127	0.220	206
Share CCM Diwani Vote - Ward (2015)	0.609	0.620	0.756	192
CCM Diwani Dummy - Ward (2015)	0.706	0.700	0.431	202
Share CCM MP Vote - District (2015)	0.601	0.601	0.320	205
CCM MP Dummy - District (2015)	0.845	0.833	0.160	205
Share CCM Prez Vote - District (2015)	0.633	0.633	0.320	205
Selected Problem				
Problem - Education	0.500	0.480	0.711	206
Problem - Health	0.250	0.255	0.860	206
Problem - Transport	0.096	0.118	0.594	206
Problem - Water	0.058	0.049	0.564	206
Problem - Other	0.096	0.098	1.000	206

Note: There are some missing values do to the unavailability of covariate data. For example, new villages and wards do not have 2018 population data or elections data.

8 Estimation

8.1 Estimands

The estimand of primary interest is the intent-to-treat effect (ITT). Treatment "uptake" (villages where a journalist visited, produced, and broadcast a report was 98.1%. Radio stations were unable to complete reports in two villages assigned to treatment because the District Executive Director explicitly prohibited reports. These two villages are still partially treated because reporters spoke with villagers and government officials, even though reports were never broadcast.

8.2 Primary Specification

The unit of observation for analyses are citizen respondents, village leader respondents, and ward leader respondents. Intent-to-treat effects will be estimated using OLS regression. The regression model

[5] To calculate p-values using randomization inference, we generated 3,000 alternative treatment assignments using the same randomization process but different seeds. The RI p-value is the proportion of randomizations that produced an average difference between treatment and control villages at least as large as the difference generated by true treatment assignment.

$$y_j = \beta d_j + \gamma_1 block_{1j} + \gamma_2 block_{2j} \ldots + \gamma_3 block_{kj} + u_j$$

expresses the outcome y_j as a linear function of the randomly assigned treatment, binary variables for each of the village pairs (blocks), and an unobserved error term u_j. Our primary interest is β, which represents the Intent-To-Treat (ITT) effect. The regression estimator is identical to the difference-in-means estimator, since the block variables are orthogonal to treatment assignment. To allow for heterogeneous treatment effects, we will interact the treatment indicator with the pre-specified covariates of interest (see section 10).

8.3 Covariate Selection

We will select covariates from a pool of all covariates using lasso regression (Bloniarz et al. (2016)). The list of eligible covariates is given in the appendix. Covariates will be drawn from the following sources:

- *Village and Ward-level data.* We will gather village and ward-level covariates using satellite-based economic measures, administrative data, and election results.
- *Service delivery data.* We will include binary indicators for different categories of village service delivery problems identified by radio stations before randomization: water, health, education, transportation, and other.
- *Respondent data.* We will include respondent-level information that cannot be plausibly affected by treatment.

8.4 Randomization Inference

We will calculate p-values using randomization inference (RI). We will also report standard errors clustered at the village level because villages are the unit of randomization. However, our primary method for hypothesis testing will be RI-generated p-values (5,000 permutations).

9 Outcomes and Hypotheses

Outcomes will be measured using administrative records and the endline cell-phone survey with elected officials, bureaucrats, and citizens. We will measure main outcomes, secondary outcomes, and intermediate outcomes that relate to the causal mechanisms described in theory section. For all outcomes, we are interested in actions in the previous 7 months, when the intervention began.

The primary outcomes of interest are (1) the degree to which the identified service delivery problem has been solved, and (2) pre-specified actions taken to resolve the problem. The secondary outcomes of interest are (1) village meetings to address the specific problem, (2) visits by government officials to address the specific problem, and (3) financial contributions to address the specific problem.

For all cases, responses marked 'don't know' and 'refuse to answer' will be coded as "0" since they indicate uncertainty about whether positive change has taken place. All outcomes will be coded so that a positive value is

associated with the predictions of the study.

9.1 Manipulation Checks

Although we already have data on where reports were implemented and broadcast, we do not know the degree to which politicians, bureaucrats, and citizens were aware of the reports, and how they shaped beliefs about the likelihood of *future* reports.

9.1.1 Awareness

9.1.1.1 Journalist visit

- *Hypothesis:* Treatment assignment increases the number of respondents aware of a journalist visit to their village
- *Direction:* One-tailed (Positive)
- *Sample:* Politicians, bureaucrats, citizens [1 if any are aware]
- *Question:* How many times has a journalist visited this village/street to address the XX problem in the past 7 months?
- *Note:* Coded as dummy and total number

9.1.1.2 Media report

- *Hypothesis:* The treatment effect increased the number of respondents aware of a media report about XX problem in their village.
- *Direction:* One-tailed (Positive)
- *Sample:* Politicians, bureaucrats, citizens [1 if any are aware]
- *Question:* Have you heard a radio report about an XX problem in your village in the past 7 months?
- *Notes:*

9.1.2 Future Beliefs

9.1.2.1 Perception of future media reports

- *Hypothesis:* The treatment increased the probability of a journalist visiting the village about the specified issue.
- *Direction:* One-tailed (Positive)
- *Sample:* Politicians, bureaucrats, citizens
- *Question:* How likely do you think it is that a journalist will visit your village/street to report on XX problem in the future?
- *Notes:* Not likely, Somewhat likely, Very likely

9.2 Primary Hypotheses

The core outcomes are the degree to which the pre-specified problem in the village has been solved and the degree to which government has responded to the problem. Problem resolution is the most important substantive outcome, but it is a more difficult test of theory than the extent of government response, especially considering the limited time frame between the intervention and endline data collection (approximately 7 months, and before a full budget cycle is complete).

9.2.1 Problem Resolution

9.2.1.1 Problem resolution (report card)

- *Hypothesis:* Treatment assignment increases the degree to which XX problem has been solved
- *Direction:* One-tailed (Positive).
- *Sample:* Report Card
- *Question:* "First, on a score of -10 (much worse) to 10 (fully solved), how would you say the XX problem has changed from 7 months ago to today?"
- *Notes:* Direct observation will give a raw score, a within-region rank, and a choice between matched pairs.

9.2.1.2 Problem resolved (survey)

- *Hypothesis:* Treatment assignment increases the proportion of respondents who say that the XX problem has been solved.
- *Direction:* One-tailed (Positive)
- *Sample:* Politicians, bureaucrats, and citizens
- *Question:* "To the best of your understanding, has the XX problem been solved?" IF YES: "Would you say XX problem has been solved completely, or that the response to XX problem is just beginning? IF NO: "Would you say that XX problem has started to be solved, or you don't have much hope that XX will ever be solved."
- *Notes:* Coded as dummy (yes/no) and branched outcome.

9.2.2 Responsiveness

9.2.2.1 Government actions taken

- *Hypothesis:* Treatment assignment increases the proportion of pre-specified actions taken to resolve the XX problem
- *Direction:* One-tailed (Positive)
- *Sample:* Report card, politician survey, bureaucrat survey
- *Notes:* See subsection 6.4 for details

9.2.2.2 Government response (report card)

- *Hypothesis:*Treatment assignment increases government responsiveness to XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Report card
- *Notes:* See subsection 6.4 for details

9.2.3 Alternative Hypotheses

9.2.3.1 Villager response (report card)

- *Hypothesis:* Treatment assignment increases villager responsiveness to XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Report card
- *Question:* First, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the OVERALL response by VILLAGERS to the XX problem?
- *Notes:* See subsection 6.4 for details

9.2.3.2 NGO/Private response (report card)

- *Hypothesis:* Treatment assignment increases NGO/Private business response to XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Report card
- *Question:* First, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the OVERALL response by the OTHERS (NGOs, businesses, and private citizens) to the XX problem?
- *Notes:* Includes non-governmental organizations, private corporations (such as mining companies), and private citizens (such as wealthy relatives living in the city)

9.3 Secondary Hypotheses

The secondary hypotheses related to the degree to which initial steps have been taken to address the specified problem. This include meetings, government visits, and financial contributions. We consider problem resolution the most important outcome, but recognize that it is a more difficult test of theory than the extent of government response, especially considering the limited time frame between the intervention and endline data collection.

9.3.0.1 Village meetings

- *Hypothesis:* Treatment assignment increases the probability that the village has had a meeting to discuss XX problem
- *Direction:* One-tailed (Positive)
- *Sample:* Politician / bureaucrat / villager survey
- *Question:* "In your understanding have you had any meetings in your village/street in the past 7 months to address the XX problem?"
- *Notes:* Can be a meeting dedicated to specific issue or a general meeting in which the problem was discussed.

9.3.0.2 Total government official visited

- *Hypothesis:* The treatment increased the likelihood that a government official from outside the village visits the village to discuss the specified issue.
- *Direction:* One-tailed (Positive)
- *Sample:* Politician / bureaucrat / villager survey
- *Question:* "Now I am going to read you a list of different government officials. Please tell me if they have visited the village/street to address the XX problem in the past 7 months."
- *Notes:* Government officials include Diwani, MP, DC, DED, or representative from relevant bureaucracy. Visits must include discussion of the relevant issue.

9.3.0.3 Total financial contributions

- *Hypothesis:* The treatment increases likelihood that the financial contributions are made to resolve the specific issue.
- *Direction:* One-tailed (Positive)
- *Sample:* Report card, politician/bureaucrat/villager survey
- *Question:* "Now I am going to read you a list of different people and groups. For each, please tell me how much that group has made financial (or in kind) contributions to solve the XX problem in your village/street"
- *Notes:* Financial contributions can include financial and in-kind contributions. We will calculate number of individuals who made contributions as well as the total number of calculations made (in report card)

9.4 Spillovers

9.4.0.1 Response to OTHER problems

- *Hypothesis:* Treatment assignment increases responses to problems in the village/street OTHER than XX problem.
- *Direction:* Two-tailed (see Notes)
- *Sample:* Report card, surveys
- *Question:* "On a score of -10 to 10, how would you score actions by [citizens, Government, NGO] to address OTHER problems in their village (NOT XX problem) in the past 7 months? [Water, Education, Health, and Infrastructure]"
- *Notes:* We use a two-tailed test because we are unsure whether responses to a certain village problem are a compliment or substitute to efforts on other village problems.

9.4.0.2 Response in OTHER villages

- *Hypothesis:* Treatment assignment increases responses to problems in OTHER villages in the ward.
- *Direction:* Two-tailed
- *Sample:* Diwani / WEO survey
- *Question:* "Has the government taken action to respond to XX problem in any OTHER villages in your ward in the past 7 months?"
- *Notes:* We use a two-tailed test because we are unsure whether responses to a certain village problem are a compliment or substitute to efforts in other villages.

9.5 Intermediate Responses

Our analysis of the mechanisms driving government responsiveness is exploratory. We will therefore examine responses by individual government officials (citizens village chairperson, village executive officer, ward councillor, ward executive officer, district government officials, MP, ministry officials, NGOs, other citizens), as well as with different forms of aggregation.

9.5.1 Exploratory Outcome Groupings

9.5.1.1 Levels of government

- Local government (Village chairperson, village executive officer)
- Ward government (Ward councillor, ward executive officer)
- District government (District chairperson, district executive officer)
- Member of Parliament (MP / Parliament)
- National/regional government (TANESCO, TARURA, RUWASA)

9.5.1.2 Type of electoral accountability

- Elected officials (village chairperson, ward councillor, MP)
- Bureaucrats (village executive officer, ward executive officer, Ministry officials)

9.5.2 Outcomes

Unless otherwise noted, we will measure the following outcomes for each actor:

9.5.2.1 Report card score

- *Hypothesis:* Treatment assignment increases citizen response to resolve XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Report card

- *Question:* "First, on a score of -10 (made things much worse) to 10 (totally solved the problem), how would you score the OVERALL response by YY to the XX problem?"
- *Notes:*

9.5.2.2 Citizen meeting attendance

- *Hypothesis:* Treatment assignment increases the number of citizens who have attended at least one village meeting.
- *Direction:* One-tailed (Positive)
- *Sample:* Citizen survey (checked by politician and bureaucrat survey)
- *Question:* "Has the government taken action to respond to XX problem in any OTHER villages in your ward in the past 7 months?"
- *Notes:* Citizens only

9.5.2.3 Citizen effort

- *Hypothesis:* Treatment assignment increases citizen effort to resolve XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Citizen survey (checked by politician and bureaucrat survey)
- *Question:* "In your best understanding, what proportion of villagers have attended at least one meeting to address XX problem in your area in the past 7 months?"
- *Notes:* Citizens only

9.5.2.4 Village visits

- *Hypothesis:* Treatment assignment increases ward government visits to address XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Surveys
- *Question:* "Now I am going to read you a list of different government officials. Please tell me if they have visited the village/street to address the XX problem in the past 7 months."
- *Notes:* Does not include citizens and village leaders

9.5.2.5 Financial contributions

- *Hypothesis:* Treatment assignment increases citizen financial contributions to resolve XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Surveys (confirmed with report card)
- *Question:* "In your understanding have XX contributed funds to solve XX problem in the past 7 months?" IF YES: "In your understanding, how much did XX contribute to help solve the XX problem in the past 7 months?"
- *Notes:* Some villages dropped if pre-specified issue does not require financial support

9.5.2.6 Political pressure

- *Hypothesis:* Treatment assignment increases citizen advocacy to resolve XX problem.
- *Direction:* One-tailed (Positive)
- *Sample:* Surveys (confirmed with report card)
- *Question:* "Now I am going to ask you about actions that XX have taken to pressure or request others to act to solve the XX problem."
- *Notes:* Includes whether applied any pressure and total number of others pressured

9.5.2.7 *Spillovers*

- *Hypothesis:* Treatment assignment increases responses to OTHER problems (or problems in OTHER villages)
- *Direction:* One-tailed (Positive)
- *Sample:* Report card, ward leader surveys
- *Question:* "On a score of -10 to 10, how would you score WARD GOVERNMENT actions to address OTHER problems in their village (NOT XX problem) in the past 7 months?" OR "On a score of -10 to 10, how would you score WARD GOVERNMENT actions to address problems in OTHER villages in the past 7 months?"
- *Notes:* Only ward leaders are asked about activities in other villages

9.5.2.8 *Efficacy*

- *Hypothesis:* Treatment assignment increases efficacy to solve service delivery problems
- *Direction:* One-tailed (Positive)
- *Sample:* Report card, ward leader surveys
- *Question:* "When there are problems with how local government is run in your community, how much can an ordinary person do to improve the situation?" AND "Please tell me if you agree or disagree with the following question: it is easy for an ordinary person to have his or her voice heard between elections in Tanzania" AND "Which of the following two statements do you agree with MORE: Statement 1: Citizens should be more active in questioning the actions of leaders. Statement 2: In our country, citizens should show more respect for authority."
- *Notes:*

9.5.2.9 *Support for media*

- *Hypothesis:* Treatment assignment increases efficacy to solve service delivery problems
- *Direction:* One-tailed (Positive)
- *Sample:* Report card, ward leader surveys
- *Question:* "If my village has a problem, a media report about that problem is likely to help solve the problem."
- *Notes:*

10 Heterogeneous Treatment Effects

We are interested in the following heterogeneous treatment effects.

- **Electoral competition**: Theories of bottom-up accountability suggest that the government will be most responsive to media reports in constituencies with higher levels of political contestation. (Besley and Burgess 2002; Grossman and Michelitch 2018; Raffler 2019). We therefore predict that effects of media reports will be stronger in wards that are more electorally competitive, measured by the percentage vote share for opposition parties in the 2015 election.

- **Bureaucratic Capacity**: Recent research suggests that government responsiveness is powerfully conditioned by bureaucratic capacity (Slough). To test this argument, we will investigate how the effects of media reports interact with underlying bureacratic capacity, as measured by the education of the Village Executive Officer and Ward Executive Officer.

- **Central Government Oversight**: Theories of top-down accountability suggest that media reports will be particularly influential in areas where central government capacity to oversee lower level officials is weak (Lorentzen 2014). To test this argument, we will investigate how the effects of media reports interact with central government oversight, as measured by the distance of the village from the District/Regional/National capital.

11 Threats to Inference

11.1 Attrition

In this study, attrition is measured by the inability to reach and/or conduct interviews in some villages. We do not anticipate significant attrition, although it is possible that government authorities will prevent data collection in some villages. To evaluate whether attrition is differentially related to treatment assignment, We will regress a dummy indicator of attrition on a treatment indicator and an indicator for each block, and calculate the p-value by comparing the observed test statistic to the test statistic observed under 3,000 hypothetical re-randomizations. If the p-value is greater than 0.05 or if the rate of attrition is less than 10%, we will report naive estimates across all villages.

We will test the robustness of our results by reporting trimming bounds. Trimming bounds are justified by a monotonicity assumption: no villages would be missing if they were assigned to the control group but observable if they were assigned to the treatment group. In this case, the monotonicity assumption is justified because the only factor related to treatment which would cause village-level attrition is government officials refusing approval to enumerators in response to negative media reports. It is implausible that villages in the control group which enumerators could not visit would have been available for data collection if a media report had taken place.

The estimand of the trimming bounds approach is the average treatment effect among always-takers, or respondents in villages which would be non-missing whether they were assigned to the control group or the treatment group. To calculate the ATE among always-takers, we first estimate Q, or the proportion of villages in the treatment group that would not have been observable if they were assigned to the control group. We then calculate upper/lower bounds on the treatment effect by ranking villages according to their average outcome of interest and subtracting the respondents from the lowest/highest Q proportion of villages in the treatment group and calculating the treatment effect.

11.2 Interference

There are two ways that interference between treatment and control villages could bias the estimates in this study. First, reports in treatment village could be taken as a signal to officials overseeing control villages (at the district, ward, or village level) that reports in control villages are more likely in the future, increasing the likelihood

that they respond to citizen needs in control villages. This form of interference would bias the estimated treatment effect downwards. Second, reports in the treatment village could cause Regional or District officials to take action in the treatment village, distracting them from taking action in control villages or taking up resources that could otherwise be used in control villages. This form of interference would bias results upwards.

11.3 Non-compliance

Two villages that were assigned to receive a report did not receive a report because the government prevented the reporter from visiting the village. It is possible that reports in other villages were either not broadcast twice (or all procedures involved in reporting and broadcasting were not followed. However, we consider most of these concerns unlikely, because. We will report the intent-to-treat (ITT) throughout the paper.

12 Ethics

12.1 Equipoise

The principle of equipoise holds that an experimental intervention is ethically justifiable only if there is reasonable uncertainty among researchers that any arm of the intervention may be better for experimental subjects than any other arm. Recently, MacKay has introduced the term "policy equipoise", which holds that all treatment arms should also be in a state of equipoise with the best reasonably attainable alternative intervention.

The uncertainty around the effects of media, accountability, and community empowerment programs in low-income countries suggests that the current project satisfies the equipoise standard. There are no experimental studies of the effects of investigative media reports on government responsiveness, and more general studies on the effect of transparency programs have produced mixed results (Grossman and Slough). Just as importantly, there is no consensus around better *alternative* interventions to address government service delivery failures at a similar cost and scale to this study's intervention (+/- 125 USD per village). Like media interventions, community empowerment programs and information treatments have produced mixed and/or modest results.

In short, current scholarly equipoise around the appropriate bundle of interventions to address government responsiveness is the central ethical justification for this project. A village receiving a media report is not obviously better off than a village that does not receive a media report, and neither village is obviously better off than a village that receives any other intervention attainable at a similar cost.

12.1.1 Scarcity

Even in the absence of equipoise, the study could be justified on the grounds of resource scarcity. Given a budget constraint by the radio station and research team, it would have been impossible to support media reports in all 200 villages. Even in the absence of policy equipoise, resource scarcity can justify randomization if no participant has a stronger moral claim to the intervention than other participants. We did consider the possibility that certain villages had a unique moral claim to media coverage relative to the rest of the sample based on the nature of their problem.

However, two considerations cut against this concern (apart from the equipoise argument from above). The first is that it would have been very difficult to differentiate the villages in "extreme need" from other villages in the sample in the absence of further investigation. All villages that radio stations identified expressed a strong need for government support. The second consideration is that the study design did not prohibit the village from receiving a media report treatment from the partner radio station or any other media outlet. If a village's situation proved particularly dire, there were still options for delivering the intervention, albeit outside the scope of this study.

Nonetheless, the research team was attentive to any village reports proposed by radio stations that seemed to demand urgent attention from the station, such that it would have been unethical to assign the village to the control group. In one case (a story of ongoing evictions and physical abuse of citizens by police), the research team recommended removing the village from the sample and arranged support for a media report outside the research design. The removal of this village occurred prior to random assignment.

12.2 Researcher Role with Respect to Implementation

This study is not a case of a researcher independently evaluating an intervention that would have occurred in the researcher's absence: the investigative media reports would not have occurred when, where, and how they did in the absence of the researcher's participation and financial support. This level of researcher involvement in the intervention design implies a unique ethical responsibility by the researcher to the participants in the study.

The research team spent 1.5 years reaching out to radio stations and building relationships with station managers and reporters to understand the scope of their reporting, safety and ethical concerns, and their interest in a large-scale research project. The investigative report template was developed in collaboration with media houses and journalists, and radio stations held primary control and ultimate veto power over where to conduct reports, whom to interview, and what to include the broadcasts. Radio stations controlled the sampling process and carried out the reporting without researcher interference. The research team, in turn, provided funds for transportation, per diem, and production totally +/- 125 USD per report. These funds did not cover the full expenses of the reporting, which were shared by the media houses. The research team also reviewed all episodes before they were broadcast to ensure that no content would be aired that would put the radio station, journalists, communities, or research team at risk. Ultimately, no content was censored by the research team, although some recommendations were made to interview government officials for "balance" that is legally mandated by the Tanzanian government.

It is important to note that in the absence of the researcher's involvement, the radio stations would still have reported, produced, and broadcast local news stories. However, the researcher's involvement undeniably shaped the nature of the reports in three ways. First, transportation support allowed journalists to visit villages further away from the station than they would otherwise generally visit. Second, the production support enabled reporters to carry out more thorough investigations than standard news reports. Finally, the research design meant that radio

stations produced more local news stories than they would typically produce.

12.3 Potential Harms to Participants or Non-participants

There are three categories of participants in this research project.

12.3.1 Journalists and Media Houses

The first category of participants is participating media houses and journalists. In the worst case scenario, journalists could have been harassed, arrested, or otherwise harmed by government officials or communities who did not want certain topics to be broadcast or aired. We took the following steps to minimize potential risks to media houses and journalists.

- The research team engaged in 1.5 years of planning conversations with media houses to ensure that the design of the intervention would not put them at legal risk.
- The research team agreed that all reports would include opportunities for government responses, in line with Tanzanian media law.
- The research team also agreed to exclude topics that media houses felt would invite government reprisal. These topics varied from region to region. In Mtwara Region, for example, we did not pursue stories related to sensitive issues around refugees entering the country from Mozambique.
- The research team made clear to journalists that they could abandon an investigation with no financial penalty if they felt that continuing to report would put them at risk, which they chose to do in two cases.
- The research team gave ultimate veto power over whether and when to air broadcasts to media houses.
- For all reports, the radio station invited government officials to respond on the record to citizen concerns in the report.
- The research team elected not to implement the project in the run-up to Tanzanian presidential election to avoid the perception of interfering with the electoral process.

12.3.2 Community Members in Treatment Areas

The second category of participants is community members in treatment areas. It is important to note that individual community members who elected to speak to journalists are not technically research subjects, in that no research data was collected from or about them. Nonetheless, it is easy to imagine a scenario in which the research project resulted in harm to community members who spoke with the media for the reports. For example, they could have experienced retribution from political actors or other community members for speaking out about controversial issues. We took the following steps to minimize potential risks to community members who collaborated with journalists and media reports:

- No community member was forced to participate in an investigative report or was named in the report without their consent.
- Any community member was free to request that their contribution be considered off the record so that their name and identifying information was not associated with the reports
- All community members were given the contact information of participating journalists and allowed to follow up if had concerns of questions about the reports.

- The research team was in weekly contact with every media house for updates on any threats to participant safety. There were no reports of adverse consequences for participating community members.

It is also plausible that the reports may have led to government responses that left community members in participating villages worse off, whether or not they were interviewed by journalists. Uncertainty around the net effects of the reports on treatment communities is part of what generates equipoise, discussed in the sections above. If we had observed systematic negative consequences for participating communities, we would have closed the project. However, no such consequences were observed.

12.3.3 Government Officials in Treatment Areas

The third category of participants is government officials in treatment areas. The actions or inactions of government officials were often the subject of media reports. Reports may therefore have had negative consequences on the careers of elected officials and bureaucrats, although more severe harm was deemed unlikely and was not observed during the research period.

We ultimately deemed negatve career outcomes for government officials an acceptable negative risk, in line with interventions to promote accountability of bureaucrats and elected officials. In line with Tanzanian media law, government officials at the village, ward, and district level were in all cases invited to respond to media reports (on or off the record) and invited to follow-up with the media house after the report was broadcast.

12.3.4 Community Members in Control Areas

The final category of individuals potentially harmed by the project is community members in control areas. There are two plausible pathways by which individuals could be harmed. First, if a given region has limited financial resources, media reports in one village could have resulted in the diversion of resources from control villages towards treatment villages (also a violation of the non-interference assumption). Second, if media houses were focused on treatment areas, they may have ignored important social concerns in control areas to the detriment of communities in those areas.

The research team took several steps to avoid these concerns:

- The research team explicitly informed media houses that they were *not* restricted from reporting on issues in control areas. This avoided the risk that serious harms occuring in control areas would be ignored.
- The research team conducted the study *after* the budget process was underway. This meant that the primary focus of the intervention was on mobilizing action by at the Ward level (the unit of randomization) rather than the District or Regional level (where budget tradeoffs would be more likely to negatively impact control areas.

Nonetheless it is impossible to discount the possibility that media reports on one area may result in less attention on other areas. On the other hand, it is possible that media reports on one area may have increased attention on other areas as political actors sought to preempt the possibility of future reports. This uncertainty is part of what motivates and ethically justifies the research design.

12.4 Potential Harm from Data Collection or Research Protocols

12.4.1 Informed Consent for Randomization

Because of the nature of the intervention, community members were not given informed consent over the randomization process. As described in the section above, however, consent was implied by voluntary participation in the reports in much the same way that consent is implied by voluntary participation in community meetings during community development interventions

12.4.2 Informed Consent for Interview Participation

Data collection was conducted independently of the intervention and the enumeration team was blind to the treatment status of villages. All subjects will be provided with both written and oral explanations of the consent process and given the option to refuse participation. All data will be collected on password protected tablets using SurveyCTO/Open Data Kit software. Upon completion of the the survey, survey data is synched to an encrypted SurveyCTO server and downloaded to a laptop held by the research team and encrypted using Boxcryptor.

12.4.3 Community Consent

Survey enumerators will secure consent from Regional, District, Ward, and Village officials before conducting any surveys. This consent is both legally mandatory and ensures the safety of enumerators and research participants.

12.5 Potential Harms to Field Staff

Because of the geographic coverage of the study, field staff will be required to travel long distances, sometimes on public transportation or motorcycle. The research team takes the health and safety of staff extremely seriously. All staff are provided with medical insurance in case of accidents. All staff are provided per diem sufficient to cover overnight stays in safe lodgings so that they do not need to travel at night. All staff are paid salaries in compliance with Tanzanian employment law and their salaries are reported to government.

Additional risks introduced by the COVID-19 pandemic are discussed in an independent section below.

12.6 Financial or Reputational Conflicts of Interest

The research team reports no financial or reputation conflicts of interest in this study.

12.7 Intellectual Freedom

The research team reports no contractual limitations to reporting the results of the study. The Tanzanian government requires that any research be submitted to their research compliance office before publication, but there are no restrictions on what findings can be published.

12.8 Feedback to Participants or Communities

Because this study involves collaboration with radio stations, there is a in-built opportunity for sharing research findings with participating communities. Radio stations have agreed to broadcast reports about the findings, which

will enable treatment, control, and non-sampled communities to hear about the results.

12.9 Foreseeable Misuse of Research Results

The most plausible misuse of research results is that the ruling party could crack down on media houses that have reported negatively against the government. While this is not out of the question, it is implausible that the research *findings* would increase the probability of this outcome because the reserach partnership and the reports themselves are already publicly known.

Depending on the findings, the ruling party may also decide that media represents a greater threat to its electoral advantage than it thought and use the research as a pretext for media crackdowns. However, the outcomes of interest in this study make that outcome unlikely. The research is intended to identify the effects of media reports on development outcomes, not gains or losses to CCM.

12.10 COVID-19

This project was implemented and data was collected in the midst of the COVID-19 pandemic. The research team takes its responsibilities related protection of research staff and subjects seriously. We obtained approval from Columbia University and Innovations for Poverty Action COVID-19 review board to carry out the data collection, and designed transportation and data collection procedures with COVID-19 risks in mind. Shortly before the start of endline data collection, the Omicron variant of COVID-19 was discovered, so endline data collection was converted to phone interviews.

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