Dictating Development?
The Political Economy of Rwanda's Fertility Transition*

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Abstract

We examine the impact of state controlled community meetings on health behavior changes in Rwanda, leveraging a policy reform that aligned local leader objectives with those of the central government. In 2006, the Rwandan government began incentivizing local leaders to promote a stigmatized health behavior: use of modern contraceptives. Community meetings are one arena where leaders meet their constituencies. Leveraging quasi-experimental variation in meeting attendance due to local weather fluctuations, we find that after this reform, non-rainy Saturdays – a proxy for a successful meeting – increase the likelihood of women adopting contraceptives by 18% within the same month. Rainfall before the reform or on other weekdays show no relationship with adoption. The same pattern holds for bednets, another incentivized but otherwise unrelated outcome, suggesting the new performance incentives for local leaders as the underlying common factor. We also provide suggestive evidence that these behavioral changes were involuntary. Our findings shed new light on Rwanda's remarkable health development indicators and challenge the notion that local institutions primarily enhance downward accountability through bottom-up pressure, highlighting instead their role in facilitating top-down monitoring, and an interdependence with performance incentives.

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1 Introduction

Who monitors whom in successful local institutions? Local institutions, such as community meetings and assemblies, can play a vital role for policy implementation and political participation in developing countries (see e.g. Besley et al., 2005; Olken, 2010; Faguet and Pal, 2023; Pradhan et al., 2014). The success of these institutions is often credited to the participation of ordinary citizens who hold their leaders accountable, ensuring *downward accountability*. Correspondingly, failure of local institutions to implement development programs is usually attributed to insufficient downward accountability (Banerjee et al., 2010; Björkman and Svensson, 2009; Björkman Nyqvist et al., 2017; Casey et al., 2012). However, development issues are not always a matter of leader performance. Often, the individuals for whom programs are designed fail to express sufficient demand. In such cases, local institutions may promote policy implementation by enabling leaders to monitor and affect community behavior, thereby strengthening *upward accountability*. This paper seeks to uncover whether downward or upward accountability is more important for the success of local institutions, particularly when leaders and community members have differing objectives regarding outcomes. To the best of our knowledge, this study is the first to approach accountability in local institutions in this manner.

In this paper, we study the Rwandan institution *Umuganda*. Umuganda meetings occur at the village level across the country on Saturdays and are part of a traditional community program. In these meetings, that are attended by one person from each household, local leaders disseminate information from central authorities and lead discussions on local issues. We combine plausibly exogenous variation in meeting attendance with the launch in 2006 of a reform that dramatically altered incentives for local leaders to push for certain behavior changes in the health domain. More specifically, the adoption of modern contraceptive methods and the acquisition of mosquito bed nets were highly desired by the central government but were unpopular among significant segments of the population, and the reform created a sudden change in objectives between local leaders and their constituencies. Comparing the effect of meetings on these health practices before and after the reform allows us to make progress on understanding the direction of accountability facilitated by these meetings.

Rwanda provides an interesting case for studying the direction of accountability in local institutions. During the second half of the 2000s, the country achieved remarkable progress towards the Millennium Development Goals. Notably, the prevalence of modern contraceptive use and the distribution of mosquito bed nets more than quadrupled between 2005 and 2010, marking some of the fastest improvements globally (Abbott et al., 2017; UNDP, 2014). This success is widely attributed to Rwanda's robust local institutions and particularly the *Umuganda* program (UNDP, 2014) that are generally regarded as enablers of bottom-up organization and pressure. However, Rwanda is also characterised by its top-down authoritarian leadership. It is important to note that many local institutions globally, even within democratic states, contain authoritarian elements. Thus, this research holds relevance for enhancing our understanding of decentralised development also beyond the Rwandan context.

We identify the effects of Umuganda on behavior changes using monthly panel data from a large, representative sample of women and households in Rwanda: the DHS. To establish causality, we leverage random variation in rainfall within villages over time. Our identifying assumption is that Umuganda attendance decreases with Saturday rainfall, as these meetings are held outdoors, and existing research has established rainfall effects on attendances in other contexts (see e.g. Madestam et al., 2013; Fujiwara et al., 2016; Collins and Margo, 2007). We use the number of "rainy" Saturdays in a month to proxy for the number of *Umuganda* meetings that suffer from low attendance in that village the same month. Rainy day counts for other weekdays serve as controls and natural placebo tests. This is an approach similar to that of Bonnier et al. (2020)[1]. Two pieces of evidence support this attribution. First, both health practices were evidently discussed at meetings (MoH, 2008, 2009). Second, we can rule out the alternative channel that Saturday rainfall increased access to the targeted health technologies, because health facilities – the only source for the most common methods that drive effects, were then closed (see e.g. Ueberschär, 2018).

We next link these effects to the control exerted over Umuganda. In April 2006, Rwanda introduced top-down performance contracts (*Imihigo*) at all administrative levels, instituting

[1] Bonnier et al. (2020) use cross-sectional data to estimate reduced-form *Umuganda* effects (Saturday rainfall effects) on civilian participation in the 1994 Rwandan genocide. In contrast, we exploit variation within observation units over time.

strong incentives for local leaders to meet predefined targets, such as increasing uptake of contraceptive methods and mosquito nets.[2] This reform created a sudden shift in local leaders' objectives to align with those of the central government. We compare the effects of *Umuganda* before and after the introduction of performance contracts. Any change in effects reveals leader's control over *Umuganda* because contracts changed their incentives first.[3] The practice of *Umuganda*, health care provision, and national development goals remained unchanged in the same period.

We find that in the first year after the introduction of performance contracts one failed *Umuganda* meeting in a month significantly reduces the likelihood of government-desired behavior changes within the same month. The relative effects are considerable: -18% for contraceptive adoption and -10% for bed net acquisition. In contrast, Saturday rainfall has no significant impact on neither contraceptive adoption nor bed net acquisition in the year preceding the performance contracts. Results are robust to different rainfall thresholds and rainy day counts for other weekdays are consistently insignificant. We are unaware of any simultaneous nationwide reform or shift that would affect these both outcomes. The results suggest that performance contracts turned *Umuganda* meetings into effective tools for enforcing central government mandates, a finding in line with them increasing upward accountability.

To understand whether this effect is likely to have operated through increasing upward accountability we search for signs of enforcement – a key component of upward accountability. First, we analyze the heterogeneity in *Umuganda* (Saturday rainfall) effects due to differences in popular support for mosquito bed nets. We exploit the inverse relationship between mosquito prevalence and altitude, finding that the impact of *Umuganda* meetings is more pronounced in high-altitude communities where mosquitoes are scarce and support for bed nets is low. This suggests that meetings induce involuntary behavior modifications regarding bed net use. Second, we study the effect of *Umuganda* on conception, i.e., the timing when women become

[2]Our decision to study behavior in an an event study of changes follows from performance indicators being defined as monthly flows, not levels.

[3]Technically, answering the question of who controls *Umuganda* only requires performance contracts being imposed top-down on leaders, which was the case in the Rwandan context. Meanwhile, evidence of popular opposition to contraception and mosquito bed nets is a prerequisite for upward accountability (through top down pressure) being the driving force behind any observed change.

pregnant. If meetings increase the adoption of modern contraceptive methods, conception rates should drop eventually. However, some women may expedite pregnancy if they feel pressured to otherwise adopt contraception, potentially leading to simultaneous increases in both contraceptive use and conception. Evidence supports this: while we saw that rainy Saturdays (missed meetings) led to less contraceptive adoption we also find that rainy Saturdays correlate with an 8% reduction in conception rates, suggesting significant enforcement in contraceptive adoption during the meetings. Notably, knowledge of modern contraceptive methods and where to obtain them was high throughout the study period, indicating that meetings likely did not provide new information.

The paper contributes to three separate literatures. First, it speaks to the literature on local institutions as instruments for development. Experimental work in this field attempts to empower citizens in project implementation through information and training (Olken, 2007; Björkman and Svensson, 2009) or through changes to participation structures (Beath et al., 2017; Olken, 2010). However, it is well known that elites often control or capture local institutions (Ban et al., 2012; Parthasarathy et al., 2019; Reinikka and Svensson, 2004; Anderson et al., 2015). This implies that such institutions are potential arenas for top-down monitoring and pressure on citizens. A growing literature on the "dark side" of local institutions examines how these bodies can promote destructive policies (Satyanath et al., 2017; Acemoglu et al., 2014). Bonnier et al. (2020) study the same institution as this paper, *Umuganda* meetings in Rwanda, in a different context and time period and find suggestive evidence that meetings before the 1994 genocide were used for propaganda resulting in more civilian perpetrators. While the health outcomes we study are not as negative, we relate to this literature by exploring how local institutions enable leaders to push unpopular policies. Similar to Acemoglu et al. (2014) and Bonnier et al. (2020), we show that participation in local institutions fosters leaders' control and influence over community behavior. This also relates to research on social sanctions (La Ferrara, 2003; Karlan, 2007; Miguel and Gugerty, 2005). Our findings challenge the common assumption that local institutions inherently promote bottom-up pressure and increase downward accountability.

The paper also speaks to the literature on performance incentives in public administration (Finan et al., 2017). Performance incentives generally strengthen upward accountability. Sim-

ilar to the literature on local institutions, some research documents strong improvements in outcomes under performance incentives (Ashraf et al., 2014; Gertler and Vermeersch, 2012; Leaver et al., 2021), while other research finds only small, temporary effects (Celhay et al., 2018; Rasul and Rogger, 2018; Olken et al., 2014). Performance incentives can also have negative side-effects. For example, Khan et al. (2015) finds that performance incentives increase bribes to tax-collectors as it strengthens their bargaining power over taxpayers, and Dhaliwal and Hanna (2017) indicate that monitoring lowers job satisfaction and leads to evasion. Our findings show that incentives may encourage local leaders to implement even unpopular policies that include restrictions of personal freedom. By jointly studying accountability and local institutions, we also connect the literature on performance incentives with that on local institutions. Both of these strands attempt to solve the same problem of accountability in delegated tasks. Our findings indicate that local institutions and performance incentives can act as complements rather than substitutes.

Finally, our findings contribute to the literature on health policy implementation in developing countries. They provide new insights into Rwanda's success story in achieving the Millennium Development Goals in health. Previous work by Abbott et al. (2017) discusses the sustainability of Rwanda's rapid improvement in these indicators concerning future aid funding. Our study, however, sheds light on the channels through which this impressive progress was attained, and therefore speaks more broadly to how incentives for public officials might affect health technology adoption.The economics literature on health technology adoption typically focuses on overcoming constraints related to information, liquidity, and risk (Dupas, 2011). Incentivizing health workers or public officials can help close information gaps about the existence or usefulness of a product (Ashraf et al., 2014) or improve access to technologies and treatments (Björkman and Svensson, 2009). In our context, neither information nor access was a constraint, making the case for incentivizing local officials less clear. Persuasion and pressure may be their primary tools to drive behavioral change. Previous research has shown that incentivizing reductions in fertility can lead to coercive and unethical behavior. This can result in later underinvestment in health or political backlash driven by mistrust (León-Ciliotta et al., 2023; Pelras and Renk, 2023).

The rest of the paper is structured as follows. Section 2 provides background information on *Umuganda* as a local institution, on performance contracts, and on development and popular support of targets. Section 3 proposes a conceptual framework. Section 4 describes the data and its construction. Section 5 explains and discusses the empirical strategy. Section 6 presents our main results, and section 7 discusses the potential mechanisms behind our findings. Finally, section 8 concludes.

2 Background

2.1 Goals, Popular Opposition, and Development

In April 2004, Rwanda's central government set ambitious goals in family planning and malaria prevention as part of its 5-year Health Sector Strategic Plan. Aligned with the UN's Millennium Development Goals, these aims included increasing modern contraceptive prevalence from 4% to 20% and raising the percentage of children sleeping under bed nets from 18% to 70% by 2010 (MoH, 2004). The primary motivation for both goals was arguably economic development. At the time, international development agencies convinced Rwanda's government that poverty reduction required reduced fertility (Solo, 2008) and granted large financial support for bed net distributions, e.g. through the U.S. President's Malaria Initiative and the Roll Back Malaria Partnership.

Many Rwandans, however, opposed these goals. Contraceptive use contrasted with strong pro-natalist norms following the 1994 genocide, which valued large families (Solo, 2008; Westoff, 2013; Kraehnert et al., 2019) and stigmatized contraception users (Berry, 2015; USAID and MoH, 2002; Farmer et al., 2015).[4]. Meanwhile, Rwanda's geography suggested limited need for bed nets as much of the population lives at altitudes where mosquitoes are rare (Bodker et al., 2003).

After the first two years of the policy, in March 2006, the government assessed that 'Up

[4]2005 DHS data reveals that 25% of men believed that women become promiscuous when using contraception, with the percentage being higher among young men. In addition, 45% of women under 30 quoted pronatalism or opposition as their reason for not using contraception while only 3% of women reported knowledge, access or cost as a reason (NISR and Macro, 2006)

to now there have been very few achievements [in family planning] in part due to a lack of advocacy at all levels of Government and civil society' (MoH, 2006, p.16). New performance contracts introduced in April 2006 incentivized local leaders to, among other things, advocate for adoption of modern contraception and bed nets.[5] By 2010, Rwanda had surpassed its targets, raising modern contraceptive use to 25.2% and increasing household bed net ownership to 83%. There was also a decline in fertility from 6 to 4 children per woman. (UNDP, 2014) Rwandan policy-makers attribute this fast-track development to performance contracts (Scher and MacAulay, 2010). We will investigate this claim quantitatively by combining data from before and after the performance contracts with data from the institution *Umuganda*, described in the next subsection.

2.2 *Umuganda*

Umuganda is a traditional, local institution in Rwanda with precursors dating back to pre-colonial times.[6] The institution, consisting in community level meetings and work on public projects, was a nationwide policy between 1973 and the early 1990's. It was suspended by the incoming government after the 1994 genocide, only to be reintroduced again in 1998 with the goal of boosting socio-economic development (MINALOC, 2011; RGB, 2020). Since then, *Umuganda* was formalized in three stages. In November 2001, it was integrated into the government's Community Development Policy. In June 2005, its organization was harmonized by the National Umuganda Policy (MINALOC, 2008). And finally, on November 17, 2007, *Umuganda* became a law (Organic Law N° 53/2007). The purpose of these policies was to embed the existing practice of *Umuganda* as a tool for policy-making into the public administrative structure. For our analysis, it is only important that no policy changed *Umuganda* in 2006.

Umuganda is a mandatory community program for all Rwandan adults, held on Saturdays. It is organized by a committee of village chiefs and consists of outdoor, physical labor (e.g.

[5] OSSREA (2007) compiles district level targets and Sommers (2012, Appendix) presents a village leader's contract for the first year of performance contracts (2006-07). Further information on district targets between 2009 and 2013 can be found in RGB (2014).

[6] Similar local institutions are common in countries of the African Great Lakes Region, notably Burundi, Ethiopia and South Sudan, and have also have been proposed for other countries. In addition, mandatory community programs also existed in many Soviet countries. In Russia, an equivalent institution was called *Subbotnik*, derived from the word 'subbota' meaning 'Saturday'.

clearing bushes or cleaning roads) followed by a meeting (Uwimbabazi, 2012).[7] The local leaders typically announce *Umuganda* on the same day through word of mouth and loudspeakers mounted on cars (RGB, 2014). During *Umuganda*, all shops must close and public transport stops. To enforce participation, local leaders have the discretion to fine absence by up to 5,000 Rwandan Francs, roughly 9 USD in 2007 and corresponding to half the monthly median wage (MINALOC, 2007). Evidence suggests that many Rwandans participate involuntarily in *Umuganda* (Mukarubuga, 2004; Uwimbabazi, 2012; Purdekova, 2011).

We argue that the meetings held after the physical labor during *Umuganda* affect behavior change. While these meetings are are meant to be a place for local leaders to mobilize, sensitize and support the population to collectively define and resolve their economic and social problems (MINALOC, 2008), in practice they often amount to local leaders communicating top-down information about government programs and policies (Uwimbabazi, 2012). They are also officially acknowledged by the government as a tool to implement development targets from performance contracts (RGB, 2014). During our period of interest in this paper, annual reports of Rwanda's Ministry of Health document that both family planning and mosquito bed nets were regularly discussed and promoted at *Umuganda* meetings (MoH, 2009, 2008).

Some dissent exists about the frequency of *Umuganda* since its reintroduction in 1998. Organic Law N° 53/2007 states that it takes place only on the last Saturday of the month (MINALOC, 2007), and most official documents follow this representation. In contrast, research suggests that *Umuganda* is, in fact, still held every week, as before the genocide (Purdekova, 2011; Uwimbabazi, 2012; NAR and Interpeace, 2016). This frequency of *Umuganda* is also reported in the 2008 revised Community Development Policy (MINALOC, 2008). In our main specification, we exploit variation from rainfall on all Saturdays in a month. For robustness we also isolate and explore the effects of specific Saturdays, such as the last Saturday of every month.

[7] *Umuganda* is often translated as "community work". This translation, however, distracts from its strong social component, the meeting, which is key to our findings.

2.3 Performance Contracts

In 2006, Rwanda's president, Paul Kagame, introduced Performance Contracts (*Imihigo*) in the public sector and beyond. On April 4, he signed the first contracts with all 30 district executives (mayors) to retie local government to central authority after decentralization. Targets were then passed down through cascading contracts to all levels of public administration and down to the individual household (MINALOC, 2010; Purdekova, 2011). Scher and MacAulay (2010) write that *Imihigo* had its roots in a pre-colonial cultural practice where leaders or warriors would publicly vow to achieve certain goals, and would face public humiliation if they failed to meet them. In the new, institutionalised version of *Imihigo*, mayor's vows and evaluations would be broadcast on radio and TV and mayors and other lower officials would risk humiliation and losing their job following bad performance on the set goals.

While *Imihigo* goals are set separately for each district and the process is officially portrayed as a system that reflects local priorities, ample evidence suggests that performance contracts set most targets top-down. In the first year after their introduction, three quarters of districts' targets were national policies and programs (GoR, 2008). Similar evidence exists at the household level. For example, in the government's 2010 Citizen Report Card survey, 78% of respondents state that they have not participated in formulating the targets set forn their communities (Munyandamutsa, 2011). The health targets in focus in this paper were an important part of *Imihigo* contracts OSSREA (2007).

Two features of Rwandan performance contracts make them particularly effective for fast-track policy implementation. First, comparable units (e.g. districts) are regularly ranked against each other. And second, contracts set very strong social and material incentives for relative performance.[8] The consequence is a rat race in which leaders try to outperform one another. Local leaders implement village targets by letting household heads vow contributions in front of the community during *Umuganda* meetings (Bugingo and Interayamahanga, 2010). Pledges are then recorded in a household's *Imihigo Booklet* and stamped upon completion. Stamped

[8] Within administration, rewards are commonly financial bonuses and promotions, whereas sanctions consist of removals from office and public shaming (Murray-Zmijewski and Gasana, 2010). At district level, for example, approximately 75% of Mayors were removed from office between 2007 and 2009 due to poor performance (Scher and MacAulay, 2010).

booklets serves as proof of "good standing" and are necessary to access certain government services like registering a marriage or birth (Sommers, 2012; Uwimbabazi, 2012). Quantitative evidence suggests some degree of compulsion in the implementation of performance targets at the local level (OSSREA, 2007) and there are even reports of fining, destruction of property and corporal punishment for refusing to contribute to fulfilling the *Imihigo* targets (Thomson, 2008; Huggins, 2009).

3 Conceptual Framework

A variety of different mechanism of *Umuganda* could, in principle, affect individual behavior. Each of these can be classified by the mode of behavior change that it generates. The two modes, which we distinguish, are voluntary and involuntary behavior change.

Voluntary behavior change can result from support or 'sensitisation' without enforcement, whereas involuntary behavior change also requires enforcement or threat thereof.

The previous section reveals that there is substantial evidence of pressure resulting from performance contracts. If performance contracts operate through *Umuganda* we can expect to also find evidence of pressure in the mechanism of *Umuganda*. In the analysis in section 7, we explore whether *Umuganda* generated involuntary behavior change. Specifically, for contraceptive use, we examine the idea that a mechanism based on pressure is likely to produce evasive behavior, and for bednet adoption we study spatial heterogeneity in popular support (and need) for this technology.

3.1 Target Implementation through Community meetings

Local leaders use the *Umuganda* community meetings to communicate government policies and moderate a collective implementation process. That means, a community's development targets are not negotiable, but the community can decide how it implements them. As a consequence, achieving a target can be conceptualized as the provision of a public good because leaders hold the entire community accountable for failures in implementation. Individuals contribute if they choose a behavior which supports the community in reaching the target. While the targeted be-

haviors, which are analyzed in this paper, were potentially beneficial for some people, they were certainly costly for others. Acquiring mosquito bed nets was most likely perceived a waste of resources in areas without mosquitoes (approximately one third of Rwanda), and contraceptive use was strongly stigmatized at the time of introduction of performance contracts.

If targeted behavior was costly, why did households and women choose it? In the literature on the private provision of public goods, punishment is an effective measure to ensure cooperation (see e.g. Fehr and Gachter, 2000). The evidence (discussed in the previous section) suggests pressure or compulsion also in the present context. With pressure, households and women change their behavior and comply with targets if the (expected) cost from sanctions exceeds the cost of the behavior. We will discuss this proposed operating mechanism of community meetings later with the results in section 6.

To achieve targets, i.e. provide the public good, a community arguably needs to follow two-steps. First, the community needs to select a sufficient number of contributors. And second, contributions must be monitored and, if necessary, enforced. *Umuganda* assumes both of these functions, selection and monitoring.[9] This organization of "contributions" is a continuous process for two reasons. First, selection and monitoring must be applied consecutively with some time in between because changes in behavior can only be promised. And second, new contributors had to be presented every month for the reporting of development progress. In this sense, any canceled or 'ineffective' *Umuganda* meetings hampers target implementation and reduces the overall number of new contributors in a month. Consequently, we analyze the effect of *Umuganda* meetings on changes in behavior.

4 Data

In this section, we describe the panel data on outcomes and rainfall. For the analysis, this data is matched through GPS coordinates and time. The panel data on outcomes of behavior change are constructed from retrospectively collected information, using dates and times that have been

[9]Selection takes the form of requiring family pledges during the *Umuganda* community meetings. A local policing unit is also formed during *Umuganda* to 'monitor inappropriate behavior in the community' (Uwimbabazi, 2012, p.67).

reported in cross-sectional Demography and Health Surveys (DHS) from Rwanda.

4.1 Family Planning

We use information from the 2010 Rwandan DHS to study women's adoptions of modern contraceptive methods and conceptions, i.e. when they become pregnant. The 2010 DHS interviewed 13,413 women who were between 15 and 49 years old and usual residents in 492 different communities. An integral part of each woman's questionnaire was a monthly calendar stretching from January 2005 to the date of the interview. In this calendar, interviewers recorded times of pregnancy and modern contraceptive use through retrospection. To ensure accuracy of the information, interviewers were required to ask a set of different questions in a recursive routine for each entry.

We construct our panel data on family planning outcomes from this retrospective calendar data. We start by buildin a panel data set indicating whether a woman is pregnant, using modern contraception or neither. We then define the two outcomes of behavior change in family planning, contraceptive adoption and conception. These outcomes take the value 100 for behavior change in a given month and 0 otherwise, facilitating the reading of estimates as percentage points later in the analysis.[10]

The coding of contraceptive adoption of a modern method is straightforward. Contraceptive adoption is an indicator that takes the value 100 on each start date of modern contraceptive use in the calendar and is 0 otherwise. For the definition of modern contraceptive methods, we follow the standard DHS classification.

The coding of conceptions is somewhat more involved. In an interview, months of pregnancy are recorded in a calendar. To record a pregnancy spell in the calendar, interviewers mark the monthly date of birth or termination and write back the status of pregnancy until the sum of marked months equals the number of completed months reported by the woman. As a consequence, pregnancies that end, for example, with birth generally consist of spells of 9 months in the calendar. However, this way of inferring the date of conception can result in inaccuracies in the date by up to two months, due to two different "rounding errors":

[10]Following DHS sampling rules, we set all information to missing for times when a woman is below age 15.

First, recording the month of birth or termination as a full month of pregnancy implicitly results in the (highly unrealistic) assumption that births or terminations occur on the last day of the month. This way of counting can lead the recorded date of conception to be off by up to one month. Thus, for most women, the recorded start of pregnancy in the calendar is between 1-30 days later than actual conception. Second, recording only completed months misses month 0 of a pregnancy, which is the month of conception. Around the world, pregnancy is counted to last 40 to 41 weeks, starting on the first day of the last menstrual period. This duration translates to 10 months with the fertilization occurring within 1-3 weeks after starting to count. In this respect, the decision to attempt pregnancy in that month actually takes place another month before the start of pregnancy is recorded in the calendar. Based on these two reasons, we code conception as an indicator that takes the value 100 two months before the start of every pregnancy in the calendar and 0 otherwise. For pregnancies that end in birth, this definition is largely identical to lagging an analogue date-of-birth-indicator by 10 months.

For the main analysis, we use April 2006, which is the introduction date of performance contracts, to split the data into a Before and an After panel data set in order to separately estimate the before and after effects of *Umuganda* meetings in our analysis. We restrict the two panel lengths to 12 months before and 12 months after the introduction of performance contracts. Narrowing the time window around the introduction of performance contracts supports the attribution of a change in effects to performance contracts.

Figure 1 presents the number of contraceptive adoptions and conceptions on each monthly date from February 2005 until July 2010. The solid black, vertical line marks the introduction of performance contracts at the beginning of April 2006. The grey shaded areas left and right of that line mark the lengths of the two panel data sets. No suspiciously high concentrations on certain dates can be observed for any of the two outcomes, suggesting that the calendar data is indeed accurate.[11] Table 1 presents summary statistics of the outcomes for the before and after panel data. Only women with at least two observations are kept in each data set because the inclusion of women fixed effects in our regressions drops women with only one observation.

[11] This observation is also confirmed when plotting the number of contraceptive adoptions and conceptions over months before the interview (see Figure **??** in the Appendix).

4.2 Mosquito Bed Nets

We use information from two rounds of the Rwandan DHS to study households' acquisitions of mosquito bed nets before and after the introduction of performance contracts. The 2005 DHS interviewed 10,146 households living in 456 communities with available GPS coordinates. The 2007-08 DHS interviewed 7,287 households in 246 geo-coded communities.[12] Both surveys collect information about mosquito bed nets in households. For each bed net acquired in the past three years, the data records the number of months before the interview when a household obtained the net. In addition, information about the source of the bed net, i.e. from where it was obtained, is available for nets acquired within the previous six months before the interview.

Figure 2 presents the raw data of the total number of mosquito bed nets that were acquired in each month before the interview. The figure shows high numbers of acquisitions on months 12, 18 and 24 in both surveys, which indicates that reporting precision deteriorates for bed nets acquired 12 months and more before the interview. Without a routine of questions to ensure data accuracy, similar to that used for 2010 DHS calendar entries, the concentrations likely are due to rounding and imprecise recall. For this reason, we restrict our analysis to bed nets acquired in months 0-11 before the interview.

Based on the raw data, we construct for each DHS a separate, retrospective, household level panel data set spanning 0-11 months before the interview. The 2005 DHS provides data before the introduction of performance contracts and the 2007-08 DHS provides data for the time thereafter. Our main outcome is an indicator that takes the value 100 if a household acquired one or more mosquito bed nets in a given month before the interview and 0 otherwise. To later explore heterogeneity in the source of bed nets, we create two additional outcome indicators of panel length 0-6 months for the 2007-08 data. The first indicator takes the value 100 if at least one bed net in a month before the interview was acquired at a Health Center and is 0 otherwise. The second indicator takes the value 100 if at least one bed net in a month was acquired from Other Sources (e.g. a shop or market) and is 0 otherwise. Table 2 presents summary statistics of the panel data on acquisitions of mosquito bed nets.

[12] The analysis samples exclude 126 households from 6 communities without GPS coordinates in DHS 2005 and 90 households from 3 communities without GPS coordinates in DHS 2007-08.

4.3 Rainfall

We construct our rainfall measures from CMORPH rainfall estimates of the US National Oceanic and Atmospheric Administration's Climate Prediction Center. This data starts in 1998 and has two advantages. First, it has very high spatial and temporal resolution that captures the rainfall variations of Rwanda's many different micro-climates. A tile (data point) in the gridded map has a side length of approximately 8 km (0.073 degrees) and measures rainfall for a 30 minutes interval (Joyce et al., 2004). This resolution facilitates the confinement of rainfall to local communities and single days, with Saturday being the day of *Umuganda* meetings. Second, validation studies suggest that CMORPH rainfall estimates are particularly precise over complex terrain like Rwanda due to the morphing of satellite images and the exploitation of both infrared and microwave electromagnetic radiation (see e.g. Abera et al., 2016). While there will always be some measurement error in satellite rainfall data, this error should work against our findings as long as it is uncorrelated with the outcomes.

We construct our rainfall measures in two steps. First, we aggregate the data to daily estimates and extract rainfall in each community based on its GPS coordinate. Second, we create rainfall measures for each weekday (Mondays, Tuesdays, Wednesdays, etc.) that count the number of "rainy" days on that weekday in a month. A rainy day is defined as a day with rainfall above a certain threshold, and a month is either a calendar month or a month before the interview, depending on the time structure of the outcome data to be matched with. We use round number thresholds from 1 mm up to 10 mm rainfall. Our preferred threshold choice is 3 mm rainfall, which we discuss in section 5.1. Measures with other thresholds are used to evaluate the robustness of our results to that choice.

For the analysis, rainfall data are matched to outcome data using community and month identifiers. Table 3 presents summary statistics of the number of rainy Saturdays in a calendar month for the 492 communities of the 2010 Rwandan DHS between April 2005 and March 2007. The statistics for rainfall on other weekdays and time periods are very similar.

5 Empirical Strategy

To identify the effect of *Umuganda* meetings on behavior change, we use variation of rainfall over time. As meeting attendance data is unavailable, we follow Bonnier et al. (2020) and estimate the reduced-form relationship. Rainfall on Saturdays proxies for low participation and cancellation (zero participation). This identification strategy rests on two assumptions. First, Saturday rainfall affects participation at *Umuganda* within a community over time (first stage). Second, the reduced-form effect of Saturday rainfall on behavior change operates only through its effect on *Umuganda* meetings.

5.1 Rainfall and *Umuganda*

Saturday rainfall should strongly affect *Umuganda* because meetings and physical labor happen outside under the open sky and, during the studied time period, are usually only communicated on the same day through word of mouth or loudspeakers mounted on circulating cars (RGB, 2014). Hence, both the program and the information about it (e.g. the meeting point and time) are likely inhibited by rainfall.

Several other studies document and exploit a negative relationship between rainfall and attendance at different types of events (see e.g. Fraga and Hersh, 2010; Madestam et al., 2013; Moreno-Medina, 2019). Closest to this paper is Bonnier et al. (2020) who study the relationship between *Umuganda* meetings in the early 1990's and later civilian involvement in the genocide. To substantiate a correlation between Saturday rainfall and cancelled meetings, they collected anecdotal evidence in the form of government and media reports on low participation at and cancellations of *Umuganda* meetings and other public events due to rainfall. [13] [14] Similar to Bonnier et al. (2020), we expect threshold effects, meaning that *Umuganda* is disproportionally affected or fails due to small dips in participation. This is supported by qualitative accounts

[13] Note that Bonnier et al. (2020) use a different rainfall dataset than this paper as they study an earlier time period. Differences may exist between theirs and our data as rainfall products have improved with time due to better satellite imagery.

[14] Most other studies use continuous measures of rainfall. For example, Collins and Margo (2007) use rainfall in April 1968 to instrument for participation in riots in the US. A large set of studies use rainfall to instrument for voter turnout on election day (see e.g. Fujiwara et al., 2016; Gomez et al., 2007; Hansford and Gomez, 2010; Lind, 2019; Fraga and Hersh, 2010).

of Umuganda such as Uwimbabazi (2012, p.216), who writes that at *Umuganda* 'successful implementation of any policy can be affected by the absence of the full participation of those especially who should benefit from these policies'. Moreover, the expectation of threshold effects is supported by theory and evidence of collective decision-making and action (see e.g. Olken, 2010; Dal Bó et al., 2010; Faillo et al., 2013).

As we cannot empirically determine the relationship between rainfall and participation at *Umuganda*, we choose the threshold that defines a rainy day based on established standards and reasoning. According to the American Meterological Society, rainfall above 2.5 mm is classified as "moderate" and above 7.5 mm as "heavy" rain (AMS, 2012).[15] Madestam et al. (2013) use the first mark and exploit both, moderate and heavy rain, by defining a rainy day to count more than 2.5 mm (0.1 inches) rainfall in their main specification. Bonnier et al. (2020) deviate from this practice and use only heavy rain above a threshold of 10 mm.[16] We believe that a lower threshold more in line with Madestam et al. (2013) is justifiable for two reasons. First, most Rwandans dislike participating in *Umuganda*,and would gladly accept an excuse not to attend. Even moderate rainfall reduces the cost of remaining absent: it is a verifiable reason for absence and also other people will be absent. Both conditions make enforcement difficult and may therefore protect from sanctions. Second, as stated above, even small dips in participation can make *Umuganda* meetings fail in terms of being effective for policy implementation, e.g. because leaders or the community cannot make binding decisions. In our analysis, this claim should result in similar effect sizes when using different thresholds.

In our preferred specification, we use a threshold of 3 mm to define a rainy day because it is the closest integer number to the standard of 2.5 mm (0.1 inches). However, we show robustness of our results to thresholds between 2 mm and 10 mm daily rainfall.

[15] Source: `http://glossary.ametsoc.org/wiki/Rain`, accessed on 29 Jan, 2025

[16] They motivate this choice with their anecdotal evidence in the form of news reports about low participation and cancellations due to rainfall. For these cases, they find daily rainfall to have ranged between 1 mm and 18 mm with a median of 8 mm. However, most events in their list with rainfall of 6 mm and more, are reported as canceled. Hence, *Umuganda* very likely experiences reduced participation already at lower thresholds. It is important to notice that news reports of extreme weather are much more likely than reports of normal weather outcomes.

5.2 Alternative explanation for an effect

Our empirical strategy relies on two counterfactual assumptions. One is that Saturday rainfall affects our outcomes only through *Umuganda* meetings and the other is that the introduction of performance contracts was the only change that altered the objectives of these meetings at the time.

Two characteristics of our analysis already limit the scope for a different channel other than *Umuganda* affecting behavioral change in the health domain. First, any such channel would need to be time varying due to the inclusion of two-way fixed effects in all our regressions. Second, it would need to be specific to Saturday rainfall because rainfall regressors for every single other day of the week control for general rainfall effects and serve as natural placebo tests. Considering these two constraints, an effect of Saturday rainfall on health behavior must operate through a reoccurring event on that day. In our analysis, we rule out that Saturday rainfall affected the access to modern contraception (e.g. market days, but also distribution of methods during *Umuganda*), which leaves very little scope for a channel unrelated to the social interactions during *Umuganda* meetings.

We turn next to the assumption that without the introduction of performance contracts *Umuganda* meetings do not affect our outcomes of behavior change. With our panel data, we can attribute the change in effects of meetings to the time when performance contracts were introduced. Hence, some other nationwide policy or change would need to have altered the practice or objectives of *Umuganda* meetings with respect to our outcomes and coincided in timing with the introduction of performance contracts. We are not aware of any such change. It is certain, however, that both our outcomes of behavior change were targeted under performance contracts and that *Umuganda* was used to implement targets.

What we discussed here is really (a) the assumption that the Saturday effect we see is really due to Umuganda and (b) thatthe change we see after the PCs is because if imihigo. But there could still be other channels than pressure and strengthened upward accountability that could work through the meeting, such as preference change or that having attended more meetings would lead to social desirability bias in the answers to the DHS survey

5.3 Specification

To estimate the effect of *Umuganda* meetings on behavior change through OLS, we run variations of the following reduced-form regression:

$$y_{it} = \sum_{d=1}^{7} \beta_d \, rain_{ctd} + \alpha_i + \tau_t + \varepsilon_{it}. \tag{1}$$

y_{it} is a binary indicator of behavior change of the observational unit i during month t. The unit of observation, i, depends on the outcome and is either an individual woman or household. Similarly, t may be either a monthly date or a month before the interview, depending on the panel structure of the outcome. $rain_{ctd}$ is the number of days with rainfall above a specific threshold on weekday d in observational unit i's community c during month t. Hence, the regression includes seven rainfall variables that count the number of rainy Mondays, Tuesday, Wednesday etc. in each community and month. In our preferred specification, a rainy day is defined by rainfall above 3 mm. α_i and τ_t are observational unit and monthly time fixed effects. In all estimations, we cluster standard errors at the community level because the community (or village) is the entity of *Umuganda* meetings and local leaders' performance targets. This allows the error term, ε_{it}, to be correlated both within communities and over time.

The coefficients, β_d, capture percentage point changes in the probability of behavior change in any given month following from an additional rainy day on the different weekdays in the same month. The interpretation as percentage point changes follows from the dependent variable, the indicator of behavior change, taking values of either 0 (no change) or 100 (change). Most interesting is the coefficient on Saturday rainfall, which can be interpreted as the effect of a failed *Umuganda* meeting. Rainfall on the other weekdays control for general rainfall effects and are placebo tests. As we will show later, their inclusion in the regressions is unimportant for the results. With unit fixed effects, the coefficients β_d are identified from temporal variation in rainfall and behavior change.

To identify the effect of performance contracts on creating behavior change through *Umuganda* meetings, we estimate equation 1 with panel date before and after the introduction of performance contracts. Subsequently, we test whether the corresponding coefficient estimates

from both regressions are statistically different. This test essentially evaluates the significance of the Differences-in-Differences.[17] We present estimates from separate regressions with before and after data to interpret each of the two point estimates on Saturday rainfall as the effect of a failed *Umuganda* meeting. Their difference, the Differences-in-Differences, are rather uninteresting. Only their statistical significance is relevant to show that performance contracts led to a change in the practice or objectives of meetings. For this reason, we directly present p-values of the Differences-in-Differences.

6 Results

6.1 Main Reduced-form Effects

Table 4 presents the relationship between the two outcomes, contraceptive adoption and bed net acquisition, and the total number of days with rainfall above 3 mm for each weekday in a month. The point estimate on Saturday rainfall can be interpreted as the effects of a failed *Umuganda* meeting, which is a meeting that is canceled or has too low attendance for effective decision-making.

Regression 1 uses a 12-months panel of women over the first year after the introduction of performance contracts, i.e. from April 2006 until March 2007. The reduced-form estimate on Saturday rainfall is strongly statistically significant at the 99% confidence level. It suggests that a failed *Umuganda* meeting reduces the probability that a woman adopts modern contraception in a given month by 0.071%. While this absolute effect seems small, the relative effect compared to the unconditional probability is -18%. Reassuringly, rainfall on any other weekday is insignificant.

Regression 2 estimates the same relationship for the year before the introduction of performance contracts, from April 2005 until March 2006. None of the coefficient estimates, including that on Saturday rainfall, is statistically significant at any conventional level. P-values for the differences in corresponding coefficient estimates between regressions 1 and 2 are presented

[17] we conduct this test by including interactions of all regressors with an after-performance-contracts-dummy, $I(t >= Apr.2006)$, and estimating this expanded equation jointly with before and after data.

one column to the right in Table 4. The difference in estimates on Saturday rainfall is statistically significant at the 99% confidence level. No other difference is statistically significant at the 90% level or below. This finding suggests that *Umuganda* meetings became an effective tool for the implementation of national family planning policy after the introduction of performance contracts.

Regressions 3 and 4 estimate an analogue relationships for the second government target, acquisitions of mosquito bed nets. The data structure in these two regressions is slightly different. Acquisitions are observed at the household level and the time dimension of the panel data are months-before-the-interview. Regression 3 uses 12-months panel data on bed net acquisitions 0 to 11 months before DHS 2007-08 interviews, which are data after the introduction of performance contracts. Regression 4 uses equivalent data from DHS 2005, before the introduction of performance contracts. In regression 3, the reduced-form estimate on Saturday rainfall is highly significant at the 99% confidence level (similar to regression 1). None of the other weekdays is statistically significant at 95% confidence level or high. The relative effect of the point estimate on Saturday rainfall is -10%.

In regression 4, rainfall on all weekdays, including Saturdays, is statistically insignificant at any conventional level (as in regression 2). The p-values for the differences in corresponding coefficient estimates from regressions 3 and 4 demonstrate that only the effect of Saturday rainfall changed significantly at the 95% confidence level. Finding the same pattern of coefficient estimates for another targeted, but otherwise unrelated outcome suggests that in fact performance contracts are responsible for aligning *Umuganda* meetings with national policy. Over the observation period, the practice of *Umuganda* arguably did not change. However, performance contracts allowed the central government to suddenly set the agenda of meetings.

Magnitude The above coefficient estimates can be considered lower bound estimates of a failed *Umuganda* meeting. The estimates should be biased toward zero because the number of rainy Saturdays is an imprecise measure of the number of failed meetings in a month. Hence, relative effects of -18% and -10% suggest that meetings have a very strong effect on the timing of behavior change, especially when also considering that there may be up to 5 meet-

ings in a month. The relative effect size is slightly larger compared to those found by Bonnier et al. (2020) who estimate the relationship between Saturday rainfall leading up to the Rwandan genocide and civilian participation rates in violence using cross-sectional data. For the period from October 1993 until March 1994, which is driving their results, they find that a rainy Saturday (defined by rainfall above 10 mm) reduced civilian participation by 10% compared to the unconditional mean.

6.2 Tracing the Effects

Table 5 traces the effects of Saturday rainfall on contraceptive adoption and bed net acquisition using the two 12-months panel data sets after the introduction of performance contracts. Regressions 1 and 3 show that only the coefficient estimates on the number of Saturdays with rainfall above 3 mm in the same month are highly significant at the 99% confidence level. All estimates on lagged Saturday rainfall can be considered placebo tests and are insignificant with one exception. The coefficient on the first lag in regression 1 is positive and statistically significant at the 90% confidence level. This finding may indicate that Saturday rainfall delays policy implementation and causes a catching up in the following month.

Regressions 2 and 4 evaluate the relationship between the two outcomes and four binary indicators that respectively take the value 1 if rainfall on the first, second, third or last Saturday of a monthly date is above 3 mm and 0 otherwise. The effects of these specific Saturdays are similar to one another within the same regression. If at all, regression 2 suggests that central Saturdays of a monthly date are slightly more important for generating the overall effect of Saturday rainfall in a month, and regression 4 suggests that Saturdays toward the end of a monthly date may be more important. However, none of the differences of Saturday rainfall coefficients in the same regression is statistically significant and all estimates have the same sign. This finding is in line with the evidence discussed in section 2 that *Umuganda* was held on multiple Saturdays every month.

Finally, the long panel data of contraceptive adoption allows us to study the effect of Saturday rainfall over time. Figure 3 presents estimates from rolling regressions over the 7th month

of a 12-months rolling window. The solid black line connects the coefficient estimates and the dashed curves mark 95% confidence intervals. It can be observed that Saturday rainfall effects become negative and statistically significant upon or shortly after the introduction of performance contracts (vertical line on April 2006). The effect continues to persist for roughly 1.5 years after the introduction of performance contracts and then seems to fade away. This finding may mechanically arise if the fraction of women who are both not using modern contraception and being affected by *Umuganda* meetings decreases over time, which is very likely the case.

6.3 Robustness Checks

In this section, we present additional robustness checks. Figure 4 shows robustness of the results after the introduction of performance contracts with respect to two dimensions. First, the effects of Saturday rainfall are robust to using different thresholds to define a rainy day. And second, they are largely unaffected by potential multicollinearity in the weekly rainfall variables. In Figure 4, each coefficient estimate (diamond) and 95% confidence interval (capped bar) is obtained from a separate regression of the outcome on the number of Saturdays with rainfall above a certain threshold, controlling only for unit of observation and time fixed effects, but not rainfall on other weekdays.

Panel (A) shows that the effect of Saturdays rainfall on contraceptive adoption is statistically significant when using thresholds between 2 mm and 10 mm rainfall. Panel (B) shows that also the effect on bed net acquisition is statistically significant for 9 out of 10 thresholds at the 95% confidence level. The coefficient estimates with a 3 mm threshold are very similar to the estimates in Table 4, suggesting that multicollinearity between the rainfall regressors does not affect the results. In addition, the similar effect sizes across the different definitions of a rainy day are consistent with threshold effects in participation at *Umuganda*, as proposed in section 5.1.[18]

[18] Figures **??** and **??** in the appendix present results from equivalent regressions of the two outcomes on rainfall on each of the other weekdays. In this battery of 120 regressions only one coefficient estimate is marginally statistically significant at the 95% confidence level, which can be expected to occur by chance from this large number of multiple hypothesis testing.

6.4 Alternative Channels

Strong evidence suggests that access to contraception and bed nets does not generate our results. With respect to contraception, two complementary pieces of information rule out this explanation. First, hormonal contraceptives (injections, pills, IUDs and implants), as the most commonly used class of methods in Rwanda, were only available at health centers and hospitals (USAID et al., 2011). And second, at the time of the analysis, health centers were closed and hospitals had high surcharges on weekends (Ueberschär, 2018), preventing access on Saturdays.

Our data supports this argument. In the 2010 DHS, 95% of hormonal method users (and 91% of any modern method users) report that their first source for the method was a health facility. Adoptions of these methods make up 90% of all adoptions and drive our results. Evidence that health facilities are closed on weekends comes from 52,539 vaccinations with their exact dates copied from children's health cards in the 2010 DHS. Only 3% of vaccinations took place on a Saturday or Sunday. Further support with respect to family planning is provided in the 2007 Rwandan DHS Service Provision Assessment. It documents (and these numbers are likely overreported) that most health facilities provided family planning services on five or less days a week (NISR et al., 2008), which most likely excludes weekends.

For mosquito bed nets, information on the source is available if the net is obtained up to 6 months before the interview. Table 6 presents results for the time after the introduction of performance contracts. The three outcomes are binary, monthly indicators that take the value 100 if a bed net was acquired from a specific source and 0 otherwise. Regression 1 estimates the relationship between rainfall and bed net acquisition from any source (as in Table 4) on this short panel. All results hold and are very similar. Regressions 2 and 3 only use acquisitions from health facilities and other sources, respectively. The results clearly show that acquisitions from other sources, mostly shops and pharmacies, are generating the effect of Saturday rainfall. Hence, access to health facilities on Saturdays is not the relevant channel.

However, health facilities are the only source for the in Rwanda commonly used hormonal contraceptive methods. Consequently, it is reasonable to conclude that the same relationship pattern between the two targeted outcomes and Saturday rainfall is likely to be generated by

something else than access. We claim *Umuganda* meetings are generating the effects as they are known to regularly take place on Saturdays and the two analyzed behavior changes were explicitly discussed in those meetings.

7 Upward Accountability as a Channel

Having documented a strong, robust effect of Saturday rainfall on two targeted behaviors, we now provide evidence indicating a mechanism for these effects based on pressure. First, we explore conception as an evasive behavior that protects against pressure to adopt modern contraception. Second, we study spatial heterogeneity in the prevalence of mosquitoes as a proxy for popular support for bed nets.

7.1 Evasive Behavior

Enforcement can lead to evasion. A way to evade contraception is getting pregnant because pregnant women must not use it. If the *Umuganda* meetings affect contraceptive adoption through pressure then meetings should also increase conception, while we should see fewer pregnancies when meetings are canceled due to rain on Saturdays. However, pregnancies will eventually naturally decrease with more contraceptive use. Since we found that rain on Saturdays reduces contraceptive use, this predicts more pregnancies when it rains on Saturdays. Therefore, by looking at the short term effect on Saturday rainfall on pregnancies in a regression, we can understand how *Umuganda* works. A negative coefficient suggests a mechanism based on pressure, while a positive coefficient indicates free choice.

Regressions 1 in Table 7 shows the relationship between conception and rainfall on different weekdays after the introduction of performance contracts. The coefficient estimate on Saturday rainfall is negative and statistically significant at the 95% confidence level. It suggests that one failed *Umuganda* meeting reduces the probability to become pregnant in the same month by 8%. This finding of a negative effect suggests that the mechanism of *Umuganda* is based on pressure. The statistical significance further indicates that a comparably large fraction of women chose conception as a behavior to evade contraceptive adoption. Otherwise, in the

displayed average effect of Saturday rainfall on conception, the negative evasion effect would not outweigh the mechanical and positive effect from reduced contraceptive adoptions. Figure **??** in the appendix presents the effects of each weekday at different rainfall thresholds from separate regressions. Panel (A) shows that the effect of Saturday rainfall is robust to using thresholds between 2 mm and 9 mm.[19]

Regression 2 in Table 7 shows the relationship between conception and rainfall on different weekdays before the introduction of performance contracts. None of the coefficient estimates is statistically significant at the 95% confidence level. The column to the right displays p-values of the differences in estimates between regression 1 and 2. Only the difference for Saturday rainfall is statistically significant at the 95% confidence level. This finding further corroborates our claim that performance contracts turned *Umuganda* meetings into an effective tool to implement national development targets and that targets were achieved through pressure.

7.2 Heterogeneity in Popular Support

Regressions 3 and 4 in Table 7 explore heterogeneity in the effects of rainfall on bed net acquisition with respect to altitude. The underlying motivation is the well-known fact that the incidence of mosquitoes strongly decreases with altitude.[20] This variation in the objective usefulness of mosquito bed nets should be strongly correlated with popular support because people are reluctant to invest time and money for something they do not need. With lower levels of support, more pressure (enforcement) is needed to create behavior change. Hence, if the mechanism of *Umuganda* is based on pressure, we can expect to find larger effects from rainy Saturdays in high altitude areas.

In the 2007-08 DHS, the median community is located at an altitude of 1,670 meters. At this altitude, the risk of contracting malaria should be close to 0%, and we can expect extremely low

[19] Regression 1 also displays an effect of Wednesday rainfall that is statistically significant at the 95% confidence level. However, Panel (E) in Figure **??** shows that this effect is only statistically significant for rainfall thresholds 3 mm and below. As the effect does not persist for larger thresholds, we believe this finding is spurious. The other Panels in Figure **??** document that all effects of other weekdays at different rainfall thresholds are statistically insignificant.

[20] In similar climate and terrain as in Rwanda, Bodker et al. (2003) study the incidence of mosquitoes in Tanzania using mosquito light traps. Their traps caught only 4 mosquitoes a year at altitude 1,700 meters above sea level, compared to 269 mosquitoes at altitude 1,000 meters and 3,282 mosquitoes at altitude 300 meters.

support for targets in mosquito bed nets. Regression 3 and 4 in Table 7 estimate the relationship between acquisition of bed nets and rainfall on different weekdays after the introduction of performance contracts respectively using only communities located above and below median altitude. Saturday rainfall is negative and statistically significant at the 95% confidence level in regression 3 and at the 90% level in regression 4. While the difference in coefficients is not statistically significant, the results show a larger point estimate and relative effect for high altitude communities. This finding is consistent with pressure as the mechanism of *Umuganda*. Furthermore, it demonstrates that the Rwandan government increased the distribution of mosquito bed nets nationwide without considering the actual necessity for them in each region.

8 Conclusion

This paper studies the role of community meetings in Rwanda for achieving development targets in health, and on their interaction with performance contacts for local leaders that were introduced in 2006. Specifically, it investigates the effects of meetings on two changes in individual behavior that were unpopular among the population, but desired by the central government: the adoption of modern contraceptive methods and the acquisition of mosquito bed nets. Identification comes from exogenous variation in meeting attendance over time induced by rainfall. This setup allows us to compare the effects before and after the introduction of performance contracts that strongly increased incentives for top-down monitoring of the targeted behaviors. We show a positive and significant relationship between community meetings and adoption of contraceptives and mosquito nets after the reform. Before the reform, these meetings have no effects on these health behaviors. The fact that we find similar effect patterns in the two unrelated but targeted health behaviors suggest that community meetings and performance incentives are complementary and form a governance system that can be used to implement a wide range of development goals. However, we find evidence that suggests that behavior change is involuntary.

These findings have two important implications. First, they challenge the common idea that community meetings generate development primarily through enabling downward accountabil-

ity through bottom-up pressure. By showing fast-track development through a local institution when the incentives for top-down monitoring were increased, our findings indicate that successful community-based development programs may, in fact, have exploited upward accountability by helping leaders control behavior in their communities. Second, our findings emphasize the importance of accounting for the institutional context in development projects. Performance incentives and community meetings are widely considered good policies on their own. However, their combination can have adverse consequences as suggested by the evidence of involuntary behavior change in Rwanda. These findings offer new insights into the mechanisms of accountability and the potential for local institutions to act as vehicles for both positive and coercive development outcomes.

Finally, this paper provides a new perspective on Rwanda's top-performance in many of the Millennium Development Goals and an explanation for its recent fertility transition. Its lessons may be valuable when assessing current and past developments in other countries with authoritarian leanings.

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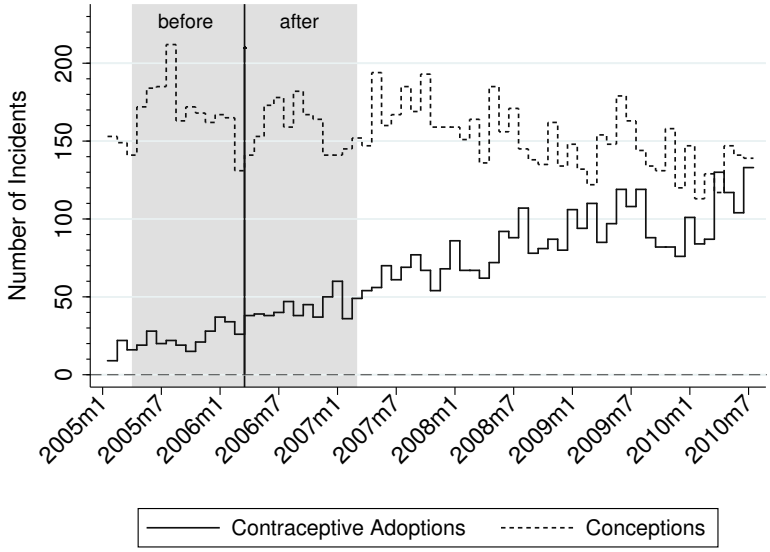
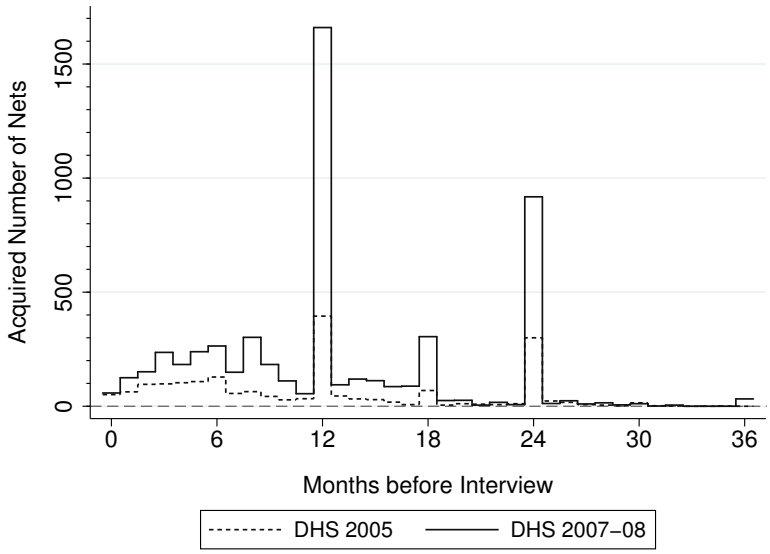
Figures and Tables

Figure 1: Number of Contraceptive Adoptions and Conceptions over Time



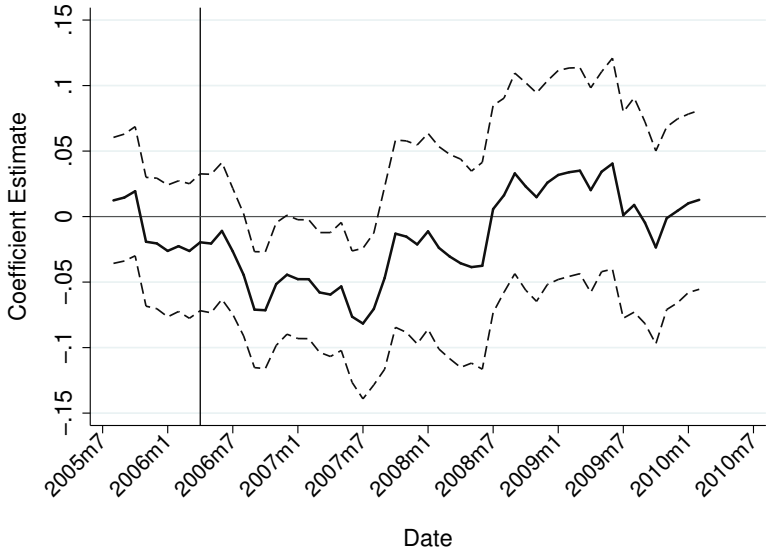
Notes: Based on 13,413 women between 15 and 49 years old and who are usual residents of interviewed households in the 2010 Rwandan DHS. The solid, vertical line marks the introduction of performance contracts in April 2006.

Figure 2: Number of Mosquito Bed Nets Acquired in Different Months



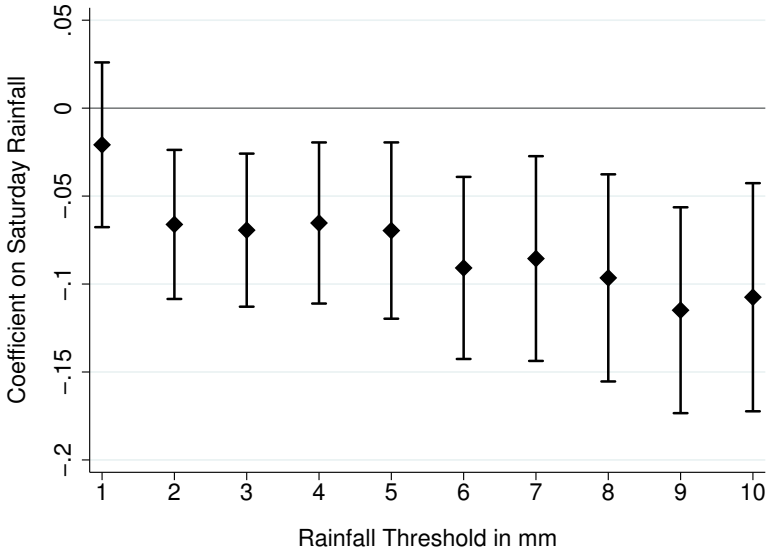
Notes: Based on 10,146 and 7,287 households with GPS coordinates in DHS 2005 and DHS 2007-08 data.

Figure 3: Effect of Saturday Rainfall on Contraceptive Adoption over Time

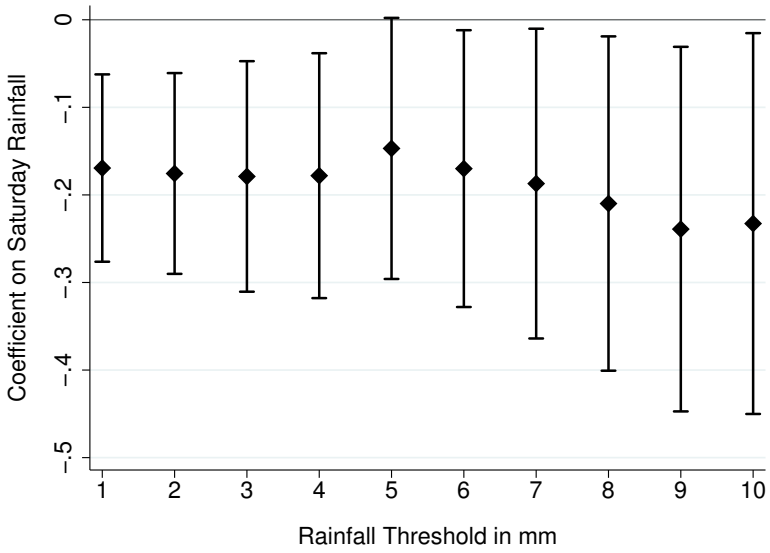


Notes: The figure presents rolling window coefficient estimates on # Sat.(Rainfall>3mm) (solid line) and 95% confidence intervals (dashed lines). The dependent variable is a monthly, binary indicator of contraceptive adoption. # Sat.(Rainfall>3mm) is the number of Saturdays with rainfall above 3 mm in a calendar month. All regressions include analogue rainfall regressors for the other weekdays. The rolling window size is 12 months. The regression estimates are displayed above the 7th month of the rolling window. The vertical line on April 2006 marks the introduction of performance contracts. Standard errors are clustered at the community level.

Figure 4: Effects of Saturday Rainfall at Different Thresholds Under Performance Contracts



Contraceptive Adoption



Bed Net Acquisition

Notes: The figures present the coefficients (diamonds) and 95% confidence intervals (capped bars) on the number of rainy Saturdays (# Sat.(Rainfall> Xmm)) when varying the rainfall threshold in separate regressions. The dependent variables, Contraceptive Adoption and Bed Net Acquisition, are monthly, binary indicators. All regressions control for unit of observation and time fixed effects. Figure (A) uses monthly-date panel data of women for April 2006 to March 2007. Figure (B) uses months-before-interview panel data of households 0-11 months before DHS 2007-08. Standard errors are clustered at the community level.

Table 1: Summary Statistics of Family Planning Outcomes

A. Before (Apr. 2005 - Mar. 2006)	Mean	Min.	Max.	Obs.
Adoption	0.228	0	100	125,193
Conception	1.610	0	100	125,193
B. After (Apr. 2006 - Mar. 2007)				
Adoption	0.395	0	100	130,966
Conception	1.445	0	100	130,966

Notes: Based on 10,629 women in Before Panel and 11,103 women in After Panel. Only women with at least two panel observations are included in each data set.

Table 2: Summary Statistics of Acquisitions of Mosquito Bed Nets

A. Before (based on 2005 DHS)	Mean	Min.	Max.	Obs.
Bed Net Acquisition	0.636	0	100	121,752
Acquisition from Health Center	0.275	0	100	71,022
Acquisition from Other Source	0.536	0	100	71,022
B. After (based on 2007-08 DHS)				
Bed Net Acquisition	1.969	0	100	87,444
Acquisition from Health Center	1.049	0	100	51,009
Acquisition from Other Source	1.057	0	100	51,009

Notes: Based on 10,146 and 7,287 households with GPS coordinates in DHS 2005 and DHS 2007-08 data. Panel lengths of variables: 12 months (0-11) for 'Bed Net Acquisition'. 7 months (0-6) for 'Acquisition from Health Center' and 'Acquisition from Other Source'.

Table 3: Summary Statistics of Rainfall on Saturdays

Rainy Saturdays (Definition)	Mean	Std. dev.	Min.	Max.	Obs.
# Sat.(Rainfall>1mm)	1.249	1.125	0	5	11,808
# Sat.(Rainfall>2mm)	0.951	1.017	0	5	11,808
# Sat.(Rainfall>3mm)	0.764	0.938	0	5	11,808
# Sat.(Rainfall>4mm)	0.620	0.838	0	4	11,808
# Sat.(Rainfall>5mm)	0.512	0.756	0	4	11,808
# Sat.(Rainfall>6mm)	0.433	0.695	0	4	11,808
# Sat.(Rainfall>7mm)	0.374	0.641	0	4	11,808
# Sat.(Rainfall>8mm)	0.325	0.597	0	4	11,808
# Sat.(Rainfall>9mm)	0.273	0.547	0	4	11,808
# Sat.(Rainfall>10mm)	0.239	0.509	0	4	11,808

Notes: Based on 492 communities in the 2010 Rwandan DHS between April 2005 and March 2007. # Sat.(Rainfall> Xmm) is the number of Saturdays with rainfall above X mm in a calendar month.

Table 4: Main Effects

Dependent variable:	Contraceptive Adoption			Bed Net Acquisition		
Panel data:	after	before		after	before	
	(1)	(2)	p-value (1)-(2)	(3)	(4)	p-value (3)-(4)
# Sat. (Rainfall $>$ 3mm)	-0.071***	0.019	[0.008]	-0.203***	-0.019	[0.017]
	(0.023)	(0.025)		(0.070)	(0.032)	
# Sun. (Rainfall $>$ 3mm)	0.015	0.029	[0.688]	0.090	0.036	[0.512]
	(0.027)	(0.022)		(0.074)	(0.035)	
# Mon. (Rainfall $>$ 3mm)	-0.020	0.004	[0.575]	-0.115*	0.009	[0.086]
	(0.032)	(0.028)		(0.066)	(0.031)	
# Tue. (Rainfall $>$ 3mm)	-0.034	-0.009	[0.522]	0.027	0.028	[0.989]
	(0.033)	(0.021)		(0.070)	(0.038)	
# Wed. (Rainfall $>$ 3mm)	-0.036	0.012	[0.213]	-0.019	-0.033	[0.869]
	(0.032)	(0.021)		(0.074)	(0.037)	
# Thu. (Rainfall $>$ 3mm)	-0.014	-0.002	[0.750]	0.065	-0.043	[0.184]
	(0.027)	(0.027)		(0.075)	(0.033)	
# Fri. (Rainfall $>$ 3mm)	0.037	-0.020	[0.106]	0.016	-0.021	[0.611]
	(0.027)	(0.023)		(0.066)	(0.032)	
Unit FE	Yes	Yes		Yes	Yes	
Time FE	Yes	Yes		Yes	Yes	
Observations	130,966	125,193		87,444	121,752	
R-squared	0.080	0.082		0.086	0.091	
Dep. var. mean	0.395	0.228		1.969	0.636	

Notes: *Imihigo* performance contracts were signed at the beginning of April 2006. 12-months panel data for the time after (before) that date is used in regressions 1 and 3 (2 and 4). The dependent variables, Contraceptive Adoption and Bed Net Acquisition, are monthly, binary indicators. # Sat.(Rainfall$>$3mm) is the number of Saturdays with rainfall above 3 mm in a calendar month (and similarly for all other weekdays). The unit of observation in regressions 1 and 2 is a woman, and a time step is a monthly date. Regression 1 uses data for April 2006 to March 2007. Regression 2 uses data for April 2005 to March 2006. The unit of observation in regressions 3 and 4 is a household, and a time step is a month-before-the-interview. Regression 3 uses data for 0-11 months before the DHS 2007-08 interview. Regression 4 uses data for 0-11 months before the DHS 2005 interview. Standard errors are clustered at community level. P-value: *** p$<$ 0.01, ** p$<$ 0.05, ** p$<$ 0.1.

Table 5: Tracing the Effects Under Performance Contracts

Dependent variable:	Contraceptive Adoption		Bed Net Acquisition	
	(1)	(2)	(3)	(4)
# Sat. (Rainfall > 3mm)	-0.068***		-0.192***	
	(0.023)		(0.073)	
First Sat. (Rainfall > 3mm)		-0.064		-0.097
		(0.057)		(0.161)
Second Sat. (Rainfall > 3mm)		-0.117*		-0.125
		(0.060)		(0.144)
Third Sat. (Rainfall > 3mm)		-0.085*		-0.265*
		(0.049)		(0.150)
Last Sat. (Rainfall > 3mm)		-0.068		-0.285***
		(0.053)		(0.104)
L1. # Sat. (Rainfall > 3mm)	0.046*		0.016	
	(0.027)		(0.065)	
L2. # Sat. (Rainfall > 3mm)	-0.020		0.089	
	(0.025)		(0.062)	
L3. # Sat. (Rainfall > 3mm)	0.011		0.045	
	(0.028)		(0.064)	
Other Rainfall Regressors	Yes	Yes	Yes	Yes
Unit FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	130,966	130,966	87,444	87,444
R-squared	0.080	0.080	0.086	0.086
Dep. var. mean	0.395	0.395	1.969	1.969

Notes: The dependent variables, Contraceptive Adoption and Bed Net Acquisition, are monthly, binary indicators. # Sat.(Rainfall>3mm) is the number of Saturdays in a month with rainfall above 3 mm. L1.# Sat.(Rainfall>3mm) is this variable lagged by one month (and similar for higher order lags). First Sat.(Rainfall>3mm) is a monthly, binary indicator which takes the value 100 if rainfall on the first Saturday of that monthly date is above 3 mm and 0 otherwise. Second, Third and Last Sat.(Rainfall>3mm) are corresponding indicators for rainfall on the other Saturdays in the calendar month. Other Rainfall Regressors are the numbers of days with rainfall above 3 mm for every other weekday. In regressions 1 and 2, the unit of observation is a woman, a time step is a monthly date and the data are for April 2006 to March 2007. In regressions 3 and 4, the unit of observation is a household, a time step is a month-before-the-interview, and the data are for 0-11 months before the DHS 2007-08 interview. Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, ** p< 0.1.

Table 6: Bed Net Acquisition from Different Sources Under Performance Contracts

Dependent variable:	Bed Net Acquisition from ...		
	Any source	Health Facility	Other source
	(1)	(2)	(3)
# Sat. (Rainfall > 3mm)	-0.211**	-0.029	-0.177**
	(0.100)	(0.070)	(0.073)
# Sun. (Rainfall > 3mm)	0.143	0.034	0.114
	(0.093)	(0.064)	(0.073)
# Mon. (Rainfall > 3mm)	-0.099	-0.015	-0.072
	(0.083)	(0.052)	(0.062)
# Tue. (Rainfall > 3mm)	0.031	0.010	0.024
	(0.095)	(0.065)	(0.068)
# Wed. (Rainfall > 3mm)	0.063	0.103	-0.055
	(0.100)	(0.066)	(0.072)
# Thu. (Rainfall > 3mm)	0.115	0.039	0.093
	(0.114)	(0.075)	(0.085)
# Fri. (Rainfall > 3mm)	0.014	0.034	-0.010
	(0.097)	(0.065)	(0.072)
Household FE	Yes	Yes	Yes
Months-before-Interview FE	Yes	Yes	Yes
Observations	51,009	51,009	51,009
R-squared	0.147	0.148	0.150
Dep. var. mean	2.082	1.049	1.057

Notes: The dependent variables, bed net acquisitions from different sources, are monthly, binary indicators that take the value 100 if a bed net was acquired from that source and 0 otherwise. # Sat.(Rainfall>3mm) is the number of Saturdays with rainfall above 3 mm in a month before the interview (and similarly for all other weekdays). The data are for 0-6 months before the DHS 2007-08 interview. Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, ** p< 0.1.

Table 7: Mechanisms

Dependent variable:	Conception			Bed Net Acquisition	
Panel data:	after	before		Median split by altitude high	Median split by altitude low
	(1)	(2)	p-value (1) − (2)	(3)	(4)
# Sat. (Rainfall > 3mm)	-0.121**	0.062	[0.024]	-0.256**	-0.178*
	(0.048)	(0.066)		(0.099)	(0.100)
# Sun. (Rainfall > 3mm)	0.050	-0.075	[0.143]	0.089	0.061
	(0.057)	(0.059)		(0.107)	(0.113)
# Mon. (Rainfall > 3mm)	0.063	0.016	[0.614]	-0.128	-0.094
	(0.062)	(0.068)		(0.079)	(0.105)
# Tue. (Rainfall > 3mm)	-0.088	-0.053	[0.705]	-0.019	0.064
	(0.061)	(0.066)		(0.099)	(0.099)
# Wed. (Rainfall > 3mm)	-0.145**	-0.005	[0.096]	-0.017	-0.012
	(0.062)	(0.058)		(0.104)	(0.106)
# Thu. (Rainfall > 3mm)	0.024	-0.133*	[0.079]	0.056	0.069
	(0.052)	(0.070)		(0.095)	(0.119)
# Fri. (Rainfall > 3mm)	0.075	-0.005	[0.323]	-0.033	0.064
	(0.055)	(0.062)		(0.086)	(0.103)
Unit FE	Yes	Yes		Yes	Yes
Time FE	Yes	Yes		Yes	Yes
Observations	130,966	125,193		43,500	43,944
R-squared	0.072	0.070		0.083	0.087
Dep. var. mean	1.445	1.610		1.733	2.203

Notes: *Imihigo* performance contracts were signed at the beginning of April 2006. The dependent variables, Conception and Bed Net Acquisition, are monthly, binary indicators. # Sat.(Rainfall>3mm) is the number of Saturdays with rainfall above 3 mm in a calendar month (and similarly for all other weekdays). The unit of observation in regressions 1 and 2 is a woman, and a time step is a monthly date. Regression 1 uses data for April 2006 to March 2007. Regression 2 uses data for April 2005 to March 2006. The unit of observation in regressions 3 and 4 is a household, and a time step is a month-before-the-interview. Both regressions use data for 0-11 months before the DHS 2007-08 interview. Regression 3 only uses observations from villages located above median altitude, whereas regression 4 only uses observations from villages at and below median altitude. The median altitude is 1,670 meters above sea level. Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, ** p< 0.1..