Local Institutions and Leader Incentives: The Political Economy of Rwanda's Fertility Transition*

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March 13, 2026

Abstract

Modern contraceptive use in sub-Saharan Africa remains low despite sustained investments. Rwanda is a notable outlier, with uptake more than quadrupling in 2005-10, alongside sharp fertility declines. We study the role of two institutions in driving this rapid change: performance contracts for leaders and local village meetings. Our data covers periods before and after a policy reform that incentivized local leaders to promote contraceptive adoption, with village meetings being a key arena for implementation. Using rainfall variation to introduce exogenous variation in meeting intensity, we find that meetings significantly influenced adoption only once performance contracts were in place, indicating that top-down factors played a crucial role in how meetings affected adoption. After the reform, non-rainy Saturdays, indicative of well-attended meetings, increase the likelihood of contraceptive adoption by 18% in the same month. Results are robust to another incentivized outcome and to different rainfall thresholds. Increased adoption is not driven by improved access to or information about contraceptives, and we show suggestive evidence that it was partly involuntary. Our findings shed new light on Rwanda's remarkable health development indicators and show that local institutions and performance incentives can act as complements in facilitating top-down monitoring.

JEL Classification: D71, D73, I12.
Keywords: Local Institutions; Family Planning; Performance Incentives; Technology adoption

*We are grateful to Sammy Rohner and Mikael Wiljanen for excellent research assistance. We thank Francesco Amodio, Marianna Battaglia, Martina Björkman Nyqvist, Konrad Burchardi, Ruben Durante, Andrea Guariso, Selim Gulesci, Leander Heldring, Macartan Humphreys, Leonardo Iacovone, Rajshri Jayaraman, Eliana La Ferrara, Julien Labonne, Andreas Madestam, Matti Mitrunen, Kate Orkin, Dina Pomeranz, Pia Raffler, Johanna Rickne, Christian Traxler, Janne Tukiainen, Diego Ubfal, Marijke Verpoorten, Christopher Woodruff and Ekaterina Zhuravskaya helpful comments and suggestions. We would also like to thank seminar and conference participants at AEL Zurich, Bocconi University, SOFI, VATT, CSAE, the EEA congress, ESPE Naples, the Nordic Conference in Development Economics, the Helsinki GSE, and the Hertie School. Funding from the Yrjö Jahnsson Foundation is gratefully acknowledged.

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1 Introduction

Increasing modern contraceptive use in sub-Saharan Africa has proven challenging, despite decades of effort and substantial investments (Dupas et al., 2025). Fertility rates remain much above replacement level, and limited use of family planning create health risks and economic challenges – not least for women. A clear outlier in this context is Rwanda where contraceptive use more than quadrupled in just five years after 2005, accompanied by a drop in birth rates similar to the average decline on the continent over the past 30 years (World Bank, 2022). In this paper, we study the role of local institutions in driving this rapid change.

In 2006 the Rwandan government introduced a system of performance contracts that incentivized leaders at all administrative levels to achieve central policy goals. These goals included the expansion of modern contraceptive use, an idea prioritized by the central government but facing resistance and stigma in the population (Solo, 2008). Performance contracts were introduced simultaneously in all districts, making it difficult to isolate the effect of this policy. We exploit variation in participation intensity in a key venue for leaders to interact with local constituencies about policy: state-controlled community meetings held in villages across the country on Saturdays.[1] Local institutions, such as village meetings and assemblies, can enable citizens to hold leaders accountable for service delivery (Björkman and Svensson, 2009; Casey, 2018), while they might help leaders influence and monitor uptake of policies facing low demand. We utilize plausibly exogenous variation in meeting attendance around the launch of performance contracts to understand if performance contracts altered the effect that the meetings had on this public health outcome. In doing this, we also shed light on the direction of accountability facilitated by these meetings with regard to the family planning policy: bottom-up accountability or top-down control.

Rwanda provides an interesting case for studying the direction of accountability in local institutions. After launching an ambitious health sector strategic plan in 2004, following the Millennium Development Goals, the country saw remarkable progress towards the goals in the early 2000's. Its increase in contraceptive use between 2005 and 2010 is one of the fastest

[1]These meetings are part of a traditional community program, *Umuganda*, that also consists in community work, which will be described in more detail in section 2.3

globally (Abbott et al., 2017; UNDP, 2014). This is widely attributed to Rwanda's visionary policy makers, in combination with local institutions such as the *Umuganda* program (World Bank Group, 2018; UNDP, 2014), usually described as an enabler of bottom-up accountability (MINALOC, 2008a). However, Rwanda is also characterised by its top-down authoritarian leadership [2]. It is important to note that many local institutions worldwide – even in democracies – contain authoritarian elements, so our findings are relevant to decentralised development beyond Rwanda.

We identify the effects of community meetings around the launch of performance contracts, on contraceptive adoption using monthly panel data constructed from from a large, representative sample of Rwandan women: the DHS. To establish causality, we leverage variation in rainfall within villages over time: the number of "non-rainy" Saturdays in a month proxies for the number of successful *Umuganda* meetings in that village the same month. Non-rainy day counts for other weekdays serve as controls and natural placebo tests. This approach is similar to that of Bonnier et al. (2020)[3]. Our identifying assumption is that (unobserved) meeting attendance decreases with Saturday rainfall, which is motivated both by specific features and location of *Umuganda* meetings and by work on attendance in outdoor events in other contexts (see e.g. Madestam et al., 2013; Fujiwara et al., 2016; Collins and Margo, 2007). Two observations support attributing contraceptive uptake to meeting intensity. First, modern contraceptives were explicitly discussed at these meetings (MoH, 2008; Sommers, 2012). Second, Saturday rainfall could not have directly affected access to modern methods, because health facilities – the source of these methods – were closed on Saturdays (see e.g. Ueberschär, 2018).[4]

We next link these effects to the performance contracts In April 2006. This reform created a sudden shift in local leaders' objectives to align with those of the central government and altered their incentives to promote contraceptive use. We compare the effects of meetings before and after the introduction of performance contracts. The identifying assumption is that the practice

[2]For instance, in 2007, Freedom house rated the country a 6 on political rights and 5 on civil liberties, using a scale between 1 and 7 for each country, with 7 representing the least free. (Freedom House, 2007)

[3]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.

[4]The most common modern contraceptives in our sample are IUDs and injectables, and 91% of the modern method users in the DHS data report health facility as the source of their contraceptives.

of *Umuganda* meetings, health care provision, and national development goals did not vary systematically across villages with different levels of Saturday rainfall.

We find that in the first year after the introduction of performance contracts one additional meeting in a month (one more non-rainy Saturday) significantly increases the likelihood of contraceptive adoption in the village in the same month by 18%, a considerable effect. In contrast, Saturday rainfall has no significant correlation with contraceptive adoption in the year preceding the performance contracts, despite the Health Sector strategic plan, that emphasised contraceptives, already being in effect at that time. Coefficients for non-rainy day counts for other weekdays are consistently insignificant. These main findings show that top-down channels were crucial for meetings to be able to affect the uptake of contraceptives. They suggest that performance contracts turned *Umuganda* meetings into effective tools for enforcing central government mandates once local leaders were incentivized. Results are robust to different rainfall thresholds, and to an alternative outcome that was also emphasized in both the Health Sector strategic plan and in performance contracts: anti-malarial bed nets.

In terms of mechanisms behind these effects, we show that meetings are unlikely to have enhanced access or information in the short run. As stated above, clinics were closed on Saturdays and the type of contraceptive methods considered here cannot be disseminated during meetings. Access was also not considered a binding constraint in survey data from the year before our period of interest. Knowledge of modern contraceptive methods and where to obtain them was high (above 95%) throughout the study period. To further understand whether the meeting effect operated through increasing pressure or control we examine whether meetings affect adoption also in places with low demand, and whether there are signs of resistance to the policy. First, we analyze the heterogeneity in how the meetings (low Saturday rainfall) affect adoption of our alternative outcome variable: bed nets. This is useful as we can exploit the inverse relationship between mosquito prevalence and altitude to assess"objective demand". We find that the impact of meetings is more pronounced in high-altitude communities with limited need for bed nets. This suggests that meetings induce involuntary and inefficient behavior modifications regarding bed net use. Second, we study the effect of *Umuganda* on conception, i.e., the timing of pregnancy. 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. Our data supports this: while non-rainy Saturdays (more meetings) increased contraceptive adoption we also find that they correlate with an 8% increase in conception rates.

The paper contributes to three separate literatures. The first is the literature on contraceptives adoption and the "different" fertility transition in sub-Saharan Africa (Bongaarts, 2017). Lower birth rates are seen as a path to poverty reduction (Canning and Schultz, 2012), which has motivated policy makers and international organizations to focus efforts and resources on expanding affordable access to contraceptive methods. Uptake remains limited, however, and recent evidence shows that financial barriers are not key constraints, and that adoption is hard to affect even when information gaps are addressed simultaneously with access (Desai and Tarozzi, 2011; Dupas et al., 2025). By shedding light on the channels through which Rwanda achieved its rapid improvement in family planning indicators our study speaks to how local institutional setups can impact uptake of family planning and other health policies.

The paper also relates to the literature on performance incentives in public administration (Finan et al., 2017). The economics literature on health technology adoption typically focuses on constraints related to information, liquidity, or risk (Dupas, 2011). By this logic, incentivizing health workers or public officials can help close information gaps about the existence or usefulness of a technology (Ashraf et al., 2014) or improve access to it (Björkman and Svensson, 2009; Björkman Nyqvist et al., 2017; Gertler and Vermeersch, 2012). In our context, neither information nor access appear to be binding constraints, which limits the actions available to policy makers to drive behavioral change. Performance incentives sometimes have perverse side-effects[5], and specifically in the fertility domain they have been shown to encourage coercive and unethical behavior (León-Ciliotta et al., 2025; Pelras and Renk, 2023). Our findings show that incentives may encourage local leaders to implement even unpopular policies that include restrictions of personal freedom.

Finally, we contribute to the literature on local institutions as instruments for development.

[5]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.

Experimental work in this field attempts to empower citizens in project implementation through information, training, or changes to participation structures (Beath et al., 2017; Björkman and Svensson, 2009; Olken, 2007, 2010). However, it is well known that local institutions are potential arenas for top-down monitoring and pressure on citizens (Ban et al., 2012; Parthasarathy et al., 2019; Reinikka and Svensson, 2004; Anderson et al., 2015; Acemoglu et al., 2014). Of particular relevance for our case is Bonnier et al. (2020) who, studying the same community meetings in the 1990's, find a link between meeting intensity just before the 1994 Rwandan genocide and local civilian perpetrators. By jointly considering accountability and local institutions, we show that performance incentives and local institutional structures can act as complements in addressing related problems of accountability in delegated tasks.

The rest of the paper is structured as follows. In section 2 we provide background information on family planning goals, policies and attitudes in Rwanda, and on the institution of *Umuganda* community meetings as well as the new performance contract policy. In section 3 we propose a conceptual framework. The data and its construction are described in section 4, followed by the empirical strategy in Section 5. In section 6 we present our main results and robustness checks while we discuss potential mechanisms behind our findings in section 7. Finally, section 8 concludes.

2 Background

In this section we provide a brief overview of Rwanda's goals and progress in the family planning domain between 2004-2010, and introduce the two central institutions studied in this paper: state controlled community meetings (*Umuganda*) and performance contracts for leaders.

2.1 Family planning goals and policy in Rwanda in the early 2000's

In April 2004, Rwanda's central government launched a 5-year Health Sector Strategic Plan, with ambitious goals for contraceptive adoption. Aligned with the UN's Millennium Development Goals, these aims included increasing modern contraceptive prevalence from 4% to 20% by 2010 (MoH, 2004). The primary motivation was arguably economic development. At the

time, international development agencies convinced Rwanda's government that poverty reduction required reduced fertility (Solo, 2008).[6]

Many Rwandans, however, opposed this goal. Contraceptive use contrasted with strong pro-natalist norms which favored large families (Solo, 2008; Westoff, 2013; Kraehnert et al., 2019) and stigmatized contraception users (Berry, 2015; USAID and MoH, 2002; Farmer et al., 2015). 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 pro-natalist ideals 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).[7] This also suggests that information about and access to contraceptives was not a binding constraint.

After the first two years of the policy, in March 2006, the government assessed that 'Up 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 and discussed in section 2.2 incentivized local leaders to, among other things, advocate for adoption of modern contraception.[8] By 2010, Rwanda had surpassed its targets, raising modern contraceptive use to 25.2%. Figure 2 displays adoption rates among *married* women in African countries that started out at similar levels of contraceptive use in year 2000 and shows how Rwanda is a clear outlier in terms of modern contraceptive adoption.[9] The increase in contraceptive use was accompanied by a decline in fertility from 6 to 4 children per woman in the same period. (UNDP, 2014) Rwandan policy-makers attribute this fast-track development to performance contracts (Scher, 2010). We will investigate this claim quantitatively by combining data from before and after the performance contracts with

[6] Another emphasized outcome was bed nets, which we study in our robustness checks: the goal was raising the percentage of children sleeping under bed nets from 18% to 70% by 2010. Rwanda was granted large financial support for bed net distributions, e.g. through the U.S. President's Malaria Initiative and the Roll Back Malaria Partnership.

[7] Meanwhile, demand for bed nets was also limited, partly due to Rwanda's geography which suggests limited need for bed nets as much of the population lives at altitudes where mosquitoes are rare (Bodker et al., 2003).

[8] 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 Rwanda Governance Board (2014).

[9] In the same period there was an increase in household bed net ownership to 83%.

data from the institution *Umuganda*, described in the next subsection.

2.2 Performance Contracts

In 2006, Rwanda's president, Paul Kagame, introduced Performance Contracts (*Imihigo*) in the public sector. On April 4, the first contracts were signed with all 30 district executives (mayors) after district level targets had been formulated. Targets were then passed down through cascading contracts to lower levels of public administration (MINALOC, 2010; Purdekova, 2011; GoR, 2008).

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 (see e.g. World Bank Group, 2018; Hasselskog and Schierenbeck, 2015). In the first year after their introduction, three quarters of districts' targets were national policies and programs (GoR, 2008), while in the government's 2010 Citizen Report Card survey, 78% of respondents state that they have not participated in formulating the targets set for their communities (Munyandamutsa, 2011). The health targets in focus in this paper were part of early *Imihigo* contracts (OSSREA, 2007; Sommers, 2012).

The performance contracts resemble modern results-based management tools (World Bank Group, 2018), but their branding as *Imihigo* ties the policy to a pre-colonial cultural practice where leaders or warriors publicly vowed to achieve certain goals, facing public shame if they failed (Scher, 2010). In the new, institutionalized version of *Imihigo*, i.e. performance contracts, media is used to make vows and evaluations public. These contracts are effective for swift policy implementation thanks to two key features. First, comparable units, such as districts, are regularly ranked against each other (World Bank Group, 2018). Second, the contracts set strong social and material incentives based on relative performance. Mayors' vows and evaluations are broadcast via radio and TV (Scher, 2010), along with awards ceremonies for top-performing mayors who receive symbolic diplomas and prizes such as laptops[10]. Conversely, mayors and other officials risk public humiliation if they perform poorly against the set goals. Within the

[10] See e.g. https://rwandarwacu.wordpress.com/2010/07/08/kagame-and-leaders-sign-imihigo-performance-contracts/ (Accessed on 2025-10-10) or https://www.flickr.com/photos/paulkagame/15029294707/in/photostream/ (Accessed on 2025-10-22)

administration, rewards often take the form of financial bonuses and promotions, while sanctions involve removal from office and public shaming (Murray-Zmijewski and Gasana, 2010). At the district level, around 75% of mayors were removed from office between 2007 and 2009 due to poor performance (Scher, 2010).

This system arguably creates a competitive atmosphere where leaders strive to outperform one another and can lead local leaders to adopt coercive strategies to meet their targets. Leaders implement village goals by having household heads pledge contributions publicly during *Umuganda* meetings (Bugingo and Interayamahanga, 2010). These pledges are documented in a household's *Imihigo Booklet*, which is stamped upon completion, serving as proof of "good standing" required 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 targets at the local level (OSSREA, 2007) and there are reports of harsh measures such as fines, property destruction, and corporal punishment inflicted on those who refuse to contribute towards the *Imihigo* targets (Thomson, 2008; Huggins, 2009).

2.3 Local community meetings

The meetings that we consider are part of a local institution in Rwanda called *Umuganda*, with precursors dating back to pre-colonial times.[11] The institution, consisting in community level meetings and work on public projects, was a nationwide policy during Rwanda's Second Republic: between 1973 and the early 1990's[12]. 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; Rwanda Governance Board, 2017). *Umuganda* was then 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, 2008a). And finally, on November 17, 2007, *Umuganda* became a law (Organic Law N° 53/2007). The purpose of these policies was to

[11] Similar local institutions are common in the region, e.g. Burundi, Ethiopia and South Sudan.

[12] It was then inspired by similar programs in North Korea and Zaïre (Desrosiers, 2023, p.204). Similar community programs also existed in the Soviet Union, sometimes under the Russian name *Subbotnik*, derived from the Russian word for 'Saturday'.

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 held on Saturdays and all able bodied citizens aged 18-65 are expected to participate (MINALOC, 2008b). It is organized by a committee of village chiefs and consists of outdoor, physical labor (e.g. clearing bushes or cleaning roads) followed by a meeting held outdoors(Uwimbabazi, 2012).[13] Desrosiers (2023, p. 348) describes Umuganda as part of ‘‘*a normative and symbolic authoritarian apparatus [that] gave local implementers a tool to tie citizens to the regime, to try and foster authoritarianism in hearts and minds, through expectations of patriotism and contributing to the country's development. Through these normative prescriptions around "good behavior," Rwandans were expected to police themselves, with the help of the system's local antennas: its administrators and authorities on the ground.*"

The location and focus of *Umuganda* is typically announced on the same day by loudspeakers mounted on circulating cars in more urban areas (Rwanda Governance Board, 2014) or through other public announcement methods, such as a person calling out from a central place in the village in more rural areas[14]. During *Umuganda*, all shops must close and public transport pauses. 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 in the study period (MINALOC, 2008b). Evidence suggests that many Rwandans participate involuntarily in *Umuganda* (Mukarubuga, 2004; Uwimbabazi, 2012; Purdekova, 2011).

We argue that the community meetings held after the physical labor during *Umuganda* can affect contraceptive adoption. While these meetings are are officially a place for local leaders to mobilize and support the population to collectively define and resolve local problems (MINALOC, 2008a), 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 for implementation of performance contract targets (Rwanda Governance Board, 2014). During our period of interest in this paper, annual

[13] *Umuganda* is often translated as "community work". This translation, however, distracts from its strong social component, the meeting, which is central in this paper

[14] an informant in Hasselskog and Schierenbeck (2015) explains that "There is a person in charge of delivering the information at 5 AM. They call him "the barker". So he stands on the top of the hill and loudly informs villagers about the work to be done. That's his own umuganda [work task]."

reports of Rwanda's Ministry of Health document that family planning was regularly discussed and promoted at *Umuganda* meetings (MoH, 2008, 2009).[15]

Some dissent exists about the frequency of *Umuganda* in different periods after its reintroduction in 1998. Organic Law N° 53/2007 states that it shall take place on the last Saturday of the month, but that the population may decide to carry it our more frequently (MINALOC, 2008b). Meanwhile, the 2008 revised Community Development Policy (MINALOC, 2008a), states that *Umuganda* takes place every Saturday at the local level. That *Umuganda* was often held weekly in our study period is also supported by several research studies (e.g. Hasselskog and Schierenbeck, 2015; Purdekova, 2011; Uwimbabazi, 2012; NAR and Interpeace, 2016). In our main specification we use rainfall variation on all Saturdays in a month. For robustness we also explore the effects of specific Saturdays, including the last Saturday of every month.

3 Conceptual Framework

In this section we discuss the ways in which community meetings can be expected to affect uptake of modern contraceptives and how performance contracts may shift these channels. We also outline implications for the timing of effects and the expected patterns for contraceptive adoption and related behaviors.

3.1 Channels through which meetings may affect adoption

Meetings can affect uptake of contraceptives through two sets of channels: The first set relies on *bottom-up* pressure. If there is unmet demand for contraceptives, citizens can use the meetings to coordinate and put pressure on their leaders to improve local access to contraceptives, including better healthcare services and larger selection of products. This function of meetings is in line with most of the literature on local institutions as vehicles for policy implementation in developing countries (see e.g. Beath et al., 2017; Björkman and Svensson, 2009; Olken, 2010).

The second set of channels rely on *top-down*, leader initiated elements. First, leaders may

[15] Another health outcome which was discussed in Umuganda meetings during this period is bed nets, which we use in our robustness checks.(MoH, 2008, 2009; Sommers, 2012)

use meetings to spread information about contraceptives, closing knowledge gaps and stimulating demand. Officials' positive messaging about contraceptives could also create opportunities for the community to correct misperceptions about norms concerning contraceptive acceptance (in the spirit of Bursztyn et al., 2020) thereby uncovering latent demand. Finally, leaders may use meetings to exert pressure – through public shaming or fostering peer enforcement – to push contraceptive adoption even absent unmet demand or major information gaps. Pressure is consistent with anecdotal descriptions in sections 2.2 and 2.3. Target attainment can be viewed as a public good: individuals contribute by adopting behaviors that advance the goal, and leader pressure means holding the community collectively accountable for failures. Achieving targets with respect to contraceptives involves (i) selecting sufficient contributors and (ii) monitoring (and, if necessary, enforcing) their contributions. *Umuganda* meetings can perform both functions[16] Therefore, canceled or ineffective meetings impede implementation and reduce the number of new adopters in a given month.

3.2 Expected effects on contraceptive adoption patterns

Timing: Bottom-up pressure reflects citizen-initiated efforts and should be unaffected by changes in leaders' incentives. If meetings affect contraceptive uptake by enabling bottom-up pressure we should expect to see a positive relationship between meetings and uptake throughout the study period. In contrast, changes in adoption coming from top-down channels are expected to manifest only after the reform: before performance contracts, leaders' views on modern contraceptives likely aligned with community attitudes while after the reform, incentives compelled leaders to promote contraceptives regardless of personal beliefs.

Behaviors: Both bottom-up mechanisms and those top-down mechanisms that operate through information diffusion or norm correction imply voluntary behavior change, with little need for enforcement. By contrast, the pressure channel relies on enforcement – or its threat – and is more likely to generate adoption also where there is low demand, or induce evasive behaviors alongside adoption.

[16]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).

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

4.1 Family Planning

We use information from the 2010 Rwandan DHS to measure our main outcome: women's adoptions of modern contraceptive methods, as well as conceptions, i.e. timing of pregnancies. The 2010 DHS covers 13,413 women aged 15-49 residing 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. An advantage with using 2010 data to measure our main outcome is that it reduces the risk of desirability bias affecting our self reported data. If women had been interviewed in 2006 and 2007 it is plausible that having attended more meetings would make them more inclined to report having used modern contraceptives as the message from the meetings would be more salient. Meanwhile, the risk of recall bias is not so concerning when it comes to the adoption of technologies such as IUDs and injections, for which documents from the health center are likely to be available.

We construct our panel data on family planning outcomes from this retrospective calendar data. We start by building 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.[17]

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

Our main outcome variable, 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.[18]

The coding of conceptions is somewhat more involved. The DHS reproductive calendar records pregnancy spells in completed months. Interviewers first mark the month of birth or termination and then "write back" month by month until the number of shaded months equals the woman's reported completed months of pregnancy. Consequently, pregnancies ending in live birth typically appear as nine-month spells in the calendar. This procedure induces two systematic timing errors that make the recorded start of pregnancy later than the biological conception. First, the month of birth or termination is counted as a full month of pregnancy, implicitly treating the event as if it occurred on the last day of the month. This convention can shift the inferred start of pregnancy forward by as much as one month relative to the true date. Second, he calendar records only completed months, omitting the month of conception itself. Clinically, gestation is counted from the first day of the last menstrual period (LMP) and averages 40-41 weeks (roughly ten calendar months) with fertilization occurring within 1 - 3 weeks after LMP. Thus, the decision and act of conception precede the first completed month recorded as the start of the pregnancy spell. These conventions imply that the calendar start of a pregnancy typically occurs about 1-2 after conception. To correct for this, we define conception as a binary indicator equal to 100 two months before the recorded start month of every pregnancy spell, and 0 otherwise. For pregnancies ending in a live birth, this is approximately equivalent to lagging a birth indicator by ten months.

Figure 4 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 either outcome, indicating that the calendar data are reliable.[19]

[18] In our main specification, for a woman who adopts in a given month t, $t+1$ and subsequent months after adoption are again coded with 0, but our results are robust to other setups such as dropping women from the sample once they have adopted or excluding them for six months after adoption.

[19] This observation is also confirmed when plotting the number of contraceptive adoptions and conceptions over months before the interview (see Figure A1 in the Appendix).

In the main analysis, we split the sample at April 2006, the introduction of performance contracts, and estimate effects of *Umuganda* meetings separately for the 12 months before and the 12 months after. Focusing on this narrow window helps attribute any change in effects to the reform. 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 our regression relies on within unit variation (women fixed effects) and thus requires at least two observations for the same unit.

4.2 Alternative outcome: Anti-Malarial Bed Nets

For our robustness checks and mechanism analysis we will use data on bed net adoption. We use information from two rounds of the Rwandan DHS to study households' acquisitions of 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 covered 7,287 households in 246 geo-coded communities.[20] Both surveys collect information about 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 is available for nets acquired within six months before the interview.

Figure 5 presents the raw data of the total number of 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. While there may still be some measurement error for the 10 and 11 months before the interview, it should not be systematically different between places with more/less Saturday rain given their overall rainfall level.

[20]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.

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 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 bed nets.

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 non-rainy day is defined as a day with rainfall below 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 reliable meeting attendance data is unavailable, we follow Bonnier et al. (2020) and estimate the reduced-form relationship. Low rainfall levels on Saturdays proxies for a well attended meeting. This identification strategy rests on two assumptions. The first is that Saturday rainfall lowers participation at *Umuganda* within a community. Second, the reduced-form effect of Saturday rainfall on behavior change at the community level operates mainly through its effect on *Umuganda* meetings.

5.1 Rainfall and *Umuganda*

We proxy for successful *Umuganda* meetings with "non-rainy" days. 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 as explained in section 2.3. Hence, both the program and the information about the meeting point are likely inhibited by rainfall. Moreover, rainfall often affects road quality and the ability of cell, sector and district leaders to travel to a specific village to attend the *Umuganda* meeting.

Several other studies document and exploit a negative relationship between rainfall and at-

tendance at different types of events (see e.g. Fraga and Hersh, 2010; Madestam et al., 2013; Moreno-Medina, 2023). 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 genocidal violence in 1994. 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. [21] [22] 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 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 (American Meteorological Society, 2012). 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.[23] 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 ac-

[21] 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.

[22] 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).

[23] 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.

cept an excuse not to attend, and 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, and related, even small dips in participation can make *Umuganda* meetings less effective for policy implementation, 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 "non-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.

5.2 Alternative explanation for the "Saturday effect"

Our empirical strategy relies on two counterfactual assumptions. One is that low Saturday rainfall affects our outcomes only through its effect on *Umuganda* meeting attendance, and the other is that the introduction of performance contracts was the only change that altered the objectives of these meetings in April 2006.

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. One possible candidate would be market access. What if a non rainy Saturdays frees up additional time to go buy contraceptives? We will discuss this potential channel and show, using additional sources and descriptive DHS data, that access is unlikely to explain the results. This 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.

5.3 Specification

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

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

y_{it} is a binary indicator of behavior change (contraceptive adoption, in our main specification) 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. $norain_{ctd}$ is the number of days with rainfall below or equal to 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 non-rainy day is defined by rainfall below 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 adoption in any given month following from an additional non-rainy day on the different weekdays in the same month. The interpretation as percentage point changes follows from the dependent variable, the adoption indicator, taking values of either 0 (no change) or 100 (change). Most interesting is

the coefficient on non-rainy Saturdays, which can be interpreted as the effect of an additional successful *Umuganda* meeting. Low 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 levels and behavior change.

To identify the effect of performance contracts on contraceptive adoption 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.[24] We present estimates from separate regressions with before and after data to interpret each of the two point estimates on non-rainy Saturdays as the effect of a well attended *Umuganda* meeting. Their difference, the Differences-in-Differences, is mostly relevant to show that performance contracts led to a statistically significant 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 results

Table 4 presents the relationship between our outcome variable, contraceptive adoption, and the total number of days with rainfall below or equal to 3 mm for each weekday in a month. The point estimate on # non-rainy Saturdays can be interpreted as the effects of an additional successful *Umuganda* meeting, which is a meeting that has sufficient 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 non-rainy Saturdays is strongly statistically significant at the 99% confidence level. It suggests

[24] 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.

that an additional *Umuganda* meeting increases 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, the coefficients for rainfall on other weekdays are statistically insignificant.[25]

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 non-rainy Saturdays, is statistically significant at conventional levels. P-values for the differences in corresponding coefficient estimates between regressions 1 and 2 are presented 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 only after the introduction of performance contracts for local leaders.

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

[25] Appendix Table A1 shows corresponding estimates to Table 4 when excluding adopters from the data for 6 months. Appendix Table A2 shows estimates for the "after" period from regressions for each weekdays without the other weekday controls. Results remain robust in these alternative specifications.

6.2 Robustness Checks

In this section, we present a number of robustness checks. We first show that our main results are robust to the choice of rainfall threshold, and unaffected by potential multicollinearity in the weekly rainfall variables. We then show that the patterns observed for contraceptives are also present for adoption of another incentive health technology, bed nets. Finally we investigate the pattern of rainfall in different weeks of the month.

6.2.1 Robustness to threshold choice

Figure 7a shows robustness of the results after the introduction of performance contracts with respect to two dimensions. First, the effects of non-rainy Saturdays 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 the figure, 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 $\leq$ a certain threshold, controlling only for unit of observation and time fixed effects, but not rainfall on other weekdays.

Figure 7a shows that the effect of Saturdays rainfall on contraceptive adoption is statistically significant when using thresholds between 2 mm and 10 mm rainfall. The coefficient estimate 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. The remaining panels of Figure 7 present results from equivalent regressions of our main outcome on rainfall on each of the other weekdays. In this battery of 60 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.

6.2.2 Robustness to alternative outcome

Table 5 estimate analogue relationships to our main result but for the another health technology that was targeted by the government both since the 2004 Health sector plan, and in Imihigo performance contracts: anti-malarial bed nets. The data structure in these two regressions is slightly different from in our main results table. Acquisitions are observed at the household level and the time dimension of the panel data are months-before-the-interview. Regression 1 of Table 5 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 2 uses equivalent data from DHS 2005, before the introduction of performance contracts. In regression 1, the reduced-form estimate on non-rainy Saturdays is highly significant at the 99% confidence level (similar to regression 1). None of the estimates for other weekdays are statistically significant at 95% confidence level and coefficients are small. The relative effect of the point estimate on non-rainy Saturdays is 10%.

In regression 2, 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 1 and 2 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 outcome suggests that 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 include these health priorities in the agenda of meetings, despite local resistance.

6.2.3 Tracing the Effects

Table 6 traces the effects of non-rainy Saturdays 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 below or equal to 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 below 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 during this period.

Finally, the long panel data of contraceptive adoption allows us to study the effect of Saturday rainfall over time. Figure 6 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 non-rainy Saturday estimates effects become positive and statistically significant upon or shortly after the introduction of performance contracts (vertical line on April 2006).[26] The estimate becomes statistically significant at the 95% level in the estimate based on the period Jan-Dec 2006 (displayed above 2006m7 in the figure) where the last 9 months are after the introduction of the performance contracts, and the effect persists for roughly a years after the introduction of performance contracts and then seems to fade away (the regression based on data from February 2007 - January 2008, displayed above 2007m8, and subsequent regression estimates, cannot be distinguished from zero). This finding may mechanically arise if the fraction of women who are neither using modern contraception nor being affected by *Umuganda* meetings decreases over time, which is likely.

[26]Note that the regression estimates are displayed above the 7th month of the rolling window used for each regression

6.3 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 also 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 bed nets, information on the source is available if the net is obtained up to 6 months before the interview. Table A3 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 5) 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 for bed net acquisition.

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 Mechanisms

We have documented a strong, robust effect of non-rainy Saturdays on contraceptive adoption in the period after performance contracts were introduced. The fact that the relationship between meetings and adoption is significant only with performance contracts in place is a first piece of evidence that top-down mechanisms are at play during the community meetings. We will now provide additional evidence indicating a mechanism for these effects based on top-down 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 the demand for bed nets.

7.1 Evasive Behavior

Enforcement can lead to evasion. One way for a woman to evade contraception is to get pregnant. If the *Umuganda* community meetings affect contraceptive adoption through pressure then meetings should also increase conception. The rationale is that a fraction of women do not wish to use contraceptives since they wish to have more children, and some of those women can expedite pregnancy in the short run to avoid the pressure to start using contraceptives. However, pregnancies will eventually naturally decrease with more contraceptive use, and we found that non-rainy Saturdays lead to increased contraceptive use. Therefore, by looking at the short term effect on Saturday rainfall on pregnancies in a regression, we can understand the mechanisms through which *Umuganda* increases contraceptives. A positive coefficient (i.e. one pointing in the opposite direction as our coefficient for contraceptives) suggests a mechanism based on pressure, while a negative coefficient (same direction as the effect for contraceptives) indicates that *Umuganda* rather reinforced local demand for contraceptives.

Regressions 1 in Table 7 shows the relationship between conception and low rainfall on

different weekdays after the introduction of performance contracts. The coefficient estimate on non-rainy Saturdays is positive and statistically significant at the 95% confidence level. It suggests that one more *Umuganda* meeting increases the probability to become pregnant in the same month by 8%. This finding 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.

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 by objective demand for bed nets

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.[27] This variation in the objective usefulness of bed nets should be strongly correlated with demand because people are reluctant to invest time and money for something they do not need. With lower levels of demand, 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 where the need for bed nets is low.

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

[27] 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.

low support for targets in bed nets. Regression 3 and 4 in Table 7 estimate the relationship between acquisition of bed nets and low rainfall on different weekdays after the introduction of performance contracts respectively using only communities located above and below median altitude. The coefficient for non-rainy Saturdays is positive 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 between the two subsamples, 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*.[28] Furthermore, it demonstrates that the Rwandan government increased the distribution of 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 in the implementation of development goals, and on their interaction with performance contacts for local leaders that were introduced in 2006. Specifically, it investigates the effects of meetings on Rwanda's spectacular progress in contraceptive adoption during the early 2000's. Identification comes from exogenous variation in meeting attendance over time induced by rainfall. We compare the effects of meetings before and after the introduction of performance contracts that strongly increased leaders' incentives for top-down monitoring of the targeted behaviors. We show a positive and significant relationship between community meetings and adoption of contraceptives and that shows up only after the performance incentive reform. Before the reform, these meetings have no effects on contraceptive adoption. The same pattern is observed for another health technology, anti-malarial bed nets. 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, and patterns consistent with pressure being used to increase contraceptive uptake.

[28] Reassuringly, our main effect for contraceptives after performance contracts were introduced is large and highly significant for both high- and low- altitude subsamples, results available upon request.

These findings have two important implications. First, they challenge the common idea that community meetings generate development primarily through enabling bottom-up pressure. By showing that fast-track development through a local institution worked only when the incentives for top-down monitoring were increased, our findings indicate that successful community-based development programs may, in fact, have helped 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 potentially adverse consequences. In the private domain on family planning, pressure to change behavior can lead to political backlash effects as shown in León-Ciliotta et al. (2025).

These findings offer new insights into the mechanisms of accountability and highlight the potential for local institutions to serve as catalysts for development outcomes achieved through either participatory, bottom-up processes or directive, top-down, methods. They also provide 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 tendencies.

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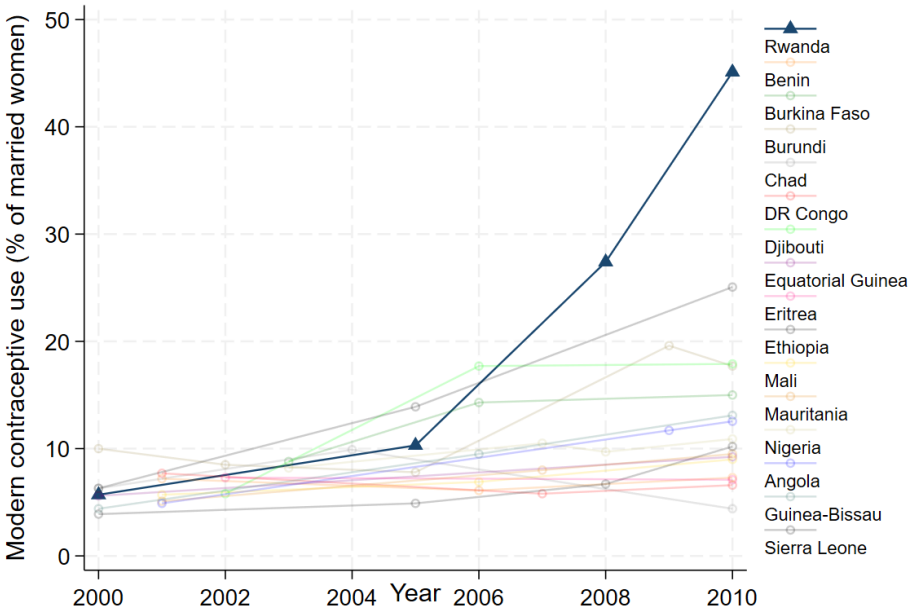
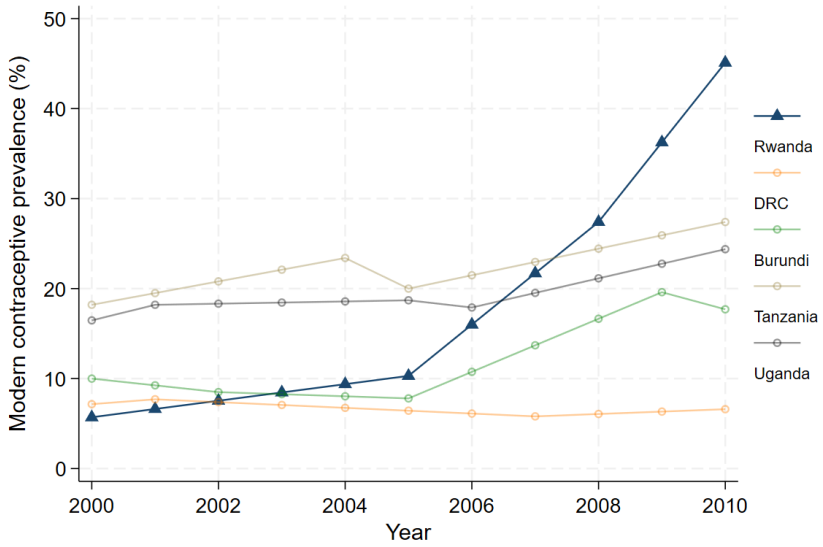
Figures and Tables

Figure 1: Contraceptive Adoption in Africa in 2000's



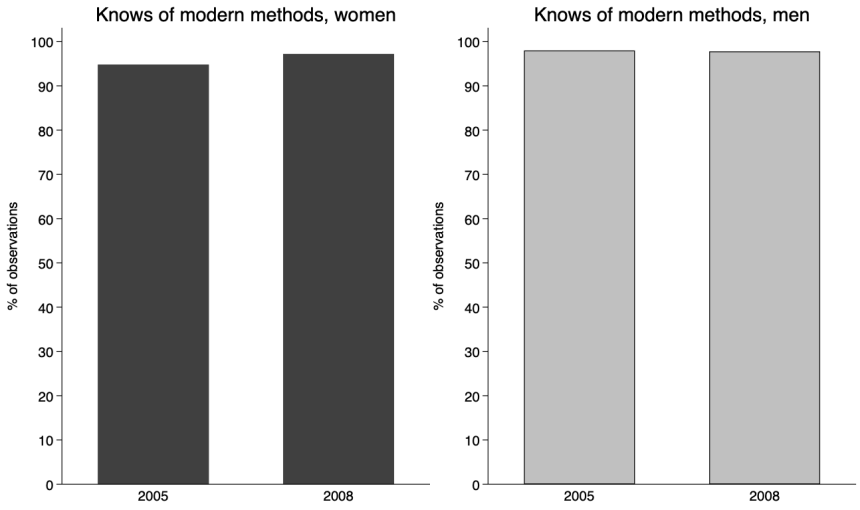
Notes: Graph based on data from UN WPP (2024); HFD (2024) – w. major processing by Our World in Data for the sample of African countries with contraceptive adoption rates similar to Rwanda (between 0-10%) in year 2000. This data shows only contraceptive adoption among married women, which is why the adoption share for Rwanda is about twice as high as for the entire sample of women.

Figure 2: Contraceptive Adoption in Neighboring Countries, 2000's



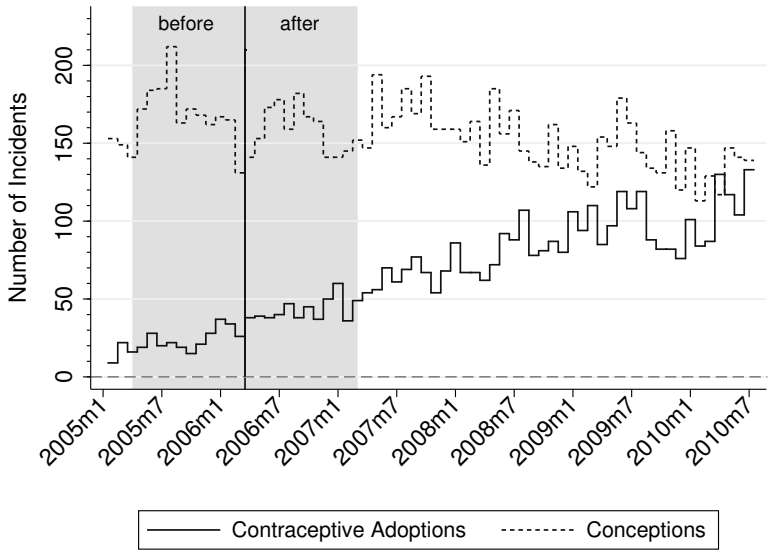
Notes: Graph based on data from UN WPP (2024); HFD (2024) – w. major processing by Our World in Data for the countries bordering Rwanda. The data shows only contraceptive adoption among married women, which is why the adoption share for Rwanda is about twice as high as for the entire sample of women.

Figure 3: Information about contraceptives



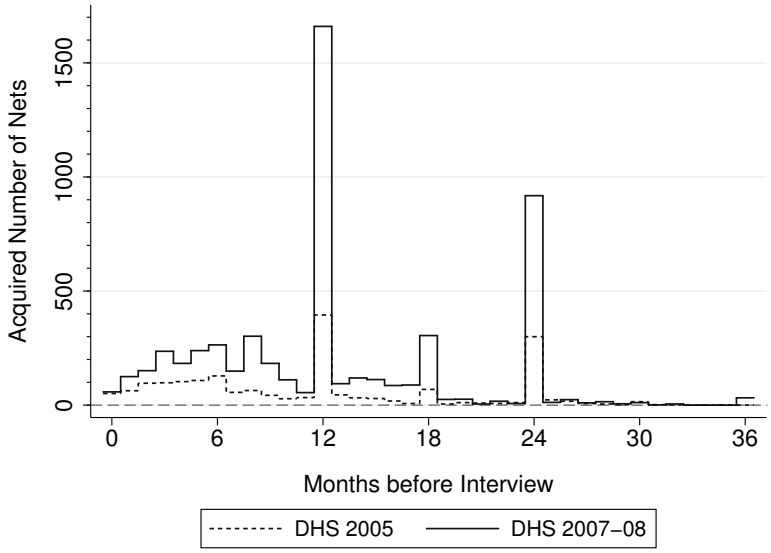
Notes: Graph based on Rwandan DHS data from 2005 and 2008, displaying the share of respondents, divided by sex, who report being familiar to at least one of the modern contraceptives listed in the DHS survey. The shares were 94.8 for women and 98.0 for men in 2005, and 97.2 for women and 97.8 for men in 2008.

Figure 4: Number of Contraceptive Adoptions and Conceptions over Time



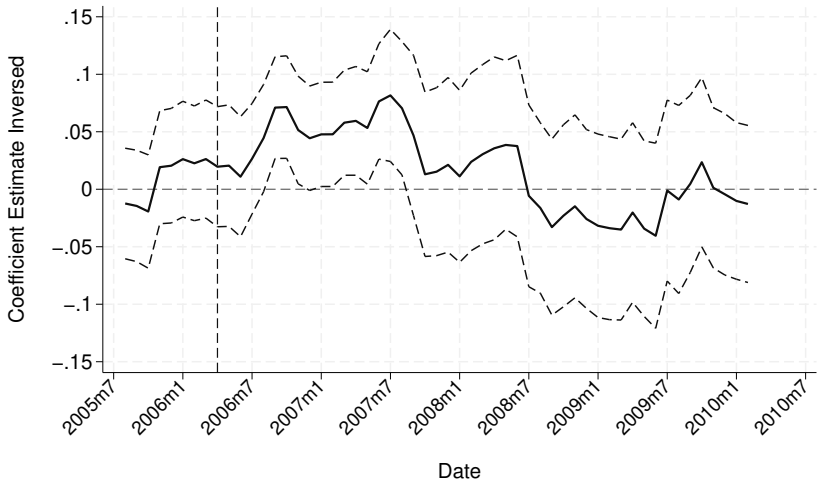
Notes: Based on 13,413 women between 15 and 49 years old and who are usual residents of interviewed households in the 2010 Rwandan DHS. The solid, vertical line marks the introduction of performance contracts in April 2006.

Figure 5: Number of Mosquito Bed Nets Acquired in Different Months

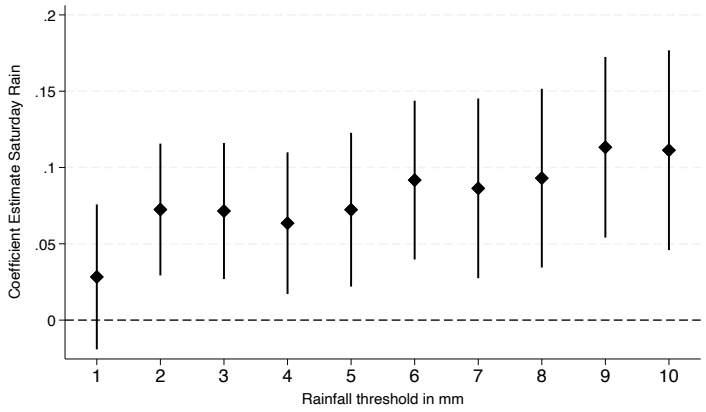


Notes: Based on 10,146 and 7,287 households with GPS coordinates in DHS 2005 and DHS 2007-08 data.

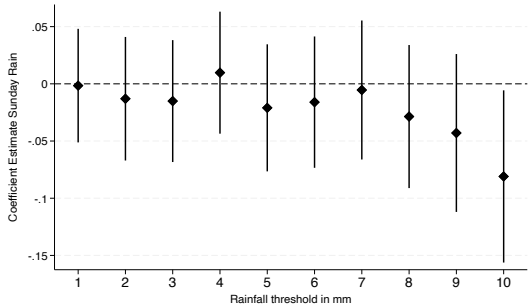
Figure 6: Effect of Low Saturday Rainfall on Contraceptive Adoption over Time



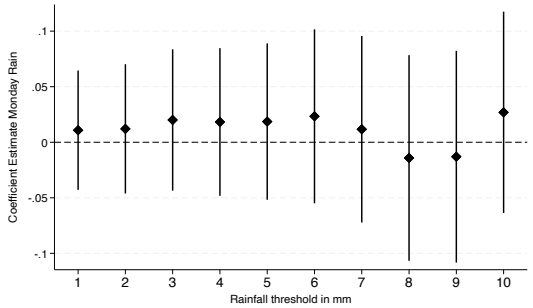
Notes: The figure presents rolling window coefficient estimates on # Sat.(Rainfall≤3mm) (solid line) and 95% confidence intervals (dashed lines). The dependent variable is a monthly, binary indicator of contraceptive adoption. # Sat.(Rainfall≤3mm) is the number of Saturdays with rainfall below or equal to 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, i.e. the estimate displayed above 2006m7 is based on the period January - December 2006, the estimate displayed above 2007m1 is based on the period July 2006 â June 2007, and so forth. The vertical line on April 2006 marks the introduction of performance contracts. Standard errors are clustered at the community level.



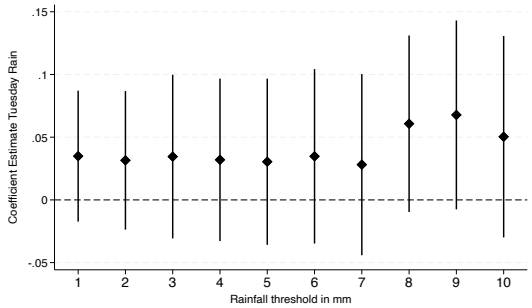
(a) Nonrainy Saturday coefficients



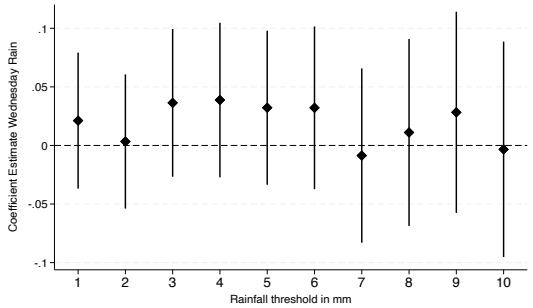
(b) Nonrainy Sunday coefficients



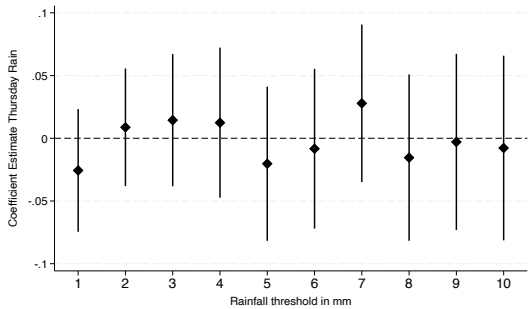
(c) Nonrainy Monday coefficients



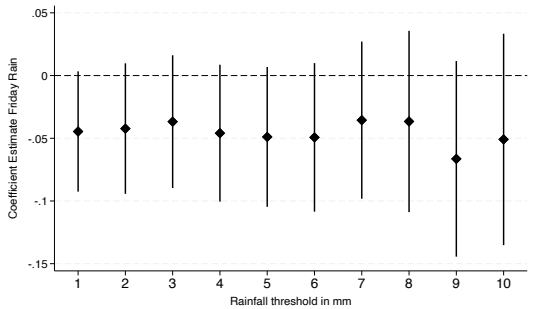
(d) Nonrainy Tuesday coefficients



(e) Nonrainy Wednesday coefficients



(f) Nonrainy Thursday coefficients



(g) Nonrainy Friday coefficients

Figure 7: Robustness of weekday coefficients in main results ("after") to different rainfall thresholds

Notes: Coefficients and 95% confidence intervals from running the same regression as in Table 4 for the period after the introduction of performance contracts, for rainfall thresholds between 1 and 10 mm.

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 (Inverse Definition)	Mean	Std. dev.	Min.	Max.	Obs.
# Sat.(Rainfall$\leq$1mm)	3.126	1.232	0	5	11,808
# Sat.(Rainfall$\leq$2mm)	3.424	1.131	0	5	11,808
# Sat.(Rainfall$\leq$3mm)	3.611	1.053	0	5	11,808
# Sat.(Rainfall$\leq$4mm)	3.755	0.968	0	5	11,808
# Sat.(Rainfall$\leq$5mm)	3.863	0.897	1	5	11,808
# Sat.(Rainfall$\leq$6mm)	3.942	0.847	1	5	11,808
# Sat.(Rainfall$\leq$7mm)	4.001	0.799	1	5	11,808
# Sat.(Rainfall$\leq$8mm)	4.050	0.764	1	5	11,808
# Sat.(Rainfall$\leq$9mm)	4.102	0.727	1	5	11,808
# Sat.(Rainfall$\leq$10mm)	4.136	0.697	1	5	11,808

Notes: # Sat.(Rainfall$\leq X$mm) is the number of Saturdays in a month with rainfall below or equal to X mm. Based on 492 communities in the 2010 Rwandan DHS, each observed for 24 months: between April 2005 and March 2007.

Table 4: Main Effects

Dependent variable:	Contraceptive Adoption		
Panel data:	after	before	
	(1)	(2)	p-value (1) - (2)
# Sat. (Rainfall $\leq$ 3mm)	0.071***	-0.019	[0.008]
	(0.023)	(0.025)	
# Sun. (Rainfall $\leq$ 3mm)	-0.015	-0.029	[0.688]
	(0.027)	(0.022)	
# Mon. (Rainfall $\leq$ 3mm)	0.020	-0.004	[0.575]
	(0.032)	(0.028)	
# Tue. (Rainfall $\leq$ 3mm)	0.034	0.009	[0.522]
	(0.033)	(0.021)	
# Wed. (Rainfall $\leq$ 3mm)	0.036	-0.012	[0.213]
	(0.032)	(0.021)	
# Thu. (Rainfall $\leq$ 3mm)	0.014	0.002	[0.750]
	(0.027)	(0.027)	
# Fri. (Rainfall $\leq$ 3mm)	-0.037	0.020	[0.106]
	(0.027)	(0.023)	
Observations	130,966	125,193	
Unit & Time FE	Yes	Yes	
Dep. var. mean	0.395	0.228	

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 regression 1 (2). The dependent variable, Contraceptive Adoption is a monthly, binary indicator. # Sat.(Rainfall$\leq$3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). The unit of observation is a woman and a time step is a calendar month. Regression 1 uses data for April 2006 to March 2007 (here we observe 11,103 women for 12 months, but aboyt 2,000 woman-month observations are missing because. Regression 2 uses data for April 2005 to March 2006. Standard errors are clustered at community level. P-value: *** p$<$ 0.01, ** p$<$ 0.05, ** p$<$ 0.1.

Table 5: Robustness: Bednets as outcome

Dependent variable:	Bed Net Acquisition		
Panel data:	after	before	
	(1)	(2)	p-value (1) - (2)
# Sat.(Rainfall≤3mm)	0.174***	0.009	[0.018]
	(0.063)	(0.030)	
# Sun.(Rainfall≤3mm)	-0.105	-0.038	[0.363]
	(0.068)	(0.031)	
# Mon.(Rainfall≤3mm)	0.151**	0.002	[0.028]
	(0.062)	(0.028)	
# Tue.(Rainfall≤3mm)	0.006	-0.030	[0.621]
	(0.066)	(0.033)	
# Wed.(Rainfall≤3mm)	0.061	0.029	[0.691]
	(0.071)	(0.033)	
# Thu.(Rainfall≤3mm)	-0.049	0.049	[0.166]
	(0.063)	(0.032)	
# Fri.(Rainfall≤3mm)	-0.103*	0.019	[0.061]
	(0.058)	(0.029)	
Observations	87,444	121,752	
Unit & Time FE	Yes	Yes	
Dep. var. mean	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 regression 3 (4). The dependent variable, Bed Net Acquisition is a monthly, binary indicator. # Sat.(Rainfall≤3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). The unit of observation 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 6: Tracing the Effects Under Performance Contracts

Dependent variable:	Contraceptive Adoption		Bed Net Acquisition	
	(1)	(2)	(3)	(4)
# Sat.(Rainfall≤3mm)	0.068***		0.171***	
	(0.023)		(0.066)	
First Sat.(Rainfall≤3mm)		0.064		0.096
		(0.057)		(0.124)
Second Sat.(Rainfall≤3mm)		0.117*		0.195*
		(0.060)		(0.117)
Third Sat.(Rainfall≤3mm)		0.084*		0.215*
		(0.051)		(0.125)
Fourth Sat.(Rainfall≤3mm)		0.009		0.187
		(0.093)		(0.132)
Fifth Sat.(Rainfall≤3mm)		0.067		0.183*
		(0.054)		(0.109)
L1.# Sat.(Rainfall≤3mm)	-0.046*		0.003	
	(0.027)		(0.062)	
L2.# Sat.(Rainfall≤3mm)	0.020		-0.074	
	(0.025)		(0.058)	
L3.# Sat.(Rainfall≤3mm)	-0.011		-0.098*	
	(0.028)		(0.057)	
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 below or equal to 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 below or equal to 3 mm and 0 otherwise. Second, Third and Last Sat.(Rainfall≤3mm) are corresponding indicators for low rainfall on the other Saturdays in the calendar month. Other Rainfall Regressors are the numbers of days with rainfall below or equal to 3 mm for every other weekday. In regressions 1 and 2, the unit of observation is a woman, a time step is a calendar month 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.

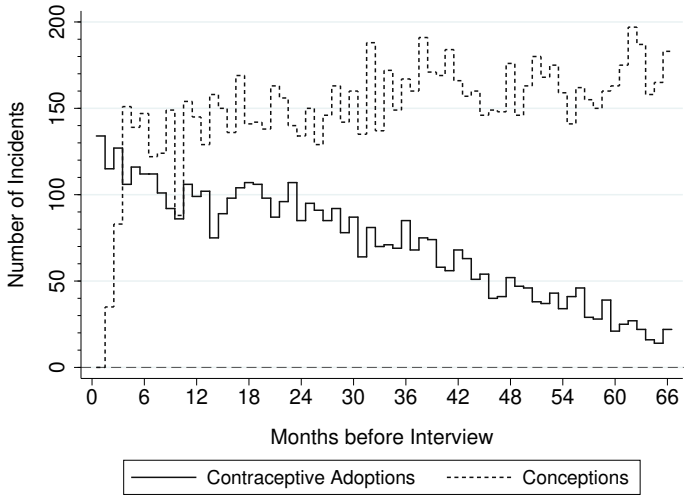
Table 7: Mechanisms

Dependent variable:	Conception			Bed Net Acquisition	
Panel data:	after	before		Median split by altitude	
				high	low
	(1)	(2)	p-value (1) − (2)	(3)	(4)
Sat. (Rainfall $\leq$ 3mm)	0.121**	-0.062	[0.024]	0.232***	0.139
	(0.048)	(0.066)		(0.087)	(0.089)
Sun. (Rainfall $\leq$ 3mm)	-0.050	0.075	[0.143]	-0.134	-0.053
	(0.057)	(0.059)		(0.095)	(0.102)
Mon. (Rainfall $\leq$ 3mm)	-0.063	-0.016	[0.614]	0.143*	0.158
	(0.062)	(0.068)		(0.080)	(0.096)
Tue. (Rainfall $\leq$ 3mm)	0.088	0.053	[0.705]	0.104	-0.069
	(0.061)	(0.066)		(0.099)	(0.090)
Wed. (Rainfall $\leq$ 3mm)	0.145**	0.005	[0.096]	0.006	0.103
	(0.062)	(0.058)		(0.103)	(0.102)
Thu. (Rainfall $\leq$ 3mm)	-0.024	0.133*	[0.079]	-0.054	-0.032
	(0.052)	(0.070)		(0.080)	(0.100)
Fri. (Rainfall $\leq$ 3mm)	-0.075	0.005	[0.323]	-0.020	-0.187**
	(0.055)	(0.062)		(0.074)	(0.091)
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$\leq$3mm) is the number of Saturdays with rainfall below or equal to 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 month. 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..

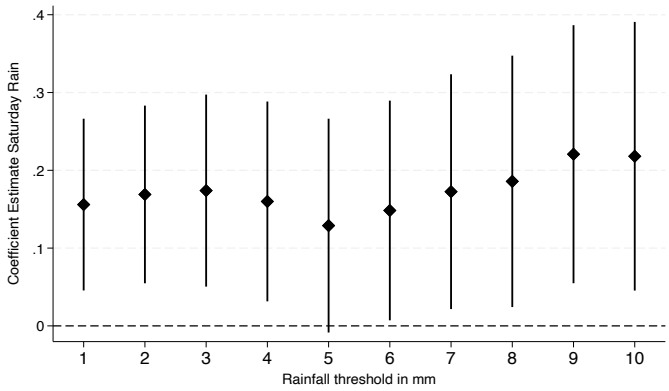
Appendix (for online publication)

Figure A1: Number of Contraceptive Adoptions & Conceptions by months before interview



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.

Figure A2: Robustness of Saturday coefficient for bed net adoption ("after") to different rainfall thresholds



Notes: Coefficients and 95% confidence intervals from running the same regression as in Table 5 for the period after the introduction of performance contracts, for rainfall thresholds between 1 and 10 mm.

Table A1: Main table, excluding adopters for 6 months

Dependent variable:	Contraceptive Adoption		
Panel data:	after	before	
	(1)	(2)	p-value (1) - (2)
# Sat. (Rainfall $\leq$ 3mm)	0.076***	-0.022	[0.004]
	(0.023)	(0.024)	
# Sun. (Rainfall $\leq$ 3mm)	-0.009	-0.033	[0.479]
	(0.027)	(0.021)	
# Mon. (Rainfall $\leq$ 3mm)	0.029	-0.005	[0.435]
	(0.033)	(0.028)	
# Tue. (Rainfall $\leq$ 3mm)	0.026	0.009	[0.652]
	(0.034)	(0.021)	
# Wed. (Rainfall $\leq$ 3mm)	0.030	-0.003	[0.393]
	(0.032)	(0.021)	
# Thu. (Rainfall $\leq$ 3mm)	0.015	-0.001	[0.677]
	(0.027)	(0.026)	
# Fri. (Rainfall $\leq$ 3mm)	-0.025	0.021	[0.193]
	(0.027)	(0.024)	
Observations	128,239	123,926	
Unit & Time FE	Yes	Yes	

Notes: The table displays estimates from identical regressions to the main results Table 4 while observations from women who adopt contraceptives are excluded from the data for 6 months after adoption. Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, ** p< 0.1.

Table A2: Main table with separate weekday estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Sat.(Rainfall≤3mm)	0.069***						
	(0.022)						
# Sun.(Rainfall≤ 3*mm*)		-0.006					
		(0.027)					
# Mon.(Rainfall≤ 3*mm*)			0.010				
			(0.033)				
# Tue.(Rainfall≤ 3*mm*)				0.039			
				(0.033)			
# Wed.(Rainfall≤ 3*mm*)					0.048		
					(0.032)		
# Thu.(Rainfall≤ 3*mm*)						0.019	
						(0.027)	
# Fri.(Rainfall≤ 3*mm*)							-0.023
							(0.027)
Observations	130,966	130,966	130,966	130,966	130,966	130,966	130,966
Unit & Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Dep. var. mean	0.395	0.395	0.395	0.395	0.395	0.395	0.395

Notes: The table displays results from regressing contraceptive adoption on number of rainy weekdays separately for each weekday in the period with performance contracts (the "'after" period in Table 4, April 2006 to March 2007). The dependent variable, Contraceptive Adoption is a monthly, binary indicator. # Sat.(Rainfall≤3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). The unit of observation is a woman and a time step is a calendar month. Regression 1 uses data for April 2006 to March 2007 (here we observe 11,103 women for 12 months, but about 2,000 woman-month observations are missing because. Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, ** p< 0.1.

Table A3: Source of mosquito nets

Dependent variable:	Bed Net Acquisition from ...		
	Any source	Health Facility	Other source
	(1)	(2)	(3)
# Sat.(Rainfall≤3mm)	0.169*	0.025	0.142**
	(0.091)	(0.062)	(0.065)
# Sun.(Rainfall≤3mm)	-0.152*	-0.025	-0.134**
	(0.088)	(0.065)	(0.063)
# Mon.(Rainfall≤3mm)	0.132*	0.009	0.116*
	(0.077)	(0.050)	(0.059)
# Tue.(Rainfall≤3mm)	-0.004	-0.005	-0.005
	(0.094)	(0.065)	(0.064)
# Wed.(Rainfall≤3mm)	-0.022	-0.067	0.045
	(0.095)	(0.061)	(0.070)
# Thu.(Rainfall≤3mm)	-0.123	-0.058	-0.082
	(0.090)	(0.062)	(0.067)
# Fri.(Rainfall≤3mm)	-0.077	-0.056	-0.023
	(0.086)	(0.059)	(0.060)
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: Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, ** p< 0.1.