Local Institutions and Leader Incentives: The Political Economy of Rwanda's Family Planning Transformation*

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Abstract

Modern contraceptive use in sub-Saharan Africa remains low despite sustained investments. Rwanda is a notable outlier: uptake more than quadrupled in 2005-10, alongside sharp and lasting fertility declines. We study the role of two institutions in driving this change: performance contracts for leaders and local village meetings. Our data cover periods before and after a reform that incentivized local leaders to promote contraceptive adoption, with village meetings serving as a key implementation arena. Using rainfall to generate exogenous variation in meeting intensity, we find that meetings increased uptake only once performance contracts were in place, indicating that top-down factors shaped their effectiveness. After the reform, non-rainy Saturdays, which proxy for well-attended meetings, increase the likelihood of contraceptive adoption by 18% in the same month. Results are robust to another targeted outcome and to different rainfall thresholds. The meeting-driven increase in uptake is unlikely to be driven by improved access; suggestive evidence indicates that adoption was partly involuntary, consistent with pressure during meetings. Our findings shed new light on Rwanda's remarkable health gains and show that local institutions and performance incentives can complement each other in facilitating top-down policy implementation.

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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 creates 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 sharp 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. Among these goals was the expansion of modern contraceptive use, an idea prioritized by the central government already for some time but facing resistance and stigma in the population (Solo, 2008). Performance contracts were introduced simultaneously in all districts, making it difficult to isolate their effect. We exploit variation in participation intensity in a key forum 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 also help leaders influence and monitor uptake of policies. We utilize plausibly exogenous variation in meeting intensity around the launch of performance contracts to asses whether the contracts altered the meetings' effect on this public health outcome. In doing this, we also shed light on the direction of accountability and influence facilitated by these meetings with respect to the family planning policy: bottom-up accountability or top-down directives.

Rwanda provides an interesting case for studying the role and direction of accountability in local institutions. After launching an ambitious health sector strategic plan in 2004, with specific targets for family planning, the country saw remarkable progress in the following decade. Its increase in contraceptive use between 2005 and 2010 is one of the fastest globally (Abbott et al., 2017; UNDP, 2014) and has been sustained over time. This is widely attributed to Rwanda's visionary policy making, including local institutions such as the *Umuganda* community meeting and work program (World Bank Group, 2018; UNDP, 2014), usually described as an enabler of bottom-up forces (MINALOC, 2008a). However, Rwanda is also characterised by

[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

authoritarian leadership,[2] and qualitative accounts document pressure and enforcement within these institutions (see e.g. Desrosiers, 2023; Huggins, 2009; Thomson, 2008). 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 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. Our identifying assumption is that (unobserved) meeting attendance decreases with Saturday rainfall, which is motivated both by specific features of these 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) and has previously been used for Rwanda's *Umuganda* meetings by Bonnier et al. (2020)[3]. This strategy also relies on the assumption that low Saturday rainfall affected our outcomes only through its effect on *Umuganda* meeting intensity. This is supported by the fact that the contraceptive technologies we study could not be disseminated at meetings, and Saturday rainfall would not have plausibly altered 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 introduction of performance contracts, a reform that abruptly aligned local leaders' objectives with those of the central government and altered their incentives to promote contraceptive use. We compare the effects of meetings held before and after the introduction of performance contracts in April 2006. This comparison is justified by two observations: *Umuganda* meetings served as official platforms for advancing performance-contract goals (Abbott et al., 2017), and modern contraceptives were a stated topic at those meetings (MoH, 2008; Sommers, 2012). We are not aware of any contemporaneous policy change that would differentially affect contraceptive uptake following non-rainy Saturdays apart from the introduction of performance contracts.

In a simple conceptual framework we distinguish between two classes of channels through

[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 meeting (Saturday rainfall) effects on civilian participation in the Rwandan genocide. In contrast, we exploit variation within observation units over time.

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

which meetings might affect take-up: bottom-up channels, in which citizens use meetings to coordinate and persuade local leaders to improve access to contraceptives, and top-down channels, in which leaders use meetings to shape individual behavior directly. The latter may operate by improving general information, facilitating belief updating about community norms, or exerting pressure through public accountability, shaming, or peer enforcement. These top-down channels imply different empirical patterns: The first two would increase voluntary adoption, while pressure-based channels may generate reluctant adoption and be accompanied by evasive responses. Moreover and importantly, bottom-up effects would be present regardless of leader incentives, whereas top-down effects should become more salient after performance contracts strengthened leaders' incentives to promote contraceptive use.

Exploiting within-woman variation in the number of non-rainy Saturdays across months, 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% relative to the average. In contrast, Saturday rainfall has no significant correlation with contraceptive adoption in the year before performance contracts. Coefficients for other weekdays are consistently insignificant. These main findings show that top-down channels were crucial: meetings only matter once local leaders are incentivized to promote contraceptive adoption. Notably, these results are driven by hormonal methods, which require a clinic visit and cannot be distributed at meetings.

It is important to note that our design identifies the effect of meeting frequency at the intensive margin: variation in the number of meetings per month, rather than the extensive margin effect of having meetings at all. We therefore cannot quantify how much of Rwanda's overall contraceptive expansion is attributable to *Umuganda*. However, our mechanism tests below speak to the channels through which meetings affected adoption more broadly. Results are robust to different rainfall thresholds, and to an alternative outcome, anti-malarial bed nets, that was also emphasized in both the Health Sector strategic plan and in performance contracts.

In terms of mechanisms, knowledge of modern contraceptive methods and where to obtain them was high (above 95%) throughout the study period and, as noted above, access was not directly affected by meetings or Saturday rainfall.[5] Agricultural income channels are also unlikely to drive our results, since crop yields respond to seasonal precipitation rather than to the high-frequency, day-specific rainfall variation that identifies our Saturday effect.

To further understand whether the meeting effect operated through bottom-up or top-down

[5]Neither was access considered a binding constraint in survey data from the year before our period of interest.

channels we examine whether there are signs of resistance to the policy, and heterogeneity across groups that differ in their underlying demand. First, 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 pressured to otherwise adopt contraception. Our data supports this: while non-rainy Saturdays (more meetings in a month) increased contraceptive adoption we also find that they correlate with an 8% increase in conception rates in the same month. This effect is driven by Catholic women, which is in line with the Catholic church being a vocal opponent of the family planning policy. Meanwhile, the increase in contraceptive adoption is positive and significant for both Catholic and non-Catholic women. Second, we analyze the heterogeneity in how the meetings (non-rainy Saturdays) affect adoption of our alternative outcome: bed nets. We exploit the inverse relationship between mosquito prevalence and altitude. We find that the impact of meetings is somewhat 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. Our findings are consistent with pressure and enforcement during the meetings, though we cannot rule out that leaders' public emphasis on contraceptives also contributed to adoption by changing perceptions about their social acceptability.

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 about nor access to contraceptives appear to be binding constraints, but the meeting format might both facilitate belief updating among the local population about the acceptability of the technology and enable leaders to invoke pressure on women and households to adopt. Performance incentives sometimes have perverse side-effects.[6] We find indications of pressure on citizens, and related work on incentives in the fertility domain have been shown to encourage coercive and unethical behavior leading to later political backlash (León-Ciliotta et al., 2025; Pelras and Renk, 2023).

Finally, we contribute to the literature on local institutions as instruments for development. 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, local institutions are also 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, study the *Umuganda* community meetings in the 1990's, and find that greater meeting intensity just before the 1994 Rwandan genocide is associated with higher local civilian participation in violence. By jointly considering accountability and local institutions, we show that performance incentives and local institutional structures can complement each other in addressing accountability problems in delegated tasks.

Section 2 of the paper provides background on Rwanda's family planning goals, policies, and attitudes, and describes *Umuganda* meetings and performance contracts. Section 3 presents the conceptual framework. Sections 4 and 5 describe the data and empirical strategy. Sections 6 and 7 present the results, robustness checks, and potential mechanisms. Section 8 concludes.

2 Background

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

[6]For example, Khan et al. (2015) finds that performance incentives increase bribes to tax-collectors, and Dhaliwal and Hanna (2017) indicate that monitoring lowers job satisfaction and leads to evasion.

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, the objective was to increase modern contraceptive prevalence from 4% to 20% by 2010 (MoH, 2004). The primary motivation was arguably economic development, with reduced fertility presented as a necessary condition for poverty reduction (Solo, 2008).[7]

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). In particular, the Rwandan Catholic church was a vocal opponent to the family planning policy focus (Solo, 2008). Data from the 2005 DHS reveals that 25% of men believed that women become promiscuous when using contraception, with a higher percentage among young men. Meanwhile, when asked about their reason for for not using contraception, 45% of women under 30 quoted pro-natalist ideals or opposition while only 3% of women reported knowledge, access or cost. (NISR and Macro, 2006).[8] DHS data from the period shows high general knowledge about modern contraceptives, above 95%, among both men and women, see Figure A2.

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). 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. By 2010, Rwanda had surpassed its targets, raising modern contraceptive use to 25.2%. Figure 1 shows how contraceptive uptake for *married* women in Rwanda over time compares to that of a synthetic Rwanda composed of other African countries that closely follows Rwanda's development pre-2006, and shows how Rwanda is a clear outlier in terms of modern contraceptive adoption since 2006.[9][10] 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).

[7] 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.

[8] Demand for bed nets was also limited due to Rwanda's geography where much of the population lives at altitudes where mosquitoes are rare (Bodker et al., 2003).

[9] Figure A1 shows how the development in Rwanda compares to its immediate neighbors.

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

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 mayors after district level targets had been formulated. Targets were then passed down through cascading contracts to the three lower levels of public administration: sector, cell and village (MINALOC, 2010; Purdekova, 2011; GoR, 2008).

While *Imihigo* goals are set separately for each district and the policy is officially portrayed as reflecting local priorities, there is ample evidence that most performance contracts targets are set top-down (see e.g. World Bank Group, 2018; Hasselskog and Schierenbeck, 2015; Munyandamutsa, 2011). In the first year after their introduction, 75% of districts' targets were national policies and programs (GoR, 2008). Family planning was part of early *Imihigo* contracts (OSSREA, 2007; Sommers, 2012).[11]

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 this new, institutionalized version of *Imihigo*, media is used to make targets public. These contracts have 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 such 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[12]. Conversely, mayors and other officials risk public humiliation if they perform poorly against the set goals. Within the 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). However, systematic data on relative district performance is not available for the first years after *Imihigo* was introduced, but only starting from 2009. This system arguably creates a competitive atmosphere

[11] 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).

[12] 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)

where leaders strive to outperform one another. 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.[13] 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[14]. 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 this formalization was to embed the existing practice of *Umuganda* as a tool for policy-making into the public administrative structure. For our analysis, it is only important that no policy changed *Umuganda* in 2006. *Umuganda* is 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 leaders and consists of outdoor, physical labor (e.g. clearing bushes or cleaning roads) followed by a meeting held outdoors (Uwimbabazi, 2012).[15] Desrosiers (2023, p. 348) describes Umuganda as part of *"a normative and symbolic authoritarian apparatus [that] gave local implementers a tool to tie*

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

[14] 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'.

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

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 [...] 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[16]. During *Umuganda*, all shops must close and public transport pauses. To enforce participation, local leaders have the discretion to fine absence (MINALOC, 2008b). Multiple sources suggests that many Rwandans participated involuntarily in *Umuganda* during the studied period (Mukarubuga, 2004; Uwimbabazi, 2012; Purdekova, 2011).

We argue that the community meetings held after the physical labor during *Umuganda* can affect contraceptive adoption. These meetings are officially acknowledged by the government as a venue for implementation of performance contract targets (Rwanda Governance Board, 2014). Although Rwandan officials portray these meetings as bottom-up arenas where communities collectively define and address local problems (MINALOC, 2008a), research suggests that in practice they often function as top-down channels through which local leaders communicate government programs and policies (Uwimbabazi, 2012). During our period of interest in this paper, annual reports of Rwanda's Ministry of Health document that family planning was regularly discussed and promoted at *Umuganda* meetings (MoH, 2008, 2009).[17]

To the best of our knowledge, no systematic, reliable, data on *Umuganda* attendance in exists for the studied period. Sources differ on the formal frequency of *Umuganda* after its reintroduction in 1998, but the evidence indicates that meetings were often held weekly during our study period. Organic Law N° 53/2007 states that *Umuganda* must take place on the last Saturday of the month, while allowing communities to hold it more frequently (MINALOC, 2008b). The 2008 revised Community Development Policy, by contrast, states that *Umuganda* takes place every Saturday at the local level (MINALOC, 2008a). Several studies likewise document weekly Saturday meetings in this period (e.g. Hasselskog and Schierenbeck, 2015; Purdekova, 2011; Uwimbabazi, 2012; NAR and Interpeace, 2016). Taken together, these sources suggest

[16] 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."

[17] 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)

that the relevant meeting schedule during our study period was weekly.

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 activate this policy instrument. We also outline implications for the timing of effects and the expected patterns for contraceptive adoption and related behaviors. Our empirical strategy exploits variation in meeting intensity, so we focus our framework on channels that operate through meetings rather than other ways in which leader incentives could affect uptake.

3.1 Channels through which meetings may affect adoption

Meetings can affect uptake of contraceptives through two classes of channels: The first class relies on *bottom-up* influence and downward accountability. If there is unmet demand for contraceptives, citizens can use the meetings to coordinate and persuade 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 class of channels rely on *top-down*, leader initiated elements. First, leaders may 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 update beliefs 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. Such pressure is consistent with the 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[18] Therefore, canceled or ineffective meetings impede implementation and reduce the number of new adopters in a given month.

3.2 Expected effect patterns of different channels

Timing: Bottom-up efforts reflect citizen initiative and should be unaffected by changes in leaders' incentives. If meetings affect contraceptive uptake by such citizen initiatives and coordination 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 belief 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. Population groups may vary in their preferences over contraceptive use and therefore respond differently when local leaders emphasize its importance. Religious denomination is one salient dimension of heterogeneity: given the Catholic church's vocal opposition to contraception, we may expect more resistance among Catholic households.

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.

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

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 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 implants, injections, and IUDs, 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 using modern contraception, is pregnant, 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.[19]

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.[20]

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 a systematic timing error because 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. Correcting for this error and taking into account that pregnancy actually lasts 9.5 months[21] places the

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

[20]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, as shown in Section 6.2.

[21]Clinically, gestation is conventionally counted from the first day of the last menstrual period (LMP) and averages 40 weeks, or almost ten calendar months, with fertilization occurring 1-3 weeks after the LMP.

start of pregnancy one month earlier than the start month recorded in the calendar. Because the relevant decision period precedes fertilization, we assign conception to the month before the corrected pregnancy start: two months before the DHS-recorded start of each pregnancy spell. For pregnancies ending in a live birth, this is equivalent to lagging the birth indicator by the number of reported full pregnancy months plus one month, most commonly 9+1 months.

Figure 2 presents the number of contraceptive adoptions and conceptions for each calendar month 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. We do not observe unusually high concentrations on certain dates for either outcome, providing reassurance about the reliability of the calendar data.[22]

In the analysis, we estimate effects of meetings separately for the 12 month periods before and the 12 months after April 2006, the introduction of performance contracts. Focusing on this narrow window helps attribute any change in effects to the reform. Table 1, panel A, 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. Unlike for reproductive health outcomes, no single DHS round contains a long retrospective module of this outcome that would allow us to construct a panel. We therefore use information from two rounds of the Rwandan DHS to study households' bed net acquisitions before and after the introduction of performance contracts. The 2005 DHS provides data from before the introduction of performance contracts and contains data from 10,146 households living in 456 communities with available GPS coordinates. the 2007-08 DHS provides data for the "after"-period and covers 7,287 households in 246 geo-coded communities.[23] 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

[22]This observation is also confirmed when plotting the number of contraceptive adoptions and conceptions over months before the interview (see Figure A3 in the Appendix).

[23]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.

the interview, showing that supply is dominated by hospitals and clinics, while pharmacies, shops and kiosk are the most common regular market sources. We restrict our analysis to bed nets acquired in months 0-11 before the interview. Figure A4 presents the raw data of the total number of bed nets that were acquired in each month before the interview, and provides the motivation for this restriction.[24]

For each of the two DHS rounds we construct a separate, retrospective, household level panel data set spanning 0-11 months before the interview. Our outcome is an indicator for whether a household acquired one or more bed nets in a given month before the interview. To 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 that separately capture whether a bed net was acquired from a Health Center or another source (e.g. a pharmacy or shop). Table 1, panel B 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

[24]The figure shows spikes in acquisitions at months 12, 18, and 24 in both surveys, suggesting that reporting precision deteriorates for nets acquired 12 or more months before the interview. Because this variable lacks the consistency checks used for DHS calendar entries, these concentrations likely reflect rounding and recall error. We therefore restrict the analysis to bed nets acquired in months 0-11 before the interview. Any remaining measurement error for months 10-11 should not vary systematically with Saturday rainfall, conditional on overall rainfall.

number of "non-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 our mechanism tests we also construct a measure of whether average rainfall at the village level was above the median in the year prior to our main study period (April 2004-March 2005).

For the analysis, rainfall data are matched to outcome data using community and month identifiers. Table 1, panel C, presents summary statistics of the number of non-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.

4.4 Additional Variables

For our mechanism tests we draw on additional 2010 DHS variables. We construct an individual-level measure of religion, a village-level altitude measure, and a village-level measure of perceived barriers to healthcare. The latter is based on a question asking whether distance to a health facility is a problem when seeking medical advice or treatment (27% answer yes in the full sample); we compute the village-level share of affirmative answers and classify villages above and below the median as high- and low-distance. Finally, we construct a district-level indicator for whether the mayor in 2006 was female, based on Powley (2008), who reports that 6% of mayors (i.e. 2 of 30 districts) were women, and a KT (Kigali Today) Press article[25] identifying the specific districts.

5 Empirical Strategy

In this section we present our empirical strategy, discuss the plausibility of its main identifying assumptions, and present our empirical specification.

[25] https://www.ktpress.rw/2024/01/where-are-they-now-a-look-back-at-the-mayors-who-left-a-mark/, accessed 2026-05-05

5.1 Rainfall and community meetings

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. We proxy for successful *Umuganda* meetings with "non-rainy" days, with the assumption that Saturday rainfall lowers participation at *Umuganda* within a community. Such an effect is likely because meetings and physical labor take place outside under the open sky and, during the studied time period, are usually only communicated on the same day (see Section 2.3). Hence, both the program and the information about the meeting point are likely inhibited by rainfall. Moreover, rain can affect road quality and the ability of cell, sector and district leaders to travel to a specific village to attend the meeting.

Several other studies document and exploit a negative relationship between rainfall and attendance at different types of events (see e.g. Fraga and Hersh, 2010; Madestam et al., 2013; Moreno-Medina, 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. [26] [27] 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 meeting participation, 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,

[26] Note that Bonnier et al. (2020) use a different rainfall dataset than this paper as they study an earlier time period. Rainfall products have improved with time due to better satellite imagery.

[27] 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).

2012). Madestam et al. (2013) use the first mark and define a rainy day as one with above 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.[28] We believe that a lower threshold more in line with Madestam et al. (2013) is justifiable for two reasons. First, most Rwandans dislike participating in *Umuganda*, and would gladly accept an excuse not to attend, 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 Potential confounders

In addition to the basic assumption that low rainfall on Saturdays affects *Umuganda* meetings, our empirical strategy, where we compare the effect of our meeting proxy before and after the introduction of performance contracts, relies on two additional assumptions. One is that low Saturday rainfall affects our outcomes only, or mainly, 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. We next discuss the plausibility of these assumptions and discuss potential confounders.

Two characteristics of our analysis already limit the scope for a channel other than the meetings 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 all other weekdays control for general rainfall effects and serve as natural placebo tests.

This rules out most channels related to agricultural income in Rwanda's predominantly ru-

[28] They motivate this choice with anecdotal evidence from news reports about low participation and cancellations due to rainfall. For their cited 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.

ral setting. Rainfall may affect agricultural output, crop yields, and thus household income, which could in turn shift contraceptive use through affordability constraints or changes in household bargaining. It may also affect agricultural labor, mobility, and household time allocation, thereby influencing women's ability to visit clinics or their receptiveness to health messaging. Yet these factors are shaped by broader rainfall patterns, which we absorb through fixed effects and rainfall controls for all other weekdays. For them to explain our findings, they would need to respond uniquely to Saturday rainfall – a pattern that appears implausible.

Considering our empirical setup, an effect of Saturday rainfall on health behavior must operate through a reoccurring event on that day. One possible candidate is market access: non-rainy Saturdays could free up time to purchase contraceptives. We show below, using additional sources and descriptive DHS data on patterns of service use, that this access channel is unlikely to explain our results.

Taken together, these considerations, along with our placebo checks using rainfall on other days of the week, leave very limited scope for channels unrelated to the social interactions during *Umuganda* meetings.

We argue that the relevant political event in April 2006 was the introduction of performance contracts. Could some other nationwide policy or change have altered the practice or objectives of *Umuganda* meetings with respect to our outcomes and coincided in timing with the introduction of performance contracts? One parallel development in Rwanda in the early 2000's is the introduction of a 30% quota for women representation in politics which resulted in a consistently high share of female parliamentarians since the 2003 election (Freedom House, 2007). Since female political leadership has been shown to shift policy choices toward women's preferences (Chattopadhyay and Duflo, 2004), an increase in female local leaders may have altered the *Umuganda* meetings to focus more on "women's issues", such as family planning. However, at the local level the increase in female leaders was slow. A 2008 report states that *"[c]ultural, social, and economic barriers make it extremely difficult for [Rwandan] women to enter politics, particularly at the local level, where the forces of tradition are the strongest"* (Powley, 2008). In line with this, only 2 out of 30 districts had female mayors after the 2006 election.[29] We will show that our results are not driven by those districts.

[29]Systematic data on lower level elected and appointed officials is not available for the study period but similar constraints that restrict women from becoming district mayors were likely present at more local levels.

5.3 Specification

To estimate the effect of *Umuganda* meetings on adoption, 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 observational unit i during month t. The unit, i, depends on the outcome and is either a woman or a household. Similarly, t may be either a calendar month 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 non-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 month fixed effects. In all estimations, we cluster standard errors at the community (village) level i.e. 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, give the percentage-point change in the probability of adoption in a month per additional non-rainy day d in that month. The units are percentage points because the outcome is an indicator coded 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 other weekdays control for general rainfall effects and are placebo tests. With unit fixed effects, the coefficients β_d are identified from temporal variation in rainfall levels and behavior change.

To identify the differential effect of *performance contracts* on adoption through *Umuganda* meetings, we estimate equation 1 separately before and after the introduction of performance contracts. We interpret each of the two point estimates on non-rainy Saturdays as the effect of a well attended meeting. Subsequently, we test whether the corresponding coefficient estimates from both regressions are statistically different from each other. 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, with unit-by-period fixed effects that control for time-invariant individual characteristics sep-

arately in the before- and after- period.[30] The interaction between non rainy Saturdays and "after", reveals if performance contracts led to a statistically significant change in the effect of meetings, and we directly present the p-values of the Differences-in-Differences term.

To examine heterogenous effects, we estimate:

$$y_{it} = \sum_{d=1}^{7} norain_{ctd} \left(\beta_d + \gamma_d After_t + \delta_d Z_i + \kappa_d After_t \times Z_i\right) + \alpha_{i,After_t} + \tau_t + \varepsilon_{it} \quad (2)$$

Where the notation is as above, $After$ denotes the period with performance contracts, as defined above, and the additional term, Z_i, is the dimension of heterogeneity measured at the individual or community level. $\alpha_{i,After_t}$ are unit-by-period fixed effects, allowing each woman's baseline to differ between the before- and after-period. Note that the coefficient for Z_i and $After$ as well as their interaction are absorbed by the unit-by-period fixed effects.

6 Results

6.1 Main results

Table 2 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.

Column (1) shows results from estimating equation 1 on 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 1% level. It suggests that an additional meeting increases the likelihood of a woman to adopt 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.

Column (2) shows results from estimating equation 1 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

[30] When the outcome is bed net, due to the different data structure, the difference-in-difference style setup includes household fixed effects and month-by-wave fixed effects.

for the differences in corresponding coefficient estimates between Columns (1) and (2) are presented one column to the right in Table 2. The difference in estimates on Saturday rainfall is statistically significant at the 1% level. No other difference is statistically significant at the 10% level or below. This shows 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, pointing to top-down rather than bottom-up channels as the key driver of meetings' effect on contraceptive adoption.

Regarding the magnitude of these effects, the above coefficient estimates can be considered lower bound estimates of the *Umuganda* meetings. The estimates are likely biased toward zero because the number of non-rainy Saturdays is an imprecise measure of the number of well attended monthly meetings. With four to five meetings typically held per month, the cumulative monthly effect implied by our estimates is substantial.

6.2 Robustness Checks

We next present a number of robustness checks. We begin by showing that our main results are robust to the choice of rainfall threshold, and unaffected by potential multicollinearity in the weekly rainfall variables. The results are also robust to excluding women who adopted or got pregnant recently. We then show that the patterns observed for contraceptives are also present for adoption of another incentive health technology, bed nets. Finally we present placebo tests and the pattern of rainfall estimates across different weeks of the month.

Figure 4a shows robustness of the results in the "after"-period to using different thresholds to define a rainy day. Each coefficient estimate (diamond) and 95% confidence interval (bar) is the Saturday coefficient from estimating equation 1 for different rainfall thresholds. The effect of Saturday rainfall on contraceptive adoption is statistically significant when using thresholds between 2 mm and 10 mm rainfall. 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 4a present the coefficients for each of the other weekdays across different thresholds, from the same estimations. Only one coefficient estimate is marginally statistically significant at the 5% level, a result that would be expected to occur by chance given the large number of hypotheses tested.

To assess whether multicollinearity between the rainfall regressors affects the main 3 mm estimate, Table A1 presents results from separate regressions of the outcome on each week-

day's rainfall individually, without controlling for the other weekdays. The Saturday coefficient (Column 1) is reassuringly very similar to the estimate in Table 2.

We next present robustness checks based on alternative definitions of the analysis sample. To align with the structure of the performance contracts, we focus on changes in contraceptive adoption and code adoption as a state that can switch on and off across months. A concern with this approach is potential misclassification of women's adoption status in subsequent months after initial uptake. A related issue is that women who become pregnant are not likely to adopt for several months. In our main specifications, these cases are coded as 0 in all months when they do not adopt contraceptives, regardless of their adoption or pregnancy status. In Table 3, Columns (1) - (2), we replicate the main analysis but exclude women from the sample for six months following adoption (consistent with the multi-month effectiveness of the most commonly used modern methods: injectables and implants). The results closely mirror those in the main table: the Saturday coefficient remains positive, statistically significant, and of similar magnitude. In Columns (3) - (4) we report an analogous exercise excluding women for a period following conception, and also find results very similar to our main findings. In Appendix Figure A6 we also show that our main estimate is not driven by specific districts but is stable to omitting one district at a time.

Next, we estimate analogue relationships to our main result but for another health technology that was targeted by the government both since the 2004 Health sector plan and in performance contracts: anti-malarial bed nets. The results are presented in Table 3, Columns (5) - (6). The data structure in these two regressions is slightly different compared to our main results table. Acquisitions are observed at the household level and the time dimension of the panel data are months-before-the-interview. Column (5) of Table 3 uses 12-months panel data on bed net acquisitions 0 to 11 months before DHS 2007-08 interviews, i.e. after the introduction of performance contracts. Column (6) uses equivalent data from DHS 2005, before the introduction of performance contracts. In Column (5), the reduced-form estimate on non-rainy Saturdays is highly significant, at the 1% level. The relative effect of the point estimate on non-rainy Saturdays is 10%.[31] In Column (6), rainfall on all weekdays, including Saturdays, in the period before performance contracts is statistically insignificant at conventional levels. The p-values for the differences in corresponding coefficient estimates between the periods demonstrate that the effect of Saturday rainfall changed significantly at the 5% level (but a similar difference is observed for Mondays). The similar coefficient pattern for another targeted outcome further

[31] Appendix Figure A5 shows robustness of this estimate to other rainfall thresholds.

supports the interpretation that performance contracts aligned *Umuganda* meetings with national policy. Over the observation period, the setup of the meetings arguably did not change. However, performance contracts allowed the central government to include these health priorities in the agenda of meetings, regardless of local demand.

6.3 Dynamics and Placebo check

The long panel data of contraceptive adoption allows us to study the effect of Saturday rainfall over time. Figure 3 presents estimates from estimating equation 1 on data from a 12-months rolling window, with estimates displayed above the first month of each window. The solid black line connects the coefficient estimates and the dashed curves mark 95% confidence intervals. Non-rainy Saturday estimates become positive and statistically significant upon or shortly after the introduction of performance contracts (vertical line on April 2006). The effect persists for roughly a year after the introduction of performance contracts and then fades away (the estimate based on data from February 2007 - January 2008 cannot be distinguished from zero). This finding may mechanically arise if the fraction of women who neither use modern contraception nor are affected by *Umuganda* meetings decreases over time, which is likely.

Table A2 presents additional results for non-rainy Saturdays on contraceptive adoption focusing on the 12-months panel after the introduction of performance contracts. Column (1) present results from a regression of adoption on number of non-rainy Saturdays along with future non-rainy Saturdays. All estimates on future (lead) Saturday rainfall can be considered placebo tests and are insignificant as we would expect.[32] Column (2) displays results including lagged Saturdays and 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 1% level. Estimates on lagged Saturday rainfall are insignificant with one exception: the coefficient on the first lag is negative and significant at the 10% level. This is consistent with inter-temporal substitution where low Saturday rainfall in month t-1 generates more adoptions that month, leaving a smaller pool of potential adopters in month t. Column (3) of Table A2 presents results from separate estimations for each Saturday of the month (first, second, third, and last). The estimates are similar in magnitude across the four Saturday "types" with better precision for the two middle Saturdays of a month. This is in line with the evidence discussed in section 2 that *Umuganda* was held on multiple Saturdays every month during the studied period.

[32] Separate regressions of each future (lead) Saturday measure, controlling for same month rainfall on all other weekdays, also yield small and statistically insignificant results.

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 this relationship emerges only once performance contracts are in place is a first piece of evidence that top-down, leader-initiated channels drive the effect. In this section we examine the nature of these channels more closely, first ruling out access and other alternative explanations, then turning to two types of evidence on whether behavioral change was voluntary or pressure-driven: first, evasive behavioral responses consistent with enforcement, and then heterogeneity in adoption across groups that differ in their demand for the health services.

7.1 Ruling Out Alternative Channels

Documented features of the institutional context suggest that access to contraception is unlikely to explain our results. Hormonal contraceptives are the dominant class of modern methods in Rwanda and, in our data, account for 90.7% of all adoptions, which drive our results. Yet during our study period, these methods were available only at health centers and hospitals (USAID et al., 2011). On Saturdays, health centers were closed and hospitals had high surcharges (Ueberschär, 2018).[33] The 2007 Rwandan DHS Service Provision Assessment further reports that most facilities provided family planning services on five or fewer days per week (NISR et al., 2008). The 2010 DHS data show that health facilities were the first source for 91% of modern method users and 95% of hormonal method users. For bed-nets, Appendix Table A3 shows that the Saturday rainfall effect is driven by acquisitions from shops and pharmacies, rather than by health facilities. Together, these facts suggest that the common relationship between Saturday rainfall and the targeted health behaviors is not driven by access.

In Table A4 we examine if adoption differs by alternative channels. We estimate equation 2 and present only the coefficients for Saturdays, before and after performance contracts were introduced. We first revisit the access channel. Column (1) explores heterogeneity by the share of women in the village who, in the 2010 DHS, report distance to the health center as a major constraint to getting care or advice when sick. If the effect operated through access, we would expect larger effects among women in high-distance villages. We find no such pattern: the interaction between non-rainy Saturdays in the after-period and high perceived distance is

[33] Also in our 2010 DHS data, health-facility activity on weekends appears rare: only 3% of 52,539 vaccinations with exact dates from children's health cards occurred on a Saturday or Sunday.

small and insignificant, and total effects are similar across both groups. While this null result should be interpreted with caution since the measure is post-period and may reflect infrastructure changes after our study window, it is consistent with access not being the operative channel. As explained in section 5.2, weather related channels working through agriculture and income are unlikely to drive our results given our reliance on specific weekday rainfall, controlling for general rainfall levels and unit by month fixed effects. In line with this, and as a direct test, Column (2) shows that results do not differ across villages depending on their general rainfall levels, as one would expect if agricultural income were the channel. The interaction between non-rainy Saturdays in the after-period and an above median rainfall indicator is statistically insignificant, and total effects reported below the table show that estimates are significant across both rainy and less rainy villages.[34] Finally, in Column (3) of Table A4 we examine heterogeneity by whether the district where a woman lives had a female mayor in 2006. Only two of the thirty districts did, so the results should be interpreted with caution. Adoption appears higher among villagers in districts with no female mayor, but the difference estimate is imprecise and not statistically significant. This finding is not in line with female mayors driving the adoption effect through shifting the policy or meeting focus in their districts toward family planning.

7.2 Evasive Behavior

Enforcement-based mechanisms may also generate evasive responses. In this context, one way for women to avoid pressure to adopt contraception is to become pregnant. If meetings operate through pressure, they may therefore also increase conceptions in the short run: some women who wish to have more children may expedite pregnancy in order to avoid being targeted for contraceptive adoption. This could occur even if, over a longer horizon, greater contraceptive use reduces pregnancies overall. The short-run effect of Saturday rainfall on pregnancies can therefore provide additional evidence on the mechanism through which meetings affect adoption. A positive coefficient – that is, an effect in the same direction as for contraceptive uptake – would be consistent with pressure inducing both adoption and evasive pregnancies. By contrast, a negative coefficient would suggest that the meetings primarily reinforced existing demand for contraception, leading to lower fertility through voluntary adoption.

Column (1) of Table 4 examines the relationship between conceptions and low rainfall on different weekdays after the introduction of performance contracts. The coefficient on non-rainy

[34] Results are also present and significant across both rainy and dry seasons, results available upon request.

Saturdays is positive and statistically significant at the 5% level, suggesting that one additional meeting increases the probability of conceiving in the same month by 8%. This pattern is consistent with a pressure-based mechanism: while meetings increased contraceptive uptake, they also appear to have induced some women to respond by accelerating pregnancy. The positive average effect on conceptions suggests that this evasive response was large enough to offset any short-run reduction in pregnancies that would mechanically result from higher contraceptive adoption.

Column (2) in Table 4 shows corresponding estimates from before the introduction of performance contracts. None of the coefficient estimates is statistically significant at the 5% level. P-values of the differences in estimates between Columns (1) and (2) (from a DID setup where rainfall counts for each weekday are interacted with an "After" indicator) show that only the difference for Saturday rainfall is statistically significant at the 5% level. The following subsection provides further evidence on the pressure channel by examining how the meeting effect varies across groups with different levels of underlying demand.

7.3 Heterogeneity by Demand

In Table 5 we study heterogeneous adoption and conception across groups that differ in their underlying demand for the targeted health behaviors. We estimate equation 2 and present only the coefficients for Saturdays, before and after performance contracts were introduced.

We first examine heterogeneity in contraceptive adoption and conception by whether a woman is Catholic, restricting the sample to Christians (98% of the population in the 2010 sample). Since the Catholic church was a strong opponent to the family planning policy we may expect its followers to have a lower demand for and more objections toward contraceptives. Results are shown in Columns (1) and (2) of Table 5. In Column (1), the point estimate for adoption among non-Catholics is somewhat larger than for Catholics, but the difference between the groups is insignificant. This pattern is consistent with a mechanism in which leaders' increased emphasis on contraceptives helps correct misperceptions about community or official acceptance of contraceptive use, thereby increasing expressed demand. In Column (2) we see that the increase in conception discussed above is driven entirely by Catholic women. The interaction between Catholic and After is significant at the 10% level and the total estimate (displayed below the table) for the "after" period is statistically significant only for this group. This suggests that Catholic women in particular resisted the policy by expediting pregnancy.

In Column (3) of Table 5 we explore heterogeneity in the effects of rainfall on bed net acquisition by altitude, which proxies for objective need. The incidence of mosquitoes decreases sharply with altitude,[35] so demand for bed nets should be substantially lower in high-altitude communities. If the meeting effect operated through genuine demand, effects should therefore be weaker at high altitude. If instead meetings operated through pressure, lower underlying demand would require more enforcement to achieve the same targets, and effects could be at least as large. The estimates are consistent with the latter: the Saturday rainfall effect is positive both above and below median altitude (1,670 m), and the total Saturday effect in the after period is somewhat larger in high-altitude communities (0.214 vs. 0.132, shown below the table). The difference is not statistically significant, and we do not wish to overstate this result. Nevertheless, the pattern is difficult to reconcile with voluntary demand as the underlying mechanism and is consistent with the view that *Umuganda* meetings increased bed net adoption through pressure rather than by meeting genuine local demand.[36]

Taken together, the evidence in this section points toward pressure and enforcement as the operative channel through which meetings under performance contracts drove contraceptive adoption. Alternative channels, including access and demand-driven voluntary behavior change, are inconsistent with the full pattern of results.

8 Conclusion

This paper investigates Rwanda's spectacular rise in contraceptive use during the early 2000s by analyzing the combined role of two institutional mechanisms – regular community meetings and the 2006 introduction of performance contracts for local leaders.

Our identification strategy leverages within-village variation over time in Saturday rainfall, using the number of non-rainy Saturdays in a month as a proxy for successful community meetings. We compare the effects of non-rainy Saturdays before and after the introduction of performance contracts that strongly increased leaders' incentives to use the meetings for promoting modern contraceptive uptake. We find a positive and significant relationship between community meetings and adoption of contraceptives that shows up only after the performance incentive reform. Before the reform, these meetings have no effects on contraceptive adoption.

[35] 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.

[36] Reassuringly, our main result for contraceptives is not differentially affected by altitude.

This suggests that top-down channels were at work during these meetings. In terms of mechanisms we can rule out that meetings or non-rainy Saturdays directly affected access: clinics were closed on Saturdays and meetings could not provide direct access to the type of contraceptives that drive our results. Other possible channels including income channels can be disregarded because for them to explain our findings, they would need to respond uniquely to Saturday rainfall and not overall rainfall patterns. We instead recover patterns consistent with pressure being used to increase contraceptive uptake: meetings also increased conception rates in the same month, suggesting that some women expedited pregnancy as an evasive response to adoption pressure. Finally, bed net adoption is at least as strongly affected by meetings in high-altitude communities where objective need for bed nets is low, pointing to pressure-driven adoption rather than meetings responding to genuine local demand.

These results add to our understanding of low contraceptive uptake across much of Sub-Saharan Africa. Recent work by Dupas et al. (2025) establishes that supply-side interventions often have limited effects due to low underlying demand. Similarly, externally initiated interventions designed to shift beliefs may fail to generate substantial behavioral change. In contrast, we show that combining leader incentives with local meetings, in the context of a national push to improve family planning services, had a sizable positive effect on contraceptive uptake. These results speak to the importance of understanding and embedding development interventions in local institutional frameworks.

Our findings also challenge the common idea that community meetings generate development primarily through enabling bottom-up mobilization. By showing that fast-track development through a local institution worked only when the incentives for top-down initiative were increased, our findings indicate that successful community-based development programs may, in fact, have helped leaders steer behavior in their communities. 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. In addition to nuancing the picture of Rwanda's top-performance in many of the Millennium Development Goals, including its recent fertility transition, the may offer valuable lessons for assessing current and past developments in other countries with authoritarian elements in their governance.

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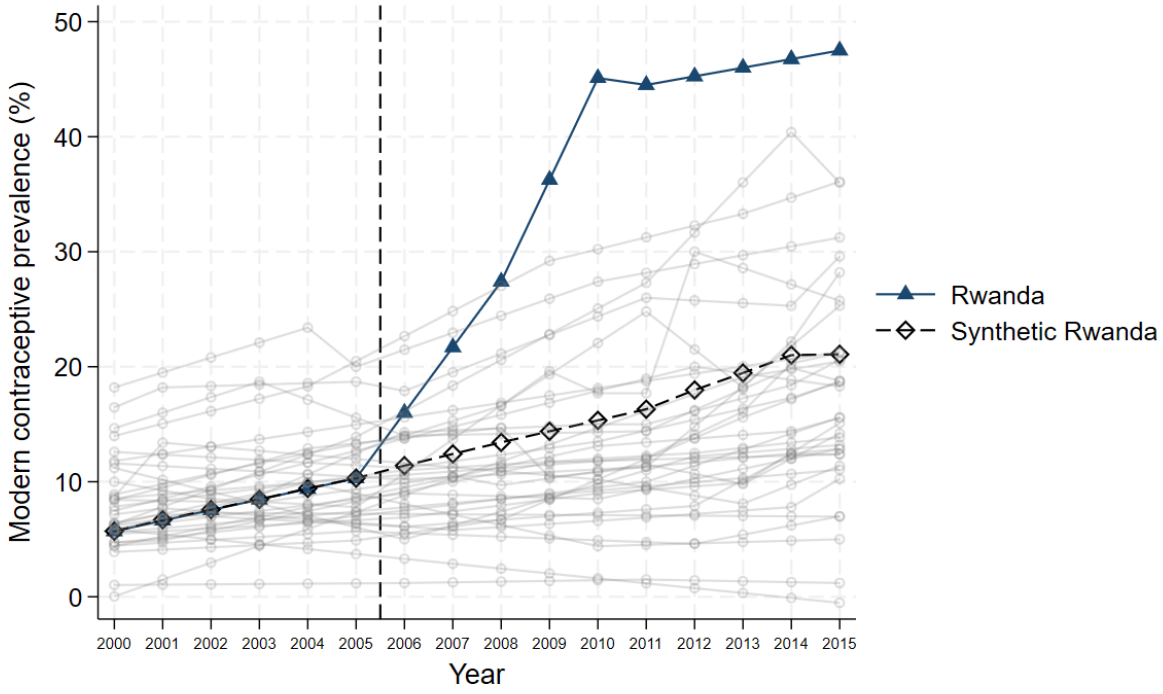
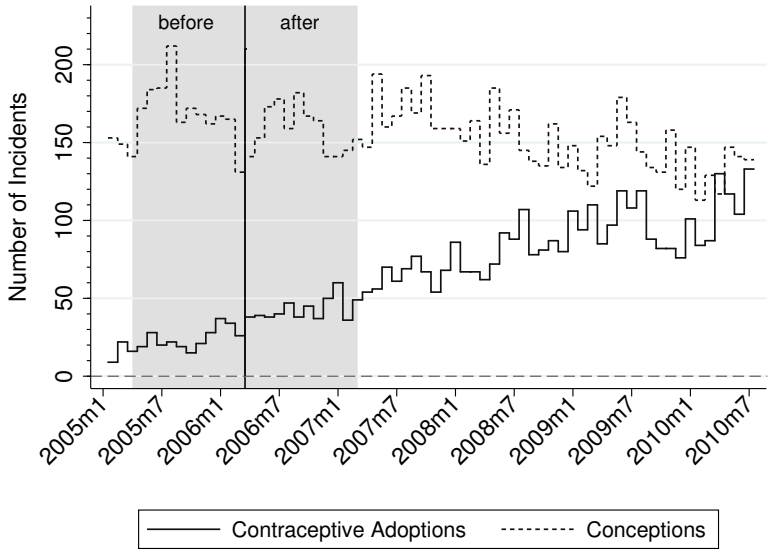
Figures and Tables

Figure 1: Contraceptive Adoption in the 2000's: Rwanda vs. Synthetic Rwanda



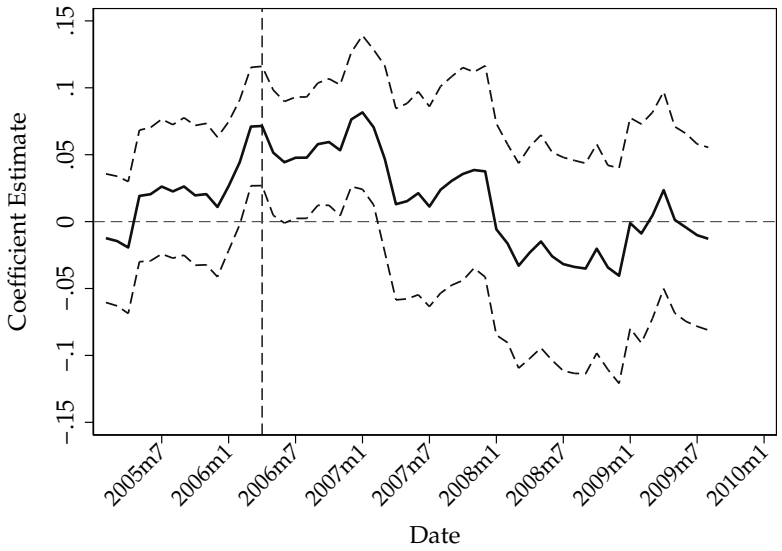
Notes: The figure presents results from a synthetic control analysis based on data from UN WPP (2024); HFD (2024) – w. major processing by Our World in Data. This data shows only contraceptive adoption for married women,; hence the adoption share for Rwanda is about twice that of the entire sample of women. The method constructs a weighted combination of donor countries – the synthetic Rwanda – that best reproduces Rwanda's pre-2006 trajectory in contraceptive adoption. Comparison unit weights: Angola (0.026), Benin (0.015), Burkina Faso (0.089), Burundi (0.014), Cameroon (0.020), Central African Republic (0.014), Chad (0.017), Congo (0.023), Cote d'Ivoire (0.019), DRC (0.015), Equatorial Guinea (0.020), Eritrea (0.017), Ethiopia (0.178), Gabon (0.017), Gambia (0.014), Ghana (0.009), Guinea (0.019), Guinea-Bissau (0.026), Liberia (0.222), Madagascar (0.025), Mali (0.018), Mauritania (0.021), Mozambique (0.020), Niger (0.011), Nigeria (0.020), Senegal (0.018), Sierra Leone (0.018), Somalia (0.007), South Sudan (0.013), Sudan (0.021), Tanzania (0.008), Togo (0.014), Uganda (0.012). Root Mean Squared Prediction Error = 0.032.

Figure 2: Number of Contraceptive Adoptions and Conceptions over Time

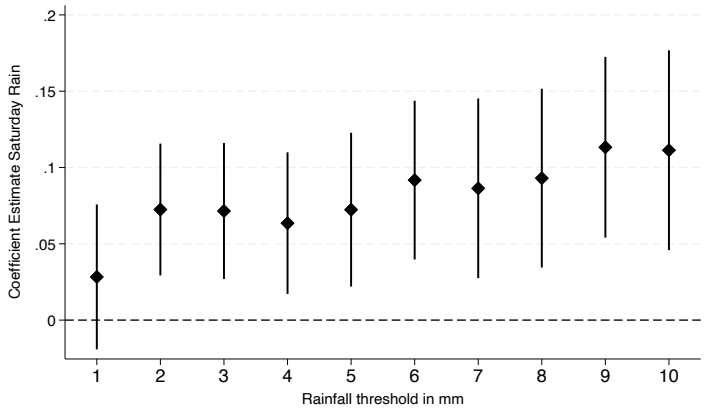


Notes: Based on 13,413 women between 15 and 49 years old and who are usual residents of interviewed households in the 2010 Rwandan DHS. The solid, vertical line marks the introduction of performance contracts in April 2006. The grey shaded areas mark our "before" and "after" performance contract periods used throughout the analysis.

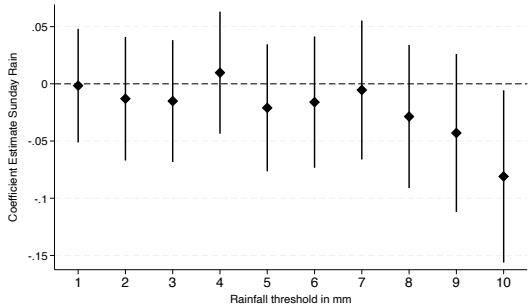
Figure 3: Effect of Low Saturday Rainfall on Contraceptive Adoption over Time



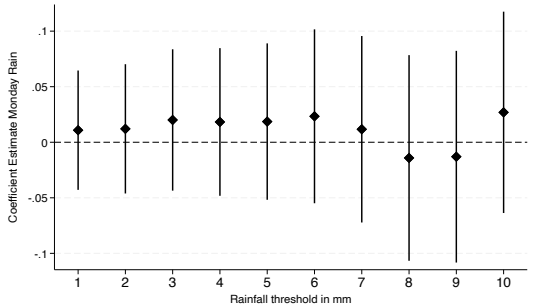
Notes: The figure presents rolling window coefficient estimates on # Sat.(Rainfall$\leq$3mm) (solid line) and 95% confidence intervals (dashed lines). The dependent variable is a monthly, binary indicator of contraceptive adoption. # Sat.(Rainfall$\leq$3mm) is the number of Saturdays with rainfall below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



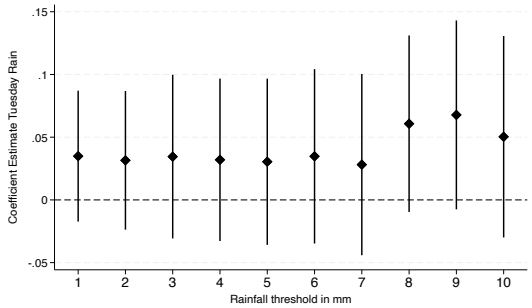
(a) Nonrainy Saturday coefficients



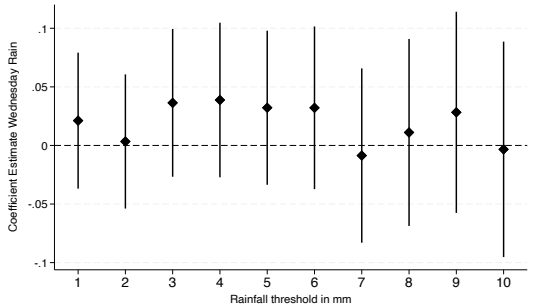
(b) Nonrainy Sunday coefficients



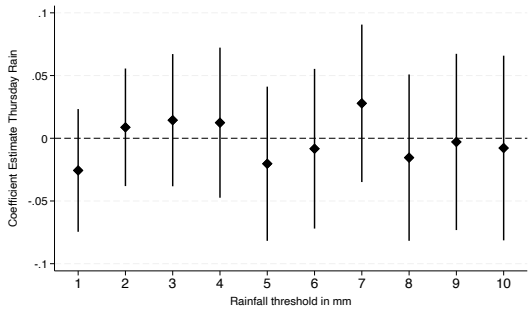
(c) Nonrainy Monday coefficients



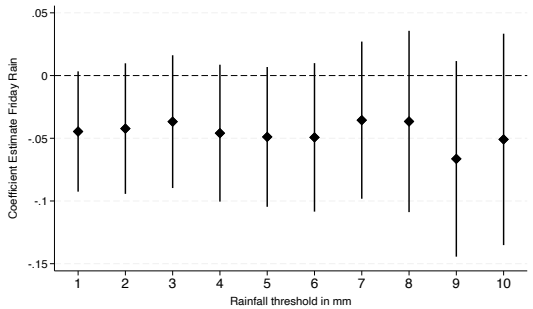
(d) Nonrainy Tuesday coefficients



(e) Nonrainy Wednesday coefficients



(f) Nonrainy Thursday coefficients



(g) Nonrainy Friday coefficients

Figure 4: Robustness of weekday coefficients in main results ("after") to different rainfall thresholds

Notes: The figure presents coefficients and 95% confidence intervals from running the same regression as in Table 2 for the period after the introduction of performance contracts, for rainfall thresholds between 1 and 10 mm.

Table 1: Summary Statistics

	Mean	Std. dev.	Min.	Max.	Obs.
Panel A: Fertility outcomes)					
Adoption (Apr. 2005 - Mar. 2006)	0.228		0	100	125,193
Conception (Apr. 2005 - Mar. 2006)	1.610		0	100	125,193
Adoption (Apr. 2006 - Mar. 2007)	0.395		0	100	130,966
Conception (Apr. 2006 - Mar. 2007)	1.445		0	100	130,966
Panel B: Bed net acquisition					
Bed Net Acquisition (2005 DHS)	0.636		0	100	121,752
Acquisition from Health Center (2005 DHS)	0.275		0	100	71,022
Acquisition from Other Source (2005 DHS)	0.536		0	100	71,022
Bed Net Acquisition (2007-08 DHS)	1.969		0	100	87,444
Acquisition from Health Center (2007-08 DHS)	1.049		0	100	51,009
Acquisition from Other Source (2007-08 DHS)	1.057		0	100	51,009
Non-rainy Saturdays					
# 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: Panel A data is based on 10,629 women in the before-panel and 11,103 women in after-panel. Only women with at least two panel observations are included in each data set. Panel B data is based on 10,146 and 7,287 households with GPS coordinates in DHS 2005 and DHS 2007-08 data, respectively. 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'. Panel C: # Sat.(Rainfallmm) is the number of Saturdays with rainfall below X mm in a calendar month. Based on 492 communities in the 2010 Rwandan DHS between April 2005 and March 2007.

Table 2: 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: Performance contracts were signed at the beginning of April 2006. Column (1) uses data for April 2006 to March 2007 ("after performance contracts"). Column (2) uses data for April 2005 to March 2006 ("before"). The dependent variable, Contraceptive Adoption is a monthly, binary indicator scaled to take values 0 and 100 and thus as percent. # Sat.(Rainfall$\leq$3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). The unit of observation is a woman and a time step is a calendar month. Standard errors are clustered at the community level. P-value: *** p$<$ 0.01, ** p$<$ 0.05, * p$<$ 0.1.

Table 3: Robustness checks

Dependent variable:	Contraceptive Adoption						Bed Net Acquisition		
	Drop 6m post adopt.			Drop 6m post concept..			Alt. outcome		
Panel data:	after	before	p-value	after	before	p-value	after	before	p-value
	(1)	(2)	(1) - (2)	(3)	(4)	(3) - (4)	(5)	(6)	(5) - (6)
# Sat. (Rainfall $\leq$ 3mm)	0.076***	-0.022	[0.004]	0.079***	-0.025	[0.002]	0.174***	0.009	[0.018]
	(0.023)	(0.024)		(0.026)	(0.027)		(0.063)	(0.030)	
# Sun. (Rainfall $\leq$ 3mm)	-0.009	-0.033	[0.479]	-0.014	-0.033	[0.645]	-0.105	-0.038	[0.363]
	(0.027)	(0.021)		(0.030)	(0.024)		(0.068)	(0.031)	
# Mon. (Rainfall $\leq$ 3mm)	0.029	-0.005	[0.435]	(0.021)	-0.005	[0.565]	0.151**	0.002	[0.028]
	(0.033)	(0.028)		(0.036)	(0.03)		(0.062)	(0.028)	
# Tue. (Rainfall $\leq$ 3mm)	0.026	0.009	[0.652]	0.04	0.013	[0.281]	0.006	-0.030	[0.621]
	(0.034)	(0.021)		(0.037)	(0.024)		(0.066)	(0.033)	
# Wed. (Rainfall $\leq$ 3mm)	0.03	-0.003	[0.393]	0.036	-0.015	[0.302]	0.061	0.029	[0.691]
	(0.032)	(0.021)		(0.035)	(0.023)		(0.071)	(0.033)	
# Thu. (Rainfall $\leq$ 3mm)	0.015	-0.001	[0.677]	0.02	0.002	[0.488]	-0.049	0.049	[0.166]
	(0.027)	(0.026)		(0.029)	(0.029)		(0.063)	(0.032)	
# Fri. (Rainfall $\leq$ 3mm)	-0.025	0.021	[0.193]	-0.042	0.022	[0.158]	-0.103*	0.019	[0.061]
	(0.027)	(0.024)		(0.029)	(0.025)		(0.058)	(0.029)	
Observations	128,239	123,926		119,503	114,736		87,444	121,752	
Unit & Time FE	Yes	Yes		Yes	Yes		Yes	Yes	
Dep. var. mean	0.395	0.228		0.395	0.228		1.969	0.636	

Notes: Both dependent variables are monthly, binary indicators scaled to take values 0 and 100 and are interpretable as percent. In Columns (1) - (4), The unit of observation is a woman observed over multiple calendar months (woman-month panel). The "after" period spans April 2006 (when performance contracts were signed) until March 2007, while the "before" period is April 2005 - March 2006. In Columns (5)-(6) the dataset is a household-by-month-before-interview panel. Column (5) uses data for 0-11 months before the DHS 2007-08 interview ("after performance contracts"). Column (6) uses data for 0-11 months before the DHS 2005 interview ("before"). # Sat.(Rainfall$\leq$3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). Standard errors are clustered at the community level. P-value: *** p$<$ 0.01, ** p$<$ 0.05, * p$<$ 0.1.

Table 4: Mechanisms: Evasive behavior

Dependent variable:	Conception		
Panel data:	after	before	
	(1)	(2)	p-value (1) - (2)
# Sat. (Rainfall $\leq$ 3mm)	0.121**	-0.062	[0.024]
	(0.048)	(0.066)	
# Sun. (Rainfall $\leq$ 3mm)	-0.050	0.075	[0.143]
	(0.057)	(0.059)	
# Mon. (Rainfall $\leq$ 3mm)	-0.063	-0.016	[0.614]
	(0.062)	(0.068)	
# Tue. (Rainfall $\leq$ 3mm)	0.088	0.053	[0.705]
	(0.061)	(0.066)	
# Wed. (Rainfall $\leq$ 3mm)	0.145**	0.005	[0.096]
	(0.062)	(0.058)	
# Thu. (Rainfall $\leq$ 3mm)	-0.024	0.133*	[0.079]
	(0.052)	(0.070)	
# Fri. (Rainfall $\leq$ 3mm)	-0.075	0.005	[0.323]
	(0.055)	(0.062)	
Observations	130,966	125,193	
R-squared	0.072	0.070	
Dep. var. mean	1.445	1.610	

Notes: Performance contracts were signed at the beginning of April 2006. The dependent variable, Conception, is a monthly, binary indicator scaled to take values 0 and 100 and is interpretable as percent. # Sat.(Rainfall$\leq$3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). The unit of observation is a woman, and a time step is a calendar month. Regression (1) uses data for April 2006 to March 2007. Regression (2) uses data for April 2005 to March 2006. Standard errors are clustered at the community level. P-value: *** p$<$ 0.01, ** p$<$ 0.05, * p$<$ 0.1..

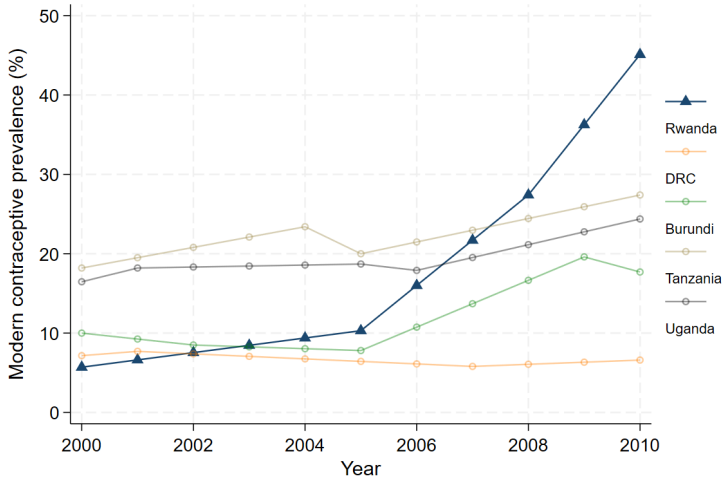
Table 5: Mechanisms: Heterogeneity by Demand and Compliance Costs

Heterogeneity by (Z):	Catholic		$>$ median altitude
Dependent variable:	Contraceptive adoption	Conception	Bed net adoption
	(1)	(2)	(3)
# Sat. (Rainfall$\leq$3mm)	0.003	-0.013	-0.015
	(0.027)	(0.080)	(0.049)
# Sat. (Rainfall$\leq$3mm)$\times$ Z	-0.046	-0.100	0.046
	(0.036)	(0.093)	(0.059)
After $\times$ # Sat. (Rainfall$\leq$3mm)	0.079**	0.095	0.147
	(0.039)	(0.099)	(0.101)
After $\times$ # Sat. (Rainfall$\leq$3mm) $\times$ Z	0.031	0.195*	0.036
	(0.051)	(0.118)	(0.133)
Observations	247,707	247,707	209,196
Other weekday rainfall controls	Yes	Yes	Yes
Sat. After effect | Z=0:	0.082***	0.082	0.132
	(0.028)	(0.059)	(0.088)
Sat. After effect | Z=1:	0.067**	0.177***	0.214**
	(0.029)	(0.062)	(0.083)

Notes: All dependent variables are monthly, binary indicators scaled to take values 0 and 100 and are interpretable as percent. Z denotes Catholic in Columns (1) - (2) and High Altitude village in Column (3). In Columns (1)-(2) the sample is restricted to Christian women (Catholics, Protestants and Seventh day adventists). The unit of observation is a woman, and a time step is a month. The "Before" period spans April 2005 to March 2006 while the "After" period uses data from April 2006 to March 2007. Catholic is an indicator =1 if a woman is Catholic, defined in the subsample of Christian women (98% of our sample). Column (3) uses data for 0-11 months before the DHS 2007-08 interview (after) and 0-11 months before the DHS 2005 interview (pre). The unit of observation in Column (3) is a household, and a time step is a month-before-the-interview. The median altitude is 1,670 meters above sea level. # Sat.(Rainfall$\leq$3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). Standard errors are clustered at the community level. P-value: *** p$<$ 0.01, ** p$<$ 0.05, * p$<$ 0.1..

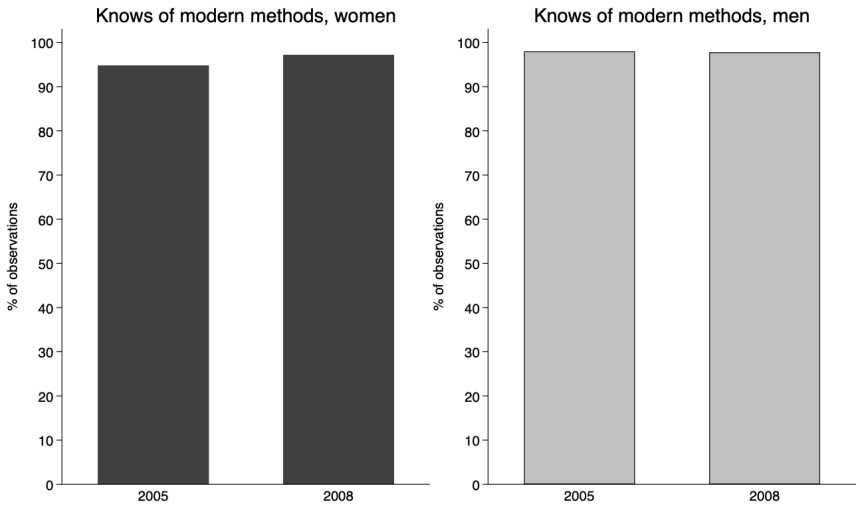
Appendix (for online publication)

Figure A1: Contraceptive Adoption in Neighboring Countries, 2000's



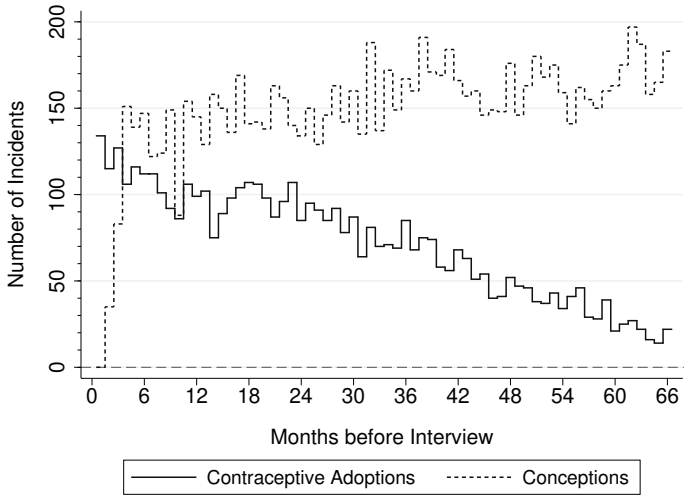
Notes: Graph based on data from UN WPP (2024); HFD (2024) – w. major processing by Our World in Data for the countries bordering Rwanda. The data shows only contraceptive adoption for married women,; hence the adoption share for Rwanda is about twice that of the entire sample of women.

Figure A2: Information about contraceptives



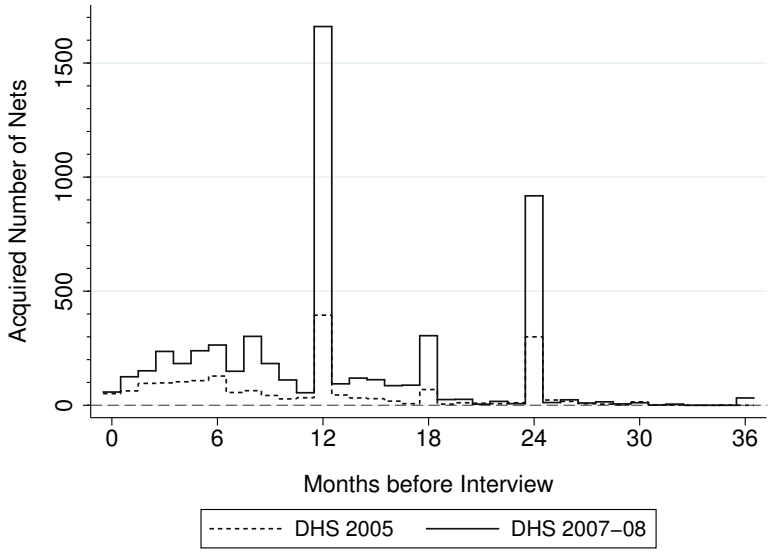
Notes: Graph based on Rwandan DHS data from 2005 and 2008, displaying the share of respondents, divided by sex, who report being familiar to at least one of the modern contraceptives listed in the DHS survey. The shares were 94.8 for women and 98.0 for men in 2005, and 97.2 for women and 97.8 for men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

Figure A3: Number of Contraceptive Adoptions & Conceptions by months before interview



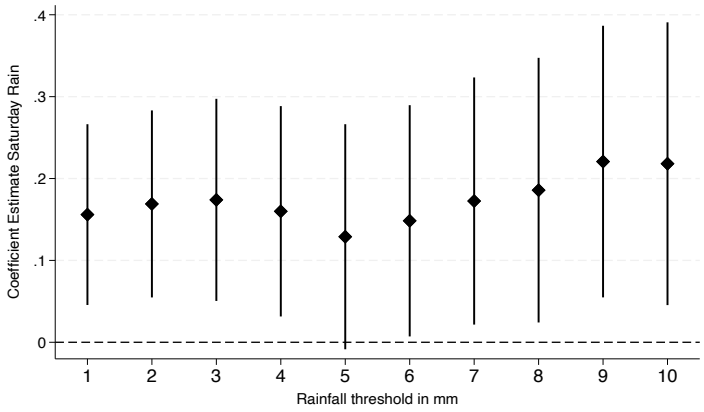
Notes: Based on 13,413 women between 15 and 49 years old and who are usual residents of interviewed households in the 2010 Rwandan DHS.

Figure A4: Number of Mosquito Bed Nets Acquired in Different Months



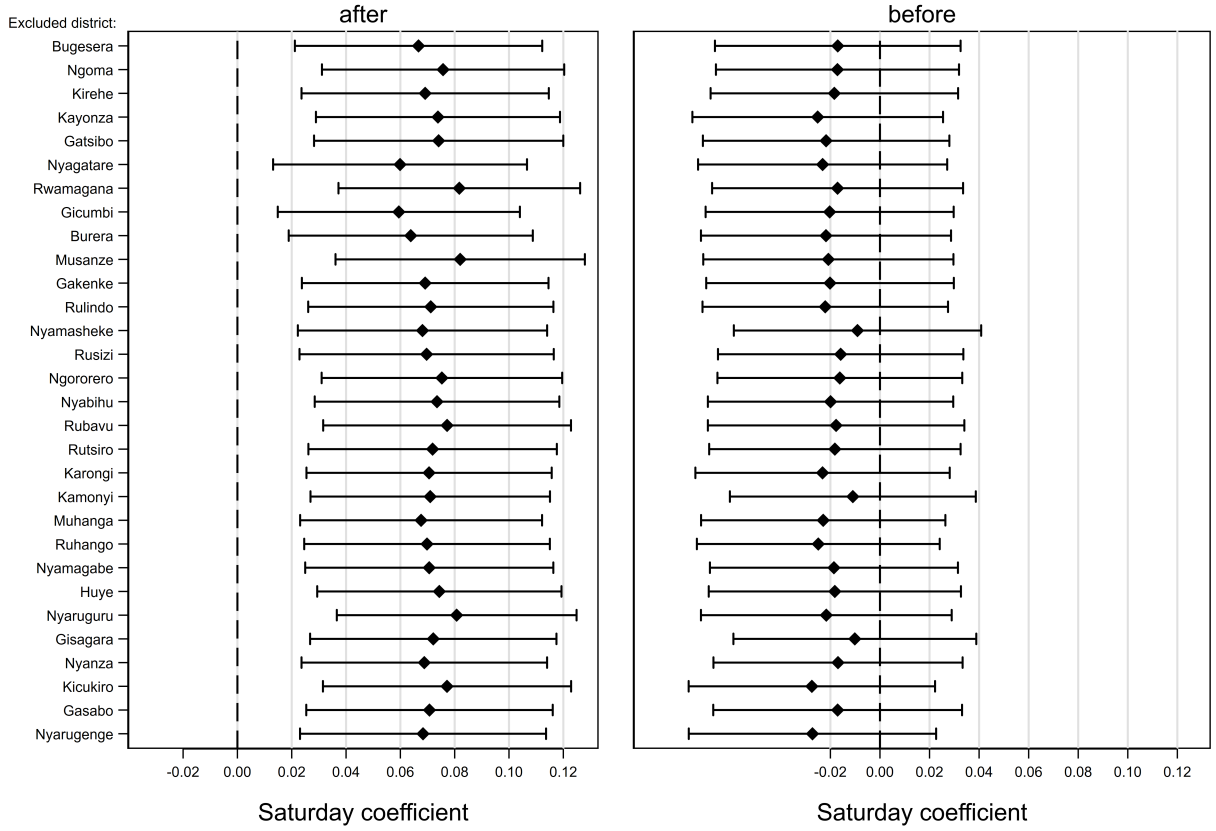
Notes: Based on 10,146 and 7,287 households with GPS coordinates in DHS 2005 and DHS 2007-08 data.

Figure A5: 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 3 for the period after the introduction of performance contracts, for rainfall thresholds between 1 and 10 mm.

Figure A6: Sensitivity analysis: main Saturday estimate while excluding one district at a time



Note: The figure shows coefficients and 95% confidence intervals for the # Saturdays with rain $\leq$ 3mm, obtained from running the same regression as in Table 2 (including all other weekday rain controls) while excluding one district at a time. The left column shows the excluded district for each estimate.

Table A1: Adoption, main regression with separate weekday estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
# Sat.(Rainfall≤ 3mm)	0.069***						
	(0.022)						
# Sun.(Rainfall≤ 3mm)		-0.006					
		(0.027)					
# Mon.(Rainfall≤ 3mm)			0.010				
			(0.033)				
# Tue.(Rainfall≤ 3mm)				0.039			
				(0.033)			
# Wed.(Rainfall≤ 3mm)					0.048		
					(0.032)		
# Thu.(Rainfall≤ 3mm)						0.019	
						(0.027)	
# Fri.(Rainfall≤ 3mm)							-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 2, 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. Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A2: Placebo check and timing of meetings

Dependent variable:	Contraceptive Adoption		
	Future Sat. (Placebo)	Lagged Sat.	individual Sat.
	(1)	(2)	(3)
# Sat.(Rainfall≤3mm)	0.075***	0.068***	
	(0.023)	(0.023)	
F1.# Sat.(Rainfall≤3mm)	0.012		
	(0.026)		
F2.# Sat.(Rainfall≤3mm)	0.026		
	(0.025)		
F3.# Sat.(Rainfall≤3mm)	0.026		
	(0.026)		
L1.# Sat.(Rainfall≤3mm)		-0.046*	
		(0.027)	
L2.# Sat.(Rainfall≤3mm)		0.020	
		(0.025)	
L3.# Sat.(Rainfall≤3mm)		-0.011	
		(0.028)	
First Sat.(Rainfall≤3mm)			0.064
			(0.057)
Second Sat.(Rainfall≤3mm)			0.117*
			(0.060)
Third Sat.(Rainfall≤3mm)			0.085*
			(0.049)
Last Sat.(Rainfall≤3mm)			0.068
			(0.053)
Other Rainfall Regressors	Yes	Yes	Yes
Unit & Time FE	Yes	Yes	Yes
Observations	130,966	130,966	130,966
R-squared	0.080	0.080	0.080
Dep. var. mean	0.395	0.395	0.395

Notes: The dependent variable Contraceptive Adoption is a monthly, binary indicator. # 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). Other Rainfall Regressors are the numbers of days with rainfall below or equal to 3 mm for every other weekday. The unit of observation is a woman, a time step is a calendar month and the data are for April 2006 to March 2007. Standard errors are clustered at the 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: The dependent variable is a monthly, binary indicator scaled to take values 0 and 100 and is interpretable as percent. Health facility sources mainly consist of hospitals and health centers. The category also contains other public facilities, and private providers including private hospitals, clinics, doctors, and infirmaries. Other sources are dominated by shops, kiosks, and pharmacies, and also include personal networks such as parents or friends, churches, and unspecified sources. #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). Standard errors are clustered at community level. P-value: *** p< 0.01, ** p< 0.05, * p< 0.1.

Table A4: Alternative mechanisms

Heterogeneity by (Z):	High distance	Rainy	Female mayor
Dependent variable:	Contraceptive adoption		
	(1)	(2)	(3)
# Sat. (Rainfall$\leq$3mm) before	-0.013	0.003	-0.018
	(0.030)	(0.030)	(0.026)
# Sat. (Rainfall$\leq$3mm)$\times$ Z	-0.014	-0.043	-0.001
	(0.035)	(0.035)	(0.048)
After $\times$ # Sat. (Rainfall$\leq$3mm)	0.087**	0.088**	0.095***
	(0.041)	(0.043)	(0.035)
After $\times$ # Sat. (Rainfall$\leq$3mm) $\times$ Z	0.015	0.011	-0.105
	(0.048)	(0.049)	(0.093)
Observations	256,159	256,159	256,159
Other weekday rainfall controls	Yes	Yes	Yes
Sat. After effect | Z=0:	0.074***	0.091***	0.077***
	(0.028)	(0.030)	(0.023)
Sat. After effect | Z=1:	0.075***	0.059**	-0.029
	(0.028)	(0.027)	(0.070)

Notes: All dependent variables are monthly, binary indicators scaled to take values 0 and 100 and are interpretable as percent. Z denotes High-distance village in Column (1), High Rain Village in Column (2) and Female mayor in Column (3). High distance is a village level indicator =1 if the share of respondents in the village that report distance to the health center as a major constraint to getting care is above the median among villages covered in the 2010 DHS. High rain village is a village level indicator =1 when average daily rainfall in mm in the 12-month period prior to our window of interest (April 2004-March 2005) was above the sample median. Female mayor is a dummy variable =1 when the district in which a village is located had a female mayor in 2006. The unit of observation in all columns is a woman, and a time step is a month. The "pre" period spans April 2005 to March 2006 while the "After" period uses data from April 2006 to March 2007. #Sat.(Rainfall$\leq$3mm) is the number of Saturdays with rainfall below or equal to 3 mm in a calendar month (and similarly for all other weekdays). Standard errors are clustered at community level. P-value: *** p$<$ 0.01, ** p$<$ 0.05, * p$<$ 0.1.