

Dictating Development?

Achieving Targets through Community Work*

Maximilian Linek[†]

— *Job Market Paper* —

This version: November 14, 2019

The most recent version is available on

www.maxlinek.eu/research

Abstract

This paper studies how central governments can use local community meetings to change individual behavior. I investigate the effect of a local institution – mandatory community work on Saturdays – before and after an accountability reform in Rwanda. The reform introduced performance incentives for local leaders of communities. I analyze the effects on four government-promoted, behavioral changes in malaria prevention and family planning. For the analysis, I compile individual level panel data. I exploit quasi-experimental variation in attendance rates over time and estimate the reduced-form effect of Saturday rainfall. An additional rainy Saturday in a month reduces the probability of (i) households to acquire mosquito bed nets, and of (ii) women to adopt modern contraception. The relative effects for both outcomes are very similarly and around 13%. Evidence on women’s (iii) contraceptive discontinuations and (iv) conceptions is consistent with pressure as the mechanism operating through community work. All Saturday rainfall effects arise only after incentivizing leaders. This pattern for targeted, but otherwise very different behavioral changes and an analysis over time suggest complementarity between community work and performance incentives. Placebo tests with rainfall on other weekdays consistently yield null results throughout. Overall, my results provide a new perspective on Rwanda’s development and shed light on the merits and perils of strong accountability in decentralized policy implementation.

JEL Classification: D73, I12, O12.

Keywords: Local Institutions; Performance Incentives; Mosquito Bed Nets; Family Planning.

*I thank Francesco Amodio, Marianna Battaglia, Konrad Burchardi, Ruben Durante, Andrea Guariso, Selim Gulesci, Leander Heldring, Macartan Humphreys, Leonardo Iacovone, Rajshri Jayaraman, Eliana La Ferrara, Julien Labonne, Kate Orkin, Dina Pomeranz, Pia Raffler, Johanna Rickne, Miri Stryjan, Christian Traxler, Diego Ubfal, Marijke Verpoorten, Christopher Woodruff and Ekaterina Zhuravskaya for many helpful comments and suggestions. I would also like to thank seminar and conference participants at AEL Zurich, Bocconi University, CSAE Oxford, EEA Cologne, and the Hertie School.

[†]The Hertie School; m.linek@phd.hertie-school.org

'Under this arrangement [performance contracts] these leaders have assured that they have gone to families, and sensitized and not only that but they follow up to assure that mosquito nets are used.' — Rwanda's President Paul Kagame, Financial Times, July 3, 2008

1 Introduction

Local institutions are an important forum in which people interact. For this reason, they are an interesting resource for policy-makers who wish to change the behavior of citizens and make progress in development. Current research on the potential of local institutions in policy implementation largely ignores pre-existing accountability structures in communities.¹ The usual assumption is that local institutions enable communities to regulate the behavior of presumably corrupt leaders or service-providers. In practice, however, local leaders' authority is often based on their power over local institutions. Rather than being held accountable, leaders routinely use them for their own purposes and to control the behavior of others (Reinikka and Svensson 2004; Martinez-Bravo et al. 2017; Acemoglu et al. 2014). This mostly unobserved accountability structure is considered to explain much variation in effects of otherwise similar local institutions (see Bardhan and Mookherjee 2006; De Janvry et al. 2012).

In this paper, I study the capacity of a collective, local institution to change individual behavior under different accountability regimes in Rwanda. The local institution is mandatory Community Work (*Umuganda*), which is a common practice in many African countries.² In Rwanda, it is performed on Saturdays and consists of physical labor followed by a general meeting. During these meetings, communities are expected to collectively resolve their 'social problems' (MINALOC 2008). My first objective is to establish the effect of this type of community work on two unrelated changes in behavior.³ The behavioral changes are (i) households' acquisitions of mosquito bed nets and (ii) women's adoptions of modern contraceptive methods. These outcomes were explicitly targeted at the local level under separate national development goals.

My second objective is to explore the operating mechanism of community work, i.e. what happens during the meeting and how does it affect behavior. To answer this question, I investigate the effect of community work on two more outcomes. These are (iii) women's discontinuations of modern contraceptives and (iv) conceptions (i.e. when they become pregnant). No explicit targets were defined for these changes at the local level, but they were implicitly targeted through the promotion of contraceptive adoptions. The joint pattern of effects on all three family plan-

1. Some studies limit their focus on local institutions without considering accountability (see e.g. Björkman and Svensson 2009; Olken 2007; Pradhan et al. 2014). To explore the mixed results of these studies, other research on local institutions experiments with accountability (see e.g. Casey et al. 2012; Banerjee et al. 2010). These studies generally find no or only marginal effects, and it remains unclear if accountability fails or local institutions are simply ineffective.

2. Mandatory community work was also common in many Soviet and some Asian countries (e.g. in Cambodia and the Philippines). In Russia, it was called *Subbotnik*, derived from the word 'subbota' which means 'Saturday'.

3. In my analysis, I am unable to distinguish the meetings from labor. Hence, I use the term 'community work' to refer to both activities. Considering that my outcomes are changes in behavior, I expect that effects arise primarily from the meetings.

ning outcomes (adoptions, discontinuations and conceptions) provides strong evidence on the operating mechanism of community work.

My third and final objective is to link the effectiveness of community work for the implementation of central policies to upward accountability. For this purpose, I exploit an accountability reform which introduced personal performance contracts at all levels of public administration in Rwanda. These contracts are imposed from the top and entail strong performance incentives for local leaders with regular monitoring by central government bureaucrats. Similar arrangements are currently used in many other countries (e.g. in China or Russia). In my main analysis, I present evidence on the effectiveness of community work to align behavior with government policy before and after the reform. Moving toward complementary, I investigate the effects of community work over time and compare them to the timing of the reform.

Rwanda is a particularly well suited case to study the three questions above (if, how, and when local institutions are effective). Unlike other African countries, Rwanda has a long history as a centrally unified, authoritarian state. As a consequence, its practice of local institutions, including community work, is traditional, strong and homogeneous across the country. In addition, top-down authority has been ingrained in people. Hence, upward accountability is either very strong, when enforced, or very weak otherwise. The turmoil of the Rwandan genocide in 1994, with a new government taking control, arguably destroyed the accountability link from local leaders up to the central government. This link was only reestablished with the introduction of performance contracts. Considering this background, my analysis likely provides an upper-bound on the capacity of local institutions to change individual behavior.

Under the two development goals – fighting malaria and reducing population growth – Rwanda’s government targeted the number of households with mosquito bed nets and women’s use of modern contraceptive methods. These development goals were proposed and financed by the international community.⁴ Between 2005 and 2010, Rwanda made enormous progress in both. The percentage of households with at least one mosquito bed net increased from 18% to 83%, and modern contraceptive prevalence among married women soared from 10% to 45%, after having remained almost constant for over 20 years. The speed of these developments is enormous by any comparison.⁵

Reliable evidence documents that many Rwandans did not approve of these behavioral changes. With respect to family planning, the 2005 Rwandan Demography and Health Survey (DHS), for example, documents that 33% of married women (who were not using modern contraceptives) were strongly pro-natalist and opposed to modern contraception. Only 3% reported knowledge, access or cost as a reason for non-use, which suggests the absence of supply-side con-

4. In Rwanda, mosquito bed nets were heavily promoted and financed through the U.S. President’s Malaria Initiative, which was launched in 2005. Similarly, also modern contraceptive methods were promoted and financed by a strong coalition of international aid donors (see Solo 2008).

5. One of the fastest developments in contraceptive use and fertility in any country worldwide occurred in China during the 1970s. Rwanda’s speed of progress between 2005 and 2010 lies level with the speed of that development. For more information on the development of family planning (and the use of local development targets) in China, see Zhang (2017).

straints (NISR and Macro 2006). However, irrespective of sentiments in the population (which were arguably strong also due to the genocide 10 years earlier (see Kraehnert et al. 2019)), high-level policy-makers considered a reduction in population growth essential for achieving economic development (Solo 2008). The rationale for raising mosquito bed net ownership across Rwanda is more obscure and may, potentially, relate to the financing and a desire to please aid donors. But, the lack of demand and support for mosquito bed nets is arguably more objective. Rwanda is a high-altitude country with at least one third of communities living at altitudes where the anopheles mosquito, which is the transmitting vector of malaria, cannot survive. I explore this heterogeneity in my analysis.

For the analysis, I compile novel and individual monthly panel data which is representative for the Rwandan population. My four outcomes are binary indicators of behavioral change. In constructing two of my outcomes (contraceptive adoptions and discontinuations) I follow the standard DHS classification of modern contraceptive methods. Roughly 90% of modern methods used in Rwanda are hormonal methods with injectables being the most common (57% of all modern methods). These methods (i.e. injections, pills, IUDs and implants) were exclusively available at health centers and hospitals (USAID et al. 2011). Reassuringly, also 90% women in 2010 DHS data report their first modern contraceptive source in current use to have been a health center or hospital. These numbers are time constant until at least 2010.

In my analysis, I exploit within household and women variation over time, using event studies. The main challenge of identifying the effect of community work on behavioral change is that variation in attendance rates is unlikely to be exogeneous. Across Rwanda, both parts of community work, the physical labor and the subsequent meeting, take place outside under the open sky. That means, attendance rates at community work should be exogeneously affected by fluctuations in weather. Hence, I use rainfall on the day of community work to identify the causal effect of community work and the subsequent meeting on behavioral change. For the time period under analysis (the mid 2000s), government and other evidence document weekly community work on Saturdays at the local level.⁶

My identifying assumption is that rainfall on Saturdays lowers attendance rates at community work. This source of exogeneous variation in Rwandan community work has previously been exploited by Bonnier et al. (2019) in cross-sectional data.⁷ In contrast to these authors, I exploit the within community variation in rainfall over time. However, similar to them, I estimate the reduced form because data on attendance rates at community work is unavailable. The relationship between rainfall and attendance rates has recently also been exploited in other contexts, all of which find a strong first stage.⁸ Several arguments (discussed later) suggest a

6. See e.g. Uwimbabazi (2012), Purdeková (2011), and MINALOC (2008).

7. Bonnier et al. (2019) find that rainy Saturdays during the time period leading up to the 1994 genocide reduce the number of civilian perpetrators in villages. They suggest that meetings after community work were used for propaganda and to agitate people.

8. For example, Madestam et al. (2013) use rainfall as an instrument for attendances to political rallies of the Tea Party Movement; Collins and Margo (2007) use rainfall as an instrument for the severity of riots (i.e. participant numbers) during April 1968 in the USA; Several studies use rainfall as an instrument for participation in democratic elections (see e.g. Fujiwara et al. 2016; Gomez et al. 2007; Hansford and Gomez 2010).

very strong first stage also for attendance rates at community work. In my analysis, rainfall on other weekdays (i.e. Mondays, Tuesdays, etc., but not Saturdays) serve as natural placebo tests in regressions with both one and multiple rainfall regressors.

I find large, significant and robust reduced form effects of rainfall on Saturdays — the day of community work — on all four outcomes. Within two years after the accountability reform, Saturday rainfall reduces the probability that (i) households acquire mosquito bed nets, and (ii) women adopt modern contraception in the same month. The relative effects of an additional rainy Saturday are very similar for both outcomes and around 13%. In contrast to contraceptive adoptions, Saturday rainfall increases the probability that (iii) women discontinue contraceptive use by approximately 20% in relative terms. The two findings for contraceptive adoptions and discontinuations are striking because health centers, as the only source for hormonal contraception (beside hospitals), were commonly closed on weekends.⁹ Beside access, also information provision and indoctrination have difficulty explaining these results.

The evidence on conceptions is more revealing because it contradicts the mechanical effect. Mechanically, Saturday rainfall should increase conception if it reduces contraceptive adoption. However, I find that an additional rainy Saturday significantly reduces the probability that women conceive in the same month by around 7%. This result can only be understood, knowing that women must not use hormonal contraceptives when pregnant. As many women were very pro-natalist and rejected contraceptive use, becoming pregnant was likely perceived as an action to hold off modern contraceptive use. This evidence is consistent with reports and a survey which suggest that targets in performance contracts of local leaders were implemented through pressure and even ‘compulsion’ (OSSREA 2007). In line with this interpretation, my analysis of heterogeneity by altitude for acquisitions of mosquito bed nets shows that Saturday rainfall effects are stronger in high-altitude areas, where we can expect that more pressure is needed to make people comply.

Finally, all effects only arise within two years after the accountability reform which introduced performance contracts. Before this reform, Saturday rainfall no effect on my outcomes. All placebo tests, i.e. regressions on one or more rainfall variables for other days, yield consistently null results. The very few exceptions, I show to be spurious. With national development targets (although lower ones) in place already before the reform, two pieces of evidence support attribution. First, to the best of my knowledge, the only commonality between the diverse behaviors under scrutiny was being targeted in local performance contracts. For example, for mosquito bed nets, I show that results are very similar irrespective of the source from where bed nets were obtained. This evidence indicates that no single other activity generates my results, but community work under performance contracts. Second, an over time rolling regression analysis of my family planning outcomes shows that effects for contraceptive adoption and its avoidance

9. See e.g. Ueberschär (2018) or the 2007 Rwandan DHS Service Provision Assessment (NISR et al. 2008). An auxiliary analysis with DHS 2010 data reveals that only 4% of childrens’ vaccinations took place on a Saturday or Sunday, in contrast to 29% predicted by a uniform distribution. This evidence also confirms that health facilities were closed (or at least not frequented) on weekends.

behavior, conception, arise immediately after the introduction of performance contracts. The effect on discontinuations is somewhat delayed, likely because women first needed to be pressured into contraceptive use.

My results summarize in three points. First, community work was used for policy implementation. Second, the evidence indicates the use of pressure to change individual behavior. Third, only under performance incentives did community work become an effective tool for the central government to achieve targets. In this sense, the paper provides a new perspective on Rwanda's fast-track development. Considering my very different outcomes, Rwanda's central government likely used community work and performance contracts to dictate development also in other domains. At a broader level, the paper documents the interplay of two governance structures, community meetings and performance incentives, which exist in many settings around the world. From the perspective of central policymakers, the evidence suggests complementarity between both. However, their combination may exert effects which strongly constrain peoples' personal freedom. Overall, my findings may provide a better understanding of developments elsewhere (e.g. in China).

The paper relates to two strands of the economic literature. The first strand investigates the effect of collective, local institutions (e.g. committees or meetings) on the implementation of programs or policies. While some research finds large effects (Pradhan et al. 2014; Björkman Nyqvist et al. 2017; Björkman and Svensson 2009), other research finds small or no effects (Casey et al. 2012; Banerjee et al. 2010; Olken 2007). The explanation for this discrepancy in results has been attributed to failures of bottom-up accountability (Martinez-Bravo et al. 2017; Reinikka and Svensson 2004; Anderson et al. 2015). However, this body of research largely falls short on investigating the functioning of local institutions and the accountability structure they are embedded in. In this paper, I specifically try to address these issues. First, I present evidence that suggests that local institutions serve to control the population and exert pressure. In this vein, my research is most closely related to Acemoglu et al. (2014) and Miguel and Gugerty (2005). Second, I show that the same institution as in much of the above research (community meetings) can be highly effective also under top-down accountability.

The second strand of literature follows directly from this last point and investigates performance incentives in public administration (Finan et al. 2017). Similar to the literature on local institutions, some research finds strong effects of performance incentives on aligning behavior (Ashraf et al. 2014; Duflo et al. 2012), while other research finds only small, temporary or even negative effects (Celhay et al. 2018; Rasul and Rogger 2018; Olken et al. 2014; Khan et al. 2015). My contribution to this literature is a case study that connects it with the literature on local institutions. It further serves as an example for a context, arguably found in many authoritarian states, in which performance incentives can fuel severe restrictions to individual freedoms and rights.

The rest of the paper is structured as follows. Section 2 provides background information on performance incentives and community work in Rwanda. Section 3 proposes a conceptual

framework. Section 4 describes the data. Section 5 outlines the empirical strategy. Section 6 presents the results. Finally, section 7 concludes.

2 Background

Performance contracts and community work are traditional practices which date back to pre-colonial times when Rwanda was a kingdom. Throughout the centuries, central governments in Rwanda have used them for indirect rule and control over activities at the local level. With deepening decentralization, the current government has revived these practices as ‘new’ strategies to ensure and benchmark results in development.

2.1 Performance Contracts and Targets

A Rwandan performance contract (*Imihigo*) is historically a leader’s test of a vassal to prove her worthiness. This understanding still persists in current practice. The leader (which at the very top is the President) requires the subordinate to publicly vow the achievement of pre-defined targets. In this process the subordinated becomes personally liable to deliver. Target achievement is honored and rewarded. However, failure is perceived as a betrayal of trust and is sanctioned.

On April 4, 2006, Rwanda’s president Paul Kagame reintroduced performance contracts as a steering instrument for policy-making. The signing ceremony with the Executive Secretaries of the districts (the Mayors) was broadcast nationwide on radio and television. The contracts stimulate strong competition among Mayors to perform relatively better than other Mayors on achieving national development targets in their districts. Progress in target achievement is monitored on a monthly basis and every three months all districts are ranked. Top-ranking districts receive a financial reward (5 million Rwandan Francs in 2010), while those at the bottom are sanctioned (Murray-Zmijewski and Gasana 2010). Between 2007 and 2009, approximately 75% of Mayors were removed from office due to failure in achieving their targets (Scher and MacAulay 2010).

Similar performance contracts as for Mayors were rolled out to all levels of administration and even beyond to the individual family and household in Rwanda. At the local level, household heads vow specific behaviors of some or all their household members in front of the community during the general meeting at community work (Bugingo and Interayamahanga 2010). These pledges are then recorded into a household’s *Imihigo Booklet*, which the household needs to purchase. If a household completes a pledge, the booklet is stamped. Stamped booklets serve as a proof of ‘good standing’ which is necessary to access government services like registering a marriage or birth (Uwimbabazi 2012). Notorious non-compliance with targeted behaviors entails sanctions like fines, destruction of property and corporal punishment (Huggins 2009; Thomson 2008).

Since their beginning, performance contracts at the community level contained targets in the number of households owning mosquito bed nets and in the number of new users of modern contraceptive methods among women.¹⁰ These targets and their reporting were aligned with Rwanda’s 2007 Economic Development and Poverty Reduction Strategy, which drastically raised many national development goals. For example, it raised the target in modern contraceptive prevalence from 18.5% by 2010 to 70% by 2012. With these ambitious targets being passed down to the local level, staff members of the United Nations Population Fund raised concern about potential coercion (Solo 2008). This concern is confirmed in an *Imihigo* Assessment from 2007 which finds that 57% of respondents across Rwanda report compulsion in the implementation of performance targets (OSSREA 2007).

2.2 Community Work

Community work is a mandatory service on Saturdays for all Rwandan citizens between 18 and 65 years of age. After arguably having been used to incite the population, culminating in the 1994 genocide (see Bonnier et al. 2019), it was resumed in 1998 to make development progress (MINALOC 2011). While, officially, community work takes place only on the last Saturday in a month, the actual number of Saturdays with community work is at the discretion of local leaders and may vary across communities and time. Evidence suggests that in many places across Rwanda community work was weekly or at least bi-weekly throughout the 2000s (Purdeková 2011; Uwimbabazi 2012; NAR and Interpeace 2016). During community work public transport stops and all shops are closed so that all Rwandans can participate. That means, community work is usually the only activity of the average Rwandan on (these) Saturdays.

With respect to development, community work is the tool for implementing targets from performance contracts at the local level. Local leaders, typically the *Umudugudu* (Village) *Committee* consisting of a chairman and four members, organize community work. They call together the community for some physical work like clearing bushes or cleaning roads. After the activity, they chair an outside meeting in which the Committee ‘mobilises, sensitises and supports the population to collectively define and resolve their economic and social problems’ (MINALOC 2008, p.18). In practice, local leaders communicate top-down information about government programs and policies (RGB 2014), which are the communities’ development targets from performance contracts.¹¹ Household heads then have to pledge contributions to these targets in front of the community.

Under this regime, most Rwandans participate involuntarily at community work and would prefer to remain absent (Uwimbabazi 2012; Purdeková 2011). However, absence from community work without a ‘good reason’ can be fined by up to 5,000 Rwandan Francs (between 2005 and

10. For a 2006 community level performance contract with these targets see Sommers (2012, Appendix). Further information on district performance contracts between 2009 and 2013 can be found in RGB (2014).

11. Annual reports of Rwanda’s Ministry of Health document that mosquito bed nets and family planning were regularly discussed and promoted at community work (see e.g. MoH 2009, 2008).

2010 roughly 9 USD and a large amount which corresponded to 12 times the daily median wage). I will elaborate on different reasons for absence related to rainfall below.

3 Conceptual Framework

In this section, I first conceptualize the process of target implementation through community work. Second, I discuss how this process is affected by rainfall.

3.1 Target Implementation through Community Work

Local leaders use community work to communicate government policies and moderate a collective implementation process. That means, a community's development targets are not negotiable, but the community can decide how it (or rather who) implements them. As a consequence, achieving a target can be conceptualized as the provision of a public good because leaders hold the entire community accountable for failures in implementation. Households (or women, depending on the target) contribute if they choose a behavior which supports the community in reaching the target. While the targeted behaviors, which are analyzed in this paper, were potentially beneficial for some people, they were certainly costly for others. Acquiring mosquito bed nets was most likely perceived a waste of time and money in places where there are no mosquitoes (approximately one third of Rwanda), and contraceptive use was strongly stigmatized at the time before the introduction of performance contracts.¹²

If targeted behavior was costly, why did households and women choose it? In the literature on the private provision of public goods, it is no secret that punishment is an effective measure to ensure cooperation (see e.g. Fehr and Gächter 2000). The evidence (discussed in the previous section) suggests pressure or compulsion also in the present context. With pressure, households and women change their behavior and comply with targets if the (expected) cost from sanctions exceeds the cost of the behavior. I will discuss this proposed operating mechanism of community work later with the results in section 6.

To achieve targets, i.e. provide the public good, a community arguably needs to follow two-steps. First, the community needs to select a sufficient number of contributors. And second, contributions must be monitored and, if necessary, enforced. Community work assumes both of these functions, selection and monitoring.¹³ This organization of contributions at community work is a continuous process for two reasons. First, selection and monitoring must be applied consecutively with some time in between because changes in behavior can only be promised. And second, new contributors had to be presented every month for the reporting of development progress. In this sense, any canceled or 'ineffective' community work hampers target implemen-

12. Women using modern contraceptives were regarded as prostitutes (Berry 2015; USAID and MoH 2002; Farmer et al. 2015) and strong pro-natalist social norms prevailed (Kraehnert et al. 2019).

13. Selection takes the form of requiring family pledges at community work. A local policing unit is also formed at community work to 'monitor inappropriate behavior in the community' (Uwimbabazi 2012, p.67).

tation and reduces the overall number of new contributors in a month. Consequently, I analyze the effect of community work on changes in behavior.

3.2 Community Work and Rainfall

Obviously, if cancelled, community work cannot assume its functions in target implementation (selection and monitoring). However, low attendance rates have the same effect. Selection of new contributors may become impossible if decisions lack legitimacy, for example because key actors (certain leaders, potential selectees) are absent. But also monitoring and enforcement at community work is not feasible if households or women which have previously been selected for contribution do not attend. In this respect, even small dips in attendance rate may render community work ineffective for target implementation.¹⁴

My estimation strategy relies on the assumption that rainfall on the day of community work lowers attendance rates. This relationship has been exploited in the literature on rainfall (see e.g. Madestam et al. 2013; Fujiwara et al. 2016; Collins and Margo 2007) and more specifically with respect to community work in Rwanda (Bonnier et al. 2019). In the present context, the effect of rainfall on attendance rates can be expected to be very strong for at least three reasons. First, work and meeting of community work take place outside under the open sky. Rain disrupts and inhibits these activities. Second, rain significantly reduces mobility because the prevailing dirt roads and foot paths become impassable. That means people may not find out whether community work takes place or cannot reach the meeting point.¹⁵ Third and most importantly, rainfall creates the expectation that also other people will be absent. Each of these arguments provides a ‘good reason’ (in the sense that it is accepted by local leaders) for missing community work and insures against the consequences of absence.

4 Data

In this section, I describe the outcomes and rainfall regressors. All panel data on outcomes are constructed from retrospectively collected information, using dates and times which have been reported in cross-sectional Demography and Health Surveys (DHS) from Rwanda.

4.1 Acquisitions of Mosquito Bed Nets

I use information from two Rwandan DHS to study households’ timing of acquisitions of mosquito bed nets before and after the introduction of performance contracts. The 2005 DHS sampled 10,146 households living in 456 communities with GPS coordinates available. The 2007-08 DHS

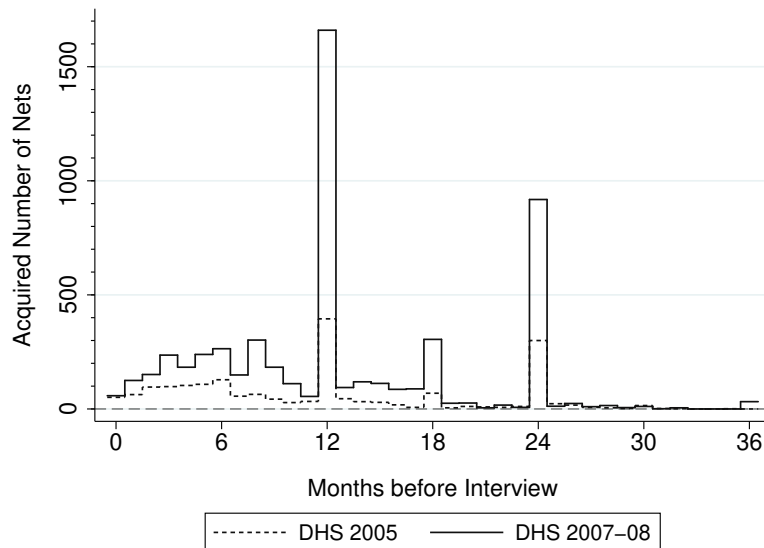
14. Uwimbabazi (2012, p.216) writes that ‘successful implementation of any policy can be affected by the absence of the full participation [at community work] of those especially who should benefit from these policies’. For theoretical and experimental work on the importance of participation in the decision-making of groups see Dal Bó et al. (2010) and Faillo et al. (2013).

15. In fact, whether community work takes place is usually announced on the same day and the information is spread through word of mouth and loudspeakers which are mounted on a car driving around (RGB 2014). Hence, rain impedes the ‘calling together’ of the community.

sampled 7,287 households in 246 geo-coded communities.¹⁶ Both surveys collect information about mosquito bed nets in households. For each mosquito bed net obtained in the past three years, the data records the number of months before the interview when a household acquired the net. Information about the source of the mosquito bed net (i.e. where it was obtained) is additionally available for bed nets acquired in the previous six months.

Figure 1 presents the raw data of the total number of mosquito bed nets which were acquired in each month before the interview. The figure shows high numbers of acquisitions on months 12, 18 and 24 in both surveys which indicates that reporting precision deteriorates for bed nets acquired 12 months and longer before the interview. Without questions to narrow down the timing of acquisitions in the survey, these concentrations likely are due to rounding and imprecise recall. For this reason, I restrict my analysis to bed nets acquired in months 0-11 before the interview.

FIGURE 1: 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.

Based on the raw data, I construct two separate, retrospective, household level panel data sets spanning 0-11 months before the interview (one data set for each DHS). My main outcome is a binary indicator which is 0 and takes the value 1 if a household acquired one or more mosquito bed nets in a given month before the interview. Furthermore, I create two additional outcome indicators of panel length 0-5 months in the 2007-08 panel data to examine heterogeneity in the source of mosquito bed nets. One indicator is 0 and takes the value 1 if at least one mosquito bed net in a month before the interview was acquired at a Health Center. The other indicator is 0 and takes the value 1 if at least one mosquito bed net in a month was acquired from Other Sources (e.g. shops). Tabel A1 in the appendix presents summary statistics of the panel data on

16. The full DHS 2005 data contains 126 households from 6 communities without GPS coordinates. The full DHS 2007-08 data contains 90 households from 3 communities without GPS data. These households are dropped from the analysis sample.

acquisitions of mosquito bed nets. For the analysis, the panel data is matched, based on GPS location and interview date, with rainfall data for each month before the interview.

4.2 Family Planning Outcomes

I use information from the 2010 Rwandan DHS to study women’s timing of adoptions of modern contraceptive methods, their timing of discontinuations of these methods, and their timing of conceptions (i.e. when they become pregnant). The 2010 DHS interviewed 13,413 women who were between 15 and 49 years old and usual residents in 492 different communities. An integral part of the questionnaire was a monthly calendar stretching from the date of the interview back to January 2005. In this calendar, interviewers recorded for each monthly date whether a woman was pregnant or using modern contraception. To complete and check the information in the calendar, interviewers were required to ask several different questions as part of the survey.

Based on this retrospective calendar data in the 2010 DHS, I construct my panel data on family planning outcomes. First, I code the calendar information and build panel data indicating whether a woman is pregnant, using modern contraception or is none of the two. Second, I define the three family planning outcomes, which are changes in women’s behavior. Third, following DHS sampling rules, I set all information to missing for times when a woman is below 15 years of age, yielding an unbalanced panel.

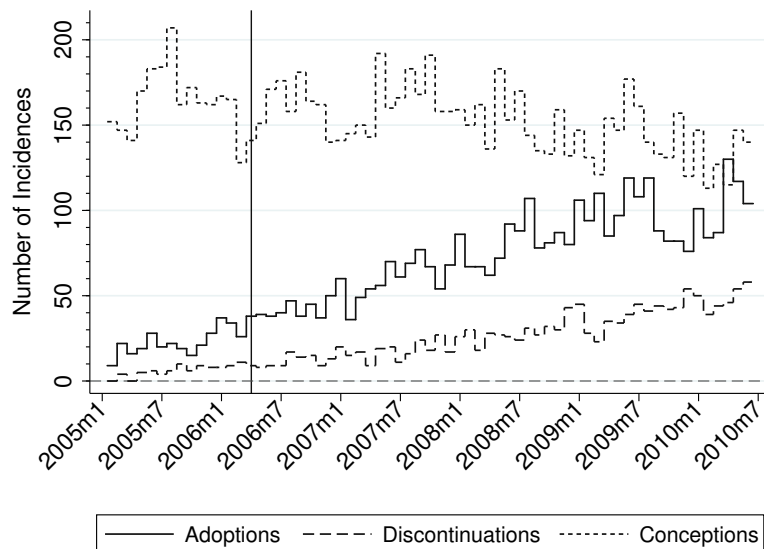
Times of pregnancy are the first entries recorded in the calendar. Interviewers enter the months of births and abortions as the last month of pregnancy in the calendar and write back this status so that the number of recorded months of pregnancy in the calendar equals the number of completed months as reported by women (usually 9 months). Unfortunately, this procedure is inaccurate for two reasons. First, it assumes that birth is given on the last day in the month of birth, which is highly unlikely. Hence, almost all women are in fact already pregnant one month earlier than recorded in the calendar. Second, the procedure assumes that pregnancy generally lasts 9 months, which is technically wrong. It is well known that pregnancy lasts on average 40-41 weeks, 10 months, calculated from the first day of the last menstrual period, approximately two weeks after which a woman conceives. Hence, this time after the last menstrual period should be the true relevant time period of conception. Following these reasons, conception in my data is a binary indicator which is 0 and evaluates to 1 two months before the first month of a recorded start of pregnancy in the calendar. For pregnancies which ended in birth, this definition is identical to lagging the date of birth by 10 months (unless a pregnancy was reported as shorter or longer).

The definition of a woman’s adoption of modern contraception is straightforward. Adoption is a binary indicator which takes the value 1 in the first month of modern contraceptive use after at least one month of non-use and is 0 otherwise. A more refined definition is required for discontinuations of modern contraceptive methods. Approximately half of all women discontinued their contraceptive use because they became or wanted to become pregnant, a status which is recorded erroneously in the calendar (as explained). Hence, for these women I define

discontinuation one month earlier, i.e. as a binary indicator which is 0 and takes the value 1 in the last month of modern contraceptive use. For all other women, I apply a definition analogue to adoption with discontinuation being a binary indicator which is 0 and takes the value 1 in the first month of non-use after at least one month of contraceptive use.

Figure 2 presents the number of adoptions, discontinuations and conceptions in each month from February 2005 until June 2010. Two facts should be noted. First, unlike with the data on the timing of acquisitions of mosquito bed nets, none of the family planning outcomes displays suspiciously high numbers of incidences on any date. This observation is true for dates close and distant to the dates of interview and also holds when plotting the sum of incidences over months before the interview (instead of the monthly date), as illustrated in figure B1 in the appendix. Second, the data count quite few adoptions and discontinuations, the variation which identifies my coefficient estimates.

FIGURE 2: NUMBER OF ADOPTIONS, DISCONTINUATIONS AND CONCEPTIONS OVER TIME



Notes: Adoptions and discontinuations of modern contraceptive methods (DHS definition). Based on 13,413 women who were between 15 and 49 years old and usual residents in the household. Vertical line marks the introduction of performance contracts in April 2006.

For my main analysis, I split the data into two panel data sets and restrict their panel length. The split is between March and April 2006, separating the data into one panel for the time period before the introduction of performance contracts and a second panel for the time period thereafter. Doing so allows me to use the same simple and coherent empirical strategy for the analysis of all outcomes, whether on mosquito bed nets or family planning. I restrict the length of the before panel to the 12 months from April 2005 until March 2006 and the length of the after panel to the 24 months from April 2006 until March 2008. The underlying reason for this restriction is that effects are not time constant, an issue which I will explore later in section 6.3. Table A2 in the appendix presents summary statistics of the two restricted and the unified full panel data sets used in the analysis. Only women with at least two panel observations are

kept in each of the three data sets because women with one panel observation drop out in my regressions which always includes women fixed effects.

4.3 Rainfall Regressors

Rwanda’s hilly terrain generates high levels of and strong spatial variation in rainfall. The rainfall data, used for the analysis, are CMORPH rainfall estimates from NOAA’s Climate Prediction Center. These estimates are available at very high spatial and temporal resolution, i.e. for rainfall tiles with approximately 8 km (0.073 degrees) side length at 30 minutes intervals (Joyce et al. 2004). Validation studies suggest that CMORPH rainfall estimates perform particularly well in detecting rainfall over complex terrain like Rwanda (see e.g. Abera et al. 2016).

I construct my rainfall regressors in two steps. First, I aggregate the data and extract daily rainfall in each community based on the GPS coordinate. Second, I create a variable for each weekday (Mondays, Tuesdays, etc.) which counts how many of these days were ‘rainy’ days in a month. A ‘rainy’ day is defined as a day with rainfall above a certain threshold. In my main analysis, I use regressors which apply two different thresholds to define a ‘rainy’ day, a low 3 mm and a high 8 mm threshold.¹⁷ However, I explore the effects of the threshold also more rigorously by increasing it in integer steps from 1 mm up to 10 mm of rainfall.

I use community identifiers and the panels’ time dimensions to match rainfall and individual level outcome data. With women’s changes in family planning behaviors at the monthly date level, also the number of ‘rainy’ days on each weekday needs to be counted over the monthly date. In contrast, the data on households’ timing of acquisitions of mosquito bed nets is available in months before the interview. Hence, for the analysis of this outcome, ‘rainy’ days must be counted over months before the interview, which are different days for each household depending on the date of the interview. I define a month before the interview as 30 days. In my coding, 0 months before the interview spans the time period from 0 to 29 days before the date of the interview. 1 month before spans 23 to 62 days before the date of the interview. All higher months (2-11) are defined as the 30 days centered around the respective month before the interview.¹⁸ Table A3 presents summary statistics of the number of ‘rainy’ Saturdays with different thresholds from February 2005 until June 2010 for all sampled communities in the 2010 Rwandan DHS. The statistics are very similar for rainfall regressors for other weekdays and time periods.

5 Empirical Strategy

The hypothesis to be tested is whether community work was used to implement performance targets. The logic which I exploit for identification is outlined in the conceptual framework above. In short, rainfall is expected to affect attendance rates at community work and low attendance

17. In meteorology, daily rainfall of up to 3 mm is classified as light rain and rainfall above 8 mm is classified as heavy rain. Furthermore, these two thresholds are very close to the opposing endpoints of rainfall thresholds which have typically been used in the economics literature (compare Madestam et al. 2013; Bonnier et al. 2019).

18. Months 2-11 before the interview are defined as the days from 30 days \times monthsbefore – 15 days until 30 days \times monthsbefore + 14 days.

rates hamper the effectiveness of community work for target implementation. Ideally, I would like to use an instrumental variable framework to identify the causal effect of community work on behavioral change. Without data on attendances at community work, my analysis relies on estimating the reduced-form relationship. Specifically, I run regressions of the following functional form

$$y_{it} = \sum_{d=1}^7 \beta_d \text{rain}_{ctd} + \alpha_i + \tau_t + \varepsilon_{it}. \quad (1)$$

y_{it} is a binary outcome indicating the change of behavior of the observational unit i during month t . Depending on the outcome an observational unit is either an individual household or woman. rain_{ctd} is the number of rainy days on a weekday d in a unit's community c during the month. The separate count variables for the number of rainy Mondays, Tuesday, etc. in the regression equation take on integer values between 0 and 5, the maximum number of any weekday in a month. α_i and τ_t are unit (household or women) and month \times calendar-year time fixed effects, respectively. The coefficients β_d capture the average effects of rainfall on weekdays d on the probability to change behavior during any given monthly of the estimation period. With unit fixed effects, these coefficient estimates are identified within units from temporal variation in rainfall and behavioral change. For all estimations, I report community clustered standard errors, allowing the error term ε_{it} to be correlated within communities over time.

The rainfall regressors in equation 1 are exogenous conditional on the fixed effects. Hence, coefficients β_d can be interpreted causally. Following the conceptual framework, I expect rainfall on Saturdays to proxy for attendance rates at community work on that day in my reduced-form regressions. Contrary, rainfall on any other day should have no effect on the outcome. Each of these other rainfall regressors can be considered a natural placebo test for my proposed channel of target implementation.

Notably, previous research finds that regressions with rainfall data of high temporal resolution regularly produce spurious significant coefficient estimates (Harari and La Ferrara 2018; Lind 2019; Fujiwara et al. 2016). The authors propose the inclusion of spatially varying time trends to mitigate spurious findings. My estimates change little with the inclusion of spatial idiosyncratic time trends and significance is generally robust, indicating that trends cannot eliminate spurious findings (results not presented). A better approach to filter out spurious effects in my analysis and check the robustness of my findings, is to vary the definition of a rainy day. For each outcome, I present estimations of equation 1 with two different definition thresholds, a low 3 mm and a high 8 mm threshold of daily rainfall. Only when the coefficient estimates are similar and significant for both thresholds should an effect be considered real. In addition to this rule of thumb, I run a battery of regressions of equation 1 with only one rainfall regressor and when varying the rainfall threshold in integer steps from 1 mm to 10 mm. I discuss the findings in the main text, but show their results only in the appendix.

TABLE 1: MAIN EFFECTS: ACQUISITION OF MOSQUITO BED NETS

Dependent variable:	Acquisition				
	'Rainy':	Rainfall > 3 mm		Rainfall > 8 mm	
		DHS:	2005	2007-08	2005
	(1)	(2)	(3)	(4)	
# Rainy Saturdays	-0.019 (0.032)	-0.203*** (0.070)	0.050 (0.041)	-0.217** (0.099)	
# Rainy Sundays	0.036 (0.035)	0.090 (0.074)	0.008 (0.043)	0.240** (0.103)	
# Rainy Mondays	0.009 (0.031)	-0.115* (0.066)	0.067 (0.048)	-0.039 (0.088)	
# Rainy Tuesdays	0.028 (0.038)	0.027 (0.070)	-0.009 (0.046)	-0.103 (0.096)	
# Rainy Wednesdays	-0.033 (0.037)	-0.019 (0.074)	-0.028 (0.046)	0.048 (0.115)	
# Rainy Thursdays	-0.043 (0.033)	0.065 (0.075)	-0.106*** (0.040)	0.027 (0.112)	
# Rainy Fridays	-0.021 (0.032)	0.016 (0.066)	-0.046 (0.045)	-0.067 (0.094)	
Household FE	Yes	Yes	Yes	Yes	
Months-before-interview FE	Yes	Yes	Yes	Yes	
Observations	121,752	87,444	121,752	87,444	
Dep. var. mean (%)	0.636	1.969	0.636	1.969	

Notes: Table reports coefficients β_d from estimation of equation 1. Acquisition is a binary indicator evaluating to 1 if a household acquired a mosquito bed net in a given month. # Rainy XXdays is a count variable between 0 and 5 of the number of rainy XXdays in a month. Estimates are reported in %. Community clustered SE. P-value: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6 Results

6.1 Mosquito Bed Nets

Table 1 presents estimates of equation 1, regressing the indicator of household acquisitions of one or more mosquito bed nets on the number of rainy days which accumulate on each of the seven weekdays in a month. All estimates are displayed in percent (i.e. scaled by 100) for ease of exposition. Columns (1) and (3) show estimates from regressions with panel data before the introduction of performance contracts (constructed from 2005 DHS data). Columns (2) and (4) are analogue regressions with panel data after the introduction (constructed from 2007-08 DHS data). The difference between each of the two regressions in a set is the definition of the explanatory rainfall variable. Columns (1) and (2) use a 3 mm threshold to define a rainy day, and columns (3) and (4) apply an 8 mm threshold in the definition.

The results in column (1) demonstrate that rainfall on any day cannot explain acquisitions of mosquito bed nets before the introduction of performance contracts. However, in column (3) when using the high, 8 mm threshold to define a rainy day, rainfall on Thursdays becomes suddenly significant. In contrast, columns (2) and (4) show that rainfall on Saturdays significantly explains acquisitions after the introduction of performance contracts. The coefficient estimates are statistically significant at the 1% and 5% level. Interestingly, while the estimate on rainfall on Saturdays is very similar across the two regressions, the coefficient on other rainfall regressors

changes strongly with the rainfall threshold. For example, rainfall on Sundays is close to zero and insignificant in column (2) with the low 3 mm rainfall threshold, but becomes significant in column (4) with the high 8 mm rainfall threshold.

Spuriously significant rainfall regressors are a common phenomenon when working with daily rainfall data in a panel setting (Lind 2019). In figure B2 in the appendix, I explore for each rainfall regressor if its relation with the outcome is spurious in the multivariate regressions of columns (2) and (4), after the introduction of performance contracts. Specifically, I estimate equation 1 with one rainfall regressor only and vary the rainfall threshold applied in the definition of the number of rainy days. The threshold is increased in integer steps from 1 mm to 10 mm, yielding 10 coefficient estimates for rainfall on each weekday from separate regressions. The results show that rainfall on Saturdays remains statistically significant at the 5% significance level for all, but one threshold (5 mm). In contrast, none of the coefficients on rainfall on any other day is statistically significant at the 5% level (with the exception of rainfall on Mondays at 5 mm).

The results in figure B2 offer two conclusions which have important implications. First, rainfall on Saturdays robustly and significantly explains acquisitions of mosquito bed nets after the introduction of performance contracts. Second, spurious significance of (other) rainfall coefficients is a problem in regressions which include multiple rainfall regressors. For example, the significant coefficient estimate on rainfall on Sundays in column (4) of table 1 is spurious because rainfall on Sundays is never significant at any rainfall threshold when included as a single regressor (figure B2).

The contrast between regressions with one and multiple rainfall regressors suggests that spurious estimates arise from some correlational pattern between the rainfall measures. This pattern may suddenly shift with a different threshold which defines a rainy day. Because spurious coefficient estimates are charged and identified through the correlational pattern, these estimates vary strongly with the rainfall threshold in multivariate regressions. As a rule of thumb may serve here that effects are real if coefficient estimates remain stable across different rainfall thresholds. Contrary, estimates are spuriously significant if significance and size vary strongly with the threshold. While this simplified rule is useful for reading results from regressions with multiple rainfall regressors, I provide an evaluation, analogue to figure B2, for each analyzed outcome in the appendix.

The coefficient estimates on the rainfall regressors in table 1 (and figure B2) are easy to interpret. A coefficient of -0.203% on rainfall on Saturdays in column (3) of table 1, for example, means that one additional rainy Saturday in a given month reduces a household's probability to acquire a mosquito bed net in the same month by -0.203%. This effect is large when contrasted with the mean of the dependent variable, 1.969%. A single rainy Saturday with above 3 mm rainfall lowers the probability of acquisition by approximately 10%. Obviously, there can be up to 5 rainy Saturdays in a month, each of which lowers the probability of acquisition further. Further regressions on separate indicators for each number of rainy Saturdays in a month, ex-

plore potential non-linearity in the effects (results not presented). Generally speaking, the effect becomes stronger with each additional rainy Saturday.

Interpretation. My findings document a reduced-form relationship between rainfall on Saturdays and acquisitions of mosquito bed nets. If rainfall generated the results directly, I would expect significant effects of rainfall on varying weekdays, especially in the regressions with a single rainfall regressor. The absence thereof demonstrates that rainfall on Saturdays creates exogenous variation in an event on that day. This event arguably is community work.

As outlined in the conceptual framework, I hypothesize rainfall to affect attendances at community work. Low attendance rates cause a breakdown of the implementation process at community work and render it ineffective for policy-making.¹⁹ However, without data on attendances, this interpretation of the results on rainfall on Saturdays relies on a cumulative body of evidence, rather than a straight forward test. Provisionally, two facts provide evidence in support of a latent first-stage with community work. First, it is ascertained knowledge that community work takes place on Saturdays across Rwanda and is used for policy-making by the government. And second, rainfall on Saturdays is insignificant in regressions with data before the introduction of performance contracts. The (arguably) only change with performance contracts was that mosquito bed nets became a performance target for local leaders. The following heterogeneity analysis provides further evidence which supports my interpretation of the results.

Heterogeneity. Table 2 presents results from an analysis of heterogeneity in the effects of rainfall, estimating equation 1 with panel data constructed from the 2007-08 DHS. A rainy day is defined by rainfall > 3 mm. Columns (1) and (2) split the sample by altitude. Columns (3) and (4) present results when using two different dependent variables. In column (3) the indicator of acquisitions of mosquito bed nets evaluates to 1 if the net was acquired at a Health Center. In column (4) the indicator evaluates to 1 if the mosquito bed net was obtained from somewhere else (e.g. a shop). This information on the source of acquisition is only available for nets acquired within the previous 6 months. Hence, columns (3) and (4) report the results from estimating equation 1 on this short panel.

It is well known that the risk of contracting malaria decreases with altitude. In East Africa, the incidence of anopheles mosquitoes, which transmit the disease, is just about zero at altitudes above 1,800 meters (Bodker et al. 2003). One third of Rwandan villages are situated above this altitude and without the vector (i.e. mosquitoes) the risk of malaria is non-existent. In practice, hence, there is no need for mosquito bed nets at these levels of altitude. However, performance contracts are very similar across Rwanda and did not account for such difference. With this in mind, it may be unsurprising that households acquired mosquito bed nets irrespective of altitude when these were performance targets. The significant coefficient estimates on rainfall on Saturdays in columns (1) and (2) of table 2 indicate that community work was used for implementation. Comparing the coefficient sizes in both regressions, also strongly supports the

19. This interpretation is consistent with finding significant effects on rainfall on Saturdays even for low rainfall thresholds.

TABLE 2: HETEROGENEITY: ACQUISITION OF MOSQUITO BED NETS

2007-08 DHS, Sample:	Altitude in meters		6 Months before Interview	
	< 1,800	≥ 1,800	... from ...	
Dependent variable:	Acquisition		Health Center	Other source
	(1)	(2)	(3)	(4)
# Rainy Saturdays	-0.152* (0.089)	-0.307*** (0.107)	-0.059 (0.081)	-0.147** (0.058)
# Rainy Sundays	0.134 (0.101)	0.009 (0.093)	0.031 (0.070)	0.086 (0.062)
# Rainy Mondays	-0.125 (0.086)	-0.116 (0.097)	0.068 (0.067)	-0.050 (0.053)
# Rainy Tuesdays	0.050 (0.083)	-0.048 (0.129)	-0.017 (0.070)	0.008 (0.052)
# Rainy Wednesdays	-0.041 (0.089)	0.060 (0.135)	0.171** (0.073)	-0.055 (0.057)
# Rainy Thursdays	0.038 (0.100)	0.080 (0.115)	0.083 (0.082)	0.029 (0.066)
# Rainy Fridays	-0.001 (0.088)	0.039 (0.090)	0.037 (0.076)	0.071 (0.065)
Household FE	Yes	Yes	Yes	Yes
Months-before-interview FE	Yes	Yes	Yes	Yes
Observations	58,212	29,232	43,722	43,722
Dep. var. mean (%)	2.134	1.642	1.107	0.702

Notes: Table reports coefficients β_d from estimation of equation 1 with data from the 2007-08 DHS. Acquisition is a binary indicator evaluating to 1 if a household acquired a mosquito bed net in a given month. # Rainy XXdays is a count variable between 0 and 5 of the number of rainy XXdays in a month. A rainy day is defined by rainfall > 3 mm. Estimates are reported in %. Community clustered SE. P-value: *** p < 0.01, ** p < 0.05, * p < 0.1. Columns (1) and (2) use data on Acquisitions 0-11 months before the interview and makes a sample split by altitude. Columns (3) and (4) use Acquisitions from specific sources as the dependent variable. Information on the source is only available for mosquito bed nets acquired 0-5 months before the interview.

anecdotal evidence of section 2 that local leaders use compulsion to implement targets. At high altitudes, when mosquito bed nets are unneeded, local leaders plausibly must use a higher level of compulsion for target implementation. With this compulsion exerted during community work, a larger coefficient estimate on rainfall on Saturdays at high altitudes is, in fact, unsurprising.

Columns (3) and (4) of table 2 speak to a potential concern that rainfall on Saturdays is unrelated to community work, but simply affects access through some other unknown but common activity on Saturdays. The coefficient estimates on rainfall on Saturdays are negative in both regressions. However, the estimate is statistically significant only in column (4) with acquisitions from a source other than the Health Center as the dependent variable. With this other source being one from a list of 8 (with a quite dispersed distribution), the results show that there seems to be no single and common activity on Saturdays, other than community work, which regulates acquisitions of mosquito bed nets and is affected by rainfall.

Overall, the results of this section provide strong evidence that community work on Saturdays was used to implement the performance target of mosquito bed nets. The purpose of the next section is to reproduce the findings for a second performance target and shed light on what happens at community work.

6.2 Family Planning

This section analyzes the effect of attendance rates at community work (proxied by rainfall on Saturdays) on three events in family planning. Women may either adopt a modern contraceptive method, discontinue it or become pregnant (i.e. conceive). While these three outcomes are interdependent, I will analyze them for simplicity separately.

6.2.1 Adoptions

Table 3 is organized analogue to table 1 and presents estimates of equation 1 from regressions of an indicator of women’s adoptions of modern contraceptive methods on the number of rainy days on each weekday in a month. Columns (1) and (3) report estimates from before the introduction of performance contracts, and columns (2) and (4) show estimates from after the introduction. For the analysis, I define the before period as the 12 months from April 2005 until March 2006. The after period, I define as stretching over 24 months from April 2006 until March 2008. Later in section 6.3, I will present estimates from rolling regressions which demonstrate that the results also hold for shorter after periods. Columns (1) and (2) in table 3 use a 3 mm threshold to define a rainy day, and columns (3) and (4) apply an 8 mm threshold in the definition. All estimates are reported in percent.

TABLE 3: MAIN EFFECTS: ADOPTIONS OF MODERN CONTRACEPTIVE METHODS

Dependent variable:	Adoption				
	‘Rainy’:	Rainfall > 3 mm		Rainfall > 8 mm	
Data:	before	after	before	after	
	(1)	(2)	(3)	(4)	
# Rainy Saturdays	0.019 (0.025)	-0.054*** (0.019)	-0.004 (0.035)	-0.089*** (0.026)	
# Rainy Sundays	0.029 (0.022)	0.017 (0.022)	0.064* (0.038)	0.030 (0.025)	
# Rainy Mondays	0.004 (0.028)	0.020 (0.023)	0.020 (0.036)	-0.022 (0.032)	
# Rainy Tuesdays	-0.009 (0.021)	-0.003 (0.025)	0.026 (0.036)	0.014 (0.028)	
# Rainy Wednesdays	0.012 (0.021)	-0.061** (0.024)	-0.011 (0.028)	-0.033 (0.030)	
# Rainy Thursdays	-0.002 (0.027)	0.009 (0.023)	0.019 (0.034)	0.020 (0.028)	
# Rainy Fridays	-0.020 (0.023)	0.021 (0.022)	-0.014 (0.024)	0.006 (0.027)	
Women FE	Yes	Yes	Yes	Yes	
Monthly-date FE	Yes	Yes	Yes	Yes	
Observations	125,193	268,648	125,193	268,648	
Dep. var. mean (%)	0.228	0.489	0.228	0.489	

Notes: Table reports coefficients β_d from estimation of equation 1. Adoption is a binary indicator evaluating to 1 if a woman started modern contraceptive use in a given month. # Rainy XXdays is a count variable between 0 and 5 of the number of rainy XXdays in a month. Estimates are reported in %. Community clustered SE. P-value: *** p < 0.01, ** p < 0.05, * p < 0.1.

The estimates in columns (1) and (3) show that rainfall does not affect women’s adoptions of modern contraceptive methods before the introduction of performance contracts. The threshold defining a rainy day is irrelevant for this result. In contrast, columns (2) and (4) report at the 1% level significant, negative coefficient estimates on rainfall on Saturdays for the time period after the introduction of performance contracts. Similar to the results on acquisitions of mosquito bed nets, the size of coefficient estimates on rainfall on other days varies quite strongly with the rainfall threshold. Rainfall on Wednesdays significantly explains adoptions in column (2) with a 3 mm threshold, but not in column (4) with an 8 mm threshold.

Figure B3 in the appendix is an evaluation of potentially spurious significances. It proceeds analogue to figure B2. That means, it presents coefficient estimates on the number of rainy days on a weekday from separate regressions of contraceptive adoption on one rainfall regressor when varying the rainfall threshold. Rainfall on Saturdays is significant at the 5% level across all thresholds, except the lowest at 1 mm rainfall. Contrary, rainfall on any other day is never significant with the exception of rainfall on Wednesdays. Rainfall on Wednesdays is significant in 4 out of 10 regressions when the defining rainfall threshold is between 3 mm and 6 mm. This finding arguable arises by chance for the particular panel length and can, hence, be attributed to multiple hypothesis testing. In an analysis (not presented) with a shorter, 12 months panel after the introduction of performance contracts rainfall on Saturdays is equally significant, but rainfall on Wednesdays is not.

The interpretation of coefficient estimates is identical to the interpretation of the regression results in the previous section on mosquito bed nets. A coefficient estimate of -0.054% in column (2) of table 3, for example, means that one additional rainy Saturday reduces a woman’s probability to adopt modern contraception in the same month by -0.054%. This effect size is 11% of the unconditional probability (0.489%) and comparable to the effect size found for acquisitions of mosquito bed nets. Regressions on separate indicators for each number of rainy Saturdays in a month show (as before for acquisitions) that the effect becomes generally stronger with each additional rainy Saturday in a month (results not presented).

The above evidence on a second performance target, strongly indicates that local leaders used community work on Saturdays for target implementation. Having the same results pattern suggests an identical mechanism how rainfall affects choice in both outcomes, adoptions and acquisitions. Two complementary pieces of information show that women received modern contraceptives almost exclusively at health facilities. First, 91% of women who use any modern contraceptive method in the 2010 DHS report that their first source of that method was a health facility and not some other source like a shop, pharmacy or outreach campaign. And second, a stable 90% used hormonal contraceptive methods. These methods were also officially only available at health facilities (USAID et al. 2011). With the results in table 3 being fully robust to using adoptions of hormonal contraceptive methods as the dependent variable (instead

of all modern methods), I can conclude that my results must be generated by women who access modern contraceptives at health facilities.²⁰

Furthermore, ample evidence demonstrates that women could not and did not visit health facilities on Saturdays during the time period under analysis. The primary reason is that most health facilities were closed on weekends and those open had high surcharges for services (Ueberschär 2018).²¹ If Saturday is the day of community work, but not the day on which women receive modern contraceptives, then what happens at community work? To investigate this question further, I analyze the effect on discontinuations of modern contraceptive use and conceptions.

6.2.2 Discontinuation

Analogue to the previous table 3, table 4 presents the estimation of equation 1 but with the dependent variable being women’s discontinuations of modern contraceptive methods. The indicator for discontinuation is regressed on the number of rainy days on each weekday in a month. Columns (1) and (3) report estimates from the 12 months before the introduction of performance contracts, and columns (2) and (4) show estimates from 24 months after the introduction. The difference between the two sets of regressions is again the threshold which defines a rainy day. Columns (1) and (2) use a 3 mm threshold and columns (3) and (4) deploy an 8 mm threshold. All estimates are reported in percent.

The estimates in columns (1) and (3) of table 4 show that rainfall cannot explain discontinuations of modern contraceptive methods before the introduction of performance contracts. Only rainfall on Tuesdays is significant in column (1), but not in column (3). In contrast, the estimates in columns (2) and (4) clearly show that rainfall on Saturdays significantly affects discontinuations after the introduction of performance contracts. No other rainfall regressor is statistically significant, with the exception of rainfall on Sundays in column (4).

As with the results for other outcomes, the coefficient estimates on rainfall days other than Saturdays vary quite strongly with the rainfall threshold in table 4. For this reason, figure B4 in the appendix evaluates whether effects are spurious in the same way as has been done for the other outcomes. It presents coefficient estimates on the number of rainy days from separate regressions of discontinuation on one rainfall regressor when varying the rainfall threshold. Rainfall on Saturdays is significant at the 5% level in 7 out of 10 regressions. Rainfall on every other day and threshold is consistently insignificant, with the exception of rainfall on Sundays, which becomes significant at 6 mm and higher rainfall thresholds. I attribute this finding in the estimate to multiple hypothesis testing.

20. This finding contrasts the previous result that rainfall on Saturdays affects acquisitions of mosquito bed nets from sources other than health facilities (see table 2). The divergence in sources also indicates that there is no single other activity responsible for the effect of rainfall on Saturdays, but community work.

21. Two data analyses provide further evidence that health services were unavailable on weekends. First, data from the 2007 Rwandan DHS Service Provision Assessment reveals that family planning services were mostly available on five or less days, i.e. not on weekends. And second, investigating the day of vaccinations from children’s health cards in 2010 DHS data shows that only 2% of children were vaccinated on a Saturday. A uniform distribution over seven weekdays should yield 14% vaccinations on Saturdays.

TABLE 4: MAIN EFFECTS: DISCONTINUATIONS OF MODERN CONTRACEPTIVE METHODS

Dependent variable: 'Rainy': Data:	Discontinuation			
	Rainfall > 3 mm		Rainfall > 8 mm	
	before (1)	after (2)	before (3)	after (4)
# Rainy Saturdays	0.012 (0.013)	0.024** (0.011)	0.025 (0.020)	0.033** (0.015)
# Rainy Sundays	-0.012 (0.011)	-0.013 (0.011)	-0.015 (0.017)	-0.037** (0.015)
# Rainy Mondays	-0.008 (0.012)	0.002 (0.012)	0.007 (0.017)	0.021 (0.016)
# Rainy Tuesdays	0.034** (0.015)	-0.002 (0.014)	0.030 (0.021)	-0.006 (0.017)
# Rainy Wednesdays	0.011 (0.013)	-0.016 (0.013)	0.005 (0.016)	-0.008 (0.015)
# Rainy Thursdays	-0.015 (0.016)	0.017 (0.012)	-0.006 (0.018)	0.016 (0.017)
# Rainy Fridays	0.013 (0.012)	-0.005 (0.012)	0.013 (0.014)	-0.013 (0.016)
Women FE	Yes	Yes	Yes	Yes
Monthly-date FE	Yes	Yes	Yes	Yes
Observations	125,193	268,648	125,193	268,648
Dep. var. mean (%)	0.065	0.145	0.065	0.145

Notes: Table reports coefficients β_d from estimation of equation 1. Discontinuation is a binary indicator evaluating to 1 if a woman stops modern contraceptive use in a given month. # Rainy XXdays is a count variable between 0 and 5 of the number of rainy XXdays in a month. Estimates are reported in %. Community clustered SE. P-value: *** p < 0.01, ** p < 0.05, * p < 0.1.

The interpretation of coefficient estimates in table 4 is the same as before. An estimate of 0.024% in column (2), for example, means that one rainy Saturday increases a woman's probability to discontinue modern contraceptive use by 0.024%. The effect size, compared to the unconditional probability of 0.145%, is 16% (column 2) to 31% (column 4). As such, the effect is, in relative terms, larger than the effect on acquisitions of mosquito bed nets or contraceptive adoptions. In further contrast to these previous results, the coefficient on rainfall on Saturdays has a positive sign.

The results in table 4 indicate that inter-personal interaction takes place at community work. Up to now, one might have hypothesized that women only receive some kind of information at community work, e.g. about contraceptives or government policy. Then, women would change behavior following community work because they learned this information there. However, the positive coefficient on rainfall on Saturdays in table 4 contradicts this explanation. It would mean that women 'unlearn' the information in times when they or others do not attend community work (due to rainfall). 'Unlearning' from one month to the next is very improbable in the Rwandan context and for a behavior as pervasive as contraception. With access being unaffected and women discontinuing when attendance rates are low, it rather must be the case that meeting and interacting at community work is determining women's choice of behavior each time again. Hence, the evidence indicates that some inter-personal interaction takes place at community

work, and this interaction is impaired when rainfall lowers attendance rate. The following results on conceptions provide evidence on the likely nature of interaction.

6.2.3 Conception

Table 5 presents estimates of equation 1 regressing an indicator of conception (i.e. when women become pregnant) on the number of rainy days on each weekday in a month. The table is organized identical to the tables before. Columns (1) and (3) report estimates from the 12 months before the introduction of performance contracts, and columns (2) and (4) display estimates from the 24 months after the introduction. The difference between the two sets of regressions is once again the applied threshold in the definition of the rainfall regressors. Columns (1) and (2) report estimates on regressors with a 3 mm threshold and columns (3) and (4) use an 8 mm threshold. All estimates are reported in percent.

TABLE 5: MAIN EFFECTS: CONCEPTION

Dependent variable: 'Rainy': Data:	Conception			
	Rainfall > 3 mm		Rainfall > 8 mm	
	before (1)	after (2)	before (3)	after (4)
# Rainy Saturdays	0.064 (0.065)	-0.076** (0.038)	0.070 (0.103)	-0.113** (0.047)
# Rainy Sundays	-0.076 (0.059)	0.016 (0.036)	-0.071 (0.092)	0.030 (0.044)
# Rainy Mondays	0.022 (0.068)	0.061* (0.037)	-0.001 (0.090)	-0.018 (0.052)
# Rainy Tuesdays	-0.059 (0.066)	-0.058 (0.037)	-0.119 (0.088)	-0.026 (0.046)
# Rainy Wednesdays	-0.012 (0.058)	-0.067 (0.041)	-0.033 (0.073)	-0.040 (0.048)
# Rainy Thursdays	-0.136* (0.070)	-0.007 (0.040)	-0.044 (0.080)	0.010 (0.050)
# Rainy Fridays	0.001 (0.062)	0.074** (0.037)	-0.066 (0.067)	0.031 (0.055)
Women FE	Yes	Yes	Yes	Yes
Monthly-date FE	Yes	Yes	Yes	Yes
Observations	125,193	268,648	125,193	268,648
Dep. var. mean (%)	1.601	1.441	1.601	1.441

Notes: Table reports coefficients β_d from estimation of equation 1. Conception is a binary indicator evaluating to 1 if a woman conceives (i.e. becomes pregnant) in a given month. # Rainy XXdays is a count variable between 0 and 5 of the number of rainy XXdays in a month. Estimates are reported in %. Community clustered SE. P-value: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The estimates in columns (1) and (3) demonstrate that rainfall cannot explain conception before the introduction of performance contracts. No coefficient including the estimate on rainfall on Saturdays is statistically significant at the 5% level. In contrast, the coefficient on rainfall on Saturdays is negative and statistically significant in columns (2) and (4) after the introduction of performance contracts. In column (4), rainfall on Saturdays is the only significant regressor. In

column (2), also rainfall on Fridays displays a similarly sized, positive and significant coefficient. This finding is spurious as evident from figure B5 in the appendix.

Figure B5 in the appendix presents coefficient estimates from separate regressions of discontinuation on one rainfall regressor when applying different thresholds in the definition of the regressor. Rainfall on Saturdays is significant at the 5% level in 7 out of 10 regressions with thresholds between 3 mm and 9 mm. Rainfall on every other day and threshold is consistently insignificant. Only rainfall on Mondays displays one significant configuration at a threshold of 4 mm. These results demonstrate that only the effect of rainfall on Saturdays is real and robust after the introduction of performance contracts.

The interpretation of coefficient estimates is unchanged. An estimate of -0.076% or -0.113% on rainfall on Saturdays in columns(2) and (4) means that a woman's probability of conception is lowered by the same percentage for every additional rainy Saturday in a month. Compared against the unconditional probability, the relative effect size is 5% and 8%. In this respect, the effect of rainfall on Saturdays on conception is comparatively smaller than for the other outcomes after the introduction of performance contracts.

The evidence on conception is suggestive of how performance targets were implemented during community work. Based on simple mechanics one would have expected that if rainfall lowers contraceptive adoption, it should, corollary, increase conception. However, table 5 shows the opposite. Rainfall on Saturdays significantly reduces a woman's probability to become pregnant. While possibly surprising, this finding aligns well with the available evidence on performance contracts, which suggests that compulsion was used to achieve contracts. Along these lines, pregnancy may have protected women (for some time) against the requirement and pressure to adopt modern contraception. In this respect, women may have used conception as an outside option to contraceptive use.

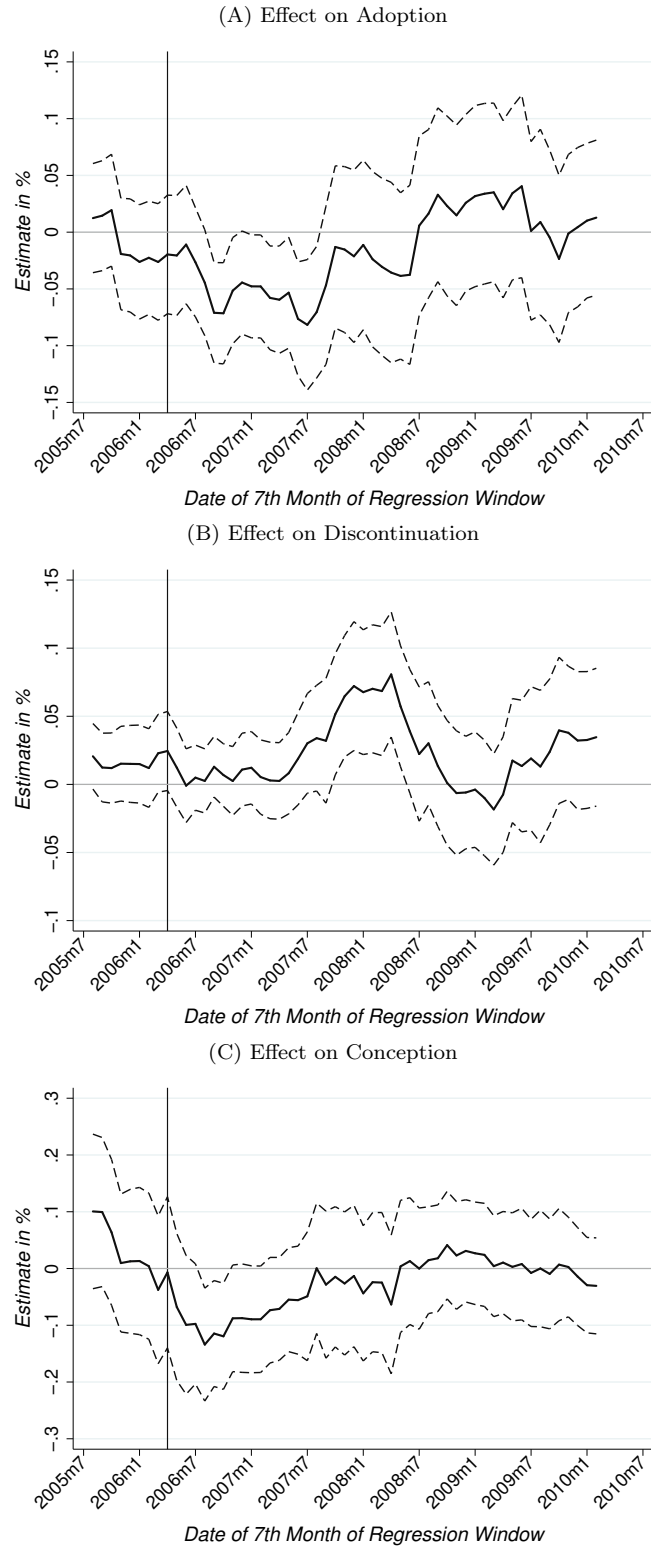
In sum, the evidence presented in this paper suggests that community work was used to implement nationwide performance targets in Rwanda. Access and information cannot explain the results, but the evidence indicates that inter-personal interaction at community work shape choices in a repeated fashion. The findings are consistent with other evidence that compulsion was used to implement development targets. This comprehensive body of evidence provides a new perspective on the Rwandan government's successes in achieving development more generally and is illustrative for the operation of governments in other authoritarian countries.

6.3 Effects on Family Planning over Time

Figure 3 illustrates how the effect of rainfall on Saturdays changes for the three family planning outcomes over time. The panels present for each outcome coefficient estimates and 95% confidence intervals on rainfall on Saturdays from 12-months rolling window estimations of equation 1. A rainy day is defined by rainfall above 3 mm. The solid black curves connect the coefficient estimates and the dashed curves mark confidence intervals. Estimates are presented in percent. The vertical line on April 2006 marks the introduction of performance contracts in Rwanda.

Shortly after the introduction of performance contracts, rainfall on Saturdays significantly reduces a woman's probability to adopt modern contraception in the same month, illustrated in

FIGURE 3: ROLLING WINDOW COEFFICIENT ESTIMATES ON # of Rainy Saturdays



Definition of *Rainy Day*: Rainfall > 3 mm. Window Size: 12 months. Controls: # of Rainy XXXdays for every other weekday. Dashed curves mark 95% CIs. Community clustered SE. Vertical line marks the introduction of performance contracts in April 2006.

figure 3A. Provided that rainfall lowers attendance rates, this effect suggests a positive effect of attendance rates at community work on the probability of contraceptive adoption. Conversely, the effect on discontinuations, shown in Figure 3B, is close to zero, but increases with time. It becomes significant roughly one and a half years after the introduction of performance contracts. This sequence is consistent with the notion that the women who generate the results first need to adopt contraception before they can discontinue. Contrary to the effect on contraceptive adoption, Figure 3B suggests that low (high) attendance rates increase (reduce) the probability to discontinue contraceptive use.

The effect pattern of Saturday rainfall on conception is displayed in Figure 3C. As with adoption, a negative effect of rainfall on Saturdays suggests that low (high) attendance rates at community work reduce (increase) the probability of conception in the same month. This direction of the effect is consistent with some women possibly using pregnancy strategically to avoid contraceptive use after the introduction of performance contracts.

While the dynamic pattern of coefficient estimates is interesting in itself, I can only speculate why it arises. My hypothesis is that the estimates change mechanically with the stock of different types of women choosing one or the other behavior. For example, a first distinction may be between women affected by community work and those not. Now, among those affected, some may opt to use modern contraceptives while others may rather choose to become pregnant. For this reason, we may observe an immediate effect of rainfall on Saturdays on adoptions and conceptions. However, only when the women affected by community work have adopted contraception can we observe an effect on discontinuations. Hence, this effect is delayed as displayed in figure 3B. At some point (maybe in 2009), it could be that an equilibrium arise in which all women who are affected by community work have sorted and choose their optimal behavior. Any changes in behavior are only conducted by women not affected by community work. In this case, it would not be surprising that community work does not affect the outcomes anymore. However, whether this rough sketch of an hypothesis can, unfortunately, not be verified with the available data and must be left for future research.

7 Conclusion

This article studies the role of community meetings and performance incentives in Rwanda to achieve development targets in health. Specifically, it investigates how community work was used by the Rwandan government to change individual behavior after introducing performance contracts at all levels of public administration. The contracts strongly incentivize local leaders to implement the targets set by the central government. To organize implementation, these leaders use community work on Saturdays to discuss, monitor and enforce compliant behavior. Findings from two different and unrelated targets suggests that performance contracts and community work form a governance structure which can be used to implement a wide range of development goals. Similar systems also operate in other countries, e.g. in China, Cuba or North

Korea. At this level, the article provides a better understanding of institutions which are used to control individual behavior in autocracies. For Rwanda, it proposes a new perspective on its top-performance in many of the Millenium Development Goals and an explanation for its recent fertility transition.

The results present strong evidence that community work was used to implement government policies after the introduction of performance contracts. The analysis exploits rainfall on the day of community work (i.e. rainfall on Saturdays) as an exogenous source of variation in attendance rates over time. Reduced-form regressions show that rainfall on Saturdays reduced (i.e. community work increased) the probability of households to acquire mosquito bed nets and women to adopt modern contraception within the first two years after the introduction of performance contracts. During the same time, rainfall on Saturdays (community work) increased (decreased) the probability of contraceptive discontinuation. Rainfall on Saturdays did not affect access to contraception. Interestingly, the effect of rainfall (community work) on conception is negative (positive). This finding is consistent with the notion that women who feared contraceptive use may have chosen to become pregnant instead. No consistently significant effects are found for rainfall on other days of the week.

Local institutions determine whether specific policies are generally beneficial. With strong incentives for local leaders, target implementation in Rwanda has arguable relied on some form of pressure rather than on voluntary contributions based on understanding. One recommendation when drafting policies is to better account for the institutional context. Generally beneficial policies like decentralization may otherwise have unintended consequences when only implementation responsibilities, but not political power is transferred from the central government to local leaders.

References

- Abera, Wuletawu, Luca Brocca, and Riccardo Rigon. 2016. “Comparative Evaluation of Different Satellite Rainfall Estimation Products and Bias Correction in the Upper Blue Nile (UBN) Basin.” *Atmospheric research* 178:471–483.
- Acemoglu, Daron, Tristan Reed, and James A Robinson. 2014. “Chiefs: Economic Development and Elite Control of Civil Society in Sierra Leone.” *Journal of Political Economy* 122 (2): 319–368.
- Anderson, Siwan, Patrick Francois, and Ashok Kotwal. 2015. “Clientelism in Indian Villages.” *American Economic Review* 105 (6): 1780–1816.
- Andreoni, James, and Laura K Gee. 2012. “Gun for Hire: Delegated Enforcement and Peer Punishment in Public Goods Provision.” *Journal of Public Economics* 96 (11): 1036–1046.
- Ashraf, Nava, Oriana Bandiera, and B Kelsey Jack. 2014. “No Margin, No Mission? A Field Experiment on Incentives for Public Service Delivery.” *Journal of Public Economics* 120:1–17.
- Banerjee, Abhijit V, Rukmini Banerji, Esther Duflo, Rachel Glennerster, and Stuti Khemani. 2010. “Pitfalls of Participatory Programs: Evidence from a Randomized Evaluation in Education in India.” *American Economic Journal: Economic Policy* 2 (1): 1–30.
- Bardhan, Pranab, and Dilip Mookherjee. 2006. “Pro-poor Targeting and Accountability of Local Governments in West Bengal.” *Journal of Development Economics* 79 (2): 303–327.
- Berry, Marie E. 2015. “When “Bright Futures” Fade: Paradoxes of Women’s Empowerment in Rwanda.” *Signs: Journal of Women in Culture and Society* 41 (1): 1–27.
- Björkman Nyqvist, Martina, Damien de Walque, and Jakob Svensson. 2017. “Experimental Evidence on the Long-run Impact of Community-based Monitoring.” *American Economic Journal: Applied Economics* 9 (1): 33–69.
- Björkman, Martina, and Jakob Svensson. 2009. “Power to the People: Evidence from a Randomized Field Experiment on Community-based Monitoring in Uganda.” *The Quarterly Journal of Economics* 124 (2): 735–769.
- Bodker, Rene, J Akida, D Shayo, W Kisinza, HA Msangeni, EM Pedersen, and SW Lindsay. 2003. “Relationship Between Altitude and Intensity of Malaria Transmission in the Usambara Mountains, Tanzania.” *Journal of Medical Entomology* 40 (5): 706–717.
- Bonnier, Evelina, Jonas Poulsen, Thorsten Rogall, and Miri Stryjan. 2019. “Preparing for Genocide: Quasi-Experimental Evidence from Rwanda.”

- Bugingo, Irene, and Reverien Interayamahanga. 2010. *A Study On The Development And Use Of Governance Indicators in Rwanda*. Kigali: Institute of Research and Dialogue for Peace (IRDPA).
- Casey, Katherine, Rachel Glennerster, and Edward Miguel. 2012. "Reshaping Institutions: Evidence on Aid Impacts Using a Preanalysis Plan." *The Quarterly Journal of Economics* 127 (4): 1755–1812.
- Celhay, Pablo, Paul Gertler, Paula Giovagnoli, and Christel Vermeersch. 2018. "Long Run Effects of Temporary Incentives on Medical Care Productivity." *American Economic Journal: Applied Economics* (forthcoming).
- Collins, William J, and Robert A Margo. 2007. "The Economic Aftermath of the 1960s Riots in American Cities: Evidence From Property Values." *The Journal of Economic History* 67 (4): 849–883.
- Dal Bó, Pedro, Andrew Foster, and Louis Putterman. 2010. "Institutions and Behavior: Experimental Evidence on the Effects of Democracy." *American Economic Review* 100 (5): 2205–29.
- De Janvry, Alain, Frederico Finan, and Elisabeth Sadoulet. 2012. "Local Electoral Incentives and Decentralized Program Performance." *Review of Economics and Statistics* 94 (3): 672–685.
- Duflo, Esther, Rema Hanna, and Stephen P Ryan. 2012. "Incentives Work: Getting Teachers to Come to School." *American Economic Review* 102 (4): 1241–78.
- Faillo, Marco, Daniela Grieco, and Luca Zarri. 2013. "Legitimate Punishment, Feedback, and the Enforcement of Cooperation." *Games and Economic Behavior* 77 (1): 271–283.
- Farmer, Didi Bertrand, Leslie Berman, Grace Ryan, Lameck Habumugisha, Paulin Basinga, Cameron Nutt, Francois Kamali, et al. 2015. "Motivations and Constraints to Family Planning: A Qualitative Study in Rwanda's Southern Kayonza District." *Global Health: Science and Practice* 3 (2): 242–254.
- Fehr, Ernst, and Simon Gächter. 2000. "Cooperation and Punishment in Public Goods Experiments." *American Economic Review* 90 (4): 980–994.
- Finan, Frederico, Benjamin A Olken, and Rohini Pande. 2017. "The Personnel Economics of the Developing State." In *Handbook of Economic Field Experiments*, 2:467–514. Elsevier.
- Fujiwara, Thomas, Kyle Meng, and Tom Vogl. 2016. "Habit Formation in Voting: Evidence from Rainy Elections." *American Economic Journal: Applied Economics* 8 (4): 160–188.
- Gomez, Brad T, Thomas G Hansford, and George A Krause. 2007. "The Republicans Should Pray for Rain: Weather, Turnout, and Voting in US Presidential Elections." *Journal of Politics* 69 (3): 649–663.

- Hansford, Thomas G, and Brad T Gomez. 2010. “Estimating the Electoral Effects of Voter Turnout.” *American Political Science Review* 104 (2): 268–288.
- Harari, Mariaflavia, and Eliana La Ferrara. 2018. “Conflict, Climate and Cells: A Disaggregated Analysis.” *The Review of Economics and Statistics* 100 (4): 594–608.
- Huggins, Chris. 2009. “Agricultural Policies and Local Grievances in Rural Rwanda.” *Peace Review* 21 (3): 296–303.
- Joyce, Robert J, John E Janowiak, Phillip A Arkin, and Pingping Xie. 2004. “CMORPH: A Method that Produces Global Precipitation Estimates from Passive Microwave and Infrared Data at High Spatial and Temporal Resolution.” *Journal of Hydrometeorology* 5 (3): 487–503.
- Khan, Adnan Q, Asim I Khwaja, and Benjamin A Olken. 2015. “Tax farming redux: Experimental evidence on performance pay for tax collectors.” *The Quarterly Journal of Economics* 131 (1): 219–271.
- Kraehnert, Kati, Tilman Brück, Michele Di Maio, and Roberto Nisticò. 2019. “The Effects of Conflict on Fertility: Evidence from the Genocide in Rwanda.” *Demography*: 1–34.
- Lind, Jo Thori. 2019. “Spurious Weather Effects.” *Journal of Regional Science* 59 (2): 322–354.
- Madestam, Andreas, Daniel Shoag, Stan Veuger, and David Yanagizawa-Drott. 2013. “Do Political Protests Matter? Evidence from the Tea Party Movement.” *The Quarterly Journal of Economics* 128 (4): 1633–1685.
- Martinez-Bravo, Monica, Priya Mukherjee, and Andreas Stegmann. 2017. “The Non-Democratic Roots of Elite Capture: Evidence From Soeharto Mayors in Indonesia.” *Econometrica* 85 (6): 1991–2010.
- Miguel, Edward, and Mary Kay Gugerty. 2005. “Ethnic Diversity, Social Sanctions, and Public Goods in Kenya.” *Journal of Public Economics* 89 (11): 2325–2368.
- MINALOC. 2008. *Community Development Policy*. Accessed September 17, 2019. <https://www.ilo.org/dyn/natlex/docs/ELECTRONIC/98538/117303/F187363017/RWA-98538.pdf>.
- . 2011. *Concept Note on Umuganda Current Status*. Accessed October 2, 2017. <http://www.rwandapedia.rw/cmises/views/workspace%253A%252F%252FspacesStore%252Ff8d00bc6-4b7b-4d92-bf64-c02373e61f25>.
- MoH. 2008. *Annual Report 2007*. Accessed May 20, 2016. <http://www.moh.gov.rw/fileadmin/templates/MOH-Reports/RAPPORT-ANNUEL-2007.pdf>.
- . 2009. *Annual Report 2008*, April. Accessed May 20, 2016. <http://www.moh.gov.rw/fileadmin/templates/MOH-Reports/Final-MoH-annual-report-2008.pdf>.

- Murray-Zmijewski, Alexandra, and Charles Gasana. 2010. *Monitoring and Evaluation in Rwanda Country Case Study*, October.
- NAR and Interpeace. 2016. *Governing With and For Citizens*. Never Again Rwanda, Interpeace, August.
- NISR and ORC Macro. 2006. *Rwanda Demographic and Health Survey 2005*. Calverton, Maryland, U.S.A.: National Institute of Statistics of Rwanda (NISR) and ORC Macro.
- NISR, MoH, and Macro International Inc. 2008. *Rwanda Service Provision Assessment Survey 2007*. Calverton, Maryland, U.S.A.: National Institute of Statistics of Rwanda (NISR), Ministry of Health of Rwanda (MoH), and Macro International Inc.
- Olken, Benjamin A. 2007. "Monitoring Corruption: Evidence from a Field Experiment in Indonesia." *Journal of Political Economy* 115 (2): 200–249.
- Olken, Benjamin A, Junko Onishi, and Susan Wong. 2014. "Should Aid Reward Performance? Evidence From a Field Experiment on Health and Education in Indonesia." *American Economic Journal: Applied Economics* 6 (4): 1–34.
- OSSREA. 2007. *Rapid and Extensive Assessment of Performance Management Contracts - Imihigo*. Organisation for Social Science Research in Eastern and Southern Africa, July.
- Pradhan, Menno, Daniel Suryadarma, Amanda Beatty, Maisy Wong, Arya Gaduh, Armida Alisjahbana, and Rima Prama Artha. 2014. "Improving Educational Quality through Enhancing Community Participation: Results from a Randomized Field Experiment in Indonesia." *American Economic Journal: Applied Economics* 6 (2): 105.
- Purdeková, Andrea. 2011. "'Even if I am not here, there are so many eyes': Surveillance and State Reach in Rwanda." *The Journal of Modern African Studies* 49 (3): 475–497.
- Rasul, Imran, and Daniel Rogger. 2018. "Management of Bureaucrats and Public Service Delivery: Evidence from the Nigerian Civil Service." *The Economic Journal* 128 (608): 413–446.
- Reinikka, Ritva, and Jakob Svensson. 2004. "Local Capture: Evidence from a Central Government Transfer Program in Uganda." *The Quarterly Journal of Economics* 119 (2): 679–705.
- RGB. 2014. *The Assessment of the Impact of the Home Grown Initiatives*. Special Issue 4. Kigali: Rwanda Governance Board.
- Scher, Daniel, and Christine MacAulay. 2010. "The Promise of Imihigo: Decentralized Service Delivery in Rwanda, 2006-2010." *Innovations for Successful Societies: Policy Note* 133.
- Solo, Julie. 2008. *Family Planning in Rwanda: How a Taboo Topic Became Priority Number One*. Accessed October 17, 2019. https://www.intrahealth.org/sites/ihweb/files/attachment-files/fp_in_Rwanda.pdf.

- Sommers, Marc. 2012. *Stuck: Rwandan Youth and the Struggle for Adulthood*. Athens, Georgia: University of Georgia Press.
- Thomson, Susan M. 2008. "Rwanda." In *Africa Yearbook Online*, edited by Andreas Mehler and Klaas Van Walraven, vol. 5.
- Ueberschär, Nicole. 2018. *Access to and Utilization of Health Care: Spatial Disparities of Health Center Utilization in Huye District (Rwanda)*. 1st ed. Vol. 3. Geographische Gesundheitsforschung. Düren, Germany: Shaker Verlag.
- USAID, MCHIP, and TRP. 2011. *Assessment of Rwanda's National Family Planning Policy And Its Five-Year Strategies (2005-2010)*. USAID, Maternal and Child Health Integrated Program, The Respond Project, June.
- USAID and MoH. 2002. *Report of a Qualitative Assessment of Family Planning in Rwanda*. USAID and Ministry of Health of Rwanda.
- Uwimbabazi, Penine. 2012. "An Analysis of Umuganda: the Policy and Practice of Community Work in Rwanda." PhD diss., University of KwaZulu-Natal.
- Zhang, Junsen. 2017. "The Evolution of China's One-Child Policy and its Effects on Family Outcomes." *The Journal of Economic Perspectives* 31 (1): 141–159.
- Zitzewitz, Eric. 2012. "Forensic Economics." *Journal of Economic Literature* 50 (3): 731–69.

Appendix (for online publication)

TABLE A1: SUMMARY STATISTICS OF ACQUISITIONS OF MOSQUITO BED NETS

<i>A. 2005 Data</i>	Mean (%)	Min.	Max.	Obs.
Acquisition of Mosquito Bed Nets	0.636	0	1	121,752
Acquisition from Health Center	0.263	0	1	60,876
Acquisition from Other Source	0.396	0	1	60,876
<i>B. 2007-08 Data</i>				
Acquisition of Mosquito Bed Nets	1.969	0	1	87,444
Acquisition from Health Center	1.107	0	1	43,722
Acquisition from Other Source	0.702	0	1	43,722

Notes: Based on 10,146 households in DHS 2005 data and 7,287 households in DHS 2007-08 data. Panel length of variables: 12 months (0-11) for 'Acquisition of Mosquito Bed Nets'. 6 months (0-5) for 'Acquisition from Health Center' and 'Acquisition from Other Source'.

TABLE A2: SUMMARY STATISTICS OF FAMILY PLANNING OUTCOMES

<i>A. Before (Apr. 2005 - Mar. 2006)</i>	Mean (%)	Min.	Max.	Obs.
Adoption	0.228	0	1	125,193
Discontinuation	0.065	0	1	125,193
Conception	1.601	0	1	125,193
<i>B. After (Apr. 2006 - Mar. 2008)</i>				
Adoption	0.489	0	1	268,648
Discontinuation	0.145	0	1	268,648
Conception	1.441	0	1	268,648
<i>C. Full Panel (Feb. 2005 - Jun. 2010)</i>				
Adoption	0.556	0	1	748,723
Discontinuation	0.201	0	1	748,723
Conception	1.339	0	1	748,723

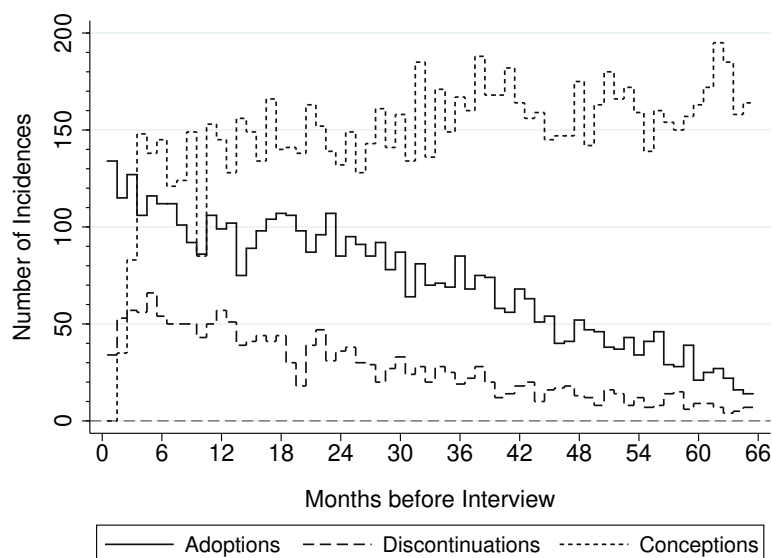
Notes: Based on 10,629 women in Before Panel, 11,669 women in After Panel and 12,963 women in Full Panel. Only women with at least two panel observations (i.e. two monthly dates at age 15-49) are included in each data set.

TABLE A3: SUMMARY STATISTICS OF RAINFALL ON SATURDAYS

Rainfall on Saturdays (Definition)	Mean	Std. dev.	Min.	Max.	Obs.
# Rainy Saturdays (Rainfall > 1 mm)	1.272	1.079	0	5	31,980
# Rainy Saturdays (Rainfall > 2 mm)	0.976	0.956	0	5	31,980
# Rainy Saturdays (Rainfall > 3 mm)	0.790	0.869	0	5	31,980
# Rainy Saturdays (Rainfall > 4 mm)	0.654	0.786	0	4	31,980
# Rainy Saturdays (Rainfall > 5 mm)	0.549	0.720	0	4	31,980
# Rainy Saturdays (Rainfall > 6 mm)	0.470	0.666	0	4	31,980
# Rainy Saturdays (Rainfall > 7 mm)	0.409	0.619	0	4	31,980
# Rainy Saturdays (Rainfall > 8 mm)	0.359	0.582	0	4	31,980
# Rainy Saturdays (Rainfall > 9 mm)	0.309	0.540	0	4	31,980
# Rainy Saturdays (Rainfall > 10 mm)	0.272	0.508	0	4	31,980

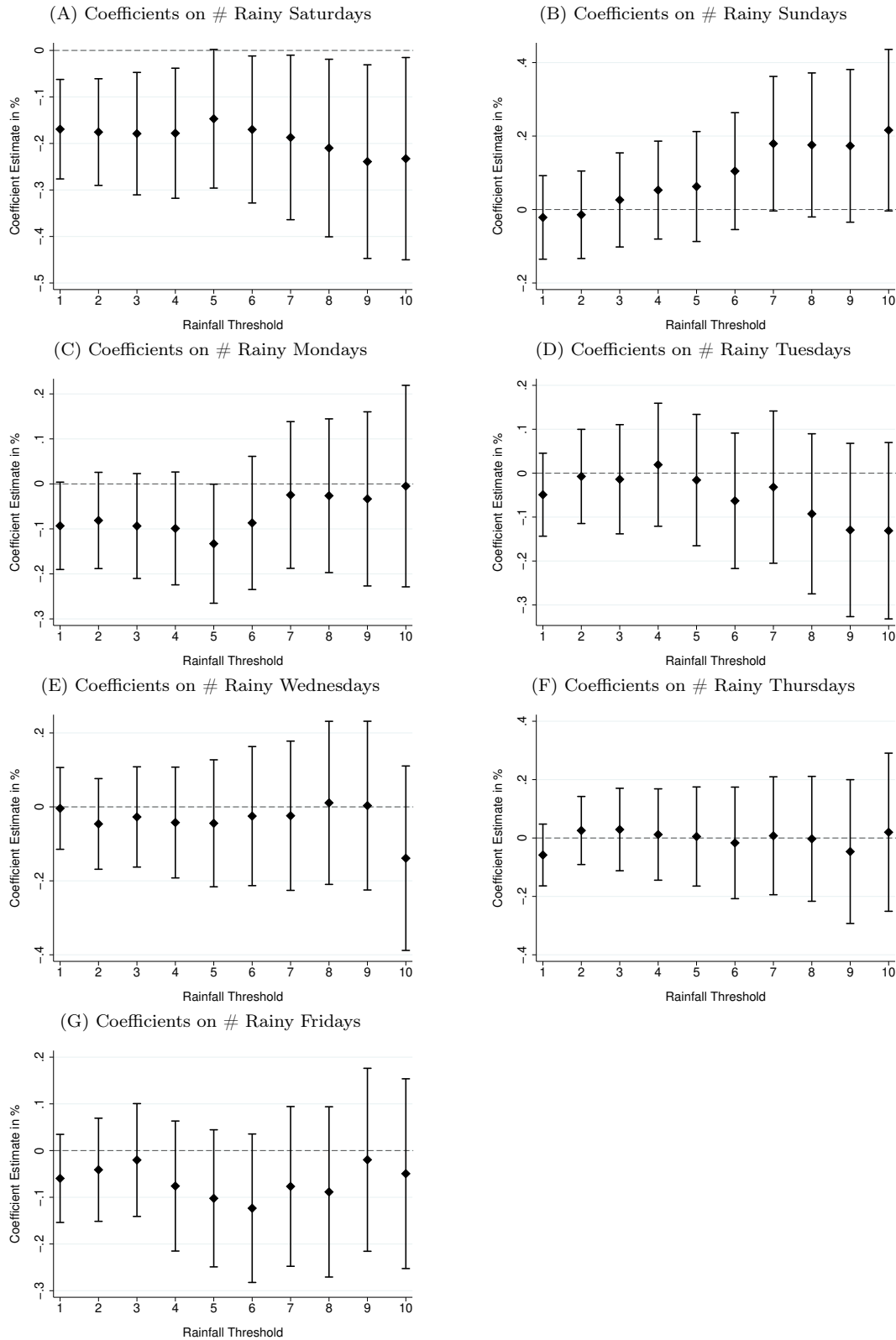
Notes: Based on 492 communities in the 2010 DHS from February 2005 until June 2010. # Rainy Saturdays counts the number of rainy Saturdays in a month. The threshold of daily rainfall which defines a rainy day is in brackets.

FIGURE B1: NUMBER OF ADOPTIONS, DISCONTINUATIONS AND CONCEPTIONS IN DIFFERENT MONTHS



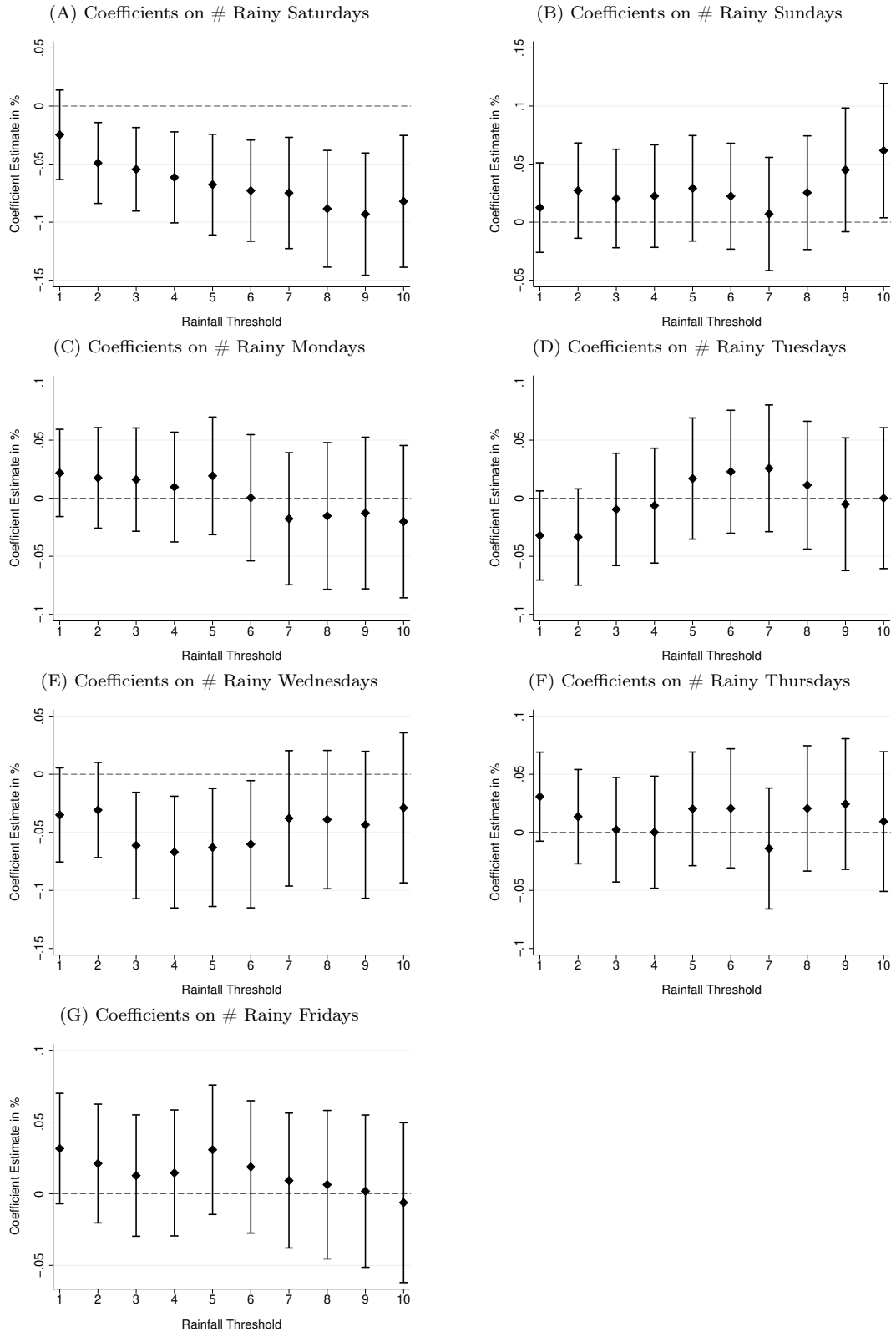
Notes: Adoptions and discontinuations of modern contraceptive methods (DHS definition). Based on 13,413 women who were between 15 and 49 years old and usual residents in the household.

FIGURE B2: ACQUISITIONS OF MOSQUITO BED NETS: DIFFERENT RAINFALL THRESHOLDS



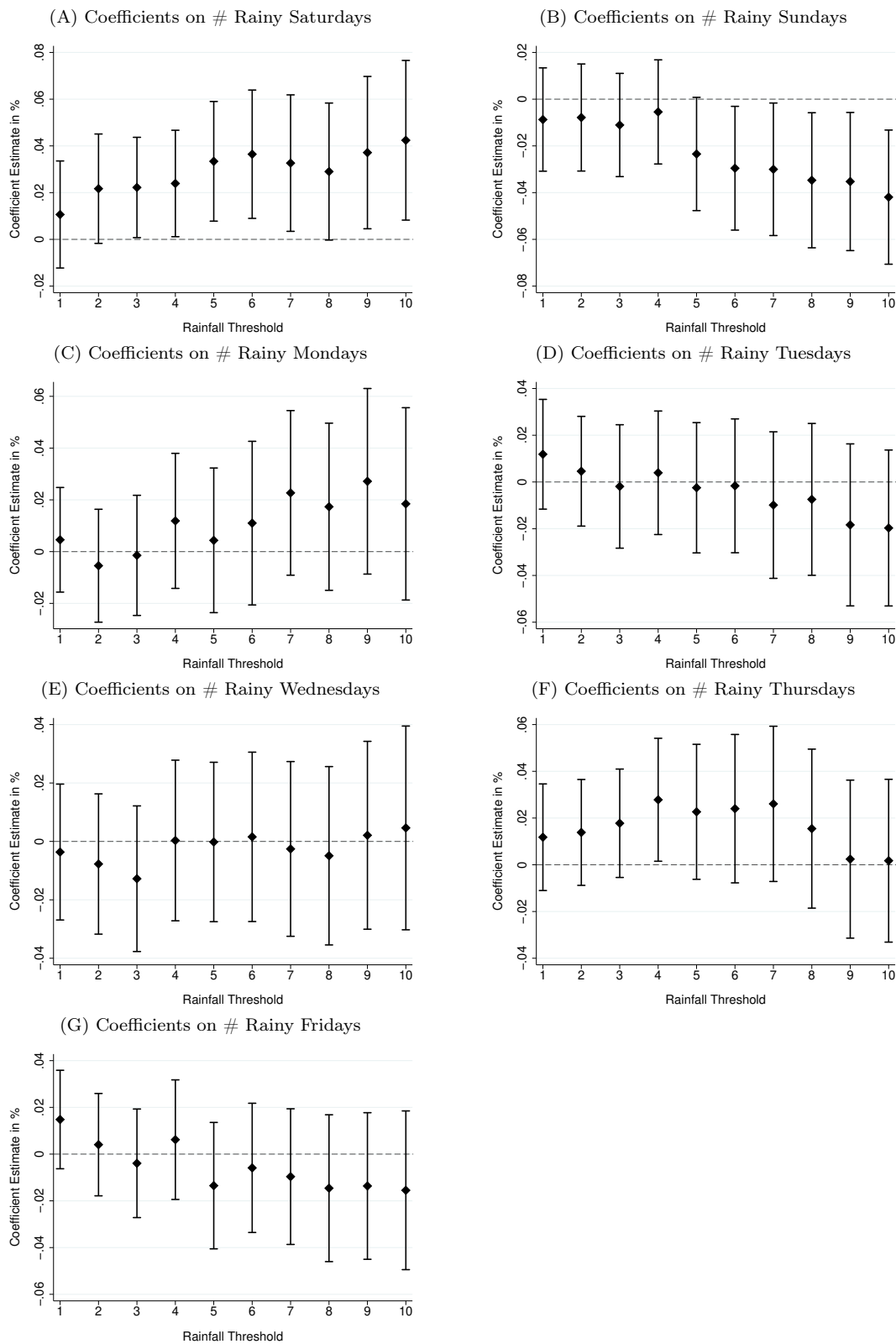
Notes: Figures show β_d coefficients (diamonds) and 95% confidence intervals based on community clustered SE (capped vertical bars) from estimations of equation 1 using panel data constructed from the 2007-08 DHS. The dependent variable is a binary indicator evaluating to 1 if a household acquired a mosquito bed net in a given month. Each diamond represents the estimate from a separate regression with # Rainy XXdays as the only rainfall regressor (i.e. no rainfall controls). The rainfall threshold which defines # Rainy XXdays is varied across the regressions in each panel. All regressions include household and months-before-interview FE.

FIGURE B3: ADOPTIONS OF MODERN CONTRACEPTIVE METHODS: DIFFERENT RAINFALL THRESHOLDS



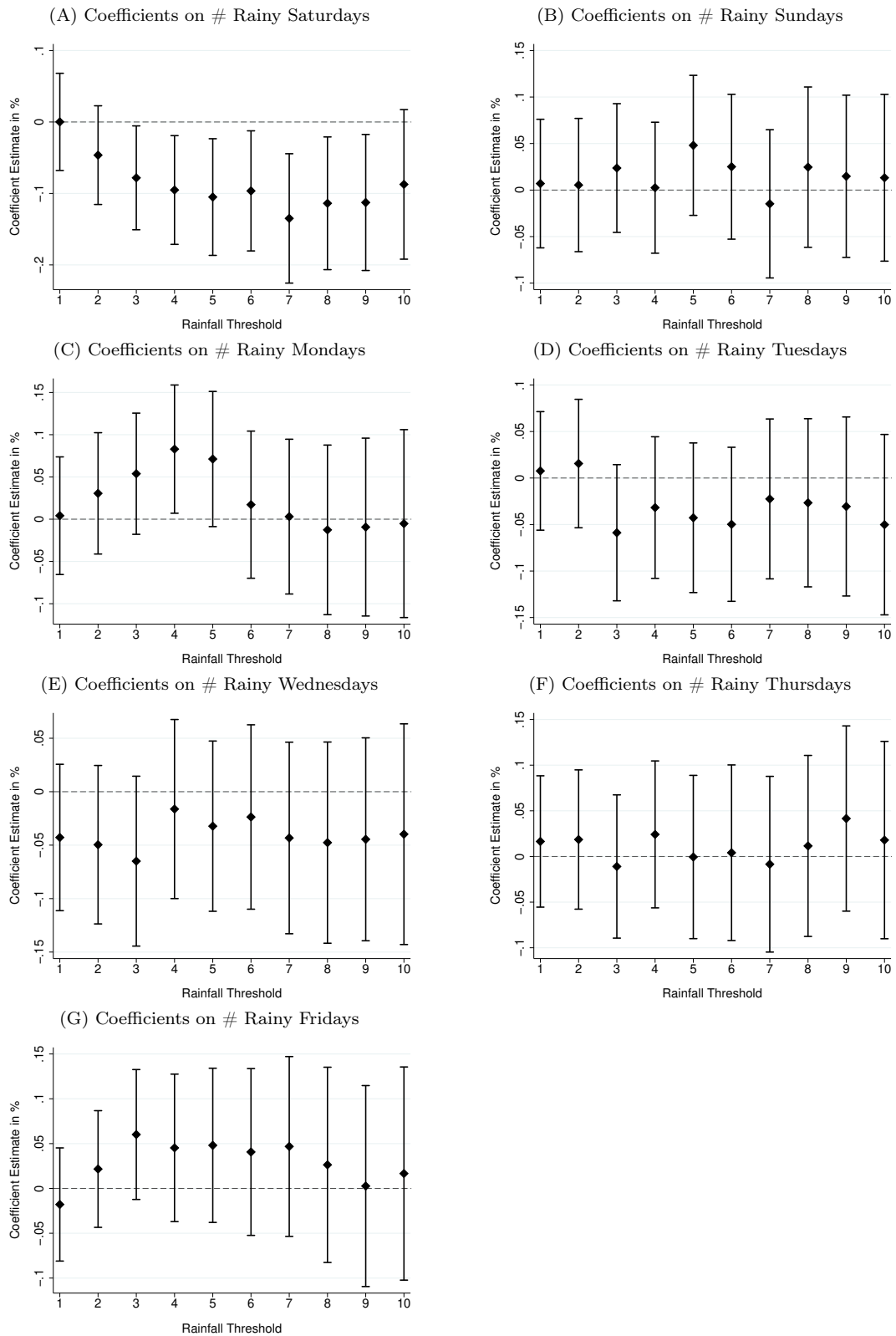
Notes: Figures show β_d coefficients (diamonds) and 95% confidence intervals based on community clustered SE (capped vertical bars) from estimations of equation 1. The dependent variable is a binary indicator evaluating to 1 if a woman adopts a modern contraceptive method in a given month. Each diamond represents the estimate from a separate regression with # Rainy XXdays as the only rainfall regressor (i.e. no rainfall controls). The rainfall threshold which defines # Rainy XXdays is varied across the regressions in each panel. All regressions include women and monthly-date FE.

FIGURE B4: DISCONTINUATIONS OF MODERN CONTRACEPTIVE METHODS: DIFFERENT RAINFALL THRESHOLDS



Notes: Figures show β_d coefficients (diamonds) and 95% confidence intervals based on community clustered SE (capped vertical bars) from estimations of equation 1. The dependent variable is a binary indicator evaluating to 1 if a woman discontinues modern contraceptive use in a given month. Each diamond represents the estimate from a separate regression with # Rainy XXdays as the only rainfall regressor (i.e. no rainfall controls). The rainfall threshold which defines # Rainy XXdays is varied across the regressions in each panel. All regressions include women and monthly-date FE.

FIGURE B5: CONCEPTIONS: DIFFERENT RAINFALL THRESHOLDS



Notes: Figures show β_d coefficients (diamonds) and 95% confidence intervals based on community clustered SE (capped vertical bars) from estimations of equation 1. The dependent variable is a binary indicator evaluating to 1 if a woman conceives (i.e. becomes pregnant) in a given month. Each diamond represents the estimate from a separate regression with # Rainy XXdays as the only rainfall regressor (i.e. no rainfall controls). The rainfall threshold which defines # Rainy XXdays is varied across the regressions in each panel. All regressions include women and monthly-date FE.