A Placebo Design to Detect Spillovers from an Education-Entertainment Experiment in Uganda*

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Abstract

Education-entertainment refers to dramatizations designed to convey information and change attitudes. Buoyed by observational studies suggesting that education-entertainment strongly influences beliefs, attitudes, and behaviors, scholars have recently assessed education-entertainment using rigorous experimental designs in field settings. Studies conducted in developing countries have repeatedly shown the effectiveness of radio and film dramatizations on outcomes ranging from health to group conflict. One important gap in the literature is estimation of social spillover effects from those exposed to the dramatizations to others in the audience members' social network. In theory, the social diffusion of media effects could greatly amplify their policy impact. The current study uses a novel placebo-controlled design that gauges both the direct effects of the treatment on audience members as well as the indirect effects of the treatment on others in their family and in the community. We implement this design in two large cluster-randomized experiments set in rural Uganda using video dramatizations on the topics of violence against women, teacher absenteeism, and abortion stigma. We find several instances of sizable and highly significant direct effects on the attitudes of audience members, but we find little evidence that these effects diffused to others in the villages where the videos were aired.

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1 Introduction

Philanthropic groups and human rights organizations routinely deploy media interventions in developing countries to promote pro-social behaviors (Blair, Littman, and Paluck, 2019), increase awareness of beneficial technologies (Heong et al., 2008; Banerjee, Barnhardt, and Duflo, 2017), correct misconceptions that contribute to the spread of disease (Abramsky et al., 2014), or discourage harmful or discriminatory behaviors (Babalola et al., 2006; UNICEF, 2005; UNFPA-UNICEF, 2014). The question is whether, and under what conditions, media campaigns on these topics change beliefs, attitudes, and behaviors.

Although the scholarly literatures on propaganda, public service announcements, and education-entertainment programs trace their origins to the 1930s, only recently have studies rigorously assessed radio or video campaigns deployed in developing countries. Inspired by early observational studies that found media dramatizations to have large effects on audience behavior (Singhal, Rogers, and Brown, 1993; Heatherton and Sargent, 2009), the past decade has seen rapid growth in randomized controlled trials evaluating media campaigns. Paluck (2009) and Paluck and Green (2009) evaluated the effects of an ethnic reconciliation radio soap opera in Rwanda by randomly assigning villages to receive recordings of the soap opera or another program on HIV prevention over the course of one year. The ethnic reconciliation program seemed to have little effect on inter-group attitudes, but its messages did affect perceived norms about inter-ethnic cooperation and listeners' proclivity to take action themselves rather than deferring to authorities. Banerjee, Barnhardt, and Duflo (2017) found that education-entertainment movies dramatizing the benefits of iron-fortified salt in Indian villages where shopkeepers were incentivized to distribute it led to an increase in product usage. Blair, Littman, and Paluck (2019) inserted scenes in a two-hour Nigerian film in which the main characters reported corruption using a text messaging hotline. The control version of the film omitted these scenes. Although both treatment and control versions of the film advertised the text messaging hotline, locations receiving the treatment version of the film became substantially more likely to receive corruption complaints to the hotline within a month of the film's distribution. By contrast, media messages not conveyed through dramatization tend to produce minimal effects. Randomly assigning radio transmitters in the Democratic Republic of Congo to air talk shows on intergroup conflict and cooperation, Paluck (2010) found these shows to have an unexpected corrosive effect on intergroup attitudes, making listeners more mindful of grievances. Galiani, Gertler, and Orsola-Vidal (2012) evaluated the effects of thirty to fifty-second encouragements to wash hands aired on randomly selected Peruvian radio stations multiple times each day for approximately one year but found no evidence of an effect on views about hand-washing or hand-washing behaviors. Sixty-second health promotion radio spots aired 6-12 times per day for months in Burkina Faso produced weak effects (Sarrassat et al., 2015). From this small assortment of studies, it appears that instructional messages and talk shows have limited effects but that dramatization may produce changes in certain attitudes and behaviors. This pattern is consistent with observational studies that have traced the consequences of the introduction of mass media and entertainment programs to regions in India (Jensen and Oster, 2009) and Brazil (La Ferrara, Chong, and Duryea, 2012).

Why might education-entertainment be an especially effective way of changing how people think and act? One hypothesis is that dramatization leads the audience to identify with the characters in the story and adopt the perspective of someone from a different background or context (Petty and Cacioppo, 1986; Bandura, 2004). Compared to an audience receiving an explicit argument or direct instructions, an audience engrossed by a dramatization of a social problem is less likely to resist the underlying message, especially when it is expressed by protagonists or conveyed by their behavior.

The influence of education-entertainment in developing countries hinges, of course, on exposure. In a country such as Uganda, where one-third of the population lives on an approximately \$2 per day (World Bank, 2016), the dearth of televisions and smartphones presents a formidable barrier to exposure to video content. On the other hand, the lack of media messages presents an opportunity, as potential audiences are often eager for entertainment, especially if it can be consumed at little or no cost. This point is especially important for video entertainment, which is beyond the means of most villagers and arguably more memorable and instructive than audio entertainment. Moreover, because these audiences have little exposure to conflicting media viewpoints, there is little risk that a message conveyed through education-entertainment will be undercut by other media sources, as often occurs in developed countries (McGuire, 1986). In our sample, only 26% of respondents own a TV and only 20% own a mobile phone with internet connectivity.

Given the challenges of exposing large segments of the population to education-entertainment, the question is whether such media campaigns have second-hand or spillover effects. If audience members absorb the media message and convey it to family members or others in their social network, the media campaign's reach expands, perhaps by a sizable factor. Outside the domain of media research, scholarly interest in spillover effects has grown markedly in recent years, and experimental designs to detect them have become increasingly sophisticated. Baird, McIntosh, and Ozler (2009) randomly varied the village-level saturation of cash transfers in Malawi to assess their effects on school attendance, and Sinclair, McConnell, and Green (2012) use a multi-level design to assess within-household and neighbor-to-neighbor spillover effects of direct mail on voting behavior. See Benjamin-Chung et al. (2018) for a recent review of applications in biomedical research and Halloran and Hudgens (2018) for a discussion of design considerations in regard to vaccine studies. We are aware of no media experiments that assess spillover effects in a design-based fashion. Most studies instead draw inferences about spillovers based on the proportion of viewers who report discussing the media messages with family or friends (e.g. Dunlop, Wakefield, and Kashima, 2008). The study most similar to ours is Alik-Lagrange and Ravallion (2019), which assesses media spillovers stemming from a randomly assigned village-level video campaign to publicize villagers' rights under the National Rural Employment Guarantee Act. Unlike our experiment, Alik-Lagrange and Ravallion's did not use a placebo control and instead identifies spillovers based on pre-post comparisons within households.

In this paper, we present a design-based strategy to assess intra-cluster diffusion of treatment effects. Our strategy allows us to assess the effectiveness of education-entertainment messaging on audiences as well as others who live in the same village. It is based on an experiment that assigns a treatment at the cluster (village) level. Importantly, we distinguish between the treatment itself (the education-entertainment videos that we aim to assess) and the way in which the treatment is delivered. In our application, we deliver education-entertainment messages during commercial breaks at film festivals that take place in local video halls in every experimental site. The film festivals attract villagers who attend Hollywood movies free of charge. Crucial to our strategy is that the treatment and the delivery mechanism can be decoupled. This allows us to assign villages to a placebo instead of a pure control condition. In the placebo condition, villagers attended festivals but were not shown videos on a given topic. We are interested in estimating treatment effects among various principal strata that are defined by the extent to which units within a cluster can be reached through the delivery mechanism. Some villagers are directly reachable, i.e., they attend the film festival. Others are indirectly reachable, i.e., they have friends and family who attend the film festival. Our key identifying assumption is that reachability is unaffected by treatment – whether or not villagers or their friends attend the film festival is unaffected by whether (or which) education-entertainment messages are screened during commercial breaks. Under this assumption, the fact that the delivery mechanism is present in all clusters allows us to measure the otherwise latent reachability strata in all treatment conditions, as we know who did and did not attend each village's film festival. As a consequence, we can estimate principal strata specific effects (e.g., the effect of indirect treatment among those who can be reached indirectly) through a simple sub-setting approach.

This design can be applied beyond the study of the within-cluster diffusion of media effects. The key design feature is a placebo condition in which the delivery mechanism is present but the treatment is not. For example, suppose all villages in a study have a health clinic. In a randomly selected subset of villages, a de-worming treatment is handed out to all health clinic patients. As long as patients visit health clinics for reasons unrelated to the de-worming pill, this design allows for the identification of the intra-village diffusion of health benefits. Similarly, all communities in a study may hold community workshops. In a random subset, information pamphlets are handed out during the workshop. As long as workshop participation is unaffected by whether pamphlets are handed out, the design enables the study of the indirect effects of leaflets on participants' family members.

Because "reachability" is akin to compliance (Angrist, Imbens, and Rubin, 1996), our paper relates to work that deals with the problem of unit-level non-compliance in cluster-randomized experiments. One approach is to build a model of compliance using pre-treatment covariates (Frangakis, Rubin, and Zhou, 2002; Jo et al., 2008). A design-based alternative is to rely on a placebo condition to make latent compliance strata manifest. While the experimental literature using placebo controls is vast, few field experiments to date have used placebo arms for this purpose. The most prominent example in the media literature is Paluck's (2009) study of Rwanda, which brought together villagers to listen to either of two soap operas, one dealing with ethnic reconciliation or another focusing on HIV. This design has proven useful in a wide variety of settings. In the context of internet advertising, designs using ghost ads serve this function by delivering treatment or placebo ads to everyone who visits a given web page (Johnson, Lewis, and Nubbemeyer, 2017). Other examples include attempts to persuade people about social issues at their doorsteps (Kalla and Broockman, 2018) or mobilize them to vote via phone calls (Gerber et al., 2010). To our knowledge, ours is the first media experiment to apply a placebo design to geographic clusters in order to identify the effects of indirect exposure among viewers' relatives and friends.

The paper is structured as follows. We begin by providing an overview of our experimental design in general terms. Next, we describe how the design is specifically applied in two rounds of experiments we conducted in different regions of Uganda, initially in 56 rural villages in 2015 and then in 112 rural villages in 2016. The subsequent two sections present our main results and an analysis of gender-specific heterogeneous effects.

We find substantial and highly significant direct effects in each of the three issue domains, teacher absenteeism, abortion stigma, and violence against women (VAW). Among those outcomes that reveal sizable direct effects, however, we do not find statistically significant evidence of diffusion to others in the community, despite the fact that our design is well-powered to detect spillovers. This conclusion holds up even when we partition respondents by gender, as specified in our pre-analysis plan. Although direct effects often vary for male and female audience members, and although friendship networks would seem to facilitate within-gender communication, analyzing the results separately for men and women reveals at most equivocal evidence of spillovers. That is the case even though the context of our study seems particularly conducive to spillovers. We target an audience that has little access to locally produced media content of high production value, increasing the likelihood that audience members will engage with our video material and share their viewing experience with others. Our sample also consists of rural communities that tend to be close-knit, which should facilitate the flow of information through social networks. We conclude that education-entertainment's effects on norms and attitudes may stem primarily from direct exposure rather than diffusion.

2 A Design To Estimate Intra-Cluster Diffusion of Treatment Effects

Setting. Consider a set of clusters $j = 1, ..., N$ where the jth cluster comprises units $i = 1, ..., M_j$. We focus on a treatment that is assigned on the cluster-level and a "delivery mechanism" that is used to deliver a treatment to units in a given cluster. Let us denote by $\mathbf{z}$ a vector of treatment assignments for the finite population of units in the experimental subject pool, where $z_{ij} = 1$ if unit i is in a cluster j that has been assigned to the treatment group, and $z_{ij} = 0$ otherwise. Similarly, denote by $\mathbf{d}$ a vector of indicators for a specific delivery mechanism, where $d_{ij} = 1$ if unit i is in a cluster j where a specific delivery mechanism is used, and $d_{ij} = 0$ otherwise.

In our case, the treatment consists of education-entertainment messages, and the treatment delivery mechanism is a film festival that screens Hollywood movies free of charge in village clusters. Those who attend the film festival would be exposed to the education-entertainment messages were these messages screened during commercial breaks in their village. The delivery mechanism reaches these villagers directly. Other villagers will not attend the film festival, but their friends and family will attend. These villagers may be reached indirectly by the delivery mechanism through peer networks. Those who do not attend the film festival and do not have friends or family who attend cannot be reached at all. Let us denote by s_{ij} the "reachability" of individual

i in village j. That is, the extent to which the individual can be reached by a given treatment delivery mechanism, which, in turn, determines the degree to which the individual *would be* exposed to a treatment *were* the delivery mechanism used to distribute this treatment in their cluster. For concreteness, assume $s_{ij} \in \{\text{directly reachable}, \text{indirectly reachable}, \text{not reachable}\}$.

Diffusion Estimands. The same treatment could be delivered through different mechanisms. For example, education-entertainment messages could be screened in local barbershops instead of through film festivals. How the effects of a given treatment might diffuse throughout a cluster may depend on the delivery mechanism. Imagine, for example, that film festival attendees tend to have a wider circle of friends than do barbershop customers. In that case, we would expect wider diffusion of messaging effects as a result of film festivals.

For a given delivery mechanism, we are interested in how treatment effects diffuse from those who can be reached directly through this delivery mechanism to those who can only be reached indirectly. For example, we would like to know if those whose friends and family were exposed to the treatment messages change their attitudes and beliefs as a result of this indirect exposure. Denoting the outcome of the ith unit in the jth cluster Y_{ij}, we assume that reachability is a fixed, pre-treatment attribute and define three mutually exclusive and exhaustive stratum-specific estimands:

$$E\left[Y_i(z_{ij} = 1) - Y_i(z_{ij} = 0) \mid s_{ij} = \text{directly reachable}\right], \tag{1}$$

$$E\left[Y_i(z_{ij} = 1) - Y_i(z_{ij} = 0) \mid s_{ij} = \text{indirectly reachable}\right], \tag{2}$$

$$E\left[Y_i(z_{ij} = 1) - Y_i(z_{ij} = 0) \mid s_{ij} = \text{not reachable}\right]. \tag{3}$$

Expression (1) is the average effect of the treatment on those directly exposed to it, among those who can be directly reached through a delivery mechanism; (2) is the average effect of the treatment on those indirectly exposed to it, among those who can be only indirectly reached through a delivery mechanism; and (3) is the average effect of the treatment on those indirectly or not exposed to the treatment, among those who cannot be reached through a delivery mechanism. Typically, one would expect treatment effects to be strongest among those who are directly reachable and to spill over to those who are indirectly reachable. In most applications, one may not expect any treatment effects among those who cannot be reached at all. Nonetheless, we consider (3) an interesting estimand. First, treatments that produce large spillover effects among those who can be reached indirectly may diffuse more widely, even beyond the group with network ties to those who can be directly reached. Second, researchers may not focus on the relevant network ties in their definition of reachability. Say, for example, researchers focus on friends and family, but diffusion happens among colleagues. In this case, we may observe effects among those defined as "not reachable" rather than among those defined as "indirectly reachable." Finding effects close to zero among those defined as not at all reachable thus lends credence to the researcher's definition and measurement of reachability. Finally, effects close to zero among those who are unreachable can also serve as evidence against the idea that effects observed among other strata are due to the idiosyncrasies of the village clusters that happened to be assigned to treatment or to treatment-specific Hawthorne effects.

Identification Problem. In most experimental applications, the treatment and its delivery mechanism cannot be decoupled: $z_{ij} = d_{ij}$ for all individuals i in all clusters j. As a result, one cannot observe s_{ij} for units in the control group. Because reachability may be correlated with potential outcomes, it is not generally true that $E[Y_i(z_{ij} = z) \mid s_{ij} = s] = E[Y_i(z_{ij} = z)]$, and therefore one cannot identify estimands 1 – 3 by naively comparing those who were directly or indirectly reached in the treatment group to the control group as a whole.

Key Identification Assumption: Revelation of Strata Through Placebo. Frangakis and Rubin (2002) present a general approach to estimating strata-specific causal effects when stratum membership is observed asymmetrically across treatment arms. This approach has been applied to estimating the Complier Average Causal Effect (CACE) in settings with imperfect treatment compliance (e.g. Angrist, Imbens, and Rubin, 1996; Imbens and Rubin, 1997; Balke and Pearl, 1997; Cheng and Small, 2006; Cuzick et al., 2007) and to the estimation of the 'Survivor Average Causal Effect' (SACE) in settings with asymmetric missingness of outcome data (Robins, 1986; Zhang and Rubin, 2003; Rubin et al., 2006; Frangakis et al., 2007; Imai, 2008; Egleston, Scharfstein, and MacKenzie, 2009; Chiba and VanderWeele, 2011). Approaches to consistent estimation of principal causal effects under partially observed strata often rely on instrumental variables estimators. Unlike these approaches, ours attempts to render strata fully observable by decoupling the delivery mechanism from the treatment itself. If the delivery mechanism reveals each unit's fixed stratum membership, then the identification and estimation of principal causal effects can be treated simply as a heterogeneous effects analysis.

The basic principle operates as follows. Film festivals attract viewers for reasons that are unrelated to the short education-entertainment videos that are aired during intermissions. In the treatment group, the film festival serves to deliver the messaging treatment during commercial breaks. In the "placebo" group, no education-entertainment messages are screened (or an education-entertainment message is screened on an irrelevant topic). The key advantage of the placebo is that it makes it possible to observe who attends the film screenings. Under the assumption that reachability is unaffected by treatment assignment, this feature allows for the identification of the above-defined stratum-specific effects of varying exposures to treatment. Formally, we require $s_{ij}(z, d) = s_{ij}(d)$ for all individuals i in all clusters j. In other words, the extent to which individual i in cluster j can be reached by the film festival is assumed to be a fixed personal attribute that is unaffected by whether or which video messages are aired during intermission breaks. If this assumption holds, s_{ij} is made observable in the control through the placebo, and estimands $1 - 3$ can be estimated by subsetting to a given stratum s and using standard estimators to estimate the average treatment effect for this stratum. For example, we can compare those indirectly reached in treatment locations to those who have been indirectly reached in locations that did not air the treatment in order to assess the effect of second-hand exposure to the treatment through social networks. Note that the advantages of the placebo in this design thus go beyond the typical benefit of being able to isolate the effect of the treatment from effects of the delivery mechanism. The key function of the placebo in this design is that it reveals the otherwise latent reachability strata in the control group.

Strength of Key Identification Assumption. Whether the identifying assumption is likely to hold depends on the nature of the delivery mechanism and the way it is presented to individuals. For example, if the advertisement of film festivals in the treatment group mentions the education-entertainment messages, the treatment status would very likely affect reachability and the identifying assumption will not hold. Advertisements for the film festivals in our study made no mention of the treatment messages.

A second feature that may determine the plausibility of the identifying assumption is the way in which researchers measure the reachability strata. For example, suppose we were to identify those who are indirectly reachable by asking film festival attendees whom they spoke to about the festival. The identifying assumptions would be jeopardized if the screening of education-entertainment messages affects whom film festival attendees spoke to. Here, we define those that are indirectly reachable merely as those who report that they have friends and family who attended the film festival, irrespective of any conversations that individuals may have had about the film festival. As we show below, the assumption has several testable implications that aid in the assessment of its plausibility.

Stable unit treatment value assumption. Our design is able to detect intracluster spillovers in the sense that it can assess diffusion of treatment effects to those indirectly exposed to the treatment. The design invokes what is sometimes referred to as the Stable Unit Treatment Value (SUTVA) assumption at the cluster level. Specifically, we assume that each unit's potential outcomes depend only on the treatment status of its cluster, and not on those of any other cluster: $Y_i(\mathbf{z}) = Y_i(z_{ij})$. This assumption leads to the definition of 'stable' potential outcomes, in the sense that every unit has exactly as many potential outcomes as there are experimental conditions. The approach can be contrasted with those that allow each unit's potential outcomes to depend on the assignment status of 'neighboring' units, and which model indirect exposure as a function of group saturation (Baird, McIntosh, and Ozler, 2009) or unit proximity (Aronow, 2012; Aronow, Samii et al., 2017). A growing number of experiments have used these latter approaches to assess spillover effects between peers (Aronow, Samii et al., 2017), spouses (Fletcher and Marksteiner, 2017), siblings (Barrera-Osorio et al., 2011), and neighbors (Perez-Truglia and Cruces, 2017). Such studies rarely use a principal strata approach. One of the few exceptions is Nickerson (2008), which uses a placebo design to assess door-to-door canvassers' effects on voting among the housemates of those who speak with canvassers directly.

No crossover effects assumption in multi-treatment design. The design presented above can be extended beyond two-arm contexts to multi-arm experiments. Suppose there are multiple binary media treatments that are all delivered through the same delivery mechanism. Different media treatments may then serve as placebo conditions for each other. One advantage of this design is its ability to identify media-generated treatment effects across a variety of substantive domains. That said, depending on the specific experimental design, additional assumptions may be needed about whether and how outcomes pertaining to one treatment are affected by other treatments (see section 3.8).

3 A Two-Part Mass Media Study In Uganda

We implement the same basic design presented above in two experiments that took place in the context of film festivals that we organized in Central Uganda in 2015 and 2016. The first film festival took place in fifty-six villages over four consecutive weekends from November to December of 2015. The second film festival took place in 112 different villages and comprised six films shown one per week over consecutive weekends, from July 30 to September 4, 2016.

3.1 Intervention

The Hollywood films and festival constitute our "delivery mechanism." Our treatment consists of three sets of three short video vignettes, each between three and eight minutes long, that we interspersed in the Hollywood films. In both studies, every village received exactly the same film festival program, featuring popular American movies that were narrated by a well-known

Luganda-speaking celebrity (see section A.2 of the online appendix). For example, in round one, *Slumdog Millionaire* was aired in all villages on the same weekend, irrespective of the treatment arm to which the village was assigned. We screened the films free of charge, advertising the film festivals throughout the village using posters, flyers and, if available, public loudspeakers. The films were thematically unrelated to the treatment.

The three sets of treatment videos each focus on a different social issue facing rural Uganda: teacher absenteeism, stigma against those who seek abortions, and violence against women. Pre-testing of the survey indicated that rural Ugandans' attitudes on these three topics are virtually uncorrelated, and so we did not expect videos on one topic to influence opinions about a different topic. We presented the vignettes to audiences by inserting them into intermissions in the Hollywood films.

In contrast to the films, our videos were written by Ugandans, the dialogue was in Luganda (the main language spoken in the Central Region of Uganda), and used Ugandan actors in a village setting so as to make it easy for viewers to identify with the characters in the videos. While an overarching narrative runs through the three vignettes for a given topic area, each vignette can also be understood as a self-contained story in isolation from the other two. We describe the narratives in more detail below. The videos on teacher absenteeism and abortion stigma were identical across the two festivals. Prior to the launch of the second study, the videos on violence against women were re-written and re-shot after round 1 found them to be ineffective. Since we focus here on whether effects on those directly reached spill over to those in their social network, we here focus on the round 2 videos on violence against women, which produced significant treatment effects among viewers. All videos can be viewed at this address: http://tiny.cc/uganda_media.

Figures 1 and 2 give an overview of the study timelines, including the data collection strategy that we describe in more detail below.

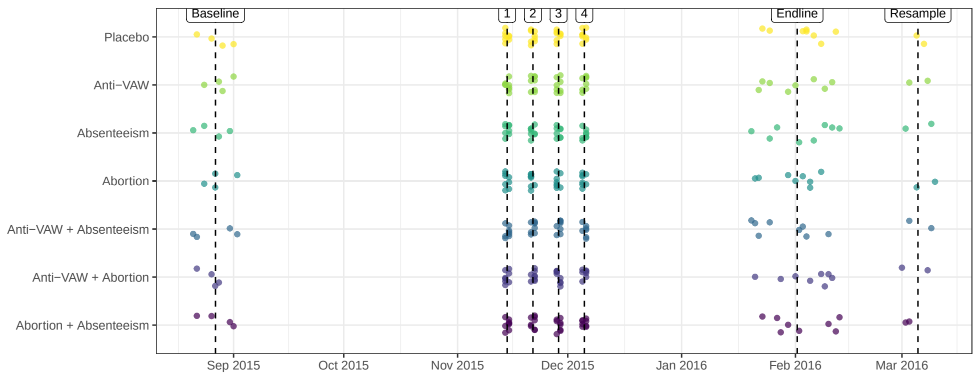

Figure 1: Timeline of round 1 media campaign and surveys.
Points represent unique visits to villages, either to screen films or to collect data. Colors and the Y axis represent the different treatment conditions. The X axis is ordered by date.

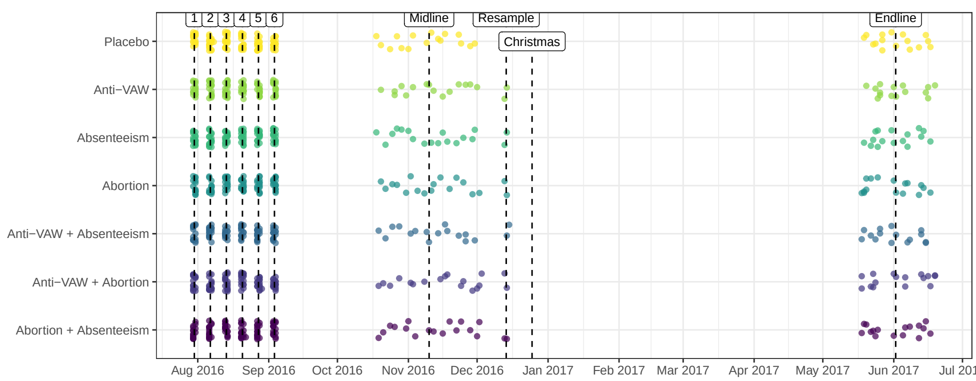

Figure 2: Timeline of round 2 media campaign and surveys.
Points represent unique visits to villages, either to screen films or to collect data. Colors and the Y axis represent the different treatment conditions, the X axis is ordered by date.

For each issue area, our video messages implicitly and explicitly express a prescriptive social norm – that is, beliefs, statements or unwritten rules about appropriate behavior in a given situation (Shaffer, 1983). For example, the videos emphasize the importance of speaking out against domestic violence by reporting cases to friends and family or village-level authorities. For abortion, the vignettes convey the obligation to help those who suffer from complications resulting from an abortion, irrespective of one's personal views about it. Finally, for teacher absenteeism, the videos convey the message that parents have a responsibility to take action in order to resolve the problem of teacher absenteeism. Additionally, the vignettes on all three topics convey the norm implicitly by modeling behavior in accordance with it. Each of the video vignettes ends with a narrator expressing the norm in a statement such as, "if you see domestic violence in your community, intervene before it's too late," "do not judge, and you will not be judged," or "we have a part to play in our children's education." A detailed description of the vignettes can be found in section A.3 of the online appendix.

Given their closeness to the audiences' context and experience, our video dramatizations seem well suited to make viewers identify with the main characters. It is rare for media with very high production value to be filmed in the local language (Luganda) using rural Ugandan villages as a setting. The videos depict situations that would be familiar to the participants in our study. And indeed the relevance of the films was apparent in a separate survey experiment we conducted wherein respondents were directly exposed to our video material on hand-held tablets prior to answering survey questions. For example, after viewing the videos on domestic violence, the vast majority (84%) of respondents said that the stories could have happened in their village. That viewers found the stories relevant to their own lives is also reflected in what they said when invited to comment on the videos, for instance: "The video is so real" or "What I have seen in the video can also happen in my home."

3.2 Site selection

In order to minimize the risk that subjects would be exposed to media messages other than the ones to which they were randomly assigned, we began by selecting villages that would be at least four kilometers apart from one another. Given the limited road network separating villages, this walking distance effectively prevents people from attending media presentations in other villages. A full description of the site selection procedure may be found in section A.4 of the online appendix.

3.3 Random assignment

We randomized at the level of the village. This method of clustered assignment potentially sacrifices statistical power because all members of a given community are assigned together to the same experimental condition. By allocating treatments over a large number of clusters we are able to estimate the clustered standard errors in a reliable manner (Imbens and Kolesar, 2016). We formed blocks of villages using an algorithm that minimized the within-block between-cluster variability in latitude and longitude, which improves power to the extent that outcomes are spatially autocorrelated. In the end, the relatively small degree of intra-cluster correlation meant that clustered assignment resulted in only a small loss of precision.

Within each block, we randomly assigned villages to one of seven treatment conditions. Three of these conditions involved the screening of video vignettes on only one of the three topics, i.e., vignettes on domestic violence only, vignettes on abortion only, or vignettes on teacher absenteeism only. Another three conditions involved the screening of vignettes on two out of the three topics, i.e., vignettes on domestic violence and abortion, vignettes on domestic violence and absenteeism, and vignettes on abortion and absenteeism. The final condition is a pure placebo condition in which the film festival consisted only of the screening of Hollywood movies without any vignettes. One advantage of including a pure placebo group is that its respondents enable us to establish that opinions toward our three focal issues are very weakly correlated, which lends credence to the idea that our design offers three distinct tests in different attitude domains. Note that our design does not include a treatment condition that involves the screening of videos on all three topics. Such a condition would have required the screening of nine video vignettes throughout a single Hollywood movie, which was deemed too disruptive to the viewing experience to be viable in practice. One consequence of this choice is that our main analysis relies on the assumption that there are no crossover effects (effects of one treatment on the outcomes related to another treatment, e.g. an effect of the teacher absenteeism treatment on outcomes regarding violence against women). In section E.1 of the online appendix, we conduct an analysis that relaxes this assumption and illustrates that our conclusions about direct effects or spillovers do not rely on it.

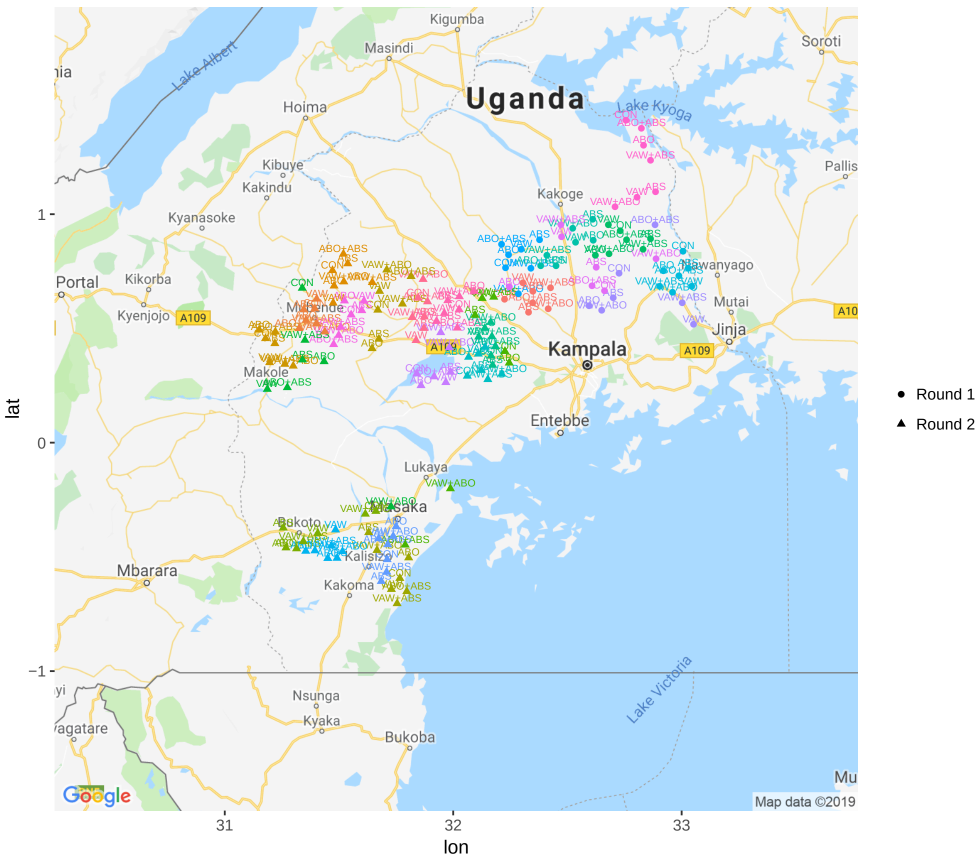

Figure 3: Clusters Included in the Study.

The map on figure 3 shows the location of the clusters in Round 1 and Round 2. The colors indicate the block, while labels indicate each of the seven treatment conditions: placebo (no vignettes), vignettes on abortion only, vignettes on domestic violence only, vignettes on teacher absenteeism only, vignettes on abortion and domestic violence, vignettes on abortion and absenteeism, and vignettes on domestic violence and absenteeism.

In Round 1, all clusters complied with the treatment assignment insofar as we were able to correctly screen the assigned films and messages in each village. In Round 2, two villages aired five of the six scheduled screenings. In one case, this was due to the video hall owner suspecting the feature-length film of spreading black magic; in another case, a local leader sought to prevent the screening apparently in an effort to extract a gratuity. In neither case do we have reason to suspect that this was due to the experimental vignette featured in the film. Because the extent of cluster-level non-compliance is so minimal, we make no statistical correction for it.

3.4 Sampling

In both rounds, we conducted endline surveys that began roughly two months after the end of the film festivals. The surveys were seemingly unrelated to the film festivals – they covered a range of topics, did not mention the films in the consent form, and survey staff were not present at screenings.

Moreover, in contrast to many media evaluations, we did not restrict our attention to those who viewed our media messages. As described briefly below and in detail in section A.5 of the online appendix, we enumerated all households living within a pre-determined radius of the video hall and drew a random sample of the residents. The resulting respondent pool therefore comprises viewers and non-viewers, which not only facilitates the study of spillovers, it has the further advantage of obscuring the connection between the media intervention and the survey.

In round 1, we also implemented a baseline survey in half the sample, using a Solomon four-group approach. To maintain comparability between rounds 1 and 2, we only make use of measurements from the endline surveys here.

We sampled individual respondents from a circular area around the video hall that was used to screen the treatment messages. Enumerators received a map for each village that depicted a circle around this video hall with a radius of between 200 and 800 meters. The radius was chosen based on the population density of the given village as judged from satellite images. Enumerators worked with village leaders (LC1 chairpersons) or other knowledgeable members of the community to produce a list of all households that reside within the circle indicated on the map. From this list, forty households were randomly selected in round 1. In round 2, we randomly selected fifty households from the list. Among the selected households, twenty (round 1) or twenty-five (round 2) were randomly chosen as households within which a female respondent would be interviewed by a female interviewer; in the remaining households, men were interviewed by male enumerators.

In section A.5 of the online appendix, we describe how we resampled young men in areas where we found few viewers. We also describe the decision to decrease the age range of our sample frame from 18-65 in round 1 to 18-50 in round 2. In the tables reported in this paper, we subset to the age group that had a positive probability of being randomly sampled in either round (18-50) and weight individuals by the inverse of their sample probability to correct for the over-sampling of likely viewers. We provide unweighted results using the full sample in section C of the online appendix. The use of weights and subsetting leaves our results essentially unchanged.

In total, we successfully conducted the round 1 endline surveys with 2,431 of the 2,450 targeted respondents, and round 2 endline surveys with 5,534 of the targeted 5,740 respondents. Taking into account the cluster-level attrition in round 2, our response rates for the two studies are 99% and 96%, respectively. We find no convincing evidence of differential attrition across experimental conditions and the cluster-level correlation between attrition and reachability rates is low ($\rho = .06$ and $\rho = .12$ for those reached directly and indirectly, respectively).

3.5 Outcomes

In most of our analyses, we pool survey data from rounds 1 and 2, treating the two rounds as one experiment. Analyses of outcomes related to violence against women draw on round 2 data only, since, as discussed above, the treatment videos screened in round 2 were different from those used in round 1.

In order to work with more reliable outcome measures, we construct multi-item indices where possible. Table 11 in the online appendix gives the wording and response distribution for each question that was used to construct a given index. The first index reflects what might be termed "conative attitudes" (Fishbein and Ajzen, 1975), or action orientations concerning teacher absenteeism. The common thread that runs through this first set of questions is whether to address the problem of teacher absenteeism through collective action, as opposed to a passive approach, such as waiting for the situation to improve on its own. The Cronbach's alpha associated with this four-item index is 0.24; this relatively low value is not a source of bias but does make it especially challenging for us to detect treatment effects given the apparent signal to noise ratio. The second outcome measure is a single item capturing the relative importance of educational goals such as "Reducing the number of bad teachers at school." A third outcome measure focuses on respondents' conative attitudes toward helping a woman who has been beaten by her husband. Each respondent is presented with a series of paired options, one of which involves action (e.g., "I would accompany her to the police") while the other involves some kind of consolation that does not culminate in the involvement of authorities (e.g., "I would calm her down and tell her that the situation is bound to get better"). The Cronbach's alpha associated with this four-item index is 0.4. Finally, we measure an outcome related to our abortion stigma messaging, intended to capture respondents' willingness to offer support to those facing ostracism.

3.6 Pre-registered analyses

In sum, our aim was to create and deploy dramatizations that would change audiences' opinions on the three topics. Our pre-analysis plan (PAP) specified that, in the event that we found significant evidence of opinion change among those who reported directly attending the film festival, we would investigate spillover effects among others in the community who did not attend (PAP p.17). It also specified that we would look not only at effects for the sample as a whole but also broken down by respondent gender (PAP p.18).

In order to facilitate presentation and interpretation of the results, we depart from our pre-analysis plan in three ways. (As can be seen in section D of the online appendix, where we report the results in the manner pre-specified, these departures do not change the substantive interpretation of our findings.) First, we here present our results subsetting to the age group common to both rounds (18-50) and reweighting units to account for the overrepresentation of young men in follow-up samples that tried to increase surveys among festival attendees in clusters with few attendees. Our PAP did not specify any reweighting procedure. Section C.2 of the online appendix provides a description of the reweighting procedure and reports the main results in the paper without weighting or subsetting. Second, as described in section D.1 of the online appendix, we collapse over two of the original strata specified in our PAP and use different labels to refer to them. Specifically, what were described as "compliers" in our PAP are here referred to as those "directly reached"; "indirect compliers" are "indirectly reached"; "apprised never-takers" and "never-takers" are labeled "not reached." We also refer to the broad group comprised of "indirect compliers," "apprised never-takers," and "never-takers" as those "not directly reached." Third, our pre-analysis plan specified several one-tailed tests. However, the choice between one-tailed and two-tailed tests turns out to be inconsequential; thus, we simply report two-tailed hypothesis tests.

3.7 Defining and measuring reachability

We seek to estimate the stratum-specific causal effects defined in equations 1 – 3. More specifically, for those outcomes that illustrate a large and statistically significant effect among viewers, we wish to estimate the effect of the videos among those who *would be* indirectly exposed to them or not exposed to them at all, were the videos screened as part of the festival in their village.

Understanding which reachability stratum respondents belong to requires gathering information on their attendance of the film festival. In order to avoid priming respondents to think about the videos when answering our outcome measures, questions about attendance were asked at the very end of the endline surveys. We classify respondents according to their answers to the attendance question. We define respondents who report that they have attended at least one film (and were therefore exposed to the assigned video messages at least once) as "directly" reached. Those who did not attend the film festivals but report that their family members or friends attended at least one screening are defined as "indirectly reached." The final group ("not reached") comprises those who did not attend the films and who do not have any friends and family who attended any screenings. Finally, in the regression tables we also report the most precise estimate of spillovers we can, by pooling the responses of all those "Not reached directly." Table 1 summarizes the strata.

Reachability Stratum	**Recently, a series of six free films (Fast and Furious, [etc.]) were screened in the kibanda in your trading center. Have you heard about the screenings and if so, how many screenings did you attend?**	**Did your friends or family attend any of the screenings?**
s_i = Directly reached	1,2,3,4,5, or 6 screenings	[Anything]
s_i = Indirectly reached	0 screenings Don't know Refuse to answer	Yes, friends and family Yes, friends Yes, family
s_i = Not reached	0 screenings Don't know Refuse to answer	No Don't know Refuse to answer

Table 1: Definition of Reachability Strata Revealed by Delivery Mechanism.

Aggregating both the initial and follow-up samples across the two studies, 1,492 (19 percent) of 7,965 subjects were directly reached. Another 3,285 (41 percent) were indirectly reached. Finally, 3,188 (40 percent) did not attend and did not have friends or family members in attendance – they were not reached, directly or indirectly. These proportions give a sense of how large spillover effects would have to be in order to have the same overall impact as direct effects on viewers.

Table 2 provides some descriptive characteristics on the initial sample. A quarter of all men attended at least one film – twice the rate of attendance by women. Film attendees are thus more likely to be men and are younger than those who did not attend. Unsurprisingly, attendees are less likely to own a television. However, they do not appear to have a markedly different standard of living: they live in dwellings with a similar number of rooms as those reached indirectly and not reached, own radios at similar rates, have similar years of schooling, and exhibit only slightly greater rates of literacy.

Gender	Men			Women		
Stratum	Reached Directly	Reached Indirectly	Not Reached	Reached Directly	Reached Indirectly	Not Reached
N	952 (25%)	1617 (43%)	1201 (32%)	467 (12%)	1527 (40%)	1801 (47%)
Age	29	33	37	29	31	32
Own TV	21%	26%	32%	15%	23%	29%
Own Radio	88%	87%	86%	72%	76%	74%
Rooms in house	3	3	3	2	3	3
Highest grade	7.3	7.1	7.2	6.3	6.7	6.6
Illiteracy	8%	10%	12%	15%	13%	18%

Table 2: Characteristics of Initial Sample (Prior to Resampling).

3.8 Testing identification assumptions

Recall that our design requires reachability to be unaffected by treatment, i.e., $s_{ij}(z, d) = s_{ij}(d)$ for all individuals i in all clusters j. One testable implication of this assumption is that the probability that a person falls into a given reachability stratum does not vary by treatment. We test and validate this stipulation in section B.2 of the online appendix. A further implication, corroborated in section B.1 of the online appendix, is that covariates are balanced across treatment conditions among respondents in a given reachability stratum.

As our design involves multiple treatment-placebo combinations (but no condition in which a village receives all three types of treatments), we also require the assumption that there are no crossover effects of, for example, messages on teacher absenteeism to abortion-related outcomes. In section E.1 of the online appendix, we explain this aspect of our design and show that our main results are robust to using an estimator that relaxes the no crossover effects assumption.

3.9 Estimation

Given our identifying assumptions, we can estimate the estimands defined in equations 1–3 using standard estimators for the average treatment effect among subsets of our data containing only respondents that fall into a given stratum s. Here, we make use of the following linear model:

$$\mathbf{Y}^m = \gamma_0^m + \mathbf{B}\boldsymbol{\gamma}^m + \tau_s^m \mathbf{z}^m + \mathbf{X}\boldsymbol{\delta}^m + \boldsymbol{\epsilon}^m. \tag{4}$$

We denote by N_s the number of respondents in stratum s. $\mathbf{Y}^m$ is an N_s-length vector of observed outcomes related to message m, γ_0^m is an intercept corresponding to the average of these outcomes among respondents in stratum s who were not assigned to message m in the reference block, $\mathbf{B}$ is an $N_s \times (K-1)$ matrix of block indicators and $\boldsymbol{\gamma}^m$ a $K-1$ vector of block fixed effects, τ_s^m is the effect of the m treatment in stratum s, $\mathbf{z}^m$ is an N_s-length vector indicating assignment to treatment m, $\mathbf{X}$ is an $N_s \times 2$ matrix of the average number of film festival attendees per village across all screenings and an indicator for whether a respondent was part of the resampling, $\boldsymbol{\delta}^m$ is a vector of corresponding effects, and $\boldsymbol{\epsilon}^m$ a vector of errors. The underlying logic of the regression model is to provide a "design-based" estimator. The block fixed effects account for differences among experimental blocks, while the resampling fixed effects account for any systematic differences in the outcomes of those sampled later. We do not anticipate any particular treatment effect heterogeneity beyond that captured by principal strata and gender, and so do not allow treatment effects to vary except across strata and gender. Adjusting for audience size dampens any incidental correlation between cluster size and potential outcomes.

4 Results

We begin by considering the effects of messaging on teacher absenteeism on willingness to take action to counter absenteeism. The first column of Table 3 indicates that the mean for this outcome measure among those reached directly in the control condition is 0.61. The estimated treatment effect among those 1,479 respondents reached directly is 0.043. The apparent increase in willingness to act is highly statistically significant ($p < 0.01$) and substantively large. To put this estimate in perspective, the village-level standard deviation among film attendees in the control group is 0.11. Clearly, the messages concerning teacher absenteeism influenced those who attended the screenings.

	Dependent variable:			
	Index of willingness to take action to counter absenteeism			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
Absenteeism	0.043^{***}	-0.001	0.008	0.002
	(0.014)	(0.009)	(0.009)	(0.007)
Control Mean	0.61	0.59	0.59	0.59
Vill. Means	0.62	0.6	0.58	0.59
Vill. SD	0.11	0.08	0.09	0.07
N Vill.	166	166	166	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,479	3,219	3,033	6,252
Adjusted R^2	0.027	0.042	0.049	0.044
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 3: Direct effects and spillovers from absenteeism messages among respondents in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

The apparent second-hand effects are more muted. Column 2 reports the estimated effect among 3,219 individuals who were possibly indirectly exposed because their family or friends attended the screenings. Treatment assignment had no statistically significant effect on those reached indirectly, generating a weakly negative point estimate of -0.001 with a standard error of 0.009. Reassuringly, we see no evidence of spillovers among those not reached, whose point estimate is weakly positive. Pooling over all respondents not directly reached, the estimated proportion of the direct effect that spilled over is $0.002/0.043 = 5\%$. The estimate is close to zero and small in comparison to its standard error.

	Dependent variable:			
	Education is an important goal			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
absenteeism	0.056**	0.012	−0.014	0.0003
	(0.022)	(0.016)	(0.017)	(0.012)
Control Mean	0.44	0.43	0.44	0.43
Vill. Means	0.43	0.43	0.45	0.43
Vill. SD	0.19	0.12	0.14	0.08
N Vill.	166	166	166	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,479	3,219	3,033	6,252
Adjusted R^2	0.007	−0.001	−0.0003	−0.001
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 4: Direct effects and spillovers from absenteeism messages among all respondents in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

A further outcome of interest is whether respondents rate education-related goals among the most important goals for their village (see Table 4). Among those who attended films in the control group, the mean is 0.44. The treatment effect among that group is again substantial, 0.056 with a standard error of 0.022 ($p < 0.05$). Among those reached indirectly, we find a positive point estimate (0.012) that is eclipsed by its standard error (0.016). This estimate falls well short of statistical significance, but taken at face value it suggests that 0.012/0.056 or 21 percent of the direct treatment effect is transmitted to those reached indirectly. The estimated effect among those not reached is unexpectedly negative. Pooling across the groups not reached directly, we see no evidence whatsoever of spillovers. The estimated effect is again close to zero.

Turning to the issue of violence against women, Table 5 reports the results from regressions in which the outcome is willingness to take action to assist victims and report incidents to village authorities. The mean among those reached directly in the control group on this index is 0.38, and the average treatment effect for this subgroup is estimated to be 0.046 with a standard error of 0.015 ($p < 0.05$). The point estimate again suggests a substantial effect in light of the fact that the village-level standard deviation is 0.09. Very little of this effect was transmitted to those reached indirectly, for whom the point estimate is just 0.004 (SE=0.012). The point estimate for those not reached is very close to zero.

On the topic of abortion stigma, Table 6 indicates willingness to help those suffering from post-abortion complications increased significantly among those reached directly. Estimated spillover effects are weakly negative for both those reached indirectly and weakly positive for those not reached – both effects fall well short of statistical significance. Pooling over all not reached directly, we obtain a weakly negative point estimate. In sum, we find little or no evidence of spillover effects.

	Dependent variable:			
	Index of willingness to take action to counter intimate partner violence			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
VAW	0.046^{**}	0.004	−0.0002	0.003
	(0.015)	(0.012)	(0.014)	(0.010)
Control Mean	0.38	0.38	0.38	0.38
Vill. Means	0.38	0.37	0.37	0.38
Vill. SD	0.09	0.07	0.09	0.06
N Vill.	110	110	110	110
Block FE	Yes	Yes	Yes	Yes
Observations	1,154	2,441	1,918	4,359
Adjusted R^2	0.011	0.005	0.013	0.006
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 5: Direct effects and spillovers from anti-VAW messages among all respondents in endline surveys following 2016 festival.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed *p*-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Willingness to help someone suffering from post-abortion complications			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
abortion	0.042^{**}	-0.007	0.001	-0.002
	(0.020)	(0.014)	(0.016)	(0.012)
Control Mean	0.82	0.8	0.78	0.79
Vill. Means	0.79	0.78	0.78	0.78
Vill. SD	0.18	0.13	0.14	0.11
N Vill.	166	166	166	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,479	3,219	3,033	6,252
Adjusted R^2	0.008	0.020	0.022	0.022
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 6: Direct effects and spillovers from anti-abortion stigma messages among all respondents in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

5 Effects by Gender

Because gender defines the lines of communication within villages and because treatment effects among film attendees may vary between men and women, we further investigate whether the apparent patterns of spillover effects change when we focus our attention solely on men or women. Table 7 suggests that with regard to taking action to address teacher absenteeism, average causal effects among those reached directly appear to be somewhat larger for men (0.049, SE=0.015) than for women (0.021, SE=0.023), although the treatment-by-gender interaction is not significant. We do not find markedly greater spillovers for men than women, however. Among those men reached indirectly the point estimate is weakly positive (0.002, SE=0.012), while for women it is weakly negative (-0.003, SE=0.013).

	Dependent variable:			
	Index of willingness to take action to counter absenteeism			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
absenteeism	0.049***	0.002	0.021	−0.003
	(0.015)	(0.012)	(0.023)	(0.013)
Control Mean	0.64	0.63	0.54	0.54
Vill. Means	0.65	0.64	0.56	0.55
Vill. SD	0.12	0.1	0.19	0.09
N Vill.	164	166	142	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,007	1,664	472	1,555
Adjusted R^2	0.023	0.063	0.065	0.025
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 7: Direct effects and spillovers from absenteeism messages among men and women in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Education is an important goal			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
absenteeism	0.043	0.001	0.095*	0.025
	(0.029)	(0.024)	(0.043)	(0.022)
Control Mean	0.48	0.46	0.36	0.4
Vill. Means	0.47	0.45	0.34	0.41
Vill. SD	0.23	0.16	0.33	0.19
N Vill.	164	166	142	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,007	1,664	472	1,555
Adjusted R^2	−0.001	−0.004	0.008	0.009
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 8: Direct effects and spillovers from absenteeism messages among men and women in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Index of willingness to take action to counter intimate partner violence			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
VAW	0.026	−0.002	0.105***	0.011
	(0.019)	(0.014)	(0.027)	(0.018)
Control Mean	0.4	0.38	0.35	0.37
Vill. Means	0.39	0.38	0.35	0.37
Vill. SD	0.12	0.09	0.19	0.1
N Vill.	110	110	97	110
Block FE	Yes	Yes	Yes	Yes
Observations	795	1,247	359	1,194
Adjusted R^2	−0.003	0.010	0.070	−0.0002
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 9: Direct effects and spillovers from anti-VAW messages among men and women in endline surveys following 2016 festival.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed *p*-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Willingness to help someone suffering from post-abortion complications			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
abortion	0.008	−0.009	0.129***	−0.011
	(0.020)	(0.018)	(0.041)	(0.022)
Control Mean	0.86	0.84	0.73	0.75
Vill. Means	0.83	0.82	0.67	0.74
Vill. SD	0.2	0.15	0.36	0.2
N Vill.	164	166	142	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,007	1,664	472	1,555
Adjusted R^2	0.002	0.022	0.045	0.026
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 10: Direct effects and spillovers from anti-abortion stigma messages among men and women in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed *p*-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

Women, on the other hand, seem to be more responsive to the message that improving education is an important community goal. The effect of direct exposure for women is 0.095 (SE=0.043), as compared to 0.043 (SE=0.029) for men who attended films. The corresponding spillover effects follow the hypothesized pattern, but only to a limited extent: for women, the estimated effect is 0.025 (SE=0.022), whereas for men it is only weakly positive (0.001, SE=0.024).

Whereas for absenteeism the treatment-by-gender interaction is insignificant for those reached directly, this interaction is significant ($p < 0.05$) for the topic of domestic violence. Women attendees are highly responsive to the videos, with an estimated average effect of 0.105 (SE=0.027), as compared to men, among whom opinion change is fairly limited (0.026, SE=0.019). However, we find relatively little evidence of spillover effects. Among men, the point estimate is weakly negative, and among women it is just 0.011 (SE=0.018), implying that 10 percent of the direct effect is transmitted to those reached indirectly.

Finally, we again see a significant treatment-by-gender interaction for willingness to help those suffering from post-abortion complications, with women who attended films showing quite strong effects. Yet, the estimated spillover effect is weakly negative, which is inconsistent with our expectations as laid out in the pre-analysis plan.

Overall, the analysis by gender provides little evidence of gender-specific spillover effects. Even focusing on instances in which the estimated direct effect of the messages is especially strong for one gender or the other, we nonetheless find limited evidence of transmission to those reached indirectly.

6 Conclusion

This paper makes two contributions to the study of media effects in developing countries. The first is methodological. We propose a placebo-controlled design to assess the effects of media messages on audiences and others in their social networks. The design allows for unobtrusive assessment of the extent to which media messages change beliefs, attitudes, and behaviors among different segments of the target population. We implemented this design in two successive experiments involving thousands of villagers in more than 150 villages. Although the design imposes a set of important assumptions about the comparability of audiences and their social networks across treatment conditions, it also allows for diagnostic tests of these assumptions, and both studies seem to have satisfied the requirements of the experimental design.

The second contribution is substantive. The literature on media effects has long speculated about the possibility of multiplier effects due to communication between audiences and others in their social network, and recent work seems to show such spillovers (Alik-Lagrange and Ravallion, 2019). Because the number of audience members tends to be considerably smaller than the number of people in their social networks, even relatively small average spillover effects may imply cumulative effects that rival the effects of direct exposure. Ex ante, there were good reasons to believe that viewing the treatment videos would lead to conversations about their content and that these conversations may affect attitudes of non-viewers. Those who attended went to two screenings on average and so would have had multiple exposures to videos that address key social issues affecting their community. The education-entertainment messages feature local actors speaking the local language and provided an immersive experience to an audience with limited access to visual media. The small, tight-knit communities in which the treatments were aired seem particularly prone to such second-hand effects: 50% of the respondents in our second-round sample indicated that they discuss things that are going on in the village every day or almost every day with nearby-neighbors, and 54% of respondents in our first-round sample say that they would be able to name everyone or almost everyone in their village. The results from our experiments suggest that even in instances where diffusion appears likely and where direct effects on audiences' opinions are large and statistically significant, second-hand effects seem meager.

Because this is the first study of its kind, the open question is whether the lack of spillover effects is a general feature of dramatized messages or rather specific to the issues or format of the videos used here. Our messages were interspersed in a larger feature-length film, and it remains to be seen whether spillover effects are more evident when the experimental treatment is the main event rather than a side show. Our videos also depict social problems and their tragic consequences; one wonders whether such "heavy" storylines discourage the kinds of interpersonal conversation through which media effects may be transmitted. Much work remains to be done to develop an evidence-based understanding of the conditions under which spillovers occur. Until then, those who seek to influence attitudes and behavior via dramatized messages should focus primarily on enlarging the audiences who are directly exposed.

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Online Appendix to:

A Placebo Design to Detect Spillovers from an Education-Entertainment Experiment in Uganda

A Additional Design Information

A.1 Outcome Measures

Table 11: Outcome Measures

Index	Question	Value	Label	Round 1	Round 2
Act against absen-teeism	Imagine that you find out that your child's teacher has been absent for 2 days this week during teaching hours. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take	0	Wait another few days to see if the problem corrects itself/Randomly assigned inaction	47%	34%
		1	Immediately begin organizing a PTA meeting, even if you know this might start some trouble	53%	66%
		NA	Don't know/Refuse	0%	0%
Act against absen-teeism	Imagine that you find out that your child's teacher has been absent for 2 days this week during teaching hours. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take	0	Pray to god/Randomly assigned in-action	21%	48%
		1	Bring it up in the village meeting	79%	52%
		NA	Don't know/Refuse	0%	0%
Act against absen-teeism	Imagine that you find out that your child's teacher has been absent for 2 days this week during teaching hours. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take	0	Send your child to a school in the neighboring village, where the teachers always come to class/Randomly assigned inaction	60%	68%
		1	Assemble a group of parents and confront the teacher	40%	32%
		NA	Don't know/Refuse	0%	0%

Act against absen-teeism	Imagine that you find out that your child's teacher has been absent for 2 days this week during teaching hours. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take	0	Allow your child to leave school to help with the garden on days when the teacher is absent	2%	-
		1	Ask the headmaster to threaten to fire the teacher	98%	-
		NA	Don't know/Refuse	0%	-
Act against absen-teeism	Imagine that you find out that your child's teacher has been absent for 2 days this week during teaching hours. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take	0	Randomly assigned inaction	-	25%
		1	Tell the LC1 chairperson to investigate why the headmaster has allowed this problem to occur	-	75%
		NA	Don't know/Refuse	-	0%
Education important goal	Here is a set of cards, which show different goals. Please choose the three that are the most important to you.	0	Did not choose 'Reducing the number of bad teachers at school'	58%	-
		1	Chose 'Reducing the number of bad teachers at school'	42%	-
		NA	Don't know/Refuse	0%	-
Education important goal	Here is a set of cards, which show different goals. Please choose the three that are the most important to you.	0	Did not choose 'Reducing illiteracy'	-	56%
		1	Chose 'Reducing illiteracy'	-	44%
		NA	Don't know/Refuse	-	0%
Act against VAW	Suppose you visit your cousin and she tells you that her husband beat her severely and asks you for help. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.	0	Randomly assigned inaction	-	43%
		1	I would notify the Nabakyala and ask her to mediate the dispute	-	57%
		NA	Don't know/Refuse	-	0%

Act against VAW	Suppose you visit your cousin and she tells you that her husband beat her severely and asks you for help. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.	0	Randomly assigned inaction	-	50%
		1	I would talk to her parents and ask them to come by to help the couple find a peaceful solution	-	50%
		NA	Don't know/Refuse	-	0%
Act against VAW	Suppose you visit your cousin and she tells you that her husband beat her severely and asks you for help. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.	0	Randomly assigned inaction	-	81%
		1	I would accompany her to the police to report the incident	-	19%
		NA	Don't know/Refuse	-	0%
Act against VAW	Suppose you visit your cousin and she tells you that her husband beat her severely and asks you for help. Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.	0	Randomly assigned inaction	-	72%
		1	I would get the LC1 chairperson involved	-	28%
		NA	Don't know/Refuse	-	0%
Help abortion	Suppose that a girl in your neighborhood has had a deliberate abortion. She has been ostracized from the community and people seem to have turned their backs on her. In this situation, two of your friends make the following two statements. Which friend would you agree with?	0	She made her choice and has violated god's rule and we should not get involved	27%	17%
		1	Regardless of what this girl did, we should be a friend to her and try to help her	73%	83%
		NA	Don't know/Refuse	0%	0%

Outcomes are combined into indices by averaging across them. Outcomes in the "Act against absenteeism" index ask respondents to choose one of two actions. In the first round, these two actions were fixed. In the second round, each "active" option was randomly paired with one of the following four "inactive" options: "Wait another few days to see if the problem corrects itself," "Send your child to a school in the neighboring village, where the teachers always come to class," "Find a tutor to instruct your child until the teacher comes back" or "Ask the headmaster to put your child into a different classroom until the teacher returns." For the outcomes in the "Act against VAW" index, the randomly assigned inaction was one of the following four options: "I would tell her that beating is often a sign of love and that she should try to work it out with her husband," "I would advise her to try harder to please her husband and things will likely improve," "I would express my sympathy for her but would tell her that every couple has to work it out for themselves" or "I would calm her down and tell her that the situation is bound to get better."

A.2 Movies Featured in Film Festival

In Round 1 of the experiment, we screened the following films, in order: Mamma Mia, Harry Potter, Slumdog Millionaire, and Oz the Great and Powerful.

In Round 2 of the experiment, we screened the following films, in order: Pirates of the Carribean, Slumdog Millionaire, Spy Kids, The Fast and the Furious, Creed, and Oz the Great and Powerful.

A.3 Detailed Description of Video Vignettes

For teacher absenteeism, the videos show that acting in accordance with the norm is effective in bringing absent teachers back to the classroom. Unlike domestic violence and abortion, teacher absenteeism is uncontroversially viewed as a social bad. The vignettes depict parents who, upon learning their children's teacher has not been coming to class, organize a meeting among members of the parent teacher association (PTA). In the first vignette they discover that the teacher has been absent from classes. In the second, they learn that the teacher has not been paid and is selling soap in the market in order to make ends meet. In the final vignette, the parents' action results in the school being monitored by a government official, whose oversight of the headmaster forces him to pay his teachers. Throughout the story, parents emphasize their responsibility to ensure that their children receive the good education that they deserve.

The vignettes on violence against women (revised for the second round study in 112 villages) contrast a storyline in which a victim of violence does not receive help with one in which the community steps in to ameliorate the situation. In the first vignette the protagonist is a sympathetic and personable woman whose husband beats her severely despite her sincere efforts to appease him. The protagonist's neighbor overhears her screams but decides not to speak out. In the second vignette, which begins with the protagonist's hospitalization and ends with her funeral, we learn that not only her neighbor, but also her daughter and parents knew about the violence. They express regret for failing to speak out sooner. In the third vignette, the setting is a nearby village in which a similar story is unfolding. The focal woman in the story is also beaten by her husband, but unlike the woman in the preceding vignette, she decides to disclose this information to her parents. Rather than scold, her parents intervene to help mediate. Moreover, the parents share the information with the local women's counselor (*Nabakyala*), who visits the household to provide guidance. The vignette closes with the couple in visibly better relations with one another.

In the case of abortion, the vignettes are designed to inspire empathy with women who are stigmatized as a result of having chosen to have an abortion. The videos follow a young woman who suffers from painful and debilitating post-abortion complications. Upon being discovered by her aunt in the first vignette, she confesses that she kept quiet about the abortion and her symptoms for fear of being ostracized by her friends and family. The aunt voices compassion and rushes her niece to the hospital, where, in the second vignette, the aunt engages in a conversation with the doctor. The doctor reiterates the obligation to help those suffering from the consequences of botched abortions and speaks out against harsh judgment. He explains that abortions are a common occurrence and warns against the medical risks of late or no treatment, connecting them to the fear of ostracism. In the final vignette, a school teacher reassures the now-recovered girl that she can come back to school without fear of being expelled.

A.4 Site Selection Strategy

The first round of our experiment took place in 56 villages in the districts of Luwero, Kayunga and Nakeseke. The second round took place in 112 villages in the districts of Mubende, Mityana, Masaka and Lwengo. To select these villages, we first identified villages with video halls in the relevant districts. This led to a set of 162 candidate villages in round 1 and approximately 300 candidate villages in round 2. We then identified and excluded potentially problematic sites (e.g., video halls that operated seasonally or did not have backup generators), narrowing down the selection to 123 candidate villages in round 1 and 247 candidate villages in round 2.

In order to minimize spillovers between villages, we used a random walk procedure to identify subsets of candidate villages in which all members were at least some specific euclidean distance from one another. In round 1, we permuted one million unique sets of 64 villages from the candidate villages, such that each village was at least 5 kilometers from its closest neighbor. In round 2, we chose sets of 125 villages with a distance constraint of 4.5 kilometers.

From among these subsets that met our distance constraint, we selected the sets of villages that maximized the total distance between villages, as well as the number of video-halls within villages in the set (some villages have more than one video hall).

In round 1, we employed a blocking algorithm to organize the sets of 64 villages into 8 blocks of 8 villages, minimizing within-block variance on latitude, longitude, and the approximate population size of each village (coded by workers on Amazon Mechanical Turk using satellite imagery). We selected the set of 64 villages that minimized the total within-block variance, and non-randomly excluded one village from each block, giving a sample of 56 villages in eight blocks (all prior to randomization).

In round 2, we only relied on the total distance between villages to make the selection of distance-eligible subsets. Due to logistical constraints, we replaced 19 villages in the initially selected set by hand-selecting other clusters sufficiently distant from the remaining set. Among the eligible set of 125 villages, we chose the 112 villages with the largest distance to the nearest unit. See figures 4 and 5 for a graphical depiction of the cluster sampling.

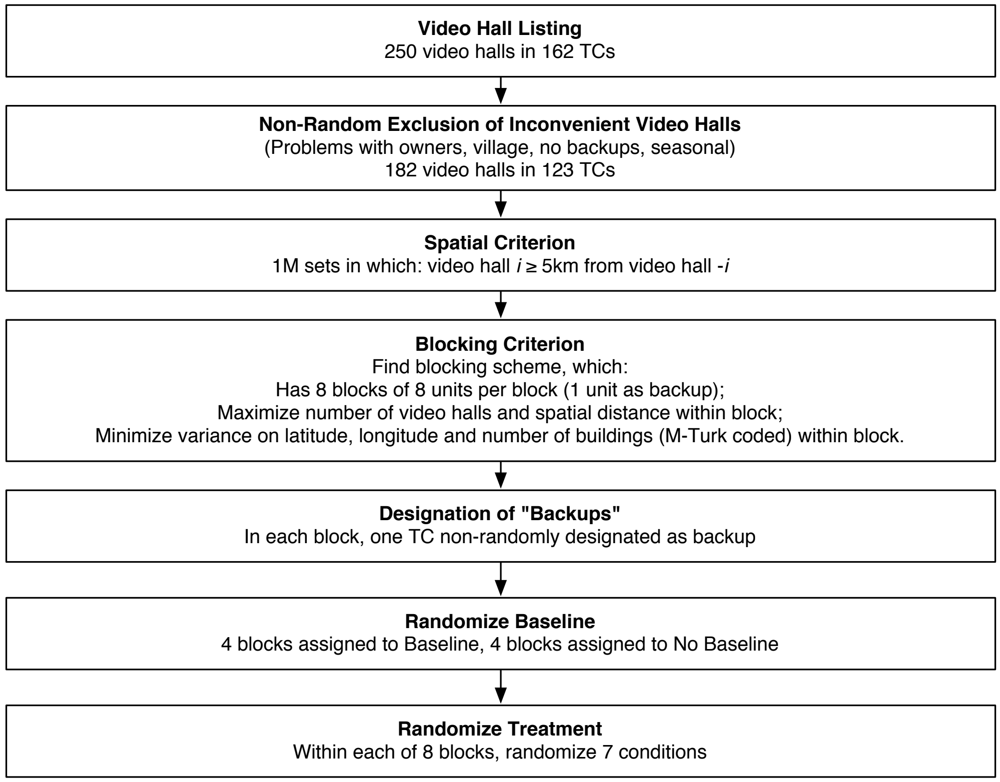

Figure 4: Sampling of clusters in round 1

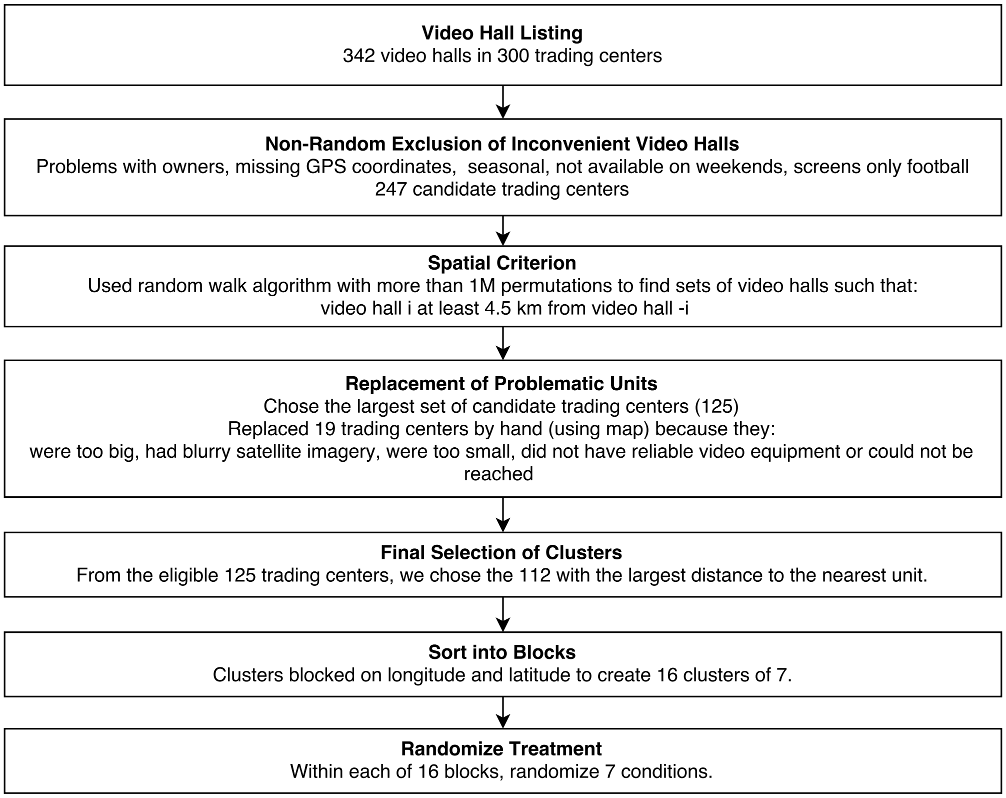

Figure 5: Sampling of clusters in round 2

A.5 Sampling Strategy

In the first round, upon meeting each household, enumerators listed all adult household members (aged 18-65) of the relevant gender and randomly selected one of them as the respondent. If no respondent of the relevant gender resided in the selected household, another household was randomly chosen from the list of households within the circle around the video hall. Enumerators always interviewed respondents of the same gender. If a respondent could not be found during the first visit of the enumerators, two additional visits were conducted before the respondent was coded as a non-response.

In the second round, there was a slight change in the sampling strategy for adults after the survey had been completed in all villages belonging to the first block. Specifically, we narrowed the age range of adult respondents from 18-65 to 18-50 and increased the number of respondents per village from 40 to 50. The first change was made to oversample film festival attendees and the second was due to additional capacities in our survey team that we had not anticipated. Since the same sampling strategy was used among villages within the same block, there is no correlation between the sampling strategy and treatment assignment within block.

Preliminary analyses that we conducted after having completed the endline survey in the first round showed that some cluster-level samples had very few responses from adult respondents who had attended at least one film. Consequently, we undertook a second round of sampling to target such those reached directly, aiming to survey 15 additional adult respondents in 14 clusters. To select the 14 clusters, we identified the two clusters in each of the 7 treatment conditions with the fewest film attendees.[1] We conducted this sampling by continuing the same random sequence of households generated in the endline, so that the sampled units are the same units that would have been sampled had we continued endline data collection. In order to over-sample film viewers, the sampling strategy within households was altered to target respondents between 18 - 35, aiming for a target of 2/3 men. This change in plan was reflected in an addendum to the pre-analysis plan submitted prior to revealing the second round of data collection.[2] In the second round, we pre-specified and followed the same procedure.

In round 2, we were unable to conduct our household survey in two villages due to resistance from the local communities. We believe that our inability to work in these locations was unrelated to the treatment status of the village. The two locations are in an area known for suspicion towards outside groups. In both locations, villagers were suspicious of the research team and in particular their motives for collecting head of household names (a component of the sampling procedure). There were fears related to land evictions and kidnapping. We deemed it unsafe to continue data collection in those areas. There was no indication from discussions with the residents of these villages that these difficulties were related to the specific treatment messages that were screened. In terms of the analysis, the above implies that we can recover an unbiased estimate of stratum-specific average treatment effects in villages that did not attrit. Therefore, in our main analysis we simply exclude villages in which we could not survey. Our response rate in round 2 is 96.4%.

[1] If there were more clusters with the same number of attendees, we randomly selected one among them.
[2] The original Pre-Analysis Plan and addendum may be found at http://egap.org/registration/2207.

B Identification

B.1 Balance on Covariates among Reachability Strata

We examine balance on observable pre-treatment covariates for those reached directly, reached indirectly, or not reached at all. For each covariate, we test for a significant relationship to the treatment using randomization inference to conduct a likelihood ratio test. In the tables below, the first column names the covariate and the following seven columns show means of covariate under the respective treatment conditions. The last column in the table shows the p-value from the likelihood ratio test. The 'full' model regresses the covariate on the six non-placebo treatment indicators, controlling for block and resample fixed effects. The restricted model regresses the covariate on block and resample fixed effects only. The observed likelihood ratio is compared to 2000 likelihood ratios simulated under the null of no effect of treatment on the covariate for all units by re-permuting the treatment assignment and re-estimating the likelihood. The p-value is equal to the proportion of such simulations at least as great as the observed likelihood ratio. Note that p-values are not adjusted to account for family-wise error rates: under independence, in expectation $x\%$ of the covariates should exhibit imbalance that is significant at the $x\%$ level.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
protestant	0.23	0.27	0.14	0.12	0.18	0.16	0.19	0.00
english_christian	0.03	0.05	0.07	0.07	0.06	0.05	0.17	0.01
munyankole	0.06	0.08	0.08	0.04	0.08	0.17	0.12	0.04
minority_tribe	0.19	0.21	0.14	0.12	0.17	0.07	0.08	0.04
domestic_work	0.02	0.04	0.07	0.03	0.06	0.02	0.04	0.04
other_person	0.04	0.08	0.09	0.12	0.07	0.03	0.09	0.07
misc_floor	0.09	0.10	0.20	0.13	0.17	0.12	0.19	0.08
minority_lang	0.07	0.05	0.09	0.03	0.06	0.03	0.02	0.08
atheist	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.09
rooms	2.48	2.44	2.19	2.50	2.72	2.49	2.36	0.10
misc_light	0.13	0.14	0.17	0.08	0.17	0.12	0.12	0.11
highest_grade	7.19	7.79	6.61	7.06	7.15	6.47	7.03	0.13
munyarwanda	0.07	0.04	0.13	0.11	0.09	0.14	0.08	0.13
munyoro	0.08	0.04	0.05	0.08	0.03	0.03	0.07	0.17
holy_spirit	0.09	0.09	0.14	0.14	0.13	0.15	0.08	0.20
university	0.05	0.04	0.03	0.05	0.06	0.02	0.02	0.21
write_only	0.05	0.03	0.06	0.04	0.02	0.05	0.02	0.21
cellphone	0.84	0.75	0.74	0.84	0.78	0.79	0.77	0.25
write_and_read	0.80	0.88	0.79	0.84	0.85	0.82	0.87	0.28
other_work	0.07	0.09	0.05	0.04	0.04	0.05	0.03	0.28
luganda_lang	0.89	0.93	0.86	0.87	0.86	0.83	0.94	0.29
catholic	0.42	0.38	0.45	0.46	0.43	0.50	0.36	0.30
runyannkole_lang	0.02	0.02	0.04	0.02	0.04	0.11	0.04	0.31
solar_light	0.28	0.31	0.28	0.40	0.34	0.33	0.24	0.32
chair	0.80	0.86	0.79	0.87	0.85	0.81	0.84	0.34
female	0.26	0.32	0.35	0.29	0.34	0.38	0.31	0.39
not_married	0.23	0.24	0.18	0.21	0.18	0.25	0.21	0.40
age	28.71	28.42	27.86	28.52	29.60	29.69	28.45	0.41
read_only	0.04	0.02	0.04	0.03	0.02	0.01	0.02	0.43
education_work	0.06	0.06	0.02	0.03	0.02	0.04	0.04	0.44
separated	0.10	0.13	0.11	0.09	0.09	0.14	0.11	0.47
manual_work	0.10	0.06	0.07	0.07	0.08	0.10	0.06	0.49
same_village	0.51	0.44	0.42	0.47	0.51	0.45	0.47	0.50
minority_religion	0.00	0.00	0.01	0.00	0.01	0.00	0.01	0.51
misc_wall	0.03	0.04	0.07	0.01	0.03	0.06	0.06	0.51
pray_private	7.95	8.18	8.16	8.01	7.97	7.94	7.82	0.52
cement_floor	0.62	0.55	0.47	0.57	0.46	0.50	0.50	0.52
motor_cycle	0.23	0.22	0.23	0.28	0.24	0.20	0.19	0.52
kerosene_light	0.24	0.32	0.32	0.25	0.32	0.28	0.38	0.53
household_other	0.14	0.12	0.11	0.09	0.08	0.10	0.11	0.53
mukiga	0.03	0.02	0.03	0.02	0.07	0.03	0.03	0.54
fumbira_lang	0.03	0.01	0.01	0.07	0.05	0.03	0.01	0.55
tv	0.26	0.16	0.19	0.25	0.17	0.17	0.19	0.56
radio	0.80	0.85	0.78	0.84	0.85	0.81	0.83	0.58
living_as_married	0.38	0.32	0.41	0.39	0.37	0.35	0.45	0.59
mobile_phone_use	3.48	3.20	3.12	3.35	3.24	3.22	3.20	0.59
transport_work	0.04	0.04	0.04	0.06	0.03	0.06	0.08	0.60
sofa	0.22	0.16	0.15	0.22	0.16	0.19	0.20	0.63
married	0.34	0.35	0.33	0.33	0.39	0.30	0.26	0.63
stone_wall	0.03	0.03	0.03	0.05	0.04	0.06	0.04	0.64
illiterate	0.11	0.06	0.12	0.09	0.11	0.12	0.08	0.64
travel_big_city	0.76	0.73	0.75	0.71	0.77	0.82	0.76	0.66
electric_light	0.23	0.12	0.12	0.15	0.08	0.13	0.12	0.67
number_children	3.36	3.06	3.16	3.09	3.73	3.51	3.29	0.67
single_hut	0.60	0.59	0.61	0.62	0.69	0.62	0.62	0.68

christian_only	0.02	0.03	0.03	0.03	0.02	0.03	0.01	0.70
mufumbira_tribe	0.01	0.02	0.02	0.07	0.04	0.04	0.01	0.71
several_huts	0.12	0.10	0.08	0.10	0.07	0.11	0.08	0.73
brick_wall	0.64	0.63	0.58	0.62	0.56	0.53	0.55	0.73
earth_floor	0.29	0.35	0.33	0.31	0.37	0.37	0.31	0.75
living_conditions	0.05	0.00	0.02	0.08	0.04	0.04	-0.07	0.76
mud_wall	0.23	0.24	0.24	0.22	0.30	0.23	0.28	0.77
cement_wall	0.07	0.06	0.08	0.09	0.08	0.11	0.07	0.77
firewood_fuel	0.51	0.51	0.62	0.59	0.62	0.62	0.58	0.77
mutooro	0.01	0.02	0.01	0.00	0.02	0.01	0.02	0.78
charcoal_fuel	0.46	0.46	0.36	0.36	0.35	0.35	0.40	0.79
agriculture_work	0.54	0.55	0.57	0.62	0.64	0.59	0.64	0.79
hospitality_work	0.05	0.07	0.03	0.04	0.04	0.06	0.04	0.81
religious_service	1.92	2.07	1.49	1.16	1.73	1.34	1.55	0.84
misc_fuel	0.04	0.03	0.03	0.04	0.02	0.03	0.02	0.84
household_spouse	0.22	0.24	0.27	0.27	0.27	0.26	0.23	0.84
household_younger	2.92	2.88	2.68	2.90	3.08	2.78	2.69	0.84
retail_work	0.12	0.10	0.12	0.09	0.09	0.09	0.07	0.84
no_work	0.03	0.06	0.03	0.04	0.04	0.05	0.04	0.84
share_house	0.28	0.31	0.31	0.28	0.24	0.27	0.30	0.86
living_conditions_compared	1.96	2.04	1.96	2.04	2.06	2.02	1.94	0.88
muslim	0.21	0.18	0.16	0.17	0.17	0.11	0.19	0.89
household_children	2.25	2.23	2.03	2.30	2.38	2.17	2.11	0.89
members	4.38	4.31	4.14	4.36	4.50	4.17	4.17	0.90
household_older	0.46	0.44	0.46	0.44	0.43	0.39	0.48	0.91
day	1.26	1.19	1.20	1.21	1.25	1.24	1.26	0.93
muganda_tribe	0.55	0.58	0.53	0.55	0.51	0.52	0.59	0.93
household_head	0.64	0.64	0.61	0.64	0.65	0.65	0.66	0.99

Table 12: Balance on covariates among those reached directly

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
chair	0.85	0.85	0.84	0.90	0.87	0.84	0.79	0.02
education_work	0.03	0.05	0.03	0.05	0.02	0.02	0.03	0.03
living_conditions_compared	2.20	2.21	2.13	2.21	2.15	2.10	2.06	0.05
transport_work	0.04	0.04	0.03	0.04	0.02	0.05	0.01	0.07
cellphone	0.83	0.82	0.77	0.83	0.78	0.83	0.75	0.10
living_conditions	0.13	0.10	0.15	0.20	0.09	0.08	0.06	0.11
catholic	0.40	0.37	0.35	0.45	0.41	0.45	0.36	0.15
stone_wall	0.03	0.01	0.04	0.03	0.04	0.05	0.05	0.21
cement_wall	0.12	0.09	0.10	0.12	0.07	0.13	0.11	0.21
highest_grade	6.95	7.24	7.16	7.01	6.46	6.64	6.69	0.21
religious_service	2.11	1.78	1.94	1.29	1.42	1.41	1.79	0.22
minority_tribe	0.16	0.15	0.11	0.14	0.13	0.08	0.14	0.22
age	31.97	31.31	31.25	31.67	32.24	32.55	31.43	0.23
minority_lang	0.06	0.03	0.09	0.03	0.05	0.03	0.04	0.23
fumbira_lang	0.02	0.02	0.01	0.06	0.04	0.02	0.00	0.26
muslim	0.20	0.21	0.20	0.15	0.15	0.11	0.16	0.27
household_older	0.49	0.56	0.48	0.50	0.54	0.45	0.49	0.27
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.31
household_head	0.58	0.54	0.59	0.57	0.54	0.60	0.58	0.31
female	0.49	0.51	0.46	0.49	0.53	0.48	0.49	0.33
minority_religion	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.34
pray_private	8.12	8.18	8.04	8.19	8.16	8.13	7.93	0.36
read_only	0.03	0.03	0.02	0.03	0.04	0.03	0.03	0.36
runyannkole_lang	0.03	0.02	0.06	0.06	0.05	0.12	0.05	0.38

mukiga	0.04	0.02	0.04	0.01	0.04	0.04	0.03	0.39
brick_wall	0.59	0.65	0.59	0.60	0.62	0.53	0.55	0.40
single_hut	0.62	0.58	0.58	0.60	0.63	0.56	0.65	0.42
luganda_lang	0.90	0.93	0.84	0.84	0.86	0.83	0.90	0.42
household_spouse	0.35	0.36	0.36	0.35	0.39	0.33	0.36	0.43
mufumbira_tribe	0.01	0.02	0.01	0.06	0.04	0.03	0.01	0.43
english_christian	0.06	0.07	0.08	0.06	0.08	0.06	0.10	0.44
other_work	0.06	0.06	0.04	0.05	0.07	0.04	0.05	0.44
mobile_phone_use	3.36	3.31	3.27	3.36	3.27	3.36	3.12	0.45
members	4.89	5.02	4.70	4.87	4.92	4.62	5.00	0.46
household_children	2.72	2.80	2.57	2.65	2.76	2.50	2.81	0.50
travel_big_city	0.72	0.69	0.73	0.68	0.73	0.74	0.68	0.50
misc_fuel	0.01	0.01	0.03	0.03	0.03	0.03	0.01	0.51
day	1.30	1.20	1.22	1.24	1.24	1.25	1.20	0.51
mutooro	0.02	0.01	0.02	0.00	0.01	0.01	0.02	0.53
separated	0.10	0.11	0.09	0.11	0.13	0.13	0.13	0.56
misc_floor	0.09	0.10	0.12	0.10	0.12	0.10	0.15	0.57
cement_floor	0.64	0.60	0.58	0.58	0.49	0.57	0.54	0.57
share_house	0.27	0.32	0.32	0.28	0.26	0.30	0.25	0.58
radio	0.82	0.81	0.85	0.82	0.82	0.79	0.80	0.59
tv	0.28	0.28	0.23	0.27	0.22	0.23	0.21	0.59
write_and_read	0.82	0.84	0.81	0.80	0.78	0.81	0.82	0.59
manual_work	0.09	0.06	0.08	0.09	0.08	0.06	0.07	0.59
rooms	2.63	2.74	2.73	2.88	2.75	2.63	2.70	0.61
university	0.04	0.05	0.06	0.05	0.04	0.04	0.02	0.61
munyoro	0.06	0.07	0.06	0.05	0.04	0.04	0.04	0.62
household_other	0.08	0.09	0.05	0.07	0.07	0.07	0.06	0.63
household_younger	3.39	3.46	3.21	3.37	3.37	3.17	3.51	0.63
retail_work	0.14	0.14	0.13	0.09	0.14	0.12	0.13	0.64
other_person	0.11	0.12	0.11	0.13	0.11	0.09	0.10	0.65
several_huts	0.10	0.10	0.10	0.12	0.11	0.14	0.09	0.66
not_married	0.13	0.15	0.15	0.15	0.12	0.15	0.12	0.67
munyarwanda	0.09	0.07	0.08	0.10	0.11	0.09	0.06	0.67
earth_floor	0.27	0.30	0.30	0.32	0.39	0.33	0.32	0.68
charcoal_fuel	0.44	0.41	0.42	0.37	0.35	0.35	0.35	0.68
electric_light	0.23	0.23	0.18	0.23	0.16	0.15	0.15	0.69
domestic_work	0.03	0.05	0.05	0.06	0.06	0.05	0.05	0.71
number_children	4.28	4.14	4.24	4.20	4.54	4.38	4.44	0.72
solar_light	0.28	0.27	0.33	0.31	0.33	0.37	0.31	0.73
munyankole	0.08	0.09	0.13	0.11	0.10	0.16	0.11	0.73
protestant	0.20	0.19	0.20	0.17	0.19	0.22	0.22	0.76
misc_wall	0.02	0.02	0.05	0.03	0.03	0.03	0.04	0.77
married	0.37	0.42	0.39	0.36	0.40	0.36	0.36	0.77
firewood_fuel	0.55	0.57	0.55	0.61	0.62	0.63	0.63	0.78
christian_only	0.02	0.03	0.03	0.02	0.03	0.02	0.03	0.81
living_as_married	0.41	0.35	0.40	0.39	0.37	0.38	0.42	0.81
same_village	0.42	0.39	0.37	0.41	0.37	0.37	0.42	0.81
illiterate	0.11	0.09	0.12	0.11	0.13	0.11	0.10	0.86
misc_light	0.12	0.11	0.14	0.11	0.10	0.11	0.13	0.89
sofa	0.27	0.27	0.24	0.26	0.24	0.22	0.25	0.89
write_only	0.04	0.04	0.05	0.05	0.05	0.04	0.05	0.91
agriculture_work	0.54	0.55	0.59	0.55	0.56	0.62	0.60	0.91
hospitality_work	0.05	0.04	0.06	0.06	0.05	0.05	0.05	0.92
muganda_tribe	0.54	0.56	0.55	0.53	0.53	0.55	0.60	0.93
motor_cycle	0.28	0.30	0.26	0.29	0.30	0.28	0.29	0.96
no_work	0.05	0.03	0.05	0.05	0.05	0.04	0.04	0.96
mud_wall	0.24	0.22	0.22	0.23	0.25	0.26	0.25	0.98

kerosene_light	0.27	0.28	0.26	0.26	0.30	0.26	0.28	0.98
holy_spirit	0.12	0.13	0.13	0.15	0.13	0.14	0.12	0.99

Table 13: Balance on covariates among those reached indirectly

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
not_married	0.09	0.11	0.14	0.12	0.13	0.16	0.07	0.02
education_work	0.07	0.04	0.04	0.03	0.03	0.06	0.05	0.03
household_other	0.06	0.10	0.09	0.08	0.06	0.10	0.04	0.05
age	32.08	32.71	31.55	33.08	33.08	32.39	33.56	0.06
married	0.44	0.41	0.33	0.38	0.44	0.39	0.46	0.06
university	0.08	0.06	0.06	0.05	0.04	0.11	0.07	0.06
minority_lang	0.05	0.04	0.11	0.04	0.07	0.04	0.04	0.06
living_conditions	0.11	0.15	0.05	0.13	-0.01	0.08	0.03	0.06
highest_grade	7.00	7.48	6.81	6.66	5.97	7.54	6.69	0.09
mutooro	0.02	0.00	0.05	0.00	0.01	0.01	0.01	0.11
single_hut	0.60	0.53	0.61	0.60	0.68	0.55	0.60	0.13
travel_big_city	0.58	0.66	0.61	0.60	0.57	0.66	0.67	0.13
luganda_lang	0.91	0.92	0.82	0.87	0.81	0.84	0.89	0.15
number_children	4.57	4.43	4.24	4.72	4.85	4.08	4.53	0.16
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.18
share_house	0.28	0.37	0.29	0.28	0.24	0.36	0.30	0.18
radio	0.77	0.84	0.79	0.80	0.74	0.80	0.80	0.19
separated	0.10	0.13	0.12	0.16	0.12	0.13	0.10	0.20
cement_wall	0.14	0.14	0.12	0.16	0.12	0.18	0.12	0.22
write_and_read	0.73	0.82	0.75	0.78	0.70	0.79	0.76	0.25
munyoro	0.07	0.03	0.06	0.07	0.05	0.02	0.04	0.25
agriculture_work	0.51	0.48	0.57	0.61	0.57	0.46	0.56	0.25
other_person	0.12	0.08	0.09	0.11	0.10	0.07	0.11	0.30
living_conditions_compared	2.22	2.32	2.20	2.26	2.11	2.24	2.22	0.30
mobile_phone_use	3.19	3.31	3.20	3.07	3.04	3.40	3.13	0.31
minority_religion	0.00	0.00	0.01	0.00	0.01	0.01	0.00	0.32
stone_wall	0.03	0.04	0.06	0.03	0.04	0.05	0.02	0.33
runyannkole_lang	0.01	0.02	0.05	0.06	0.07	0.07	0.05	0.33
domestic_work	0.10	0.10	0.11	0.06	0.07	0.10	0.09	0.35
mukiga	0.03	0.03	0.05	0.02	0.06	0.03	0.02	0.36
mufumbira_tribe	0.04	0.01	0.01	0.03	0.04	0.05	0.02	0.38
misc_floor	0.09	0.09	0.12	0.11	0.12	0.07	0.12	0.41
household_spouse	0.46	0.43	0.47	0.42	0.44	0.43	0.50	0.41
fumbira_lang	0.03	0.02	0.02	0.03	0.04	0.06	0.01	0.42
cement_floor	0.64	0.72	0.62	0.63	0.52	0.64	0.61	0.43
kerosene_light	0.22	0.26	0.28	0.29	0.28	0.21	0.28	0.44
day	1.20	1.22	1.28	1.26	1.22	1.25	1.20	0.44
hospitality_work	0.04	0.07	0.04	0.05	0.05	0.06	0.03	0.44
earth_floor	0.27	0.20	0.26	0.26	0.35	0.29	0.27	0.45
household_older	0.60	0.64	0.59	0.57	0.56	0.59	0.53	0.46
manual_work	0.07	0.04	0.07	0.04	0.06	0.07	0.07	0.46
other_work	0.07	0.06	0.04	0.04	0.06	0.07	0.06	0.46
cellphone	0.77	0.81	0.82	0.75	0.75	0.82	0.77	0.48
illiterate	0.17	0.11	0.15	0.14	0.19	0.14	0.17	0.48
living_as_married	0.38	0.38	0.45	0.35	0.35	0.36	0.39	0.49
several_huts	0.12	0.10	0.09	0.12	0.08	0.08	0.10	0.52
munyankole	0.06	0.06	0.10	0.10	0.12	0.09	0.09	0.53
rooms	2.66	2.71	2.57	2.91	2.71	2.64	2.72	0.54
brick_wall	0.58	0.64	0.54	0.60	0.56	0.56	0.62	0.55
no_work	0.03	0.05	0.03	0.04	0.04	0.05	0.03	0.55

retail_work	0.10	0.17	0.12	0.14	0.12	0.15	0.12	0.57
muslim	0.19	0.21	0.16	0.15	0.17	0.16	0.18	0.58
household_children	2.83	2.69	2.60	2.83	2.90	2.62	2.73	0.61
minority_tribe	0.17	0.19	0.14	0.17	0.16	0.15	0.15	0.66
sofa	0.33	0.28	0.25	0.26	0.24	0.28	0.30	0.67
english_christian	0.07	0.06	0.08	0.09	0.08	0.06	0.10	0.68
religious_service	1.92	1.92	1.50	1.54	1.54	1.82	1.77	0.68
holy_spirit	0.17	0.16	0.17	0.15	0.15	0.19	0.15	0.70
tv	0.32	0.32	0.30	0.29	0.25	0.34	0.25	0.70
mud_wall	0.23	0.16	0.25	0.19	0.25	0.19	0.20	0.72
charcoal_fuel	0.45	0.47	0.44	0.38	0.36	0.46	0.39	0.72
misc_wall	0.02	0.02	0.03	0.01	0.04	0.03	0.04	0.74
catholic	0.37	0.34	0.36	0.41	0.39	0.38	0.35	0.75
female	0.62	0.60	0.64	0.62	0.59	0.60	0.61	0.75
misc_fuel	0.00	0.01	0.01	0.01	0.01	0.02	0.02	0.76
chair	0.83	0.84	0.82	0.85	0.83	0.84	0.81	0.76
household_head	0.48	0.48	0.44	0.50	0.50	0.47	0.46	0.76
misc_light	0.13	0.15	0.14	0.13	0.15	0.13	0.13	0.77
firewood_fuel	0.55	0.52	0.55	0.61	0.62	0.52	0.59	0.77
protestant	0.16	0.19	0.19	0.15	0.18	0.18	0.17	0.80
write_only	0.05	0.04	0.06	0.04	0.06	0.04	0.05	0.80
household_younger	3.43	3.38	3.23	3.45	3.49	3.25	3.41	0.81
pray_private	8.15	8.20	8.15	8.30	8.15	8.14	8.14	0.83
members	5.03	5.02	4.82	5.03	5.06	4.83	4.94	0.83
same_village	0.31	0.31	0.28	0.34	0.29	0.30	0.31	0.83
read_only	0.04	0.02	0.03	0.03	0.05	0.03	0.03	0.85
muganda_tribe	0.51	0.58	0.51	0.52	0.47	0.54	0.55	0.85
electric_light	0.25	0.32	0.25	0.27	0.21	0.32	0.23	0.86
munyarwanda	0.10	0.10	0.08	0.09	0.10	0.10	0.11	0.86
transport_work	0.02	0.03	0.02	0.02	0.03	0.03	0.02	0.87
motor_cycle	0.25	0.27	0.27	0.26	0.24	0.28	0.25	0.92
solar_light	0.30	0.22	0.27	0.24	0.26	0.25	0.27	0.95
christian_only	0.04	0.04	0.04	0.04	0.03	0.03	0.04	0.96

Table 14: Balance on covariates among those not reached

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
living_conditions	0.12	0.12	0.10	0.17	0.04	0.08	0.05	0.01
minority_lang	0.05	0.03	0.10	0.04	0.06	0.03	0.04	0.05
not_married	0.11	0.14	0.14	0.14	0.12	0.15	0.10	0.06
minority_religion	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.08
chair	0.84	0.85	0.83	0.88	0.85	0.84	0.80	0.08
age	32.02	31.93	31.39	32.38	32.63	32.47	32.47	0.10
mutooro	0.02	0.01	0.03	0.00	0.01	0.01	0.01	0.12
cement_wall	0.13	0.11	0.11	0.14	0.09	0.15	0.12	0.13
living_conditions_compared	2.21	2.26	2.16	2.24	2.13	2.16	2.14	0.13
highest_grade	6.97	7.34	6.99	6.84	6.23	7.07	6.69	0.13
write_and_read	0.78	0.83	0.78	0.79	0.74	0.80	0.79	0.17
single_hut	0.61	0.56	0.60	0.60	0.66	0.56	0.63	0.22
education_work	0.05	0.05	0.03	0.04	0.03	0.04	0.04	0.22
religious_service	2.01	1.84	1.73	1.42	1.47	1.60	1.78	0.24
household_other	0.07	0.09	0.07	0.08	0.06	0.08	0.05	0.24
luganda_lang	0.90	0.92	0.83	0.86	0.84	0.83	0.90	0.25
transport_work	0.03	0.04	0.02	0.03	0.02	0.04	0.02	0.26
catholic	0.38	0.36	0.35	0.43	0.40	0.41	0.36	0.28
share_house	0.28	0.34	0.30	0.28	0.25	0.33	0.27	0.28

muslim	0.19	0.21	0.18	0.15	0.16	0.13	0.17	0.29
household_spouse	0.40	0.39	0.41	0.38	0.42	0.38	0.43	0.30
household_older	0.55	0.60	0.53	0.53	0.55	0.52	0.51	0.30
travel_big_city	0.65	0.68	0.67	0.64	0.65	0.70	0.68	0.31
mukiga	0.04	0.03	0.04	0.02	0.05	0.04	0.02	0.31
pray_private	8.14	8.19	8.09	8.25	8.15	8.13	8.03	0.33
minority_tribe	0.17	0.17	0.12	0.15	0.14	0.12	0.15	0.33
english_christian	0.06	0.06	0.08	0.08	0.08	0.06	0.10	0.34
stone_wall	0.03	0.03	0.05	0.03	0.04	0.05	0.04	0.36
other_person	0.12	0.10	0.10	0.12	0.10	0.08	0.10	0.36
runyannkole_lang	0.02	0.02	0.06	0.06	0.06	0.10	0.05	0.36
misc_floor	0.09	0.10	0.12	0.11	0.12	0.09	0.14	0.37
separated	0.10	0.12	0.10	0.13	0.12	0.13	0.11	0.39
household_children	2.78	2.75	2.59	2.74	2.82	2.56	2.77	0.39
mobile_phone_use	3.27	3.31	3.23	3.21	3.16	3.38	3.12	0.39
cellphone	0.80	0.82	0.80	0.79	0.77	0.82	0.76	0.40
read_only	0.03	0.03	0.03	0.03	0.05	0.03	0.03	0.40
number_children	4.42	4.27	4.24	4.46	4.68	4.24	4.48	0.41
university	0.06	0.05	0.06	0.05	0.04	0.07	0.04	0.41
illiterate	0.14	0.10	0.14	0.13	0.16	0.13	0.13	0.41
other_work	0.07	0.06	0.04	0.04	0.06	0.05	0.05	0.43
members	4.96	5.02	4.76	4.95	4.98	4.72	4.97	0.44
cement_floor	0.64	0.65	0.60	0.60	0.51	0.60	0.57	0.45
rooms	2.65	2.73	2.66	2.89	2.73	2.64	2.71	0.46
munyoro	0.07	0.05	0.06	0.06	0.05	0.03	0.04	0.48
married	0.40	0.42	0.36	0.37	0.42	0.38	0.41	0.49
earth_floor	0.27	0.25	0.28	0.29	0.37	0.31	0.29	0.50
misc_fuel	0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.50
radio	0.79	0.82	0.82	0.81	0.78	0.79	0.80	0.52
household_younger	3.41	3.42	3.22	3.41	3.43	3.21	3.46	0.52
brick_wall	0.59	0.64	0.56	0.60	0.59	0.54	0.58	0.53
manual_work	0.08	0.05	0.07	0.07	0.07	0.06	0.07	0.56
day	1.25	1.21	1.25	1.25	1.23	1.25	1.20	0.58
living_as_married	0.40	0.36	0.42	0.37	0.36	0.37	0.40	0.59
mufumbira_tribe	0.02	0.02	0.01	0.04	0.04	0.04	0.02	0.59
female	0.56	0.55	0.55	0.56	0.56	0.54	0.55	0.60
munyankole	0.07	0.08	0.12	0.10	0.11	0.13	0.10	0.62
fumbira_lang	0.02	0.02	0.02	0.05	0.04	0.04	0.01	0.66
tv	0.30	0.30	0.26	0.28	0.23	0.28	0.23	0.68
charcoal_fuel	0.44	0.44	0.43	0.38	0.35	0.40	0.37	0.72
same_village	0.36	0.35	0.33	0.38	0.34	0.34	0.37	0.73
protestant	0.18	0.19	0.20	0.16	0.19	0.20	0.20	0.74
sofa	0.30	0.28	0.25	0.26	0.24	0.25	0.28	0.76
write_only	0.05	0.04	0.06	0.05	0.05	0.04	0.05	0.77
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.78
firewood_fuel	0.55	0.55	0.55	0.61	0.62	0.58	0.61	0.79
several_huts	0.11	0.10	0.10	0.12	0.09	0.11	0.10	0.80
retail_work	0.12	0.15	0.13	0.11	0.13	0.13	0.12	0.80
kerosene_light	0.24	0.27	0.27	0.28	0.29	0.24	0.28	0.81
misc_wall	0.02	0.02	0.04	0.02	0.04	0.03	0.04	0.82
agriculture_work	0.53	0.52	0.58	0.58	0.56	0.54	0.58	0.84
christian_only	0.03	0.04	0.04	0.03	0.03	0.02	0.03	0.86
electric_light	0.24	0.27	0.21	0.25	0.18	0.23	0.19	0.86
household_head	0.53	0.51	0.52	0.54	0.52	0.54	0.52	0.87
muganda_tribe	0.52	0.57	0.53	0.52	0.50	0.54	0.57	0.90
misc_light	0.12	0.13	0.14	0.12	0.13	0.12	0.13	0.91
munyarwanda	0.09	0.09	0.08	0.10	0.10	0.10	0.08	0.91

holy_spirit	0.15	0.14	0.15	0.15	0.14	0.17	0.14	0.95
solar_light	0.29	0.25	0.30	0.28	0.30	0.31	0.29	0.95
hospitality_work	0.05	0.05	0.05	0.06	0.05	0.06	0.04	0.95
domestic_work	0.07	0.07	0.08	0.06	0.06	0.07	0.07	0.96
mud_wall	0.23	0.19	0.23	0.21	0.25	0.22	0.22	0.97
no_work	0.04	0.04	0.04	0.04	0.04	0.05	0.03	0.99
motor_cycle	0.27	0.29	0.27	0.27	0.27	0.28	0.27	1.00

Table 15: Balance on covariates among those not reached directly

B.2 Stratum Membership by Treatment Status

We test whether the stratum membership of respondents is affected by the treatment by computing a likelihood ratio permutation test. If a participant's response indicates they are "directly reached" we code their stratum variable 1, those "indirectly reached" are coded as 2, and those "not reached" as 3. This variable is modeled as a multinomial logit process. We run a "full" model that specifies the self-selection into reachability strata as a function of treatment status, block fixed effects, and a resample indicator, and compare this to a "nested" model that restricts the coefficient on all treatment indicators to 0. We simulate 1000 log likelihood ratios between these models under the sharp null of no treatment effect on stratum membership for all units by permuting the treatment assignment. We then obtain p-values by calculating the proportion of simulated likelihood ratios that are at least as large as the observed likelihood ratio.

The results from this test are plotted on Figure 6. The observed likelihoods are not highly unlikely under the null hypothesis of no treatment effect on compliance choice for all units. We fail to reject the null at 5% confidence in all tests. Note that the p-value is below the $\alpha = .1$ threshold among women in the test that uses "pure" treatment categories.

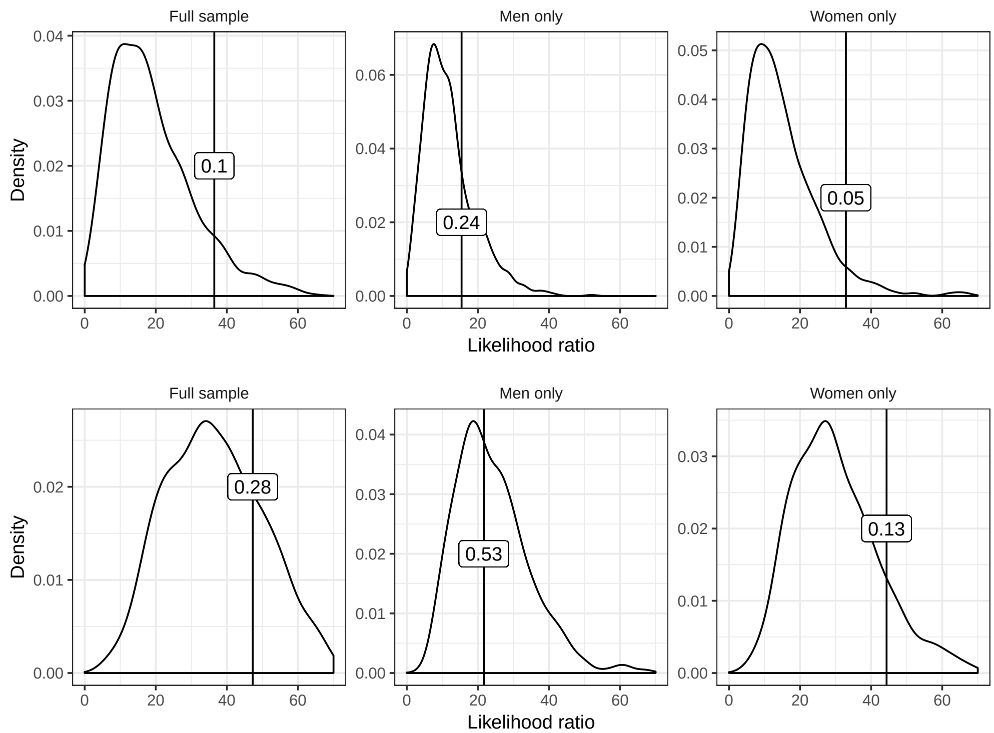

Figure 6: Tests of the assumption that treatment does not affect stratum membership.
Y axis plots the density of likelihood ratios simulated under the sharp null of no treatment effect on compliance status for all units. The X axis plots the value of the likelihood ratio. Vertical line indicates the observed likelihood ratio, and its label indicates the p-value. The first row derives from a full model in which treatment status is coded into "pure" treatment indicators (any VAW messaging, any abortion messaging, any absenteeism messaging). The second row derives from a full model in which treatment status is defined according to the seven conditions in the randomization: VAW, Absenteeism, Abortion, VAW + Absenteeism, VAW + Abortion, Abortion + Absenteeism, Placebo.

C Sample Reweighting

C.1 Reweighting Method

As described in section A.5 above, both rounds of the experiment involved a second wave of follow-up sampling in which we over-sampled young men, as this demographic was deemed most likely to feature film festival attendees. Specifically, we only sampled additional respondents aged 18 - 35 years, and aimed for a 2/3 male-female ratio.

This scheme leads to an overrepresentation of young men in the final sample for which we did not originally specify any reweighting scheme. In the main results of this paper, we subset to respondents aged 18 - 50 and weight our regressions so that our inferences pertain to the population both samples represent (18 - 50 year-olds living in the catchment area of the video halls).

Since the sampling happens within clusters, we define weights at the cluster level. Let us denote by n_{j1}^{YM} and n_{j1}^{YW} the number of "young" (18-35 year old) men and women sampled in cluster j in the first wave of sampling, and by n_{j2}^{YM} and n_{j2}^{YW} the number of young men and women sampled in cluster j in the second, follow-up, wave of sampling. Let $n_{j1}^{\neg Y}$ and $n_{j2}^{\neg Y}$ denote the number of people (men and women) aged 36-50 sampled in cluster j in each round of sampling.

We then reweight each young man and each young woman in the sample by $w_j^{YM} = \frac{n_{j1}^{YM}}{n_{j1}^{YM}+n_{j2}^{YM}}$ and $w_j^{YW} = \frac{n_{j1}^{YW}}{n_{j1}^{YW}+n_{j2}^{YW}}$, respectively, and reweight each person who is aged 36-50 by $w_j^{\neg YM} = \frac{n_{j1}^{\neg YM}}{n_{j1}^{\neg YM}+n_{j2}^{\neg YM}} = \frac{n_{j1}^{\neg YM}}{n_{j1}^{\neg YM}} = 1$.

Suppose, for example, that a given cluster contains 20 young men, 20 young women and 60 not young people. In an initial sample of 10, we sample 2 young men and 2 young women, and 6 not young people. Then, we resample 10 more young men and 10 more young women. In that case, we have a final sample that has $12/30 = 40\%$ young men, which is well above the correct proportion of $20/100 = 20\%$. In this example, $n_{j1}^{YM} = 2$, $n_{j2}^{YM} = 10$, $n_{j1}^{\neg YM} = 8$, $n_{j2}^{\neg YM} = 0$. Thus, $w^{YM} = 2/(2+10) = 0.166667$. Applying these weights, we now correctly estimate that the proportion of young men in the cluster is $12w^{YM}/(12w^{YM} + 8w^{\neg YM}) = 20\%$.

C.2 Main results from paper without reweighting

	Dependent variable:			
	Index of willingness to take action to counter absenteeism			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
absenteeism	0.040^{***}	-0.001	0.005	0.001
	(0.013)	(0.009)	(0.009)	(0.007)
Control Mean	0.61	0.59	0.59	0.59
Vill. Means	0.63	0.6	0.58	0.59
Vill. SD	0.11	0.08	0.09	0.07
N Vill.	166	166	166	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,492	3,285	3,188	6,473
Adjusted R^2	0.026	0.044	0.055	0.049

Notes: *p<0.1; **p<0.05; ***p<0.01

	Dependent variable:			
	Index of willingness to take action to counter absenteeism			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
absenteeism	0.046***	0.003	0.017	−0.004
	(0.015)	(0.012)	(0.023)	(0.013)
Control Mean	0.64	0.63	0.54	0.54
Vill. Means	0.65	0.64	0.56	0.55
Vill. SD	0.11	0.1	0.2	0.09
N Vill.	165	166	143	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,017	1,716	475	1,569
Adjusted R^2	0.021	0.060	0.062	0.027
Notes:				$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

	Dependent variable:			
	Education is an important goal			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
absenteeism	0.056**	0.010	−0.014	−0.001
	(0.023)	(0.016)	(0.016)	(0.011)
Control Mean	0.44	0.43	0.43	0.43
Vill. Means	0.43	0.43	0.44	0.43
Vill. SD	0.19	0.12	0.13	0.08
N Vill.	166	166	166	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,492	3,285	3,188	6,473
Adjusted R^2	0.008	−0.001	0.0002	−0.0005
Notes:				$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

	Dependent variable:			
	Education is an important goal			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
absenteeism	0.040	−0.001	0.103**	0.023
	(0.029)	(0.023)	(0.043)	(0.023)
Control Mean	0.48	0.45	0.36	0.41
Vill. Means	0.47	0.45	0.34	0.41
Vill. SD	0.23	0.15	0.33	0.19
N Vill.	165	166	143	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,017	1,716	475	1,569
Adjusted R^2	0.0003	−0.004	0.013	0.008
Notes:				$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

	Dependent variable:			
	Index of willingness to take action to counter intimate partner violence			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
VAW	0.047^{***}	0.004	0.001	0.003
	(0.015)	(0.012)	(0.014)	(0.010)
Control Mean	0.38	0.38	0.38	0.38
Vill. Means	0.38	0.37	0.37	0.38
Vill. SD	0.09	0.07	0.09	0.06
N Vill.	110	110	110	110
Block FE	Yes	Yes	Yes	Yes
Observations	1,156	2,447	1,931	4,378
Adjusted R^2	0.013	0.005	0.013	0.007
Notes:				*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:			
	Index of willingness to take action to counter intimate partner violence			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
VAW	0.028	−0.002	0.104^{***}	0.010
	(0.019)	(0.014)	(0.027)	(0.018)
Control Mean	0.4	0.38	0.35	0.37
Vill. Means	0.4	0.38	0.35	0.37
Vill. SD	0.12	0.09	0.19	0.1
N Vill.	110	110	97	110
Block FE	Yes	Yes	Yes	Yes
Observations	797	1,253	359	1,194
Adjusted R^2	−0.002	0.010	0.070	−0.001
Notes:				*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:			
	Willingness to help someone suffering from post-abortion complications			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
abortion	0.044^{**}	−0.002	−0.006	−0.003
	(0.019)	(0.014)	(0.016)	(0.012)
Control Mean	0.82	0.8	0.79	0.79
Vill. Means	0.79	0.79	0.79	0.79
Vill. SD	0.17	0.13	0.13	0.11
N Vill.	166	166	166	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,492	3,285	3,188	6,473
Adjusted R^2	0.010	0.019	0.016	0.019
Notes:				*p<0.1; **p<0.05; ***p<0.01

	Dependent variable:			
	Willingness to help someone suffering from post-abortion complications			
	Reached Dir. - Men	Reached Ind. - Men	Reached Dir. - Women	Reached Ind. - Women
	(1)	(2)	(3)	(4)
abortion	0.012	-0.003	0.129^{***}	-0.007
	(0.020)	(0.017)	(0.040)	(0.022)
Control Mean	0.86	0.84	0.73	0.75
Vill. Means	0.84	0.82	0.67	0.74
Vill. SD	0.18	0.15	0.37	0.2
N Vill.	165	166	143	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,017	1,716	475	1,569
Adjusted R^2	-0.00002	0.022	0.049	0.026
Notes:				*p<0.1; **p<0.05; ***p<0.01

D Results as Pre-Specified

D.1 Original Definition of Principal Strata

In the PAP, we pre-specified four principal strata. We used the term "compliers" to refer to the stratum that we now call "directly reached" and the term "indirect compliers" to refer to the stratum that we now call "indirectly reached." Within the stratum that we now call "not reached," we originally planned to distinguish between "apprised never-takers" and "never-takers." "Apprised never-takers" are those who neither attended nor had friends or family members who attended but who were nevertheless *aware* of the film festival. "Never-takers" are those who did not attend, had no friends or family who attended and were unaware of the film festival. Since we have not found the distinction between "apprised never-takers" and "never-takers" to be a theoretically useful or empirically relevant distinction, we have combined these two strata in the main text. In section D below, we make use of the original strata and show that doing so makes no difference to the results.

D.2 Main Results

	Dependent variable:				
	Index of willingness to take action to counter absenteeism				
	Compliers	Indirect Compliers	Apprised Never-Takers	Never-Takers	All Non-Compliers
	(1)	(2)	(3)	(4)	(5)
absenteeism	0.040***	-0.001	0.015	-0.003	0.001
	(0.013)	(0.009)	(0.014)	(0.012)	(0.007)
Control Mean	0.61	0.59	0.59	0.6	0.59
Vill. Means	0.63	0.6	0.58	0.58	0.59
Vill. SD	0.11	0.08	0.12	0.11	0.07
N Vill.	166	166	166	165	166
Block FE	Yes	Yes	Yes	Yes	Yes
Observations	1,492	3,285	1,484	1,704	6,473
Adjusted R^2	0.026	0.044	0.054	0.067	0.049
Notes:					*p<0.1; **p<0.05; ***p<0.01

Table 16: Direct effects and spillovers from absenteeism messages among respondents in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:				
	Education is an important goal				
	Compliers	Indirect Compliers	Apprised Never-Takers	Never-Takers	All Non-Compliers
	(1)	(2)	(3)	(4)	(5)
absenteeism	0.056**	0.010	−0.035	0.001	−0.001
	(0.023)	(0.016)	(0.023)	(0.024)	(0.011)
Control Mean	0.44	0.43	0.45	0.42	0.43
Vill. Means	0.43	0.43	0.44	0.43	0.43
Vill. SD	0.19	0.12	0.2	0.19	0.08
N Vill.	166	166	166	165	166
Block FE	Yes	Yes	Yes	Yes	Yes
Observations	1,492	3,285	1,484	1,704	6,473
Adjusted R^2	0.008	−0.001	0.0002	0.007	−0.0005
Notes:					*p<0.1; **p<0.05; ***p<0.01

Table 17: Direct effects and spillovers from absenteeism messages among all respondents in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed *p*-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:				
	Index of willingness to take action to counter intimate partner violence				
	Compliers	Indirect Compliers	Apprised Never-Takers	Never-Takers	All Non-Compliers
	(1)	(2)	(3)	(4)	(5)
VAW	0.047***	0.004	−0.008	0.009	0.003
	(0.015)	(0.012)	(0.018)	(0.017)	(0.010)
Control Mean	0.38	0.38	0.38	0.37	0.38
Vill. Means	0.38	0.37	0.37	0.37	0.38
Vill. SD	0.09	0.07	0.12	0.12	0.06
N Vill.	110	110	110	109	110
Block FE	Yes	Yes	Yes	Yes	Yes
Observations	1,156	2,447	953	978	4,378
Adjusted R^2	0.013	0.005	0.023	0.009	0.007
Notes:					*p<0.1; **p<0.05; ***p<0.01

Table 18: Direct effects and spillovers from anti-VAW messages among all respondents in endline surveys following 2016 festival.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed *p*-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:				
	Willingness to help someone suffering from post-abortion complications				
	Compliers	Indirect Compliers	Apprised Never-Takers	Never-Takers	All Non-Compliers
	(1)	(2)	(3)	(4)	(5)
abortion	0.043**	−0.002	−0.030	0.017	−0.003
	(0.019)	(0.014)	(0.022)	(0.021)	(0.012)
Control Mean	0.82	0.8	0.8	0.78	0.79
Vill. Means	0.79	0.79	0.8	0.78	0.79
Vill. SD	0.17	0.13	0.16	0.18	0.11
N Vill.	166	166	166	165	166
Block FE	Yes	Yes	Yes	Yes	Yes
Observations	1,492	3,285	1,484	1,704	6,473
Adjusted R^2	0.009	0.019	0.036	0.010	0.019
Notes:					*p<0.1; **p<0.05; ***p<0.01

Table 19: Direct effects and spillovers from anti-abortion stigma messages among all respondents in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

D.3 Results by Gender

	Dependent variable:			
	Index of willingness to take action to counter absenteeism			
	Compliers - Men	Indirect Compliers - Men	Compliers - Women	Indirect Compliers - Women
	(1)	(2)	(3)	(4)
absenteeism	0.046***	0.003	0.017	−0.003
	(0.015)	(0.012)	(0.023)	(0.013)
Control Mean	0.64	0.63	0.54	0.54
Vill. Means	0.65	0.64	0.56	0.55
Vill. SD	0.11	0.1	0.2	0.09
N Vill.	165	166	143	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,017	1,716	475	1,569
Adjusted R^2	0.021	0.060	0.062	0.027
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 20: Direct effects and spillovers from absenteeism messages among men and women in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Education is an important goal			
	Compliers - Men	Indirect Compliers - Men	Compliers - Women	Indirect Compliers - Women
	(1)	(2)	(3)	(4)
absenteeism	0.040	-0.001	0.103^{**}	0.023
	(0.029)	(0.023)	(0.043)	(0.023)
Control Mean	0.48	0.45	0.36	0.41
Vill. Means	0.47	0.45	0.34	0.41
Vill. SD	0.23	0.15	0.33	0.19
N Vill.	165	166	143	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,017	1,716	475	1,569
Adjusted R^2	0.0003	-0.004	0.013	0.008
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 21: Direct effects and spillovers from absenteeism messages among men and women in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Index of willingness to take action to counter intimate partner violence			
	Compliers - Men	Indirect Compliers - Men	Compliers - Women	Indirect Compliers - Women
	(1)	(2)	(3)	(4)
IPV	0.028	-0.002	0.104^{***}	0.010
	(0.019)	(0.014)	(0.027)	(0.018)
Control Mean	0.4	0.38	0.35	0.37
Vill. Means	0.4	0.38	0.35	0.37
Vill. SD	0.12	0.09	0.19	0.1
N Vill.	110	110	97	110
Block FE	Yes	Yes	Yes	Yes
Observations	797	1,253	359	1,194
Adjusted R^2	-0.002	0.010	0.070	-0.001
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 22: Direct effects and spillovers from anti-VAW messages among men and women in endline surveys following 2016 festival.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Willingness to help someone suffering from post-abortion complications			
	Compliers - Men	Indirect Compliers - Men	Compliers - Women	Indirect Compliers - Women
	(1)	(2)	(3)	(4)
abortion	0.012	−0.003	0.124***	−0.007
	(0.020)	(0.017)	(0.040)	(0.022)
Control Mean	0.86	0.84	0.73	0.75
Vill. Means	0.84	0.82	0.67	0.74
Vill. SD	0.18	0.15	0.36	0.2
N Vill.	165	166	143	166
Block FE	Yes	Yes	Yes	Yes
Observations	1,017	1,716	475	1,569
Adjusted R^2	−0.00002	0.022	0.043	0.026
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 23: Direct effects and spillovers from anti-abortion stigma messages among men and women in endline surveys following 2015 and 2016 festivals.

Coefficients estimated using the pre-registered least-squares regression, conditioning on block fixed-effects and an indicator for resampling. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

D.4 Balance Tables

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
protestant	0.24	0.28	0.14	0.13	0.18	0.16	0.19	0.00
english_christian	0.03	0.05	0.07	0.07	0.06	0.05	0.16	0.01
minority_lang	0.07	0.05	0.09	0.03	0.06	0.03	0.02	0.04
munyankole	0.06	0.08	0.08	0.04	0.08	0.16	0.11	0.05
domestic_work	0.02	0.05	0.07	0.03	0.06	0.02	0.04	0.05
other_person	0.04	0.08	0.09	0.12	0.06	0.03	0.09	0.07
minority_tribe	0.20	0.21	0.13	0.13	0.17	0.08	0.09	0.07
atheist	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.10
misc_floor	0.09	0.10	0.20	0.12	0.16	0.12	0.18	0.11
rooms	2.48	2.44	2.20	2.51	2.71	2.54	2.41	0.13
misc_light	0.12	0.14	0.18	0.09	0.17	0.12	0.12	0.14
munyarwanda	0.07	0.04	0.13	0.11	0.09	0.13	0.08	0.14
munyoro	0.08	0.04	0.05	0.08	0.03	0.03	0.07	0.15
highest_grade	7.28	7.77	6.67	7.06	7.20	6.53	7.08	0.16
write_only	0.05	0.03	0.06	0.04	0.02	0.04	0.02	0.17
holy_spirit	0.08	0.08	0.14	0.14	0.13	0.15	0.09	0.18
cellphone	0.85	0.75	0.75	0.85	0.79	0.79	0.78	0.23
luganda_lang	0.89	0.93	0.86	0.88	0.85	0.83	0.94	0.23
catholic	0.41	0.38	0.45	0.46	0.43	0.50	0.36	0.24
university	0.05	0.04	0.03	0.05	0.06	0.02	0.02	0.24
write_and_read	0.81	0.88	0.79	0.84	0.85	0.82	0.88	0.26
other_work	0.07	0.09	0.05	0.04	0.04	0.05	0.03	0.28
runyannkole_lang	0.02	0.02	0.04	0.02	0.04	0.11	0.04	0.29
chair	0.80	0.85	0.79	0.88	0.85	0.81	0.84	0.30
age	28.46	28.48	28.20	28.63	29.71	29.98	28.47	0.34
solar_light	0.27	0.29	0.28	0.39	0.34	0.32	0.24	0.36
female	0.25	0.32	0.35	0.28	0.33	0.38	0.30	0.36
education_work	0.06	0.06	0.02	0.03	0.02	0.04	0.05	0.39
mukiga	0.02	0.02	0.03	0.02	0.08	0.03	0.03	0.40
separated	0.11	0.13	0.12	0.09	0.09	0.14	0.11	0.45
read_only	0.04	0.02	0.04	0.03	0.02	0.01	0.02	0.45
misc_wall	0.03	0.04	0.07	0.01	0.03	0.06	0.06	0.46
manual_work	0.10	0.06	0.07	0.07	0.09	0.11	0.07	0.48
same_village	0.52	0.44	0.43	0.48	0.52	0.45	0.47	0.51
tv	0.26	0.16	0.19	0.25	0.18	0.17	0.19	0.52
not_married	0.23	0.24	0.18	0.22	0.18	0.25	0.21	0.52
transport_work	0.04	0.04	0.04	0.06	0.03	0.06	0.09	0.52
motor_cycle	0.22	0.22	0.22	0.27	0.24	0.20	0.19	0.54
mobile_phone_use	3.50	3.23	3.14	3.37	3.25	3.22	3.20	0.54
minority_religion	0.00	0.00	0.01	0.00	0.01	0.00	0.01	0.55
pray_private	7.91	8.14	8.18	7.97	7.97	7.97	7.81	0.55
electric_light	0.24	0.12	0.13	0.16	0.10	0.13	0.13	0.55

cement_floor	0.63	0.55	0.48	0.56	0.47	0.51	0.51	0.56
kerosene_light	0.24	0.33	0.31	0.26	0.31	0.29	0.37	0.56
travel_big_city	0.76	0.73	0.75	0.71	0.76	0.81	0.76	0.57
fumbira_lang	0.02	0.01	0.01	0.07	0.05	0.03	0.01	0.57
stone_wall	0.04	0.03	0.03	0.05	0.03	0.06	0.04	0.59
married	0.33	0.35	0.33	0.33	0.39	0.30	0.26	0.60
household_other	0.15	0.13	0.12	0.09	0.09	0.10	0.12	0.61
mufumbira_tribe	0.01	0.02	0.02	0.08	0.03	0.04	0.01	0.61
living_as_married	0.38	0.33	0.41	0.38	0.37	0.35	0.44	0.63
radio	0.81	0.85	0.79	0.84	0.85	0.82	0.83	0.64
number_children	3.25	3.05	3.27	3.13	3.77	3.51	3.32	0.67
single_hut	0.60	0.59	0.61	0.62	0.69	0.62	0.61	0.69
brick_wall	0.63	0.64	0.58	0.62	0.55	0.53	0.55	0.69
illiterate	0.10	0.07	0.11	0.09	0.11	0.12	0.08	0.69
christian_only	0.02	0.03	0.03	0.03	0.02	0.03	0.01	0.71
sofa	0.22	0.16	0.15	0.22	0.16	0.20	0.21	0.72
hospitality_work	0.06	0.07	0.03	0.04	0.04	0.06	0.04	0.72
several_huts	0.12	0.10	0.08	0.10	0.07	0.11	0.09	0.73
earth_floor	0.28	0.35	0.32	0.31	0.37	0.37	0.31	0.74
mud_wall	0.23	0.23	0.23	0.22	0.31	0.24	0.27	0.77
cement_wall	0.07	0.06	0.08	0.09	0.08	0.11	0.08	0.80
charcoal_fuel	0.46	0.46	0.36	0.36	0.36	0.35	0.40	0.80
firewood_fuel	0.51	0.51	0.60	0.60	0.62	0.62	0.58	0.80
share_house	0.27	0.31	0.31	0.27	0.24	0.27	0.31	0.82
retail_work	0.12	0.09	0.12	0.09	0.09	0.08	0.07	0.82
no_work	0.04	0.06	0.03	0.04	0.03	0.05	0.04	0.83
mutooro	0.01	0.02	0.01	0.00	0.02	0.01	0.02	0.84
agriculture_work	0.53	0.54	0.58	0.62	0.63	0.57	0.63	0.84
household_children	2.28	2.21	2.04	2.32	2.37	2.17	2.16	0.85
religious_service	1.89	2.02	1.55	1.14	1.79	1.34	1.52	0.86
household_spouse	0.21	0.23	0.27	0.26	0.26	0.25	0.22	0.86
living_conditions	0.04	0.01	0.04	0.08	0.04	0.03	-0.06	0.86
living_conditions_compared	1.95	2.03	1.96	2.04	2.06	2.01	1.95	0.88
members	4.44	4.31	4.17	4.40	4.51	4.19	4.23	0.88
household_younger	2.96	2.88	2.71	2.94	3.07	2.80	2.75	0.88
muslim	0.21	0.18	0.16	0.17	0.17	0.12	0.19	0.89
household_older	0.47	0.43	0.46	0.44	0.44	0.39	0.48	0.91
misc_fuel	0.04	0.03	0.03	0.04	0.02	0.03	0.03	0.92
muganda_tribe	0.55	0.58	0.54	0.55	0.51	0.52	0.60	0.92
day	1.25	1.19	1.21	1.22	1.25	1.24	1.25	0.96
household_head	0.64	0.64	0.62	0.64	0.65	0.65	0.66	0.99

Table 24: Balance on covariates among Compliers

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
chair	0.85	0.85	0.85	0.90	0.87	0.85	0.79	0.01
living_conditions_compared	2.20	2.21	2.12	2.21	2.15	2.10	2.07	0.04
education_work	0.03	0.05	0.03	0.05	0.02	0.02	0.03	0.04
transport_work	0.04	0.04	0.03	0.03	0.02	0.05	0.01	0.07
stone_wall	0.03	0.01	0.04	0.03	0.03	0.06	0.05	0.09
living_conditions	0.13	0.10	0.15	0.19	0.09	0.08	0.07	0.11
catholic	0.40	0.36	0.35	0.45	0.42	0.44	0.36	0.12
cellphone	0.83	0.82	0.78	0.82	0.78	0.82	0.75	0.13
cement_wall	0.12	0.10	0.10	0.13	0.08	0.14	0.12	0.17
minority_lang	0.06	0.03	0.09	0.03	0.05	0.03	0.04	0.17
muslim	0.20	0.22	0.20	0.14	0.16	0.11	0.16	0.18
fumbira_lang	0.02	0.02	0.01	0.06	0.04	0.02	0.00	0.24
religious_service	2.08	1.95	1.97	1.27	1.57	1.50	1.78	0.27
highest_grade	6.95	7.25	7.15	7.07	6.54	6.70	6.74	0.28
minority_religion	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.29
female	0.47	0.49	0.44	0.47	0.51	0.47	0.48	0.29
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.30
household_head	0.58	0.56	0.60	0.59	0.55	0.61	0.58	0.32
brick_wall	0.59	0.65	0.59	0.60	0.61	0.53	0.56	0.34
age	32.32	31.75	31.54	32.03	32.41	32.60	31.56	0.34
mukiga	0.04	0.02	0.04	0.01	0.04	0.04	0.02	0.35
runyannkole_lang	0.03	0.02	0.06	0.06	0.05	0.11	0.05	0.36
mufumbira_tribe	0.01	0.02	0.01	0.05	0.04	0.03	0.01	0.36
pray_private	8.10	8.18	8.05	8.17	8.15	8.14	7.94	0.37
household_spouse	0.33	0.35	0.34	0.33	0.38	0.32	0.35	0.37
household_children	2.73	2.79	2.55	2.64	2.77	2.50	2.80	0.38
day	1.30	1.20	1.22	1.23	1.24	1.25	1.19	0.40
luganda_lang	0.90	0.93	0.84	0.85	0.86	0.84	0.91	0.40
members	4.90	5.02	4.68	4.85	4.93	4.64	4.99	0.41
minority_tribe	0.17	0.16	0.12	0.15	0.12	0.10	0.14	0.41
other_work	0.06	0.06	0.04	0.05	0.07	0.04	0.05	0.44
single_hut	0.63	0.58	0.58	0.60	0.62	0.56	0.64	0.45
radio	0.82	0.81	0.85	0.82	0.83	0.79	0.79	0.45
mobile_phone_use	3.36	3.34	3.28	3.35	3.27	3.36	3.13	0.45
travel_big_city	0.72	0.68	0.73	0.68	0.72	0.74	0.69	0.46
household_older	0.49	0.55	0.46	0.48	0.53	0.46	0.49	0.49
english_christian	0.05	0.07	0.08	0.07	0.08	0.06	0.09	0.50
tv	0.28	0.30	0.23	0.27	0.24	0.24	0.21	0.52
separated	0.10	0.11	0.09	0.11	0.13	0.14	0.13	0.53
misc_floor	0.09	0.10	0.12	0.11	0.12	0.10	0.14	0.54
cement_floor	0.64	0.61	0.58	0.59	0.50	0.58	0.54	0.54
mutooro	0.01	0.01	0.02	0.00	0.01	0.01	0.02	0.54
misc_fuel	0.01	0.02	0.03	0.03	0.03	0.03	0.02	0.55
household_younger	3.41	3.47	3.20	3.37	3.40	3.18	3.50	0.55

read_only	0.03	0.03	0.02	0.03	0.04	0.03	0.03	0.55
munyoro	0.07	0.06	0.06	0.05	0.04	0.03	0.04	0.58
manual_work	0.09	0.06	0.08	0.10	0.09	0.06	0.07	0.59
share_house	0.27	0.32	0.31	0.29	0.26	0.31	0.26	0.61
rooms	2.63	2.74	2.70	2.87	2.74	2.64	2.70	0.61
write_and_read	0.82	0.84	0.81	0.81	0.78	0.81	0.81	0.61
munyarwanda	0.08	0.07	0.07	0.10	0.11	0.09	0.06	0.64
domestic_work	0.03	0.05	0.04	0.06	0.06	0.05	0.05	0.65
household_other	0.08	0.09	0.05	0.07	0.07	0.07	0.07	0.67
charcoal_fuel	0.43	0.42	0.43	0.37	0.36	0.35	0.36	0.68
married	0.38	0.43	0.39	0.37	0.40	0.37	0.35	0.68
university	0.04	0.05	0.06	0.05	0.04	0.04	0.02	0.68
electric_light	0.24	0.24	0.18	0.23	0.18	0.17	0.15	0.69
munyankole	0.08	0.09	0.12	0.10	0.09	0.16	0.11	0.69
earth_floor	0.27	0.29	0.30	0.31	0.38	0.32	0.31	0.70
not_married	0.14	0.15	0.15	0.16	0.12	0.16	0.13	0.72
living_as_married	0.40	0.34	0.40	0.38	0.36	0.37	0.42	0.75
same_village	0.42	0.40	0.37	0.42	0.37	0.38	0.43	0.75
several_huts	0.10	0.10	0.10	0.12	0.11	0.13	0.10	0.76
other_person	0.11	0.11	0.11	0.12	0.10	0.09	0.10	0.76
retail_work	0.14	0.14	0.13	0.10	0.14	0.12	0.13	0.76
misc_wall	0.02	0.02	0.05	0.02	0.03	0.03	0.04	0.77
misc_light	0.12	0.11	0.14	0.12	0.11	0.11	0.13	0.77
firewood_fuel	0.55	0.57	0.54	0.60	0.61	0.62	0.63	0.77
christian_only	0.02	0.03	0.03	0.02	0.03	0.02	0.03	0.79
protestant	0.20	0.20	0.20	0.18	0.19	0.22	0.23	0.80
solar_light	0.28	0.26	0.32	0.30	0.32	0.35	0.30	0.81
illiterate	0.11	0.09	0.12	0.11	0.12	0.12	0.10	0.81
number_children	4.39	4.24	4.31	4.28	4.57	4.32	4.49	0.84
sofa	0.27	0.28	0.24	0.26	0.24	0.22	0.25	0.86
hospitality_work	0.05	0.04	0.06	0.06	0.05	0.06	0.05	0.87
write_only	0.04	0.04	0.05	0.05	0.05	0.04	0.05	0.89
muganda_tribe	0.53	0.56	0.56	0.53	0.54	0.54	0.60	0.89
agriculture_work	0.54	0.55	0.59	0.55	0.55	0.60	0.60	0.94
motor_cycle	0.28	0.29	0.26	0.29	0.30	0.27	0.29	0.95
no_work	0.05	0.03	0.05	0.05	0.05	0.05	0.04	0.95
mud_wall	0.24	0.22	0.22	0.22	0.24	0.25	0.24	0.97
kerosene_light	0.26	0.28	0.26	0.27	0.29	0.26	0.29	0.98
holy_spirit	0.12	0.13	0.13	0.14	0.13	0.14	0.12	0.99

Table 25: Balance on covariates among Indirect Compliers

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
married	0.47	0.44	0.32	0.37	0.51	0.35	0.49	0.00
separated	0.09	0.11	0.10	0.21	0.08	0.15	0.10	0.01
household_other	0.04	0.09	0.06	0.09	0.04	0.10	0.02	0.03
read_only	0.02	0.02	0.06	0.04	0.04	0.01	0.01	0.03
munyarwanda	0.08	0.09	0.06	0.10	0.09	0.07	0.16	0.03
household_older	0.51	0.71	0.57	0.51	0.58	0.59	0.54	0.04
education_work	0.07	0.04	0.02	0.02	0.01	0.05	0.05	0.04
highest_grade	6.90	7.72	6.80	6.50	5.96	7.48	6.51	0.05
number_children	4.71	4.15	4.53	4.39	5.30	4.10	4.90	0.07
university	0.08	0.07	0.04	0.04	0.02	0.09	0.06	0.10
mutooro	0.02	0.01	0.06	0.00	0.00	0.01	0.02	0.10
living_conditions_compared	2.26	2.37	2.15	2.31	2.19	2.23	2.35	0.11
luganda_lang	0.94	0.94	0.84	0.89	0.83	0.82	0.91	0.14
household_spouse	0.45	0.48	0.50	0.40	0.50	0.46	0.54	0.15
living_as_married	0.35	0.37	0.48	0.33	0.32	0.39	0.37	0.15
not_married	0.10	0.12	0.11	0.13	0.09	0.14	0.05	0.16
chair	0.83	0.88	0.77	0.83	0.83	0.86	0.84	0.17
retail_work	0.08	0.18	0.14	0.15	0.13	0.18	0.11	0.17
living_conditions	0.14	0.15	0.00	0.20	0.05	0.06	0.05	0.17
motor_cycle	0.20	0.31	0.30	0.28	0.26	0.25	0.30	0.19
share_house	0.29	0.38	0.34	0.28	0.24	0.40	0.31	0.20
misc_wall	0.01	0.01	0.04	0.00	0.05	0.01	0.04	0.20
age	33.17	32.81	31.91	33.68	33.65	32.30	34.37	0.23
munyoro	0.07	0.03	0.06	0.07	0.03	0.01	0.03	0.23
other_person	0.15	0.09	0.11	0.11	0.10	0.06	0.11	0.24
radio	0.77	0.85	0.75	0.82	0.78	0.80	0.82	0.25
household_head	0.51	0.43	0.43	0.52	0.45	0.44	0.44	0.25
minority_lang	0.04	0.04	0.10	0.02	0.06	0.04	0.03	0.25
single_hut	0.57	0.52	0.58	0.60	0.67	0.53	0.58	0.27
misc_floor	0.08	0.09	0.14	0.09	0.07	0.07	0.11	0.27
fumbira_lang	0.01	0.01	0.01	0.03	0.04	0.04	0.02	0.27
agriculture_work	0.55	0.46	0.60	0.57	0.52	0.44	0.57	0.29
mukiga	0.03	0.03	0.05	0.02	0.06	0.04	0.02	0.30
rooms	2.62	2.76	2.59	2.92	2.89	2.54	2.82	0.32
transport_work	0.02	0.03	0.02	0.01	0.04	0.01	0.03	0.32
earth_floor	0.25	0.20	0.28	0.29	0.36	0.33	0.26	0.34
other_work	0.06	0.07	0.02	0.05	0.08	0.04	0.06	0.34
mufumbira_tribe	0.03	0.01	0.00	0.02	0.03	0.04	0.01	0.36
muslim	0.18	0.23	0.17	0.12	0.22	0.13	0.18	0.41
munyankole	0.04	0.05	0.08	0.10	0.12	0.12	0.09	0.42
illiterate	0.18	0.10	0.14	0.11	0.16	0.13	0.18	0.45
runyannkole_lang	0.01	0.01	0.05	0.07	0.06	0.10	0.04	0.48
several_huts	0.14	0.09	0.08	0.11	0.08	0.08	0.12	0.49
domestic_work	0.11	0.08	0.08	0.09	0.07	0.11	0.10	0.51
minority_religion	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.55
travel_big_city	0.58	0.64	0.60	0.57	0.57	0.66	0.66	0.56

hospitality_work	0.04	0.06	0.04	0.07	0.06	0.08	0.03	0.57
mobile_phone_use	3.18	3.27	3.12	3.09	3.10	3.39	3.13	0.57
english_christian	0.09	0.04	0.06	0.09	0.08	0.06	0.08	0.59
kerosene_light	0.24	0.23	0.31	0.28	0.28	0.21	0.28	0.59
write_and_read	0.74	0.83	0.74	0.81	0.76	0.79	0.78	0.60
cement_wall	0.15	0.17	0.10	0.17	0.11	0.14	0.13	0.62
cement_floor	0.67	0.71	0.58	0.62	0.56	0.60	0.63	0.63
write_only	0.05	0.04	0.07	0.03	0.04	0.07	0.04	0.64
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.66
household_children	2.89	2.65	2.75	2.82	3.06	2.62	2.74	0.66
no_work	0.02	0.06	0.03	0.04	0.04	0.06	0.02	0.66
day	1.21	1.16	1.24	1.27	1.20	1.28	1.19	0.68
stone_wall	0.03	0.05	0.05	0.04	0.04	0.05	0.02	0.73
manual_work	0.07	0.06	0.07	0.04	0.06	0.09	0.06	0.73
same_village	0.31	0.30	0.28	0.36	0.28	0.31	0.31	0.74
protestant	0.17	0.17	0.21	0.17	0.17	0.20	0.19	0.75
minority_tribe	0.18	0.18	0.13	0.13	0.18	0.15	0.13	0.75
cellphone	0.78	0.81	0.82	0.76	0.77	0.80	0.77	0.76
household_younger	3.50	3.33	3.29	3.45	3.61	3.14	3.40	0.77
members	5.00	5.04	4.87	4.97	5.20	4.72	4.94	0.78
muganda_tribe	0.55	0.60	0.56	0.55	0.48	0.55	0.55	0.81
tv	0.31	0.37	0.28	0.31	0.28	0.31	0.25	0.82
catholic	0.37	0.36	0.36	0.45	0.36	0.40	0.35	0.84
female	0.61	0.64	0.66	0.60	0.63	0.64	0.65	0.84
mud_wall	0.20	0.13	0.24	0.16	0.21	0.19	0.18	0.86
firewood_fuel	0.54	0.55	0.55	0.60	0.59	0.56	0.63	0.86
electric_light	0.27	0.37	0.24	0.30	0.29	0.26	0.23	0.88
charcoal_fuel	0.45	0.45	0.44	0.39	0.40	0.42	0.37	0.88
holy_spirit	0.15	0.17	0.17	0.13	0.13	0.17	0.16	0.90
pray_private	8.24	8.21	8.19	8.20	8.00	8.09	8.20	0.90
misc_light	0.13	0.14	0.14	0.11	0.13	0.14	0.11	0.91
misc_fuel	0.00	0.00	0.01	0.00	0.01	0.01	0.00	0.91
religious_service	1.81	1.94	1.54	1.42	1.80	1.49	1.91	0.92
sofa	0.29	0.27	0.26	0.25	0.24	0.25	0.31	0.92
christian_only	0.03	0.02	0.02	0.03	0.02	0.02	0.03	0.93
solar_light	0.27	0.21	0.26	0.23	0.25	0.27	0.28	0.97
brick_wall	0.61	0.64	0.57	0.63	0.58	0.61	0.63	0.98

Table 26: Balance on covariates among Apprised Never-Takers

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
motor_cycle	0.30	0.24	0.24	0.24	0.23	0.29	0.19	0.02
household_older	0.66	0.53	0.57	0.59	0.48	0.57	0.48	0.04
write_and_read	0.71	0.81	0.77	0.76	0.65	0.79	0.76	0.04
domestic_work	0.09	0.12	0.12	0.04	0.06	0.09	0.08	0.05
holy_spirit	0.18	0.14	0.16	0.17	0.16	0.22	0.12	0.06
not_married	0.09	0.12	0.15	0.12	0.15	0.18	0.10	0.06
cement_wall	0.16	0.13	0.16	0.16	0.14	0.24	0.15	0.07
travel_big_city	0.57	0.67	0.60	0.60	0.55	0.66	0.69	0.07
minority_lang	0.07	0.03	0.12	0.05	0.08	0.03	0.06	0.07
radio	0.79	0.83	0.83	0.77	0.70	0.79	0.79	0.09
female	0.58	0.53	0.60	0.60	0.51	0.53	0.51	0.13
living_conditions	0.09	0.13	0.11	0.09	-0.06	0.10	0.03	0.13
read_only	0.05	0.03	0.01	0.03	0.05	0.05	0.05	0.14
university	0.08	0.06	0.07	0.06	0.06	0.12	0.08	0.15
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.16
misc_light	0.15	0.17	0.17	0.16	0.20	0.12	0.17	0.19
write_only	0.06	0.04	0.05	0.05	0.07	0.02	0.05	0.19
rooms	2.73	2.64	2.64	2.92	2.56	2.80	2.78	0.22
cement_floor	0.61	0.73	0.66	0.63	0.52	0.72	0.60	0.23
pray_private	7.99	8.21	8.15	8.44	8.27	8.21	8.14	0.24
living_conditions_compared	2.18	2.26	2.27	2.24	2.05	2.27	2.14	0.24
stone_wall	0.02	0.04	0.07	0.04	0.03	0.04	0.03	0.25
brick_wall	0.53	0.64	0.51	0.58	0.53	0.52	0.59	0.25
highest_grade	6.99	7.22	6.93	6.76	5.95	7.69	6.97	0.25
illiterate	0.17	0.12	0.17	0.16	0.23	0.14	0.14	0.27
single_hut	0.62	0.52	0.64	0.59	0.68	0.58	0.62	0.29
luganda_lang	0.88	0.91	0.80	0.88	0.82	0.87	0.88	0.30
agriculture_work	0.50	0.50	0.56	0.63	0.60	0.47	0.58	0.30
household_children	2.88	2.66	2.42	2.92	2.71	2.62	2.70	0.33
catholic	0.36	0.31	0.34	0.37	0.42	0.33	0.33	0.34
household_head	0.49	0.56	0.48	0.52	0.59	0.54	0.54	0.34
runyannkole_lang	0.01	0.03	0.05	0.04	0.06	0.03	0.05	0.34
mukiga	0.03	0.03	0.04	0.01	0.06	0.02	0.03	0.35
manual_work	0.07	0.03	0.06	0.05	0.07	0.05	0.07	0.35
electric_light	0.23	0.27	0.26	0.26	0.17	0.41	0.23	0.36
mutooro	0.03	0.00	0.03	0.01	0.01	0.01	0.02	0.36
number_children	5.06	4.83	4.61	5.16	4.96	4.42	4.69	0.37
misc_floor	0.10	0.09	0.09	0.13	0.15	0.07	0.13	0.39
munyarwanda	0.10	0.11	0.09	0.08	0.11	0.13	0.05	0.39
household_spouse	0.43	0.34	0.43	0.40	0.34	0.36	0.40	0.43
several_huts	0.11	0.11	0.11	0.14	0.08	0.09	0.08	0.44
share_house	0.27	0.37	0.25	0.27	0.24	0.33	0.29	0.44
protestant	0.16	0.23	0.20	0.15	0.18	0.17	0.19	0.45
day	1.20	1.29	1.28	1.25	1.23	1.22	1.22	0.45
fumbira_lang	0.03	0.04	0.03	0.02	0.04	0.06	0.00	0.45
members	5.18	4.92	4.71	5.20	4.93	4.97	4.95	0.48
munyoro	0.07	0.03	0.07	0.07	0.07	0.02	0.05	0.48
earth_floor	0.29	0.18	0.26	0.24	0.33	0.21	0.26	0.51

minority_religion	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.54
other_person	0.09	0.06	0.08	0.11	0.10	0.07	0.08	0.54
sofa	0.36	0.30	0.25	0.28	0.24	0.34	0.31	0.55
household_other	0.09	0.11	0.09	0.08	0.07	0.10	0.06	0.55
kerosene_light	0.21	0.30	0.25	0.28	0.26	0.21	0.27	0.56
mufumbira_tribe	0.03	0.01	0.02	0.04	0.04	0.05	0.03	0.56
other_work	0.07	0.05	0.05	0.03	0.06	0.09	0.05	0.56
english_christian	0.05	0.08	0.09	0.08	0.07	0.06	0.10	0.57
chair	0.83	0.80	0.86	0.86	0.83	0.82	0.79	0.57
age	32.88	33.30	33.01	33.88	34.46	34.13	35.18	0.59
hospitality_work	0.04	0.07	0.03	0.04	0.04	0.04	0.03	0.59
tv	0.32	0.27	0.33	0.28	0.23	0.39	0.29	0.60
muganda_tribe	0.48	0.55	0.48	0.51	0.46	0.55	0.58	0.60
mobile_phone_use	3.20	3.39	3.27	3.08	3.08	3.45	3.17	0.60
charcoal_fuel	0.43	0.49	0.44	0.37	0.33	0.47	0.40	0.62
education_work	0.07	0.05	0.05	0.04	0.04	0.07	0.05	0.62
munyankole	0.07	0.08	0.11	0.08	0.10	0.06	0.07	0.66
household_younger	3.51	3.38	3.14	3.61	3.45	3.41	3.47	0.67
firewood_fuel	0.57	0.50	0.54	0.61	0.65	0.51	0.57	0.69
cellphone	0.77	0.82	0.83	0.75	0.75	0.85	0.79	0.70
transport_work	0.02	0.03	0.02	0.03	0.02	0.04	0.02	0.71
mud_wall	0.25	0.17	0.23	0.21	0.27	0.17	0.19	0.73
minority_tribe	0.20	0.21	0.17	0.20	0.16	0.17	0.18	0.75
muslim	0.20	0.19	0.15	0.18	0.13	0.17	0.18	0.76
solar_light	0.33	0.22	0.26	0.24	0.25	0.21	0.25	0.79
married	0.43	0.37	0.37	0.40	0.40	0.42	0.44	0.82
misc_fuel	0.00	0.01	0.02	0.02	0.02	0.02	0.03	0.85
separated	0.10	0.15	0.12	0.12	0.14	0.15	0.11	0.87
misc_wall	0.03	0.03	0.02	0.01	0.04	0.03	0.03	0.89
no_work	0.03	0.05	0.03	0.03	0.03	0.03	0.03	0.92
christian_only	0.04	0.06	0.06	0.04	0.04	0.04	0.07	0.94
religious_service	1.99	1.82	1.50	1.78	1.46	2.05	1.78	0.94
living_as_married	0.38	0.39	0.42	0.34	0.36	0.32	0.38	0.96
same_village	0.33	0.33	0.30	0.34	0.31	0.29	0.33	0.98
retail_work	0.13	0.15	0.11	0.12	0.12	0.13	0.11	0.99

Table 27: Balance on covariates among Never-Takers

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
living_conditions	0.12	0.12	0.11	0.17	0.04	0.08	0.05	0.01
minority_lang	0.06	0.03	0.10	0.03	0.06	0.03	0.04	0.04
chair	0.84	0.84	0.83	0.87	0.85	0.84	0.80	0.06
minority_religion	0.01	0.00	0.01	0.00	0.00	0.00	0.00	0.08
not_married	0.12	0.14	0.14	0.14	0.12	0.16	0.10	0.08
cement_wall	0.14	0.12	0.12	0.15	0.10	0.16	0.13	0.12
highest_grade	6.95	7.35	7.02	6.85	6.26	7.14	6.74	0.13
age	32.68	32.32	32.02	32.94	33.22	32.94	33.20	0.15
write_and_read	0.77	0.83	0.78	0.79	0.75	0.80	0.79	0.16
muslim	0.19	0.21	0.18	0.15	0.16	0.13	0.17	0.18
transport_work	0.03	0.04	0.02	0.03	0.02	0.04	0.02	0.18
mutooro	0.02	0.01	0.03	0.01	0.01	0.01	0.02	0.19
education_work	0.05	0.05	0.03	0.04	0.02	0.04	0.04	0.20
living_conditions_compared	2.21	2.26	2.17	2.24	2.14	2.18	2.16	0.21
household_other	0.07	0.10	0.07	0.08	0.07	0.09	0.05	0.21
separated	0.10	0.12	0.10	0.14	0.12	0.14	0.12	0.21
single_hut	0.61	0.55	0.60	0.59	0.65	0.56	0.62	0.23
mukiga	0.03	0.02	0.04	0.01	0.05	0.03	0.02	0.23
household_spouse	0.39	0.38	0.40	0.37	0.40	0.36	0.41	0.25
luganda_lang	0.90	0.93	0.83	0.87	0.85	0.84	0.90	0.25
catholic	0.38	0.35	0.35	0.42	0.40	0.40	0.35	0.27
household_children	2.81	2.73	2.55	2.76	2.82	2.56	2.76	0.27
share_house	0.28	0.35	0.30	0.28	0.25	0.33	0.28	0.28
stone_wall	0.03	0.03	0.05	0.03	0.04	0.05	0.04	0.31
number_children	4.65	4.35	4.44	4.56	4.84	4.30	4.64	0.33
illiterate	0.15	0.10	0.14	0.13	0.16	0.13	0.13	0.33
travel_big_city	0.65	0.67	0.67	0.63	0.65	0.70	0.68	0.34
pray_private	8.11	8.20	8.11	8.25	8.14	8.15	8.05	0.35
runyannkole_lang	0.02	0.02	0.05	0.06	0.05	0.09	0.05	0.36
religious_service	1.99	1.92	1.75	1.45	1.59	1.64	1.81	0.38
members	5.00	5.00	4.73	4.98	4.99	4.75	4.97	0.38
household_younger	3.46	3.42	3.20	3.46	3.46	3.23	3.47	0.38
rooms	2.65	2.72	2.66	2.90	2.73	2.66	2.75	0.41
other_person	0.12	0.10	0.10	0.12	0.10	0.08	0.10	0.41
university	0.06	0.05	0.06	0.05	0.04	0.07	0.05	0.41
misc_fuel	0.01	0.01	0.02	0.02	0.02	0.02	0.02	0.42
married	0.41	0.42	0.37	0.38	0.43	0.38	0.41	0.43
english_christian	0.06	0.06	0.08	0.08	0.08	0.06	0.09	0.44
misc_floor	0.09	0.09	0.12	0.11	0.12	0.08	0.13	0.44
mobile_phone_use	3.27	3.33	3.25	3.21	3.18	3.39	3.14	0.44
cement_floor	0.64	0.66	0.60	0.61	0.52	0.62	0.58	0.45
household_older	0.54	0.58	0.52	0.52	0.53	0.52	0.50	0.45
other_work	0.06	0.06	0.04	0.04	0.07	0.05	0.05	0.46
cellphone	0.80	0.82	0.80	0.79	0.77	0.82	0.77	0.49
munyoro	0.07	0.05	0.06	0.06	0.05	0.03	0.04	0.49
brick_wall	0.58	0.64	0.57	0.60	0.58	0.54	0.58	0.51
living_as_married	0.38	0.36	0.42	0.36	0.35	0.36	0.40	0.52
earth_floor	0.27	0.25	0.28	0.28	0.36	0.29	0.29	0.53
minority_tribe	0.18	0.17	0.13	0.16	0.15	0.13	0.15	0.54
mufumbira_tribe	0.02	0.02	0.01	0.04	0.04	0.04	0.02	0.55

radio	0.80	0.82	0.82	0.81	0.79	0.79	0.80	0.56
manual_work	0.08	0.05	0.07	0.07	0.08	0.07	0.07	0.58
read_only	0.03	0.03	0.03	0.03	0.04	0.03	0.03	0.59
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.62
munyankole	0.07	0.08	0.11	0.10	0.10	0.12	0.09	0.63
fumbira_lang	0.02	0.02	0.02	0.04	0.04	0.04	0.01	0.64
day	1.25	1.21	1.24	1.25	1.23	1.25	1.20	0.65
same_village	0.37	0.36	0.33	0.39	0.34	0.34	0.37	0.65
misc_light	0.13	0.13	0.15	0.13	0.14	0.12	0.14	0.73
charcoal_fuel	0.44	0.44	0.43	0.38	0.36	0.40	0.37	0.73
protestant	0.18	0.20	0.20	0.17	0.18	0.21	0.21	0.74
several_huts	0.11	0.10	0.10	0.12	0.10	0.11	0.10	0.77
tv	0.30	0.31	0.27	0.28	0.25	0.29	0.24	0.77
sofa	0.30	0.28	0.25	0.26	0.24	0.26	0.28	0.78
write_only	0.05	0.04	0.05	0.05	0.05	0.04	0.05	0.79
firewood_fuel	0.55	0.55	0.54	0.60	0.62	0.58	0.61	0.80
holy_spirit	0.15	0.14	0.15	0.14	0.14	0.17	0.13	0.82
female	0.54	0.53	0.53	0.54	0.54	0.53	0.53	0.82
retail_work	0.12	0.15	0.13	0.12	0.13	0.14	0.12	0.82
misc_wall	0.02	0.02	0.04	0.02	0.04	0.03	0.04	0.83
kerosene_light	0.24	0.27	0.27	0.27	0.28	0.24	0.28	0.83
muganda_tribe	0.52	0.57	0.54	0.53	0.50	0.55	0.58	0.83
christian_only	0.03	0.03	0.04	0.03	0.03	0.03	0.04	0.84
munyarwanda	0.09	0.09	0.07	0.09	0.10	0.10	0.08	0.84
electric_light	0.25	0.28	0.22	0.26	0.20	0.25	0.19	0.85
agriculture_work	0.53	0.52	0.58	0.58	0.56	0.53	0.59	0.85
household_head	0.54	0.53	0.53	0.55	0.53	0.55	0.53	0.90
hospitality_work	0.04	0.05	0.05	0.06	0.05	0.06	0.04	0.92
solar_light	0.29	0.24	0.29	0.27	0.29	0.30	0.28	0.94
mud_wall	0.23	0.19	0.23	0.20	0.24	0.21	0.21	0.95
domestic_work	0.07	0.07	0.07	0.06	0.06	0.07	0.07	0.96
no_work	0.04	0.04	0.04	0.04	0.04	0.05	0.03	0.99
motor_cycle	0.27	0.28	0.26	0.27	0.27	0.27	0.26	1.00

Table 28: Balance on covariates among Non-Compliers

E Robustness to Alternative Strategies

E.1 Relaxing the Assumption of No Crossover Effects

Because our experiment does not feature an arm in which viewers were exposed to all three messages (VAW, absenteeism, and abortion stigma), the comparison of subjects that were assigned to a given message to subjects that were not assigned to that message is slightly imbalanced. Consider, for example, the VAW message. The VAW-untreated group is more likely to have been exposed to either absenteeism or abortion stigma treatments. In order to see how this imbalance arises, note that we have seven experimental conditions: placebo, VAW, absenteeism, abortion, VAW+absenteeism, VAW+abortion, and absenteeism+abortion. The VAW treatment group comprises VAW, VAW+absenteeism, and VAW+abortion, whereas the control group comprises the remaining four groups. The average marginal effect of the VAW message could be identified by comparing VAW to placebo, by comparing VAW+absenteeism to absenteeism, or by comparing VAW+abortion to abortion. Because we do not have an VAW+absenteeism+abortion group, we do not have a treated counterpart to the absenteeism+abortion control group.

An alternative estimator to the one used in this paper simply excludes the absenteeism+abortion group and includes a fixed effect for the VAW+absenteeism and absenteeism groups (to control for the effects of the absenteeism treatment) and a fixed effect for the VAW+abortion and abortion groups (to control for the effects of the abortion treatment). The alternative estimators for the absenteeism and abortion treatments are analogous. As can be seen in tables 29 to 32, we find that these estimators produce very similar point estimates.

	Dependent variable:			
	Index of willingness to take action to counter absenteeism			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
absenteeism	0.044^{***}	-0.002	0.005	0.0004
	(0.015)	(0.009)	(0.009)	(0.007)
abortion	-0.009	0.024^{**}	-0.0003	0.011
	(0.017)	(0.011)	(0.011)	(0.009)
VAW	0.019	0.017	-0.024^{*}	-0.003
	(0.020)	(0.011)	(0.010)	(0.008)
Control Mean	0.61	0.59	0.59	0.59
Vill. Means	0.62	0.6	0.58	0.59
Vill. SD	0.1	0.08	0.09	0.07
N Vill.	142	142	142	142
Block FE	Yes	Yes	Yes	Yes
Observations	1,244	2,817	2,742	5,559
Adjusted R^2	0.024	0.044	0.054	0.047
Notes:				$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 29: Direct effects and spillovers from absenteeism messages among all respondents in endline surveys following 2015 and 2016 festivals using an estimator that is unbiased in the presence of cross-over effects.

Coefficients estimated using least-squares regression, conditioning on block fixed-effects and an indicator for resampling. All analyses exclude respondents from clusters assigned to the VAW+abortion treatment condition. *VAW* is a fixed effect for the VAW+absenteeism and VAW groups and *abortion* is a fixed effect for the absenteeism+abortion and abortion groups. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Education is an important goal			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
absenteeism	0.075^{***}	0.013	-0.018	-0.001
	(0.025)	(0.017)	(0.017)	(0.012)
abortion	-0.035	-0.007	0.023	0.002
	(0.029)	(0.020)	(0.021)	(0.014)
VAW	0.008	-0.047^{**}	0.043^{*}	-0.007
	(0.034)	(0.021)	(0.020)	(0.014)
Control Mean	0.42	0.43	0.44	0.43
Vill. Means	0.41	0.43	0.44	0.43
Vill. SD	0.19	0.12	0.12	0.08
N Vill.	142	142	142	142
Block FE	Yes	Yes	Yes	Yes
Observations	1,244	2,817	2,742	5,559
Adjusted R^2	0.007	0.0002	0.001	-0.0001
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 30: Direct effects and spillovers from absenteeism messages among all respondents in endline surveys following 2015 and 2016 festivals using an estimator that is unbiased in the presence of cross-over effects.

Coefficients estimated using least-squares regression, conditioning on block fixed-effects and an indicator for resampling. All analyses exclude respondents from clusters assigned to the VAW+abortion treatment condition. *VAW* is a fixed effect for the VAW+absenteeism and VAW groups and *abortion* is a fixed effect for the absenteeism+abortion and abortion groups. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Index of willingness to take action to counter intimate partner violence			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
VAW	0.056^{***}	0.002	-0.007	-0.002
	(0.016)	(0.011)	(0.015)	(0.010)
absenteeism	-0.028	-0.012	0.010	-0.002
	(0.020)	(0.013)	(0.019)	(0.013)
abortion	-0.056^{**}	0.014	-0.007	0.005
	(0.021)	(0.014)	(0.019)	(0.012)
Control Mean	0.37	0.38	0.39	0.38
Vill. Means	0.37	0.37	0.38	0.38
Vill. SD	0.1	0.07	0.1	0.06
N Vill.	94	94	94	94
Block FE	Yes	Yes	Yes	Yes
Observations	954	2,101	1,680	3,781
Adjusted R^2	0.026	0.006	0.019	0.010
Notes:				*p<0.1; **p<0.05; ***p<0.01

Table 31: Direct effects and spillovers from anti-VAW messages among all respondents in endline surveys following 2016 festival using an estimator that is unbiased in the presence of cross-over effects.

Coefficients estimated using least-squares regression, conditioning on block fixed-effects and an indicator for resampling. All analyses exclude respondents from clusters assigned to the absenteeism+abortion treatment condition. *absenteeism* is a fixed effect for the VAW+absenteeism and absenteeism groups and *abortion* is a fixed effect for the VAW+abortion and abortion groups. Standard errors are clustered at the village level. Two-tailed p-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.

	Dependent variable:			
	Willingness to help someone suffering from post-abortion complications			
	Reached Directly	Reached Indirectly	Not Reached	Not Reached Directly
	(1)	(2)	(3)	(4)
abortion	0.059^{***}	-0.008	-0.003	-0.005
	(0.021)	(0.014)	(0.017)	(0.012)
absenteeism	-0.027	-0.009	0.002	-0.005
	(0.022)	(0.019)	(0.020)	(0.015)
VAW	-0.042	0.008	0.044^{*}	0.022
	(0.024)	(0.017)	(0.020)	(0.014)
Control Mean	0.81	0.8	0.79	0.79
Vill. Means	0.78	0.8	0.79	0.79
Vill. SD	0.17	0.12	0.13	0.11
N Vill.	143	143	143	143
Block FE	Yes	Yes	Yes	Yes
Observations	1,294	2,842	2,730	5,572
Adjusted R^2	0.008	0.017	0.019	0.020
Notes:				$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 32: Spillovers from anti-abortion stigma messages among all respondents in endline surveys following 2015 and 2016 festivals using an estimator that is unbiased in the presence of cross-over effects.

Coefficients estimated using least-squares regression, conditioning on block fixed-effects and an indicator for resampling. All analyses exclude respondents from clusters assigned to the VAW+absenteeism treatment condition. *VAW* is a fixed effect for the VAW+abortion and VAW groups and *absenteeism* is a fixed effect for the absenteeism+abortion and absenteeism groups. Standard errors are clustered at the village level. Two-tailed *p*-values are calculated by comparing the observed estimate to 2000 estimates simulated under the sharp null of no effects for all units by permuting the treatment assignment 2000 times.