Silence Begets Violence: A mass media experiment to prevent violence against women in rural Uganda*

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Abstract

Preventing violence against women (VAW) requires witnesses to come forward, yet willingness to report is often undermined by social sanctions against those suspected of fabricating allegations. Our theory of the micro-politics of information disclosure in interdependent communities elucidates the role of social norms in preventing VAW. We present experimental evidence from a media campaign attended by over 10,000 Ugandans in 112 rural villages that featured three short videos designed to encourage reporting of VAW in the household. Results indicate a substantial reduction in VAW over a 6-month period following the campaign. Investigation of mechanisms reveals that women in the treatment group became less likely to believe that they would be labeled a gossip if they were to report an incident of VAW, and their personal willingness to speak out increased substantially. We find no evidence of a deeper change in core values pertaining to VAW.

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Introduction

In order to govern effectively, governments require information obtained through voluntary disclosure by citizens. Without the information needed to punish and apprehend offenders, for example, governments may find it difficult to deter crime and other harmful conduct. However, disclosing information can be costly to those who blow the whistle. In practice, unwillingness to report crimes and cooperate with police inhibits apprehension and prosecution of criminals (Buckley et al. 2016), and fear of speaking out about corruption (De Graaf 2010) or personal misconduct can allow abuse to continue unabated. Given these constraints, many have argued "informal social control is more effective than formal social control" by the state (Schwartz and DeKeseredy 2008, 183), and suggest a reliance on local actors – such as community councils and neighborhood watches – for the regulation of social harms (Sampson, Raudenbush, and Earls 1997). Suggestions of this kind often assume informal actors' embededdness in the community provides them with an informational advantage. When it comes to the regulation of practices that occur within the private sphere of the home, however, even local actors may not have the information they would need to act. A fundamental question is therefore how to encourage socially-beneficial information disclosure (Blair, Littman, and Paluck 2017).

In this paper, we focus on the micro-politics of disclosure surrounding violence against women in the household. We develop a theory that suggests widespread belief in the prevalence of false accusations may stifle disclosure because such beliefs result in social sanctions against anyone who comes forward with information. We argue that a key lever for improving formal and informal authorities' ability to address social harms is to undermine anti-disclosure norms by convincing people that revelations are credible. We present some of the first field experimental evidence illustrating mass media's potential to achieve this goal. Our findings indicate a media-induced shift in norms away from sanctioning those who report incidents of violence against women is accompanied by a major reduction in actual incidents of violence against women (VAW).

We conduct a mass media experiment in which Ugandan villagers were exposed to a placebo-controlled education-entertainment campaign designed to convince audiences that VAW is common and to encourage viewers to speak out. The campaign comprised 670 film screenings in 112 villages, attended by over 10,000 adults. We measure outcomes through seemingly unrelated midline and

endline surveys, respectively, two and eight months after the conclusion of the media campaign. In the control group, two-thirds of women believed they would face social sanctions for reporting incidents of VAW; our campaign reduced this belief by 18%. Women became substantially more willing to report incidents of VAW to authorities and agents of the state, as well as to family members. Moreover, men and women became more likely to believe that their fellow community members would intervene to stop VAW. In the communities where we screened our anti-VAW campaign, this apparent erosion of a norm against speaking out coincides with a substantial decrease in violence: we estimate the probability that women in a household experienced violence over a six-month period following our films decreases by at least 5 percentage points, effectively preventing violence in hundreds of households.

These findings are important for at least two reasons. First, violence against women is a widespread policy problem thought to afflict 30 percent of women globally (Devries et al. 2013) and 44 percent of married women in rural Uganda (DHS 2001-2015). Addressing gender-based violence has been high on the agenda of international organizations working in developing societies (Htun and Weldon 2012), but many efforts to ameliorate violence against women employ intensive grassroots mobilization campaigns aimed at changing core values. Evidence for the effectiveness of such efforts is mixed,[1] and implementation costly to scale. Mass media campaigns to reduce VAW are common across the globe,[2] but our study presents the first rigorous evidence that mass media campaigns constitute an effective means for violence reduction.

Second, our findings elucidate an important avenue for improving formal and informal governance. It is noteworthy that our campaign failed in many respects: we did not change audiences' core values about the morality of VAW, nor did we appear to convince those who saw the films that various means of informal social control are effective to address the problem. How can we explain that we changed behavior without changing core values? It turns out that participants in our study almost universally reject forms of VAW more severe than slapping and believe that intervention by the community can prevent it from occurring. The real barrier to action in this context appears to

[1] In Uganda, Abramsky et al. (2014, 2016) show the SASA! campaign brought about large reductions in violence against women, for example. Wagman et al. (2015) presents another successful example of an intensive outreach campaign directed at men and boys. However, a large-scale evaluation in 250 slums in four cities in India assessed efforts to reduce VAW through women's self-help groups and workshops with men and boys, and found very little evidence of success (Holden et al. 2016). For meta-analysis see Bourey et al. (2015), and for reviews of evidence see Fulu and Kerr-Wilson (2015) and Picon et al. (N.d.).

[2] See section B.3 of the Online Appendix for a cross-national collection of examples of anti-VAW media campaigns.

be fear that reporting violence will result in being labeled as a gossip or meddler. A core theoretical implication of our model is that widespread beliefs in the prevalence of false accusations can inhibit disclosure of information about harmful practices. This is in line with the literature on the barriers to reporting sexual assault, which frequently points to the fear of not being believed as an explanation for witnesses' reticence (Sable et al. 2006). Our findings suggest that, in certain cases, such fears may be the principal obstacle to state and non-state efforts to reduce social harms, and that media may be an effective way to address such fears. In fact, in situations where powerful actors benefit from concealment of their socially harmful behavior, they may foster the view that disclosures are strategically-motivated lies – "fake news," as it were. By increasing the credibility of disclosures, it may be possible for education-entertainment to ameliorate social harms without effecting deeper attitudinal change.

This paper is organized as follows. In section 1 we provide contextual background to the problem of VAW in rural Uganda, and section 2 develops a theory of how beliefs about gossiping inhibit reporting by creating anti-disclosure norms. Section 3 describes our messaging campaign and its connection to our theory, while section 4 presents the research design. Our main results, presented in section 5, suggest our campaign caused a substantial reduction in VAW. We present evidence in section 6 that this reduction may be due to an increase in willingness to report accompanied by a decrease in perceived social sanctions against disclosure of VAW. Section 7 rules out several alternative explanations. Section 8 concludes by discussing how the results obtained here might generalize to other policy domains and inform our theoretical understanding of citizen willingness to disclose information to formal and informal authorities.

1 Background

The UN definition of violence against women (VAW) comprises a wide range of physical and emotional abuse that can occur within the private or public spheres.[3] In this paper much of the discussion focuses on physical violence inflicted by intimate partners within the home. Opinion surveys suggest permissive attitudes toward such violence are widespread in Uganda. Figure 1 shows

[3]The UN Declaration on the elimination of violence against women defines VAW as "any act of gender-based violence that results in, or is likely to result in, physical, sexual, or psychological harm or suffering to women, including threats of such acts, coercion or arbitrary deprivation of liberty, whether occurring in public or private life" (United Nations General Assembly 1994). We focus our attention primarily on physical abuse, the form of VAW that our campaign addresses explicitly.

women from Uganda, by comparison to other countries, are especially likely to state that husbands are justified in beating their wives in at least one of five scenarios.

This survey evidence may seem to imply that the main obstacle to violence prevention is its wholesale acceptance. However, this study and other recent work uncover important nuances in Ugandan public opinion towards VAW. Tsai et al. (2017), for example, find that VAW is perceived as acceptable by a majority of respondents when the woman is framed as having intentionally contravened gendered standards of behavior, but only by a small minority when her behavior is described as unintentional. This corresponds to findings from our qualitative fieldwork that suggests violence is condoned only when it is seen as serving some "pedagogical" end. Further, we find that not all forms of physical violence are seen as acceptable. In surveys we conducted in 2016 among rural Ugandans, 31% of respondents said that a husband is justified in beating his wife when she disobeys him. When asked whether they meant that she should be slapped or beaten with more force than that, only 5% of those who initially said that beating was justified endorsed more severe violence. The widespread reluctance to condone physical violence perceived as more forceful than slapping suggests that, if given the opportunity, bystanders may indeed see the need to come forward. In fact, 88% of respondents in the control group state that other people should intervene to stop violence if they learn of a husband beating his wife every evening. Yet almost one-third of rural Ugandan women in the 2011 DHS survey report that they had been punched with a fist, kicked or dragged, strangled or burnt, or threatened with knife or other weapons.[4]

One may reason that the inability to prevent violence that is widely seen as illegitimate stems primarily from a lack of state capacity. Law enforcement institutions are inaccessible in many parts of Uganda. Formal police posts are sparsely distributed in rural areas: our 2017 survey found that 75% of rural respondents see police on patrol in their village less than once per week, and more than 40% see them less than once per month.

Despite the lack of police presence, however, rural areas are not without state actors whose administrative purview includes VAW. Each village in Uganda is headed by an LC1 chairperson. Formally, the LC1 chairperson presides over the Local Council, which sets and implements policy for the village, and leads the Local Council Court, which has jurisdiction over civil law matters

[4]These figures from the 2011 DHS are calculated using variables *D105a-f*, and *j*, which excludes measures of emotional and sexual abuse.

including cases of domestic violence. LC1 chairpersons regulate village life in ways that seem more similar to 'informal social control' than to formal acts of law enforcement. For instance, an LC1 chairperson may levy symbolic sanctions against violent offenders or intervene on behalf of women at risk. Although Ugandan village leaders are often characterized as socially conservative, our survey data suggest that LC1 chairpersons (N=55) are in fact significantly more opposed to violence against women than the general population (18% of LC1 chairpersons and 31% of all villagers endorse the view that a husband has a legitimate reason to beat his wife if she disobeys). A similar role is played by *Nabakyalas*, or representatives of women in the village, who in our surveys almost never endorse violence against women. Those who experience or are bystanders to violence may thus seek help from village leaders, even where access to formal police is limited.

A large literature suggests, however, that both formal and informal governance may be severely constrained by a lack of information. The hypothesis that states require information to govern gained prominence with Scott's (1998) notion of 'legibility' and is central to both the literature on policing as well as Ostrom's (1990) work on the co-production of public goods by states and citizens. Community-level actors are often thought to have an inherent advantage over the state in terms of access to information. Yet, when it comes to sanctioning socially harmful behavior that occurs in the private sphere, such as violence against women in the home, even community-level actors must rely on women who experience violence or witnesses who happen to overhear the incident in order to acquire the information needed to act. Indeed, in our survey, community members do not seem to be well informed about the total number of incidents of violence against women that happen in their village (i.e., beyond those to which they were immediately privy).[5]

We have seen that widespread acceptance of VAW and lack of access to authorities cannot entirely account for the high rates of violence against women in rural Uganda. In addition to these factors, we argue that the reluctance on the part of witnesses to disclose what they know is an important reason for the continued prevalence of violence against women. For example, when asked what they would do if they learned that their cousin had been severely beaten, three-quarters of the respondents in our 2016 midline survey said they would prefer to merely express sympathy to

[5]We asked both men and women about the frequency with which assaults occur in their community and asked follow up questions about assaults against women. Within the same location, men's and women's reports are weakly correlated, and both are weakly correlated with a separate battery of questions, described below, which asks women about incidents occurring within their household. See Tables 30 and 31 in E.2 of the Online Appendix.

the cousin, rather than alerting the LC1 chairperson. Just one in ten respondents would be willing to report this type of incident to the police. Such reluctance to disclose information has been documented with regard to other phenomena, such as corruption, but what seems to discourage information disclosure in the case of violence against women is the anticipation of social sanctions in the form of being labeled as a 'gossip' when speaking out. We will show how such social sanctioning can arise when communities (possibly mistakenly) are skeptical of accusers' allegations.

[Figure 1 about here.]

2 Theory: Violence Against Women and Anti-Disclosure Norms

Social sanctions related to the notion of 'gossiping' feature prominently in the open-ended comments that respondents volunteered when explaining their reluctance to share information with their community about a hypothetical violent incident. Explanations focused on the fear of being accused of spreading rumors. Women face especially high reputational and physical costs for disclosing information that may incriminate men: 56% of women indicate that they would be labeled a gossip were they to disclose that a neighbor beats his wife. Finally gossiping stands out as an act that survey respondents see as particularly deserving of violence: our 2015 survey, conducted in a neighboring rural region of Uganda, found that among six scenarios "gossiping with neighbors" was most likely to induce respondents to condone VAW.

Why might witnesses face sanctions when speaking out about violence? Even when individuals disclose what they know about violence against women, such information may not be easily verifiable. Husbands may, possibly strategically, inflict violence in ways that are not visible or attributable to them. Such uncertainty about whether violence has actually taken place allows for the notion that individuals may fabricate accusations. Especially when it comes to sexual violence, research suggests that the belief in the prevalence of false accusations is widespread in many contexts (Ferguson and Malouff 2016; Wheatcroft and Walklate 2014; Tuerkheimer 2017). The stakes of such accusations are especially high in rural communities where mobility is low – over half of the male respondents in our 2016 survey have lived their entire adult lives in the same village.

The belief that false accusations are common can lead to an *anti-disclosure norm*, a situation in which those who could disclose information about violence would be shunned by the community for gossiping and therefore refrain from speaking out. In section B.4 of the Online Appendix, we

demonstrate this logic formally. We focus on the interaction between a a woman who may have experienced violence or bystander, hereafter 'the witness,' who holds private information about whether a potential perpetrator has committed violence, and an uninformed actor, 'the community.' We implement the idea of a possibly biased belief in false accusations by assuming that the community is uncertain about the type of the witness: in the minds of the community, the witness may be a 'gossip,' whom we assume to be a type who always reports that the potential perpetrator was violent, regardless of the true state of the world. Alternatively, the witness may be a 'truthful type' who prefers for the community to sanction perpetrators if and only if they did commit violence. After observing whether the potential perpetrator committed violence, the witness decides either to report to the community that the potential perpetrator was violent or to stay silent. If the witness speaks out, the community decides whether to believe the report and take action against the accused or, instead, to ostracize the witness for gossiping. The community prefers to intervene if violence has taken place and to scold a witness who levies false accusations.

[Figure 2 about here.]

The model draws attention to how reporting behavior reflects beliefs about the prevalence of 'gossips' and the prevalence of violence. The results are summarized in Figure 2. If the community believes that most witnesses are 'gossips' (p is low) and that violence is uncommon (q is low), the sole equilibrium will correspond to the *anti-disclosure norm*: those who speak out about violence will face social sanctions and thus witnesses who are truthful types will not report what they know.[6] If, however, the community believes that violence is common (high q) and that witnesses prefer the community to sanction perpetrators only if violence actually occurred (high p), an equilibrium exists in which violence is reported when it is committed and reporters do not face sanctions. We refer to this equilibrium as a *transparency norm*.

This theory suggests several causal pathways through which a media campaign could undermine an anti-disclosure norm and encourage the emergence of a transparency norm. A campaign that changes beliefs about both the motivations of those who speak out against violence and the frequency with which violence against women takes place may change a situation in which only the

[6] See Shepsle (2006) for work that conceives of norms as equilibria. This conception of a 'norm' is closest to the idea of a 'descriptive' as opposed to a 'prescriptive' norm in social psychology.

anti-disclosure equilibrium exists to one that allows for a transparency norm to arise. Similarly, a campaign may shift beliefs such that a situation in which both equilibria exist gives way to one where only the transparency norm prevails. In Figure 2, these changes correspond to rightward shifts to the neighboring region. Importantly, though, both equilibria can exist if community members believe that violence is uncommon (q low enough) and that 'gossips' are rare enough (p high enough). Beyond changing beliefs, a second important function of a media campaign may therefore be to provide a focal point that allows communities located in the dark grey region in Figure 2 to coordinate on the transparency equilibrium.

While not part of our formalization, the shift from a situation in which no one speaks out to one in which violent acts are disclosed may decrease violence in at least two ways. First, potential perpetrators may anticipate intervention by the community and so be deterred from committing violence in the first place. Alternatively, perpetrators may continue to commit violence at first, but the disclosure of such acts will lead the community to intervene and stop further escalation.[7]

3 A Mass Media Campaign to Counter Violence Against Women

Our anti-VAW media campaign consists of three short video vignettes screened during the intermission at film festivals held in video halls (*bibanda*) across a broad swath of rural Uganda (see Figure 3 for a map). Each vignette is between four and a half and eight minutes long. While an overarching narrative runs through the three vignettes, each can also be understood as a self-contained story in isolation from the other two. The narrative of the videos is outlined in Section B.2 of the Online Appendix, and they can be viewed at this address: http://tiny.cc/Uganda_VAW_media_campaign. The film festival comprised six films shown one per week over consecutive weekends, from July 30 to September 4, 2016 in each of the 112 communities where the study took place (see Figure 4 for a timeline).

[Figure 3 about here.]

[Figure 4 about here.]

[Figure 5 about here.]

[7]Note that if a media campaign reduces violence, individuals may adjust their expectations and conclude that violence is less frequent (lower q). A media campaign that dramatizes the occurrence of violence may therefore have an ambiguous *net* effect on q.

Central to our theory of how the campaign affects the behavior of audiences is the concept of "vicarious learning" (Bandura 1962). According to this theory, people acquire new ways of responding to social situations not only through direct experience, but also by making inferences based upon the observation of others' behavior. Bandura (2004) points out that such learning need not take place through the observation of actual behavior: people may also acquire new ways of acting based on behavior modeled in fictional dramatizations.

Our campaign uses education-entertainment to convey what Bandura terms a "differential modeling" narrative:

> Characters representing relevant segments of the viewing population are shown adopting the beneficial attitudes and behavior patterns. [...] Other characters personify negative models exhibiting detrimental views and lifestyles. Transitional models are shown transforming their lives by moving from uncertainty or discarding adverse styles of behavior in favor of beneficial ones. Differential modeling contrasts the personal and social effects of different lifestyles. Viewers are especially prone to draw inspiration from, and identify with, transforming models by seeing them surmount similar adverse life circumstances. (Bandura 2004, 83)

Specifically, we compare two villages whose norms surrounding VAW mirror the anti-disclosure and transparency equilibria defined formally above.

The first vignette begins in the "anti-disclosure" village. The protagonist is a sympathetic and personable woman whose husband beats her severely despite her sincere efforts to appease him. As depicted in the first panel of Figure 6, 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, we move to the "transparency" village. 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 (as depicted in the bottom panel of Figure 6). The vignette closes with the couple in visibly better relations with one another. A voiceover confirms that the situation has improved and implores the viewer to speak out before it is too late when they learn of violence in their community.

Vicarious learning seems particularly likely to result from these dramatizations given their close-

ness to the audiences' context and experience. 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 very 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.[8] 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."

Vicarious learning is especially important for our theory insofar as it enables audiences to update their beliefs about both the prevalence of violence in their community and the motivations of those who claim that violence has occurred. The films present a strong case against the idea that allegations stem from baseless gossip. The videos not only depict people reporting an actual incident, but they also show these reports being believed by those who receive the second-hand accounts.

Another source of vicarious learning is common knowledge (Schelling 1960). In our case, the experience of viewing the videos in a communal setting enables audiences to update about how others in their community may be updating in light of their shared viewing experience. Arias (2016) has argued that media interventions directed at violence against women are especially powerful when aired in a communal setting. The communal exposure to the differential modeling storyline may enable coordination on the transparency equilibrium.

[Figure 6 about here.]

3.1 Ethical Considerations

We took a number of steps to ensure the appropriateness of our media campaign for the setting in which we work in order to make sure that it was respectful of participants' rights and well-being. We sought and obtained IRB approval both in Uganda and in the U.S. We worked with a production company that employs Ugandan script-writers and actors and has extensive experience producing and screening public service announcements in Ugandan video halls. We vetted our

[8] We did not include questions about respondents' views on the videos in our main survey to preserve, as much as possible, its unobtrusive character.

messages extensively: prominent Ugandan NGOs specialized in the prevention of intimate partner violence reviewed and provided commentary on them, and we conducted three days of focus groups on the vignettes among different age and gender groups in two rural Ugandan villages not included in the study.

4 Research Design

Aside from the messages on VAW that are the focus of this paper, our field experiment was designed to test the effects of two other sets of video vignettes. These concern the stigma surrounding abortions and the problem of teacher absenteeism, respectively. We assigned villages to receive either: one set of vignettes (e.g., anti-VAW only); a combination of two sets (e.g., abortion stigma and anti-VAW); or a placebo (just the Hollywood movie with no vignettes). In total, this creates seven experimental conditions. Prior to random assignment, villages were organized into 16 blocks of seven in order to minimize within-block variance in latitude and longitude (see Figure 3). The analyses in this paper will compare respondents from clusters (villages) that were exposed to the messages on VAW (anti-VAW, abortion and anti-VAW, and anti-VAW and absenteeism) to the respondents in clusters assigned to all other conditions (collectively referred to as the control group). We discuss the identifying assumptions behind this design in section C.1 of the Online Appendix, and show that our results are robust when these assumptions are relaxed in sections D.1 and D.2.

Almost all sites complied with the treatment assignment insofar as we were able to correctly screen the assigned films and messages.[9] We measure individual-level compliance based on responses to questions about attendance of the screenings posed at the end of our surveys conducted months later.[10]

In most analyses, we focus on *compliers*, respondents who indicated they attended at least one of the screenings. (Section C.1 of the Online Appendix defines compliers and describes their attributes.) By comparing compliers in the treatment group to compliers in the control group, we obtain unbiased estimates of the complier average causal effect.[11] As Figure 5 shows, attendance

[9] In two villages only five of the six scheduled screenings took place. In one case, a video hall owner suspected that the movie *Oz The Great and Powerful* promoted 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 noncompliance was related to the experimental vignette featured in the film.

[10] "Recently, a series of six free films (Pirates of the Caribbean, Creed, Fast and Furious, Spy, Slumdog Millionaire, Oz The Great And Powerful) were screened in the *kibanda* [video hall] in your [village]. Have you heard about the screenings and if so, how many screenings did you attend?"

[11] See section D.2 of the Online Appendix for a discussion of alternative estimators that invoke different modeling

rates were similar across experimental conditions. In sections C.2 and C.3 of the Online Appendix we provide evidence that the treatment is not statistically significantly related to the rate at which people attended screenings or to the attributes of those who attended.

Compared to the rest of the sample, compliers are more likely to be men, young, and consume news media, and they are less likely to own a television (see Table 2 in section C.1 of the Online Appendix). The over-representation of men is related to the image of video halls in Uganda. Visiting a video hall tends to be seen as more appropriate for men than for women, as the video content presented typically consists of soccer matches and action movies. To counter this perception, our film festival was explicitly marketed as an event open to both women and men. As a result, our sample of compliers encompasses a sizable share of women (31%).

Measurement of outcomes took place in two waves, illustrated on Figure 4. In our midline survey in late October 2016, we interviewed respondents from randomly selected households in proximity to the video halls included in the study. Sampling was not conditional on attendance of the screenings, and the survey, which was conducted weeks after the film festival, was billed as an unrelated public opinion poll, in order to avoid Hawthorne effects. We successfully interviewed 5,344 women and men in 110 of our 112 villages. The response rate was 96%, with most of the nonresponse coming from two villages where we were not able to conduct the survey due to resistance from local residents. As explained in the appendix, we believe that our inability to work in these locations was unrelated to the treatment status of the villages. Our main analysis therefore excludes villages in which we could not survey. Section D.4 of the Online Appendix shows the robustness of our main results to agnostic methods of imputing missing values.

In our endline survey in late May of the following year, we returned to the 110 villages in which we successfully conducted the midline survey in order to re-interview those who had reported attending at least one screening (compliers). Of the 1,156 midline compliers, we were able to re-interview 1,035, giving a follow-up rate of 90%.

In the following two sections, we discuss the main results of this study. To ensure comparability across analyses, almost all results are reported among the group of compliers who were interviewed in both the midline and the endline survey. As Tables 20 to 23 in the Online Appendix show, the results remain unchanged when we include all compliers who were interviewed only in the midline

assumptions but produce similar results.

survey or all compliers interviewed only in the endline survey.[12] When it comes to the effect of our treatment on respondents' experience of VAW, we report results among all respondents as well as the subset of compliers interviewed at the endline. Our theory allows for the possibility that the media campaign reduces victimization not only among women who see our videos, but also among women in households neighboring those who viewed our messages, and who are now more willing to report what they see and hear.

5 Reduction in Violent Incidents

The media campaign was successful in reducing intra-household violence against women. Our main measure of violence asks women respondents in the endline survey to count the number of times that they can recall a woman in their household, including themselves, experiencing violence over the six-month period preceding the survey. Conscious of the potentially traumatic nature of these questions, enumerators were instructed to reassure respondents that they may answer these questions only if they were comfortable doing so and never asked to know the identities of the women or men involved.[13]

Table 1 presents the effect of the treatment on this self-reported measure under 'Number of Incidents.' Columns under 'Any Incidents' report effects on a binary measure coded 1 if the number of incidents was greater than 0, and 0 otherwise. Rather than ask the respondent how many times VAW occurred, the 'Violence Frequency' measure asked those who reported more than 0 incidents whether violence occurred almost every day (coded 4), around once a week (coded 3), about once a month (coded 2), less than once a month (coded 1), or almost never (coded 0). Those who report 0 incidents are coded 0 on this measure.

Effects in columns 1, 2, 4, 5 and 7 are estimated among all women respondents in the endline survey ($N = 1,036$), irrespective of whether or not they attended the campaign screenings. We

[12] During the endline survey, we also conducted surveys with a new sample of 915 adults, interviewed as part of our efforts to gather data on more women compliers and the parents of teenagers sampled in a separate survey. In addition, we conducted interviews with members of village health teams (VHTs), who are well-positioned to understand norms about reporting violence. Details on the sampling strategy can be found in section F of the Online Appendix.

[13] Following Follingstad and Rogers (2013), we measure violence against women in a redundant manner in order to corroborate responses across questions. The first question asked "How many specific incidents since last Christmas can you remember when a woman in your household, including yourself, was a victim of violence?" The second question asked "In many of the villages we have visited, men sometimes beat women. Thinking again of the time that has passed since last Christmas, would you say that this has happened more than about once a week to a woman in your household, including yourself?" The question is then branched to categorize responses into "almost never," "less than once a month," "about once a month," "once a week," and "almost every day."

present the results at both the individual level (columns 2-3 and 5-8) and at the village cluster level (columns 1 and 4); both approaches produce similar estimates. When analyzing individual-level responses, standard errors are clustered at the village level, the unit of random assignment.

[Table 1 about here.]

Columns 1 - 3 suggest that the screenings reduced the average rate at which women in treatment communities experienced violence by roughly .15 - .35 of an incident, from a baseline of just over half an incident on average. In our pre-analysis plan, we hypothesized that the campaign might either sensitize people to the issue of violence against women without deterring violence (thus increasing this measure) or deter violence (thus decreasing this measure). Accordingly, we pre-registered a two-tailed hypothesis test, and our estimates fall short of significance at the $\alpha = 10\%$ level.

On the other hand, columns 4 - 6 show the campaign clearly lowered the probability that a household experienced any incidents of VAW during this period. Column 4 shows that the campaign reduces the village-level proportion of women respondents who report any violence in their household by 7 percentage points. The probability of observing effects of such magnitude due to sampling variability if there were no true effect is less than 1%. Column 5 reports a similar estimate, this time estimating the effect using individual data as opposed to village-level aggregates. Here anti-VAW messaging reduces the probability that women in a household experienced violence over the prior six months by 5 percentage points from a baseline of 20 percent, effectively reducing the probability of a woman experiencing violence by one-quarter in relative terms. Column 6 suggests that effects of the intervention are especially strong among women who actually attended the films, although further analysis of the interaction between treatment and attendance shows it to be of borderline statistical significance.

Columns 7 and 8 report the effects of the anti-VAW media on the frequency-based measure of victimization. Given the ordinal outcome measure, we fit an ordered logit model. The estimates indicate a reduction of the odds of a household experiencing violence almost every day as opposed to once a week, once a month or never by roughly 30 percent, which is significant at the $\alpha = 10\%$ level in a two-tailed test. Again, the estimated effects are especially strong and statistically significant among compliers.

We employ a weighted bootstrap method to estimate the total number of households that were

prevented from experiencing any violence against women by the campaign (see section E.3 of the Online Appendix). The campaign prevented women from experiencing any violence in roughly six households in each of the 48 villages where we screened the anti-VAW campaign, with a 95% confidence interval ranging from 1 to 11 households per village.

6 From Anti-Disclosure to Transparency

Having demonstrated an apparent reduction in violence, we now turn to the mechanisms suggested by our theory. First, Our treatment increased the propensity to speak out when encountering incidents of violence, especially among women. Second, this shift is accompanied by a decreased expectation that reporting an incident of VAW will results in social sanctions from the community. Third, the campaign also increased attendees' perceptions, most notably among men, that acts of VAW would be met with intervention by the community. Finally, we provide evidence in favor of additional testable implications of our theory for the effect of the presence of other bystanders on respondents' willingness to disclose information.

Increased willingness to report

Tables 2 and 3 show those who attended screenings became significantly more willing to report violence. The outcomes in columns 1-8 are based on questions that ask respondents to imagine discovering that their cousin has been severely beaten by her husband and offer respondents a choice between two actions, coding the outcome 1 if the respondent chooses the reporting option and 0 if they choose an option that implies inaction. These items measure the respondent's willingness to report the incident to the parents of the woman who experiences violence, the women's representative in the village (Nabakyala), the LC1 chairperson, and the police. The outcome variable used in the last two columns takes the average of these items.

[Table 2 about here.]

[Table 3 about here.]

Table 2 shows substantial and lasting effects on women's willingness to report violence: as columns 9-10 show, women who attended screenings with anti-violence messaging are 9 to 13 percentage points more likely to report to others ($p < .01$). A striking feature of these effects is their

persistence over time: we find highly significant estimated effects at both the two-month midline and the eight-month endline. The effects on men are suggestive but not as strong: the messaging increases the average measure of willingness to report among men compliers by 2-4 percentage points.

Improved community response to reports

Table 4 shows the increased willingness to report coincides with a decrease in the perceived social sanctions faced by those who come forward. As column 2 shows, exposure to the media campaign strongly reduces the probability that women expect to be scolded for gossiping (coded 1), rather than encouraged for doing the right thing (coded 0), if they were to report a hypothetical incident. The 11 percentage point reduction ($p < .05$) brings women – the group for whom reporting may be most costly – in the treatment group roughly in line with men in the control group. The reduction in expected sanctions appears to center on social repercussions from the community at large: column 4 provides little support for the alternative hypothesis that the campaign reduced respondents' fear that friends or family of an accused perpetrator would take revenge.

Accompanying this change in perceived norms is an increased expectation that members of the community would intervene in an incident of violence. Columns 5-6 show that men became 4-5 percentage points more likely to state that, if people in their community were to find out about a man beating his partner, those same people would intervene personally or mobilize others (coded 1) rather than minding their own business (coded 0). It is particularly important that this belief increases among men, since they are the principal perpetrators of violence against women. In columns 7-8 we see that this shift in beliefs is mirrored to some extent among women.

[Table 4 about here.]

Other bystanders increase willingness to report

According to our theory, witnesses do not speak out about VAW under the anti-disclosure norm because other community members would not believe their reports and would sanction them for gossiping. When an anti-disclosure norm prevails, bystanders should therefore be more willing to report if other community members also know that violence took place and are able to confirm the bystander's report. Knowing that others also know about the incident reduces the bystander's

concern about being labeled as a gossip, even when anti-disclosure norms dominate.

By contrast, where a transparency norm predominates, bystanders' willingness to report should not depend on whether other community members can verify their reports. In a world where bystanders do not expect to be sanctioned for reporting, corroboration by others loses its importance.

To test these predictions, we included the following survey experiment in our endline survey. Respondents were presented with a hypothetical scenario in which, while walking home, they observe a neighbor beating his wife. At random, respondents were asked one of the following two questions:

- (Others Observe = 0): Suppose **you know that you are the only one who has observed the incident**, would you report it to a local leader (such as the village leader or women's counselor)?
- (Others Observe = 1): Suppose **you see a group of people from your community standing nearby, discussing the incident**, would you report it to a local leader (such as the village leader or women's counselor)?

In line with our theory, we expect our campaign to increase willingness to report, irrespective of whether or not others are present. However, the campaign should undermine anti-disclosure norms that discourage reporting that cannot be corroborated. Thus, while we expect the presence of others to encourage reporting among those in the control group, we expect our treatment to diminish the importance of corroboration for the decision to report. In other words, we expect a negative interaction between the two treatments.

[Table 5 about here.]

Table 5 reports the results of this survey experiment and how it interacts with our media campaign. Columns 1-3 illustrate that, in line with our expectation, the presence of other community members in a hypothetical incident of VAW increases the proclivity to report the incident to village level authorities by about 4 percentage points ($p < .05$). Consistent with tables 2 and 3, the anti-VAW campaign has about the same magnitude of effect on the proclivity to report.[14] It is

[14]While the marginal effect of the treatment on this measure of reporting proclivity is similar to effects on the reporting outcomes reported on tables 2 and 3, it should be noted that the baselines are quite different. The additive index indicates respondents give pro-reporting responses in about 40% of questions in the control, whereas the free-riding vignette exhibits baseline pro-reporting responses at rate of over 80%. The latter asked respondents whether they would report a hypothetical case of domestic violence to a local leader (yes or no), rather than offering a potentially more attractive alternative answer as in the paired vignettes that form the additive reporting index.

important to note that the coefficient on the interaction between the presence of other community members and the anti-VAW media, while not very precisely estimated, is negative. In other words, the presence of others in a hypothetical incident of VAW appears to matter for the willingness to report the incident among respondents in the control group, but not necessarily for those in the treatment group. This evidence supports the notion that our treatment weakened anti-disclosure norms and lessened the risk to whistle-blowers.[15]

7 Ruling Out Alternative Explanations

Having suggested that violence decreased in the wake of the media campaign due to increased willingness to speak out and a growing sense that doing so would be in line with community norms, we next address some alternative explanations for our findings. Some of these competing accounts amount to methodological critiques, while others focus on alternative substantive pathways. We address each in turn.

7.1 Reduction in Violence Not Measurement Artifact

Because violence against women is so difficult to measure, any experiment that purports to show a reduction in incidence must immediately address the critique that the purported treatment effect is a measurement artifact. While such an interpretation cannot be ruled out conclusively, four facts militate against it.

First, the overall pattern of treatment effects that we observe among women respondents is at odds with the notion that the apparent reduction in violence is due to eagerness on the part of the treatment group to please researchers by reporting fewer incidents. As we discuss below, we find negligible effects on many outcomes that were more closely connected to the messages in our treatment videos, such as whether husbands have legitimate cause to beat their wives. The effects we observe are largely confined to willingness to report violence against women, not core attitudes about the legitimacy of husbands being violent toward their wives.

Second, the hypothesis that women in the treatment group were offering socially desirable responses is undercut by the relatively weak inter-item correlations across different outcome measures, such as self-reported violence in the household, perceptions of views held by the community, and

[15] While in line with our theory, these findings run counter to the literature on the "bystander effect" in social psychology, which finds that the presence of others decreases the proclivity of bystanders to intervene in an emergency due to free-riding (Fischer et al. 2011).

the respondent's own willingness to report incidents of violence (see Table 35 in section E.5 of the Online Appendix). If women in the treatment group were trying to paint a rosy picture about household relationships in their community, they were not doing so with much verve or consistency.

Third, we designed the study so that there was little apparent connection between the film festival and the surveys that measured outcomes months later. In order to avoid priming respondents, the question used to measure household violence against women refers to incidents that occurred "since last Christmas," months after the film festival concluded. One may worry that respondents in the endline perceive a connection between the film festival and the surveys because they were asked whether they had attended the film festival during the midline. However, as can be seen in Table 34 in section E.4 of the Online Appendix, we do not find any evidence that our main results are driven by the responses of compliers who had already been interviewed in the midline. If anything, treatment effects on victimization are larger among those taking our survey for the first time.

Another methodological critique is that the apparent relationship between assigned treatment and reported violence is a statistical fluke, brought about by a lucky draw of villages into the treatment group. Although our study features multiple outcome measures and therefore invites concerns about family-wise error rates, other village-level patterns run counter to this hypothesis. The midline survey featured large numbers of respondents who neither attended the films nor knew someone who attended. We find no trace of treatment effects among these respondents (men or women) on views about violence against women, perceptions of what the community thinks about it, or its importance as a policy problem. The apparent lack of effects among non-attendees suggests that the villages assigned to the treatment group were in no way distinctive in terms of their views about VAW or their exposure to it.

[Figure 7 about here.]

7.2 Reduction in Violence Not Due to Perpetrators' Change of Heart

One alternative explanation for how our treatment reduced violent incidents is that potential perpetrators saw our messaging and changed their moral appraisals of violent behavior. For example, the dramatization of a sympathetic wife who is beaten while trying to accommodate her husband's impossible demands might encourage empathy with women who experience violence. The remorse

expressed by the husband when his wife is hospitalized could similarly encourage men to regard domestic violence as behavior that can have tragic consequences.

The results, however, provide little support for these hypotheses. The top panel of Figure 7 focuses on the attitudes of men who are either married or in relationships, the very men who are potentially at risk of committing the type of violence depicted in the videos. We find no evidence that the media campaign changed their minds to any appreciable degree. Viewers in the treatment group were no more likely than their control group counterparts to reject violence against women as illegitimate (e.g., when a wife disobeys her husband) in either the midline or the endline survey.

7.3 Increase in Reporting Not Due to Other Effects on Compliers

By depicting involvement of family members and village authorities as an effective means to prevent future violence, perhaps our video messages convinced viewers that bringing incidents of violence to the attention of these actors is worthwhile. Yet, over 70% of women and over 80% of men in control villages already viewed such intervention as effective at preventing future violence. And, as illustrated in panels 2 and 3 of Figure 7, we do not see strong evidence that our campaign changed perceptions of efficacy among men and women compliers. Nor do we see strong evidence of an effect on respondents' perception of whether their community sees well-meaning interference in others' affairs as generally acceptable.

Taken together, the results presented on Figure 7 support our interpretation that the media campaign reduced violence by decreasing the perceived social consequences of reporting, especially among women, and changing potential perpetrators' expectations about the community reaction they would face if they committed violence.

8 Discussion

Crucial to the theoretical interpretation of our key result – a decline in the share of households experiencing VAW – is the finding that media exposure changes certain outcomes but not others and influences certain participants more than others. Although education-entertainment is often said to have special persuasive influence because audiences come to identify with the main characters and let down their guard when encountering new viewpoints (Slater and Rouner 2002), it should be stressed that we see very little evidence of attitude change on the the acceptability of VAW and no indication whatsoever that the treatment causes viewers to rethink their position about gender

hierarchy more broadly.[16] By the same token, the dramatization of unjust and excessive violence did not induce audiences to become more empathetic toward women who are beaten by their husbands.

Nevertheless, we observe a substantial reduction in the proportion of households reporting violence against women in the eight-month endline survey. What might explain the apparent decline in violence? The answer seems to hinge on newfound willingness among viewers in treated locations to report incidents to police, village leaders, and families. This change is especially pronounced among women, for whom reporting ordinarily carries special risks given widespread disdain for 'gossips.' Women exposed to the anti-violence videos, which dramatized a community being supportive of a woman who speaks out about domestic violence, months later expressed substantially more willingness to take complaints to the police and became more sanguine that their allegations will be believed rather than denounced. For their part, male viewers in treated sites sensed this change in public outlook and became more likely to indicate eight months later that their community would intervene in response to incidents of domestic abuse. Drawing on the terminology from our formal model, the media campaign disrupted an anti-disclosure equilibrium in which abusers could act with impunity because would-be reporters feared the social repercussions of being labeled as gossips.

Stepping back, violence against women in Uganda has much in common with stubborn social problems in other domains and regions. The key ingredients consist of a negative social behavior that governments seek to regulate coupled with the lack of information to do so. In this case, Ugandan law prohibits domestic violence, but the state and its informal agents have little ability to detect and address violations unless witnesses come forward. Petty corruption by bureaucrats (De Graaf 2010), endemic theft of local utilities (Smith 2004), and illegal logging (Tacconi 2012) present analogous governance problems due to an inability to monitor and punish violations.

Solutions to policy problems of this type come in many forms. New institutions may be created to realign incentives, as in the case of community organizations that distribute resources in return for environmental management (Brandt, Nolte, and Agrawal 2016). New technologies may make it easier to detect and prosecute corruption or theft (Smith 2004). The state may forcefully bring unruly sectors to heel, sometimes razing entire neighborhoods in the process (Scott 1998), or a softer approach may be taken to change attitudes and behaviors through public outreach campaigns. In

[16] For example, there is no effect on responses to questions such as, "Do you agree that it is more important that a boy goes to school than a girl?" or "Do you agree that the father not the mother should have the final say in the household?"

the case of violence against women, the latter approach has predominated. Intensive NGO-led efforts, such as the SASA! campaign in Uganda (Abramsky et al. 2014, 2016), have attempted to bring community members and leaders together to reduce acceptance of power abuses within the home and to build collective capacity to prevent and respond to violence against women (Wagman et al. 2015). Although these multi-faceted campaigns have sometimes proven effective in changing attitudes and reducing the frequency of violent incidents (see footnote 1), they are prohibitively expensive to bring to scale in the context of developing regions such as East Africa.

Mass media campaigns provide a potentially cost-effective alternative that can be deployed on a vast scale. The question is, to what extent and under what conditions do media campaigns generate policy-relevant changes in attitudes and behavior? Few media campaigns have been evaluated rigorously, and most randomized trials have focused on health-related messaging. Nevertheless, the literature has begun to offer theoretically informative insights concerning the conditions under which media messages shape attitudes and behavior. Studies have repeatedly found weak effects from information campaigns whose messages are not conveyed through dramatization. For example, radio campaigns in Africa designed to encourage hand-washing (Galiani, Gertler, and Orsola-Vidal 2012), early childhood check-ups (Sarrassat et al. 2015), and communication with public officials (Grossman, Humphreys, and Sacramone-Lutz 2014, 704) generated little apparent change in listener behavior. Education-entertainment programs have been more successful in changing behaviors. Blair, Littman, and Paluck (2017) found that embedding an encouragement to report corruption in a feature-length film induced Nigerian viewers to report hundreds of instances of corruption. Exposure to a yearlong radio soap opera reduced Rwandan listeners' deference to authority (Paluck and Green 2009). Although there has yet to be a direct experimental test of the relative effectiveness of information-only campaigns and education-entertainment campaigns, public health studies suggest that education entertainment has succeeded where information campaigns have failed (Banerjee, La Ferrara, and Orozco 2017; Banerjee, Barnhardt, and Duflo 2017).

Skeptics of media-induced effects have long contended that "propaganda" rarely succeeds in changing attitudes (Hovland, Lumsdaine, and Sheffield 1949; Berelson, Lazarsfeld, and McPhee 1954). In many ways, our inability to change core attitudes is in line with this literature. At the same time, our study demonstrates a new way in which eduction-entertainment campaigns can affect policy-relevant behaviors. We show that those who witness VAW tend be reluctant to report

what they know for fear of social sanctions. Our theory suggests that such reluctance can arise if the dominant perception is that violence is rare and that false accusations are common. Especially when it comes to sexual violence, the view that women tend to fabricate accusations is widely held in many contexts (Tuerkheimer 2017). Moreover, powerful actors who can act with impunity as long as anti-disclosure norms prevail will have incentives to sustain such beliefs. Just as defenders of gender hierarchy benefit from dismissing women's allegations of abuse as malevolent gossip, so too do corrupt politicians by subjecting whistle-blowers to a campaign of intimidation, defamation, or outright violence (Friebel and Raith 2004; Banerjee and Duflo 2006; Ting 2008).

An important question concerns the conditions under which education-entertainment can disrupt a non-disclosure equilibrium. In a concurrent study conducted in a nearby rural region, we found that willingness to report violence to police or community leaders was unaffected by our videos when they were shown to viewers individually in the context of a lab-like experiment, despite the close attention that respondents were paying to the treatment videos and short time that elapsed between exposure and outcome measurement. Apparently, many viewers will only speak out if they believe that others regard reports as credible, and communal exposure to anti-violence messages helps propagate this belief. A second scope condition for education-entertainment to encourage witnesses to speak out through the channels discussed above is that there must be a realistic expectation that community members and local authorities are willing to take action to stop the behavior in question were they to believe that it actually occurred. This condition implies that changes in local leadership can have important effects on reporting and, by extension, the prevalence of VAW, a potentially fruitful line of future research. In sum, the micro-politics of reporting socially harmful behavior in interdependent communities involves intricate strategic calculations, and forces that disrupt an anti-disclosure equilibrium can have profound policy consequences.

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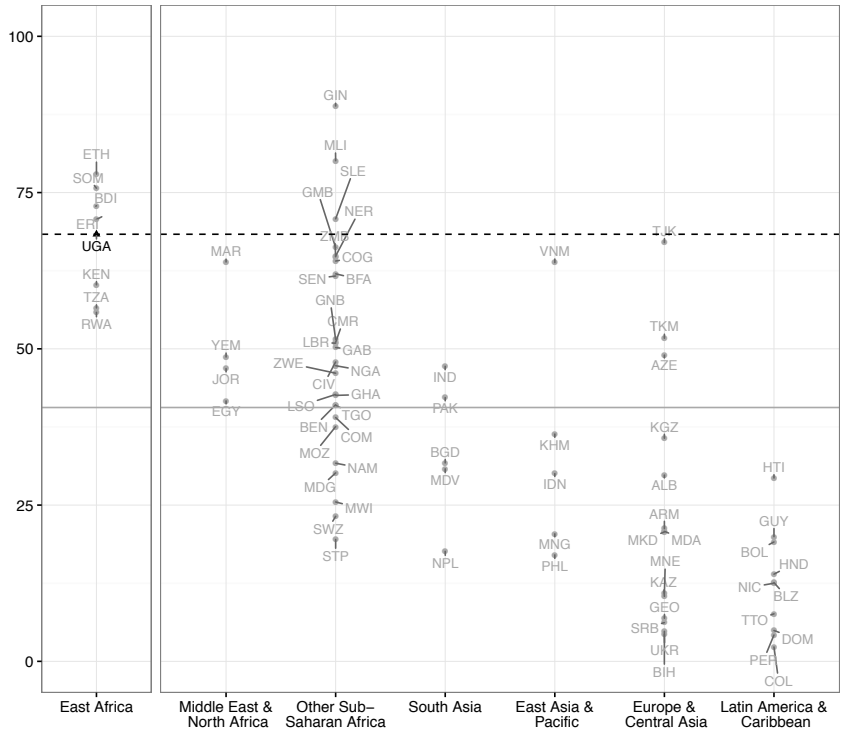

Figure 1: Average percentage of women who state that it is acceptable for a man to hit his wife in at least one of five scenarios.

Scenarios include: when she argues with him, burns the food, goes out without telling him, neglects the children, or refuses to have sex with him (DHS 2001-2015). Points show percentages by region and country, solid line shows average for all countries in sample, dashed line shows mean for Uganda.

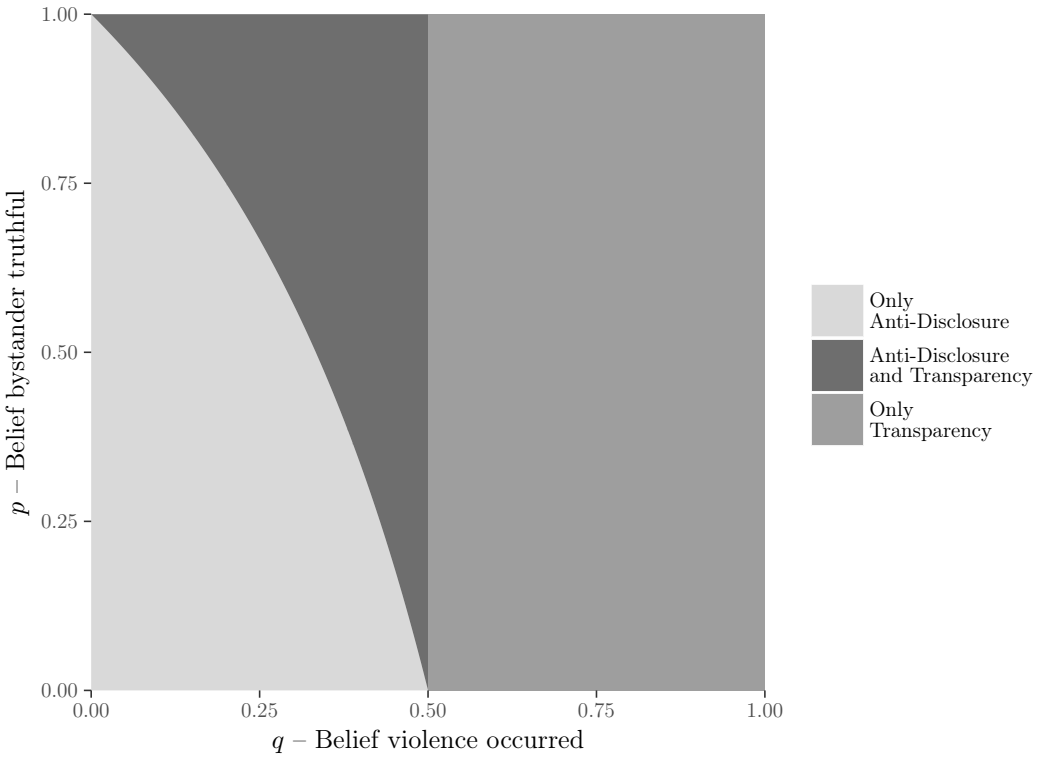

Figure 2: Existence of equilibria as a function of prior beliefs.

Plot assumes $\frac{s}{a} = 1$. See section B.4 of the Online Appendix for the full model.

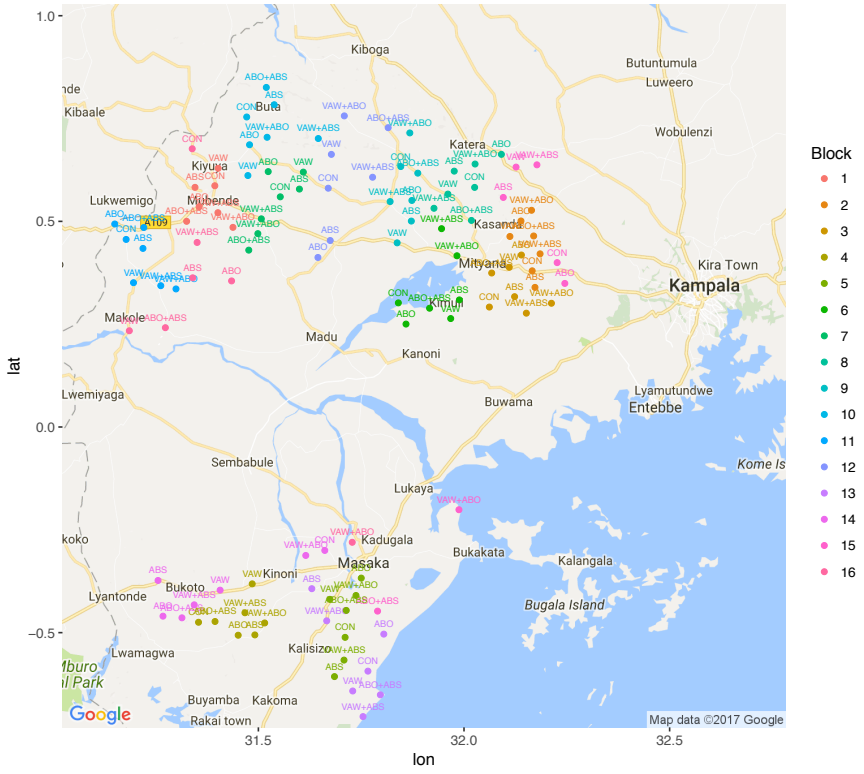

Figure 3: Clusters Included in the Study.

Colors indicate blocks within which random assignment occurred, while labels indicate the treatment condition to which the village was assigned.

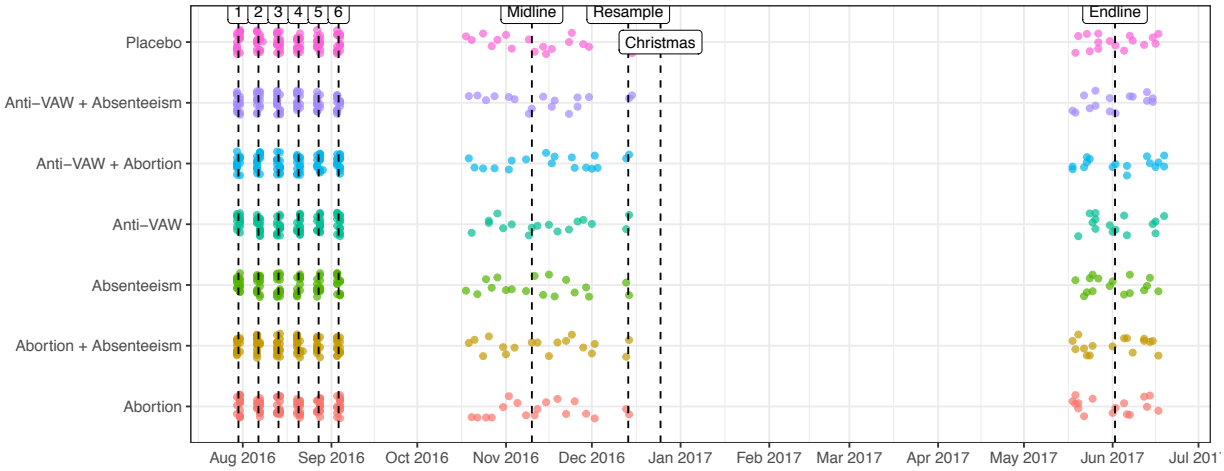

Figure 4: Timeline of media campaign, midline and endline surveys.

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. The film screenings numbered 1-6 featured the following Hollywood films, in order: Pirates of the Carribean; Slumdog Millionaire; Spy; The Fast and the Furious 7; Creed; and Oz The Great and Powerful.

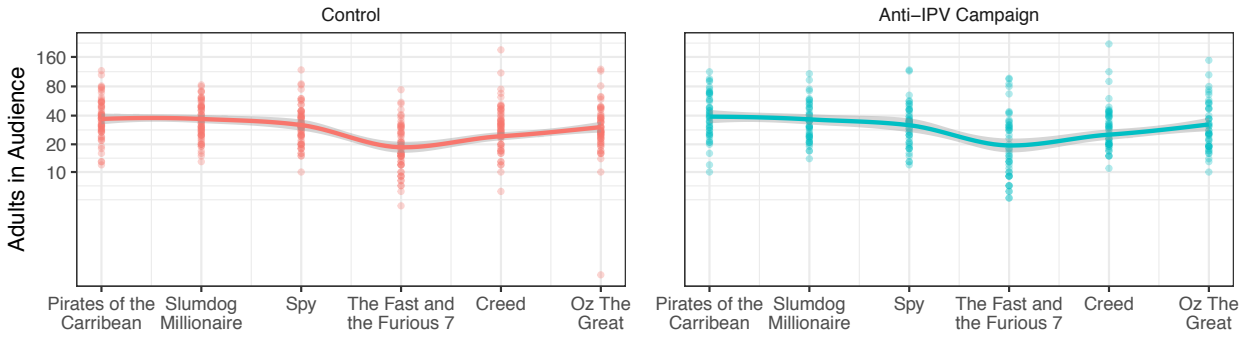

Figure 5: Adult attendance of screenings by treatment status.

The horizontal axis presents the films in chronological order. The vertical axis reports the number of adults attending a screening. Points represent a single screening, lines represent LOESS-smoothed average over time and confidence interval. Left panel reports only screenings in control villages, right panel reports attendance in villages assigned to anti-VAW campaign.

Figure 6: Excerpts from the Anti-VAW media campaign.

From top: a neighbor overhears the horrific screams of a woman experiencing a violent attack by her husband nextdoor but fails to report it to those who could intervene; after an initial violent incident in which Richard beats his wife, he eventually becomes so violent that one day she is hospitalized; the family of the woman who experienced violence intervenes in a couple's affairs, and report the issue further to the woman's counselor (*Nabakyala*).

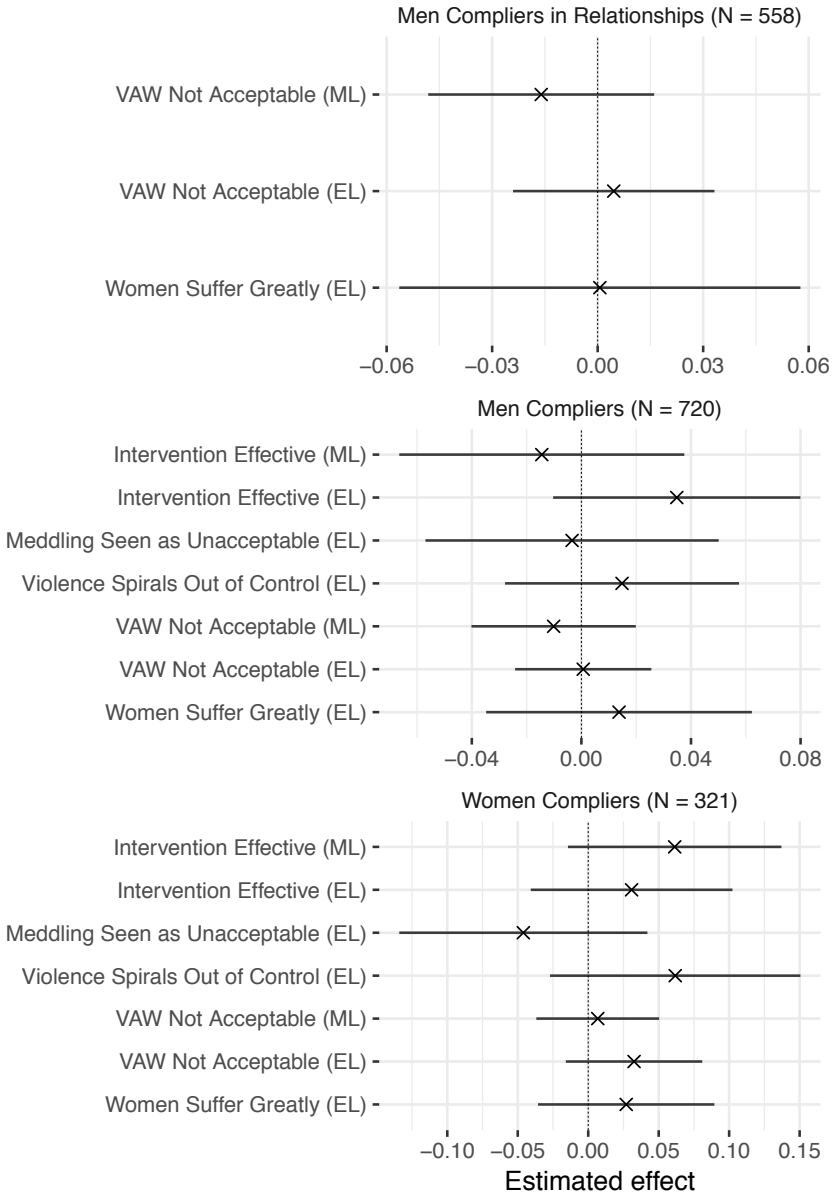

Figure 7: Alternative pathways through which the treatment could have decreased VAW or increased the willingness to report.

Crosses indicate the estimated effect size, bars indicate 90% confidence intervals that are computed using the standard normal approximation of the randomization distribution. See section E.1 of the Online Appendix for tables, section B.1 for details on model specifications and section A for details on question wording.

	Number of Incidents			Any Incidents			Violence Frequency	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Anti-VAW Media	−0.177	−0.146	−0.346	−0.069***	−0.048**	−0.132***	−0.296*	−0.676**
	(0.113)	(0.091)	(0.226)	(0.026)	(0.022)	(0.049)	(0.169)	(0.287)
Control Mean	0.56	0.59	0.59	0.19	0.2	0.2	0.42	0.42
RI p-values: IPV	0.128	0.159	0.138	0.009	0.038	0.007	0.088	0.014
Hypothesis	Two	Two	Two	Two	Two	Two	Two	Two
Sample	All W	All W	W compl.	All W	All W	W compl.	All W	W compl.
Analysis Level	Clus.	Indiv.	Indiv.	Clus.	Indiv.	Indiv.	Indiv.	Indiv.
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Estimator	OLS	OLS	OLS	OLS	OLS	OLS	Ordered Logit	Ordered Logit
Observations	110	1,036	356	110	1,036	356	1,036	356
Adjusted R^2	−0.033	0.002	−0.006	0.057	0.014	0.026		

*p<0.1; **p<0.05; ***p<0.01

Table 1: The effect of anti-VAW mass media on incidents of violence against women over the preceding six-month period (endline). All outcomes were measured during the endline survey. Analyses labelled "All W" are conducted among all women in the endline, regardless of compliance status, those labelled "W compl." are conducted among women compliers only. Analyses in columns labelled "Indiv." use individual respondents as the unit of observation, those labelled "Clus." are conducted at the village level, after collapsing individual responses to the cluster-level using cluster-level means. Columns 7 and 8 report results from an ordered logit model. All other estimates rely on OLS. See section B.1 of the Online Appendix for details on model specifications and section A of the Online Appendix for details on question wording.

Would report VAW to:	Parents		Counselor		LC1 Chairperson		Police		Reporting Index	
	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Anti-VAW Media	0.095*	0.144***	0.076*	0.171***	0.058	0.061	0.111***	0.130***	0.085***	0.126***
	(0.057)	(0.053)	(0.056)	(0.058)	(0.049)	(0.057)	(0.040)	(0.048)	(0.030)	(0.035)
Control Mean	0.44	0.46	0.52	0.58	0.4	0.22	0.2	0.16	0.39	0.36
RI p-values	0.068	0.008	0.1	0.004	0.133	0.148	0.008	0.008	0.006	0
Hypothesis	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	321	321	321	321	321	321	321	321	321	321
Adjusted R^2	0.053	0.010	0.020	0.044	0.002	0.019	0.002	0.018	0.039	0.076

*p<0.1; **p<0.05; ***p<0.01

Table 2: The effect among women compliers of anti-VAW mass media on attitudes towards sharing information about VAW. All analyses are run on the individual respondent level. See section B.1 of the Online Appendix for details on model specifications and section A of the Online Appendix for details on question wording.

Would report VAW to:	Parents		Counselor		LC1 Chairperson		Police		Reporting Index	
	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Anti-VAW Media	0.080**	0.037	0.037	0.055*	0.021	0.062*	−0.046	0.019	0.023	0.043*
	(0.035)	(0.036)	(0.037)	(0.034)	(0.034)	(0.036)	(0.025)	(0.035)	(0.019)	(0.025)
Control Mean	0.5	0.49	0.61	0.61	0.35	0.29	0.23	0.2	0.42	0.4
RI p-values	0.021	0.176	0.183	0.068	0.281	0.056	0.946	0.318	0.142	0.05
Hypothesis	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	720	720	720	720	720	720	720	720	720	720
Adjusted R^2	0.005	0.004	−0.014	0.008	0.002	0.003	0.001	0.007	0.001	0.019

*p<0.1; **p<0.05; ***p<0.01

Table 3: The effect among men compliers of anti-VAW mass media on attitudes towards sharing information about VAW.
All analyses use individual respondents as the unit of observation. See section B.1 of the Online Appendix for details on model specifications and section A for question wording.

	Social Repercussions		Personal Retribution		Community Would Intervene			
	Endline		Endline		Midline	Endline	Midline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Anti-VAW Media	−0.034	−0.114**	−0.021	−0.029	0.052*	0.043*	0.081*	0.068
	(0.034)	(0.051)	(0.041)	(0.060)	(0.029)	(0.029)	(0.057)	(0.047)
Control Mean	0.49	0.63	0.52	0.58	0.75	0.79	0.72	0.73
RI p-values	0.199	0.042	0.344	0.334	0.052	0.096	0.092	0.106
Hypothesis	Upr	Upr	Lwr	Lwr	Upr	Upr	Upr	Upr
Sample	Men	Women	Men	Women	Men	Men	Women	Women
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	720	321	720	321	720	720	321	321
Adjusted R^2	−0.001	0.002	0.003	0.010	0.020	0.016	−0.002	0.003

*p<0.1; **p<0.05; ***p<0.01

Table 4: The effect among compliers of anti-VAW mass media on perceptions of the social sanctions associated with reporting.
All analyses use individual respondents as the unit of observation. See section B.1 of the Online Appendix for details on model specifications and section A for question wording.

	Would Report IPV Incident		
	(1)	(2)	(3)
Others Observe	0.041**	0.043**	0.061**
	(0.023)	(0.023)	(0.033)
Anti-VAW Media		0.040*	0.061**
		(0.019)	(0.031)
Others Observe x Anti-VAW Media			−0.042
			(0.044)
Control Mean	0.81	0.79	0.79
Block FE	Yes	Yes	Yes
Observations	1,041	1,041	1,041
Adjusted R^2	0.0005	0.003	0.003

*p<0.1; **p<0.05; ***p<0.01

Table 5: The effect of bystander presence on willingness to report hypothetical incident of VAW among compliers (endline).

See section B.1 of the Online Appendix for details on model specifications and section A of the Online Appendix for details on question wording.

Online Appendix to 'Silence Begets Violence: A mass media experiment to prevent violence against women in rural Uganda'

Donald P. Green, Anna Wilke, Jasper Cooper

Contents

A Codebooks

This section provides information about the question wording and coding of outcome measures broken down by tables and figures in the main paper and the appendix.

The responses "Don't know" and "Refuse to answer" have been coded as missing for all outcomes.

A.1 Results in Paper

Table 1

Number of Incidents (Columns 1-3)

- Take a moment to think back over life in your household since last Christmas. During that time, can you remember any incidents in which a woman in your household, including yourself, was a victim of violence? If Yes: How many specific incidents since last Christmas can you remember when a woman in your household, including yourself, was a victim of intra-household violence?
  - Integer

Any Incidents (Columns 4-6)

- Recoding of Number of Incidents.
  - 1 = Number of Incidents > 0
  - 0 = Number of Incidents $= 0$

Violence Frequency (Columns 7-8)

- In many of the villages we have visited, men sometimes beat women. Thinking again of the time that has passed since last Christmas, would you say that this has happened more than about once a week to a woman in your household, including yourself? If yes: Did it happen just about once a week, or almost every day? If no: Did it happen about once a month, less than once a month, or almost never?
  - 0 = Almost never
  - 1 = Less than once a month
  - 2 = About once a month
  - 3 = Just about once a week
  - 4 = Almost every day

Tables 2 and 3

All outcomes in these tables are based on the following question stem:

- 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.

Each respondent was asked to make four decisions, each of which involved a choice between two options. In each pair of options, one option involved reporting to a village-level actor, while the other option implied inaction. Different respondents were randomly assigned to be faced with different pairs of options. The randomization was restricted in the following ways: Each pair contained an 'active' and an 'inactive' option. Respondents were never asked twice about the same action and the order of the 'active' options remained constant.

Report to Parents (Columns 1-2)

- Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.
  - 1 = I would talk to her parents and ask them to come by to help the couple find a peaceful solution
  - 0 = One of the following:
    - I would calm her down and tell her that the situation is bound to get better
    - I would express my sympathy for her but would tell her that every couple has to work it out for themselves
    - I would advise her to try harder to please her husband and things will likely improve
    - I would tell her that beating is often a sign of love and that she should try to work it out with her husband

Report to Counselor (Columns 3-4)

- Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.
  - 1 = I would notify the Nabakyala and ask her to mediate the dispute (If respondent is Nabakyala: I would try to mediate the dispute in my role as Nabakyaala)
  - 0 = One of the following:
    - I would calm her down and tell her that the situation is bound to get better
    - I would express my sympathy for her but would tell her that every couple has to work it out for themselves
    - I would advise her to try harder to please her husband and things will likely improve
    - I would tell her that beating is often a sign of love and that she should try to work it out with her husband

Report to LC1 Chairperson (Columns 5-6)

- Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.
  - 1 = I would get the LC1 chairperson involved (If respondent is an LC1 chairperson: I would get involved to mediate the dispute in my role as the LC1 chairperson)
  - 0 = One of the following:
    - I would calm her down and tell her that the situation is bound to get better

    - * I would express my sympathy for her but would tell her that every couple has to work it out for themselves
    - * I would advise her to try harder to please her husband and things will likely improve
    - * I would tell her that beating is often a sign of love and that she should try to work it out with her husband

Report to Police (Columns 7-8)

- Suppose there are only two actions that you can take. Please tell us which one you would prefer to take.
  - 1 = I would accompany her to the police to report the incident
  - 0 = One of the following:
    - * I would calm her down and tell her that the situation is bound to get better
    - * I would express my sympathy for her but would tell her that every couple has to work it out for themselves
    - * I would advise her to try harder to please her husband and things will likely improve
    - * I would tell her that beating is often a sign of love and that she should try to work it out with her husband

Reporting Index (Columns 9-10)

- The index is created by summing the variables Parents, Counselor, LC1 Chairperson and Police and then dividing by 4

Table 4

Social Repercussions (Columns 1-2)

- If you noticed your neighbor was beating his wife, and told other people about it, how would most of the people in your village react?
  - 0 = They would say that I did the right thing
  - 1 = They would scold me for gossiping and starting rumors

Personal Retribution (Columns 3-4)

- If you were to report such an incident to the a local leader (such as the LC1 or Nabakyala), do you think the family or friends of the husband would try to take revenge against you?
  - 0 = No
  - 1 = Yes

Community Would Intervene (Columns 5-8)

- If people in your community were to find out that a man was beating his wife or girlfriend, how would most of them react?
  - 0 = They would mind their own business and let the couple work it out on their own
  - 1 = They would either intervene themselves or get local leaders or the family to intervene

Table 5

Would Report IPV Incident

- Imagine that you are walking back from the garden one day, and through the bushes you see someone from your community beating his wife, who is silently crying and cowering in the corner of their home. (...) Continuing with the story of the wife and the husband that you observe on the way home from the garden
  - (Others Observe = 0): Suppose you know that you are the only who has observed the incident, would you report it to a local leader (such as the LC1 or Nabakyala)?
  - (Others Observe = 1): Suppose you see a group of people from your community standing nearby, discussing the incident, would you report it to a local leader (such as the LC1 or Nabakyala)?
  - 0 = No
  - 1 = Yes

Figure 7

VAW Not Acceptable

This outcome is an Index which created of the four items listed below. All respondents were asked the first item (disobey) but only one of the other three items. VAW Not Acceptable is created by averaging the two questions that a respondent was asked.

- In your opinion, does a man have a good reason to hit his wife if she disobeys him? If yes: Should she be slapped or should more force be used than that? If no: What if she persists in disobeying the husband? Does he then have good reason to hit her?
  - 0 = More force than that
  - $\frac{1}{3}$ = Slapped
  - $\frac{2}{3}$ = Yes
  - 1 = No
- In your opinion, does a man have a good reason to hit his wife if she spends a lot of time chatting with friends in the market?
  - 0 = Yes
  - 1 = No
- In your opinion, does a man have a good reason to hit his wife if she does not complete her household work to his satisfaction?
  - 0 = Yes
  - 1 = No
- In your opinion, does a man have a good reason to hit his wife if he is under immense financial pressure and becomes angry when she asks for money to buy food for the family?
  - 0 = Yes

- 1 = No

Women Suffer Greatly

- Imagine that you are walking back from the garden one day, and through the bushes you see someone from your community beating his wife, who is silently crying and cowering in the corner of their home. Some people say that women like her suffer greatly when they are hit by their husbands. Other people say that women like her tend to exaggerate their suffering. Which statement comes closest to your view?
  - 0 = Women like her tend to exaggerate their suffering
  - 1 = Women like her suffer greatly when they are hit by their husbands

Intervention Effective

- Suppose a man beats his wife almost every evening. Some people think that if friends and family take action, they can stop the violence. Others believe that meddling in the couple's affairs won't help because things will eventually go back to how they were. Which comes closest to your view?
  - 0 = Meddling in the couple's affairs won't help because things will eventually go back to how they were
  - 1 = If friends and family take action, they can stop the violence

Meddling Seen as Unacceptable

- In some places, people believe that it is acceptable to intervene in other people's private affairs if doing so might help to solve some problem. In other places, people believe that it is wrong to interfere in other people's private affairs. Is your community more like the first or the second kind of place?
  - 0 = More like the first kind
  - 1 = More like the second kind

Violence Spirals Out of Control

- Think of a man who has never beaten his wife before but beats her for a first time. Some people think he will probably not necessarily do so again. Others think that now that he has started beating her, he will continue to beat her, and soon her life will be in danger. Which view do you most agree with?
  - 0 = He will probably not do so again
  - 1 = Now that he has started beating her, he will continue to beat her, and soon her life will be in danger

A.2 Results in Appendix

Table 16

See the description of Tables 1 to 4 above.

Table 17

Reporting Index (Column 1)

- The index is created by summing the variables Parents, Counselor, LC1 Chairperson and Police and then dividing by 4.
- See description of outcomes in Tables 2 and 3 above for description of individual items.

Report to Parents (Column 2)
See description of outcomes in Tables 2 and 3 above for more detail.

- 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.
  - 1 = I would talk to her parents and ask them to come by to help the couple find a peaceful solution
  - 0 = One of the following:
    - I would calm her down and tell her that the situation is bound to get better
    - I would express my sympathy for her but would tell her that every couple has to work it out for themselves
    - I would advise her to try harder to please her husband and things will likely improve
    - I would tell her that beating is often a sign of love and that she should try to work it out with her husband

Report to Counselor (Column 3)
See description of outcomes in Tables 2 and 3 above for more detail.

- 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.
  - 1 = I would notify the Nabakyala and ask her to mediate the dispute (If respondent is Nabakyala: I would try to mediate the dispute in my role as Nabakyaala)
  - 0 = One of the following:
    - I would calm her down and tell her that the situation is bound to get better
    - I would express my sympathy for her but would tell her that every couple has to work it out for themselves
    - I would advise her to try harder to please her husband and things will likely improve
    - I would tell her that beating is often a sign of love and that she should try to work it out with her husband

Personal Retribution (Column 4)

- If you were to report such an incident to the a local leader (such as the LC1 or Nabakyala), do you think the family or friends of the husband would try to take revenge against you?
  - 0 = No
  - 1 = Yes

Tables 18 and 19

Any Incidents (Columns 1-2 in Table 18)

- Recoding of Number of Incidents (see description of Table 1 above)
  - 1 = Number of Incidents > 0
  - 0 = Number of Incidents $= 0$

Reporting Index (Columns 3-4 in Table 18 and Columns 1-2 in Table 19)

- The index is created by summing the variables Parents, Counselor, LC1 Chairperson and Police and then dividing by 4.
- See description of outcomes in Tables 2 and 3 above for description of individual items.

Social Repercussions (Column 5 in Table 18 and Column 3 in Table 19)

- If you noticed your neighbor was beating his wife, and told other people about it, how would most of the people in your village react?
  - 0 = They would say that I did the right thing
  - 1 = They would scold me for gossiping and starting rumors

Tables 20 to 23

See description of Tables 1 to 4 above.

Tables 24 and 25

See description of Tables 18 and 19 above.

Table 26

See description of Table 1 above.

Tables 27 to 29

See description of Figure 7 above.

Table 30

Number of Incidents (Comm.) (Columns 1-2)

Midline:

- As far as you know, roughly how many times has someone been badly hurt or beaten in your community since last Christmas? Just give your best guess. Of those incidents, how many of them involved a woman being beaten?
  - Integer

Endline:

- As far as you know, roughly how many times has someone been badly hurt or beaten in your community since last September? Just give your best guess. Of those incidents, how many of them involved a woman being beaten?

- Integer

Any Incidents (Comm.) (Columns 3-4)

- Same questiona as Number of Incidents (Comm.)
  - 1 = Number of Incidents (Comm.) > 0
  - 0 = Number of Incidents (Comm.) $= 0$

Viol. Freq. (Comm.) (Columns 5-6)

- In many of the villages we have visited, husbands sometimes beat their wives. Thinking back over the past two months, would you say that this happened more than about once a week in your community? If yes: Did it happen just about once a week, or almost every day? If no: Did it happen about once a month, less than once a month, or almost never?
  - 0 Almost never
  - 1 Less than once a month
  - 2 About once a month
  - 3 Just about once a week
  - 4 Almost every day

Number of Incidents (VHT) (Columns 7-8)
Midline:

- Since September, how many people have come to you with the following concerns? Violence against a woman in the family
  - Integer

Endline:

- Since December 2016, how many people have come to you with the following concerns? Violence against a woman in the family
  - Integer

Table 31

N EL W
Same variable as Number of Incidents in Table 1.

- Take a moment to think back over life in your household since last Christmas. During that time, can you remember any incidents in which a woman in your household, including yourself, was a victim of violence? If Yes: How many specific incidents since last Christmas can you remember when a woman in your household, including yourself, was a victim of intra-household violence?
  - Integer

Frq ML and Frq EL
Same variable as Viol. Freq. (Comm.) in Table 30.

- In many of the villages we have visited, husbands sometimes beat their wives. Thinking back over the past two months, would you say that this happened more than about once a week in your community? If yes: Did it happen just about once a week, or almost every day? If no: Did it happen about once a month, less than once a month, or almost never?
  - 0 Almost never
  - 1 Less than once a month
  - 2 About once a month
  - 3 Just about once a week
  - 4 Almost every day

Cnt ML and Cnt EL
Same variable as Number of Incidents (Comm.) in Table 30.
Midline:

- As far as you know, roughly how many times has someone been badly hurt or beaten in your community since last Christmas? Just give your best guess. Of those incidents, how many of them involved a woman being beaten?
  - Integer

Endline:

- As far as you know, roughly how many times has someone been badly hurt or beaten in your community since last September? Just give your best guess. Of those incidents, how many of them involved a woman being beaten?
  - Integer

Cnt VHT
Same as variable as Number of Incidents (VHT) in Table 30.
Midline:

- Since September, how many people have come to you with the following concerns? Violence against a woman in the family
  - Integer

Endline:

- Since December 2016, how many people have come to you with the following concerns? Violence against a woman in the family
  - Integer

Tables 33 and 34

Any Incidents

- Recoding of Number of Incidents (see description of Table 1).
  - 1 = Number of Incidents > 0
  - 0 = Number of Incidents $= 0$

Table 35

VAW Not Acceptable

This outcome is an Index which created of the four items listed below. All respondents were asked the first item (disobey) but only one of the other three items. VAW Not Acceptable is created by averaging the two questions that a respondent was asked.

- In your opinion, does a man have a good reason to hit his wife if she disobeys him? If yes: Should she be slapped or should more force be used than that? If no: What if she persists in disobeying the husband? Does he then have good reason to hit her?
  - 0 = More force than that
  - $\frac{1}{3}$ = Slapped
  - $\frac{2}{3}$ = Yes
  - 1 = No
- In your opinion, does a man have a good reason to hit his wife if she spends a lot of time chatting with friends in the market?
  - 0 = Yes
  - 1 = No
- In your opinion, does a man have a good reason to hit his wife if she does not complete her household work to his satisfaction?
  - 0 = Yes
  - 1 = No
- In your opinion, does a man have a good reason to hit his wife if he is under immense financial pressure and becomes angry when she asks for money to buy food for the family?
  - 0 = Yes
  - 1 = No

Reporting Index

- The index is created by summing the variables Parents, Counselor, LC1 Chairperson and Police and then dividing by 4.
- See description of outcomes in Tables 2 and 3 above for description of individual items.

Any Incidents

- Recoding of Number of Incidents (see description of Table 1).
  - 1 = Number of Incidents > 0
  - 0 = Number of Incidents $= 0$

B Supplementary Information

This section of the appendix provides non-statistical information not included in the main text of the article.

- Subsection B.1 explains the model specifications used in the main analyses, the meaning of the rows at the bottom of the tables, and how summary statistics are calculated.
- Subsection B.2 explains the narrative of the three anti-VAW videos employed in the campaign.
- Subsection B.3 provides a collection of anti-VAW mass media campaigns from around the world.
- Subsection B.4 presents our formal model of the way in which beliefs about the prevalence of violence and gossiping affect norms around disclosure.

B.1 Explanatory Note on Main Tables

Unless otherwise indicated, analyses use individual respondents from our household surveys as the unit of observation. Some analyses are conducted at the village level, after collapsing individual responses to the cluster level using cluster-level means. Where applicable, this is indicated in the row labeled "Analysis Level" in the respective tables. Information about the sample on which analyses are conducted is included in the table caption and in the row "Sample". "All W" stands for "All Women" and "W. compl." stands for "Women compliers". HH stands for all respondents from the household survey, $HH(M)$ stands for men respondents from the household survey and VHT stands for our surveys with members of Village Health Teams.

For all individual-level analyses, standard errors are clustered at the level of the village. As specified in our pre-analysis plan, all analyses condition on the average audience size over the six screenings and all analyses run on the individual level condition on an indicator for whether the respondent was included in follow-up sampling. Specifications with "Block FE" include an indicator variable for the cluster's block. No other covariates are included unless this is indicated in the table. Specifications with "Covariates" also include a set of covariates which are taken from the endline and selected through a lasso regression procedure as explained in our pre-analysis plan.

All p-values are calculated using the pre-registered randomization test in which the treatment is permuted 2000 times to simulate effects under the sharp null hypothesis of no effects for all units. The row labeled "Hypothesis" in each table indicates the direction of the hypothesis test (two-tailed, lower, upper) for each column. As pre-registered, outcome values that are missing at the respondent-level are imputed using the pre-registered multiple imputation through chained equations (MICE) approach, conditioning only on outcomes from the same family. We show in section D.3 below that the results are robust to using a listwise deletion approach to missingness (no imputation).

B.2 Narrative for Anti-VAW Videos

The plot structure involves two parallel stories of domestic violence. Whereas one ends tragically, in the other, community intervention prevents the continuation of violence. The first vignette revolves around Miriam, a middle-aged woman whom we see returning home from choir practice. As she begins to prepare dinner for her family, the focus shifts to her husband Richard, standing in line at the local mill. Having deposited his sack of maize at the mill, Richard overhears two other villagers discussing his inadequacy for a position on the local savings cooperative Richard had been seeking. Visibly upset, Richard returns home and orders his wife to make him tea. Quick thereafter, he demands a bath. Sensing her husband's tension, Miriam hurries to obey his commands. Yet, given the unavailability of a second kettle, she is forced to choose between preparing the tea and the bath and prioritizes the latter. Richard ignores her attempted explanation, becoming more and more upset until he begins to beat her. We see a neighbor emerge from her house, alerted by Miriam's cries. She sighs at Richard's actions, making clear this is not the first time that the neighbor has overheard Richard beating his wife. She pauses as if weighing what to do but ultimately takes no action. It is implied that the neighbor could have intervened at this point, even in the absence of a cry for help by the victim.

The second vignette is set in a hospital room. Miriam lies in bed breathing through an oxygen mask, surrounded by her family. The narrator explains that Richard's beating is responsible for Miriam's condition. We hear Richard's thoughts as he regrets his lack of self-restraint, and those of Miriam's parents, who wish that they had intervened when Miriam reached out. The video also depicts the emotional pain of Miriam's daughter who has witnessed Richard's attack. The vignette ends with Miriam's parents mourning her death.

At the beginning of the third vignette, we watch Miriam's funeral while the narrator recalls what happened. The story then shifts to a neighboring village where we encounter another family with a similar problem. Again, we are introduced to a wife who is beaten by her husband when he is frustrated for reasons beyond her control and in spite of her efforts to please him. This time, however, the wife receives help from her parents to whom she reaches out.[1]

Her parents arrive in the family's home, and we see the father talking to his son-in-law, giving him advice on how to find peaceful solutions when conflict arises. The mother consoles her daughter, reminding her domestic violence is unacceptable. The family consults the women representative of the village, who steps in and monitors the situation. The video closes with the situation improving.

At the end of each video, the narrator reaffirms the need for community members to take action when encountering domestic violence.

[1]In principle, this may be construed as putting the onus on victims to solve their own problems, rather than portraying the society or the state as responsible for adopting a proactive stance against VAW. However, it is worth emphasizing that this narrative choice does not reflect a normative statement about victims' responsibilities, but a positive description on the part of the Ugandan scriptwriter to accurately reflect the difficulties such situations pose. In many cases, only the victim may know about a violent incident, and so it may indeed be necessary for victims to speak out.

B.3 Anti-VAW Media Campaigns Around the World

Campaign	Country	Implementer	URL
Alrokam	Arab States	UN Women Regional Office for Arab States	https://www.youtube.com/watch?v=o2Wt0CmpgyQ
Strong Hands Stop VAWG	Thailand	Bangkok Mass Transit Authority	https://www.youtube.com/watch?v=6YyvNQiaqd0
How to Be More Than a Bystander	Canada	Ending Violence Association	https://www.youtube.com/watch?v=AlcRzaaZaqw
Isn't It Time Someone Called CUT!	UK	Women's Aid	https://www.youtube.com/watch?v=j4RjsqPYaS0
Rape - It's Your Fault	India	All India Bakchod	https://www.youtube.com/watch?v=8hC0Ng_ajpY
Le film choc	France	Féderation Nationale Solidarité Femmes	https://vimeo.com/17086120
Monsters in the Closet	USA	National Domestic Violence Hotline	https://www.youtube.com/watch?v=LbRba9XHKKw
Stairs	Germany	Bundesverband Frauenberatungsstellen und Frauennotrufe	https://www.youtube.com/watch?v=l7HqwAleUQg
Ring the Bell - Bell Bajao	India	Breakthrough, Ministry of Women and Child Development, UNIFEM, UN Trust Fund	https://www.youtube.com/watch?v=9t3BPv8tBP4
The Johannesburg Drums Experiment	South Africa	People Opposing Women Abuse	https://www.youtube.com/watch?v=BW30WslahMc
Who Are You?	New Zealand	Who Are You Campaign	https://www.youtube.com/watch?v=iUj2OHLAG3w
Who Will You Help?	Canada	Ontario Government	https://www.youtube.com/watch?v=opPb2E3bkoo
It Rarely Stops	USA	National Domestic Violence Hotline	https://www.youtube.com/watch?v=WL3rfk2iFww
Do You See Her?	UK	Women's Aid	https://www.youtube.com/watch?v=sk0YJsDXx24
Ring the Bell	China	UN Women	https://www.youtube.com/watch?v=abkj99Zx8Kg
Violencia de Género	Mexico	Tecate Beer	https://www.youtube.com/watch?v=f9E5h_i_KYA
Domestic Violence Awareness, Prevention and Support	USA	Mexican Consulate	https://www.youtube.com/watch?v=u8kzV9abPbw
No More	USA	NFL	https://www.youtube.com/watch?v=rTJT3fVv1vU
Marie et Fred	Belgium	Wallonia-Brussels Federation	https://www.youtube.com/watch?v=aj27cA6irvU

Table 1: Examples of anti-VAW media campaigns from around the world.
Trigger warning: Videos contain depictions of graphic violence.
All videos accessed the 28th of November 2017.

B.4 A Model of Information Disclosure and Social Sanctioning

We build a simple formal model to demonstrate the logic of our theoretical argument. We focus on a one-shot interaction between an informed actor who knows whether a violent incident has taken place and an uninformed actor who is uncertain about whether violence has occurred. The informed actor could be the potential victim of violence or a neighbor who has inside knowledge about the

situation in a close-by household. For simplicity, we will refer to the informed actor as 'the witness' and to the uninformed actor as 'the community'. The state of the world can be such that violence has taken place ($\theta = V$) or such that no violence has occurred ($\theta = \neg V$). The witness is fully informed about the state of the world and the community's prior belief is that violence occurs with probability $q \in (0, 1)$. After the state of the world has been determined, the witness makes a choice $R \in \{0, 1\}$ of whether to report to the community that violence has taken place or not. If the witness does not make a report, the game ends. If the witness makes a report, the community observes this decision and chooses to either intervene to prevent future violence ($S = 1$) or to levy social sanctions against the witness for 'gossiping' ($S = 0$).

The community faces uncertainty with regard to the type of the witness. The witness can be a gossip ($\gamma = G$) or a truthful type ($\gamma = T$). To simplify, we assume that gossips always report that violence has taken place, irrespective of the state of the world. Treating them as behavioral types is equivalent to assuming that they receive so much utility from making accusations that they will always do so. Note that gossips may not actually exist in the population. Instead, the model could reflect a situation in which communities mistakenly believe that they exist. A truthful witness acts strategically and receives a benefit $b > 0$ from community intervention if and only if violence actually occurred. When reporting violence to the community, a truthful witness incurs an opportunity cost $c < b$. She suffers an additional reputation cost $r > 0$ if she is publicly scolded for gossiping. The utility of a truthful witness can be summarized as

$$u_w(R, S, \theta | \gamma = T) = \begin{cases} b - c & \text{if } R = 1 \text{ and } S = 1 \text{ and } \theta = V, \\ -c & \text{if } R = 1 \text{ and } S = 1 \text{ and } \theta = \neg V, \\ -c - r & \text{if } R = 1 \text{ and } S = 0 \\ 0 & \text{otherwise} \end{cases} \tag{1}$$

The witness is aware of her type. The community believes that the witness is of the truth-telling type with probability $p \in (0, 1)$. The community gains a benefit $a > 0$ from taking action to stop violence if and only if violence has taken place and a benefit $s > 0$ from scolding a witness if and only if the witness gossips for no reason, – i.e. reports that violence has taken place if the state of

the world is $\neg B$).[2]

$$u_c(R, S, \theta) = \begin{cases} a & \text{if } R = 1 \text{ and } S = 1 \text{ and } \theta = V, \\ s & \text{if } R = 1 \text{ and } S = 0 \text{ and } \theta = \neg V \\ 0 & otherwise \end{cases} \tag{2}$$

Results

Gossips in this model never change their behavior. We therefore focus on the behavior of truthful witnesses. A truthful witness will never report that violence occurred if it did not in fact occur, since doing so is costly and does not yield any benefits to a truthful type. There can thus be only two types of pure strategy equilibria in this game[3]: an equilibrium in which truthful witnesses never report that violence took place, irrespective of whether it did, and an equilibrium in which truthful witnesses report violence if and only if it happened. We are interested in the conditions under which these two equilibria exist. The following proposition summarizes the result.

Proposition 1. *Assume that witnesses who are gossips always report that violence took place.*

- ***Anti-disclosure norm only.*** *For* $\frac{q}{1-q} < \frac{q}{(1-p)(1-q)} < \frac{s}{a}$*, the only existing equilibrium is one in which a truthful witness does not report that violence has occurred irrespective of whether it did and the community sanctions witnesses who report violence.*

- ***Anti-disclosure norm and Transparency norm.*** *For* $\frac{q}{1-q} < \frac{s}{a} < \frac{q}{(1-p)(1-q)}$*, there are two equilibria: 1) The truthful witness does not report that violence has occurred irrespective of whether it did and the community sanctions witnesses who report violence. 2) A truthful witness reports violence if and only if it occurred, and, in response to a report, the community takes action to prevent violence instead of sanctioning the witness.*

- ***Transparency norm only.*** *For* $\frac{s}{a} < \frac{q}{1-q} < \frac{q}{(1-p)(1-q)}$*, the only existing equilibrium is one in which a truthful witness reports violence if and only if it occurred, and, in response to a*

[2] If we assume, instead, that communities derive utility from scolding witnesses who are gossiping types – regardless of whether violence has occurred or not – the results would be very similar. One difference would be that the anti-disclosure equilibrium that we drive below would always exist.

[3] We look for Perfect Bayesian Equilibria.

report, the community takes action to prevent violence instead of sanctioning the witness.

$\frac{q}{(1-p)(1-q)}$ is increasing in p and q. Hence, at low levels of p and q, only the anti-disclosure equilibrium exists. An equilibrium in which those who are informed speak out about the violence inflicted on women (transparency norm) can only be sustained if the community believes that violence happens sufficiently frequently or that gossips are rare. In a region where q is not too high, both equilibria may exist if the community is optimistic enough about the prevalence of truthful types. If the community comes to believe that violence is very frequent, the anti-disclosure norm ceases to be an equilibrium and only the transparency norm prevails.

Proof. Suppose first that we are in the equilibrium where the truthful witness reports that there was violence if and only if violence has actually occurred, i.e. $R(V|\gamma = T) = 1$ and $R(\neg V|\gamma = T) = 0$. If the community observes a report by the witness, she updates her beliefs according to Bayes' rule. Her posterior belief q' that violence has occurred is given by

$$\begin{aligned} q' = prob(V|R=1) &= \frac{prob(R=1|V) * prob(V)}{prob(R=1|V) * prob(V) + prob(R=1|\neg V) * prob(\neg V)} \\ &= \frac{q}{1-p+pq}. \end{aligned} \tag{3}$$

Recall that the community receives a benefit a from intervening if violence has occurred and a benefit s from scolding a witness who reports that violence has occurred if it has not. Conditional on observing a report, the difference between the community's expected utility from intervening to stop violence and from scolding the witness is thus $\Delta u_c = q'a - (1-q')s$. The community will intervene to stop violence if $\Delta u_c > 0$ and scold the witness otherwise. Hence, the community will intervene to prevent violence if

$$\frac{q}{(1-p)(1-q)} > \frac{s}{a}. \tag{4}$$

Next, consider the decision of the truthful witness. Suppose that no violence has occurred, i.e. $(\theta = \neg V)$. If the truthful witness reports that violence has occurred, she suffers the opportunity cost

c of reporting and earns no benefit, since truthful types gain nothing by making false accusations. Hence, regardless of the community's response, a truthful witness will never make a report if violence has not occurred. Second, suppose a violent incident took place, i.e. $\theta = V$. If $\frac{q}{(1-p)(1-q)} > \frac{s}{a}$, the community will intervene to prevent violence upon observing a report. In this case, the witness will earn a payoff of $b - c$ from making a report and 0 from remaining silent. The witness will thus find it optimal to report the violent incident. If $\frac{q}{(1-p)(1-q)} < \frac{s}{a}$ the community will not intervene when observing a report. The witness will earn $-c - r$ from making a report and 0 from not reporting that violence happened. The witness will thus not find it optimal to report the violent incident. In other words, the transparency equilibrium exists for $\frac{q}{(1-p)(1-q)} > \frac{s}{a}$. Note that the left-hand side of this inequality is increasing in q and in p, which establishes that the transparency equilibrium exists for q and p large enough.

Second, consider the equilibrium where the truth-telling witness never intervenes regardless of whether violence has taken place, i.e. $R(V|\gamma = T) = R(\neg V|\gamma = T) = 0$. When would a truthful witness find it optimal to not report a violent incident that has taken place? If she does not make a report, she receives a payoff of 0. If she does make a report, her payoff depends on the response by the community. Since we are considering an equilibrium in which truthful types never report, the community infers that any reports must come from witnesses who are gossips. Gossips do not condition their reporting decision on the state of the world. Hence, the community cannot infer anything about the occurrence of violence from a report and her posterior belief equals her prior q. Following the same reasoning as above, the community will choose to sanction the witness if

$$\frac{q}{1-q} < \frac{s}{a} \tag{5}$$

and will intervene to stop future violence from happening otherwise. Conditional on violence having occurred, the truthful witness finds it optimal to refrain from reporting provided that the community would sanction the witness were she to make a report. Therefore, the anti-disclosure equilibrium exists if the condition in equation 5 holds. Note that $\frac{1}{1-p} > 1$. Combining equations 4 and 5, we arrive at the cut offs for equilibrium existence given in the proposition.

□

C Identification Strategy

This section of the appendix presents the main identifying assumptions behind our study and tests them empirically.

- Subsection C.1 presents the key estimands of the study and shows how we estimate them. It provides a formal definition of the cross-over assumptions underlying the design and describes the characteristics of the subpopulations of interest (compliers and non-compliers).
- In subsection C.2 we provide a formal test of the hypothesis that compliance is unaffected by the treatment, both at the individual and cluster level. We are unable to reject the hypothesis that the treatment does not affect the probability that an individual went to view one of the screenings. We also show that the number of attendees of the screenings – as measured by enumerators – does not bear a statistically signficant relationship to the treatment. These results support the notion that the treatment did not cause people to attend or avoid going to the films at all.
- Results presented in subsection C.3 support the claim that the treatment did not cause people with different observable characteristics to attend the films. We assess balance on a range of covariates among various subsamples in our data, and find roughly 5% of covariates are significantly imbalanced at the $\alpha = 5\%$ level, which is consistent with the null hypothesis of no imbalance.

C.1 Estimation of Complier Average Causal Effects

We denote a vector of random assignments, $\mathbf{z}^m$, where the superscript indicates the message to which the respondent was assigned,

$$m \in \{\text{placebo}, \text{VAW}, \text{abortion}, \text{absenteeism}\},$$

$z_i^m = 1$ when individual i in village j was assigned to message m, 0 otherwise. We can then define a respondent-level compliance function, $d_i^m(z^m)$, where d_i^m indicates the actual treatment the respondent received. Let $d_i^m = 1$ when the respondent attended at least one film showing message

m, and $d_i^m = 0$ otherwise. A "complier" is thus any respondent for whom $d_i^m(z^m = 1) = 1$; a never-taker is any respondent for whom $d_i^m(z^m = 1) = 0$. .

Since every village (cluster) screened the video message to which it was assigned (including screenings in the control condition, where films were shown with no treatment vignettes), we assume that these are the only compliance types in the population. We make two additional assumptions tested formally below. First, we assume $d^{m=k}(z^{m=k}) = d^{m=l}(z^{m=l})$, for all k and l. In other words, we assume that the specific treatment condition does not affect compliance. Given this assumption, the placebo "reveals" the same compliers as the other treatments. Second, we assume that $Y^{m=k}(z^{m\neq k} = 1) = Y^{m=k}(z^{m=k} = 0)$, for all k. In other words, we assume no crossover effects: m-specific outcomes are unaffected by assignment to non-m treatments. Most importantly for the results presented in this paper, we assume that VAW-specific outcomes are unaffected by the absenteeism and abortion messages. We relax this assumption below, when we consider crossover effects and agnostic estimators.

In our main specifications, we are interested in the following causal estimand

$$\tau_{\text{com}} = E[(Y_i^{VAW}(d_i^{VAW}(z^{VAW} = 1))) - Y_i^{VAW}(d_i^{VAW}(z^{VAW} = 0))) \mid d_i(1) = 1], \tag{6}$$

which reveals the average causal effect of the VAW treatment messages on VAW-related outcomes among compliers.

We estimate τ_{com} by fitting the following linear model among subsets of our data containing only compliers:

$$Y_{ij}^{VAW} = \alpha + \tau z_j^{VAW} + \mathbf{X}_j^{\top}\boldsymbol{\gamma} + \delta r_{ij} + \epsilon_{ij}, \tag{7}$$

where Y_{ij}^{VAW} is the outcome of interest for individual i in cluster (village) j, α is an intercept and z_j^{VAW} is a treatment assignment indicator which takes the value 1 if a respondent resides in a village which was assigned to the VAW treatment. $\mathbf{X}_j^{\top}$ is a vector containing block indicators and a the average audience size across all screenings that took place in a given village. r_{ij} is an indicator for whether respondent i was part of the second round of endline sampling. ϵ_{ij} is an individual-level error term which is adjusted for clustering at the village level. Consistent with the pre-analysis

plan, we report one-sided p-values for most of our outcomes and two-tailed p-values for some. P-values are calculated through randomization inference by computing the sampling distribution of the estimator under the sharp null of no (positive) effect for all units.[4]

The regression model above estimates the average treatment effect among individual compliers. One could conduct an analogous estimation at the cluster level after collapsing the outcome to cluster means. The latter approach has the advantage of simplicity but changes the weighting of individual respondents (giving more weight to respondents from clusters with fewer compliers).

This estimation approach assumes no effect of treatment on compliance. Table 2 shows descriptive statistics of a set of covariates for adult respondents by compliance type. In some analyses we do not condition the analysis on attendance, and simply analyze effects among all randomly sampled respondents.

C.2 Orthogonality of Compliance and Treatment

In a placebo-controlled design, the average treatment affect among compliers can be identified by subsetting to those who complied in each condition, provided that respondents who comply have the same potential outcomes distributions in expectation. In our study, this implies the treatment did not affect *who* attended the treatment.

We test whether the compliance status of respondents is affected by the treatment by computing a likelihood ratio permutation test, modeling the compliance status of the respondent as the outcome of a logit data-generating process. The results are displayed in table 3. First, we compare a model that includes only block fixed effects and a resample indicator to one that also includes an indicator for whether a cluster was assigned to the VAW treatment condition. Second, we also report results from the most principled test of the null of no effect of the treatment on compliance status, insofar as it uses all 7 arms of the treatment and does not require the assumption that compliance is unaffected by combinations of the treatment. Both tests are done among all respondents and separately among men and women. Finally, we also test whether the number of compliers per cluster (focusing on compliers that were interviewed in both the midline and the endline) is affected by treatment. We do so by using an F-test to compare a linear model that predicts the number of compliers using

[4]We also ran all our analyses including covariates. As pre-specified in our pre-analysis plan, we used lasso regression to select the minimal number of covariates that best predict each outcome from a large set of covariates, and included only these in our estimation. As can be seen in section D.3 of this appendix, the results do not change much.

	Mean among non-compliers	Mean among all compliers	Mean among panel compliers
Woman	0.55 (n = 4372)	0.31 (n = 1156)	0.31 (n = 1041)
Age (in yrs)	32.07 (n = 4372)	29.13 (n = 1156)	29.59 (n = 1041)
Less than 8 yrs of education	0.65 (n = 4370)	0.67 (n = 1156)	0.67 (n = 1041)
Married or living as married	0.75 (n = 4370)	0.71 (n = 1156)	0.72 (n = 1041)
Ever been to big city	0.74 (n = 4372)	0.81 (n = 1156)	0.8 (n = 1041)
Consumes news every day	0.67 (n = 4369)	0.73 (n = 1155)	0.74 (n = 1040)
Uses mobile phone every day	0.71 (n = 4365)	0.71 (n = 1153)	0.73 (n = 1039)
Main language is Luganda	0.86 (n = 4372)	0.87 (n = 1156)	0.88 (n = 1041)
Catholic	0.43 (n = 4372)	0.47 (n = 1156)	0.47 (n = 1041)
Protestant	0.15 (n = 4372)	0.16 (n = 1156)	0.16 (n = 1041)
Muslim	0.15 (n = 4372)	0.15 (n = 1156)	0.16 (n = 1041)
Prays at least once a day	0.84 (n = 4369)	0.79 (n = 1156)	0.79 (n = 1041)
Number of rooms in house	2.73 (n = 4372)	2.47 (n = 1156)	2.49 (n = 1041)
Mud wall	0.24 (n = 4372)	0.26 (n = 1156)	0.25 (n = 1041)
Brick wall	0.6 (n = 4372)	0.59 (n = 1156)	0.6 (n = 1041)
Owns radio	0.81 (n = 4372)	0.83 (n = 1156)	0.83 (n = 1041)
Owns TV	0.28 (n = 4372)	0.19 (n = 1156)	0.19 (n = 1041)
Owns cell phone	0.8 (n = 4372)	0.79 (n = 1156)	0.8 (n = 1041)

Table 2: Characteristics of Adult Respondents by Compliance Type

block fixed effects and the resamlpe indicator to one that also includes an VAW treatment indicator or the full set of treatment indicators for 6 of the 7 treatment conditions. We again do this test separately for the number of men and women compliers. All tests are done using randomization inference. Moreover, we show in Table 4 that the number of men and women who attended the film screenings, as reported by enumerators who were present during the screenings, is balanced across treatment conditions.

Taken together, these tests support the contention that our design is able to recover the complier average treatment effect (CATE) by subsetting analysis to compliers.

Restricted Model	Unrestricted Model	Sample	P-Value
Pr(Complier| Block + Resample Indicators)	Pr(Complier| Block + Resample + VAW Treatment Indicators)	All Respondents (ML, N = 5528)	0.507
Pr(Complier| Block + Resample Indicators)	Pr(Complier| Block + Resample + 7 Treatment Condition Indicators)	All Respondents (ML, N = 5528)	0.091
Pr(Complier| Block + Resample Indicators)	Pr(Complier| Block + Resample + VAW Treatment Indicators)	Women (ML, N = 2743)	0.250
Pr(Complier| Block + Resample Indicators)	Pr(Complier| Block + Resample + 7 Treatment Condition Indicators)	Women (ML, N = 2743)	0.152
Pr(Complier| Block + Resample Indicators)	Pr(Complier| Block + Resample + VAW Treatment Indicators)	Men (ML, N = 2785)	0.923
Pr(Complier| Block + Resample Indicators)	Pr(Complier| Block + Resample + 7 Treatment Condition Indicators)	Men (ML, N = 2785)	0.207
E(N Compliers| Block + Resample Indicators)	E(N Compliers| Block + Resample + VAW Treatment Indicators)	110 Clusters	0.528
E(N Compliers| Block + Resample Indicators)	E(N Compliers| Block + Resample + 7 Treatment Condition Indicators)	110 Clusters	0.108
E(N Women Compliers| Block + Resample Indicators)	E(N Compliers| Block + Resample + VAW Treatment Indicators)	110 Clusters	0.248
E(N Women Compliers| Block + Resample Indicators)	E(N Compliers| Block + Resample + 7 Treatment Condition Indicators)	110 Clusters	0.228
E(N Men Compliers| Block + Resample Indicators)	E(N Compliers| Block + Resample + VAW Treatment Indicators)	110 Clusters	0.968
E(N Men Compliers| Block + Resample Indicators)	E(N Compliers| Block + Resample + 7 Treatment Condition Indicators)	110 Clusters	0.120

Table 3: Tests of the assumption that treatment does not affect compliance.

	Placebo	VAW	ABO	ABS	ABO+ABS	VAW+ABO	VAW+ABS	p-value
Average women attendees per screening	11.62	10.99	10.24	10.85	12.36	14.78	11.32	0.52
Total women attendees per trading center	69.75	65.94	61.44	65.12	74.19	88.69	67.94	0.52
Average men attendees per screening	23.83	24.59	22.49	24.97	20.59	26.53	21.08	0.75
Total men attendees per trading center	143.00	147.56	134.94	149.81	123.56	159.19	126.50	0.75
Average attendees per screening	35.46	35.58	32.73	35.83	32.96	41.31	32.40	0.73
Total attendees per trading center	212.75	213.50	196.38	214.94	197.75	247.88	194.44	0.73

Table 4: Balance of reported attendance across all conditional treatment conditions.

C.3 Balance on Covariates

We examine balance on observable pre-treatment covariates, focusing primarily on the subsamples from the midline and endline survey data that are used to estimate the main results. For each covariate in each subsample, 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.

Note that balance tables are always estimated using all available covariates. The number of covariates included in the balance tables nevertheless varies as a function of: a) whether the tests are conducted among the midline or endline data (different questions were asked in each round); b) variation in the variable among subsamples (some variables, such as whether the respondent speaks a minority language, have very little variation overall and eventually no variation as samples become increasingly narrower). The balance tables can be summarized as follows:

- Table 5 reports balance of 94 covariates across the seven treatment conditions among all respondents in the midline: 4/94 (5%) tests exhibit a p-value equal to or less than .05.

- Table 6 reports balance of 94 covariates across the seven treatment conditions among all compliers in the midline: 4/94 (5%) tests exhibit a p-value equal to or less than .05.

- Table 7 reports balance of 90 covariates across the seven treatment conditions among all compliers in the midline: 7/90 (8%) tests exhibit a p-value equal to or less than .05.

- Table 8 reports balance of 93 covariates across the seven treatment conditions among all compliers in the midline: 2/93 (2%) tests exhibit a p-value equal to or less than .05.

- Table 9 reports balance of 57 covariates across the seven treatment conditions among all respondents in the endline: 2/57 (4%) tests exhibit a p-value equal to or less than .05.

- Table 10 reports balance of 56 covariates across the seven treatment conditions among all women in the endline: 0/56 (0%) tests exhibit a p-value equal to or less than .05.

- Table 11 reports balance of 57 covariates across the seven treatment conditions among all compliers in the endline: 3/57 (5%) tests exhibit a p-value equal to or less than .05.

- Table 12 reports balance of 55 covariates across the seven treatment conditions among women compliers in the endline, irrespective of whether they were in the midline: 5/55 (9%) tests exhibit a p-value equal to or less than .05.

- Table 13 reports balance of 55 covariates across the seven treatment conditions among women compliers in the endline who were also in the midline: 7/55 (12%) tests exhibit a p-value equal to or less than .05.

- Table 14 reports balance of 56 covariates across the seven treatment conditions among men compliers, irrespective of whether they were in the midline: 2/56 (4%) tests exhibit a p-value equal to or less than .05.

- Table 15 reports balance of 56 covariates across the seven treatment conditions among men compliers, irrespective of whether they were in the midline: 2/56 (4%) tests exhibit a p-value equal to or less than .05.

Averaging across all tables, we find an overall rate of 5%. The pattern of minor imbalances we see is entirely consistent with the hypothesis of covariates being orthogonal to treatment status among the various subgroups among which our main effects are estimated.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
living_standard	1.10	1.02	1.03	1.14	1.05	0.92	1.01	0.01
living_conditions	0.12	0.08	0.08	0.16	0.03	0.05	0.02	0.01
english_christian	0.07	0.08	0.09	0.09	0.10	0.07	0.14	0.02
education_work	0.06	0.04	0.03	0.05	0.02	0.04	0.04	0.02
misc_floor	0.08	0.08	0.15	0.13	0.14	0.11	0.16	0.05
minority_lang	0.03	0.02	0.09	0.02	0.06	0.03	0.03	0.07
mutooro	0.02	0.01	0.04	0.01	0.01	0.01	0.02	0.08
living_conditions_compared	2.19	2.19	2.10	2.20	2.05	2.10	2.08	0.10
not_married	0.15	0.16	0.15	0.17	0.14	0.18	0.12	0.11
highest_grade	7.47	7.43	6.90	7.00	6.34	6.71	6.77	0.12
cement_floor	0.69	0.62	0.55	0.57	0.45	0.54	0.54	0.15
minority_tribe	0.07	0.09	0.05	0.07	0.06	0.04	0.07	0.15
muslim	0.20	0.20	0.15	0.15	0.14	0.09	0.16	0.16
living_standard_children	1.56	1.50	1.54	1.52	1.48	1.43	1.50	0.17
catholic	0.40	0.42	0.42	0.47	0.46	0.51	0.40	0.17
job_kampala	0.94	0.93	0.96	0.93	0.94	0.95	0.95	0.19
luganda_lang	0.92	0.93	0.82	0.84	0.82	0.80	0.90	0.19
religious_service	1.92	1.82	1.66	1.33	1.46	1.21	1.57	0.20
minority_religion	0.00	0.00	0.01	0.00	0.01	0.00	0.01	0.21
rooms	2.64	2.66	2.60	2.93	2.73	2.59	2.59	0.21
sofa	0.33	0.27	0.23	0.27	0.22	0.23	0.26	0.21
stone_wall	0.03	0.03	0.04	0.03	0.03	0.06	0.03	0.22
chair	0.88	0.87	0.85	0.90	0.88	0.85	0.85	0.22
age	31.25	31.11	30.73	31.66	32.05	31.90	31.49	0.22
mukiga	0.04	0.03	0.05	0.02	0.08	0.05	0.03	0.23
living_as_married	0.39	0.35	0.44	0.36	0.35	0.39	0.43	0.25
earth_floor	0.23	0.30	0.30	0.30	0.40	0.35	0.30	0.26
tv	0.33	0.29	0.23	0.31	0.21	0.24	0.22	0.26
electric_light	0.29	0.25	0.17	0.26	0.13	0.17	0.16	0.27
munyoro	0.09	0.05	0.08	0.07	0.05	0.03	0.06	0.27
female	0.50	0.50	0.50	0.50	0.49	0.49	0.49	0.28
illiterate	0.10	0.09	0.13	0.11	0.14	0.13	0.11	0.31
write_and_read	0.82	0.85	0.79	0.81	0.77	0.79	0.82	0.33
cement_wall	0.11	0.07	0.09	0.10	0.06	0.10	0.10	0.34
brick_wall	0.63	0.67	0.57	0.61	0.58	0.54	0.57	0.35
pray_private	8.03	8.10	7.99	8.19	8.09	8.03	7.99	0.37
other_work	0.05	0.06	0.04	0.04	0.03	0.04	0.03	0.37
runyannkole_lang	0.02	0.03	0.07	0.07	0.07	0.12	0.06	0.42
survey_luganda	0.98	0.98	0.97	0.96	0.96	0.95	0.99	0.42
members	4.53	4.61	4.39	4.72	4.58	4.31	4.54	0.43
married	0.36	0.39	0.33	0.36	0.40	0.32	0.35	0.45
separated	0.09	0.10	0.08	0.12	0.11	0.12	0.10	0.45
household_children	2.40	2.41	2.28	2.52	2.49	2.25	2.41	0.47
household_younger	3.03	3.11	2.92	3.22	3.11	2.88	3.07	0.47
close_relatives	0.89	0.89	0.86	0.89	0.87	0.83	0.88	0.48
manual_work	0.09	0.06	0.07	0.06	0.08	0.07	0.07	0.48
kerosene_light	0.20	0.25	0.30	0.24	0.29	0.24	0.28	0.50
witchcraft	1.27	1.18	1.23	1.21	1.21	1.25	1.25	0.51
share_house	0.30	0.34	0.30	0.26	0.24	0.31	0.29	0.51
charcoal_fuel	0.51	0.46	0.43	0.38	0.37	0.38	0.41	0.51
transport_work	0.03	0.03	0.03	0.03	0.02	0.04	0.03	0.52
cellphone	0.83	0.82	0.78	0.81	0.77	0.80	0.78	0.53
household_other	0.08	0.09	0.07	0.08	0.06	0.07	0.05	0.54
single_hut	0.61	0.58	0.61	0.62	0.68	0.59	0.62	0.55
several_huts	0.09	0.08	0.09	0.12	0.08	0.10	0.09	0.56
other_person	0.10	0.09	0.08	0.11	0.08	0.07	0.08	0.57
university	0.08	0.06	0.06	0.06	0.05	0.06	0.05	0.57
household_spouse	0.36	0.35	0.37	0.35	0.36	0.34	0.38	0.58
household_older	0.50	0.51	0.47	0.50	0.46	0.43	0.47	0.60
fumbira_lang	0.03	0.02	0.02	0.07	0.05	0.05	0.01	0.60
munyankole	0.08	0.10	0.14	0.11	0.13	0.17	0.13	0.60
protestant	0.16	0.16	0.17	0.12	0.14	0.15	0.15	0.62
misc_fuel	0.01	0.02	0.03	0.03	0.03	0.03	0.02	0.63
domestic_work	0.04	0.05	0.05	0.03	0.04	0.04	0.05	0.67
dist_to_video_hall	597.89	928.32	587.05	1021.48	1329.57	1573.94	568.96	0.67
mufumbira_tribe	0.03	0.02	0.02	0.06	0.05	0.05	0.02	0.68
misc_wall	0.01	0.02	0.06	0.02	0.04	0.04	0.05	0.69
firewood_fuel	0.47	0.51	0.55	0.59	0.60	0.59	0.56	0.69
living_conditions_tribe	-0.08	-0.08	0.00	-0.06	-0.09	-0.09	-0.08	0.69
mobile_phone_use	3.33	3.34	3.17	3.28	3.16	3.29	3.19	0.70
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.73
misc_light	0.08	0.08	0.07	0.06	0.07	0.09	0.09	0.74
mud_wall	0.21	0.20	0.25	0.23	0.29	0.26	0.25	0.76
solar_light	0.29	0.28	0.34	0.32	0.37	0.36	0.32	0.76
travel_big_city	0.75	0.77	0.75	0.73	0.75	0.79	0.76	0.76
munyarwanda	0.11	0.10	0.10	0.12	0.12	0.13	0.09	0.76
village_official	0.06	0.06	0.08	0.06	0.07	0.06	0.07	0.77
agriculture_work	0.52	0.53	0.60	0.59	0.62	0.57	0.60	0.77
day	1.25	1.22	1.24	1.25	1.27	1.26	1.21	0.78
frequency_discussion	1.80	1.73	1.84	1.79	1.80	1.80	1.78	0.78

christian_only	0.02	0.02	0.03	0.02	0.02	0.02	0.01	0.80
number_children	3.81	3.89	3.87	4.03	4.21	3.96	4.01	0.81
write_only	0.04	0.04	0.05	0.05	0.05	0.05	0.04	0.83
muganda_tribe	0.57	0.59	0.52	0.54	0.50	0.53	0.58	0.83
same_village	0.40	0.37	0.35	0.40	0.38	0.36	0.39	0.85
radio	0.81	0.83	0.81	0.83	0.81	0.80	0.82	0.86
holy_spirit	0.15	0.13	0.14	0.16	0.14	0.15	0.12	0.87
household_head	0.56	0.57	0.56	0.57	0.58	0.59	0.56	0.91
read_only	0.03	0.03	0.03	0.03	0.04	0.03	0.03	0.91
men_beaten	1.03	1.04	1.09	1.09	1.04	1.15	0.97	0.96
retail_work	0.14	0.16	0.13	0.13	0.13	0.14	0.13	0.96
motor_cycle	0.27	0.27	0.25	0.27	0.26	0.25	0.26	0.98
hospitality_work	0.05	0.06	0.06	0.06	0.06	0.06	0.05	0.99
no_work	0.05	0.06	0.06	0.06	0.06	0.06	0.05	0.99

Table 5: Balance on covariates among **all respondents in the midline sample**, irrespective of compliance or gender.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
living_standard_children	1.52	1.29	1.53	1.56	1.61	1.35	1.50	0.01
english_christian	0.03	0.06	0.06	0.08	0.07	0.06	0.19	0.01
minority_tribe	0.09	0.17	0.08	0.07	0.08	0.03	0.04	0.02
cellphone	0.87	0.74	0.72	0.86	0.80	0.76	0.80	0.03
misc_floor	0.09	0.09	0.21	0.14	0.19	0.12	0.20	0.06
atheist	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.07
motor_cycle	0.22	0.20	0.21	0.32	0.21	0.21	0.16	0.07
munyankole	0.07	0.10	0.09	0.04	0.10	0.18	0.13	0.08
protestant	0.21	0.23	0.15	0.10	0.16	0.14	0.16	0.09
minority_lang	0.03	0.04	0.08	0.02	0.05	0.03	0.01	0.10
highest_grade	7.37	7.66	6.45	7.09	7.06	6.18	6.92	0.12
university	0.07	0.05	0.03	0.04	0.06	0.01	0.02	0.13
kerosene_light	0.16	0.33	0.35	0.24	0.31	0.30	0.37	0.14
electric_light	0.26	0.12	0.11	0.14	0.07	0.07	0.11	0.14
tv	0.29	0.14	0.14	0.24	0.18	0.15	0.20	0.14
munyarwanda	0.08	0.04	0.16	0.12	0.10	0.16	0.09	0.15
living_standard	1.08	1.01	0.98	1.16	1.00	0.92	0.94	0.16
cement_floor	0.69	0.51	0.43	0.56	0.44	0.49	0.50	0.16
rooms	2.49	2.34	2.25	2.54	2.72	2.54	2.33	0.18
age	28.70	28.51	27.96	28.59	30.10	30.44	28.83	0.18
education_work	0.08	0.06	0.02	0.02	0.01	0.03	0.04	0.18
mobile_phone_use	3.49	3.12	3.05	3.43	3.28	3.12	3.32	0.18
luganda_lang	0.91	0.93	0.86	0.86	0.84	0.80	0.94	0.20
stone_wall	0.02	0.01	0.04	0.04	0.03	0.06	0.03	0.21
misc_wall	0.01	0.04	0.09	0.01	0.02	0.07	0.07	0.23
earth_floor	0.22	0.40	0.36	0.31	0.37	0.39	0.31	0.24
write_only	0.05	0.02	0.06	0.04	0.02	0.05	0.02	0.26
living_conditions_tribe	-0.03	-0.15	0.03	-0.08	-0.14	-0.10	-0.26	0.29
holy_spirit	0.09	0.06	0.12	0.14	0.12	0.13	0.08	0.31
sofa	0.26	0.14	0.13	0.21	0.15	0.19	0.17	0.32
catholic	0.44	0.44	0.48	0.49	0.45	0.55	0.39	0.33
frequency_discussion	1.88	1.68	1.94	1.73	1.94	1.79	1.82	0.35
write_and_read	0.81	0.88	0.79	0.88	0.85	0.81	0.87	0.35
mukiga	0.03	0.02	0.04	0.02	0.10	0.04	0.03	0.35
munyoro	0.09	0.03	0.06	0.08	0.02	0.03	0.07	0.35
dist_to_video_hall	431.91	254.12	297.63	240.32	1332.04	1100.26	232.31	0.35
witchcraft	1.38	1.24	1.33	1.31	1.20	1.34	1.24	0.36
charcoal_fuel	0.53	0.50	0.35	0.36	0.38	0.32	0.42	0.36
other_work	0.05	0.07	0.05	0.03	0.01	0.03	0.02	0.36
survey_luganda	0.98	0.96	0.97	0.99	0.98	0.95	0.98	0.36
firewood_fuel	0.43	0.46	0.62	0.59	0.59	0.65	0.55	0.38
misc_light	0.10	0.08	0.09	0.05	0.10	0.10	0.09	0.39
married	0.33	0.36	0.34	0.37	0.41	0.30	0.25	0.39
living_as_married	0.37	0.31	0.40	0.35	0.33	0.37	0.47	0.39
living_conditions	0.12	-0.02	0.01	0.10	0.05	0.03	-0.10	0.39
read_only	0.05	0.02	0.03	0.02	0.01	0.01	0.02	0.40
domestic_work	0.02	0.01	0.03	0.01	0.03	0.01	0.04	0.40
illiterate	0.10	0.07	0.13	0.06	0.12	0.13	0.08	0.43
runyannkole_lang	0.02	0.02	0.05	0.03	0.05	0.13	0.04	0.43
solar_light	0.32	0.31	0.31	0.42	0.40	0.36	0.26	0.46
brick_wall	0.71	0.64	0.54	0.65	0.57	0.54	0.56	0.47
pray_private	7.71	8.09	8.01	7.92	8.00	7.88	7.71	0.48
chair	0.84	0.86	0.82	0.89	0.87	0.82	0.87	0.48
other_person	0.05	0.08	0.08	0.09	0.05	0.03	0.07	0.53
single_hut	0.57	0.58	0.62	0.65	0.69	0.65	0.60	0.55
religious_service	2.08	2.22	1.24	1.04	1.70	1.22	1.21	0.58
fumbira_lang	0.03	0.01	0.01	0.09	0.05	0.04	0.01	0.59
female	0.28	0.29	0.31	0.27	0.31	0.38	0.30	0.60
number_children	2.98	3.17	3.23	3.08	3.63	3.63	3.20	0.60
same_village	0.51	0.42	0.46	0.49	0.53	0.45	0.46	0.60
muslim	0.22	0.18	0.13	0.17	0.16	0.08	0.17	0.61
household_older	0.39	0.34	0.42	0.41	0.34	0.37	0.45	0.62
men_beaten	1.40	1.24	1.50	1.01	1.09	1.17	1.09	0.63
mufumbira_tribe	0.02	0.02	0.02	0.09	0.04	0.04	0.01	0.66
agriculture_work	0.54	0.56	0.61	0.66	0.68	0.62	0.64	0.67
christian_only	0.02	0.02	0.04	0.02	0.01	0.03	0.01	0.68
household_other	0.12	0.11	0.09	0.08	0.07	0.08	0.10	0.68
job_kampala	0.94	0.95	0.94	0.92	0.96	0.96	0.95	0.71
transport_work	0.04	0.04	0.04	0.06	0.03	0.06	0.08	0.71

share_house	0.32	0.33	0.31	0.26	0.25	0.25	0.32	0.72
manual_work	0.10	0.06	0.07	0.06	0.09	0.09	0.06	0.73
mud_wall	0.21	0.24	0.26	0.22	0.32	0.25	0.28	0.74
radio	0.82	0.85	0.77	0.84	0.85	0.83	0.83	0.74
minority_religion	0.00	0.00	0.01	0.01	0.01	0.00	0.01	0.75
several_huts	0.12	0.09	0.08	0.09	0.06	0.10	0.09	0.77
hospitality_work	0.05	0.08	0.04	0.04	0.04	0.06	0.05	0.77
no_work	0.05	0.08	0.04	0.04	0.04	0.06	0.05	0.77
not_married	0.23	0.24	0.18	0.22	0.19	0.24	0.20	0.80
muganda_tribe	0.62	0.59	0.52	0.56	0.53	0.50	0.60	0.81
travel_big_city	0.81	0.80	0.80	0.78	0.82	0.84	0.80	0.84
mutooro	0.02	0.03	0.02	0.01	0.02	0.01	0.02	0.84
day	1.24	1.16	1.17	1.22	1.25	1.25	1.25	0.85
separated	0.06	0.09	0.08	0.06	0.07	0.09	0.08	0.85
living_conditions_compared	2.01	1.95	1.91	2.05	2.00	2.00	1.90	0.86
household_spouse	0.22	0.21	0.25	0.27	0.23	0.26	0.22	0.86
misc_fuel	0.04	0.04	0.04	0.05	0.03	0.03	0.02	0.88
household_head	0.65	0.68	0.65	0.65	0.70	0.66	0.68	0.89
close_relatives	0.86	0.91	0.89	0.89	0.87	0.87	0.90	0.91
retail_work	0.13	0.10	0.11	0.09	0.09	0.09	0.07	0.91
household_children	2.00	1.99	1.94	2.12	2.15	2.19	2.00	0.94
cement_wall	0.06	0.06	0.07	0.08	0.05	0.08	0.06	0.97
household_younger	2.64	2.66	2.64	2.76	2.84	2.84	2.61	0.97
village_official	0.10	0.09	0.10	0.08	0.08	0.07	0.09	0.98
members	4.03	4.00	4.06	4.16	4.18	4.21	4.07	0.99

Table 6: Balance on covariates among **all compliers in the midline sample**, irrespective of gender or presence in endline sample.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
english_christian	0.08	0.07	0.12	0.09	0.11	0.04	0.24	0.01
frequency_discussion	2.42	1.54	2.08	1.70	1.97	2.01	1.98	0.01
illiterate	0.14	0.10	0.26	0.05	0.19	0.18	0.08	0.02
catholic	0.50	0.46	0.56	0.47	0.39	0.63	0.43	0.03
brick_wall	0.75	0.80	0.34	0.67	0.58	0.56	0.41	0.04
cellphone	0.69	0.49	0.52	0.77	0.73	0.73	0.65	0.04
luganda_lang	0.92	0.98	0.90	0.81	0.79	0.82	0.96	0.05
pray_private	7.94	8.29	8.16	8.19	8.65	8.22	8.08	0.07
write_and_read	0.81	0.83	0.62	0.88	0.77	0.78	0.84	0.07
age	29.75	26.22	27.06	28.81	29.73	30.67	29.02	0.09
religious_service	1.22	1.12	1.28	1.02	1.50	1.31	0.96	0.10
electric_light	0.19	0.05	0.10	0.05	0.06	0.05	0.16	0.10
same_village	0.28	0.22	0.34	0.21	0.32	0.27	0.14	0.10
other_person	0.11	0.24	0.16	0.26	0.15	0.05	0.12	0.12
misc_wall	0.00	0.02	0.10	0.02	0.00	0.01	0.10	0.13
men_beaten	2.11	1.22	1.50	0.63	1.27	1.04	0.67	0.15
munyoro	0.14	0.02	0.06	0.02	0.00	0.04	0.08	0.15
misc_fuel	0.00	0.00	0.00	0.02	0.00	0.00	0.00	0.16
stone_wall	0.00	0.00	0.12	0.09	0.03	0.10	0.08	0.17
living_standard_children	1.58	1.44	1.70	1.58	1.71	1.38	1.63	0.18
cement_wall	0.06	0.00	0.12	0.00	0.08	0.06	0.10	0.18
minority_lang	0.00	0.02	0.04	0.02	0.08	0.00	0.00	0.19
holy_spirit	0.06	0.07	0.12	0.12	0.23	0.12	0.08	0.21
munyankole	0.03	0.12	0.14	0.05	0.15	0.17	0.18	0.22
household_children	2.61	2.46	2.10	2.86	2.71	2.58	2.04	0.23
highest_grade	6.89	6.78	5.08	6.63	5.95	5.55	6.02	0.23
minority_tribe	0.00	0.10	0.10	0.05	0.11	0.04	0.04	0.23
solar_light	0.31	0.27	0.32	0.40	0.42	0.27	0.14	0.24
christian_only	0.06	0.07	0.00	0.02	0.02	0.03	0.00	0.25
household_head	0.11	0.24	0.18	0.09	0.21	0.27	0.20	0.25
close_relatives	0.81	0.98	0.86	0.88	0.87	0.86	0.84	0.25
earth_floor	0.31	0.49	0.52	0.44	0.44	0.54	0.37	0.28
education_work	0.08	0.05	0.00	0.02	0.02	0.04	0.02	0.28
charcoal_fuel	0.58	0.56	0.36	0.28	0.39	0.31	0.51	0.29
household_older	0.97	0.73	0.86	0.91	0.77	0.71	0.84	0.29
several_huts	0.06	0.02	0.12	0.02	0.03	0.03	0.02	0.30
day	1.25	1.37	1.24	1.28	1.55	1.27	1.33	0.31
cement_floor	0.64	0.46	0.38	0.47	0.42	0.40	0.45	0.33
firewood_fuel	0.42	0.44	0.64	0.70	0.61	0.69	0.49	0.33
radio	0.67	0.76	0.70	0.72	0.84	0.65	0.69	0.33
motor_cycle	0.28	0.24	0.22	0.35	0.27	0.17	0.10	0.34
misc_floor	0.06	0.05	0.10	0.09	0.15	0.06	0.18	0.35
protestant	0.11	0.20	0.08	0.07	0.13	0.08	0.10	0.36
members	4.89	4.41	4.32	5.00	4.69	4.53	4.10	0.38
domestic_work	0.06	0.05	0.10	0.05	0.10	0.03	0.10	0.38
other_work	0.00	0.02	0.00	0.05	0.02	0.00	0.02	0.38
job_kampala	0.92	0.98	0.98	0.93	0.98	0.96	0.98	0.39
single_hut	0.56	0.66	0.46	0.67	0.63	0.65	0.61	0.40
fumbira_lang	0.06	0.00	0.00	0.14	0.05	0.06	0.00	0.43
separated	0.08	0.22	0.14	0.07	0.15	0.19	0.16	0.44
household_younger	2.92	2.68	2.46	3.09	2.92	2.82	2.27	0.44
mukiga	0.00	0.05	0.02	0.05	0.08	0.01	0.00	0.45
tv	0.17	0.12	0.14	0.16	0.10	0.09	0.18	0.46
living_conditions_tribe	0.00	-0.05	0.28	0.09	0.05	-0.10	-0.08	0.48
number_children	3.67	3.20	3.84	3.65	4.26	4.00	3.43	0.50
runyannkole_lang	0.03	0.00	0.06	0.02	0.08	0.12	0.04	0.52
manual_work	0.03	0.02	0.02	0.00	0.03	0.05	0.04	0.52
mobile_phone_use	2.67	2.44	2.18	2.91	2.85	2.90	2.73	0.54

read_only	0.03	0.02	0.04	0.00	0.03	0.00	0.02	0.55
household_spouse	0.78	0.68	0.74	0.86	0.74	0.67	0.69	0.56
married	0.42	0.27	0.30	0.40	0.40	0.27	0.20	0.56
living_conditions	0.22	-0.02	0.06	0.30	0.05	-0.10	0.12	0.56
sofa	0.25	0.17	0.14	0.26	0.15	0.17	0.31	0.57
university	0.03	0.02	0.00	0.05	0.05	0.01	0.00	0.59
muslim	0.19	0.12	0.12	0.23	0.13	0.12	0.14	0.60
mufumbira_tribe	0.03	0.00	0.02	0.12	0.05	0.08	0.00	0.60
not_married	0.08	0.12	0.06	0.02	0.10	0.12	0.10	0.61
mutooro	0.06	0.05	0.02	0.00	0.02	0.01	0.06	0.62
write_only	0.03	0.05	0.08	0.07	0.00	0.04	0.06	0.66
travel_big_city	0.75	0.73	0.74	0.58	0.77	0.81	0.76	0.68
village_official	0.03	0.05	0.02	0.05	0.00	0.06	0.06	0.69
rooms	2.47	2.41	2.18	2.33	2.68	2.47	2.24	0.74
kerosene_light	0.22	0.39	0.36	0.33	0.34	0.42	0.43	0.74
living_as_married	0.42	0.39	0.50	0.51	0.35	0.42	0.53	0.74
muganda_tribe	0.67	0.56	0.54	0.58	0.47	0.50	0.53	0.75
agriculture_work	0.61	0.56	0.70	0.72	0.61	0.67	0.55	0.76
misc_light	0.11	0.10	0.14	0.07	0.05	0.10	0.06	0.77
living_conditions_compared	2.14	2.05	1.90	2.14	2.13	2.06	2.10	0.81
retail_work	0.14	0.20	0.10	0.07	0.15	0.14	0.12	0.84
share_house	0.39	0.32	0.42	0.30	0.34	0.32	0.37	0.88
household_other	0.11	0.07	0.08	0.05	0.05	0.06	0.10	0.89
living_standard	1.06	1.12	0.98	1.16	0.98	0.94	0.96	0.92
chair	0.72	0.78	0.76	0.86	0.81	0.78	0.76	0.92
mud_wall	0.19	0.17	0.32	0.21	0.31	0.26	0.31	0.93
witchcraft	1.33	1.24	1.28	1.33	1.18	1.28	1.18	0.96
hospitality_work	0.08	0.10	0.06	0.07	0.08	0.08	0.14	0.96
no_work	0.08	0.10	0.06	0.07	0.08	0.08	0.14	0.96
munyarwanda	0.08	0.10	0.10	0.14	0.13	0.15	0.10	0.97
dist_to_video_hall	206.58	287.22	189.62	191.86	365.63	2420.92	183.67	0.99
survey_luganda	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	
minority_religion	0.00	0.00	0.00	0.00	0.00	0.00	0.00	
transport_work	0.00	0.00	0.00	0.00	0.00	0.00	0.00	

Table 7: Balance on covariates among **women compliers in the midline sample**, irrespective of presence in endline sample.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
minority_tribe	0.12	0.19	0.07	0.08	0.07	0.03	0.04	0.00
english_christian	0.01	0.06	0.04	0.07	0.06	0.08	0.17	0.04
munyarwanda	0.08	0.02	0.18	0.12	0.09	0.17	0.08	0.06
dist_to_video_hall	519.14	240.28	347.17	258.13	1760.02	282.71	253.22	0.06
mobile_phone_use	3.81	3.41	3.45	3.62	3.47	3.26	3.57	0.06
atheist	0.00	0.00	0.02	0.00	0.00	0.00	0.00	0.07
cellphone	0.94	0.85	0.81	0.90	0.83	0.79	0.87	0.08
kerosene_light	0.13	0.31	0.35	0.21	0.29	0.23	0.34	0.09
misc_floor	0.10	0.11	0.26	0.15	0.21	0.16	0.20	0.12
minority_lang	0.04	0.05	0.10	0.02	0.04	0.05	0.01	0.13
living_conditions	0.09	-0.02	-0.02	0.03	0.05	0.11	-0.19	0.13
living_standard_children	1.49	1.23	1.45	1.56	1.56	1.33	1.44	0.14
rooms	2.49	2.31	2.28	2.62	2.74	2.58	2.36	0.14
other_work	0.06	0.09	0.07	0.03	0.01	0.06	0.03	0.14
electric_light	0.29	0.15	0.11	0.18	0.07	0.09	0.09	0.15
tv	0.34	0.15	0.15	0.27	0.21	0.18	0.20	0.17
single_hut	0.57	0.55	0.69	0.64	0.71	0.65	0.59	0.18
write_only	0.05	0.01	0.06	0.03	0.03	0.06	0.01	0.18
sofa	0.26	0.12	0.13	0.19	0.15	0.20	0.11	0.20
university	0.09	0.06	0.05	0.04	0.07	0.02	0.03	0.20
mukiga	0.04	0.01	0.06	0.02	0.11	0.06	0.04	0.22
agriculture_work	0.52	0.56	0.57	0.64	0.71	0.59	0.68	0.22
survey_luganda	0.98	0.95	0.96	0.98	0.97	0.92	0.97	0.22
misc_wall	0.01	0.05	0.08	0.01	0.04	0.10	0.05	0.23
cement_floor	0.71	0.53	0.45	0.59	0.45	0.54	0.52	0.23
radio	0.88	0.89	0.81	0.88	0.86	0.94	0.89	0.23
motor_cycle	0.19	0.18	0.21	0.31	0.19	0.24	0.18	0.23
protestant	0.25	0.24	0.18	0.11	0.18	0.17	0.18	0.25
misc_light	0.10	0.07	0.07	0.04	0.13	0.10	0.10	0.25
christian_only	0.00	0.00	0.06	0.02	0.01	0.04	0.01	0.26
holy_spirit	0.10	0.06	0.12	0.15	0.08	0.14	0.08	0.26
munyankole	0.09	0.09	0.07	0.04	0.08	0.19	0.11	0.26
highest_grade	7.56	8.03	7.07	7.26	7.56	6.56	7.31	0.28
hospitality_work	0.03	0.07	0.03	0.03	0.03	0.06	0.01	0.29
no_work	0.03	0.07	0.03	0.03	0.03	0.06	0.01	0.29
domestic_work	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.32
earth_floor	0.19	0.36	0.29	0.26	0.34	0.30	0.28	0.33
living_standard	1.09	0.96	0.98	1.15	1.01	0.90	0.94	0.34
education_work	0.08	0.07	0.03	0.03	0.01	0.02	0.04	0.34
share_house	0.29	0.34	0.26	0.25	0.21	0.20	0.30	0.35
chair	0.89	0.90	0.85	0.90	0.90	0.85	0.92	0.38
living_as_married	0.35	0.28	0.35	0.29	0.31	0.33	0.45	0.39
luganda_lang	0.91	0.91	0.83	0.88	0.86	0.79	0.94	0.40
married	0.30	0.40	0.36	0.36	0.41	0.33	0.26	0.41
living_conditions_tribe	-0.04	-0.19	-0.09	-0.14	-0.23	-0.10	-0.33	0.41
witchcraft	1.40	1.23	1.36	1.31	1.21	1.37	1.26	0.43
several_huts	0.14	0.11	0.06	0.11	0.07	0.15	0.11	0.44
retail_work	0.13	0.06	0.12	0.10	0.07	0.06	0.05	0.44

muslim	0.23	0.20	0.14	0.15	0.17	0.06	0.18	0.46
stone_wall	0.02	0.02	0.00	0.02	0.03	0.04	0.01	0.46
munyoro	0.06	0.03	0.06	0.09	0.04	0.02	0.07	0.46
age	28.29	29.47	28.38	28.51	30.26	30.30	28.75	0.48
frequency_discussion	1.67	1.73	1.88	1.74	1.93	1.66	1.75	0.48
runyannkole_lang	0.02	0.03	0.05	0.03	0.04	0.13	0.04	0.48
day	1.24	1.07	1.14	1.20	1.11	1.23	1.22	0.49
cement_wall	0.06	0.09	0.05	0.11	0.04	0.09	0.04	0.52
read_only	0.05	0.02	0.02	0.03	0.01	0.02	0.03	0.52
firewood_fuel	0.43	0.47	0.61	0.55	0.58	0.62	0.58	0.53
charcoal_fuel	0.52	0.47	0.34	0.39	0.38	0.33	0.39	0.55
transport_work	0.05	0.06	0.06	0.09	0.04	0.10	0.11	0.55
household_spouse	0.01	0.01	0.03	0.05	0.01	0.02	0.02	0.56
religious_service	2.41	2.67	1.22	1.05	1.79	1.16	1.32	0.58
write_and_read	0.81	0.91	0.86	0.88	0.88	0.83	0.89	0.63
solar_light	0.32	0.33	0.30	0.44	0.39	0.41	0.32	0.64
household_older	0.16	0.17	0.22	0.22	0.14	0.16	0.29	0.65
mufumbira_tribe	0.01	0.03	0.02	0.09	0.04	0.02	0.02	0.65
catholic	0.42	0.43	0.44	0.50	0.48	0.51	0.38	0.66
brick_wall	0.69	0.57	0.63	0.64	0.56	0.52	0.63	0.66
household_head	0.86	0.87	0.87	0.85	0.91	0.90	0.89	0.68
mutooro	0.00	0.02	0.02	0.01	0.02	0.01	0.00	0.69
minority_religion	0.00	0.00	0.01	0.01	0.01	0.00	0.01	0.71
pray_private	7.62	8.00	7.94	7.82	7.72	7.67	7.55	0.71
not_married	0.29	0.29	0.24	0.30	0.23	0.31	0.25	0.73
fumbira_lang	0.02	0.01	0.02	0.07	0.05	0.02	0.01	0.74
mud_wall	0.22	0.27	0.24	0.22	0.33	0.25	0.26	0.75
manual_work	0.13	0.08	0.09	0.09	0.12	0.12	0.07	0.77
other_person	0.02	0.01	0.04	0.03	0.01	0.02	0.04	0.78
men_beaten	1.13	1.24	1.50	1.15	1.01	1.25	1.27	0.78
household_other	0.13	0.12	0.10	0.09	0.08	0.09	0.10	0.80
same_village	0.60	0.51	0.51	0.59	0.62	0.56	0.60	0.80
number_children	2.71	3.15	2.95	2.86	3.36	3.40	3.10	0.83
living_conditions_compared	1.96	1.91	1.91	2.02	1.94	1.96	1.82	0.84
muganda_tribe	0.60	0.60	0.51	0.56	0.56	0.51	0.63	0.84
job_kampala	0.95	0.94	0.93	0.92	0.95	0.96	0.94	0.89
village_official	0.13	0.10	0.14	0.09	0.11	0.08	0.11	0.92
close_relatives	0.88	0.88	0.90	0.89	0.86	0.88	0.93	0.94
members	3.70	3.83	3.94	3.85	3.95	4.01	4.05	0.97
household_children	1.76	1.79	1.87	1.85	1.91	1.94	1.98	0.98
household_younger	2.54	2.65	2.72	2.63	2.81	2.85	2.76	0.98
travel_big_city	0.84	0.83	0.83	0.85	0.84	0.87	0.82	0.98
misc_fuel	0.05	0.06	0.06	0.06	0.04	0.05	0.04	0.99
separated	0.05	0.04	0.06	0.05	0.04	0.03	0.04	0.99
illiterate	0.09	0.06	0.06	0.07	0.09	0.10	0.08	0.99

Table 8: Balance on covariates among **men compliers in the midline sample**, irrespective of presence in endline sample.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
minority_tribe	0.05	0.12	0.08	0.04	0.10	0.04	0.05	0.01
english_christian	0.02	0.04	0.03	0.04	0.05	0.03	0.09	0.02
holy_spirit	0.12	0.08	0.14	0.19	0.13	0.17	0.09	0.07
radius	376.77	432.35	448.73	529.10	504.78	387.11	417.56	0.13
village_official	0.14	0.11	0.07	0.07	0.11	0.07	0.09	0.18
principal	0.31	0.26	0.22	0.21	0.22	0.21	0.26	0.19
illiterate	0.11	0.08	0.16	0.12	0.16	0.16	0.10	0.20
mukiga	0.05	0.03	0.03	0.03	0.09	0.03	0.04	0.21
household_head	0.51	0.59	0.56	0.56	0.63	0.60	0.56	0.23
munyoro	0.08	0.06	0.07	0.07	0.03	0.02	0.08	0.23
minority_lang	0.03	0.03	0.08	0.02	0.06	0.03	0.01	0.25
doctor	0.34	0.37	0.35	0.31	0.30	0.31	0.41	0.25
day	1.30	1.24	1.26	1.32	1.25	1.34	1.25	0.26
luganda_lang	0.92	0.93	0.83	0.86	0.81	0.82	0.92	0.26
survey_luganda	0.98	1.00	0.98	0.99	0.98	0.97	0.99	0.26
muslim	0.20	0.21	0.14	0.14	0.15	0.07	0.17	0.27
mutooro	0.02	0.02	0.04	0.03	0.03	0.00	0.04	0.28
female	0.59	0.53	0.54	0.54	0.46	0.55	0.51	0.29
teacher	0.60	0.64	0.58	0.59	0.53	0.51	0.58	0.30
highest_grade	6.97	7.09	6.21	6.26	6.22	6.08	6.47	0.32
write_and_read	0.83	0.87	0.76	0.82	0.80	0.78	0.85	0.35
munyankole	0.08	0.10	0.11	0.07	0.10	0.17	0.10	0.35
domestic_work	0.07	0.06	0.05	0.03	0.04	0.03	0.05	0.38
age	35.16	34.61	33.91	34.79	35.16	35.44	34.14	0.41
atheist	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.42
catholic	0.40	0.41	0.42	0.44	0.43	0.52	0.38	0.44
write_only	0.03	0.03	0.04	0.05	0.03	0.05	0.03	0.45
christian_only	0.18	0.13	0.16	0.12	0.12	0.11	0.16	0.45
university	0.07	0.04	0.04	0.03	0.04	0.03	0.03	0.50
education_work	0.04	0.03	0.01	0.01	0.02	0.02	0.01	0.51
manual_work	0.09	0.08	0.04	0.08	0.08	0.08	0.08	0.52
other_work	0.04	0.05	0.07	0.04	0.04	0.06	0.04	0.52
mobile_phone_use	3.41	3.33	3.19	3.34	3.29	3.42	3.46	0.54
mufumbira_tribe	0.03	0.01	0.03	0.09	0.05	0.07	0.01	0.55
fumbira_lang	0.03	0.01	0.03	0.09	0.06	0.06	0.02	0.57
cooperative	2.72	2.75	2.72	2.72	2.65	2.72	2.73	0.57
read_only	0.04	0.03	0.03	0.01	0.02	0.02	0.03	0.59
police	0.26	0.28	0.28	0.22	0.24	0.22	0.23	0.61

runyannkole_lang	0.02	0.03	0.07	0.03	0.06	0.10	0.04	0.66
not_married	0.13	0.17	0.15	0.14	0.13	0.16	0.12	0.69
religious_service	1.82	1.84	1.42	1.35	1.61	1.22	1.39	0.72
judge	0.13	0.09	0.10	0.09	0.12	0.10	0.11	0.74
munyarwanda	0.09	0.09	0.16	0.11	0.09	0.13	0.11	0.75
official	0.20	0.21	0.20	0.16	0.17	0.18	0.18	0.78
married	0.54	0.53	0.55	0.53	0.58	0.48	0.54	0.79
muganda_tribe	0.60	0.57	0.48	0.56	0.50	0.52	0.57	0.80
separated	0.13	0.14	0.13	0.13	0.12	0.17	0.13	0.83
transport_work	0.03	0.03	0.04	0.03	0.02	0.03	0.04	0.84
other_person	0.09	0.08	0.08	0.06	0.06	0.08	0.07	0.84
retail_work	0.09	0.09	0.10	0.11	0.06	0.11	0.10	0.88
living_as_married	0.20	0.16	0.17	0.20	0.17	0.19	0.21	0.92
clergy	0.50	0.53	0.49	0.52	0.52	0.52	0.52	0.97
frequency_discussion	1.45	1.43	1.52	1.45	1.51	1.46	1.46	0.98
agriculture_work	0.60	0.60	0.62	0.64	0.66	0.61	0.62	0.99
hospitality_work	0.05	0.05	0.05	0.06	0.06	0.06	0.05	0.99
no_work	0.05	0.05	0.05	0.06	0.06	0.06	0.05	0.99
living_conditions_compared	2.19	2.16	2.14	2.14	2.14	2.14	2.15	0.99

Table 9: Balance on covariates among **all respondents in the endline sample**, irrespective of gender or compliance.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
village_official	0.17	0.13	0.08	0.05	0.10	0.07	0.06	0.09
illiterate	0.11	0.09	0.24	0.16	0.22	0.21	0.09	0.09
female	1.00	1.00	1.00	1.00	1.00	1.00	1.00	0.11
read_only	0.03	0.05	0.04	0.00	0.04	0.01	0.02	0.12
minority_tribe	0.05	0.09	0.09	0.03	0.09	0.05	0.06	0.12
atheist	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.13
highest_grade	6.83	6.57	5.73	5.67	5.02	5.90	6.18	0.14
radius	369.80	425.20	433.11	521.88	503.68	387.36	415.49	0.19
write_and_read	0.84	0.83	0.68	0.78	0.73	0.75	0.84	0.22
write_only	0.01	0.03	0.04	0.06	0.01	0.02	0.05	0.24
munyankole	0.09	0.09	0.09	0.07	0.10	0.18	0.09	0.24
munyoro	0.11	0.06	0.07	0.08	0.03	0.02	0.07	0.26
mobile_phone_use	3.25	3.17	2.85	3.06	3.02	3.24	3.27	0.32
age	36.46	35.71	34.61	35.95	36.26	35.61	34.56	0.34
mufumbira_tribe	0.04	0.01	0.02	0.09	0.06	0.06	0.01	0.36
english_christian	0.02	0.02	0.03	0.02	0.05	0.02	0.06	0.36
muslim	0.18	0.18	0.13	0.16	0.11	0.08	0.15	0.36
household_head	0.23	0.32	0.26	0.28	0.26	0.33	0.21	0.37
education_work	0.04	0.02	0.01	0.01	0.01	0.02	0.01	0.38
holy_spirit	0.14	0.10	0.14	0.19	0.16	0.19	0.11	0.38
domestic_work	0.10	0.12	0.09	0.05	0.10	0.06	0.09	0.41
university	0.06	0.02	0.04	0.02	0.02	0.04	0.01	0.42
mutooro	0.03	0.02	0.05	0.04	0.04	0.01	0.06	0.42
luganda_lang	0.91	0.94	0.82	0.83	0.79	0.83	0.92	0.48
fumbira_lang	0.04	0.02	0.03	0.11	0.07	0.06	0.03	0.50
retail_work	0.09	0.13	0.09	0.13	0.06	0.16	0.12	0.50
manual_work	0.08	0.04	0.03	0.05	0.04	0.07	0.05	0.50
minority_lang	0.03	0.02	0.08	0.04	0.07	0.02	0.01	0.52
religious_service	1.43	1.28	1.30	1.32	1.40	1.33	1.12	0.52
married	0.52	0.47	0.52	0.51	0.52	0.41	0.52	0.53
mukiga	0.05	0.02	0.03	0.01	0.07	0.03	0.01	0.53
not_married	0.10	0.13	0.09	0.11	0.08	0.11	0.06	0.55
survey_luganda	0.97	1.00	0.97	0.99	0.96	0.97	0.99	0.55
official	0.15	0.17	0.15	0.10	0.17	0.17	0.15	0.62
other_work	0.03	0.04	0.05	0.04	0.01	0.03	0.02	0.66
doctor	0.34	0.35	0.34	0.34	0.28	0.35	0.43	0.67
catholic	0.41	0.43	0.43	0.44	0.40	0.52	0.40	0.68
separated	0.19	0.23	0.22	0.21	0.21	0.28	0.18	0.69
day	1.34	1.28	1.34	1.37	1.26	1.36	1.39	0.71
principal	0.29	0.28	0.20	0.22	0.24	0.22	0.27	0.76
teacher	0.60	0.60	0.55	0.61	0.50	0.52	0.55	0.78
runyannkole_lang	0.02	0.02	0.07	0.02	0.07	0.09	0.04	0.79
christian_only	0.22	0.20	0.22	0.16	0.19	0.16	0.23	0.82
judge	0.13	0.08	0.11	0.08	0.12	0.11	0.12	0.83
other_person	0.13	0.11	0.11	0.11	0.07	0.12	0.11	0.84
cooperative	2.70	2.70	2.72	2.68	2.62	2.68	2.70	0.89
living_as_married	0.19	0.16	0.17	0.18	0.18	0.20	0.23	0.90
muganda_tribe	0.54	0.58	0.51	0.56	0.49	0.50	0.56	0.90
agriculture_work	0.59	0.57	0.61	0.63	0.66	0.57	0.63	0.93
hospitality_work	0.06	0.07	0.09	0.09	0.10	0.09	0.08	0.95
no_work	0.06	0.07	0.09	0.09	0.10	0.09	0.08	0.95
frequency_discussion	1.34	1.24	1.32	1.30	1.35	1.22	1.31	0.95
munyarwanda	0.09	0.12	0.12	0.12	0.12	0.14	0.13	0.97
police	0.26	0.24	0.25	0.22	0.21	0.23	0.25	0.98
clergy	0.50	0.54	0.49	0.53	0.54	0.52	0.54	0.98
living_conditions_compared	2.13	2.12	2.16	2.07	2.11	2.08	2.13	0.99
transport_work	0.00	0.00	0.00	0.00	0.00	0.00	0.00	

Table 10: Balance on covariates among **all women in the endline sample**, irrespective of compliance

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
minority_tribe	0.05	0.15	0.08	0.04	0.13	0.03	0.03	0.00
english_christian	0.03	0.07	0.06	0.08	0.08	0.06	0.17	0.01
minority_lang	0.03	0.04	0.09	0.02	0.05	0.03	0.00	0.05
doctor	0.37	0.32	0.36	0.31	0.34	0.25	0.43	0.06
atheist	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.08
radius	379.82	457.81	471.23	554.79	512.57	381.56	439.31	0.08
principal	0.36	0.23	0.21	0.22	0.21	0.21	0.23	0.09
age	29.95	29.49	28.92	30.03	31.28	31.82	29.95	0.10
munyarwanda	0.07	0.09	0.19	0.11	0.08	0.15	0.10	0.16
police	0.31	0.32	0.36	0.24	0.28	0.23	0.21	0.16
university	0.06	0.05	0.03	0.04	0.07	0.01	0.02	0.17
catholic	0.46	0.43	0.47	0.49	0.44	0.58	0.39	0.17
luganda_lang	0.91	0.93	0.85	0.86	0.86	0.82	0.96	0.21
munyankole	0.09	0.10	0.12	0.06	0.08	0.19	0.11	0.21
teacher	0.63	0.65	0.57	0.62	0.54	0.50	0.62	0.25
education_work	0.05	0.05	0.01	0.01	0.02	0.02	0.01	0.26
munyoro	0.07	0.05	0.09	0.07	0.02	0.02	0.08	0.27
highest_grade	7.15	7.59	6.45	7.00	7.07	6.16	6.94	0.28
write_only	0.05	0.02	0.06	0.04	0.02	0.05	0.02	0.28
domestic_work	0.05	0.03	0.05	0.01	0.04	0.02	0.04	0.30
mobile_phone_use	3.55	3.27	3.26	3.53	3.45	3.46	3.59	0.31
holy_spirit	0.08	0.06	0.11	0.13	0.13	0.10	0.08	0.34
runyannkole_lang	0.03	0.02	0.05	0.02	0.04	0.12	0.03	0.35
other_person	0.04	0.08	0.08	0.03	0.05	0.08	0.08	0.35
cooperative	2.77	2.83	2.74	2.79	2.68	2.78	2.80	0.38
manual_work	0.09	0.11	0.04	0.10	0.09	0.09	0.11	0.39
survey_luganda	0.99	0.99	0.99	0.99	0.99	0.97	1.00	0.39
mukiga	0.04	0.03	0.02	0.05	0.09	0.03	0.03	0.41
village_official	0.12	0.09	0.06	0.05	0.11	0.08	0.11	0.43
write_and_read	0.80	0.88	0.79	0.88	0.84	0.80	0.88	0.43
mutooro	0.01	0.03	0.03	0.01	0.02	0.01	0.03	0.45
separated	0.08	0.09	0.04	0.05	0.08	0.10	0.09	0.47
female	0.31	0.30	0.29	0.27	0.30	0.38	0.30	0.54
illiterate	0.11	0.08	0.12	0.07	0.13	0.14	0.08	0.54
muganda_tribe	0.64	0.53	0.46	0.55	0.54	0.53	0.61	0.54
day	1.24	1.26	1.20	1.30	1.30	1.36	1.22	0.55
not_married	0.18	0.23	0.22	0.17	0.16	0.25	0.20	0.55
hospitality_work	0.04	0.06	0.03	0.03	0.06	0.04	0.06	0.56
no_work	0.04	0.06	0.03	0.03	0.06	0.04	0.06	0.56
other_work	0.04	0.07	0.07	0.04	0.05	0.08	0.07	0.56
religious_service	2.25	2.30	1.23	1.05	1.77	1.22	1.22	0.56
living_conditions_compared	2.23	2.05	2.11	2.18	2.11	2.09	2.13	0.56
official	0.26	0.26	0.24	0.24	0.17	0.18	0.20	0.57
married	0.48	0.50	0.54	0.53	0.57	0.44	0.50	0.59
transport_work	0.07	0.05	0.07	0.06	0.04	0.05	0.08	0.61
muslim	0.23	0.19	0.14	0.16	0.16	0.07	0.17	0.65
mufumbira_tribe	0.03	0.02	0.02	0.11	0.04	0.04	0.01	0.68
christian_only	0.02	0.02	0.04	0.02	0.02	0.03	0.01	0.69
fumbira_lang	0.04	0.01	0.01	0.10	0.05	0.03	0.01	0.70
living_as_married	0.26	0.19	0.20	0.25	0.19	0.21	0.21	0.75
agriculture_work	0.57	0.55	0.60	0.65	0.62	0.62	0.53	0.77
judge	0.12	0.09	0.08	0.10	0.13	0.09	0.09	0.77
clergy	0.57	0.53	0.47	0.49	0.52	0.53	0.49	0.80
household_head	0.65	0.68	0.67	0.66	0.72	0.67	0.70	0.82
read_only	0.04	0.02	0.03	0.01	0.02	0.01	0.02	0.83
frequency_discussion	1.61	1.48	1.65	1.49	1.60	1.63	1.55	0.90
retail_work	0.09	0.06	0.11	0.09	0.08	0.08	0.10	0.96

Table 11: Balance on covariates among **all compliers in the endline sample**, irrespective of gender or presence in midline sample.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
mobile_phone_use	3.26	2.78	2.38	3.20	3.02	3.25	3.41	0.03
english_christian	0.05	0.07	0.11	0.07	0.08	0.04	0.20	0.03
other_work	0.00	0.09	0.00	0.07	0.00	0.03	0.02	0.04
catholic	0.56	0.47	0.56	0.49	0.39	0.63	0.46	0.04
munyoro	0.10	0.04	0.11	0.07	0.00	0.01	0.04	0.05
household_head	0.13	0.29	0.13	0.11	0.23	0.31	0.20	0.06
married	0.49	0.38	0.58	0.62	0.48	0.36	0.52	0.06
radius	374.36	480.00	475.56	575.56	501.64	382.67	456.52	0.08
illiterate	0.13	0.09	0.22	0.04	0.18	0.19	0.07	0.09
christian_only	0.05	0.11	0.00	0.09	0.05	0.08	0.00	0.10
village_official	0.21	0.20	0.07	0.09	0.11	0.08	0.04	0.11
religious_service	1.18	1.07	1.24	1.00	1.49	1.28	0.98	0.11
age	31.23	29.42	27.20	30.62	31.90	32.13	30.33	0.12
munyankole	0.08	0.11	0.13	0.04	0.07	0.24	0.11	0.13
domestic_work	0.15	0.09	0.18	0.07	0.13	0.05	0.17	0.14
holy_spirit	0.05	0.07	0.09	0.07	0.21	0.09	0.11	0.14
write_and_read	0.82	0.84	0.64	0.89	0.77	0.77	0.87	0.18
separated	0.13	0.20	0.07	0.07	0.21	0.21	0.13	0.20
minority_tribe	0.05	0.07	0.11	0.02	0.11	0.03	0.04	0.20
highest_grade	6.82	6.93	5.00	6.56	5.80	5.52	6.30	0.21
read_only	0.03	0.02	0.04	0.00	0.05	0.00	0.00	0.26
other_person	0.08	0.13	0.18	0.11	0.03	0.19	0.17	0.28
luganda_lang	0.92	0.93	0.87	0.80	0.82	0.80	0.96	0.29
minority_lang	0.00	0.04	0.07	0.04	0.08	0.01	0.00	0.29

doctor	0.41	0.33	0.36	0.33	0.36	0.25	0.50	0.29
principal	0.33	0.27	0.13	0.27	0.26	0.27	0.17	0.29
clergy	0.64	0.47	0.40	0.53	0.56	0.53	0.50	0.29
frequency_discussion	1.62	1.22	1.44	1.20	1.46	1.37	1.33	0.38
education_work	0.08	0.02	0.00	0.02	0.02	0.03	0.02	0.41
runyannkole_lang	0.03	0.02	0.07	0.00	0.05	0.13	0.04	0.43
teacher	0.67	0.56	0.42	0.71	0.52	0.52	0.54	0.43
mutooro	0.03	0.04	0.02	0.00	0.03	0.01	0.09	0.46
fumbira_lang	0.05	0.00	0.00	0.16	0.05	0.05	0.00	0.48
official	0.15	0.22	0.13	0.18	0.16	0.08	0.13	0.54
university	0.03	0.02	0.00	0.04	0.05	0.01	0.00	0.58
mukiga	0.00	0.04	0.02	0.04	0.08	0.01	0.02	0.59
living_conditions_compared	2.13	1.93	2.02	2.18	2.03	1.96	2.07	0.59
write_only	0.03	0.04	0.09	0.07	0.00	0.04	0.07	0.60
mufumbira_tribe	0.08	0.00	0.02	0.16	0.05	0.07	0.00	0.62
not_married	0.10	0.16	0.13	0.07	0.08	0.19	0.13	0.75
judge	0.05	0.09	0.07	0.04	0.13	0.08	0.11	0.75
police	0.38	0.33	0.33	0.20	0.33	0.31	0.28	0.75
muganda_tribe	0.62	0.53	0.42	0.53	0.52	0.47	0.59	0.77
hospitality_work	0.05	0.13	0.07	0.09	0.10	0.07	0.11	0.80
no_work	0.05	0.13	0.07	0.09	0.10	0.07	0.11	0.80
cooperative	2.79	2.73	2.78	2.80	2.70	2.71	2.80	0.83
survey_luganda	0.97	1.00	0.98	0.96	0.98	0.97	1.00	0.85
munyarwanda	0.05	0.16	0.16	0.13	0.13	0.16	0.11	0.86
agriculture_work	0.56	0.49	0.60	0.64	0.61	0.61	0.50	0.89
female	1.00	1.00	1.00	1.00	1.00	1.00	1.00	0.91
day	1.41	1.44	1.38	1.38	1.31	1.33	1.50	0.91
retail_work	0.10	0.09	0.07	0.07	0.08	0.12	0.13	0.93
muslim	0.18	0.13	0.16	0.20	0.11	0.09	0.13	0.93
living_as_married	0.28	0.27	0.22	0.24	0.23	0.24	0.22	0.94
manual_work	0.05	0.04	0.04	0.04	0.05	0.08	0.04	0.95
transport_work	0.00	0.00	0.00	0.00	0.00	0.00	0.00	
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	

Table 12: Balance on covariates among **all women compliers in the endline sample**.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
catholic	0.51	0.49	0.55	0.49	0.38	0.66	0.44	0.03
radius	354.29	479.49	483.33	594.87	509.09	385.29	451.16	0.03
married	0.46	0.36	0.57	0.62	0.51	0.34	0.51	0.04
english_christian	0.06	0.08	0.12	0.08	0.09	0.04	0.21	0.04
illiterate	0.14	0.10	0.24	0.03	0.20	0.21	0.05	0.05
munyoro	0.11	0.05	0.12	0.05	0.00	0.01	0.05	0.05
mobile_phone_use	3.26	2.77	2.45	3.21	3.07	3.19	3.44	0.05
age	30.63	27.38	27.74	30.05	31.07	32.25	29.72	0.06
other_work	0.00	0.08	0.00	0.05	0.00	0.03	0.02	0.09
holy_spirit	0.06	0.08	0.10	0.08	0.24	0.10	0.09	0.11
luganda_lang	0.91	0.97	0.88	0.79	0.80	0.82	0.95	0.14
religious_service	1.23	1.10	1.29	1.00	1.55	1.32	0.98	0.15
write_and_read	0.80	0.82	0.64	0.90	0.76	0.76	0.88	0.16
minority_lang	0.00	0.03	0.07	0.05	0.09	0.00	0.00	0.16
household_head	0.11	0.26	0.14	0.10	0.22	0.29	0.21	0.17
minority_tribe	0.03	0.08	0.12	0.03	0.13	0.03	0.02	0.17
domestic_work	0.14	0.10	0.17	0.05	0.13	0.04	0.14	0.17
village_official	0.20	0.15	0.07	0.08	0.11	0.07	0.05	0.25
highest_grade	6.89	6.85	4.93	6.31	5.91	5.50	6.28	0.25
other_person	0.06	0.15	0.19	0.10	0.04	0.18	0.19	0.25
christian_only	0.06	0.08	0.00	0.03	0.02	0.03	0.00	0.28
clergy	0.66	0.49	0.40	0.51	0.56	0.54	0.49	0.28
munyankole	0.09	0.10	0.12	0.05	0.07	0.22	0.09	0.32
runyannkole_lang	0.03	0.00	0.05	0.00	0.05	0.12	0.05	0.34
frequency_discussion	1.66	1.21	1.50	1.21	1.45	1.43	1.35	0.34
principal	0.34	0.26	0.14	0.28	0.24	0.26	0.19	0.40
education_work	0.09	0.03	0.00	0.03	0.02	0.03	0.02	0.44
judge	0.06	0.05	0.07	0.03	0.15	0.09	0.09	0.44
read_only	0.03	0.03	0.05	0.00	0.04	0.00	0.00	0.46
fumbira_lang	0.06	0.00	0.00	0.15	0.05	0.06	0.00	0.47
mutooro	0.03	0.05	0.02	0.00	0.02	0.01	0.09	0.47
separated	0.14	0.18	0.07	0.08	0.18	0.21	0.14	0.50
doctor	0.37	0.31	0.36	0.33	0.35	0.24	0.47	0.54
official	0.14	0.23	0.14	0.15	0.16	0.09	0.14	0.60
teacher	0.66	0.54	0.45	0.72	0.51	0.51	0.56	0.61
mufumbira_tribe	0.09	0.00	0.02	0.15	0.05	0.07	0.00	0.62
mukiga	0.00	0.05	0.02	0.05	0.09	0.01	0.02	0.64
living_conditions_compared	2.14	1.87	2.02	2.13	2.05	1.93	2.05	0.64
not_married	0.11	0.15	0.14	0.05	0.09	0.19	0.14	0.65
agriculture_work	0.54	0.44	0.62	0.69	0.58	0.62	0.51	0.66
write_only	0.03	0.05	0.07	0.08	0.00	0.03	0.07	0.67
university	0.03	0.03	0.00	0.03	0.05	0.01	0.00	0.72
hospitality_work	0.06	0.15	0.07	0.08	0.11	0.06	0.12	0.72
no_work	0.06	0.15	0.07	0.08	0.11	0.06	0.12	0.72
muslim	0.20	0.10	0.14	0.23	0.13	0.09	0.14	0.72
munyarwanda	0.03	0.15	0.17	0.15	0.13	0.16	0.12	0.73
police	0.37	0.33	0.36	0.21	0.31	0.26	0.23	0.75
muganda_tribe	0.63	0.51	0.40	0.51	0.51	0.47	0.60	0.76
living_as_married	0.29	0.31	0.21	0.26	0.22	0.26	0.21	0.84
survey_luganda	0.97	1.00	0.98	0.95	0.98	0.97	1.00	0.84
cooperative	2.80	2.74	2.76	2.77	2.71	2.68	2.81	0.84
female	1.00	1.00	1.00	1.00	1.00	1.00	1.00	0.85

manual_work	0.06	0.05	0.02	0.05	0.05	0.07	0.05	0.86
retail_work	0.11	0.10	0.07	0.05	0.09	0.13	0.14	0.90
day	1.40	1.36	1.38	1.36	1.40	1.32	1.49	0.94
transport_work	0.00	0.00	0.00	0.00	0.00	0.00	0.00	
atheist	0.00	0.00	0.00	0.00	0.00	0.00	0.00	

Table 13: Balance on covariates among **women panel compliers in the endline sample**.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
minority_tribe	0.06	0.18	0.06	0.05	0.12	0.03	0.05	0.01
domestic_work	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.03
minority_lang	0.04	0.04	0.09	0.01	0.04	0.04	0.00	0.07
principal	0.37	0.23	0.23	0.19	0.20	0.17	0.25	0.09
day	1.17	1.20	1.13	1.26	1.24	1.37	1.10	0.10
atheist	0.00	0.00	0.02	0.00	0.00	0.00	0.00	0.10
english_christian	0.01	0.07	0.04	0.08	0.07	0.07	0.15	0.12
munyarwanda	0.09	0.05	0.19	0.09	0.06	0.13	0.08	0.14
radius	395.06	450.55	471.30	540.54	511.03	378.26	435.85	0.16
education_work	0.04	0.07	0.02	0.01	0.01	0.01	0.01	0.18
cooperative	2.77	2.87	2.74	2.79	2.68	2.85	2.80	0.19
write_only	0.06	0.01	0.06	0.04	0.04	0.06	0.00	0.20
mutooro	0.00	0.02	0.03	0.01	0.02	0.00	0.00	0.22
teacher	0.63	0.70	0.61	0.59	0.54	0.50	0.64	0.26
retail_work	0.07	0.04	0.12	0.12	0.08	0.04	0.08	0.28
age	29.42	31.00	29.94	30.57	31.82	32.06	30.44	0.30
university	0.07	0.05	0.05	0.05	0.07	0.01	0.03	0.30
holy_spirit	0.09	0.05	0.12	0.14	0.08	0.11	0.08	0.34
doctor	0.36	0.34	0.35	0.30	0.32	0.26	0.42	0.37
mukiga	0.06	0.02	0.02	0.05	0.10	0.04	0.04	0.38
luganda_lang	0.91	0.91	0.84	0.89	0.87	0.81	0.95	0.41
household_head	0.88	0.87	0.89	0.87	0.93	0.90	0.92	0.43
other_work	0.05	0.07	0.10	0.04	0.07	0.11	0.08	0.43
police	0.27	0.31	0.35	0.24	0.27	0.23	0.21	0.43
official	0.32	0.26	0.28	0.27	0.17	0.23	0.23	0.45
highest_grade	7.27	7.89	6.96	7.23	7.47	6.47	7.06	0.49
manual_work	0.11	0.13	0.05	0.11	0.10	0.10	0.13	0.49
muslim	0.23	0.23	0.15	0.13	0.19	0.07	0.19	0.50
survey_luganda	1.00	0.99	0.99	1.00	0.99	0.97	1.00	0.51
religious_service	2.67	2.78	1.19	1.06	1.88	1.14	1.31	0.52
judge	0.15	0.12	0.07	0.13	0.14	0.10	0.08	0.53
living_as_married	0.25	0.13	0.19	0.25	0.17	0.17	0.21	0.54
village_official	0.09	0.07	0.06	0.06	0.11	0.08	0.13	0.58
runyannkole_lang	0.02	0.03	0.05	0.03	0.04	0.12	0.04	0.60
transport_work	0.10	0.07	0.09	0.08	0.05	0.08	0.11	0.62
catholic	0.43	0.41	0.44	0.50	0.46	0.52	0.38	0.63
christian_only	0.01	0.00	0.06	0.04	0.02	0.03	0.02	0.68
mufumbira_tribe	0.00	0.02	0.03	0.09	0.05	0.03	0.01	0.69
munyoro	0.05	0.05	0.07	0.07	0.04	0.03	0.09	0.69
other_person	0.04	0.04	0.04	0.00	0.05	0.03	0.04	0.72
munyankole	0.09	0.10	0.11	0.06	0.10	0.17	0.13	0.76
muganda_tribe	0.65	0.55	0.49	0.58	0.52	0.57	0.59	0.78
frequency_discussion	1.60	1.62	1.76	1.60	1.67	1.76	1.65	0.78
living_conditions_compared	2.27	2.13	2.15	2.23	2.14	2.20	2.17	0.79
not_married	0.21	0.25	0.24	0.21	0.19	0.27	0.23	0.82
separated	0.05	0.04	0.04	0.04	0.04	0.03	0.07	0.85
write_and_read	0.80	0.90	0.85	0.86	0.86	0.83	0.88	0.85
agriculture_work	0.58	0.60	0.59	0.63	0.65	0.63	0.56	0.85
read_only	0.04	0.02	0.02	0.02	0.01	0.02	0.03	0.86
married	0.49	0.57	0.54	0.50	0.60	0.52	0.50	0.87
fumbira_lang	0.02	0.01	0.02	0.07	0.05	0.03	0.01	0.89
hospitality_work	0.04	0.02	0.01	0.02	0.04	0.03	0.03	0.89
no_work	0.04	0.02	0.01	0.02	0.04	0.03	0.03	0.89
mobile_phone_use	3.69	3.49	3.56	3.67	3.54	3.64	3.63	0.92
clergy	0.53	0.56	0.50	0.50	0.49	0.52	0.49	0.96
illiterate	0.10	0.07	0.07	0.08	0.10	0.10	0.09	1.00
female	0.00	0.00	0.00	0.00	0.00	0.00	0.00	

Table 14: Balance on covariates among **all men compliers in the endline sample**.

	PLA	VAW	ABO	ABS	ABO_ABS	VAW_ABS	VAW_ABO	p-value
minority_tribe	0.06	0.18	0.06	0.05	0.12	0.04	0.04	0.00
domestic_work	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.04
minority_lang	0.04	0.04	0.10	0.01	0.03	0.05	0.00	0.06
day	1.16	1.21	1.12	1.28	1.25	1.39	1.11	0.07
atheist	0.00	0.00	0.02	0.00	0.00	0.00	0.00	0.10
write_only	0.06	0.01	0.06	0.03	0.03	0.06	0.00	0.12
principal	0.37	0.22	0.23	0.20	0.20	0.18	0.25	0.12
munyarwanda	0.09	0.06	0.20	0.09	0.06	0.14	0.09	0.13
english_christian	0.01	0.07	0.04	0.08	0.07	0.07	0.16	0.13
radius	391.14	448.31	466.35	540.19	514.06	379.28	434.31	0.16
education_work	0.04	0.07	0.02	0.01	0.02	0.01	0.01	0.19
cooperative	2.76	2.87	2.73	2.79	2.67	2.85	2.79	0.20
mutooro	0.00	0.02	0.03	0.01	0.02	0.00	0.00	0.21
teacher	0.62	0.70	0.62	0.58	0.55	0.49	0.65	0.28
doctor	0.37	0.33	0.37	0.30	0.34	0.25	0.42	0.29

university	0.08	0.06	0.05	0.05	0.07	0.01	0.03	0.31
retail_work	0.08	0.04	0.12	0.10	0.08	0.05	0.08	0.32
police	0.28	0.31	0.36	0.25	0.27	0.22	0.20	0.32
luganda_lang	0.91	0.91	0.84	0.89	0.88	0.81	0.96	0.36
christian_only	0.00	0.00	0.06	0.02	0.02	0.04	0.01	0.36
holy_spirit	0.09	0.06	0.12	0.15	0.09	0.10	0.08	0.39
village_official	0.09	0.07	0.06	0.05	0.11	0.08	0.14	0.40
muslim	0.24	0.22	0.14	0.13	0.18	0.06	0.19	0.45
other_work	0.05	0.07	0.10	0.04	0.07	0.12	0.09	0.47
household_head	0.89	0.87	0.88	0.87	0.93	0.90	0.91	0.48
manual_work	0.10	0.13	0.05	0.11	0.11	0.10	0.14	0.48
age	29.65	30.42	29.40	30.03	31.38	31.55	30.05	0.49
survey_luganda	1.00	0.99	0.99	1.00	0.99	0.97	1.00	0.49
mukiga	0.06	0.02	0.02	0.05	0.09	0.05	0.04	0.51
highest_grade	7.27	7.91	7.06	7.25	7.57	6.56	7.23	0.53
religious_service	2.71	2.82	1.21	1.07	1.87	1.15	1.32	0.54
runyannkole_lang	0.03	0.03	0.05	0.03	0.04	0.13	0.03	0.56
living_as_married	0.25	0.13	0.19	0.25	0.17	0.18	0.21	0.57
catholic	0.43	0.40	0.43	0.50	0.47	0.53	0.37	0.57
other_person	0.03	0.04	0.04	0.00	0.05	0.02	0.04	0.59
munyoro	0.05	0.06	0.08	0.07	0.03	0.03	0.10	0.60
mufumbira_tribe	0.00	0.02	0.02	0.09	0.04	0.03	0.01	0.61
official	0.32	0.27	0.28	0.27	0.18	0.23	0.23	0.61
judge	0.15	0.10	0.08	0.12	0.12	0.09	0.09	0.61
transport_work	0.10	0.07	0.10	0.08	0.05	0.08	0.12	0.66
not_married	0.20	0.26	0.25	0.21	0.19	0.28	0.23	0.72
munyankole	0.09	0.10	0.12	0.07	0.09	0.17	0.12	0.74
muganda_tribe	0.65	0.54	0.48	0.57	0.55	0.56	0.61	0.76
write_and_read	0.80	0.90	0.86	0.87	0.87	0.82	0.88	0.77
separated	0.05	0.04	0.03	0.04	0.04	0.04	0.07	0.79
frequency_discussion	1.59	1.61	1.71	1.59	1.66	1.76	1.64	0.79
agriculture_work	0.58	0.60	0.59	0.64	0.63	0.62	0.54	0.84
living_conditions_compared	2.27	2.12	2.14	2.21	2.14	2.20	2.17	0.84
read_only	0.04	0.02	0.02	0.02	0.01	0.02	0.03	0.85
fumbira_lang	0.03	0.01	0.02	0.07	0.05	0.02	0.01	0.87
married	0.49	0.56	0.53	0.50	0.60	0.50	0.50	0.88
hospitality_work	0.04	0.02	0.01	0.02	0.04	0.03	0.03	0.90
no_work	0.04	0.02	0.01	0.02	0.04	0.03	0.03	0.90
mobile_phone_use	3.68	3.48	3.59	3.65	3.61	3.63	3.66	0.95
clergy	0.53	0.55	0.50	0.48	0.51	0.52	0.49	0.97
illiterate	0.10	0.07	0.07	0.08	0.09	0.10	0.09	1.00
female	0.00	0.00	0.00	0.00	0.00	0.00	0.00	

Table 15: Balance on covariates among **men panel compliers in the endline sample**.

D Robustness Checks

In this section of the appendix we illustrate that our main results (those presented in Tables 1, 2, 3 and 4 of the paper) are robust to alternative estimation strategies.

- Subsection D.1 reports results of the pre-registered procedure for testing for cross-over effects between the VAW-related outcomes and non-VAW related treatment messages (absenteeism and abortion stigma). It illustrates that results are robust to an estimation strategy in which we allow for the presence of crossover effects.

- Subsection D.2 addresses robustness of our main results to the imbalance in the treatment induced by our crossover design. The substantive interpretation and statistical significance of our main results do not change when using an alternative estimator that does not assume absence of crossover effects.

- Subsection D.3 illustrates that our main results are robust to strategies in which we: do not impute values that are missing at the item-level instead of using multiple imputation via chained equations (MICE); include on the righthandside of the regression equation covariates selected through the pre-registered lasso procedure; and include all compliers in the mid-line and endline, rather than restricting attention to those compliers in both datasets (panel compliers).

- Subsection D.4 shows that our main results are robust to the pre-registered procedure for dealing with attrition using extreme value bounding.

D.1 Crossover Effects

When designing the study, we expected each of the three messages to affect only views and perceived norms in that issue domain. In other words, we expect messages about domestic violence to affect views and perceived norms regarding domestic violence but not regarding abortion or absenteeism. Thus, our core model is one in which views or norms regarding a given topic are represented as a function of village-level exposure to messages on that topic and that topic only.

We use the term "crossover effects" to refer to the effects that videos on one substantive topic may have on outcomes associated with another substantive topic. For example, if the VAW videos

were to affect attitudes about abortion, that would constitute a crossover effect.

The existence of crossover effects is not entirely implausible, because all our treatments convey the message that one should take action of some sort. We thus test for the existence of crossover and interaction effects, where interaction effects refer to the effects of particular combinations of video messages. If crossover and interaction effects exist, we expect them to be positive. We also expect them to be strongest for the abortion and VAW treatments given that both are related to views on gender roles.

For each outcome we analyze, we conduct a two-step diagnosis for crossover effects.

1. The first step is only run if the outcome is abortion- or VAW-related; as specified above, these are two outcomes where we do see a positive interaction as plausible. We take the observed difference in F-statistics between the main model that only has the relevant treatment indicator and covariates on the right-hand side, and an augmented model that contains the other treatment indicator (VAW if the outcome is abortion-related, and vice versa) and its interaction with the main treatment indicator. We obtain a p-value for the difference in F-statistics.

2. In the second step, which we carry out for all models, we will run the 'saturated crossover' model, which contains all treatment indicators and their pairwise interactions. We follow a similar F-test procedure using randomization inference as described in step 1, and obtain a p-value for the observed difference in F-statistics.

As stated in our pre-analysis plan, if the p-value obtained in step 2 is less than .05, we report the fully saturated model from step 2. If the p-value obtained from step 2 is greater than .05 but that from step 1 is less than .05, we report the IPV-Abortion interaction model from step 1. If neither p-value is less than .05, we report the main specification with only the relevant treatment indicator and covariates on the right-hand side.

Table 16 reports the results for the first step under "abortion interaction p-value," and those for the second step under "fully saturated p-value."

Outcome	Wave	Subset	Abortion interaction p-value	Fully saturated p-value
Number of Incidents	endline	all women	0.339	0.360
Any Incidents	endline	all women	0.528	0.832
Reporting Index	endline	complier men	0.115	0.051
Report To Parents	endline	complier men	0.549	0.550
Report to Counselor	endline	complier men	0.441	0.538
Report to LC1 Chairperson	endline	complier men	0.370	0.119
Report to Police	endline	complier men	0.090	0.210
Reporting Index	endline	complier women	0.036	0.177
Report To Parents	endline	complier women	0.012	0.079
Report to Counselor	endline	complier women	0.019	0.137
Report to LC1 Chairperson	endline	complier women	0.646	0.632
Report to Police	endline	complier women	0.974	0.916
Reporting Index	midline	complier men	0.760	0.158
Report To Parents	midline	complier men	0.856	0.312
Report to Counselor	midline	complier men	0.464	0.618
Report to LC1 Chairperson	midline	complier men	0.651	0.729
Report to Police	midline	complier men	0.929	0.330
Reporting Index	midline	complier women	0.554	0.220
Report To Parents	midline	complier women	0.357	0.208
Report to Counselor	midline	complier women	0.391	0.544
Report to LC1 Chairperson	midline	complier women	0.490	0.730
Report to Police	midline	complier women	0.259	0.068
Personal Retribution	endline	complier men	0.504	0.040
Personal Retribution	endline	complier women	0.725	0.259
Social Repercussions	endline	complier men	0.517	0.343
Social Repercussions	endline	complier women	0.886	0.844
Community Would Intervene	midline	men compliers	0.118	0.133
Community Would Intervene	endline	men compliers	0.181	0.106
Community Would Intervene	midline	women compliers	0.346	0.604
Community Would Intervene	endline	women compliers	0.852	0.991

Table 16: F-tests for the presence of crossover effects.

We see some sporadic indications of crossovers on the intervene index among men and women in the midline and endline. In line with our pre-analysis plan, we report results from the models used in steps 1 and 2 as a function of the p-values in Table 16. The substantive interpretation and statistical significance of the results remains unchanged.

	Reporting Index	Report to Parents	Report to Counselor	Personal Retribution
	Endline	Endline	Endline	Midline
	(1)	(2)	(3)	(4)
Anti-VAW Media	0.212^{***}	0.311^{***}	0.328^{***}	-0.121
	(0.048)	(0.062)	(0.076)	(0.093)
Anti-Abortion Stigma Media	0.095^{**}	0.167^{**}	0.156^{**}	-0.172
	(0.044)	(0.074)	(0.085)	(0.089)
Anti-Absenteeism Media				-0.209
				(0.077)
VAW x Abortion	-0.169	-0.338	-0.317	0.152
	(0.070)	(0.105)	(0.112)	(0.122)
VAW x Absenteeism				0.098
				(0.110)
Abortion x Absenteeism				0.218^{**}
				(0.110)
RI p-values	0	0	0	0.914
Sample	Women	Women	Women	Men
Block FE	Yes	Yes	Yes	Yes
Observations	321	321	321	720
Adjusted R^2	0.089	0.030	0.062	0.012

*p<0.1; **p<0.05; ***p<0.01

Table 17: Unrestricted models from pre-registered crossover analysis procedure (Compliers).

See Tables 2 and 4 for more details on the main specifications. All analyses use individual respondents as the unit of observation. All p-values are calculated using randomization inference. Significance stars are based on a two-sided test for the effect of the main treatment (*Anti-VAW Media*). See section B.1 of the appendix for details on model specifications and section A of the appendix for details on question wording.

D.2 Agnostic Estimation

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 the VAW-treated subjects with the VAW-untreated subjects is slightly imbalanced. 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). As can be seen in tables 18 and 19, we find that this estimator of the VAW treatment effect produces very similar point estimates but larger standard errors due to the diminished number of subjects.

A slightly less agnostic approach would be to include the absenteeism+abortion group in the analysis along with the aforementioned fixed effects; this approach in effect introduces the assumption that the fixed effects are additive. Again, this modeling approach produces similar results. The model we use throughout this paper is a special case of this specification, imposing the constraint that these fixed effects are zero.

	Any Incidents		Reporting Index		Social Repercussions
	Endline	Endline	Midline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)
Anti-VAW Media	−0.046*	−0.064**	0.094***	0.125***	−0.143**
	(0.024)	(0.028)	(0.032)	(0.039)	(0.052)
Abortion	0.036	0.052	−0.074	0.003	−0.053
	(0.026)	(0.034)	(0.043)	(0.048)	(0.074)
Absenteeism	0.020	0.027	−0.072	0.017	−0.034
	(0.030)	(0.034)	(0.044)	(0.051)	(0.087)
Control Mean	0.18	0.2	0.34	0.41	0.61
RI p-values VAW	0.08	0.026	0.006	0.002	0.018
RI p-values Abortion	0.256	0.142	0.944	0.498	0.278
RI p-values Absenteeism	0.535	0.458	0.94	0.365	0.338
Hypothesis	Two	Two	Upr	Upr	Lwr
Analysis Level	Indiv.	Clus.	Indiv.	Indiv.	Indiv.
Block FE	Yes	Yes	Yes	Yes	Yes
Observations	900	94	266	266	266
Adjusted R^2	0.010	0.030	0.067	0.074	0.004

*p<0.1; **p<0.05; ***p<0.01

Table 18: Effects among women using an estimator that is unbiased in the presence of cross-over effects.

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. The row 'Hypothesis' indicates the direction of the hypothesis test for both the effect of 'Anti-VAW media' and the two fixed effects. The RI p-values are included in the rows below. These are calculated by re-randomizing based on all clusters and subsequently excluding observations from clusters assigned to the absenteeism+abortion treatment condition from the analysis. Estimates in column 1 and 2 are based on responses from all women in the endline survey (not only compliers). Estimates in columns 3,4 and 5 are based on responses from women compliers. The analyses in column 1 and 3 to 5 use individual respondents as the unit of observation. The analysis in column 2 is conducted at the village level, after collapsing individual responses to the cluster-level using cluster-level means. See section B.1 of the appendix for details on model specifications and section A of the appendix for details on question wording.

	Reporting Index		Social Repercussions
	Midline	Endline	Endline
	(1)	(2)	(3)
Anti-VAW Media	0.041**	0.054**	−0.037
	(0.021)	(0.025)	(0.036)
Abortion	−0.031	−0.092	−0.074*
	(0.028)	(0.029)	(0.041)
Absenteeism	0.008	−0.040	−0.120***
	(0.028)	(0.031)	(0.047)
Control Mean	0.38	0.43	0.47
RI p-values VAW	0.046	0.033	0.2
RI p-values Abortion	0.836	0.996	0.077
RI p-values Absenteeism	0.398	0.87	0.008
Hypothesis	Upr	Upr	Lwr
Analysis Level	Indiv.	Indiv.	Indiv.
Block FE	Yes	Yes	Yes
Observations	592	592	592
Adjusted R^2	0.008	0.047	0.010

*p<0.1; **p<0.05; ***p<0.01

Table 19: Effects among men using an estimator that is unbiased in the presence of cross-over effects.

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. The row 'Hypothesis' indicates the direction of the hypothesis test for both the effect of 'Anti-VAW media' and the two fixed effects. The RI p-values are included in the rows below. These are calculated by re-randomizing based on all clusters and subsequently excluding observations from clusters assigned to the absenteeism+abortion treatment condition from the analysis. All estimates are based on responses from men compliers and all analyses use individual respondents as the unit of observation. See section B.1 of the appendix for details on model specifications and section A of the appendix for details on question wording.

D.3 Robustness of Main Results to Alternative Estimation Strategies

Main results presented in Tables 1, 2, 3 and 4 of the paper are estimated using multiple imputation via chained equations (MICE) and do not condition on covariates beyond resample fixed effects and average audience size. In the following tables we show that our results are robust to alternative strategies that do not use imputations or condition on covariates selected using the pre-registered lasso strategy. In addition, Tables 2, 3 and 4 focus on panel compliers for the sake of comparability. We show here that our results are robust to considering effects among all compliers in the midline

(endline), irrespective of their presence in the endline (midline). The tables can be summarized as follows:

- Table 20 shows that the statistical significance and substantive interpretation of the main effects of the treatment on violence remain largely unchanged by adopting either a strategy in which no imputation is used or lasso-selected covariates are included in the righthandside of the regression equation. The effects of the treatment on violence frequency among all women fall short of statistical significance when incorporating the lasso-selected covariates, possibly due to the increased complexity of the model being fit.

- Table 21 shows that the statistical significance and substantive interpretation of the main effects of the treatment on attitudes towards reporting among women compliers remain largely unchanged by the use of alternative analysis strategies.

- Table 22 shows that the statistical significance and substantive interpretation of the main effects of the treatment on attitudes towards reporting among men compliers remain largely unchanged by the use of alternative analysis strategies. The effect on reporting to the village leader fall short of statistical significance when using covariates in the endline, and that on reporting to the village counselor falls short of significance in the endline when using all compliers instead of those only included in the panel.

- Table 23 shows that the statistical significance and substantive interpretation of the main effects of the treatment on perceptions of the costs associated with reporting remain largely unchanged by the use of alternative analysis strategies. The effect on the view the community would intervene falls short of significance in both the midline and endline for men when using covariates, while the use of such covariates makes the same effect significant for women in the endline.

Outcome	Original sample	Alternative specification	Alt. Est.	Orig. Est.
Number of Incidents (EL)	All women	No imputations	-0.15 (p = 0.150)	-0.15 (p = 0.159)
Number of Incidents (EL)	All women	Lasso covariates	-0.13 (p = 0.204)	-0.15 (p = 0.159)
Number of Incidents (EL)	Women compliers (all)	No imputations	-0.35 (p = 0.134)	-0.35 (p = 0.138)
Number of Incidents (EL)	Women compliers (all)	Lasso covariates	-0.28 (p = 0.200)	-0.35 (p = 0.138)
Any Incidents (EL)	All women	No imputations	-0.05 (p = 0.034)	-0.05 (p = 0.038)
Any Incidents (EL)	All women	Lasso covariates	-0.05 (p = 0.038)	-0.05 (p = 0.038)
Any Incidents (EL)	Women compliers (all)	No imputations	-0.13 (p = 0.006)	-0.13 (p = 0.007)
Any Incidents (EL)	Women compliers (all)	Lasso covariates	-0.13 (p = 0.007)	-0.13 (p = 0.007)
Violence Frequency (EL)	All women	No imputations	-0.30 (p = 0.081)	-0.30 (p = 0.088)
Violence Frequency (EL)	All women	Lasso covariates	-0.28 (p = 0.118)	-0.30 (p = 0.088)
Violence Frequency (EL)	Women compliers (all)	No imputations	-0.68 (p = 0.012)	-0.68 (p = 0.014)
Violence Frequency (EL)	Women compliers (all)	Lasso covariates	-0.62 (p = 0.025)	-0.68 (p = 0.014)

Table 20: Robustness of main results reported in Table 1 to alternative estimation strategies.
Outcome indicates the outcome and whether it was measured in the endline (EL) or midline (ML). *Original sample* indicates the participants among whom the results in the original table were estimated: All women indicates that the sample included all of the women in the sample, irrespective of compliance status; women compliers (all) indicates that the sample includes all of the compliers surveyed in the sample, and not only those who were present in both the midline and endline samples (panel compliers). Alternative specification indicates the alternative approach used.

Outcome	Original sample	Alternative specification	Alt. Est.	Orig. Est.
Parents (ML)	Women compliers (panel only)	No imputations	0.09 (p = 0.071)	0.09 (p = 0.068)
Parents (EL)	Women compliers (panel only)	No imputations	0.14 (p = 0.008)	0.14 (p = 0.008)
Parents (ML)	Women compliers (panel only)	Lasso covariates	0.09 (p = 0.068)	0.09 (p = 0.068)
Parents (EL)	Women compliers (panel only)	Lasso covariates	0.14 (p = 0.008)	0.14 (p = 0.008)
Parents (ML)	Women compliers (panel only)	Women compliers (all)	0.10 (p = 0.046)	0.09 (p = 0.068)
Parents (EL)	Women compliers (panel only)	Women compliers (all)	0.13 (p = 0.010)	0.14 (p = 0.008)
Counselor (ML)	Women compliers (panel only)	No imputations	0.08 (p = 0.100)	0.08 (p = 0.100)
Counselor (EL)	Women compliers (panel only)	No imputations	0.17 (p = 0.004)	0.17 (p = 0.004)
Counselor (ML)	Women compliers (panel only)	Lasso covariates	0.08 (p = 0.088)	0.08 (p = 0.100)
Counselor (EL)	Women compliers (panel only)	Lasso covariates	0.18 (p = 0.004)	0.17 (p = 0.004)
Counselor (ML)	Women compliers (panel only)	Women compliers (all)	0.07 (p = 0.084)	0.08 (p = 0.100)
Counselor (EL)	Women compliers (panel only)	Women compliers (all)	0.13 (p = 0.016)	0.17 (p = 0.004)
LC1 Chairperson (ML)	Women compliers (panel only)	No imputations	0.06 (p = 0.133)	0.06 (p = 0.133)
LC1 Chairperson (EL)	Women compliers (panel only)	No imputations	0.06 (p = 0.156)	0.06 (p = 0.148)
LC1 Chairperson (ML)	Women compliers (panel only)	Lasso covariates	0.05 (p = 0.148)	0.06 (p = 0.133)
LC1 Chairperson (EL)	Women compliers (panel only)	Lasso covariates	0.04 (p = 0.228)	0.06 (p = 0.148)
LC1 Chairperson (ML)	Women compliers (panel only)	Women compliers (all)	0.10 (p = 0.024)	0.06 (p = 0.133)
LC1 Chairperson (EL)	Women compliers (panel only)	Women compliers (all)	0.04 (p = 0.208)	0.06 (p = 0.148)
Police (ML)	Women compliers (panel only)	No imputations	0.11 (p = 0.008)	0.11 (p = 0.008)
Police (EL)	Women compliers (panel only)	No imputations	0.13 (p = 0.010)	0.13 (p = 0.008)
Police (ML)	Women compliers (panel only)	Lasso covariates	0.11 (p = 0.008)	0.11 (p = 0.008)
Police (EL)	Women compliers (panel only)	Lasso covariates	0.13 (p = 0.008)	0.13 (p = 0.008)
Police (ML)	Women compliers (panel only)	Women compliers (all)	0.14 (p = 0.000)	0.11 (p = 0.008)
Police (EL)	Women compliers (panel only)	Women compliers (all)	0.09 (p = 0.036)	0.13 (p = 0.008)

Table 21: Robustness of main results reported in Table 2 to alternative estimation strategies.
Outcome indicates the outcome and whether it was measured in the endline (EL) or midline (ML). *Original sample* indicates the participants among whom the results in the original table were estimated: women compliers (panel only) indicates that the sample includes only women compliers surveyed in both midline and endline samples. Alternative specification indicates the alternative approach used.

Outcome	Original sample	Alternative specification	Alt. Est.	Orig. Est.
Parents (ML)	Men compliers (panel only)	No imputations	0.08 (p = 0.027)	0.08 (p = 0.021)
Parents (EL)	Men compliers (panel only)	No imputations	0.04 (p = 0.166)	0.04 (p = 0.176)
Parents (ML)	Men compliers (panel only)	Lasso covariates	0.08 (p = 0.020)	0.08 (p = 0.021)
Parents (EL)	Men compliers (panel only)	Lasso covariates	0.03 (p = 0.204)	0.04 (p = 0.176)
Parents (ML)	Men compliers (panel only)	Men compliers (all)	0.08 (p = 0.025)	0.08 (p = 0.021)
Parents (EL)	Men compliers (panel only)	Men compliers (all)	0.03 (p = 0.195)	0.04 (p = 0.176)
Counselor (ML)	Men compliers (panel only)	No imputations	0.04 (p = 0.183)	0.04 (p = 0.183)
Counselor (EL)	Men compliers (panel only)	No imputations	0.05 (p = 0.068)	0.05 (p = 0.068)
Counselor (ML)	Men compliers (panel only)	Lasso covariates	0.04 (p = 0.178)	0.04 (p = 0.183)
Counselor (EL)	Men compliers (panel only)	Lasso covariates	0.06 (p = 0.058)	0.05 (p = 0.068)
Counselor (ML)	Men compliers (panel only)	Men compliers (all)	0.04 (p = 0.148)	0.04 (p = 0.183)
Counselor (EL)	Men compliers (panel only)	Men compliers (all)	0.04 (p = 0.129)	0.05 (p = 0.068)
LC1 Chairperson (ML)	Men compliers (panel only)	No imputations	0.02 (p = 0.260)	0.02 (p = 0.281)
LC1 Chairperson (EL)	Men compliers (panel only)	No imputations	0.06 (p = 0.051)	0.06 (p = 0.056)
LC1 Chairperson (ML)	Men compliers (panel only)	Lasso covariates	0.02 (p = 0.324)	0.02 (p = 0.281)
LC1 Chairperson (EL)	Men compliers (panel only)	Lasso covariates	0.04 (p = 0.108)	0.06 (p = 0.056)
LC1 Chairperson (ML)	Men compliers (panel only)	Men compliers (all)	0.04 (p = 0.166)	0.02 (p = 0.281)
LC1 Chairperson (EL)	Men compliers (panel only)	Men compliers (all)	0.07 (p = 0.036)	0.06 (p = 0.056)
Police (ML)	Men compliers (panel only)	No imputations	-0.05 (p = 0.947)	-0.05 (p = 0.946)
Police (EL)	Men compliers (panel only)	No imputations	0.02 (p = 0.348)	0.02 (p = 0.318)
Police (ML)	Men compliers (panel only)	Lasso covariates	-0.04 (p = 0.932)	-0.05 (p = 0.946)
Police (EL)	Men compliers (panel only)	Lasso covariates	0.03 (p = 0.198)	0.02 (p = 0.318)
Police (ML)	Men compliers (panel only)	Men compliers (all)	-0.04 (p = 0.956)	-0.05 (p = 0.946)
Police (EL)	Men compliers (panel only)	Men compliers (all)	0.02 (p = 0.298)	0.02 (p = 0.318)

Table 22: Robustness of main results reported in Table 3 to alternative estimation strategies.
Outcome indicates the outcome and whether it was measured in the endline (EL) or midline (ML). *Original sample* indicates the participants among whom the results in the original table were estimated: men compliers (panel only) indicates that the sample includes only men compliers surveyed in both midline and endline samples. Alternative specification indicates the alternative approach used.

Outcome	Original sample	Alternative specification	Alt. Est.	Orig. Est.
Community Would Intervene (ML)	Men compliers (panel only)	No imputations	0.05 (p = 0.048)	0.05 (p = 0.052)
Community Would Intervene (EL)	Men compliers (panel only)	No imputations	0.04 (p = 0.096)	0.04 (p = 0.096)
Community Would Intervene (ML)	Men compliers (panel only)	Lasso covariates	0.04 (p = 0.108)	0.05 (p = 0.052)
Community Would Intervene (EL)	Men compliers (panel only)	Lasso covariates	0.03 (p = 0.140)	0.04 (p = 0.096)
Community Would Intervene (ML)	Men compliers (panel only)	Men compliers (all)	0.04 (p = 0.082)	0.05 (p = 0.052)
Community Would Intervene (EL)	Men compliers (panel only)	Men compliers (all)	0.05 (p = 0.074)	0.04 (p = 0.096)
Community Would Intervene (ML)	Women compliers (panel only)	No imputations	0.08 (p = 0.092)	0.08 (p = 0.092)
Community Would Intervene (EL)	Women compliers (panel only)	No imputations	0.07 (p = 0.106)	0.07 (p = 0.106)
Community Would Intervene (ML)	Women compliers (panel only)	Lasso covariates	0.09 (p = 0.082)	0.08 (p = 0.092)
Community Would Intervene (EL)	Women compliers (panel only)	Lasso covariates	0.10 (p = 0.030)	0.07 (p = 0.106)
Community Would Intervene (ML)	Women compliers (panel only)	Women compliers (all)	0.10 (p = 0.034)	0.08 (p = 0.092)
Community Would Intervene (EL)	Women compliers (panel only)	Women compliers (all)	0.05 (p = 0.186)	0.07 (p = 0.106)
Personal Retribution (EL)	Men compliers (panel only)	No imputations	-0.02 (p = 0.356)	-0.02 (p = 0.344)
Personal Retribution (EL)	Men compliers (panel only)	Lasso covariates	-0.02 (p = 0.344)	-0.02 (p = 0.344)
Personal Retribution (EL)	Men compliers (panel only)	Men compliers (all)	-0.02 (p = 0.316)	-0.02 (p = 0.344)
Personal Retribution (EL)	Women compliers (panel only)	No imputations	-0.03 (p = 0.319)	-0.03 (p = 0.334)
Personal Retribution (EL)	Women compliers (panel only)	Lasso covariates	-0.02 (p = 0.382)	-0.03 (p = 0.334)
Personal Retribution (EL)	Women compliers (panel only)	Women compliers (all)	0.01 (p = 0.564)	-0.03 (p = 0.334)
Social Repercussions (EL)	Men compliers (panel only)	No imputations	-0.03 (p = 0.199)	-0.03 (p = 0.199)
Social Repercussions (EL)	Men compliers (panel only)	Lasso covariates	-0.03 (p = 0.238)	-0.03 (p = 0.199)
Social Repercussions (EL)	Men compliers (panel only)	Men compliers (all)	-0.03 (p = 0.241)	-0.03 (p = 0.199)
Social Repercussions (EL)	Women compliers (panel only)	No imputations	-0.11 (p = 0.042)	-0.11 (p = 0.042)
Social Repercussions (EL)	Women compliers (panel only)	Lasso covariates	-0.13 (p = 0.020)	-0.11 (p = 0.042)
Social Repercussions (EL)	Women compliers (panel only)	Women compliers (all)	-0.12 (p = 0.034)	-0.11 (p = 0.042)

Table 23: Robustness of main results reported in Table 4 to alternative estimation strategies.
Outcome indicates the outcome and whether it was measured in the endline (EL) or midline (ML). *Original sample* indicates the participants among whom the results in the original table were estimated: women / men compliers (panel only) indicates that the sample includes only women / men compliers surveyed in both midline and endline samples. Alternative specification indicates the alternative approach used.

D.4 Extreme Value Bounds

As mentioned in section F.2, we were unable to conduct our midline and endline household surveys in two out of the 112 villages in our sample. As we explain in section F.2, our failure to conduct the surveys seemed to be unrelated to the treatment status of these villages. We therefore simply exclude these villages from our analyses in the main part of the paper. In this section, we report extreme value bounds for our main estimates as a robustness check.

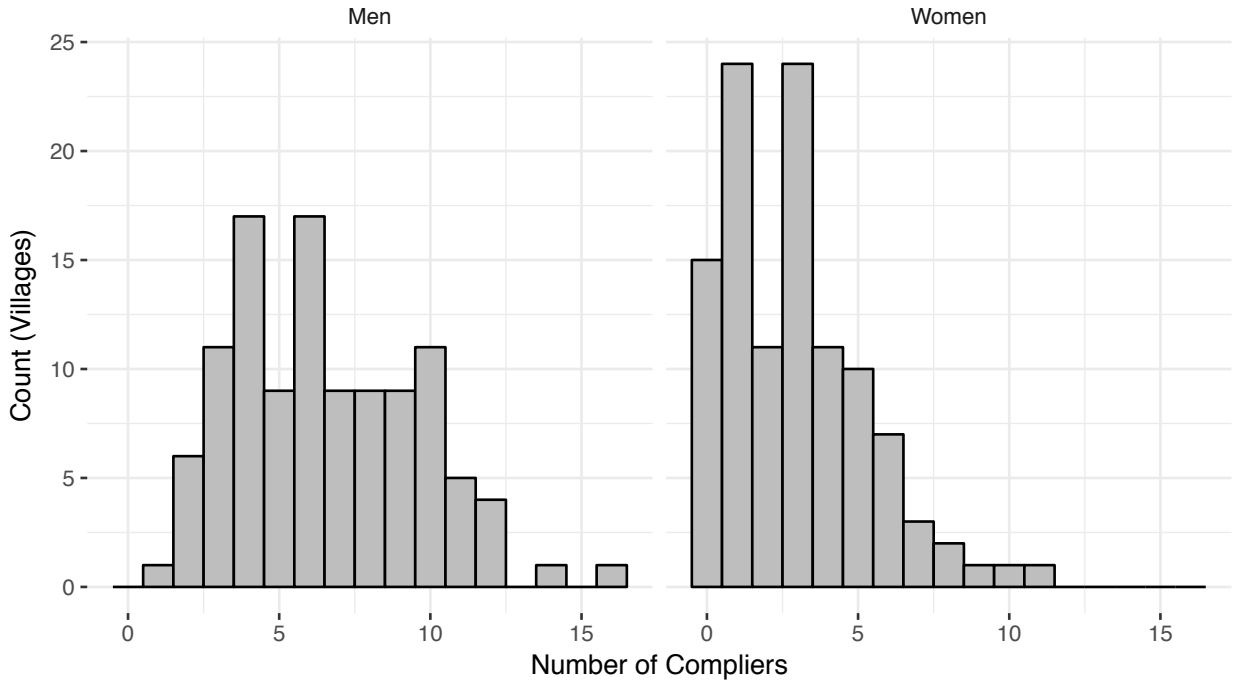

Figure 1: Distribution of the number of men and women compliers across villages.

To obtain bounds of complier average causal effects, we first predict the number of women and men compliers in the attrited villages. In line with our pre-analysis plan, we rely on a negative binomial model with the following predictors to model the number of women and men compliers per village in our midline sample: average attendance of women and, respectively, men during our screenings, the length of the radius from which respondents were sampled, block fixed effects as well as latitude and longitude of the village (video hall). We obtain the predicted number of men and women compliers for the two attrited villages in the midline. Subsequently, we multiply this number by the average of the cluster-level response rates for men and women compliers in the endline (.89 and .90 respectively). This procedure predicts 5 men and 2 women compliers for one and 7 men and 2 women compliers for the other attrited village.[5] See Figure 1 on how this compares to the distribution of compliance across villages.

Tables 24 and 25 report estimates among women and men compliers, respectively. Both of the attrited villages have received the VAW treatment. Upper bounds are thus obtained by imputing

[5]When using all compliers and overall attendance, the models predict a total number of 7 and 9 compliers, respectively. For men compliers, the negative bimodial model does not converge, but a poisson model gives the same prediction. When directly predicting the number of compliers in the endline, we obtain almost the same results (1 predicted complier more in one case and 1 complier less in another).

the highest (lowest) possible values of the outcome for the compliers in the attrited clusters for positive (negative) effects. Conversely, lower bounds are obtained by imputing the lowest (highest) possible value of the outcome variable for the compliers in the attrited clusters for positive (negative) effects. All estimates are based on our usual specification that controls for block fixed effects, average attendance and an indicator for whether the respondent has been sampled during the re-sampling round. The latter has been set to zero for the hypothetical respondents in the attrited clusters.

Overall, our results are remarkably robust to the extreme value bounds approach. The estimates in table 24 suggest that the lower bound of the effect of our media intervention on the proportion of households that experienced any violence in the six months preceding our survey is a reduction of ten percentage points among compliers. In other words, our evidence suggests that our media intervention caused a substantial reduction in violence experienced by women compliers even if we presume that all complier households that would have been sampled in the attrited clusters had experienced violence in the six months preceding our household survey. Similarly, even according to the lower bounds in table 24, our media intervention substantially increased the willingness to intervene in hypothetical cases of VAW among women compliers and reduced the perception among women compliers that they would be scolded for gossiping were they to report a husband for beating his wife. The corresponding effects among men compliers were not as strong to begin with, but the lower bounds still point in the expected directions.

Finally, we further assess the robustness of our findings on violence reduction by estimating extreme value bounds for effects on the cluster level. The analyses in table 26 use the village as the unit of analyses after collapsing individual-level responses using cluster-level means. Upper and lower bounds are obtained by imputing the empirically observed minimum and maximum on the cluster-level for the attrited clusters. These analyses include all endline respondents, not only compliers. Again, even the lower bounds suggest that our media campaign caused a substantial reduction in victimization.

	Any Incidents (EL)	Reporting Index (ML)	Reporting Index (EL)	Social Repercussions (EL)
Observed effect	-0.131	0.085	0.126	-0.114
RI p-value observed	0.006	0.006	0.000	0.042
Upper Bound	-0.136	0.101	0.141	-0.127
RI p-value upper bound	0.004	0.002	0.000	0.026
Lower Bound	-0.101	0.066	0.106	-0.093
RI p-value lower bound	0.056	0.028	0.006	0.078

Table 24: Extreme value bounds for estimates among women compliers.

All analyses use individual respondents as the unit of observation. See tables 1, 2 and 4 in the main text for the same estimates. Bounds are obtained by imputing the lowest (0) and highest (1) possible values of all outcome variables for the predicted number of women compliers in the two attrited villages. The resample indicator is set to 0 for all respondents in the attrited villages. See section B.1 of the appendix for details on model specifications and section A for question wording.

	Reporting Index (ML)	Reporting Index (EL)	Social Repercussions (EL)
Observed effect	0.023	0.043	-0.034
RI p-value observed	0.142	0.050	0.199
Upper Bound	0.046	0.067	-0.050
RI p-value upper bound	0.046	0.014	0.108
Lower Bound	0.007	0.028	-0.012
RI p-value lower bound	0.362	0.154	0.383

Table 25: Extreme value bounds for estimates among men compliers.

All analyses use individual respondents as the unit of observation. See tables 3 and 4 in the main text for the same estimates. Bounds are obtained by imputing the lowest (0) and highest (1) possible values of all outcome variables for the predicted number of men compliers in the two attrited villages. The resample indicator is set to 0 for all respondents in the attrited villages. See section B.1 of the appendix for details on model specifications and section A for question wording.

	Any Incidents	Number Of Incidents	Violence Frequency
Observed effect	-0.069	-0.177	-0.127
RI p-value observed	0.009	0.128	0.041
Upper Bound	-0.077	-0.201	-0.146
RI p-value upper bound	0.004	0.074	0.014
Lower Bound	-0.052	-0.075	-0.091
RI p-value lower bound	0.064	0.574	0.168
Maximum	0.600	3.000	1.300
Minimum	0.000	0.000	0.000

Table 26: Extreme value bounds for estimates among all women endline respondents collapsed to the village level.

All outcomes were measured during the endline survey. The analyses are based on responses from all women in the endline survey (not only compliers). Analyses are conducted at the village level, after collapsing individual responses to the cluster-level using cluster-level means. See table 1 in the main text for the same estimates. Bounds are obtained by imputing the lowest (see row *Minimum*) and highest (see row *Maximum*) value observed for a given outcome after taking cluster-level means for the two attrited clusters. See section B.1 of the appendix for details on model specifications and section A of the appendix for details on question wording.

E Supplementary Analyses

This section of the appendix reports extra analyses that support arguments made in the paper.

- Subsection E.1 provides the full tables underlying the coefficient plot (Figure 7) in the paper. As in the plot, none of the analyses exhibit statistically significant effects.
- Subsection E.2 reports the effect of the treatment on peoples' perception of the prevalence of violence in their communities. While we see very little evidence of an effect, this subsection also shows that the correlation between the rate of violence provided in firsthand victimization accounts and respondents' perceptions of prevalence is low, suggesting people are not well-informed about the true rate of violence in their communities (in line with the idea that violent offenders are able to render their crimes difficult to detect).
- Subsection E.3 presents our method for extrapolating estimates of the total number of households in which the campaign is estimated to have prevented violence from occurring over the six-month period preceding our endline survey. We estimate that a total of 302 households did not experience any violence as a result of the campaign, which is equivalent to six households in each of the 48 villages where we screened the anti-VAW videos.
- Subsection E.4 shows how the effect of the campaign on violence against women varies by whether the respondent had been interviewed before or for the first time when interviewed in the endline, and by socio-economic status. Both interactions are insignificant and are signed in a direction that lends no support to the notion that the reduction in violence is a measurement artefact due to priming or fear of anticipated sanctions against a partner, respectively. See section 7 for a discussion. Note we estimate heterogeneity by socio-economic status among panel compliers because we do not have the necessary covariates to compute this index for the compliers who were in our endline but not our midline survey, as these covariates were typically merged in from the midline responses.
- Subsection E.5 illustrates that correlations among the main outcomes are very low. In a world where respondents reported a reduction in violence because they were aiming to please researchers, we should expect these correlations to be high. Thus, the subsection presents additional evidence against the effect being a measurement artefact.

E.1 Tables for Coefficient Plot (Figure 7)

	VAW not Acceptable		Women Suffer Greatly
	Midline	Endline	Endline
	(1)	(2)	(3)
Anti-VAW Media	−0.016	0.005	0.001
	(0.020)	(0.017)	(0.035)
Control Mean	0.87	0.84	0.71
RI p-values	0.758	0.426	0.494
Hypothesis	Upr	Upr	Upr
Block FE	Yes	Yes	Yes
Observations	558	558	558
Adjusted R^2	−0.007	−0.011	−0.013

*p<0.1; **p<0.05; ***p<0.01

Table 27: Table of results for first panel of Figure 7 (Men Compliers in Relationships).

	Intervention Effective		Meddling Seen...	Violence Spirals...	VAW Not Acceptable		Women Suffer...
	Midline	Endline	Endline	Endline	Midline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Anti-VAW Media	−0.014	0.035	−0.003	0.015	−0.010	0.001	0.014
	(0.032)	(0.027)	(0.033)	(0.026)	(0.018)	(0.015)	(0.029)
Control Mean	0.85	0.85	0.41	0.19	0.82	0.86	0.7
RI p-values	0.666	0.122	0.468	0.294	0.686	0.503	0.329
Hypothesis	Upr	Upr	Lwr	Upr	Upr	Upr	Upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	720	720	720	720	720	720	720
Adjusted R^2	0.002	0.023	0.004	0.003	−0.002	0.001	−0.002

*p<0.1; **p<0.05; ***p<0.01

Table 28: Table of results for second panel of Figure 7 (Men Compliers).

	Intervention Effective		Meddling Seen...	Violence Spirals...	VAW Not Acceptable		Women Suffer...
	Midline	Endline	Endline	Endline	Midline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Anti-VAW Media	0.061	0.031	−0.046	0.062	0.007	0.033	0.027
	(0.046)	(0.044)	(0.054)	(0.054)	(0.026)	(0.029)	(0.038)
Control Mean	0.71	0.78	0.47	0.26	0.75	0.75	0.85
RI p-values	0.121	0.26	0.215	0.141	0.404	0.188	0.278
Hypothesis	Upr	Upr	Lwr	Upr	Upr	Upr	Upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	321	321	321	321	321	321	321
Adjusted R^2	−0.014	−0.006	0.017	−0.023	−0.008	−0.011	0.037

*p<0.1; **p<0.05; ***p<0.01

Table 29: Table of results for third panel of Figure 7 (Women Compliers).

E.2 Effects on Community-Level Measures of Violence

In this section we report the effects of the anti-VAW campaign among other measures to that reported in the main results table 1.

Table 30 reports the effects of the anti-VAW campaign on community-level perceptions of the prevalence of VAW. Columns 1-2 record whether respondents believe women in their community are beaten: almost every day (coded 4), around once a week (coded 3), about once a month (coded 2), less than once a month (coded 1), or almost never (coded 0). Columns 3-6 record the number of times that respondents believe women in their community were beaten over the preceding 3, respectively 6 months.[6] There is no statistically significant effect on any outcome, among any of the subsets (see caption for explanation of subsetting).

[6]In the midline survey, respondents were asked about the number of times a women had been beaten "since September." In the endline survey, respondents were asked about the number times a women had been beaten "since Christmas."

	Number of Incidents (Comm.)		Any Incidents (Comm.)		Viol. Freq. (Comm.)		Number of Incidents (VHT)	
	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Anti-VAW Media	−0.059	−0.036	0.009	−0.005	0.043	−0.070	−0.088	0.921
	(0.076)	(0.120)	(0.023)	(0.028)	(0.058)	(0.116)	(0.544)	(2.237)
Control Mean	0.66	1.02	0.31	0.45	1.36	1.58	3.09	9.79
RI p-values	0.444	0.773	0.685	0.858	0.47	0.55	0.872	0.687
Hypothesis	Two	Two	Two	Two	Two	Two	Two	Two
Sample	HH	HH	HH	HH	HH	HH (M)	VHT	VHT
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	110	110	110	110	110	110	112	111
Adjusted R^2	−0.079	0.006	−0.090	0.128	0.086	−0.020	0.055	−0.026

*p<0.1; **p<0.05; ***p<0.01

Table 30: The effect of anti-VAW mass media on perceptions of the prevalence of violence against women.

Analyses in columns 1 to 5 are run on reports from all household respondents (not only compliers). Estimates in columns 6 are based on all male household respondents. Estimates in columns 7 and 8 are based on reports by members of Village Health Teams. All analyses are conducted at the village level, after collapsing individual responses to the cluster level using cluster-level means. See section B.1 of the appendix for details on model specifications and section A of the appendix for details on question wording.

While the results presented in Table 30 would appear to undercut the main results presented in Table 1 of section 5, we believe that the community-level measures simply do not present a reliable measure of the true underlying rate of violence, due in part to the private nature of VAW. Table 31 illustrates the unreliability of these measures, indicating the low correlation between reports at the village-level. While correlations are reasonably high among the same groups (men, women, VHTs) at different times or across different questions, correlations among these groups are typically very low, rarely exceeding .2. Thus, we are more inclined to believe results on individuals' self-reported victimization than on community-wide estimates, which appear to exhibit a high degree of measurement error.

	N EL W	Frq EL W	Frq EL M	Cnt EL M	Frq ML M	Frq ML W	Cnt ML M	Cnt ML W	Cnt ML VHT	Cnt EL VHT
N EL W	1.00	0.00	0.22	0.13	0.20	0.16	0.08	0.16	-0.03	0.00
Frq EL W	0.00	1.00	0.09	0.11	0.16	0.12	0.11	0.22	0.11	-0.02
Frq EL M	0.22	0.09	1.00	0.47	0.42	0.06	0.26	0.06	-0.05	0.18
Cnt EL M	0.13	0.11	0.47	1.00	0.42	0.25	0.40	0.13	-0.09	0.26
Frq ML M	0.20	0.16	0.42	0.42	1.00	0.26	0.51	0.22	0.07	0.14
Frq ML W	0.16	0.12	0.06	0.25	0.26	1.00	0.27	0.40	0.02	0.13
Cnt ML M	0.08	0.11	0.26	0.40	0.51	0.27	1.00	0.10	0.08	0.06
Cnt ML W	0.16	0.22	0.06	0.13	0.22	0.40	0.10	1.00	-0.05	0.09
Cnt ML VHT	-0.03	0.11	-0.05	-0.09	0.07	0.02	0.08	-0.05	1.00	0.21
Cnt EL VHT	0.00	-0.02	0.18	0.26	0.14	0.13	0.06	0.09	0.21	1.00

Table 31: Correlation between perceptions of the prevalence of violence against women, across genders, survey rounds and samples.

All correlations are across clusters, calculated by first collapsing to the cluster level by taking the mean. 'N EL W' is the number of times that women in the endline recall a woman in their household, including themselves, having been beaten over the preceding 6 months; 'Frq EL W' and 'Frq EL M' are the perceived frequency with which women and men respondents in the endline believe women in their community are beaten, respectively, with 'Frq ML W' and 'Frq ML M' measuring the same outcomes at midline; 'Cnt EL M', 'Cnt ML M' and 'Cnt ML W' measure the number of times men in the endline, men in the midline, and women in the midline can recall a woman in their community having been beaten over the preceding 3, respectively 6, months; 'Cnt ML VHT' and 'Cnt EL VHT' measure responses to the same question given by members of the village health team.

E.3 Extrapolation of Main Results on Violence

In order to estimate the number of households within which the anti-VAW messaging prevented violence from occuring, we take the following steps:

1. **Estimate the proportion of complier households in treated clusters** by taking the average number of respondents who report having seen at least one film when asked during the midline survey in clusters that had anti-VAW messaging in them.
2. **Estimate the number of complier and non-complier households in the sample frame of treated clusters** by summing the product of, on the one hand, the number of households listed in each cluster where anti-VAW messaging took place, and, on the other, the corresponding proportion of compliers and non-compliers as calculated in 1.
3. **Estimate the effect of the treatment on the probability of VAW in the household over the past six months** among both complier and non-complier women in the endline, by subsetting to the respective groups.
4. **Estimate the number of households that did not experience violence** among complier and non-complier households by multiplying the corresponding quantities calculated in 2 and 3. The total number of households in which violence was prevented is estimated by summing these two numbers.

Using these methods we estimate that VAW was prevented in a total of 302 households. Our VAW treatment was implemented in 48 villages: expressed in per-village terms, we thus estimate we prevented VAW in 6 households per village in which our campaign took place.

We characterize statistical uncertainty around this estimate through bootstrapping. While the number of households in the sample frame is a known quantity, the proportion of complier households (estimated in step 1) and the effect of the treatment among complier and non-complier subgroups (step 3) are both subject to uncertainty generated by the sampling procedure. We conduct the following steps 1,000 times:

1. **Bootstrap the midline data** by resampling respondents within their clusters with replacement.
2. Repeat steps 1-2 as above.
3. **Bootstrap the endline data** by resampling respondents within their clusters with replacement.
4. Repeat steps 3-4 as above, and store results.

This procedure gives a bootstrap distribution for three statistics of interest: the number of complier households that did not experience VAW over the preceding six months due to the treatment,

the number of non-complier households that did not experience VAW over the preceding six months due to the treatment, and the total number of households within which VAW was prevented over the six months preceding the endline. Table 32 presents the 95% confidence intervals generated by taking the 2.5th and 97.5th percentiles from the bootstrap distributions of the statistics.

	Compliers	Non-Compliers	Weighted Combination
2.5%	-271	-333	-517
97.5%	-73	83	-59

Table 32: Percentiles from distribution of bootstrapped extrapolations of treatment effect on household violence probability among treated villages.

E.4 Heterogeneity of Main Effect on Violence

	Any Incidents
Anti-VAW Media	-0.130^{***}
	(0.052)
Socioeconomic Status	-0.035
	(0.034)
Socioeconomic Status x Anti-VAW Media	-0.008
	(0.064)
Control Mean	0.3
RI p-values IPV	0.009
RI p-values Interaction	0.909
Hypothesis	Two
Sample	Compliers
Block FE	Yes
Observations	321
Adjusted R^2	0.016

*p<0.1; **p<0.05; ***p<0.01

Table 33: Heterogeneous effects of anti-VAW mass media on incidents of violence against women (endline).

The outcome is measured during the endline survey. The analysis is based on responses from women compliers in the endline survey and uses individual respondents as the unit of observation. See section B.1 of the appendix for details on model specifications and section A of the appendix for details on question wording. *Socioeconomic Status* is a latent measure of a respondent's socioeconomic status which summarizes the following five covariates and has been obtained through factor analysis: *Illiterate* is an indicator for whether the respondent says that she cannot read or write (endline). *Living Conditions* reflects the enumerator's assessment (endline) of how the respondent's living conditions compare to those of others in the village (0 = Much Worse, 1 = Worse, 2 = Same, 3 = better, 4 = much better). *Highest Grade* measures the highest education level attained by the respondent (endline) ranging from 0 (No education) to 16 (university). *N children HH* measures the number of children living in the household (midline). *Asset Index* is an additive index of five items that ask whether the respondent's household owns a TV, a radio, a chair, a sofa or a motorcycle and an indicator for whether the walls of the respondent's house are made of a material other than mud (midline). The index ranges from 0 to 1.

	Any Incidents
Anti-VAW Media	−0.163
	(0.139)
Interviewed in Midline	−0.055
	(0.109)
Anti-VAW Media x Interviewed Midline	0.034
	(0.142)
Control Mean	0.31
RI p-values	0.283
Hypothesis	two
Block FE	Yes
Observations	356
Adjusted R^2	0.021

*p<0.1; **p<0.05; ***p<0.01

Table 34: Heterogeneity of main violence finding by whether respondent was interviewed in midline. Results estimated among all compliers in endline.

E.5 Correlations Among Key Outcomes

	VAW Not Acceptable	Reporting Index	Any Incidents
VAW Not Acceptable	1.00	0.06	-0.05
Reporting Index	0.06	1.00	0.03
Any Incidents	-0.05	0.03	1.00

Table 35: Correlations among women between attitudes toward the moral status of VAW, attitudes toward reporting of VAW, and number of reported VAW incidents in the household. N = 1,036.

F Sampling Strategy

This section presents our sampling strategy.

- Subsection F.1 describes the method via which we selected villages into the study population.
- Subsection F.2 describes how, conditional on having selected villages, we selected households and individuals within households for inclusion in our study. It explains response rates, attrition and our approach to follow-up sampling.

F.1 Sampling Strategy for Clusters

To select the 112 villages included in our study, we first conducted a census of villages with video halls in the districts of Mubende, Mityana, Masaka and Lwengo, which led to the identification of 342 video halls in approximately 300 candidate villages. As illustrated in Figure 2, we identified and excluded potentially problematic sites according to a number of pragmatic criteria,[7] narrowing down the selection to 247 candidate villages.

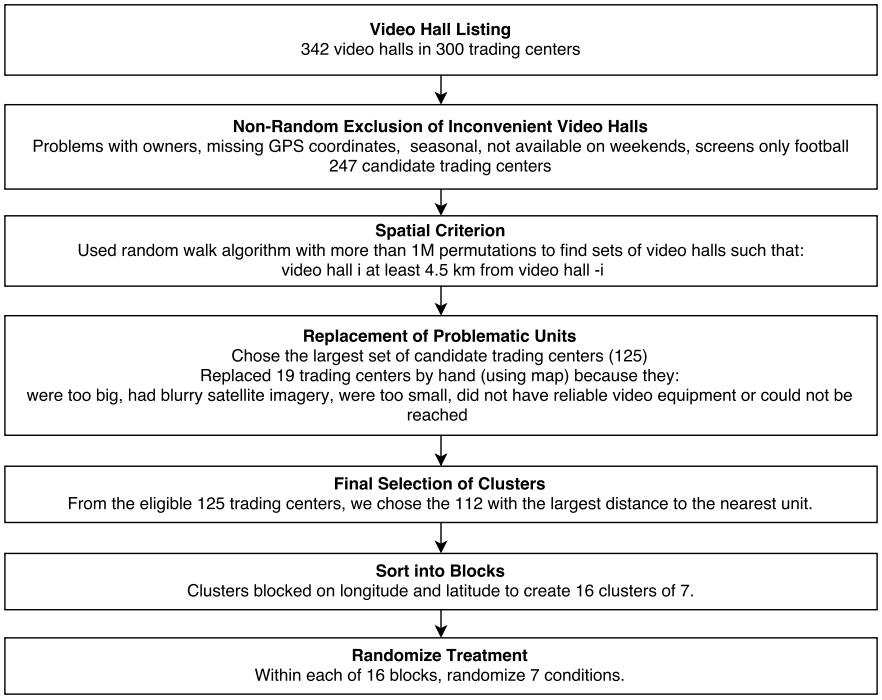

Figure 2: Method for Sampling Clusters from Mubende, Mityana, Masaka and Lwengo.

[7] We excluded video halls that operated seasonally, could not be rented on weekends, for which the GPS coordinates were missing, that only screened football matches instead of movies, and where enumerators indicated it may be difficult to successfully screen movies.

We then used a random walk algorithm to choose a set of 125 villages from the 247 candidate villages such that each villages is at least 4.5 kilometers from its closest neighbor. We imposed this distance constraint in order to address concerns about spillovers. Due to various practical concerns (see Figure 2), 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 then chose the 112 villages with the largest distance to the nearest unit. The respondents in the clusters selected in this manner comprise our sample.

F.2 Sampling Strategy for Individuals

Within the villages in our study, we conducted household surveys among adults and teenagers as well as surveys with members of Village Health Teams (VHTs). In this project, we draw on surveys conducted among adult household members and VHTs.

Household respondents were sampled from a circular area around the video hall that was used to screen the treatment messages. During the first survey wave (midline survey), 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. For the midline survey, 50 households were randomly selected from this list. Twenty-five of those were randomly chosen as households within which a women would be interviewed by a female interviewer; in the remaining households men were interviewed by male enumerators.

Upon meeting each household, enumerators listed all adult household members (aged 18-50) of the relevant gender and randomly selected one of them as the adult respondent. If no adult 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. 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. To ensure our sampling and surveying methods were appropriate to the local context and respectful of subjects' rights, we pre-tested the survey and the questionnaire in non-experimental villages. Interviewers were extensively trained and supervised

to make sure that respondents were interviewed alone and out of earshot of others and that their responses were kept confidential.

As will be explained in more detail below, villages have been grouped into blocks of seven units. During the midline survey, 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 compliers 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.[8]

In total, we planned to conduct 5530 interviews with adult respondents (40 respondents in the first 7 and 50 respondents in the other 105 villages) during the midline survey. We successfully conducted 5344 of these in 110 of our 112 villages. Unfortunately, we were not able to conduct any household surveys 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.

Preliminary analyses that we conducted after having completed the midline survey in the remaining 110 villages 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 in these 110 villages to target such compliers, 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 compliers.[9] We conducted this sampling by continuing the

[8]Note also that we will not be re-weighting the villages to account for the fact that some have more expected compliers than others due to sampling, since our estimand is the ATE among the compliers that we find. Effects on the rate of VAW incidents will be estimated both on the individual and the cluster level. This is in accordance with an addendum to our Pre-Analysis plan.

[9]In one such case, the two clusters with the second lowest number of compliers had the same number of compliers.

same random sequence of households generated in the first round of sampling, so that the sampled units are the same units that would have been sampled had we continued the main midline data collection. In order to over-sample compliers, 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. Figure 3 contains information about our midline sample. Our response rate among household respondents in the midline survey (main data collection and re-sampling combined) is 96.4%.

For the endline survey, we returned to the same villages and re-interviewed the compliers that had been sampled during the midline survey. We managed to re-interview 1035 of the 1156 compliers that were interviewed during the midline survey, giving a follow-up rate of 90%. In addition to re-interviewing compliers, we also conducted surveys with a new sample of 915 adult household respondents during the endline survey. 684 of those were parents of teenagers whom we interviewed as part of a second teenager survey. Additionally, we interviewed 231 additional respondents as part of our efforts to gather data on more women compliers. These additional respondents were sampled by, again, continuing the same random sequence of households generated in the first round of sampling in the midline.

Finally, we also conducted interviews with members of VHTs in the 112 villages in our sample during both the midline and endline. Members of VHTs are local volunteers whose task it is to provide advice on medical questions to the residents of the village. Each volunteer is responsible for a set of households that he or she is supposed to visit regularly. VHTs do receive compensation and do not typically provide medication. During the midline, we managed to interview at least one VHT member in each of the villages in our sample. In total, we conducted 320 midline interviews with VHT members, an average of around 3 per village. As part of the endline survey, we interviewed 338 members of VHTs in 111 clusters. The VHT members interviewed during the endline are not necessarily the same as those interviewed during the midline, since we are focus on current members in each of the survey waves.

We randomly selected one among the two.

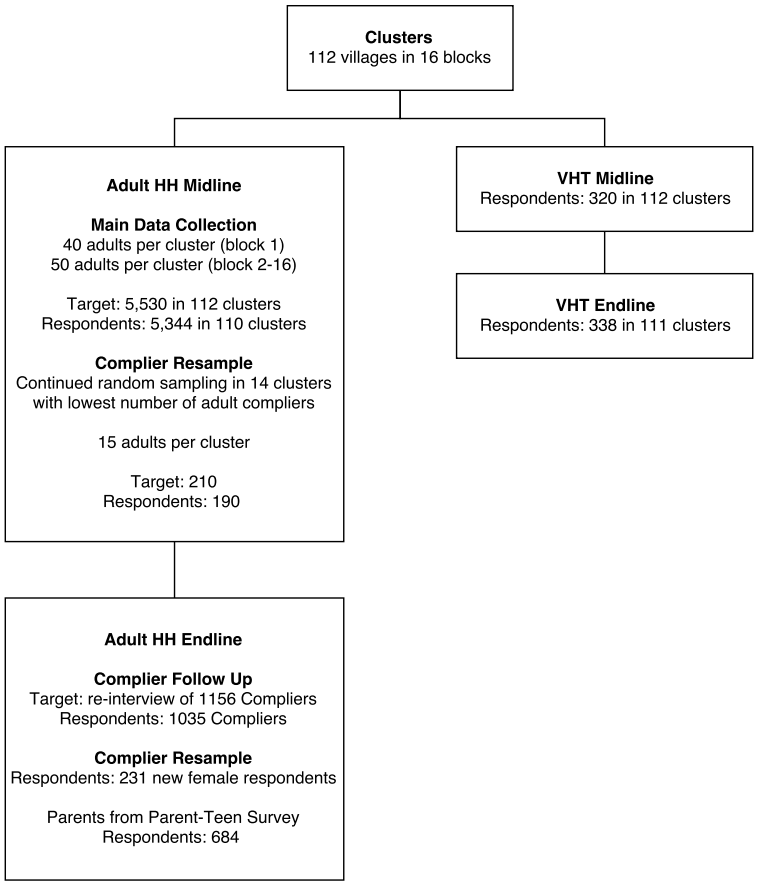

Figure 3: Sample from Individual Surveys

VHT refers to the Village Health Team. Clusters refer to villages.