

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’ embeddedness 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. \(N.d.\)](#).

²See 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

³The 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

⁴These 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

⁵We 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 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

⁶See 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.]

⁷Note 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

⁸We 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

⁹In 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?”

¹¹See 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

¹²During 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.

¹³Following 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

¹⁴While 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

¹⁵While 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

¹⁶For 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 education-entertainment campaigns can affect policy-relevant behaviors. We show that those who witness VAW tend to 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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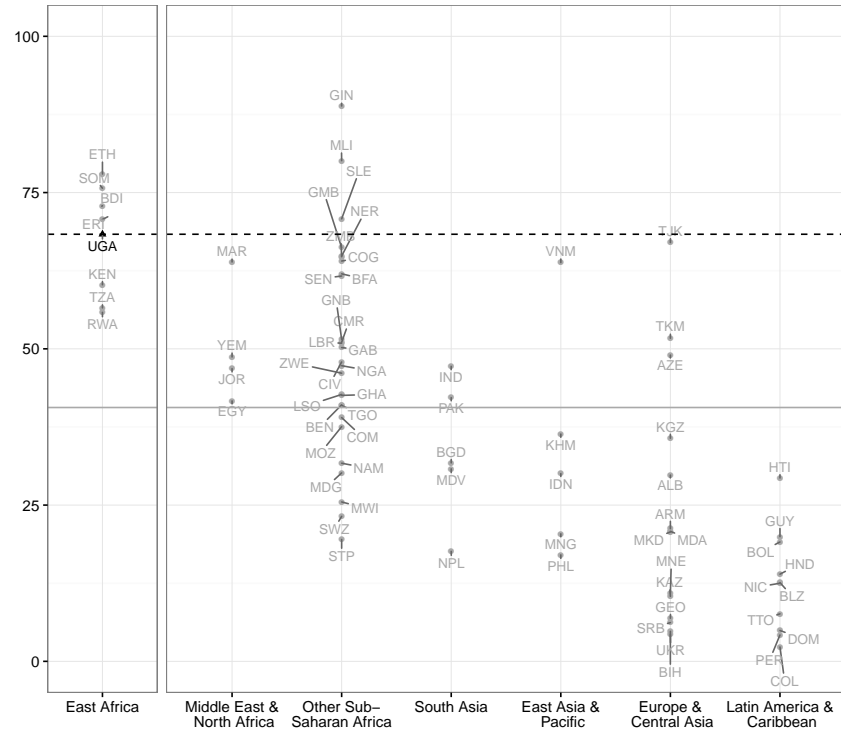


Figure 1: Average percentage of women who state that it is acceptable for a man to hit his wife in at least one of five scenarios.

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.

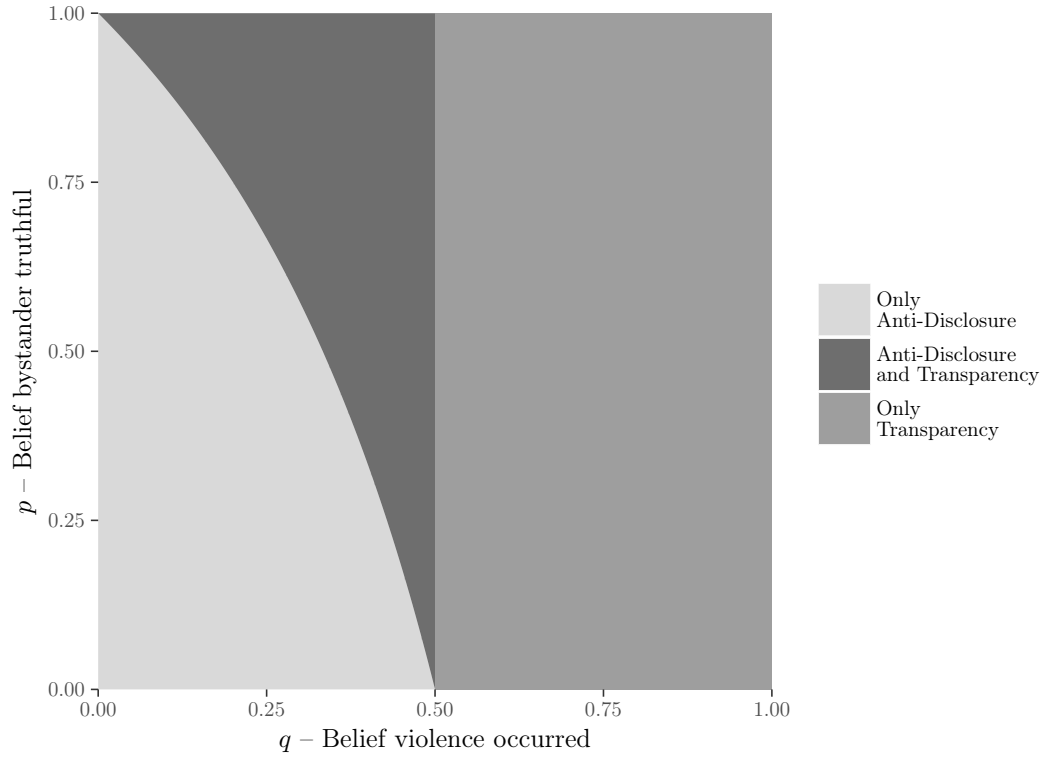


Figure 2: Existence of equilibria as a function of prior beliefs.
Plot assumes $\frac{s}{a} = 1$. See section B.4 of the Online Appendix for the full model.

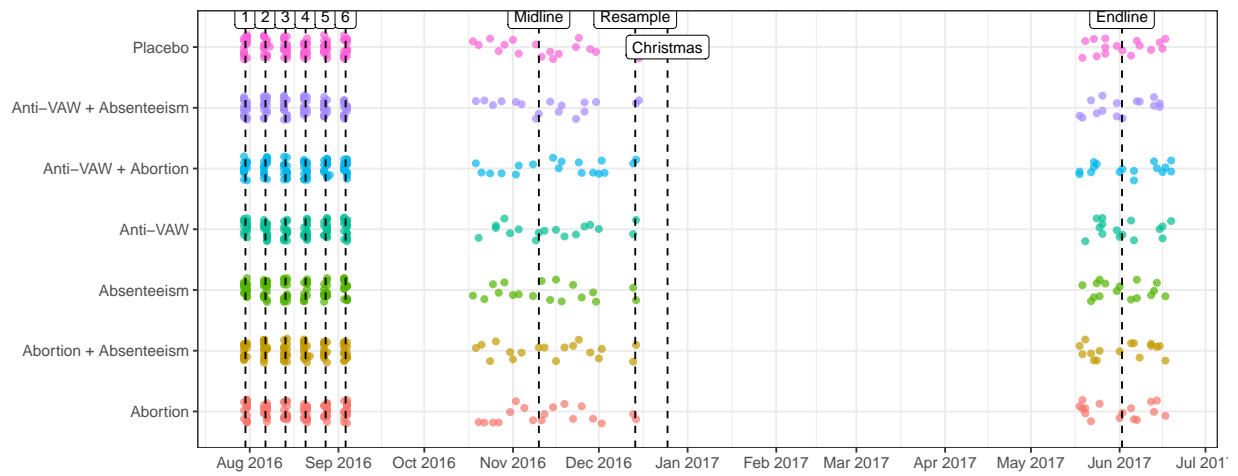


Figure 4: Timeline of media campaign, midline and endline surveys.

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.

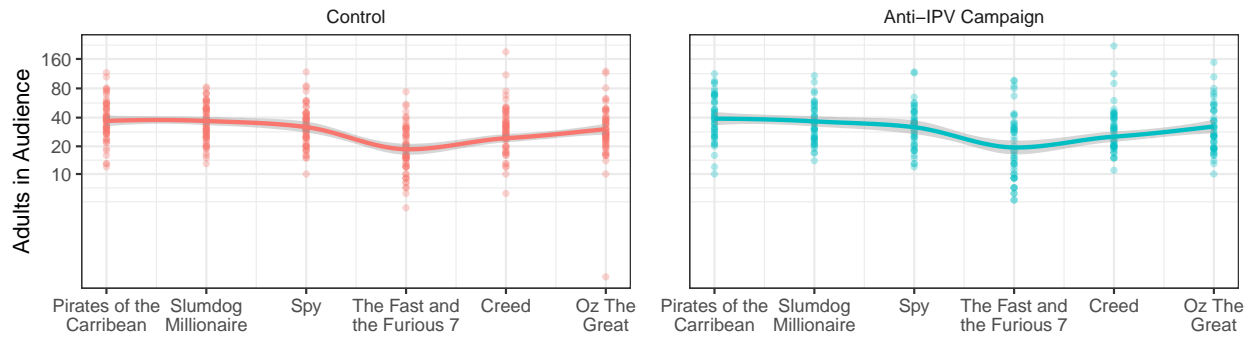


Figure 5: Adult attendance of screenings by treatment status.

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*).

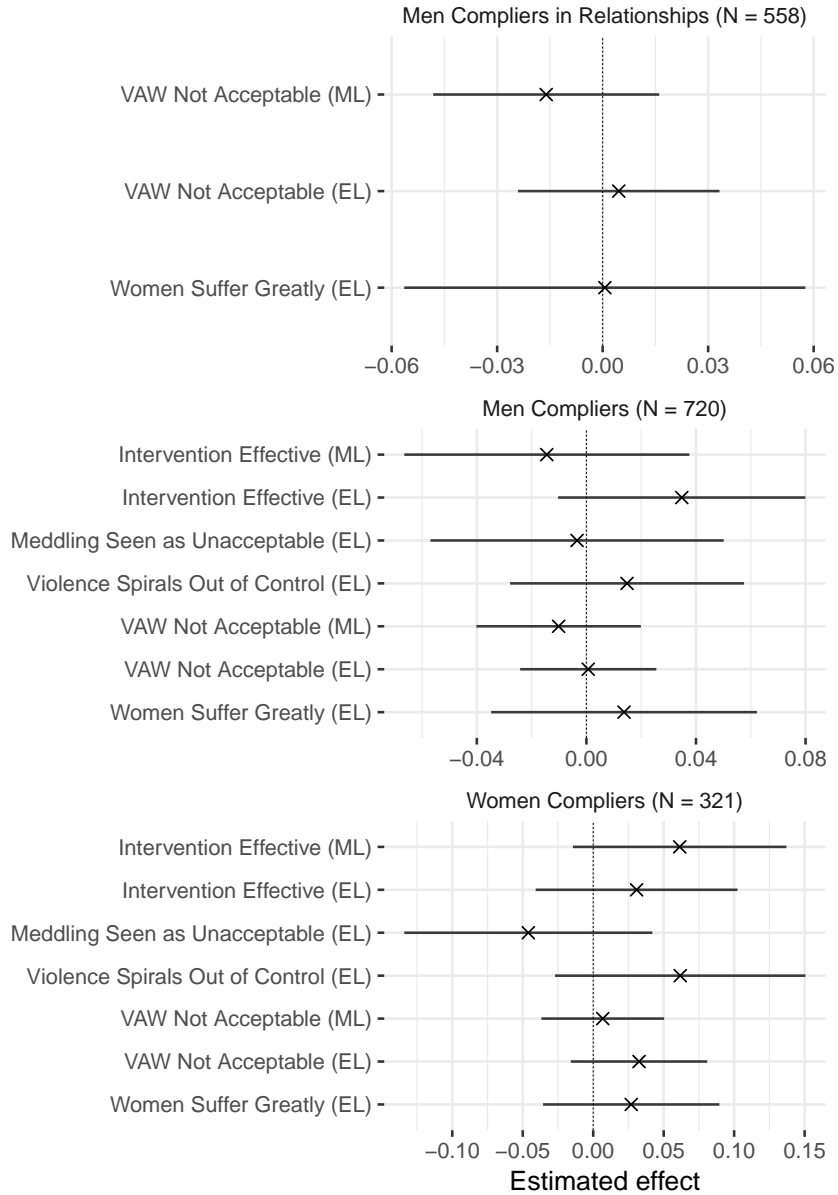


Figure 7: Alternative pathways through which the treatment could have decreased VAW or increased the willingness to report.

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 (0.113)	-0.146 (0.091)	-0.346 (0.226)	-0.069*** (0.026)	-0.048** (0.022)	-0.132*** (0.049)	-0.296* (0.169)	-0.676*** (0.287)
Control Mean	0.56	0.59	0.59	0.19	0.2	0.2	0.42	0.42
RI <i>p</i> -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.

<i>Would report VAW to:</i>	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* (0.057)	0.144*** (0.053)	0.076* (0.056)	0.171*** (0.058)	0.058 (0.049)	0.061 (0.057)	0.111*** (0.040)	0.130*** (0.048)	0.085*** (0.030)	0.126*** (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 <i>p</i> -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.

<i>Would report VAW to:</i>	Parents			Counselor			LC1 Chairperson			Police			Reporting Index		
	Midline	Endline	(1)	Midline	Endline	(2)	Midline	Endline	(3)	Midline	Endline	(4)	Midline	Endline	(5)
	(0.035)	(0.036)	0.037	(0.037)	(0.034)	0.055*	(0.034)	(0.036)	0.062*	(0.025)	0.019	(0.035)	(0.019)	0.043*	(0.025)
Anti-VAW Media	0.080**	0.037	0.037	0.037	0.055*	0.055*	0.021	0.062*	0.021	0.046	0.019	0.019	0.023	0.043*	0.023
Control Mean	0.5	0.49	0.61	0.61	0.61	0.61	0.35	0.29	0.23	0.2	0.42	0.2	0.42	0.4	0.4
RI <i>p</i> -values	0.021	0.176	0.183	0.183	0.068	0.068	0.281	0.056	0.946	0.318	0.142	0.318	0.142	0.05	0.05
Hypothesis	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr	Upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	720	720	720	720	720	720	720	720	720	720	720	720	720	720	720
Adjusted R ²	0.005	0.004	-0.014	-0.014	0.008	0.008	0.002	0.003	0.001	0.007	0.001	0.007	0.001	0.019	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 (0.034)	-0.114** (0.051)	-0.021 (0.041)	-0.029 (0.060)	0.052* (0.029)	0.043* (0.029)	0.081* (0.057)	0.068 (0.047)
Control Mean	0.49	0.63	0.52	0.58	0.75	0.79	0.72	0.73
RI <i>p</i> -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** (0.023)	0.043** (0.023)	0.061** (0.033)
Anti-VAW Media		0.040* (0.019)	0.061** (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.