Encouraging community action against teacher absenteeism: a mass media experiment in rural Uganda

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ABSTRACT

Chronic teacher absenteeism is widespread in Uganda, with approximately 30% of public school teachers absent on any given day. Absenteeism and other problems that arise in Uganda's public education system are often attributed to a lack of public oversight and parental involvement. In an effort to develop a scalable method of encouraging community engagement on this issue, the present study assesses the extent to which entertainment-education videos increase willingness among Ugandans to take action against absenteeism. Working in collaboration with Ugandan screenwriters and local actors, we developed video dramatisations that depicted the problem of absenteeism and how parents mobilised to address it. We assess the

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persuasive effects of these dramatisations both under lab-like conditions, to gauge immediate effects, and in the field, to gauge effects two months and eight months after a placebo-controlled media campaign attended by over 10,000 Ugandans in 112 villages. Although the persuasive effects are weaker in the field than the lab setting, the former remain substantial even after eight months. The demonstrated ability of entertainment-education to change public views on this issue sets the stage for policy experiments that test whether entertainment-education campaigns have downstream effects on absenteeism and public school performance more generally.

1. Introduction

Chronic teacher absenteeism is common throughout the developing world, and particularly in Uganda. Random audits of a representative sample of public primary schools in six low and middle income countries in 2003 found an average national absenteeism rate of 19%, with Uganda having the highest rate among countries audited. In Uganda, 27% of teachers were absent on any given day (Chaudhury et al., 2006). If anything, absenteeism has worsened over time a study that revisited Uganda ten years later in 2013 found a 30% absenteeism rate (Bold et al., 2017). These rates are even higher in rural parts of Uganda; another study by the World Bank in 2013 observed absenteeism rates of up to 35% in the Northern region of the country (Wane and Martin, 2013).

Absenteeism in developing countries is thought to stem from a variety of factors, such as labor contracts that give teachers little incentive to show up (Bennell, 2004; Cilliers et al., 2018; Lavy, 2007; Norton, 1998), lack of supervision by school officials (Kremer et al., 2005; Muralidharan et al., 2017; Patrinos, 2013), or corruption in school bureaucracies that leaves teachers unpaid or irregularly paid (Ugoani, 2016). A variety of policy solutions have been proposed and tested. Field experiments have demonstrated the value of introducing robust methods for verifying and incentivising attendance (Duflo et al., 2012). Absenteeism has also improved when schools have hired additional contract teachers and when local school committees have received administrative training (Duflo et al., 2015). Qualitative research in Africa has stressed the importance of increasing professionalisation in the recruitment and training of teachers (Ejere, 2010).[1]

Although bureaucratic oversight, incentives, and professionalisation may be ef-

fective ways of dealing with absenteeism, they are top-down solutions that require resources and administrative capacity. In remote regions of East Africa where resources and capacity are limited, large scale interventions along these lines may be difficult to orchestrate and sustain. An alternative policy approach is to encourage more active parent and community involvement in monitoring absenteeism. Unlike many school-related problems, such as drug use or bullying, teacher absenteeism is relatively easy to detect; attentive parents notice when teachers at their children's schools are chronically absent. The policy challenge is translating community concern into meaningful collective action. In much of the developing world, parents are apprehensive about lodging formal complaints or bringing issues to the attention of local councils or officials (Banerjee et al., 2010; Lieberman et al., 2014). Rather than express their grievances publicly, parents may find ways to sidestep the problem. For example, in semi-structured conversations with parents in rural Uganda, we learned that a common response to absenteeism was for parents to transfer their children to another school, perhaps a religious school in the same village or a school in a different village where a close relative resides. Absenteeism thus presents the classic dilemma between what Albert Hirschman characterised as 'voice' versus 'exit' (Hirschman, 1970; Ogawa and Dutton, 1997; Paul, 1992).[2]

The present paper focuses on an intermediate link in the causal chain between effective local organising and reduced teacher absenteeism: encouraging the community to choose the voice option to remedy absenteeism. Before evaluating whether parental involvement has meaningful policy effects on absenteeism, it make sense to first investigate whether there are scalable methods for increasing parents' willingness to take action, especially in a context where parents often have exit options. Inspired by the growing literature on entertainment-education (Bandura, 2003), which has produced promising effects in randomised trials in the domain of health (Banerjee et al., 2015), ethnic conflict (Paluck and Green, 2009), and government corruption (Blair et al., 2019), we called public attention to the issue of teacher absenteeism through a series of short videos deployed in rural areas of Uganda. The videos, which were written by local screenwriters and filmed on location, sought to convince audiences that problems of absenteeism can be solved when parents join together and bring their grievances to the

attention of local leaders. These videos were tested in rural Uganda under lab-like conditions but also under more naturalistic field conditions. Our placebo-controlled field experiment inserted brief absenteeism videos into the commercial breaks of feature-length movies, which were aired as part of a free six-week film festival. The field test 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. Results for the placebo videos on the topic of abortion stigma are reported in Wilke et al. (2020); results for the placebo videos on the topic of violence against women are reported in Green et al. (2020) and Wilke et al. (2020). Both studies find substantial effects on audiences' willingness to take action in support of victims but weak effects on general attitudes, such as whether husbands have a legitimate right to beat their wives. Wilke et al. (2020) examines whether any of the videos, including those on teacher absenteeism, have spillover effects on others in the village who did not attend the films; none of the apparent effects on audiences seem to have been transmitted to others in the village. Our focus here is on the effects of first-hand exposure to videos on the topic of teacher absenteeism.

The main results of the lab and field experiments may be summarised as follows. Respondents who watched the three absenteeism videos during the course of a survey administered on a tablet computer showed much greater willingness to contact local officials, speak up at a community meeting, bring the issue to the attention of the Parent Teacher Association (PTA), and join forces with other parents to remedy the problem of absenteeism by comparison to their control group counterparts who watched videos on unrelated topics. Of course, the obtrusive manner in which subjects were shown the videos and then asked questions about their willingness to take action may overstate the persuasive effects of the intervention. The large-scale field experiment, which employed an unobtrusive design, indeed found smaller but nevertheless substantial effects. Two months after exposure to the absenteeism videos, viewers in treated villages were 0.042 scale points higher on an index of willingness to take action than viewers in placebo villages; since the inter-village standard deviation on this scale among placebo villages is 0.106, this estimated effect translates into a change of

0.4 village-level standard deviations. Eight months after exposure, the apparent separation between treatment and control villages in terms of willingness to take action remained unabated. That said, the treatment videos' persuasive effects did not extend much beyond the confines of the script. Exposure to the videos seems to change viewers' opinions about the specific topic of absenteeism but not on the more general topic of school funding as a policy priority. It appears that entertainment-education is a promising, scalable approach to increasing community involvement to address absenteeism specifically.

Although our research design does not allow us to assess the effects of our intervention on actual rates of absenteeism or student learning, there are reasons to think that parental involvement could set in motion positive outcomes. A large existing literature considers the question whether community monitoring can be an effective solution to poor service provision and finds mixed results (see e.g. Björkman and Svensson, 2009; Olken, 2007; Raffler et al., 2020). In our case, the theory of change linking parental involvement and policy outcomes seems to involve two complementary processes. First, by changing the vigilance of parents, the videos potentially raise the probability of detection of an absenteeism problem and increase the pressure on administrators to remedy it. Second, attitude change among parents and the community more generally increases the probability that village leaders perceive the issue of absenteeism as something that demands their oversight, which in turn would increase pressure on administrators. Future work should assess whether messaging campaigns like ours do indeed increase parental involvement, reduce absenteeism, and ultimately improve educational outcomes.

2. Developing the Experimental Media Content

Our teacher absenteeism media campaign consists of three short video vignettes. Each vignette is between three and a half and four and a half minutes long.[3] The videos dramatise the prescriptive social norm that parents have a responsibility to take action in order to resolve the problem of teacher absenteeism. The videos show that acting in accordance with the norm is effective in bringing absent teachers back to the classroom.

The first vignette begins with a father finding out from his son that the teacher has been absent from class for weeks, as depicted in the first panel of Figure B1 in the Supplemental Materials. The father vows to take it up with the PTA and asks his son to tell him if this happens again, despite the principal's threats to expel students who tell their parents. In the second vignette, two parents discover an absent teacher selling soap at the market during the day rather than teaching their children at school. The parents admonish him for not telling the PTA and parents about the situation; he protests that he has to make ends meet somehow as he has not been paid for three months. In the third vignette, we see the results of the parents' efforts. As depicted in the third panel of Figure B1, a government official visits the problem school and confronts the principal with the report from the PTA documenting the extent of teachers' absences from class. The boy from the first vignette rushes home to tell his father the good news that teachers have returned to their classrooms. Throughout the story, parents emphasise their responsibility to ensure that their children receive the education that they deserve. Importantly, the protagonists band together collectively, a message echoed by a voiceover at the end of each vignette to the effect of 'we all have a part to play in our children's education.' The videos portray the involved parent as a hero, not as a troublemaker.

The content of the videos was inspired by the theory of social learning (Bandura, 1962), which holds that 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' behaviour. Bandura (2003) points out that such learning may occur when people observe behaviour modeled in fictional dramatisations. Vicarious learning seems particularly likely to result from our dramatisations given their closeness to the audiences context and experience. Media with high production value is seldom filmed in the local language (Luganda) using rural Ugandan villages as a setting. The videos depict situations that would be familiar to the participants in our study. The relevance of the films was apparent in reactions to the lab study described below wherein rural Ugandans were directly exposed to these videos on hand-held tablets. The vast majority (87%) of viewers said that the stories could have happened in their village. See appendix A in the Supplemental Material for a description of the steps we took

to ensure that the videos where appropriate for the study context.

3. An Experiment Under Controlled Viewing Conditions

In order to assess the immediate persuasive effects of the videos, we conducted a lab-in-the-field survey experiment in the Central District of Uganda. The rural locations were similar to the villages featured in the field experiment described below. A total of 689 respondents were interviewed from a previously drawn random sample of households located within a specified radius from the village video hall.[4] After a preliminary series of questions, we showed participants a randomly-assigned selection of the video messages in a one-on-one setting on portable media devices. Participants either saw the three teacher absenteeism videos or three videos on the subject of domestic violence.[5] Outcomes were measured by survey questions asked shortly after presentation of the videos.

Our primary outcome measures focus on the tension between choosing 'exit' options and 'voice' options in response to the issue of absenteeism. Questions asked respondents which of two actions they would prefer to take upon finding out that their child's teacher has been absent for two days this week. Each respondent was asked to make four decisions. For each choice, one option involved active intervention, while the other option implied passivity or exit. Respondents were randomly assigned to be faced with different pairs of options. The randomisation of items was restricted such that respondents were never asked twice about the same action, and the order of the active options remained constant from one respondent to the next. The active actions (coded 1) are as follows:

- *Involve LC1 Chair*: 'Tell the LC1 chairperson to investigate why the headmaster has allowed this problem to occur.'
- *Tell village*: 'Bring it up in the village meeting.'
- *Use PTA*: 'Immediately begin organising a PTA meeting, even if you know this might start some trouble.'
- *Assemble group*: 'Assemble a group of parents and confront the teacher.'

The four passive options (coded 0) are as follows:

- *Find a Tutor*: 'Find a tutor to instruct your child until the teacher comes back.'
- *Ask Headmaster*: 'Ask the headmaster to put your child into a different classroom until the teacher returns.'
- *Wait*: 'Wait another few days to see if the problem corrects itself.'
- *Neighbouring Village*: 'Send your child to a school in the neighbouring village, where the teachers always come to class.'

The proportion of respondents who selected each active option is shown in Table 1, separately for each passive option offered. The table shows that some active options (table columns) were clearly more attractive than others; the option to involve the LC1 chair (a local government leader) was the most attractive, while the option to assemble a group of parents to confront the teacher was least attractive. Among the passive options (table rows), the option to "Wait another few days to see if the problem corrects itself" was by far the most attractive. Corresponding results from our field experiment may be found in the Supplemental Materials, Tables D5 to D8.

[Table 1 about here]

Administrative errors led to a small amount of non-compliance with treatment assignment in our lab-like study. Out of 628 respondents assigned to video vignettes on domestic violence, 12 were wrongly shown the videos on teacher absenteeism. Conversely, 12 out of 61 respondents assigned to the absenteeism condition were erroneously shown a video on domestic violence. Section C.1 of the Supplemental Materials provides more details on non-compliance and argues that administrative errors are unlikely to be related to potential outcomes. Table 2 presents the results of an instrumental variable regression in which treatment assignment is used as an instrument for whether a respondent did indeed see the absenteeism video. Table C2 in the Supplemental Materials shows that results are very similar if outcomes are regressed directly on an indicator for whether a respondent saw the absenteeism videos.

As can be seen in Table 2, exposure to the videos dramatising the issue of absenteeism has immediate effects on viewers' willingness to take action. Across the four

action items, those in the assigned treatment group were between 10 and 13.4 percentage points more willing than the control group to endorse the voice option rather than the exit option. The composite index shows an average effect of 12.3 percentage points with a standard error of just 4.4 percentage points ($p < 0.01$). To put this estimate in perspective, bear in mind that the standard deviation of this index across placebo villages in the field study we report below is 10.6 percentage points. Exposure to the videos in a lab-like setting is tantamount to replacing a village at the median in terms of willingness to take action with a village at the 85th percentile.[6]

[Table 2 about here.]

4. A Placebo-Controlled Field Experiment

Relatively few rural Ugandan households own televisions; to reach large numbers of people, a video campaign must be deployed in public settings. Our intervention therefore presented the experimental videos in local video halls (*bibanda*), which ordinarily screen action movies or broadcast soccer matches for the admission price of roughly US $0.10. Most trading centers, the commerce hub for a village, feature a *bibanda*. We rented these video halls for six consecutive weekends, from July 30 to September 4, 2016, during which we hosted a free film festival (see Figure B2 for a timeline and listing of films). Popular English-language films were aired, overdubbed in Luganda by a well-known VJ who translated the dialogue and narrated the films. Film festivals were held in 112 communities across a wide swath of rural Uganda to the west of Kampala (see Figure B3 for a map). Villages were chosen to be at least 4km apart so as to limit attendance from other villages. Given a sparse network of roads and limited transportation options, this distance is likely sufficient to prevent inadvertent treatment of residents from other villages. Prior to random assignment, these sites were organised into 16 blocks of seven in order to minimise within-block variance in latitude and longitude (see Figure B3).

As described in Green et al. (2020) and Wilke et al. (2020), the experimental manipulation was the video messages that aired during the films' commercial breaks. Of the 112 villages, 48 were randomly treated (i.e., shown the absenteeism videos during

the breaks). The remaining villages were either shown films without interruption or with videos that only covered the topics of domestic violence or abortion stigma.[7] The analyses in this paper compare respondents from clusters (villages) that were exposed to the messages on teacher absenteeism to respondents in clusters that were exposed to all other conditions (collectively referred to as the control group).

Almost all sites complied with the treatment assignment insofar as we were able to correctly screen the assigned films and messages.[8] To measure individual-level attendance at the film festivals, we use village population surveys conducted months later, as described in the next section.[9]

In the analysis that follows, we focus on *compliers*, respondents who indicated at the end of the survey conducted two months later that they attended at least one of the screenings. By comparing compliers in the treatment group to compliers in the control group, we obtain unbiased estimates of the complier average causal effect. As Figure B4 shows, attendance rates were similar across experimental conditions, and elsewhere we provide extensive 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 (Green et al., 2020). The comparability of compliers across experimental arms reflects the fact that audiences were attracted by the feature films, not the messages aired during the commercial breaks.

Compared to those who did not attend any of the screenings ('never-takers'), compliers are more likely to be men, are younger, and are, unsurprisingly, less likely to own a television (see Table 3). 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%). Among compliers, 72% of men and 95% of women have at least one child, which makes absenteeism personally relevant to them. Never-takers and compliers are broadly similar in terms of their standard of living: they live in dwellings with a similar number of rooms, own radios at similar rates, have similar years of schooling and exhibit similar degrees of literacy.

[Table 3 about here.]

4.1. Post-Intervention Surveys

Measurement of outcomes took place in two waves, as depicted in Figure B2. 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 several weeks after the film festival, was billed as an unrelated public opinion poll, in order to avoid Hawthorne effects. Details on the sampling strategy can be found in Green et al. (2020).

We successfully interviewed 5,534 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. We believe that our inability to work in these locations was unrelated to the treatment status of the villages. Our analysis therefore excludes villages in which we could not survey. For 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 1,041, giving a follow-up rate of 90%. In order to assess the persistence of effects over time, the results reported below hold the respondent pool constant; we focus solely on compliers who were interviewed in both the midline and the endline survey. Similar results are obtained when we expand the set of compliers to include those who answered only one of the follow-up surveys (see Table D9 in the Supplemental Materials).

4.2. Statistical Model and Results

We use least-squares regression in order to estimate the complier average treatment effect of the absenteeism messages on viewers' responses to surveys administered two and eight months later. We do so by fitting the following linear model among the

subset of our data that contains only compliers:

$$Y_{ij} = \alpha + \tau z_j + \mathbf{X}_j^\top \boldsymbol{\gamma} + \delta r_{ij} + \epsilon_{ij}, \tag{1}$$

where Y_{ij} is the survey outcome of interest for individual i in cluster (village) j, α is an intercept and z_j is a treatment assignment indicator which takes the value 1 if a respondent resides in a village which was assigned to the absenteeism message treatment. $\mathbf{X}_j^\top$ is a matrix containing block indicators and the average audience size[10] across all screenings that took place in a given village. r_{ij} is an indicator for whether respondent i was selected as part of a second round of midline sampling. ϵ_{ij} is an individual-level error term. All coefficients in this model are estimated separately for each of the two survey waves.

Given the cluster-randomised design, standard errors are clustered at the level of the village. p-values are calculated using the pre-registered randomisation inference test in which the treatment is permuted 3000 times to simulate effects under the sharp null hypothesis of no treatment effect for any subject. The row labeled 'Hypothesis' in each table indicates the pre-registered direction of the hypothesis test (two-tailed, lower, upper) for each column. Outcome values that are missing due to failure to respond to specific questions are imputed using the pre-registered multiple imputation through chained equations (MICE) approach.

4.3. Increased willingness to act on absenteeism

Table 4 shows that those who attended screenings became more willing to take action against teacher absenteeism. At the two-month midline survey, respondents in treated villages expressed more support for direct action than their control group counterparts on all four proposed actions. The estimated effect on the additive index that averages across all four measures is highly significant ($p < .01$). The estimated average effect on the index is 0.042 (SE = 0.016), a substantial effect but less than half of what we observed in our lab test. The estimated effect on the additive index remains substantial in the eight-month endline survey ($\hat{b} = 0.048$, SE = 0.017). Again, using the distribution of village-level outcomes in the placebo group as a yardstick, the estimates suggest

that the videos in essence moved the treatment group from the 50th percentile to the 63rd percentile for the midline, and to the 77th percentile for the endline.[11] Tables D10, D11, and D12 in the Supplemental Materials show that the results remain similar when controlling for the 'inactive' answer option presented to the respondent, when the covariate for audience size is dropped, and when controlling for a set of covariates selected via LASSO. Appendix D.5 in the Supplemental Materials shows that the estimated effect on conative attitudes remains statistically significant (at least at the 10% level) even if we correct p-values for multiple comparisons. This is true for two different correction procedures and whether the set of multiple comparisons includes the 13 tests considered in this paper or, instead, tests of the 9 primary hypotheses listed in our pre-analysis plan, which pertain to media messages on absenteeism, abortion, and violence against women. None of the outcomes reported below reach significance levels that meet this stringent standard.

[Table 4 about here.]

4.4. Community norms and perceived efficacy

In addition to measuring how respondents themselves would act if faced with an absenteeism problem, we also measured how they thought parents should behave in such cases. Should parents hold the school accountable for teachers' repeated absence, or rather leave management to the school administration? In Table 5 columns 1-2 show that at midline those who attended screenings were 4.7 percentage points more likely to believe that parents should hold the school accountable for repeated absence. This estimated effect dissipated to 0.031 by endline. Despite becoming somewhat more likely to think that parents should act, compliers were no more optimistic that the norm in their village was that parents *would* act by confronting the headmaster rather than waiting to get all the facts (columns 3-4). Nor did the videos increase viewers' belief that parents' action would actually be effective in reducing absenteeism (columns 5-6). The lack of effect in this case may reflect the very high baseline belief that action is effective: fully 77% of the control group expressed this view at midline, and 83% did so at endline.

Theoretically, can the robust findings for conative attitudes be reconciled with the rather weak effects on other evaluations and perceptions? One interpretation is that the videos' persuasive effects are channeled through Bandura-type mechanisms: the video dramatisation models desirable behaviour, and viewers came to see themselves in the role of the protagonist who responds to the absenteeism crisis affecting his or her household by taking collective action. As we move outside the confines of the script to assessments about what others in the respondent's own village would do and with what effect, the treatment becomes less influential. It does not appear that respondents updated their views about the prevailing sentiment within their own community based on the fact that others in the village viewed the same videos.

If this interpretation is correct, we should see little or no effects on respondents' policy goals about education outside the immediate confines of absenteeism. In keeping with this conjecture, we find substantial treatment effects when respondents are able to choose outcomes such as 'Reducing the number of bad teachers at school' (midline) or 'Reducing teacher absenteeism' (endline) from a list of possible goals for the community (see Table 6).[12] However, when policy goals are expressed at a higher level of generality, we find no effect. Respondents were asked, 'Suppose that the government has more money to spend on rural areas such as this one due to economic growth. If that happens, what would you most like to see the government spend on?' Treatment group respondents were not more likely to select 'more schools and teachers' from a list of policy goals; the estimated effect is weakly negative in the midline survey and weakly positive in the endline survey. Similarly, when respondents were asked in the endline survey to imagine voting in a local election in which candidates espoused different policy goals (building roads, strengthening law enforcement, providing health services, or improving schools), the treatment group is no more likely to prefer the candidate whose platform stresses schools.

To this point, we have focused solely on the attitudes of compliers, those who attended the film festival. Our placebo-controlled design allows us to identify spillover effects among those residing in the same villages who did not attend the film festival. For example, we can restrict the analysis to respondents who did not attend the film festival. Within this stratum, comparing those in treatment villages to those in placebo

villages allows us to detect whether the messages of the videos spread to non-viewers. In Wilke et al. (2020) and Table D13 of the Supplemental Materials we show that there are no spillover effects among those who did not attend the film festival. Even when restricting the analysis to respondents who did not attend the film festival but knew someone who attended, we find no evidence of spillovers across all outcome measures. Given the lack of spillovers, it seems that the way to maximize the impact of this form of entertainment-education is to maximize the size of the audience.

[Tables 5 and 6 about here.]

4.5. Reported absenteeism

Although our study was designed to assess the persuasive effects of media on attitudes and perceptions, this section offers a provisional look at the policy effects. In Table 7 we present the frequency of teacher absenteeism as reported by respondents, and the incidence of parental complaints about teacher absenteeism as reported by Village Health Team members (VHTs). The results are equivocal. The estimated effect on reported absenteeism frequency at endline among respondents is insignificant, both as reported by compliers (column 1) and those who did not see the videos (column 2). On the other hand, Columns 3 and 4 show that VHTs in treated villages are 12.4 percentage points less likely to report that parents ever complain to them about absenteeism at endline, though the effect at midline was weakly in the opposite direction. The overall outcome on this variable likely comprises two opposing effects, which is why we pre-registered a two-tailed test: an increase in complaints to VHTs caused by the increased salience of absenteeism but a decrease in complaints reflecting an actual decline in absenteeism. An optimistic interpretation might be that by the endline, the latter effect predominated, resulting in an overall decrease in complaints.

These limited impacts are not surprising, given that our design is ill-suited for measuring the effect of the treatment on actual absenteeism. Individual schools might draw students from villages in different treatment conditions, and statistical power is limited both by sample size and the lack of treatment saturation at the school level. Rather, our design allows us to understand the effects of the media messages on

behavioural attitudes as an exploratory first step. In section F of the Supplemental Materials, we propose an experimental design that could more convincingly address the policy-relevant question of whether these media messages can reduce actual absenteeism.

[Table 7 about here.]

5. Discussion

The present study affirms the growing literature on entertainment-education by showing that dramatisations of social problems can change conative attitudes, or the willingness to take action. Using two complementary research methods, we show that entertainment-education videos have the potential to change viewers' willingness to address teacher absenteeism through collective action. Lab-in-the field tests show that exposure to the three vignettes under controlled conditions greatly increases viewers' willingness to work together to remedy absenteeism. Field experiments show that these effects also materialise in more naturalistic viewing environments where the videos are embedded in feature-length movies over the course of a six-week film festival. In the field setting, viewers who attended the film festival on more than one weekend encountered the videos repeatedly during commercial breaks, in much the same way that viewers encounter ads via commercial television.

The effects on behavioural orientations are weaker in the field than in the lab, reflecting the different viewing experience as well as the delayed and unobtrusive measurement of outcomes two months and eight months after exposure. Nevertheless, the persuasive effects of the videos in the field are sizable and persistent, often amounting to almost half of a village-level standard deviation eight months after the intervention. We speculate that the videos, by portraying the protagonists as heroes rather than trouble-makers, overcome rural Ugandans' reticence about speaking out when confronted with chronic absenteeism.

These results offer encouragement to policymakers who would use community oversight as a lever to address teacher absenteeism. Despite their reluctance to intervene personally to address absenteeism, study participants widely believe that parental

involvement would be effective. Fully 83% of parents in the placebo group believed that intervention by parents would reduce absenteeism over the long term. Entertainment-education seems to be a promising way to encourage parents to overcome their hesitation to act.

That said, we are quick to acknowledge the limits of these persuasive effects. As we broaden the scope of outcome measures, we find that effects diminish as we move afield from absenteeism in particular to education as a broad policy priority. Although those exposed to the treatment videos were much more likely to list reducing absenteeism among the most important community goals, these respondents were not more likely to support candidates whose platform referred to improving schools more generally. These results may imply that a series of short vignettes is incapable of reordering subjects' core policy preferences; changes of this sort may require a longer and more sustained dramatisation akin to serialised or feature-length dramatisations tested in other policy domains (Paluck and Green, 2009, for example,).

The next step in this line of research is to investigate whether the videos tested here (or more extensive dramatisations of the same topic) not only affect attitudes but also rates of absenteeism. Optimizing the experimental design for this purpose not only means changing the sampling frame to focus specifically on villages with high baseline rates of absenteeism; it also invites researchers to explore ways of broadening the audience that is exposed to the message, perhaps through multiple screenings in public places, and of reinforcing the message via endorsements from public officials or community leaders.

Notes

[1]The experimental literature has called into question the hypothesis that absenteeism can be remedied by reducing class size (Duflo et al., 2015) or paying teachers bonuses when their students perform well on assessment tests (Glewwe et al., 2010). Correlational studies (for example, Kremer et al. (2005)) have questioned the value of teacher pay and local school associations.

[2]In a school system where funding follows the student, parents who choose to exit a school with chronic teacher absenteeism and re-enrol their child in another school would create an incentive for schools to take action to reduce absenteeism. However, in a context like rural Uganda, where school funding may be unresponsive to changes in student numbers, the policy effects of this 'exit' incentive may be weak.

[3]The narrative of the videos is outlined in the Supplemental Materials in Figure B1, and they can be viewed at the following address: `http://tiny.cc/uganda_media` (along with the videos for our concurrent domestic violence and abortion stigma campaigns).

[4]This lab study was conducted with respondents from a pilot study that we conducted in November and December of 2015. Interviews were conducted in villages that received either the abortion stigma video treatment or the placebo treatment (film festival only) during the pilot study; therefore, no respondents had previously seen the videos shown in our lab experiment. Note that all of the participants in the lab study had attended the film festival during the pilot study. Therefore, they are comparable to the compliers whose responses we analyze in the field experiment below.

[5]Subjects were randomly assigned to the teacher absenteeism videos or one of ten alternative versions of the domestic violence treatments. The eleven treatment arms were assigned with equal probability and random assignment was blocked by enumerator. See section C.1 of the Supplemental Materials for more details on the assignment procedure. Although the design does not maximize the precision with which the absenteeism treatment effect is estimated, it is amply powered to detect effects of the size found in our pilot study.

[6]As can be seen from Tables C3 and C4, the results change very little when controlling for fixed effects that pertain to the blocks used to assign treatment in the pilot field experiment or a set of covariates that have been selected via least absolute shrinkage and selection operator (LASSO). Table C5 shows that exposure to our video vignettes does not appear to affect respondents' views on whether others should or would act to counter teacher absenteeism. Neither do there seem to be any appreciable effects on respondents' views on whether action would be effective. We find similar patterns in our field experiment.

[7]In addition to the placebo condition, there were six treatment conditions: three for each of the three topics and three more for each pair of possible topics where villages received two of the three possible treatments.

[8] In two villages only five of the six scheduled screenings took place. Noncompliance seemed to be idiosyncratic and unrelated to the video messages embedded in the commercial breaks.

[9]The attendance question reads, '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?'

[10]We controlled for audience size as a proxy for the size of the village (which is our treatment cluster). Doing so mitigates bias to our treatment estimates from any correlation between cluster size and potential outcomes. Results without the audience size covariate are similar, as shown in Table D11. Our pre-analysis

plan specifies the use of least absolute shrinkage and selection operator (LASSO) to select covariates from among respondents' background characteristics; as shown in Table D12 in the Supplemental Materials, the LASSO-selected covariates change the estimates and standard errors only negligibly.

[11]These findings echo results from a pilot study carried out in November to December 2015 in a sample of 56 villages (half the size of this experiment). In that study, the average increase in willingness to act across four similar outcome measures was 6.0 percentage points ($p < .01$) two months after the videos were shown (equivalent timing to our midline for the current study). The main difference in the pilot study has to do with which specific outcome measures move in response to treatment. Estimates of treatment effects on the main conative attitude outcomes from the pilot study are shown in Table E1 in the Supplemental Materials. A meta-analysis of the pilot study and our main study results is shown in Tables E2 and E3 in the Supplemental Materials.

[12]This list of goals included reducing hunger, violence against women, poor sanitation, pollution, deaths during childbirth, lack of electricity, and lack of access to credit.

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	Involve LC1 Chair	Tell village	Use PTA	Assemble group
Find a tutor	0.81	0.64	0.56	0.34
Ask headmaster	0.82	0.50	0.63	0.32
Wait	0.45	0.28	0.37	0.13
Neighbouring village	0.84	0.62	0.69	0.37

Table 1. Conative attitudes among participants of lab-in-the field experiment, by passive and active options offered (N = 689)

Respondents were asked what they would prefer to do if they discovered their child's teacher was absent two days that week. Respondents were randomly assigned four different pairs of options, comprising an 'active' and a 'passive' alternative. Respondents were never asked twice about the same action and the order of the active options remained constant. The 'active' actions coded 1 are as follows: *Involve LC1 Chair*: 'Tell the LC1 chairperson to investigate why the headmaster has allowed this problem to occur.' *Tell village*: 'Bring it up in the village meeting.' *Use PTA*: 'Immediately begin organising a PTA meeting, even if you know this might start some trouble.' *Assemble group*: 'Assemble a group of parents and confront the teacher.' The four 'passive' actions coded 0 are as follows: 'Find a tutor to instruct your child until the teacher comes back,' 'Ask the headmaster to put your child into a different classroom until the teacher returns,' 'Wait another few days to see if the problem corrects itself,' 'Send your child to a school in the neighbouring village, where the teachers always come to class.' Each cell shows the proportion of respondents choosing the 'active' option (columns), by 'passive' option offered (rows).

	Involve LC1 Chair	Tell village	Use PTA	Assemble group	Index
	(1)	(2)	(3)	(4)	(5)
absenteeism	0.134**	0.128*	0.130*	0.100	0.123***
	(0.068)	(0.083)	(0.082)	(0.081)	(0.044)
Control Mean	0.71	0.51	0.55	0.28	0.51
p-values	0.025	0.061	0.057	0.108	0.003
Hypothesis	upr	upr	upr	upr	upr
Block FE	No	No	No	No	No
Observations	689	689	689	689	689
Adjusted R^2	0.005	0.007	0.002	0.005	0.020

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table 2. Estimated effects of absenteeism videos on conative attitudes among participants of lab-in-the-field experiment

Estimates stem from instrumental variables regressions that use a binary indicator for whether a respondent was assigned to the absenteeism video as an instrument for whether the respondent was indeed shown the absenteeism video. Heteroscedasticity robust standard errors are shown in parentheses. p-values are based on a normal approximation to the sampling distribution. See the caption of table 1 for the wording of the outcomes used in columns 1 to 4. The *Index* outcome in column 5 is a simple average of these four outcomes. As per the main specification in our pre-analysis plan for the lab-in-the-field experiment, the analyses shown in this table do not include block fixed effects. The row labeled "Hypothesis" shows the direction of hypothesis tests.

Stratum	Never-Takers		Compliers	
Gender	Men	Women	Men	Women
N	1990 (45%)	2388 (55%)	797 (69%)	359 (31%)
Parent	83%	93%	72%	95%
Age	33	31	29	29
Own TV	30%	27%	21%	13%
Own Radio	88%	75%	88%	72%
Rooms in house	3	3	2	2
Highest grade [0,16]	7.1	6.9	7.3	6.0
Illiteracy	10%	14%	8%	15%
Has visited big city	80%	69%	84%	74%

Table 3. Characteristics of midline sample in field experiment, broken down by compliance stratum

	Involve LC1 Chair		Tell village		Use PTA		Assemble group		Index		
	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline	Midline	Endline	Overall
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
absenteeism	0.030	0.047*	0.047*	0.070**	0.030	0.042*	0.060**	0.034	0.042***	0.048***	0.045***
	(0.024)	(0.026)	(0.031)	(0.030)	(0.028)	(0.028)	(0.030)	(0.028)	(0.016)	(0.017)	(0.014)
Control Mean	0.77	0.74	0.56	0.55	0.7	0.56	0.33	0.32	0.59	0.54	0.57
RI p-values	0.144	0.051	0.075	0.015	0.158	0.091	0.035	0.147	0.009	0.009	0.002
Hypothesis	upr	upr	upr	upr	upr	upr	upr	upr	upr	upr	upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,041	1,041	1,041	1,041	1,041	1,041	1,041	1,041	1,041	1,041	1,041
Adjusted R^2	0.009	0.004	0.001	0.015	0.002	−0.003	0.013	−0.0005	0.014	0.006	0.014

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

Table 4. Estimated effects of absenteeism videos on conative attitudes among "compliers" in field experiment

Standard errors clustered at the village-level are shown in parentheses. p-values calculated using randomization inference. See the caption of table 1 for the wording of the outcomes used in columns 1 to 8. The *Index* outcome in columns 9 and 10 is a simple average of these four outcomes, calculated, respectively, for midline and endline outcome measures. The outcome in column 11 is the average of the midline and endline indices. As per our pre-analysis plan for the field experiment, the estimates in this table stem from specifications that include block fixed effects. The row labeled "Hypothesis" shows the direction of hypothesis tests.

	Parents should act		Community would intervene		Intervention is effective	
	Midline	Endline	Midline	Endline	Midline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)
absenteeism	0.047**	0.031	0.008	0.029	0.010	−0.016
	(0.023)	(0.027)	(0.032)	(0.028)	(0.023)	(0.022)
Control Mean	0.7	0.7	0.33	0.29	0.77	0.83
RI p-values	0.038	0.154	0.427	0.174	0.341	0.746
Hypothesis	upr	upr	upr	upr	upr	upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,041	1,041	1,041	1,041	1,041	1,041
Adjusted R^2	0.0001	0.0001	0.003	0.007	0.002	−0.001

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

Table 5. Estimated effects of absenteeism videos on perceived norms and efficacy among "compliers" in field experiment

Standard errors clustered at the village-level shown in parentheses. The outcomes are coded as follows: *Parents should act*: 'Suppose a teacher is repeatedly absent from school during teaching hours. Which of the following statements comes closest to your view?' 0 = 'Parents intervention only leads to conflict and discord. Its better to leave the management of the school to the administrators.' 1 = 'Parents should call a meeting of the PTA and hold the school accountable for the teacher's absence.' *Community would intervene*: 'Teachers are often absent during school hours. In some villages, parents wait to get all the facts before taking any action, in order to avoid creating unnecessary conflict. In other villages, the parents immediately come together and confront the headmaster. What is your village like?' 0 = 'More like the first villages, where parents wait to get all the facts,' 1 = 'More like the second villages, where parents immediately confront the headmaster.' *Intervention is effective* 'Some people think that it is hard to organise parents to do something about teacher absenteeism and that anything they achieve will quickly disappear. Other people think that parents can come together over a long stretch of time and reduce absenteeism. Which comes closest to your view?' 0 = 'It is hard to organise if parents get together to do something about teacher absenteeism and anything they achieve will quickly disappear,' 1 = 'Parents can come together over a long stretch of time and reduce absenteeism.' As per our pre-analysis plan for the field experiment, the estimates in this table stem from specifications that include block fixed effects. The row labeled "Hypothesis" shows the direction of hypothesis tests.

	Discussed absenteeism		Teachers/absenteeism important		Schools important		Candidate platform
	Midline	Endline	Midline	Endline	Midline	Endline	Endline
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
absenteeism	0.044	−0.013	0.079***	0.060**	−0.030	0.027	0.007
	(0.033)	(0.037)	(0.027)	(0.032)	(0.025)	(0.027)	(0.024)
Control Mean	0.35	0.41	0.43	0.44	0.32	0.28	0.59
RI p-values	0.107	0.642	0.005	0.043	0.857	0.18	0.404
Hypothesis	upr	upr	upr	upr	upr	upr	upr
Block FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,041	1,041	1,041	1,041	1,041	1,041	1,041
Adjusted R^2	0.011	0.009	0.011	0.020	0.006	0.007	0.169

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

Table 6. Estimated effects of absenteeism videos on perceived salience and importance of absenteeism among "compliers" in field experiment

Standard errors clustered at the village-level shown in parentheses. The dependent variables are coded as follows: *Discussed absenteeism* 'Think about the conversations you have had with friends or family over the past week. Which of the following topics did you discuss, if any?' 1 if selected 'Teachers who don't come to school.' *Teachers/absenteeism important* 'Here is another set of cards, which show different goals for your village. Please choose the three that are currently the most important to you.' At midline: 1 if selected 'Reducing the number of bad teachers at school'. At endline: 1 if selected 'Reducing teacher absenteeism.' *Schools important* 'Suppose that the government has more money to spend on rural areas such as this one due to economic growth. If that happens, what would you most like to see the government spend on:' 1 if selected 'more schools and teachers.' *Candidate platform* 'Imagine a village about one days walk from here is having an election to replace its LC1 chairperson. There are two candidates. Let me tell you about each one and you can tell me which of the two you think should be elected. The first candidate is named [Mr. / Mrs.] [Nantuume / Ntakitimana / Barigye] and [he / she] promises to [improve schools / improve access to healthcare / improve roads / crack down on stealing in the village]. The second candidate is named [Mr. / Mrs.] [Semakula / Uwineza / Natukunda] and [he / she] promises to [improve schools / improve access to healthcare / improve roads / crack down on stealing in the village].' (Selections in brackets randomly assigned, question asked twice so each respondent chose two of the four policy platforms overall, paired randomly. Gender was randomised only for the second time the question was asked; the first time both candidates were presented as male) 1 if selected 'improve schools,' 0 if selected alternative. In estimating the treatment effect on this outcome, we control for the candidate's name (reflecting ethnicity and gender), as well as the randomised alternative policy choice to the 'improve schools' option that the respondent was offered. As per our pre-analysis plan for the field experiment, the estimates in this table stem from specifications that include block fixed effects. The row labeled "Hypothesis" shows the direction of hypothesis tests.

	Reported absenteeism		Parents complain	
	Endline		Midline	Endline
	(1)	(2)	(3)	(4)
absenteeism	0.031	−0.009	0.091	−0.124*
	(0.070)	(0.066)	(0.062)	(0.061)
Control Mean	1.57	1.38	0.48	0.51
RI p-values	0.674	0.439	0.141	0.057
Hypothesis	two	lwr	two	two
Block FE	Yes	Yes	Yes	Yes
Sample	Compl.	Never-T	VHT	VHT
Analysis Level	Indiv.	Indiv.	Clus.	Clus.
Observations	1,041	852	112	111
Adjusted R^2	0.013	0.001	0.188	0.022

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

Table 7. Estimated effects of absenteeism videos on on perceived levels of teacher absenteeism from field experiment

Standard errors clustered at the village-level shown in parentheses. The dependent variables are coded as follows: *Absenteeism* 'Since Christmas, to the best of your knowledge, were teachers in the local school absent during teaching hours once a week or more?' Scored 0-3 for 'Almost every day', 'Once a week', 'A few times', 'Not at all', respectively. *Parents complain*: 'Do mothers or fathers of schoolchildren ever complain to you that the teachers are not showing up to teach their children in the local schools?' 0 'No', 1 'Yes'. As per our pre-analysis plan for the field experiment, the estimates in this table stem from specifications that include block fixed effects. The row labeled "Hypothesis" shows the direction of hypothesis tests. The analysis in column 1 subsets the sample to compliers, while that in column 2 subsets the sample to never-takers. Estimates in columns 3 and 4 are based on a sample of village-health team members (VHTs). The unit of analysis in columns 3 and 4 is the village cluster, i.e., the responses of VHTs from the same village cluster have been collapsed to the cluster-level using means.