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TV, female empowerment and demographic change in rural India

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Replication Paper 2

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Abstract

In a paper that challenges received wisdom about female empowerment and demographic change in South Asia, Robert Jensen and Emily Oster (2009) identify the causal beneficent impacts of cable TV, which include reducing women's tolerance of spousal violence, son preference and fertility and increasing female autonomy and school enrolment in rural India.

In our pure and scientific replications we adjust index constructions, correct a programming error and change a variable so that Jensen and Oster's key coefficients on tolerance of spousal beatings, indicators of female autonomy, and school enrolment shrink in size and become statistically weaker. Triangulation against alternative data sets casts doubt on some reported values.

Stressing the absence of a theory of change informed by media and development research, we introduce relevant descriptive statistics and simple respecifications of Jensen and Oster's main model and uncover a more complex underlying 'story' manifested in heterogeneous effects by age and social identity, and notable spillover effects.

A robust result is that the introduction of cable TV does not affect women without education, while son preference may be unrelated to cable TV. These empirical insights, some of which were previsaged in an earlier working paper by Jensen and Oster (2007), open the door to alternative theories of change, and to substantive revisions of policy recommendations.

JEL N35, 015, I25

Keywords: cable TV, India, domestic violence, women's autonomy, fertility, education, panel data

Contents

List of figures and tables

Abbreviations and acronyms

3ie	International Initiative for Impact Evaluation
DHS	Demographic and Health Surveys
DISE	District Information System on Education
IPJ	Vegard Iversen and Richard Palmer-Jones
MCA	multiple correspondence analysis
NFHS	National Family Health Surveys
PCA	principal component analysis
SARI	Survey of Aging in Rural India

1. Introduction

In the South Asian context, female disadvantage is usually seen as an outcome of deeply engrained patriarchal cultures, reflected in kinship and other social practices (Dyson and Moore 1983; Rahman and Rao 2004). The literature addressing female disadvantage and women's empowerment[1] in these settings emphasises access to resources such as: education (Murthi *et al*. 1995); rights in land (Agarwal 1994); waged employment (Rosenzweig and Schultz 1982; Sen 1990); entrepreneurial opportunities; credit; and so forth; it also differentiates between types of empowerment (Basu and Koolwal 2005).

In a paper that runs contrary to this received wisdom, Robert Jensen and Emily Oster (2009)[2] suggest that what are usually understood to be rigid behavioural norms and attitudes (Bourdieu 1977), which are also widely believed to respond only to radical revisions of property regimes or levelling of educational and other opportunities, may change rapidly once cable TV arrives in a village.

Using the three year panel data set from the Survey of Aging in Rural India (SARI), Jensen and Oster identify the impact of cable, or 'the power of TV', on indicators of women's status and demographic change by differencing out stable village and individual characteristics while adding controls for income and pre-existing differential trends (Jensen and Oster 2007, p.10). They find that cable TV bolsters female autonomy and reduces women's tolerance of spousal beatings, son preference and fertility rates. Drawing on administrative data from the District Information System on Education (DISE) in Tamil Nadu, they also report that 'continued exposure to cable (years of access) increases (total) enrolment' (Jensen and Oster 2007, p.1087).

The preoccupation with women's resources and opportunities within academic and policy circles thus appears to be at least in part misguided, since the potential for benevolent ideational change can be entrusted to popular TV. However, these results contrast with the continuing disadvantage females have when it comes to survival, [3] which have remained steady over a period that has seen extensive penetration of cable TV (National Readership Studies Council 2006; Television Audience Measurement 2012[4]). They also contrast with some trends observed for tolerance of spousal beatings between the Indian National Family Health Surveys (NFHS) rounds 2 and 3.[5]

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[1] In the present context the terms 'status' and 'empowerment' are generally used as synonyms. The *Measure DHS* website describes the '**Status of women'** section of the women's questionnaire on which Jensen and Oster's survey instrument is modelled as follows: The questionnaire asks about various aspects of women's empowerment, including decision making and autonomy, and about attitudes towards domestic violence. Demographic and Health Survey website <www.measuredhs.com/What-We-Do/Survey-Types/DHS-Questionnaires.cfm#CP_JUMP_16179>. See Basu and Koolwal (2005) for a nuanced conceptual discussion.

[2] We note that there are two Jensen and Oster papers entitled 'The Power of TV: Cable Television and Women's Status in Rural India,' which are quite different from each other. We use Jensen and Oster as short for Jensen and Oster (2009) in the *Quarterly Journal of Economics* and Jensen and Oster (2007) in reference to their NBER Working Paper.

[3] The national juvenile (0-6) sex ratio reported in the Indian census fell from 927 in 2001 to 914 in 2011 (Ramaiah *et al*. 2011).

[4] For more information find details about the survey at www.tamindia.com/tamindia.

[5] These contrasts are discussed in more detail in section 4.

There has been much work on 'media effects', mainly concerned with children and adolescents (Strasburger and Donnerstein 1999; Strasburger *et al*. 2008), often in the context of 'pro-social' programming, but with far from conclusive results (Bryant and Thompson 2002).

Jensen and Oster report a small number of 'correlational' studies that indicate the strong effects of media – TV in particular – in other contexts (Brazil, Mexico) and some ethnographic studies from South Asia, but do not mention earlier evidence from India that runs against the thrust of their findings. Existing literature on the introduction of TV is also not strongly indicative of such effects.

Jensen and Oster's results are particularly surprising in that they focus entirely on the introduction of cable TV (Chapman 2005), and do not attempt to delineate the effects of explicitly pro-social or persuasive TV programming which were present in the other studies to which they refer (La Ferrara *et al.* 2012; 2008). Even in cases where the impact of persuasive TV has been convincingly demonstrated in randomised control trials or well conducted analysis of observational data, estimates of impacts have typically not been generalisable across contexts, shown to exist in the long run, convincingly linked to models or mechanisms of persuasion, or identified in the context of competing messages (DellaVigna and Gentzkow 2010, p.665).

Given the widespread view that even sophisticated correlational work is unlikely to throw sufficient light on the effects of the introduction of cable TV (McGuire 1986; Zaller 1996; Bryant and Thompson 2002, pp.55-57), we subject Jensen and Oster to pure, statistical and scientific replication. Conducting a replication study of micro-econometric research may be considered to comprise three processes: checking that the original data and methods do in fact produce the reported results (Collins 1991); using plausible alternative variable computations and estimation methods with the same raw data and estimation model and using the same or convincing alternative estimation methods with the same or similar data sets to test robustness of results using the maintained or rival hypotheses – or respectively pure, statistical and scientific replication (Hamermesh 2007[6]).

We discover programming and coding errors or unreported influential assumptions and test whether Jensen and Oster's results are robust to the construction of their key indexes which capture tolerance of violence and female autonomy. These concepts are qualitative, inevitably hard to measure and their translation into quantitative indexes is unlikely to be straightforward and unambiguous. Moreover, these different indicators are proxies for, rather than direct indicators of, women's status or autonomy. They may also relate to different concepts or types of empowerment or autonomy (England *et al.* 2000; Basu and Koolwal 2005). We therefore explore whether variable construction affects Jensen and Oster's results. We find that their results become weaker once corrected for questionable variable construction and a programming error.

Informed by the relevant literature on empowerment, and on media and development, we also pay attention to the mechanisms or theories of change through which the impacts of the introduction of cable TV may plausibly materialise in different pathways and modes of empowerment. The coarse evidence on changes in TV viewing habits and their variation

[6] We use slightly different definitions of the three categories of replication described by Hamermesh (2007:716). See also Easley *et al*. (2000).

across village types demonstrates, for instance, that the most compelling transformations occur in households without TV in villages with cable, suggesting that the mechanisms through which social change occurs may be quite different from those implied by Jensen and Oster's results.

Our further scrutiny of impact heterogeneity reveals, among other issues, that the introduction of cable TV has no impact on illiterate women.[7] Similar results on education are reported in Jensen and Oster (2007), but were not emphasised or taken forward in Jensen and Oster. Such insights are crucial for policy and suggest that policy recommendations and the relevant theory and underlying process of change may be less straightforward than Jensen and Oster's findings and discussion recognise.[8]

The rest of the paper is organised as follows: section 2 reviews the literature on media and social change including women's empowerment and demographic change, and the methodological approach taken by Jensen and Oster. To facilitate comparisons between our text and the original paper, we follow Jensen and Oster's broad outline with the first part of our paper focusing on the analysis using SARI data before moving to that based on the DISE data. In section 3 we report briefly on the checking and pure replication using SARI data and find that this analysis can be exactly replicated, and for the most part does what it says. However, lacking access to the original survey instruments and raw data limits the scope of our replication. The outcome variables representing attitudes to domestic violence and female autonomy are subject to internal and external validity concerns and under the scientific replication heading we explore the construction of these composite variables and use disaggregation to examine the plausibility of the effects found by Jensen and Oster and their meaning. We also estimate social spillover effects from cable TV introduction – i.e. the impacts of cable on women from households without a TV, finding sizable such effects, including for what we interpret as 'hard to change' variables. Finally, we implement simple and relevant respecifications of Jensen and Oster's main model. New and subtle insights suggestive of a more complex underlying 'story' that lends only partial support to the main hypotheses emerge from this exercise. In section 4 we shift the focus to school enrolment using the DISE-data. After finding a programming error, a questionable variable construction, and asserting the limitations of the estimation data and approach, we suggest that the analysis of the DISE data lends little support to the overall argument. Section 5 concludes with regard to both whether it is plausible to claim a causal relation between the introduction of cable TV and women's empowerment, and to the methods and merits of replication.

[7] It is common in the literature to equate illiteracy with having no education. This is a simplification and in this paper we use Jensen and Oster's education variable, which captures years of schooling to distinguish between women with some and women with no education.

[8] There is a marked change in the discussion between Jensen and Oster and the earlier working paper (Jensen and Oster 2007). It is noteworthy that Jensen and Oster do not refer to their earlier working paper so that many readers will not be aware of this arguably more insightful study. In addition, an individual fixed effects model limits the policy interest of Jensen and Oster's results by removing many observed and unobserved time-invariant, policy-relevant variables such as ethnicity, caste, religion, access to markets, and so on from the analysis. Unobserved time-varying variables could also potentially confound the results.

2. Literature review

Jensen and Oster argue that the introduction of cable TV in village India has pro-social effects, which are manifested in: women's reduced tolerance for domestic violence; greater female autonomy; a decline in son preference and fertility rates; and increased school enrolment. Jensen and Oster's starting point is the rapid proliferation of access to television in the developing world, India included. According to the NFHS rounds 2 and 3, TV overtook radio as the most important mass communication medium owned by rural Indian households between 1998-1999 and 2005-2006. In 1998-1999, 40.9 per cent of rural households owned a radio while 43.6 per cent owned a TV. By 2005-2006, 58 per cent owned a TV, while radio ownership remained the same.

Jensen and Oster contend that TV increases the availability of information about the outside world, exposing viewers – especially in remote rural villages – to (urban) lifestyles very different from their own.[9] In particular, urban women featuring in popular TV serials are more educated, marry later and often work outside the home as professionals or in other coveted jobs. Popular soap operas such as *Kyunki Saas Bhi Kabhi Bahu* have themes evolving around family and gender relations – in this particular case, in an industrial family in Mumbai – and are, according to Jensen and Oster, catalysts for social change.

However, those familiar with Indian TV will know that many soap operas espouse conservative and traditional rather than pro-social values, with viewers identifying with conservative and reactionary characters even when more empowered characters, or characters who become empowered, are on offer (Rogers *et al*. 1995). Furthermore, urban is not always equivalent with 'modern' – for example, the average urban Indian woman is about half as likely to be in the labour force as her rural counterpart.[10] Similarly, modernisation and rising prosperity does not necessarily improve female status as illustrated in the declining juvenile sex ratio in census rounds since 1991 (Subramaniam and Jayaraj 2004; Mishra *et al*. 2011).

As noted above, there is an enormous literature on "media effects", including those of cable and satellite TV and the internet media, reflecting concerns that their transformative potential is not always for the better, depending on the point of view (Briggs and Burke 2002; Chapman 2005). Numerous models for understanding media effects have been proffered – including agenda setting, framing, cultivation, priming, persuasion, uses and gratification theory – in relation to a wide range of effects (for example, on violence, sexual attitudes and behaviour, fears, stereotyping) and in relation to marketing and planned communication (Bryant and Thompson 2002; Bryant and Oliver 2011). Such models have always drawn attention to the contexts within which TV viewing takes place (Mason and Smith 2003); Jensen and Oster, however, offer limited insight into the appropriate media effects framework to interpret their results.

[9] The possible effects of TV have been of considerable concern since its invention, but there seems little consensus as to what they are or how to assess them (Livingstone 1996). Furthermore, there is little conclusive evidence linking TV with the pro-social dimensions of women's empowerment, while there is much literature focusing on the anti-social effects of the media's portrayal of women and of violence against children, in particular (Bryant and Oliver 2011).

[10] Other assertions appear inflated – for example, Jensen and Oster's (2007, p.6) cite of Johnson's (2001) claim of a 50–year-old villager who had learnt 'about using the court system to address grievances'. Indian courts are generally well beyond the reach of the average rural resident.

The research literature on media and development includes 'evaluations' of attempted social engineering through TV serials. La Ferrara *et al*. (2008, 2012) report how the Brazilian TV network *Rede Globo* produced high-quality soap operas with pro-social messages from the 1970s, with outreach seemingly related to a decline in the fertility rate. Inspired by promising findings from the broadcasting of six *telenovelas* aimed at promoting literacy, family planning and gender equality in Mexico, India broadcast its first pro-development soap opera *Hum Log* in 1984-1985 (Brown and Cody 1991). *Hum Log* was broadcast three times a week on the state channel Doordarshan over an 18- month period and achieved record audience ratings of 90 per cent in the north and 40 per cent in the south, where knowledge of Hindi is more limited (ibid).

In spite of the unannounced and subtle promotion of pro-social beliefs and behaviours and viewers' intense involvement with the characters in the serial, the main findings from a sample of 1,170 male and female viewers, 82 per cent of whom had watched some of the episodes of *Hum Log*, were negative: exposure to the series did not increase viewer awareness of women's issues. Singhal *et al*. (1993a) concluded that among the resources conducive to the transmission of pro-social messages were 'the network of social service organisations needed to support the behavioural changes promoted by the program's pro-development messages' (p.160); and '[An infrastructure to support the implementation of development messages requires organisations of skilled professional such as the family counsellors, literacy teachers, agricultural experts, health workers, and so forth' (p.162).

Rogers *et al*. (1995) reported that *Hum Raahi*, a further attempt at pro-social programming in India may have been more successful, although their evidence is largely based on open-ended focus group discussions. They found that 'at a minimum, [*Hum Raahi*] raised consciousness about the problems of gender inequality' (p.295). Perverse outcomes are also possible if viewers identified 'with negative role models [who]…embody power and material success', partly because 'the actress who portrayed [such a person]...turned in a charismatic performance…[leading to] oppositional readings' (p.296).

Jensen and Oster cite more recent ethnographic accounts to support the beneficial causal pathways between access to cable TV at village level and the attitudes and behaviour of rural female viewers.[11] In support of findings which contrast with the expectations of the structural (rather than ideational) view of gender disadvantage in South Asia, it could be argued that: the findings on *Hum Log* and *Hum Raahi* are dated; their information derives from samples which differ from the mainly rural focus of SARI; the survey and secondary data methodologies used by Jensen and Oster may well produce different results from those of Brown and Cody (1991), Singhal *et al* (1993a, 1993b) and Rogers *et al*. (1995); cable TV has undoubtedly reached much larger audiences; and viewers more than 30 years ago may have been less receptive to the type of changes that Jensen and Oster report. However, and in spite of the above antecedents, Jensen and Oster only very superficially explore the causal pathways between TV viewing and the social setting in which such viewing may occur; the specific content of popular TV programmes; and how content may spur attitudinal and behavioural change, since their empirical work focuses exclusively on the availability of cable at village level.

[11] See also Priyadarshani and Rahim (2010) and Budd (2010).

Since Jensen and Oster do not examine TV viewing habits and their associations with attitudinal and behavioural change in any depth[12] one cannot rule out that Jensen and Oster's village-has-cable TV variable may pick up something quite different from the impact on rural women of cable TV viewing. The causal effect of cable TV that Jensen and Oster identify could be quite unrelated to TV viewing and might instead, for instance, resemble the effect of joining a self-help group (which, among other benefits, provides a forum for discussion among women) or the availability of supportive services as shown in the discussion of *telenovelas* and pro-social programming above.

For the two main measures of female empowerment, tolerance of violence and female autonomy, constructed by Jensen and Oster, there are, as shown later in this paper, only minor differences in the effect of access to cable TV on women from households with and without TV. This suggests strong attitudinal and behavioural spillovers (that may be confounded by unobserved time-variant common factors). Equally important, and excepting fertility rates, we find that access to cable TV has no impact on women with no education. Given that about 54 per cent of the women in SARI have no education, this is crucial. Similar but less incisive results for the effects of education appear in Jensen and Oster (2007), but are not mentioned or taken forward in Jensen and Oster.

Another concern with the meaning that can be attached to the findings, acknowledged by Jensen and Oster, is the use of self-reported attitudinal responses as measures of the effect of TV on tolerance of spousal violence in place of other recommended, more direct measures (CEBP 2010). Attitudinal measures are subject to growing scepticism unless correlated with actual outcomes such as the incidence of domestic violence or corroborated by triangulation (*ibid*). The SARI questionnaire was explicitly modelled on DHS surveys (NFHS2) and used an almost identical set of questions to capture respondent attitudes to spousal violence. Both NFHS2 and 3 asked additional questions about experiences of domestic violence; reported attitudes towards, and experiences of, violence reported in these surveys are not always well correlated.

This is not an insignificant matter. Figure 1 in Appendix B illustrates levels, variations and associations of female tolerance for and experiences of spousal violence in India by region. It shows significant differences between and within the regions, with an apparent – if limited – association between experience and acceptability other than in the east,[13] which is ethnically and culturally diverse.[14]

2.1 Media effects

Jensen and Oster refer to a number of empirical studies which show the effects of TV on attitudes and behaviour in the West (Gentzkow and Shapiro 2004; DellaVigna and Kaplan 2007), and in developing countries (Olken 2006; Chong *et al*. 2007; subsequently La Ferrara *et al.* 2008, 2012). These are correlational studies which, if they have a theoretical causal framework linking TV with practice, can be said to fall within the realm of the

[12] Jensen and Oster (2007) pays more careful attention to context than Jensen and Oster.
[13] Jammu (jm in Figure 1 in Appendix B) is an outlier for the states classified as 'north.'
[14] Following Dyson and Moore (1983) we would expect regional differences related to cultural differences. The eastern region is comprised predominantly of Indo-Aryan language-speakers in West Bengal, Assam and Orissa, and mixed Indo-Aryan and Dravidian language speakers in Chhattisgarh and Jharkhand.

knowledge, attitudes, practice paradigm rather than the perspectives characteristic of media theorists (Bryant and Thompson 2002).

Studies of media effects on a wide range of topics have criticised such approaches on a number of grounds, including neglect of the contexts and pathways through which knowledge and action may be constructed in social contexts. Media theorists posit connections between the messages contained in media, their interpretation by viewers and those with whom viewers are in contact, and other determinants of actions. For example, Stuart Hall's (1973, 1980) 'encoding/decoding model' set the stage for work that moved on from a basic 'message->action' causal pathway to one in which texts are differentially perceived and interpreted by active audiences (Livingstone 1996, 2007). Thus, La Ferrara *et al.* (2008, 2012), in a study that otherwise strongly resembles Jensen and Oster's methodological approach, link the content of specific TV programmes with fertility rates by identifying the impacts of the gradual spread of a TV channel that produced high-quality programmes with implicit pro-social messages (the main characters have few children and thus 'small' families).

3. The SARI analysis

3.1 Pure replication

As noted above, Jensen and Oster use two data sets (SARI and DISE) to establish their conclusions. We discuss the data made available by Jensen and Oster below. Jensen and Oster use both sets of data to estimate the following model:

$$y_{ivt} = \beta c_{vt} + \gamma_{iv} + \delta_t + \tau X_{ivt} + \varepsilon_{ivt} \quad (1)$$

where y_{ivt} is outcome *y* for individual *i* in village *v* in year *t,* γ_{iv} are individual fixed effects, δ_t are year dummies (2002 & 3) and X_{ivt} are a set of controls including household income and the age and age-squared of the respondent,[15] and c_{vt} is a dummy representing the presence of cable (or not) in village *v* in year *t*. With the SARI data, identification depends on the 21 villages which get cable for the first time during the second and third of the three years of the survey, and the model is estimated using standard panel data methods with standard errors clustered at the village level. With the DISE data set, the model is estimated using the Prais-Winston method to account for serial correlation.

[15] In SARI estimations, X includes 'interactions between a year indicator and state dummies, income, education, age and age-squared, village population density, electrification status, and distance to nearest town' (Jensen and Oster 2009, p.1072). All covariates except income are for the base year 2001. In fact, since most of the covariates are specified at person or village level, this is a multi-level model, as explained in appendix 1.

The SARI data set

The SARI data are from a panel survey of 2,700 households containing a person aged 50 and over, in Bihar, Haryana, Goa, Tamil Nadu and New Delhi. The sample was drawn in two stages: in the first stage 180 villages were selected at random 'from district lists'; in the second, 15 households were selected at random from village 'registration lists'.[16]

The raw data from the survey are no longer available and the data made available on our request to the authors are the final data set used in Jensen and Oster's analysis (personal communication with Jensen and Oster). More variables are provided than those used in the published estimations, and the village level files can be independently produced from the individual level files.

Many variables of interest are constructed from raw data not available to us (or, apparently, to Jensen and Oster); for some variables – including the key composite outcome variables – the constructions can be inferred without much difficulty. Jensen and Oster were forthcoming in answering questions in general, including on how these composite variables were constructed.

The SARI panel data set shared by Jensen and Oster comes with clear and well organised .do files that facilitate a pure replication of their descriptive statistics (Jensen and Oster Table II[17]) and regression results on female attitude to spousal violence (the index mn_outcome), female autonomy (the index mn_real), son-preference (the binary dependent variable wants_son), and fertility (the binary dependent variable is_pregnant) (ibid, Table IV). Since neither the raw data nor the original survey instruments are available, we are unable to form an impression about any prior data cleaning that may have taken place.[18]

Pure replication of the SARI-based tables – both descriptive statistics and estimations – was successfully achieved using both the code supplied by the authors and our reconstructions of the Jensen and Oster code.[19]

4. SARI: Scientific replication

Having successfully replicated Jensen and Oster's SARI results, motivated by the literature discussed above and the limitations of Jensen and Oster in regard to theories of change, we move on to closer examination of variable constructions, estimation models and estimation methods with the SARI data.

[16] The Delhi sample is predominantly urban, while the rest are all rural.
[17] Throughout this replication study we use the original roman numerals to reference Jensen and Oster's tables and figures.
[18] Jensen and Oster informed us that the SARI data set to which we have had access is identical to the data set they received from their Indian counterparts who were responsible for survey implementation.
[19] Our own code to reproduce all the results derived from the SARI data in Jensen and Oster can be obtained from us. We have only one point of difference with Jensen and Oster, regarding panel B of Table IV from their paper. Our computation of the variable representing the 'pre-trend' differs from theirs, but makes no apparent meaningful difference to the results – for example we do not find that pre-trends account for the effects of introduction of cable TV.

On the whole it is likely that the estimation model could be of greater significance because the estimation method (using the Stata xtreg command[20]) is quite appropriate to the data and estimation model. However, we also register that the SARI data are multi-level, including a treatment variable that is at a higher level than the observation level (for example 'vill_has_cable' is a village-level variable, while the dependent variables and many covariates are individual-level variables).[21]

4.1 Outcome variable constructions

Jensen and Oster construct simple attitude indexes representing the acceptability of domestic violence (mn_outcome), female autonomy (mn_real), and measures of son preference and fertility, from raw survey variables without critically reflecting upon the methods used. Index variables are sensitive to their constructions (Everitt and Dunn 2001; Chatfield and Collins (1980), so it is worth exploring whether their approach influences the results. For example, the female autonomy measure (mn_real) is constructed from questions on decision making about healthcare, purchases and visiting friends and relatives, and whether permission is required either to go to the market or to visit friends or relatives.

These questions parallel those used in Demographic and Health Surveys (DHS), including the NFHS, which have a number of questions addressing women's empowerment and autonomy (see also Kishore and Johnson 2004).[22] In particular, the SARI data mirror questions asked in NFHS2. We compare some NFHS2 and 3 results with those from SARI to obtain external validity clues. A further reason for exploring the index constructions is that different components of these indexes may reflect different forms or scales of empowerment (Basu and Koolwal 2005), and hence be differently related to the introduction of cable TV.

A description of each of the variables used by Jensen and Oster is presented in Table 1[23] in Appendix A.

Jensen and Oster construct their female autonomy measure (mn_real) aggregating decisions about: (i) healthcare for the respondent (collapsed to 0/1 and 1 if the woman makes the decision on her own or jointly with other household member (s)); (ii) purchase of major household items (ditto); (iii) whether the respondent will visit or stay with family or friends (ditto); (iv) whether the respondent has money to spend on her own. In addition, Jensen and Oster include whether the respondent needs permission to visit the market (v) and relatives or friends (vi).

The latter variable (permission to visit relatives or friends) appears to lead to a double count since it is highly associated with (iii) (Pearson chi2(2) = 177.2842 Pr = 0.000), although there are considerable inconsistencies.[24] It seems more appropriate, therefore, to

[20] More information on this Stata command is available at www.stata.com/help.cgi?xtreg.
[21] See appendix 1.
[22] The DHS questions are located at www.measuredhs.com/What-We-Do/Survey-Types/DHS-Questionnaires.cfm.
[23] Since we do not have the original questionnaires or instructions, we cannot comment further on the ways in which these data were produced.
[24] For example 35 per cent of respondents who say she did not need permission to visit relatives reported that others decided whether she could stay with family/friends.

use an autonomy measure that includes at most five of these six variables. Below we check the sensitivity of Jensen and Oster's results to introducing this adjustment in the autonomy variable. We also construct alternative indexes using principal component and multiple correspondence analyses, which are typically used and considered appropriate for index construction from responses to multiple questions (Filmer and Pritchett 1997; Kolenikov and Angeles 2009).

Another important issue is whether some of the variables in (mn_real) are driving the results, or whether leverage is fairly evenly divided across (i) to (vi); if a subset or even just one of these raw variables is driving the results, this would matter for interpretation and policy analysis.[25]

We next consider the index variable (mn_outcome), which captures women's attitudes towards spousal violence. The six questions on attitudes to spousal violence mimic similar questions in NFHS2 (see Table 1 in Appendix A), specifically whether women perceive a husband beating his wife to be justified if: (i) he suspects of her of being unfaithful; (ii) her natal family does not give the expected jewellery, money or other things; (iii) she shows disrespect for him; (iv) she leaves the home without telling him; (v) she neglects the children; and (vi) if she doesn't cook food properly.

The thematic proximity to NFHS presents an opportunity to evaluate the external validity of Jensen and Oster's variable values. An important concern arises with the second attitude variable, which reports very different values to the same variable in NFHS data, especially in Tamil Nadu. As illustrated in Figure 2 in Appendix B, Tamil Nadu is exceptional for this variable also when compared to other SARI states. While the SARI estimate for 2001 suggests that about 80 per cent of women in Tamil Nadu approve of spousal beatings if the wife's natal family does not give expected jewellery, money or other things, the corresponding estimate for the same variable in NFHS2 is 3.1 per cent.[26]

To check whether this affects the overall conclusion, we explore the associations of the individual components of each index, and introduce alternative outcome variable indexes based on those that do not duplicate others, or can be considered unreliable. Using Jensen and Oster's benchmark specification (their Table IV, Column 1), Table 2 in Appendix A shows that three of the six attitude variables do not respond to the introduction of cable, one responds strongly, one weakly and one in-between.[27]

It transpires that the attitude variable with most leverage – (ii) the natal family not giving the expected jewellery, etc. – is the one afflicted by the serious external validity concerns reflected in the high proportion of respondents perceiving wife beating as acceptable if her family does not deliver sufficient dowry, especially in the Tamil Nadu sample. When we exclude this variable from the index, as constructed by Jensen and Oster, the coefficient of

[25] Notice that Jensen and Oster (2007) present results from a similar exercise in their appendix Table 1.
[26] This question is omitted in NFHS3, so the equivalent column in the third panel of Figure 2 in Appendix B refers to another variable (beating is acceptable if the wife argues with her husband). Of course, the NFHS data have their own problems – see, for example, Kishore and Gupta (2009), Schoumaker (2009) and Schatz and Williams (2011).
[27] Jensen and Oster (2007), appendix Table 1 reports similar results; according to Emily Oster, the small differences reported there are likely due to minor differences in the estimation specification (personal communication).

the adjusted tolerance measure becomes smaller in size and only marginally significant, thus raising doubts about the robustness of one of Jensen and Oster's main findings.[28] A further observation can be made about the tolerance for beatings variables that may be most likely to respond to the introduction of cable. It is likely that the 'bad cook' variable is easier to change than, say, the 'wife being unfaithful'.[29] We add similar reflections on the autonomy variables and also return to this issue in the discussion of spillover effects below.

Applying a similar logic to the autonomy variable, where positive coefficients reflect greater autonomy, we find that four of the six component questions are positively and significantly associated with the introduction of cable, while two are not. As with the attitudes to violence index, disaggregation furnishes more subtle insights about the particular aspects of female autonomy that the introduction of cable affects. Apart from having the advantage of being a (reported) outcome rather than attitude based, the autonomy index is not vulnerable to the double counting of the 'visit' variables (iii and vi); the autonomy index with only five components remains significant at the 1 per cent level.

Nevertheless, it is somewhat odd that the autonomy variable iii – 'whether the woman will stay with friends of family' – is unaffected while variable vi – 'needs permission to visit family or friends' – responds very strongly to the arrival of cable. It is also noteworthy that mobility 'norms' appear to be more amenable to change than decisions about own healthcare.[30]

Which variables are likely to be easy or hard to change? Basu and Koolwal (2005) propose three types of indicators of female empowerment. In ascending order of difficulty to change, because they suggest greater degrees of ability to 'freely make choices' (p.17), these are: socio-economic variables; variables reflecting women's instrumental value to their family and children's well-being (variables that may not challenge norms but which enhance a woman's 'responsibility' for family well-being); and variables reflecting a woman's capability for selfish decision making, or her ability to be 'self-indulgent'.

Using variables from NFHS2, Basu and Koolwal categorise contributions to earnings, deciding what to cook, deciding on major purchases and permission to visit market as pertinent to responsibility, while self-indulgent empowerment is related to not requiring permission to visit relatives, deciding to obtain own healthcare, being allowed to set aside money and not tolerating being beaten under any circumstances.

While these are clearly not the final words on this matter – for example, one might dispute the categorisations of female labour force participation and having a say in major household purchases – four prime candidates for variables in the SARI data which might be harder to change would be: (i) decisions about own healthcare; (ii) permission required to visit family/friends (outside the village); (iii) having money for own use; and (iv) – somewhat less clear-cut – having a say in the purchase of major household items. We find no effect on

[28] When Tamil Nadu is excluded from the sample none of the 'can_beat' or index of status variables has a significant coefficient on village-has-cable.
[29] We are grateful to an anonymous reviewer for pointing this out to us, although we note that Basu and Koolwal suggest that tolerance for beating in any circumstances indicates a lack of what they describe as 'self-indulgent' empowerment (2004, p. 23).
[30] Notice also in Figure 2 and in spite of the strong increase in the access to TV, the very formidable 'backlash' in the tolerance of beatings for women's infidelity between NFHS2 and 3 in Bihar, Tamil Nadu, Goa and Haryana.

own healthcare decisions, while (ii), (iii) and (iv) all show positive effects. Seen through a feminist lens, these are not minor distinctions.

It is clear, moreover, that simple averaging or counting of dichotomous or ordinal variables does not exhaust the possibilities for constructing quantitative variables from these qualitative questions. With the results reported later in Table 2, we therefore use principal component and multiple correspondence analyses (PCA and MCA) to compute outcome variables, since these techniques plausibly reflect the implicit latent variables (Filmer and Pritchett 2001; Kolenikov and Angeles 2009; Le Roux and Rouanet 2010).

Using the same component questions in index construction as Jensen and Oster, both PCA and MCA indexes have the expected sizes and signs. As with Jensen and Oster's indexes, the size and significance for the PCA and MCA indexes of the coefficients on 'vill_has_cable' are affected in absolute size and in statistical significance by changing the component variables.

The results for the tolerance of violence measure echo the above in the sense that the adjusted measure (omitting question (ii) on money or jewellery) is only marginally significant, at 10 per cent. For the autonomy measure, dropping the 'whether woman will visit or stay with family or friends' component increases the coefficient size and leaves the significance of the coefficient on 'vill_has_cable' intact.

For both the PCA and MCA indexes, the effects of dropping the 'will visit' component increases the absolute size of the coefficient of interest (vill_has_cable) without affecting the statistical significance, but the reverse is true when the 'permission to visit' variable is dropped (i.e. both absolute size and statistical significance are reduced, and in the case of the MCA index, dropping 'permission' gives a coefficient on 'vill-has-cable' that is not statistically significant – $p>0.10$). This indicates that the results of the Jensen and Oster specification are sensitive to the way, in particular, the tolerance index was constructed.[31]

4.2 Gender and TV viewing habits

Jensen and Oster devote much space to their overall narrative centring on the progressive and transformative impact of cable TV on attitudes and behaviours of women. While Jensen and Oster (2007) pay some attention to the TV viewing habits of women and men and to how a closer scrutiny of such habits may help us understand the mechanisms through which social change may manifest itself, their 2009 paper pays very limited attention to them. Gender-based viewing habits are particularly important, as they directly relate to the plausibility of the causal impact Jensen and Oster claim to identify.

The theoretical literature on media effects reviewed in Section 2 postulates many potential influences of media, and numerous ways in which media may impact on attitudes and behaviour. In the remainder of the paper we attempt to close in on these mechanisms[32] and thus the underlying theory of change; we start this in Figure 2 in Appendix B, which

[31] Similar results are obtained when we take a structural equation modelling approach to this multiple indicator multiple causes (MIMIC) model.
[32] Doing this includes elements of both statistical and scientific replication. Jensen and Oster (2007) explore the effects of watching TV by instrumenting the measure of watching with cable access (appendix 2, Table 3), but do note differences by ownership of TV.

illustrates that women's and men's viewing habits appear to be quite similar. Figure 2 also demonstrates the sharp contrast in viewing habits between villages with and without cable for members of households without their own TV. Put differently, viewing in villages with cable TV appears to have a strong social dimension for people in households without TVs, since more people watch TV outside their home and thus probably in the company of others. This may help us to better understand why and under what conditions access to cable TV – whether through TV viewing itself or in other and less obvious ways – may trigger the transformative effects that Jensen and Oster report. This also opens for the possibility that it is not cable TV per se that causes the reported effects, but the social context in which such viewing takes place.

The social context of viewing is acknowledged to be important in the literature on media effects discussed in Section 2 (and noted by Jensen and Oster 2007, p.28). It is also possible that the arrival of cable in a village stimulates collective viewing, although it could be that its novelty value wears off quickly.

We use the SARI data to explore, in some more detail: who watches TV; whether cable TV leads to more TV viewing by women or men; whether there are different effects on households without TV;[33] and whether such effects appear to be enduring or of a more temporary nature.[34]

For rural India, it is also important for policy to examine the extent to which household or individual attributes (or social identities) are associated with TV viewing habits. This is because caste-based and other restrictions on social interactions are generally thought to be central to the type of transformative potential Jensen and Oster report. The social dimensions of viewing hinted at later in our study may not, for instance, extend to women who are uneducated, or from Scheduled Castes or Scheduled Tribes or other marginalised or socially excluded groups. They study of the effects of such restrictions is therefore submerged by the use of individual fixed effects in Jensen and Oster's specifications.[35]

Table 3 in Appendix A reports the effects of introducing cable TV on TV viewing separately for women and men. The dependent variable is whether individual *i* watches TV at least once a week (as reported by the respondent for themselves and their husbands).[36]In

[33] There is only one household which is reported as changing its ownership of a TV in the three survey years. We presume this is a coding error and that the variable actually reports whether a household has a TV in 2001.

[34] Section 4.4 of Jensen and Oster (2007) explores TV viewing habits; however, they only report the results of instrumenting reported TV watching by respondents with access to cable (Jensen and Oster (2007) appendix Table 3). Interestingly, the coefficient on the husband's TV watching reported by respondents instrumented with access to cable is larger than the coefficient on self-reported TV watching instrumented in this way (results available from the authors).

[35] The potential of a mixed-level model to study both random and fixed coefficient models (Rabe-Hesketh and Skrondal 2008; Gelman and Hill 2007) is outlined in appendix 1.

[36]Since all information in SARI is based exclusively on interviews with women, there are concerns over the reliability of information on the TV viewing habits of men (husbands). According to SARI respondents, male and female watching are highly correlated:

addition to the 'village has cable' variable, we interact 'household has TV' with 'village has cable' and control separately for whether the household has TV. This specification allows disentangling the effects on viewing of having TV in villages with cable and of not having TV in villages with cable. We also, for reasons given above, add controls for social identity (caste), religion, levels of education and state of residence.

There are interesting contrasts between the determinants of viewing among women and men. Unsurprisingly, households with TV are more likely to watch TV than others. 'Village has cable' has a separate and large effect on TV viewing, but this effect is almost neutralised if the household has TV. What we do detect, as Figure 3 in Appendix B also suggests, is that access to cable at the village level has a strong effect on the viewing of members of households without TV. This could partly, as Table 3 in Appendix A Columns 3 and 4 suggest, reflect a novelty or curiosity effect that may be important for passing verdicts about Jensen and Oster's causal claim. There is also a slight increase in viewing for women from households with TV, while for men, those from households with TV watch less in villages with cable.

Ethnicity has no effect on the viewing habits reported by women, while 'Scheduled Caste' and to a lesser extent 'Backward Caste' men watch less TV than others. Hindu women (and men to a lesser extent) watch more TV than others, while Muslim men watch less. Another key insight is that women with above secondary education (the reference group) watch far more TV than others: the difference is most pronounced when compared to women with no education. The education effects are weaker for men, but like the women, men with no education are much less likely to watch. The regional effects are more pronounced for men, with a particularly striking contrast between men and women in Delhi, which is the only (and entirely) urban context in this data set.

As indicated in Figure 3 in Appendix B, the fact that most of the changes in TV viewing habits when there is cable TV at village level appear to occur in households without TV is surprising; this will inform our analysis and reasoning about changes in behaviour and norms. Table 4 in Appendix A digs deeper and distinguishes between households where the wife has no or some education in villages with and without cable. The contrast is again startling.[37]

Correlations among TV watching and whether household has its own TV				
	watches_tv	**husb_watches_tv**	**vill_has_cable**	**has_tv**
watches_tv	1.00			
husb_watches_tv	0.72 (0.00)	1.00		
vill_has_cable	0.40 (0.00)	0.39 (0.00)	1.00	
has_tv	0.64 (0.00)	0.54 (0.00)	0.30 (0.00)	1.00
Source: authors' calculations				

[37] In villages which got cable in 2002 or 2003, the proportion of women with no education who watch TV in households without a TV rises nearly 50 per cent to 0.436, compared to 0.294 for those in villages that already had cable TV in 2001, as shown in the following table.

Association of TV watching with year of access to cable and TV ownership

Woman's education	Village has cable 2001	Village gets cable 2002 or 2003	Village never has cable

4.3 Towards a respecification of Jensen and Oster's main model

Our observations so far suggest that it is helpful, to be able to comprehend the mechanisms through which the reported impacts of cable TV may materialise, to distinguish between households with and without TV in villages with cable. Figure 4 in Appendix B portrays changes over time in the tolerance of beatings, female autonomy and son preference in villages with and without cable, in households with and without TV and by TV viewing habits.

Our main intuition from Jensen and Oster's overall narrative would be that tolerance of beatings, autonomy and son preference consistently move in the right direction among women in villages with cable. However, careful disaggregation shows that this is often not the case.

Considering *tolerance of beatings* first, the most striking observation in Figure 4 in Appendix B is the high initial values of tolerance among women from households without TV in villages with and without cable. Indeed, the highest tolerance of spousal beatings is observed for TV-watching women in households without TV in villages with cable. Over time, it is also evident that the most pronounced reduction in tolerance for beatings is observed among TV-watching women from households without TV in villages without cable, followed by women from similar households in villages with cable. Notice that where we would *a priori* expect the main action to be, namely among TV-watching women from households with TV in villages with cable, tolerance of spousal beatings does not seem to alter throughout the SARI years.

If we shift attention to the other indicators, there is little if any observable movement among TV-watching women from households with TV in villages with cable. While tolerance of beatings declines among TV-watching women in households without TV in villages with cable, *son preference* appears to increase. A clear strengthening of son preference over time is also observed among TV- and non-TV-watching women in households without TV in villages without cable.

These observations suggest that the uniform progress Jensen and Oster point towards, when more carefully disaggregated, is imbued with contradictions and movements in different directions for each of the indicators of interest. These are not revealed by the fixed-effects estimation approach adopted by Jensen and Oster.

4.4 Spillover effects

We next turn to estimating social externalities or spillover effects, making comparisons of women in households without TV in villages with and without cable. A significant coefficient on the 'village-has-cable' variable for women from households without their own TVs suggests the presence of spillovers. Before reporting the results, however, we revisit the

	No TV	Has TV	No TV	Has TV	No TV	Has TV
No education	.294	.816	.436	.873	.061	.729
At least primary education	.534	.946	.618	.947	.249	.924

Source: Authors' calculations from SARI data.

issue of the tolerance and autonomy variables that may be more or less likely to change in response to the arrival of cable.

As noted previously, the 'bad cook' variable appears a more plausible candidate for a change in tolerance levels than the 'dowry' variable. For the autonomy variables, Basu and Koolwal (2005) distinguish between variables that enable 'women to take selfish charge of things from measures that might be more ambiguous' (p.18).

The 'self-indulgent' variables are considered to be harder to change, by virtue of being more contested. The latter, on the other hand, may simply enable women to become more effective wives and carers and therefore encounter less resistance. These are thus attuned to the fulfilment of gender roles, while the 'self-indulgent' variables relate to greater and more direct control over own body and over certain forms of mobility.

The four hard-to-change autonomy variables proposed above are: (i) decisions about own healthcare (ii) permission required to visit family/friends (outside the village) (iii) having money for own use and, somewhat less clear-cut, (iv) having a say in the purchase of major household items.

What does the study of spillovers add to these considerations? Our expectation and hypothesis would be that stronger spillover effects would be observed for changes that are less likely to be contested.

We now repeat the regressions for each of the tolerance and autonomy variables in Table 2 in Appendix A with 'village has cable' coefficients. The results are reported in Table 5 in Appendix A starting with the *tolerance* variables; we register notable spillover effects for the 'bad cook' and 'dowry' variables, but not the neglect of children or in the aggregated tolerance index.[38]

For the *autonomy* variables, we register spillovers in the aggregate index and for two of the four significant coefficients, namely for women having a say in the purchase of major household items and for whether permission is required to visit family/friends outside the village. There are no direct or indirect (spillover) effects on decisions about own healthcare.

The spillovers for the autonomy variables appear for two of the remaining four hard-to-change variables. In total, thus, we observed spillovers for three of the five 'hard to change' tolerance and autonomy variables. This fuels concerns about potential and time-variant confounding factors, rather than the arrival of cable TV, as the underlying driver of social change.

We next introduce a simple modification of Jensen and Oster's model by including a variable interacting the education status of women with village-has-cable to distinguish the effects on illiterate women. We estimate this respecified model for a variety of sub-groups and for the indicators of female empowerment, son preference and fertility. It is important to note that these, our main respecifications, are close but not identical to those reported in appendix Table 2 in Jensen and Oster (2007). In spite of the crucial implications for policy,

[38] In fact, women in households without TV in villages with cable are more likely than all other groups to accept beating for neglect of children.

these results are not mentioned in Jensen and Oster or in the web appendix to Jensen and Oster. The full results are reported in Table 6 in Appendix A.[39]

We are now in a better position to interpret the results of cable TV on tolerance of spousal beatings, on female autonomy and on the demographic variables of interest – son preference and fertility. We start with the results for the full sample, which are reported in Column 1. Recall that for the tolerance of spousal beating, negative signs reflect reductions in tolerance while positive coefficients in the autonomy regression reflect an increase in female autonomy.

For women without education, the 'empowering' effect of residing in a village with cable, in terms of reducing the tolerance of spousal beatings, is close to negligible (-0.32 + 0.26) and on closer inspection, confirmed to be zero.[40] The results on female autonomy are similar and confirm that the empowering effects of cable are limited to women with education. This significantly modifies judgments (and policy implications) about the transformative potential of introducing cable TV, since the main transformative impacts that Jensen and Oster report do not extend to the majority of the women (53.8 per cent of who are illiterate) in the SARI panel data set.[41]

We next consider the impacts for women from households with and without TV, by viewing habits, by social identity and by age group. These results should be seen as suggestive rather than conclusive, and are quite different for tolerance of spousal beatings and autonomy.

Starting with the tolerance variable, the difference in the impacts of introducing cable for households with and without TV appears to be quite small (Columns 2 and 3). In line with the above discussion, this is indicative of attitudinal externalities reflecting the social context in which women from households without TV viewed TV (i.e. in the homes of others or in public places). Recall that we found no spillovers for the tolerance index in Table 5 in Appendix A: here we do observe spillovers for women with some education. Note also that the effect of no education, in this case, does not extend to women from households with TV.

[39] For all specifications reported in Table 6 we use Jensen and Oster's time trends and other controls, individual fixed effects and standard errors clustered at the village level. These estimations were completed prior to discovering Jensen and Oster's 2007 paper.

[40] Another way to see these results is to run the regression with an interaction between a dummy for whether the woman has education (primary or above) with 'vill_has cable' rather than the dummy for zero education. In this case, the coefficient on 'vill_has_cable' turns insignificant, and the entire cable effect is captured by the interaction of education with 'vill_has_cable'.

[41] Jensen and Oster (2007, p.19) state: 'The results show consistent evidence of larger effects among the better educated. The effects also appear to be somewhat larger among older people, although this effect is less consistent. In general, however, these results are difficult to interpret. More educated people could be more responsive because they are better at processing information (Grossman 1972), but this may also reflect the fact that they are much more likely to watch television. The results on age also have a number of interpretations: older women may have more ability to assert themselves once they come to expect more autonomy, or they may have only their husbands to convince, while younger women may have both a husband and a mother-in-law. In general without knowing something more about what actually goes on within the household, these breakdowns are interesting but difficult to draw strong conclusions from.'

There is no real impact on women who do not watch TV (Column 4), while the net impact of zero on women without education for the whole population extends to women in villages with cable who watch TV regularly (Column 5). There are no discernible changes in the tolerance of spousal beatings among women from Scheduled Caste or Scheduled Tribe backgrounds in villages with cable (Column 6). This might reflect the type of social restrictions alluded to before. Finally, the effect is statistically stronger (although the coefficient is smaller) for women below the age of 35. However, for women above 35, having an education or not makes no difference in the progressive effect of cable.

Turning to the autonomy variable, the results are, in some respects, strikingly different in spite of the aggregate results (Column 1) which, as noted, came across as quite similar to those for the tolerance index. The 'no education' effect is strongly significant for women from households with TV and for women who watch TV in villages with cable (Columns 3 and 5). For uneducated women, there are no autonomy gains from cable TV introduction.

In contrast to tolerance results, however, autonomy benefits do extend to women from Scheduled Caste and Scheduled Tribe backgrounds, and having no education is not a handicap with this effect. Despite this, the effect is larger on women from other castes, provided that they have some education.

Finally and notably, autonomy gains are strongly age dependent. Gains accruing to women below the age of 35 are almost negligible, while the main progress is made by women older than 35. As with the tolerance result, there is a gain, albeit much smaller, for women without education.

Turning to the son preference variable, we report results using the same specifications as above. The aggregate analysis which Jensen and Oster report and we reproduce in Column 1 suggests a reduction in son preference within villages with cable. The number of observations is in this case much smaller (N=1699). From Column 2 it appears that the main effect occurs for women from households without TV, but given the sample sizes it is necessary to tread carefully. There are no significant coefficients until we reach the non-scheduled caste/tribe column, where the negative coefficient reappears. To check whether this coefficient is driven by women's TV viewing, we disaggregate non- scheduled caste/tribe women into those who do and do not watch TV. The two groups are very similar in size, but there are no significant coefficients among those who do not watch TV. For TV viewers we find that the 'village has cable' coefficient is insignificant, while the interaction term with having no education is large, positive and statistically significant at the 5 per cent level. It thus appears that another of Jensen and Oster's main results – on son preference – may be unrelated to TV viewing within villages with cable.

With regard to fertility, the standard specification encounters a colinearity problem. We therefore estimate the model separately for women with some and no education. In contrast to all other indicators, the 'village has cable' variable is now significant and suggestive of fertility decline only for women without education. In the panel regression of 'is_pregnant', the coefficient on 'village_has_cable' is -0.039 (t=-1.76, p<= 0.081, n=4071)) for women without education and -0.031 (t = -1.30 p<= 0.19, n=2943) for women with some education respectively.[42]

[42] Full results are available from the authors.

4.5 DISE data

For the DISE data, code for all processing and variable construction from the original (official) data has been provided by Jensen and Oster, along with their original data. These data are the age and class enrolment of all schools in 19 randomly selected blocks of five districts of Tamil Nadu,[43] which were selected using NFHS2 as having low cable penetration at the time of the study. The raw data supplied to us cover 2002-2007 (not 2001, as reported by Jensen and Oster[44]) and are used to estimate a variant of equation (1) with different units of observation, estimation method and covariate specification.

The outcome variables are absolute numbers rather than rates of enrolment with the assumption (which is tested) that population did not vary meaningfully during the data period. The unit of observation is the village; the outcome variable is the log of enrolment for 6-10 and 11-14 age groups; and for the cohort aged 6-7 in 2002. The controls are whether more than half of the schools in the village have electricity, village school-age population, and distance from the nearest town.[45]

4.6 Replication

Jensen and Oster provided code that produces all the results they reported from the DISE data. A minor partial exception is their Figure VII, which was produced by code listed under Figure 3, with the results copied and pasted into MS Excel where the graph was produced. While we should have been able to infer how Figure VII had been produced we did not, and our versions of Jensen and Oster's Figure VII are produced in Stata, and differed quite significantly from Jensen and Oster's.

Using the Jensen and Oster data set[46] we have been independently able to produce all the published results from the Jensen and Oster data constructions, including their Figure VII, once Jensen and Oster pointed out to us their method.[47]

Using our data constructions, our replication of their Figure VII, which in the original demonstrates a rise in enrolment of the fixed cohort over time, shows a more plausible decline in enrolment of the fixed cohort. This difference arises in part because of minor errors in Jensen and Oster's code, which are described in appendix 2, and an important difference in interpretation of the appropriate treatment of villages which report zero enrolment of the fixed cohort.

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[43] Since each district has at least one block, districts are strata; no weighting is used to account for different populations of the chosen blocks.
[44] A final data set provided by Jensen and Oster contains cases from 2001, but it appears that the raw data from this year are no longer available. Since we found errors in Jensen and Oster's code computing enrolment data, we do not use their final DISE estimation data.
[45] These three variables are contained in the DISE data. Variables such as distance from nearest town should be constant over years, but are not. Electricity is also not always constant over years and is set to present if it is reported to be present in more years than not. Population is for 2005 only, although it is also available in the data for 2007; it too can vary quite significantly between these two years. Attempts to merge these village data with village data from the census, which would provide more contextual variables, were reported to have been unsuccessful because of difficulties in matching names of villages (p.1087). We can confirm this difficulty.
[46] By this we mean the data set produced using the original Jensen and Oster code sent to us.
[47] Our code and these results are available on request.

With regard to the latter, Jensen and Oster recode the observations for villages which report zero enrolment in any year to missing, while we retain them as zeros. The coding errors[48] occur in the constructing of their outcome variables (enrolment) for the fixed cohort and for the 11-14 age group. Recalculating the variables of interest and re-estimating the descriptive statistics and the model proposed by Jensen and Oster and reported in their Table VI, we find that the descriptive statistics of the affected variables differ from their counterparts in Jensen and Oster Table II, Panel C Columns 1-3, and the estimated coefficients of equation 1 also differ in equations involving the variables affected by this error.[49] Our estimates include both those due to the correction of the coding errors and the different treatment of zeros mentioned above.[50] [51] The different treatment of zero enrolment makes the main difference to the results.

We now elaborate on and motivate the alternative assumptions about the meaning of the value of zero enrolment in villages in the fixed cohort. The zero enrolments mostly occur at education levels 4 and 5 in villages which have only one school. Officially, children in India start school at age 6-7 in level 1, and primary school ends at level 5 (10-11). So if a school goes up to level 5, it might be expected that the children who were in that school in levels 1-3 would also be in the same school in levels 4 and 5. However, most primary schools only have levels 1-5, while other schools continue to level 8. The large number of children who start younger than six may well move on to other schools, which may mean having to go to a school in another village at age 10 if their native village lacks a school beyond level 5. It could also be that the quality of schooling on offer in the village of residence is worse than that on offer nearby, especially if the former offers classes only up to level 5. In such as case, it would not be surprising if pupils – especially those showing academic promise – moved to these 'better' schools during their later primary school years.

Under the above assumption we would find most members of the fixed cohort still in school in villages in the 19 sample blocks even if they are not in schools in the villages where they started their schooling. Children who moved to 'better' schools in villages outside the sample blocks would be balanced by children moving into the 'better' schools in the sample blocks. Hence zero enrolment in classes in poorer quality schools –especially those which do not offer classes beyond level 5 - would indeed be zero, not missing (since the children would be enrolled in better quality schools nearby).

We next test the assumption that villages which have zero enrolment in any primary level year are likely to have schools of a worse quality and are less likely to have an English-medium school than those with non-zero enrolment in all the primary years. We show that

[48] The errors are now acknowledged at http://faculty.chicagobooth.edu/emily.oster/papers/update.pdf
[49] We discuss this issue in some detail later in paper.
[50] Note that in Jensen and Oster Figure VII the treatment of zeros results in loss of observations since they exclude observations for all years of villages that have zero enrolment in any year; for the estimations in Jensen and Oster Table VI, all non-zero observations are included – i.e. including non-zero enrolment observations from those villages, all of whose observations are excluded from the calculations reported in Jensen and Oster Figure VII.
[51] Jensen and Oster also recode to missing enrolment aggregates which jump from zero to 10 or more in sequential years; we see no particular reason to adjust this particular characteristic in the data. It is also noteworthy that, although nearly 80 per cent of villages have a constant number of schools, this can vary over the panel period quite widely, casting doubt on the integrity of the data (if the number of schools in a village varies widely between years, the data may be unreliable).

plausible measures of school quality of the latter (which remain in the Jensen and Oster sample) are significantly higher than those with zero enrolments.[52]

Table 7 in Appendix A reports the first two principal components from a set of school quality variables by sample: the Jensen and Oster sample is villages with five years of non-zero enrolments, while our figure comprises villages with five years of non-missing observations that are not in the Jensen and Oster sample. It is clear that the mean quality scores are quite different between these two groups (larger means better quality for component 1, and smaller means better for component 2). The differences, at $p<0.001$, are significant. Table 7 also reports the likelihood of villages having any school with English as the primary medium of instruction; clearly those villages left in the Jensen and Oster sample are much more likely to have an English medium school than those that Jensen and Oster have dropped. The difference is also significant at $p< 0.0000$.

If our conjecture is correct, the Jensen and Oster schools may show increased enrolment, especially in school levels 4 and 5, even if overall enrolment of this cohort is declining. Meanwhile, those in our sample (all schools in all villages with non-missing enrolment) will show the expected (declining) trajectory of enrolment. Figures 5a and 5b in Appendix B report our replications of Jensen and Oster's Figure VII using our data construction (the line with square markers). As a robustness check we include the cohort that was 6-7 in 2003-4, and also a line in each graph for enrolment in villages which already had cable in 2002. Note that the y-axis is mean total enrolment per village.

Jensen and Oster rationalise their construction (denoting zero village enrolment as missing) by assuming that the children who had been in schools which report zero enrolment had '...moved to another school. It seemed inappropriate to us to include these as '0s'. So, as an alternative, we assumed 0s were missing.' (email from Oster to Palmer-Jones, 13/07/2012 21:17).

Having recoded the zeros to missing, Jensen and Oster then dropped all observations for villages that have a missing value of enrolment in any of the panel years to create what they call a balanced panel. It is this later move that drops villages from their Figure VII for all years even though the missing values/zeros mainly occur in the later levels. We, on the other hand, include these villages, provided they have a non-missing entry (including zeros) in all five years of the panel. Clearly, Figure 5a, shows no visual evidence, for either cohort of significance, of increases in enrolment immediately following the year in which villages get cable (see the lines with square markers in relation to the vertical line denoting access to cable in both figures).

For the version of equation (1), estimated with our construction of variables from the DISE data (Table 8 in Appendix A), the results are now weaker.[53] In Panel A, our results with the same village level covariates as Jensen and Oster are fairly close to theirs (Jensen and Oster Table VI), but the coefficients are smaller. This finding is due to correcting the

[52] Note that the treatment of villages with zero enrolments mainly affects the differences between our Figure 5a and Figure 5b in Appendix B and Jensen and Oster's Figure VII. Jensen and Oster include these villages in the estimations reported in their Table VI.

[53] When comparing Table 8 Columns 1 and 2 in Appendix A with Jensen and Oster Table VI Columns 1 and 2.

programming errors in Jensen and Oster, since the model specifications are the same. However, this model does not control for school quality, which, as we showed above, affects enrolment and is correlated with years of access to cable.

With the addition of variables representing school quality and English medium teaching (IPJ covariates in Table 8 Columns 3 and 4 in Appendix A), the coefficients on 'years cable' are no longer significant. The school quality variables in the specifications with additional covariates are the first principal component from the principal components analysis of school and school buildings variables given in the DISE data reported in Table 7 in Appendix A, and a dummy variable for whether the village has an English medium school in any of the years of the panel.[54]

For the 6-10 year olds (Table 9, Panel A in Appendix A) the coefficients on years' access to cable are not significant for girls but they are for boys, even with the augmented covariate specification. With Jensen and Oster's covariates, there is no significant upward trend in enrolment for the 11-14 year olds, but there is for boys aged 11-14 when using the additional covariates described above.

Comparing Table 8 (Jensen and Oster's fixed cohort) Panel A, Columns 1 and 2 with 3 and 4, we observe that the coefficient on the 'years of cable' variable is no longer significant once we include controls for school building quality and the presence of an English medium school. Other results not presented here show that it is the 'buildings quality' variable that eliminates the significance of the 'village has cable' variable.

Furthermore, separating out the sample used by Jensen and Oster in their Figure VII shows that the statistically significant increase in enrolment in these villages is nearly offset by the statistically significant decrease in enrolment in the villages that are left out of that sample (results not presented). Hence, pooling all villages with non-missing observations (i.e. including those with zero enrolments) results in a non-significant coefficient on years since access to cable, as the increase in enrolment in better quality schools is offset by the decline in enrolment in poorer quality schools.

In the estimation using trends after 2002 only the coefficient on the fixed cohort 'boys' enrolment' is actually negative (and significant in Column 3 Panel B). While including block-specific trends results in positive but non-significant coefficients for the first fixed cohort, the effects are both positive *and* significant for the second fixed cohort (Columns 7 and 8, Panel C). With control for pre-trends (Panel D), the estimations with the additional covariates for building quality and presence of an English medium school have coefficients on 'years of cable access' which are near to zero and not statistically significant.

Results for the rolling age groups (6-10 and 11-14) are shown in Table 9 in Appendix A. Here, when we do not control for school quality (Columns 1, 2, 5 and 6) we see positive and significant effects of years of access to cable for boys in the 6-10 and 11-14 age groups

[54] These variables are not well described in the data set, but we infer their meaning and coding from the descriptions given at www.dise.in/publications.htm. Categorical variables are included as 0/1 dummies for each category; the classroom and room variables are included as shares of all classrooms/buildings. Jensen and Oster include a 'village [school age] population' variable, but in many cases this variable is zero. This made ethnic composition variables unusable since their inclusion reduced the sample size considerably. Dropping the village population variable and its interaction with year has no meaningful effect on the results.

(Panel A), but no effects when only years after 2002 are included. When the estimation is done with block trends (Panel C), boys aged 6-10 and girls aged 11-14 all show positive and significant effects with both specifications. The results with control for pre-trends (Panel D) are the same as for Panel C. These results are less consistent and much weaker when estimated with the buildings quality variable and the English medium school dummy (Columns 3, 4, 6 and 7) than when estimated using the Jensen and Oster estimation specifications, and with our data construction rather than the Jensen and Oster data construction (Jensen and Oster Table VI).

In summary, we see that the DISE data analysed in these ways do not provide strong evidence in support for the assertion that cable TV increases school enrolment. There is no visual evidence in Figure 5a or 5b in Appendix B to support an argument for an increase in enrolment in the year, or year after, respondents gained access to cable TV. Furthermore, the evidence from the time series estimation (Table 8 and Table 9 in Appendix A) is weaker than in Jensen and Oster, and virtually non-existent when controls for school quality and presence of an English medium school are introduced.

Overall, the evidence from the DISE data is further weakened because of the lack of other village-level covariates, which could have accounted for endogenous placement of cable TV in villages which were more likely to increase enrolment. Jensen and Oster acknowledge this weakness.[55] Covariates which are not available would include the availability of supportive services which the literature discussed in Section 2 suggests are pertinent to pro-social outcomes from exposure in the media to empowering messages.

[55] There are also significant weaknesses in the DISE database. Not only is there the question of the zero values for village school-age population; there are also differences in the number of schools in villages over years, and so on. It might be worth obtaining an updated and more complete version of the DISE database, assessing it for improvements and using it to repeat the analysis. This might also allow linking to the census, as the updated DISE database (2009) has considerably more variables – including geographical coordinates of villages.

5. Conclusion

In this paper we argue that media effects theory and evidence raises doubts about the pro-social effects of unsupported modern media exposure, and gender and development theories and evidence contrast strongly with the implications of the results reported by Jensen and Oster.

We successfully replicated Jensen and Oster's results for the SARI data set. Corrections for variable construction weaknesses and external validity concerns reduced the statistical power of Jensen and Oster's results for tolerance of spousal beatings and to a lesser extent for female autonomy, their two main indicators of female empowerment. We also detected errors and plausible differences in the variable construction from the DISE data set that, once corrected, weakened the causal impact of cable TV on school enrolment. Further doubt can be cast on the DISE results due to the lack of controls for possible confounding village variables that could account for both higher enrolment and the length of time that villagers had had access to cable TV.

Given the existing literature on media and development which has tracked and sought to identify the 'impacts' of pro-social TV programme content on attitudes and behaviour along gender and other dimensions, it is hard to recover a convincing theory of change from Jensen and Oster's aggregated results. Our more extensive use of the SARI data set, which resembles the analysis undertaken by Jensen and Oster (2007) – including a closer scrutiny of male and female viewing habits and the use of simple respecifications of Jensen and Oster's main model to disaggregate by whether women have education or not – confirmed important and contradictory heterogeneity for Jensen and Oster's two main indicators of female empowerment.

The results are also, for the two main variables, suggestive of contradictions in the intensity and possible nature of the externalities accruing to women from households without TV. This opens the door to suggestions that less straightforward underlying processes (and theories) of social change may be at work.

While decline in tolerance of domestic violence does not extend to women from minority (Scheduled Caste and Scheduled Tribe) backgrounds, the main results are representative of what we find when restricting analysis to women below the age of 35. For autonomy, in contrast, there are positive effects for Scheduled Caste and Scheduled Tribe women, with the main effects being observed for women above the age of 35. A key finding is that the positive effect of cable TV on female empowerment is restricted to women with some education. We also suggest that the results on son preference and fertility are fragile and that the changes in fertility rates, in contrast to the other findings, mainly occur among women with no education.

The replication was greatly facilitated by ready access to final data and code for analysis, although a proper assessment of Jensen and Oster's contribution would have been more complete had the SARI survey instruments and raw data been available, and should probably have included the results reported in Jensen and Oster (2007). Our replication benefitted from a constructive interaction with the original authors. Access to the raw DISE data enabled us to identify a programming error and plausible differences in variable construction after finding that our descriptive statistics did not match Jensen and Oster's.

Using comparable data (NFHS2 and NFHS3) we were unable to strongly support the external (and, to some extent, internal) validity of the study.

A more extended review of the relevant literature – particularly going beyond correlational studies to look at mixed and qualitative methods research – suggested that the lack of a strongly articulated theory of change underlying the apparent empirical results warranted careful scientific replication exploring both alternative mechanisms and concepts of empowerment and alternative empirical models, and hence further exploration of the empirical data. One obvious line of research would be a more extensive analysis of the Indian NFHS data.[56]

Our replication results indicate a nuanced and qualified understanding, which considerably qualifies the conclusion of an apparent beneficial impact of the introduction of cable TV, restricting it largely to an already relatively privileged group (those with some education). A significant result of the replication would appear to be that there needs to be further research to tease out whether, for whom, and by what pathways and mechanisms, general TV programming (and pro-social programming) may have beneficent impacts, if any.

[56] See Kishor and Gupta 2009 for an example of this research direction.

Appendix A: Tables

Table 1 Variables used to construct spousal violence acceptability and female autonomy indexes

Survey variables	*NFHS2 questions*	*NFHS3 questions*	*Variable type*	*SARI codes*	*Recodes*
(i) healthcare decisions	s511b	v743a			
(ii) purchase of major household items	s511c	v743b	Categorical	1=self 2=husband 3=self+husband 4=others 5=self+others	1 if 1 or 3 or 5, 0 otherwise
(iii) decision on whether to visit or stay with family or friends	s5111d	v743d			
(iv) has money to spend on her own/allowed to set money aside	s513	v743f	Binary (0/1)	1 = Yes	
(v) needs permission to visit the market	s512a	s824a	Ordinal: 1 alone 2 with other 3 not at all	0 not need 1 need permission 2 not allowed to go	2 not need 1 need permission 0 not allowed to go
(vi) needs permission to visit relatives/friends	s512b	(s824c)			
	(s513)	w124	Binary (0,1)	0 = No 1 = Yes	
tolerance of spousal beatings: six variables	s514a-f	v744a-e s829f,g			
son preference for next child	v627-9[57]	v627-9	Categorical	1=boy 2=girl 3=not matter 4=other	1 if boy 0 otherwise

Note: In many cases there are equivalent questions in NFHS2; several questions were dropped or changed between NFHS2 and NFHS3, including one on the acceptability of beating if relatives do not provide money/dowry – see Figure 2 in Appendix B). NFHS2 and 3 questions on autonomy variables are couched in terms of whether respondent has 'final say'; there is no duplicate 'permission to visit' variable, although question s824c in the NFHS3 reports 'allowed to go to places outside this village'.

[57] NFHS2 and 3 have questions on ideal number of boys/girls/either (v627-9), not on desired sex of next child.

Table 2 Coefficient of 'village has cable' for components of composite outcome variables representing women's status and autonomy

Dependent variable (attitudes to beating)	Coefficient on village has cable	Dependent variable (autonomy)	Coefficient on village has cable
(1)	(2)	(3)	(4)
(i) he suspects her of being unfaithful	0.013 (0.016)	(i) own healthcare decisions	0.019 (0.023)
(ii) her natal family does not give money or jewellery	-0.054*** (0.020)	(ii) purchase of major household items	0.080*** (0.023)
(iii) she shows disrespect	-0.028 (0.023)	(iii) decision on whether to visit or stay with family or friends	-0.025 (0.020)
(iv) she leaves home without telling him	0.016 (0.014)	(iv) has money to spend on her own	0.027** (0.013)
(v) she neglects the children	-0.036* (0.018)	(v) needs permission to go to market	0.064*** (0.013)
(vi) she cooks badly	-0.071** (0.031)	(vi) needs permission to visit family or friends	0.041*** (0.013)
Tolerance measure (mn_outcome – 6 variables)	-0.161** (0.073)	Autonomy measure (mn_real – 6 variables)	0.026*** (0.006)
Adjusted tolerance measure (excludes 'her natal family does not give money or jewellery')	-0.107* (0.062)	Adjusted autonomy measure (5 variables – excludes (iii))	0.033*** (0.007)
		Adjusted autonomy measure (5 variables – excludes (vi))	0.027*** (0.008)
Tolerance measure normalised 0,1	0.027** (0.012)		
Adjusted tolerance measure (normalised)	-0.021* (0.012)	Autonomy variable is already normalized on 0,1)	
Tolerance measure (PCA 6 variables)[3]	-0.147** (0.067)	Autonomy measure (PCA – 6 variables)[3]	0.131*** (0.038)
Tolerance measure (MCA 6 variables)[4]	0.091** (0.041)	Autonomy measure (MCA – 6 variables)[4]	-0.054*** (0.026)
Tolerance measure (PCA 5 variables[3] excludes 'her natal family does not give money or jewellery')	-0.113* (0.0623)	Autonomy measure (PCA – 5 variables)[3]- excludes (iii)	0.180*** (0.0413)
		excludes (vi)	0.086** (0.041)
Tolerance measure (MCA 5 variables- excludes 'her natal family does not give money or jewellery')[4]	0.0715* (0.0396)	Autonomy measure (MCA – 5 variables)[4]– excludes (iii)	-0.094*** (0.028)
		Excludes (vi)	-0.026 (0.031)

Notes: 1. * p<0.1, ** p<0.05, *** p<0.01

2. Estimations replicate Jensen and Oster Table IV Column (1) and (3), with the exception of different dependent variable; estimations include controls as in Jensen and Oster. These estimations employed the xtreg command in Stata on dichotomous variables.

3. Outcomes estimates by PCA: positive sign indicates increase in approval for beating.

4. Outcomes estimated by MCA: a negative sign indicates an increase in approval for beating.

Table 3 TV viewing habits, by gender (SARI data)

	Watched TV in previous week (Pooled probit/margins[1])				test (1)= (2)		test (3)= (4)	
	respondent	husband	respondent	husband	Chi2	P value	Chi2	P value
	(1)	(2)	(3	(4)	(5)	(6)	(7)	(8)
village has cable	0.305***	0.250***			3.288	0.070		
	(0.047)	(0.034)						
village had cable 2001			0.142***	0.167***			0.565	0.452
			(0.053)	(0.037)				
village got cable 2002			0.458***	0.361***			17.900	0.000
			(0.046)	(0.042)				
village got cable 2003			0.282***	0.264***			0.177	0.674
			(0.049)	(0.035)				
household has TV	0.765***	0.635***	0.730***	0.612***	20.290	0.000	15.921	0.000
	(0.049)	(0.041)	(0.048)	(0.042)				
hh has TV * village has cable	-0.275***	-0.369***	-0.159***	-0.295***	4.688	0.030	8.741	0.003
	(0.056)	(0.049)	(0.057)	(0.052)				
log income per capita	0.017***	0.008***	0.020***	0.010***	6.674	0.010	9.754	0.002
	(0.003)	(0.003)	(0.003)	(0.003)				
Scheduled Tribe	0.148	0.010	0.052	-0.062	3.471	0.063	2.677	0.102
	(0.158)	(0.102)	(0.087)	(0.054)				
Scheduled Caste	-0.050	-0.121***	-0.032	-0.108***	5.046	0.025	5.769	0.016
	(0.041)	(0.036)	(0.040)	(0.036)				
other backward caste	-0.026	-0.069**	-0.038	-0.076**	2.819	0.093	2.397	0.122
	(0.036)	(0.031)	(0.035)	(0.031)				
no education	-0.256***	-0.245***	-0.265***	-0.252***	0.143	0.705	0.214	0.644
	(0.029)	(0.026)	(0.026)	(0.025)				
primary	-0.109***	-0.099***	-0.109***	-0.097***	0.081	0.776	0.115	0.734
	(0.035)	(0.028)	(0.034)	(0.029)				
secondary and above	0.115*	-0.032	0.110*	-0.035	6.261	0.012	6.510	0.011
	(0.062)	(0.042)	(0.061)	(0.043)				
Muslim	-0.129***	-0.216***	-0.139***	-0.220***	9.408	0.002	10.632	0.001
	(0.046)	(0.037)	(0.032)	(0.030)				
Christian	-0.116***	-0.021	-0.071	-0.001	2.490	0.115	1.339	0.247
	(0.043)	(0.051)	(0.050)	(0.047)				
rural	-0.029	0.125***	-0.022	0.136***	18.790	0.000	19.333	0.000
	(0.050)	(0.048)	(0.049)	(0.048)				
Tamil Nadu	0.311***	0.476***	0.412***	0.520***	22.515	0.000	11.376	0.001
	(0.063)	(0.051)	(0.047)	(0.032)				
Haryana	0.245***	0.351***	0.260***	0.359***	13.390	0.000	12.143	0.001
	(0.049)	(0.042)	(0.040)	(0.034)				
Goa	0.410***	0.468***	0.444***	0.478***	2.089	0.148	0.811	0.368
	(0.075)	(0.053)	(0.058)	(0.045)				
Delhi	0.212***	0.530***	0.334***	0.599***	52.266	0.000	39.667	0.000
	(0.057)	(0.055)	(0.051)	(0.050)				
N	17796		17796					

* p<0.1 ** p<0.05 *** p<0.01; SEs are clustered at the village level. Coefficients are marginal probabilities.

Notes: 1. results are from pooled regression of respondent's and husband's watching of TV

Table 4 Association of TV watching (at least once per week) with access to cable, ownership of TV and gender

	Village has cable		*Village does not have cable*	
Households where wife has some education	**Households with TV**	**Households without TV**	**Households with TV**	**Households without TV**
Women watch TV at least once a week	94.6 % (n=2015)	58.3 % (n=826)	92.8 % (n=724)	27.1 % (n=668)
Households where wife has no education				
Women watch TV at least once a week	83.0 % (n=826)	38.2 % (n=1157)	74.2 % (n=528)	7.3 % (n=2415)

Note: Figures in brackets are the sum of those who do and do not watch TV.
Source: Authors' calculation from SARI data

Table 5 Spillover effects from cable TV: comparing women from households without TV in villages with and without cable

Tolerance of beatings			Autonomy		
	all	No TV		all	No TV
(i) he suspects her of being unfaithful	0.013 (-0.016)	0.026 (-0.02)	(i) own healthcare decisions	0.019 (-0.023)	-0.011 (-0.021)
(ii) her natal family does not give money or jewellery	0.054*** (-0.02)	-0.058** (-0.024)	(ii) purchase of major household items	0.080*** (-0.023)	0.080*** (-0.03)
(iii) she shows disrespect	-0.028 (-0.023)	-0.02 (-0.03)	(iii) decision on whether to visit or stay with family/friends	-0.025 (-0.02)	-0.024 (-0.026)
(iv) she leaves home without telling him	0.016 (-0.014)	0.02 (-0.018)	(iv) has money to spend on her own	0.028** (-0.014)	0.019 (-0.018)
(v) she neglects the children	-0.036** (-0.018)	-0.005 (-0.02)	(v) needs permission to go to market	0.065*** (-0.024)	0.043 (-0.027)
(vi) she cooks badly	-0.071** (-0.031)	-0.085** (-0.033)	(vi) needs permission to visit family/friends	0.041*** (-0.013)	0.034*** (-0.009)
Tolerance measure	-0.161** (-0.073)	-0.121 (-0.089)	Autonomy measure	0.026*** (-0.006)	0.018** (-0.009)
PCA 6 variables	-0.147** -0.067	-0.11 -0.082	PCA 6 variables	0.131*** (-0.038)	0.085 (-0.053)
MCA 6 variables	0.091** (-0.041)	0.068 (-0.051)	MCA 6 variables	-0.054** (-0.026)	-0.029 (-0.033)

Note: Coefficients are estimated with the full set of controls. Standard errors and p values as above.

Table 6 TV, women's status and demographic behaviour in rural India

	Full sample	No TV	Has TV	Does not watch TV	Watches TV	SC/ST	Non SC/ST	Below 35	35 & above
	1	2	3	4	5	6	7	8	9
Tolerance of spousal beatings									
village has cable	-0.320***	-0.27**	-0.37***	-0.12	-0.036***	-0.32	-0.33***	-0.22***	-0.38**
	(0.082)	(0.130)	(0.098)	(0.165)	(0.100)	(0.32)	(0.086)	(0.078)	(0.19)
village has cable X	0.26***	0.22*	0.21	0.118	0.35*	0.365	0.23**	0.23*	0.176
woman is illiterate	(0.095)	(0.134)	(0.167)	(0.152)	(0.182)	(0.324)	(0.105)	(0.129)	(0.20)
	N=7014	N=4391	N=2623	N=3589	N=3425	N=1533	N=5481	N=4169	N=2845
Female autonomy									
village has cable	0.051***	0.033**	0.071***	-0.02	0.064***	0.039**	0.053***	0.023*	0.12***
	(0.012)	(0.015)	(0.016)	(0.05)	(0.015)	(0.018)	(0.014)	(0.012)	(0.027)
village has cable X	-0.041***	-0.023	-0.063***	0.009	-0.062***	-0.031	-0.042**	-0.022	-0.096***
woman is illiterate	(0.015)	(0.0168)	(0.023)	(0.05)	(0.022)	(0.028)	(0.017)	(0.0178)	(0.03)
Son preference									
village has cable	-0.14**	-0.17**	-0.079	-0.027	-0.16	-0.052	-0.158**[58]	-0.127*	Small N
	(0.068)	(0.077)	(0.1053)	(0.02)	(0.106)	(0.054)	(0.073)	(0.07)	
village has cable X	0.096	0.13	-0.018	0.056	0.25	-0.05	0.128	0.069	
woman is illiterate	(0.077)	(0.084)	(0.133)	(0.051)	(0.163)	(0.089)	(0.091)	(0.08)	
	N=1699	N=1142	N=557	N=898	N=801	N=338	N=1361	N=1557	

Notes: estimates include the same controls as Jensen and Oster. The X symbol denotes an interaction effect.

[58] To check this result further, we disaggregated into those who watch and do not watch TV. The groups are about equal in size; there is no significant coefficient for those watching TV, while for women not watching TV, the interaction term of zero education and village has cable is positive, large and significant at the 5 per cent level.

Table 7 Mean school building quality (average PCA score for all villages)

	PCA 1[1]		PCA 2	
	Jensen and Oster sample[2]	authors' sample[3]	Jensen and Oster sample[2]	authors' sample [3]
index of building quality	-0.34	-1.15	-0.05	0.12
	All schools			
	Jensen and Oster sample[2]		authors' sample[3]	
proportion having English medium school[4]	0.29		0.02	

Notes:

1. The school building quality variables (PCA1 and PCA2) are the first two principal components of a PCA of school quality variables in the DISE database. These two components account for more than 30 per cent of the total variation. For PCA1 bigger is better, for PCA2, smaller is better.
2. Cases in the Jensen and Oster sample – i.e. cases with five years (2003-2007) of non-zero enrolment.
3. Cases in the authors' sample, but not in the Jensen and Oster sample– i.e. cases with five years (2003-2007) non-missing enrolment that have fewer than five years non-zero enrolment.
4. Any school with English as the primary medium in any year; difference significant at $p < 0.0000$.

Table 8 Effects of cable on education in DISE data; IPJ data set, fixed cohorts

	Fixed cohort 2002-2003				Fixed cohort 2003-2004			
	Jensen and Oster covariates[1]		IPJ covariates		Jensen and Oster covariates		IPJ covariates	
	girls	boys	girls	boys	girls	boys	girls	Boys
	1	2	3	4	5	6	7	8
Panel A: All villages; no block trends								
village has cable	0.0295	0.0225	0.0162	0.00386	-0.00186	0.0196	0.0000963	0.0183
	(0.63)	(0.48)	(0.36)	(0.09)	(-0.05)	(0.53)	(0.00)	(0.51)
years of cable access	0.0321**	0.0321**	0.0007	0.00206	0.0226**	0.0267**	0.00214	0.00465
	(2.12)	(2.12)	(0.05)	(0.14)	(1.98)	(2.34)	(0.19)	(0.42)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	N	N	N	N	N	N	N	N
Jensen and Oster controls[1]	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	4284	4302	4253	4272	4777	4800	4744	4767
R^2	0.868	0.870	0.886	0.886	0.887	0.895	0.898	0.908
Panel B: Villages with cable after 2002; no block trends								
village has cable	0.0550	0.0395	0.0387	0.0198	0.0137	0.0351	0.0122	0.0323
	(0.0475)	(0.0462)	(0.0452)	(0.0445)	(0.0384)	(0.0384)	(0.0373)	(0.0370)
years of cable access	0.00249	0.0259	-0.04**	-0.0173	-0.00116	0.0102	-0.0251	-0.0178
	(0.0216)	(0.0211)	(0.0206)	(0.0205)	(0.0166)	(0.0160)	(0.0162)	(0.0155)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	N	N	N	N	N	N	N	N
Jensen and Oster controls	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	2579	2589	2552	2562	2938	2948	2909	2919
R^2	0.862	0.869	0.886	0.886	0.868	0.881	0.881	0.896

Panel C: All villages; block trends								
village has cable	0.0513	0.0395	0.0170	-0.00325	0.0203	0.0622	0.000138	0.0333
	(0.0472)	(0.0475)	(0.0457)	(0.0460)	(0.0373)	(0.0380)	(0.0363)	(0.0369)
years of cable access	0.0493***	0.0597***	0.0142	0.0252	0.0675***	0.0755***	0.0415***	0.0478***
	(0.0175)	(0.0178)	(0.0169)	(0.0172)	(0.0134)	(0.0134)	(0.0131)	(0.0130)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	Y	Y	Y	Y	Y	Y	Y	Y
Jensen and Oster controls	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	4284	4302	4253	4272	4777	4800	4744	4767
R^2	0.880	0.880	0.895	0.894	0.896	0.903	0.904	0.913
Panel D: All villages; control for pre-trends								
village has cable	0.0284*	0.0271*	0.00114	-0.000510	0.0204*	0.0241**	0.00237	0.00471
	(0.0159)	(0.0159)	(0.0153)	(0.0153)	(0.0119)	(0.0118)	(0.0116)	(0.0114)
years of cable access	-0.00740	-0.0153	0.00647	-0.0103	-0.0111	-0.00756	0.00118	0.00527
	(0.0243)	(0.0246)	(0.0232)	(0.0236)	(0.0189)	(0.0192)	(0.0183)	(0.0184)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	N	N	N	N	N	N	N	N
Jensen and Oster controls	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	4284	4302	4253	4272	4777	4800	4744	4767
R^2	0.868	0.869	0.886	0.886	0.887	0.895	0.898	0.908

Notes: Authors' calculations are from DISE data. Figures in brackets are standard errors adjusted for serial correlation. Level of probability: *p < 0.1 **p < 0.05 ***p < 0.01

1. Same covariates as Jensen and Oster, except Jensen and Oster code their 'village has electricity' which we code 1 = has and 0 = does not have electricity, and we dropped the village population variables. IPJ include the first building quality PCA variable and English medium dummy and their interactions with year.

Table 9 Effects of cable on education in DISE data; IPJ data set, 6-10 and 11-14 years

	6-10				11-14			
	Jensen and Oster covariates		IPJ covariates		Jensen and Oster covariates		IPJ covariates	
	girls	boys	girls	boys	girls	boys	girls	boys
	1	2	3	4	5	6	7	8
Panel A: All villages; no block trends								
village has cable	-0.00951	0.00434	-0.0059	0.0122	-0.0956	-0.0988	-0.0906	-0.0939
	(-0.52)	(0.24)	(-0.33)	(0.68)	(-1.34)	(-1.44)	(-1.29)	(-1.40)
years of cable access	0.00556	0.0174***	0.00548	0.0152***	-0.00302	0.0203	0.0190	0.0429**
	(1.03)	(3.16)	(1.02)	(2.80)	(-0.14)	(0.96)	(0.89)	(2.03)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	N	N	N	N	N	N	N	N
Jensen and Oster controls[1]	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	5160	5161	5125	5126	2763	2705	2750	2691
r2	0.962	0.962	0.964	0.965	0.904	0.908	0.914	0.917
Panel B: Villages with cable after 2002; no block trends								
village has cable	-0.00414	0.00212	-0.0002	0.0102	-0.0693	-0.0810	-0.0538	-0.0611
	(0.0196)	(0.0198)	(0.0191)	(0.0192)	(0.0697)	(0.0720)	(0.0673)	(0.0698)
years of cable access	-0.00654	0.0147*	-0.007	0.0129*	-0.0506*	-0.0474	-0.0410	-0.0398
	(0.00787)	(0.00804)	(0.0077)	(0.00784)	(0.0298)	(0.0294)	(0.0293)	(0.0292)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	N	N	N	N	N	N	N	N
Jensen and Oster controls[1]	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	3214	3215	3185	3186	1427	1446	1415	1435
R^2	0.951	0.951	0.955	0.955	0.921	0.926	0.935	0.937

Panel C: All villages; block trends								
village has cable	-0.00457	0.0144	-0.0046	0.0173	-0.0239	-0.0487	-0.0147	-0.0457
	(0.0190)	(0.0191)	(0.0186)	(0.0187)	(0.0715)	(0.0749)	(0.0704)	(0.0740)
years of cable access	0.00703	0.0304***	0.00608	0.0280***	0.0465*	0.0103	0.0583**	0.0202
	(0.00651)	(0.00659)	(0.0064)	(0.00652)	(0.0246)	(0.0251)	(0.0242)	(0.0249)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	Y	Y	Y	Y	Y	Y	Y	Y
Jensen and Oster controls[1]	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	5160	5161	5125	5126	2705	2763	2691	2750
r2	0.963	0.964	0.965	0.966	0.918	0.913	0.925	0.920
Panel D: All villages; control for pre-trends								
village has cable	0.00600	0.0180***	0.00598	0.0162***	0.0210	-0.00507	0.0412*	0.0151
	(0.00558)	(0.00568)	(0.0055)	(0.00559)	(0.0225)	(0.0227)	(0.0222)	(0.0225)
years of cable access	0.0000386	0.00451	0.00146	0.00922	-0.0281	-0.0371	-0.0360	-0.0436
	(0.00932)	(0.00947)	(0.0092)	(0.00929)	(0.0362)	(0.0369)	(0.0355)	(0.0362)
demographic controls	Y	Y	Y	Y	Y	Y	Y	Y
block-specific trends	N	N	N	N	N	N	N	N
Jensen and Oster controls[1]	Y	Y	Y	Y	Y	Y	Y	Y
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	5160	5161	5125	5126	2705	2763	2691	2750
R^2	5160	5161	5125	5126	2705	2763	2691	2750

Notes: Authors' calculations are from DISE data. Figures in brackets are standard errors adjusted for serial correlation.
Level of probability: *p < 0.1 **p < 0.05 ***p < 0.01

Appendix B: Figures

Figure 1 Acceptability and experience of being beaten by husband (NFHS3)

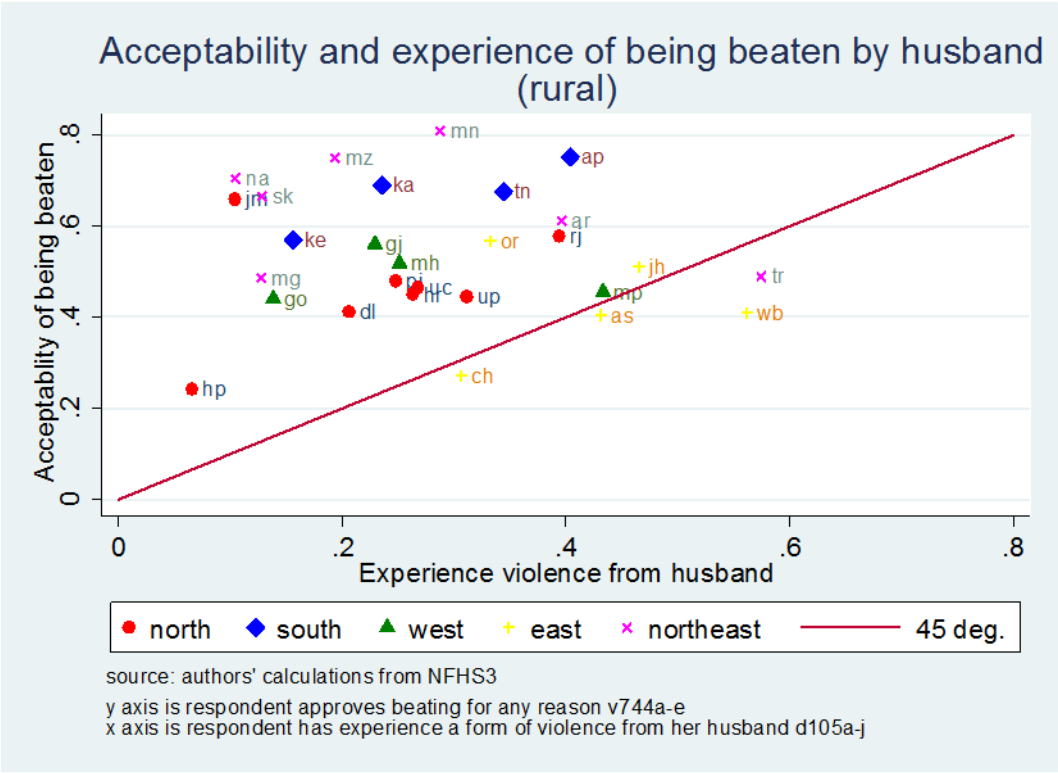
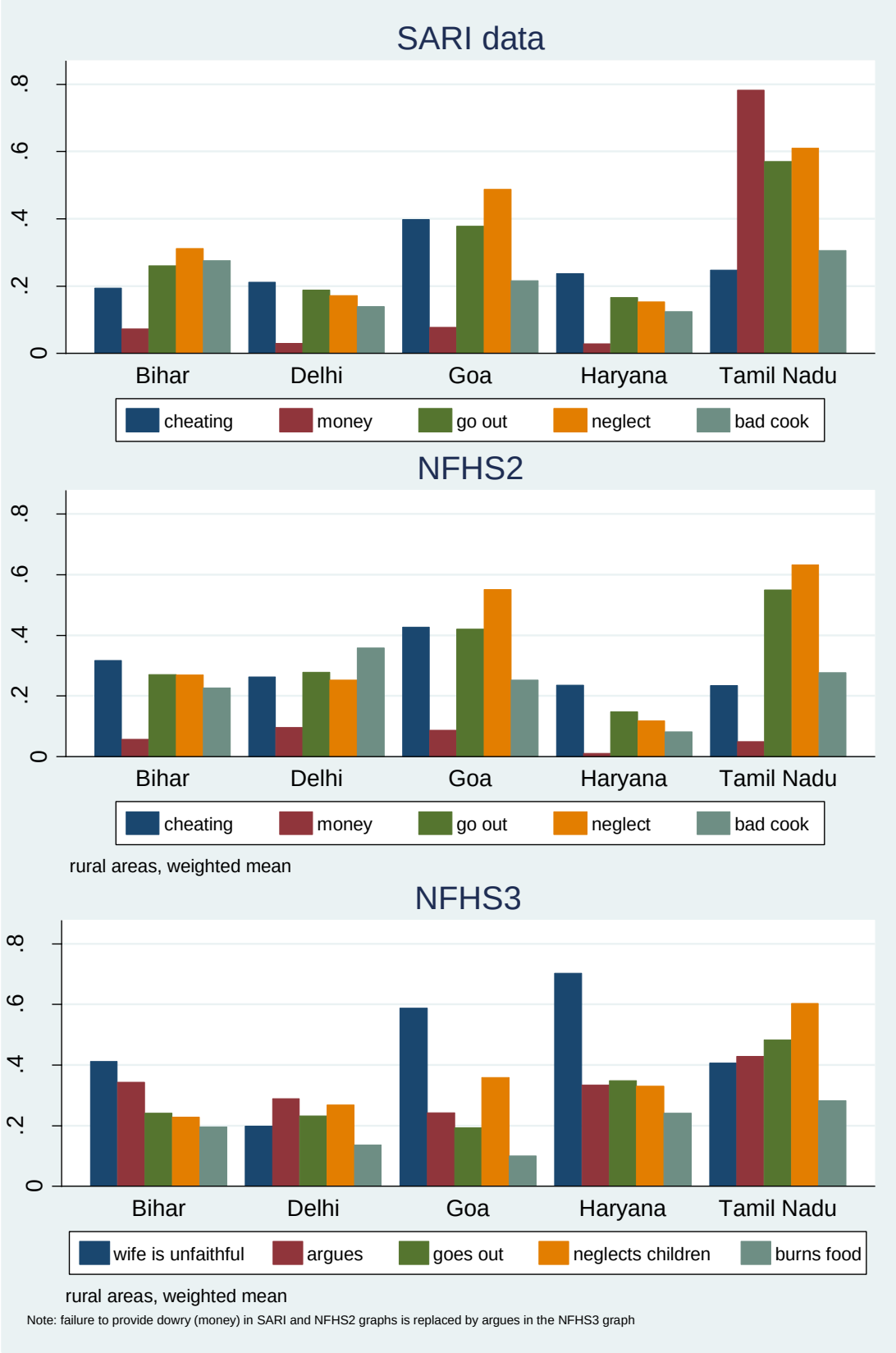
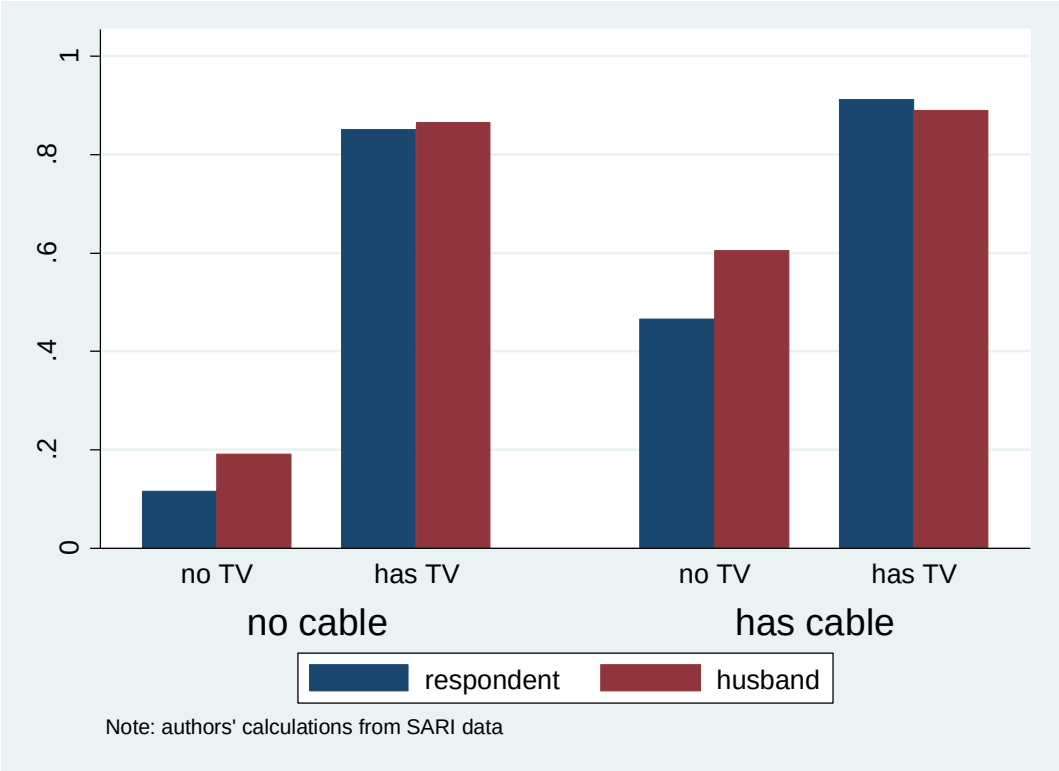


Figure 2 Tolerance of spousal beatings, by state and cause



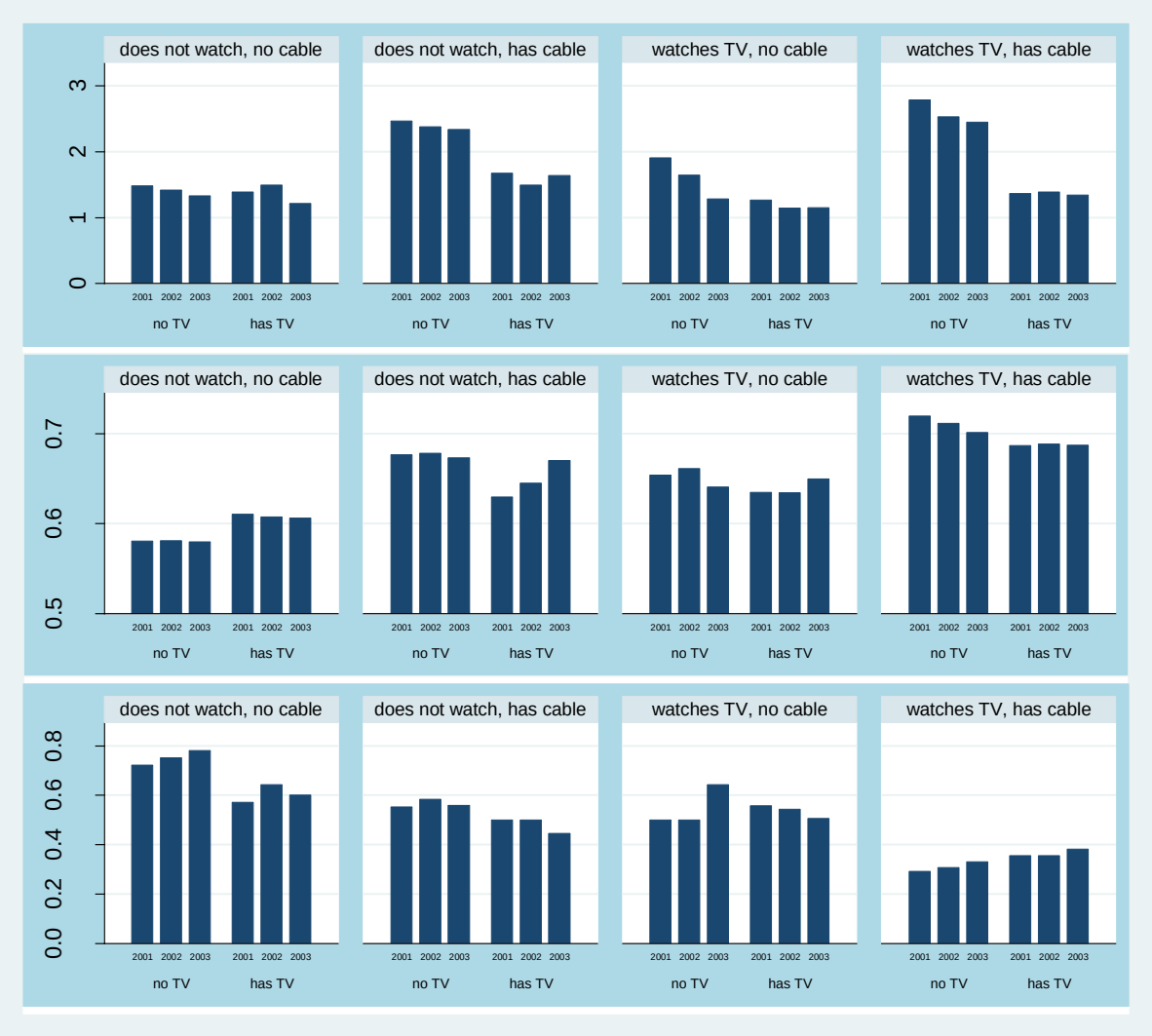
Note: failure to provide dowry (money) in SARI and NFHS2 graphs is replaced by argues in the NFHS3 graph

Figure 3 Reported TV watching by viewers in villages with and without cable, by TV ownership and gender



Note: authors' calculations from SARI data

Figure 4 Tolerance to beatings, women's autonomy, and son preference, by access to cable, TV ownership, and TV watching

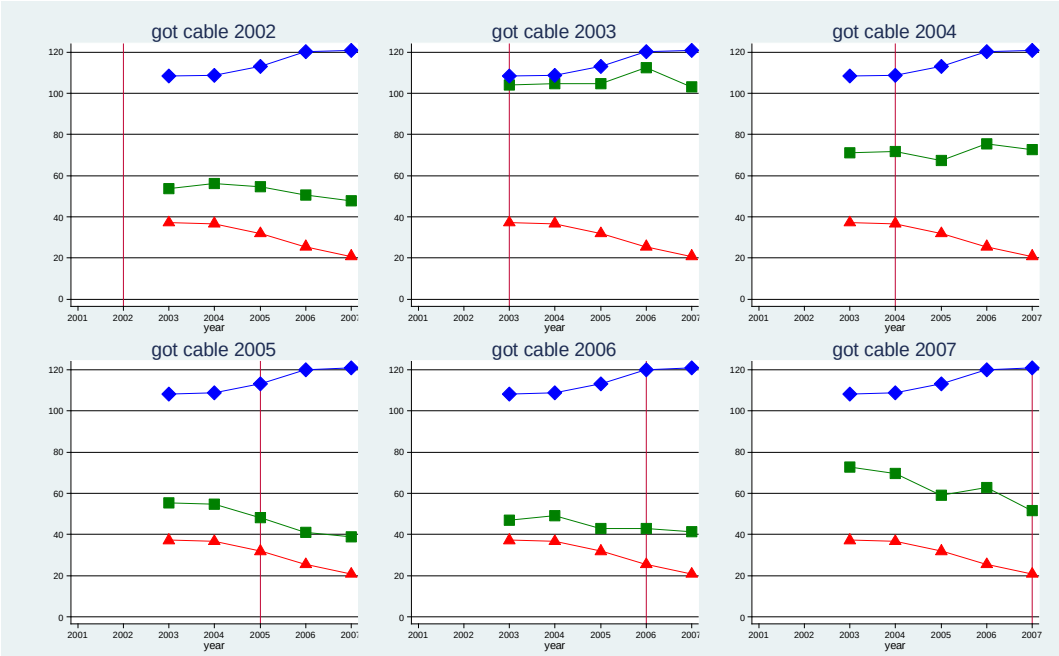


Notes:

The y axes denote variable values: for tolerance of beatings the maximum value is 6, while son preference and autonomy is in the 0-1 range. The y axis for autonomy has been truncated at both ends.

The y axes are the mean values of the outcome indexes 'mn_outcome' (beating), son preference, and 'mn_real' (autonomy) respectively.

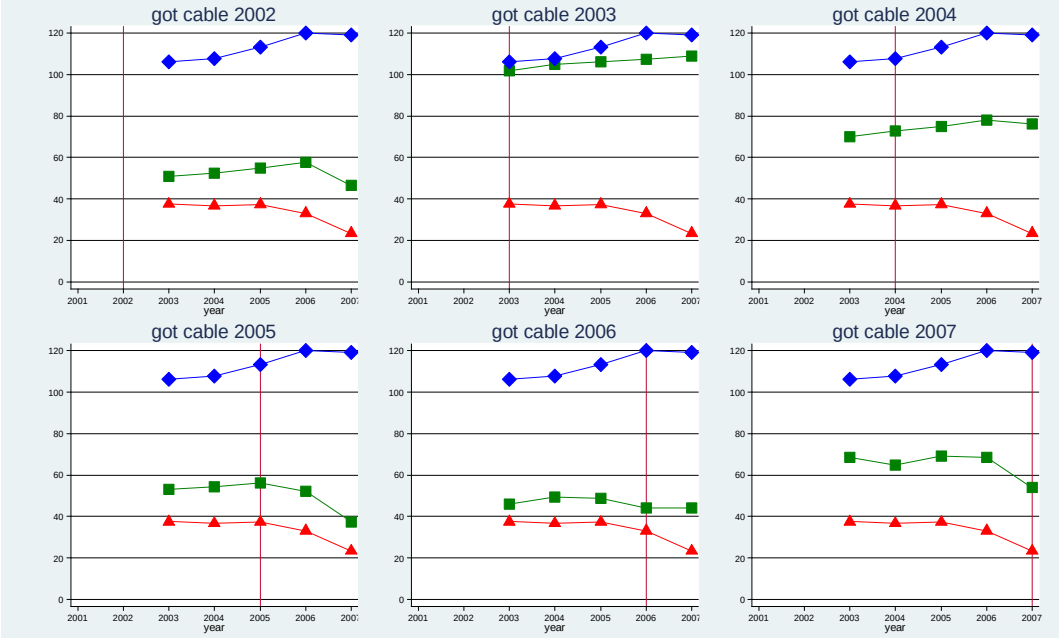
Figure 5a Enrolment of cohort 6 to 7-year-olds in 2002-2003, by year and year of access to cable TV



Note: y variable is the mean enrolment in villages inclucing those with zero pupils in a year

Legend: green squares - got cable in year
blue biamonds - got cable before 2002
red triangles - never got cable

Figure 5b Enrolment of cohort 6 to 7-year-olds in 2003-2004, by year and year of access to cable TV



Note: y variable is the mean enrolment in villages inclucing those with zero pupils in a year

Legend: green squares - got cable in year
blue biamonds - got cable before 2002
red triangles - never got cable

Appendix C: Jensen and Oster methodology

Jensen and Oster report that their model, used with both the SARI and the DISE data, is:

$$(2) \qquad y_{ivt} = \beta c_{vt} + \gamma_{iv} + \delta_t + \tau X_{ivt} + \varepsilon_{ivt}$$

In this model, y_{ivt} is outcome y for individual *i* in village *v* in year *t*. γ_{iv} are individual fixed effects, δ_t are year dummies (2002 and 2003) and X_{ivt} are as set of controls including household income and the age and age-squared of the respondent in the SARI data; c_{vt} is a dummy representing the presence of cable (or not) in village *v* in year *t*.

In the model estimated with the SARI data, identification depends on the 21 villages which get cable for the first time during the second and third years of the three-year survey.

This model is largely correct for the estimation with DISE data where the controls include: dummies for year; village; whether the village had electricity (0/1); year[59] * electricity; village population aged 6-14 in 2005; village population * year; the log of distance to Thana Headquarters; and log of distance * year.

Village population does not change within the time period covered, nor does distance; whether a village is electrified can change.[60] Although estimated by the Prais-Winston procedure, this model has some multi-level features in that some controls are at the village (unchanging) level.

The model estimated with the SARI data is more evidently multi-level. Jensen and Oster report that in their estimations with the SARI data, X includes 'interactions between a year indicator and state dummies, income, education, age and age-squared, village population density, electrification status, and distance to nearest town' (2009, p.1072). 'All covariates except income, age and age squared are for the base year 2001' (ibid, p.1078). But this is not exactly correct. In fact, the model estimated by Jensen and Oster is:

$$(3) \qquad y_{ivt} = \beta c_{vt} + \gamma_{iv} + \delta_t + \varphi L_{ivt} + \tau^j y_t . X_{iv}^j + \theta^k y_t . X_{v2001}^k + \omega_j y_t . S_j + \varepsilon_{ivt}$$

In this model, y_{ivt} is outcome y for individual *i* in village *v* in year *t*. γ_{iv} are individual fixed effects, δ_t are year dummies (2002 and 2003), and c_{vt} is a dummy representing the presence of cable (or not) in village *v* in year *t*, as above. Additionally, y_t is year, taking values 2001, 2002 and 2003 for t = 1, 2, 3 and is interacted with the following three sets of control variables:

1. X_{ivt} is a set of controls for individual *i* of village *v* in year *t*, including the log of household income per capita (the household in which the respondent resides) and her age and age squared. Age does not appear separately, but age-squared does;

[59] Year is 2003 – 2007

[60] There are variables for building quality in the final data set, but these are not used in the analysis.

2. X_{iv} are a set of controls for individual *i* in village *v* which are fixed for the base year of the panel period (2001), including years of education and sex, and do not vary over the panel period; and

3. X_{v2001} are a set of controls for village *v* set in the year 2001, including the log of population density, the log of distance to nearest large town, and whether the village is electrified (which does not change over the panel period).

$y_t S_j$ are year * state dummies (j=1…5).

Thus the controls are specified at different levels and for different periods; all are multiplied by the numerical value of the year in which the data were recorded (y_t), and only age-squared and the two year dummies appear as themselves. Controls and variables of interest are at the individual, household and village levels. Individual variables vary over years (and age and age squared – in a deterministic way); some – such as watching TV, husband watching TV – are individual, but time invariant. Examples of household level variables include log of income per capita, ethnicity, TV ownership and religion. A significant proportion of respondents come from the same household. Village level variables include 'village has cable', electrification, log of population, log of population density and distance from nearest town. Hence Jensen and Oster's description in the footnote to their Table 4 (p.1078) accurately reports the linear interaction of year with controls, but does not convey well the true nature of the model.

This is a multi-level model, with covariates at individual (time-varying and time-invariant), household, village and state levels. Individual, household and village are nested within each other and state. All four levels are crossed with year; the coefficient of interest is a village rather than individual level variable.

The main dependent variables are indexes constructed from variables which can vary over time. Indeed, responses to some of the questions – such as, 'Is it acceptable for a husband to beat his wife if she cheats on him?' – can be 'no', 'yes' and 'no' in 2001, 2002 and 2003, or other different responses in different years. Similarly, answers to questions such as 'Who decides whether to buy major household goods?' can be 'self' or 'others' in the three years of the survey, with the pro-social values not necessarily following the introduction of cable TV.

The advantage of specifying the model as mixed is that it allows both random and fixed effects (Rabe-Hesketh and Skrondal 2008; Gelman and Hill 2007). We have experimented with various specifications of mixed models without finding important changes to the results presented here; but this is an area for further work.

Appendix D: Coding errors and differences

Specific issues in coding

The DISE data consist of six years of enrolment data (2002-2007) in schools in five districts of Tamil Nadu, together with some files on characteristics of schools and the villages in which they are located. Jensen and Oster compute the numbers of age cohort in each village for each year for both specific age ranges and a fixed cohort that was aged 6 to 7 in 2002.

Jensen and Oster compute the total of 6-14 year olds, 6-10 year olds, and some other groupings, including the fixed cohort (7-8 in 2003, 8-9 in 2004 and so on).

Error 1: 6-14-year-olds

In their "DISE Setup.do" file for the 2002-2004 data, they start counting the 6-14-year-olds by including 5-year-olds, and do the same for the 6-10s. The 11-14 year olds are computed by subtraction. From 2005 onwards they do not include the 5–year-olds in the 6-14s, but still include them in the 6-10 aggregate. Therefore Jensen and Oster lose the 5-year-olds from the 6-14 group but not the 6-10 group, thus finding fewer 6-14-year-olds than 6-10 year-olds when there are more 5-year-olds in school than 11-14-year-olds.

This causes there to be negative numbers of 11-14-year-olds enrolled. Later in the code, the log of the 11-14-year-olds is taken, which results in missing values for negative enrolments. Jensen and Oster then drop these observations from some of the Table 6 estimations (Columns 5 and 6) and all of the Figure 7 graph samples. In the latter case this apparently gives directions of change that are quite different from those depicted.[61]

Thus in the calculations for 2002-2004, line 44 (about) the code (slightly amended for legibility) reads:

```
replace totalenroll614_boy=totalenroll614_boy+c1_5b+c1_6b+c1_7b+c1_8b ///
        +c1_9b+c1_10b+c1_11b+c1_12b
* NOTE includes 5 year olds
replace totalenroll614_girl=totalenroll614_girl+c1_5g+ c1_6g+c1_7g+c1_8g ///
        +c1_9g+c1_10g+c1_11g+c1_12g
* NOTE includes 5 year olds
```

Line 98 reads:

```
foreach age in 5 6 7 8 9 10 {           // includes 5 year olds
foreach grade in 1 2{
replace totalenroll610_boy=totalenroll610_boy + c`grade'_`age'b
replace totalenroll610_girl=totalenroll610_girl + c`grade'_`age'g
}}
```

[61] Note that the results we report here are preliminary, but we have adequate confidence in them at this stage.

For 2005-2006 (around line 336), 2006-2007 (around line 598) and 2007-2008 (around line 891) it reads:

```
replace totalenroll614_boy=totalenroll614_boy+c1_6b+c1_7b+c1_8b ///
          +c1_9b+c1_10b+c1_11b+c1_12b  // EXCLUDES 5 year olds
replace totalenroll614_girl=totalenroll614_girl+c1_6g+c1_7g///
        +c1_8g+c1_9g+c1_10g+c1_11g+c1_12g
```

For 6-10-year-olds, at line 647 (2006-2007, and similarly for 2005-2006 and 2007-2008), it reads:

```
for each age in 5 6 7 8 9 10 {   // includes 5 year olds
for each grade in 1 2{
replace totalenroll610_boy=totalenroll610_boy + c`grade'_`age'b
replace totalenroll610_girl=totalenroll610_girl + c`grade'_`age'g
}
}
```

Clearly five-year-olds have been added to the 6-14 aggregate but not 6-10 aggregate, hence there is a significant possibility that the computed 11-14 aggregate will be negative, as described above.

Error 2: Fixed cohort (6-7 in 2002-3)

There also appears to be an overestimate of the fixed cohort in 2007-2008 (when the original 6-7-year-olds are 11-12). Jensen and Oster's code constructing the fixed cohort (6-7 in 2002) in 2007-2008 for Class 6 enrolment aged 11-12 reads:

```
replace totalenrollfixed_boy=totalenrollfixed_boy+c6_10b+c6_12b
replace totalenrollfixed_girl=totalenrollfixed_girl+c6_10g+c6_12g
```

while it should be:

```
replace totalenrollfixed_boy=totalenrollfixed_boy+c6_11b+c6_12b
replace totalenrollfixed_girl=totalenrollfixed_girl+c6_11g+c6_12g
```

This code adds an extra year cohort (10-year–olds, who would have been five in 2002) to the fixed cohort in 2007-2008, resulting in an over-estimate of enrolment in that year.

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