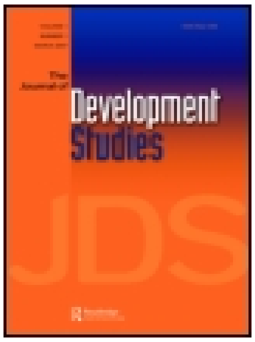

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All You Need is Cable TV?

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All You Need is Cable TV?

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ABSTRACT *Robert Jensen and Emily Oster find that arrival of cable TV in rural India reduces women's tolerance of spousal violence, son preference and fertility, and increases women's autonomy, and school enrolment. These results are mostly replicated using their data and code. However, cable TV does not affect uneducated women. Theoretically informed index construction reduces the tolerance of violence effect, and weakens that on autonomy. We have statistical power concerns, and find errors and questionable assumptions in school enrolment constructions. Using our data constructions, effects sizes and significance are weakened. These results suggest that pure, statistical and scientific replication have merit.*

1. Introduction

Female disadvantage in South Asia is usually considered an outcome of deeply engrained patriarchal cultures, reflected in kinship and other social practices (Dyson & Moore, 1983; Rahman & Rao, 2004). The literature addressing women's empowerment in the region emphasises access to resources such as: education (Murthi, Guio, & Dreze, 1995); rights in land (Agarwal, 1994); waged employment (Rosenzweig & Schultz, 1982; Sen, 1990); entrepreneurial opportunities; credit; and so forth. It also differentiates between types of empowerment (Basu & Koolwal, 2005).

The findings reported by Jensen and Oster (2009; henceforth JO);[1] suggest that what are usually understood as rigid behavioural norms and attitudes (Bourdieu, 1977) that require radical revisions of property regimes or a levelling of educational and other opportunities to change, may transmute rapidly once cable TV arrives in a village. Using the three year panel dataset from the Survey of Ageing in Rural India (SARI), JO identify the impact of cable TV arrival on indicators of women's status and demographic change by differencing out stable village and individual characteristics, controlling for income and pre-existing differential trends (Jensen & Oster, 2007, p. 10). They find that cable TV reduces women's tolerance of spousal beatings, son preference, and fertility rates, and increases women's autonomy and school enrolment rates for some age and sex groups in the same year that cable arrives. Using administrative data from the District Information System on Education (DISE) in Tamil Nadu, they report that exposure to cable increases (total) enrolment linearly with the years of access to cable TV.

These results are important and challenge what many development economists and feminist scholars might consider plausible both with respect to the *speed* and the *catalysts* of social change (Foster & Rosenzweig, 1995; Seguino, 2007).[2] JO's results are also at odds, as Iversen and Palmer-Jones (2014; IPJ from now) discuss at length, with research on the impact of popular and socially engineered (pro-social) TV serials on attitudes towards women in India.[3]

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Since this evidence dates back to the mid-1980s, it is possible that rural India, by the time SARI was implemented, had become more responsive to modern media exposure.

Research on 'media effects' has mainly been concerned with children and adolescents (Strasburger & Donnerstein, 1999; Strasburger, Wilson, & Jordan, 2008), often in the context of 'pro-social' programming, with ambiguous results (Bryant & Thompson, 2002). JO discuss a small number of 'correlational' studies that indicate strong effects of media – TV in particular – in other contexts (Brazil, Mexico) and some ethnographic studies from South Asia: they do not discuss earlier and more granular evidence from India (see endnote three) that disagrees with their findings. This literature has proliferated in recent years – see reviews in DellaVigna and La Ferrara (2015) and La Ferrara (2016) – and suggests that carefully tailored and targeted TV 'campaigns', disseminating messages and information and often focusing on fertility and reproductive decision-making, can quickly induce change. Much less is known about the speed and impacts of *general* (cable) TV exposure which is the issue of interest here.[4]

JO focus entirely on the introduction of cable TV (Chapman, 2005), not the effects of explicitly pro-social or persuasive TV programming which were the subjects in other studies to which they refer (La Ferrara, Chong, & Dureya, 2008; La Ferrara, Chong, & Durya, 2012). Even where the impact of TV has been convincingly demonstrated – in randomised control trials or well conducted analysis of observational data – impact estimates are not generalisable across contexts; shown to exist in the long-run; succinctly linked to models or mechanisms of persuasion; or identified in the context of competing messages (DellaVigna & Gentzkow, 2010, p. 665). Even sophisticated correlational work, it has been argued, is unlikely to throw sufficient light on the effects of the introduction of cable TV (McGuire, 1986; Zaller, 1996; Bryant & Thompson, 2002, p. 55–57).

Conducting a replication of micro-econometric research may be considered to comprise three processes (Hamermesh, 2007)[5]: checking (Collins, 1991) that the original data and methods produce the reported results (pure replication); robustness testing (statistical replication), for example examining the foundations and merits of variable (index) construction and estimation methods using the same (or similar) raw data and estimation model; and, finally, using the same or convincing alternative estimation methods with the same or similar datasets to explore rival explanations and further test robustness (scientific replication).

In our pure replication using SARI and DISE data, we find that JOs analysis can be mostly and exactly replicated if we use the estimation data and code they provided.[6] When, in our pure replication, we correct or adjust data construction code, we find that our alternate estimation data yields some meaningfully different results.

In our statistical and scientific replication, we examine whether JO's results depend on the construction of their main tolerance of spousal violence and female autonomy indexes. These qualitative concepts are hard to measure and to translate into quantitative indexes. Furthermore, women's status is not unidimensional and does not have the same implications in different contexts or for different groups. Basu and Koolwal (2005) critique the measures of women's status in standard Demographic and Health Surveys (DHS) (and National Family and Health Surveys [NFHS]) (see further below): some variables may conflate women's household responsibilities with empowerment, and are therefore ambiguous.[7] Filtering out such ambiguity is desirable when studying changes in women's status. Observed through a lens of feminist economics and scholarship, and with regard to development theory and practice, it is therefore of interest to discern which among the variables in JO's composite indexes that drive change.

We find that JO's tolerance of violence results depend on the variables included, and on the measure of empowerment used. With regard to the latter, using Basu and Koolwal's (2005) zero tolerance of spousal violence interpretation, there is no effect from cable TV arrival. A further observation is that one variable included in JO's tolerance of spousal violence index is not as described in JO and Jensen and Oster (2007) (see below). JO's autonomy results are robust for their own measure and interpretation.[8] For a Basu and Koolwal (2005) type interpretation of women's empowerment, differentiating between autonomy variables representing women's domestic role and those representing their self-determined autonomy, the results are more mixed.

We also find that there is no impact of cable TV on the status of women with no education (53.8% of the women in the SARI panel) in rural India. While Jensen and Oster (2007) point to larger impacts for more educated women, they do not mention that the effect on women with no education is effectively zero,[9] and thus do not report on this crucial heterogeneity in cable TV impacts.[10]

JO report positive effects on school enrolment for young girls (six to 10) and the fixed cohort of girls aged six in 2001 in the year cable TV arrives and with years of access to cable in the SARI data, but not for boys. We find problems in the construction of the SARI village level estimation dataset and the variable used in testing for pre-trends. While only the latter has a meaningful effect on the results, we argue that the statistical power of the SARI analysis to detect an effect, when there is one, is very low given the small effect sizes, residual variance, and relatively low 'p' values, suggesting that any statistically significant results based on the SARI data, including on enrolment, may be false positives.

In their analysis of the DISE data on school enrolment in Tamil Nadu, enrolment increases linearly with years of access for their fixed cohort (six to seven in 2002) of boys and girls and for young girls in all specifications of their estimating equation (not immediately and only in the year cable TV arrives, as in the SARI results). Our replication supports some but not all of the findings using the DISE data: however, once we add controls for school quality (and correct what we construe as some errors in JO's variable construction code), the DISE results seem too weak to support JO's claims.

Our paper is laid out as follows.[11] To facilitate comparisons between our text and JO, we follow their broad outline focusing first on the SARI data analysis before turning to DISE. The second section reports briefly on the pure replication using SARI data. Under the scientific replication heading, we examine the construction of the tolerance of spousal violence and autonomy composite variables and estimate the social spillover effects from cable TV introduction. To explore heterogeneity, we implement simple respecifications of JO's main model. In the third section, we focus on school enrolment and statistical power in the SARI panel. This is followed by Section 4 which addresses school enrolment using the DISE-data; asserting the limitations of the estimation data and approach, we suggest that this analysis lends little support to the overall argument. The final section concludes and reflects on the policy implications of the study in light of the replication.

2. The SARI analysis

2.1. Pure replication

As noted, JO use two datasets (SARI and DISE) and 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 and 2003) and X_{ivt} are a set of controls including household income and the age and age-squared of the respondent;[12] 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. 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' (JO, p. 1072). With the DISE dataset, the model is estimated using the Prais-Winston method to account for serial correlation. 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.[13]

2.2. The SARI dataset

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'.[14]

The raw data from the survey are no longer available and the data made available by the authors are the final estimation dataset used in JO's analysis (personal communication). More variables are provided than used in the published estimations. The analysis of education enrolment using the SARI dataset in JO is at village level (although individual level analysis is reported in their online appendix). Code constructing the village level education estimation dataset from the individual level is not available, and some discrepancies between the individual and our reconstructed village education datasets are noted below.

Many of the variables of interest are constructed from raw data not available to us or to Jensen and Oster (personal communication); for some variables – including the key composite outcome variables – constructions can be replicated easily. The SARI panel dataset shared by Jensen and Oster[15] comes with clear and well organised .do files that facilitate a pure replication of their results from the estimation datasets provided of women's attitude to spousal violence (the index mn_outcome), women's autonomy (the index mn_real), son-preference (the binary dependent variable wants_son), and fertility (the binary dependent variable is_pregnant), and enrolment (Jensen and Oster Tables II, IV and V).[16] One significant issue that arose during the replication process (while exploring differences between SARI descriptives for Tamil Nadu and the same variable in NFHS 2)[17] was that the description given by JO of one variable (purported to be '*whether it was acceptable for a husband to beat his wife if her natal family does not give expected jewellery [sic]*) included in their tolerance index, is misleading. It was in fact '*whether it was acceptable for a husband to beat his wife if she took money that was meant to be used for other things in the household and misused it or used it for herself.';* the implications of this difference are discussed below.

2.3. SARI replication

We address five aspects of the statistical and scientific validity of the association of women's status with cable TV: whether index construction affects JO's results, the interpretation of women's status implied by JO's index instructions, the heterogeneity of cable impacts across rural women, the external validity of the findings, and the statistical power of the empirical analysis. The literature touched upon above distinguishes between variables representing orthodox gender roles (female household responsibilities) and 'discretionary' or self-indulgent variables, which have very different implications for women's 'empowerment'. Basu and Koolwal (2005), discussing Demographic and Health Survey (DHS) women's empowerment variables, suggest 'zero tolerance' as the most appropriate construct for tolerance of beatings. Similarly, for women's autonomy, some variables are ambiguous and may only capture orthodox gender roles and household responsibilities rather than self-determined choice. This justifies exploration of whether the measure and interpretation of empowerment matters,[18] and is a line of inquiry of interest to scholars working on gender and development (see also Kishor & Johnson, 2004).[19,20] Secondly, we compare the SARI findings with those of similar analysis of other relevant datasets, in particular the Indian NFHS2, on which the SARI survey was partly based. Thirdly, because many economics studies are seriously under-powered, and hence at non-trivial risk of reporting false positives (Button et al., 2013; Ioannidis & Doucouliagos, 2013; Ioannidis, Stanley, & Doucouliagos, 2016), we explore the statistical power of the SARI analysis.

Table 1. Variables used to construct tolerance of spousal beatings and female autonomy measures (indexes)

Survey variables	NFHS2 questions	NFHS3 questions	Variable type	SARI codes	Recodes
Women's attitudes (status) tolerance of spousal beatings: six variables	s514a-f	v744a-e s829f,g	Binary (0/1)	0 = No 1 = Yes	
Women's autonomy (i) healthcare decisions	s511b	v743a	Categorical	1=self 2=husband	1 if 1 or 3 or 5, 0 otherwise
(ii) purchase of major household items	s511c	v743b		3=self+husband 4=others	
(iii) decision on whether to visit or stay with family or friends	s5111d	v743d		5=self+others	
(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	0 not need 1 need permission	2 not need 1 need
(vi) needs permission to visit relatives/friends	s512b	(s824c)	2 with other 3 not at all	2 not allowed to go	permission 0 not allowed to go
can keep money/has money for own use	(s513)	w124	Binary (0,1)	0 = No 1 = Yes	
son preference for next child	v627-9	v627-9	Categorical	1=boy 2=girl 3=not matter 4=other	1 if boy 0 otherwise

Notes: 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 1). 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'.

2.4. Outcome variable construction

JO construct simple indexes representing the acceptability of domestic violence and female autonomy, and other measures capturing son preference and fertility. A description of each variable used by Jensen and Oster is presented in Table 1.

The domestic violence variables are binary answers to questions about the acceptability of a husband beating his wife in various situations; the female autonomy measure 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. There are difficulties in interpreting the meaning of these answers, especially the autonomy questions. In particular it is not clear whether 'deciding on her own' represents the most empowered state, since it can also represent a situation where 'a partner contributes little' (Schatz & Williams, 2011).

These indexes deliberately mimicked similar questions in NFHS2 (see Table 2), with the one exception noted above.[21] The thematic proximity to NFHS provides an opportunity to assess the external validity of the SARI variable values. The second 'attitude towards beating' variable gives very different values to the same variable in NFHS data, especially in Tamil Nadu. As illustrated in Figure 2 below, Tamil Nadu stands out not only for this variable but 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.[22]

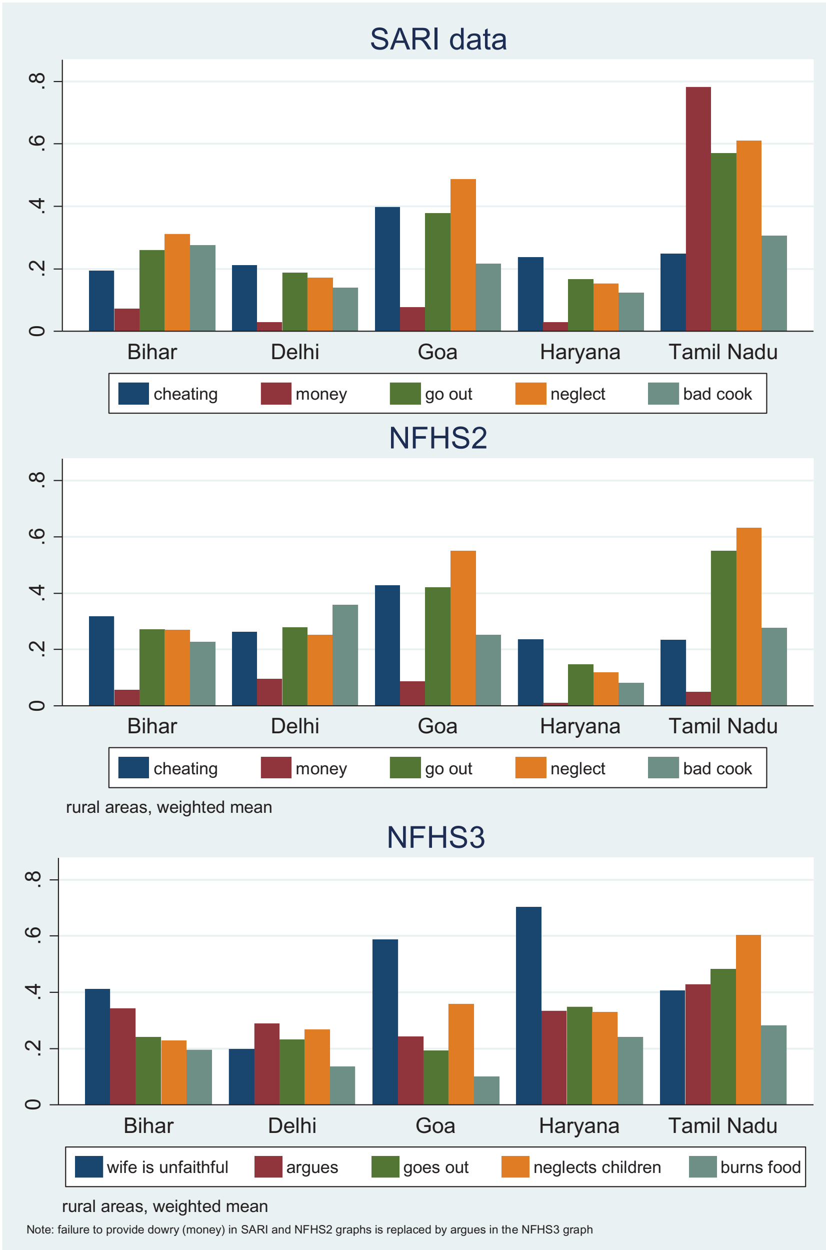

Figure 1. Tolerance of spousal beatings, by state and cause.

However, as noted above, it turned out that, because of a recent '*tragic case involving an alleged dowry murder in Tamil Nadu'* which was receiving much attention in the press, the question actually asked in the SARI survey was '*whether it was acceptable for a husband to beat his wife if she took money that was meant to be used for other things in the household and misused it or used it for herself.'*[23] This question means something very different and is 'leading' in a manner that the original question was not. This makes it harder to assess external validity and casts doubt on the variable's value in construction of the tolerance of violence index.

Table 2. Coefficient of 'village has cable' for components of composite measures (indexes) representing women's tolerance of (attitude to) beating and women's autonomy

Dependent variable (attitude 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	0.028** (0.014)
(v) she neglects the children	−0.036** (0.018)	(v) needs permission to go to market	0.065*** (0.024)
(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)
Basu & Koolwal's zero tolerance of beatings	−0.002 (0.020)	Autonomy measure based on Basu and Koolwal (i, iii, & iv)[a]	−0.006 (0.005)
		Autonomy measure based on Basu and Koolwal (i, iv, & vi)	0.018** (0.008)
		Autonomy measure based on Basu and Koolwal (i+, iii+, & iv)[b]	−0.007 (0.012)
		Autonomy measure based on Basu and Koolwal (i+, iv, & vi)	0.029*** (0.010)

Notes: * p < 0.1; ** p < 0.05; *** p < 0.01. 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. Outcomes estimates by PCA and MCA are available in Supplementary Materials Table A1. [a]Index based on recoding of decision variables – i, iii, iv, vi = 1 if woman takes decisions alone or has own money/does not need permission. [b]Woman takes decisions alone or with others – i, iii, iv, vi = 1 if woman takes decisions alone or with others/has own money/does not need permission.

Using JO's benchmark specification (their Table IV, Column 1), Table 2 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.[24] The attitude variable with most leverage – (ii) reported by JO as the natal family not giving the expected jewellery, and so forth – is the one afflicted by external validity concerns. When we exclude this variable from JO's index, the coefficient of this adjusted tolerance measure becomes smaller in size and only marginally significant, introducing doubts about the robustness of one of JO's main findings.[25]

In addition, different questions within each theme corresponds to different dimensions of empowerment (see Sandberg & Rafail, 2013). Critiquing the measures used in DHS and NFHS, Basu and Koolwal (2005) propose three types of indicator 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 wellbeing (variables that reflect actions which may not challenge norms but instead enhance a woman's 'responsibility' for family wellbeing); and variables reflecting a woman's capability for egoistic decision-making, or her ability to be 'self-indulgent'. They proceed to propose alternative and in their view more robust ways to measure female empowerment. The need to distinguish between different types of autonomy, and whether some types of women's mobility or responsibility can be considered autonomy at all, has gained theoretical backing in Self Determination Theory (SDT) (Deci & Ryan, 2000), enhancing understandings of subjective wellbeing where there is psychological accommodation to patriarchal domination (Alkire, 2005); such distinctions are embodied in the Relative Empowerment Indexes proposed by Ibrahim and Alkire (2007) and operationalised in Vaz, Pratley, and Alkire (2016). It arises also from more general questioning of the meaning and value of autonomy as reflecting a Western educated, industrialised, rich and democratic (WEIRD) context (Henrich, Heine, & Norenzayan, 2010; see also Duvendack & Palmer-Jones, 2017, p. 668), and from qualitative empirical studies of autonomy and their implications for wellbeing in South Asia (Rao, 2014).

For tolerance of spousal violence, Basu and Koolwal (2005) consider zero tolerance the most appropriate interpretation. If we replace the composite status variable with a dummy that takes the value 0 if there is at least one situation where a woman tolerates spousal beatings and the value 1 if there is zero tolerance, the coefficient on village has cable is no longer statistically significant (Table 2).

The autonomy index is behaviour rather than attitude based, although the answers may represent normative or other response biases (Kalton & Schuman, 1982), and positive coefficients reflect more autonomy. Table 2 shows that four of the six component questions in JO's index are positively and significantly associated with cable TV introduction. The autonomy index is not vulnerable to the seeming double counting of the 'visit' variables (iii and vi), remaining significant at the 1 per cent level (five variables).

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 culturally dominant (patriarchal) female roles, or responsibility, which may be relatively easy to change. In contrast, self-indulgent or discretionary empowerment (self-determined agency) is related to not requiring permission to visit relatives, deciding to obtain own healthcare, and being allowed to set aside money. We observe no effect of cable on own healthcare decisions; a significant effect of cable on one visit variable but not the other; and a significant and positive effect of cable on own money. An index that captures Basu and Koolwal's concerns and aligns with feminist scholarship more broadly, might thus combine the decision of whether to seek own healthcare (column 3 i), whether she herself decides to visit family and friends (iii), and whether she has money for own use (iv). There is no impact of cable TV of this index. However, if the 'needs permission to visit' variable (vi) replaces the decision whether to visit family and friends variable (iii), the autonomy index is significant at the 5 per cent level. Finally, if for the latter index, decision-making in (i) includes joint decision-making with her husband or others, the coefficient on the index is significant at the 1 per cent level.

2.5. Owning, watching, and being affected by TV: spillover effects

As noted in the introduction, JO lack a theory of change linking the arrival of cable TV to the impacts they report. At a very simple level one can hypothesise two possible mechanisms; (1) through watching more TV, and (2) through social changes brought about by those who watch TV when interacting with others who do not (Bandura, 2009, original 1994). However, owning a TV is not a necessary condition for watching since TV can be viewed in the homes of others and/or in public places. While there are variables for owning and watching TV in the SARI dataset, there are no variables reporting where TV is watched or the type of programmes viewed.

The data show that reported TV watching, by either respondent or by her account of her partner's watching, is not dependent on owning a TV. Reported TV viewing correlates positively with variables such as income per capita, owning a TV, being more educated, not being scheduled caste, and village having access to cable TV (Table 3; details in Supplementary Materials Table A2). Further, the coefficients on some of both the status (tolerance of beatings) and autonomy empowerment variables are positive and significant for households which do not own a TV (see Table 4). Thus, there are positive 'spillover' effects on those who do not own a TV, suggesting either that they have been watching TV in the houses of, generally better off, other households, or in public places, or have been affected by broader social interactions.

Table 3. 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 with TV	Households without TV	Households with TV	Households without TV
Households where wife has some education				
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 4. Spillover effects

Status variables	Owns TV	No TV	Autonomy variables	Owns TV	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)

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

Furthermore, these positive direct and indirect (spillover) effects occur approximately instantaneously rather than in the gradual manner observed, for example, with adoption of new agricultural technologies (Foster & Rosenzweig, 1995) or in the type of literacy sharing hypothesised to occur within rural households (Basu & Foster, 1998; Iversen & Palmer-Jones, 2008), or in the analysis of school enrolment using the DISE data (see below). While, as discussed above, Jensen and Oster (2007) refer to studies that support such rapid change, this usually relates to sharply focused campaigns, which contrasts with the more diffuse 'intervention' introducing cable TV represents. Given the panel fixed effects estimation,[26] this fuels concerns about unobserved time-variant confounding factors, rather than the arrival of cable TV, as underlying drivers of social change.

The growing importance attached to TV as a cost effective avenue for transforming the lives of the poor (for example La Ferrara, 2016), makes the analysis of heterogeneity pertinent. A notable feature of Supplementary Materials Table A2 is that having no education is associated with not reporting watching TV. We therefore explore the impacts of cable TV on the tolerance and autonomy of women

Table 5. Women's status and autonomy and effects of Cable TV

		Full sample
Outcome variable	variables	1
Tolerance of spousal beatings (mn_outcome)	village has cable	−0.320***
		(0.082)
	village has cable X woman is illiterate	0.26***
		(0.095)
		N = 7014
Female autonomy (mn_real)	village has cable	0.051***
		(0.012)
	village has cable X woman is illiterate	-0.041***
		(0.015)
		N = 7014
Son preference	village has cable	−0.14**
		(0.068)
	village has cable X woman is illiterate	0.096
		(0.077)
		N = 1699

with no education (who generally do not own or watch TV), by including a variable interacting with the education status of women with village has cable.

As can be seen in Table 5, for women without education, the 'empowering' effect of residing in a village with cable, in terms of reducing the tolerance of spousal violence, is close to negligible (= −0.32 + 0.26). 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 JO report do not extend to the majority of the women (53.8%) in SARI.

2.6. Cable TV and enrolment in education

JO provide two analyses which suggest that cable TV enhances school enrolment, particularly of young girls, using data from the SARI survey and from the DISE data, respectively. We find the data, code, and results for both problematic. Using the SARI data, JO present results at village level in Table V and at individual level in their online appendix. While the individual level results in JO's online appendix Table W3 are almost exactly reproduced, we cannot exactly replicate their village level data from the individual data, and our village level estimation data gives results which differ from those in JO Table V and Figures VI (a) and (b), and some are weaker. Our findings for the test of pre-trends are quite different.[27]

JO report that their DISE analysis shows that enrolment increases with duration of access to cable TV (that is not instantaneously, and non-increasing over time as in their SARI analysis). In our replication of the DISE estimation dataset and analysis we find minor mistakes in the data preparation code, and we differ from JO as to whether one variable should be recoded.[28] Using our data construction, the results of the analysis of the DISE data are too weak to be considered as evidence that duration of access to cable TV is associated with increases in school enrolment.[29]

2.7. SARI education[30]

The graphics and analysis of these data in JO follows the same pattern as with their analysis of the empowerment and fertility dependent variables. JO figures VI (a) and (b) show the enrolment of girls and boys aged six to 14 by access to cable TV and by year, and Table V reports the regression results of access to cable, years of access to cable, and the effects of including pre-trends. All these regressions are undertaken with village level aggregates although individual level analysis is reported in their online appendix.

The SARI estimation data and code provided by JO generally produce the results reported in JO, although with minor differences (not reported here). Using the covariates corresponding to their description in the footnote to Table V (which are not exactly the same as used in the code provided by JO – see our Supplementary Materials Table A4), and the village level data provided by JO, there are small differences for both girls and boys in the fixed cohort, while the results for the younger (six to 10) and older (11–14) children are almost identical to those in JO Table V. Similarly, the JO data and code for the individual level analysis produces results very similar if not identical to those reported in the online appendix (again not reported here).[31]

However, the village level estimation dataset provided by JO is significantly different from that we produce from the individual dataset, as reported in more detail in our Supplementary Materials even though the individual level dataset produces the results in JO's online appendix. Our village level dataset has very different values for 'gets cable next year' (see Table A6), produces quite different numbers of cases in some of the estimations, and a different interpretation of some graphs (for example Table A6 and Figure A1(a) and A1 (b)). While the coefficients (and numbers of cases for the fixed cohort) differ for Table V Panels A and B (Supplementary Materials Table A6), the substantive differences are in our reproduction of Panel C. Here we find that the variable 'gets cable next year' gets a negative and significant ($p < 0.05$) coefficient and makes the coefficient of 'vill_has_cable' insignificant for boys in the fixed cohort, and the young and older girls and boys, and significant and negative for girls in the fixed cohort (Panel C2 Table 6). Panel C1 in Table 6 shows the result where only villages which get cable in 2002 and 2003 are classified as 'getting cable next year'. Panel C2 adds the three villages which got cable in 2004 to those in Panel C1 as 'getting cable next year'. Panel C3 uses the JO variable for villages which get cable next year (see Supplementary Materials Table A7 for an explication of their pre-trends variable and Tables A8 and A9 for alternative specifications). Panels C1, C2, and C4 are run with our village level data since we have no explanation of how JO constructed their village level education data and hence no reason to prefer their construction. Only Panel C3 shows a positive coefficient on 'vill_has_cable' for young girls ($p < 0.01$), similar to JO Table V Panel C (where the coefficient on the fixed cohort of girls is also positive and significant, $p < 0.10$). The other three panels show negative and significant coefficients on 'gets cable next year' for the fixed cohort of girls and boys, and the six to 10 and 10–13 year old boys, implying depression of enrolment in the year before cable arrives.

It is not clear what to make of this, or the differences between our village level data and theirs. However, we do not think great significance can be assigned to these results because of lack of transparency of data construction, or, indeed, other results using the SARI data, because of the low statistical power of the SARI analysis as explained below.

3. Statistical power of the SARI analysis

Notwithstanding the statistical performance of JO's autonomy index and to a lesser extent the tolerance index and the results of the analysis of the effects on enrolment in education in the SARI data, some qualifications and caveats – in addition to those flagged in the preceding sections – about drawing any conclusions should be registered based on the low power of the identification strategy for the SARI survey analyses.

Many economics analyses are underpowered (Ioannidis & Doucouliagos, 2013; Ioannidis et al., 2016), leading to non-trivial risks of false positives (Button et al., 2013), which can be exacerbated by researcher or publication biases (Camfield, Duvendack, & Palmer-Jones, 2014; Maniadis, Tufano, & List, 2014).

The low power of the JO identification strategy can readily be demonstrated for a design consisting of the 21 treatment villages, out of a total of 180, that change their access to cable TV in the survey period. While there are no well-established methods of determining power in observational or quasi-experimental studies ex-post, it is not unreasonable to calculate power as if ex-ante using a natural experiment research

Table 6. Replication of Table V Panel C robustness test effect of pre-trends on enrolment, SARI data

	Fixed cohort		6–10		11–13	
Age Group	Girl	Boy	Girl	Boy	Girl	Boy
Panel C1: does not include villages which get cable in 2004[a]						
vill_has_cable	0.034	−0.125	0.068	−0.156*	−0.038	−0.082
	(0.066)	[0.076]	[0.052]	[0.090]	[0.099]	[0.083]
vill_has_cable + 1 (IVPJ)	−0.05	−0.131*	−0.066	−0.158**	−0.006	−0.053
	(0.054)	[0.074]	[0.067]	[0.071]	[0.063]	[0.060]
N	273	274	393	403	378	400
R^2	0.077	0.112	0.094	0.061	0.166	0.067
Panel C2: includes indicator of getting cable next year for villages which get cable in 2004[a]						
vill_has_cable	0.005	−0.126*	0.054	−0.141*	−0.062	−0.118
	(0.063)	[0.074]	[0.049]	[0.085]	[0.096]	[0.086]
vill_has_cable + 1 (IVPJ)	−0.089*	−0.130**	−0.082	−0.138**	−0.035	−0.098*
	(0.047)	[0.056]	[0.052]	[0.054]	[0.065]	[0.059]
N	273	274	393	403	378	400
R^2	0.081	0.114	0.096	0.062	0.166	0.071
Panel C3: IVPJ dependent, JO construction of the 'village has cable next year' and control variables[b]						
vill_has_cable	0.069	−0.029	0.119***	−0.051	−0.037	0.073
	(0.069)	[0.087]	[0.045]	[0.099]	[0.073]	[0.105]
vill_has_cable + 1 (JO)	−0.04	−0.006	−0.05	−0.031	0.100*	0.036
	(0.052)	[0.042]	[0.042]	[0.039]	[0.060]	[0.058]
N	273	274	359	355	338	351
R^2	0.094	0.209	0.111	0.03	0.102	0.098
Panel C4: IVPJ data and estimation construction indicators of year and relative to getting cable						
Gets cable (IVPJ)	−0.016	0.029	0	−0.162*	0.028	0.005
	(0.101)	[0.045]	[0.046]	[0.068]	[0.109]	[0.116]
Vill has cable + 1 (IVPJ)	−0.102	0.019	−0.146**	−0.121	0.053	0.016
	(0.120)	[0.106]	[0.051]	[0.087]	[0.096]	[0.080]
Vill has cable + 2 (IVPJ)	−0.025	0.14	−0.039	0.005	0.071	0.138
	(0.123)	[0.093]	[0.070]	[0.094]	[0.121]	[0.117]
N	273	274	393	403	378	400
R^2	0.081	0.115	0.104	0.075	0.169	0.075

Notes: [a]Authors' data construction in Panels A & B; JO village level dependent and control variables in Panel C including for vill_has_cable_next_year. [b]These results are repeated when we use JO's getting cable next year but IVPJ dependent and control constructions or JO's dependent and controls. Standard errors in parentheses, *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

design. Thus, there are four treatments which can be compared in two ways; the villages which get cable (21) can be compared with the 91 that always have cable. Or the treatment villages can be compared with the 68 that never have cable in the survey period. Since we are comparing proportions in the case of enrolment, we can use the Stata command 'power twoproportions' with these research designs to estimate power.[32] This procedure suggests that a sample size of 180 at differences in enrolment between villages with and without cable TV from 0.05 to 0.25 would have very low power.

Alternatively, we can compute power by simulation;[33] this involves generating datasets with characteristics similar to those of the natural experiment and, iterating numerous times, counting the proportion of datasets which yield a statistically significant effect. Figure 2 shows the results. We estimate the statistical power using 1000 random draws from a simulated panel dataset similar to the SARI design and effect sizes from 0.05 to 0.25.[34] The first panel shows that, with the exact village sample sizes of the SARI dataset, statistical power is very low for the modest effect sizes reported except when serial correlation

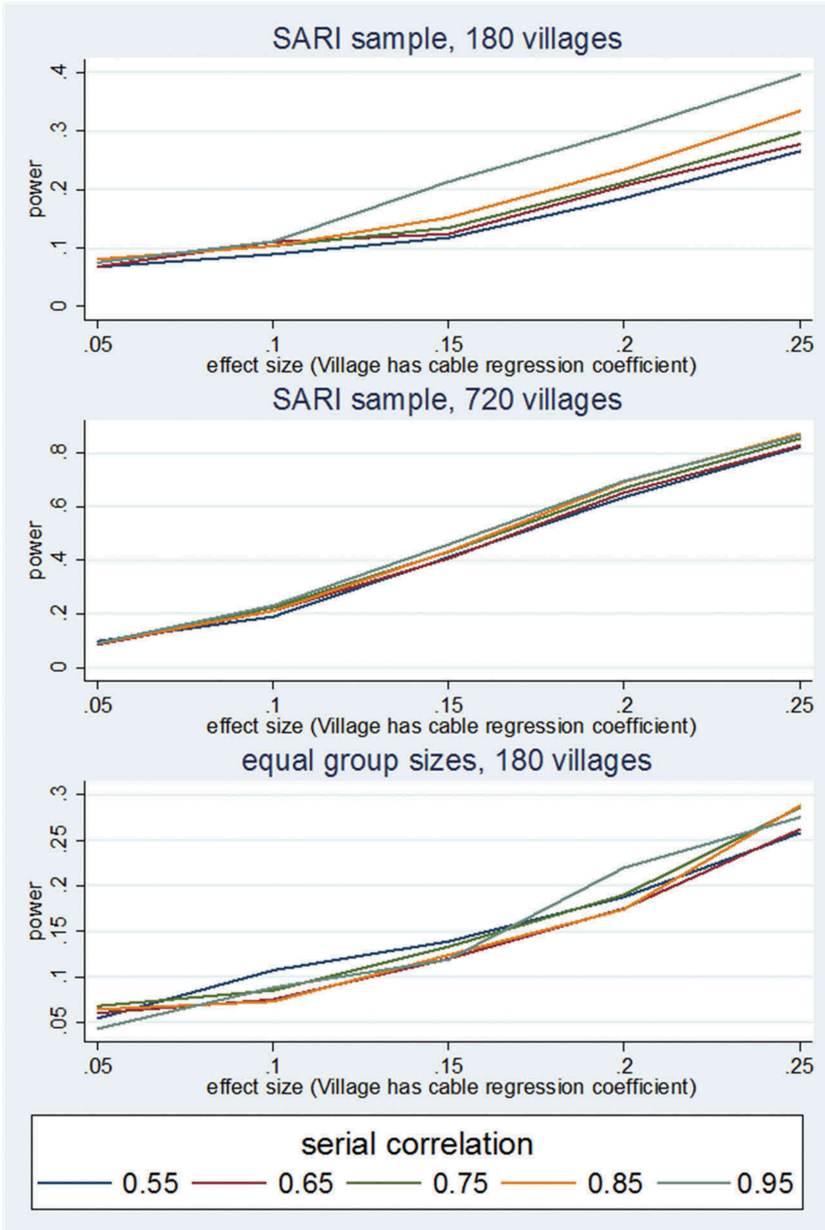

Figure 2. Simulation analysis of statistical power in a SARI type survey.

(assuming a Toeplitz structure) over years is very high (0.95). This is also true when the overall sample size is four times that found in the SARI survey, when the proportion of villages which get cable during the survey period is much higher than the 21 out of 180 villages in the survey, and where the standard deviation of the village level error term varies from one to three.[35]

Using either approach we see that for the modest effect sizes found in the SARI empowerment, fertility, and enrolment analyses (typically between 5 and 20%), and substantial village level error term, the sample sizes suggested to give adequate power (80%) are much larger than in SARI.

4. School enrolment in Tamil Nadu in District Information System for Education (DISE) data

For the DISE data, code for all processing and variable construction from the original (official) data was provided by Jensen and Oster, along with their original data. These data are the numbers enrolled by age and class of all schools in 19 randomly selected blocks of five districts of Tamil Nadu,[36] 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 2001–2007, as reported by Jensen and Oster[37]) and are used to estimate a variant of Equation (1) with different units of observation, estimation method, and covariate specification.

Following JO, the outcome variables are absolute numbers enrolled per village per year 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 (numbers of children in school) for six to 10 and 11–14 age groups; and for the cohort aged six to seven in 2002.

4.1. DISE analysis replication

While the dataset provided by Jensen and Oster[38] together with their estimation code produced all their published results, including their Figure VII,[39] our data constructions produced a somewhat different estimation dataset. This was particularly evident in our reproduction of Figure VII. In the original there appears to be a rise in enrolment of the fixed cohort over time; our reproduction shows a decline (drop outs would not have been uncommon).

This difference arises in part because of minor errors in JOs code,[40] and an important difference in treatment of villages which report zero enrolment (of the fixed cohort). JO recode the observations for villages which report zero enrolment in any year to missing for all years, and drop villages which have any zero enrolment for any year from the data in their Figure VII; the zero observations for these villages are, inexplicably, retained in their Prais-Winston estimations. No observations from these villages are dropped from either our reproduction of Figure VII (see Supplementary Materials) or the Prais-Winston estimations, and we can see no good reason for dropping them. We describe the recoding of zeros to missing also as an error; the reasons why we think the villages with zero enrolments should be retained in all analyses without alteration are in our Supplementary Materials (see also Iversen & Palmer-Jones, 2015).

Recalculating the variables of interest and re-estimating the descriptive statistics and the model proposed by JO, reported in their Table VI, but including both the changes we suggest, produces descriptive statistics of the affected variables different from their counterparts in JO Table II, Panel C Columns 1–3, and the estimated coefficients of Equation (1) also differ in equations involving the relevant variables.[41] The different treatment of schools with zero enrolment makes the main difference to the results.

For the version of Equation (1), estimated with our construction of variables from the raw DISE data (Table 7), the results are now weaker.[42] In Panel A, our coefficients using the same village level covariates as JO are smaller than theirs (JO Table VI). However, this model does not control for school quality, which, as we argue in Iversen and Palmer-Jones (2015), and in our Supplementary Materials, affects enrolment and is correlated with years of access to cable.

With the addition of variables representing school quality[43] and English medium teaching ('IPJ covariates' in Table 7 columns 3 and 4), the coefficients on 'years cable' are no longer significant.[44] Separating out the sample used by JO 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).

In the estimation using trends after 2002, only the coefficient on the fixed cohort 'boys' enrolment' is significant and actually negative (Table 7 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. A similar replication for the rolling age groups (6–10, 11–14) is shown in our Supplementary Materials (Table A10). It yields similar results which are less consistent and much weaker when estimated with the buildings quality

Table 7. Effects of cable on education in DISE data; IPJ dataset, fixed cohorts

	Fixed cohort 2002–2003				Fixed cohort 2003–2004			
	Jensen and Oster covariates[a]		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	0.034	0.028	0.018	0.007	0.003	0.026	0.003	0.023
	(0.72)	(0.60)	(0.40)	(0.16)	(0.09)	(0.71)	(0.08)	(0.63)
years of cable access	0.036**	0.037**	0.001	0.003	0.027**	0.032***	0.004	0.007
	(2.39)	(2.43)	(0.09)	(0.23)	(2.42)	(2.86)	(0.32)	(0.60)
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[a]	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	0.867	0.869	0.886	0.886	0.887	0.894	0.898	0.908
Panel B: Villages with cable after 2002; no block trends								
village has cable	0.060	0.045	0.042	0.023	0.019	0.042	0.015	0.035
	(0.047)	(0.046)	(0.045)	(0.044)	(0.038)	(0.038)	(0.037)	(0.037)
years of cable access	0.007	0.031	−0.040*	−0.014	0.004	0.015	−0.022	−0.015
	(0.022)	(0.021)	(0.021)	(0.020)	(0.017)	(0.016)	(0.016)	(0.015)
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	0.870	0.886	0.886	0.868	0.881	0.881	0.897
Panel C: All villages; block trends								
village has cable	0.053	0.042	0.017	−0.003	0.022	0.065*	0.001	0.034
	(0.047)	(0.047)	(0.046)	(0.046)	(0.037)	(0.038)	(0.036)	(0.037)
years of cable access	0.052***	0.063***	0.015	0.026	0.070***	0.079***	0.042***	0.048***
	(0.017)	(0.018)	(0.017)	(0.017)	(0.013)	(0.013)	(0.013)	(0.013)
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	0.895	0.902	0.904	0.913
Panel D: All villages; control for pre-trends								
village has cable	0.033**	0.032**	0.002	0.001	0.026**	0.030**	0.004	0.007
	(0.016)	(0.016)	(0.015)	(0.015)	(0.012)	(0.012)	(0.012)	(0.011)
years of cable access	−0.005	−0.013	0.007	−0.009	−0.009	−0.004	0.003	0.007
	(0.024)	(0.025)	(0.023)	(0.024)	(0.019)	(0.019)	(0.018)	(0.018)
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

(*continued*)

Table 7. (*Continued*)

	Fixed cohort 2002–2003				Fixed cohort 2003–2004			
	Jensen and Oster covariates[a]		IPJ covariates		Jensen and Oster covariates		IPJ covariates	
	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys
	1	2	3	4	5	6	7	8
buildings and English language controls	N	N	Y	Y	N	N	Y	Y
N	4284	4302	4253	4272	4777	4800	4744	4767
R^2	0.867	0.869	0.886	0.886	0.887	0.894	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. [a]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.

variable and the English medium school dummy (Columns 3, 4, 6, and 7) than when estimated using the JO estimation specifications, and with our data construction rather than the JO data construction in 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, especially of girls. There is no visual evidence in Figure A4(a) or A4(b) to support an argument for an increase in enrolment in the year when, or year after, respondents gained access to cable TV. Furthermore, the evidence from the time series estimation (Table 7) is weaker than in JO using their model specification, 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 likely to increase enrolment. JO acknowledge this weakness.[45] Covariates which are not available would include the availability of supportive services which the literature discussed above suggests are pertinent to pro-social outcomes from exposure in the media to empowering messages.

5. Conclusion

In this paper we argue that media effects theory and evidence raise doubts about the pro-social effects of modern media exposure unsupported by other interventions, and that gender and development theories and evidence disagree with the implications of the results reported by JO. Their results for the SARI dataset can be replicated using their estimation datasets and code. Adjustments for questionable variable constructions and external validity concerns, reduce the statistical significance of JOs tolerance of spousal violence index: however, the coefficient on their autonomy index remained significant at a 1 per cent level for a variety of specifications. For alternative measures of tolerance and autonomy, drawing on Basu and Koolwal (2005), and Self-Determination Theory, we find no cable TV effect on tolerance, while the autonomy result is weaker.

With our construction of the village level SARI education dataset, we find unexplained constructions of variables, and differences between the estimation specifications in the code and those described in JO's text. While for the most part our attempt to replicate what JO report does not have substantive effects on the results, except in the case of school enrolment, it does affect some robustness tests, specifically whether pre-trends may confound the effect of getting cable. Further, we show that the statistical power of the estimations of the effects on enrolment (and other outcome variables) using the SARI data and effect sizes, is very low, making false positives not unlikely. We also detected errors in and suggest plausible alternatives for the construction of some variables in the DISE dataset that, once altered, 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 plausible school quality (or possibly school size) variables that confound the estimated relationship between 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 JO's aggregated results. More extensive analysis of the SARI dataset, which resembles the analysis undertaken by Jensen and Oster (2007), including a closer scrutiny of male and female viewing habits and disaggregation by whether women have education or not – confirmed important heterogeneity on the impact on JO's two main indicators of female empowerment; specifically there is no effect for uneducated women (more than 50% of the women in sample).

The replication was greatly facilitated by ready access to final data and code for analysis, although a proper assessment of JO's contribution would have been more complete had the SARI survey instruments and raw data been available, together with all data and variable construction code. JO would have been strengthened had it included more of the results reported in Jensen and Oster (2007). Using comparable data (NFHS2 and NFHS3), we were unable to strongly support the external (and, to some extent, internal) validity of the study. 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 JO's. Our replication benefitted from initially constructive interaction with the original authors, but was hampered by restricted communication later.

JO's focus on introduction of cable TV suggests that the main lesson for policy is to accelerate access to cable TV, with no need for (i) concern about TV programme content; (ii) mitigation of what some would construe as the disempowering images of women often occurring on cable TV; (iii) supportive interventions to counteract such messages and to support pro-social outcomes. However, our results suggest contradictions in the intensity and possible nature of the externalities accruing to women from households without TV. This may imply that less straightforward underlying processes of social change may be at work, with rather different implications for interventions to empower women, especially for those without TVs and those apparently unaffected by whatever processes had been at work (such as the uneducated).

Our replication has value as not only confirming that the code and data largely reproduces the results reported while finding some minor errors, indicating a nuanced and qualified understanding, which considerably qualify the conclusion of an apparent beneficial impact of the introduction of cable TV. This effect is largely restricted to an already relatively privileged group (those with some education). Besides reinforcing the uses of replication to identify errors and alternatives in the coding of data construction and estimation, and for subgroup analysis (identifying impact heterogeneity), a significant result of the replication would appear to be that there needs to be further research to tease out whether, for whom, by what pathways and mechanisms, and under what conditions, general TV programming (and pro-social programming) may have beneficent impacts, if any. It seems unwise to rely much on the mere spread of cable TV.

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Disclosure statement

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Notes

1. 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 JO as short for Jensen and Oster (2009) in the *Quarterly Journal of Economics* and Jensen and Oster (2007) when referring to their NBER Working Paper. The former never mentions the latter.
2. Ting, Ao, and Lin (2014) find support for TV as a catalyst for improvements in women's status in rural India using a wider set of measures and Propensity Score Matching to tackle endogeneity but are unable to address the speed of change.
3. Inspired by findings from six TV novellas that aimed to promote gender equality in Mexico, India broadcast its first pro-development soap opera 'Hum Log' in 1984–1985 (Brown & Cody, 1991). 'Hum Log' featured thrice weekly over an 18 month period and achieved record-popular audience ratings of 90 per cent in North and 40 per cent in South India. In spite of subtle promotion of pro-social beliefs and behaviours and viewers' intense involvement with serial characters, exposure did not increase viewer awareness of women's issues.
4. Carefully tailored campaign-type interventions, in this case a specially produced entertainment movie about MNREGA in India, positively affected perceptions but had no effects on behaviour, scheme delivery or outcomes (Dutta, Murgai, Ravallion, & van de Walle, 2015).
5. We use slightly different definitions of the three categories of replication described by Hamermesh (2007, p. 716). See also Easley, Madden, and Dunn (2000) and Brown, Cameron, and Wood (2014).
6. Our own code to reproduce all the results derived from the SARI data in JO is available in Supplementary Materials. For the SARI analysis we have differences with Jensen and Oster regarding panel B of Table IV and panel C of Table V; our computation of the variable representing the 'pre-trend' differs from theirs, but makes no apparent meaningful difference to the results of Table IV but does to those of Table V. Our data preparation code for Table V produces differences in the estimation dataset (see below).
7. Such categorisations may, of course, be contested. For other critical engagement with DHS-based indicators of women's status, see Ghuman, Lee, and Smith (2004) and the discussion of Self Determination Theory below.
8. PCA and MCA based measures of autonomy have similar coefficients and 1% statistical significance (see Supplementary Materials).
9. According to Jensen and Oster (2007, p. 19):

   > 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 [. . .] 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.

10. La Ferrara (2016) illustrates why clarity about such heterogeneity may be essential: 'poor countries with low literacy rates clearly constitute an environment where the easily accessible "language" of television and radio gives them a comparative advantage over other means of communication'. This argument becomes much weaker with the knowledge that there is no effect for illiterate women in rural India.
11. We refer the interested reader to IPJ for a more extensive discussion of the literature on media and social change.
12. 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 of IPJ.
13. 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 present in all years for a village; JO set electricity to 'present' in each year 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. We could not merge the DISE villages with village data from the Indian census, which would have provided more consistent, and more contextual, variables, as also noted by JO (p. 1087).
14. The Delhi sample is predominantly urban, while the rest are all rural.
15. Jensen and Oster informed us that the SARI dataset to which we have had access is identical to the dataset they received from their Indian counterparts who were responsible for survey implementation.
16. We use the original roman numerals to reference Jensen and Oster's tables and figures and digits for figures and tables in this paper and the Supplementary Materials.
17. See discussion in IPJ.
18. JO uses simple Borda indexes (simply adding the variable values together). IPJ explores whether results are sensitive to index construction methods (Everitt & Dunn, 2001; Chatfield & Collins, 1980); employing the same variables in PCA and MCA indexes (Filmer & Pritchett, 2001; Kolenikov & Angeles, 2009). We found no meaningful differences due to index construction methods. Jensen and Oster (2007) present a somewhat similar exercise in their appendix Table 1, but this paper and its results are not mentioned or referred to in JO.

19. The DHS questions are located at www.measuredhs.com/What-We-Do/Survey-Types/DHS-Questionnaires.cfm.
20. Our approach here, which Berk Özler criticised, closely resembles that in Jensen and Oster (2007). According to Özler 'to explore which component of the index is most responsible for the observed effect [. . .] exactly the kind of thing that editors recommend cutting from final publications – unless such analysis leads to important insights'. As noted by Basu and Koolwal (2005), there may be compelling theoretical grounds for treating components as potentially having different meanings and implications. We do find such insights.
21. The permission to visit relatives or friends variable appears to lead to a double count since it closely resembles and is highly associated with question (iii) (whether the respondent will visit or stay with family or friends – Pearson chi2(2) = 177.2842 Pr = 0.000) although there are striking inconsistencies; 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.
22. This question is omitted in NFHS3, so the equivalent column in the third panel of Figure 2 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, Kishor and Gupta (2009), Schoumaker (2009), and Schatz and Williams (2011).
23. Notice also in Figure 1 and in spite of the strong increase in the access to TV, the formidable rise in tolerance of beatings for women's infidelity between NFHS2 and NFHS3 in Bihar, Tamil Nadu, Goa, and Haryana.
24. 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).
25. 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.
26. Is it plausible that school enrolment will immediately increase (if indeed it did) with the arrival of cable TV?
27. JO have declined to comment on requests for clarification of their SARI education data constructions (email Jensen and Oster, 11/02/2017).
28. Before discussions with JO, we concluded that this latter issue was a mistake, since we could see no justification for it. Subsequent exchanges have not altered our view as to the error of their treatment of this variable (see Jensen & Oster, 2014; Iversen & Palmer-Jones, 2015).
29. IPJ provide an extended discussion of the DISE data processing and analysis.
30. Due to an oversight, our earlier works omitted replication of Figures 6 (a) and (b) and Table V in JO.
31. Since we do not have the raw survey data, we cannot comment on the construction of variables at individual level, but see comments on the 'educ2' variable in our Supplementary Materials.
32. For example 'power twoproportions .7, test(lrchi2) diff(.1 .15 .2 .25) power(.2 .3 .4 .5 .6 .7 .8) nratio(.1) table(N) graph (yline(180))'. Supplementary Materials Figure A1 shows that for the modest effect sizes estimated in JO (18%) the total sample sizes required (assuming equal proportions of treated) is much larger than the 180 villages in the SARI survey; conversely, that only for large effect sizes ($p_2 - p_1 > 0.25$) would samples of the size of SARI yield acceptable power.
33. http://www.stata.com/support/faqs/statistics/power-by-simulation/ and http://blog.stata.com/2014/07/18/how-to-simulate-multilevellongitudinal-data/ .
34. Because 'vill_has_cable' is a dummy variable, it corresponds to the difference between the means of the villages with and without cable.
35. The smaller the standard deviation of the error term, the more precisely the effect of cable is estimated.
36. Since each district has at least one block, districts are strata; no weighting is used to account for different populations of the chosen blocks.
37. A final dataset 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 as we cannot confirm that they do not have the variable calculation problems we identified.
38. By this we mean the dataset produced using the original Jensen and Oster code sent to us.
39. Our code and these results are available on request.
40. The errors are acknowledged at http://faculty.chicagobooth.edu/emily.oster/papers/update.pdf and we do not discuss them further here.
41. JO 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 since large changes in the number of schools in a village seems unlikely.
42. Compare Table 7 columns 1 and 2 with JO Table VI columns 1 and 2.
43. These variables are not well described in the dataset, 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. JO 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. While some variables in the school quality indicator may reflect school size, which is also reflected in the dependent variable, this is not necessarily the case with the English language medium indicator. To the extent this is a problem, the problem lies in the use of total enrolment rather than enrolment as a ratio as the dependent variable.
44. Other results not presented here show that it is the 'buildings quality' variable that eliminates the significance of the 'village has cable' variable.

45. 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.

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