Restricting Online Pornography and Violence Against Women: Evidence from the United States and India

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Abstract

Governments increasingly restrict online pornography in an effort to reduce violence against women. The effect of such restrictions is theoretically ambiguous. Pornography may increase aggression through exposure to degrading or violent content, but it may also displace behaviors that create opportunities for violence. We study two large restrictions in two countries and find opposite effects. In the United States, state age-verification laws raised the cost of access, and induced the largest platforms to withdraw from the treated states. Exploiting the staggered adoption of the age-verification laws, as well as a comparison of adjacent counties across states, we find a decline of 1.4 (3%) monthly non-sexual assaults against female victims per 100,000 residents. In India, a 2018 nationwide order blocked 827 pornographic websites. We compare places with higher internet access before and after the policy, and instrument with a measure of how costly it was for a geographic location to access the internet based on the number of lightning strikes in the area. We find that the policy ban increased annual rape incidents by 1.05 per 100,000 residents in India, while crimes not specifically directed at women do not rise. Both policies materially reduced access. Evidence on circumvention and substitution, offender age, and local age composition and household structure suggests that the balance between exposure and behavioral substitution differs across settings. Pornography restrictions therefore can have a differential effect on violence against women, and evaluating them requires attention to how users adjust to the regulations and to the local context.

Keywords: pornography, violence against women, sexual violence, internet regulation, age verification, difference-in-differences.

JEL codes: J12, J16, K42, L86, O33.

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1 Introduction

The largest pornography website received 3.8 billion visits in July 2026, ranking it ninth among all websites in the world, ahead of WhatsApp, TikTok, and Amazon.[1] Restricting access to pornography is however the norm rather than the exception. Pornography is illegal to produce, distribute, or possess in at least 97 countries that together hold 73 percent of the world's population (Figure 1). Among countries where it is legal, multiple policies still try to limit access, through internet-service-provider blocks, age-verification mandates, or platform liability.

A central justification for these restrictions is that pornography contributes to violence against women, which nearly one in three women worldwide has experienced (World Health Organization, 2021). Since 2016, sixteen U.S. state legislatures have declared pornography a public health crisis, citing its contribution to sexual violence among the harms.[2] The Indian blocking order we study was issued by a court hearing the case of a group rape by schoolboys who, according to the police, had watched pornography beforehand. The premise is that less exposure means less violence. These interventions now affect hundreds of millions of users, and there is no causal evidence on whether they reduce violence against women.

The sign of the effect is ambiguous in theory. Pornography can increase violence by exposing users to degrading or aggressive content that shapes how they treat women. It can also reduce violence by occupying time in which potential offenders would otherwise encounter victims, or by providing a private outlet that lowers the immediate propensity to offend (Dahl and DellaVigna, 2009). Which channel dominates is an empirical question. While laboratory and survey studies estimate the association between an individual's consumption and their attitudes or self-reported aggression, we instead ask whether policy bans on pornography reduce violence against women, and under what conditions. Studying a policy ban differs from studying consumption directly, because a ban lowers consumption for some users, leaves it unchanged for those who circumvent, and shifts what is consumed toward sources that remain reachable, including sites that don't comply with the law and whose content may differ from that of the blocked sites. This is likely the object of interest for policy makers.

We answer this question by studying two large bans and find opposite signs. U.S. state age-verification laws, which led the largest platform to withdraw from treated states, reduced non-sexual assaults against women by about 3 percent. India's 2018 nationwide blocking order raised reported rape by roughly a third in the districts most exposed to it. Both policies materially reduced access, and in both countries users partly circumvented them. Evidence on who changed behavior and where the effects fall suggests that the balance between exposure and substitution differs across settings.

[1] Data for Pornhub from the global ranking for July 2026 of Semrush Traffic Analytics. The eight sites ranked above it are Google, YouTube, Facebook, Instagram, ChatGPT, Reddit, Wikipedia, and X. Similarweb, which uses a different methodology, ranks the site 25th for August 2026. The last figures Pornhub disclosed itself, for 2019, were 42 billion visits in the year and 115 million per day.

[2] Utah passed the first such resolution in April 2016. Fifteen states followed with similar language by 2019 (Governing, "Is Porn a Public Health Crisis? 16 States Say Yes", 2019).

In the U.S., Louisiana enacted the first age-verification law in 2022, and by the end of 2024, fifteen states had one in effect. The laws require pornographic websites to verify that visitors are adults, which raises the cost of access for every user of a compliant site. Pornhub's decision to withdraw from these states raised it further. Searches for the platform fall abruptly at states' own effective dates while comparison states are stable, and browsing data show that total time on adult websites fell by about 10 percent despite circumvention (Brown et al., 2026). Using the staggered timing of the laws and county-month crime records between 2022 and 2024, we find that assaults against female victims fall by 1.3 monthly incidents per 100,000 residents, or 3 percent of the outcome mean, while rape and other sexual offenses do not measurably change. Comparing crime rates in the same month between adjacent counties that share labor markets, media markets, and demographics but sit on opposite sides of a treated border, we also find a decline in assaults against women of a similar magnitude.

In India, the restriction was a nationwide blocking order. In October 2018, after a court ordered the government to enforce a dormant 2015 list, the Department of Telecommunications instructed every internet provider to block 827 pornographic websites. The block was implemented at the network level, and measurements from the Open Observatory of Network Interference show that the share of tests classifying pornography domains as inaccessible rose from 16 to 48 percent, and to near 90 percent for the most visited domains. Because the order applied everywhere at once, we compare districts by their exposure to it. The block only binds where people already had mobile internet, so districts with 3G or 4G coverage before the ban lost more access than districts that were connected only afterward. Because internet connectivity is not random, we adapt an instrumental variable approach from Manacorda and Tesei (2020) and Guriev et al. (2021) that exploits the frequency of lightning strikes in a geographic area. Lightning raises the cost of building and maintaining mobile infrastructure, and because the equipment that protects towers is expensive, it predicts lower coverage in poorer states. Using district-year crime records between 2011 and 2022, we find that the ban raised reported rape by 1.05 annual cases per 100,000 residents, or 37 percent of the outcome mean, and assault against women by 2.2 cases, or 32 percent, although the assault estimate is imprecise under weak-instrument-robust inference. Murder, theft, burglary, motor-vehicle theft, and aggregate property crime do not rise, so the pattern is difficult to reconcile with a generic post-ban crime shock or a change in reporting.

We interpret the differential effects across settings in three parts. First, both policies raised the cost of access rather than eliminating it, and users in both countries responded in the same way. Searches for VPNs and proxies rose at the policy date and remained well above their pre-policy level a year later, and searches shifted from the restricted sites toward sites and generic terms that stayed reachable. Our estimates are therefore effects of making access more costly, and the sign reversal is not a comparison between a policy that worked and one that was ignored. Second, local characteristics shape the size of the effects, but none of them on its own explains why the sign differs. Income does not matter in either country. In the United States, the decline is larger in counties with more young adult men and is concentrated among adult alleged offenders rather than

the minors the laws were written to protect. In India, the increase in assault is concentrated where women's relative position is weakest, in districts with fewer female-headed households and larger literacy gaps between women and men, and it is close to zero in districts nearer parity. Measures of gender attitudes are less informative. Third, the two countries are too different for one to provide useful evidence for the other: even the Indian districts closest to gender parity lie far from the typical U.S. county. These patterns cannot separate differences in who changed consumption from heterogeneous reactions to the ban. What they show is that the same kind of restriction can affect different populations in different environments, so that the relative weight between the mechanisms of exposure and substitution changes with the setting.

Our main contribution is to provide causal evidence on pornography restrictions at the scale of actual policies, and to show that their effect varies across settings. The question has been studied for decades in psychology and public health, mostly with cross-sectional surveys and laboratory experiments (Peter and Valkenburg, 2016), and that literature is divided. Wright et al. (2016) find that consumption is associated with self-reported sexual aggression in general-population samples, and Malamuth et al. (2000) and Hald et al. (2010) report that exposure predicts attitudes supporting violence against women, especially among men already at risk. Ferguson and Hartley (2022) reanalyze the same studies and find no reliable link once study quality and publication bias are taken into account, and Grubbs and Kraus (2021) conclude that use plausibly has effects in both directions. Our paper adds to this literature by providing field evidence from a policy that changed access, and identifying its effect on sexual violence.

Economists have studied the question in the field, for media in general and pornography in particular, and their evidence turns on the tension between exposure and substitution. Dahl and DellaVigna (2009) show that violent movies reduce violent crime on the evenings they screen: the films keep likely offenders occupied, and this incapacitation effect outweighs any effect of the violent content itself. The same logic applies to pornography, but studies that use the spread of the internet as a shock to its availability disagree on which channel is larger. Kendall (2007) finds that early internet access in U.S. states lowered reported rape and attributes the decline to substitution away from offending. Bhuller et al. (2013) find that broadband rollout in Norway raised sex crime, and Gibbons and Rossi (2021) document a rise in rapes during a YouTube outage, which they attribute to a shift toward pornography viewing. In each case, pornography is only one of many things that internet access changed simultaneously. We study policies aimed at pornography itself, and we show that the opposing signs in this literature can arise from the same two channels: which prevails depends on how a restriction changes actual consumption, who is affected, and the environment in which the behavioral response occurs.

The rest of the paper proceeds as follows. Section 2 describes the two policies and the evidence that they changed access. Sections 3 and 4 present the data, empirical strategy, and results for the United States and India. Section 5 examines how users adjusted and where the effects fall. Section 6 concludes.

2 Two Policies

Figure 1 maps the legal status of pornography by country. Pornography is illegal to produce, distribute, or possess in 97 countries that together hold 73 percent of the world's population, and it is partially restricted in 8 more countries holding another 7 percent.[3] Most of these countries have low but rapidly rising internet access, so the restrictions bind for a growing number of people. The two policies we study come from opposite ends of this map. In the United States, where pornography is legal, restrictions have taken the form of state laws on access. India, the most populous country in the world, belongs to the group of countries where it is illegal.

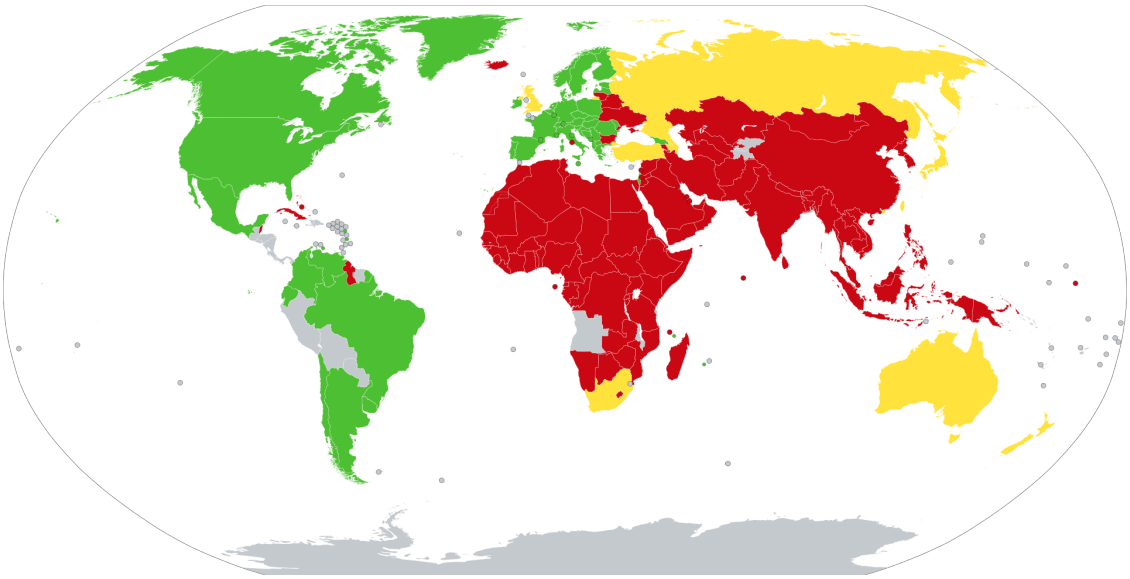


Figure 1: Legal status of pornography by country

Notes: Green marks countries where pornography is legal, yellow countries where it is partially legal or restricted, and red countries where it is illegal. Gray marks countries without data. Source: Wikipedia, "Pornography laws by region."

2.1 United States: state age-verification laws

Louisiana enacted the first state age-verification law in June 2022, effective January 1, 2023. The law requires any website whose content is more than one third pornographic to verify that visitors are adults, through government identification or a digital-identity service, and lets residents sue sites that do not comply. Other states adopted similar statutes, with enforcement by the attorney general in most of them. Figure 2 shows each state's effective date. Fifteen states had a law in effect by the end of 2024, and twenty-seven did by mid-2026. The Supreme Court upheld the Texas statute in June 2025, removing the main legal challenge to the laws.[4]

[3] Our tabulation of the country classification underlying Figure 1, using United Nations population estimates for 2023. The illegal category includes countries such as India where production and distribution are banned but private viewing is not. India and China alone account for 35 percent of the world's population.

[4] *Free Speech Coalition v. Paxton*, decided June 27, 2025.

The laws raised the cost of access in two steps. The verification requirement itself imposes a cost on every visitor to a compliant site, who must hand over identification to view legal content. Pornhub's response then raised the cost further. In Louisiana its parent company complied through the state's digital-identity wallet and reported that traffic from the state fell by about 80 percent. In every other state whose law took effect, with the exception of Ohio, it blocked all visitors from the state rather than collect identity documents, arguing that verification on individual sites endangers user privacy and pushes traffic toward sites that do not comply. Because Pornhub is the largest provider, its withdrawal removed the main legal source of pornography for residents of those states. The blocks usually began within days of the effective date, as Appendix Table 4 shows state by state. Texas is the one large exception, because the law was enjoined until March 2024. No state has regained access since.

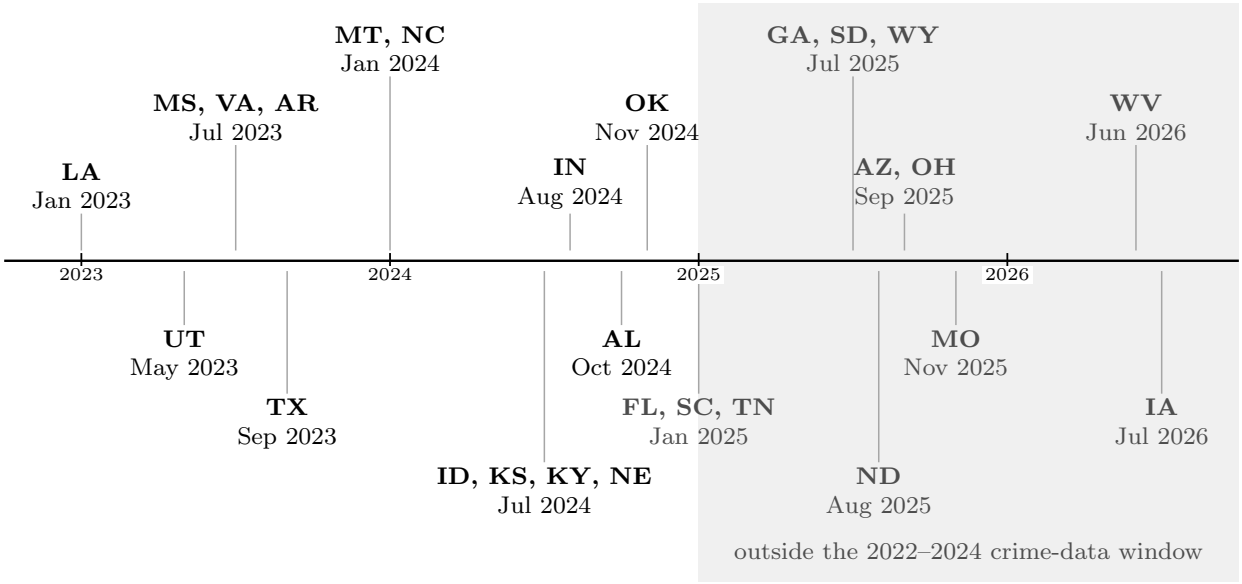


Figure 2: Effective dates of U.S. state age-verification laws

Notes: Each pin marks the month in which a state's age-verification law took effect. States with the same month share a pin. The shaded area covers dates after the 2022–2024 crime-data window. States in that area serve as not-yet-treated comparison states in the analysis.

Search behavior confirms that these laws changed access. Figure 3 aligns Google search interest for Pornhub around each state's effective date. Searches fall sharply in treated states at event time zero, while the comparison series is stable. The graph uses the 14-state search sample for which a consistent historical series was assembled, not the larger crime sample, and serves as descriptive validation. Using individual browsing histories, Brown et al. (2026) estimate that the restrictions reduced total time spent on adult websites by approximately 10 percent, despite circumvention through virtual private networks and substitution toward noncompliant sites.

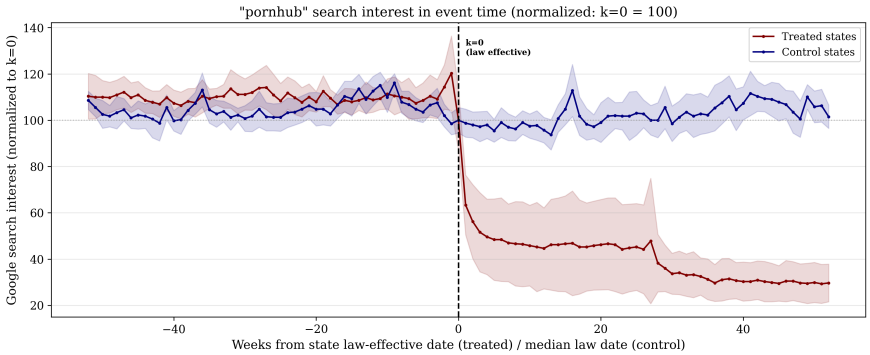


Figure 3: United States: Pornhub search interest around effective dates

Notes: Google Trends search interest for Pornhub around state age-verification effective dates. The figure uses a 14-state validation sample (eight treated and six comparison states) and is not the estimating sample for crime outcomes. It measures platform search interest, not consumption.

2.2 India: a nationwide blocking order

India's Information Technology Act allows the government to direct internet service providers to block online content. The first attempt to use it against pornography came in July 2015, when the Department of Telecommunications ordered providers to block 857 websites. The order drew immediate public criticism, and within days the department told providers they were free to unblock any site that did not host child pornography.

The order was revived three years later. In September 2018, the Uttarakhand High Court took up the case of a group rape at a boarding school in Dehradun, committed by fellow students who had told the police that they watched pornography before the attack. On September 27, 2018, the court directed the government to enforce the 2015 list. The Ministry of Electronics and Information Technology received the order on October 8, and the Department of Telecommunications instructed all licensed providers to block 827 of the 857 websites, dropping 30 that no longer hosted pornographic content. The order applied to the whole country and was implemented at the network level, so every subscriber of a complying provider faced the same block. Because users could still reach blocked sites through virtual private networks (VPNs) or mirror domains, the policy raised the cost of access rather than eliminated it.

Measurements from the Open Observatory of Network Interference (OONI) show that the cost rose substantially. Figure 4 plots the quarterly share of OONI tests of pornography domains in India classified as inaccessible. Across 127,382 tests of 29 pornography domains between 2017 and 2025, the share classified as inaccessible rose from 15.8 percent before the order to 48.0 percent afterward, an increase of 32.2 percentage points. The most visited domains show post-order inaccessibility rates near 90 percent. The break after October 2018 is large and persistent. Together with the U.S. evidence, this establishes that both policies had bite, so the empirical designs are not comparing nominal laws with no market response.

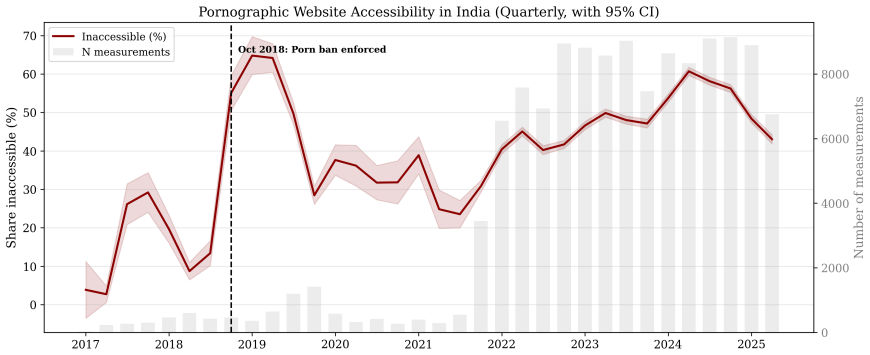


Figure 4: India: share of pornography domains inaccessible

Notes: Quarterly share of OONI tests of pornography domains in India classified as inaccessible, with 95 percent confidence intervals. The vertical line marks the October 2018 blocking order.

3 United States: Restrictions Reduce Assaults Against Women

3.1 Data and empirical strategy

The U.S. laws took effect state by state between January 2023 and November 2024, so identification comes from the staggered timing of adoption. We treat each law and the platform withdrawal it triggered as one restriction and date treatment by the law's effective month. Fifteen states are treated within the crime-data window. Ten further states adopted laws that took effect in 2025 or later and serve as not-yet-treated comparison states.

The analysis uses NIBRS victim-segment records aggregated to county-month cells from 2022 through 2024. The main sample contains 97,875 observations from 2,983 counties and covers roughly 95 percent of the U.S. population. Agencies in these counties record about 2.9 rapes and 43 assaults against female victims per 100,000 residents each month; administrative counts understate both because most incidents are never reported (Palermo et al., 2014). Sexual offenses combine rape, fondling, and statutory rape. Assault combines aggravated assault, simple assault, and intimidation, and we split it by victim sex. Assault is therefore a non-sexual category of violence, and the two groups are mutually exclusive under the primary-offense coding used in the analysis. The female-assault outcome is broader than completed sexual violence and may include intimate-partner violence, threats, and other violence directed at women.

Treatment varies at the state-month level. Let g_s denote the month in which state s's law took effect and let $D_{st} = \mathbf{1}\{t \geq g_s\}$. Appendix Figure 10 maps the counties in each treatment group. The natural starting point is the two-way fixed-effects regression

$$y_{cst} = \alpha_c + \lambda_t + \beta D_{st} + \varepsilon_{cst}, \tag{1}$$

with county fixed effects α_c and calendar year-month fixed effects λ_t, and standard errors clustered by state, the level of policy assignment. We do not include state-by-year fixed effects. Treatment switches on at the state-month level and never switches off, so state-by-year fixed effects absorb all

treatment variation in state-years that are treated throughout and leave the effect identified only from each state's adoption year. In our panel they remove about 60 percent of the residual variation in D_{st}.

With staggered adoption, β has a known weakness. It is a weighted average of every two-group, two-period comparison in the panel, and some of those comparisons use states that adopted earlier as controls for states that adopt later (Goodman-Bacon, 2021). If the effect of a restriction changes with time since adoption, those comparisons subtract a moving treatment effect from the estimate. Some cohorts can then receive negative weight, and β can take the wrong sign even when the effect has the same sign in every state (de Chaisemartin and D'Haultfœuille, 2020). In this case, fifteen states adopt over twenty-three months, and the behavioral response to losing access can plausibly build or fade as users adjust.

Our preferred estimates therefore use the staggered difference-in-differences estimator of Callaway and Sant'Anna (2021). For each adoption cohort g and month $t \geq g$, the group-time average treatment effect is

$$ATT(g,t) = E\left[Y_{ct} - Y_{c,g-1} \mid G_c = g\right] - E\left[Y_{ct} - Y_{c,g-1} \mid D_{s(c)t} = 0\right], \tag{2}$$

where c indexes counties, Y_{ct} is incidents per 100,000 residents, G_c is the adoption month of the county's state, and the comparison group consists of counties in states not yet treated in month t. Each $ATT(g,t)$ is estimated with the doubly robust inverse-probability-weighted estimator of Sant'Anna and Zhao (2020). We report the average of $ATT(g,t)$ over all post-treatment cohort-months, weighted by cohort size, and event-time averages that pool the $ATT(g,t)$ with the same distance $t-g$ from adoption. The estimator never uses already-treated states as controls, so the comparisons that distort β in equation (1) do not enter. Inference is again clustered by state. We also repeat the design among counties adjoining state borders, mapped in Appendix Figure 11, and report a comparison with border-segment-by-month fixed effects in Table 2. We also report imputation estimates that fit untreated outcomes using the same fixed effects and average the differences between observed and imputed untreated outcomes after adoption.

3.2 Results

Panel A of Table 1 compares TWFE, Callaway–Sant'Anna, and imputation estimates for the entire county panel. The clearest response is in assault against female victims: a decline of 1.288 monthly incidents per 100,000 residents, equal to 3.0 percent of the outcome mean. Total assault falls by 1.878 incidents, while completed sexual offenses show no detectable change. The distinction is substantively informative. U.S. age-verification laws do not appear to shift the most severe, narrowly defined sex offenses over this horizon. They instead move a broader margin of violence and intimidation against women.

The event-time estimates in Figure 5 support this reading. The full-sample Callaway–Sant'Anna event study shows no systematic pre-treatment movement in female-victim assault and a negative

Table 1: United States: restriction effects in the full panel and border counties

	Rape			Assault, female victims		
	(1) TWFE	(2) CSDID	(3) Imputation	(4) TWFE	(5) CSDID	(6) Imputation
Panel A. Entire county panel						
Policy effect	-0.029	-0.018	-0.006	-1.591**	-1.288	-1.585**
	(0.081)	(0.208)	(0.075)	(0.717)	(0.873)	(0.732)
Observations	97,875	96,597	97,817	97,875	96,597	97,817
Counties	2,983	2,949	2,989	2,983	2,949	2,989
State clusters	50	50	50	50	50	50
Outcome mean	2.912	2.921	2.913	43.131	43.295	43.123
Panel B. Border counties						
Policy effect	0.047	0.135	0.065	-2.114**	-2.509**	-2.244**
	(0.139)	(0.180)	(0.110)	(0.917)	(1.268)	(0.927)
Observations	36,717	36,261	36,720	36,717	36,261	36,720
Counties	1,117	1,105	1,120	1,117	1,105	1,120
State clusters	47	47	47	47	47	47
Outcome mean	2.817	2.832	2.816	43.447	43.611	43.447

Notes: Outcomes are monthly incidents per 100,000 residents, January 2022–December 2024. Columns 1–3 report rape and columns 4–6 assaults against female victims (aggravated assault, simple assault, and intimidation). Panel A uses the entire available county panel; Panel B restricts to counties adjoining state borders. TWFE includes county and calendar year-month fixed effects. CSDID reports Callaway–Sant'Anna aggregate treatment effects using not-yet-treated controls and doubly robust inverse-probability weighting. Imputation fits untreated outcomes with county and calendar year-month fixed effects and averages post-treatment effects. Standard errors, in parentheses, are clustered by state. Observations and county counts refer to each estimator's sample; sample requirements differ across estimators. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

post-treatment response. The border-county event study displays a similar pattern.

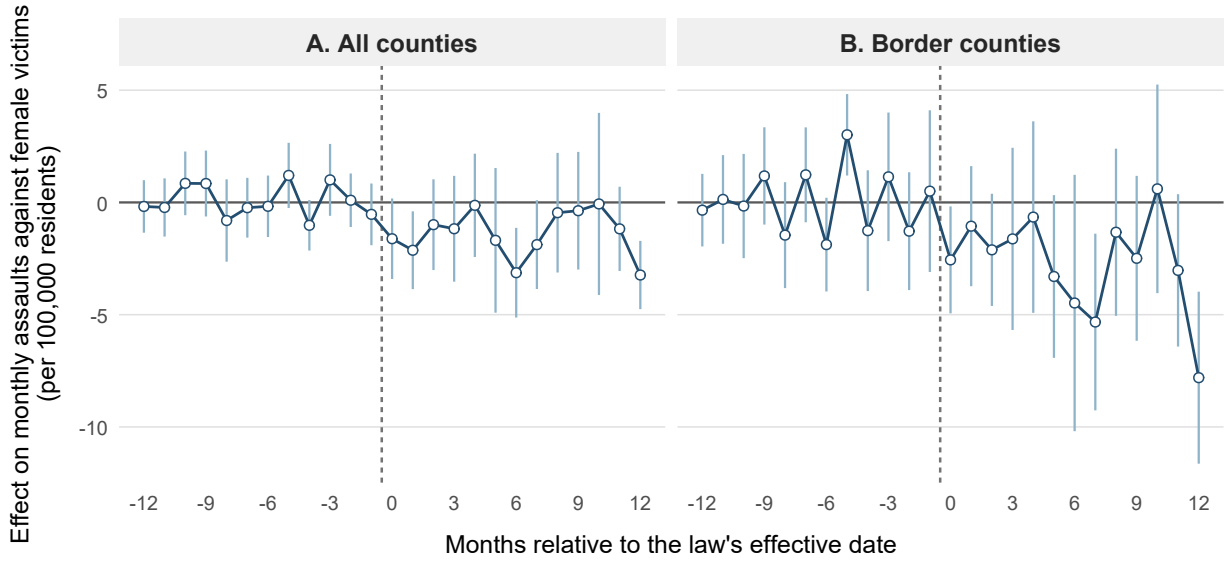


Figure 5: U.S. age-verification laws and assaults against female victims

Notes: Panel A reports Callaway–Sant'Anna event-time estimates for the full county sample, using not-yet-treated states as controls. Panel B applies the same estimator and treatment definition to counties adjoining state borders. Event time is measured relative to each state's effective date. Points are event-time treatment-effect estimates and bars are 95 percent confidence intervals based on standard errors clustered by state. The dashed line separates the final pre-treatment month from the law's effective month. The border exercise changes the geographic sample but otherwise preserves the preferred staggered-DiD design.

The two-way fixed-effects regression in equation (1) provides a cross-check. With county and calendar year-month fixed effects, assault against female victims falls by 1.591 incidents per 100,000 (standard error 0.717) and total assault by 2.811 (1.289), while rape (−0.029, standard error 0.081) and all sexual offenses (−0.071, 0.145) do not change. These estimates match the preferred staggered estimates in sign and are similar in magnitude. The imputation estimates in Table 1 also show a decline in female-victim assault and little change in rape.

We also repeat the preferred Callaway–Sant'Anna estimator after restricting the sample to counties adjoining state borders. This changes the geographic sample while leaving the estimator, treatment definition, and not-yet-treated comparison rule unchanged. The estimated effect on female-victim assault becomes −2.509 incidents per 100,000 (SE 1.268, $p = 0.048$). The associated event study is flat before treatment and negative after implementation. Because the border restriction sacrifices substantial sample size but improves geographic comparability, we treat this as a robustness exercise rather than as a replacement for the full-sample estimate. Panel B of Table 1 reports all three estimators for this border sample; Appendix Section B maps the sample.

Table 2 compares border counties using county and border-segment-by-calendar-month fixed effects, similar to the labor literature that compares minimum-wage effects across state borders. A segment is an unordered pair of adjacent states. Each county is assigned to the segment on which it has the most adjacent out-of-state counties, with ties broken by segment name. The main identification assumption is that counties on opposite sides of a border segment are comparable in

the absence of treatment. The design therefore compares crime rates in the same month between counties that share labor markets, media markets, and demographics but sit on opposite sides of a treated border. Consistent with previous estimates, assaults against female victims fall by 1.4 incidents per 100,000 residents while there is no detectable change in rape.

Table 2: United States: restriction effects with border-segment-by-month fixed effects

	Rape		Assault, female victims	
	(1) TWFE	(2) Imputation	(3) TWFE	(4) Imputation
Policy effect	0.190	0.243	-1.241	-1.407**
	(0.194)	(0.193)	(0.911)	(0.711)
Observations	36,599	34,926	36,599	34,926
Counties	1,115	1,120	1,115	1,120
State clusters	47	47	47	47
R^2	0.259	–	0.698	–
Outcome mean	2.812	2.830	43.417	43.133
Treated observations	4,667	2,877	4,667	2,877

Notes: Outcomes are monthly incidents per 100,000 residents, January 2022–December 2024. The sample consists of counties adjoining state borders, with each county assigned to one unordered pair of adjacent states as described in the text. All columns include county and border-segment-by-calendar-year-month fixed effects; none includes state-by-year fixed effects. Observations are county-months, with no duplication across adjacent-county pairs. Standard errors, in parentheses, are clustered by state. Imputation fits untreated observations and excludes treated county-months whose counterfactual cannot be imputed, including fully treated segment-months. TWFE drops singleton fixed-effect cells, whereas imputation can retain untreated singleton observations; the sample counts therefore differ. Counties are distinct counts within each estimation sample. R^2 is reported for TWFE only. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Section 5 shows that this decline is concentrated among incidents attributed to adult rather than adolescent alleged offenders. The age pattern is useful because the statutes were framed around minors, while platform withdrawal changed access for adults as well.

4 India: Restrictions Increase Violence Against Women

4.1 Data and empirical strategy

Unlike the U.S. laws, the October 2018 order applied to the whole country at once, so the design cannot compare treated with untreated places at the same date. Identification instead comes from differences in exposure. The block only binds where people had mobile internet before it, so districts with more pre-ban connectivity lost more access.

The analysis combines district-level crimes reported by India's National Crime Records Bureau with population, mobile coverage, night lights, forest cover, particulate pollution, and state-level income.[5] The balanced underlying panel contains 412 districts observed annually from 2011 through

[5]Crime counts are from the district tables of the National Crime Records Bureau's *Crime in India*. Population is the annual district population projected with the ratio method of the International Institute for Population Sciences (Dhar, 2022): each district's 2001–2011 change in its share of the state population is carried forward, using the variant

2022, of which 410 enter the regressions once the controls are required. Where the crime bureau reports parts of a 2011 district separately, such as city and rural police units or renamed subdistricts, we fold them into the district so that the area covered by the counts is constant over time. The eleven districts of Andhra Pradesh are excluded in 2022, when a reorganization into new districts left the reported counts partial. The principal outcomes are reported rape and assault against women per 100,000 residents. We also examine insult to modesty, cruelty by a husband or relatives, kidnapping of women, dowry deaths, and a summary index. Police in these districts record about 2.9 rapes and 6.8 assaults against women per 100,000 residents each year. Reporting is an important measurement margin for all administrative data on violence against women (Palermo et al., 2014); the design therefore asks whether reporting changes differentially with pre-ban connectivity at the policy date.

The baseline specification is

$$y_{dst} = \alpha_d + \delta_{st} + \beta \left(Connected_d \times Post_t \right) + X'_{dt}\gamma + \varepsilon_{dst}, \tag{3}$$

where d, s, and t index district, state, and year; $Post_t$ begins in 2019 because the order arrived in October 2018; and $Connected_d$ identifies districts with more than 10 percent 3G/4G coverage before the ban. The preferred comparison group consists of districts that obtained mobile internet in 2019, keeping the comparison close to the threshold of connectivity. District fixed effects absorb permanent differences and state-by-year fixed effects absorb state-specific shocks. Controls include district night lights, forest cover, particulate pollution, population-bin trends, and 2G coverage.

Pre-ban internet availability is not random. We therefore adapt the lightning-based mobile-connectivity instruments of Manacorda and Tesei (2020) and Guriev et al. (2021). Following Guriev et al. (2021), we allow the relationship between lightning and mobile connectivity to differ with local economic capacity. Let $Poor_s$ denote the established below-median split of average state GDP per capita over 2013–2016. Our preferred excluded instrument is

$$Z_{dst} = HighLightning_d \times Poor_s \times Post_t.$$

Because lightning raises the cost of building and maintaining mobile infrastructure, our instrument includes a binary measure of high lightning area relative to the median lightning density across the district-years in the panel. Available technologies to protect equipment and towers from power surges are expensive, and hence poorer states should be more affected by lightning strikes.

the Institute selected for each state, and applied to the official state projections (National Commission on Population, 2020); the series is interpolated to mid-year and rescaled to the 2011 Census population of the area whose crimes are actually reported. Appendix Table 7 shows that the headline estimates are similar with fixed 2011 denominators or with geometric extrapolation of 2001–2011 growth. Mobile coverage (2G, 3G, and 4G) comes from the Collins Bartholomew Mobile Coverage Explorer. Night lights (VIIRS annual means), forest cover (vegetation continuous fields), and PM2.5 pollution are district aggregates from the SHRUG platform (Asher et al., 2021). State GDP per capita at constant prices is from the Reserve Bank of India. Lightning stroke density is from the World Wide Lightning Location Network, as in Guriev et al. (2021). District age and household structure used in Section 5 come from the 2011 Census, and the India Patriarchy Index from Singh et al. (2022).

This differs slightly from the full-sample specification in Guriev et al. (2021), where both the poor- and rich-country lightning interactions predict 3G coverage. In our data, adding the rich-state arm as a second excluded instrument leaves the 2SLS point estimates virtually unchanged but lowers the joint Kleibergen–Paap statistic to 4.42. We report this original two-arm specification in Appendix Table 6. Because the preferred first stage remains moderate, we also report Anderson–Rubin inference.

4.2 Results

Table 3 reports the first stage, OLS, and preferred India IV estimates. The pornography restriction raises reported rape by 1.049 annual cases per 100,000 residents and assault against women by 2.192 cases. These coefficients correspond to approximately 37 and 32 percent of the sample means. The point estimates from ordinary least squares are also positive but substantially smaller: 0.209 for rape and 1.436 for assault. The preferred poor-state instrument has a cluster-robust first-stage F-statistic of 8.82. Weak-instrument-robust Anderson–Rubin tests reject a zero effect for rape ($p = 0.018$) but not for assault against women ($p = 0.162$), so the assault IV estimate is positive and of a similar relative size but imprecise.

Table 3: India: first stage and restriction effects on rape and assault against women

	Exposure	Rape		Assault against women	
	(1)	(2)	(3)	(4)	(5)
	First stage	OLS	IV	OLS	IV
High lightning × Poor × Post	-0.276***				
	(0.093)				
Connected × Post		0.209*	1.049**	1.436***	2.192
		(0.126)	(0.502)	(0.458)	(1.566)
Anderson–Rubin p-value			0.018		0.162
Observations	4,909	4,909	4,909	4,903	4,903
Districts	410	410	410	410	410
R^2	0.819	0.870	–	0.840	–
Dependent-variable mean	0.186	2.871	2.871	6.831	6.831
KP rk Wald F	8.82		8.82		8.81

Notes: Crime outcomes are annual incidents per 100,000 residents, 2011–2022, using the IIPS projected population of the area covered by the crime records. Connected identifies districts with more than 10 percent pre-ban 3G/4G coverage; Post begins in 2019. Column 1 estimates the first stage for Connected × Post on the rape regression sample. IV columns use High lightning × Poor × Post as the sole excluded instrument; the rich-state interaction is not included. All columns include district and state-by-year fixed effects and controls for night lights, forest cover, particulate pollution, population-quintile trends, and 2G coverage. OLS and IV use the same outcome-specific sample. Standard errors, in parentheses, are clustered by district. The Kleibergen–Paap rk Wald F and weak-instrument-robust Anderson–Rubin p-value are reported for each second stage. R^2 is reported for the first-stage and OLS regressions, including absorbed fixed effects. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 6 shows the first stage and reduced-form timing underlying the preferred India IV. Panel A reports the poor- and rich-state lightning coefficients from the expanded first-stage regression. Panels B and C report year-specific reduced-form coefficients for rape and assault against women. For presentation, the figure reverses the sign of the high-lightning instrument, so that positive

coefficients correspond to greater predicted restriction exposure. For rape, the pre-policy coefficients are not jointly significant ($p = 0.306$), and the coefficient is positive in every full post-policy year from 2019 through 2022. Assault against women also rises in each full post-policy year, although its pre-period evidence is less clean ($p = 0.120$) and the 2018 transition-year estimate is already positive. We therefore view the assault timing as supportive rather than definitive.

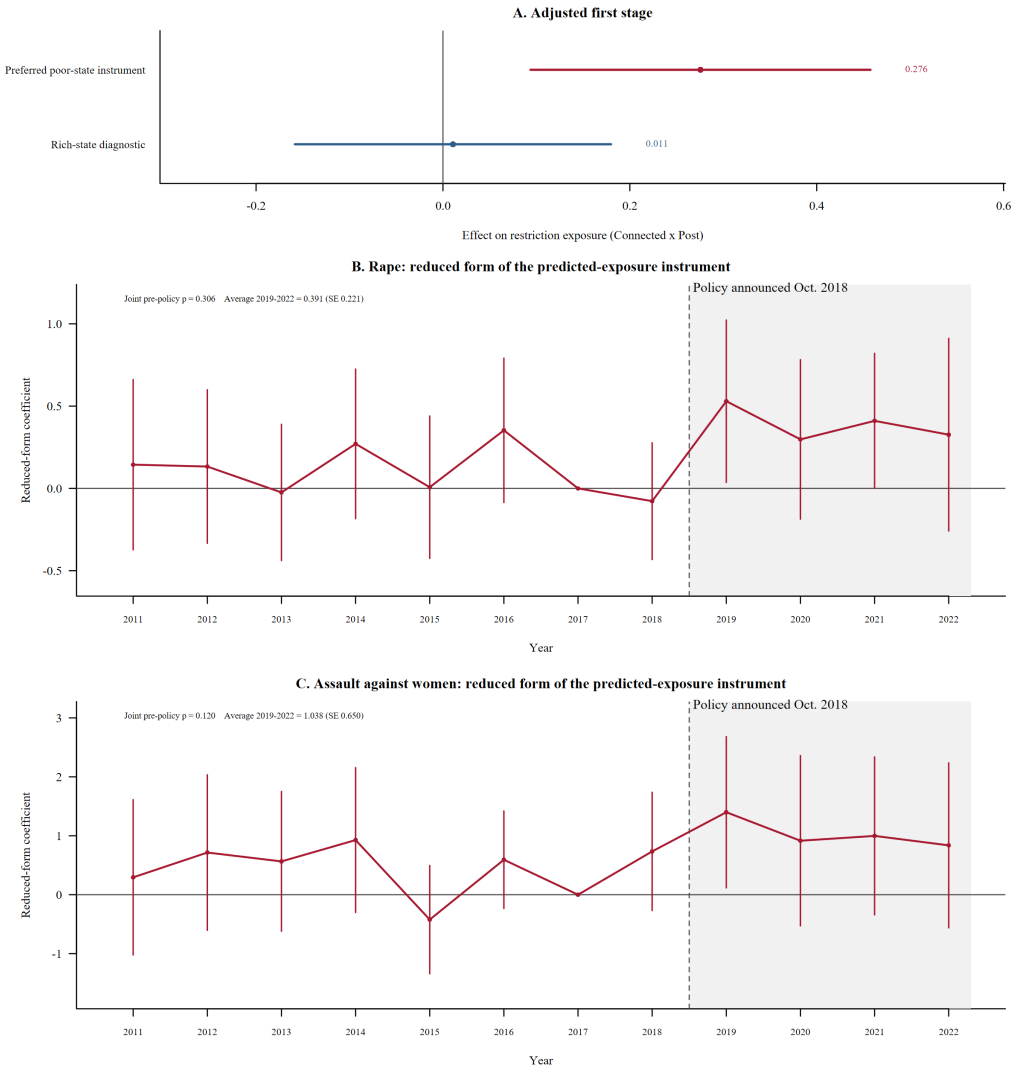


Figure 6: India first stage and reduced-form dynamics

Notes: Panel A reports coefficients from an expanded first-stage regression containing the poor- and rich-state lightning interactions. Only the poor-state interaction is excluded in the preferred IV; the rich-state interaction is shown as a first-stage diagnostic. Panels B and C report year-specific reduced-form coefficients for rape and assault against women, respectively, relative to 2017. For presentation, the figure uses low lightning, defined as one minus the baseline high-lightning indicator, so that positive coefficients correspond to greater predicted restriction exposure; this sign normalization does not affect the IV estimates or first-stage tests. The dashed line marks the October 2018 policy change, and 2019 is the first full post-policy year. All regressions use the main sample, controls, district fixed effects, and state-by-year fixed effects. Standard errors are clustered by district; bars show 95 percent confidence intervals.

To rule out that our instrument might be correlated with higher rates of any crime for other

reasons besides exposure to the policy, we look at whether other types of non-sexual crimes responded. Appendix Table 5 shows that none of the IV estimates from the other types of crimes is positive and statistically distinguishable from zero. Murder, theft, burglary, motor-vehicle theft, and aggregate property crime are all negative and imprecise in the IV panel. A generic deterioration in policing, economic conditions, or crime reporting after 2018 would have instead affected multiple crime categories together.

5 Interpreting the Sign Reversal

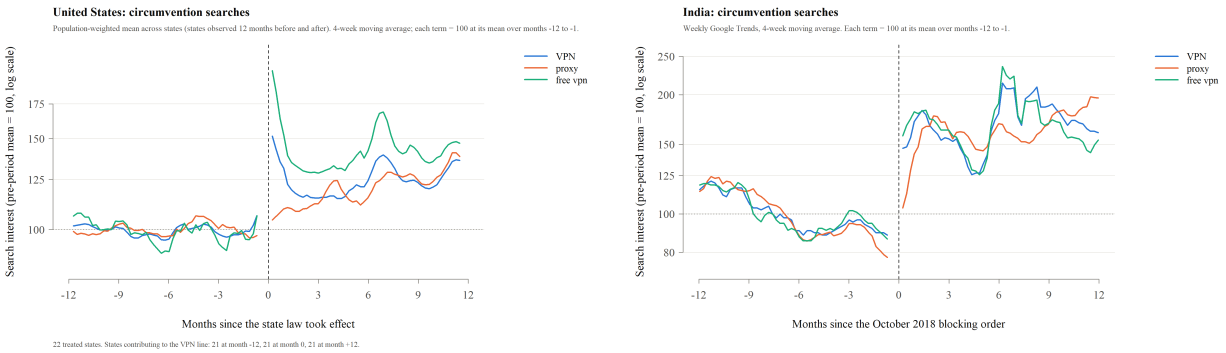
The U.S. and India results are difficult to reconcile with either a purely harmful-exposure view of pornography or a pure substitution view. The same broad intervention of making online pornography harder to access is followed by less violence against women in one setting and more in the other. The relevant question is which channel dominates for the users whose behavior a particular restriction changes. We first discuss that, even if both policies were different, both targeted pornographic consumption and likely decreased it overall. Then, we examine whether certain population groups within each country might have had differential effects that could help us understand why the two countries experienced different outcomes.

5.1 How users adjusted when access was restricted

Both policies raised the cost of reaching pornography, rather than eliminating it entirely. In the U.S., laws required compliant sites to verify age, which raised the cost of access for all users, especially for underage ones, and led the largest platform to withdraw from treated states. In India, the Government issued a list of websites to be blocked. In each case a user who wanted to keep consuming pornography could do so in several ways, including routing traffic through a virtual private network (VPN), using a mirror domain, or accessing different unblocked sources, which could potentially present different kinds of content. Section 2 established that both policies did seem to reduce access to several websites. Before understanding how crime responded, we explore how users adjusted their consumption patterns as access became more costly.

In both countries, we see that users reacted by searching for more ways to circumvent the restrictions. Figure 7 shows Google searches for VPN, proxy, and free VPN around each policy's effective date. In the United States, searches for VPN and free VPN jump in the weeks a state's law takes effect, proxy searches rise more gradually, and all three remain 30 to 45 percent above their pre-law level a year later. In India, the same three terms rise by about half within weeks of the block and are 70 to 85 percent above their pre-order level in the second half of the first year. This already suggests that our estimates are intention-to-treat effects of raising the cost of access rather than effects of eliminating it.

We also observe that there is substitution between sources, where users shifted away from blocked websites and tried to access alternative ones. In the U.S., the parent company of Pornhub and several other large sites, Aylo, withdrew from nearly every state. Other large sites, several of them



(a) United States **(b)** India

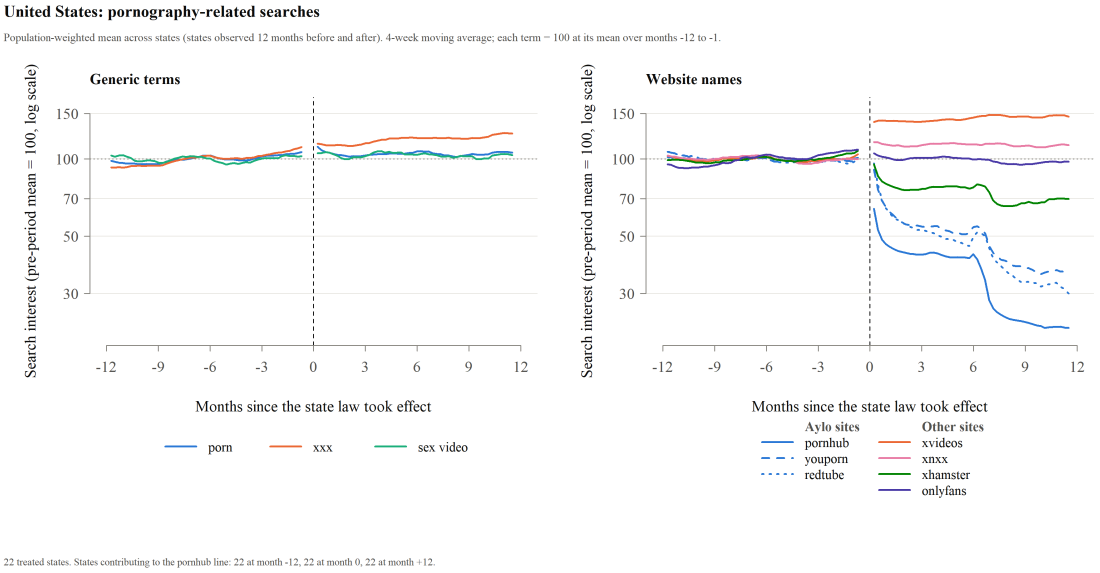
Figure 7: Circumvention searches around the restrictions

Notes: Weekly Google Trends search interest for the terms VPN, proxy, and free VPN, by months since the restriction took effect, shown as four-week moving averages computed separately before and after week zero on a log scale. Each series is set to 100 at its own mean over months −12 to −1. Panel (a) averages state-level series, weighted by 2022 population, across the 22 states whose age-verification laws took effect at least twelve months before the end of the search data in August 2026; week zero is the law's statutory effective date. Panel (b) uses national series for India; week zero is the week of October 21, 2018, when providers began blocking. Sparse state-by-term series are dropped, so the number of states behind each U.S. line differs slightly. The series measure search activity, not circumvention itself, and no comparison group is used.

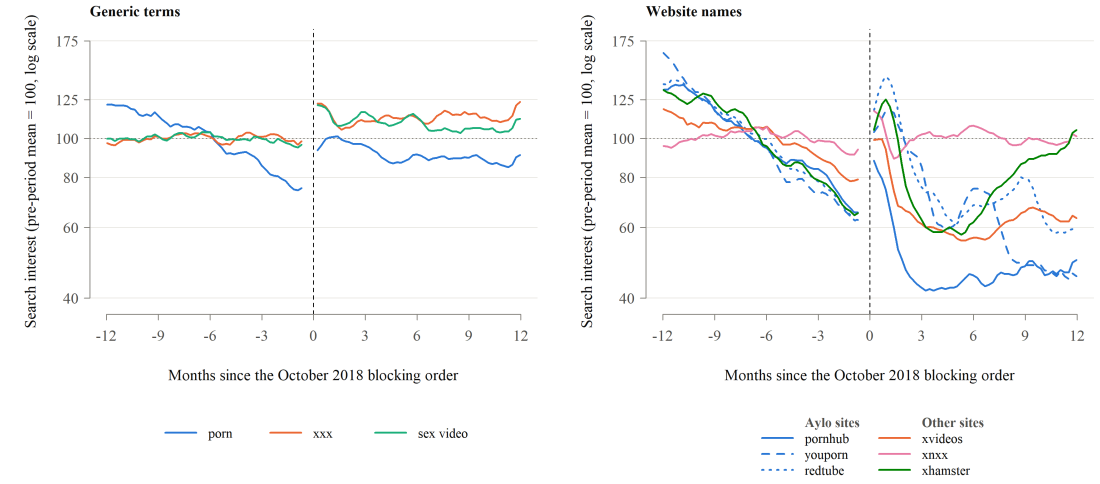
less established or operated from outside the United States, kept serving visitors without verification. The restriction therefore removed the most visited sources while leaving close substitutes in place. Panel (a) of Figure 8 shows that searches for three Aylo sites fall together at the effective date, and by twelve months after the law Pornhub searches are at roughly one fifth of their pre-law level. Searches for other large sites that did not withdraw increased, as well as searches for the generic terms, consistent with users looking for sites they had not used before. However, overall consumption likely decreased in the U.S. Brown et al. (2026) use individual browsing histories and suggest that the laws overall reduced total time spent on adult websites by about 10 percent, despite the substitution toward noncompliant sites and greater use of virtual private networks we also document.

In India where providers blocked 827 domains, we also see a similar pattern. In this case, panel (b) of Figure 8 shows that searches for six different websites, all blocked by the government, fell relative to their level just before the order. Generic terms hold steady or rise. Together with the rise in VPN searches, this indicates that Indian users also circumvented the block and searched for alternative sources. Unlike the United States, where there's evidence that total adult-site use fell (Brown et al., 2026), for India, no comparable browsing data exist. However, given that the block covered the main domains at the network level we also consider that a reduction in consumption alongside circumvention is plausible.

Importantly, we do not interpret Google searches as a measure of pornography consumption. Site-name searches are navigational, so a fall can reflect fewer users or a switch to mirror names, and a rise in generic terms can reflect search for new sources rather than more use. It shows that both restrictions were partly circumvented and that users in both countries redirected their search



(a) United States



(b) India

Figure 8: Pornography-related searches around the restrictions

Notes: Weekly Google Trends search interest by months since the restriction took effect, shown as four-week moving averages computed separately before and after week zero on a log scale. Each series is set to 100 at its own mean over months −12 to −1, so levels are comparable over time within a term but not across terms. Left panels show generic terms and right panels website names, with Aylo's three sites in one color. Panel (a) averages state-level series, weighted by 2022 population, across the 22 states observed for twelve months on each side of their statutory effective date; equal weights give the same picture. Panel (b) uses national series for India, with week zero the week of October 21, 2018, when providers began blocking. All six sites in panel (b) were on the blocking list, and several were already declining before the order, so their drop at week zero should be read against the level just before it. The series measure search activity rather than consumption and use no comparison group.

toward sources that remained reachable. It cannot tell us what content users ultimately consumed, how much, or what offline activity replaced foregone consumption.

5.2 Where and among whom the effects are larger

If both policies decreased pornographic consumption, what might explain a decrease in sexual violence in the U.S. while an increase is observed in India? Is it related to different demographic characteristics, income, cultural norms or other factors? We try to reconcile the different observed outcomes in both settings by examining whether different population characteristics in each country might present larger or smaller effects. For example, if income differences between the two countries are a leading explanation, we might expect to see a smaller increase or even a decrease in sexual violence in richer parts of India. While some of these factors might be too different between the two countries and require extrapolations (Appendix Figure 13 shows that their distributions barely overlap across the two countries), they might be indicative of the underlying mechanisms at play. Importantly, we cannot distinguish differential compliance rates (e.g. users who did not circumvent the bans) from heterogeneous effects, so we understand any differences as aggregating both margins.

To study the differential effects, we interact the policy event with a pre-policy moderating characteristic in separate TWFE regressions. Let T_{dst} denote policy exposure: $Connected_d \times Post_t$ in India and the state-law indicator D_{st} in the United States. Each regression includes interactions of the same moderator with time dummies, so the differential effect is identified by comparing exposed and unexposed places rather than by any common change in those population groups.

$$y_{dst} = \alpha_d + \delta_t + \beta_1 T_{dst} + \beta_2 \left(T_{dst} \times M_d\right) + \sum_r \theta_r \left(M_d \times \mathbf{1}\{t = r\}\right) + X'_{dt}\gamma + \varepsilon_{dst}, \tag{4}$$

where M_d is the pre-policy moderator. Figure 9 presents the β_2 coefficients from this policy-exposure-by-moderator interaction for rape and assault against women. Each moderator is centered and standardized within country, so β_2 measures how the policy effect changes with a one-standard-deviation increase in that variable. We express coefficients as a percent of each country's pre-policy outcome mean and show 90 percent confidence intervals.

We first consider whether the difference in outcomes is related to the income of the area's population. Income correlates with education, urbanization, and other factors that might determine both the prevalence of sexual violence and the ability to circumvent the policy bans. Using median income in the U.S. and night lights in India, we don't find evidence that places with higher or lower income experienced different effects. At most, we see that the signs for both types of crimes in the two countries are negative (between -0.2 and -4.2 percent of the pre-policy mean per standard deviation), which would suggest that places with higher economic resources had a larger decline in the U.S., or a smaller increase in India, than poorer places after exposure to the ban, but these estimates are not significant.

We then ask whether the age composition of the population might explain the different effects observed. India is a considerably larger country with a younger population than the U.S., and the

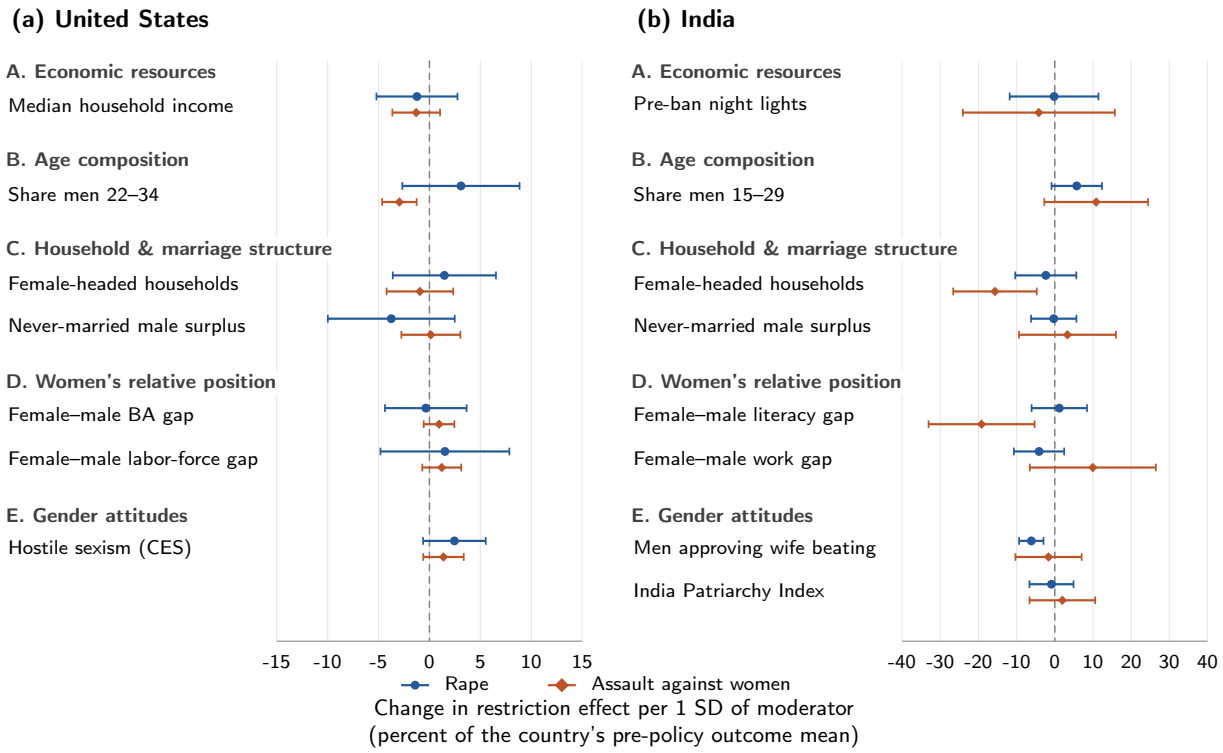


Figure 9: Differential policy ban effects by local characteristics

Notes: Each point corresponds to the β_2 coefficient from equation 4. Moderating variables are standardized within country. India includes district and state × year fixed effects, annual night lights, forest cover, $PM_{2.5}$, 2G coverage and population-quintile linear trends. The U.S. includes county and calendar year-month fixed effects, without additional covariates. Treatment is pre-ban 3G/4G coverage above 10% × post-2018 in India, and a state law in force in the U.S. Confidence intervals at 90% use district clusters in India, state clusters for its state-level measures (27), and state clusters in the U.S. Coefficients and intervals are multiplied by 100/pre-policy mean. Fixed means are unweighted rate averages in the age-model sample (rape, assault): India, 2011–2017 (2.993, 6.404 per 100,000 annually); U.S., 2022 (3.162, 43.579 monthly). Assault denotes NCRB assault with intent to outrage women's modesty in India, and female-victim aggravated/simple assault and intimidation in NIBRS. Resources are $\ln(1 + \text{mean 2013–2016 night lights})$ and log 2021 income. Age-share denominators are men aged 10+ (India Census 2011) and 18+ (ACS 2018–2022). Headship uses all households; male surplus is $(M - F)/(M + F)$ for never-married men and women aged 20–39. Education/work gaps are female minus male. Wife-beating approval is the NFHS-4 state share endorsing any justification; CES county sexism means are shrunk toward state means. The Patriarchy Index follows Singh et al. (2022).

age composition of the population might be related to both the prevalence of sexual violence and the ability to circumvent the policy bans. Brown et al. (2026) show that younger adults in the U.S. were more likely to circumvent the ban through the use of VPNs compared to older cohorts, which would make the effects among younger populations smaller. Different cohorts might also react differently to the policy ban given that they have likely had different exposure to sexual relationships as well as time exposure to pornographic content.

We find opposite effects between the two countries on the sexual violence rates in places with younger populations.[6] In the U.S. we find evidence that counties with relatively younger adult populations had even lower assault rates following the policy, despite their higher ability to circumvent the ban. In the average county, the laws reduced female-victim assault by 1.4 incidents per 100,000 residents per month (about 3 percent of the pre-policy mean). In a county with a one-standard-deviation larger share of young men (4.4 percentage points), the decline is 2.7, about twice as large, while in a county one standard deviation below the mean it is close to zero.[7] We do not find a similar gradient for rape. This contrasts with India, where, while imprecise, these young adult places correlate with higher sexual violence rates after the policy (5.8 and 10.8 percent of the pre-policy mean per standard deviation for rape and assault).[8] Even though these estimates are only suggestive, they may further highlight other differences between the two countries, such as different social norms and gender attitudes, that make consensual relationships, a potential substitute for pornography consumption and sexual violence, more or less acceptable. We examine this below.

Next, we look at whether the household and marriage environment might explain the different effects observed. Comparing effects by the share of female-headed households, we only detect significant differences in India, where locations with a larger share of female-headed households were less likely to see assaults against women increase following the ban; we find no such difference for rape. In the average district, assault against women rose by 1.2 incidents per 100,000 residents per year (about 19 percent of the pre-policy mean). In a district with a one-standard-deviation larger share of female-headed households (5.4 percentage points), the increase is only 0.2 and not significant, and splitting districts at the national median gives an increase of 2.4 below the median

[6] In India we use the share of men aged 15–29 among men aged 10 and older in the 2011 census. Since the ban occurred 7 years later, this cohort was roughly 22–36 years old at the time of the ban, comparable to the share of men aged 22–34 among men aged 18 and older that we use for the U.S., with the caveat that migration and mortality might have changed it during this period. While in our datasets we observe that men are commonly the perpetrators and women are the victims, the shares of young men and young women are highly correlated (0.88 across Indian districts), so we cannot separate the effects of either gender.

[7] The U.S. incident data also allow us to look directly at the age of the alleged offenders (Appendix Table 9). The decline in assaults is essentially zero among offenders younger than 18, small among those aged 18–21 (-0.38 monthly incidents per 100,000 residents), and much larger among adults: -1.50 for ages 22–35 and -1.96 for ages 36 and older, and we reject that the effects are equal across age groups ($p = 0.005$). Female-victim assaults follow the same pattern. This is notable because the statutes were justified as restrictions on minors, while the decline is concentrated among adults. However, the data cannot tell whether the smaller estimates for younger offenders reflect their greater circumvention (Brown et al., 2026) or a weaker response to a given change in access.

[8] The rape gradient also depends on how district population is measured. With a fixed 2011 denominator it is 0.27 annual incidents per 100,000 residents per standard deviation (standard error 0.13) and significant at the 5 percent level, while extrapolating 2001–2011 population growth gives 0.13 (0.12), because districts with more young adults also grew faster (Appendix Table 8).

and 0.04 above it (Appendix Table 11).[9] However, it is hard to interpret this result as female-headed households might indicate greater female autonomy and bargaining power, or it might indicate male absence, widowhood, or household disruption, and in either case, the reactions to the greater autonomy or absence of men might be different. In turn, we don't find any differential response based on the surplus of never-married men, an indicator for a more competitive marriage market.

To get a better understanding of how women's autonomy might play an important role, we look at women's relative standing in the local environment. Looking at the female-minus-male gaps in education and work participation, we find that in the U.S., even though imprecise, counties where women had a better standing had a relatively smaller decline in assaults following the ban (or larger crime rates relative to counties where women had a worse relative standing): about 1 percent of the pre-policy mean per standard deviation for both the college-degree and labor-force gaps, with confidence intervals that include zero. Meanwhile, in India, we find the opposite result: women's better relative standing, at least as measured by literacy, is associated with a smaller increase in assaults against women following the ban. A district where the literacy gap is 6.2 percentage points smaller (one standard deviation) offsets roughly the entire average increase in assault (19 percent of the pre-policy mean). The same pattern holds for secondary and higher schooling once we compare the share of women to the share of men who reach each level (a ratio), rather than the difference in percentage points, which is mechanically small where few people reach higher levels; we find no similar pattern for rape or for the gap in work participation (see Appendix Figure 14). As with female-headed households, it also relies largely on differences across states. Reconciling these results is difficult, because taken at face value it could suggest more retaliation on the side of men in the U.S., while also showing that greater female autonomy protected women in India.

Finally, we explore the role played by local gender norms. In the U.S., a measure of county-level sexism shows that counties with more sexist attitudes would have larger sexual violence rates following the ban relative to counties with less sexist attitudes, or at least a smaller decline (2.5 and 1.4 percent of the pre-policy mean per standard deviation for rape and assault), although neither estimate is significant. This measure is based on a survey of county residents, and while it is not a perfect measure of local gender norms, it correlates with other markers of more conservative attitudes, such as Republican vote share (0.45) and evangelical adherence (0.29), which show similar gradients for assault (see Appendix Figure 14). In India, by contrast, more conservative gender norms, as measured by the share of men in the state who approve of wife beating, are associated with a smaller increase in rape following the ban: one standard deviation higher approval offsets most of the average increase in rape (6.2 percent of the pre-policy mean per standard deviation, although with only 27 states to compare). We do not find a similar gradient for assault, nor for the district-level Patriarchy Index (Singh et al., 2022), which combines measures of male and

[9]The pattern holds when we add interactions with the urban share, the female-minus-male gaps in literacy and work participation, the surplus of never-married men, and the Patriarchy Index all at once (Appendix Table 12), although it weakens when only the literacy gap is added, since the two measures move together across districts. It also comes mostly from differences across states: comparing districts within the same state, the gradient is smaller and not significant (-0.44 annual incidents per 100,000 residents per within-state standard deviation, $p = 0.23$, compared with -1.00 across all districts).

generational domination, patrilocality, son preference, and women's socioeconomic position.[10] While in India's case it could suggest less violence when women perhaps have less autonomy, in contrast to what we explored earlier, it could also suggest that in these areas women are less likely to report sexual violence.

Appendix Table 13 further decomposes the effects inside and outside intimate relationships. The estimates do not indicate that the response in either country is confined to intimate-partner violence.

5.3 Putting the evidence together

What can we learn from this evidence? First, both policies changed how users reached pornography, but neither eliminated it. Our estimates are therefore the effects of making access more costly rather than of removing it, and the sign reversal is not a comparison between a policy that worked and one that was ignored.

Second, local characteristics shape how large the effects are, but none of them on its own explains why the sign differs between the two countries. Income does not seem to matter in either country. In the U.S., the decline in assaults is larger in counties with more young adults and is concentrated among adult offenders rather than among the minors the laws were meant to protect. In India, the increase in assaults against women is concentrated in districts where women's relative position is weakest: where fewer households are headed by women and where women's literacy and schooling lag furthest behind men's, while districts closer to parity saw almost no increase. This holds across several measures of women's schooling, although it relies mostly on differences across states and we cannot separate women's standing from other regional differences. It is also unlikely to be driven by reporting alone, because if women in these districts were more likely to report violence, a given increase in assaults should look larger there, not smaller. The evidence on gender attitudes is weaker. In India, approval of wife beating is associated with a smaller increase in rape, which could also reflect less reporting, while neither the Patriarchy Index nor the U.S. measure of sexism shows a clear difference.

Third, the two countries are too different to use one to predict the other. Even the Indian districts where women's standing is closest to men's remain far from the typical U.S. county (Appendix Figure 13), so these within-country differences cannot tell us whether India would have seen a decline had its local conditions resembled those of the U.S. What they do suggest is that the consequences of restricting pornography depend on the setting in which the change in consumption takes place, and in India, on women's relative position in that setting. This is consistent with the hypothesis that restrictions on access to pornography shift different behavioral margins in different settings, so the relative importance of exposure and substitution changes with the setting.

[10] Without the moderator-by-year controls, the Patriarchy Index interaction with assault is large and positive, but that pattern reflects an increase in assault in more patriarchal districts that also occurs in unexposed districts.

6 Conclusion

Pornography restrictions are often motivated by the view that reducing exposure will protect women. Our results show that this implication is not general. U.S. age-verification laws and the resulting platform withdrawals reduced assaults against female victims, with no detectable change in rape or other sexual offenses. India's 2018 blocking order instead increased reported rape and assault against women in districts more exposed to the restriction. In both settings, the effects are concentrated on particular crime margins rather than appearing as broad changes in criminal activity.

The sign reversal is the central result of the paper. Pornography may affect violence through two competing channels. Exposure can increase aggression, while private consumption can substitute for behaviors associated with offending. Which channel dominates depends on how a particular restriction changes actual behavior and on the population and environment in which that change occurs. The mechanism evidence does not isolate a single mediator, but it points in this direction: users in both settings circumvented and substituted, the crime response is concentrated among adults, and its magnitude varies with local age composition in the United States and with household structure and women's relative literacy in India, although the data cannot separate who changed consumption from who responded to a given change.

The policy implication is that restrictions on digital content cannot be evaluated from the statutory intervention alone. Their consequences depend on how platforms and users respond and on the social setting in which those responses take place.

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A U.S. Age-Verification Laws and Platform Response

Appendix Table 4 lists every state with an age-verification law in effect as of September 2026, the statutory effective date used to define treatment, and the date on which Pornhub blocked visitors from the state.

Table 4: U.S. state age-verification laws and Pornhub's response

State	Law effective	Pornhub response	In sample
Louisiana	Jan 1, 2023	Complied (LA Wallet)	Yes
Utah	May 3, 2023	May 1, 2023	Yes
Mississippi	Jul 1, 2023	Jun 30, 2023	Yes
Virginia	Jul 1, 2023	Jun 30, 2023	Yes
Arkansas	Jul 31, 2023	Jul 28, 2023	Yes
Texas	Sep 1, 2023	Mar 14, 2024	Yes
Montana	Jan 1, 2024	Jan 1, 2024	Yes
North Carolina	Jan 1, 2024	Jan 1, 2024	Yes
Idaho	Jul 1, 2024	Jun 27, 2024	Yes
Kansas	Jul 1, 2024	Jun 27, 2024	Yes
Kentucky	Jul 15, 2024	Jul 10, 2024	Yes
Nebraska	Jul 18, 2024	Jul 15, 2024	Yes
Indiana	Aug 16, 2024	Jun 27, 2024	Yes
Alabama	Oct 1, 2024	Sep 30, 2024	Yes
Oklahoma	Nov 1, 2024	Nov 1, 2024	Yes
Florida	Jan 1, 2025	Jan 1, 2025	No
South Carolina	Jan 1, 2025	Jan 1, 2025	No
Tennessee	Jan 13, 2025	Jan 1, 2025	No
Georgia	Jul 1, 2025	Jul 1, 2025	No
South Dakota	Jul 1, 2025	Jul 1, 2025	No
Wyoming	Jul 1, 2025	Jun 30, 2025	No
North Dakota	Aug 1, 2025	Aug 1, 2025	No
Arizona	Sep 26, 2025	Sep 26, 2025	No
Ohio	Sep 30, 2025	Not blocked	No
Missouri	Nov 30, 2025	Dec 1, 2025	No
West Virginia	Jun 12, 2026	Jun 12, 2026	No
Iowa	Jul 1, 2026	Jul 1, 2026	No

Notes: The table lists every state with an age-verification law in effect as of September 2026, ordered by effective date. The third column gives the date on which Pornhub blocked visitors from the state. "In sample" marks the fifteen states whose laws took effect during the 2022–2024 crime-data window; states with later dates serve as not-yet-treated comparison states. Louisiana is the only state where the platform complied instead of withdrawing. Ohio's law does not reach the platform under the statute's exemption for interactive computer services. Effective dates are statutory dates, except Tennessee, where the date is the day an injunction was stayed.

B U.S. Border-County Robustness

Counties on opposite sides of state borders can share regional labor markets, media markets, and demographics while being exposed to different state age-verification policies. We therefore use border counties as a more geographically comparable robustness sample. Our primary border exercise applies the same Callaway–Sant'Anna estimator used in the full sample to the 1,117 counties adjoining a state border. This preserves the treatment definition, not-yet-treated comparison rule, and estimator while changing only the geographic sample. The exercise is nevertheless a robustness design rather than the main specification because restricting to border counties discards roughly two thirds of the county-month observations and online access can cross state borders.

Appendix Figure 10 shows the treatment status of every county in the main sample, and Appendix Figure 11 the subset used in the border exercise.

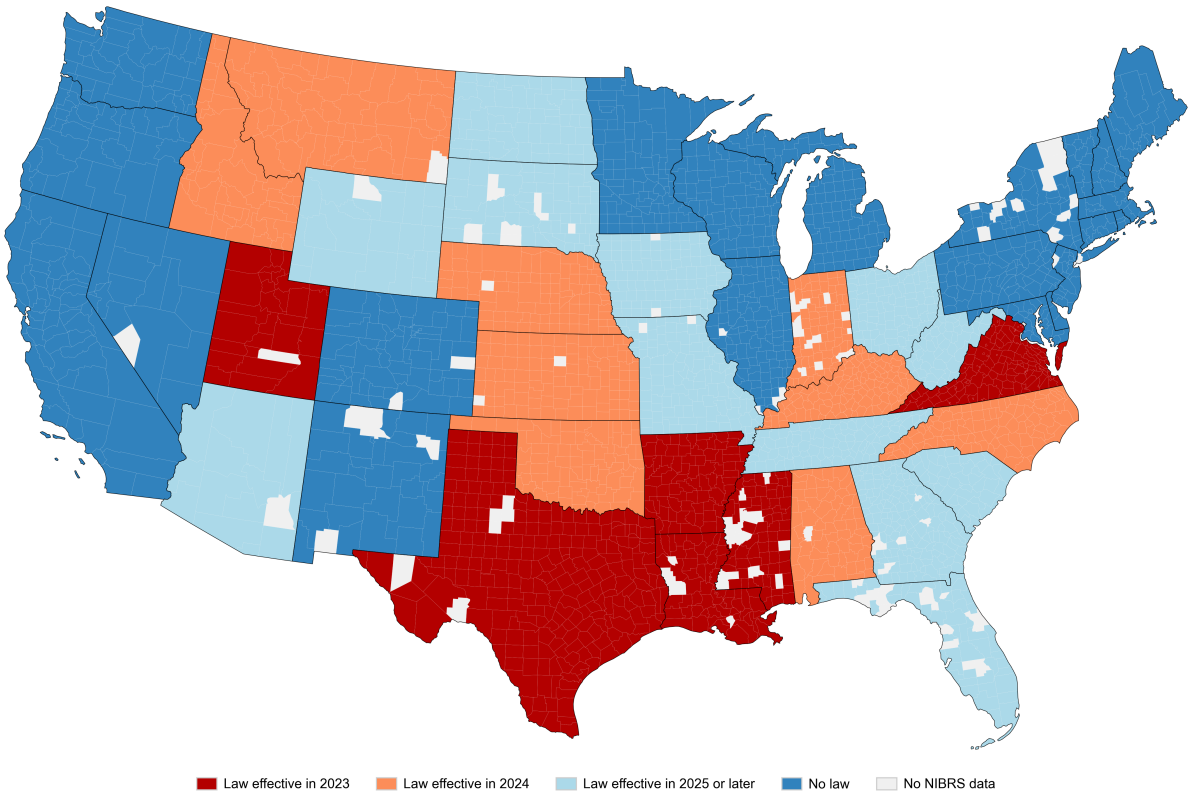


Figure 10: Counties in the main U.S. sample by treatment status

Notes: Dark red and orange identify counties in states whose age-verification laws took effect in 2023 and 2024, respectively. Light blue identifies states whose laws took effect in 2025 or later. These states are untreated throughout the 2022–2024 window and serve as not-yet-treated comparison states. Dark blue identifies states with no law, and light gray counties have no NIBRS data. Treatment in the analysis is defined at the state by year-month level from each law's effective month. The map groups states by the year of that month only for visualization.

Applying the preferred staggered-DiD estimator to the border-county sample yields an estimated effect on female-victim assault of -2.509 incidents per 100,000 (SE 1.268, $p = 0.048$), compared with -1.288 (SE 0.873, $p = 0.140$) in the full sample. The corresponding event study shows no

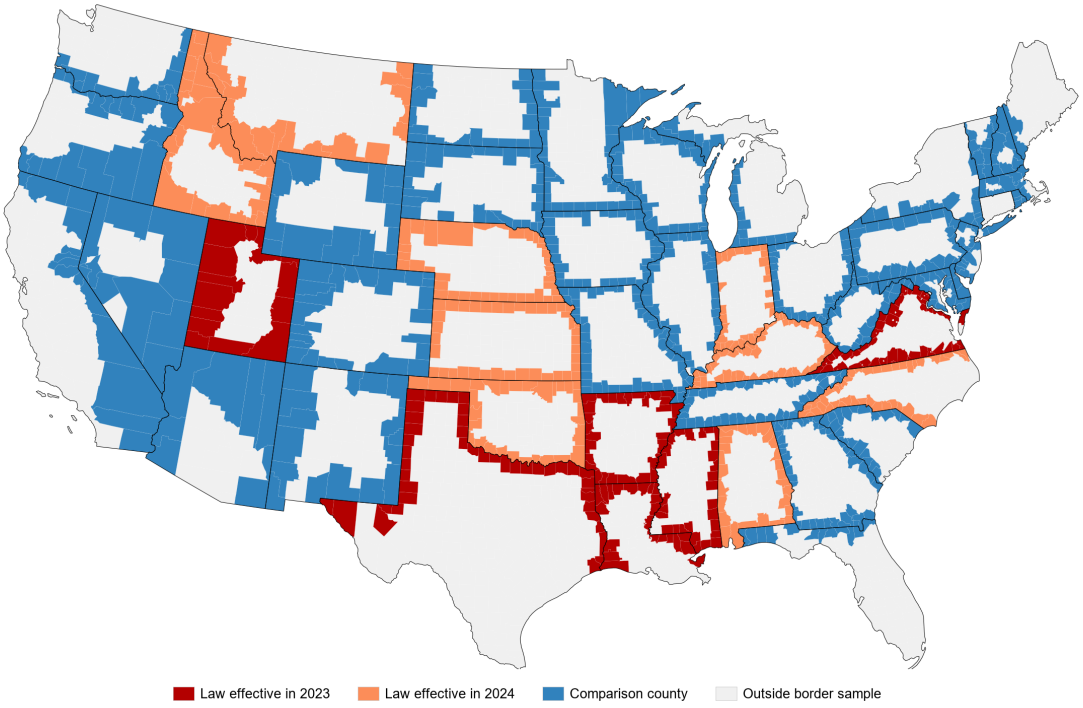


Figure 11: Counties in the U.S. border-sample robustness exercise

Notes: Colored counties adjoin a state border and appear in the broad border sample. Dark red and orange identify counties in states whose age-verification laws became effective in 2023 and 2024, respectively. Blue identifies comparison counties; light gray counties lie outside the border sample. Future-treated states whose laws began after the crime-data window serve as comparisons during 2022–2024. As in Appendix Figure 10, treatment is defined at the state by year-month level and the map groups states by year only for visualization.

systematic pre-treatment movement and a negative post-treatment response. The larger border estimate should not be interpreted as evidence that geographic restriction mechanically strengthens the effect; its value is that the same estimator produces the same qualitative conclusion in a more geographically comparable sample.

The border-segment comparison is reported in Table 2, with separate TWFE and imputation columns for rape and female-victim assault. It adds segment-by-calendar-month fixed effects to the county fixed effects, with each county assigned to one state-border segment. The female-assault estimates remain negative but are closer to zero than in the broader border-county comparison. The imputation sample excludes treated observations whose counterfactual cannot be imputed under these fixed effects.

C India IV Robustness

Table 5: India: restriction effects on crimes against women and other crimes

Outcome	OLS	IV	AR p	Mean	Observations
Panel A. Crimes against women					
Rape	0.209*	1.049**	0.018	2.871	4,909
	(0.126)	(0.502)			
Assault against women	1.436***	2.192	0.162	6.831	4,903
	(0.458)	(1.566)			
Insult to modesty	−0.078	−0.055	0.779	0.602	4,824
	(0.076)	(0.198)			
Cruelty by husband or relatives	0.461	1.348	0.497	7.073	4,893
	(0.349)	(1.950)			
Kidnapping of women	−0.146	0.518	0.763	4.612	4,903
	(0.268)	(1.747)			
Dowry deaths	−0.049*	−0.048	0.800	0.527	4,836
	(0.025)	(0.189)			
Violence-against-women index	0.009*	0.032	0.190	0.153	4,779
	(0.005)	(0.025)			
First-stage coefficient range		[−0.277, −0.276]			
Kleibergen–Paap F-statistic range		8.81–8.87			
Panel B. Other crimes					
Murder	−0.151***	−0.468	0.286	2.209	3,566
	(0.057)	(0.424)			
Theft	−3.103	−6.008	0.588	38.760	3,566
	(2.036)	(11.000)			
Burglary	0.013	−0.368	0.818	6.844	3,160
	(0.426)	(1.611)			
Motor-vehicle theft	−2.248***	−5.590	0.176	15.035	3,160
	(0.766)	(4.259)			
All property crime	−5.488*	−11.561	0.424	63.116	3,160
	(2.870)	(14.500)			
First-stage coefficient range		[−0.276, −0.268]			
Kleibergen–Paap F-statistic range		7.97–8.76			
District fixed effects	Yes	Yes			
State × year fixed effects	Yes	Yes			
Additional time-varying controls	Yes	Yes			

Notes: Each outcome is estimated separately using the specification in Table 3. Outcomes are annual incidents per 100,000 residents, except the violence-against-women index, which is the first principal component of the six listed crimes, rescaled to zero–one. IV uses High lightning × Poor × Post as the sole excluded instrument. All models include district and state-by-year fixed effects, night lights, forest cover, particulate pollution, population-quintile trends, and 2G coverage. Standard errors, in parentheses, are clustered by district. AR p is the Anderson–Rubin p-value for the IV effect. First-stage coefficient and Kleibergen–Paap F ranges span the outcome-specific estimation samples within each panel. Mean and observations refer to the common OLS/IV outcome sample. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Table 6 compares the preferred poor-state instrument with the original two-arm instrument and the strict poor-minus-rich DDD.

Table 6: Alternative parameterizations of the India lightning instrument

	(1) Preferred poor-state	(2) Original two-arm	(3) Strict DDD
Rape	1.049**	1.072**	0.420
	(0.502)	(0.505)	(0.829)
Anderson–Rubin p-value	0.018	0.031	0.601
Assault against women	2.192	2.125	4.005
	(1.566)	(1.559)	(2.907)
Anderson–Rubin p-value	0.162	0.267	0.126
Violence-against-women index	0.032	0.032	0.023
	(0.025)	(0.025)	(0.034)
Anderson–Rubin p-value	0.190	0.373	0.495
First-stage F-statistic range	8.81–8.87	4.42–4.44	4.34–4.38
District fixed effects	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes
Additional time-varying controls	Yes	Yes	Yes

Notes: The preferred specification uses the poor-state lightning interaction as the sole excluded instrument. The original specification uses the poor- and rich-state interactions jointly as excluded instruments. The strict DDD additionally controls for lightning interacted with the post period, so that its excluded first-stage variation is the poor-minus-rich difference. All models otherwise use the same sample, controls, and fixed effects. Anderson–Rubin tests are cluster-robust. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Population denominators

Every India rate divides reported counts by the projected annual district population described in Section 4. No district population is measured between the 2011 Census and the next census, so any annual series is a projection, and the concern is that districts whose population grew faster after 2011 have mechanically drifting rates under a fixed denominator. Appendix Table 7 re-estimates the headline OLS and 2SLS results with the fixed 2011 Census population of the covered area, with the official 2011 district total, and with each district's 2001–2011 growth rate extrapolated through 2022. The estimates move by at most a third of a standard error, and the rape 2SLS estimate remains significant under every denominator. Appendix Table 8 applies the same comparison to the India age gradient of Figure 9, which is the one heterogeneity result that depends on the choice, and reports it for four definitions of the young-adult population.

Table 7: India headline estimates across population denominators

	IIPS projection (main)	Fixed 2011 coverage-consistent	Official 2011 total	2001–2011 growth extrapolation
Panel A. Rape				
OLS	0.209*	0.199	0.201	0.180
	(0.126)	(0.133)	(0.133)	(0.122)
2SLS, preferred poor-state instrument	1.049**	1.126**	1.150**	1.000**
	(0.502)	(0.562)	(0.564)	(0.486)
Anderson–Rubin p-value	0.018	0.019	0.016	0.023
Panel B. Assault against women				
OLS	1.436***	1.481***	1.483***	1.315***
	(0.458)	(0.472)	(0.472)	(0.448)
2SLS, preferred poor-state instrument	2.192	2.505	2.558	2.040
	(1.566)	(1.756)	(1.760)	(1.514)
Anderson–Rubin p-value	0.162	0.147	0.138	0.180
First-stage F-statistic	8.8	8.8	8.8	8.8
District fixed effects	Yes	Yes	Yes	Yes
State × year fixed effects	Yes	Yes	Yes	Yes
Additional time-varying controls	Yes	Yes	Yes	Yes

Notes: Columns differ only in the denominator of the outcome rate: the IIPS ratio-method projection used throughout the paper (Dhar, 2022), the 2011 Census population of the area covered by the reported counts, the official 2011 district total, and the covered-area 2011 population grown at the district's 2001–2011 annual rate. All models use the preferred sample, controls, district and state-by-year fixed effects, and district-clustered standard errors; the 2SLS rows use the poor-state lightning instrument. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 8: India age gradient in the rape and assault effects across population denominators

	Rape			Assault against women		
	IIPS projection (main)	Fixed 2011	2001–2011 growth	IIPS projection (main)	Fixed 2011	2001–2011 growth
Panel A. Young-adult male share (men 15–29 / men 10+)						
Exposed × Post × moderator	0.172	0.266**	0.131	0.694	0.866	0.578
	(0.120)	(0.134)	(0.116)	(0.528)	(0.556)	(0.525)
Panel B. Young-adult female share (women 15–29 / women 10+)						
Exposed × Post × moderator	0.147	0.227*	0.115	0.362	0.473	0.222
	(0.121)	(0.134)	(0.115)	(0.419)	(0.422)	(0.397)
Panel C. Young-adult share, both sexes (persons 15–29 / persons 10+)						
Exposed × Post × moderator	0.161	0.250*	0.123	0.575	0.722	0.443
	(0.120)	(0.133)	(0.115)	(0.467)	(0.485)	(0.458)
Panel D. Young sex ratio (men 15–29 / women 15–29)						
Exposed × Post × moderator	0.009	0.058	−0.032	0.711	0.836	0.647
	(0.151)	(0.167)	(0.147)	(0.614)	(0.643)	(0.606)
Mean dependent variable	2.871	3.073	2.809	6.831	7.252	6.651
Observations	4,909	4,909	4,909	4,903	4,903	4,903
Districts	410	410	410	410	410	410

Notes: Each cell is the coefficient on Exposed × Post × moderator from the specification of Figure 9 (moderator-by-year indicators, district and state-by-year fixed effects, baseline controls), with the moderator named in the panel heading standardized nationally over the estimation sample. The moderators are built from the 2011 Census age tables; ages 15–29 in 2011 correspond to roughly 22–36 at the 2018 order. Columns differ only in the population denominator, as in Appendix Table 7. Standard errors, in parentheses, are clustered by district. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix Figure 12 reports the rape estimate across alternative specifications, all using the poor-state instrument of the main estimates.

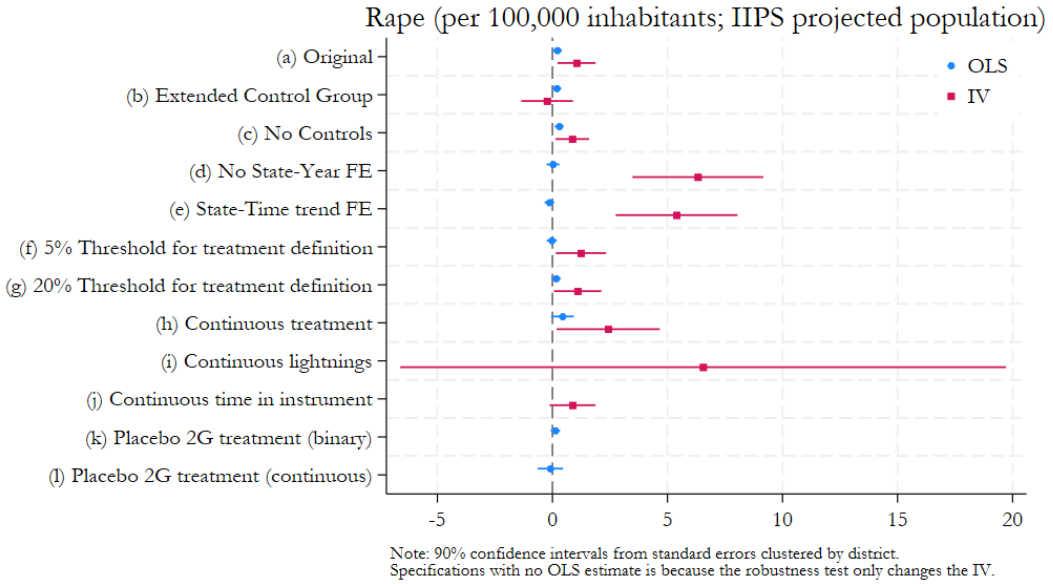


Figure 12: India rape estimates across alternative specifications

Notes: The figure reports OLS and IV point estimates with 90 percent confidence intervals for the effect of the 2018 restriction on rape per 100,000 residents. Every IV estimate uses the poor-state instrument of Table 3. Specifications vary the fixed effects, controls, treatment threshold and comparison group. Two rows change the instrument itself, replacing the lightning indicator with continuous lightning or the post-restriction indicator with a linear trend. The two 2G placebo rows report OLS only. Standard errors are clustered by district. The vertical line denotes zero. Estimates based on alternative first stages should be read jointly with their corresponding first-stage strength.

D Supplementary Mechanism Evidence

D.1 Additional heterogeneity measures

Figure 13 compares the pre-policy levels of the structural moderators in Figure 9 across the two countries, with definitions matched as closely as the published census tables allow. The distributions barely overlap, so the country-specific gradients describe variation around very different baseline levels.

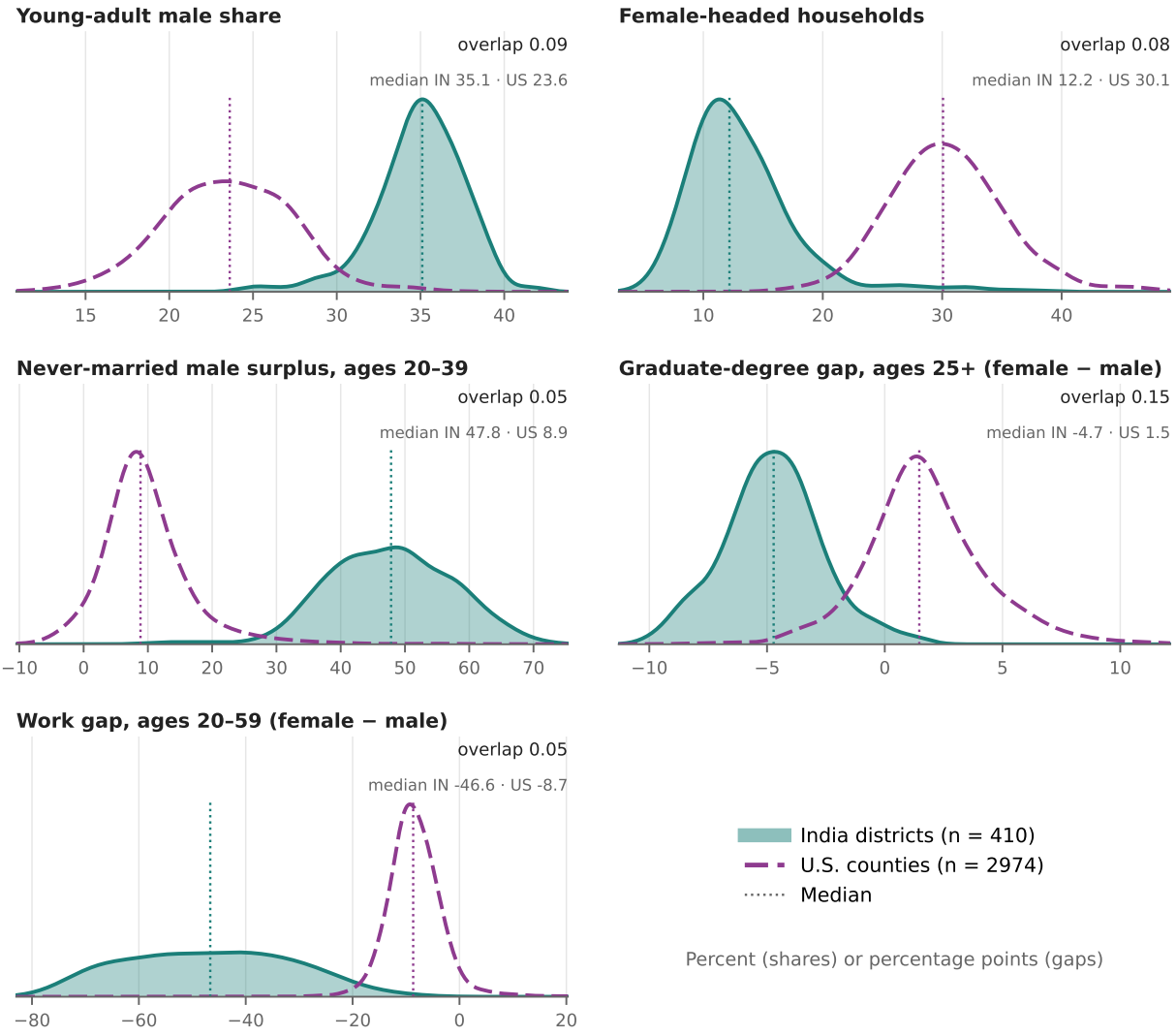


Figure 13: Pre-policy moderator distributions in India and the United States

Notes: Population-weighted Gaussian kernel densities over the 410 Indian districts (2011 Census) and 2,974 U.S. counties (ACS 2018–2022) in the estimation samples of Figure 9. Shares are in percent and gaps in percentage points; dotted lines mark weighted medians; "overlap" is the common area under the two densities. Young-adult male share: men 15–29 over men 10+ in India (approximately men 22–36 over men 17+ at the 2018 ban) and men 22–34 over men 18+ in the U.S. Female-headed households: female-headed over all households in India; female householders without a spouse (family and nonfamily) over all households in the U.S. Never-married male surplus: $(M - F)/(M + F)$ among never-married adults aged 20–39. Graduate-degree gap: female minus male share with a graduate (India) or bachelor's (U.S.) degree or higher, ages 25+. Work gap: female minus male share working at ages 20–59 (main and marginal workers in India; employed, including armed forces, in the U.S.). Headship, education and work are harmonized and differ from the regression moderators. Axes omit each country's most extreme 0.5%.

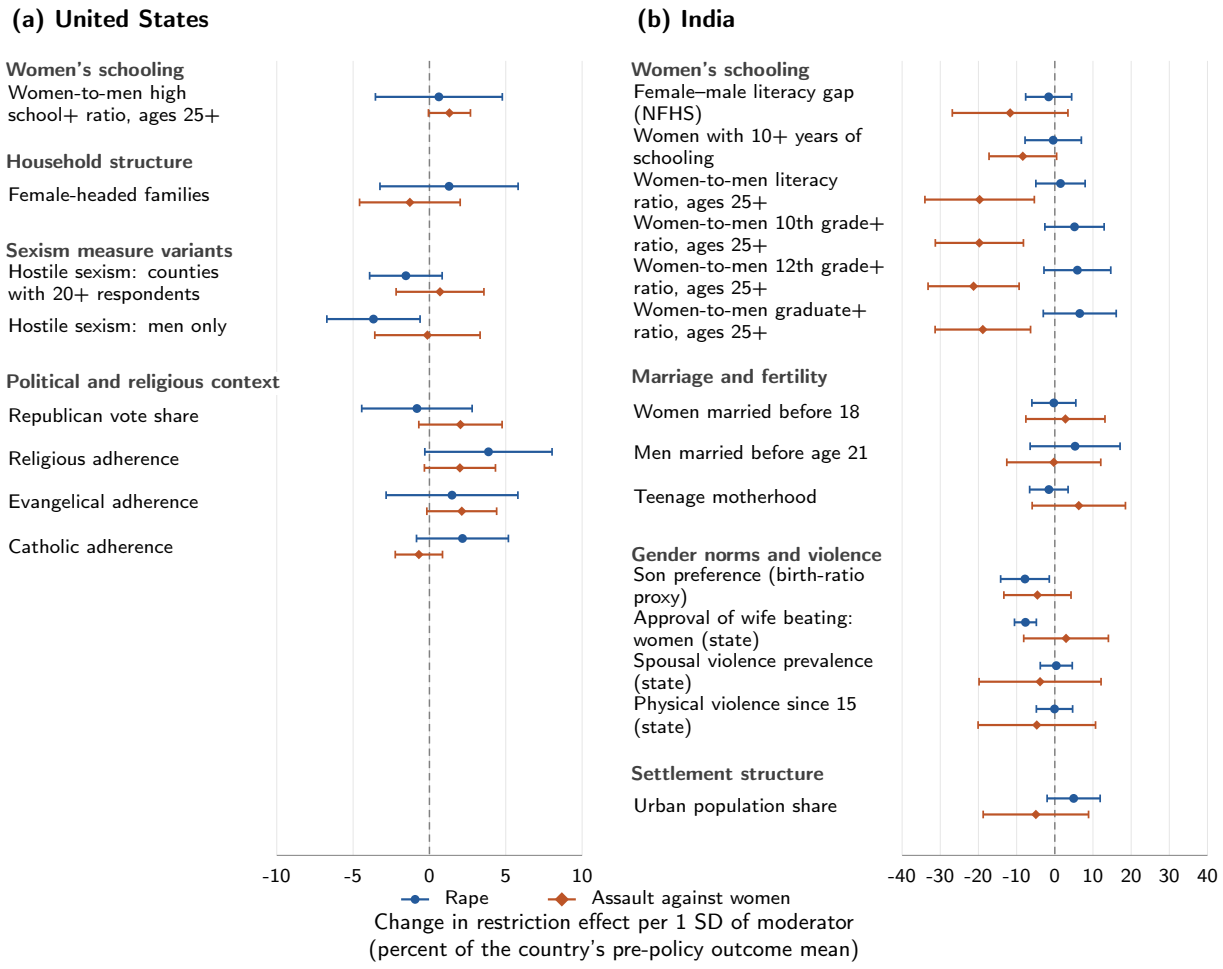


Figure 14: Additional moderators of restriction effects

Notes: Each point corresponds to the β_2 coefficient from equation 4. Moderating variables are standardized within country. India includes district and state × year fixed effects, annual night lights, forest cover, $PM_{2.5}$, 2G coverage and population-quintile linear trends. The U.S. includes county and calendar year-month fixed effects, without additional covariates. Confidence intervals at 90% use district clusters in India, state clusters for its state-level measures (27), and state clusters in the U.S. Treatment, normalization and assault definitions follow Figure 9. NFHS-4 (2015–2016) literacy covers ages 15–49; early marriage, women aged 20–24 and men aged 25–29; teenage motherhood, ages 15–19. Census schooling ratios (India, 2011; U.S., ACS 2018–2022) divide the share of women by the share of men aged 25+ who are literate, have completed 10th grade or higher, 12th grade or higher (U.S.: high-school graduate or higher), or hold a graduate degree or higher. Son preference is the negative female-to-male birth ratio. State measures record women's approval of any wife-beating justification, spousal physical/sexual violence among ever-married women, and physical violence since age 15. U.S. headship uses families as denominator. CES variants use raw county means with 20+ respondents or 10+ men; vote shares and adherence are from 2020. Samples vary; men's early marriage has 185 district clusters.

D.2 U.S. offender age

Table 9: U.S. restriction effects by alleged-offender age

Offender age	Rape	Any sexual offense	Assault	Female-victim assault
Age ≤ 14	0.005 (0.012)	-0.004 (0.027)	-0.113 (0.144)	-0.066 (0.085)
Age 15–17	-0.010 (0.019)	-0.011 (0.041)	0.020 (0.162)	-0.014 (0.085)
Age 18–21	-0.012 (0.015)	-0.041 (0.028)	-0.384** (0.184)	-0.206** (0.096)
Age 22–35	-0.014 (0.033)	-0.025 (0.051)	-1.499* (0.871)	-0.899* (0.529)
Age 36+	0.028 (0.037)	0.025 (0.061)	-1.964** (0.828)	-1.096** (0.499)
Observations	9180	9180	9180	9180
States	51	51	51	51
Equal age effects p-value	0.420	0.418	0.005	0.062
State × age fixed effects	Yes	Yes	Yes	Yes
Age × calendar-month fixed effects	Yes	Yes	Yes	Yes
State × year fixed effects	No	No	No	No

Notes: Each column is a state–age-group–month fixed-effects regression. Outcomes are monthly incidents per 100,000 residents. The models include state-by-age-group and age-group-by-calendar-month fixed effects; state-by-year fixed effects are omitted because the laws are assigned by state, and their inclusion leaves every between-age contrast and the equality test unchanged. Standard errors are clustered by state. The equality row tests whether all age-group treatment effects are equal. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

D.3 India instrumental-variables interactions

Appendix Table 10 re-estimates the continuous India heterogeneity exercises using the IV design, with the same moderator-by-year controls. The interacted first stages are weak, with Kleibergen–Paap statistics between 1.4 and 3.5. None of the interactions is statistically distinguishable from zero, and several have the opposite sign from their OLS counterparts. We therefore do not use these estimates to corroborate the OLS heterogeneity results.

D.4 Household structure above and below the national median

Appendix Table 11 replaces the continuous household-share interaction with a national median split. The measure is harmonized across countries: female-headed households are divided by all households. The U.S. estimates move in the same direction for rape but are imprecise; the exercise makes the attenuation of the India assault effect especially transparent.

D.5 Female-headed households and competing district characteristics

Appendix Table 12 asks whether the female-headed-household interaction in Figure 9 proxies for other district characteristics. Each panel adds to the baseline interaction the interaction with one competitor, and the last panel adds all five. The competitors are the 2011 urban population share, the female-minus-male literacy and work-participation gaps, the never-married male surplus at ages 20–39, and the India Patriarchy Index, each standardized nationally and each entering with its own moderator-by-year indicators. The female-headship interaction for assault is stable against urbanization, the work gap, the marriage-market measure, and the patriarchy index, and it is -1.200

Table 10: India mechanism heterogeneity: instrumental variables

	Rape	Assault against women
Panel A. Young-adult male share		
Exposed × Post	0.906	0.000
	(0.694)	(2.687)
Exposed × Post × moderator	−0.254	7.342
	(1.486)	(5.855)
Kleibergen–Paap F-statistic	2.46	2.48
Panel B. Female-headed household share		
Exposed × Post	0.971	1.974
	(0.699)	(3.019)
Exposed × Post × moderator	−0.212	−0.307
	(0.505)	(2.555)
Kleibergen–Paap F-statistic	3.44	3.45
Panel C. India Patriarchy Index		
Exposed × Post	0.664	3.069
	(0.818)	(3.807)
Exposed × Post × moderator	0.528	−0.870
	(1.114)	(4.617)
Kleibergen–Paap F-statistic	1.39	1.39
Mean dependent variable	2.879	6.857
Observations	4,885	4,881
Districts	408	408
District fixed effects	Yes	Yes
State × year fixed effects	Yes	Yes
Additional time-varying controls	Yes	Yes

Notes: Each panel–outcome cell is a separate 2SLS regression. Exposed × Post and its interaction with the moderator named in the panel heading are instrumented by the preferred poor-state lightning instrument and its interaction with the nationally standardized moderator. Models include district and state-by-year fixed effects, the baseline time-varying controls, and the moderator interacted with year indicators (not reported). The moderator level is absorbed by district fixed effects. Standard errors, in parentheses, are clustered by district. The low Kleibergen–Paap statistics motivate treating these estimates as diagnostic. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 11: Restriction effects by female-headed-household prevalence

	India		United States	
	Rape	Assault against women	Rape	Female-victim assault
Below national median	0.239	2.380***	−0.205	−0.741
	(0.170)	(0.704)	(0.140)	(0.623)
Above national median	0.187	0.037	0.050	−2.124*
	(0.149)	(0.689)	(0.079)	(1.061)
p-value: equal effects	0.801	0.029	0.084	0.202
Observations	4,909	4,903	97,551	97,551
Districts/counties	410	410	2,974	2,974
Clusters	410	410	49	49

Notes: The table reports restriction effects below and above each country's national median share of female-headed households, estimated in one regression per outcome that includes separate calendar year-month (United States) or year (India) effects for the two groups. The United States uses county fixed effects; India uses the baseline OLS specification with district and state-by-year fixed effects and the time-varying controls. The final row reports the test of equal effects across the two groups. Outcomes are incidents per 100,000 residents, monthly in the United States and annual in India. Standard errors, in parentheses, are clustered at the level of policy assignment: by state in the United States and by district in India. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

(0.504) with all five competitors included. The literacy gap is the one measure that absorbs part of it when the two are compared on their own; the two are correlated 0.41 across districts, and we read them as a bundle describing women's relative standing rather than as separable channels.

Table 12: Female-headed households and competing district characteristics, India

	Rape	Assault against women
Panel A. FHH alone		
Exposed × Post	0.227*	1.226***
	(0.129)	(0.460)
Exposed × Post × FHH share	−0.071	−1.003**
	(0.145)	(0.425)
Panel B. FHH and urban population share		
Exposed × Post	0.206	1.473***
	(0.145)	(0.466)
Exposed × Post × FHH share	−0.074	−1.002**
	(0.147)	(0.453)
Exposed × Post × urban population share	0.105	−0.213
	(0.128)	(0.593)
Panel C. FHH and literacy rate, female minus male		
Exposed × Post	0.224	1.058**
	(0.139)	(0.507)
Exposed × Post × FHH share	−0.060	−0.649
	(0.155)	(0.444)
Exposed × Post × literacy rate, female minus male	−0.007	−1.086*
	(0.139)	(0.576)
Panel D. FHH and work-participation rate, female minus male		
Exposed × Post	0.178	0.894
	(0.135)	(0.592)
Exposed × Post × FHH share	−0.048	−1.148**
	(0.145)	(0.465)
Exposed × Post × work-participation rate, female minus male	−0.079	0.849
	(0.118)	(0.669)
Panel E. FHH and never-married male surplus, ages 20–39		
Exposed × Post	0.208	1.090**
	(0.132)	(0.461)
Exposed × Post × FHH share	−0.062	−0.937**
	(0.151)	(0.442)
Exposed × Post × never-married male surplus, ages 20–39	−0.027	−0.019
	(0.112)	(0.510)
Panel F. FHH and India Patriarchy Index		
Exposed × Post	0.226*	1.199***
	(0.130)	(0.453)
Exposed × Post × FHH share	−0.090	−1.267**
	(0.165)	(0.493)
Exposed × Post × India Patriarchy Index	−0.023	−0.448
	(0.125)	(0.404)
Panel G. FHH and all five competitors		
Exposed × Post	0.127	0.877
	(0.146)	(0.561)
Exposed × Post × FHH share	−0.026	−1.200**
	(0.164)	(0.504)
Exposed × Post × urban population share	0.215	0.302
	(0.145)	(1.031)
Exposed × Post × literacy rate, female minus male	−0.092	−0.840
	(0.204)	(0.610)
Exposed × Post × work-participation rate, female minus male	−0.021	0.491
	(0.146)	(0.917)
Exposed × Post × never-married male surplus, ages 20–39	−0.031	0.296
	(0.123)	(0.597)
Exposed × Post × India Patriarchy Index	0.077	−0.967*
	(0.180)	(0.500)
Mean dependent variable	2.871	6.831
Observations	4,909	4,903
Districts	410	410
District fixed effects	Yes	Yes
State × year fixed effects	Yes	Yes
Moderator × year indicators	Yes	Yes
Additional time-varying controls	Yes	Yes

Notes: OLS. FHH share = female-headed households as a share of all households, Census 2011. Competitors are 2011 Census district characteristics (urban population share; female minus male literacy rate, population aged 7+; female minus male work-participation rate, Census workers over population; never-married men minus women as a share of all never-married persons aged 20–39) and the IIPS India Patriarchy Index (NFHS-4). Pre-ban night lights = log of one plus mean 2013–2016 VIIRS radiance. Exposed = district whose maximum pre-period 3G/4G area coverage exceeds 0.1; Post = years 2019–2022. Each moderator is standardized to a national z-score over the districts in the estimation sample (mean zero, unit standard deviation), so "Exposed × Post" is the effect at the national mean of every moderator. Every regression includes, for each moderator it interacts, the moderator interacted with calendar-year indicators (2018 omitted; not reported), district fixed effects, state × year fixed effects, and the time-varying controls of the main specification (night lights, vegetation cover, PM2.5, 2G coverage, and 2011 population quintile × linear trends). Outcomes are counts per 100,000 of the IIPS ratio-method projected district population (coverage-consistent, mid-year). Standard errors clustered by district in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.6 Intimate-partner and non-partner violence

Table 13: Localization inside and outside intimate relationships

	Baseline	Alternative estimator
Panel A. India: inside- and outside-household outcomes		
	OLS	IV
Rape	0.209* (0.126)	1.049** (0.502)
Assault on women	1.436*** (0.458)	2.192 (1.566)
Cruelty by husband or relatives	0.461 (0.349)	1.348 (1.950)
Dowry deaths	-0.049* (0.025)	-0.048 (0.189)
Panel B. United States: victim–offender relationship		
	TWFE	CSDID
All female-victim assault	-1.591** (0.717)	-1.288* (0.762)
Broad IPV assault	-0.465 (0.291)	-0.214 (0.392)
Known non-IPV or stranger assault	-0.483 (0.336)	-0.611 (0.468)
Unknown relationship	-0.629** (0.248)	-0.379 (0.330)
IPV minus non-IPV	0.018 (0.247)	0.397 (0.541)

Notes: India estimates use the district specification, with OLS and IV reported side by side; the IV column uses the preferred poor-state instrument. Cruelty by a husband or relatives is the available administrative proxy for intimate-partner violence. U.S. categories use NIBRS victim–offender relationship codes; the TWFE column includes county and calendar year-month fixed effects, and the alternative estimator is Callaway–Sant'Anna staggered difference-in-differences. The India OLS difference between cruelty and assault, estimated on the district-years where both are observed, is -0.894 ($p = 0.094$), so cruelty rises by less than assault; the U.S. TWFE difference between intimate-partner and known non-partner assault has $p = 0.94$. Standard errors are in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

D.7 India term-level search substitution

Appendix Figure 15 decomposes the national India search composite into its five component terms. Searches for the generic terms "xxx" and "sex video" rise after the October 2018 order, while searches for Pornhub, Xvideos, and the generic term "porn" decline. The figure is descriptive and does not establish that the policy caused the before–after changes.

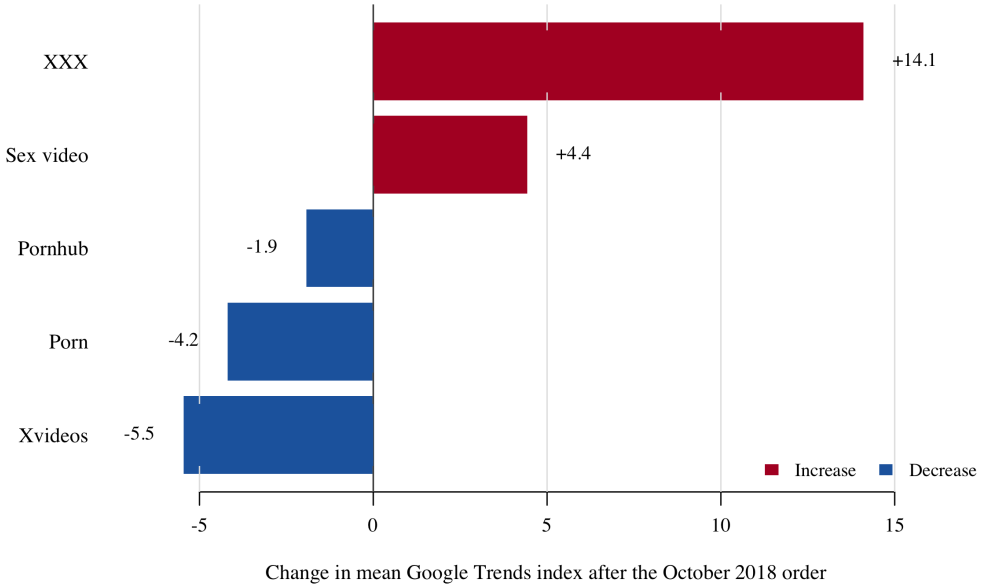


Figure 15: Changes in India pornography-related searches by term

Notes: Each bar is the difference between a term's mean weekly national Google Trends index after and before the October 2018 blocking order. The sample runs from January 2017 through December 2020. The five terms were retrieved in a single Google Trends query, so they share a common scale and the bars can be compared across terms. Each index measures a term's share of all Google searches in India, relative to the largest weekly share among the five terms. The figure is descriptive rather than a causal policy estimate.